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Cue Awareness in Avoiding Effortful Control

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Abstract

Based on a recent metacognitive account, cognitive effort is the result of an inferential evaluation made over explicitly available cues. Following from this account, we present here a pre-registered experiment that tested the specific hypothesis that explicit awareness of cues that are aligned with cognitive demand is a prerequisite in avoiding effortful lines of action. We attempted to modulate levels of effort avoidance behavior by introducing an incentive (between-subjects) to monitor two lines of action that, unbeknownst to individuals, varied in the probability of a task switch. Importantly, previous research has demonstrated that the difference in these probabilities is relatively opaque to individuals. We did not find strong evidence for our incentive manipulation having an effect on demand avoidance as indexed by individuals' choices in a block of the task where avoiding effort was instructed. However, we do find that being aware of the task-switching cue appears to increase the likelihood of demand avoidance. We consider these results within the context of the metacognition of cognitive effort.

Keywords: cognitive effort, conscious awareness, metacognition, cognitive control, cue-utilization

Cue Awareness in Avoiding Effortful Control

Generally it is hypothesized that engaging the control systems required to override a habitual or automatic response is effortful, and the putative level of effort involved in such engagement weighs as a cost to-be-avoided in decision-making (e.g., Apps, Grima, Manohar, & Husain, 2015; Botvinick, 2007; Botvinick & Braver, 2015; Chong et al., 2017; Dunn, Inzlicht, & Risko, 2017; Inzlicht, Schmeichel, Macrae, 2013; Kool, McGuire, Rosen, & Botvinick, 2010; Kool, McGuire, Wang, & Botvinick, 2013; Kool & Botvinick, 2014; Kurzban, Duckworth, Kable, & Myers, 2013; Pessiglione & Delgado, 2015; Prévost et al., 2010; Shenhav, Botvinick, Cohen, 2013; Shenhav et al., 2017; Vassena, Holroyd, & Alexander, 2017; Verguts, Vassena, & Silvetti, 2015; Westbrook & Braver, 2015; 2016). Such control-linked behaviors can range in granularity from attempting to suppress inappropriate responses to specific stimuli (e.g., the flankers task, Eriksen & Eriksen, 1974; the Stroop task, see MacLeod, 1991 for a review), to switching between tasks (Monsell, 2003), to engaging in deliberate analytical thinking rather than automatic intuitive thinking (Pennycook, Fugelsang, & Koehler, 2015), to resisting temptations, regulating emotions, and reaching novel planned goals (see de Ridder et al., 2012 for a review). Here, we provide a pre-registered test of a recent account that proposes that awareness of cues associated with cognitive demand is a necessary condition in avoiding lines of action associated with effortful control.

The Relations Between Control, Awareness, and Effort

Recently, the relation between effort-based decision-making and conscious awareness has received attention. This relation entails specifically considering whether conscious awareness of a cognitive effort signal is a prerequisite to individuals generating behaviors that are in line with the notion of effort minimization (i.e., decisions or evaluations; Desender et al., 2017; Dunn et al. 2016; Mulert, Menzinger, Leicht, Pogarell, & Hegerl, 2005; Naccache et al., 2005; Shenhav et al., 2017; Westbrook & Braver, 2015). Two straightforward competing hypotheses can be derived from this consideration related to effort-based decision-making: (1) effort avoidance can proceed at the implicit level (i.e., without conscious awareness), or (2) effort avoidance can only occur when individuals become explicitly aware of attributes of a task that signal a difference in demand, or generate a feeling of conscious effort. Importantly, the former does not preclude the influence of explicit awareness in action selection; rather, both likely influence behaviors. In contrast, the latter *does* preclude the influence of effort avoidance proceeding as a wholly implicit process. Below, we briefly review the available evidence related to both hypotheses in relation to cognitive control broadly (for a more thorough review see Desender & Van den Bussche, 2012), and cognitive effort more specifically.

Cognitive Control in the Absence and Presence of Awareness

Engagement of the human controlled processing system has been long associated with increases in cognitive effort (Botvinick & Braver, 2015). Interestingly, evidence suggests that cognitive control can be engaged without conscious awareness. Much of this work has focused on conflict adaptation, which consists of preventing irrelevant information from exerting a detrimental influence on ongoing performance (e.g., the Gratton effect; Gratton, Coles, & Donchin, 1992). For instance, Diede and Bugg (2017) demonstrated response adaption effects, as indexed by response times and a difference in average pupil diameter (i.e., a common index of cognitive demand; see, Joshi, Li, Kalwani, & Gold, 2016) across varying conflict frequency conditions, despite the fact that individuals were unable to report any differences across conditions in conflict frequency (see also, Bugg & Diede, 2017 for an example utilizing a pre-cued list paradigm). Furthermore, control without awareness has been demonstrated during response inhibition, where individuals learn to withhold a response based on a specific cue. Van Gaal, Ridderinkhof, Scholte, and Lamme (2010) demonstrated that inhibitory responses will similarly slow down when the cue is presented unconsciously through masking. Though unconscious, processing of these cues was correlated with areas commonly involved in inhibitory control including the inferior frontal cortex (IFC) and the pre-supplementary motor area (pre-SMA).

Though control without awareness has been widely demonstrated, several groups have argued the contrary. Dehaene and Naccache (2001) suggest that without reaching consciousness, stimuli are unable to initiate general top-down control. Here, consciousness holds a special position in initiating planning, evaluation, and control. Following from this notion, Desender and colleagues (2014) demonstrated that conflict adaptation effects on masked trials were only triggered by a subjective experience of conflict, and this experience was dissociated from actual observed congruency effects (see also, Desender, van Lierde, & van den Bussche, 2013; Reuss, Desender, Kiesel, & Kunde, 2014; Questienne, Van Opstal, Van Dijck, & Gevers, 2016; c.f., Abrahamse & Braem, 2015). Similarly, in a setting where prime awareness was manipulated, Heinemann, Kunde, and Kiesel (2009; see also Schouppe, de Ferrerre, Van Opstal, Braem, & Notebaert, 2014) examined the role of awareness in generating the context-specific proportion congruent (CSPC) effect. The CSPC effect is the demonstration that congruency effects can vary depending on context-features that appear more or less simultaneously with the response stimuli (Crump, Gong, & Milliken, 2006). Heinemann and colleagues (2009) used both weakly and strongly masked irrelevant prime information (i.e., the distractor) in a Flanker task. The context manipulation did not produce the specific CSPC effects when the prime was strongly masked. It was thus concluded that conscious access to the incompatible prime was necessary for the effect to unfold. Thus, as noted by Desender and colleagues (2014), "…participants will adapt their behavior only if they have an experience of conflict" (p. 680).

Focusing on cognitive control, the evidence suggests that awareness can play a role in some contexts, but does not seem to be required to initiate controlled processes. This is of particular interest for the current investigation given controlled processes' relation to cognitive effort. Next, we turn to the available, albeit limited, evidence specifically focused on cognitive effort and awareness.

Cognitive Effort and Awareness

In their review of cognitive effort Westbrook and Braver (2015) noted that control costs (specifically opportunity costs) need not always be conscious to influence behavior. However, as discussed by Hagger (2013), the opportunity cost model of effort proposed by Kurzban et al. (2013) is arguably unclear on whether the processes associated with computing these costs work at an automatic-level of processing outside of conscious awareness. Hagger (2013) notes that utilizing "next-best alternatives" in computations may imply a need for conscious awareness of what those alternatives are. Nonetheless, there is some empirical evidence to support the claim that effort avoidance can unfold in the absence of awareness of effort cues. In their influential work on demand avoidance, Kool and colleagues (2010) utilized a demand selection task (DST) where individuals made free choices about which of two options they preferred to engage with on a trial-by-trial basis. Critically, and unbeknownst to participants, these options were manipulated to vary in the levels of cognitive demand (e.g., more switching vs. less switching). Interestingly, in the first of their experiments, Kool and colleagues (2010) demonstrated that eight of their 43 participants produced patterns of choices consistent with effort avoidance, even though they self-reported no awareness of the difference of the probability of a task switch across the two choice options. Furthermore, Botvinick (2007) notes additional demonstrations of behavior consistent with demand avoidance for subsets of individuals who lack explicit awareness of differences in cognitive demand.

The specific association between cue awareness and demand avoidance has recently been highlighted within a cue-based metacognitive account of cognitive effort offered by Dunn and colleagues (2016; see also Dunn & Risko, submitted). Following from cue-utilization accounts of metacognitive judgments (e.g., Koriat, 1997; Mueller, Dunlosky, & Tauber, 2016), they suggested that cognitive effort results from an inferential metacognitive evaluation made over explicitly available cues. Within the study of metacognition, a distinction is often made between two sources of evaluation: (1) inferential processes, and (2) sheer subjective feelings. Importantly, the former entails a more conscious process, and the phenomenological quality of the latter lies in a variety of subtle processes that occur below full consciousness (Kelley & Jacoby, 1996; Koriat, 2000; Koriat, Nussinson, Bless, & Shaked, 2008). Basic hierarchical metacognitive models posit a lack of direct-access between two cognitive systems. Specifically, evaluative meta-level processes which are associated with areas of the prefrontal cortex (PFC) imperfectly monitor information in the lower object-level of cognitive processing (e.g., task demand) which are associated with areas of the posterior cortex (Fleming & Dolan, 2012; Nelson & Narens 1990; Shimamura, 2000; 2008). Given the disconnect between these two systems, a cue-based metacognitive account posits that individuals monitor variation across lines of action by way of cues during action selection (e.g., demand avoidance). This is in contrast to having the ability to index and utilize some veridical signal associated with cognitive demand at the object-level (if at all possible). Following from this conception, individuals' judgments and behaviors related to effort would be expected to dissociate (but not always) from common measures of cognitive demand (e.g., performance, physiological measures). This finding has indeed been demonstrated across many contexts (Desender et al., 2017; Dunn et al., 2016; Dunn & Risko, 2016; Dunn & Risko, submitted; Kool et al., 2010; Gold et al., 2014; McGuire & Botvinick, 2010; Naccache et al., 2005; Westbrook, Kester, & Braver, 2013). Dunn and colleagues (2016) further suggested that an explicit awareness of cues may be required to avoid effortful lines of action given the decision is generated at the introspective meta-level of processing; a crucial aspect of consciousness (Cleeremans, Timmermans, & Pasquali, 2007; Lau, 2008). A recent set of experiments directly supports this hypothesis.

