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Unconscious cerebral initiative and the role of conscious will in voluntary action

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Abstract: Voluntary acts are preceded by electrophysiological "readiness potentials" (RPs). With spontaneous acts involving no preplanning, the main negative RP shift begins at about -550 ms. Such RP's were used to indicate the minimum onset times for the cerebral activity that precedes a fully endogenous voluntary act. The time of conscious intention to act was obtained from the subject's recall of the spatial clock position of a revolving spot at the time of his initial awareness of intending or wanting to move (W). W occurred at about -200 ms. Control experiments, in which a skin stimulus was timed (S), helped evaluate each subject's error in reporting the clock times for awareness of any perceived event.

For spontaneous voluntary acts, RP onset preceded the uncorrected Ws by about 350 ms and the Ws corrected for S by about 400 ms. The direction of this difference was consistent and significant throughout, regardless of which of several measures of RP onset or W were used. It was concluded that cerebral initiation of a spontaneous voluntary act begins unconsciously. However, it was found that the final decision to act could still be consciously controlled during the 150 ms or so remaining after the specific conscious intention appears. Subjects can in fact "veto" motor performance during a 100-200-ms period before a prearranged time to act.

The role of conscious will would be not to initiate a specific voluntary act but rather to select and control volitional outcome. It is proposed that conscious will can function in a permissive fashion, either to permit or to prevent the motor implementation of the intention to act that arises unconsciously. Alternatively, there may be the need for a conscious activation or triggering, without which the final motor output would not follow the unconscious cerebral initiating and preparatory processes.

Keywords: conscious volition; event-related chronometry; free will; mental timing; motor organization; readiness potentials; unconscious processes; voluntary action

One of the mysteries in the mind—brain relationship is expressed in the question: How does a voluntary act arise in relation to the cerebral processes that mediate it? The discovery of the "readiness potential" (RP) opened up possibilities for experimentally addressing a crucial feature of this question. The RP is a scalp-recorded slow negative shift in electrical potential generated by the brain and beginning up to a second or more before a self-paced, apparently voluntary motor act (Deecke, Grozinger & Kornhuber 1976; Gilden, Vaughan & Costa 1966; Kornhuber & Deecke 1965). The long time interval (averaging about 800 ms) by which RP onset preceded a self-paced act raises the crucial question whether the conscious awareness of the voluntary urge to act likewise appears so far in advance. If a conscious intention or decision to act actually initiates a voluntary event, then the subjective experience of this intention should precede or at least coincide with the onset of the specific cerebral processes that mediate the act.

This issue has recently been subjected to experimental tests and analyses, which I shall review briefly (Libet, Gleason, Wright & Pearl 1983; Libet, Wright & Gleason 1982; 1983). The experimental findings led us to the conclusion that voluntary acts can be initiated by unconscious cerebral processes before conscious intention appears but that conscious control over the actual motor performance of the acts remains possible. I shall discuss these conclusions and their implications for concepts of "the unconscious" and of conscious voluntary action. I propose the thesis that conscious volitional control may operate not to initiate the volitional process but to select and control it, either by permitting or triggering the final motor outcome of the unconsciously initiated process or by vetoing the progression to actual motor activation. (The reader is referred to our original cited research papers for the full details of the experimental techniques and observations together with their evaluation, etc.)

1. Definitions of voluntary action and will

Since the meanings assigned to the terms "voluntary action" and "will" can be quite complicated and are often related to one's philosophical biases, I shall attempt to clarify their usage here. In this experimental investigation and its analysis an act is regarded as voluntary and a function of the subject's will when (a) it arises endogenously, not in direct response to an external stimulus or cue; (b) there are no externally imposed restrictions or compulsions that directly or immediately control subjects' initiation and performance of the act; and (c) most important, subjects *feel* introspectively that they are

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performing the act on their own initiative and that they *are free* to start or not to start the act as they wish. The significance of point (c) is sharply illustrated in the case of stimulating the motor cortex (precentral gyrus) in awake human subjects. As described by Penfield (1958) and noted by others, under these conditions each subject regarded the motor action resulting from cortical stimulation as something done *to* him by some external force; every subject felt that, in contrast to his normal voluntary activities, "he," as a self-conscious entity, had not initiated or controlled the cortically stimulated act.

The technical requirements of experiments do impose limits on the kinds of voluntary choices and settings available to the subject. The nature of the acts must be prescribed by the experimenter. In the studies to be discussed here the acts were to consist uniformly of a quick flexion of the fingers or wrist of the right hand; this yielded a sharply rising electromyogram (EMG) in the appropriate muscle to serve as a trigger for O-reference time. The subjects were free, however, to choose to perform this act at any time the desire, urge, decision, and will should arise in them. (They were also free *not* to act out any given urge or initial decision to act; and each subject indeed reported frequent instances of such aborted intentions.) The freedom of the subject to act at the time of his choosing actually provides the crucial element in this study. The objective was in fact to compare the time of onset of the conscious intention to act and the time of onset of associated cerebral processes. The specific choice of what act to perform was not material to the question being asked.

Volitional processes may operate at various levels of organization and timing relative to the voluntary act. These may include consciously deliberating alternative choices as to what to do and when, whether or not to act, whether or not to comply with external orders or instructions to act, and so on. If any of these processes are to result in the motor performance of a voluntary act, they must somehow work their way into a "final common motor activation pathway" in the brain. Without an overt motor performance any volitional deliberation, choosing, or planning may be interesting for its mental or psychological content, but it does not constitute *voluntary action.* It is specifically this overt performance of the act that was experimentally studied by us.

In the present experimental paradigm subjects agree to comply with a variety of instructions from the experimenter. One of these is an expectation that the subject is to perform the prescribed motor act at some time after the start of each trial; another is that he should pay close introspective attention to the instant of the onset of the urge, desire, or decision to perform each such act and to the correlated spatial position of a revolving spot on a clock face (indicating "clock time"). The subject is also instructed to allow each such act to arise "spontaneously," without deliberately planning or paying attention to the "prospect" of acting in advance. The subjects did indeed report that the inclination for each act appeared spontaneously ("out of nowhere"), that they were consciously aware of their urge or decision to act before each act, that they felt in conscious control of whether or not to act, and that they felt no external or psychological pressures that affected the time when they decided to act (Libet et al. 1982; Libet, Gleason, Wright & Pearl 1983).

Thus, in spite of the experimental requirements, the basic conditions set out above for a voluntary act were met. Conditions for the subject's decision as to when to act were designated to represent those one could associate with a conscious, endogenously willed motor action, So that one could study the cerebral processes involved in such an act without confusing them with deliberative or preparatory features that do not necessarily result in action.

Finally, one should note that the voluntary action studied was defined operationally, including appropriate and reliable reports of introspective experiences. The definition is not committed to or dependent upon any specific philosophical view of the mind-brain relationship. However, some implications that are relevant to mind-brain theories will be drawn from the findings.

2. Cerebral processes precede conscious intention

Two experimental issues have to be resolved in order to obtain a relevant answer to the questions about the relative timing of conscious intentions and cerebral processes in the performance of voluntary acts: (1) Is the RP a valid indicator of cerebral processes that mediate voluntary acts? (2) How can one meaningfully measure the onset of the conscious intention, urge, or will to perform a specific voluntary motor act?

2.1. RPs in voluntary acts

Self-paced acts were used in the discovery of RPs (Gilden et al. 1966; Kornhuber & Deecke 1965) and in subsequent RP studies (e.g., Deecke et al. 1976; Shibasaki, Burrett, Halliday & Halliday 1980; Vaughan, Costa & Ritter 1968). Such acts have features that may compromise the exercise of free volition or confuse its interpretation: (a) Recording an RP requires averaging many events. When these self-paced acts are repeated in a continuous series, with irregular intervening intervals of 3-6 sec as selected by the subject, they become boring and may come to be performed in a stereotyped and almost automatic way, with no assurance that conscious control could be exercised in each trial, (b) Since subjects were asked to act within an allotted time interval, they may be under pressure consciously or unconsciously to plan to act within the time limit; that is, the subject's voluntary choice of when to act may be compromised by an external requirement, (c) Subjects are required not to blink until just after each act. The need to blink may impel the subject to act, thus serving as an external controlling factor.

In a study of what we termed "self-initiated" acts, these external forces were minimized or eliminated (Libet et al. 1982). Each trial in an averaging series of 40 trials was initiated as a separate independent event after a flexible delay determined by each subject's own readiness to proceed; there was no limit on the time in which subjects were to act; they were given the option to blink if necessary. For each trial, subjects were asked to perform a simple quick flexion of the wrist or fingers at any time they felt the "urge" or desire to do so; timing was to be entirely "ad lib," that is, spontaneous and fully endoge-

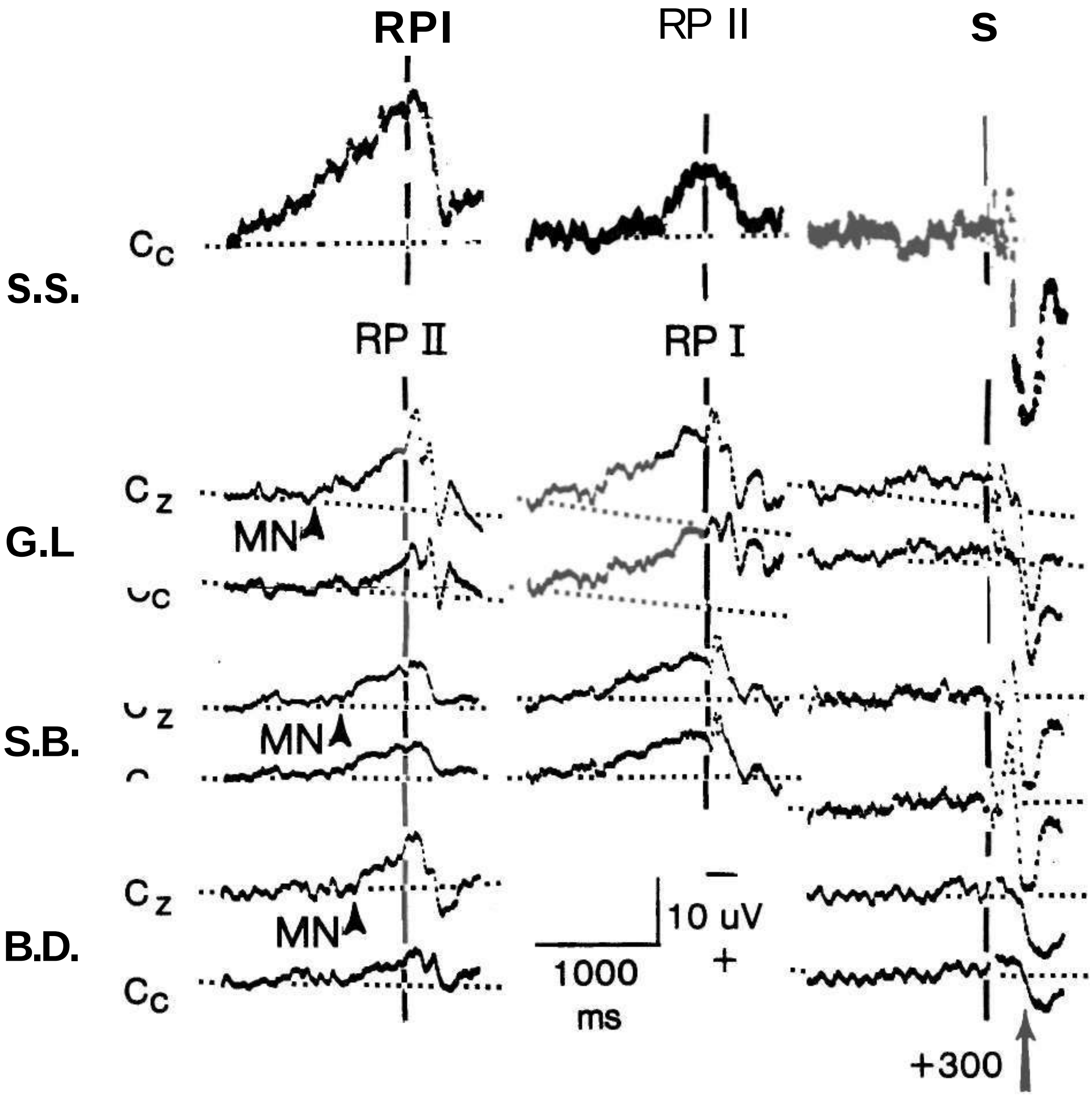

Figure 1. Readiness potentials (RP) preceding self-initiated voluntary acts. Each horizontal row is the computer-averaged potential for 40 trials, recorded by a DC system with an active electrode on the scalp, either at the midline-vertex (Cz) or on the left side (contralateral to the performing right hand) approximately over the motor/premotor cortical area that controls the hand (Cc).

When every self-initiated quick flexion of the right hand (fingers or wrist) in the series of 40 trials was (reported as having been) subjectively experienced to originate spontaneously and with no preplanning by the subject, RPs labeled type II were found in association. (Arrowheads labeled MN indicate onset of the "main negative" phase of the vertex recorded type II RPs in this figure; see Libet et al. 1982. Onsets were also measured for 90% of the total area of RP; see Table IB). When an awareness of a general intention or preplanning to act some time within the next second or so was reported to have occurred before some of the 40 acts in the series, type I RPs were recorded (Libet et al. 1982). In the last column, labeled S, a near-threshold skin stimulus was applied in each of the 40 trials at a randomized time unknown to the subject, with no motor act performed; the subject was asked to recall and report the time when he became aware of each stimulus in the same way he reported the time of awareness of wanting to move in the case of self-initiated motor acts.

The solid vertical line through each column represents 0 time, at which the electromyogram (EMG) of the activated muscle begins in the case of RP series, or at which the stimulus was actually delivered in the case of S series. The dashed horizontal line represents the DC baseline drift.

For subject S.S., the first RP (type I) was recorded before the instruction "to let the urge come-on its own, spontaneously" was introduced; the second RP (type II) was obtained after giving this instruction in the same session as the first. For subjects G.L., S.B., and B.D., this instruction was given at the start of all sessions. Nevertheless, each of these subjects reported some experiences of loose preplanning in some of the 40-trial series; those series exhibited type I RPs rather than type II. Note that a slow negative shift in scalp potential that precedes EMGs of self-initiated acts (RP) does not precede the skin stimulus in S series. However, evoked potentials following the stimulus are seen regularly to exhibit a large positive component with a peak close to +300 ms (arrow indicates this time); this P300 event-related potential had been shown by others to be associated with decisions about uncertain events (in this case, the time of the randomly delivered stimulus), and it also indicates that the subject is attending well to the experimental conditions. (Modified from Libet et al. 1982.)

nous. (For full technical details see Libet et al. 1982; Libet, Gleason, Wright & Pearl 1983.) Subjects reported that they were aware of the urge or intention to move before every act in the series; that is, the acts were not automatic or involuntary "tics." The absence of any larger meaning in this act appears to exclude external psychological or other factors as controlling agents. Acts of this kind may thus be taken as paradigmatic examples of unrestricted volition, at least in regard to choosing when to act. The basic initiating process for these simpler volitional acts may be the same as that for the actual motor expression of other, more complex forms of voluntary action, since the latter are manifested behaviorally only when final decisions to move have been made.

These self-initiated, endogenous acts were indeed found to be preceded by RPs (Libet et al. 1982). When all 40 self-initiated acts in an averaging series were performed with this spontaneous ad lib timing, with no reports of specific preplanning to act, the recordable averaged RP generally had an onset for its main negative rise at about 550 (±150) ms before the motor act began; these were called "type II" RPs (see Figure 1). (As is customary, the beginning of the muscle activity is signaled by the onset of the electromyogram, EMG, recorded at an appropriate muscle. This provides the "0-time" trigger for averaging the preceding scalp potential at the vertex and for other timing features.)

In some trials, subjects did report experiencing some general preplanning or preparation to act in the near future a few seconds before the act, despite the encouragement to be completely spontaneous. These occurrences were reported during the "debriefing" conducted at the end of each series of 40 trials. In those series that included even a small number of such reported experiences, a ramplike RP with onset at about —1050 ms (±175) was typically recorded (the "type I" RPs, Figure 1); these RPs were called type I because they resembled those RPs previously described for self-paced acts (e.g., Deecke et al. 1976). However, subjects all insisted that the more specific urge or intention to perform the actual movement was still experienced just before each act in a type I series, just as in the type II series; and they clearly distinguished this urge or intention from any advance feelings of preplanning to move within the next few seconds. In other experiments that required deliberate preplanning by instructing the subject to act at a preset time, there appeared a large ramplike RP that resembled the type I RP of our self-initiated acts. We concluded, therefore, that the RP component that starts at about —550 ms, the one that predominates in type II RPs recorded when all acts in a 40-trial series are spontaneous, is the one uniquely associated with an exclusively endogenous volitional process. The latter process is distinguished from a looser preintentionality or general preparation-to-act-soon that is not necessarily endogenous (Libet et al. 1982).

2.2. *Timing the conscious intention to act*

It presented a difficult challenge to devise the operational criteria for determining the time at which the subjects become aware of wanting or deciding to act. One begins with the premise that this subjective event is only accessible introspectively to the subject himself; some kind of report of this by the subject is therefore a requirement (Libet 1966; 1973; 1981b). Conscious subjective experience, in this case an awareness of the endogenous urge or intention to move, is a primary phenomenon; it cannot be defined in an a priori way by recourse to any externally observable physical event, including any behavioral action not directly representative of the subject's introspective report (Beloff 1962; Creutzfeldt & Rager 1978; Eccles 1980; Libet 1965; 1966; 1981a; 1981b; Nagel 1979: Popper & Eccles 1977; Thorpe 1974). The report, whether a verbal one or some other motor indication (e.g., pressing an answer key), *cannot* be an immediate one made as soon as the conscious experience has occurred: (a) Cerebral preparations for the motor action of reporting might introduce some confusing RPs of their own. (b) There could be a substantial delay for neurally organizing and achieving the motor actions required to make the report, (c) When a premium is put on the speediness of a response, as in measuring reaction time to a stimulus, there is no assurance that the motor response directly indicates when an actual subjective experience has occurred. The fast response to a stimulus can represent an unconscious mental process; but when the subject becomes consciously aware of the stimulus some hundreds of ms later (Libet 1965; 1966; 1973), the experience can be subjectively referred backward in time to an early neural signal (Libet 1981a; 1982; Libet, Wright, Feinstein & Pearl 1979).

For present purposes the experience of the time of the first awareness of wanting to move ("W") was related by the subject to his observation of the "clock position" of a spot of light revolving in a circle on the face of a cathode ray oscilloscope (CRO); the subject subsequently recalled and reported this position of the spot. (For technical details see Libet, Gleason, Wright & Pearl 1983.) Thus, the timing of this experience was converted to a reportable, visually related spatial image, analogous to reading and later recalling the clock time for any experience. This indicator of the time of first awareness of the intention to move could then be compared to (a) the actual time of the voluntary motor act, as indicated by the EMG recorded from the appropriate muscle, and (b) the time of appearance of the simultaneously recorded RP that is generated by the brain in advance of each act. For all self-initiated acts studied, the actual mean Ws for each series of 40 acts averaged about —200 ms (Table 1); that is, subjects reported becoming consciously aware of the urge to move 200 ms before the activation of the muscle (EMG) (Libet, Gleason, Wright & Pearl 1983).

2.3. *Difference between RP onset and reported time of conscious intention, W*

The RP onset time was found to be consistently in advance of W, the time of initial awareness of wanting to move (Table 1). For all of the series in which all 40 acts were experienced as fully spontaneous and unplanned, the average RP onset of (type II, described above) was about —535 ms relative to the initiation of muscle action (as indicated by the EMG). Reported times of conscious intention to act (W) in these same series with type II RPs averaged about —190 ms. The average onset of these RPs therefore *precedes* average W by about 345 ms. (For the significance of the even larger discrepancy in series ex-

Table 1. *Average times (ms) of reported awareness and recorded readiness potentials (RP)for all experimental series on 5 subjects, in 6 or more separate sessions for each subject. Each series consisted of 40 trials in which subjects reported only W or M or S times in that entire series. (Modified from Libet, Gleason, Wright ir Pearl 1983.)*

A. Reported awareness times (ms) relative to recorded muscle activation (EMG).

	W[a]		(W-S)[b]		M[c]		(M-S)	
Subject	n[d]	X				X	n	X
S.B.	8	-125	5	-123	4	-59	4	-19
GL.	8	-282	5	-136	4	-202	4	-60
B.D.	7	-152	4	-249	4	+51	4	-32
S.S.	7	-246	4	-145	4	-118	4	-7
CM.	8	-227	4	-165	4	-103	4	-20
Grand averages	38	-207	22	-160	20	-86	20	-28

B. reported time of conscious intention (W) related to recorded RP onset, separated for type I and II (see text).

Type of RP, for W series	Reported awareness times		Onset of RP (in W series)		(Onset RP) *minus* (W) using onset of:		(Onset RP) *minus* (W-S), using onset		
	n	W	**RPmn**	**RP 90%**	**RPmn**	**RP90%**	n	**RPmn**	**RP 90%**
II	20	-192	-535	-527	-343	-333	14	-366	-323
I	12	-233	-1025	-784	-825	-522	6	-950	-585

[a] W = time of first awareness of wanting to move (see text). [b]*S* was based on reported time of awareness of the sensation elicited by a near-threshold electrical stimulus pulse to the hand, delivered at a randomly irregular time in each trial. The attentive and other conditions (subject's observing and recalling "clock time" for each S) closely paralleled those for the W and M series, except that the event was an externally induced sensory one instead of a self-initiated motor one. The difference (S) between reported and actual stimulus times may be regarded as a measure of the subject's error or "bias" when observing and reporting under the experimental conditions employed (see text and Libet et al. 1982; Libet, Gleason, Wright & Pearl 1983). Almost all subjects exhibited a negative net bias for S (except for B. D.). For (W-S) values, the S bias exhibited by each subject is subtracted from the W values available in the same sessions. [C]M was time reported for subjects' awareness that they were actually moving, instead of wanting to move as for W. The consistently negative though smaller values for M suggest that it reflects the time of initiation of the final motor cortical output, i.e., the endogenous "command to move" (McCloskey et al. 1983), rather than the awareness of proprioceptive sensory impulses evoked after onset of the movement (see text). [d]n = number of series, each of 40 trials. Each average or X value for n series is the mean of the mean Ws (or mean Ms), each of which was determined for each series of 40 trials (see Libet, Gleason, Wright & Pearl 1983). [e] Onsets of RP, relative to EMG (electromyogram indicating that the activation of the muscle has started), are given for both the "main negative shift" (MN), as estimated by eye, and for the time at which the last 90% of the total area under the RP tracing begins.

hibiting type I RPs, those recorded when some acts were preplanned, see Libet, Gleason, Wright & Pearl 1983.)

This riming relationship, with the "physical" (cerebral process) preceding the "mental" (conscious intention), held not just for average values of all series but for each individual series of 40 self-initiated acts in which RP and W were recorded simultaneously. Although RPs of 40 events were averaged to produce the recorded RP, statistical and mathematical evaluation of the experimental data strongly supported the view that each individual RP precedes each conscious urge (see Libet, Gleason, Wright & Pearl 1983). The timing relationship also held regardless of which of the available parameters was used either to measure the onset of the RP (for the onset of its main negative component or for 90% of its area), or for W (using either the "actual" or the "order" mode of recall of the clock position of the revolving spot at the time of conscious intention; see section 2.4.3). Confidence in the significance of the difference between RP onset and W is further raised by the feet that it was almost invariably large in all the individual series when compared to the standard error of the mean value for W in each respective series. In addition, the individual W time reported for each act in a series of 40 trials was almost never negative to (timed in advance of) the onset of the averaged RP recorded for that series. In view of the foregoing considerations (and additional methodological checks listed in Libet, Gleason, Wright & Pearl 1983), the substantial interval by which RP onset precedes W appears sufficiently reliable. Questions about the validity and meaning of the values must still be considered.

