The Limits of Perceived Control: Novel Task-Based Measures of Control under Effort and in Anhedonia.

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ABSTRACT

Previous research presents a paradox in relation to the value of exerting personal control such that personal control is generally reinforcing, but its value may also be limited in some individuals and under certain circumstances. Across two studies, this dissertation takes a step towards exploring the limitations of perceived control at the process-level by manipulating perceived control via the provision of choice. Manuscript 1 examined limitations of perceived control in the context of effort costs and found that actual control, but not illusory control, may be necessary to enhance motivation in the context of physical effort, suggesting that perceived control may be limited in the context of effort. Manuscript 2 examined limitations of perceived control in relation to self-reported symptoms of anhedonia and found that responsivity to personal control was diminished in those with higher levels of anhedonia. Together these studies examined factors associated with limitations in appetitive personal control and suggest avenues for future research exploring perceived control processes and how they may interface with reward processes, which has potential implications for developing interventions to alleviate reward-related deficits found in anhedonia.

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GENERAL AUDIENCE ABSTRACT

Past research has shown that exerting personal control (actively influencing things in your life) is generally desired and motivating, but for some individuals and in some circumstances personal control may be less desirable or motivating (sometime people do not want to be in control). Across two studies, this dissertation explored why perceived control (the belief that one has influence over outcomes in one's life) might not be desired or motivating. In both studies, participants experienced perceived control during experiments when they were given choices within computerized games, believing themselves to have control over outcomes in the game. Manuscript 1 examined how perceived control may be less desirable when people must exert physical effort and found that people may be less inclined to believe they have control when their choice leads to a physical effort requirement. Manuscript 2 examined whether people want to be in control when they are experiencing anhedonia, a set of psychiatric symptoms that includes diminished motivation and reduced responses to reward (for example, paying less attention to rewards in the environment). This study found that people with anhedonia symptoms did not seem to want to be in control as much as psychologically healthy people. During the computerized game, people with anhedonia did not try to make their own choices when they had an opportunity to. Together these studies examined different factors associated with people not wanting to be in control or finding personal control less motivating. This research has implications for developing therapies for people with anhedonia, particularly symptoms related to not actively taking control.

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GENERAL INTRODUCTION

The Limits of Perceived Control: Novel Task-Based Measures of Control under Effort and in Anhedonia.

The concept of personal control, broadly defined as an individual's own control over outcomes in their life, has been the subject of a vast body of research (Reich & Infurna, 2016). Out of this body of work, two general approaches have emerged, with one approach conceptualizing control as a trait and the other approach conceptualizing control as a process (Infurna & Reich, 2016). At the trait-level, perceived control can be defined as a general belief in one's ability to exert control over outcomes and greater trait perceived control has been linked to positive outcomes across a variety of domains, including education, work, health, finances, and social relationships (Kalechstein & Nowicki, 1997; Lachman & Weaver, 1998; Nowiki & Duke, 2016). Further, a sense of control is thought to be essential to wellbeing (Leotti, Iyengar & Ochsner, 2010; Reich & Infurna, 2016; Ryan & Deci, 2000) and higher perceived control is associated with better mental and physical health (Ghane, Sullivan-Toole, DelGiacco & Richey, 2019; Infurna, Ram & Gerstorf, 2013; Langer & Rodin, 1976).

At the process-level, perceived control and closely related constructs have been examined in terms of associated cognitive and affective processes (e.g., Ly, et al., 2019; Maier & Seligman, 2016; Moore & Obhi, 2012). Making choices is a ubiquitous means by which humans exercise control (Leotti, Iyengar & Ochsner, 2010), and numerous research studies have used the provision of choice to experimentally induce a sense of control. For example, a common research design for such studies offers participants either a choice or no choice between one or more activities in a laboratory or classroom setting and, based on the choice condition (e.g., free choice, no choice, choice among extensive options), examines subsequent motivational outcomes such as time spent voluntarily engaging in the activity (considered a proxy measure for intrinsic motivation), objective performance level on the activity, and self-reported measures of enjoyment, liking, etc. (for review see Patall, Cooper & Robinson, 2008). Further, a large and growing body of research has utilized even very simple choices in the context of computerized tasks (e.g., choosing between two stimuli associated with equivalent proportions of probabilistic outcomes) to examine process-level effects of perceived control.

