January 2019

Volition and the Readiness Potential

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Submitted to the Faculty of the Graduate School of
Eastern Kentucky University
in partial fulfillment of the requirements for the degree of

MASTER OF SCIENCE

2019

ABSTRACT

In the "Libet study" the onset of movement-related brain activity preceded the reported time of the conscious intention to move, suggesting that non-conscious brain processes predetermine voluntary movements (Libet, Gleason, Wright, & Pearl, 1983). While the study's basic results have been replicated, its validity and assumptions have been questioned. Dominik et al. (2017) provided evidence against the study's assumption that movement and intention to move are distinct events. In this study, in which researchers did not train participants to distinguish between movement and intention, reports for intention and movement were identical. This differed from the Libet study, in which intention was reported significantly earlier in time than movement. The current study sought to replicate the findings of Dominik et al. Participants (N = 22) were assigned to one of two groups. Both groups performed the same tasks, differing only in order of task completion. In both tasks participants made mouse clicks while tracking time via an analog clock. In one task participants reported the moment they initiated their click. In the other participants reported the moment they intended to click. Crucially, when given instructions for the initial task, they were not told about the existence of the other task. Results showed an interaction of group and task, $F(1, 970) = 89.571$, $p < .001$, $\eta_p^2 = .085$. The most crucial pairwise comparison, on the initial task, revealed no difference in movement and intention reports. These findings suggest that intention reports in the Libet study may be invalid.

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I. Introduction

The famous and often cited "Libet study" by Benjamin Libet and colleagues explored the neuroscience of voluntary movement (Libet, Gleason, Wright, & Pearl, 1983). Libet et al. were primarily concerned with the electroencephalographic (EEG) activity preceding voluntary movements and how this activity temporally relates to the conscious intention to make such movements. The fact that distinctive EEG consistently precedes voluntary movements had been discovered by independent groups of researchers in the 1960s (Kornhuber & Deecke, 1965; Gilden, Vaughan, & Costa, 1966). These researchers found that a slow negative shift in EEG activity consistently precedes voluntary movements by a range of .5 – 2 seconds. Kornhuber and Deecke (1965) originally called this activity the "Bereitschaftspotential," but it has come to be known as the "*Readiness Potential*" (RP; refer to Appendix A for a list of abbreviated terms used throughout the paper). In these studies and in the Libet study, voluntary movements were self-paced spontaneous movements of the wrist and/or fingers.

The existence of the RP as a genuine precursor to voluntary movements was not in doubt at the time of the Libet study (Libet et al. 1983) and is not in doubt today, as it has been consistently found in replications (Keller & Heckhausen,1990; Haggard & Eimer, 1999; Trevena & Miller, 2002; Lau, Rogers, Haggard, & Passingham, 2004). The purpose of the Libet study was not to demonstrate the existence of the RP but to build upon RP research by introducing of a method of timing the subjective conscious intention associated with the voluntary movement. With this additional measure, the Libet study provided the first investigation of the temporal relation between the conscious intention to move and the activity leading up to the movement. The Libet study became famous because it injected some of the first neuroscientific evidence into the debates concerning volition, consciousness, and free will. By operationalizing the timing of participant's conscious intention, Libet et al. offered a way to compare a subjective intention to objective measures of the nature and timing of related brain activity.

EEG, ERPs, and the Readiness Potential

EEG activity is measured via electrodes on the scalp. For neurons to produce activity that can be measured at the scalp, they must fire (i.e., propagate an electrical signal) simultaneously with thousands of other such neurons (Kappenman & Luck, 2011). These collections of neurons must be oriented such that, when active, their post-synaptic potentials (PSPs) sum together to produce a significantly large electrical signal which travels towards the scalp. How PSPs reach the scalp and become measurable is similar to how photons of light create the beam of a flashlight; the beam can be seen only when many photons are traveling in more or less the same direction at around the same time. If only a few photons are activated or the photons are scattered in every direction, no beam will be created. When measuring EEG, electrical activity will not reach the surface and, thus, will not be measurable, for those neurons which do not fire with sufficient numbers of other neurons or which are not oriented in such a manner that their activity reaches the scalp. Most EEG activity is produced by the activity of pyramidal neurons in the cerebral cortex, since these neurons are more likely than others to meet all these requirements.

When EEG activity is measured at a specific site on the scalp and averaged across many trials, an event-related potential (ERP) is created. As the name indicates, the ERP is an electrical potential related to a specific neural event. ERPs are graphically represented as waveforms. By averaging across many trials, which can be done within a single participant or across multiple participants, only the EEG activity related to the neural event in question remains. The activity that is not related to the neural event (i.e., noise) averages to zero as more trials are averaged, so that a distinctive waveform of the event-related activity is formed. ERPs are created by time-locking EEG activity to the event in question, whether the event be a sensory stimulus, a response, a spontaneous movement, etc.

The RP is one such ERP waveform (Figure 1). As noted, it is a slow negative shift in activity occurring between .5 to 2 seconds before a spontaneous voluntary movement. It occurs at the Cz electrode site on the standard 10-20 System of EEG measurement. Cz, also known as the vertex, is the site at the top of the skull, equidistant from the ears and halfway between the inion and nasion of the skull (i.e., between the most anterior and posterior points of the skull). Kornhuber and Deecke (1965) asked participants to make freely-paced spontaneous wrist flexions and recorded their EEG activity at Cz**.** They created an ERP by time-locking EEG data collection to the wrist movement and taking the grand average of all participants' data across all trials. Since the actions carried out by participants were endogenous and spontaneous – that is, since they were not responses to external stimuli and were not preplanned – Kornhuber and Deecke concluded that the RP was a precursor to spontaneous voluntary movements. Subsequent replications (Deecke, Scheid, & Kornhuber, 1969; Libet, Wright, & Gleason 1982), as well as independent experiments (Gilden, Vaughan, & Costa, 1966; Vaughan, Costa, & Ritter, 1968; Brinkman & Porter, 1979; Roland, Larsen, Lassen, &

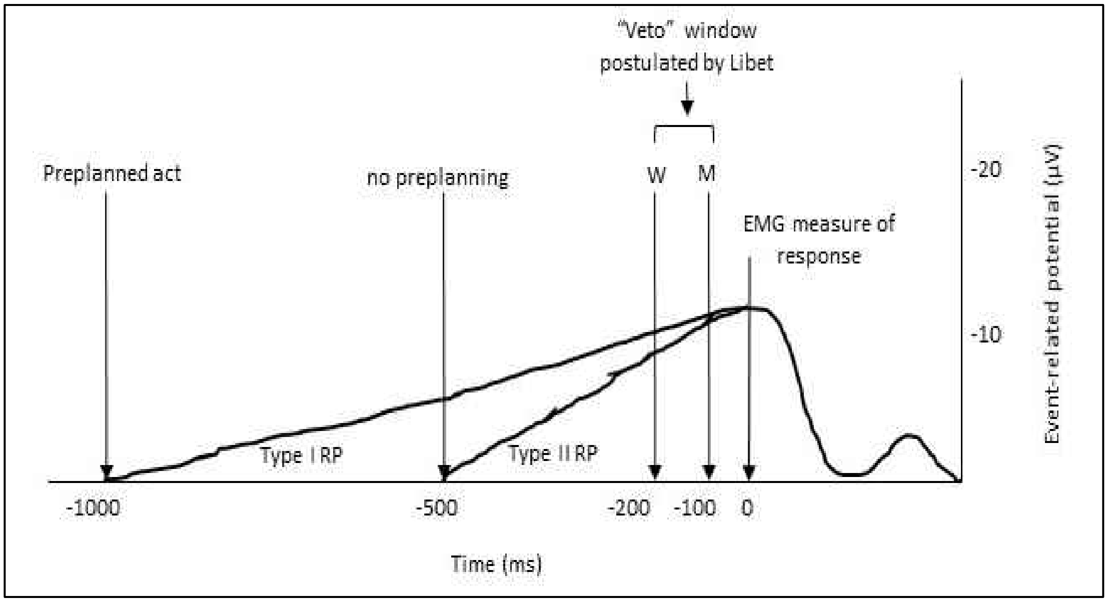

Figure 1: Basic results of the Libet study.
The two waveforms are the event-related potentials for two different types of readiness potentials.

Source: Pocket, S., Banks, W. P., & Gallagher, S. (2006). Introduction. In S. Pockett, W. P. Banks, & S. Gallagher (Eds.), *Does consciousness cause behavior?* (pp.1-6). Cambridge, MA: MIT Press.

Skinhoj, 1980), led to the RP being firmly established as a genuine precursor to voluntary movements. Citing many of these studies and others, Eccles (1982) noted that the RP had been localized to the supplementary motor area (SMA). Eccles speculated that if conscious intention could be measured, it would precede the RP. That is, he claimed that the SMA activity measured by the RP is caused and, thus, preceded by conscious intention. This is where the Libet study entered the picture.

The Libet Study

The primary aim of the study by Libet et al. (1983) was to effectively measure the conscious intention to make a self-paced, spontaneous voluntary movement and to temporally compare this intention to the RP generated before the movement. Conscious intention was measured via participants' reports of the position of a clock hand when they experienced the intention to move. The famous and controversial result of the Libet study was that the RP began, on average, about 350ms *before* the reported conscious intention to move (see Results and Conclusions of the Libet Study subsection below). The temporal sequence of events was understood to be the RP, then conscious intention, then movement. Libet et al. concluded that the RP, not the conscious intention to move, was the initiator of voluntary movement. Since the RP did not temporally correspond to the onset of conscious intention, it was concluded that it reflected the brain's unconscious initiation of movement. They tentatively concluded that conscious control plays a restricted role in the "initiation and control" of voluntary movements, at least in simple voluntary movements like those carried out by their participants (Libet et al., 1983, p. 641).

In an attempt to salvage the notion that conscious intention plays some role in the initiation and control of voluntary movements, Libet et al. (1983) hypothesized that a small window of time – the approximately 150-200ms between the conscious intention to move and the movement itself – potentially provided individuals with a short period in which to inhibit a previously unconsciously prepared movement. This is known as the *veto window* and is shown in Figure 1. In other words, although Libet et al. claimed that their findings were evidence that consciously intended movements are actually unconsciously initiated, they noted the possibility of conscious intention having a binary ability to allow or disallow those movements to occur in the final 200ms before they are programmed to occur. The main thrust of the Libet study and its many replications and variants has been the temporal relation between the RP and the conscious intention to move. The current study is mainly concerned with this relation and has not examined the possibility of a veto window. The implications of the Libet study for the role of conscious intention in voluntary movements are explored in the Discussion section.

The Libet study has been replicated and innovated upon dozens of times. As noted by Banks and Pockett (2007), most replications have produced similar results and found similar relationships between the RP and conscious intention in voluntary movements. Some examples are Keller and Heckhausen (1990), Haggard and Eimer (1999), Trevena and Miller (2002), and Lau, Rogers, Haggard, and Passingham (2004). Many researchers and theorists have made bold interpretations of the findings, arguing that the findings could be generalized to all consciously intended actions. For many, the conclusion was that unconscious activity is the true casual source of intentions and actions; conscious intention was relegated to the status of an impotent epiphenomenon. In this view, the conscious sense of agency (i.e., control over movements) is an illusion. Some theorists have used the study's results as a foundational part of broad theoretical accounts of consciousness, in which conscious decision making is an illusory mental construct rather than a causal mechanism (Gazzaniga, 2000; Wegner, 2002; Eagleman, 2004; Choudhary & Blakemore, 2006; Matsuhashi & Hallett 2008; Harris, 2010). For example, Eagleman (2004) relies heavily on the Libet study and some of its replications in formulating a model of volition in which voluntary movements are mainly unconsciously generated and conscious intention is inferred after the movement occurs. It should be noted that this and other theories are not wholly dependent upon the Libet study or its variants; they cite other independent evidence as well, such that they might continue to have support for their theories even in the absence of the Libet study's results. However, these theories would be considerably weaker and/or more speculative without these results.

Other researchers and theorists have criticized and scrutinized various aspects of the study, including its assumptions, conceptual basis, operationalization, methodology, data gathering, statistical analyses, and interpretations. Although the majority of replications have more or less duplicated the original findings, philosophical critiques and the results of many recent empirical studies have raised serious doubts about the Libet study's basic results and typical interpretations (Mele, 2013; Nachev & Hacker, 2014). For example, Schurger et al. (2012) and others provided evidence that the characteristics of the RP are largely artifacts of the process of creating an ERP from voluntary movements. In a conceptual analysis, Nachev and Hacker (2014) questioned the assumption that spontaneous voluntary movements always have a reportable intention before they occur. Dominik et al. (2017) provided evidence that participants may report the movement and the intention to move at the same time if they are not previously informed that the two events are supposed to be two separate events (see Other Criticisms section below). Current research must resolve these and other issues beyond reasonable doubt before the findings and the theories built upon them can be embraced. Before exploring the study's results, interpretations, and criticisms in more detail, let us first examine the experimental methodology employed in this novel and groundbreaking study.

Experimental Setup of the Libet Study

The Libet study (Libet et al., 1983) asked participants to monitor a revolving dot on an oscilloscope while making voluntary wrist flexions. The oscilloscope and rotating dot served as a fast-moving clock used by participants to make estimates of the timings of three events (see Figure 2 for an example of a Libet clock). Participants were to report the position of the dot when they estimated each event to have occurred. The "S" event was the perception of a tactile *stimulus* (i.e., a pulse) delivered on the back of the hand. The "W" event was when the participant *wanted* to or "felt the urge" to make a wrist flexion (Libet et al., 1983, p. 625). The "M" event was when the *movement* actually occurred. Participants were specifically told that the movement should be "a quick abrupt flexion of the fingers and/or the wrist" of the right hand after the dot had made one full revolution (Libet et al., 1983, p. 625). Electroencephalographic (EEG) data was collected during the entire experiment. It was time-locked to electromyographic (EMG) activity caused by the wrist flexions. The computer began recording EEG data once the wrist flexion caused EMG activity to surpass a threshold value. Participants were trained to make brisk movements so that the EMG data would trigger data recording no later than 10-20ms after movement.

Six right-handed individuals participated in the study. They underwent extensive training and were given strict instructions about how to complete all experimental tasks. They were split into two groups. Participants made W, M, and S reports in distinct series, with each series containing 40 trials (refer to Appendix A as needed for a table of abbreviated terms). Only one type of report was made for each trial within a series; there was no interspersing of reports within a single series. Group 2 participants were

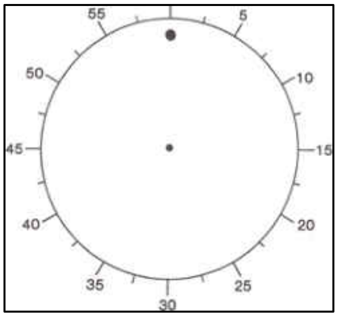

Figure 2: Example of analog clock used in Libet study.

Source: Doyle, B. (2018). Benjamin Libet. Retrieved from http://www.informationphilosopher.com/solutions/scientists/libet/

given instructions to “let the urge to act appear on its own at any time without any preplanning or concentration on when to act,” in order to encourage them to be “spontaneous” and “capricious” in their movements (Libet et al., 1983, p. 625). These instructions were not given to Group 1 until the latter one-third of sessions. In the tactile stimulus series, pulses were delivered to the back of the right hand at irregular intervals after the dot had made one revolution. Participants were then to report S time.

The researchers emphasized to participants that the W report should refer to the earliest appearance of “W awareness” corresponding to a “specific urge to act” (Libet et al., 1983, p. 627). This was done so that participants would distinguish the urge to act that immediately preceded action from any general preplanning that might occur earlier in time. Additionally, this helped participants distinguish between W and M reports; for M reports they were instructed to report the “awareness that [they] actually moved” (Libet et al., 1983, p. 627). Within W series, Libet et al. made a post hoc distinction between Type I, Type II, and Type III series, which had been made in prior research (Libet et al., 1982). Type I series were those in which the participants admitted to having preplanned actions on some trials. They specifically wanted to avoid any general preplanning of movements; they wanted participants to move as soon as they “felt the urge” to do so, as opposed to formulating some plan to move like waiting for the revolving dot to reach some decided-upon interval on the oscilloscope (Libet et al., 1983, p. 625). Type II series were those in which no preplanning was reported. Type III series were a special category of Type II series in which the RP showed less activity than usual.

Participants used two distinct modes of recall in order to make W, M, and S reports. The *absolute mode* asked them to simply report at which clock second the dot was when the event in question (W, M, or S, depending on the series and trial) occurred. The *order mode* asked them to report whether the final position of the dot, which continued to move even after the event in question occurred, was earlier or later on the oscilloscope than the position of the dot when the event occurred. In other words, once the dot finally stopped the participant had to report whether the stopping place was before or after W, M or S time. There were no meaningful differences in the results yielded by these two methods. Subsequent replications and variants have almost exclusively used only the absolute method, and this study has conformed to that pattern.

Results and Conclusions of the Libet Study

Averaged across all participants, W reports came out to be about 200ms earlier than EMG onset time (i.e., when the movement occurred). Figure 1 shows a timeline of these results. Participants’ average M time was about 90ms prior to actual movement. Participants’ S reports were used to “correct” W and M reports for any bias associated with the method of reporting (Libet et al., 1983, p. 631). Libet et al. (1983) believed that any common biases associated with the revolving dot method of reporting would be stable across W, M, and S. Since the delivered stimulus was objectively measured, S reports could be compared to this measure in order to calculate their accuracy. This bias averaged to about 50ms and was an early bias; participants reported S times as 50ms earlier than when the stimulus was actually delivered. A 50ms correction was applied to W and M reports, such that the average W report after correction was about 150ms prior to movement (corrected from 200ms), and the average M report was about 40ms prior to movement (corrected from 90ms). W reports consistently preceded M reports across participants, and this difference averaged to about 115ms. Of course, this correction method assumes that biases are correlated across all three conditions. It assumes that an exogenous tactile stimulus, an endogenous intention, and an internally initiated movement can truly be compared in terms of perception and reporting bias.

The RP onset was determined by two methods. The first method involved two researchers independently using "eye-ball inspection" to determine the discrete time point at which the "main negative shift" began (i.e., when the slow negative shift characteristic of the RP began) (Libet et al., 1983, p. 632). The second method computed the discrete time point at which 90% of the area under the RP preceded EMG zero-time. In other words, this method made a preliminary calculation of the total area under the RP waveform between the RP and movement onset and then set the RP onset as the time point after which 90% of this total area occurred. There were no meaningful difference in the results yielded by these two methods.

As previously highlighted, RP onset was found to precede W. The effect was "highly significant" even when accounting for possible moderating factors such as mode of recall, mode of calculating RP onset, and type of RP (I, II, or III) (Libet et al., 1983, p. 634). As expected, Type I RPs (i.e., those series in which participants admitted to having done some preplanning), had the largest intervals between RP and W, but Type II and Type III RPs (i.e., those series in which participants claimed that all movements were spontaneous) still had substantial intervals. The Type II RPs and intervals were deemed most appropriate for use by Libet et al. (1983) because they showed normal RP behavior (unlike Type III RPs) and did not involve preplanning (unlike Type I). The main findings which this study cites and which have been cited by others – that the RP onsets about 500ms prior to movement and that W occurs about 150ms prior to movement, leading to an interval between the RP and W of about 350ms – come from the analysis of Type II RPs. Within Type II RPs, some participants' individual series did not conform to these grand averages, but most did. For a discussion of the importance of outliers and individual performance, see the Other Criticisms section below. Libet et al. hypothesized that the prolonged RP activity before W time in Type I series, which was 650ms or more, was "not necessarily associated with freedom of choosing when to act," while the RP for the more common Type II and Type III series reflected "the cerebral volitional process uniquely involved in initiating a freely voluntary, fully endogenous act" (Libet et al., 1983, p. 636). From these results, Libet et al. (1983) made the general conclusion that:

> the brain evidently 'decides' to initiate or, at the least, prepare to initiate the act at a time before there is any reportable subjective awareness that such a decision has taken place. It is concluded that cerebral initiation even of a spontaneous voluntary act, of the kind studied here, can and usually does begin unconsciously. (p. 640)

Libet et al. (1983) also claimed that since previous research had established wrist flexions as reliable examples of spontaneous freely voluntary acts, the results and conclusions could be extended to other spontaneous voluntary acts performed without conscious deliberation or planning. However, they specifically note that their results do not preclude the possibility that other types of voluntary movements may follow a different temporal sequence of events. They specifically referred to voluntary acts involving some form of deliberation. They did not attempt to extrapolate their results, which pertained to voluntary movements, to non-spontaneous voluntary movements, particularly those involving conscious deliberation. Libet et al. (1983) also proposed the aforementioned veto window between W and M times, in which participants might have the ability to consciously abort or inhibit unconsciously planned movement.

Criticism of the Readiness Potential

The fundamental discovery of the Libet study (Libet et al., 1983) was the temporal relation between the RP and intention. This relation would come into question if one or both of these components (i.e., the RP or W reports) was found to be systematically biased by operationalization, methodology, or some other source of error. Let us begin with a closer examination of the RP. Verbaarschot, Farquhar, and Haselager (2015) replicated the Libet study and demonstrated that disparate results can be obtained for RP onset. The RP onset is the time at which distinct RP activity (i.e., a slow negative shift at Cz and/or nearby electrodes) can be observed in comparison to baseline activity in the same area. The key to determining RP onset is finding a distinct departure from baseline activity, but there is no single universally agreed-upon method of calculation for making this determination. These authors used the same two calculation methods as Libet (i.e., the eyeball inspection method and the 90% area method) but also introduced a third, more robust method using a t-test. The t-test method's criterion for RP onset is the three consecutive discrete time points at which activity was significantly different than baseline activity in the predicted direction. Thus, the RP onset occurs when the slow negative shift begins and continues for at least three consecutive time points. This method led to significantly later RP onsets than the other two methods. However, Trevena and Miller (2002) used a method similar to the t-test method and found earlier RP onsets, so it is not clear whether using a more robust method will lead to earlier or later RP onsets. Neither method is completely objective, since the eyeball inspection method requires a researcher decision and the t-test method involves an arbitrary use of three time points rather than some other number, but some level of arbitrariness seems to be inherent in calculating RP onsets. The current study will use both the t-test method and the eyeball inspection method in order to compare their results (see Future Analyses subsection of Discussion).