2.4. Validity of criteria for the time of a conscious intention to act

Because subjective experiences are not directly accessible to an external observer, it may be logically impossible for the external observer to determine directly any feature of the experience (Creutzfeld & Rager 1978; Libet et al. 1979; Nagel 1979). This restriction applies also to the actual time of a subjective experience (Harnad, unpublished; Libet et al. 1979). We do not normally apply the criterion of logical impossibility to the validity of introspective reports by the people around us in everyday life although we do attempt to evaluate the accuracy of these reports. I do not know of any serious believer in Berkeleyan solipsism, even though that position may be logically unassailable. (On the other hand, the descriptions even of externally observable physical events cannot be regarded as having an absolute validity; they have been appropriately viewed as mental representations or constructs elicited by or developed from the available sensory experiences, e.g., Margenau 1984.)

One is always faced, then, with the unacceptable alternative of not attempting to study a primary phenomenological aspect of our human existence in relation to brain function because of the logical impossibility of direct verification by an external observer. Or one can attempt to evaluate the accuracy of the introspective report and gain confidence in its validity by applying indirect controls, tests and converging operations. In the present study we rely on the subject's ability to associate his introspective awareness (of the urge or decision to move) with the (later reported) position of a visually observed revolving spot, the "clock time." The crucial experimental question thus becomes: Is there any convincing way of estimating what might be the discrepancy between actual and reported times (for the subject's introspective experience of the urge to move)? The several independent types of control evidence discussed below provide confidence that the accuracy of the reported clock times is sufficient for present purposes (i.e., for determining the significance of the difference between RP onset and time of conscious intention).

2.4.1. Comparisons of simultaneous events. Our method requires that the subject observe simultaneously, for later report, the conscious urge or intention to move and a visual experience of "clock position" for the revolving spot on the CRO. Subjective timing comparisons of simultaneous but disparate events are known to be subject to potential errors (see Boring 1957; Efron 1973; Sternberg & Knoll 1973). However, we introduced a control series in each experimental session to help measure such an error. For this, a skin stimulus was delivered at an irregular, randomized time after the start of each trial and the subject reported the time of his awareness of that stimulus. All procedures were otherwise the same as in series of self-initiated acts (except that awareness of the stimulus replaced awareness of the urge to move). The actual time of the stimulus in the control series was later known to the investigator, and the discrepancy between the subject's reported riming and the actual stimulus time could be objectively determined. To the extent that simultaneous observation of visual clock time and awareness of skin sensation shares similar processes and difficulties with simultaneous observation of clock time and awareness of urge to move, one may regard any measured "error" in reports of stimulus time as an estimate of the potential error in reports of W (time of awareness of wanting to move). Skin sensations were commonly reported to occur somewhat in advance of (negative to) the actual delivery time, reminiscent of the prior entry effect (e.g., Allan; 1978; Boring 1957). However, the amount of the error found in the stimulus series did not qualitatively alter the difference between onset of RP and W; in fact, it generally enlarged the difference (Table 1).

2.4.2. Judging onset time of an endogenous mental event. It might be proposed that subjects do not judge the onset of an endogenous mental event such as conscious intention the same way they judge the onset of an experience induced externally by a skin stimulus. In relation to such a suggestion we note:

a. Each subject was instructed to "watch for" and report the earliest appearance of the awareness in question, and subjects did not raise any difficulties about doing this.

b. The onset time even of an intracerebrally generated event of some complexity, although admittedly induced by an applied stimulus, can be reported with no significant delays. In earlier work (Libet et al. 1979), onset time of a vaguely perceived near-threshold sensation elicited by a stimulus to a cerebral somatosensory structure (medial lemniscus) was judged subjectively to differ by only a few tens of ms from the sharper sensation elicited by a skin stimulus. In addition, both the medial lemniscus and the sensory cortex required repetition of stimulus pulses (at 20 per sec) for at least 200 ms, to elicit any subjective sensory experience at all in those experiments. Yet the subjects could consistendy report a different onset time for each; they reported that the medial lemniscus-induced sensation began with no significant delay relative to the sensation elicited by a single pulse stimulus to the skin, whereas onset of the cortical sensation was delayed by the amount of the required stimulus duration (Libet et al. 1979).

c. For two different though related endogenous mental events related to the same voluntary act, the subjects consistently reported different onset times with an appropriate direction of difference. Under the identical experimental conditions for studying the self-initiated acts, the subjects were asked to report the clock time for their awareness of actually moving (M) instead of for awareness of wanting to move (W). M values were, unexpectedly, negative to EMG-0 time and slightly but consistently negative to reported times for awareness of skin stimulus (S) in which no movement was involved (see Table LA).

Because M times were slightly before actual movement, this suggested that M may reflect awareness associated with the immediate initiation of cerebral motor outflow (Libet, Gleason, Wright & Pearl 1983). This would be in accord with the findings by McCloskey, Colebatch, Potter & Burke (1983) that subjective timing of one's own "command to move" preceded the EMG by up to 100 ms; a sensation of having already moved, elicited by input from peripheral sensory sources, was found to be separately reportable with an appropriately delayed time. M thus appears to be an endogenous mental event, different from but related to W. Nevertheless, the subjects did not confuse their reports of onset times for M with those of W; reports of W times (for awareness of wanting to move) were consistendy negative to (in advance of) M times (for awareness of actually committing the movement), by about 120 ms on the average.

2.4.3. Modes of reporting. One way to test and improve confidence in the validity of the reported timings lies in using different and independent but converging modes of observing and reporting. Two quite different modes were used for reporting the "clock positions" of the CRO spot at the time of awareness: (a) absolute readings and (b) order relative to final stopping positions of the CRO spot, varied randomly (see Libet, Gleason, Wright & Pearl 1983). Yet both modes produced values for W that were essentially indistinguishable. (When reporting in the "order" mode, subjects had to recall the position of the moving spot [at the time of initial awareness of the urge to act] only with respect to a final resting position of the spot that was varied randomly in different trials. Subjects needed to make judgments about whether the CRO spot came to rest at a clock position that was "earlier" or "later" than the recalled position of the revolving spot when they were aware of the urge; they did not have to specify an absolute clock position of the moving spot associated with W [Libet, Gleason, Wright & Pearl 1983]. See also McCloskey et al. [1983] for an analogous order method for timing judgments.)

2.4.4. Nonrecallable initial awareness of conscious intention?It might be argued that a nonrecallable phase of a conscious urge exists, so that the reported time would apply only to a later, recallable phase of awareness. However, one should note that to report W time, the subject need recall only the clock position of the revolving spot at the time he first becomes aware of the urge or intention to move and not necessarily the initial awareness itself. In any case, there is no evidence for a nonrecallable initial awareness. But, like some other conceivable hypothetical uncertainties in timing an endogenous mental event, such a hypothesis cannot be excluded since it is presently not experimentally testable.

2.5. *RP as indicator of cerebral initiation*

For the experimental question about the initiation of a voluntary act, one must also consider whether the onset of recorded RP is a valid indicator of the time when cerebral processes begin to produce the act. The precise role of the cerebral activity represented by the RP in the initiation of the voluntary process is yet to be determined. It appears likely that the component of the RP associated with volitional preparation to act is generated in the supplementary motor area, a portion of the cerebral cortex located on the mesial surface of each hemisphere facing the midline (Deecke & Kornhuber 1978; Eccles 1982a; Libet et al. 1982). RPs associated with spontaneous self-initiated acts (type II) are indeed distinctly maximal at the vertex of the head (Libet et al. 1982), a scalp site that is above and adjacent to the supplementary motor areas. It has been proposed that the initial neuronal events in all voluntary movements arise in the supplementary motor areas (Eccles 1982). However, for present purposes it is not necessary that the full role of the supplementary motor area of the RP processes be established. It is only necessary to accept the RP as a valid *indicator* of *minimum* onset times for cerebral processes that initiate the voluntary act, even if these processes should be initiated elsewhere in the brain.

It might be proposed that the RP does not indicate directly or indirecdy the specific initiation of the voluntary act. Rather, the RP might represent preprogramming processes that develop periodically without signifying a volitional function. The actual initiation of a given voluntary act would then depend on conscious activation or triggering of one of these preparatory sequences so as to generate an actual motor discharge. Such a proposal would seem to be an ad hoc speculation not supported by the experimental evidence, (a) The proposal would predict that endogenous RPs appear repeatedly without any associated subjective awareness developing and with no actual voluntary movements occurring. This has not been experimentally demonstrated and would seem to be untestable with present techniques. The RP that precedes an individual voluntary act is not clearly discernible from the background rhythmic activity; averaging of the pre-EMG periods (1.4 sec) for 40 acts gave us a usable though still noisy RP shift at the vertex. However, one should note that individual spontaneous negative and positive slow potential (SP) shifts have been successfully recorded during 5-sec periods preceding a choice reaction test and found to be related to proficiency of performance (Born, Whipple & Stamm 1982)[5]. These interesting spontaneous SPs were apparendy maximal at frontal rather than vertex sites and they were either negative or positive in polarity; they presumably reflect processes different from those of the negative RP that is maximal.at the vertex and obtained in a different mental context, (b) The recorded RPs in self-initiated acts do not exhibit any special electrophysiological event that might signal introduction of an activating process at the reported time of about -200 msec for the conscious urge (Libet et al. 1982; Libet, Gleason, Wright & Pearl 1983). (For RPs in self-paced acts see also Deecke et al. 1976; Shibasaki et al. 1980.) (c) The available evidence suggests that an RP precedes every voluntary act as well as the conscious awareness of the urge to perform each act (Libet et al. 1982; Libet, Gleason, Wright & Pearl 1983). Consequently, the proposal against RP initiation of the act would at best result in a two-stage mediation; "preparatory" cerebral processes would still unconsciously initiate the volitional sequence but consummation of the actual motor action would depend on a conscious control function. This sort of role for the conscious function is compatible with the thesis being advocated in this paper.

Is it possible that the subject's introspective observation of his conscious intention for each act would itself introduce a cerebral process that affects the recorded RP (a question raised by an anonymous editorial reviewer)? In a small number of experiments RPs were recorded for series of 40 self-initiated movements in which no reports of awareness time were requested from or made by the subjects. The RPs of these "no-report" series were similar in form and onset times to RPs of the "report" series (Libet et al. 1982; Libet, Gleason, Wright & Pearl 1983). Furthermore, reporting the time of awareness of a sensory stimulus delivered at a randomly irregular time ("S" series) required the same kind of attention and introspection by the subjects as did the reporting in self-initiated acts; yet there were no significant pre-event potentials at all in association with the stimulation experiments (e.g., Figure 1; Libet et al. 1982; Libet, Wright & Gleason 1983). One may conclude that the "introspective process" did not affect the RPs in any manner significant to the conclusions in the study, and that if there were any electrophysiological correlates of introspective observation or of the attentive state required for it, they are not manifested in the scalp recordings of RPs at the vertex.

3. Unconscious initiation of voluntary acts

Onsets of RPs regularly begin at least several hundred ms before reported times for awareness of any intention to act in the case of acts performed ad lib. It would appear, therefore, that some neuronal activity associated with the eventual performance of the act has started well before any (recallable) conscious initiation or intervention is possible. This leads to the conclusion that cerebral initiation even of a spontaneous voluntary act of the land studied here can and usually does begin *unconsciously.* (The term "unconscious" refers here simply to all processes that are not expressed as a conscious experience; this may include and does not distinguish among preconscious, subconscious, or other possible nonreportable unconscious processes.) Put another way, the brain "decides" to initiate or, at least, to prepare to initiate the act before there is any reportable subjective awareness that such a decision has taken place.

It might be argued that unconscious initiation applies to the kind of spontaneous but perhaps impulsive voluntary act studied here, but not to acts involving slower conscious deliberation of choices of action. The possible role of unconscious cerebral activities in conscious deliberation is itself a difficult and open question. In any case, after a deliberate course of action has been consciously selected, the specific voluntary execution of that action, i.e., the cerebral activation and implementation of the actual motor deed, may well be related to that for the ad lib kind of act we have studied. Even when a more loosely defined conscious preplanning has appeared a few seconds before a self-initiated act, the usual specific conscious intention to perform the act was consistently reported as having been experienced separately just prior to each act by all subjects (Libet et al. 1982; Libet, Gleason, Wright & Pearl 1983). This leads me to propose that the performance of every conscious voluntary act is preceded by special unconscious cerebral processes that begin about 500 ms or so before the act.

3.1. Cerebral basis of unconscious mental functions

A role for "the unconscious" in modifying and controlling volitional decisions and actions was advocated long ago (e.g., Freud 1955; Whyte 1960). This role was inferred from analyses of strong but indirect psychological evidence. The present experimental findings provide direct evidence that unconscious processes can and do initiate voluntary action and point to a definable cerebral basis for this unconscious function.

In addition, these findings are in accord with a previous general hypothesis that dealt with the question of how the subjective conscious experience of each individual is related to his cerebral processes and what distinguishes this from unconscious processes. That hypothesis proposed that some substantial time period of appropriate cerebral activity lasting hundreds of ms may be required for eliciting many forms of specific conscious experiences (Libet 1965). The hypothesis developed out of experimental findings that cortical activities must persist for up to 500 ms or more before "neuronal adequacy" for a conscious sensory experience is achieved (Libet 1966; 1973; 1981a; 1982; Libet et al. 1979). This led to the further inference, supported by evidence, that those cerebral activities which did not persist sufficiently long would remain at unconscious levels. The present evidence suggests that a similar substantial period of cerebral activity may also be required to achieve "neuronal adequacy" for an experience of conscious intention or desire to perform a voluntary act. The experience of the conscious intention to act would, in these terms, arise as a secondary outcome of the prior unconscious initiating process; nevertheless, it could still have a role either in completing the initiating process ("conscious trigger") or in blocking its progression ("veto").

4. The conscious function in voluntary action

If the brain can initiate a voluntary act before the appearance of conscious intention, that is, if the initiation of the specific performance of the act is by unconscious processes, is there any role for the conscious function? It is of course possible to believe that active conscious intervention to affect or control a cerebral outcome does not exist and that the subjective experience of conscious control is an illusion (e.g., Harnad 1982). However, such a belief is not required even by a monist, determinist theory, as seen in Sperry's (1980) formulation of an emergent consciousness that can interact with and affect neuronal activity; and the theoretical physicist Margenau (1984) has claimed that conscious intervention in brain function can occur without any expenditure of energy or violation of the known physical laws. In any case, the potentialities for conscious control may be considered at a phenomenological level; that is, we can for the present discuss operational possibilities for conscious control at a level which does not require a commitment to any specific philosophical alternatives for mind-brain interaction, whether these be determinism versus free will or epiphenomenalism versus mental intervention.

I propose that conscious control can be exerted before the final motor outflow to select or control volitional outcome. The volitional process, initiated unconsciously, can either be consciously permitted to proceed to consummation in the motor act or be consciously "vetoed." In a veto, the later phase of cerebral motor processing would be blocked, so that actual activation of the motoneurons to the muscles would not occur. Such a role is feasible since conscious intention is reported to appear about 150 to 200 ms before the beginning of muscle activation (signaled by the EMG), even though it occurs several hundred ms later than the cerebral initiating processes. The late cerebral processes thought to lead more directly to descending discharge in the pyramidal cells may be reflected in the so-called final motor potential (MP) component near the end of the RP shortly before the muscle activation. An MP that is generated in the premotor/motor cortex contralateral to the activated hand begins about 50 ms (Deecke et al. 1976) or perhaps as little as 10 ms (Shibasaki et al. 1980) before the muscle EMG. There would remain a net period of about 100 to 200 ms in which conscious control could block the onset of the MP. A "premotion positivity" (PMP) may also develop about 90 ms (Deecke et al. 1976) or about 50 ms (Shibasaki et al. 1980) before the EMG. The significance of this component is. still unclear. But even if the PMP is

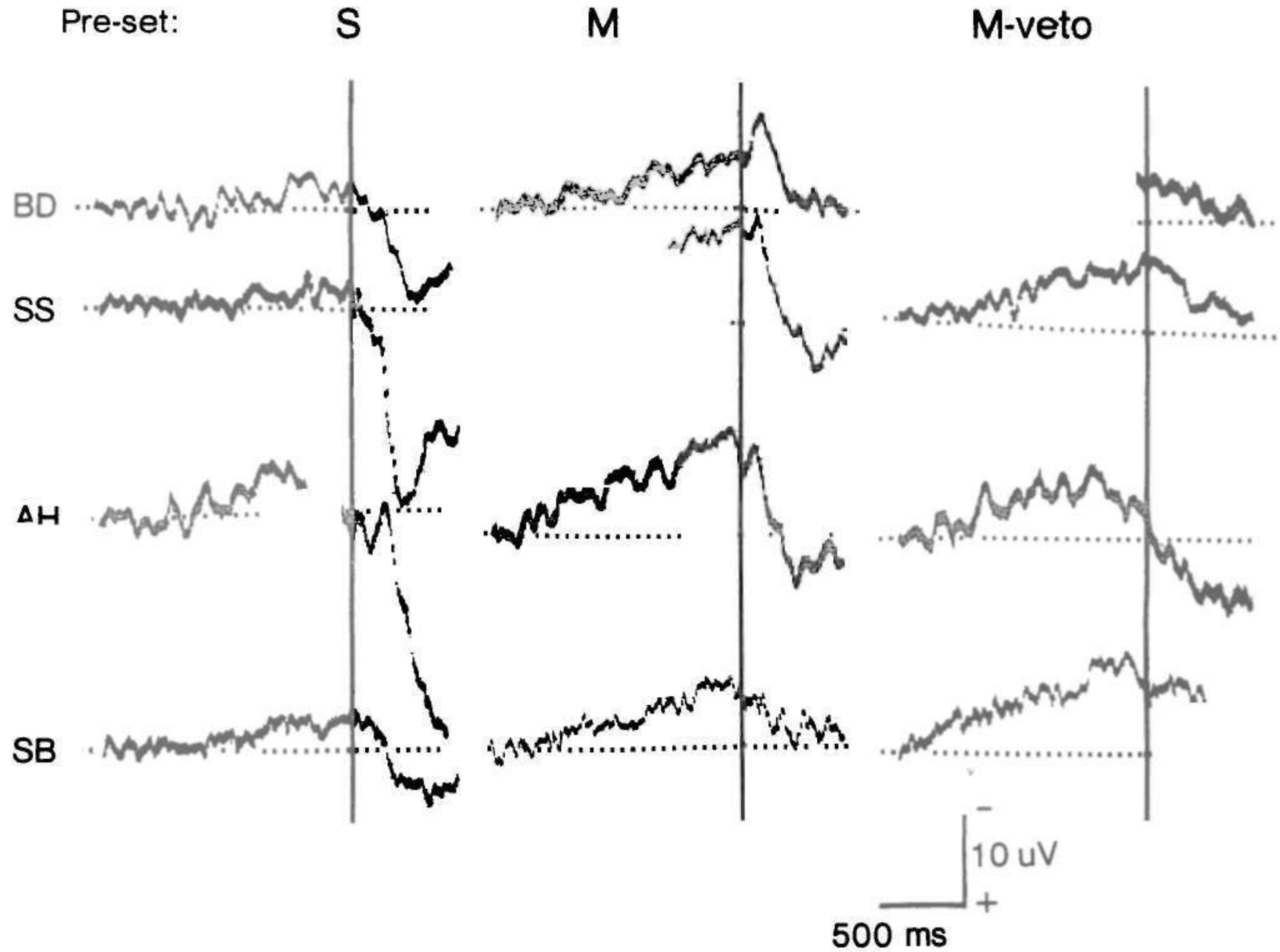

Figure 2. Pre-event vertex potentials when preparation to act is "vetoed." In column "M" (motor), the time for the subject to perform each of the 40 acts was preset (prearranged), so that preplanning was regularly expected of the subject. The recorded slow negative shift in potential preceding 0 (EMG) time resembles the type I RP found for those self-initiated acts for which endogenous preplanning was reported (Libet et al. 1982); it also resembles the RP of "self-paced" acts (e.g., Deecke et al. 1976). In the column "M-veto," subjects were instructed (a) to adopt the same mental sets as in the M series (preparing to move at the designated preset time) but (b) "to veto" this intention when the revolving CRO spot arrived within about "2.5 to 5 sec" of clock dial (actually about 100 to 200 ms) before the preset time. The absence of any observable motor activation was confirmed by monitoring the EMG at sufficiendy high gain. The computer trigger for preset 0 times in the absence of an EMG was supplied by an operator in another room. In spite of the absence of actual muscle activations, a ramplike prepotential like that in the M series was regularly exhibited, representing the developing intention and preparation to move; note, however, that these M-veto RPs tended (for 3 of the subjects shown) to terminate their negative rise within some 150 to 250 ms before 0 time, at about the presumed time for reversing the intention to act. (For the fourth subject, S.B., the preset M potential in 3 other experiments was larger and rose with a steady ramp form until at least 50 to 100 ms before 0 time, unlike the M recorded in the session shown here; see Libet et al. 1982.) In column "S," a skin stimulus delivered at similar preset times replaced the preparation to act. Pre-event potentials were absent or relatively insignificant in the S series, in spite of attention and anticipation for each event being similar to those in M and M-veto series, in that the subject had to watch for and report those events in which the stimulus was omitted at the preset time. (Modified from Libet, Wright & Gleason 1983.)

assumed to reflect cortical motor activation just preceding the efferent discharge (Deecke et al. 1976) there would still remain about 60 to 100 ms after the "corrected" time of conscious intention, or 110 to 150 ms after the uncorrected time, in which conscious control could affect the PMP process.

4.1. Evidence for "veto" control

The evidence for conscious veto is of two kinds: (a) Subjects in our study of RPs and conscious timings reported that during some of the trials a recallable conscious urge to act appeared but was "aborted" or somehow suppressed before any actual movement occurred; in such cases the subject simply waited for another urge to appear, which, when consummated, constituted the actual event whose RP was recorded (Libet et al. 1982). However, there is presently no technique available for recording and analyzing any RPs that may be associated with such spontaneous, irregularly appearing conscious urges to act that do not lead to an actual motor event, (b) In series of acts to be performed at prearranged times, subjects were instructed in advance to veto the developing intention/preparation to act and to do this about 100 to 200 ms before the prearranged clock time at which they were otherwise supposed to act. In these series a ramplike pre-event potential was still recorded during >1 sec before the preset time (Figure 2, "M-veto"), even though no actual muscle activation occurred (Libet, Wright & Gleason 1983). This resembles the RP of self-initiated acts when preplanning is present (Libet et al. 1982, type I RP). The form of the "veto" RP differed (in most but not all cases) from those "preset" RPs that were followed by actual movements; the main negative potential tended to alter in direction (flattening or reversing) at about 150-250 ms before the preset time (Libet, Wright & Gleason 1983). This difference suggests that the conscious veto interfered with the final development of RP processes leading to action. (Whether the above-mentioned MP or PMP components of RP are specifically eliminated by such a conscious veto remains to be analyzed.) In any case, the preparatory cerebral processes associated with an RP can and do develop even when intended motor action is vetoed at approximately the time that conscious intention would normally appear before a voluntary act.

The veto findings suggest that preparatory cerebral processes can be blocked consciously just prior to their consummation in actual motor outflow. As an alternative study, we might have randomly presented an external signal at which the subject would veto the prearranged or preset act. (External signaling to veto an act after a given *self-initiated* RP has begun is not technically feasible, since the individual RPs are not sufficiently discernible from the background EEG activity.) However, an externally signaled veto would not be an endogenous conscious process; as a quick reaction to a sensory signal it could even be generated unconsciously. It would of course be even more desirable to study the uninstructed veto of a spontaneous, self-initiated act, but, as mentioned, this is not presently possible technically because an objective trigger time for averaging RPs would not be available.

4.2. *Conscious "trigger" versus "veto"*

An alternative mode of conscious control might lie in a requirement that a conscious "trigger" finally impel the unconsciously initiated cerebral processes to achieve the actual motor act. Conscious control would then have an active role in completing or consummating the volitional process; the absence of a positive conscious trigger would mean no actual motor act occurs. If one grants the availability of the veto process, then an active trigger role becomes a redundant and unnecessary means of achieving conscious control. On the other hand, it is conceivable that both modes of control, active trigger and veto blockage, are available. Whether by active positive triggering or by vetoing the completion of the volitional process, the conscious function may be thought of as selecting from among the possible acts developed by the unconscious initiating processes.

