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Reply

Timing of conscious experience: Reply to the 2002 commentaries on Libet's findings

Benjamin Libet*

Center for Neuroscience, University of California, 809 Plum Lane, Davis, CA 95616, USA

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1. Introduction

The June 2002 special issue of *Consciousness and Cognition* was devoted to the topic "Timing Relations Between Brain and World." Most of the articles dealt with re-interpretations and analyses of Libet's findings and conclusions. I was pleased that this topic was receiving this further attention by thoughtful individuals.

I was given the advance opportunity to reply to the first three papers by Pockett, Trevena and Miller, and Gomes. However, my reply was placed in the issue to follow eight other commentaries. Most of these authors, and those of the five additional articles, were apparently not aware of my reply arguments.

I take this opportunity to present evidence and analyses bearing on the papers that followed the first three articles. I trust this will end the present debate without further commentaries replying to my present paper. Several of the authors have had their say in more than one article; I should not have to reply to further statements by them. Let the readers now make their own judgments based on the previous and present discussions of the issues.

I cannot deal with all the issues and arguments raised by all the articles presented. That would require a book-length discussion. I will try to deal with the arguments and proposals that seem to have the most direct impact on our findings and their implications.

Van de Grind (2002) correctly recognizes that my approach to the problem is a kind of limited positivistic one. (He kindly labels the problem of the relation between external time and mental time "Libet's problem.") My approach from the start has been to ask questions that can be answered experimentally, and to avoid making assumptions about the neural substrate involved unless these

* Fax: 1-530-757-8827.

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can be tested experimentally. Also, I accept the subject's reports as valid, unless there is evidence against that (for example, not reliable in relation to varied stimulus strengths or durations.).

The whole issue of the brain–mind relationship has been and still is rife with proposals that involve untested and untestable assumptions. If we are to develop a solid basis for solutions to the Libet problem, as well as to other aspects of how neural activities of the brain give rise to conscious subjective experience, we had better not distort the experimental evidence to fit hypothetical biases and assumptions; and we should try to proceed with further experimental observations rather than with armchair conjectures. When experimental discoveries point to surprising and unexpected relationships, they should not be dismissed as mystical, or "miracles" or dualistic or neurally unacceptable. To paraphrase a statement attributed to Niels Bohr, any experimentally based theory that is not bizarre has no chance of being an important discovery.

Van de Grind is also correct in stating that "critics of Libet's conclusions introduce hypothetical neural processes in their theory, but Libet (2000) sees them as ad hoc assumptions."

However, Van de Grind then finds ways to accept the critics' hypothetical proposals as reasonable ways of eliminating the need for our tested discoveries of delays for awareness, of backward referral of the subjective time for a sensation, and of the initiation of a voluntary act well before the conscious intention or wish to act appears.

2. Validity of our subjective timings

2.1. Delays in conscious sensory awareness

We had produced evidence that awareness of a sensory event is delayed by up to about 500 ms (reviewed by Libet, 1993). Direct stimulation of somatosensory cortex (postcentral gyrus) required trains of pulses lasting about 500 ms, at the threshold intensity for a sensation. Such a requirement has been confirmed by Ray et al. (1999), and by Amassian, Somarsondaram, and Rothwell (1991) and others.

A single stimulus pulse to the skin is well known to be able to elicit a sensation. But the neuronal events in the cerebral cortex elicited in response to the single threshold pulse at the skin, go on for 0.5 s or more. If the later evoked responses are eliminated by lowering the skin pulse to just subthreshold intensity or by drugs like atropine or general anesthetics, then there is no conscious sensation.

However, the nature of the delay has been subjected to different interpretations. We have argued that the available evidence indicates that the *duration per se*, of appropriate neural activities, is the "code" for eliciting awareness; that is, there would not be a specific neural event at the end of the duration of activities. There is some experimental evidence to support this view. Appearance of a specific neural response at the end of a series of neural activations is another view, but there is no direct evidence for this view.

Pockett's (2002a) argument that the delay is simply due to a facilitation of neural responses with repetition of stimulus pulses to the sensory cortex, was dealt with by Libet (2002). Breitmeyer (2002) supports this conjecture by Pockett, but it seems he has not read the reply by Libet (2002). The same seems to be true for the commentaries by Durgin and Sternberg (2002) and by Ramakrishna (2002).