These authors also demonstrated that RP onset was significantly affected by which EEG filters were used (Verbaarschot et al, 2015). Drastic differences in RP onset were obtained when using a high-pass filter of .5Hz versus 1Hz. The difference in the two filters was that the 1Hz filter removed frequencies between .5Hz and 1Hz, while the .5Hz filter did not remove them. The 1Hz filter found RP onsets at about 500ms prior to movement and the .5Hz filter found RP onset at about 1200ms prior to movement. Thus, a substantial difference of about 700ms was found between the two filters. These findings demonstrate that determining RP onset is a precarious endeavor in which minor differences in methods can lead to drastic changes. Future replications should follow the example of Verbaarschot et al. and employ and report multiple methods of determining RP onset. This study will calculate RP onset using the two different high-pass filters of .5Hz and 1Hz (see Future Analyses subsection of Discussion).

As noted, the RP was already well established and accepted at the time of the Libet study as cerebral activity occurring in the SMA and preceding voluntary movement. The RP has been considered to be the unconscious initiator of voluntary movements and to represent the earliest stages of movement preparation. In light of recent studies, however, the RP has been reinterpreted as being largely an artifact of time-locking EEG signals to voluntary movements in order to create ERPs (Schurger, Sitt, & Dehaene, 2012; Jo, Hinterberger, Wittmann, Borghardt, & Schmidt, 2013; Schurger, Mylopoulos, & Rosenthal, 2016; Schmidt, Jo, Wittmann, & Hinterberger, 2016). Borrowing from well-established research on the nature of perceptual decision-making, Schurger et al. (2012) modeled the RP using a *leaky stochastic accumulator* model. In such models, which have long been accepted for perceptual decision-making, a perceptual decision is made once accumulating sensory evidence reaches one threshold over another. Once a particular perceptual choice accumulates enough evidence to cross a threshold, a visual decision is made in favor of that choice over other competing choices. In the Libet task, there is no sensory evidence to accumulate for use in the decision to move; the choice of when to move is supposed to be fully spontaneous, when participants experience the urge to move. In their model, Schurger et al. (2012, p. 16776) posit that such a decision uses “slow intrinsic noise” in place of sensory data; subjects accumulate such noise and “wait for a random threshold-crossing event.” This noise is comprised of fluctuating shifts in cerebral activity, which can be positive or negative. In this view, participants base the decision to move – that is, they base their conscious intention to move – on fluctuating shifts in internal noise. The shifts are largely random, but when averaged together into an ERP, they give the appearance of a negative shift slowly increasing over time (i.e., the RP). This prolonged RP waveform is a misleading artifact caused by time-locking EEG activity to voluntary movement; a prolonged RP is responsible for the large interval between RP onset and intention, which was fundamental to the Libet study's findings and interpretations.

According to Schurger et al. (2012), the temporal autocorrelation of stochastic (i.e., random) fluctuations in neural activity is responsible for this artifact. Temporal autocorrelation refers to how fluctuating shifts in EEG activity self-correlate by repeating in time over and over again. Thus, if activity which includes such shifts is time-locked and averaged into an ERP, it will appear non-random; the created ERP will appear to slowly accumulate in a negative or positive direction instead of being averaged out to zero. Schurger et al. (2012) replicated the Libet study and used their leaky stochastic accumulator model to effectively predict the actual RP data they obtained from participants. These authors concluded that the decision to move is likely not made unconsciously at the point in time corresponding to the RP onset. Instead, they propose that it may occur closer in time to the actual movement and be "coincident with average subjective estimates of the time of the intention to move (Schurger et al., 2012, p. 16776)." In other words, if some or all of the RP is comprised of fluctuating activity unrelated to the conscious decision to move, the actual decision may indeed be consciously determined. Conscious intention may coincide with or precede the cortical motor activity leading to movement, after all, because the purported onset of movement initiation (i.e., the RP onset) has been shown to be invalid. Of course, such a finding does not preclude the possibility that there is other discoverable activity which may consistently precede conscious intention in voluntary movements, but the finding does show that the RP onset is not a valid indicator of specific unconscious movement preparation (see the Philosophical Issues subsection of the Discussion for elaboration). In a follow-up paper, Schurger et al. (2016) claimed that the activity corresponding to voluntary movement initiation is likely much later than the Libet study and other research has indicated. They cited their own work and the work of others as providing converging evidence that the “neuronal commitment to move” is most likely 150-200ms before actual movement (Schurger et al., 2016, p. 79). This time frame is basically identical to that of the intention times reported by the Libet study and its replications. They speculated that the fluctuations which comprise the RP may influence the time of conscious intention but that the slow build-up of negative activity that is characteristic of the RP is not indicative of an unconscious decision to initiate movement.

Many questions still remain, however. What kind of influence does the RP have on conscious intention? If temporal autocorrelation is inevitable when making these types of ERPs, why are negative shifts autocorrelating instead of positive shifts? Building on the results of Schurger et al. (2012; 2016), Jo et al. (2013) found that the negative amplitude of the RP for voluntary movements is due to the averaging process. By sorting individual trials by the type of slope of the RP (positive or negative), these authors determined that the RP is actually a mixture of positive and negative slow accumulating activity. Trials in which the RP was positive accounted for 32.84% all trials. Because the proportion of negative RP trials was much larger than the proportion of positive RP trials (67.16% vs. 32.84%), the grand average RP became negative. It is a well-established fact that ERPs can lead to such misleading results and must be interpreted with caution (Luck, 2014). However, the authors pointed out that voluntary movements are clearly more likely during negative shifts, since negative shifts outnumbered positive shifts at about a 2:1 ratio. In this study participants also completed no-movement trials in which they passively listened to tones. For these, there was not a negative RP prior to tone onset when an ERP was calculated, due to the fact that the number of negative and positive shifts had almost an equal ratio (50.45% positive and 49.55% negative). This was further evidence that the 2:1 ratio of shifts is specifically associated with voluntary movements. Based on this unequal ratio and the fact that movements can occur following a positive RP, the authors concluded that negative shifts *facilitate* but do not *determine* the conscious intention to move. Specifically, the processes reflected by negative shifts may simply "facilitate a movement in the near future" in some way but not reflect decision processes; they "might more readily lead to an impulse to act" but are not correlates of motor planning (Jo et al., 2013, p. 500). If negative shifts are correlates of motor planning, they should be present in 100% of trials, but this is obviously not the case.

Another recent study replicated these RP findings, finding that about 33% of trials had positive shifts while about 67% had negative shifts (Schmidt et al., 2016). These results are shown in Figure 3. These authors distinguished between the *early RP* and the *late RP*, a distinction which has been made elsewhere as well and will be employed in the remainder of this paper (Shibasaki & Hallett, 2006; Jo et al., 2013; Verbaarschot et al., 2015; Schmidt et al., 2016). All previous and forthcoming

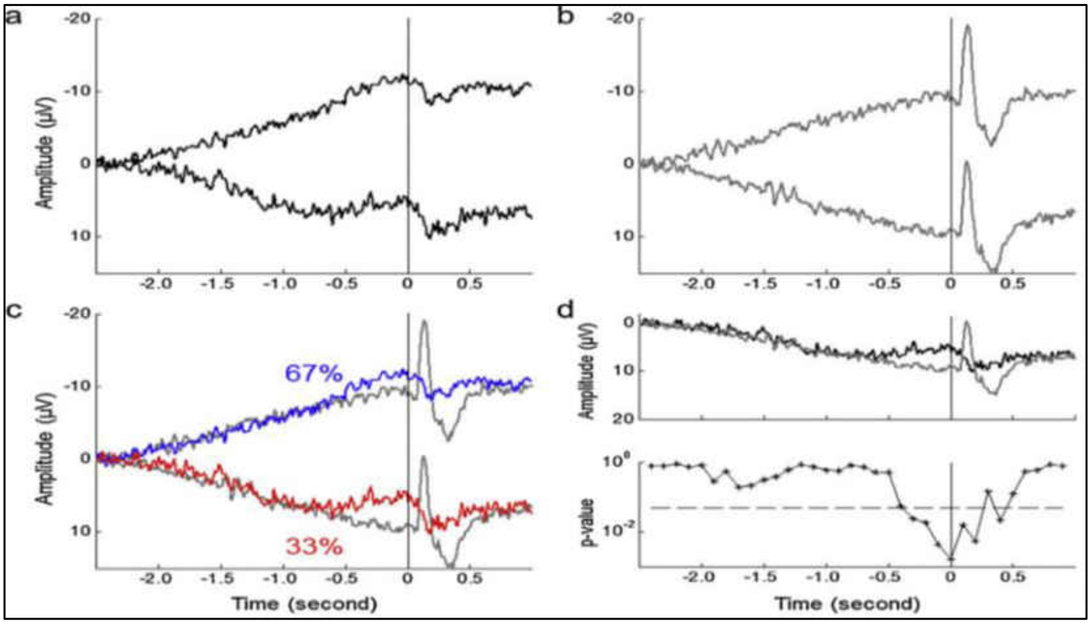

Figure 3: Results of the study by Schmidt et al. (2016)

Source: Schmidt, S., Jo, H., Wittmann, M., & Hinterberger, T. (2016). *'Catching the waves' – slow cortical potentials as moderator of voluntary action.* Neuroscience and Biobehavioral Reviews, 68, *639-650.*

references to the *RP* apply to the entire RP, which is comprised of both the early RP and the late RP. In a general review of research related to the RP, Shibasaki and Hallett (2006) showed that the early RP occurs up until 300-400ms before movement, at which point the late RP begins. The early RP is a slowly increasing negative shift, and the late RP is marked by an abrupt increase in the slope of this negative shift. Verbaarschot et al. (2015) also found that the late RP consistently has a steeper slope in comparison to the early RP. While the early RP activity is found bilaterally in the SMA and is maximal at the Cz electrode site, the late RP occurs in the contralateral motor cortex and is maximal at the C1 or C2 electrode site, depending on which hand was involved in the movement (Shibasaki & Hallett, 2006). Thus, the late RP is much closer in time to the conscious intention than is the early RP, and it occurs in the motor cortex of the hemisphere contralateral to the hand that is being used to respond. Shibasaki and Hallett (2006, p. 2341), writing before the findings of Jo et al. (2013) and Schmidt et al. (2012; 2016), noted that the early RP likely reflects general cortical “readiness” or “excitability” for movement and postulated that the late RP might be related to the conscious processes related to the intention to move.

Schmidt et al. (2016) found a late RP onset of about 400ms prior to movement and noted that the early RP is most likely not related to specific movement preparation. This accords with the findings of Jo et al. (2013), which showed a split between the early RP and late RP, around 500ms prior to movement. Both groups of researchers concluded that the early RP plays a limited role in voluntary movements, most likely that of an urge. In this view, voluntary movements are possible whether the early RP is positive or negative, but negative shifts increase the probability of a person experiencing an impulse which may be acted upon. However, both groups of researchers specifically state that their findings apply only to the early RP, not the late RP. However, both groups note that the onset of the late RP is the point at which positive and negative shifts begin to differ in behavior. Prior to this point, they show no significant differences in behavior (e.g., amplitude, fluctuations) aside from their polarity. However, at this point, slow positive shifts became steep negative shifts, while negative shifts continued in their negative direction but also became steeper. This is an important finding because it indicates that the late RP, unlike the early RP, should be consistently negative. Thus, the late RP is likely to provide a more reliable and realistic ERP which would more genuinely reflect a specific underlying pattern of activity related to voluntary movement.

Schmidt et al. (2016) found that the late RP begins about 400ms prior to movement, while Jo et al. (2015) found that it begins about 500ms prior to movement. Shibasaki and Hallett (2006) placed it between 300-400ms prior to movement. Refer to Figure 4 for a depiction of the split between the early RP and late RP. On the whole, the late RP appears to occur sometime between 300-500ms prior to movement. Future research is needed to examine the exact timing of the late RP, as well as its temporal relationship to intention. This study is primarily concerned with this relationship, not with the relationship between the early RP and intention. As discussed below, many lines of evidence suggest that the late RP and intention may be closer in time than most previous studies suggest (also, see the Other Criticisms subsection below).

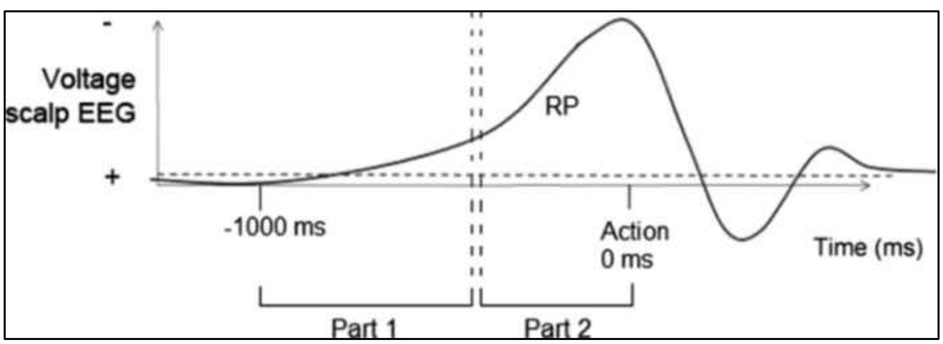

Figure 4: Idealized depiction of the RP in relation to movement ("Action").
The RP has been split into Part 1 and Part 2 at about -400ms, corresponding to the early RP and late RP.
Source: Verbaarschot, C, Farquhar, J, & Haselager, P. (2015). Lost in time…The search for intentions and readiness potentials. *Consciousness and Cognition, 33,* 300-315.

The previously cited studies of Jo et al. (2013) and Schmidt et al. (2016) found the late RP to occur about 400-500ms prior to movement. This is about 200-300ms earlier than most reported intention times, but these authors did not collect intention reports in these studies which could be compared to the late RP onset. However, Schmidt et al. (2016) cited a previous study by Jo et al. (2016) as evidence that the conscious decision to move occurs during the late RP. This study examined event-related desynchronization (ERD) of beta-band activity over the motor cortex contralateral to the hand used for movement. Beta-band activity, occurring at 13-30Hz, is an established index of waking consciousness, most notably associated with attention, concentration and motor control. Beta-band ERD is the desynchronization of beta-band oscillations, which has been shown to be related to general movement preparation (Wijk, Beek, & Daffertshofer, 2012). Jo et al. (2016) showed that the beta-band ERD of the contralateral motor cortex was positively correlated with earlier subjective experiences of urges (i.e., intention reports) in the 200-400ms prior to movement, as well as in the 0-200ms before movement. Those with higher increases in ERD during these time intervals had earlier reports. This task was similar to the standard Libet-style task; participants were asked to make spontaneous voluntary movements. The authors suggested that there may be an early and late beta-band ERD, with the early part reflecting general movement preparation (i.e., readiness to move when the urge arises) and the late part reflecting the conscious intention to move, somewhere within the final 0-400ms before movement.

Miller, Shepherdson, and Trevena (2011) provided evidence that monitoring the clock during Libet-style tasks is at least partially responsible for the RP. In one of the study’s two experiments, participants completed a variant of the Libet task under two conditions. Both conditions asked for a spontaneous key press, but only one condition required participants to monitor the clock and make an intention report; the other group simply made the movement. The clock-monitoring condition led to RPs with significantly more negative amplitude shifts in the 2s preceding the button press. In the study’s other experiment, participants also underwent two clock conditions (i.e., clock-monitoring and no clock-monitoring) and made judgments of pitch tones by pressing the key corresponding to the tone with higher pitch. Clock-monitoring likewise led to significantly more negative RP shifts in this experiment, in the two seconds prior to tone onset. Note that the RP was not calculated for the key press indicating judgement of tone pitch; it was calculated for the onset of the tone itself. While these results were calculated for the overall RP, they are probably equally applicable to the late RP.

This finding has two important implications. First, in non-movement tasks as well as movement tasks, RP activity is influenced by clock-monitoring. The clock itself is consistently leading to more negative shifts in the RP. For this reason, Miller et al. (2011) recommend a new approach to Libet-style studies in which participants make voluntary movements without clock-monitoring on one set of trials (movement-only trials) and with clock monitoring on other trials (intention report trials). This would allow the RP to be calculated without the influence of clock-monitoring. The current study has followed the advice of these authors; as one part of the study, the late RP and intention were measured on separate trials (see Future Analyses subsection of Discussion).

Second, this finding provides further corroboration that at least part of the RP is not from specific unconscious movement preparation, since RPs like those prior to voluntary movements can also be calculated prior to tone onsets. Because there cannot have been specific movement preparation at the time of passively waiting for a tone, they cannot be exclusively from unconscious motor initiation leading inevitably to movement. Hermann, Pauen, Min, Busch, and Rieger (2008, p. 156) had similar findings and concluded that the RP “seems to reflect a general expectation or an unspecific motor preparation of both hands.” In this study, participants had to respond to visual stimuli by making a left or right hand button press, and the RP was calculated for the time leading up to the stimulus presentation. The results clearly showed an RP well before the stimulus onset, which indicates that an RP was calculated before a specific movement was made. The aforementioned research on the RP, which indicated that it is likely related to an increased urge to move but not specific initiatory activity, accords with this finding. In addition, the study by Schurger et al. (2012, p. 16777) included a task called “Libetus Interruptus” which further supports these findings. In this task, participants were to make a movement immediately after hearing an auditory signal. These researchers found that participants had faster reaction times when ongoing stochastic fluctuations were most similar to the RP. That is, participants responded fastest to auditory signals when there was a random negative shift already occurring prior to the signal. This was interpreted as an indication that the negative activity characteristic of the early RP is related to the threshold for acting on an urge; when the activity is more negative, it is closer to threshold. Thus, a voluntary movement is facilitated or made more likely by ongoing negative shifts, which is why these movements occur disproportionately during negative shifts. This is the reason that the RP (i.e., a slow negative shift) is found when an ERP is time-locked to such movements.

This accords with the aforementioned studies by Schmidt et al. (2016) and Jo et al. (2013), which demonstrated that the early RP contains negative and positive trials at a 2:1 ratio. Whether these shifts are related to conscious activity, unconscious activity, urges, or some other phenomena is one question for future research. While most of these authors have claimed that the RP is related to the urge to move, it seems plausible that RPs reflect general movement preparation or expectation, given the evidence that some RPs could not have been for specific motor plans. It also seems plausible that the RP is related to generic movement preparation – to a literal neuronal “readiness” to move. Perhaps negative and positive shifts are related to the waxing and waning, respectively, of the general readiness of the involved motor networks. As the brain anticipates a movement which it already knows much about (i.e., what movement will be made, how to make it, the window of time in which it will be made, etc.), perhaps negative activity reflects its increased readiness, while positive shifts are returns to a less ready state.

In light of all these findings, the early RP and late RP must be put into proper perspective. Although Miller et al. (2011) found convincing evidence that clock-monitoring contributes to the negativity of the RP, this does not appear to be the main cause of the RP. In the studies by Schurger et al. (2012) and Hermann et al. (2008), RPs were found in the absence of clock-monitoring and intention reports. In addition, one can confidently conclude that what remains of the RP after clock-monitoring effects are removed is not from unconscious initiatory activity that leads to movement or conscious intention. It is most likely related to the urge to move or to general movement preparation. As indicated by Shibasaki and Hallett (2006), Jo et al. (2013), Schmidt et al. (2016), and Verbaarschot et al. (2015), the late RP is consistently negative and is steeper than the early RP, such that its ERPs should more accurately reflect specific neural events rather than an unequal ratio of positive and negative shifts. Furthermore, the study by Jo et al. (2016) suggested that the beta-band ERD occurring in the final 400ms before movement – which is nearly identically was related to the conscious decision to move. For these reasons, this study is primarily concerned with the late RP, not the RP, and its temporal relationship to intention. In order to provide meaningful findings about the nature of the relationship between the late RP and intention, this study's main focus must be the precise measurement of the timing of each. It investigates the precise influence of clock-monitoring on the late RP (Miller et al., 2011) and will employ multiple methods of determining late RP onset, as well as multiple data filters (Verbaarschot et al., 2015). See the Future Analyses subsection of the Discussion for more details.

Other Criticisms

The validity of intention reports has come under much scrutiny. Using discrepancies in the original Libet study as a starting point, a recent study (Dominik et al., 2017) provided evidence that participants may not naturally make a distinction between moving and intending to move. In this study, participants were divided into groups and asked to complete the movement (M) task and the intention (W) task. The groups differed only in the order in which they completed the tasks. Crucially, when completing the first task, groups were not informed of the nature of the other task. Results showed that means did not differ between the groups on the first task completed, despite the fact that one group was reporting intention and one group was reporting movement. The means relative to the objective time of movement for each group were near -10ms (i.e., 10ms prior to the objective time of movement). Thus, both groups made reports near the actual time of movement.

Dominik et al. (2017) speculated that the intention reports in the Libet study may be artifacts of the experimental setup and/or the instructions given to participants. Perhaps participants in the normal Libet study setup, instead of actually perceiving intention in the tasks, are forced into cognitively inferring that intention reports must occur before movement reports. Dominik et al. argued that their findings were evidence that there is only one event to report in the Libet study, that of movement. When participants are asked to report intention but are not aware of a distinction between intention and movement, they simply report intention at the same time as movement. When participants *are* aware of the movement/intention distinction, as in a normal Libet study setup, they infer that their intention reports should precede their movement reports. Dominik et al. (2017, p. 260) claim that in the normal Libet study setup, participants show an “anchoring” effect; the experience of moving and making a movement report serves as an anchor from which to infer when an intention report *should* occur. Dominik et al. concluded that their findings are evidence that intention reports in the Libet study lack validity, since they appear to be inferences rather than genuine reports.

Other lines of research have also provided evidence that intention is inferred or constructed. Banks and Isham (2009), as well as Haggard and Clark (2003), provided evidence that intention reports in the Libet study are retrospectively inferred based on the time of movement and can be influenced by pairing the movement with a delayed auditory tone. These reports were shown to be delayed more as the tone was more delayed from movement time. Although these authors concluded that their results were evidence that conscious intention does not cause the movement, the results might just as well lead to the conclusion that obtained intention reports are an invalid artifact of the experimental setup. Alternatively, it could be that the study by Dominik et al. (2017) has more accurately captured intention reports than traditional Libet studies; perhaps intention simply occurs more or less at the same time as movement. More specifically, it could be that the perception of intention occurs around the same as movement. Banks and Pockett (2007) noted the existence of the *intentional binding effect*, which makes intentions seem closer in time to their effects than they actually are. If this effect is at play in the Libet study, perhaps the experimental setup of Dominik et al. allows intentions to be reported closer in time to when they are actually perceived. These and other issues, including other possible interpretations of results, are explored by the current study. The major aim of the current study was to attempt to replicate the results of Dominik et al.