In general, studies examining the impact of choice have demonstrated beneficial effects from exerting personal control via choice. For example, Patall and colleagues conducted a meta-analysis of 41 behavioral studies on this topic and found that, in general, the provision of choice increased intrinsic motivation as well as effort and performance on tasks (Patall, Cooper & Robinson, 2008). Further, data from functional neuroimaging studies have shown that choice itself, apart from any additional benefit incurred from the choice, activates reward circuitry (Cockburn, Collins & Frank, 2014; Fujiwara, et al., 2013; Leotti & Delgado, 2011; Tricomi, Delgado & Fiez, 2004; Wang & Delgado, 2019) in much the same way as primary (O'Doherty, et al., 2002) and secondary (Delgado, et al., 2000; Knutson, et al., 2001) rewards. Thus, it has been proposed that choice itself is inherently rewarding, desired, and motivating (Leotti & Delgado, 2011; Leotti, Iyengar & Ochsner, 2010; Murayama, et al., 2017).

However, the benefits of personal control and choice are not universal (Anderson, 2003; Chua & Iyengar, 2006; Katz & Assor, 2006; Patall, 2012). At the trait-level, people vary in their desire for control, with some people reporting, for example, less enjoyment of being a leader or making their own decisions (Burger & Cooper, 1979; Burger, 2016). While the meta-analysis by Patall and colleagues found overall positive effects of choice on motivation and related constructs, there was considerable variation in the effects found across studies and a sizable portion of studies found negative effects when participants were given a choice (Patall, et al., 2008). Indeed, it appears that in certain contexts, people show a preference against expressing personal control, by avoiding active choices and sticking with a default option or by avoiding making a choice altogether (Anderson, 2003). For example, people demonstrate a bias towards options that are presented as the status quo (Samuelson & Zeckhauser, 1988), even when such biases are suboptimal and compromise task performance (Fleming, Thomas & Dolan, 2010). Further, making choices may lead to poorer motivation-related outcomes when choices are difficult, when the options are not attractive, or when choices do not allow individuals to exert personal control (Botti & Iyengar, 2004; Iyengar & Lepper, 2000; Katz & Assor, 2006; Moller, Deci & Ryan, 2006; Reeve, Nix & Hamm, 2003). Thus, just as there appears to be value for exerting personal control, there appears to be a contrasting value for passivity and avoiding personal control. Neuroimaging evidence also supports this notion as decisions to passively stick with a default option have been shown to engage neural reward structures, which were similarly engaged when those same participants won money (Yu, Mobbs, Seymour & Calder, 2010).

These contradictory results present a paradox in relation to the value of choice and suggest that the value of exercising control or perceiving oneself to be in control may vary greatly across contexts and individuals. While there is strong evidence that perceiving and exercising one's control over outcomes is healthy, desired, rewarding, and motivating, there is also evidence that the beneficial effects of perceived control may be limited under certain conditions and in some individuals. As there appears to be a strong connection between responsivity to personal control and diminished reward-related processes in internalizing psychopathologies, especially those with anhedonic features (Alloy & Abramson, 1979; Cloitre, et al., 1992; Franck, et al., 2001; Hofmann, 2005; Maeda, et al., 2013; Msetfi, Murphy & Simpson, 2007; Romaniuk, et al., 2019; Späti, et al., 2015), it is important to understand personal control at the process-level and to understand the limitations of appetitive personal control. Thus, this dissertation takes a step towards exploring the limitations of perceived control across two studies examining control at the process-level. More specifically, both studies demonstrate diminished motivational or reinforcing effects of perceived control in relation to (Manuscript 1) a context of physical effort and (Manuscript 2) individual differences in self-reported reward responsivity. To date, the tools available to measure control have limited research in the domain of perceived control at the process-level. While decades of research on perceived control has produced numerous self-report measures, there remains a lack of objective, behavioral measures of responsivity to control at the process-level. Thus, in each manuscript, a novel behavioral task was designed to objectively examine different aspects of control-related processes.