Desender and colleagues (2017) had individuals freely choose between performing a task in either high-demand or low-demand conditions. Demand was manipulated by the proportion of response conflict trials associated with each option. Critically, subliminal priming was used to ensure that participants were not aware of the visual stimuli (i.e., congruency and incongruency) creating the difference in demand across options. That is, the cues from which effort could have been inferred were putatively unavailable to individuals. Accordingly, across three experiments, only individuals who became aware of these differences (as indexed using signal detection theory) demonstrated choices consistent with demand avoidance. This finding held even in light of identical task performance across aware and unaware individuals. Moreover, a multiple regression confirmed that awareness was indeed the main factor driving demand avoidance. In line with the cue-based metacognitive account of effort (Dunn et al., 2016), the authors argued that actual differences in cognitive demand across options does not drive demand avoidance, but rather, it is the awareness of the cues related to demand that signal a difference across options that ultimately drives demand avoidance.

Present Investigation

The present investigation sought to extend the hypothesis that awareness of effort cues is a prerequisite to evaluating and avoiding effortful lines of action. We made use of a recent demonstration of individuals' relative lack of awareness of differences in specific task switching probabilities. Dunn and Risko (submitted) showed that demand avoidance across two options associated with a 70% and 30% probability of task-switch respectively was at chance levels, and self-reported awareness of the specific task switching cue (i.e., more vs. less switching) was relatively low. This finding is unsurprising given several findings of individuals being unaware of even large proportion manipulations (e.g., Crump, Gong, & Milliken, 2006; Risko & Stolz, 2010; Schmidt, Crump, Cheesman, & Besner, 2007). Here we looked to moderate levels of demand avoidance above chance by introducing an incentive to raise awareness of the switching cue without affecting performance in the DST (see Botvinick & Rosen, 2009; Desender, Van Opstal, & Van den Bussche, 2017; Dunn et al., 2016; Dunn & Risko, submitted; Gold et al., 2014; Kool et al., 2010; McGuire & Botvinick, 2010). Prior to outlining our specific predictions, a description of operational definitions is in line.

Presently, we tested the cue-based metacognitive account of cognitive effort described above (Dunn et al., 2016; Dunn & Risko, submitted). We use the term effort cue to refer to an attribute(s) of a task available to an agent on which an inference about demand could be made. Cognitive demand in the present context is associated with task-switching (i.e., continually reconfiguring processing resources to achieve some goal; Monsell, Yeung, & Azuma, 2000), where more switching relative to less switching can be argued to reflect higher demands on the control system (Kool et al., 2010). For example, more switching may signal the need for a more proactive level of control (Braver, 2012). Here, the critical cue is the difference in the probability of a task-switch on a trial-by-trial basis (i.e., 30% vs 70%; c.f., Gold et al., 2014; Kool et al., 2010; McGuire & Botvinick, 2010, that used a 10% vs. 90% manipulation) across tasks, where the presumption is that individuals hold the belief that more switching is more effortful than less switching (Kool et al., 2010). An evaluation of effort can be utilized whether situated in a task (e.g., disengaging from an action through a retrospective evaluation of effort) or not (e.g., avoiding an action outright through a prospective evaluation of effort; see also Westbrook & Braver, 2015). We are concerned with the latter for the current investigation. An evaluation of cognitive effort does not necessarily have to track with cognitive demand (i.e., here, the demand associated with task-switching), but can, if effort cues and the level of demand are aligned in the same direction (Dunn & Risko, 2015). For example, in the present context the task switching cue (i.e., 30% vs. 70%) is aligned in this positive direction with the level of demand. Nonetheless, from the above we posit that this specific difference in switching is generally opaque to individuals.

Last, we define awareness as the introspective awareness of effort cues while situated within a task. Here, we follow Block's (1995) distinction between phenomenal- and access-consciousness. Importantly, we mean the latter as pertaining to awareness. A perceptual state or experience is access-conscious, if its content (e.g., a cue) becomes available to the higher-level cognitive processes whereby it can be used to control reasoning and behavior. Similarly, Snodgrass, Bernat, and Shevrin (2004) propose that *reflective consciousness* operates as a higher-order metacognitive process which selects subsets of phenomenal experience for further evaluative processes. Information becomes accessible when it is sufficiently clear to produce reasonable confidence and deemed task-relevant. One key element of this form of consciousness is reportability (Block, 1995; Overgaard & Sandberg, 2012; Snodgrass, Kalaida, & Winer, 2009). Positioned within the cue-based metacognitive account of effort then, the cue or cues in question (here, the difference in the probability of task switching that are associated with cognitive demand) must reach this level of awareness for an individual to avoid the more demanding line of action.

In the current experiment individuals completed a variant of the DST consisting of three blocks based on the *choice/no-choice* paradigm (Siegler & Lemaire, 1997): the first two were "forced-choice" where individuals garnered experience with both of the options, the third block was "free-choice" where individuals were specifically instructed to generate a less-effortful preference for one the options. Individuals were instructed to base their selections specifically on identifying a least-effortful option given that individuals’ preferences in free-choice contexts can be considered to be extremely labile (Payne et al., 1993; Lichtenstein & Slovic, 2006). Therefore, this directed instruction can give a stronger indication of how each manipulation may affect individuals’ effort-based decisions (Dunn & Risko, submitted; Dunn et al., 2016). As noted above, demand was manipulated by the probability of a task-switch on a trial-by-trial basis. Here, we consider the high demand option to be associated with a 70% probability of a switch and the low demand option to be associated with a 30% probability of a switch. The notion that more switching is more demanding on the executive control system (relative to less switching) is in line with several extant theories of executive control as well as empirical work (e.g., (Botvinick & Braver, 2015; Botvinick & Rosen, 2009; Friedman et al., 2008; Gilbert & Burgess, 2008; Kool et al., 2010; McGuire & Botvinick, 2010; Miyake et al., 2000; Yeung & Monsell, 2003). In addition, we specifically indexed demand following previous work on switching by performance in the first two blocks, first by overall performance (i.e., response times and errors) across the two options (i.e., the demand effect), and second in terms of the difference between repeat and switch trials (i.e., switch costs). Following from previous work using task-switching in the DST (e.g., Botvinick & Rosen, 2009; Dunn & Risko, submitted; Kool et al., 2010), we expected overall response times (RTs) to be longer for the high-demand option and switch costs to be smaller in the high-demand option. We did not expect errors to vary across options given previous work using the same probabilities of switching (Dunn & Risko, submitted). Use of the forced-choice blocks allows for unbiased estimates (relative to the free-choice block) of the performance associated with each level of demand. Comparison of these estimates with individuals’ behaviors in the free-choice blocks allows for a clear indication of how performance costs associated with switching (Monsell, 2003) are related to effort-based decisions.

Previous work has demonstrated that performance-based incentives can enhance performance in task-switching contexts (Aarts et al., 2010) and predict greater neural activity in control related areas such as the PFC and the anterior cingulate cortex (ACC) (Westbrook & Braver, 2016). In an attempt to leave performance unaffected across the incentive groups, we used an incentive that was not performance-based (i.e., where individuals would try harder during task-switches in the incentive condition because better performance was rewarded). That is, our goal was to draw attention to the task related stimuli more generally by an explicitly presented incentive (e.g., Bijleveld, Custers, & Aarts, 2011). As noted above, conscious awareness (i.e., access-consciousness) selects subsets of information that becomes accessible when it is deemed task-relevant. Thus, our manipulation is aimed at "pushing" the switching cue into awareness without incentivizing differential investment of cognitive control (e.g., "try harder for more reward"). Specifically, the incentive was manipulated between-subjects and consisted of an instruction prior to the DST that correctly answering a question about the task would be rewarded with a $2 bonus. We specifically opted for a between-subjects manipulation of incentive, as the goal of the study is to assess potential differential levels of awareness of the switching cues. The benefits of utilizing between-subjects manipulations, as opposed to within-subject manipulations, to assess individuals' experiences of experimental attributes are well noted in the judgment and decision-making literature (for a review see Hsee & Zhang, 2010). Specifically, between-subjects designs arguably offer a more realistic view of individuals' experience and reasoning

(Kahneman & Frederick, 2002; Tversky & Kahneman, 1983). That is, within-subject designs can often inflate differences in measured experiences across options given information about experimental manipulations is (in theory) available (e.g., carry-over effects), and may thus show inconsistencies with an individual's actual experience relative to between-subjects designs (Hsee, Loewenstein, Blount, & Bazerman, 1999). As noted above, following from recent work (Dunn & Risko, submitted), we hypothesized that individuals in the no-incentive condition will demonstrate choices in the third free-choice block near chance (i.e., no demand avoidance). We further hypothesized that the incentive condition will increase awareness of the task-switching cue and thus drive rates of demand avoidance above chance levels relative to individuals that do not receive the incentive.

We index awareness by a post-DST no-loss gambling task. Gambling procedures have been used extensively in assessing individuals' subjective awareness of stimuli and their associated confidence (for a review see Overgaard & Sandberg, 2012). For example, Dienes and Seth (2010) demonstrated that no-loss gambling is similarly sensitive relative to verbal reports of confidence as a measure of awareness in an artificial grammar task. Important for the current study, no-loss gambling additionally controls for risk-aversion given the individuals cannot lose any reward (i.e., the $2 bonus offered presently), and provides a high motivation to respond in an unbiased manner given the direct contrast between staying (i.e., on their answer) or gambling. Furthermore, this procedure provides a clear indication of guessing through the choice of a 50/50 gamble in a non-idiosyncratic way that is lacking in many confidence scales: when a participant chooses to gamble, they are betting on a random process rather their own answer (Dienes & Seth, 2010; Schurger & Sher, 2008).

In the present no-loss gambling task, individuals were first asked whether one option switched more than the alternative. They then had the option to stick with their answer in hopes of receiving the $2 bonus if correct or accept a 50/50 gamble for the chance to win the $2 bonus (see below for more details). Thus, individuals made a judgment about their experience in the form of the switching question, and made a choice that reflects the strength of their experience (i.e., their confidence) in the form of gambling or not. With regard to the no-loss gambling task, we hypothesize that individuals in the incentive condition will overall be more accurate and confident in their answer to the switching question given the predicted heightened awareness of the cue relative to the no incentive condition. Furthermore, those individuals that accurately identify the high-switching option and are confident in their answer to the switching question should exhibit higher rates of demand avoidance relative to those individuals that do not become aware.

Method

In the following we report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study (Simmons, Nelson, & Simonsohn, 2012). The pre-registration protocol (https://osf.io/8d2rj/register/565fb3678c5e4a66b5582f67) and all data and code for the study is freely available via the Open Science Framework (https://osf.io/khtn3/).