Would the appearance of a conscious trigger or veto also require its own period of prior neuronal activity, as is postulated for the development of the conscious urge or intention to act and for a conscious sensory experience? Such a requirement would imply that conscious control of the volitional outcome, whether by veto or by an activating trigger, is itself initiated unconsciously. 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. Such a postulate can be in accord either with a monist view, in which a conscious control function could be an ongoing feature of an already emergent conscious awareness (Margenau 1984; Sperry 1980), or with a dualist interactionist view (Popper & Eccles 1977).

5. Free will and individual responsibility

This is not the place to debate the issue of free will versus determinism in connection with an apparently endogenous voluntary action that one experiences subjectively as freely willed and self-controllable (see Eccles 1980; Hook 1960; Nagel 1979; Popper & Eccles 1977). However, it is important to emphasize that the present experimental findings and analysis do not exclude the potential for "philosophically real" individual responsibility and free will. Although the volitional process may be initiated by unconscious cerebral activities, conscious control of the actual motor performance of voluntary acts definitely remains possible. The findings should therefore be taken not as being antagonistic to free will but rather as affecting the view of how free will might operate. Processes associated with individual responsibility and free will would "operate" not to initiate a voluntary act but to select and control volitional outcomes. (Voluntary action and responsibility operating behaviorally within a deterministic view would, of course, be subject to analogous restrictions.)

Some may view responsibility and free will as operative only when voluntary acts follow slower conscious deliberation of alternative choices of action. But, as already noted above, any volitional choice does not become a voluntary action until the person moves. In the present study, the subjects reported that the same conscious urge or decision to move that they experienced just before each voluntary act was present and that it was similar whether or not any additional experience of general preplanning had already been going on. Indeed, the reported times for awareness of wanting to move were essentially the same for fully spontaneous acts and those with some preplanning (Libet, Gleason, Wright & Pearl 1983). One might therefore speculate that the actual motor execution even of a deliberately preselected voluntary act may well involve processes similar to those for the spontaneously voluntary acts studied by us. The urge or intention actually to perform the voluntary act would then still be initiated unconsciously, regardless of the preceding kinds of deliberative processes.

The concept of conscious veto or blockade of the motor performance of specific intentions to act is in general accord with certain religious and humanistic views of ethical behavior and individual responsibility. "Self-control" of the acting out of one's intentions is commonly advocated; in the present terms this would operate by conscious selection or control of whether the unconsciously initiated final volitional process will be implemented in action. Many ethical strictures, such as most of the Ten Commandments, are injunctions not to act in certain ways. On the other hand, if the final intention to act arises unconsciously, the mere appearance of an intention could not consciously be prevented, even though its consummation in a motor act could be controlled consciously. It would not be surprising, therefore, if religious and philosophical systems were to create insurmountable moral and psychological difficulties whenthey castigate individuals for simply having a mental intention or impulse to do something unacceptable, even when this is not acted out (e.g., Kaufinann 1961).

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Problems with the psychophysics of intention

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Several methodological and conceptual problems come to mind after a reading of Libet's article. For one, the timing of all consciously apprehended events under investigation was measured relative to the "clock position" of a dot revolving in a circle. Similar timing methods plagued by several problems have been used for over 100 years. Using a revolving dial, Wundt (1904) noted that the perceived time of a sensory event relative to the simultaneously visually perceived position of the rotating dial depended crucially on the angular rate of the dial's rotation and the other sense being stimulated. Libet's work is based on a single angular dot velocity; hence, despite acceptance of his particular implementation of the procedure by refereed journals, there is a very strong possibility that his measures are idiosyncratic.

Moreover, the timing of S, the awareness of a tactile stimulus, does not serve as a clear control that allows one to regard any timing "error" here as an indication of the potential error found in timing W, the awareness of the intent to act. First, judgments of intermodal sensory simultaneity depend on the particular senses investigated and the stimuli used/ Besides the prior entry effect noted by Libet, intrinsic latency and processing rate differences among senses as well as latency differences introduced extrinsically by use of a near-threshold tactile stimulus relative to a clearly suprathreshold visual dot stimulus (Libet, Gleason, Wright & Pearl 1983) render use of any one estimate of timing error arbitrary and suspect. Second, attending to W may not be equivalent to attending to S, as Libet assumes. Indeed, one can voluntarily allocate attention to endogenously produced cognitive/mental processes as well as to mental processes produced exogenously by sensory stimuli. However, in the latter case a compulsory, stimulus-evoked allocation of attention is typically also engaged, as illustrated by Remington's (1980) and Jonides's 1981) studies of attention to brief suprathreshold visual stimuli. Insofar as Libet's near-threshold tactile stimuli were above threshold, their presentation would also evoke such an obligatory or nonvoluntary attention.

Even if one were to pass over these pertinent methodological problems, several concerns of a more conceptual nature need addressing. First, in what sense can the voluntary acts as operationally defined by Libet be paradigmatic of volitional action generally, particularly when he draws certain weighty religio-ethical implications from his findings? As Libet admits, his experimentally reduced acts of finger/wrist flexion occur in the absence of any larger meaning. Hence they are as limited in application to our understanding of volitional action as use of nonsense syllables is to our understanding of memory. By what rules do we proceed from these experimental findings to human volitional action (or memory) occurring inextricably within a rich, varied, and meaningful context? William James (1950) held that a *strictly* voluntary act must be guided throughout its whole course not only by volition but also by idea and perception. Moreover, he observed that consciousness, besides being primarily a selective, intentional process, is more or less intense depending on action's being more or less significant and hesitant (nonhabitual), that is, where indecision is present to a greater or lesser degree. Consequendy, one might at least require that subjects choose freely among several actions, each of which carries some practical consequence (cost and benefit) rather than merely choosing to act or not in some stereotyped and inconsequential way.

To counter the requirement that a stricdy voluntary act be characterized by slow conscious deliberation and existential alternatives of action, Libet notes that no volitional choice becomes voluntary action until the person moves. The implication is that Libet's paradigmatic acts tap this final, effective conscious intent, which invariably appears approximately 350 ms after an RP is generated but 200 ms before one actually moves. It should be noted 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 and in a way that is not comparable with more ecologically and existentially valid voluntary and intentional acts.

To illustrate, up to this point I was not consciously aware of intending to write down these thoughts. Yet a prior intention to write a critique occurred days ago. In fact, however, I could have chosen to intentionally write out my critique word by word, that is, with clear awareness of each intent to write each work just prior to writing it. Yet this or Libet's "hyperintention" brought about by self- or by experimental instruction in no way represents my voluntary actions in general. At best the hyper-awareness of intention functions as a monitor retrospecting on my much earlier plan, decision, or intention to write rather than as an instigator, motivator, or modulator of writing activity. In this view, the awareness of intent, though it falls just after the onset of RP and just before the onset of movement, poses neither a scientific nor a philosophical problem and has little if any bearing on issues of free will and responsibility.

Finally, even if one admits the legitimacy of Libet's procedure and interpretation, Libet hedges on and skirts around an important issue. Libet would have it that one can discuss the operational possibilities of conscious control of action on purely phenomenological grounds without commitment to specific philosophical alternatives such as determinism versus free will or epiphenomenalism versus mental intervention. Such a phenomenological bracketing is well-nigh impossible since it asks one to suspend any thesis of reality including the metaphysical assumptions hidden behind the very scientific enterprise being undertaken by Libet. In the context of his work, how can one talk of possibilities of conscious control, and not turn this talk into idle chatter, without taking a stand in particular on epiphenomenalism versus mental (conscious) intervention? On the one hand, if the conscious permissive "trigger" or restrictive "veto" is preceded by causally efficacious yet unconscious neural activity just as in the case of the consciously experienced intent to move (Harnad 1982), then that consciousness is mere afterthought, a reflection on events outside its causal control and, therefore, epiphenomenal. On the other hand, consciousness is a fact to each of us. Insofar as its existence is undeniable, it is a troublesome and abiding enigma, particularly to any accepted version of natural evolution. For to have evolved it must be as causally efficacious as is the hand that writes these words. Hence consciousness, including any conscious "trigger" or "veto," calls for some form of mental intervention. As scientists, we cannot stand on the sidelines and suspend or bracket the thesis of natural evolution. To do so would further mystify consciousness to a degree warranting silence.

Free will and the functions of consciousness

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Libet attempts nothing less than a beginning of the physiology of free will, an area where philosophical work previously has enjoyed a total lack of empirical restraint. The philosophical issues won't go away yet, however, and they remain important to interpreting the experiments. Two problems deserve special comment: the demand characteristics of the experiment and the generalization from millisecond-level operations to long-term behavioral planning.

A careful analysis of the experimental conditions reveals that the subjects' wills were not as free as the Libet article implies, for the small, sharp movements that they were instructed to make were not freely willed but were requested by the experimenter. The will of a subject was no more free in this design than in reaction-time experiments; the only difference between this experiment and the latter paradigms is that the instruction and the movement are decoupled in time. While performing the task, the subjects do nothing more than obey the instructions. The acts are a step removed from the instructions, and the issue of the source of timing for the irregularly repeated acts is an important one, but the behaviors should not be confused with instances of free will. It is even possible that free will, like the mind-body problem, will disappear as our understanding of the physiology of experience increases.

In a sense the subjects in the Libet experiments are asked to behave as though they had free will, whether such a thing really exists or not. Under these circumstances it is not clear whether we are seeing some fundamental property of the human nervous system or merely the program that the subject has set into play. To give another example of this process, consider a subject in a psychophysical experiment who is asked to draw boxes on pieces of paper. The psychologist could study the box-drawing machine as though it were designed only for this task, and the dynamics of the behavior, its physiological concomitants, and so on could be studied in detail. Box-drawing centers could be found in the brain, box-detecting circuits could be described in the visual system, and the prebox potentials could be analyzed. The artificiality of the task, though, would not be apparent no matter how detailed the analysis; in fact, the more detailed the analysis the less likely it is that the results will be interpreted as specialized operations of a more general-purpose machine. The subject has programmed himself to behave as if he were a box drawer and nothing else. Similarly, Libet's RPs may have characteristics unique to the rather specialized and unusual tasks required of his subjects. This is not to say that Libet's paradigms are invalid but only that they should be interpreted with caution.

The temptation to overgeneralize a specific task with its unique demand characteristics may also be related to the generalization of the veto principle at the end of Libet's article. The Bible's injunction not to commit adultery, we may expect, will be handled very differently from Libet's injunction not to move the fingers on a given trial. The confusion of levels is an error that I have called "Uttalism"after Uttal's (1971) injunction that properties of single-cell receptive fields cannot automatically be applied to behaviors of the whole organism. This problem has arisen in visual masking, where neurophysiologically based models, whether computer simulations (Bridgeman 1971; 1978; Weisstein 1972) or qualitative theories (Breitmeyer & Ganz 1976), rely on mechanisms too limited to reflect the subleties of real human behavior. No amount of tinkering with these theories will deal with practice and attention effects, for example, nor will they explain strong effects of rather small differences in stimulus patterns on masking. Similarly, the Libet data, important as they are, should not be confused with physiological studies of self-control in human behavior.

The finding that consciousness enters after the beginning of an identifiable set of neurological events can be viewed in the context of consciousness as a neurological system like any other, with specific jobs that help the organism to function effectively. Its jobs include handling situations that are difficult, dangerous, or novel (Norman & Shallice 1980), and it serves among other things to establish action schemata, order their priorities, and monitor their progress. Thus consciousness must be involved when a behavior is about to be executed, if that behavior might interfere with other ongoing schemata. In Libet's special case the only ongoing task is to sit still. Here, that stage of organizing a behavior that first requires access to consciousness can occur only a few hundred milliseconds before the behavior begins. We do not yet know what happens in the more general case, when other action programs are being executed at the same time.

Consciousness and motor control

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It is a truth universally acknowledged that a physiologist in possession of a metaphysical prejudice must be in want of philosophical help. It is inconceivable save with reference to some such prejudice that Libet would find it necessary at the end of his paper, to postulate functions whose existence would be incompatible with everything he had up to that point been at pains to show. These are "conscious control functions," which "can appear without prior initiation by unconscious cerebral processes." But everything up to then would have disposed us to believe that motor acts are the consequence of exactly such initiating processes, revealed to the consciousness of the agent about 350 ms after onset, with the motor act itself taking place about 150 ms thereafter, barring endogenous intervention. But then, in that last fateful interval, abrupdy and without experimental motivation, between the intention and the act falls the shadow of alien ideas. These are the "conscious control functions" that "trigger" or "veto" the act and that spring, cerebrally unsummoned, into being. Freud famously said the hysterical symptom seems to have no knowledge of anatomy. When a physiologist relaxes his laboratory scruples in favor of what must be physiologically mysterious, he is to be diagnosed as in the grip of a kind of metaphysical hysteria.

Surely conscious control functions have some physiological substance if they have physiological effects. And surely it should be an empirical matter whether or not their occurrence be cerebrally initiated through that kind of neuronal activity which precedes the occurrence of subjectively experienced intentions or "wantings to act." So why should it seem necessary to postulate them as thus unpreceded unless one believes precedent unconscious activity must queer some theory held dear by the writer - perhaps a position on the free will question? If Libet is right that "the present experimental findings do not exclude the potential for 'philosophically real' individual responsibility and'free will," why should he act as though they did exclude that by postulating what he feels must be in place in order that responsibility and freedom have application? Philosophy must learn to live with scientific truth.

It seems to me that the existence of free will does not have as close a connection with "conscious deliberation of alternative choices of action" as Libet supposes. Choosings between alternative courses of action, in the preponderance of motor acts we perform, occur as the outcome of deliberations of which we are barely conscious, if at all. A slow-motion film of Matisse shows the artist making countless decisions with his fingers that at normal speed looks like a single confident chalk stroke defining the edge of a leaf. He may or may not have been conscious of each decision, but I suspect that he was conscious only of drawing a leaf. Consciousness, in moral theory, plays its role only in connection with premeditation, for which there is neither time nor occasion in the sort of spontaneous choosings we do in life and in the sort of laboratory Libet's work presumes. Happily, we are so wired that deliberation may occur without the mediation of consciousness at all. Consciousness is evolution's gift to us for rather special deliberative employment having to do, as responsibility and free will have to do, with *courses* of action - with *projects* - rather than the basic sorts of acts involving the simple flexion of a muscle or the moving of a hand to no further purpose.

Suppose one were to designate as intentions the entire cerebral processes that eventuate in motor acts, rather than restricting die intention to that fragment of the cerebral process which becomes conscious? The concept of intention was framed well before there was knowledge of cerebral process, but once it is accepted that much of deliberative action transpires without becoming conscious to the agent - because its being conscious would reduce our efficiency as agents - the concept might easily be extended to cover more than would have been necessary in periods when the mental and the conscious were closely identified. We might indeed think, in those cases in which some segment of the intention becomes conscious, of the preceding segment as *preconscious* intention. Then, in the standard case, this is what happens: The intentional is formed; some milliseconds later the agent becomes conscious of his intention; some milliseconds later the motor act occurs as intended. Why do we need an extra "trigger" since there is no empirical basis for its existence but only a "necessary postulation"? It would be like requiring a trigger in mechanics in order to explain the fact that a body, moving in a straight line with uniform velocity, continues to move in a straight fine with uniform velocity, when in fact all we need is an explanation of acceleration, or change in direction and velocity. Why should not the intention be enough to trigger the movement? I surmise that Libet thinks that simply allowing to take place what is already in process is too passive a role for conscious intention if freedom is to be robust enough for our moral vision of ourselves. In my view, all we need to explain is *changes* in intention. But these can be well under way before we are conscious of the change, with the entire cerebral process, including the fragment of it that is conscious, as the veto of the previous intention. There is plenty of time to abort the action if the intention arises before consciousness of veto.

In brief, instead of the conscious control functions playing the special on-off role of metaphysical switches, we have the play of cerebral processes, in which consciousness informs us of what we have decided to do. Whether these decisions themselves are free belongs to a different topic, but my claim is that freedom and consciousness have less to do with each other, and certainly so in the execution of simple behaviors, than Libet supposes. Once he realizes that it is only because he believes that they have much more to do with each other than the data he presents justifies, he may drop from the inventory these curious operations that owe their existence in his article to an insufficiendy self-conscious agenda.

Knowing what we are embarked upon need not be a causally inert fact about ourselves when in fact we are embarked upon projects with horizons wider than the circumscribed boundaries of the laboratory. In these straitened confines, the projects to which responsibility and freedom have application scarcely can flourish. Commonly we do not simply move our hands; we do so with larger purposes in mind-to wave away a canape, to signal the death of a gladiator, to stifle by gesture the cackle of subordinates, to set up perturbations for the distraction of a wasp, or to express some agitation or other through the language of the body. Our minds bent upon these, consciousness simply assures us we are in contact with ourselves.

The time course of conscious processing: Vetoes by the uninformed?

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Perhaps the most important feature of this latest in the series of ingenious experiments by Libet and his colleagues is the demonstration it provides that the neurophysiological basis of conscious awareness can be subjected to meaningful analysis. This has profound philosophical import, the more so since it adds further evidence for the probable uniqueness of the neural processes accessible to or directly producing conscious experience.

It has long been apparent that many, indeed probably most, neural transactions are utterly devoid of or incapable of an element of consciousness-for example, autonomic regulation, hormonal release, adaptations in visuomotor control, cerebellar activity, and all neuronal discharge during most of a night's sleep (see Doty 1975). A particularly dramatic example is the loss of visual sensation despite demonstrably continuing retinal input when one is viewing a Ganzfeld (Boianowski & Doty 1982) or absolutely fixated image (Rozhkova, Nickolayev & Shchadrin 1982); the same is probably true for the disappearance of stimuli rotating about a fixed locus in the peripheral visual field (Hunzelmann & Spillmann 1984). On the other hand, in these instances the absence of a direct conscious concomitant to the neuronal activity in the forebrain clearly does not mean that such activity is inaccessible to consciousness. Rather, these phenomena of visual loss are probably an extreme example of the workings of that still mysterious tool of consciousness, selective attention. Thus, it is apparent that there are neural processes that he forever outside the domain of conscious experience and that there are others for which a conscious concomitant is elective.

In still other instances it seems that information garnered from sensorial processes lacking an experiential component can nevertheless be incorporated into the guidance of movements consciously controlled. These issues have previously been well discussed in these pages in relation to the phenomena of afferent discharge from muscle spindles (Roland 1978) or blindsight (Campion, Latto & Smith 1983). However, the fact that unconscious neuronal activity is constantly in play during movement seems well recognized, as in the common inability to perform properly a habitual, rapid movement while endeavoring to exert conscious control over all its components. (Try intellectually constructing and planning the motions of your fingers in tying your shoes!)

Now, perhaps Libet's experiments are detecting this, the unconscious components of an organized movement. There is a voluntary initiation of these components, just as there can apparendy be a voluntary cancellation (veto) of them. The actual decision to release the movement occurs only against the background of readiness, the point at which the subconscious set of the neuronal program, possibly being arranged in striated-cerebellar circuitry, is acceptably complete. The unconscious part, just as in tying one's shoe, proceeds pari passu with, and apparendy slighdy ahead of, the overt and consciously released movement; but this does not mean that the unconscious components proceed or arise independendy of conscious control. After all, the neurons for each are all embedded and intertwined within the same brain; and one does not know yet whether the neuronal transactions resulting in conscious perception are a manifestation of a special type of neuron or a special form of activity within groups of neurons of diverse form and chemistry.

It seems to me that this is a much more satisfactory explanation of Libet's fascinating observations, that an aura of unconscious preparation for movement perpetually surrounds the ever-moving focus of consciousness, and that the apdy named "readiness potential" (Kornhuber & Deecke 1965), which Libet records prior to the "decision" to actually perform the movement, is a manifestation of this process. The alternative, which he seems to favor, is that "the brain" proceeds independendy of conscious control to prepare movements, which can then be either consciously allowed or consciously "vetoed." The great flaw in this interpretation is that, if the preparatory movement is wholly outside conscious control, how could a conscious process then "know" what will ensue if it fails to veto the brain's proposal? In this scheme, consciousness is relegated to an intuitive process of guessing what it may be that "the brain" is up to and being ever on the alert that the demons of the unconscious do not set in motion some act inappropriate to the conscious plan. While such views of brain processes may be permissible in the poetic fantasy of Freudian psychology, they are not neurophysiologically convincing.

Mental summation: The timing of voluntary intentions by cortical activity

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My commentary starts with an acceptance of the extraordinary findings reported by Libet. With great ingenuity he has been able to train subjects to report retrospectively the timing of their voluntary intention to make a simple sharp movement. I am not concerned with the subde distinctions he makes between types of conscious endogenously willed motor actions, for example, whether or not the subject was cognizant of planning in advance. For me the decisive discovery is that the subjectively experienced onset of intention to move is about 200 ms before the muscle activation and about 350 ms *after* the onset of the readiness potential (RP), which provides some integrated signal of the cortical activity preceding the movement.

To simplify my hypothesis, I will assume that the voluntary intention to move acts on the supplementary motor area (SMA) [see Goldberg; "Supplementary Motor Area Structure and Function," this issue] and thence through the various pathways to the motor cortex and so by the pyramidal tract to bring about the movement (cf. Eccles 1982b). It is very tempting to follow Libet in interpreting these findings as establishing that cortical activity (of the SMA, for example) initiates not only the voluntary movement but also, after some hundreds of milliseconds, the introspective experience of having initiated the movement, which thus becomes an illusory experience. I shall consider later Libet's veto hypothesis, by which he attempts to preserve the responsibility of the conscious self by means of its power to veto the ongoing cortical activities that would otherwise lead to the movement.

I now present a hypothesis that accepts all of Libet's experimental observations but that nevertheless preserves fully the role of conscious intention in initiating the movement. The hypothesis has several components.

(1) It is proposed that there is a fluctuating background of activity in the cerebral cortex and in the SMA that can in part be generated by the reticular activating system and that was proposed by Oshima (1983), possibly to involve a "set" for movements.

(2) As discovered by Libet, the mental intentions reported by subjects begin about 200 ms before the movement. The hypothesis is that *these intentions tend to be timed unconsciously* by the subjects so as to take advantage of the spontaneous fluctuations in the cortical activity ((1) above). Since the RP as observed is formed by the averaging of a large number (fifty to hundreds) of recordings of scalp potentials with zero time given by the onset of the electromyogram, it is a mistake to assume tacitly that the averaging eliminates the random fluctuations. If there is a tendency for the initiation of the movements to occur during the excitatory phases of the random spontaneous activity, the earlier phase of the RP may be no more than the averaging of the premonitory spontaneous activity. If that is so, the RP does not signify that cortical activity initiates the movement. Instead, the hypothesis is that the spontaneous fluctuations of cortical activity merely adjust the phase of the conscious initiation to the intention some 200 ms before the movement.

(3) It is further postulated that this timing of the intention in relation to the phases of cortical activity is a learned phenomenon having the advantage that it secures opportunistically the most effective occasions for initiating voluntary actions. The lower right comer of Figure 1 illustrates the hypothesis. It is to be noted that the activities of the SMA are reciprocally related to the mental intentions, the arrows being directed both ways across the frontier between mind and brain.