Banks and Pockett (2007) also point out the possibility that clock-monitoring may lead to delays in intention reports due to a memory or perception bias. If the Libet-style clock naturally leads to inevitably delayed reports of intention, there may be no way to know, since there is no objective measure of the subjective phenomenon of intention. Further, Banks and Pockett (2007, p. 661) note that there may be "systematic reporting priority" when two events are occurring simultaneously (i.e., monitoring the clock to make a report and monitoring oneself for intentions). If such an effect is occurring, there may be no way to know which event is being given monitoring priority and in which direction this would bias the intention reports. Since the tasks in the Libet study involve multiple ongoing cognitions and decisions (e.g., inhibit movement and blinking, wait one rotation before moving, monitor the clock, move spontaneously but do not preplan, etc.), this effect is likely to be present (see Discussion).

In a review of potential confounding variables in the Libet study, Pockett and Miller (2006) found a significant difference of about 30ms between movement reports based on how the report was framed. The report was earlier when participants were told to report the beginning of a key press rather than the end. Since this study employed a key press instead of the Libet wrist flexion task, it is possible that such a discrepancy is larger or smaller in the wrist flexion task. These authors noted that most, if not all, studies have failed to make such a distinction between start and end of movement. They conclude that all future studies should give precise instructions about which event to report, and this study has followed this advice (see Method).

In a review of common criticisms of the Libet study, Banks and Pockett (2007, p. 659) also noted that although there have been many replications which basically accord with the original findings of the Libet study, "the numbers are very variable." In other words, although most replications have produced similar overall results, there are considerable differences in data among experiments, among participants, and even within the same participant. Some participants in the Libet study actually produced intention reports that preceded RP onset. Trevena and Miller (2002), as well as Miller et al. (2011), found that some intention times came after movement. Banks and Pockett dismissed such variability, arguing that the means and standard deviations found in the Libet study were too small to reverse the main findings; the interval between the RP and intention was thought to be too large for this variability to make a difference. However, recall that the main finding of the Libet study was the temporal relation between the RP and intention. As we have seen, the late RP is a better index of motor preparation than the early RP, and it is possible that there is enough variability in the Libet study – as well as in its variants and replications – to reverse the main findings if one compares the late RP and intention, since the typically found interval between late RP onset and intention (200-300ms or less) is much smaller than the interval between RP onset and intention (350ms or more). This is another reason the current study emphasizes the relation between the late RP and intention. The importance of examining individual data will be further discussed later in this section.

Banks and Pockett (2007) also reviewed systematic sources of error and variability in the Libet study and its replications. One candidate for error is the *flash-lag effect*, a well-established visual illusion in which a moving object is misperceived when compared with a stationary flashing object. In the Libet study, by analogy, the misperception of the revolving dot might be caused by trying to compare it with one's conscious intention to move (i.e., making the intention report). Banks and Pockett note that the effect could cause errors up to 100ms in either direction, but it is not known if the effect does in fact occur in the experiment. As before, Banks and Pockett dismiss the importance of this potential error on the basis of the large interval between the entire

RP and intention, but this error may be large enough to change the relation between the late RP and intention. Another potential source of error is the processing time required to make a visual report using the revolving dot method. Most studies do not address this issue and simply assume that there is no lag between the dot's actual position and the participant's real-time perception of the dot. However, since it takes about 100ms for the visual cortex to process visual stimuli, the real-time intention report of the dot may be 100ms behind where the dot actually is. This means that reports would actually be closer in time to movement than is perceived. Libet (1985) anticipated this criticism and cited earlier work in which they claimed to have proven that such perceptual lags are canceled out by the nature of conscious processing. However, Banks and Pockett (2007, p. 661) note that this proposed mechanism, *subjective back-referral,* is not well accepted. Thus, it is not clear whether there are lags of this sort. This study does not examine the possibility of the flash lag effect or subjective back referral, but their possibility must be acknowledged when results are interpreted.

In addition, Banks and Pockett (2007) commented on the use of S (stimulus) reports by the Libet study. As noted, Libet et al. (1983) measured each participant's error when using the clock to report the timing of a stimulus delivered to the back of the hand and found an overall error rate of 50ms (i.e., the participants reported the stimulus 50ms too early). Consequently, Libet et al. corrected intention reports by making them 50ms later in time. However, Banks and Pockett noted that biases were different among participants and even varied by day. Further, the Libet study and its replications also show considerable variation in movement reports, with some coming before or after the actual movement. It is unclear whether participants are making reliable and stable reports, and it is unclear whether errors in reporting are uniform across all types of reports. Thus, variation among and within participants' biases in reporting is another potential error which could enlarge or shorten the temporal interval between the late RP and intention.

One recent study had a participant whose average intention report preceded RP onset (Verbaarschot et al., 2015). While this result may have been due to failure to follow instruction (i.e., failure to make a truly spontaneous movement), this result raises an important issue. The authors noted that most studies have only examined averages across all participants and trials, to the neglect of individual results. Thus, although Libet's study (Libet et al. 1983) and its replications show a consistent overall effect, individual participants' results may contradict the findings. As noted, in the Libet study and some replications, some intention times were indeed found to precede the RP. While the study by Verbaarschot et al. (2015) focused on the temporal relation between the RP and intention, the same conclusions apply to the late RP and intention. Alongside grand averages, individual averages must also be examined, even individual trials if possible.

We have already seen that time-locking EEG can create misleading artifacts. Examining individual data is one way to prevent further artifacts. Importantly, even if the grand average strongly demonstrates a temporal relation between the late RP and intention, one or more individual averages may show a violation of the temporal relation. If that occurs, the supposed temporal relation may be cast into doubt. If that does not occur, then then confidence in the temporal relation is strengthened. Since the RP and late RP have both been claimed to reflect the unconscious initiatory processes that cause movement, their temporal relation to intention and movement reports must *always* hold true. That is, even one indubitable violation of the supposed causal relation would be enough to invalidate it. Causal relations must hold true 100% of the time, and the only way to answer the question of causation between the late RP and intention is to examine both grand and individual averages. Furthermore, outliers should not be easily dismissed because they do not fit the presumed causal relation. Many studies have removed outliers from the data or trimmed means on the intention task, assuming they are incorrect because they stray too far from expected values (Verbaarschot, 2015). Of course, outliers are usually due to chance, corrupted data, participant errors, etc., but if they must be removed, the process should be precisely reported and justified, and the study's results must be tentatively interpreted. In addition, it should be noted that it has always been the case temporal succession of events does not necessarily entail causation (Papanicolaou, 2017). It is one thing to prove that one event *precedes* another event (e.g., the RP precedes intention reports), but quite another to prove that the first event *causes* the second.

Contradicting evidence regarding the timing of intention reports – as well as discrepancies in early RP and late RP data – may be the result of individual differences in participants among studies. This is especially important for studies, including Libet's original study, which use a small sample size. Most studies have produced results showing intention times between approximately 150-200ms prior to movement. However, a study by Penton, Thierry, and Davis (2014) found an average intention time of -253ms, and Lau et al. (2004) found an average intention time of -228ms. Such discrepancies have largely been ignored due to the fact that they were miniscule compared to the onset of the RP and thus incapable of reversing the main finding of the temporal relation between the RP and intention (Banks and Pockett, 2007; Penton et al., 2014). However, in light of the previously discussed evidence indicating that the late RP is the appropriate activity to compare to intention, these small differences may indeed be significant. As noted, based on converging lines of evidence and their own research, Schurger et al. (2016, p. 79) hypothesized that the actual time of the "neuronal commitment to move" is about 150-200ms prior to movement, which is clearly in line with and even later than average intention reports. Clearly, both the late RP and intention reports require more precise and consistent measurement.

Various individual differences should be explored as potential factors contributing to the variation in the late RP and intention reports. The study by Penton et al. (2014) found a relationship between attributional style and intention reports; those with a more pessimistic style had earlier reports. These authors speculated that earlier reports, which would lead to longer intervals between intention and movement, might be associated with individuals perceiving themselves as having less control over their actions. Factors such as locus of control, inhibitory control, interoceptivity, and timing of S reports should also be incorporated into analyses. Penton et al. did not find significant differences in intention reports based on interoceptive sensitivity but did find this factor to be correlated with attributional style. The current study examined attributional style but not interoception or other related factors. Future research could also compare participants' early and late performance. These analyses would help illuminate normal and abnormal response patterns. Associations between all these individual factors and the early RP, late RP, and intention reports should be explored. It seems possible that many of these factors might be mediating or moderating variables which influence intention reports and/or the behavior of the RP and late RP. If individuals show deviations from the grand averages, these differences should be explored, and individual factors might illuminate the data. Lastly, there has been minimal exploration of what the experience of the task is actually like for the participants. The phenomenology of completing the task, making the decision, and making reports needs to be qualitatively explored in future research. It seems possible that multiple phenomenologies of conscious intention and/or agency exist, and they might be reflected by different intention reports (Nahmias, Morris, Nadelhoffer, & Turner, 2004).

Unfortunately, the terminology for the impetus to make the voluntary movements in the intention and movement tasks, as well as what is to be reported in the intention task, varies from one study to another. Some examples are “intention,” “urge to act,” and “sense of intention.” The lack of standardization of terms across studies seems like a problem, since participants are likely to behave differently depending on which word is chosen. For example, it seems likely that a person would behave differently if told to act on an urge instead of told to act on an intention. The instructions to the participants are somewhat uniform, however; they typically ask participants to be “spontaneous” and not “pre-plan” any of their acts. Thus, the overall concept of intention that is usually employed is one of passive reception. Participants are to allow the urge to act to spontaneously arise. The common, everyday meaning of conscious intention does not necessarily carry this connotation of passivity (Papanicolaou, 2017), and it seems likely that participants could easily become confused. The validity of intention reports as valid measures of intention, and the validity of the entire experimental task as measuring a consciously intended movement, has been called into question. For some, the task is too restrictive to count as a consciously intended movement, and many questions have been raised about the ecological validity of the type of movement in the Libet study (Banks and Pockett, 2007; Mele, 2013; Nachev & Hacker, 2014). We will return to these and other questions in the Discussion.

II. Statement of Problem and Hypothesis

The literature review makes it clear that there is much variability in intention reports, with some research even suggesting that these reports may be an artifact of the experimental setup (Dominik et al., 2017). The basic question raised by Dominik et al. is whether the intention reports pioneered by Libet et al. (1983) are valid measures of intention. The findings and conclusions of the Libet study center on the temporal relation of the RP and intention. These findings depend on the validity of the RP and intention reports. If it is shown that the manner in which the Libet study and its replications have measured intention is invalid, then the findings of the Libet study are also invalidated. As discussed, participants in the study by Dominik et al., when naïve about the distinction between moving and intending to move, showed no difference in reports for movement and intention. The current study centered on replicating the methodology and results of Dominik et al. (2017). I expected to corroborate the basic findings of these authors. Specifically, I hypothesized that because participants would not be induced to make a distinction between moving and intending to move, intention reports and movements reports would not differ.

III. Method

Participants

A power analysis with a desired power level of .90 and an alpha level of .01 revealed that a minimum sample size of 16 participants was required. This power analysis was based on published data from a variant of the Libet study (Verbaarshot et al., 2016). The current study aimed to collect data from twice this amount to account for attrition and data loss. This study collected data from 29 students (7 male) from the Psychology Department at Eastern Kentucky University. Students ranged in age from 18 to 64 years (M = 22.07). Three students were left-handed. All participants were recruited using the SONA system and given outside activity credit for their participation. The study required 1.5 hours of participation. Three credits were given to those who fully participated. Before beginning the study, a participant was required to affirm that he/she: (a) was 18 years of age or older, (b) possessed normal or corrected vision, (c) possessed normal or corrected hearing, (d) was free of any prior or current neurological, psychological, or sleep disorder, (e) possessed no learning or reading disability, and (f) was not pregnant.

Seven participants were excluded from analyses after post-hoc examination of data. Of these, four were excluded due to yielding unanalyzable EMG data, one was excluded due to having heard of the Libet study (per the PEQ; see below), one was excluded due to having data accidentally deleted, and one was excluded due to data being corrupted for an unknown reason. After these exclusions, participants (N = 22; 6 male; 3 left-handed) ranged in age from 18 to 64 years (M = 22.55).

Materials

Libet Clock and Visual Stimuli. All experiments used visual stimuli presented in E-Prime 2.0 (Psychology Software Tools, USA). Following the example of recent research (e.g., Jo et al., 2013; Schmidt et al., 2016), a Libet-style analog clock with a diameter spanning a visual angle of approximately 3° was presented on a computer screen (see Figure 5). The clock was identical to that used by Jo et al. (2013), as these researchers were generous enough share their coded E-Prime file which included a programmed clock. The visual angle of 3° was approximated with each participant by standardizing the setup of equipment rather than stabilizing participants' heads with a harness. This standardization involved keeping the computer monitor, desk, and chair in the same position for each participant. The 3° visual angle was achieved by having participants' eyes approximately 67cm away from the computer monitor, on which a 3.5cm clock face was presented. The clock hand rotated clockwise with a revolution period of 2.55 seconds. It was programmed to "rotate" in unison with the 60Hz refresh rate of the computer monitor (i.e., the next clock tick occurred simultaneously with each screen refresh). There were 153 tick marks, each one corresponding to about 16.667 milliseconds (i.e., 2.35° of the clock face), which was the maximum temporal resolution of the clock. All visual stimuli, including the clock and all its parts and on-screen instructions, were white and presented on a gray background.

Consent Form. The IRB-approved Consent Form (Appendix B) provided an overview of what the study entailed, and it detailed inclusion criteria, participant rights, privacy, safeguarding of personal information, and all potential risks to safety and comfort.

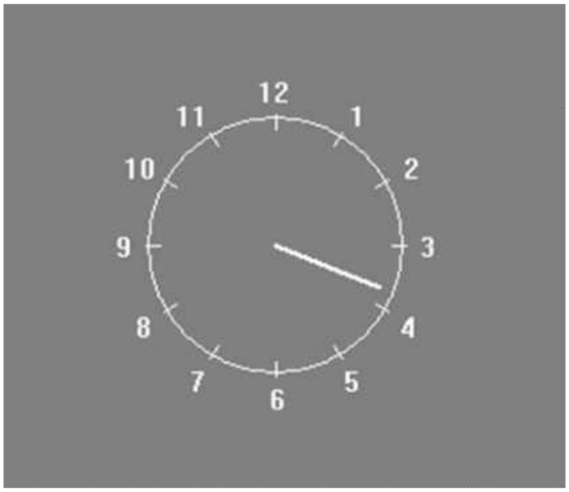

Figure 5: Enlarged image of the analog clock used in tasks.

Attributional Style Questionnaire. The Attributional Style Questionnaire (ASQ; Appendix C) was developed by Peterson et al. (1982). Many versions of the measure exist, such as the Revised ASQ and the Expanded ASQ (EASQ). However, the original ASQ continues to be widely used alongside these alternate versions. The previously discussed study by Penton et al. (2014) employed the original ASQ. The ASQ examines how individuals perceive and explain life events in terms of internality (i.e., whether the person or some external factor is responsible), stability (i.e., whether the cause will continue to exist), and globality (i.e., whether the cause occurs in multiple life domains). It asked participants to read a description of an event. There were six positive and six negative events. Participants answered four questions about each event before proceeding to the next event. First, they wrote down what the major cause of each event would be if the event happened to them. Then responded to three questions using a 7-point Likert scale. The first question pertained to internality; it required a rating from 1 – 7, with 1 corresponding to “totally due to other people or circumstances” and 7 corresponding to “totally due to me.” The second question pertained to stability; it required a rating from 1 – 7, with 1 corresponding to “will never again be present” and 7 corresponding to “will always be present.” The third question pertained to globality; it required a rating from 1 – 7, with 1 corresponding to “influences just this particular situation in my life” and 7 corresponding to “influences all situations.” The test was analyzed and scored using the method outlined by Penton et al. (2014), which yielded a composite score and a binary classification of positive or negative attributional style.

Personal Information Sheet. The Personal Information Sheet (Appendix D) asked participants to provide basic demographic information. This included sex, age, and race. In addition, it asked participants to identify their handedness and to indicate if they were taking any medication or substance which might impair their ability to focus during the experiment. Instead of asking for a name, it only asked for a SONA code. No information from the form was used as exclusion criteria. The information on this form was used to aid in understanding the nature of the data sample.

Post-Experiment Questionnaire. The Post-Experiment Questionnaire (PEQ; Appendix E) was given to participants at the conclusion of the study. It consisted of a series of questions about participants’ experiences with and perceptions of the study. Researchers administered the questionnaire verbally and recorded responses. Participants were asked to respond to nine yes/no questions and were given the opportunity to briefly explain their responses. The final question asked them to select one of three terms to best describe what they reported in the W Task (i.e., intention, urge, or impulse) or to provide their own word. Responses to questions were coded according to the response given (i.e., yes, no, or maybe/unsure). Follow-up explanations of responses were not coded but were used to provide qualitative information about participants' experiences with the study (see Discussion).

Debriefing Form. The Debriefing Form (Appendix F), given to participants after conclusion of the study, provided an overview of the original study conducted by Libet et al. (1983). It discussed how the current study attempted to address some of the criticisms of the Libet study. The form explained to participants how and why they were assigned to one of two groups (i.e., M-First or W-First) and the purpose of the tasks they completed. It also explained how data analysis would proceed.

Apparatus for Behavioral and Psychophysiological Data. The recording of all behavioral data was completed using E-Prime 2.0 software (Psychology Software Tools, Inc.). The recording of all physiological data was completed using AcqKnoweldge 3.9.1 software (Biopac Systems, Inc.). These software programs ran on two separate computers, each on an HP ProDesk 600 G2 MT (HP Inc.), but were linked together for data signaling and collection. A BENQ model RL2455 computer monitor with a 60Hz refresh rate was used. The monitor was set at a screen resolution of 1280 X 1024. Auditory stimuli were presented with Dell A225 speakers, standardized at 100% system volume and 50% speaker volume. A Logitech M100 USB mouse was used on all tasks. Tests revealed that the mouse had a maximum polling rate of 125 Hz (i.e., maximum temporal resolution of 8ms) on the computer.

Electromyography (EMG) data were collected and analyzed via a Biopac MP100A recording system (Biopac Systems, Inc.). On this system EEG100C amplifiers were used, with a gain of 20,000, a 35 Hz LPN (low pass notch) filter, and .1 Hz HP (high pass) filter. For EMG recording two Biopac EL258 Ag-AgCl (silver-silver chloride) unshielded electrodes with holes were used. Impedance of electrodes was checked using a UFI 1089 Checktrode MKIII (UFI). Following standard procedures, the satisfactory impedance level for electrodes was 5 KΩ or below.

Prior to running participants EMG pilot testing was conducted, in which EMG signal clarity was assessed at various locations on the hand, wrist, and fingers. These tests revealed that optimal EMG could be obtained using one electrode behind the trapezoid bone (i.e., lowest knuckle) of the right index finger and one electrode on the ventral forearm of the right arm, just below the wrist (see Figure 6). The EMG signal was used to objectively measure when the participants' finger movement began during a mouse click. The mouse click initiates a digital trigger in the software programs, but this trigger is inevitably delayed in time past the earliest moment of movement, since completing the press of a mouse button necessarily takes more time to execute than simply initiating the first movement to press the button. The EMG signal was used to correct the digitally recorded triggers for mouse clicks (see Data Analysis). This was a crucial aspect of the study, as it was essential to know with precision the moment participants' movements began (see EMG subsection of Discussion).

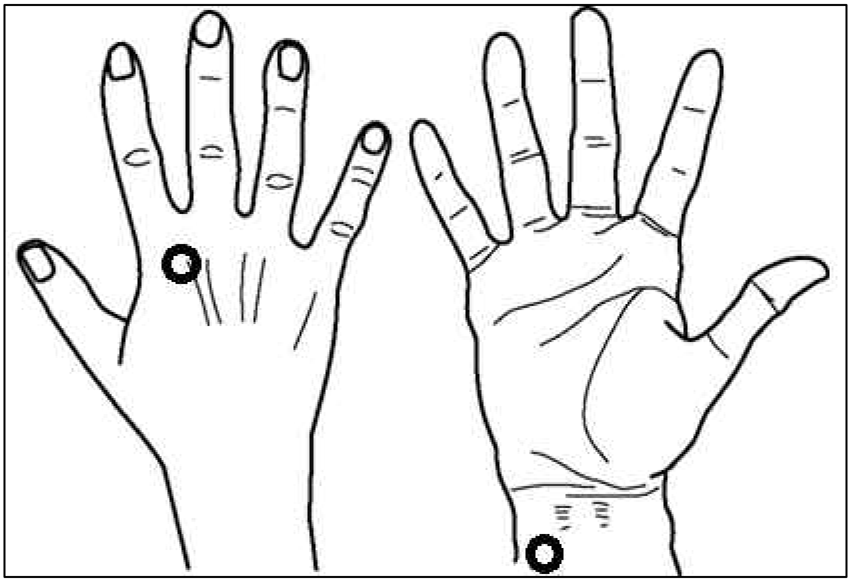

Figure 6: Standardized placement of EMG electrodes.

Tasks

Participants completed three distinct tasks. All tasks consisted of training trials, regular trials, and a 60 second break after half of the regular trials were completed. All tasks used a Libet-style clock. This clock consisted of a clock face with a hand rotating clockwise at a revolution period of 2.55 seconds (see Materials for a detailed description). Participants viewed this clock in order to note the location of the clock hand when a certain event occurred. The event depended on which task they were completing (see the specific task descriptions below). Participants noted the position of the clock hand at the time of a certain event and then later reported this position. They reported this position on a screen on which the clock face, but not the clock hand, was present. Participants simply clicked on the clock face to indicate where the clock hand was when the event occurred (see Figure 7 for an example of a report screen).