Participants

Initial sample. Two hundred (171 females) University of Waterloo undergraduate students participated in the experiment in exchange for research credit through the psychology department's recruitment program. Optional stopping methods were utilized given that Bayes factors (BF) are the primary means of inference for the current study (Rouder, 2014; Schönbrodt, Wagenmakers, Zehetleitner, & Perugini, 2017). Initially, a starting sample size of $N = 40$ was selected ($n = 20$ for each between-subjects group), after which data collection would terminate if a BF across the incentive and non-incentive groups for low-demand selections (see below) demonstrated evidence for either the null (i.e., no difference in low-demand selections) or alternative hypothesis (i.e., some difference in low-demand selections) using a BF threshold value of greater than or equal to 10 (i.e., strong evidence; Jeffreys, 1961; Lee & Wagenmakers, 2013). If the BF computed for the difference between the low- and high-demand conditions did not reach this threshold, then an additional sample of 40 participants was collected. This was pre-determined to carry on until $N = 200$, at which point data collection would be terminated regardless of the level of evidence reached.

Exclusions and rationale. Following collection of the $N = 200$ sample (i.e., BFs never reached the above outlined criteria of 10; see Results section below), we required an additional 17 individuals due to an imbalance of two between-subjects order variables due to a program error. These variables were the left/right position of the high-demand deck and the presented order of high/low demand decks during the practice sessions (i.e., which demand condition was forced first). Replacement was done by identifying and removing the most recently recruited individuals whose data met the conditionoverbalance criteria. Additionally, one individual was identified as not completing the first two forced-choice blocks as instructed (i.e., not selecting the cued option), therefore their data were excluded from all analyses. Thus, our final sample size was $N = 199$.

Design

The present experiment utilized a 2 (Incentive: informed vs. uninformed) x 2 (Demand: high vs. low) mixed design. Incentive was manipulated between-subjects and demand was manipulated within-subjects.

Apparatus

The DST (Kool et al., 2010) was programmed in MATLAB using the Psychophysics toolbox (Brainard, 1997; Pelli, 1997; Kleiner et al, 2007)[1]. The program was delivered using a 24" LCD monitor with participants positioned approximately 70cm away. Participants utilized a wired standard optical mouse in order to respond.

Stimuli

Stimuli followed previous experiments utilizing the DST (see Dunn & Risko, submitted; Gold et al. 2014; Kool et al., 2010). The decks presented were either low- or high-demand and presented to the left and right of center on the screen. The locations of the high- and low-demand options were not communicated to participants. The low-demand option consisted of a 30% probability of a task-switch, whereas the high-demand option consisted of a 70% probability of a task switch. Once the cursor was placed on a target centered on the screen, both options were activated and participants were free to choose an option that would reveal a colored digit (blue or yellow). Digits consisted of "1", "4", "7", and "8". At this point, the participant was required to make a response. If the digit was blue, the participants were required to perform a magnitude judgment (greater than or less than 5). If the number was yellow, then participants were required to perform a parity judgment (odd or even). Participants clicked left on the mouse for a less than five or odd response, or right clicked for a greater than five or even response.

Procedure

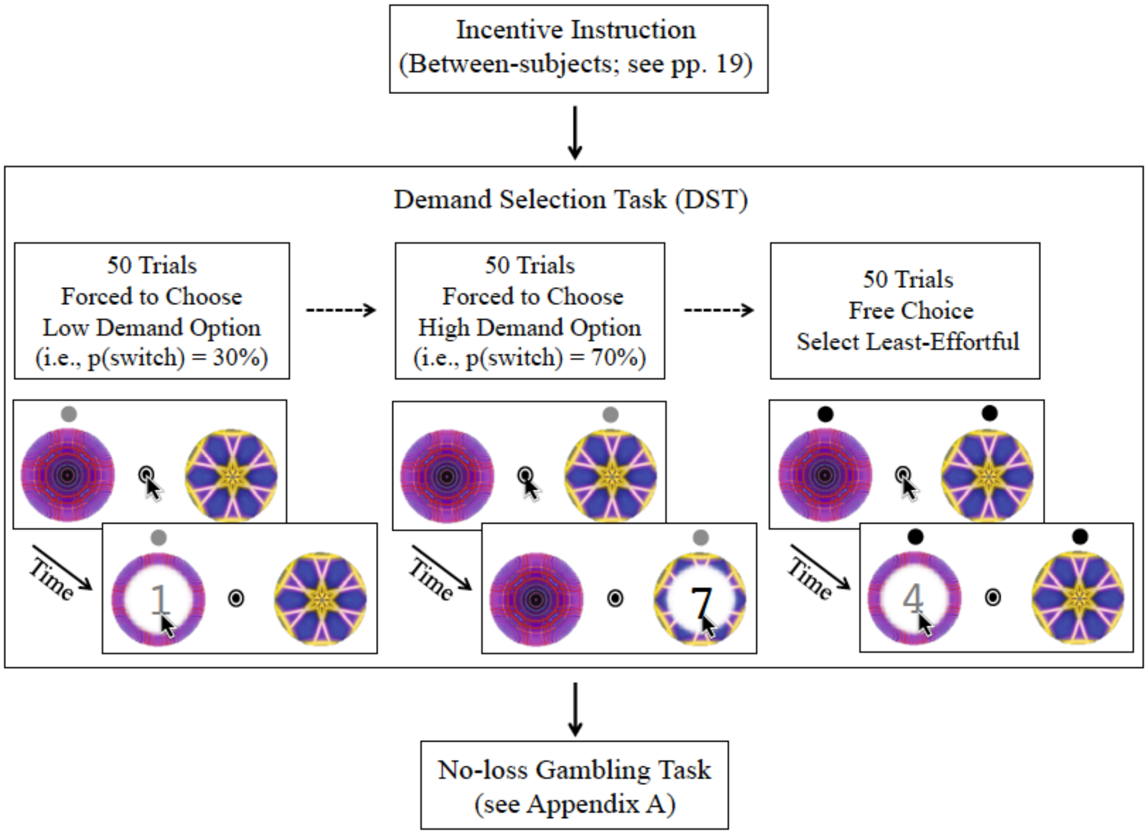
Individuals first completed a practice session with the DST to ensure understanding of the instructions and task procedure. The practice session consisted of three blocks of trials. In the first block of trials, participants were asked to respond to numbers contingent on the color the number was presented in. All participants were provided with a reference graphic of the correct response mappings during the first practice block. After the first block, participants' access to the reference was removed and they were informed that they would receive explicit feedback about their accuracy in the next practice block. Participants were instructed that they were required to achieve an overall accuracy of >= 90% (at least 54/60 trials correct) to proceed to the main experiment. If the accuracy threshold was not satisfied, individuals were allowed to retry the practice session. If they were unable to meet the threshold on the second try of their practice, the experiment was terminated due to time constraints. The third block of practice consisted of exposure and experience with the primary DST where participants were to select from one of two options, and subsequently respond to a number similarly to the main experiment.

Upon completion of the practice session, individuals were given instructions on how to complete the main experiment. The main DST consisted of three blocks of 50 trials each. Fifty trials for each block was chosen given recent work using the DST that demonstrated that overall individuals' choices asymptote at approximately 35 trials (Gold et al., 2014). In the first two blocks, participants were instructed to select the option cued by a grey colored circle above the patch (i.e., forced-choice). The cue would then switch after 50 trials to the opposing option to ensure equal experience with each deck[2]. To signify the final free-choice block, two blue colored circles appeared above both options (see Figure 1). The visual appearance and locations of the low- and high-demand demand options stayed the same throughout the DST. For the third free-choice block, participants were asked to begin selecting the deck they believed to be least effortful (Dunn & Risko, submitted; see similarly, Gold et al., 2014).

At the end of the task instructions, the incentive manipulation was delivered. Here, participants in the incentive group were informed that after the DST, providing a correct answer to a question about characteristics of the task would reward them with a $2 bonus in addition to earning a research credit. However, individuals in the no-incentive condition were not provided this instruction and therefore had no knowledge of this incentive prior to completing the DST. Characteristics of the DST such as the order of the forced-choice blocks pertaining to low- and high-demand and the location of each option were randomized and counterbalanced across all participants.

At the conclusion of the main DST, all participants completed a no-loss gambling task (see Appendix A. for full protocol). Again, the no-incentive group was unaware of this task prior to the main DST. First, participants were instructed that they were to choose from one of two facedown cards that were labeled either “Win” or “No Win”. Choosing the “Win” card would result in winning a $2 bonus whereas the “No Win” card would not. Unbeknownst to individuals, both cards were labeled “Win”. After selection, both cards remained facedown so that the participant would not know the outcome. Next, participants were asked to think back to the main DST, specifically whether the deck on the left or the right (counterbalanced) tended to switch between colors more often than the other deck (e.g., *"In the previous part of the task the option on the left tended to switch between colors more often than the option on the right?"*). Participants were informed that a correct answer to the question would lead to winning the $2 bonus, whereas an incorrect answer would not. Last, participants were then asked to make a choice between either the card they selected prior to their answer about the task or the validity of the answer they provided as to which outcome they would like to determine their winnings. Following this decision, all participants were provided with a $2 regardless of their answers. Individuals were not told whether their answers were in actuality correct or not to avoid participants sharing that their answer had no bearing on whether they received the bonus or not (please see Figure 1 for a general overview of the experimental procedure).

Figure 1. General Experimental Overview



Note: the above example is a case where a participant would be required to choose the low-demand option first followed by the high-demand option. The order of these forced-choice options was counterbalanced across participants in the experiment.