(4) In the further development of the hypothesis we have to consider how the mental event of an intention can cause changes in the neuronal responses of the SMA. Let us first focus attention on a single synaptic bouton, which may be, for example, on a pyramidal cell of SMA. As shown for very diverse central synapses by Jack, Redman, and Wong (1981) and by Korn and Faver (1985), a presynaptic impulse evokes the liberation from the bouton of a single synaptic vesicle probabilistically, the probability factor being usually less than 1 in 2. This probability can be increased or decreased with consequent changes in synaptic effectiveness. As described by Akert, Peper, and Sandri (1975), each bouton has a single paracrystalline structure, the presynaptic vesicular grid that holds about 50 synaptic

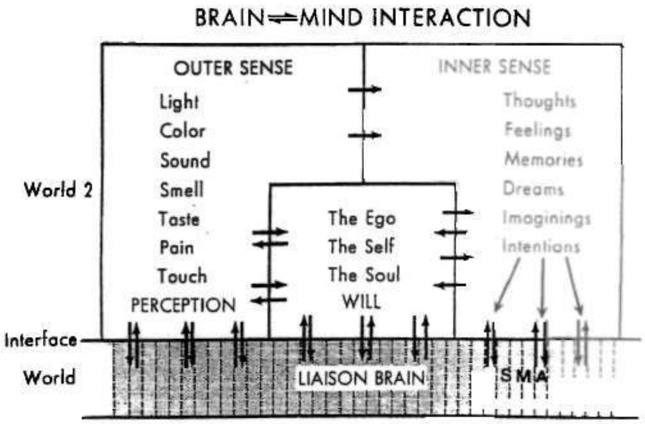

Figure 1. (Eccles). Information flow diagram for brain-mind interaction in human brain. The three components of World 2 - outer sense, inner sense, and psyche or self- are diagrammed with their communication shown by arrows. Also shown are the lines of communication across the interface between World 1 and World 2 - that is, from the liaison brain to and from these World 2 components. The liaison brain is the columnar arrangement indicated by the vertical broken lines.

vesicles, and somehow it controls the probability of their emission. The hypothesis is that the immaterial mental event of intention acts analogously to a probability field of quantum mechanics, as proposed by Margenau (1984), and modifies the probability of emission of a synaptic vesicle by a presynaptic impulse. Thus an intention is effective only insofar as there is an adequate quota of presynaptic impulses; hence the necessity for the learned timing of intentions in relation to the fluctuating waves of SMA background activity.

(5) Any effect of a mental intention in altering probabilities of quantal emission from a bouton is orders of magnitude too small to cause the sequence of neuronal actions leading to an effective discharge of motor pyramidal cells. It is conjectured that there has to be an immense collusive action of the mental intention on the multitude of boutons on one neuron and on a large assemblage of similarly acting neurons. This is in accord with the findings of Brinkman and Porter (1979) that, when a monkey is carrying out a voluntary act, there is excitation of many similarly acting neurons in the supplementary motor area 100 to 200 ms before the onset of the electromyogram.

(6) Furthermore, according to the hypothesis there is also a reverse flow of information (Figure 1), the SMA activity being subconsciously "sensed" when a mental intention is being initiated. This is the most obscure component of the hypothesis. Yet it is generally recognized that in the perceptual areas of the cortex much activity can occur subconsciously, as in the refined experiments of Libet (1973) on somatosensory perception, where weak repetitive stimulation of the somatosensory cortex may have to continue for 0.5 sec before the cortical activity reaches the threshold for conscious perception.

The veto experiments of Libet are very ingenious and offer further evidence of mental control of cortical activity with the late flattening of the RP.

In conclusion, the hypothesis here presented offers a general explanation of the findings of Libet while preserving the essential character of dualist interactionisms. The early phase of the RP may be no more than an artifact arising from the technique of averaging. There is no scientific basis for the belief that the introspective experience of initiating a voluntary action is illusory.

NOTE

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Brain mechanisms of conscious experience and voluntary action

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For many years Libet has been carrying out carefully controlled crucial electrophysiological experiments on the relation between electrical stimulation and responses in sensory cortex and pathways in the conscious human brain and verbal reports of conscious awareness with the surprising result that it seems to require considerable time (about 500 ms) for activity in sensory systems to reach the threshold of conscious awareness. The precise neuronal mechanisms involved in this delay have not been specified. It has long been known from experiments carried out under light barbiturate anaesthesia or natural sleep that evoked potentials and unitary responses from single cells in sensory cortex (somatic, visual, or auditory) are preserved, even including the complex information processing involved in feature detection in visual cortex as studied by Hubel and Wiesel, in states that probably preclude conscious awareness (light barbiturate anaesthesia).

Libet now uses the "readiness potential" (RP) to time unspecified cortical events that precede an ad libitum voluntary motor act as compared to the timing of the subject's conscious awareness of intention to move, with the surprising conclusion that willed voluntary movements arise out of brain mechanisms that precede conscious awareness of the intention to move and must therefore be subconscious. Controls on the reliability of subjective reports of the timing of conscious awareness of intention to move depend on the accuracy of memory, introducing another important factor that in my opinion has not given adequate consideration. Is it not possible that brain mechanisms underlying awareness may occur without those which make possible the recall of this awareness in memory afterward? Patients with epileptic automatisms, for example, may carry out many apparently intentional complex motor acts, often remarkably appropriate ones (such as driving in traffic), without being able to recall having done so afterward. A similar state of apparently "automatic" behavior may occur with certain drugs such as scopolamine. I realize that it may be impossible to dissociate mechanisms of awareness from those of memory recall under the conditions of these experiments, but there is a problem here that should be given serious consideration.

Concerning the more philosophical implications of these studies, Libet should be commended for his ingenious and precise experiments, which have clarified, if not solved, the age-old problem of mind-brain relationships. I agree that mental events can be considered scientific data even though they are difficult to measure, and that they may well play a most important role in the direction of behavior and consequently of the brain mechanisms underlying this behavior, while at the same time mental events must depend upon highly integrative brain functions (i.e., interactionism rather than dualism). It may well be that there are specialized neuronal systems extending throughout cortical and subcortical structures but separate from specific afferent and efferent pathways to cerebral cortex, which mediate mechanisms of conscious awareness, analogous to the outworn hypothesis of the reticular system or the "centrencephalic system" of Penfield.

Libet has provided us with important temporal constraints on two aspects of this problem: the temporal summation required for conscious awareness and the delay in awareness of conscious intention of voluntary movement. I would suggest that he now direct more of his attention to brain circuits separate from the primary sensory or motor pathways in the search for mechanisms more closely related to mechanisms of consciousness, as originally suggested by Hughlings Jackson in his search for brain mechanisms of "highest level seizures."

Voluntary intention and conscious selection in complex learned action

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Libet's experiments are limited to the recording of readiness potentials (Kornhuber & Deecke 1965), which precede the decision to make or veto brief finger flexions. These simple movements are made voluntary, but the will acts here only as a trigger. Willed intention is more important in goal-directed and complex movements such as writing. These also contain many unconscious mechanisms and become partly automatized by learning. Slow brain potentials recorded during action may give additional information complementing the analysis of readiness potentials that appear before movement.

I agree with Libet that the conscious will mainly selects and controls our action and that unconscious preparatory cerebral mechanisms are important. I doubt Libet's assertion, however, that the subject's will does not consciously initiate specific voluntary acts. It is true in complex and learned movement too that several more or less unconscious motivations contribute to the action. In man, however, even emotional or instinctive actions and skilled movements can be voluntarily initiated, directed, and set for their duration, as they can be inhibited and blocked by will. Willed intention is normally related to consciousness.

Cerebral correlates of intention. The interaction of instinctive, willed, and learned factors in human decisions to act can be demonstrated by skilled movements and mental activity such as language and calculation tasks. Cerebral correlates of these conscious acts have been recorded in man (Jung 1984).

The electrophysiological correlates of goal-directed and writing movements are large surface negative potentials that appear as an increase of the readiness potentials at the precentral and parietal cortex (Grunewald-Zuberbier et al. 1978; Jung et al. 1982). The aiming potentials terminate in a positive shift when the goal is reached (Figure 1A). The preparatory body posture and balance accompanying the consciously steered goal-directed movement become unconscious after the primarily will-controlled movement is trained (Jung 1981; 1982). The aiming potentials are probably related to the willed performance of goal-directed movements and to their programming. Normally, our consciousness is concerned only with the goal and not with the automatic and learned mechanisms of action involved in its pursuit. Owing to the limits of conscious information content, conscious intention is only a small part of the whole action program.

Limited capacity of consciousness. In conscious perception and voluntary action the information flow of the human nervous system is extremely reduced from the input of 10^7 to about 20-50 bits/sec (Kiipfmfiller 1971). This narrow range of consciousness necessitates selective processing and automatized programs for all voluntary skilled movements (Jung 1981). Such unconscious motor programs are acquired by learning.

Let me explain the selective and restricted role of the conscious contribution to complex action by the experience of goal-directed movements and other tasks. As a subject in the experiment shown in Figure 1A, I was consciously aware of my aiming intention during the action and of two other intentions that were in the background and less salient. The first intention, to direct the object to the goal, began with the readiness potential of 1 sec duration and continued for 3 sec. The second, to fixate the target and not to look to my hand, was less conscious, and the third, to suppress blinking by staring, was sometimes interrupted by involuntary blinks. Of course, special activation of arm muscles, needed during the task, was not conscious. Hence, the voluntary conscious intention to reach the target was combined with a negative veto to avoid eye movements and associated with automatized hand movements.

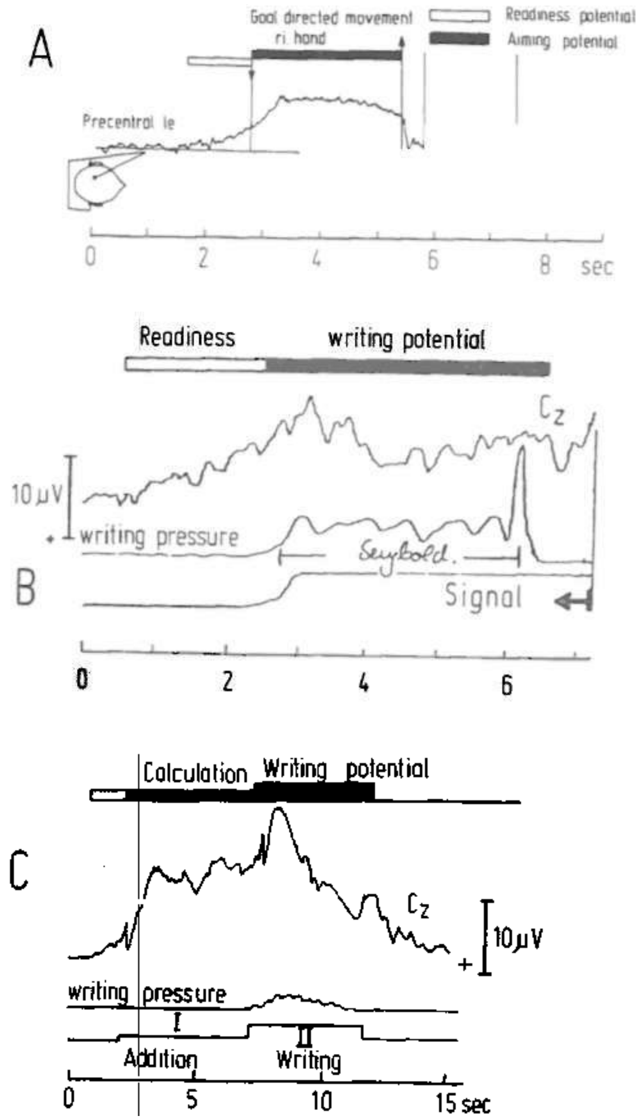

Figure 1. Jung). Readiness, aiming, writing, and calculation potentials in normal right-handers. Reference: A, mastoids; B, C, earlobes. *A.* Aiming potential *(Zielbewegungspotential)* in left precentral region (C_3) during goal-directed movements of right hand (between arrows). Backward averaging of 34 trials. The readiness potential begins 1 sec before movement starts, approximately with the conscious decision to reach the target. Adapted from Grunewald-Zuberbier et al. (1978) and Jung (1981). B. Writing potential at vertex (C_z) while subject writes his name. The readiness potential begins 2 sec before movement starts, together with the intention to write. Hand movement is recorded as writing pressure. Backward averaging of 32 trials. Adapted from Jung et al. (1982). *C.* Calculation potential during addition of 2 two-digit numbers followed by writing potential. Both are triggered by acoustic signals. During the initial period of expectation and ocular fixation a small readiness potential arises. A large negative calculation potential follows in period I after hearing the task numbers. Writing down the results cause:; the second largest peak in period H. The triggering signals elicit evoked potentials that precede the slower potentials. A conscious intention to calculate and write follows the perception of these signals. Forward averaging of 32 trials. Experiment no. MSV 135/1.

Writing one's name, as shown in Figure IB, was preceded by a conscious decision to start each writing sequence. However, the subject was not aware of the detailed performance of finger movements since repeated name writing had become automatized. In 1C the subject consciously intended the mental calculations of the perceived numbers in period I and the writing down of the result in period II. The special mode used in problem solving, however, often *failed* to be consciously experienced. Such acts had been automatized by years of learning writing and calculation.

Variations of conscious timing. For the writing act shown in Figure IB, Libet's final time of decision, said to arise about 200 ms before action, would imply an extremely long "unconscious initiative" of several seconds. The readiness potentials before repeated word writing may last 2-3 sec; that is, they can be six times longer than the readiness potentials recorded before simple movements (Schreiber et al. 1983). We interpret this as a sign of more complex and thus more time-consuming preprogramming. It appears improbable that such cerebral potentials of 3 sec duration are initiated "unconsciously" without willed intention. Our subjects tell us that they experience a first impulse to act well before writing begins. This preparatory intention, however, is rather vague, and we are not conscious of the learned complex cerebral programs of writing after they become automatized during 8 to 10 years of learning in school.

During training of skilled movements, which before learning had been guided by conscious control, a progressive reduction of conscious intention occurs, thus leading to automatization. The willed intention to start such trained motion programs becomes restricted to voluntary triggering and timing.

In summary, I do not deny unconscious elements in voluntary movements. Rather, I stress the importance of preconscious motivation, learned and automatized mechanisms, that is, of unconscious programs that contribute to voluntary action. I doubt only that Libet's experiments can prove the unconscious initiation of all self-paced voluntary acts. His results may be explained by the small information capacity of conscious introspection and of recall during the combined observation of the clock and the intention to move.

Consciousness as an experimental variable: Problems of definition, practice, and interpretation

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The traditional role of the electrophysiologist has been to relate electrical events in the brain to sensory inputs and motor outputs. More recently there have been attempts to relate electrophysiological activity to the cognitive processes, such as expectancy (Grey Walter, Cooper, Aldridge McCallum & Winter 1964) and selective attention (Hillyard, Simpson, Woods, Van Voorhis & Munte 1984), that he between these inputs and outputs. It is normally assumed that aspects of these processes may be part of our conscious experience and that we may sometimes draw on evidence from conscious experiences to suggest how they operate, but as theoretical constructs they are essentially neutral in their relation to consciousness. Expectancy and selective attention can be identified and discussed without having to specify what parts of the processes are conscious and what parts are unconscious. Libet is now extending the paradigm still further by attempting to relate electrophysiological activity directly to conscious experiences. He is well aware of the enormous problems raised by doing this and he addresses most of them impressively and effectively. But it is the function of *BBS* Commentaries to raise questions and dwell on perceived weaknesses. This can be done under three headings.

1. Problems of definition. The three conditions laid down in section 1 of the target article as necessary for establishing voluntariness of action seem reasonable. But condition (c), that the subject *feels* he is acting voluntarily, is not easy to handle operationally and is therefore of doubtful usefulness. How do we know the subject's self-reports are accurate? Feelings of voluntariness are labile and difficult to tie down, as the attempts to investigate the voluntariness of actions performed under posthypnotic suggestion (Wagstaff 1981) or of subjects' obedience to authority (Milgram 1974) make clear. The problem here is the compliance, which may be operating on the feelings of voluntariness or on the report of that feeling, or both. Supposing Penfield (1958) had stimulated some point higher up in the chain of motor control than the motor cortex and found not only that consistent and repeatable movements were produced but that the subjects also reported that they felt that they performed the act on their own initiative and that they were free to start or stop the act as they wished. Now suppose the patients were shown exactly what was being done to them and then asked again whether their movements were voluntary. If the explanation had been made properly, they would be bound to say they were not acting voluntarily. Which self-report would be the correct one? Maybe it does not matter too much to Libet whether or not the report is accurate in the context of voluntariness, but this difficulty of reliability applies to all reports of conscious experience, including the report of conscious intentions, which is the central variable in Libet's experiments. It must matter if thsse are not reliable.

2. *Practical problems.* There are two difficulties here. The first concerns the reliability of self-reports of awareness and is really a more operational restatement of the problem raised at the end of the last section. The subjects are being asked to report when they are first aware of the intention to act. Assuming there is a gradual development of awareness, this is equivalent to making a threshold judgment using an ascending method of limits (for a more detailed account of threshold determination procedures, see Haber & Hershenson, 1980, chap. 2). As a technique, such judgments are open to criterion shifts, and therefore threshold shifts, when the subject changes his biases and expectations. The technique would also normally be expected to overestimate the threshold, leading in Libet's experiments to a time-of-occurrence estimate later than if it had been possible to use more reliable threshold techniques. Similar difficulties arise in all attempts to use awareness as an experimental variable, for example, in studies of blindsight, discriminative behavior elicited by stimuli of which the patient is unaware as the result of damage to his visual cortex (Campion & Latto 1985; Campion, Latto & Smith 1983), and in attempts to demonstrate semantic activation without conscious identification (Holender 1986; Latto & Campion 1986). The fundamental problem is that it is not possible to use adequate, criterion-free signal-detection procedures in a situation where the independent variable of signal or stimulus strength is the subject's conscious experience and is therefore not only not under experimental control but is also unknown to the experimenter.

The second practical difficulty for Libet is in the attempt to estimate relative timing. He has shown in other experiments (Libet, Wright, Feinstein & Pearl 1979) that the experience of a discrete stimulus may be subjectively referred back in time to an earlier neural signal closer to (though still later in time than) the stimulus. That is, the event was perceived as happening about 200 ms before the neural processes necessary to produce its perception were complete. It is therefore likely that in the present experiments the perceived time at which the moving spot is at a certain position would also be referred back. Now if the perceived time at which the awareness of the intention to act occurs is also referred back by exactly the same amount, there is no problem. The two events, awareness and the perception of the spot, will be in synchrony and the timing of the latter will be an objectively accurate guide to the timing of the former. But if the perceived time at which the awareness of intention occurs is not referred back, in the way that the perceived time of direct cortical stimulation is apparently not referred back (Libet et al. 1979), or if it is referred back by a different amount, then the timing procedure is invalidated. There is no evidence either way on this. It is worth noting, however, that if awareness of intention is not referred back at all, then the timing procedure would give a time for its occurrence that was late by the amount the perception of the spot was being referred back.

There are therefore two possible reasons why Libet's time-of-occurrence estimates for the awareness of intention might appear late relative to the more objective measurements of the time-of-occurrence of the readiness potential, in addition to Libet's own explanation that the readiness potential does actually develop before conscious awareness.

3. *Problems of interpretation.* If we accept Libet's evidence and conclude that the initiation of a voluntary act is unconscious, at least for his experimental situation of deciding when to make a voluntary movement, then we should surely also accept that the whole process of voluntary action might sometimes be unconscious. How would Libet's condition (c) (see section 1) apply in such a case? It would require the passive and retrospective reporting of events that at the time they occurred were not open to conscious experience. So, applying condition (c), at the time it occurs the action is involuntary, but the subsequent reporting of a feeling of voluntariness retrospectively converts it into a voluntary act. Alternatively, we have to conclude that condition (c), and consciousness, are irrelevant to the question of whether or not to act is voluntary.

Libet suggests that the reason why conscious awareness does eventually develop in his situation is in order that there may be a conscious veto on action. He does not make it clear why this veto has to be conscious. If the initiation of a voluntary act can be unconscious, why could not the subsequent veto also be unconscious? Nor is the evidence for a veto presented in section 4.1 very strong. His subjects' anecdotes could be interpreted in other ways, for example, as the occurrence of mistaken feelings that an act had been initiated. And the findings (Libet, Wright & Gleason 1983) from the experiment with preset responses that were not therefore voluntary according to Libet's own criteria but that could be voluntarily suppressed by the subject when instructed to do so by the experimenter are using a paradigm so different from the central one that it is difficult to generalise between them.

If the veto is set aside as a role for consciousness, we are left with consciousness as a passive process taking a few hundred milliseconds to develop, both for external stimuli and for internal decision-making processes of the land described here. Even without all the other difficulties outlined above, this rather barren conclusion should be enough to suggest that the electrophysiological investigation of conscious awareness is not yet a fruitful branch of science. The slow negative potential over the frontal lobes was first described by Grey Walter et al. (1964). Perhaps the siren call of its currently fashionable name, the readiness potential, with its implied association with consciousness, should be rejected in favour of a return to Grey Walter's original and far more neutral label, the contingent negative variation.

Do we "control" our brains?

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Libet is to be congratulated on the care and ingenuity with which he has articulated his position - a challenge to the rest of us to articulate our views with similar care. Given the presupposition that our conscious initiative means *"control* of the volitional process," it may (as he suggests) be "necessary to postulate that conscious control functions can appear without prior initiation by unconscious cerebral processes." This presupposition, however, seems to me unwarranted and arguably mistaken. There is, I think, an alternative way of looking at the relation between conscious control and brain activity that would wholeheartedly support Libet's emphasis on human responsibility without requiring the dubious postulate he thinks necessary.

We all know what it means to control the movement of a car by using our limbs; we also speak of consciously controlling, for example, a skilled finger movement, and people have even learned to control (i.e., regulate) the firing of their own motor neurones under suitable feedback. What makes these cases of "control" is that we have criteria in terms with which to *evaluate* what happens. Mere outward causal linkages are not sufficient. What we cannot in principle evaluate, we cannot control.

I see no reason to hold that in this sense we normally "control" (or should have any wish to control) our most central brain processes. That our conscious thinking, valuing, and choosing (sometimes) *determines the form* of our action is, I believe, a fact of daily experience. That such conscious mental activities have direct correlates in our brain activity seems a well-founded hypothesis, especially if the correlated brain activity is thought of in informational/stochastic rather than physical/energetic categories. That we consciously *control* these correlates, however, does not follow.

To see the logical non sequitur here, consider first an inanimate example. The autopilot in an aircraft in a clear sense evaluates and controls the plane's altitude, speed, and the like. It does so in and through an internal computational network of physical processes, which are ultimately linked to receptors and effectors in the aircraft. But does it in the same sense "control" these internal processes? Surely not; these are processes that it has no means of evaluating, for it is in them that it has its own being as an evaluative controller. Its evaluative and other computational processes certainly *determine the form* of their physical embodiment; but it would be a confusion of categories to say that they *control* it.

Now of course we are conscious agents, while autopilots (we believe) are not; but the same distinctions between categorical levels of analysis must clearly be recognised in the human case. If, as I suggest, we think of our conscious agent as *embodied in* our physical brain activity, then some (though not all) of that activity will have its form determined by our conscious thinking, valuing, and deciding. Motor acts casually dependent on such activity may then be under conscious control; but it would make no sense - it would involve a confusion of category levels - to conclude that we must therefore be able to "control" the cerebral correlates of our own thinking or that the cerebral correlates of a conscious decision must appear without causal initiation in prior cerebral processes.

From this perspective there is a clear distinction between those cerebral processes which are, and those which are not, direct *correlates* of conscious experience; but it would be inept to apply the category conscious/unconscious to any cerebral process as such. If, as I have argued elsewhere (MacKay 1951; 1966; 1982), the direct correlate of conscious experience is cerebral activity at a self-supervisory evaluative level, Libet's data have a simple and instructive interpretation. Far from suggesting that we have no conscious control over acts whose cerebral causes antedate our awareness of the urge to act, they would merely indicate that conscious volition is embodied in a stochastic cerebral process, in which the setting of evaluative criteria is not triggered until some prior physical process reaches a critical threshold. Whether or not "vetoing" is possible thereafter, the action is ours, because it is in that very same stochastic process that we have our being as conscious and deliberative agents. On this view the link between conscious decision and action is even more intimate than that between cause and effect (MacKay 1965; 1980). More firmly than any interactionist hypothesis, it pins to our own door responsibility for all we consciously choose to do.