The first manuscript entitled, "Control and effort costs influence the motivational consequences of choice", examines how varied levels of control over exerting physical effort impacts task motivation in terms of preference and performance. The second manuscript entitled, "Choice bias: An objective measure of personal control responsivity", examines control-seeking in relation to individual differences in reward responsivity. Finally, there is a brief discussion related to the need to study perceived control at the process-level in order to inform potential interventions related to personal control.

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MANUSCRIPT 1

Control and Effort Costs Influence the Motivational Consequences of Choice

Abstract

The act of making a choice, apart from any outcomes the choice may yield, has, paradoxically, been linked to both the enhancement and the detriment of motivation. Research has implicated two factors in potentially mediating these contradictory effects: the personal control conferred by a choice and the costs associated with a choice. Across four experiments, utilizing a physical effort task disguised as a simple video game, we systematically varied costs across two levels of physical effort (Low-Effort, High-Effort) and control over effort costs across three levels of choice (Free-Choice, Restricted-Choice, and No-Choice) to disambiguate how these factors affect the motivational consequences of choosing within an effortful task. Together, our results indicated that, in the face of effort requirements, illusory control alone may not sufficiently enhance perceptions of personal control to boost motivation; rather, the experience of actual control may be necessary to overcome effort costs and elevate performance. Additionally, we demonstrated that conditions of illusory control, while otherwise unmotivating, can through association with the experience of free-choice, be transformed to have a positive effect on motivation.

Introduction

While people often prefer to make their own decisions, there are also circumstances in which choosing is less desirable. Indeed, the act of making a choice, separable from any extrinsic gains or losses the decision may incur, has been linked to both motivational enhancements and decrements (Patall, *et al.,* 2008; Scheibehenne, *et al.,* 2010). From these paradoxical findings, however, two major factors have emerged as potential mediators of the positive versus negative consequences of choice: (1) the personal control provided by a choice and (2) the costs associated with making a choice (Patall, 2012). Thus, the utility of making a choice, separate from the utility of its outcomes, may be recast as a joint function of the control provided by and costs associated with a given choice. However, the relative contribution of these factors on the effects of making a choice remains unknown. Accordingly, the purpose of the current study is to disambiguate how control and costs affect the motivational consequences of choosing within an effortful task.

There is substantial evidence that the act of making choices, in and of itself, is intrinsically rewarding and motivating (Leotti, *et al.,* 2010; 2015). For example, across human and nonhuman studies there is a measurable preference for options that lead to a subsequent, additional choice over options leading to a forced-choice, even when there are no material differences in eventual outcomes (Bown, *et al.,* 2003; Catania & Sagvolden, 1980; Suzuki, 1997, 1999, 2000) and neuroimaging studies have demonstrated that free-choices, bestowing no additional extrinsic reward, enhance neural activation in value-related regions, when anticipating a choice (Fujiwara, *et al.*, 2013; Leotti & Delgado, 2011) and when evaluating outcomes linked to choice (Cockburn, *et al.,* 2014; Tricomi, *et al.,* 2004). These studies suggest that intrinsic value is assigned to the very process of active decision making. Furthermore, engaging in active decision making can confer a variety of performance-related benefits, facilitating intrinsic motivation, performance, and effort exertion (Bhanji, & Delgado, 2014; Murayama, *et al.,* 2015; Murty, *et al.,* 2015; Patall, *et al.,* 2008).