Results

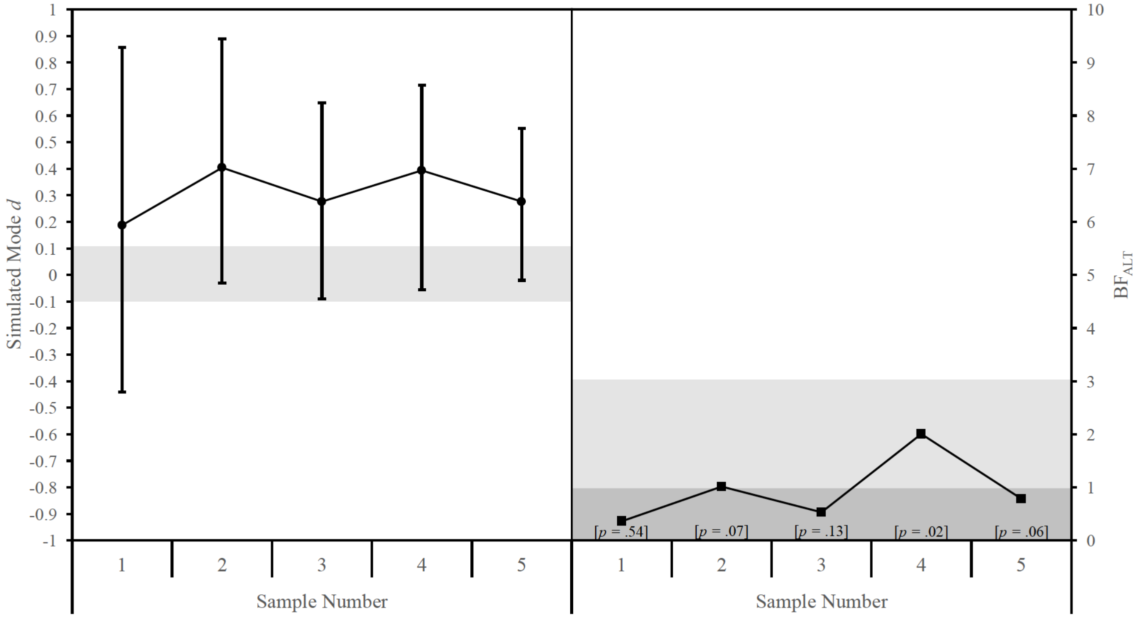
Results are reported first for the Bayesian optional stopping procedure, followed by less-effortful choices in the free-choice block, and no-loss gambling data. Last, we report overall block performance (i.e., response times and accuracy) and switch costs in terms of response time (RT) and accuracy all derived from the first two forced-choice blocks (see Table 1). Our primary means of analyzing the data is through Bayesian methods. For between- and within-group comparisons as well as ANOVAs, Bayes

Factors (BF) are the primary statistic of interest. Bayes Factors were computed using the BayesFactor package (Morey & Rouder, 2015) in R (R Core Team, 2014). For Bayesian ANOVAs, we focus on model comparisons between lower- and higher-level models using BFs (e.g., the interaction model versus the main effect model). Bayes Factors calculated for the ANOVA models in the BayesFactor package are presented as referenced to the random effect error model as the null model. We further compare high-level models when appropriate and is explicitly noted. A default prior of r = $\sqrt{2}/2$ (i.e., "Medium") was utilized for analyzing differences in means for the demand selection data. For ANOVA's and proportion analyses, a default fixed r scale = ½ was utilized. Grades of evidence categories for BFs follow the criteria outlined by Lee and Wagenmakers (2013; see also Jeffreys, 1961): 1-3 "*Anecdotal*", 3-10 "*Moderate*", 10-30 "*Strong*", 30-100 "*Very Strong*", > 100 "*Extreme*". To supplement BFs, Bayesian estimation analyses were conducted on focal effect sizes using the BEST package (Kruschke, 2014) in R. Ninety-five percent Highest Density Intervals (HDI), as well as the simulated mode effect size (i.e., the maximum *a posteriori* estimate; MAP) are presented. Effect sizes associated with within-subject comparisons are Cohen's *d* using SD_{avg} as the standardizer term (Cumming, 2012, p. 291) and generalized eta squared η_G^2 for more appropriate comparison to between-subject and within-subject effect sizes (see Bakeman, 2005). Frequentist statistics are also presented to facilitate interpretation of results where appropriate, though as noted above, our conclusions focus on the Bayesian analyses.

Bayesian Optional Stopping

Optional stopping focused on the effect of incentive versus no incentive on the overall proportion of less-effortful choices for the low-demand option in the third block of the DST (i.e., a positive effect size). As is apparent in the right panel of Figure 2, BFs never reached a magnitude to signal moderate evidence for an effect of the incentive manipulation on less-effortful choices, though at several sample numbers the effect would be considered significant or marginal at the $\alpha = .05$ level. Thus, data collection stopped at the $N = 200$ criteria as stated above. Considering Bayesian estimation of the effect size in the left panel of Figure 2, it is apparent, however, that the effect was in the correct direction (i.e., the incentive manipulation increased selections of the low-demand option) across all sample numbers. Next, we report the full results of the DST considering the entire sample collected.

Figure 2. Results from the Bayesian optional stopping procedure.



Note: each sample consisted of $n = 20$ per group and the analyses shown here aggregate samples at each step (i.e., sample number five contains the entire final sample of $N = 199$). The left panel displays Bayesian estimation analyses of the mode Cohen's *d* value for the focal effect. Error bars represent 95% Highest Density Intervals (HDI). The shaded band represents a Region of Practical Equivalence (ROPE) ranging from $d = -1$ to $d = 1$ (Kruschke, 2014). The right panel displays the BFs for the between-subject tests for each sample number. The unshaded area represents the range of BFs associated with the "*Moderate"* evidence category and above, the lighter band represents the "*Anecdotal"* category, and the darker band represents the area in which a BF would represent evidence for the null hypothesis (i.e., no difference across the groups; Lee & Wagenmakers, 2013). In addition, *p*-values for each between-subject *t*-test are presented in brackets.

Demand Selection Task

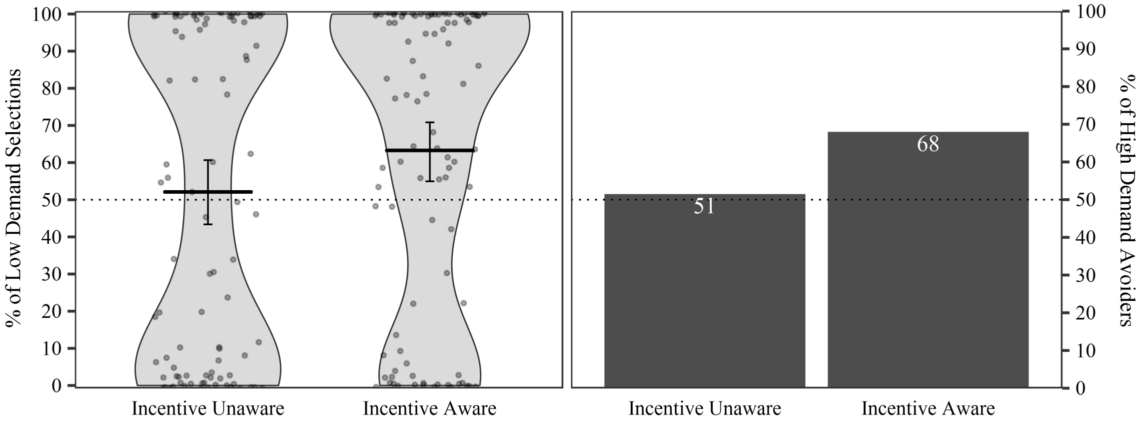
To determine whether participants' less-effortful selections (i.e., selecting the 30% probability of a switch option) in the third free-choice block differed as a function of incentive, a Bayesian *t*-test as well as a Welch's independent samples *t*-test (Welch, 1947) was conducted. Results demonstrated little evidence for the alternative hypothesis that the percentage of low-demand selections differed across the incentive conditions,

$BF_{ALT} = .78$, MAP $d = .27$, 95% HDI [-.01, .55], $t(195.18) = 1.87$, $p = .06$, $d = .26$ [3]. On average, those in the incentive condition selected the low-demand option 63% of the time ($SD = 40\%$), tested against chance, $BF_{ALT} = 16.56$, MAP $d = .35$, 95% HDI [.14, .56], $t(99) = 3.29$, $p = .001$. On average, individuals in the no incentive condition selected the low-demand option 52% of the time, tested against chance, $BF_{NULL} = 8.06$, MAP $d = .05$, 95% HDI [-.15, .26], $t(100) = .48$, $p > .1$, (see Figure 2).

Given known issues associated with averaging over individuals' choice patterns (Estes & Maddox, 2005; Liew, Howe, & Little, 2016), we additionally categorized participants into two groups: (1) demand-avoidant if they choose the low-demand option more than 50% of the time, and (2) not demand-avoidance if they choose the low-demand option less than (or equal to) 50% of the time. Sixty-eight percent ($n = 68$) of individuals were categorized as avoidant in the incentive group, whereas 52% ($n = 52$) of individuals were categorized as avoidant in the no-incentive group. Results demonstrated anecdotal evidence that knowledge of the incentive influenced demand avoidance categorization $BF_{ALT} = 2.82$, $\chi^2(1) = 4.96$, $p = .03$. Specifically, extreme evidence was demonstrated that the percentage of demand-avoidant individuals differed from chance in the incentive aware condition, $BF_{ALT} = 120.64$, MAP = .67, 95% HDI [.58, .75], $p < .001$ binomial test. Evidence for the null hypothesis was demonstrated for the incentive unaware condition, $BF_{NULL} = 4.0$, MAP = .52, 95% HDI [.43, .62], $p = .84$ binomial test (see Figure 3).

Overall the incentive condition did not differ in terms of avoidance relative to the no incentive condition, with the HDI suggesting only a small effect (i.e., that incentive increased avoidance) considered liberally (see the left panel of Figure 2). The same general pattern was demonstrated with respect to individuals being categorized as demand-avoidant. Again, only anecdotal evidence was reached when considering avoiders and non-avoiders across the incentive conditions. Nonetheless, the rate of avoidance and the percentage of avoiders in the incentive condition was greater than chance.

Figure 3. Percentage of less-effortful selections and percentage of "Demand Avoiders" for the incentive conditions.



Note: the left panel displays a violin plot of the mean percentage (black band) of low-demand selections (i.e., the 30% probability of a switch option) for the incentive condition and no incentive condition in the third free-choice block of the DST. Error bars represent 95% bias corrected and accelerated (BCa) intervals. The right panel displays the percentage of individuals categorized as being a Demand Avoider for the incentive (*n* =100) and no incentive conditions (*n* = 99). The white number within the bars denotes the count of Demand Avoiders for each condition.