Toward a psychophysics of intention

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Consider the following *Gedanken* experiment. The experiment generally follows Libet's design - a subject generates a simple and well-defined, self-initiated act, such as wrist flexion, at various points in times wholly of the subject's own choosing - but it contains some modifications. The first two modifications, though they make my paradigm technically impossible, nevertheless remain true to the spirit of Libet's. First, I shall assume that we are able to measure the "readiness potential" or RP on each and every occasion on which it occurs. Second, I shall assume that if we otherwise follow Libet's paradigm, we will find that every occurrence of an RP leads, about 400 ms later, to a report of conscious intent to perform the motor act (and is unrelated to any possible decision within the next 150 ms or so to "veto" the performance of the act). In other words, I am assuming that the RP is not irregular in appearance or an artifact of averaging but is regularly and reliably related to a subsequent awareness of conscious intent.

Now, however, I wish to make a more profound change - a *"Gedankener"* change - in the experimental paradigm. Assume it were possible within the course of an experimental session to "stop action," both physiological and mental, to halt the proceedings at a point, say, 200 ms after the main negative shift of the RP, which would be about 200 ms before the subject would report, under normal circumstances, becoming aware of conscious intent. With the sequence of events on "hold" in this wholly imaginary experiment, I would like to be able to query the subject, "Do you think you are likely to want to move your wrist within the next few tenths of a second?" Of course, as a control measure I would ask the same question of the subject at other, randomly chosen, points in time, points at which no RP had been in evidence in recent moments. Were all of this possible I strongly suspect that subjects would be much more likely-to acknowledge an intent to act at "test" moments - that is, during the supposed "unconscious interval" between the occurrence of the readiness potential and the first awareness of an intent to act - than at "controls."

This suspicion is largely represented by an analogy I would like to draw between intentions to perform voluntary acts and perceptions of weak signals. Consider now a parallel experiment on signal detection: On a given trial a weakly luminous light may be flashed, and the subject is asked to respond either "yes," a light was detected, or "no," it was not. In fact, the light is flashed on half of the trials; the other half contains "blanks." Under a given set of conditions (instructions, rewards or punishments for various types of correct or incorrect responses) and with a suitably selected fight intensity, a subject will correctly detect (respond "yes" to) a certain fraction of the presentations of the light stimulus (hits) but will also incorrectly identify (respond "yes" to) some smaller proportion of the nonstimulus or blank presentations (false alarms). As instructions, rewards, and so forth are manipulated but the intensity of the light is held constant, the percentages of hits and false alarms rise and fall in tandem.

To construct this example, I have freely borrowed from the enormous literature on the theory of signal detectability (see, for example, Green & Swets 1966; Swets, Tanner & Burdsall 1961), a theory that interprets these kinds of findings as follows: Every presentation of the sensory stimulus produces an internal response, which adds to the always present, ineluctable noisy background, a background that is continuously fluctuating. To the subject, then, the task of detecting a stimulus becomes one of distinguishing stimulus-plus-noisy-background from noisy background alone. The subject tries to maximize performance by setting up a cutoff along the dimension of the underlying internal sensory response: When that response exceeds the cutoff,' the subject response "yes"; otherwise, the subject responds "no." The overt response is therefore joinly a function of the internal sensory reaction to the stimulus and the particular cutoff or criterion. In principle, some information is available on every stimulus trial. A telling finding is, for instance, that in a multiple-choice paradigm (a signal is presented to one of four possible locations), when first guesses are incorrect, second guesses can be correct at a frequency above chance.

May an analogous "criterion" exist for the reporting of conscious intention to perform motor acts? In a sense what I am proposing is that up to some point near (within 100-200 ms of) the projected time of voluntary motor activation, "awareness" of the intention is as much a function of the "criterion for reporting" as it is of the strength of the underlying intention itself. Just as many a subject in a psychophysical "threshold" experiment will set a high criterion, avoiding "false alarms" but at the cost of "missing" many stimulus trials, so too may some time interval following an RP be one of high criterion for reporting an awareness to act. It follows from this analysis that the final brief moment before action, or veto, is one in which either the criterion drops to a level sufficiently low that intention is dramatically evident or the "intensity" of the underlying intent increases markedly, for it is likely that the "intensity" of intentions themselves can fluctuate, can differ from occasion to occasion or over lime.

But the most significant point, I think, is the possibility that there may be some very general processes or mechanisms governing the transition from nonawareness to awareness, from nonperceived to perceived, across the so-called threshold of consciousness. Might we seek a unified theory of conscious elements (percepts, intentions, et al)? Rather than dichotomize between not aware and aware, I would suggest a probabilistically determined continuum. Long ago, Leibniz (1916) argued both for the existence of "unperceived perceptions" and for a continuity in the gradations or qualities of consciousness. The conscious entities he identifies as monads. Although Leibniz denied that temporality applies to monads, I propose the opposite-that there is a temporal continuity in which I would call the "potential awareness" of voluntary acts, and that it is precisely this temporal continuity to which a psychophysical model applies.

Conscious and unconscious processes: Same or different?

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Being cognitive psychologists interested in the study of unconscious perceptual processes, we read Libet's review of his research program with great interest to see whether it provided new insights into the relationship between conscious and unconscious processes. Unfortunately, we were disappointed. From our point of view, Libet's research only documents an implicit assumption made by many cognitive psychologists, namely, that self-reports of conscious awareness are based on underlying brain processes. Furthermore, although some of Libet's conclusions concerning the relationship between unconscious brain processes and conscious awareness are interesting, these ideas lack empirical support, because they are based on speculations that are untestable using his methodology.

To understand our conclusions, it is useful to consider the approach adopted by cognitive psychologists to study unconscious perceptual processes. Two basic questions have guided research in this area: (1) Does perceptual information for which there is no conscious awareness influence behavior? (2) Do conscious and unconscious perceptual processes lead to distinguishable behavioral consequences? In order to answer these questions, cognitive psychologists have used research designs that allow the potentially distinctive effects of conscious and unconscious perceptual processes to be compared or contrasted. The experiments, in their simplest form, involve two separate conditions: One condition involves the assessment of performance following the presentation of unconscious perceptual information, while the second condition involves an evaluation of performance when the same perceptual information leads to conscious awareness.

In contrast to the approach adopted by cognitive psychologists, Libet's critical empirical findings are derived from observations made within a single experimental condition. The key finding underlying the entire paper is that a readiness potential (RP) *always* precedes a self-report of conscious awareness, which, in turn, *always* precedes a voluntary action. Thus, in Libet's experiments, RPs, conscious awareness, and voluntary action are *perfectly* correlated. Given this perfect correlation, it is impossible to distinguish the potentially distinctive behavioral consequences of brain processes that do and do not lead to conscious awareness. Furthermore, without evidence to indicate that RPs in the *absence* of reported awareness also precede behavioral acts, there is no empirical support for Libet's critical assumption that RPs and self-reports of conscious awareness reflect unconscious and conscious processes, respectively. In fact, given the perfect correlation between the two measures, there is simply no need to distinguish between these measures theoretically.

The only conclusion that can be made with confidence on the basis of Libet's findings is that conscious awareness of an impending voluntary action is always preceded by specific brain processes. This empirical observation, although interesting, only confirms a generally held implicit assumption. As long as it is assumed that conscious awareness is based on underlying brain processes, an assumption consistent with the views of most cognitive psychologists, then it is not surprising that certain brain processes occur prior to self-reports of conscious awareness. In fact, how could it be otherwise? For example, in our studies of perceptual awareness for visual stimuli (e.g., Cheesman & Merikle 1984; in press), activity in the optic nerve must logically occur prior to conscious awareness of the stimuli. Thus, by establishing that brain processes always precede self-reports of conscious awareness, Libet has only confirmed a necessary implicit assumption made by most cognitive psychologists. In our opinion, if RPs always precede reports of conscious awareness, then this entire sequence of brain and behavioral responses should be viewed as reflecting conscious activity.

Finally, Libet's findings do not address the questions that cognitive psychologists find most interesting. These questions concern the separate or distinctive roles of unconscious and conscious processes in determining voluntary action. Because of inherent limitations it is not possible to use Libet's methodology to investigate either the distinctive contributions of conscious and unconscious processes or the interactions that may occur between these two types of processes. Thus, even though Libet discusses a number of interesting ideas concerning how conscious and unconscious processes may interact, his empirical findings do not provide any support for these speculations, since his results demonstrate only that voluntary actions are preceded by two perfecdy correlated events: RPs and self-reports of conscious awareness. It is this correlation that must be eliminated before the distinctive roles of conscious and unconscious processes can be established.

Conscious decisions

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Libet distinguishes two possible functional roles for the urge to move specifically coming to consciousness: veto and trigger. The difference is that a conscious veto is something whose absence leads to the action's occurring; a conscious trigger, on the other hand, is something necessary for an action, so that without the trigger the action does not occur.

Libet, however, draws attention to earlier work according to which a short (300-ms) period of appropriate cerebral activity is required to achieve the "neuronal adequacy" necessary for a conscious experience. He proposes that the same is true of the relationship between the readiness potential, RP, and the decision to act at about -200 ms. This suggests a third role, a more functionally epiphenomenal one, according to which the apparent conscious decision is neither a veto nor a trigger but *merely* the coming to consciousness of an unconscious process already in progress and indicated by the RP.

Libet's arguments for the veto hypothesis amount to arguments for the functional efficacy of the conscious experience of a decision. First, subjects not infrequendy report the conscious experience of an intention to act followed by the acts being aborted. Second, in different experiments with instructions to veto at preset times when the decision becomes conscious, the same RP occurs but flattens out at about the time (—200 ms) at which the intention becomes conscious and veto reportedly occurs. Libet notes that such evidence must be indirect because of the fact that the individual RP spike is not detectable above background noise and must hence be derived by averaging. Both of these arguments are, however, consistent with the veto itself arising from a prior unconscious veto process and only "incidentally" later coming to consciousness, as the former point about neuronal adequacy would also suggest. It must be conceded that the second argument adds the feature that conscious instructions by the experimenter are a causally relevant factor that it is not unreasonable to suppose operates via a conscious veto mechanism. But this is not logically forced on us. Indeed, in all these experiments, conscious instructions by the experimenter at the beginning are (partly) responsible for the unconscious RP when it occurs, if not its exact timing.

There is one standard argument for the functional efficacy of the conscious experience of intention: Whatever seems real to consciousness (even if it is an illusion) needs explanation and so is not functionally epiphenomenal. But to concede that consciousness has a function is not to say that the function is specifically *veto or trigger.* Furthermore, the implied evolutionary argument here cuts both ways: Since we are focusing on the last-minute decision to move, rather than "diffuse" preplanning (which presumably has whatever "higher" function conscious deliberation has), then the mechanisms involved may well be older and evolutionarily prior to conscious decisions.

For all this, Libet's case for a causally relevant conscious decision is persuasive. However, there is a specific problem with the conscious-trigger hypothesis, namely, that the conditions of the experiments are artificial to the extent that much normal movement occurs when our minds are very much on other things. Conceivably a trigger occurring later than the onset of the RP still invariably occurs, unconscious but sufficiently like a conscious decision to be worth retaining that term for. But aside from the difficulty of verifying such a trigger, it seems neater to opt for a positive veto function. This also fits better with the efficacy of instructions: If instructions to veto operated to hold back a trigger, then this looks like an internal veto mechanism anyway. It is harder to agree with Libet's suggestion that both trigger and veto functions might be independendy present in the conscious experience. Remember that before the conscious trigger would operate, there has been a rising RP for 300 ms already. But a true trigger is *necessary* for action. In its absence the action would not occur, irrespective of a veto function, consigning the latter to an epiphenomenal role. Similarly for vetos: In the absence of a veto, action proceeds. The only way to have both, it would seem, is to have them acting in series, which would be a more complicated conscious mechanism. All of this, of course, accepts Libet's assumption that vetos and triggers are not trivial converses of one another (with "trigger" defined as "absence of veto" and vice versa). The trivial converse manoeuvre is a way to have both together at merely linguistic cost, but there does seem to be a genuine difference between vetos and triggers that such linguistic legislation would obscure. Again, the fact that much action occurs with one's mind on other things suggests that conscious vetos have an (occasional) role but that conscious triggering does not. A better role for a trigger would be whatever *unconscious mechanism sets off the RP rise.*

Finally, the moral implications are, I suspect, not what Libet proposes, even though I am sympathetic with Libet's general position at this point. Libet seems to be operating on some sort of *moral responsibility* theory, according to which one can be held responsible only for one's exercisings of conscious control. While such exercisings obviously are morally significant, it seems better to base moral prescriptions on those which will be efficacious in moral education. Remember that we are dealing with initially unconscious final decisions to act, subject to last-minute conscious veto or trigger. At least if the veto model is correct, such final unconscious decisions are exceedingly dangerous beasts, well deserving of castigation and the attention of moral educators. (We have already argued that prior instructions affect the arising of the RP.) The same point would apply on the third, consciousness-as-functionally-epiphenomenal suggestion. Only on the conscious-trigger model would the preceding unconscious process be serving a less-than-morally-vital function.

Brain physiology and the unconscious initiation of movements

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The new technology introducing a variety of means to monitor different aspects of brain activity has made it possible to obtain a many-sided and detailed picture of brain processes occurring during various subjective experiences and behavior. However, although each subjective experience may have its unique brain state, the reverse is not true: Data from diverse sources are rapidly accumulating to suggest that a number of brain processes occur in response to sensory stimuli and underlie essential aspects of information processing but have no representation in conscious experience (e.g., Naatanen in press; Naatanen et al. 1978; 1980; 1982; in press). Libet and his associates' insightful research on the initiation of movement, which in an interesting way links physiological and subjective data, appears to provide a particularly important case of brain processes with no simultaneous subjective counterpart. This is because, as Libet claims, these brain processes precede the conscious experience of the intention and decision to initiate a movement. Such brain processes are usually regarded as conscious if the movement occurs in the absence of abrupt environmental change or stimuli.

This appears miraculous, and we should therefore examine very carefully the situation and data giving rise to such a radical claim. Libet's central thesis is that well before (by some hundreds of milliseconds) we consciously decide (or experience an intention) to perform a motor act, the movement-related slow potential called the readiness potential (RP) or *Bereitschaftspotential,* discovered by Kornhuber and Deecke (1965), starts to develop in our brains. This means that if we could monitor the readiness potential on-line on a single-trial basis (i. e., if the signal-to-noise ratio were good enough to make it possible to determine the presence of the RP in the raw unaveraged EEG, we would be able to see in advance when the subject was going to experience an intention to perform the instructed movement.

First of all, I am convinced of the soundness of this data-base from some of my own pilot work of over a decade ago. Puzzled by the long duration of the RP before the actual movement compared to the fact tbat even unwarned motor responses in reaction-time experiments occur within a much shorter time from stimulus onset (see, e.g., Naatanen 1971; Naatanen & Koskinen 1975; Naatanen & Merisalo 1977), I, in pilot experiments with T. Jarvilehto, tried to "fool" the cerebral RP generator by concentrating on reading a book and suddenly, acting on movement decisions occurring "out of nowhere" by pressing a response switch. In this way we tried to produce a movement with no preceding RP or with only a very short one. Nevertheless, much to our surprise, RPs of quite a long duration were still there although the subject felt he had (immediately) followed a sudden, spontaneous urge to press the switch.

Although Libet's data-base is unassailable, his conclusions can be questioned. He seems to ignore the fact that the specific nature of the movement was determined in detail by the instructions, practice, and preceding repetitions, and that hence the only decision of the subject involved the *timing* of this *preplanned* movement. Moreover, even the decision to perform this movement can be regarded as already having been made (consciously] by him at the beginning of the experiment: The subject knows and has agreed that he is going to produce quite a large number of these movements sooner or later, within some reasonable time, before he can leave (and receive his payment), and that it is only the timing of each single movement of this specified type that is under his control - and even that not fully but within certain quite wide limits. Consequently it appears to be somewhat questionable to describe this motor act as "spontaneous" ar "fully endogenous" and occurring with "no preplanning." It is accordingly not possible to agree with Libet's main conclusion that "cerebral initiation of a spontaneous voluntary act begins unconsciously." This conclusion means (and was intended to mean — judging from the author's discussion of free will) that even the *type* of motor act to be performed is unconsciously chosen (a v eto of a conscious decision is also regarded as possible, however). Perhaps, but this cannot be concluded from the present data, since the type of motor act and whether it would be repeatedly performed during the session was *consciously* decided by the subject on receiving the experimental instructions. Cosequently, the discussion of the possible implications of Libet and his associates' results for the issue of free will involves an unnecessary expression of concern. If I decide to go to a liquour store - regarded by some in this country as an immoral decision - I am sure there is no RP preceding this decision, whereas an RP might precede the conscious experience of deciding to initiate the chain of muscular events leading to this end.

Nevertheless, the brain's deciding *when* to perform a preplanned motor act well before the mind decides this is certainly of sufficient interest to warrant discussion in these respected pages. This specified motor act is, presumably, in some state of facilitation for reasons discussed above, that is, there is some central, and perhaps even peripheral, facilitation of this particular motor pattern that might contribute to the dissociation between RP development and its "subjective counterpart." Moreover, after each instance of performing this movement, there might be a sublte conscious decision with regard to the moment of the next movement in the sequence, which might then trigger the RP onset with this predetermined delay. (The distribution of the intermovement intervals might be highly informative here: Some deadline rather than the Bernoulli type of distribution might be reflected in it.)

In any case, Libet and his associates' work has provided a model case of the ingenious application of available physiological and psychological measures to understanding the mind-body relationship in the initiation of a preplanned (and repeated) motor act with spontaneous (within certain limits) timing.

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Libet's dualism

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Libet presents two principal theses: Given the experimental setting and his findings concerning volitional processes,

(i) A subject's spontaneous, conscious urge to act is initiated by an unconscious cerebral process signaled by RPs.
(ii) The act itself, "conscious voluntary action . . . may operate . . . to select and control" the volitional process.

Except for possible doubts about the veridicality of the introspection and reporting of urges and possible questions about timing, it seems to me the experiments combined with the cited supporting experiments do indeed establish (i) and (ii).

There remains, however, a dangling question whether some mediating cerebral process precedes the occurrence of conscious control (ii). According to Libet, "there is presendy no technique available for recording and analyzing any RPs that may be associated" with conscious *vetoes* of an act. And presumably there are no techniques for identifying the role of cerebral processes, if any, in positive *triggering* of an act, although this is not very clear from what Libet says. Of course, absence of adequate technique does not imply that there are no such underlying processes.

Nevertheless, Libet concludes that it would indeed be *necessary* to postulate:

(iii) Conscious control functions (ii) can appear without prior initiation by unconscious cerebral processes.

Thus conscious voluntary control is autonomous with respect to the brain.

From what he says in the last section of the paper I suspect that Libet, in suggesting this postulate, wants to make room for a kind of scientific warrant for the proposition that human beings have voluntary control of at least some of their actions, in the straightforward sense of popular psychology and ethics. In our nonphilosophical moments most of us feel that conscious actions are not merely part and parcel of a purely physical, cerebral stream of events. As agents we *cause* actions. So lacking evidence as to physical causal factors in conscious voluntary control, the postulate provides an appealing sop to our ordinary intuitions. It even has some scientific warrant inasmuch as it enjoys support, according to Libet, from Margenau and Popper and Eccles, as indicated below.

I believe, however, that the following considerations indicate that Libet might better have suspended judgment, awaiting either further experiments from which something more definite could be concluded about the presence or absence of RPs (or other laboratory indicators) preceding conscious voluntary activity, or developments in cognitive psychology that could afford better clues as to the role of actions in intentional life.

Notice that the postulate is *not* grounded in Libet's definition of "voluntary action," which stipulates (a) that the action must arise endogenously, (b) that it must be without external constraints, and (c) that the subject must feel free to act if he wishes. For a conscious event (e.g., an urge [i]) could arise endogenously and be initiated by a cerebral process, and the subject could feel free (satisfying [c]) without being free, which is hardly news. Moreover, the proposition is not supported in any way by the experiment, since reporting the feeling of freely performing an act does not entail that the conscious control function can appear without initiation by a cerebral process. That the control function, as postulated, can occur withour prior initiation by brain processes is not supported by the experiment, by the principal results (i) and (ii) that derive from it, or by the underlying definitional concepts.

However, the postulate (iii) is not inconsistent with (i) and (ii), although as a set they are curiously *incoherent.* Why is one mental event - the urge - initiated by a brain process, while the other - the conscious voluntary act - is not (at least, why is it postulated not to be so initiated)?

This incoherent mix betrays a strange sort of double dualism. Before explaining what I mean, let me clear up in advance a possible misunderstanding about the term "initiate." This can be understood in a direct, empirical way within the context of experiment. As initiating event, the RP-signaled process is the head of a uniform, regular sequence, with the felt urge being the contiguous second element. (This is slightly reminiscent of Hume; however, Hume's analysis of "cause" does not mix putatively physical with phenomenological events - ideas, impressions - so one hesitates to say on Humean grounds that "initiate" means "cause"). But Libet must mean in these interpretational passages more than empirical regularity. I suspect there are all sorts of deep cerebral processes that regularly and uniformly precede conscious voluntary control but are wholly without influence on action. So I suggest that Libet must mean by "process A initiates event B" that B would not *normally occur* without A, that is to say, that A *causes* B. It is not easy to grasp the force of (iii) unless he means "cause" by "initiate," which of course loads an added philosophical burden on an otherwise neutral experimental term.

If this is right, a certain type of cerebral process causes an urge to act, whereas, by the postulate, conscious control functions can occur without being caused by forerunning or concomitant cerebral processes. The *double* dualism is this: (1) some mental events, that is, urges, are caused by physical events. This is a form of interactionism (not of substantive interactionism, as no claim has been made that there is a substantial mind being influenced by a material brain); on the other hand, (2) other mental events, that is, conscious active control, might be features of an emergent conscious awareness that has "already developed" (cf. Margenau 1984). (As an alternative Libet suggests that the postulate "can be in accord with a dualistic interactionist view" [Popper & Eccles 1977]. But this idea is unintelligible. If conscious control is uncaused by unconscious cerebral process it certainly cannot be the result of interaction with the brain, unless the interaction is something the subject is *conscious* of or else there is some kind of extracerebral bodily process that causes it - both of which are extremely unlikely.) So conscious voluntary control is part of a conscious stream parallel to, but not interacting with, cerebral process.

Adoption of Libet's suggested postulate leads to an interpretation of the experiment having the incredible consequence that there is an *interactive* dualism of physical and mental events, as in the case of urges, and yet a parallel *noninteractive* dualism, as instanced by voluntary actions. The remedy to this confusion is to drop the postulate and pursue the elusive connection via further experiment, possibly within the conceptual framework of a strictly materialistic view of mind and brain.

Timing volition: Questions of what and when about W

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Libet is to be congratulated on having both the courage to seek experimental answers to deep and difficult questions about mind, brain, and conscious control of voluntary movement and for finding such ingenious scientific methods for attacking such resilient questions. Conclusions drawn from work in this area are likely to be monumental ones and as such will demand the most solid foundations possible. As Libet is clearly aware, one of the more difficult points in his work is the self-report of the "urge," W. He has tried to assess the timing of this report by at least partly independent methods and hence to increase its reliability. Aside from the question of when W occurs there may also be some question of what the subjects are reporting. In the main task the subject is asked to initiate a movement at a self-chosen and pseudorandom time. There must be some way in which this point in time is chosen, that is, there must be some initiation. The mechanism responsible for initiation must be mostly in a state (or in a mode, or at a level) that does not cause initiation and on pseudorandom occasions must go to a state that does. One possibility for such a mechanism is a fluctuating potential occasionally crossing some threshold and producing an initiation. Upon back-averaging from the result of the initiation (the electromyogram, EMG) one might find something very much like the RPs recorded. In a sense there would then indeed be an unconscious initiation of the movement when the pseudorandom fluctuation crossed threshold but one that was fully set up by the "consciousness." What is being suggested here is that the instructions to produce spontaneous movements may cause the subjects to create an unusual mental state in which brain potentials trigger a previously willed decision.