I had pointed out that DCRs (direct cortical responses to local stimulus pulses) did not show any evidence of facilitatory responses during a 500 ms threshold train of 20 per second electrical pulses to the somatosensory cortex of the human subject (Libet, 2002). Pockett (2002b) proposed that our recorded DCRs may not represent neural responses and may even be "mostly artifact." In fact, DCRs have been studied experimentally for decades and have been shown to represent postsynaptic responses of dendrites of cortical neurons. This view of the source of DCRs was developed by experimental studies, including those with microelectrodes inserted to various depths in the cortex of non-human mammals (see Ochs, 1962; Ochs & Clark, 1968a, 1968b). Our findings with DCR recordings therefore contradict Pockett's proposal. They also make largely invalid a number of the other commentaries that rely on Pockett's facilitation proposal; these are the articles by Joordens, van Duijn, and Spalek (2002), Breitmeyer (2002), and Ramakrishna (2002).

Van de Grind (2002) believes that the finding of a "conscious experience (taking) place 500 ms after its external cause is no problem for any theory. Consciousness need not be in a hurry because virtually all time-critical processes are automatic and nonconscious." This view is in full accord with our findings in this area.

2.2. Backward referral (antedating) of subjective timing

One of the most difficult "Libet problems" arose when our experimental evidence demonstrated that the conscious awareness of a somatosensory sensation is delayed by up to 500 ms, even though the initial sensory input arrives at the primary sensory cortex after a 10–30 ms after a stimulus to skin. The difficulty lay in the fact that people do *not* perceive any delay between the stimulus and their subjective awareness of the stimulus. To deal with this paradox we proposed and tested a rather outrageous hypothesis. This proposed that although the awareness of the stimulus is in fact delayed by up to about 500 ms, the *subjective timing* of that awareness is referred back in time to the appearance of the primary evoked response of the cortex (that appears after 10–30 ms delay) (Libet, Wright, Feinstein, & Pearl, 1979). Fortunately, we were able to test this hypothesis experimentally by (a) matching the subjective timing of a cortically induced sensation with that for a skin pulse, and (b) doing the same matching for a sensation elicited by a necessary train duration of stimulus pulses applied to the ascending sensory pathway in the brain (medial lemniscus) in relation to the subjective timing of a skin pulse. The critical point in this test was the *absence* of a primary cortical evoked potential (EP) with direct cortical stimuli; but the presence of that EP response after each pulse in medial lemniscus. Yet the pulse trains of both the cortical stimulus and the medial lemniscus stimulus required substantial *durations* of repetitive stimulus pulses (up to about 500 ms) to elicit any sensory awareness. The surprising finding was that the subjective timing for the delayed awareness of the medial lemniscus stimuli was statistically similar to that of a single skin pulse, while the subjective timing for the cortical stimuli appeared to be delayed by up to 500 ms after that for skin pulse. It should be recalled that we had several lines of evidence to support the view that awareness of a single skin pulse is also delayed by up to about 500 ms (Libet, 1981, 1993). (This is contrary to the erroneous proposal by Pockett, 2002a, that awareness appears after 80 ms; see Libet, 2002.) The test results for the backward referral hypothesis thus provided strong evidence for antedating of delayed sensory awareness to the time of the initial primary evoked response;

no such antedating occurs with a direct cortical stimulus, as these stimulus pulses do not elicit any primary evoked response.

2.3. Klein's alternative analysis (Klein, 2002a)

I appreciate the attempt of Stanley Klein to subject our experimental test data (for backward referral) to a different statistical analysis and interpretation. Klein concluded that our subjects were "unable to make an accurate ordering response" (about the timing order in which the two separate sensations occurred). He argued that our data are weak and "provide evidence that there is no compelling reason to invent exotic or ad hoc mechanisms to account for Libet's data." But Klein nevertheless proposed that "there are good arguments for a backward referral mechanism" (in accord with our hypothesis) "to help the subject make sense out of the tangled chaos of asynchronous information associated with experienced events." Some other commentators seized upon Klein's interpretation of alleged weakness in our experimental test results, to believe that he abolished the validity of our proposal for antedating (backward referral) of a sensory awareness (see Gomes, 2002b; Pockett, 2002b).