Conversely, there are also certain contexts in which choice is discounted or actively avoided (Anderson, 2003; Burger, 1989; Dhar, 1997; Leotti & Delgado, 2014; Samuelson & Zeckhauser, 1988; Stephens, *et al.,* 2011), even when this bias for passivity is suboptimal and leads to increased error commission (Fleming, *et al.,* 2010). This propensity for passive decision strategies suggests that avoiding decisions can also carry utility, a supposition supported by functional neuroimaging evidence demonstrating that passively maintaining a default option, rather than making an active decision, engaged the same neural region activated by winning money (Yu, *et al.,* 2010). Furthermore, in some circumstances, making choices may have deleterious effects on motivation and performance (Burger, 1989; Flowerday & Schraw, 2003; Flowerday, *et al.,* 2004; Gourville & Soman, 2005; Iyengar, *et al.,* 2004; Iyengar & Lepper, 1999, 2000; Vohs, *et al.,* 2008).

As choice can, depending on context, have a differing impact on preference, outcome valuation, performance, and motivation, there is a need to identify contextual factors that mediate these effects. One potential mediating factor can be drawn from a frequent theme in psychological research: that perceptions of personal control are intrinsically motivating and psychologically adaptive (Alloy & Abramson, 1979; Bandura, 1997; deCharms, 1968; Eitam, *et al.,* 2013; Leotti, *et al.,* 2010, 2015; Miller, 1979; Rodin & Langer, 1977; Rotter, 1966; Ryan & Deci, 2000; Seligman, 1972) even to the point that individuals tend to perceive control even where none exists (Langer, 1975). Thus, it may not be the act of decision making *per se* that bestows psychological benefits but instead the sense of personal control conferred by making a decision. Consistent with this proposition, evidence suggests that the degree to which a decision signals personal control may mediate between the beneficial versus detrimental effects of choice (Katz & Assor, 2006; Patall, 2012). Further, studies in which decision scenarios were designed to dissociate perceptions of control from choice have demonstrated that choices engendering perceptions of control, rather than the mere act of choice, were linked to motivational benefits (Moller, *et al.,* 2006; Reeve, *et al.,* 2003). On the other hand, choice scenarios that are perceived as *controlling* the individual, rather than as an opportunity for the individual to *express control*, are linked to motivational detriments (Deci & Ryan, 1987; Moller, *et al.*, 2006; Patall, *et al.,* 2008; Pittman, *et al.,* 1980). On the whole, however, very few studies have directly, empirically assessed the role of personal control in decision making.

The cost/benefit analysis, a core concept from economics (Wallis & Rushworth, 2014), suggests that in addition to perceptions of control, another potential mediator of the beneficial versus detrimental consequences of choice, are the costs associated with making a decision (Patall, 2012). Effort is frequently cited as a principal cost in decision-making, biasing choice against effort expenditure, and many studies have demonstrated an effort discounting effect, whereby effort decreases the utility of related outcomes (Bitgood & Dukes, 2006; Botvinick, *et al.,* 2009; Garbarino & Edell, 1997; Kool, *et al.,* 2010; Kurniawan, *et al.,* 2010; 2013; Miller, 1968; Westbrook, *et al.,* 2013). Similarly, there is evidence that as decision-related costs increase, the positive effects of choice are undermined. Making choices in a context of high costs—effort costs, a loss frame, or negative emotions—can attenuate the appeal of engaging in active choice compared to remaining with a default option, receiving the outcomes of another agent's choice or ceding the responsibility for making a choice altogether (Beattie, *et al.,* 1994; Fleming, *et al.,* 2010; Garbarino & Edell, 1997; Gourville & Soman, 2005; Iyengar & Lepper, 2000; Kool, *et al.,* 2010; Leotti & Delgado, 2014; Samuelson & Zeckhauser, 1988; Sunstein, 2014). Furthermore, there is evidence that making choices in a context of increased costs can give rise to negative motivational consequences such as reduced satisfaction with outcomes, increased negative emotions, and diminished performance (Bruyneel, *et al.,* 2006; Garbarino & Edell, 1997; Hafner, *et al.*, 2016; Iyengar & Lepper, 2000; Sagi & Friedland, 2007; Yu, *et al.,* 2010; Vohs, *et al.,* 2008).