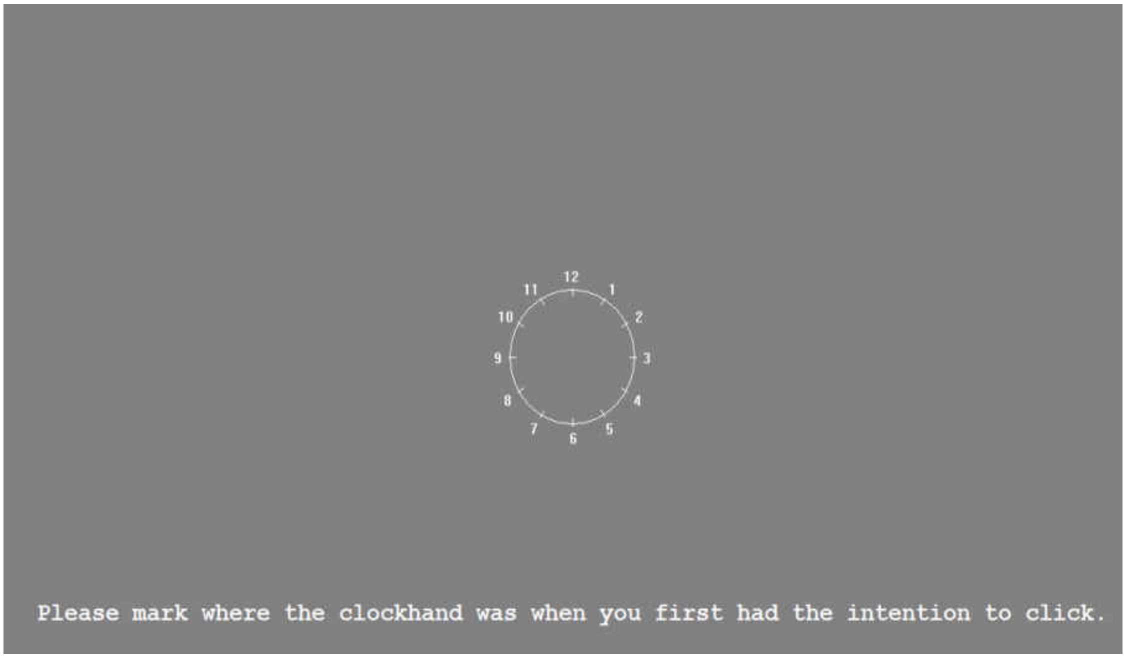

Figure 7: Report screen from the W Task.

When the clock was first presented during a trial, the clock face appeared for a random interval of 1000-2000 milliseconds before the clock hand appeared and started rotating. The clock hand could begin rotating from any position on the clock. A mouse click was required in order to stop the clock hand rotation. After this click, the rotation continued for a random interval of 1000-2000 milliseconds. On the next screen, participants clicked on the clock face to make the report of the event in question. As noted, Libet et al. (1983) found no difference in recall modes, and since most replications use only the absolute recall mode (i.e., reporting the actual perceived position of the clock hand), the current study employed only this recall mode (see Introduction).

S Task. Participants waited to hear a tone (a standard beep of 100ms duration) played through the speakers while viewing the clock. The beep was played at a random time during the 2.5-7.5 second interval after the clock hand appeared (Jo et al., 2013). They were to then click the mouse any time after they heard the tone in order to stop the rotation and bring up the report screen. Participants were asked to report where the clock hand was at the moment they first heard the beep.

M Task. Participants were asked to make a spontaneous voluntary movement – a mouse click – at the time of their choosing. Following standard procedure, they were, however, instructed to wait for the clock hand to make one full rotation from its starting point before moving, so that an EMG baseline could be calculated during data analysis (e.g., Jo et al., 2013). Researchers emphasized to participants that the mouse clicks needed to be spontaneous and not preplanned. Following the click, participants were asked to report where the clock hand was at the moment they *first* initiated their hand movement to click the mouse. This was done in accordance with the findings of Pockett and Miller (2006), which, as discussed, showed a significant difference in reports of movement depending on whether the beginning or the end of a movement was emphasized.

W Task. This task was identical to the M task in all aspects but one. Instead of reporting the moment of movement initiation, participants reported the moment they first had the intention to move. Because the aforementioned findings of Pockett and Miller (2006) seem extendable from movement reports to intention reports, researchers emphasized that these reports indicate the *earliest* moment of the intention to move.

Procedure

Upon arrival, participants were asked to provide their unique SONA code for identification purposes. They were then brought into the participant room. Researchers first discussed the consent process with participants, providing them with an overview of all the information on the consent form and allowing participants to take as much time as needed to read the consent form. Once participants had finished reading, the researchers allowed them to ask questions. They then asked them to provide consent by signing the form. Researchers then asked participants to complete the Personal Information Sheet and the ASQ. Once those were completed, researchers connected participants to the EMG equipment. All tasks required moving and pressing the left button of a standard computer mouse. Though participants' handedness was determined prior to completion of tasks, all participants were required to use a right-handed mouse, so all EMG connections were to the right hand and arm (see Materials). Before giving instructions for the first task, researchers ensured that the chair, table, and computer monitor were in their standardized positions to approximate the correct distance from the participants' eyes to the screen, in order to achieve the correct visual angle for the clock. The overhead lights in the room were turned off for all participants unless they requested that they remain on. The speakers and computer system volume were checked to ensure they were at standardized levels.

Refer to Figure 8 for a visual representation of the flow of task completion. All participants completed the same three computer tasks (i.e., the S Task, M Task, and W Task), but the order of task completion differed between groups. All participants completed the S Task first, but participants differed in the order in which they

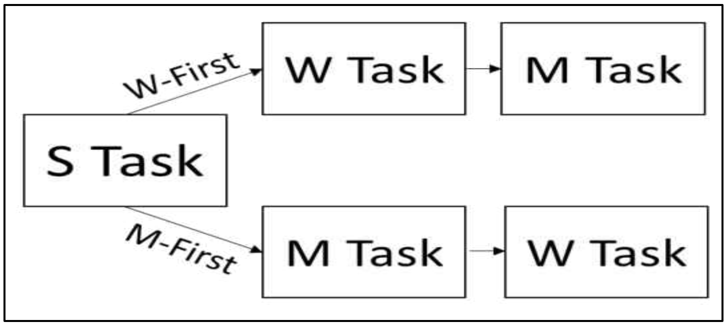

Figure 8: Task order by group.
M-First: Group in which the M Task was completed prior to the W Task
W-First: Group in which the W Task was completed prior to the M Task
S Task: Report of onset of stimulus (tone)
M Task: Spontaneous mouse click, then report of movement
W Task: Spontaneous mouse click, then report of intention

completed the M Task and W Task. Prior to arrival, participants had been randomly assigned to one of two groups, the M-First group or the W-First group. Aside from the difference in order of task completion, the two groups underwent the experiment identically. The M-First group completed the M Task prior to the W Task, and the W-First group completed the W Task prior to the M Task.

Prior to initiating the S Task, researchers gave participants general information, rules and instructions for the clock. Participants were informed that the clock hand could start in any random position. They were instructed to fixate their eyes on the center of the clock and note the clock hand position rather than following the clock hand with their eyes or using some other strategy to determine its position. They were also instructed to minimize blinking and moving during trials. Participants were told they could move at their own pace through trials and, thus, could blink and move after individual trials were completed (before clicking to initiate the next trial). All these instructions were given in order to minimize any potential interference with participants' ability to complete the tasks and interference with the EMG recording itself, which is extremely sensitive to movement.

Participants were informed that they would first complete training trials with the researchers present and then complete regular trials once the researchers had left the room. Researchers ensured that participants had appeared to understand the task before leaving the participant room, as evidenced by performance on training trials. After one task was complete (i.e., all regular trials were complete), researchers returned to the room to access the next task on the computer and provide instructions. This allowed the participants to take a short 1-2 minute break from completing tasks.

The S Task consisted of five training trials and 30 regular trials. Participants were allowed to move at their own pace once a trial was complete; they had to click after a trial ended before the next trial would begin. They were instructed to monitor the clock hand while waiting to hear the beep, and they were instructed to simply click sometime after they heard the beep in order to end the clock rotation. Researchers emphasized that the task was not about reaction time to the beep (i.e., how quickly they clicked after the beep was not important) and that the report of the clock hand was the key aspect of the task (see Appendix G for the on-screen instructions presented for the S Task).

After completion of the S Task, participants completed either the M Task or W Task, which consisted of 5 training trials and 60 regular trials. Crucially, participants were not given any indication that there would be a separate event to report in their third task when completing this second task. That is, participants were simply given instructions to make a spontaneous voluntary movement (i.e., a mouse click at any time after the clock had made one rotation; see Tasks subsection of Method) and, depending on group, told to report either the moment they initiated their movement or the moment they had the intention to move. Those in the M-First group, completing the M Task, were not told about the possibility of making an intention report, and those in the W-First group, completing the W Task, were not told about the possibility of making a movement report. Researchers were deliberate in not sharing this information with participants, and the language used mirrored that of Dominik et al. (2017). Even the title of the study, which was printed on forms given to participants, was changed to avoid any possible influence. The study was originally named “Conscious Intention and Free Will: A Hybrid Replication of the Libet Study,” but it was changed to “Volition and the Readiness Potential” so that the word “intention” would not appear on forms given to participants.

Given the lack of agreement concerning the term used to describe the impetus to make a voluntary movement, as well as what is reported in the W Task, researchers deliberately used both the words “urge” and “intention” when giving instructions to participants. Specifically, researchers used both words when verbally describing the spontaneous click to be made in both the M Task and W Task, as well as the report to be made in the W Task. Researchers used the phrase “report where the clock hand was when you first had the intention, or urge, to click.” In addition, the on-screen instructions for both tasks asked participants to “click when the urge arises,” and the instructions for the W Task asked participants to “report the time when you first had the intention to click” (see Appendices H and I for the instructions presented on screen to participants for the M Task and W Task, respectively).

As noted (see Tasks subsection of Method), researchers emphasized that participants make a spontaneous, unplanned mouse click. After all trials were complete, participants were prompted with a question about preplanning: “Did you preplan any of your movements during the regular trials you just completed?” (see Appendix J for a script of the preplanning question presented for the W Task; the question was identical for each task, aside from changing to denote the total number of regular trials completed). Participants were to respond by estimating how many trials they preplanned.

Participants then completed the third task (i.e., either the M Task or W Task, depending on group). Researchers emphasized that the task was identical to the one just completed except that the current one required a different kind of report. Researchers again required participants to complete five training trials before proceeding to complete the 60 regular trials. Participants were prompted with a preplanning question at the end of the third task as well. After this task, researchers reentered the room, unconnected the EMG electrodes, and informed participants that the study had ended for the day.

Participants returned for a second day of the study (see Future Analyses subsection of Discussion for an overview of this second part of the study). At the end of the second day, they completed the PEQ, which is included in the analyses of the current study. They were also given the Debriefing Form at the end of the second day.

Data Analysis

ASQ. Responses to the ASQ were collected, but they are not appropriate for comparison to W Task results from the current study. Penton et al. (2014) compared attributional style with performance on the W Task in a normal Libet-style study. As we have seen, the crux of this study is that it departs from the normal Libet study setup, in that participants are not trained to distinguish between movement and intention reports and are in one of two groups which differ in order of task completion. Thus, analyses on the ASQ were not conducted for this study. As noted, participants returned for a second day after completing the current study. Analyses will be conducted to compare the ASQ to these results (see Future Analyses subsection of Discussion).

S Task. Individual averages and standard deviations for the S Task are presented in Appendix K[1]. The purpose of the S Task was to determine participants' average error in using the analog clock to make reports of events (Libet et al., 1983). Libet et al. used the average error of an individuals' S Task reports (i.e., report of the onset of an external stimulus compared to its objective onset) to correct all reports made by participants in all tasks. The current study intended to follow this methodology, but due to the extreme variability in within-subjects results on the task, we decided to not use S Task results to correct other reports. This variability was present even when reports equal to or greater a clock quadrant (i.e., three clock numbers, or approximately 638ms) were treated as outliers and removed from the data. Standard deviations, after outliers were removed, ranged from 38 to 215 milliseconds (M = 93). The variability within participants' reports was likely due to a combination of the small quantity of trials collected (30) and the inherent imprecision of using a small analog clock to make reports (see Materials for a detailed description of the clock and Limitations for criticism of the clock). We decided it was optimal to allow this variability to remain in

[1] All tables of results are presented in appendices.

the reports of the other tasks; this seemed preferable to correcting *all* reports by an *average* error amount with a large standard deviation amount, calculated from 30 trials. Furthermore, Dominik et al. (2017) did not collect data on the S Task, so using S Task results to correct the M Task and W Task would have been a departure from the study whose methodology we aimed to replicate as much as possible.

EMG. As discussed, EMG data were collected in order to determine the objective time that hand movement began when participants made their spontaneous mouse clicks. Researchers visually inspected and corrected digital triggers on all trials for all tasks other than the S Task (wherein the objective time of the mouse click was unimportant because the objective event in question was the tone) so that they would indicate the objective time that movement began. Note that it was not important to know the objective time of other mouse clicks made by participants during tasks (e.g., clicks on the clock face to make reports), so EMG corrections were not made for these clicks. The onset of EMG was determined by visual inspection of individual trials; the variability in data within and among participants prevented development of a standardized measure for determining onset. The criterion for determining onset was a significant departure from the preceding baseline of activity. In particular, researchers looked for either an uptick or downtick in activity that led to a prolonged departure from baseline all the way up to and past the time of the digital trigger. See Figure 9 for an example of EMG activity compared to a trigger. In data files, researchers replaced the timestamp of the digital triggers with the timestamp indicated by the onset of EMG activity. For descriptive purposes, each participants' average EMG correction for the M Task and W Task was calculated (Appendix L). Visual inspection of EMG data

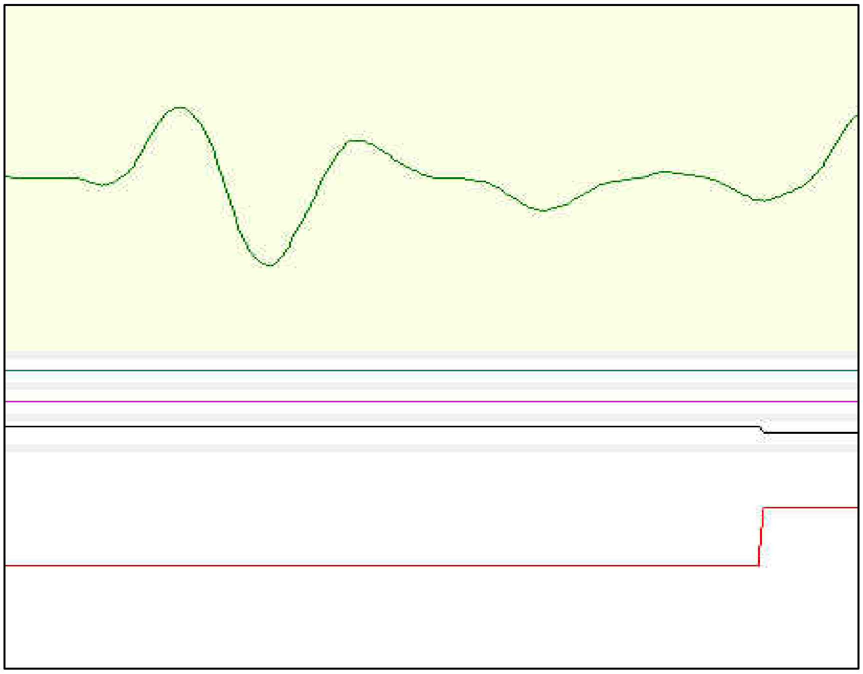

Figure 9: Screenshot of EMG onset (top pane) compared to digital trigger (bottom pane).
On this trial, EMG activity begins about 128 milliseconds prior to the trigger.

revealed that nearly all EMG onset times occurred between 70-200ms prior to the triggers. The very small quantity of trials with EMG times outside this range was excluded from data analysis. That is, these trials were not included in calculations for the M Task and W Task. In addition, those trials in which it was unclear when EMG activity began (e.g., a noisy signal or appearance of multiple clicks) were also excluded from data analysis. Analyses showed that EMG corrections averaged -130 milliseconds (SD = 29) for the M task and -133 milliseconds (SD = 30) for the W Task.

The crucial aspect of the M Task and the W Task is the report made by participants. In each task, data analysis centers on comparing the report to movement onset as objectively determined by EMG onset. Calculations were set up such that movement onset time was subtracted from the report time. In this way, positive numbers reflected late reports, and negative numbers yielded early reports. For example, if a movement occurred 1000ms after a trial began and the participant reported that the movement occurred at 1200ms, the difference of objective movement time minus report time is 200ms, indicating a report that was 200ms too late.

During data analysis, actual comparisons between movement times and reported times were carried out using clock time (i.e., numbers between 1-12) before they were converted to milliseconds. Recall that participants made reports by clicking on the clock face to indicate the clock hand's position when an event occurred. These reports were stored as clock time with precision of three decimal places. For example, a participant clicking on the clock face halfway between 3 and 4 would yield a reported clock hand position of 3.500. The digital triggers corresponding to mouse clicks were also stored as clock time, so a mouse click occurring when the clock hand was at 6 would be stored as 6.000. These triggers, as previously discussed, were manually corrected with EMG data. Once corrections were made, movement times were subtracted from reports and then converted from clock time into milliseconds. Those trials on which there was discrepancy of 1000ms or more (i.e., approximately 4.71 units of clock time) between report and movement time were treated as outliers and removed from the data.

IV. Results

Participant, group, and grand average means for the M Task and W Task are presented in Appendix M. A 2X2 Mixed Measures Analysis of Variance (ANOVA) was conducted in order to determine if there was an interaction effect of group and task. As discussed, there were two groups, M-First (n = 10; 2 males; average age 20.3 years) and W-First (n = 12; 4 males; average age 24.42 years) and two tasks, the M Task and W Task.

The results of the ANOVA indicated a significant interaction of group and task, F (1, 970) = 89.571, p < .001, η_p^2 = .085. Group membership affected participants' performance on the tasks. There was also a main effect of group, F (1, 970) = 15.401, p < .001, η_p^2 = .016 and a main effect of task, F (1, 970) = 21.768, p <.001, η_p^2 = .022.

Pairwise comparisons were conducted within the ANOVA via analyses of simple effects. Figure 10 contains a graph of the means and standard errors used in the pairwise comparisons between groups. Means were significantly different on all comparisons except the most crucial one: there was no difference between groups on the second task completed (i.e., the first task after the S Task). That is, there was no difference in reports on the M Task of the M-First group (M = .114ms) and the W Task of the W-First group (M = .108ms).

Turning now to the other pairwise comparisons, on the M Task, the W-First group (M = .066ms) made significantly earlier reports compared to the M-First group (M = .114ms). On the W Task, the M-First group (M = -.011ms) made significantly earlier reports compared to the W-First group (M = .108ms). Regarding the third task completed, the W Task reports made by the M-First group (M = -.011ms) were significantly earlier than the M Task reports made by the W-First group (M = .066ms). The only possible within-groups comparison, between the M Task and W Task, was significant. In both groups the third task completed was reported significantly earlier than the second task. In the M-First group, reports were significantly earlier for the W Task (M = -.011ms) than the M Task (M = .114ms). In the W-First group, reports were significantly earlier for the M Task (M = .066ms) than the W Task (M = .108ms). In other words, regardless of whether participants first made intention or movement reports, they then reported the other event as occurring earlier in time.

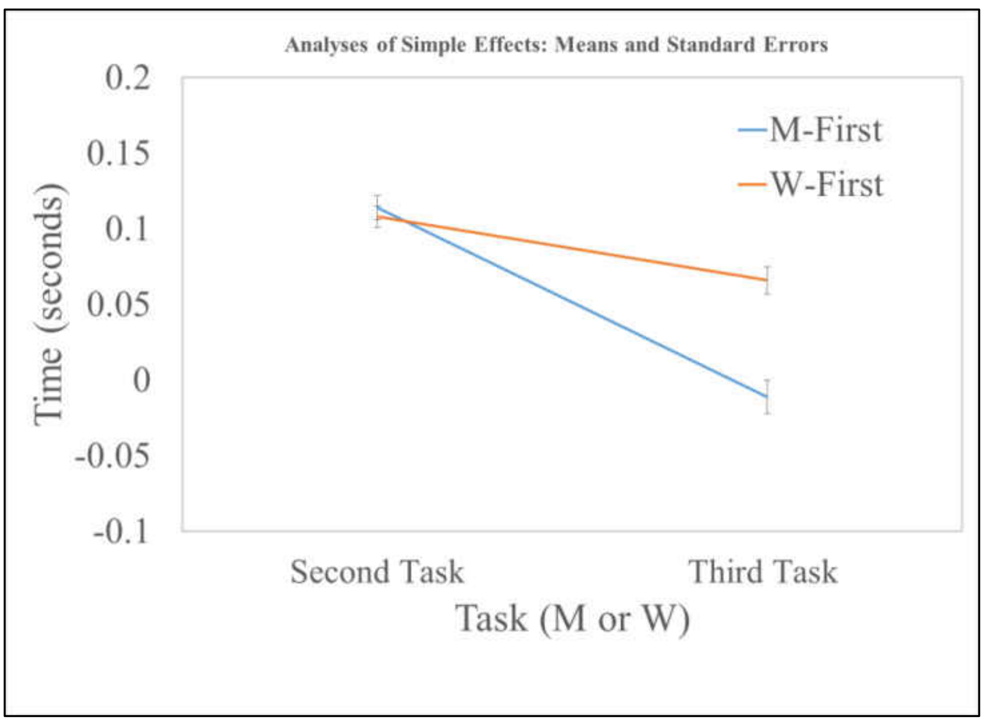

Figure 10: Line graph comparing performance on tasks.
The M-First group completed the M Task as “second task” and the W Task as “third task” and the W-First group completed the W Task as “second task” and the M Task as “third task.” There was no difference between groups on the second task.

The same analyses were conducted on a dataset *without* EMG corrections in order to determine whether the corrections changed results. This dataset used the digital triggers as they were and did not take into account any EMG data. The results of the ANOVA indicated a significant interaction of group and task, $F (1, 1278) = 95.585$, $p < .001$, $\eta_p^2 = .070$. There was a main effect of group, $F (1, 1278) = 21.589$, $p < .001$, $\eta_p^2 = .017$ and a main effect of task, $F (1, 1278) = 21.469$, $p <.001$, $\eta_p^2 = .007$. Though means, p-values, and effect sizes inevitably varied slightly, all findings of significance and non-significance were identical to the EMG-corrected dataset, both for the ANOVA and the analyses of simple effects (see above). See Appendix N for participant, group, and grand average means for the M Task and W Task on this dataset.

Individual Chi-Square Tests of Independence comparing the M-First and W-First groups on responses to the PEQ were not significant for any of the 10 questions (see Appendix O for these results). However, Question 5 (Did you feel there was a gap in time between your intentions and your movements?), Question 6 (Did you always feel an intention to move before you moved?), Question 8 (Did you tend to make your clicks at similar places?), and Question 10 (Which word best describes what you reported in W tasks: intention, urge, impulse, or some other word?) are of note. These questions deal with the construct of intention in the study and/or the study's methodology the study, and there appeared to be no clear consensus among participants' responses.

V. Discussion

The major aim of the current study was to replicate the results of Dominik et al. (2017), which showed that there was no difference in movement reports and intention reports when participants were not trained to make an a priori distinction between moving and intending to move. I hypothesized that I would corroborate the basic findings of Dominik et al., and the results of the current study supported this hypothesis. Prior to being made to distinguish between moving and intending to move, one group reported the time of intention and one group reported the time of movement. These reports were not significantly different between groups. This calls into question the validity of the intention reports made in the Libet study.