However, I consulted with my statistical colleague, Dennis Pearl (Professor of Statistics at Ohio State University). After a careful review of Klein's re-analysis of our data, we conclude that his method made some unwarranted assumptions, and that our evidence does support our hypothesis of backward referral. Klein assumes that the subject's responses of "together, T" are not valid information of the subject's actual perception. (In the T responses, the subjects reported that the two sensations being compared had occurred simultaneously, i.e., "together".) Klein treated our tests as if they involved a forced choice. He treated the subject's responses as indicating one sensation had to be either before or after the other one. He treated T responses as if they did not really reflect a subjectively synchronous perception for the two sensations (e.g., skin pulse vs LM stimulus). He thus assigned probability values to the T responses and assigned them to one of the other two categories of temporal order (i.e., a sensation was regarded as either "first" or not). But the instructions to the subjects asked them to report what they actually perceived, *not* to report T simply because they did not know how to place it in relation to being or not being first. That is, the subjects were not guessing, and they told us they were not guessing. Furthermore, Klein employed a parametric approach, assuming a Gaussian form for the psychometric function that does not fit the data unless a guessing parameter is added that does not fit the experiment. The analysis in Libet et al. (1979) adopted a non-parametric approach that is described in an Appendix A to this paper.

Klein claimed that there was too much variability in our subjects' reports, as treated in his re-evaluation of them, to provide convincing evidence for our hypothesis. But, in fact, we gave the standard deviations (*SD*) for each series of the data in all of our tables of the original results (Libet et al., 1979). It can be seen that the significance of our results is not undermined by the kind of variability shown there. Indeed, a direct inspection of the data seems to be convincing without resorting to statistical re-evaluations. For example, in Table 2A we gave all the responses for series in which the order of the stimuli to the left versus the right hand was reported, and for series (in the same subject) for the order of perception for stimuli to one hand versus stimuli to the medial lemniscus (LM) was reported. The "mean shifts" in millisecond for the series with stimuli to right and left *hands* were not greatly different from the mean shifts for the accompanying series of a stimulus to one hand versus one to LM. (The mean shift is the best time delay to use for one of the stimuli in order to get close to the center around which the subject is reporting the relative orders for the other stimulus, in all the temporally coupled pairs in that block.) Indeed, when the mean shifts for the coupling of skin versus LM are subtracted from the mean shifts for couplings of left and right skin stimuli (Table 2B), it is seen that the differences are small, relative to the actual durations of the LM stimuli. That argued for the conclusion that there was no established delay of an LM perception relative to a skin stimulus perception!

Gomes (2002b) continues to propose his same objections to our findings that there is a delay of up to about 500 ms for sensory awareness, and that a backward referral of subjective timing explains why we feel no such delay occurred. Gomes continues to make his argument on assumptions about latencies being substantially different in our different experiments. Without again going into the details about his proposals (see Libet, 2000, 2002), one simple view would appear to make Gomes' assumptions unacceptable. We could not know, in any such experiments, the actual time of when conscious awareness of a given stimulus actually appeared. We can impose limitations on this time, based on the experimental findings, and in our conclusions we did not make assumptions beyond that. I may add that some subjects spontaneously said "now" to indicate when they detected the stimulus, expressions that were outside our instructions to wait before reporting sensory awareness. Nevertheless, these vocal expressions appeared to occur close to the time of our hypothesized delay; they did not support the proposal of Gomes (and Churchland and Glynn) that there was an *added* latency of up to 500 ms, *following the end* of an adequate 500 ms train of pulses, before sensory awareness appeared.

2.4. Consciousness in the volitional process

Libet, Gleason, Wright, and Pearl (1983), produced evidence that the brain appears to initiate a freely voluntary act well before the subject is aware that he/she wishes or feels the urge to act. The recorded readiness potential (RP) precedes by 350 ms the subject's report of being conscious of the wish (W) to act. However, W precedes the act by 150–200 ms, so there is time for the conscious function to control the final outcome; it can stop or veto the process so that no act occurs (Libet, 1985). Commentators have proposed a variety of possible difficulties with the direct conclusions from these experimental findings.