No-loss Gambling Task

To determine whether correct identification of the high switching option (i.e., a 70% probability of a switch) varied as a function of incentive, a chi-squared BF analysis was computed for the 2 (Incentive condition) x 2 (Demand identification) count data. There was little evidence that identification of the high-switch deck varied across the incentive conditions, $BF_{NULL} = 2.78$, $\chi^2(1) = 1.22$, $p = .27$. Individuals in the incentive group correctly identified the high-switching deck 71% of the time, $BF_{ALT} > 1{,}000$, $p <$

.001 binomial test against chance, whereas the no-incentive group correctly identified the deck 62% of the time, $BF_{ALT} = 4.8$, $p = .02$ binomial test against chance. With respect to the decision to wager (i.e., taking the 50-50 gamble rather than sticking with one's answer to the switching question), a chi-squared BF analysis demonstrated that wagering rates did not differ across the conditions, $BF_{NULL} = 2.63$, $\chi^2(1) = 1.3$, $p = .25$. The incentive group gambled 39% of the time, $BF_{ALT} = 2.4$, $p = .04$ binomial test against chance, and 30% for the no-incentive group, $BF_{ALT} = 395.7$, $p < .001$ binomial test against chance. Thus, most individuals stayed with their given answer to which option switched more in the DST across the incentive conditions. Taken together, the incentive manipulation did not appear to differentially affect levels of awareness as indexed by the no-loss gambling task.

We further examined the relation between the decision to wager and accuracy at identifying the high-switching deck, a 2 x 2 contingency table was analyzed crossing individuals' decision to gamble with their accuracy at identifying the high demand deck. The four cells generated from this table were: (1) Gamble | Incorrect, (2) Gamble | Correct, (3) No Gamble | Incorrect, and (4) No Gamble | Correct (see Table 1). There was no evidence demonstrated that accuracy in identifying the high switching option varied a function of deciding to gamble or not, $BF_{NULL} = 1.82$, $\chi^2(1) = 1.31$, $p = .25$.

Table 1. Observed cell frequencies of individuals and sample sizes resulting from the crossing of gambling decision and identification of the high demand option.

		Gambling Decision	
		Gamble	Stay
Accurate at identifying the high demand option?	Incorrect	14% n = 27	20% n = 39
	Correct	21% n = 42	46% n = 91

Performance

Block Performance (Forced-choice). Error trials were removed for all RT analyses. In addition, an outlier analysis was performed to exclude trial-level response times based on a 2.5 standard deviation cut-off (Van Selst & Jolicoeur, 1994). Using this criterion, 2.7% of all trials were removed.

With respect to block RT, a 2 (Incentive) x 2 (Demand) mixed BF ANOVA was performed. First in reference to the error-only model, results demonstrated no evidence of a main effect of Incentive, $BF_{NULL} = 3.3$, and extreme evidence for both a main effect of Demand and the interaction between Incentive and Demand ($BF_{ALT} > 1{,}000$ for both). We then compared the Demand main effect model to the Demand-by-Incentive interaction model. This comparison produced a BF of 19.91 in support of the Demand main effect model best fitting the block RT data relative to the interaction model. Correspondingly using NHST, the Demand main effect was significant, $F(1, 197) = 84.4$, $p < .001$, whereas the Incentive main effect, $F(1, 197) = .71, p = .4$, $\eta_G^2 = .003$, and the Demand-by-Incentive interaction were not, $F(1, 197) = .002, p = .97$, $\eta_G^2 < .0001$. Thus, RTs varied as a function of high- (70% probability of switching) and low-demand (30%

probability of switching) with RTs being overall slower in the high-demand condition, and were not further modulated by incentive as hypothesized.

With respect to block accuracy, a 2 (Incentive) x 2 (Demand) mixed BF ANOVA was performed. Anecdotal evidence was demonstrated in support of a main effect of Incentive, $BF_{ALT} = 2.46$, a main effect of Demand, $BF_{ALT} = 1.06$, and an interaction between Incentive and Demand, $BF_{ALT} = 1.12$, relative to the error only model. Considering NHST, the main effect of Incentive just reached significance, $F(1, 197) = 5.4$, $p = .02$, $\eta^2_G = .02$, as did the main effect of Demand, $F(1, 197) = 4.76$, $p = .03$, $\eta^2_G = .003$. The interaction between Incentive and Demand did not reach significance, $F(1, 197) = .76$, $p = .38$, $\eta^2_G = .0006$. Thus, there is little evidence (in terms of BFs) that accuracy varied as a function of Incentive or Demand. Though, NHST demonstrated a relatively small effect of incentive where accuracy was slightly better in the incentive condition. Furthermore, there was as small effect of demand in accuracy where accuracy was better in the low demand condition.

When considering overall performance in the first two forced-choice blocks, then, we find that RTs were slower in the high-demand condition where a 70% probability of a task-switch was faced relative to the low-demand option where a 30% probability of a task-switch was faced. Accuracy at the two tasks only slightly differed across the high- and low-demand conditions and incentive conditions. In addition, RTs did not vary as a function of incentive. Next, we consider switch costs as an additional index of controlled processing.

Switch Costs. A 2 (Incentive) x 2 (Demand) x 2 (Switch/Repeat Trials) mixed BF ANOVA was performed on RTs and accuracy for switch trials in the forced-choice blocks. First considering evidence relative to the error model, there was extreme evidence (i.e., $BF_{ALT} > 1{,}000$) for a main effect of Switch/Repeat Trials in RT, the two-way interaction of Switch/Repeat Trials and Demand, the two-way interaction between Incentive and Switch/Repeat Trials, and the three-way interaction between Incentive, Demand, and Switch/Repeat Trials. To determine the best fitting model to the data considering the four models above, BFs were sorted by magnitude and tested against one another. Overall, the interaction between Switch and Demand had the highest overall BF (1.17×10^{26}) and yielded BFs between 3.7 and 1854.6 against the competing higher-level models. Compared against the main effects of Switch and Demand, the interaction yielded a BF of 1.06×10^{21} and 9.27×10^{8} (respectively). Furthermore, NHST demonstrated a significant main effect of Switch/Repeat Trials, $F(1, 197) = 95.75$, $p < .001$, $\eta_G^2 = .05$, a two-way interaction between Switching and Demand, $F(1, 197) = 18.84$, $p < .001$, a non-significant two-way interaction between Incentive and Switch, $F(1, 197) = .009$, $p = .92$, $\eta_G^2 < .0001$, and a three-way interaction between Incentive, Demand, and Switch/Repeat Trials, $F(1, 197) = .15$, $p = .7$, $\eta_G^2 < .0001$. Therefore, we can conclude with observed moderate to extreme evidence that the best fitting model of the RT switch cost data is the interaction between Switching and Demand confirming our prediction above. Specifically, switch costs were larger in the low-demand condition relative to the high-demand condition.

In terms of accuracy, there was evidence favoring the null hypothesis (i.e., the higher-level model did not differ from the random effect error model) for a main effect of Switch, $BF_{NULL} = 6.25$, the two-way interaction between Incentive and Switch, $BF_{NULL} = 3.7$, the two-way interaction between Switch and Demand, $BF_{NULL} > 100$, and the three-way interaction between Incentive, Switch and Demand, $BF_{NULL} > 1,000$. Thus, all BFs favored the null error model with at least positive evidence demonstrating that accuracy did not vary at any level as a function of switch/repeat trials. NHST confirmed this pattern, with all models being non-significant, all *F's* < 1.68*,* all *p's* > .2.

When considering switching and demand in the first two forced-choice blocks, we find that switch costs in terms of RTs were smaller in the high-demand condition relative to the low-demand option, replicating the same pattern demonstrated elsewhere (Botvinick & Rosen, 2009; Dunn & Risko, submitted; Kool et al., 2010). We return to this pattern of RTs in the General Discussion. Again, accuracy did not vary as a function of demand and switch/repeat trials, and both RTs and accuracy did not vary as a function of incentive.

Table 2. Individuals' Low-Demand Selections and Descriptive Statistics for Performance in the Demand Selection Task

	Incentive		No Incentive	
	Low Demand Option	High Demand Option	Low Demand Option	High Demand Option
Block RT(ms)	1195(261)	1341(319)	1161 (276)	1299 (314)
Repeat Trials	1141 (266)	1287 (332)	1109 (280)	1232 (333)
Switch Trials	1326 (328)	1369 (344)	1284 (336)	1339 (342)
Switch Costs	185 (253)	82 (244)	176 (259)	107 (246)
Accuracy	98.30% (12.93%)	97.48% (15.67%)	96.59% (18.16%)	96.24% (19.02%)
Repeat Trials	98.44% (12.38%)	97.46% (15.74%)	96.70% (17.88%)	96.63% (18.05%)
Switch Trials	97.98% (14.09%)	97.49% (15.65%)	96.35% (18.76%)	96.07% (19.44%)
Switch Cost	.46% (4.73%)	.03% (5.09%)	.35% (5.78%)	.56% (6.02%)

Note: The low-demand option consisted of a 30% probability of a task switch. The high-demand option consisted of a 70% probability of a task switch. Selections are presented for the low-demand option from the free-choice (3rd) block. Performance is presented for the forced-choice (1st and 2nd) blocks. Standard deviations are in parentheses.

Non-preregistered Analyses

We note here that all of the following analyses were not preregistered.

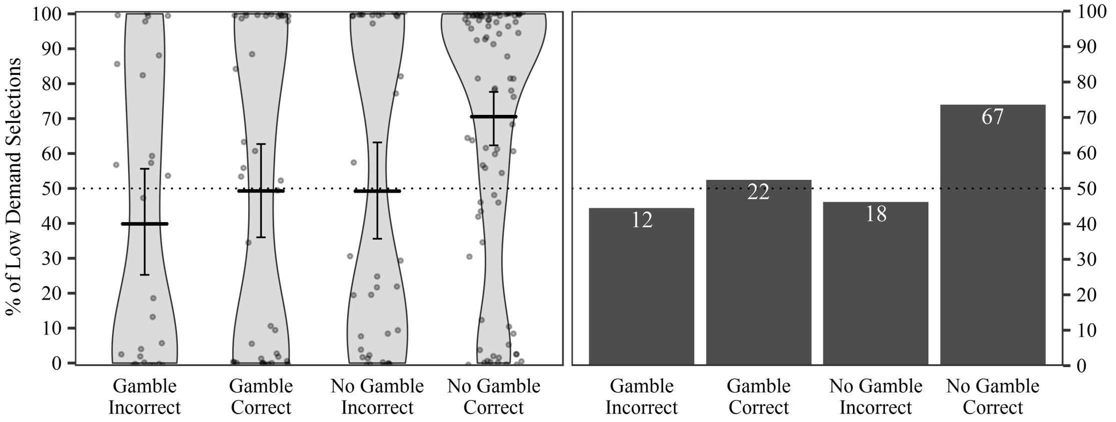
Demand Avoidance as a function of Awareness. In the following, we ignore the incentive/no-incentive variable given the lack of effects reported above, and focus on the association between correctly identifying the high-switching option, wagering, and avoiding demand. Individuals low-demand deck selections in the third free-choice block were analyzed using a one-way (Gambling and Accuracy Categorization: Gamble | Incorrect, Gamble | Correct, No Gamble | Incorrect, and No Gamble | Correct) between subject ANOVA. This analysis yielded very strong evidence for an effect of gambling and accuracy categorization, $BF_{ALT} = 57.57$, $F(3, 195) = 5.83$, $p \leq .001$, $\eta_G^2 = .08$. As apparent in Figure 4, individuals in the No Gamble | Correct category demonstrated significantly higher rates of demand avoidance relative to the three other categories, $\mathrm{BF}_{\mathrm{ALT}} \geq 6.60$, $p \leq .01$ for all comparisons. No other comparisons across the categories demonstrated positive evidence for the alternative, $\mathrm{BF}_{\mathrm{ALT}} \leq .35$, $p \geq .37$ for all comparisons.