The possibility that the subject is essentially monitoring some brain potential (or some correlate thereof) and initiating a movement when this potential exceeds some criterion may be open to experimental test. The distinction to be made is between a potential associated with movement and a potential associated with the requirement of spontaneous initiation. The experimentis as follows. In circumstances that are otherwise the same, subjects are asked to choose (and later report) a clock position on a pseudorandom, spontaneous basis. That is, just as in Libet's main experiment, the urge to "act" should come out of nowhere, but in this case the "action" would be simply to note the clock time. With recording of the EEG and the clock position an average could be constructed later by back-averaging from the reported clock time (of the urge). The discovery of a potential preceding the urge would suggest that the type II RP stems from the requirements of spontaneous initiation, while a failure to find a potential would strengthen the interpretation of the type II RP as the harbinger of the motor act. Such an experiment might at least help determine whether the recorded potentials are more clearly associated with the voluntary act (physical) or the decision (mental).

A second and less testable point is that the subjects may be reporting the "peak" of an urge that actually has an extent in time. That is, perhaps we should not imagine the production of an instantaneous urge that is then sent out to the appropriate motor control areas and generates activity (from which idea we would expect the urge to precede the RP); instead, the urge may have a start, a rise, and a peak. If for the moment we think of the urge as having a physical source and form, it may be that the urge is produced by areas or cell groups connected to the areas that produce RPs; the start of an urge would start an RP, the rise of an urge would produce the rise of the RP, and so on. Such a system might produce an (unrecorded) "urge waveform" that precedes the RP by a few tens of milliseconds. This early RP might reflect the motor system's being "readied" in an effort to anticipate as well as possible the outcome of the "will's" decision and hence to save time (this is somewhat analogous to "look-ahead" computer methods). Since in the experimental situation the likely motor act is quite predictable and only the time is unknown, readying the system for the motor performance is not unreasonable, so the very beginning of the "urge waveform" might very well begin the production of the RP. When asked to report an instant in which the urge occurred, however, the subjects may be choosing the peak of the "urge waveform" (which follows the beginning of the RP) instead of the beginning of the "urge waveform" (which leads the RP). Perhaps if the subjects could be instructed to choose between two (or more) movements as well as to choose a time, all in a spontaneous matter, then an anticipatory RP would be less likely since the desired movement would be less predictable.

Sensory events with variable central latencies provide inaccurate clocks

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Libet's earlier analyses of central timing processes for sensory experiences have been cogent and clever, his views on complementary experiences associated with motor acts also are often insightful. However, unless I misunderstand Libet's methodology and rationale, a serious logical flaw exists in his determination of the absolute times of conscious intention to act (W), awareness of actual movement (M), and awareness of a tactile sensation (S). If so, alterations in the interpretation of Libet's absolute values are required, although the relative times between some of these events may still be generally correct.

Libet measures the time of the first awareness of wanting to move (W) by having the subject report, retrospectively, his observation of the "clock position" of a spot of light revolving on an oscilloscope screen when such an experience occurred. By relating this to the clock time when the actual motor act began, using a record of the electromyogram (EMG) from the appropriate muscle, Libet claims to have determined that subjects become "consciously aware of the urge to move 200 ms before the activation of the muscle."

The perceived position of the clock at the time a subject experienced awareness appears to be confused with the actual time when the awareness took place. Such readings do not occur instantaneously. Sensory events are registered centrally only after a latency of up to several hundred milliseconds. A clock value of "0 ms" is transduced, coded, and transmitted through the retina, optic pathways, and subcortical and cortical regions before it can be "read" as stating "0 ms." By that time, of course, the face of the physical clock tells a very different time, "N ms."

Consequently, the clock time described by the subject as occurring simultaneously with his intention to move is a central representation of an event that occurred N msec earlier. The actual time of the occurrence that Libet wants to measure is N ms later than the value the observer reported. It is difficult to estimate the value of N. Fitts and Deininger (1954) found reaction time in a clock-reading task to be about 400 ms, a value that must include sensory, motor, and decisional components. If N is as long as 300, then the subject's awareness of the urge to move does not occur 200 msec *before* activation of the muscle, as Libet proposes. Rather, it occurs -200 + N or 100 ms *following* the movement. If N is 100 ms, the awareness occurs -200 + N or 100 ms *before* the beginning of the EMG. Clearly both positive and negative times are possible because of the lability of the central latency and uncertainty whether early or late components of the neural response are involved.

The determination of M, the "clock time for the awareness of actually moving," suffers from the same defect. Libet notes that "M values were, unexpectedly, negative to EMG -0 time." Again, consider that the time described by the subject was the time on the clock N ms before that reading was actually perceived. Real time is N ms later. If N is 300 ms, the true value of M changes from Libet's reported -86 ms to -86 + 300 or +214 ms. The perception of movement occurs *subsequent* to actual movement, iind the "negative" value that emerges from Libet's method is not unexpected.

The earlier reinterpretation offered above suggested that W could really be +100 ms (if N is 300 ms). This implies that the subject does not become conscious of the urge to move until 100 ms after the movement has occurred. Before dismissing this as counterintuitive, consider that the second part of the reinterpretation suggests that M, the time when the movement is perceived, is +214 ms, indicating that the movement itself is not perceived until 214 ms after it has taken place. The urge to move is perceived 114 ms before the movement is perceived in both Libet's analysis [-200 - (-86) = -114] and my own (100

— 214 = —114). Libet's values may reflect the relative times of the critical events, but they do not correctly reflect either their absolute value or their sign. Since N is unknown, no accurate values of W, M, or S can be obtained.

A further complication arises in the proper determination of M. As Libet indicates, a judgment regarding the occurrence of movement may accompany either the motor command or feedback from the movement. If it is the latter, the latency of the appropriate reafferent signal must also be considered in determining the relationship between recall clock times and true latency between critical central events.

Likewise, the value reported for S, the time when a skin stimulus is perceived, is subject to additional problems. To determine W, Libet compares a peripherally initiated event (visual examination of the clock) with a central event (intention to move). In measuring S, he compares two peripherally initiated events, those triggered by clock movement and skin stimulation. Both of them will require a considerable latency (almost certainly different) before they are perceived.

Those latencies are influenced by both stimulus characteristics and task demands. If conduction time were equivalent in the visual and somatosensory systems for one set of parameters, adjustment of intensity for either signal could tip the balance in one direction or the other (Rollman 1974). Given that the tactile task involves simply detecting the presence of a stimulus on the skin while the visual task requires discrimination of clock position, latency for the second judgment is likely to be considerably greater. If the decision about the time of touch onset occurs when the neural representations of the tactile pulse and the clock position joindy reach some central locus, the longer-latency visual event must have taken place prior to the presentation of the tactile signal. Under such conditions a negative value for S must occur (it was about —50 ms for Libet's parameters).

This outcome follows from the differential transmission times for the two stimuli; Libet's footnote to Table 1 labels it "error" or "bias." The wide potential variability in the value of S as a consequence of changes in stimulus parameters, plus the fact that a tactile pulse is a peripheral event whereas the intention to move arises centrally, negates taking Libet's S as "a measure of the potential error in reports of W."

Libet has wrestled admirably with the complexities underlying the timing of conscious intention to act. Unfortunately, the situation seems even more complex than he anticipated.

Are the origins of any mental process available to introspection?

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Putting to one side questions of methodology and the issue of how a special causal role for a "conscious" process can be established, I shall argue that there are a number of logical and conceptual problems with Libet's thesis. The thesis is that the initiation of a voluntary motor act is under the control of a system or systems whose activity is not accessible to conscious introspection, at least until some time after it has begun, while the processes causing a modification of such an act are closely associated in time with introspectively derived feelings of control over it.

First, this thesis depends crucially upon the assumption that there is a necessary relationship between the execution of a voluntary "willed" action, such as a finger movement, and the prior existence of the variable chosen by Libet to index the onset of the processes leading up to the action, the readiness potential (RP). Thus, it would, for example, be necessary to plausibly rule out the existence of individuals in whom, as a result of, say, a brain lesion, RPs have been abolished, but not the capacity for voluntary action. To my knowledge, no such study has yet been carried out, and in the absence of any relevant data pertaining to this issue the proposition that a necessary relationship exists between RPs and voluntary movement is at least questionable. In addition, in the absence of any knowledge as to the precise functions with which the RP is associated, it seems premature to propose that the emergence of an RP indicates the onset of processes leading to a *specific* voluntary act, as opposed to the beginnings of some more general "arousal" or "priming" process serving as the precursor to a wide range of potential acts. The choice of the specific act to be performed may indeed be associated with the very process giving rise to the introspectively experienced "will" to perform that act. Inasmuch as the emergence of the RP prior to the time of this feeling of an urge to act is associated with exclusively nonspecific aspects of motor output, a crucial role would indeed exist in the initiation of an act for the processes associated with its conscious "willing" (but see below). Although denied as such by Libet, this position seems significandy at variance with the essence of the thesis advanced in the target article.

A further difficulty concerns the limited scope given to the notion of an act or action. Within the framework of contemporary cognitive psychology it is not uncommon for there to be no hard and fast conceptual distinction between overt motor acts and their covert, mental analogues (see, for example, Posner, 1980, for such an exposition with respect to mechanisms of visual search and attention). In this vein, I argue that it is quite reasonable to consider a covert mental event such as a "consciously" taken decision to be a type of voluntary act. This being so, one might reasonably question whether the precursors of such an act are any more amenable to conscious introspection than those associated with an overt action such as a finger movement. A relevant example in the present context is the decision to "veto" a previously initiated finger movement. This is considered by Libet to be an example of the role of conscious control in motor function: specifically, to "select or control volitional outcome." On the basis of the above arguments, the precursors of the "veto" decision might themselves have origins that are as inaccessible to introspection as those associated with the original decision to initiate the act in question. One is therefore forced to the conclusion that there is no evidence for the conscious control of the initiation of *any* definable overt or covert act; the origins of all behaviour, whether this is ultimately expressed in an observable motor act or not, and irrespective of whether any aspect of its precursors eventually enters consciousness, may arise from processes to which we have no introspective access.

Thus the distinction drawn by Libet between the intention to act and the fulfillment of that intention, in terms of the former being outside an individual's "control" and the latter within it, ceases to be meaningful. Although it may be reasonable to argue that a necessary component of any "voluntary" act is an introspective awareness of an intention to execute it, this is not the same as arguing that this awareness itself has a special causal status. To reiterate, the origins of this awareness, and of any modifications to it, may always precede and thus determine its contents.

Conscious intention is a mental fiat

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Libet jumps from neurophysiology straight to philosophy as if there were no psychology in between. Contemporary psychology indeed has little to say about the "conscious will," but the will was a standard topic for earlier psychologists who took introspection seriously without neglecting physiology. To Americans, William James is the "classical" psychologist par excellence, and he included a long chapter on the "will" in his *Principles of Psychology* (James 1890). Another classical psychologist who spent his entire scientific life on "the analysis of the will" was Narziss Ach (1935). Libet shares with classical psychology a reliance on introspection and even some of its technical procedures. It is therefore appropriate to relate his work to the viewpoints maintained by psychologists such as James and Ach.

Libet finds it surprising that the "conscious will" does not have the function to "initiate a specific voluntary act" but only serves to "select and control volitional outcome." But a similar conclusion was reached by the classical psychologists. In Ach's analysis of the will, the concept of "determination" is central. It refers to the fact that once a certain task has been adopted by a subject, the selection and control of subsidiary mental processes is performed at an unconscious level. However, this does not mean that the execution of an overt voluntary act has no conscious antecedents except for a general "planning" stage (i.e., Ach's determination). As long as an act is not automatized, there will be "intentional sensations" representing the act or its outcome, and there will be some kind of mental consent to the occurrence of the act (Ach 1935, p. 122). The same thought had been expressed by William James (1890, vol. 2, p. 501): "An anticipatory image . . . of the consequences of a movement, plus (on certain occasions) the fiat that these consequences become actual, is the only psychic state which introspection lets us discern as the forerunner of our voluntary acts." James thought that the anticipatory image was obligatory, while the fiat was needed only when inhibitory influences had to be overcome. But when both are involved, the anticipatory image precedes the fiat.

How can we relate these concepts of classical psychology to the events in Libet's experiment? The critical event reported by Libet's subjects receives somewhat different names: "endogenous urge or intention to move"; "wanting to move"; "conscious intention to act." A distinction was made between acts that "were experienced as fully spontaneous and unplanned" and acts where "some general preplanning or preparation" was experienced. On the basis of Libet's description, one can identify the "general preplanning" with Ach's determination, or rather with its conscious equivalent, and the "intention to act" to the "consent" or "fiat" of both Ach and James. Because Libet's experiments involved a choice between two incompatible acts (responding and not responding), William James would have consented that the fiat was necessary.

So far, then, Libet's results are well in line with the viewpoints of classical psychology and provide them with a physiological underpinning. A general determination precedes the consent to allow to happen a specific act selected by the determination; the determination is occasionally reinstated in conscious form, and when this happens, it has a specific cortical correlate ("type I" RPs); however, the selection of the specific act occurs at an unconscious level, and it is noted only after the fact, at the stage of consent or fiat.

But what about James's anticipatory image or Ach's intentional sensations? Libet mentions the possibility that a "nonrecallable phase of a conscious urge exists," but he rejects this possibility as untestable. However, perhaps the question is not so much whether or not a certain event is "recallable" but whether or not it will be noticed at all. And here we should accept the premise that introspection works selectively, that in introspection we find only those events that we have been led to expect. Libet's subjects were apparently instructed to observe events related to the "volitional" aspect of mental activity as envisaged by everyday psychology; the occurrence of anticipatory images was never reported, perhaps because it was never asked for. The "expert observer" of introspective psychology should be reintroduced, and bis attention should be directed to the possibility that a movement might be imaged before one "wants" or "intends" to act. If such judgments can indeed be made, they too can be timed with Libet's methods, and such timing might result in a coincidence with "type II" RPs. This would constitute an alternative interpretation of these RPs, removing much of the mystery with which they are surrounded in Libet's account. The fiat would then be preceded by a neural event having an immediate conscious correlate.

Another concept that bears some demystification is the "conscious veto." Its existence was demonstrated, long ago, in a situation remarkably similar to the indicator stimulus paradigm used by Libet for timing internal events. In one variant of the "complication experiment," the observer is asked to synchronize his response to the moment when a moving dot crosses a line. After some practice, such synchronization can be done with an accuracy of around 20 ms. The observer can then be instructed not *to* respond when the dot stops before crossing the line. The time needed for the "inhibition of a prepared voluntary act" (Hammer 1914) was found by Flachsbart-Kraft (1930) to be in the region of around 150 ms relative to the anticipated transit of the moving dot, and Woodworth (1938, p. 301) briefly mentions this work. The task used by the old investigators is the same as that used by Libet, and so are the "inhibition times." Ach (1935, p. 115) noted that the "inhibition time" was equivalent to simple visual reaction time, and from this he deduced that the inhibition of voluntary impulses and the time needed for it is a special case of the inhibition exerted by one antagonistic response on another. Thus, the "flashlike counter-command' (Ach 1935) consists in the replacement of one prepared voluntary act by another; there is nothing mysterious or even ethically relevant about the "conscious veto."

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The uncertainty principle in psychology

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The arguments in Libet's target article are based on two time measures: RP onset and awareness of wanting to move (W). These are signs of, respectively, physiological and psychological processes. The reliability and significance of these two measures therefore, require careful consideration.

The RP onset is determined by the appearance of a negative deflection from baseline in the averaged EEG recording. Unfortunately, the stability of the pre-RP baseline is an averaging artifact, since EGG (electrocorticogram) recordings with implanted electrodes in monkeys (Stamm & Gillespie 1980) show continual baseline fluctuations at frequencies of several seconds. Similar pre-event baseline flucutations have been reported during single-trial scalp recordings from human subjects (Bauer & Nimberger 1981; Born, Whipple & Stamm 1982). Furthermore, there is convincing evidence from physiological and psychological investigations that these fluctuations represent changes in neuronal excitability, with surface negativity indicative ofheightened excitability, or cortical arousal. In Libet's experiments, the procedure suggests fairly long intervals between successive acts (report of "half-day sessions"), during which the subject is presumably relaxed and probably bored. It is accordingly conceivable that the subject's mental and behavioral processes tend to start during a period of heightened cortical excitability, that is, during the negative phase of the endogenous baseline fluctuation. This baseline shift would be obscured in the averaged EEG recording because of the considerable variability in RP onset of single events. Even a modest negative bias before RP onset would result in considerably shorter RP durations than those obtained from averaged recordings, which show a very slow rate of increased negativity. This argument can be experimentally examined only with single-trial recordings, which are now technically feasible.

The significance of the W event is difficult to understand, despite Libet's arguments that this is a valid index for timing the subject's awareness. He supports the short W latency with the control experiment of reported awareness of a skin stimulus. Unfortunately, this paper does not present the data for the times between reported and actual application of the stimulus, but an earlier publication (Libet, Wright & Gleason 1982) reports the mean times for six subjects as between —167 ms and +83 ms. This wide range, with some awareness times seemingly preceding the actual stimulus delivery, raise further questions about the timing of the mental processes. Assessments for the durations of mental activity have been obtained with reaction time paradigms that indicate response latencies of several hundred milliseconds for simple reactions and of 1 sec or more when a choice response is required (Bom, Whipple & Stamm 1982). The attentive demands placed on Libet's subjects are severe, with instructions to: relax, gaze at the rapidly sweeping dot on the CRO, avoid eye blinks, monitor both one's internal state (intention) and the external "clock," execute the finger movements, and remember the dot position. While many of these functions are processed in parallel channels, they will certainly interact and prolong each other. Consequendy, Libet's arguments for near simultaneity between the subject's internal state and his report is not well substantiated. The constraints for temporal assessments of internal states may be designated as the analogue of the Heisenberg uncertainty principle in physics, that is, that self-monitoring of an internal process interferes with that process, so that its precise measurement is impossible. According to these considerations, the subject's intention for a finger movement occurs at a considerable time before the W measure.

My arguments for a later onset of the true RP and earlier intent for the act than the times reported by Libet would lead to a reversal of the temporal sequence for these events. Certainly the assignments for these quite fragile measures in terms of unconscious and conscious functions is at best premature and does not contribute to our understanding of mental processes.

Mind before matter?

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Awareness of an intention appears to occur after a physiological activity (the "readiness potential") otherwise associated with preparation for muscular contraction. Libet suggests that we can therefore conclude that the conscious will lags behind a decision to act that is itself physiologically based. We find this idea unacceptable on both conceptual and empirical grounds. In what follows, we shall reply to Libet's arguments by first pointing out the absence of conceptually acceptable hypotheses. We shall also argue that a number of mental operations intervene between a physiological correlate of a mental process and our awareness of experiencing that mental process. In effect, we suggest that Libet and his collaborators have undertaken a test for which there is no other outcome than the one they found.

We assume that it is not possible for a conscious intention to be formulated without any underlying physiological activity; this variety of monism, known as emergent materialism, is implicit in many contemporary cognitive theories. When we need to make assumptions about the relationships between cognitive processes and their underlying physical substrate - when identifying the possible psychological consequences of a deficit such as acquired dyslexia, for example - the physiological data are in agreement with the notion that normal psychological abilities are dependent on the normal operation of a normal brain. Unless the brain is intact, for instance, it will not operate appropriately, and this confirms the position that psychological performance is dependent on physiological competence. This does not imply that psychological experiences in some way *follow* the activity of the brain, but simply that the brain is necessary for psychological processes to be possible.

Our working assumption is that the activity of the brain, which is theoretically observable to a physiologist, is responsible for the experiences of the owner of the brain. Without those physiological activities, the experiences would not be possible. Accordingly, the question arises as to whether Libet and his colleagues *could* have found reports of intentions prior to the observation of the putative "readiness potentials." The answer is a very clear "no" and for the same reasons that music cannot be heard from a gramophone record that is not being operated in a specific way. The record is the physical substrate for the music, and its operation is correlated in time with the generation of the music. If the mind is the product of the physiological activation of the brain, as we shall suppose, then awareness could never precede the observation of such activation. Since becoming aware of an intention is but one of a series of mental processes associated with volitional movement, it is quite likely that awareness will follow after the intention itself. We are distinguishing here between conscious intention and the subsequent awareness of having intended to take some action. In other words, observers' reports can suggest only temporal contiguity, not simultaneity. Given that Libet's experiments could not, in principle, observe awareness of intention prior to the "readiness potential," we can now turn to the question of why they appear to show that physiological processes occur prior to the associated psychological processes.

To determine the time of onset of conscious volition, the experiments use a task that necessarily incorporates delays in the self-reports elicited. It may not be possible to avoid observing delays without using indirect inferential procedures, for self-reports must rely on experiential data collected some time in the psychological past. The task required that participants judge the position of a rapidly moving spot of light as soon as they become aware of having had an intention to move (Libet, Gleason, Wright & Pearl 1983): the problem is that making this judgment requires the use of mental processes that are limited resources (sometimes identified with "consciousness") and are necessary for volitiona1 motor planning.

If limited mental resources are dedicated to the command of motor actions, what is then left to make an accurate judgment about the relative simultaneity between consciousness and a sensory event? In more detail now, the minimum necessary processes are as follows: The participant intends to move a hand, and at some time after this may become aware of having intended to move a hand. Here we have the conscious intention followed by awareness of that intention, and there is no suggestion *of* any unconscious initiation. The distinction between the two processes is the first source of delay in Libet's procedure. Awareness of having had an intention does not necessarily Follow, and with certain overpracticed actions the performer need not be aware of any intention or planning. Examples include tying one's shoelaces when dressing, shifting gears when driving a car, or holding a racquet at the approriate angle when playing tennis. These are cases of automatized actions (Reason 1979; Underwood 1982), in which the presence of an envirDnmental calling pattern is in itself sufficient to initiate an action sequence. These cases are possible when the relationship between environmental conditions and their appropriate actions are invariant and can be overlearned. Lack of awareness cannot be taken as evidence of lack of conscious intention, however, and the two must be seen as separate processes.

After becoming aware of having had an intention the participants must then refocus their attention on a moving spot of light. This is not the same as redirecting their gaze, of course, for it is quite possible to gaze at some point in space while attending to one's thoughts of future motor actions. To return attention from the thoughts to the visual world would require time, however, and this is the second source of delay in Libet's procedure. The extent of the delay attributable to attention switching is something which is not established (Broadbent 1971).

The position of the spot of light must then be judged and remembered. These final two processes are common to both the experimental task and the control task, and so their importance can be neglected here. However, the first two processes are not present in the control task and therefore allow us to dismiss its use: becoming aware of having had an intention ("metavolition"?) and redirecting attention from this thought process to a point in space. If it can be demonstrated that the time required to become aware of having had an intention is of negligible duration, and that the time to switch attention between a cutaneous sensation and. visual space (the control task) is the same as that to switch from a thought to visual space, then the data would be more convincing.

We are also curious to know how Libet is able to distinguish between a volitional intention to act, which is said to be unconscious, and an intention to veto an act, which is said to be conscious. Does a veto require an intention, and why should it differ from other intentions by being conscious?

Nineteenth-century psychology and twentieth-century electrophysiology do not mix

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Libet has summarized a curious research program aimed at the identification of the time of occurrence of a conscious mental process that leads to the generation of a voluntary motor act. At the heart of this program is the assumption that people are directly aware (by introspection) of some sort of endogenous brain process that controls voluntary movement. This is essential if consciousness is to have the regulatory role that Libet's hypothesis proposes. However, the assumption appears to be incorrect. Laszlo (1966) has shown that following compression of the upper arm with an inflatable cuff, active kmesthesis disappears well before a loss of motor ability. Subjects are able to squeeze something or tap their fingers even though they deny that any movement is occurring and refuse to believe that the anesthetized hand is moving until they are permitted to verify this visually. This demonstrates that humans have no direct awareness of the brain processes that generate hand movement. Not only the initiation but the entire process of generating a movement is unavailable to introspection. One knows that one's hand has moved only as a result of kinesthetic feedback from it. Further support for this conclusion can be found in a report by Melzack and Bromage (1973) that a feeling of being able to produce voluntary movement in a phantom limb (produced experimentally by injection of a local anesthetic into the region of the brachial plexus) is completely dependent on the preservation of residual electromyogram (EMG) activity (and, presumably, feedback from it) in the affected limb.