The results of the ANOVA indicated that performance on the M Task and W Task depended on group membership. Recall that group membership determined the order in which the M Task and W Task were completed. Therefore, the significance of the ANOVA indicates a significant order effect of task completion. The analyses of simple effects were significant for all possible comparisons but one: means did not significantly differ between the M Task in the M-First group and the W Task in the W-First group (see Figure 10). Crucially, there was no significant difference in these reports, despite the fact that each group was completing a different task with its own particular instructions. In the Libet study (Libet et al., 1983) these reports differed by over 100ms (i.e., M Task reports around -40ms and W Task reports around -150ms). Recall that in the current study this comparison was between the second task completed in each group (the task following the S Task; see Method and Figure 8). When given instructions for this second task, participants were instructed to make a spontaneous click and told to either report their movement (M Task) or intention/urge (W Task). Researchers were careful to not inform participants of the existence the third task and its other type of report.

Note that on these reports the groups' grand average means for the EMG-corrected dataset were around 110 milliseconds, while their grand average means for EMG-uncorrected dataset were around -15 milliseconds (see Appendices M and N, respectively). The former indicates late reports relative to the objective time of movement, and the latter indicates early reports. Recall that Dominik et al. (2017) found grand average means around -10ms on this comparison. Thus, though the current study and Dominik et al. (2017) agree in terms of group means not differing significantly on the crucial comparison, the grand average means between studies appear to differ by over 100ms when the EMG-corrected dataset is used. When the EMG-uncorrected dataset is used, the means between studies are comparable (i.e., -10ms versus -15ms). This dataset likely shows agreement with that of Dominik et al. because these authors only reported EMG-uncorrected data. It appears that Dominik et al. accepted the digital triggers of key presses as accurate and did not use EMG data to correct the digital triggers initiated by the key presses of participants. Dominik et al. do not report the use of EMG or other data to determine the objective onset of movement. The EMG-corrected dataset of the current study corrected all digital triggers via EMG data (see Data Analysis subsection of Method). Both datasets in the current study agree with the findings of significance of Dominik et al., but only the EMG-uncorrected dataset, which matches those authors' methodology, also shows comparable grand average means.

Note that the EMG-corrected dataset is the focus of the following discussion unless otherwise noted.

The between-subjects comparisons that were significant showed that groups did differ in performance on the M Task, as well as the W Task, and that there was a difference between groups in the third task completed (i.e., the W Task in the M-First group and the M Task in the W-First group). These results strengthen confidence in the sole nonsignificant difference between groups (i.e., between the M Task in the M-First group and the W Task in the W-First group), though it should be emphasized that a finding of nonsignificance does not indicate that two groups are identical but, rather, that there is no evidence that the two are different.

These findings can be interpreted in two main ways. Recall that the study by Dominik et al. (2017), whose basic results I have replicated, is centered on the validity of intention reports in the Libet study. The basic question is whether intention reports actually reflect an experience of intention, and whether they do so accurately. Put another way, the most fundamental question about the validity of intention reports is whether or not intention itself is actually present in the spontaneous voluntary movements carried out in the M Task and W Task. The answer to that question dictates whether or not another question is asked. If one holds that intention is not present in the voluntary movements, then one must conclude that intention reports in the Libet study are invalid, and no further questions are needed. If one holds that intention is present in the voluntary movements, then the question becomes whether intention reports accurately capture intention. There are multiple ways of answering this question, as well. I will discuss these issues by examining them in terms of the two possible main interpretations of the current study's findings regarding the presence of intention in the tasks. Then I will look at the larger picture of what these findings, and the Libet study, might tell us about intention in general.

Interpretation 1: **Intention is not present during the tasks.** On one hand, results appear to indicate that individuals do not make a natural distinction between the experiences of moving and intending to move, at least for simple movements like those carried out in this study. In the traditional Libet study, researchers may be implicitly inducing (i.e., creating) this distinction. Libet et al. (1983) appear to take the distinction for granted. Assuming two phenomenally distinct events of intention and movement, Libet et al. trained participants extensively on all tasks from the beginning, and replications have followed suit. This may have led to demand characteristics in results on the M Task and W Task in all versions of the Libet study which made the seemingly innocent distinction between moving and intending to move. Participants may have implicitly or explicitly forced themselves to make this distinction, despite uncertainty or personal experience to the contrary. Other researchers have provided evidence or argued that individuals may infer intention in the Libet study (Haggard & Clark, 2003; Banks & Isham, 2009; Pockett & Purdy, 2011).

It may be that individuals do not naturally make such a distinction because "intention" is not phenomenally experienced in the tasks. Dominik et al. (2017) argue that there is only one event that occurs during the Libet task, that of movement. This is why participants, when allowed to make reports reflecting their natural phenomenology, make intention reports indistinguishable from movement reports, both of which are near the objective time of movement. Compare this to the results of Libet et al. (1983), in which there was a difference of over 100ms between movement and intention reports, and only movement reports occurred near the objective time of movement. Recall that the results of Dominik et al. showed both reports near 10ms prior to movement, which is why these authors could argue that both reports were near the time of movement. Both reports in the current study were around 110ms post-movement, which was a departure from the results of Dominik et al. However, these authors did not correct their data with EMG, while the current study did (see above). The current study's results from the dataset uncorrected by EMG were comparable to the reports from Dominik et al.: both reports were around 15ms prior to movement (Appendix N). The EMG-corrected data of the current study appear to show that both reports are made late in time relative to movement onset. This may appear to weaken the original interpretation of Dominik et al. that movement is the single event being reported, since the reports are far removed in time from movement. This is not the case, of course, because the M Task reports were also far removed in time from movement; both reports were around 110ms post-movement. The methodology of the current study simply captured the objective time of movement onset differently than Dominik et al., and perhaps differently than many other studies, necessarily shifting both types of reports later in time (see EMG subsections of Method and Discussion). Thus, the results of the current study support the argument of Dominik et al. that only one event is being reported in the tasks: movement.

Dominik et al. (2017, p. 260) argue that the difference in M Task reports and W Task reports in the traditional Libet study results may occur because intention reports are an "artificially induced impression" produced by "mere experience with M [Task] measurements." Dominik et al. call this "anchoring" and note that participants likely report intentions before movements because intentions logically precede execution of movement. That is, the clearly experienced phenomenon of movement may serve as an objective reference point. In the absence of a genuine *experience* of an intention, intention may be cognitively *inferred* as logically occurring prior to the objectively anchored time of movement.

The results for the third task completed in the current study, which partially conform to the findings of Dominik et al. (2017), are further evidence of this anchoring effect. Recall that participants in both groups made earlier reports on their third task than their second task. However, the event in the third task was reported significantly earlier in the M-First group (i.e., W Task; M = -.011ms) than in the W-First group (i.e., M Task; M = .066ms) (see Results). The results of Dominik et al. (2017, p. 260) showed the same pattern, with a difference between groups of 234 milliseconds. Dominik et al. also found a significant within-groups difference between tasks in their M-First group, whereas this difference was not significant in their W-First group. They argued that these results indicated an anchoring effect in the M-First group but not the W-First group. In other words, those who first completed the M Task had their experience with movement reports to anchor their W Task reports. They could then cognitively infer that intention should precede the times they reported on the M Task. (Note that the methodology for the M-First group mirrors what normally occurs in the Libet study, both in terms of methodology and results. Participants make intention reports only after becoming familiar with movement reports, and intention is reported significantly earlier in time than movement). Those who first completed the W Task, on the other hand, were unaware of the movement/intention distinction when making intention reports and, consequently, made them close in time to movement. The results of Dominik et al. showed that M Task reports in this group were not significantly different than W Task reports, so we are left to assume that in both tasks participants reported the time of movement.

However, the current study's results depart from Dominik et al. here, in that the difference in means between the W Task and M Task in the W-First group *was* significant. As noted, in both groups reports on the third task were earlier than those on the second. The means of Dominik et al. showed the same pattern, but the difference failed to reach significance. It seems counterintuitive that the W-First group would report movements before intentions. Perhaps some sort of anchoring effect is inevitable in the setup of studies like the current one, regardless of the type of report first made. Alternatively, perhaps at least some of the difference between the first report and the second report is due to differences in motivation and arousal. By the time participants reach the final task, they have completed numerous trials of an extremely taxing yet boring task (see Limitations). Perhaps this influences performance on the third task, including one's cognitions about making reports. Regardless of the reason for this discrepancy between studies, the current study does corroborate the most important evidence of Dominik et al. regarding the anchoring effect: there was no difference between reports on the second task, and intention reports were made significantly earlier in time when completed after the M Task.

Why might intention be absent from the movements carried out in the Libet study? As noted in the introduction, many questions have been raised about the *ecological validity* of the movements. It seems plausible that the movements are too simple or artificial to actually involve "intention" as commonly experienced. The movements are voluntarily initiated, but they seem to be so stripped of purpose and context that they involve no deliberation and decision-making (i.e., intention). Indeed, participants are told to simply move "spontaneously" without preplanning, which seems to preclude the possibility of intentional deliberation. It is worth asking whether "intention" can be phenomenally present in such movements. Indeed, it may be a paradox to "intend" a movement while simultaneously acting "spontaneously" without preplanning. Is the Libet study trying to measure intention despite instructing individuals to move without intention? This would accord with the skepticism voiced by Nachev and Hacker (2014) that the Libet study makes the implicit, yet unproven, assumption that a conscious intention always precedes a spontaneous voluntary movement.

Papanicolaou (2017, p. 314) discusses the distinction between *acts* and *mere movements*. Acts are intentional in that they are carried out for some meaningful goal or purpose, while mere movements are not. What is asked of participants in the Libet study, for Papanicolaou, is not an act but a mere movement, with no consequence or significance other than in the minimum sense of continuing a research study. The mere movements of the Libet study, in a decontextualized experimental setting, can give the false appearance of being meaningful acts because some acts involve similar movements. After all, to act one must usually move in some way. Papanicolaou gives the example of pressing a button to vote or buy stocks. In these cases, moving to press a button is accompanied by a host of mental processes, such as those for planning, deliberation, judgment, attention, impulse control, and the like. In the Libet study, there is a movement to press a button, but is not supposed to involve such mental processes. One is supposed to simply move spontaneously, and one's mental processes are to be focused on following the rules for using the clock to make reports. In short, the Libet study is designed to produce extremely simple, nearly meaningless movements rather than acts, and these mere movements may be devoid of intention. It may be impossible to capture intention in a task that is deliberately designed to be simple and basically purposeless.

The movements in the Libet study, whether they be mouse clicks, wrist flexes, or some variation thereof, are certainly voluntary, but perhaps only in the most minimal sense. The term "voluntary" seems to entail intention, but the results of the current study suggest that all voluntary movements are not necessarily intentional. Many authors have pointed out that in the Libet study the only "decision" to be made is about *when* to act, which is supposed to occur without prior planning or deliberation (e.g., Haggard, 2008; Schmidt et al., 2016; Papanicolaou, 2017). The decision *to* act is already made when the participant begins individual trials; by understanding instructions and agreeing to complete blocks of trials, the participant has already decided *that* and *how* he/she will move, and the only question is *when* to move. At best, the M Task and W Task are when-decisions (Haggard, 2008; Schmidt et al., 2016). These decisions seem very different from everyday decisions associated with voluntary acts of will. Intention may simply be absent in when-decisions, or at least in those made in a decontextualized, sterile experimental environment like the Libet study setup. This apparent lack of ecological validity was one reason for skepticism from many authors about the attempts to extrapolate from the Libet study to everyday life (e.g., Banks and Pockett, 2007; Mele, 2013; Nachev & Hacker, 2014; Navon, 2014; Kihlstrom, 2017; Papanicolaou, 2017). For this reason, and others, many authors were not ready to interpret Libet's findings as evidence against "free will" (see Philosophical Issues subsection for more discussion of free will).

Lastly, it is worth noting that some authors have argued that there is not a positive phenomenology associated with agency (Chadha, 2016). On this view, there is no "agentive" feeling that accompanies voluntary movements or acts; one simply moves or acts voluntarily. Chadha (2016, p. 203) notes that there is a phenomenological feeling of the *loss* of authorship of one's actions in cases of abnormal agency, such as "alien control" in schizophrenia. However, this does not entail that there was originally a feeling of normal agency that was lost or replaced. Simply put, Chadha argues that one only experiences a feeling associated with agency if something goes awry during the process of agency. If all goes well, there is no salient feeling that one is the author of one's actions; one simply *is* the author of one's actions. This account might explain why movements in the Libet study may be voluntary yet devoid of reportable intention.

Whatever the reasons might be for the absence of intention in the spontaneous voluntary movements carried out in the Libet study, this interpretation of results is a deadly blow to the basic conclusions of the Libet study. If one interprets the findings of the current study and those of Dominik et al. as indicating that intention is not present, then intention reports in the Libet study, as well as in all its replications, are invalidated. The crux of the Libet study is the comparison of the RP to intention via intention reports. If intention reports are invalidated, then the findings regarding the temporal relation of the RP and intention are also invalidated and must be discarded.

Interpretation 2: Intention is present during the tasks. This interpretation avoids the categorical invalidation of intention reports that necessarily followed from the interpretation that intention was not present during the tasks. On this view, intention is present in the tasks, occurring as a distinct event. In general, as we shall see, this is a more skeptical interpretation of the current study's results. There are three versions of this interpretation. The first version interprets the results of the current study and those of Dominik et al. (2017) as indicating that intention occurs at the same time as movement. The second version holds that intention reports in the Libet study and its replications are accurate and that there is some flaw in the current study and that of Dominik et al. The third version holds that intention is present but inevitably misreported. I will examine each of these in turn.

First, it may be that the results of the current study and of Dominik et al. (2017) reflect accurate reports for the W Task. Perhaps intention is present in the tasks but misreported in traditional Libet study. That is, it is possible that the commonly induced distinction between movement and intention leads to inaccurate intention reports, and only the current study and that of Dominik et al., which did not force such a distinction on the second task completed, accurately captured intention reports on the second task (i.e., intention occurs around the same time as movement). It seems far from implausible that in spontaneous voluntary movements, intention could more or less coincide with movement. Indeed, feedback from the PEQ strongly suggests that many individuals may have experienced intention in this way. Many reported that there was no gap between intentions and movements. Some were unsure if there was a gap. For example, one noted that both reports "might be the same moment in time." If this is indeed true, studies must find a way to avoid interfering with the natural phenomenology of the experience of moving itself in order to accurately capture intention. The current study provides one example, but future research might attempt other approaches. For example, participants might be told about the M Task and W Task and the concept of "movement" versus "intention" from the beginning, but they could be provided clear instructions that there are no "right" reports and that they could report the two events as occurring in any temporal order or as coinciding in time.

Second, it is possible that intention is present and the Libet study has accurately captured intention reports all along. On this view, there must be some methodological flaw(s) in the current study and the study of Dominik et al. giving rise to their unique findings. Critics of the current study might point to the experimental setup in which participants were given minimal instructions, for example (see Limitations for a review of potential issues with the current study). Thus, this interpretation disregards the findings of the current study and accepts the standard findings of the Libet study.

Third, it is possible that intention is present in the task but is affected by any and all attempts to report (i.e., measure) it. Consequently, it may be impossible to capture accurate reports of intention. Furthermore, even if it were possible to capture accurate reports, it might be impossible to verify that a study has done so. In short, this interpretation of results grants the presence of intention in the tasks but questions the validity of all intention reports, including those in the current study. For one, intention, like other psychological phenomena, is likely to be affected by the attention focused upon it. A form of the Heisenberg Uncertainty Principle (HUP) appears to be at play here, in which attempts to introspectively examine (i.e., become conscious of and report on) an ongoing mental process necessarily change the process. (I must credit Miller, Shepherdson, and Trevena (2011) with first applying the HUP to the Libet study. They discussed the HUP in relation to the influence of the clock on the RP, pointing out how clock monitoring is necessary for reports yet may change, or even cause, the RP).

To use Heideggerian language, intention may be inextricable with one's *purposive engagement* with the world, and it may be impossible to accurately abstract about the *occurrent* features of intentional engagement (Käufer & Chemero, 2015, pp. 50-76). In other words, it is one thing to engage intentionally, but another to try to introspect during the process of ongoing intentional engagement and then attempt to divulge meaningful information from these abstractions. Just as one cannot know the location of a particle without changing it (e.g., bouncing a photon off the particle inevitably changes its position and momentum), one may not be able to observe and/or report on one's intention without changing its phenomenology, such as the perception of its onset. As Wasserman (1985) argued, the act of reporting a conscious intention is itself an additional voluntary act. Surely the insertion of additional variables into one's phenomenology has some sort of effect.

These additional variables add up to a considerable cognitive load for participants during tasks, one which has likely been underestimated. The tasks, despite consisting of only a few simple *overt* components, are attentionally demanding. Participants are asked to comply with a litany of rules and requests, both overarching ones for the entire study and particular ones for individual tasks. Refer to Figure 11 for a visual representation of some of the variables contributing to cognitive load during the M Task or W Task. For example, during trials participants must refrain from moving and blinking, keep their eyes on the center of the clock, wait for the clock to make one rotation before moving, move spontaneously "without preplanning," note the ever changing position of the clock hand, remember the clock hand position, and maintain general attentional arousal. All of these variables may interfere with one's natural intentional engagement, one's phenomenological experience of intention, and/or one's ability to report intention.

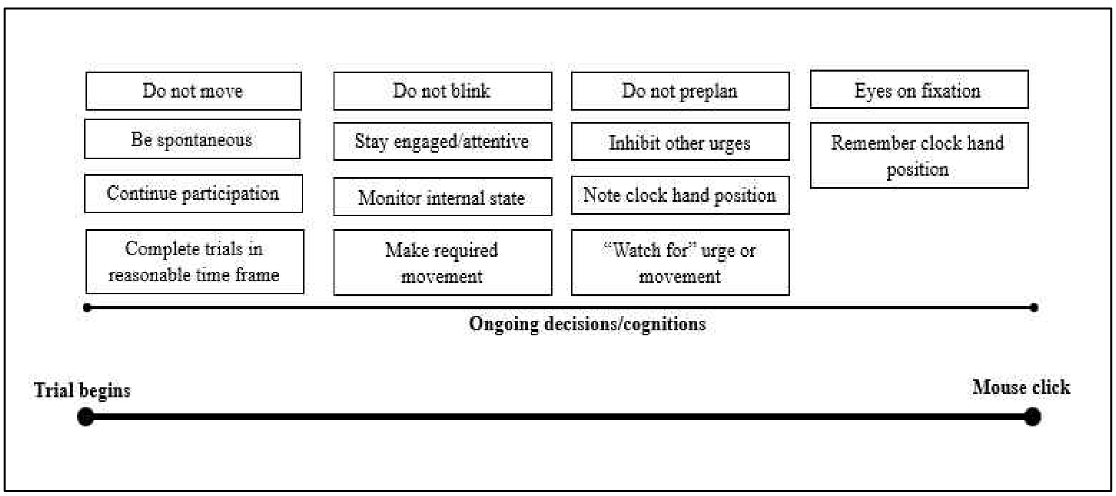

Figure 11: Representation of cognitive load during tasks.

Unfortunately, if the onset of intention is indeed misreported due to such a process of interference with one's natural phenomenology, it may be impossible to know in which direction this is occurring. Papanicolaou (2017) noted how the presence of multiple demands for attention during tasks can lead to the well-established *effect of prior entry*, in which multiple simultaneous events cannot occur all at once. He noted the demand for at least three simultaneous acts during Libet-style tasks: making a movement, observing the clock, and remembering the position of the clock at the time of an event. He argued that the presence of the effect of prior entry means that only one of these events can occur at a time. It is unclear – and perhaps impossible to determine – which two of the three events are delayed in time. Thus, even if we forego other reservations about individuals' ability to accurately perceive and report on his/her experiences, this effect indicates that only one event can be perceived at a time, leading to an unknown amount of error in perception and/or reporting for all other simultaneous events. Since there are many additional ongoing demands like those previously discussed, this effect may be extremely significant. This appears to be the same troubling phenomenon noted by Banks and Pockett (2007), which they referred to as *systematic reporting priority*.

Papanicolaou (2017) also discussed the unclear distinction between activity and passivity in the Libet study. He noted that it is often assumed that participants are passive observers of their mental states and, thus, can make objective reports about them. It is as if participants can objectively observe their mental states without interference or prejudice in the same way that, for example, astronomers are thought to observe the night sky. Papanicolaou (2017, p. 314) emphasizes that participants are active creators of their mental states, noting that they "make choices: choices do not 'happen' to them." Though the assumption of passivity is obviously erroneous, Papanicolaou argues that it has been taken for granted since the original study of Libet et al. By undercutting this assumption, he undercuts much of the credibility of the intention reports made in any forms of Libet study. For one, he undercuts their status as supposedly "objective" reports. Furthermore, this is yet another example of the HUP in which the attempt to observe and report on a subjective phenomenon is likely to change the phenomenon, or, at the least, the ability to make accurate reports about the phenomenon.

When does intention begin? The results of the current study may appear to indicate that it begins right at the time of movement and that the traditional Libet study makes reports biased to be earlier in time. However, the possibility of a HUP applies equally to *all* attempts to garner reports about intention, including the current study; any attempt to affect the natural process of intention is likely to lead to inaccurate reports of intention. Thus, intention may be present in the tasks but necessarily unamenable to measurement, no matter the methodological design. Furthermore, since intention is probably best thought of as a dynamic process spread out in time rather than a discrete, phenomenologically clear event (Verbaarschot, Haselager, & Farquhar, 2016), the question of "when" intention begins may not be completely coherent. The attempt to measure intention as a clear, distinct event may yield empirical results, but those results may inevitably be inaccurate and misleading. Different methodologies may necessarily produce different empirical results because each inserts a distinct set of variables into participants' cognitive load, thereby unduly influencing the experience, and reporting, of intention.

It is also possible – in this study as in any form of the Libet study – that the previously discussed phenomenon of intentional binding is at play during the tasks, causing intention to be perceived closer in time to its effect (i.e., movement) than is actually the case (Banks & Pockett, 2007). This would allow for intention to be present during the tasks but perceived around the time of movement, as in the current study. If this is the case, then the current study has more accurately captured the phenomenology of the experience of intention than the traditional Libet study. However, the aforementioned problems remain: how could one ever verify that this was actually the case?