Similar to above, we next assessed whether gambling and correctly identifying the high-switching option was independent of being categorized as demand avoidant via a 4 x 2 contingency table. Participants demonstrated strong evidence that their accuracy and gambling decision were not independent of one another, $\mathrm{BF}_{\mathrm{ALT}} = 28.53$, $\chi^2(3) = 13.87$, $p < .005$. Coinciding with the results for low-demand selections, there were more demand avoidant individuals in the No Gamble | Correct category relative to the other three categories, $\mathrm{BF}_{\mathrm{ALT}} \geq 4.83$, $p \leq .01$ for all comparisons. No other comparisons across the categories demonstrated positive evidence for the alternative, $\mathrm{BF}_{\mathrm{ALT}} \leq .44$, $p \geq .69$ for all comparisons (see Figure 4).

Individuals that correctly identified the high-switching option and did not gamble on their judgment (i.e., the No Gamble | Correct) category demonstrated higher rates of demand avoidance. This category can be considered (relatively) the “most” aware of the four categories. The other three categories yielded rates of demand avoidance at chance whether indexed by percentage of low-demand selections or percentage of demand avoiders. Specifically, individuals that did not correctly identify the high switching deck though were confident in their choice (i.e., the No Gamble | Incorrect category) overall demonstrated avoidance near chance. Similarly, both categories that can be considered to have guessed on the state of their experience in the DST (i.e., Gamble | Incorrect, Gamble

| Correct) overall demonstrated avoidance near chance. Thus, becoming aware of the correct difference between the options in terms of switching and having high confidence in that experience appeared to work conjunctively in driving effort avoidance above chance levels.

Figure 4. Percentage of less-effortful selections and percentage of “Demand Avoiders” for gambling behavior and high-demand identification accuracy categorization.



Note: the left panel displays a violin plot of the mean percentage (black band) of low-demand selections (i.e., the 30% probability of a switch option) for the four cells based on the crossing of individuals gambling decision and accuracy at identifying the high-demand option. Error bars represent 95% bias corrected and accelerated (BCa) intervals. The right panel displays the percentage of individuals categorized as being a Demand Avoider for these four conditions. The white number within the bars denotes the count of High Demand Avoiders for each condition (see Table 1 for cell *n*’s).

Alternative Categorization of High Demand Avoiders. The analyses of demand avoidance using the above dichotomy (i.e., high-demand avoidant vs. not avoidant) is arguably too liberal of a criterion for classifying individuals based on their less-effortful choices. It is relatively clear examining Figures 2 and 3 that a number of participants selected the high-demand option only near 50% of the time. Despite their choices not meaningfully varying from chance, these individuals were classified above in a manner that potentially does not adequately describe their behavior (i.e., as a high-demand avoider). Here, we additionally analyze how avoidance categorization differed following inclusion of a tertiary level of demand avoidance: high-demand indifference. We revised our original parameters to classify individuals as high-demand indifferent if they chose the low-demand option between 36 and 64% of the time. These percentages were chosen as they represent the largest non-significant deviation from chance in a two-tailed exact binomial test with 50 trials (see Table 3).

First, for the incentive manipulation, the proportion of high demand avoidant participants decreased in both conditions. For the incentive group, the proportion of high-demand avoiders decreased from 68% to 55%, while the proportion of high-demand avoiders decreased in the no incentive group from 52% to 45%. Anecdotal evidence was demonstrated that these values were similar, $BF_{NULL} = 2.32$, $\chi^2(1) = 1.45, p = .23$. This result does somewhat differ from the original finding of anecdotal evidence that knowledge of the incentive influenced demand avoidance dichotomous categorization, $BF_{ALT} = 2.82$, $\chi^2(1) = 4.96, p = .03$.

Next, considering the gambling and demand identification categories, the percentage of high demand avoiders reduced for every category similarly to the incentive conditions above. Critically, however, positive evidence was demonstrated that more individuals were categorized as avoiders for the No Gamble | Correct category relative to the No Gamble | Incorrect category, $BF_{ALT} = 5.12$, $\chi^2(1) = 5.41, p = .02$, and Gamble | Incorrect category, $BF_{ALT} = 33.52$, $\chi^2(1) = 8.49, p = .004$. Only anecdotal evidence was demonstrated for a difference between the No Gamble | Correct and Gamble | Correct categories, $BF_{ALT} = 2.16$, $\chi^2(1) = 3.75, p = .052$. Overall, these results closely match the results using the dichotomous categorization of high demand avoiders above. That is, the "most" aware of the four categories was associated with higher levels of effort avoidance even when using this more stringent criterion.

Table 3. Trichotomous categorization of avoidance behavior as a function of incentive (upper panel) and gambling and demand identification categories (lower panel).

	No Incentive	Incentive
High Demand Avoidant	45.5% *n* = 45	55% *n* = 55
High Demand Indifferent	45.5% *n* = 45	28% *n* = 28
High Demand Seeking	9% *n* = 9	17% *n* = 17

	Gamble Incorrect	Gamble Correct	No Gamble Incorrect	No Gamble Correct
High Demand Avoidant	30% *n* = 8	44% *n* = 17	41% *n* = 17	64% *n* = 58
High Demand Indifferent	52% *n* = 14	54% *n* = 21	45% *n* = 19	21% *n* = 19
High Demand Seeking	18% *n* = 5	2% *n* = 1	14% *n* = 6	15% *n* = 14

Performance as a function of Awareness. A straightforward prediction following from the above results is that individuals that become aware of differences in demand produce different levels of performance when engaging the different options. As an example, individuals that become aware may show differential effects in response to the increase in demand in terms of control, such as larger preparation effects to handle frequent task-switching (Monsell, 2003). This modulation of control may then lead to an increase in awareness of the demand difference across options. To examine this, we conducted additional analyses on performance using gambling and accuracy categorization as a factor. Following the above experiment, all performance estimates here are taken from the first two blocks of the DST.

A 4 (Gambling and Accuracy Categorization) x 2 (Demand) mixed-design ANOVA was used to assess whether individuals' demand effect (i.e., the difference between the high- and low-demand options) varied as a function of level of awareness. A direct comparison of the interaction model against the error model produced extreme evidence for the alternative, $BF_{ALT} > 1{,}000$. Furthermore, a comparison of the interaction model against the simple main effect model of Demand demonstrated extreme evidence that the main effect model better described the data, $BF_{ALT} = 137.5$. This demonstrates that there were not differential demand effects across the four categories. NHST additionally confirmed a non-significant interaction, $F(3, 195) = 1.62, p = .19, \eta_G^2 = .004$.

Furthermore, performing the same analysis adding Switch/Repeat Trials as a factor demonstrated that the interaction model, though showing extreme evidence for the alternative relative to the error-only mode, $BF_{ALT} > 1{,}000$, was outperformed by 11 of the other models (out of 17). The model of a Demand and Switch/Repeat main effect demonstrated extreme evidence in better describing the data, $BF_{ALT} = 1.3 \times 10^9$. Furthermore, this dual main effect model demonstrated strong evidence in outperforming the three main effect model (inclusion of gambling and accuracy categorization), $BF_{ALT} = 19.88$. Again, NHST confirmed this with a non-significant three-way interaction $F(3, 195) = .86, p = .46, \eta_G^2 = .0007$. Thus, following the above demand effect analysis, there were not differential switch costs across the four categorizations (see Table 4).

Table 4. Mean response times for gambling behavior and high-demand identification accuracy categorization.

	Gamble | Incorrect	Gamble | Correct	No Gamble | Incorrect	No Gamble | Correct
Low-Demand				
Block RT (ms)	1459 (1258)	1252 (1020)	1260 (952)	1246 (990)
Repeat Trials	1404 (1991)	1170 (943)	1189 (889)	1179 (947)
Switch Trials	1567 (1376)	1414 (1143)	1416 (1060)	1406 (1072)
Switch Costs	163 (281)	244 (212)	227 (231)	227 (272)
High-Demand				
Block RT (ms)	1648 (1648)	1391 (1148)	1355 (980)	1480 (1206)
Repeat Trials	1561 (1491)	1304 (917)	1294 (950)	1369 (1148)
Switch Trials	1689 (1690)	1431 (1238)	1382 (992)	1533 (1229)
Switch Costs	128 (318)	127 (179)	88 (207)	164 (260)

Note: standard deviations are presented in parentheses.

General Discussion

The present investigation sought to extend the hypothesis, under a cue-based metacognitive account of cognitive effort, that awareness of effort cues is a prerequisite to avoiding effortful lines of action. We attempted to moderate levels of avoidance in a DST where effort-based decisions were instructed by introducing an incentive that was focused on having individuals monitor the task. First, we did not find differences in performance across the incentive conditions. Though individuals in the incentive condition were categorized as being demand avoidant above chance-level, we did not observe statistical differences across the incentive conditions in either rates of demand avoidance or percentage of individuals categorized as avoiders. Nonetheless, when considering awareness through the no-loss gambling task, it was fairly clear that individuals who correctly identified the high-switching option and did not gamble (i.e., the relatively highest level of awareness of the switching cue), demonstrated demand avoidance above chance. In the following we consider the current results within a cue-based metacognitive account of cognitive effort and consider future directions for the study of cognitive effort.

Cue-Based Metacognition of Cognitive Effort

The cue-based metacognitive account of cognitive effort posits that individuals monitor for variations across lines of action by way of cues, and utilize these cues to infer demand in a qualitative manner when making effort-based decisions (i.e., avoiding effort). One distinct prediction made from this account is that, given cognitive effort is the result of a meta-level evaluation, individuals may need to be consciously aware of cues to make these types of decisions. That is, effort cues must reach a particular level of conscious awareness (e.g., Block, 1995) in order to factor into an evaluation of effort. Some initial evidence supports this hypothesis (Desender et al., 2017), and the current experiment overall adds additional credence to the claim.