These results raise an interesting question. If people are aware of their own voluntary movement only as a result of sensory feedback, how can Libet's subjects tell that they "want" to move nearly 200 ms prior to the onset of recorded EMG activity? This apparent problem may be due to nothing more than a failure of Libet's recording technique to detect peripheral neuromuscular changes that herald the onset of a voluntary movement. It is well known that there are changes in the excitability of monosynaptic spinal reflexes in humans well before a voluntary movement (e.g., Papakostopoulos & Cooper 1973) and early researches by Jacobson (1930a; 1930b) and others demonstrated that low-amplitude EMG bursts in the relevant muscles accompany imagining a movement or thinking about it. Thus, it is probable that when Libet's subjects detect that a movement is imminent they are reacting to a peripheral sensory event, that is, to changes in their muscles, rather than to an endogenous mental or brain event.

The foregoing results are consistent with the general conclusion that humans have little or no direct awareness of the central processes that cause their own behavior. Libet appears to be unaware of the history of attempts to investigate the mind by introspection. This was a serious scientific endeavor in the period of (approximately) 1880-1910. As a result of this work it became apparent that "mental" processes are generally not open to direct examination. We are aware of physical events in the external world and those inside our own bodies and of very little else. Such knowledge as we do have of the causes of our own behavior is the result of inference rather than direct awareness (Hebb 1980; Skinner 1974).

Libet's conceptual approach to his work is an excellent illustration of the low level of communication that generally exists between the behavioral sciences and mainstream neuroscience. The greatest advances in behavioral research in this century have been made by the Lorenz-Tinbergen school in Europe and the operant conditioning school, which has been associated particularly with B. F. Skinner in America. Both schools have found it advantageous to abandon the introspective mentalistic approach to behavior that has been an integral part of Western philosophy for centuries. Leading cognitive psychologists have also recognized that mental activity is largely unavailable to introspection (e.g., Pylyshyn 1973; Nisbett & Wilson 1977). Unfortunately, most neuroscientists are unaware of the conceptual advances made in behavioral work or else do not understand how to apply these insights to their own work. Consequendy they tend to accept the philosophical and psychological hypotheses of the seventeenth to nineteenth centuries as self-evident truths. As a result of this weak conceptual basis, attempts to relate brain electrophysiology to mental processes have generally been unsuccessful. However, if one attempts to relate brain electrophysiology to *behavior,* there may be a greater prospect of success, as I have attempted to show in previous papers (Viinderwolf & Robinson 1981; Vanderwolf, 1983a; 1983b).

Conscious wants and self-awareness

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Professor Libet's ingenious experimental procedures provide remarkably detailed information about the temporal structure of cerebral events preceding voluntary movement. However, clarifying the notion of a conscious mental state may help in assessing the relevance of his data to issues concerning the role of consciousness in the production of voluntary action.

Libet takes his data to show (or at least to suggest strongly) that the initiating causes of voluntary movement (readiness potentials, or RPs) are not conscious mental states. But the notion of a conscious mental state is ambiguous in a relevant and theoretically important way. A *conscious mental state* may be either *a mental state of which one is conscious* (i.e., a mental state that is an object of self-awareness) or *a state of being conscious of some mental state* (i.e., a state of self-awareness which has a mental state as its object). It is the latter notion that Libet seems to have in mind. For in his view, it is only the state (W) that occurs 300 to 400 ms after the onset of the RP state that counts as a conscious mental state.

However, the initiating (RP) state may well be a conscious mental state in the former sense, and there is some basis for holding that it is the first sense that is relevant to the case at hand. When we speak of a conscious want, urge, or intention we normally mean a *want of which we are conscious* or aware, whereas in both psychoanalytic and everyday parlance, an unconscious want or desire is one of which we are not conscious or of which we have no awareness. Urges, wants, and desires are not likely to be conscious states in our second sense (i.e., states of self-awareness) since they are not normally states of awareness at all. They are motivational states, which should not be confused with the awareness we may have of them.

Thus if what Libet's subject becomes aware of when he becomes aware of his intending or wanting to move (W) is just the causally initiating RP state, then that RP state will count as a conscious want or intention in our first sense, since it is a mental state of which the subject is self-aware.

However, given the time delay between the onset of RP and the onset of W, it might seem that the RP is not a conscious want at the time when it initiates the causal production of a movement but only becomes one 300 to 400 ms later. The significance of this time delay depends on further causal and temporal facts about the brain. If consciousness is a sort of self-monitoring or self-scanning process, there will always be some time lag between the onset of a cerebral state and awareness of that state. Libet indeed gives some indirect support for such a view when he notes that 200 ms of stimulation is required to produce subjective awareness of a cerebral event. If no mental state ever becomes a conscious state (i. e., an object of self-awareness) until several hundred milliseconds after its onset, then RP states would be no less conscious states than any other mental states.

Restated in light of our distinction, Libet's claim is still an interesting and important one. His data appear to show that under his experimental conditions the event initiating a voluntary movement is not a state of self-awareness. However, that result may still be compatible with RPs being conscious wants or intentions in every respect in which wants or intentions are ever conscious (i.e., they become wants or intentions of which we are conscious in as short an interval as the self-scanning process of the brain allows).

Neural/mental chronometry and chronotheology

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Given enough commitment to a cause, anyone can fail to take account of otherwise obvious basic principles. Such is the case for Libet, whose target article overlooks fundamental measurement concepts and also pays no attention to relevant empirical findings of psychology. Libet is thereby led to make two egregious errors:

1. He fails to distinguish between a measuring operation and the thing being measured; these do not have to be coincident or synchronous. A. classic example would be the determination of a star's velocity by measuring its red shift. The spectral measuring operation and the star being measured are separated by vast amounts of space and time. This general metrical caveat applies equally well to the brain/mind problem. It specifically applies to Libet's attempt to use objective phenomena (spots of light, skin shocks, and electrophysiological potentials) to measure neural/mental timing. Such an attempt cannot succeed without establishing the temporal relations between these objective phenomena and the neural/mental activities they purport to measure.

This caution would apply even if no one had ever done any empirical research on the brain/mind. But of course such research has been done, and it has taught that bioelectric signals take time to propagate through the brain. It has similarly taught that mental propagation takes time as well. Indeed, the elementary fact that a sensation is delayed with respect to the stimulus that evoked it has been known for centuries. Elsewhere, I have noted the ancient origins of this idea in the work of Ptolemy and Francis Bacon (Wasserman 1978).

2. Libet also fails to consider that information does more than just propagate between the environment and the brain/mind. Information also has to be processed. There is no reason to assume that complex neural/mental information-processing operations do not take time. Instead there is a copious literature that indicates that information processing does take time. Elsewhere, a review of some of that literature has been given (Wassermm & Kong 1979). The work cited in that review shows that mental chronometry is a serious discipline. This is a literature with which Libet appears to be unfamiliar. It also suggests a different interpretation of Libet's findings that can be brought out by a careful examination of the details of Libet's own experiments.

A subject voluntarily chooses to move, and his movement is measured by means of the EMG (electromyogram). Some hundreds of milliseconds before the EMG-defined movement appears, a set of externally measurable readiness potentials (RPs) appears on the scalp. So far, so good. The RP and EMG are both objectively measurable with adequate precision. But the subject also reports when he became aware of his intention to move by observing a rotating spot and reporting as a clock coordinate: the position the spot was in when the intention began. This clock position is converted into objective time measurements designated as W. Here is where the trouble begins: The time when the external objective spot occupies a given clock position can be determined easily, but this is not the desired result. What is needed is the time of occurrence of the internal brain/mind representation of the spot. Libet does not recognize this problem and concludes that subjects begin to make voluntary movements without being aware of what they are doing. The quantity RP - W is offered as a measure of the interval of "unconscious initiative"; it is claimed to be more than 300 ms.

It is easy to show, however, that RP — W cannot be accepted as a valid measuring tool. Metrical principles permit the possibility of a delay (D) between the neural/mental representation of the spot-clock relative to the objective spot-clock. And ample data exist to show that D is not zero. Hence RP - W must be in error by an amount equal to D that would increase the correct value. This error might be discounted because accounting for it would only make Libet's claimed effect larger. But that would be too narrow an approach; a proper approach would recognize that this particular error is merely one undeniable exemplar of a class of metrical problems that afflict Libet's argument.

Further examination reveals these other problems: RP — W + D would be a fair measure only if it took zero time to process the neural/mental representation of the display in order to determine the position of the spot. The underlying assumption that produced this crucial proposition can be readily demonstrated by critically examining the asymmetry in Libet's treatment of the sensory and motor parts of his experiment:

Consider how Libet views the motor task that requires the subject to flick his fingers or hand. This is about as easy a motor task as one could imagine; no specific flick is demanded as long as the flick exceeds a minimum amount. (Note that use of the EMG removes any ballistic delays due to limb inertia. The quantity RP - EMG is a measure only of the difference in time of two bioelectric potentials, one in the arm and the other in the head.) The quantity RP — EMG estimates the minimum time required lor the neural/mental processing of the motor flick; this minimum quantity comes out at about 500 ms. Most of this delay is not due to simple axonal conduction delays. There is no reason to be surprised that processing a simple motor task takes this much time; there are many comparable results.

But there is also a sensory task to be done. This sensory analysis must be initiated by the same voluntary initiative that initiates the motor task. Logically, there is no requirement for both tasks to start together even though the instructions would seem to call for joint onset. But whether they do or do not start simultaneously can be determined only by research, not by assumptions. What we do know is that the sensory task is at least as complicated as the motor task; the subject must analyse the information in the clock-spot representation and decide where the spot is. Is it possible that the senory analysis takes 0 ms while the motor programming takes 500 ms? The greatest problem of this line of work is that no attempt was made by Libet to consider the problem of this analysis time. This failure is central, for if the clock-spot processing time were only a few hundred milliseconds, then the effect claimed by Libet would vanish.

Libet does offer a putative control in the form of a separate sensory experiment in which the subject relates the visual clock-spot (W) to the somesthetic sensation evoked by an electrical stimulus delivered to the skin (S). But this experiment is no control at all; it is afflicted by the same metrical problems that affect the main experiment. For the external S and W are not of interest. Rather, it is their internal representations that matter. Both S and W have to propagate into the brain/mind, so both will have delayed representations. How likely is it that both will be subject to identical propagation delays? Furthermore, the representations of both S and W have to be processed. Is it likely that both will take the same amount of time to be processed? Finally, the processing of W in the control experiment takes place under conditions different from those of the processing of W in the main experiment. Is it likely that the W-processing time is the same in both experiments? These questions are not addressed by Libet. Instead, all we have is the fact that S can be computed (from the grand averages for W and for W - S) to differ by 47 ms from W. This just means that some differential delay exists. In order for the two stimuli to seem to be simultaneous, one has to precede the other by 47 ms to overcome the differential delay. But the putative control experiment does not give any basis for using this simple objective measurement to determine absolutely any of the multiple internal delays described above.

Libet's research has provided several exemplars of the metrical problems that affect neural/mental chronometry. The shock-spot sensory experiment and the finger-flick motor experiment both confirm the existence of neural/mental delays. The actual experiments themselves are not original. It is only Libet's interpretation of these commonplace data that is striking, and this interpretation founders when its basis is examined.

Pardon, your dualism is showing

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Libet's intriguing experiments on electrical stimulation of human cortex and their implications for the mind-body problem (Libet 1966; 1973; Libet et al. 1979) have provoked considerable controversy (e.g., Churchland, 1981a, 1981b; Popper & Eccles 1977; Libet 1981a), and the target article promises to continue in that tradition. Perhaps more than any other investigator, Libet has ingeniously combined subjective and objective variables in his experiments in a way that consistendy rubs our noses in one aspect or another of the mind-body problem.

Other commentators will no doubt wish to quibble with aspects of the experimental procedures, data analysis, the possible role of "prior *'entry* effects" and other judgment biases, the distinction between type I and type II RPs, and the magnitude of the within- and between-subject variability (see Tables 1 and 2 in Libet et al. 1983). Instead, I will accept for purposes of discussion Libet's major finding that RPs begin 350-400 ms before subjects report the "initial awareness of intending or wanting to move *(W)"* in order to concentrate upon his fundamental assumption that W judgments must either precede or coincide with RPs in order for conscious intention to initiate voluntary movements.

My title is intended as gende encouragement for Libet to make explicit his tacit assumptions regarding conscious experience and the mind-body problem because I believe they have caused him to overlook alternative explanations that pose no difficulty for the concept of conscious initiation of voluntary movements.[1] In my opinion, Libet's fundamental assumption about the temporal relationship that should exist between RPs and W judgments is decidedly (substance) dualist in character. He assumes: "If a conscious intention or decision to act actually initiates a voluntary event, then the subjective experience of this intention should precede or at least coincide with the onset of the specific cerebral processes that mediate the act." I characterize this assumption as dualist because it makes sense only if one believes that "conscious intention" is not mediated by a physical process or processes in the brain but by something else. That is, it makes sense only if we assume that conscious intention to move is not part of "the specific cerebral processes that mediate the act."

In contrast, if we assume that conscious intention is one of the many brain processes that contribute to the initiation of a voluntary movement, then the explanation for the obtained results is straightforward. According to this view, the brain process(s) that mediate the conscious intention to act *must* begin before subjects can report that they are aware of that intention. To assume otherwise is to assume that conscious intention arises full-blown, out of nothing, instantaneously ("the Devil made me do it"), a prospect that is decidedly dualistic. Unless conscious experience is totally unlike every physical process we know anything about, it must have a nonzero time course; if its time course is anything [ike that of other brain processes, then tens or hundreds of milliseconds is certainly reasonable.

If it seems strange to suggest that some of the neural events that contribute to conscious experience should be detectable before the completion of the process(es) that mediate that experience, consider the same question applied to a multiuser computer operating system that allocates computer resources as a function of the number and priority of competing tasks (I am not, of course, suggesting that conscious experience is analogous to an operating system in any deep sense; I simply assume that both are complex processes exhibited by suitably organized physical systems). An important element in such an operating system is a scheduling routine that intermittendy examines the competing tasks and the available resources in order to determine which tasks will receive which resources next. The key point is that the scheduler is itself a program that takes time to execute. Consequently, there is a time period during which the scheduler is executing but is not yet complete so that no scheduling "decision" has yet been made (on the scheduler's current pass). In a similar manner, if conscious intention is mediated by a physical process in the brain, then the neural events that mediate subjects' conscious intention to act must necessarily begin before subjects become consciously aware of them. We have little difficulty dealing with the fact that it takes time to become aware of external sensory stimuli (the current S judgments notwithstanding).[2] Indeed, Libet's own cortical stimulation studies (Libet et al. 1979) emphasize just how much time may be necessary. It is therefore surprising that Libet has difficulty with the possibility that similar time intervals would be required to become aware of internal states such as those upon which W judgments are based.

There is an alternative reading of Libet's assumption that W judgments should cpncide with or precede RP onset that does not commit him to such a strong dualist position but that is inconsistent with other parts of the target article as well as with other RP data. He might be assuming that conscious intention is indeed a physical brain process but that RPs reflect exclusively motor activity and hence must be preceded by subjects' W judgments. The difficulties with this interpretation are: (a) RPs as defined and measured by Libet are generally interpreted as reflecting various preparatory processes assumed to occur in advance of actual motor activity, which is thought to be reflected in scalp recordings only in the iast 50-100 ms before EMG onset (for review, see Deecke et al. 1984); (b) the neural generators of RPs and other premovement potentials have not been fully determined, although other structures as well as primary motor cortex appear to be involved (e.g., Arezzo & Vaughan 1975; Gemba et al. 1980; Hashimoto et al. 1980); and (c) even if RPs exclusively reflected motor activity, Libet would need to explain why motor-related activity would be evident in scalp recordings and preceding activity associated with the intention to move would not.

The possibility that at least some of the activity that contributes to RPs preceding voluntary movements may be generated by neurons that contribute to conscious intention raises interesting suggestions concerning the functional role of the neural system(s) that generate RPs (e.g., Deecke et al. 1976; Popper & Eccles 1977). However, as Libet correctly notes, the onset of RPs should be interpreted only as an indicator of the "minimal onset times for cerebral processes that initiate the voluntary act" since even earlier activity could be present and not evident in scalp recordings. This is because RPs and other surface electrical potentials are aggregate, incomplete measures of the neural events occurring at a particular time (see Vaughan 1982; Wood & Allison 1981). The neurons that generate RPs

I have tried to suggest how the obtained temporal relationship between RP onset and W judgments can be explained without resorting either to a nonphysical basis for conscious experience (i. e., substance dualism) or to Libet's conclusion that all so-called voluntary actions are "unconsciously initiated." Neither of these (to me) undesirable conclusions is required if conscious experience (both of external stimuli and internal states) is mediated by physical processes in the brain that take time to operate. According to this hypothesis, Libet's conclusion regarding unconscious initiative is correct only in the restricted sense that components of the neural system that mediates conscious experience cannot themselves mediate that experience in the same way that components of an operating system's scheduler cannot themselves mediate scheduling. Thus, although the components of the system that mediates conscious experience are themselves unconscious, this does not mean that conscious intention must be limited to a subsequent "veto" role over "unconscious initiative," as Libet suggests.[3] Self-reference and part-whole relationships are among the reasons why conscious experience is the perplexing philosophical and scientific question that it is. There are plenty of reasons to be concerned about the role of unconscious processes in cognition and behavior (e.g., Dennett 1978; Fodor 1983; Freud 1925), but the possibility that RPs precede W judgments should not be among them.

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NOTES

1. Lest I be guilty of hiding my own assumptions and biases, I briefly summarize them here. So little is known about the properties and mechanisms of conscious experience that adopting any position is risky business. Nevertheless, I believe that it is more reasonable as a provisional hypothesis to assume that conscious experience is an as-yet-unknown property or capacity of a suitably organized physical system (i.e., one that obeys the laws of physics as we know them) than it is to assume that it involves some substance or phenomenon that lies outside the confines of physical law. Because this is a working hypothesis, I am eager to entertain logical arguments that it is incoherent, empirical evidence that it is incorrect, or data/theories suggesting that the relevant physical laws are seriously flawed - in this respect, I'm from Missouri.

2. That S judgments preceded the sensory stimulus by approximately 50 ms illustrates the type of "prior entry effects" and other judgment biases that can occur even in temporal-order tasks much simpler than those employed here (see Sternberg & Knoll 1973). As Libet notes, however, the direction of the bias in the S condition is opposite to that required to explain away the fact that RPs preceded W judgments (assuming that similar judgment biases occur in the S and W tasks), and the error in the M condition is similar to that reported by McCloskey et al. (1983).

3. Here again the scheduler analogy can be helpful. At the level of the individual instructions of the scheduling routine, the scheduler is a fixed, deterministic process. However, at the level of the scheduler as a whole and its interaction with the remainder of the system, the outcome of each execution of the scheduler is not fixed or deterministic because it depends on the competing tasks and available resources at the time (i.e., on the environment in which it executes). For additional discussion of how rigid, "dumb" processes at one level can underlie what appear to be flexible, "smart" processes at another, see Hofstadter (1979) and Dennett (1984).

Author's Response

Theory and evidence relating cerebral processes to conscious will

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Not unexpectedly, the commentators have raised a number of controversial issues. These center on the validity and meaning of the experimental observations and on alternative interpretations of their implications. Commentators had many different kinds of arguments to make on the same general issue. This made it difficult not only to draw' together all comments on a related issue but also to cite every relevant commentary; I hope I will be forgiven for any such omissions of citation. I thank the commentators for their conscientious efforts and am gratified that many of them find merit in our experimental questions, design, and observations, even when they do not fully agree with my conclusions and proposals about volitional processes.

Validity and meaning of the experimental observations

1.1. Time of conscious intention (W). Our experimental values for W were subjected to a variety of critical comments regarding validity, reliability, and quantitative significance. (Many of these criticisms were already anticipated and discussed in the target article [TA] section

2.4.) Much of the criticism appears to reflect differences in experimental approaches to investigating conscious events. My own approach, perhaps influenced by my being a physiologist, has been to accept direct observations (in this case reported times of awareness) as primary evidence; the meaning of such evidence should not be altered unless it is necessitated by other directly relevant observations. A number of the psychologists among the commentators appear to bring with them outlooks conditioned either by behaviorist methodology and philosophy (overt and covert) or by a history of attempts to conceptualize conscious perceptual and volitional processes that are based on observations not directly relevant to the issue of introspective awareness and its timing.

The distinction between a subjective experience (which is only introspectively accessible to the individual), and some externally observable physical or "behavioral" event still seems to elude some commentators. The sterility and irrelevance of behavioristic studies for the mind-brain issue have been recognized even by many former traditional behaviorists (including the late David Krech, 1969, as expressed in his William James lecture of 1967 before the American Psychological Association). **Vanderwolf** adheres to a "classical" behaviorism. His insistence that mental activity is largely unavailable to introspection and that we are only aware of physical (i.e., sensory) events is unrealistic; he would appear to be denying that he is aware of his own thoughts. He argues that reports of W [time of awareness of intention (wanting)] appearing before muscle activation must really be due to detection of sensory signals from unrecorded premovements of muscles. This ad hoc construction is required by his philosophy but is without any experimental basis; our EMG (electromyogram) recording was sensitive enough to pick up single motor unit potentials. Vanderwolf's citations on volition and kinesthesia are misleading: Laszlo (1966) reported that speed of key tapping but *not* volitional power was affected by kinesthetic loss; Melzack and Bromage (1973) dealt with the feeling of actually being able to move a "phantom" (not normal) limb; they in no way indicated that subjects were unable to generate a conscious intention or wish to move.

The possibility of discrepancy between actual and reported times for the subject's awareness of wanting to move was discussed in TA 2.4, but this possibility was raised in different ways by a number of commentators as still providing a serious challenge to my use of reported W values in establishing the relationship of conscious intention to the initiation of the voluntary act.

Some commentators **(Latto, Marks, Ringo)** propose that awareness (of the urge to move) must arise in a graded manner and reach some peak or "threshold" before the subject can or will report it. If this is correct, some degree of awareness would actually be present before reported W. It would accordingly reduce or eliminate the temporal difference between RP onset and conscious intention. The experimental basis for this view appears to He in signal-detection studies, from which Marks argues that subjects would set a high criterion in selecting a threshold level of awareness to be reported. Even if such a detection theory were applicable here, there is no basis for assuming that our subjects wanted to avoid false alarms" and thus waited for a "strong signal." There was no test for the correctness of their report; any W report time was completely acceptable. Hence there was no reason for reluctance to report *any* awareness. But, more fundamentally, signal-detection studies are based on forced choices; their results are not directly applicable to studies of awareness processes (see Libet 1979; 1981a; 1981b). In our present study, subjects were asked to associate "clock time" with their earliest awareness of the urge to move. They did not report being aware of any preceding graded intention or urge (except for the different and separate awareness of preplanning when that occurred with type I RPs; see Libet et al., 1982). When a subject reports that he feels or is aware of absolutely nothing, whether as here in the period before W or in experiments with stimulation of sensory cortex (e.g., Libet 1973; 1982), I regard it as a distortion of the primary evidence for an investigator to insist, on the basis of a (possibly misapplied) theory, that the subject really was aware of something.

Some commentators propose that there are various other cerebral time factors that could make the reported time of conscious intention (W) significantly different from its actual time **(Breitmeyer, Latto, Rollman, Stamm, Underwood, Wasserman**). The difficulty centers chiefly on the potential for delays in becoming visually aware of the position of the revolving CRO (cathode ray oscilloscope) spot (clock time). Such delays could affect the temporal relationship between the reported clock time (W) and the actual introspective awareness of wanting to move. That there is probably a substantial delay (in hundreds of milliseconds) for becoming aware of a sensory stimulus would indeed follow from our own earlier direct experimental studies of this issue (Libet 1965; 1966; 1973; 1981a, 1982). The existence of such a delay is therefore conceded, even though the reasons for delay offered by most commentators were either speculative or based on irrelevant data. (For example, long reaction times [RTs] are cited by Rollman and by Stamm as evidence for lengthy "mental processing." But RTs do not measure or depend on awareness and cannot be used as primary indicators of when sensory awareness is achieved -e.g., Libet 1973; 1981a. Incidentally, I was not unaware of Wasserman's views about mental chronometry; see Libet 1979.)