Given the conflicting evidence for the utility of choice itself, and the theoretical basis for control and cost to mediate between the beneficial and detrimental effects of decision making, the current study sought to dissociate the motivational consequences of these two factors by varying the level of control conferred by choices across different levels of effort costs. Specifically, we utilized two levels of physical effort costs (High-Effort and Low-Effort), and three levels of control over effort costs: real control (Free-Choice), illusory control (Restricted-Choice) and no control (No-Choice), to examine how preference and performance were affected by control and cost factors.

We had three overarching hypotheses across four experiments comprising this study. Based on effort discounting theory, we hypothesized that (I) patterns of preference would favor and performance would be generally enhanced for lower compared to higher effort trials (Low-Effort > High-Effort). Based on evidence that perceptions of control have positive motivational effects, we further hypothesized that (II) preference patterns would favor higher levels of control and performance would improve as a function of the amount of control available (Free-Choice > Restricted-Choice > No-Choice). Finally, based on a combination of evidence suggesting that a context of high costs and low control may produce a particularly damaging coalition, we anticipated that preference for and performance on an effortful task should be undermined most severely at the junction of high effort costs and low personal control (i.e. no available choices). Thus, we hypothesized that (III) preference and performance would be most strongly diminished when no choice is offered, but effort expenditures are high.

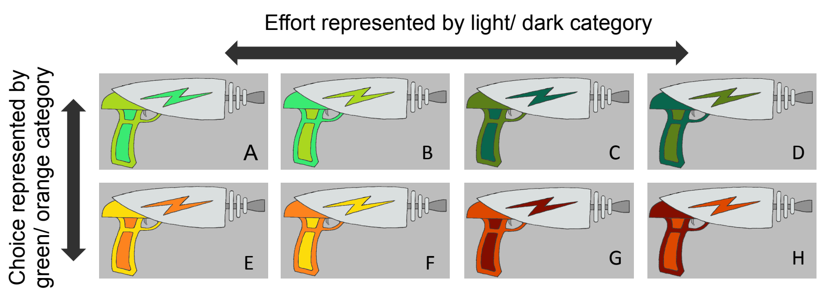
General Method

Participants were adult undergraduate students, recruited from Rutgers University-Newark, who provided written informed consent in accordance with the Declaration of Helsinki and were compensated with course credit. The Institutional Review Board of Rutgers University approved the study. To test study hypotheses, a novel physical effort task was created in E-Prime (Psychology Software Tools, Pittsburgh, PA), which employed, in different combinations across four experiments, three choice conditions offering different levels of control (described below) over two levels of physical effort costs. In order to facilitate task-engagement, the paradigm was presented as a video game in which participants fought aliens using "blaster" weapons. The blasters were "charged" manually by quick, repeated key presses (physical effort requirement), represented in real-time by an on-screen "charge bar", which was incrementally filled in red with each key press. Blasters only "fired" at an alien if they were fully charged in a pre-allotted amount of time.