The issue of *inferential distance* is lurking here. The measurement problems discussed in the preceding paragraphs are a fundamental problem for the Libet study because the study is designed to make inferences about intention rather than measuring it. That is, the issues are encountered because of use of *reports* of intention. It is a rather obvious point that the Libet study indirectly attempts to measure intention, and that the goal of Dominik et al. (2017) and the current study was to determine the validity of these attempts, but it is a point worth emphasizing. If there were some universally accepted method of directly measuring intention, perhaps through neuroimaging, then the Libet study would not have to rely on reports of intention, but this is not the case. In the absence of an unequivocally established index of intention (if such an index is even possible), reports of intention are necessary.

Furthermore, the previously discussed issues regarding task interference with one's natural phenomenology reflect a secondary process of inferential distance. As discussed, it is one thing to carry out an intentional action but quite another to introspect on that action and try to make reports about it, especially when additional variables are introduced (e.g., all the rules for monitoring the clock and making movements and reports). Thus, if intention does exist in the tasks, there is likely inferential distance between the experience of intention and the report thereof. Therefore, there seem to be two types of inferential distance between the phenomenon targeted for measurement (i.e., intention) and the data yielded about that phenomenon (i.e., intention reports). The reports are not only indirect measurements of intention but also likely to be imperfect representations of the experience of intention. Note, however, that it would be a mistake – a form of *cognitive elitism* – to claim that one's inability to accurately report his/her intention means that he/she does not have intention. Surely one can have an experience without being able to accurately report on it.

The first version of this interpretation appears particularly damaging to the Libet study. Because it holds that intention actually occurs at the time of movement, it invalidates the intention reports in the Libet study and its replications. However, it could potentially strengthen the findings of the Libet study, since it might place intention even later in time past the late RP. If intention is indeed occurring closer to the objective time of movement, then perhaps the gap between the late RP and intention is even larger than it originally appeared. Even if the onset of the late RP is pushed later in time, perhaps around 200ms prior to movement, it would still precede intention reports occurring near the time of movement. The second version of this interpretation, in which there must be some flaw in the current study, leaves the Libet study completely undamaged. The third version of this interpretation is damaging to the validity of intention reports in any version of the Libet study. Thus, proponents of the Libet study can cleave to the first or second version of this interpretation.

General Interpretation. To sum up, most of the possible interpretations of the current study are damaging to the validity of intention reports in the Libet study. If results are interpreted as indicating that intention is not present in the tasks, then the intention reports in the Libet study are invalidated. On this view, intention reports are inferred and, thus, not genuine indicators of an experience of intention. Consequently, the comparison of intention reports with the RP is also invalidated, and conclusions of the Libet study regarding conscious intention must be discarded. Dominik et al. argued that an anchoring effect causes these invalid intention reports. They suggested that movement serves as an objective anchor point from which intention reports are cognitively inferred to precede. Future research might attempt to provide evidence for this anchoring effect on other types of reports. For example, a study could ask participants to deliberately shift movement reports backward or forward in time after making normal movement reports. If the anchoring effect is real, it seems plausible at individuals would be able to anchor these reports as well.

If results are interpreted as indicating that intention is present in the tasks, then two out of the three possible versions of this interpretation invalidate the intention reports in the Libet study. If one takes intention reports in the second task completed to be accurate, such that intention occurs near the time of movement, then the standardly reported time of intention in the Libet study, occurring about 150ms prior to movement, is invalidated. Note, however, that intention would still follow the RP in time if it occurs near the time of movement, and the interval between the RP and intention might be enlarged. Thus, this interpretation might actually lend credence to the original conclusions of the Libet study. On another version of this second interpretation, one might hold that intention is present in the tasks and that the Libet study captured intention accurately all along, while the current study has some methodological flaw(s) leading to its unique intention reports. This would appear to be a last resort for a defender of the Libet study, and one holding this view would need to spell out the flaws in the current study while justifying the assumptions and apparent limitations of the Libet study. On the third version of the second interpretation, one might claim that intention is present in the tasks but that the attempts to measure it inevitably fail. That is, the natural phenomenology associated with carrying out intentions is affected by the methodology of each study, such that no study can ever accurately measure intention. The introduction of a clock to make reports, for example, may inevitably alter the experience of intention. This view undercuts the validity of all intention reports, including those of the current study.

Thus, for those who wish to defend the Libet study, there appear to be only two alternatives, both within the interpretation that intention is present in the tasks. On one hand, they can accept the results of the current study and thereby accept invalidation of traditional intention reports but maintain the temporal relation between the RP and intention. On the other hand, they can reject the results of the current study and thereby maintain the validity of traditional intention reports.

Main Effects

The main effects of group and task were significant but not central to the purposes of the current study. The main effect of group was the comparison of groups across the combined results of the M Task and W Task (i.e., when the tasks were not distinguished from one another). The main effect of task was the comparison of tasks across the groups combined (i.e., when the groups were not distinguished from one another). To the best of my knowledge, there is no prior research examining or theorizing about these main effects. Future research might explore whether they indicate any phenomenon of significance or are simply an artifact of the methodology employed by studies like the current one. It seems likely that main effects of group and task are inevitable, but unimportant, consequences of an experimental design in which groups differ in the order in which they complete tasks.

EMG

The EMG results of the study were somewhat surprising. I expected to find much less within- and between-subjects variability in EMG onset times relative to digital triggers. I assumed that most participants would make a simple mouse click in more or less the same way each time. Before pilot testing, I had hoped the digital triggers were close enough in time to movement to render EMG data collection unnecessary. Pilot testing made it clear that this was not the case and that EMG data should be collected, but, nonetheless, I anticipated being able to calculate an average EMG correction for each participant and subtract all triggers by that time. Data analysis quickly revealed more than expected variability in the EMG, and it was necessary to manually determine movement onset trial by trial. Many trials had to be excluded from analysis due to unclear EMG onsets or multiple click-like events. Often participants were making pre-click movements with their hand, and sometimes the amplitude of these movements exceeded that of the click-related EMG activity. As noted, EMG onset times ranged between about 70-200ms prior to trigger onsets.

This seemingly mundane finding regarding calculations for movement onset may be extremely important for the Libet study. Many researchers have not precisely reported how they captured the moment of movement initiation (e.g., Haggard & Eimer, 1999), though Jo et al. (2013), for example, noted that they placed an accelerometer on the mouse itself to determine the onset of the button press. Accelerometers measure with precision the moment an object begins to move (i.e., accelerate). One is left to wonder, in the absence of detailed reporting, how precise the moment of movement initiation has been measured in previous research. If researchers have not adequately accounted for the variance in movement (i.e., by measuring it precisely and correcting for it on individual trials) then this variance is a source of error in all Libet study tasks (other than the S Task). If the objective time of movement onset is not properly calculated, each individual trial will be affected backward or forward in time by the error. Consequently, the RP will be improperly time-locked, and reports on the M Task and W Task will be shifted. The results of the current study recommend further testing of EMG within the Libet study, or, at the least, improved reporting of how the objective time of movement is calculated. Accelerometers may be an alternative to EMG, though it would seem they capture the earliest moment a button begins moving rather than the earliest moment of bodily movement. As noted, the overall findings of significance in the current study were consistent whether data were corrected or uncorrected by EMG.

However, these corrections may be more important when comparing intention to the late RP.

Future Analyses

Participants who completed the current study returned on the next day for a second study. This second study has not been reported or discussed within this paper because data have not yet been analyzed. This study, which I will call Experiment 2, was a hybrid replication of the Libet study. It aims to incorporate many of the methodological suggestions and recent findings discussed in the Introduction. Specifically, in Experiment 2 I aim to compare the late RP, rather than the entire RP, to intention reports. In addition, I aim to use multiple methods to filter EEG data and to calculate late RP onset. I will analyze the temporal relation of the late RP and intention reports on individual, as well as grand, averages, and I will attempt to determine how various individual differences (e.g., attributional style, performance on the PEQ) impact the late RP and task performance. Based on the recommendation of Miller et al. (2011), I developed a new task to measure the late RP without the influence of clock-monitoring. See Figure 12 for the flow of task completion in Experiment 2. I expect the late RP to generally precede intention reports on grand averages, though deviations may emerge from this pattern for individual participants. Individual and grand average deviations may also emerge depending on which combination of data filtering and calculation methods is chosen. Nonetheless, all results from Experiment 2 should be tempered by the findings of the study presented in this paper. If the validity of intention reports is highly questionable, then the results of all studies relying upon intention reports should be examined with caution.

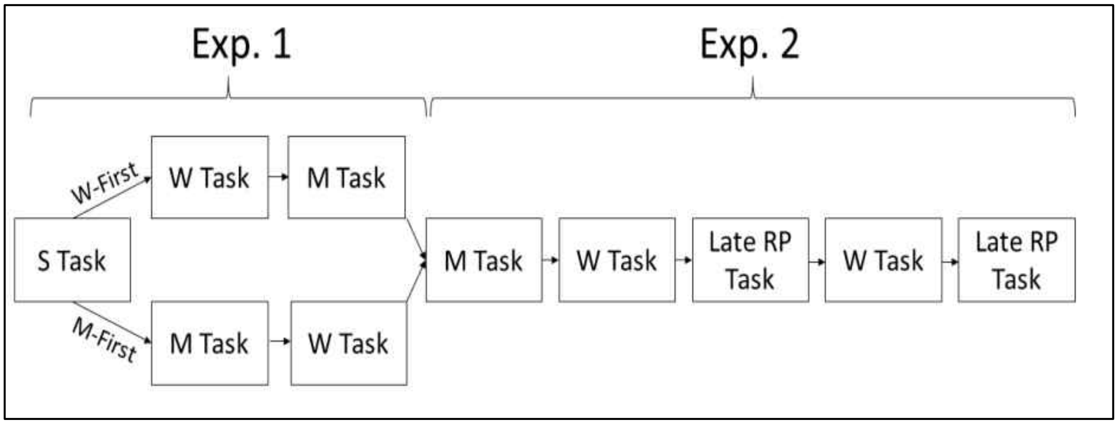

Figure 12: Task order by group, including Experiment 2.
M-First: Group in which the M Task was completed prior to the W Task
W-First: Group in which the W Task was completed prior to the M Task
S Task: Report of onset of stimulus (tone)
M Task: Spontaneous mouse click, then report of movement
W Task: Spontaneous mouse click, then report of intention
Late RP Task: Spontaneous mouse click without monitoring clock; no report made

Philosophical Issues

Let us now examine more generally what implications the Libet study might have for free will. For the sake of argument, this discussion will assume the validity of intention reports. As noted, most research has implicated the bilateral SMA for the RP and the contralateral pre-SMA and motor cortices for the late RP, and some studies have found correlates of conscious intention in the prefrontal and parietal cortices (Eccles, 1982; Sirigu et al., 2004; Pirtosek, Georgiev, & Gregoric-Kramberger, 2009). An fMRI variant of the Libet study was able to build a post-hoc predictive model using prefrontal and parietal activity (Soon, Brass, Heinze, & Haynes, 2008). These researchers allowed participants an extended time to choose which hand to use to make a button press. The model was able to predict which hand would be used up to 10 seconds before the movement occurred, with about 60% accuracy. Fried, Mukamel, and

Krieman (2011) used intracranial recording to predict the intention to move in advance, with varying degrees of accuracy depending on which neuron populations were used and how far in advance the prediction was made. For example, at 700ms prior to the reported time of intention, the prediction accuracy was above 80% in the SMA. Using neurons in frontal areas, the prediction could be made 1000ms prior to the time of reported intention with more than 70% accuracy. Based on these and other results, many recent models of voluntary movement show movement initiation and preparation first occurring in prefrontal areas, then travelling to the pre-supplementary motor area (pre-SMA), then to the SMA and cortical motor areas (Nachev, Wydell, O'Neill, Husain, & Kennard, 2007; Pirtosek et al. 2009; Sakata et al., 2017). If such models are accurate, the activity shown in late RP would likely be eliminated as a possible source of the ultimate origin of movement initiation.

Prefrontal areas are associated with conscious processing and executive functions. Thus, if they are implicated as the source of spontaneous voluntary movements, then it is at least possible, if not likely, that conscious intention is playing a role in initiating the movements after all. The late RP, occurring later in the process, would be secondary to this initiatory activity and likely related to specific post-intention movement preparation (see Introduction for a review of the RP and see Appendix A for a list of abbreviated terms). If this is indeed the case, then the results of the Libet study and its replications would be invalidated, though we might never know the true cause(s) of the error in the study. The implication would be that the late RP is ultimately caused by conscious intention.

The Libet study has been endlessly cited and replicated because of its philosophical implications, particularly those for agency and free will. Many took the findings of the Libet study as proof that conscious intention was an epiphenomenon, since it was shown to be consistently preceded by the RP. The inference was that non-conscious brain processes, reflected by the RP, were responsible for the decision to make a voluntary movement. Conscious intention, occurring after the RP, came after this non-conscious decision had already been made. Now that the relationship between the entire RP and intention has been discarded as not meaningful, the relationship between the late RP and intention is up for consideration. In all likelihood, inferences analogous to those regarding the RP and intention will be drawn for this relationship as well. If the late RP is shown to precede intention, perhaps the same claims will be made that were made of the causal power of the non-conscious brain processes reflected by the RP. It seems likely that the same conclusions concerning the causal impotence of conscious intention would be reached. However, it is likely that many of the same criticisms would also apply.

One issue is the hypothesized veto window of Libet et al. (1983), which is the interval between reported intention times and the time of movement, in which a person may inhibit a previously prepared movement. If the veto window is a possibility, then the late RP might be shown to precede intention, but the underlying non-conscious processes reflected by the late RP might not be the actual cause of intention or the movement, since an intention to veto the movement could be formed after the late RP. More generally, the temporal relation between two events does not guarantee that the earlier event causes the other, and this is true regardless of whether the late RP is shown to precede intention or not. It may be that the late RP is a necessary but insufficient condition for movement. Other processes, including some form of conscious intention, may be other necessary conditions. Many of the other various criticisms previously discussed, such as the critique of intention reports as invalid, will remain intact regardless of whether the RP or late RP is examined.

What would occur if intention reports were indeed shown to precede the late RP? Even if future research were to somehow show that conscious intention precedes the late RP and originates movement, this, of course, would not indicate that conscious intention does not have its own neural correlates. The fact that recent models have implicated frontal areas as the ultimate source of movement initiation indicates that conscious intention associated with initiating voluntary movements may be ultimately traced there, while the SMA, pre-SMA, and motor cortices may only reflect movement preparations which follow conscious intention. If that is the case, then it is possible that researchers will eventually arrive at a specific description of neural activity in frontal areas involved in conscious initiation of movement. However, the problem of reductionism, which is common in cognitive neuroscientific research, would remain.

The problem is that conscious intention, even if shown to be causally potent on the psychological level, could possibly be reduced to neural activity. This possibility re-raises the question of whether conscious intentions have causal power, but it shifts the question to the level of philosophical inquiry, at which point we run into the *mind-body problem* and the *free will* debate. One basic issue is whether conscious intention is actually causally potent if it can be entirely reduced to physiological activity. There are volumes written on the subject of reductionism, particularly the reduction of the mental to the physical (for a brief overview of issues, see Richards, 2010, pp. 119-130). For our purposes here, I will focus on the issue of causation. Specifically, I will explore whether the problem of reductionism might be circumvented, depending on one's metaphysical assumptions about causation. If it is possible that causation can occur non-physically, then complete reduction of the mental to the physical, and all the issues raised thereby, might be avoided.

The mind-body problem revolves around the issue of mental states and physical states, which prima facie seem to be two completely different types of things. Mental states – in this case, intentions – appear to be private and non-physical, while physical states – in this case, neural activity – are publicly observable and measurable (Robinson, 2016). A classic mind-body question noted by Robinson (2016) is whether mental states are a subset of physical states and can be reduced to them (or vice versa). Robinson also asks how the two states can causally interact; can each state cause the other or is the causality only in one direction? The most famous attempted resolution of the problem is Rene Descartes's *substance dualism*. Descartes proposed a metaphysical system in which reality consists of two distinct types of substances: extended substances, which are physical objects in space, and thinking substances, which are minds outside of space. Descartes was an *interactionist* in that he claimed that the two substances interface and interact in the pineal gland. For Descartes, the mind was wholly different from the body but controlled the body and received sensations from it. Among the alternatives to substance dualism, substance monism is of note. *Substance monism* claims that there is only one kind of state. Most substance monists are *physical monists*; they believe that mental states are just a subset of physical states. In other words, everything that exists, including thoughts, feelings, beliefs, etc., is material. There is no need to posit an interaction between mental and physical states because there is no true ontological distinction between them; the apparent distinction is only in their qualitative properties because both are fundamentally physical.

It seems reasonable to assume that the Libet study was approaching conscious intention – and, by proxy, free will and the mind-body problem – from a substance monistic standpoint, particularly physical monism. This should not be surprising, as it is the default approach of contemporary science and philosophy (Robinson, 2016). Neuroscientific research into the physical nature of mental states is at the threshold of the mind-body problem and inevitably makes metaphysical assumptions. By definition, scientific investigations into the nature of the mind assume that mental states have physical correlates. Mental phenomena are assumed to have physiological correlates that are measurable with neuroimaging or other technology. Substance dualism is also compatible with this assumption. Its proponents hold that mental events can cause physical events even though the mental events themselves, existing outside of space and time, are undetectable. Conscious intention would presumably arise out of a person's non-physical intention, but physical (neural) activity would still arise. However, physical monism is typically preferred over substance dualism because it does not have to explain such immaterial causation. The basic difference in the two views, in terms of causality, is that physical monism holds that the physical account is *the* account; the physical world is causally closed. There is no causality outside the physical world, such that mental states can theoretically be completely understood in physical terms. Substance dualists, on the other hand, allow for non-physical causality. Thus, the physical account of the world is not complete; the physical world is not causally closed because it also involves non-physical mental causation. No physical account of the world can capture this mental causation, even though mental states can have physical correlates.

For the sake of argument, let us imagine that neuroscientific research was able to establish that conscious intention does cause voluntary movement and had provided us with an exhaustive neuroscientific account of this causation. These findings would not rule out the possibility of substance dualism because immaterial causation, though undetectable, could be at play. Although the possibility of such causation is routinely dismissed, it must be acknowledged that it is done so on the basis of metaphysical assumptions rather than empirical evidence. The evidence itself cannot decide the issue one way or the other. Thus, two fundamental aspects of the current study should be acknowledged. It is approaching conscious intention within a physical monist framework, and this framework assumes, but does not prove, its metaphysical claims. Evidence collected within a physical monist framework, including evidence to be collected by the current study, does not prove its metaphysical claims and cannot rule out the claims of substance dualism or other approaches.

Keeping this in mind, we can better examine any implications that this study might have for the issue of free will. There is a variety of competing conceptions of free will. Many conceptions differ drastically from one another, while others differ only in nuanced ways, and nearly every issue pertaining to free will is subject to debate and competing conceptions (Timpe, 2018). This diversity of conceptions has led McKenna & Coates (2015) to attempt a definition of free will that is neutral with respect to conceptual and theoretical commitments. For these authors, free will is "the unique ability of persons to exercise control over their conduct in the manner necessary for moral responsibility" (McKenna & Coates, 2015, p. 4).

In order to explore what is meant by having control over conduct, we must first define determinism. *Determinism* is the notion that every event is caused (i.e., determined) by previous events, such that any given event can only occur in one way, given all the preceding events that led up to it. In other words, given the causal history of the universe at any moment in time, only one future is possible. *Causality*, here, refers to the operation of natural laws and processes. Of course, determinism is also subject to various conceptions and is not universally accepted. For example, it competes with conceptions of probabilistic causation, in which causes increase the likelihood of certain outcomes but do not strictly determine specific outcomes. However, the two major conceptions of free will have this conception of determinism as their starting points, either accepting determinism and then spelling out its implications (*compatibilism*) or juxtaposing it with free will and rejecting either it or free will (*incompatibilism*).

Compatibilists claim that free will is possible within a deterministic universe; free will is compatible with determinism. Incompatibilists claim that if the universe is indeed deterministic, then free will is not possible; free will is incompatible with determinism. Again, each conception contains within itself an array of specific theories which spell out the details of how the relation between free will and determinism is or is not possible. To simplify matters, I will frame the two conceptions in terms of the so-called "ability to do otherwise" (Nahmias et al., 2004). Incompatibilists argue that the ability to do otherwise is necessary for free will. On this view, if John is presented with options A, B, and C and is causally determined to choose option A (i.e., he could not have chosen otherwise), then he did not have free will. Having free will entails having freedom from determinism, such that John can decide upon the choice for himself and create his future. Furthermore, moral responsibility is not possible unless one has such freedom. Compatibilists argue that John has free will despite the inability to choose otherwise. In this sense, freedom is the ability to carry out an action that one attempts to carry out, and John is responsible for his actions despite being determined to choose in one way over another. The only way for John to lack free will is for him to be prevented from carrying out his action by some force of coercion or constraint. For example, John would lack free will if he wanted to choose A but was forcibly made to choose B or if option A was no longer possible for some reason.

Substance dualism is mostly associated with *libertarian* notions of free will. Libertarians are incompatibilists who claim that free will does exist. Many tend to invoke some form of non-physical soul or mind as the source of freedom (i.e., substance dualism). On this view, free will could not exist within a physical universe in which there is total determinism. Free will does exist, however, because in this universe there is *not* total determinism; the source of one's will is non-physical and, thus, outside the physical deterministic scheme. On the other end of the spectrum, *hard determinists* are incompatibilists who argue that the entire universe is physical and deterministic and that there is no room for free will. Thus, physical monism is more associated with hard determinism, while substance dualism is more associated with libertarianism.

The Libet study and many of its replications and variants tend to discuss free will without spelling out which major conception (compatibilism or incompatibilism) and/or which specific conception (e.g., libertarianism or hard determinism) they are employing. However, it is reasonable to assume that most have some form of incompatibilism in mind and are looking to the Libet study and other neuroscientific research to settle the question of whether the mind is just another physically determined system. When authors claim that findings do or do not provide evidence against the notion of free will, they are indicating that free will is scientifically measurable and that specific results could demonstrate that free will does not exist.

Here we are at the crux of the issue. For incompatibilists, the metaphysical assumption made about the nature of causation will determine whether all mental states are reducible to physical states. For hard determinists, who assume complete physical causation, such reduction is possible. For libertarians, who do not assume complete physical causation (i.e., at least some mental states are non-physical), such reduction is not possible. From the hard determinist perspective, the Libet study and its successors, as well as other psychophysiological research, can provide further evidence that all mental states are physical. That is, such research can show that, or how, the mental can be reduced to the physical. For libertarians, such research may provide information about the physical correlates of mental states but cannot capture the entire causal picture. From the libertarian perspective, no matter how well the mental appears to be reduced to the physical, one cannot rule out the possibility of immaterial causation.