One should not confuse *what* is reported by the subject with *when* he may become introspectively aware of what he is reporting. As described in TA 2.4.2(b), our earlier studies provided direct evidence for this distinction and for a subjective referral backward in time. The latter automatically "corrects" one's conscious perception to coincide with the real time of the stimulus (Libet et al. 1979; Libet 1981a; 1982). This can explain, for example, why a runner in a race can take off within 50-100 ms after the starting gun, presumably well before he becomes introspectively aware of the stimulus, but later reports that he heard the gun *before* taking off. Now, although it may take substantial time for cerebral processes to develop the introspective awareness of the urge to move, as I indeed postulate, there is no basis for expecting a subjective backward referral of its perceived timing. Backward referral has only been found in the timing of an external sensory stimulus, and even then it specifically requires the primary cortical response to the fast sensory projection pathway for its mediation (see TA 2.4.2).

Latto and **Rollman** assume that the appearance of conscious intention must coincide with the *delayed* awareness of visual clock spot position for both to be regarded as simultaneous by the subject. The backward referral for the visual spot would then lead to an incorrect (earlier) report of clock time for conscious intention. But one should recognize that the subject was required only to associate conscious intention with a visual signal (the position of the revolving spot) whose *content* he would report some seconds after the event. He did not have to be concurrently *aware* of the visual signal in order to associate it correctly with conscious intention; this associated visual position was recalled later, after awareness of it. This is analogous to making fairly correct subjective observations and appropriate associations with respect to diverse sensory stimuli and endogenous experiences in general, even when cortical delays in actual awareness may differ (see Libet et al. 1979; Libet 1981a). The more appropriate inference, from the existence of variable though substantial cerebral delays in awareness of sensory stimuli, is that sensory signals can be meaningfully identified well before introspective awareness of them develops (Libet 1978; Libet 1981a, 1982).

The validity of our skin-stimulus experiment as a control for error in reporting simultaneous events was considered in TA 2.4.1 but was further called into question by some commentators (**Breitmeyer, Stamm, Underwood**). Stamm cites the broad range of our observed skin timings for all subjects (—167 to +83 ms) as a significant uncertainty in timing such mental processes. But data in our original report (Libet, Gleason, Wright & Pearl 1983) showed that the mean timing for each individual subject consistently exhibited small standard errors (SEs) (not far from 20 ms in all cases) and was thus characteristic for each. When each subject's characteristic timing of skin stimuli was subtracted from his reported W times for conscious intention, the "net" W times still followed the onset of RP in the same subject by intervals close to those obtained for the grand average of W times. That is, the actual errors in timing skin stimuli, regardless of the individual, did not appreciably affect die crucial difference between RP onset and W times. It should also be noted that the reliability of W reports, which worried some commentators (e.g., Latto) was very good. SE values for each series of 40 trials were typically not far from 20 ms and had no statistical impact on the significance of mean W values.

Suggestions concerning hypothetical differences between times needed for attention to a skin stimulus compared to W (**Breitmeyer, Stamm, Underwood & Neimi**) do not seem to be applicable to our studies. Our subjects were asked to attend continuously to the revolving clock spot and to wait for the appearance of the conscious urge to move (in the W series) or of the conscious sensory experience (in the skin-stimulus series, S). There is no operational reason to believe there was a significant time difference between attentional factors in these two associations, W with spot versus S with spot.

In any case, having subjects associate awareness of a skin stimulus (instead of an urge to move) with the clock position of the revolving spot provided the best available control experiment for assessing the error under the specific conditions used when obtaining reports of W times. The measured timing errors with skin stimuli were not large enough to affect the significance of W timings (relative either to RP [readiness potential] onset or EMG onset), and there is presently no definitive experimental basis for believing that the error in associating conscious intention with clock position would be so much larger as to affect the significance of W. Even if the potentially relevant but speculative errors in W timings proposed by commentators were valid, they would probably not be large enough to affect the significance of the RP-W-EMG temporal relationship.

Regarding unreported awareness, Scheerer suggests that an additional component of introspective intention may precede the one reported by our subjects. This is attributed to William James (an "anticipating image") and N. Ach ("intentional sensations"). There is no basis for believing that such a hypothetical component was "missed" by our subjects (Ss). They already had a good image of the anticipated act well before each trial. The free volitional feature was purely one of choosing *when* to act. Also, they were asked to report any introspective feelings that might have preceded the reported earliest awareness of W. The only additional reported awareness was the one for preplanning to act some time within the next few seconds (associated only with some series and a different, "type I" RP; Libet et al., 1982). Ss consistently distinguished this more occasional preplanning awareness from the consistent conscious urge immediately associated with each act.

The possibility that some conscious awareness might develop earlier than W, but without any associated memory processes, and hence without being recallable (Jasper), does present a problem, but was not experimentally testable. Examples of "automatic" complex behavior that is inaccessible to recall (such as that during certain epileptic seizures) could be regarded as unconscious manifestations rather than as actions associated with conscious awareness without memory, just as many actions and reactions of normal people appear to be accomplished unconsciously with no specific awareness of them. In any case, as I noted in TA 2.4.4, our subjects did not actually have to recall *any* awareness to make a W report; they only had to be able to associate the clock position of the revolving spot with the first awareness of an urge to move, and later to recall and report that associated spatial image. As indicated above, it would not be necessary to be immediately aware of the associated visual signal in order to recall its appropriate content later.

1.2. What does the recorded RP represent? The spontaneity of the voluntary acts under study was questioned direcdy by **Naatanen** and indirectly by **Ringo**. I would reiterate that each trial was conducted as a separate event, at the subject's convenience, with no set intertrial interval, and that in those series associated with a type II RP, subjects reported experiencing full spontaneity with no preplanning in every trial (Libet et al. 1982). Our distinction between type I and II RPs, associated with the presence and absence respectively of preplanning experiences, has more recently been confirmed by Goldberg, Kwan, Borret, and Murphy (1984).

Latto suggests that the subjects' reports of *feeling* they are acting voluntarily may represent a compliance with what is expected rather than an endogenous process. Even if, as in Latto's hypothetical experiment, a cerebral stimulus site could be found to produce a movement associated with a feeling of volition, it does not follow that the subject would, as Latto predicts, report not acting voluntarily when shown that his movement was instigated by the stimulus. Based on our own extensive experience with subjects reporting conscious sensory responses to cerebral stimuli (Libet 1973), I am certain that the subjects would still report what they *felt* in Latto's hypothetical case, namely, a feeling of wanting to move, even though they would also recognize that the actual instigator was external. There is no necessary conflict for the subject when he reports *a feeling* whose nature he can distinguish from what he observes externally as a physical occurrence; the subject knows what his own experience was and is always encouraged to describe that.

Several commentators (**Eccles**, **Ringo, Stamm)** propose that the averaged RP, as recorded over 40 events, is actually masking spontaneous or random fluctuations in slow pre-event potentials and that the onset of the meaningful RP is later and perhaps coincides with the time of conscious intention. In such a case there would be no reason to conclude that the cerebral processes initiating the voluntary act precede the appearance of conscious intention. (This issue was already considered in part in TA 2.5).

a. Regardless of the circumstances under which any prepotentials arise, they must be contributing a regular component to the recorded average RP. If they were so irregular and random as not to contribute to the RP whose onset is measured by us, they would have been canceled out by the averaging.

b. No change in contour or components of the averaged RP appears at the reported time of conscious intention (W), that is, at about 150 to 200 ms before muscle action (EMG), whether the RPs represent spontaneous voluntary acts (Libet et al. 1982) or self-paced ones (Deecke et al. 1976). There is therefore no electrophysiological evidence for a distinction between early, random fluctuations (that lead to or allow the appearance of the actual initiating process) and a late potential associated with conscious intention and actual "decision" to initiate the act. Note also that "motor potentials" associated with the development of final outflow from contralateral motor cortex appear well after W time (see Deecke et al. 1976; Shibasaki et al. 1980). They do not account for the main negative rise even in the type II RP that at W time (—200 ms) is still maximal at the vertex (near the supplementary motor area).

c. The vertex recorded RP is associated only with preparation for actual movement, not with processes of attention, expectancy, or any other possible fluctuations that may play some role in volition (e.g., Libet et al. 1982; Libet, Wright & Gleason 1983). There is no basis for regarding the early RP as nonspecific, as **Rugg** suggests. The whole RP, whose onset provides the basis for our thesis, is directly related to an impending voluntary motor act. That is, if the recorded averaged RP reflects individually variable and random fluctuations, each of which develops into a full volitional act, such fluctuations would have to originate in the motor preparatory structure(s) responsible for the whole recorded RP. If other processes or fluctuations do precede and help initiate the recorded RP, their bioelectric counterparts are not appreciably recordable at the vertex, at least under our experimental conditions. (Incidentally, the slow component of the contingent negative variation may be in a special subgroup of RPs--see Libet, Wright, & Gleason 1983--rather than the converse, as proposed by **Latto.**

d. Even if (despite the foregoing) one were to assume that the early portion of the averaged RP does represent spontaneous or random rises in cerebral "excitability," the putatively more definitive and intentional initiating process would have to await the development of each such excitability rise to a level that permits the initiating process to proceed (also argued by **Rugg).** Such a mechanism would still impose a limitation on *when* a specific voluntary initiating process could arise. The conscious function could not then itself decide when to move; it could only select which adequate but randomly appearing fluctuation to proceed with. The conscious initiating process would become the "trigger" that follows an unconscious though nonspecific preprocess. As **Eccles** puts it, conscious intention would itself be *timed* unconsciously. Such a view would differ from my proposal for conscious control only in regarding the preprocesses as not constituting specific initiators of the act.

Ringo suggests an experimental test that he believes may establish whether the type II RP (recorded with spontaneous voluntary acts) represents some pseudorandom fluctuation which, when it crosses some threshold level, leads to a conscious initiation of the movement. He proposes that the subject be "asked to choose (and later report) a clock-position on a pseudorandom spontaneous basis." Although such a perceptual choosing "action" might indeed be a spontaneous endogenous event, it cannot be regarded as the equivalent of an urge to move, since no motor act is expected or contemplated. On the evidence that RPs are associated only with preparation or intention to move (Libet, Wright & Gleason, 1983), one would expect no similar pre-event potential with the choosing of a visual signal. On the other hand, the evidence already indicates that RPs can appear even when the intention to act is not consummated in an actual motor event (as in our "veto" experiment). That in itself would not settle the issue of whether the RP represents "nonspecific" fluctuations that may lead to conscious initiation or a "specific" initiating process that precedes appearance of conscious intention. Other considerations are required, as discussed previously.

Rugg argues that my thesis requires that there be a "necessary" relationship between a voluntary action and the RP processes that precede the act. As indicated in TA 2.5, the RP need only reflect the fact that some cerebral process consistently begins well before W times; this would be true even if it should turn out that the RP process is only directly related to the volitional preparatory process and could be dispensed with without losing the potential for voluntary action. The consistent and regular onset of RP before the voluntary acts studied must mean that RP processes, regardless of how they are causally related to the voluntary act, have been set into motion in relation to the impending act. Surely Rugg is not suggesting that the RPs were simply chance occurrences unrelated to impending voluntary acts.

Merikle & Cheesman argue that one must demonstrate that RPs precede behavioral acts even without conscious intention in order to accept our thesis that RP onset before W signifies an unconscious motor initiating process. But the whole RP has in fact been shown to appear only in association with a preparation to move. As indicated above, to show that relevant cerebral processes start before W, RP processes need not themselves be in the direct cerebral path leading to the voluntary act. On the other hand, evidence does suggest that the RP processes (probably occurring in the supplementary motor area) are directly involved in preparations to act. This, together with our present evidence that RP onset substantially precedes conscious intention, does suggest that RP provides a significant physiological indicator of those unconscious behavioral actions that involve preparation (in contrast to motor reactions to unsignaled stimuli); but, contrary to Merikle & Cheesman's view, such additional studies are not crucial to my present thesis about RP and conscious intention.

On the other hand, **Van Gulick** proposes that RP processes which precede W should not be regarded as unconscious but rather as a state of conscious intention, of which the subject later becomes "self-aware" (at W time). This appears to impose a semantic play of words on the actual findings. Since there is no operational manifestation of any awareness of intention until W, one should at most refer to the preceding RP processes as developers, but not direct representations, of a state of conscious intention.

1.3. How is **our experimental act related to "normal" voluntary action?** There are several concerns about the significance of the act we studied, a spontaneously initiated quick flexion of fingers or wrist, in relation to voluntary actions in general **(Breitmeyer, Bridgeman, Danto, Jung, Latto).** We wanted our measurements of relative *timings* (for the onsets of RP and W) to be quantitative and operationally definable, without reliance on intuitive impressions or speculations. Such an objective is much more difficult, if not impossible, to achieve with any of the more common voluntary actions recommended by the commentators for study. Even in our paradigm, the not infrequent appearance of an experience of preplanning within some seconds before the act had to be (and fortunately could be) clearly distinguished from the experience of the conscious intention that more immediately preceded each spontaneous voluntary act. The earlier, temporally looser awareness of preplanning can be regarded as a more deliberative form of conscious intention, but it was not possible to time its onset relative to the onset of the accompanying "type I" RP (which arose considerably earlier than type II RPs in series of acts devoid of preplanning; Libet et al. 1982).

On the other hand, characteristics of preplanning experiences and other considerations led me to propose that even more deliberate voluntary actions, when they finally reach the condition of performing the actual motor act, include processes with characteristics similar to those studied in our simpler, spontaneous voluntary act (see TA 1 and 3). None of the commentaries appear to me to present any convincing evidence or argument against such a proposal. RPs are of course not unique to the special acts we studied, contrary to Bridgeman's concern. The experiments described by **Jung** provide important studies of how RPs can be detected in and used for analyzing the more complex actions of writing, aiming, and so on. RPs have also been associated with the onset of speaking (Grozinger, Kornhuber & Kriebel 1977). Our conclusion - that the 350-400 ms by which RP onset precedes W indicates a period of unconscious initiative for the acts we studied - should not be taken to imply that the seconds-long RPs before repeated word writing (in Jung's experiment) indicate a correspondingly long unconscious initiative. As I have already noted, conscious intention in a more deliberative, preplanning situation is distinguishable from our "W."

I accept **Jung's** proposal that in a fully learned skill like writing conscious intention is concerned primarily with the "goal and not with the automatic and learned mechanisms of action." As **Breitmeyer** notes, one need not be aware of intending to write immediately before each word. However, it does not follow that nonautomatic acts, that is, those in which conscious intention precedes each act *(is* in our studies), cannot be preceded by an unconscious cerebral process. Obviously, many deliberately planned intentions involve acts that are not automatic or overlearned but rather are each immediately preceded by an intention to act. Motor acts that have become "automatic" and are not accompanied by an experience of intention before each act are not of interest when one is studying the nature of conscious intention and control, even though they are set into motion by a general intention. Indeed, the available evidence suggests that an automatic act, even though it follows a general intention, is not preceded by any substantial RP (e.g., Libet et al. 1982).

2. Conscious control and the mind-brain relationship

Given our experimentally based conclusion that cerebral processes initiating or leading to a voluntary act are initially unconscious, I looked for a way in which the appearance of conscious intention (at 350-400 ms after RP onset but 150-200 ms before muscle activation, EMG) might still play a role in determining the outcome of the unconsciously initiated process. For this, I proposed two possibilities: (a) Conscious intention could signify a conscious triggering process, without which the volitional process would not be consummated; this would agree in principle with the process postulated by Eccles, although we differ on how to interpret the prior portion of the RP. (b) Alternatively, when conscious awareness of the intention to move has appeared, an ensuing conscious function might veto or block the consummation into a motor act. The veto alternative was the more attractive one to me, as well as to **Mortensen.** (I agree with Mortensen that the trigger and veto functions would probably have to operate in series if both modes of control were to be independently present.) Scheerer's suggestion that our conscious veto experiment was equivalent to a simple visual reaction-time paradigm misses some crucial distinctions. In the earlier work he cites, subjects were instructed not to respond if a moving dot stopped before crossing a line that otherwise signaled them to respond. Such an inhibition or veto would indeed measure a reaction-time response to a sensory signal and could in principle be accomplished even unconsciously. In our experiment the subject consciously knew in advance that he was about to veto and was not reacting to a signal having an unknown incidence.

2.1. Are prior cerebral processes required for conscious control? There is nothing in our new evidence to entail that a conscious veto or trigger is not itself initiated by preceding cerebral processes, as correcdy noted by a number of commentators (**Danto, Doty, Latto, Mortensen, Nelson, Rugg, Underwood & Niemi, Wood**; a related argument was made by Harnad, 1982). With such prior processes, any conscious control would itself be initiated unconsciously, as in the case of conscious awareness of intention to move. That is a viable proposition and could perhaps lead to certain testable inferences. Indeed, it would be in accord with my own general hypothesis that a substantial period of cortical activity is in general required in order to elicit a conscious experience (e.g., Libet 1965; 1981a, 1982). However, one must remain open about the applicability of such a general hypothesis to all forms of conscious experience, particularly in the area of intention for and control of voluntary acts. After all, it must also be noted that there is presendy no direcdy applicable evidence *against* the appearance of a conscious control function without prior unconscious cerebral processes.

The arguments for and against the necessity of prior unconscious processes in conscious control really concern matters of philosophical viewpoint, rather than matters of scientific substance. The view that consciousness cannot be primary seems to be based on the widely held premise that some form of identity theory correcdy describes the mind-brain relationship (i.e., conscious experience is assumed to be a property or introspectively observable aspect of the underlying neural activities: (**Danto, MacKay, Merikle & Cheesman, Underwood & Niemi, Van Gulick, Wood**). The argument is then that since a conscious experience is based on neural activities, it would require prior causation by cerebral processes. In the general assumption that there always has to be an appropriate ongoing background of cerebral function (to make any mental or conscious manifestations possible), all modern theories of the mind-brain relationship would be in agreement. But we are considering the sufficient, not merely the necessary, conditions, that is, which specific neural activities are uniquely involved in the direct and immediate development or appearance of the conscious function?

I would argue that my proposal of a conscious control that would not itself be initiated unconsciously is compatible with any mind-brain theory. There is no logical imperative in any mind-brain theory that requires specific neural activity to precede the appearance of a conscious event or function. Such a condition cannot be established by a priori arguments and must be experimentally shown to exist, as has been done for a conscious sensory experience (Libet 1973; 1982) and for conscious intention (Libet, Gleason, Wright & Pearl 1983). Even identity theory would be compatible with the occurrence of sudden, spontaneous neural patterns that were immediately associated with conscious events. The issue of prior neural processes would therefore not be primarily one of monism versus dualism, as explicidy suggested by **Nelson**. For similar reasons, and contrary to the view of **Wood**, dualism is not necessarily present or implied in the proposition we experimentally tested, namely, that awareness of the intention to act should precede or coincide with the onset of the RP *if* a conscious intention initiates a voluntary act. On any mind-brain theory, even a determinist one, there could be no a priori assurance that conscious intention (whatever its underlying nature) would follow the onset of neural sequences specifically generating a voluntary act; indeed, many scientists and philosophers have tended to write and speak as if the reverse were true.

The foregoing considerations also bear on the suggestion of **Doty** (who has contributed greatly to our understanding of conscious processes) that conscious control must form part of the same process that unconsciously initiated the conscious intention to move. This is an acceptable argument, as indicated above, as long as one is not concerned to provide a mechanism for conscious control as a spontaneous initiative not developed out of prior unconscious processes. According to Doty, the flaw in having the conscious veto arise separately after conscious intention is that the conscious process would not "know" what motor act will ensue if it fails to veto. But even after the appearance of conscious awareness of intention to act, there remains 100-150 ms in which the conscious function could "evaluate" and decide on whether to veto that intention. We are still far from being able to say with any confidence how quickly a conscious function could evaluate and block the processes leading to an act once conscious intention has appeared.

2.2. Responsibility and free will. An appropriate caution is recommended by **Bridgeman** against too facile a transference from our results, based on simple spontaneous voluntary acts, to the larger issues of voluntary behavior in general, self-control, and free will. However, he misses the mark in viewing our subject's acts as not freely willed and as equivalent to reaction-time responses. When a subject is acting at times that he experiences as having himself chosen spontaneously, it seems ad hoc and unsupported to regard his acts as unwilled, programmed responses to special instructions. I have already indicated why our overall findings do suggest some fundamental characteristics of the simpler acts that may be applicable to all consciously intended acts and even to responsibility and free will (if the latter do exist). Scientific progress has almost always depended on discoveries made with simpler, controllable experimental paradigms which then provide the basis for larger inferences. The problems of brain function in relation to conscious voluntary action may also require such an experimental analysis. Speculations and theories not based on experimental data directly relevant to the experience of conscious intention have thus far provided little more than representations of personal philosophical viewpoints.

There can of course be different ways to interpret the significance of the results for the issue of individual responsibility. **MacKay** carefully sets forth a reasoned argument based on a form of identity theory, an argument related to that made by **Doty** and some others. MacKay argues that "what we cannot in principle evaluate, we cannot control." That is, the conscious function would have to be able to evaluate the outcome of impending neural actions in order to control them. But subjective experience is in a phenomenological category that does not include any of the externally observable physical systems that provide the reference analogies in **MacKay's** argument, and it may be a mistake to argue that "the same distinctions between categorical levels of analysis" must apply. We should not yet presume to know a priori the rules that describe how the conscious function must operate. In any case, I have already argued above that even such a philosophy as MacKay's does not necessarily or logically exclude the appearance of conscious control without specific prior processes.

MacKay also argues that if our conscious agency is embodied in our physical brain activity the forms of cerebral activity associated with conscious intention and control must be developing out of and inherent in the whole sequence of processes. He argues that this kind of identity between conscious decision and neural action should serve firmly to "[pin] to our own door responsibility for all we consciously choose to do." However, those for whom MacKay's view of responsibility may not represent a convincingly active process can legitimately turn to an interactionist approach, whether monist emergent (Sperry 1980; Jasper) or dualist (Popper & Eccles 1977). The available scientific evidence does not discriminate in favor of one or the other of these views whether interactionist or not.

Eccles sets forth, in a systematic and straightforward fashion, a hypothesis of how a conscious entity operating within a dualist interactionist framework might work to initiate voluntary acts and implicitly exert control and responsibility. Eccles has ingeniously adapted his philosophical view to the opportunities and constraints presented by our observations; one would hope for a similar kind of impact from our experimental findings on other mind-brain models that are likewise compatible with the data. However, I must repeat that my own interpretation of the meaning of the RP processes that precede conscious intention differs from that of Eccles (see above, section 1.2). A consideration of all known features of RPs has led me to postulate a more specific initiating role for the unconscious processes that precede conscious intention. In my proposal, the conscious function selects among the unconsciously initiated volitional motor impulses by either triggering one to completion or preferably (see TA section 4.2) by vetoing it. In Eccles's proposal, conscious intention is the specific initiator, but it is nevertheless timed unconsciously to occur when a nonspecific cortical change becomes favorable for proceeding with a motor act.

Finally, I accept Mortensen's contention that the unconscious initiations of conscious intentions are deserving of "moral education," to the extent that this can be efficacious in affecting the tendency to or the context in which our unconscious initiating impulses to act arise. However, when any such moral education takes the form of imparting feelings of guilt, shame, or malevolence for an unconsciously initiated process, it would seem to be demanding responsibility for something not directly manageable at the conscious level. Actual motor performance of the act is both consciously controllable (in my thesis) and ethically meaningful, since it is the motor act that has a real impact on one's fellow man. Moral constraints on actual voluntary motor actions, rather than on the having conscious intentions or urges to act, would thus be based upon realistically achievable goals of responsibility.

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