2.5. Timings of conscious intentions to act: Accuracy of reports

In our experiments relating the time of cerebral onset (readiness potential (RP)) to the subject's reported time of the earliest conscious awareness of the intention or wish (W) to act. The subjects reported W from the position of a fast moving "clock" to indicate when W occurred.

Many of the alleged difficulties find faults with the method we used for a subject to report a "clock-time" associated with the subject's first becoming aware of wanting to move (W). Various kinds of objections are raised about the validity of W, whether because of sensory delays and disbelief in backward referral, and other theoretical possibilities (see Durgin & Sternberg, 2002; Gomes, 2002a, 2002b; Joordens et al., 2002; Pockett, 2002a, 2002b). Van de Grind (2002) and others believe that studies of the timing of visual percepts, like the "flash-lag" effect, raise doubts about the validity of our *W* measurements of the subjective time of the reports of the "clock-times." But the conditions in which "flash-lag" is observed do not properly apply to our experimental conditions.

But a critical control feature in our experiments on voluntary acts seems to be missed or ignored by the commentators in making their proposals. In every session of experiments, three kinds of series (40 trials in each) were run (in random order in different sessions for a given subject). Scalp recordings of RPs and of sensory evoked potentials were made in all trials. In one of the three series, subjects were asked to make a spontaneous quick flexion of the wrist and/or fingers while watching the fast revolving "clock" spot of an oscilloscope. After this trial, subjects reported the clock time associated with their first becoming aware of the wish or urge to move (*W*). In another series, the procedure was the same, except that the subject was asked to report the clock-time associated with his/her awareness of actually moving (*M*). In a third series, the subject was instructed not to move (and to relax the hand, as checked with EMG recordings), but to report the time of a weak skin sensation (*S*). The latter was produced by a randomly timed electrical near threshold pulse to the same hand. We knew objectively the actual time of each pulse but the subject knew nothing in advance about when a pulse would appear. The subjects' reports for times of *S*, relative to the actual *S* time, averaged about −50 ms. That is *S* times were reported as being somewhat earlier than the actual time.

It should be noted that the basic procedure, for attentive monitoring of the revolving clock spot and of noting visually and later recalling the clock position of the spot, was the same for the *S* series as it was for the other series (*W* or *M*). That means that the possible difficulties proposed by the commentators, even if they were to be valid difficulties, would only modify the *W* (or *M* reports by about −50 ms, as seen in the experimental control *S* series! Our *W* and *M* values were corrected for this −50 ms bias. We can, therefore, dismiss all those proposed difficulties as not significantly affecting our conclusions, since our conclusions were based on values in the hundreds of millisecond.

2.6. Validity of the RP measurements

Miller & Trevena (2002) continue to argue that RPs always involve smearing of the onset, and so the times of onset for RPs and for LRPs (recorded laterally over the premotor/motor areas) cannot be accurately determined. I already discussed the precautions we had taken to minimize such possible errors (Libet, 2002).

Miller and Trevena still insist that the onsets, even of LRPs, in voluntary acts, do *not* precede the reports of *decisions* to move. This is based on their findings that 20% of the decision times preceded the LRPs in their experiments. But, as already noted (Libet, 2002), they dismiss the other *80%* of the reports in which decision times *followed* the onsets of their LRPs. I cannot see how that can prove their contention. Furthermore, Haggard & Eimer (1999) found that all of their LRPs preceded the reports of decision times, even when they separated the early decision times from the later ones!

2.7. Free will

I defined genuine free will as an expression of an action that is not completely controlled or limited by the deterministic nature of the laws of physics and chemistry, etc.

I appreciate Stanley Klein's (2002b, 2002c) sympathetic attempts to deal with this issue. However, I think his remarks about "emergence" do not quite make it. The properties of a system emerge from, but are not predicted by, the properties of the elements making up that system. For example, the unique properties of benzene are not evident in those of the six carbon and six hydrogen atoms making up that system.

It is not, therefore, dualistic to propose that a non-physical mental phenomenon can emerge from appropriate activities of neurons (and glia) in the brain. Such a proposal does not argue that the subjective mental phenomenon is an entity separable from the brain; this mental phenomenon could not exist without the brain from which it emerges. It is thus quite different from the Cartesian view of dualism. (Parenthetically, definitions like those of dualism are only useful if they help us understand something. Furthermore, dualism of any kind is as viable as monism, unless it can be contradicted by evidence. Of course, if a given view is not potentially falsifiable, then it should be regarded as a metaphysical, rather than a scientific one.)