Choice and Effort Conditions

Four variations on this task were implemented, comprising four separate experiments with non-overlapping participant samples. The full set of choice (control) and effort (cost) conditions are defined here; however, each individual experiment involved a different subset of these conditions, further detailed in each respective experiment and indexed in Table 1. The full set of conditions involved two levels of effort costs (Low-Effort and High-Effort, as defined by the number of key presses required) and three different levels of control provided across choice conditions (Free-Choice, Restricted-Choice, and No-Choice, as defined by the level of control over effort costs conferred by the blaster options offered). Participants were not explicitly informed as to which blaster cues represented which choice and effort conditions and their only information regarding the choice and effort conditions was through experience with the task. In the No-Choice condition, a single blaster was offered individually without another option. In the Free- and Restricted-Choice conditions, two blasters were offered for participants to choose between. The key difference between these two choice conditions was that in the Free-Choice condition (Experiment 4 only), participants were offered a choice between one High- and one Low-Effort blaster and were allowed to freely choose which they preferred to use, while in the Restricted-Choice condition, participants were offered a choice between two different colored blasters in the same effort category (e.g., a choice between two High-Effort blasters), such that their choice actually had no effect on the amount of effort required to fill the charge bar. Thus, the Restricted-Choice condition offered, at most, illusory control. Within an individual participant's game, subtle blaster color categories represented the choice and effort conditions, with two similarly colored exemplar blasters in each category. See Figure 1 for a representation of the blaster cues used. Again, no explicit information was given regarding how the color categories mapped onto choice and effort contingencies. Having two exemplar blasters per condition allowed implementation of the Restricted-Choice condition with a choice between two blasters from the same effort category. Across all experiments, the choice and effort conditions were presented in a random order. To fill the charge bar, Low-Effort blasters required a random number of presses between 11 and 20, while High-Effort blasters required a random number of presses between 21 and 30.

All experimental choice and effort contingencies were imbued with some ambiguity in order to facilitate subjective inferences regarding perceptions of control. Specifically, three design features were implemented: (1) no explicit information was given to participants about the effort levels or the nature of the choice conditions; rather, participants' only information about experimental conditions was acquired through playing the game; (2) blaster cues representing choice and effort conditions were organized into subtle color categories and the mapping of choice and effort contingencies onto the color scheme was also not explicitly stated; and (3) effort requirements (number of presses) were randomly drawn from ranges so that effort contingencies were somewhat uncertain. We expected that these ambiguities would facilitate subjective inferences regarding the level of control conferred by the choice conditions, particularly in the Restricted-Choice (illusory control) condition.



	Stimuli Sets	Choice, Low-Effort	No-Choice, Low-Effort	Choice, High-Effort	No-Choice, High-Effort
Experiment 1:	1			A,B	C,D
	2			C,D	A,B
	3			E,F	G,H
	4			G,H	E,F
Experiment 2:	1	A,B		E,F	
	2	E,F		A,B	
	3	C,D		G,H	
	4	G,H		C,D	
Experiment 3:	1	A,B	C,D	E,F	G,H
	2	G,H	A,B	C,D	E,F
	3	E,F	G,H	A,B	C,D
	4	C,D	E,F	G,H	A,B
Experiment 4:	1	A,B	C,D	E,F	G,H
	2	G,H	A,B	C,D	E,F
	3	E,F	G,H	A,B	C,D
	4	C,D	E,F	G,H	A,B

Figure 1: Example blaster stimuli set and chart indexing color-counterbalancing of all stimuli sets used. Across all experiments, conditions were counterbalanced with respect to color (to create four blaster sets per experiment) within the following constraints: color-hue (green vs. orange) represented choice conditions and color-value (lightness vs. darkness) represented effort conditions. The full set of blaster stimuli used across all experiments, are represented in the graphic and labeled A through H. The chart indexes how each of these individual blasters A—H (labeled in the chart cells) were organized in a counterbalanced fashion to create four sets of stimuli (sets represented by rows in the table) within each of the four experiments. Experiment 1 used either the green (stimuli set 1 and 2) or orange blasters (stimuli sets 3 and 4) with color-value distinguishing between effort levels. Experiment 2 used either the light (stimuli sets 1 and 2) or dark blasters (stimuli sets 3 and 4) with color-hue distinguishing between choice conditions. Experiments 3 and 4 used all eight blaster stimuli to represent the choice and effort conditions. Across all experiments, there were two exemplar blasters per condition.