Our manipulation, which attempted to moderate levels of avoidance, was relatively unsuccessful at pushing rates of avoidance above those in the no incentive condition. Nonetheless, individuals in the incentive condition overall demonstrated avoidance above chance, whereas this was not the case for those in the no incentive condition. The resulting null effect (or small effect at best), was likely due to the incentive manipulation being purposely broad to attempt to leave performance unaffected across conditions. Though, we were successful at keeping performance similar across the conditions.

Outside of the incentive manipulation we did find clear evidence that individuals who noticed a difference in switching across the options and were confident in that experience (i.e., 46% of the current sample in the No Gamble | Correct category) overall demonstrated rates of demand avoidance above chance. This was not the case for the other three categories, suggesting that awareness indeed plays an important role in effort avoidance. This was additionally confirmed in a more conservative analysis of avoidance using three categories of behavior. Interestingly, when considering rates of avoidance for the highly aware individuals in the present study, we demonstrate similar rates relative to previous work using a larger difference in switching (i.e., 90% vs. 10%; Kool et al., 2010; McGuire & Botvinick, 2010). For at least one previous instantiation of the DST (i.e., Kool et al, 2010, Experiment 1) awareness was assessed and indeed most individuals were aware of the switching cue. Thus, it is difficult to determine whether it was a veridical signal of demand driving avoidance, or an awareness of the large difference in switching (though these are often aligned in experiments; Dunn & Risko, 2015). Based on our present results, the latter appears to be the more parsimonious explanation. Correspondingly, Dunn and Risko (submitted) recently demonstrated an increase in self-reported awareness as the difference in switching across options became larger, and that cues that are more salient but less cognitively demanding are avoided at a higher rate.

Moreover, indices of control (i.e., performance) were similar across all of the categories. The interaction between switch/repeat trials and low/high demand (i.e., switch costs were smaller in the high-demand option relative to the low-demand option) signifies that control processes related to task-switching were deployed. Within the context of a dual mechanism framework of control (Braver, 2012), smaller switch costs in the high-switching context can be considered to indicate the use of a more proactive mode of control (e.g., maintaining both tasks-sets in a partially activated state) when switching occurs frequently. Importantly, this occurred regardless of awareness of the difference in switching across the options (i.e., a 70% probability versus a 30% probability). Therefore, differential control for nearly half of the current sample proceeded without awareness of the difference in the specific control-related cue. This coincides, with work outlined in the introduction that control can be deployed without awareness. Awareness, however, appears to be required for the *avoidance* of the demand related to the engagement of that control.

Though the overall findings of the current study are relatively clear, several shortcomings can be addressed in future work. First, it is important that we curb any strong causal claims related to cue awareness and avoidance as it pertains to the current experiment. It is possible that individuals' selections drove answers and betting behaviors in the no-loss gambling task. That is, rather than actually becoming aware of the switching cues in the DST, individuals simply relied on their choice behavior to make their answers and gambling decision. The no-loss gambling task as a measure of awareness here cannot delineate between these accounts given the delivery was post-DST. As discussed below, conjunctive measures of awareness can further aid in making stronger causal claims. Second, our incentive manipulation may have been too general for individuals, and attention may have been paid to different aspects of the task other than the frequency of switching (e.g., the colors of the response items; location of the options; Kool et al, 2010). This latter consideration may go some way in explaining observed preferences for the high-demand rather than the low-demand option (see Figures 2 and 3). Kool and colleagues (2010) were successful at addressing this issue by varying the location of the options across several blocks. Third, we utilized only one block of free-choice after the forced-choice experience phase of the DST. Using multiple blocks in a within-subjects design may provide further important information on how awareness is affected by levels of demand and experience by varying the proportion of switching across several blocks. In this vein, Dunn and Risko (submitted) demonstrated that self-reported awareness of the difference in task switching and avoidance increases as the difference between two options becomes larger. Though in this case, differential demand requirements (as indexed through performance measures) could not be ruled out as the main determinant of increased awareness (c.f., the present results where presumed control was similar across awareness categories). Furthermore, utilizing multiple blocks of the DST may give a clearer picture of individuals gambling behaviors with respect to awareness and demand. It has been argued that choice behaviors are under the influence of stochastic processes (Becker, DeGroot, & Marshack, 1963), and thus, a single delivery of the no-loss gambling protocol may be insensitive to individuals' actual propensity to gamble on their answers. For example, several groups have used discounting paradigms to examine the reward-effort relation across many trials and blocks (e.g., Białaszek, Marcowski, & Ostaszewski, 2017; Chong et al., 2017; Westbrook et al., 2013).

Following from the current results, key questions then to consider are how individuals become aware of cues when situated in a task (e.g., Dehaene & Changeux, 2011) and utilize cues when making effort-based decisions. Though differing in specification, extant accounts of conscious access posit that awareness proceeds in a serial manner and is the result of an assembly of information that can be shared across a "workspace" (for a review see Dehaene & Changeux, 2011). For example, the dynamic core hypothesis (Tononi & Edelman, 1998) states that information only becomes accessible once encoded information achieves differentiation (i.e., the isolation of specific content out of much other potential content) and integration (i.e., the formation of a coherent representation). Within the Global Neuronal Workspace (GNW) model (Dehaene, Sergent, & Changeux, 2003; Dehaene & Naccache, 2001), stimulus-relevant information supported by feedback and feed-forward connections is "ignited" into a global state of consciousness through the mutual reinforcement of neuronal activity. At these points of conscious awareness, representations (e.g., cues) can then be selected for further metacognitive processes (Snodgrass et al., 2004) largely supported by various sub-regions of the PFC (e.g., the rostrolateral PFC, Fleck et al., 2006; Medalla & Barbas, 2010; ventral medial PFC, Hogan, Galaro, & Chib, 2017; Schnyer, Nicholls, & Verfaellie, 2005; lateral PFC, Mcguire & Botvinick, 2010; left dorsolateral PFC, Lau & Passingham, 2006). Under the framework elaborated here, then, the information tied to effort cues must proceed through this process to be evaluated and (potentially) be deemed effortful and avoided. That is, without the availability and awareness of cues, an action will not be evaluated as being effortful.

Conscious access is associated with the need to accumulate evidence and some form of criterion where information produces reasonable confidence (or reliability; Michaelian, 2009; 2012; Yeung & Summerfield, 2012), and is deemed task-relevant (Snodgrass et al., 2004). Applied to the present context, individuals in the No Gamble | Correct category may have accumulated evidence related to the frequency of task switching in each option, and importantly the qualitative difference in switching across the options at different rates. Considering the temporal dynamics of awareness and avoidance represents an intriguing future direction. For example, specific ERP components related to conscious awareness, such as the P3 (Del Cul, Baillet, & Dehaene, 2007; Klein et al., 2007), can be examined in multiple-block DST paradigms to gauge when individuals become aware of relatively opaque cues (e.g., through masking or proportion manipulations), and when a differential evaluation of cognitive effort is reached. Modeling techniques such as diffusion models (Ratcliff, 1978; Ratcliff & McKoon, 2008) may prove to be especially useful in disentangling the issue of information accumulation and criterion points. Importantly, such methods have already been applied within the domain of metacognitive judgments. Applying diffusion models to confidence judgments and error-monitoring, Yeung and Summerfield (2012) argued that such methods best represent metacognitive judgments in temporally extended tasks. Specifically, diffusion models can specifically model how evidence strength and reliability are encoded in parallel during evidence accumulation leading to a decision (e.g., which option is more effortful). Or individual differences may exist in decision criterion setting for determining whether a cue is reliable enough to signal a difference in effort (e.g., individuals' β values in Signal Detection Theory, Long & Shelnutt, 1973).

The present results emphasize several important directions for future research in effort-based decision-making, especially when considering cue awareness as a determinant of action selection. Aggregate measures of awareness would be especially useful, including behavioral, physiological, and anatomical indices (Seth, Dienes, Cleeremans, Overgaard, & Pessoa, 2008), to determine what leads individuals to become aware of cues, utilize those cues, and evaluate effort.

Cue-based Inference of Effort Contrasted to Sheer Subjective Feelings of Effort

There is the important case of the subsets of individuals that demonstrated demand avoidance in the three categories that can be considered to be less aware than the

No Gamble | Correct category (c.f., Figure 3). Straightforwardly, given these individuals were putatively unaware (to some degree) of the switching cue, and simply chose an option at random in the free-choice block. This then may have led to choices occurring around chance for the three categories. Alternatively, it may possible that, although overall choices were at chance, individuals became *partially* aware of some aspect of demand, though this information was unreportable (or was reported incorrectly) in the gambling task. That is, some information may have been experienced at the phenomenal level, but did not meet the conditions (e.g., high evidence, see above) to move to the higher-level access consciousness where evaluations could then be made (Block, 1995).

As noted in the introduction, research on metacognition emphasizes the difference between contributions of processes that operate within full consciousness (i.e., cue-utilization) and below full consciousness (i.e., a sheer subjective feeling; Kelley & Jacoby, 1996; Koriat, 2000; Koriat, Nussinson, Bless, & Shaked, 2008). Reliance on a sheer subjective feeling of effort may have been the case for individuals that did not correctly identify the high switching deck though were confident in their choice (i.e., the No Gamble | Incorrect category). Individuals in this category demonstrated avoidance at chance. This group may have felt a difference in effort across the options during the DST, but lacked the level of awareness to determine the correct source of the difference (e.g., the difference in switching; Dunn et al., 2016) and randomly inferred one alternative was more effortful in the free-choice block. Furthermore, the potential reliance on a sheer feeling of effort then led to high confidence in their choice (i.e., choosing not to gamble). That is, these individuals felt a difference in effort which they were confident about, though they were unable to directly map the difference. Furthermore, there is the case of those individuals who, based on our measure of awareness, were unaware of the difference in switching but still avoided the high-demand option above chance (i.e., those in the Gamble | Incorrect category). Overall, this is a relatively small number of individuals in our sample (*n* =12 for the dichotomous categorization, and*, n* = 8 for the trichotomous categorization). Previous work examining cognitive effort in a DST has shown similar results related to unaware participants (Kool et al., 2010). Though the number of these individuals appear extremely small, they will require important consideration moving forward. As noted above, aggregate measures of awareness would help shed light on the processes associated with partial/no awareness of aspects of tasks and effort avoidance.