Thus, it is clear that one's metaphysical assumptions determine how one interprets scientific evidence and how one approaches the problems of reductionism and causation. Even if a supposedly exhaustive account of the neural underpinnings of conscious intention were to be produced, one could not claim that it was a total, unequivocal psychophysiological reduction and that “free will” had been disproved. One can only make such claims after many metaphysical assumptions have been made. Any empirical claim built upon a priori assumptions cannot be total or unequivocal, whether it is a claim made by hard determinists, libertarians, or others. The specter of reductionism is indeed a possible threat to free will, since it is possible that all mental states, including one’s intentions, are physically determined. However, it should be clear that it may be impossible to resolve the problem of reductionism, since the nature of causality and related issues depend on one’s metaphysical assumptions. Libertarians and hard determinists may be in an interminable deadlock, for it would seem that no amount of empirical evidence can determine which set of assumptions is correct. Furthermore, note that even if complete physical causation were proven and psychophysiological reductionism achieved, compatibilist free will (i.e., free will is compatible with determinism; see above) would survive to compete with the hard determinist claim that complete physical causation entails the non-existence of free will.

As noted, the current study, as well as other versions of the Libet study and cognitive neuroscientific research in general, approaches mental events like intention from a physical monist perspective. We tend to assume that by capturing correlates of mental states, we can come to a complete causal picture of the mind. From the previous discussion, it should be clear that such research is only one line of inquiry into the debates about free will. Claims about psychophysiological reductionism, mental causation, and free will are ultimately grounded on a priori metaphysical assumptions.

The current study has not adhered to or argued for any one conception of free will. Instead, it had strived to simply present findings and make clear what these can and cannot show. The current study can provide evidence about physical phenomena associated with conscious intention, but it cannot decide philosophical issues like the mind-body problem or the free will debate. Findings must be interpreted within the physical monist framework in which they were collected, but they are not proof of the framework, and the framework itself, fundamentally based on assumptions, can never be definitive proof against "free will."

VI. Limitations

Terminology. As discussed in the Introduction, there is a lack of standardization in the instructions and terminology in the replications and variants of the Libet study. There is a lack of consistency in phrases and terms in how "intention" reports and "intention" in the context of voluntary movements are framed to participants. Because of this, researchers in the current study deliberately used the two most common words, urge and intention, when referring to making spontaneous movements and to making reports on the W Task (see Method). As with other aspects of methodology, these differences could account for some of the variance in results among studies. Future researchers could seek to determine whether simple differences in phrasing have an impact on results. If possible, this aspect of methodology should be standardized. Question 10 of the PEQ, which asked which term is most appropriate for what is reported in the W Task, may shed some light on which terms to use (see Appendix O). Responses revealed that seven participants preferred the word "urge" and six preferred the word "impulse." Only two of 19 participants preferred the word "intention." Of the four who opted for a different word, one said that it was a toss-up between urge and impulse but definitely not an intention. Another said that he/she was waiting to get the "thought about clicking" and then clicked "when I heard the word click." Another coined the phrase "spontaneous decision to act" and emphasized that "urge" was not the correct descriptor. One participant noted that he/she was definitely not reporting an impulse but, rather, something between an urge and impulse. A few participants noted that "urge" did not seem quite right because the impetus to move was not a bodily event but a mental event. A few noted that “intention” did not seem quite right because there was not a normal process of decision-making.

This feedback suggests that urge or impulse may be better words to use than intention. However, it also indicates that individual differences are likely to exist with respect to each person’s definition of the word and, in particular, what the word means to the person as he/she proceeds through tasks. There are likely diverse understandings of words like intention and urge. Researchers may wish to clearly define any terms used in an experiment, but this leads to a further issue, related to the original question raised by Dominik et al. (2017) and address in this study: does providing a priori distinctions, like definitions, induce demand characteristics? That is, even if researchers try to remove individual differences in understanding of a term, this may induce confusion and/or invalid task performance. This seems to be another case of a HUP; the more a natural subjective experience or understanding is tampered with, the more it is likely to be unduly biased or changed. This possibility lends even more credibility to the interpretation of results which held that intention was present in the tasks but that attempts to measure intention inevitably alter one’s natural phenomenology (see Discussion).

Richards (2010, p. 294) has noted how applying language to psychological phenomena is a highly reflexive enterprise. Crucially, how one labels, discusses, and thinks about psychological phenomena is likely to affect the phenomena. Furthermore, a more fundamental issue looms; given the notorious difficulty of externally verifying introspective experience, how could researchers ever know participants were conforming to what they have in mind for “urge” or whatever word is chosen?

Researchers may simply have to accept individual differences in the understanding of vague terms like intention or urge and/or in the phenomenology of carrying out movements like those in the Libet study. Future research could attempt to examine the influence of different word choices on participants. Different groups could complete identical tasks and simply vary based on which word is chosen to describe the impetus to move in the M Task and W Task and what to report in the W Task.

Control Condition. A limitation of the current study is its lack of a control condition in which participants undergo the normal Libet study methodology. A control condition would include two more groups: an M-First Control and a W-First Control, each of which completed tasks in the same order as their experimental group counterparts. These groups would differ in that they would be fully informed of both the M Task and W Task at the beginning of the experiment, and of the distinction between them. In this way, it could be determined whether the results of the current study are in any way influenced by the mere order of task completion rather than the withholding of information that keeps participants naïve about the distinction between movement and intention.

Untreated Factors. A variety of untreated factors may have contributed some variance to results. For example, I did not attempt to measure or correct small lags that may have occurred between linked hardware and software systems. I collected data on handedness and gender but have not conducted advanced analyses involving these and other variables as covariates. In addition, I did not collect data on other variables which might have impacted task performance, such as working memory capacity, familiarity with computers, and frequency of using a computer mouse. I did not counterbalance variables such as mouse response mapping and time of day of sessions. I did not have a harness available to achieve a maximally stable visual angle, so individual differences in height, movement, sitting style, etc. may have caused slight deviations from the desired visual angle. Unfortunately, responses to the preplanning question at the end of the M Task and W Task, which was intended to identify and/or remove potentially incorrectly executed trials, were not saved due to researcher error in programming the tasks. Therefore, no blocks of trials could be discarded on the basis of reported preplanning.

The results of the current study were based on group averages. It is possible that individuals experienced the tasks and instructions differently. Indeed, as noted above (see Results), there was variability on many questions of the PEQ, though this variability was not significant between groups. Specifically, there was division among participants regarding whether there was a gap in time between intentions and movements, whether an intention was always felt before they moved, and what term to use to label the phenomenon they were reporting in the W Task. Some individuals may readily feel and/or make a distinction between moving and intending to move, while others may not. For example, one participant reported that she felt she was "lying" about her reports on the W Task to comply with instructions. It seems reasonable to suppose that individual differences exist in the phenomenology of carrying out movements like those in the Libet study (Nahmias et al., 2004). Though the PEQ did not detect any differences between groups, a more rigorous and validated test, designed specifically to test for differences in phenomenologies, might have found differences between groups. Such differences might have existed and contributed unknown variance to the data. Future studies should administer tests for these differences prior to completion of tasks, so that groups can be weighted appropriately.

Standardization. Another limitation of the current study, one extendable to other studies in the Libet paradigm, is that it uses visual stimuli from one experiment (Jo et al., 2013) but replicates the methodology of another experiment (Dominik et al., 2017). It seems plausible that small differences in stimuli could significantly impact findings. For example, the clock used in the current study uses a rotating clock hand (see Method), while Dominik et al. used a rotating dot. The nature of the clock face also differed in many ways (e.g., color, fixation stimulus, number of intervals), though the basic setup (e.g., rotating clockwise, revolution period of 2.56 seconds) was mostly identical. Without standardized stimuli, it seems important to note the possibility that at least some of the variance in results among studies, such as the discrepancies in the findings of the current study compared to the findings of Libet et al. (1983), is due to differences in stimuli. Future researchers might make a project of standardizing stimuli and other aspects of the Libet study's methodology. In the absence of such standardization, researchers should seek to mirror all aspects of the studies whose findings they are seeking to replicate. The current study could be improved, then, by changing the nature of its stimuli to match that of Dominik et al. Alternatively, the current study's stimuli could be changed to mirror that of Libet et al. or a more recent replication of the study. Lastly, it should be noted that the current study used mouse clicks, while Dominik et al. used button presses and Libet et al. used wrist flexions. Though all these movements appear to be similar, one cannot rule out the possibility that some of the differences between studies are attributable to differences in type of movement.

Clock Precision. Another limitation of the current study extendable to the Libet study in general is that participant reports may appear more precise than they actually are. As noted, each tick on the clock spanned approximately 16.667 milliseconds, or 2.35° of the clock face (see Materials). Recall that participants, as they noted clock hand position, were supposed to be looking at the center of the clock rather than the clock hand or clock face. It seems unlikely that such a setup would allow participants to note and report clock hand position with accuracy on the scale of tens of milliseconds. One participant specifically noted that looking at the center prevented an accurate report of clock hand position and that he/she could only report a number “close by” the probable position. The purpose of the S Task was to try to account for general bias in using the clock. As discussed, there was extreme variability in S Task results (Appendix K). This may reflect, at least in part, a human subject limitation in using the Libet-style clock. It may be that the maximum attainable temporal resolution with the clock is on the order of 50ms or more, as reflected by S Task results. For example, though the clock ticks every 16.667ms, the average person may do well just to report clock hand position within 3 or more ticks or its actual location.

This is an important consideration for all versions of the Libet study, since effects are on the order of hundreds of milliseconds or less. This becomes even more important when comparing the late RP and intention, since these two “events” appear to onset even closer in time than the RP and intention in the traditional Libet study. This alone should cause us to reconsider the argument of Banks and Pockett (2007) that various methodological issues with the Libet study should be ignored (see Introduction). These authors argued that such variability could be ignored because the temporal order of the RP and intention, being 350ms or greater, was not likely to be reversed by any individual factor. The imprecision of the clock method of reporting, as well as other variables contributing to variance in reports, could significantly shrink or even reverse the interval between the late RP and intention reports (assuming the validity of traditional intention reports).

Motivation and Arousal. It seems plausible that motivation and arousal might account for some of the findings of the current study. It may be that participants are more engaged in the first task (S Task) and second task than the third task, which might have led to confounding factors in the third task completed. The second task used the same clock as the S Task but was novel in that it departed significantly from S Task methodology. The third task, by comparison, was identical to the second task aside from requiring a different type of report. All tasks, of course, were repetitive and simple. Participants' motivation to engage may have waned once they realized that the third task was minimally novel and of the same nature as the second task. Though the second task only lasted about 7-10 minutes, participants completed 60 regular trials, which was undoubtedly taxing. The third task also consisted of 60 regular trials. Thus, it is possible that some of the differences between the second and third task completed are attributable to differences in attentional engagement. However, it should be noted that this potential confound does not affect the core finding of the nonsignificant difference between groups on the second task completed. Future studies should find ways to ensure that motivation and arousal are sustained. For example, researchers might build in more breaks within tasks or give a longer break between tasks.

VII. Conclusions and Future Directions

I was able to replicate the basic results of Dominik et al. (2017). My basic goal was to determine whether the intention reports used by Libet et al. (1983) were valid measures of intention. Nearly all possible interpretations of the current study's results are damaging to the validity of these intention reports. Those who wish to defend the validity and findings of the Libet study are left with some recourse, but the prospects are not good (see Discussion). However, more replications of the study by Dominik et al. are needed. Future studies could overcome the aforementioned limitations of the current study. For example, all potential covariates could be measured and included in analyses. In these replications and in any version of the Libet study, researchers should ensure they are precisely capturing the moment of movement onset, and they should clearly report the procedures employed to do so. The basic findings of the current study and other versions of the Libet study will become clearer as more variance is removed from the data and as more standardization is achieved across studies. However, from the foregoing discussion of results and of the philosophical issues in play, it seems reasonable to assume that some level of uncertainty will always be present in attempts to scientifically study conscious intention. No matter how much progress is made, or apparently made, it may be that studying conscious intention inevitably runs up against intractable problems like inferential distance, causes irresolvable dilemmas like various forms of the Heisenberg Uncertainty Principle, or leads us to the non-empirical realm of a priori metaphysical assumptions.

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Appendices

Appendix A: Abbreviations

Appendix A: Abbreviations

Abbreviated Terms		
Abbreviation	**Term**	**Definition**
EEG	electroencephalography	Measurement of the brain's electrical activity via electrodes on the scalp
EMG	electromyography	Measurement of bodily movement via electrodes placed on target areas
ERP	event-related potential	Waveform elicited by a specific neural event; created by averaging many EEG readings at a single electrode site
RP	readiness potential	ERP waveform found to precede voluntary movements; Slow negative shift in activity at Cz electrode site between .5 and 2 seconds before a voluntary movement
W	"wanting" time	Time of conscious awareness of the urge or the "wanting" to move, as reported by participants
M	movement time	Time of actual movement, as reported by participants
S	stimulus time	Time of perception of an external stimulus, as reported by participants
SMA	supplementary motor area	Source of the RP; located in the posterior area of frontal cortex
Early RP	early readiness potential	Earliest portion of the RP, occurring between ~1000-400ms prior to movement
Late RP	late readiness potential	Latest portion of the RP, occurring between movement and ~400ms prior to movement

Abbreviated Terms (continued)		
Abbreviation	**Term**	**Definition**
ERD	event-related desynchronization	A temporary, localized decrease in amplitude of a particular band (frequency) of rhythmic brain activity
ASQ	Attributional Style Questionnaire	Yields composite score and binary classification of positive or negative attributional style
PEQ	Post-Experiment Questionnaire	Includes 10 questions about participants' experiences with various aspects of the study
S Task	Stimulus Task	Participants reported the onset of a tone
M Task	Movement Task	Participants made a spontaneous voluntary movement and reported the moment they initiated their movement
W Task	Wanting (Intention) Task	Participants made a spontaneous voluntary movement and reported the moment they intended to move
Late RP Task	Late Readiness Potential Task	Participants made a spontaneous voluntary movement without monitoring an analog clock
M-First Group	Movement Task-First Group	Participants in this group completed the S Task, M Task, and the W Task, respectively
W-First Group	Wanting (Intention) Task-First Group	Participants in this group completed the S Task, W Task, and the M Task, respectively
HUP	Heisenberg Uncertainty Principle	In physics: the speed and position of an object cannot be exactly measured simultaneously

Appendix B: Consent Form

Appendix B: Consent Form

Consent to Participate in a Research Study

Conscious Intention and Free Will: A Hybrid Replication of the Libet Study

Why am I being asked to participate in this research?

You are being invited to take part in a research study about making simple movements (pressing a button) while viewing a clock on a computer screen. You are being invited to participate in this study because you are an undergraduate student at Eastern Kentucky University. If you take part in this study, you will be one of about 40 people to do so.

Who is doing the study?

The person in charge of this study is Paul Sanford at Eastern Kentucky University. He is being guided in this research by Adam Lawson, Ph.D. There may be other people on the research team assisting at different times during the study, including Nicole King & Madison Major.

What is the purpose of the study?

By doing this study, we hope to learn about the timing of brain processes related to conscious movement.

Where is the study going to take place and how long will it last?

The research procedures will be conducted at Room 107 in the Cammack Building on the campus of Eastern Kentucky University (468 Lancaster Ave., Richmond, KY 40475). You will need to come to 107 Cammack on two consecutive days for the study. Each visit will take about 1.5 hours, for a total of 3 hours over two days. You will earn 6 outside credit units on SONA for your participation.

What will I be asked to do?

There are several tasks to be completed, with some on Day 1 and others on Day 2. You will be randomly assigned to one of two groups, with the difference being the order of tasks you are asked to do. You will be fully informed of the differences in the groups after you have completed the study. Each day you will be hooked up to our physiology equipment, which measures electroencephalographic (EEG) activity and electromyographic (EMG) activity.

Day 1:

1. You will begin by completing a Personal Information Sheet and the Attributional Style Questionnaire. This should take about 15 minutes.
2. Next you will be hooked up to the physiology equipment.
    a. A standard electrode cap will be placed on your head, with five electrode sites used to collect data.
    b. You will complete two separate blocks of tasks. Each block will consist of five training trials and then 40 normal trials. On both blocks, you will be making a key press with the index finger of your dominant hand.
    c. For these tasks, you will be monitoring a revolving dot on a digital clock to make certain types of reports, which will be explained in detail as you progress through the experiment. You will mentally note the dot's position on the clock and then use a mouse to indicate that position when a certain event occurred.
    d. Each block will take about 25 minutes. You will be allowed to rest for two minutes after the first block and for one minute halfway through each block. Within each block, you will be allowed to rest 2-3 seconds between each trial you complete. This process will take about one hour.
    e. You will then be unhooked from the EEG system. You will be allowed to use our towels and sink to clean your hair if you wish, and then you may leave. In all, you will spend about 1.5 hours with us on Day 1.

Day 2:

1. You will be hooked up to the physiology equipment.
    a. This time, you will complete six blocks of tasks. Each block will consist of 30 trials. There are four different tasks to complete. You will complete two of them once and two of them twice, for a total of six blocks. Each block will contain only one type of task.
    b. For five out of the six blocks, you will be asked to make a key press with the index finger of your dominant hand.

c. For most of the blocks, you will be using the same clock to make reports as you did in Task 1. The specific events and instructions will be described as you progress through the study.
d. Each block will take about 8 minutes. You will be allowed to rest for two minutes between each block and for one minute halfway through each block. Within each block, you will be allowed to rest 2-3 seconds between each trial you complete.
e. This entire process should take about one hour. You will then be unhooked from the equipment and allowed to clean up your hair if you wish.

2. Before leaving, you will be asked to complete the Post-Experiment Questionnaire. Then you will be debriefed on the experiment. This should take about 15 minutes. In all, you will spend about 1.5 hours here on Day 2.
3.

Are there reasons why I should not take part in this study?

Only those individuals who meet ALL the following requirements will be allowed to participate.

- EKU students who are registered in the Psychology department's research signup (SONA) system.
- At or above 18 years of age.
- Having normal or corrected vision.
- Having normal or corrected hearing.
- NOT having any prior nor current neurological disorder.
- NOT having any learning or reading disability.
- NOT pregnant.

What are the possible risks and discomforts?

To the best of our knowledge, the things you will be doing have no more risk of harm than you would experience in everyday life.

The physiological and computer equipment with which you will come into contact will be electrically shielded to ensure that risk of electrical shock is no greater than their everyday use of electronics. To minimize the risk of electrical shock during a storm, the experiment will be suspended when lightning is in the area.

EEG hookup with an electrode cap can be mildly uncomfortable if worn for long periods of time. The cap may fit tightly to your head, putting mild pressure on your scalp. To minimize this discomfort, you will only be hooked up to the physiology equipment while you are completing the computer tasks. As soon as you finish, the equipment will be removed, before you complete the rest of the study.

To establish connections between your skin and the electrodes, we use hypoallergenic saline gel, which is not known to harm hair or skin. However, dried gel can give the appearance of dandruff or dried hair gel. We recommend that you bring a hat or hair wrap to wear after you leave. You will be allowed to rinse or wash your hair in the sink in Cammack 111. Although we have no knowledge of any aversive reactions to the saline gel, we recommend washing your hair before the end of the day as a precaution.

Upon arrival, you will be asked to provide your SONA identification code, which the researcher will use to verify that you are the correct participant. Your name will be recorded on this form but on no others. This consent form will be securely kept separately from all other documentation, so only your SONA code will appear with all other documentation and data. All computers are kept in secure research rooms and contain security codes in order to ensure that non-researchers do not have access to the data. Paper records, including the consent form, and electronic files of data will only be stored in Cammack 109 on the EKU Richmond campus. The room is kept locked, and data files will only be stored in locked cabinets and locked computers. All data is computerized (no paper trail) and will be kept for at least 3 years following the study's completion. Computer data is coded using your SONA code, so no personally identifiable information is kept with the computer data. No personally identifiable information will be kept on any document held by the researcher, except for the consent form, which will contain the name and 5-digit SONA code of the participant.

Will I benefit from taking part in this study?

You will not get any personal benefit from taking part in this study other than 6 SONA credits (see below).

Do I have to take part in this study?

If you decide to take part in the study, it should be because you really want to volunteer. You will not lose any benefits or rights you would normally have if you choose not to volunteer. You can stop at any time during the study and still keep the benefits and rights you had before volunteering.

If I don't take part in this study, are there other choices?

If you do not want to take part in the study, there are other studies on SONA in which you can participate to earn SONA credits.

What will it cost me to participate?

There are no costs associated with taking part in this study.

Will I receive any payment or rewards for taking part in the study?

You will receive 6 SONA credits for taking part in this study. If you should have to quit before the study is finished, the credits you receive will be based on the proportion of time you were in the study.

Who will see the information I give?

Your information will be combined with information from other people taking part in the study. When we write up the study to share it with other researchers, we will write about this combined information. You will not be identified in these written materials.

We will make every effort to prevent anyone who is not on the research team from knowing that you gave us information, or what that information is. For example, your name will be kept separate from the information you give, and these two things will be stored in different places under lock and key.

There are extreme circumstances in which we may have to show your information to other people, including a court order, a suspicion of child abuse, or a concern that you may harm yourself. Keep this in mind when talking to us. Also, we may be required to show information that identifies you to people who need to be sure we have done the research correctly; these would be people from such organizations as Eastern Kentucky University ethics boards (i.e., IRB).

Can my taking part in the study end early?

If you decide to take part in the study, you still have the right to decide at any time that you no longer want to participate. You will not be treated differently if you decide to stop taking part in the study.

The individuals conducting the study may need to end your participation in the study. They may do this if you are not able to follow the directions they give you, if they find that your being in the study is more risk than benefit to you, or if the agency funding the study decides to stop the study early for a variety of scientific reasons.

If you should have to quit before the study is finished, the credits you receive will be based on the proportion of time you were in the study.

What happens if I get hurt or sick during the study?

If you believe you are hurt or you get sick because of something that is done during the study, you should call Paul Sanford at (270) 776-4305 immediately. It is important for you to understand that Eastern Kentucky University will not pay for the cost of any care or treatment that might be necessary because you get hurt or sick while taking part in this study. That cost will be your responsibility. Also, Eastern Kentucky University will not pay for any wages you may lose if you are harmed by this study.

Usually, medical costs that result from research-related harm cannot be included as regular medical costs. Therefore, the costs related to your child's care and treatment because of something that is done during the study will be your responsibility. You should ask your insurer if you have any questions about your insurer's willingness to pay under these circumstances.

What if I have questions?