<u>Stimuli Sets</u>

All eight blasters were used in each experiment, either with all eight used within each participant (Experiments 3 and 4) or with a set of four stimuli used within each participant, counterbalanced such that all eight stimuli were used between participants (Experiments 1 and 2). See Figure 1 for an example of the blaster stimuli sets used in each experiment. Across all experimental stimuli sets, color-hue category (green vs. orange) represented choice conditions and color-value category (lightness vs. darkness) represented effort conditions; within these constraints, conditions were fully counterbalanced across color categories, such that for each experiment, conditions were mapped onto color categories to create four versions of blaster stimuli, used across participants. For example, light green blasters represented Choice, Low-Effort for one participant's game and No-Choice, High-Effort for another participant's game. Thus, we aimed to minimize the chances of differences (both within an experiment and between experiments) resulting from preference for one color blaster over another. In experiments in which a condition was not used (Choice in Experiment 1; High-Effort in Experiment 2), one respective color category was excluded from each of the stimuli sets used in that particular experiment (e.g., in Experiment 1, only green *or* orange blasters were used for an individual participant's set of stimuli) The specific mapping of condition to color category that was used in each experiment will be further detailed in the respective experiment.

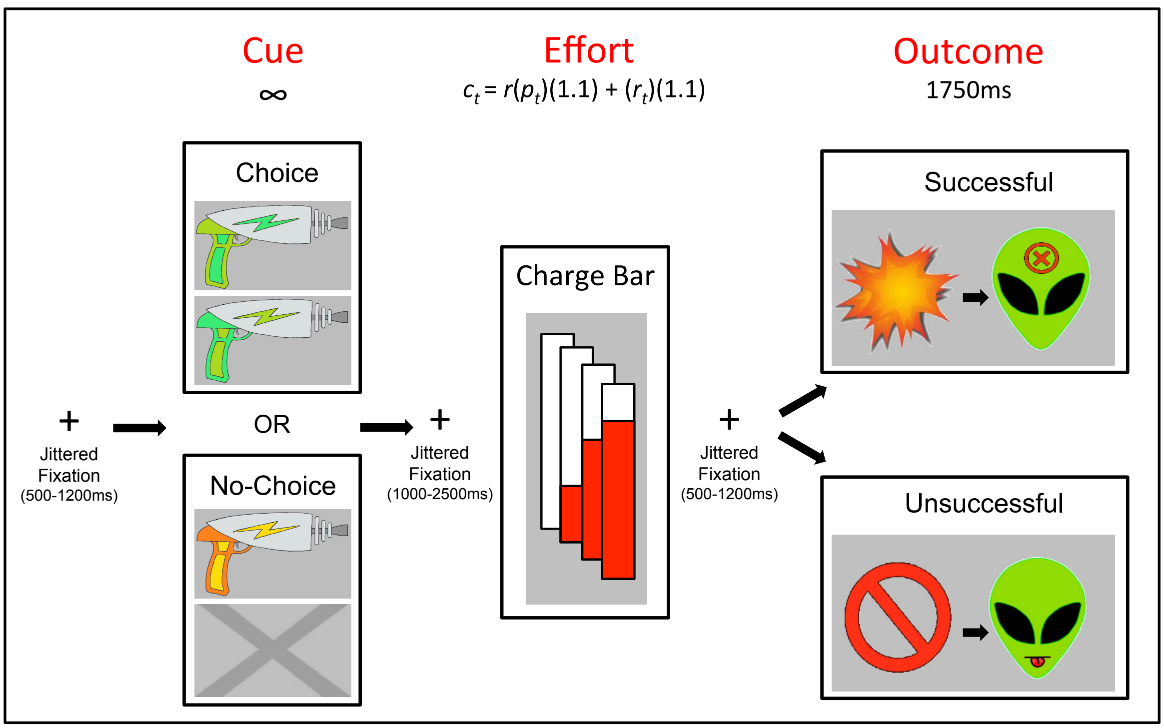


Figure 2: Schematic of a single trial within the tasks.
During the cue period, a screen presented blaster options (two options for Choice or a single blaster for No-Choice) until the participant responded. When the "charge bar" appeared, participants began making fast, repetitive key presses to fill the bar according to the effort requirement. Time allotted on a given effort trial was determined by the formula $c_t = r(p_t)(1.1) + (r_t)(1.1)$, where c_t is allotted time, r is the required number of presses, and r_t and p_t are the participant's average pre-game

