"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 experiment is 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

Gary B. Rollman

*Department of Psychology, University of Western Ontario, London, Ontario, Canada N6A 5C2*

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, and 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 jointly 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?

Michael D. Rugg

*Psychological Laboratory, University of St. Andrews, St. Andrews, Fife KY16 9JU, Scotland*

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 significantly 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

Eckart Scheerer

*Department of Psychology, University of Oldenburg, D-2900 Oldenburg, Federal Republic of Germany*

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