Conscious Awareness of Cues in Physical Effort

The current study specifically focused on the relation between conscious awareness and the avoidance of cognitive demand, with our general conclusion stating that awareness of effort cues appears to be an important prerequisite of successful demand avoidance. Nonetheless, evidence exists pertaining to the modulation of physical effort in the absence of awareness of relevant cues. Aarts and colleagues (2008) demonstrated that individuals that were subliminally primed with the concept of physical exertion were faster at applying force to a hand grip (i.e., squeezing). Similarly, Pessiglione and colleagues (2007) demonstrated that individuals will exert more physical effort in response to increased reward, even when reward cues are presented below awareness. Thus, in both cases conscious awareness of cues was not required to affect physical effort expenditure. Indeed, Marcora (2009) concluded conscious awareness of central motor commands through afferent feedback from skeletal muscles, heart, and lungs does not contribute significantly to the perception of effort during exercise. This is in contrast to our claim made here (and elsewhere; Desender et al., 2017; Dunn et al., 2016) that awareness does play a critical role in the cognitive effort domain.

At face value, it is reasonable to hypothesize that effort is one signal applied across cognitive and physical domains given the demonstrated overlap of neural systems involved in exerting both forms of effort (e.g., Boksem & Tops, 2008; Chong et al., 2017; Marcora, Bosio, & de Morree, 2008; Schmidt et al., 2012; Westbrook & Braver, 2015). However, attempts to integrate processes associated with physical effort exertion into cognitive effort frameworks (e.g., Baumeister, Bratslavsky, Muraven, & Tice, 1998; Gailliot & Baumeister, 2007) have been met with great skepticism on theoretical and empirical grounds (e.g., Gibson, 2007; Job, Walton, Bernecker, & Dweck, 2013; Lange & Eggert, 2014; Lurquin & Miyake, 2017; Kurzban, 2010). For example, Inzlicht and Marcora (2016) noted that the attempted application of a theoretical framework aimed at explaining physical fatigue (i.e., the Central Governor Model; Noakes, 2012) does little to help explain behavioral effects presumed to be related to mental fatigue. This is not to claim that physical effort models cannot be informative to those aimed at cognitive effort (Inzlicht & Marcora, 2016). Though it is important to highlight the divergences between findings and consequently apply skepticism that effort is the same phenomenon across domains. Importantly, recent work has begun to examine how individuals evaluate cognitive versus physical effort conjointly to further highlight contrasts and commonalities (Dunn, Koehler, & Risko, 2017; Potts, Pastel, & Rosenbaum, 2018; Risko & Gilbert, 2017).

Cognitive Effort, Awareness, and Schizophrenia

Perhaps one of the most studied clinical populations as it relates to effort are individuals with schizophrenia. Indeed, effort-based decision-making paradigms are prevalent in examining motivational symptoms of patients (e.g., Gold et al., 2013; Green, Horan, Barch, & Gold, 2015; Hartmann-Riemer, Kirschner, & Kaiser, 2018; Treadway, Peterman, Zald, & Park, 2015). A consistent demonstration is that individuals with schizophrenia are less willing than healthy individuals to expend effort to obtain reward, and this deficit in decision-making has been shown to positively correlate with severity of negative symptoms and level of functioning in many studies (for a review see, Culberth, Moran, & Barch, 2018). For instance, Fervaha and colleagues (2013) demonstrated that patients will opt to expend more effort at a rate less than controls for trials of high but uncertain incentive value, and these patterns of choices were related to neurocognitive deficits. Barch and colleagues (2014) demonstrated that schizophrenic patients made fewer difficult-task choices in a decision-making task, and this frequency was associated with more severe negative symptoms.

Beyond impairment in decision-making, patients with schizophrenia also demonstrate impairments in conscious processing. Specifically, patients with schizophrenia exhibit an elevated threshold for conscious perception, while subliminal processing is preserved (Berkovitch, Dehaene, & Gaillard, 2017). For example, patients exhibit elevated thresholds for processing during visual masking (Green, Lee, Wynn, & Mathis, 2011) and are less likely to report perceptions of unexpected events in inattentional blindness experiments (Hanslmayr et al., 2013). Furthermore, high-level metacognitive monitoring of outcomes is additionally subject to this dissociation between conscious and unconscious processing (Berkovitch, et al., 2017). Issues with error-monitoring are widely prevalent in individuals with schizophrenia, specifically hypoactivity, where the magnitude of error-responses are muted relative to healthy individuals (Alain, McNeely, He, Christensen, & West, 2002; Bates, Kiehl, Laurens, & Liddle, 2002). As a specific example (Charles et al., 2017), individuals with schizophrenia performed a masked number comparison task while awareness and error-detection were assessed. Results demonstrated that patients generated altered error-detection responses for conscious trials in terms of a decreased error-related negativity (ERN). Controls and patients, however, both performed above chance in evaluating their error commission on unconscious trials. The authors concluded that metacognition in schizophrenia is specifically altered, whereas non-conscious performance monitoring is preserved.

At least one common denominator is shared between the negative symptoms related to motivation and consciousness in schizophrenia: the role of dopamine in the brain. Pyramidal cells and their dendrites are smaller in the dorsolateral PFC of schizophrenic patients relative to controls, and the specific genes that are disrupted in the disorder affect the dopamine D2 receptors which are a key in prefrontal synaptic transmission (Ross, Margolis, Reading, Pletnikov, & Coyle, 2006). Specific models of schizophrenia hypothesize a dysregulated firing of dopamine neurons, leading to hyperactive neuronal activity in responses to non-predictive reward cues, as well as hypoactivity to relevant information (e.g., when an error is committed). It is likely that this dysfunction leads to many of the negative motivational and effort-related symptoms for patients (for recent reviews see, Fervaha, Foussias, Agid, & Remington, 2013; Howes, McCutcheon, & Stone, 2015). Interestingly, dopamine also plays a key role in the disruption of conscious awareness in schizophrenics. For instance, Berkovitch and colleagues (2017) note that dysfunction may be linked to the dopamine system in conjunction with glutamatergic NMDA cortical circuits. Specifically, disinhibition of dopaminergic cells projecting to the striatum via D2 receptors may result in aberrant dopamine bursts, or decreased activity of dopaminergic neurons projecting to the PFC via D1 receptors may be the cause. Furthermore, pharmacological studies of clinical patients recovering from post-traumatic minimally conscious state (MCS; Giacino, Fins, Machado, & Schiff, 2012) and persistent vegetative state caused from severe head injury (Matsuda, Matsumura, Komatsu, Yanaka, & Nose, 2003) have demonstrated that stimulating dopamine circuits may increase conscious awareness. Importantly, dopamine also plays a critical role in cognitive control, and cognitive and physical effort (Botvinick & Braver, 2015; Froböse & Cools, 2018; Kurniawan, Guitart-Masip, & Dolan, 2011; Westbrook & Braver, 2016). For instance, dopamine D2 antagonists reduce reward-seeking effort expenditure (Salamone, Correa, Farrar, & Mingote). Though the dopamine circuitry in the brain is complex and related to a wide range of behaviors, it is clear that the relation between schizophrenia and dopamine can provide key insights into the study of conscious awareness and cognitive effort.

Conclusion

Cognitive effort has benefited from a recent resurgence in interests from researchers. One important goal of these endeavors is to understand how individuals make effort-based decisions. The present study looked to further understand this decision processes. Overall, awareness does appear to be an important component of evaluating cognitive effort. This proposal opens several doors for future research focused on the relation between effort and conscious awareness.

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Footnotes

1. The original DST program used in Experiment 3 of Kool et al. (2010) was provided by the first author.
2. Moderate evidence in favor of the null hypothesis was demonstrated such that high demand location had no effect on free-choice selections, $BF_{NULL} = 3.63$, $t(195.7) = 1.11$, $p = .26$, $d = .16$, 95 CI [-.12, 44].
3. Additionally, a sensitivity analyses was performed by utilizing the two other prior scales (i.e., 1 “Wide” and $\sqrt{2}$ “Ultrawide) provided by the BayesFactor package. Coinciding with the BF associated with the default prior, no evidence was observed using the other two r scales for an effect of incentive awareness on choices, $BF_{ALT} = .59$ (wide), $BF_{ALT} = .44$ (ultrawide),
4. A two-way ANOVA sensitivity analysis was also conducted. A comparison of the dual main effect model (Accuracy ID & Gambling Decision) to the interaction model yielded anecdotal evidence in favor of the main effect model, BF = 1.75.

Appendix A

Gambling Protocol (Post-DST)

"In this part of the study I am going to ask you a question about certain aspects of the task you just completed. You will have the opportunity to make an additional $2 dollars by placing a wager on one of two options concerning the questions I ask."

"Before we ask you the question I am going to allow you to choose from two cards that I will shuffle. One card is a "WIN" card where you will win $2 if selected and the other is a "NO WIN" card where you will not win the $2 if selected. I will then ask you a question pertaining to the task. You can respond with either "YES" or "NO" to the question that is presented to you."

"You will then be asked to place your wager. You can either choose to stay with the answer you personally provided or choose the card that you selected prior to answering the question. If you choose to stick with your provided answer you will only win the $2 bonus if your answer is correct. Similarly, if you choose the card that was selected prior to answering the question you will only win the $2 bonus if you selected the "WIN" card."

"Only after you have made your wager for the question will any winnings be revealed."

##At this point ask if the participant has any questions and reiterate what you need to prior to moving on.

##Shuffle the two cards in front of the participant and ask them to make a choice. Do not reveal the card yet; just place it to the side.

Question #1: "Think back to the two options in the previous task that you selected between (these are the options that were displayed on the left and right of the screen). One of the two options had a tendency to switch between the tasks more often than the other. For example, one option may have displayed yellow digits more often than blue digits, whereas the other displayed the two at a somewhat similar rate."

"Please answer YES or NO to the following question: In the previous part of the task the option on the [left/right] tended to switch between colors more often than the option on the [left/right]?"

Circle one

YES **NO**

Appendix A (cont.)

"Now you will place your wager. Do you want to stick with your answer (*reiterate their answer*) that if correct you will win a $2 bonus? Or do you want to choose the card you selected prior to answering the question with the chance to win $2 if you selected the win card?"

Circle one

Chose Their Answer **Chose Card**

##At this point you can reveal that the participants' wager was correct. We are paying everyone the $2 regardless of their answer/choice of card.

Note: all text contained in quotation marks was read aloud by the research assistant to the participant. Text after the hash tags presented in italics are instructions to the research assistant only and were not read aloud to participants. The left/right statements within the switching question were counterbalanced across participants.