Klein is correct in stating that Roger Sperry had been arguing that the emergence of a mental entity would not violate determinism, even when he proposed that the mental function could "supervene" in the functions of cerebral neurons. (I did read all of Sperry's relevant writings on this.) But, Klein was evidently not aware of Sperry's change of heart on this, in his final years. He came to believe that determinism simply fails to allow or explain free will, and he proposed that we allow for a non-deterministic process since it is not contradicted by any evidence (see Doty, 1998).

I elaborated this view by proposing a field theory to deal with issues of free will and of the unity of subjective experience (Libet, 1994). I described a crucial experiment to test this theory; I would not have proposed such a theory without an experimental test. The proposed field is not dualistic in the Cartesian sense; it emerges from and does not exist without appropriate cerebral activities. The test is difficult to arrange and has, unfortunately, not yet been performed.

2.8. The veto

We should also note that the factual ability to consciously *veto* the performance of a volitional process, provides a role for conscious will in controlling whether we act (Libet, 1985). Our finding that the initiation of a voluntary act occurs *unconsciously*, before a subject is aware of the wish or urge to act, does not preclude an active role for the conscious function. Although delayed, the conscious W appears about 150 ms *before* the act.

It has been argued by a number of commentators that a veto function must itself be preceded by unconscious cerebral processes that produce it. In such a case, the veto could also have been developed before its conscious appearance and would not be convincing evidence for non-deterministic free will. This view was fully analyzed in my paper on free will (Libet, 1999), and I shall not repeat it all here. I made the distinction between the achievement of the conscious awareness (of the wish to act) and the appearance of a *conscious control* function like the veto. There is no imperative necessity that requires an unconscious development of this control function. I did not exclude the possibility that preceding processes could "inform" the conscious veto, but with the actual conscious veto not decided or determined by these preceding processes.

2.9. Quantum mechanics and free will

Pockett (2002a) engages in a brief argument with Klein (2002a, 2002b) on dualism in quantum mechanics and its role in free will. I am not sufficiently versed in quantum theory to get into that debate. I do agree that the existence or non-existence of free will may not be solved by the introducing quantum mechanics. "The randomness aspect of quantum mechanics gives random will rather than Free Will" (Klein, 2002a). It would still be a deterministic view, in that the natural laws are being obeyed. But, on other grounds, Stapp (1999) argues that a classical deterministic view of Free Will does not work.

2.10. Rosenthal's view

Philosopher David Rosenthal (2002) did not attempt to evaluate our experimental finding. Instead, he argued that our "results appear to conflict with our commonsense picture of how mental functioning interacts with bodily occurrences." Following Pockett's suggestion that our "results seem to deny to consciousness any major role in the conduct of our day to day affairs," Rosenthal feels that our results "seem to compromise our sense of free agency."

Such arguments do not take into account the evidence that conscious will can control our voluntary actions by blocking or vetoing a voluntary process before it can produce the act. Admittedly, our results seemed to eliminate a role for conscious will in *initiating* a voluntary act, but they retain an active role for conscious will in controlling the final outcome of the unconsciously initiated volitional process. I have already dealt with the argument that the conscious veto itself may be elicited and determined by preceding unconscious processes, thus eliminating free agency. Rosenthal (and others) do not accept a conscious veto that is not determined unless "we have independent experimental evidence that this conscious veto does not itself occur after a nonconscious neural veto." How to design such a negative experiment is not obvious. But, based on what we already know experimentally, it is possible to accept the view that the conscious veto is not determined by a nonconscious neural veto (Libet, 1999).

Rosenthal digresses to point out that many sensory responses are unconscious. That is an observation I fully agreed with (as early as Libet, 1965). We have, in fact, attempted to study the neural differences between sensory inputs that are unconscious and those that achieve consciousness (Libet, 1985; Libet et al., 1991). There is a fundamental, experimentally demonstrated neural distinction between unconscious *detection* of a sensory signal and the conscious *awareness* of that signal. Rosenthal is unaware of that experimental work. In an analogous situation, there is nothing in our experimental results that denies the possibility of volitional acts occurring unconsciously.