Before you decide whether to accept this invitation to take part in the study, please ask any questions that might come to mind now. Later, if you have questions about the study, you can contact the investigator, Paul Sanford, at paul_sanford2@mymail.eku.edu or (270) 776-4305. If you have any questions about your rights as a research volunteer, contact the staff in the Division of Sponsored Programs at Eastern Kentucky University at 859-622-3636. We will give you a copy of this consent form to take with you.

What else do I need to know?

You will be told if any new information is learned which may affect your condition or influence your willingness to continue taking part in this study. You may ask the researcher questions at any time.

I have thoroughly read this document, understand its contents, have been given an opportunity to have my questions answered, and agree to participate in this research study.

____ __________________________

Signature of person agreeing to take part in the study   Date

____

Printed name of person taking part in the study

____

Name of person providing information to subject

Appendix C: Attributional Style Questionnaire

Appendix C: Attributional Style Questionnaire

Attributional Style Questionnaire

Please try to vividly imagine yourself in the situations that follow. If such a situation happened to you, what would you feel would have caused it? While events may have many causes, we want you to pick only one—the *major* cause if this event happened to *you.* Please write this cause in the blank provided after each event. Next we want you to answer some questions about the *cause.* To summarize, we want you to:

1. Read each situation and vividly imagine it happening to you.
2. Decide what you feel would be the *major* cause of the situation if it happened to you.
3. Write one cause in the blank provided.
4. Answer three questions about the *cause.*
5. Go on to the next situation.

You meet a friend who compliments you on your appearance.

1. Write down one major cause ____.

2. Is the cause of the complement due to something about you or something about other people or circumstances? (Circle one number)

Totally due to other people or circumstances — Totally due to me

1 2 3 4 5 6 7

3. In the future when a friend compliments you on your appearance, will this cause again be present? (Circle one number)

Will never again be present — Will always be present

1 2 3 4 5 6 7

4. Is the cause something that just influences you getting a compliment from a friend, or does it also influence other areas of your life? (Circle one number)

Influences just this particular situation in my life — Influences all situations

1 2 3 4 5 6 7

You have been looking for a job unsuccessfully for some time.

1. Write down one major cause ____.

2. Is the cause of your unsuccessful job search due to something about you or something about other people or circumstances? (Circle one number)

Totally due to other people or circumstances — Totally due to me

1 2 3 4 5 6 7

3. In the future when looking for a job, will this cause again be present? (Circle one number)

Will never again be — Will always

present be present

1 2 3 4 5 6 7

4. Is the cause something that just influences looking for a job, or does it also influence other areas of your life? (Circle one number)

Influences just this particular situation in my life

Influences all situations

1 2 3 4 5 6 7

You become very rich.

1. Write down one major cause ____.

2. Is the cause of your becoming rich due to something about you or something about other people or circumstances? (Circle one number)

Totally due to other people or circumstances

Totally due to me

1 2 3 4 5 6 7

3. In the future if you become rich again, will this cause again be present? (Circle one number)

Will never again be present — Will always be present

1 2 3 4 5 6 7

4. Is the cause something that just influences becoming rich, or does it also influence other areas of your life? (Circle one number)

Influences just this particular situation in my life — Influences all situations

1 2 3 4 5 6 7

A friend comes to you with a problem and you don't try to help.

1. Write down one major cause ____.

2. Is the cause of your not helping due to something about you or something about other people or circumstances? (Circle one number)

Totally due to other people or circumstances — Totally due to me

1 2 3 4 5 6 7

3. In the future when a friend comes to you with a problem, will this cause again be present? (Circle one number)

Will never again be present						Will always be present
1	2	3	4	5	6	7

4. Is the cause something that just influences you when a friend comes to you with a problem, or does it also influence other areas of your life? (Circle one number)

Influences just this particular situation in my life						Influences all situations
1	2	3	4	5	6	7

You give an important talk in front of a group and the audience reacts negatively.

1. Write down one major cause ____.

2. Is the cause of the negative audience reaction due to something about you or something about other people or circumstances? (Circle one number)

Totally due to other people or circumstances — Totally due to me

1 2 3 4 5 6 7

3. In the future when you give an important talk in front of a group, will this cause again be present? (Circle one number)

Will never again be present						Will always be present
1	2	3	4	5	6	7

4. Is the cause something that just influences you when you give an important talk in front of a group, or does it also influence other areas of your life? (Circle one number)

Influences just this particular situation in my life						Influences all situations
1	2	3	4	5	6	7

You do a project that is highly praised.

1. Write down one major cause ____.

2. Is the cause of the praise due to something about you or something about other people or circumstances? (Circle one number)

Totally due

to other

people or Totally due to

circumstances me

1 2 3 4 5 6 7

3. In the future when you complete projects, will this cause again be present? (Circle one number)

Will never

again be Will always

present be present

1 2 3 4 5 6 7

4. Is the cause something that just influences you receiving praise for a project, or does it also influence other areas of your life? (Circle one number)

Influences

just this

particular

situation Influences

in my life all situations

1 2 3 4 5 6 7

You meet a friend who acts hostilely toward you.

1. Write down one major cause ____.

2. Is the cause of your friend's hostility due to something about you or something about other people or circumstances? (Circle one number)

Totally due

to other

people or Totally due to

circumstances me

1 2 3 4 5 6 7

3. In the future when you meet with friends, will this cause again be present? (Circle one number)

Will never

again be Will always

present be present

1 2 3 4 5 6 7

4. Is the cause something that just influences your friends' hostility towards you, or does it also influence other areas of your life? (Circle one number)

Influences

just this

particular

situation Influences

in my life all situations

1 2 3 4 5 6 7

You can't get all the work done that others expect of you.

1. Write down one major cause ____.

2. Is the cause of your inability to get all the work done due to something about you or something about other people or circumstances? (Circle one number)

Totally due

to other

people or Totally due to

circumstances me

1 2 3 4 5 6 7

3. In the future when you are trying to get all the work done that others expect of you, will this cause again be present? (Circle one number)

Will never

again be Will always

present be present

1 2 3 4 5 6 7

4. Is the cause something that just influences your ability to get all the work done, or does it also influence other areas of your life? (Circle one number)

Influences

just this

particular

situation Influences

in my life all situations

1 2 3 4 5 6 7

Your spouse (boyfriend/girlfriend) has been treating you more lovingly.

1. Write down one major cause
____.

2. Is the cause of this more loving treatment due to something about you or something about other people or circumstances? (Circle one number)

Totally due

to other

people or Totally due to

circumstances me

1 2 3 4 5 6 7

3. In the future when you are treated more lovingly by your spouse (boyfriend/girlfriend), will this cause again be present? (Circle one number)

Will never

again be Will always

present be present

1 2 3 4 5 6 7

4. Is the cause something that just influences how you are treated by your spouse (boyfriend/girlfriend), or does it also influence other areas of your life? (Circle one number)

Influences

just this

particular

situation Influences

in my life all situations

1 2 3 4 5 6 7

You apply for a position that you want very badly (e.g. important job, graduate school admission) and you get it.

1. Write down one major cause ____.

2. Is the cause of you getting the position due to something about you or something about other people or circumstances? (Circle one number)

Totally due to other people or circumstances						Totally due to me
1	2	3	4	5	6	7

3. In the future when you apply for a position you want badly, will this cause again be present? (Circle one number)

Will never again be present						Will always be present
1	2	3	4	5	6	7

4. Is the cause something that just influences you when you are applying for positions you want badly, or does it also influence other areas of your life? (Circle one number)

Influences just this particular situation in my life						Influences all situations
1	2	3	4	5	6	7

You go out on a date and it goes badly.

1. Write down one major cause
   ____.

2. Is the cause of your date going badly due to something about you or something about other people or circumstances? (Circle one number)

Totally due
to other
people or
circumstances

Totally due to
me

1 2 3 4 5 6 7

3. In the future when you go on a date, will this cause again be present? (Circle one number)

Will never
again be
present

Will always
be present

1 2 3 4 5 6 7

4. Is the cause something that just influences going on a date, or does it also influence other areas of your life? (Circle one number)

Influences
just this
particular
situation
in my life

Influences
all situations

1 2 3 4 5 6 7

You get a raise.

1. Write down one major cause
____.

2. Is the cause of your raise due to something about you or something about other people or circumstances? (Circle one number)

Totally due to other people or circumstances						Totally due to me
1	2	3	4	5	6	7

3. In the future when you get a raise, will this cause again be present? (Circle one number)

Will never again be present						Will always be present
1	2	3	4	5	6	7

4. Is the cause something that just influences you getting a raise, or does it also influence other areas of your life? (Circle one number)

Influences just this particular situation in my life						Influences all situations
1	2	3	4	5	6	7

Appendix D: Personal Information Sheet

Appendix D: Personal Information Sheet

SONA ID: ____________________

Gender ______________________

Age (in years) __________________

Handedness (circle one): Left Right Ambidextrous

Place an X next to all racial demographics that apply:

________ Caucasian

________ Hispanic

________ African American

________ Asian

________ Pacific Islander

________ Native American

________ Other: ______________

Are you taking any medication or any substance which might affect your ability to focus during a computer task?

Yes / No

If yes, then briefly explain:

Appendix E: Post-Experiment Questionnaire

Appendix E: Post-Experiment Questionnaire

Instructions to researcher: For items 1-9, ask participants to respond Yes/No to each question and record their responses. Ask them to explain each answer. For item 10, have participants select among the options and explain their reasoning.

1. Were you confused by any aspects of the study?
2. Did your performance on tasks change as you progressed through the study?
3. Are you confident that most of your intention reports were accurate?
4. Are you confident that most of your movement reports were accurate?
5. Did you feel there was a gap in time between your intentions and your movements?
6. Did you always feel an intention to move before you moved?
7. Did you use a strategy when monitoring the clockhand?
8. Did you tend to make your clicks at similar places (e.g. near one or two particular spots)?
9. Have you ever heard of the Libet study?
10. Which word best describes what you reported in W tasks: intention, urge, impulse, or some other word?

Appendix F: Debriefing Form

Appendix F: Debriefing Form

Thank you for your participation. The main purpose of this study was to examine the brain processes involved in performing a conscious action. We will compare the various reports you made with the activity your brain showed during your button presses. This study was a replication of a study known as the "Libet study," which was originally conducted by Benjamin Libet and colleagues (Libet, Gleason, Wright, & Pearl, 1983). The Libet study seemed to show that the onset of brain activity associated with conscious (voluntary) movements preceded the intention to move.

An EEG (brainwave) wave known as the *readiness potential* was shown to begin about one-third of a second before the reported intention to move. This finding was somewhat surprising, since it seemed to show that the brain had perhaps already made the unconscious decision to move before the conscious intention to move had occurred. The Libet study continues to be replicated and expanded upon by researchers, but it has been widely criticized for many reasons. For example, it is debatable whether the extremely simple movements made by participants truly involve what we would call conscious intentions. This study is an attempt to address some of the Libet study's criticisms, but it is based on the same idea and experimental setup.

There were two groups in the current study, which differed in the order in which they completed the Movement (M) task and Wanting (W) task on Day 1. One group completed the M task and then the W task, and the other group completed the W task and then the M task. Otherwise the two groups were identical. You had a 50% chance of being chosen for either group. We did this because we wanted to compare the performance of each group on the first task completed. A recent study suggested that when participants are not given much information about all the tasks that they will complete, they might report their movement and their intention to move as happening at the same point in time rather than at different times (Dominik et al., 2017). This is important because it could be that in these kinds of experiments, intentions are inferred rather than actually experienced.

One major innovation of this study is the use of only the *late* part of the readiness potential to compare to intention reports, since recent research has shown that only this part is possibly related to the movement (Schmidt, Jo, Wittmann, & Hinterberger, 2016). Many of the other innovations of this study will come during data analysis. For example, the EEG data will be filtered in multiple ways to see how different filters affect the onset of the readiness potential. The use of the Attributional Style Questionnaire was included because attributional style (positive vs. negative) has been shown to be correlated with intention reports (Penton, Thierry, & Davis, 2014). This study developed the Post-Experiment Questionnaire in order to better understand what the experiment was like for you (e.g., if you were confused by any aspects of the experiment). We are interested in seeing how this and other information gathered will associate with your brain activity and reports of conscious intention.

The “late RP” task, in which you made finger presses without making reports, will be used to calculate the average onset of the readiness potential. The W task, in which you made reports of your intention, will be used to calculate the average onset of your conscious intention to move. The M task, in which you made reports of when you actually moved, will be compared to the W task. The S task, in which you reported when the tone occurred, will be used to determine how accurate you are with using the clock to make reports. Whatever your error is in making S reports (e.g., making them 50ms too late) will be subtracted from your W and M reports to correct them. Once data collection is complete, your data will be combined with the data of all other participants into a grand average. We will compare your individual data with this grand average.

If you are interested in knowing the outcomes of this study or others in Dr. Lawson's Psychophysiological Lab, let the researcher know and you will be added to our lab newsletter that we send out twice a year. If you have any questions, please contact Paul Sanford at (270) 776-4305 or paul_sanford2@mymail.eku.edu.

For more information:

Dominik, T., Dostal, D., Zielina, M., Smahaj, J., Sedlackova, Z., & Prochazka, R. (2017). Libet's experiment: Questioning the validity of the urge to move. *Consciousness and Cognition, 49,* 255-263.

Libet, B., Gleason, C.A., Wright, E. W., & Pearl, D. K. (1983). Time of conscious intention to act in relation to onset of cerebral activity (readiness-potential). *Brain, 106 (3),* 623-642.

Penton, T., Thierry, G. L., & Davis, N. J. (2014). Individual differences in attributional style but not in interoceptive sensitivity, predict subjective estimates of action intention. *Frontiers in Human Neuroscience, 8,* 1-6. http://doi.org/10.3389/fnhum.2014.00638.

Schmidt, S., Jo, H., Wittmann, M., & Hinterberger, T. (2016). 'Catching the waves' – slow cortical potentials as moderator of voluntary action. *Neuroscience and Biobehavioral Reviews, 68,* 639-650.

Appendix G: S Task Instructions

Appendix G: S Task Instructions

Hello! In this task, a small clock will be presented in the middle of the screen. After a short time, it will start to rotate from a random position. Please focus on the middle of the clock and at the same time pay attention to the position of the clockhand.

You will be listening for a brief tone to be played. You will report where the clockhand was when you first heard the tone.

To do this, first press the left mouse button anytime after the tone is played. This will stop the clock. Note that this is NOT a reaction time task - do not try to press the mouse right when the tone occurs. Just press the button after within a few seconds after you hear the tone. Then follow the instructions on screen to report the clockhand position.

You will first complete some training trails so you can get used to the task. Then you will complete some regular trials.

Press the left mouse button to continue.

Appendix H: M Task Instructions

Appendix H: M Task Instructions

Hello! In this task, a small clock will be presented in the middle of the screen. After a short time, it will start to rotate from a random position. Please focus your eyes on the center of the clock and at the same time pay attention to the position of the clockhand. During this task you will be clicking the left mouse button and reporting where the clockhand was when you began your click.

Please allow the clockhand to make at least one full rotation before you make your click. You may click anytime after it has made one rotation.

Please make your click as spontaneously as possible – click when the urge arises. Do not plan the click ahead of time. Be spontaneous on each trial.

After your click, follow the on-screen instructions to make your report. Please report the time when you began your movement (when you began your click).

A final note: please try to keep from blinking or moving your eyes or body during the task. If you must blink or move, please do so between trials. Press the left mouse button to continue.

Appendix I: W Task Instructions

Appendix I: W Task Instructions

Hello! In this task, a small clock will be presented in the middle of the screen. After a short time, it will start to rotate from a random position. Please focus your eyes on the center of the clock and at the same time pay attention to the position of the clockhand. During this task you will be clicking the left mouse button and reporting where the clockhand was when you had the intention to click.

Please allow the clockhand to make at least one full rotation before you make your click. You may click anytime after it has made one rotation.

Please make your click as spontaneously as possible – click when the urge arises. Do not plan the click ahead of time. Be spontaneous on each trial.

After your click, follow the on-screen instructions to make your report. Please report the time when you first had the intention to click.

A final note: please try to keep from blinking or moving your eyes or body during the task. If you must blink or move, please do so between trials. Press the left mouse button to continue.

Appendix J: Example of a Preplanning Question (W Task)

Appendix J: Example of a Preplanning Question (W Task)

Did you preplan any of your movements during any of the 60 regular trials you just completed?

If no, please press 0.

If yes, please press the number next to the correct estimate of how many times you preplanned.

1 = 5 times or less

2 = 6-10 times

3 = 11-15 times

4 = 16-20 times

5 = 21 or more times

Appendix K: S Task Performance

Appendix K: S Task Performance

S Task Performance		
Participant	Average (seconds)	St. Dev. (seconds)
	0.048	0.089
	0.091	0.102
	0.141	0.115
	0.054	0.116
	0.058	0.088
	0.015	0.054
	0.040	0.133
	0.043	0.115
	0.067	0.148
	0.033	0.076
	0.082	0.155
	0.062	0.085
	0.005	0.050
	0.021	0.109
	0.003	0.079
	0.006	0.084
	0.152	0.088
	0.042	0.098
	0.020	0.074
	0.195	0.215
	0.017	0.068
	0.104	0.043
	0.053	0.077
	0.059	0.103
	0.064	0.075
	0.053	0.038
	0.004	0.099
	0.043	0.070
	0.030	0.064
Grand Average	**0.055**	**0.093**

Appendix L: EMG Corrections

Appendix L: EMG Corrections

EMG Corrections				
	M Task (seconds)		W Task (seconds)	
Participant	Average	Standard Deviation	Average	Standard Deviation
	0.103	0.027	0.107	0.029
	0.134	0.024	0.137	0.024
	0.148	0.024	0.137	0.030
	0.138	0.027	0.135	0.027
	0.141	0.028	0.128	0.026
	0.150	0.021	0.150	0.022
	0.145	0.022	0.143	0.026
	0.128	0.030	0.142	0.027
	0.140	0.027	0.147	0.028
	0.131	0.024	0.144	0.030
	0.141	0.030	0.140	0.023
	0.108	0.023	0.116	0.023
	0.131	0.028	0.127	0.029
	0.117	0.024	0.132	0.029
	0.125	0.030	0.131	0.046
	0.112	0.026	0.115	0.023
	0.141	0.028	0.149	0.025
	0.145	0.025	0.143	0.026
	0.117	0.020	0.133	0.026
	0.142	0.030	0.146	0.023
	0.107	0.021	0.108	0.025
	0.131	0.028	0.134	0.026
Grand Averages	**0.130**	**0.029**	**0.133**	**0.030**

Appendix M: M Task and W Task Performance

Appendix M: M Task and W Task Performance

M Task and W Task Performance				
Participant	M Task (seconds)		W Task (seconds)	
M-First	Average	St. Dev.	Average	St. Dev.
	0.113	0.078	0.096	0.176
	0.109	0.075	0.095	0.077
	0.157	0.076	0.063	0.246
	0.164	0.371	0.006	0.435
	0.156	0.122	-0.363	0.217
	0.100	0.182	-0.054	0.123
	0.133	0.068	-0.463	0.267
	0.089	0.153	0.049	0.185
	0.091	0.104	0.077	0.135
	0.223	0.051	0.245	0.097
Average	**0.117**	**0.169**	**-0.007**	**0.282**
W-First				
	0.031	0.174	0.099	0.109
	0.097	0.105	0.125	0.080
	0.079	0.284	0.010	0.225
	-0.026	0.132	0.212	0.087
	0.098	0.050	0.132	0.040
	0.023	0.067	0.010	0.051
	0.183	0.052	0.183	0.043
	0.144	0.056	0.094	0.125
	0.153	0.055	0.089	0.104
	-0.180	0.293	0.011	0.364
	0.104	0.047	0.132	0.043
	0.105	0.064	0.216	0.109
Average	**0.066**	**0.166**	**0.110**	**0.161**
Grand Averages	**0.088**	**0.169**	**0.058**	**0.230**

Appendix N: M Task and W Task Performance (Data Uncorrected by EMG)

Appendix N: M Task and W Task Performance (Data Uncorrected by EMG)

M Task and W Task Performance (EMG Uncorrected)				
Participant	M Task (seconds)		W Task (seconds)	
M-First	Average	St. Dev	Average	St. Dev
	0.010	0.071	0.005	0.079
	-0.022	0.076	-0.044	0.073
	0.020	0.071	-0.188	0.198
	0.044	0.414	-0.112	0.465
	0.034	0.114	-0.369	0.437
	-0.240	0.168	-0.208	0.121
	0.014	0.088	-0.457	0.412
	-0.058	0.178	-0.095	0.176
	-0.046	0.108	-0.069	0.123
	0.094	0.053	0.109	0.092
Average	**-0.015**	**0.183**	**-0.134**	**0.302**
W-First				
	-0.192	0.170	-0.035	0.127
	-0.044	0.103	0.004	0.081
	-0.051	0.327	-0.125	0.333
	-0.169	0.130	0.068	0.086
	-0.022	0.120	-0.011	0.034
	-0.086	0.064	-0.104	0.051
	0.054	0.039	0.057	0.032
	0.018	0.080	-0.032	0.124
	0.030	0.054	-0.029	0.154
	-0.262	0.333	-0.121	0.369
	-0.009	0.047	0.000	0.039
	0.010	0.132	0.126	0.160
Average	**-0.060**	**0.189**	**-0.016**	**0.170**
Grand Averages	**-0.040**	**0.188**	**-0.069**	**0.245**

Appendix O: PEQ Results

Appendix O: PEQ Results

Post-Experiment Questionnaire Results									
Question	Group	Yes	Maybe	No			Chi Sq.	df	Asymptotic significance (2-sided)
PEQ1	M-First	4	0	5			0.460	1	**0.498**
	W-First	6	0	4					
PEQ2	M-First	5	1	3			1.845	2	**0.398**
	W-First	8	0	2					
PEQ3	M-First	8	0	1			0.006	1	**0.937**
	W-First	9	0	1					
PEQ4	M-First	9	0	0			0.950	1	**0.330**
	W-First	9	0	1					
PEQ5	M-First	3	1	5			4.560	2	**0.102**
	W-First	7	2	1					
PEQ6	M-First	1	2	6			0.281	2	**0.869**
	W-First	2	2	6					
PEQ7	M-First	3	0	6			1.552	1	**0.213**
	W-First	1	0	9					
PEQ8	M-First	7	0	2			0.693	1	**0.405**
	W-First	6	0	4					
PEQ9	M-First	0	0	9			0.950	1	**0.330**
	W-First	0	1	9					
		intention	urge	impulse	other				
PEQ10	M-First	2	4	2	1		3.767	3	**0.288**
	W-First	0	3	4	3				