The significance that Rosenthal attaches to "commonsense" in his various arguments is a bit strange. Since when do common sense views countermand experimental evidence? It is interesting that one other philosopher (Gomes, 2002b) likes Rosenthal's argument as based on "good reason," while Patricia Churchland (1981) has excoriated common sense views of consciousness as "folk tales," not worthy of serious consideration.

Rosenthal suggests the possibility "that an earlier developing volition simply prepares one to act, and a distinct subsequent volition initiates the specific action." We had earlier not only made such a distinction, but had experimental evidence that supports it (Libet, 1985; Libet et al., 1983).

The process of voluntarily preparing to act is different from the final process "to act now." After all, one can deliberate about or plan to act all day without ever acting.

In conclusion, I thank the commentators for their efforts. I have tried to deal with their chief concerns and reinterpretations of our experimental findings. I hope this will conclude the interchange about those issues.

Appendix A. D.K. Pearl, Ohio State University, Columbus, OH 43210-1247

Statistical Analysis of the data in Tables 1, 2, and 3 of Libet et al. (1979) *Brain* **102** p. 193–224.

A.1. Formulating the model

Define the random variable S (which varies with time) so that the subject would say "Tie" for any administered time delay in the interval $S \pm \Delta/2$. We would like to estimate the mean shift $\mu = E(S)$. In this formulation we assume the number Δ to be constant from trial to trial within a block of observations.

We cannot observe S directly for an administered time delay x. Instead we are told only if $S > x + \Delta/2$ (e.g., when the subject says "S_R first") or if $S \leqslant x - \Delta/2$ ("S_L first") or if $x - \Delta/2 < S \leqslant x + \Delta/2$ ("Tie"). Next we define the shift function $F(s)$ = probability that $S \leqslant s$. Now if k trials are performed at time delay x, then we have the following trinomial probability:

$$\text{Prob}[\text{subject says "}S_R\text{" } k_1 \text{ times, "Tie" } k_2 \text{ times, and "}S_L\text{" } k-k_1-k_2 \text{ times at time delay } x]$$
$$= \frac{k!}{k_1!k_2!(k-k_1-k_2)!}\left[1 - F\left(x+\frac{\Delta}{2}\right)\right]^{k_1}\left[F\left(x+\frac{\Delta}{2}\right) - F\left(x-\frac{\Delta}{2}\right)\right]^{k_2} F\left(x-\frac{\Delta}{2}\right)^{(k-k_1-k_2)}.$$

Specific cases of this model when F is the Normal or Logistic distribution function are common in statistical analysis computer packages. However, goodness-of-fit significance tests rejected both of these special cases for the data presented by Libet et al. (1979). Thus, no assumptions about the shape of F were made in the non-parametric approach taken, except that it is an increasing function for which we can specify time delays A and B with $F(A) = 0$ and $F(B) = 1$.

A.2. Estimating the mean shift

Under the model above, the mean shift, μ, is given by $\mu = B - \int_A^B F(s)\,\mathrm{d}s$. The principle of maximum likelihood (the likelihood here being the product of many trinomial probabilities) was used in order to arrive at an estimate, $\hat{F}$, for the shift function (using linear interpolation between points for which we have information). The calculation of $\hat{F}$ can be achieved easily using the EM algorithm (Dempster, Laird, & Rubin, 1977). The estimate of the mean shift was then taken to be $\hat{\mu} = B - \int_A^B \hat{F}(s)\,\mathrm{d}s$ which can be expressed as a linear combination of terms like $\hat{F}(x_i + (\hat{\Delta}/2))$ and $\hat{F}(x_i - (\hat{\Delta}/2))$ where the x_i's are the administered time delays. The standard error of $\hat{\mu}$ is then straightforwardly computed by substituting $\hat{F}$ for F in the formula for the standard error of this linear combination (a conservative bound was used for some terms in this formula).

Finally, the independence of different blocks of observations allowed the comparisons between them made in Tables 1B, 2B, and 3B of Libet et al.

As noted in the paper, some error was introduced into this analysis due to the interpolation used between the administered time delays. This error appeared to be no more than 10 ms in experiments using five different time intervals or 30 ms in experiments using three.

Sections A.1 and A.2 were written by Dennis Pearl.

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