reaction time to make an initial key press and time between key presses, respectively. Outcomes indicating whether the charge bar was successfully filled in the given time were displayed for 1750ms.

Task

See Figure 2 for a representative schematic of a single trial within the tasks. At the beginning of each trial, a blaster cue screen presented blaster options for the trial (two options for Choice conditions or a single blaster for the No-Choice condition) was displayed for an unlimited time until the participant responded to activate the indicated blaster. Next, a jittered fixation was displayed before the "charge bar" appeared so that participants could not exactly predict the charge bar onset, thus minimizing preemptive first presses. When the effort period began at the appearance of the charge bar, participants were to begin making fast repetitive key presses until the charge bar was filled. The charge bar was the same size for Low- and High-Effort trials but filled at different rates, depending on a pre-set number of presses required on a given trial. The number of required presses was not explicitly indicated to participants but could be implicitly estimated from the rate at which the bar was filled.

In order to standardize subjective difficulty, the task was individually calibrated to each participant's performance on a pre-game training task (Knutson, *et al.,* 2000; Mangels, *et al.,* 2006), such that success rates were high across all conditions. The specific time allotted for the charge bar on a given trial was determined by the formula $c_t = r(p_t)(1.1) + (r_t)(1.1)$, where c_t is allotted charge time, r is the required number of presses for a given trial, and r_t and p_t are the participant's average pre-game reaction time to make an initial key press, and the participant's average pre-game time between successive presses, respectively. Average individual pre-game reaction time values (both r_t and p_t) were both multiplied by 110% so that the task would be challenging but not impossible for participants. If the participant successfully completed the required number of presses in the allotted time, then the blaster "fired" and a "pow" symbol was displayed for 500ms, followed by an alien with an "X" on its head, displayed for 1250ms, indicating the alien was successfully "blasted." If the required presses were not completed in the allotted time, then a "No" symbol was displayed for 500ms, followed by an alien icon, displayed for 1250ms, indicating the alien escaped.

<u>Preference and Performance</u>

Four experiments tested the effects of different combinations of choice (control) and effort (costs) conditions (see Table 1) on participants' preference for and performance on choice- and effort- related trials. In each experiment, participants were asked to rate each blaster in the stimuli set on seven point Likert-type scale, with a one indicating "I don't like it at all" and a seven indicating "I like it a lot". Participants rated each blaster both before and after playing the game and during both rating sessions all blasters were visible on the screen with one blaster at a time indicated with a red rectangle as the one to be rated. As all blaster cues were visible on the screen during the ratings, all preference ratings were inherently in relation to the whole set of blaster cues used in the game. To control for individuals' prior color predilections, preference was operationalized in all experiments as an individual's change in preference from before to after playing the game. Performance was operationalized as the number of successful trials in a given condition. Experiment 1 tested the effects of variable effort alone, using only No-Choice trials of Low- or High-Effort. Experiment 2 tested the effects of mere choice, using only Low-Effort trials preceded by either No-Choice or a Restricted-Choice. Experiments 3 and 4 tested the combined effects of choice and effort, both using two levels of effort (Low-Effort, High-Effort) and varied levels of control over effort costs. The key difference between Experiment 3 and 4 is that Experiment 3 utilized two levels of control across choice conditions (Restricted-Choice, No-Choice), while Experiment 4 utilized three levels of control across choice conditions (Free-Choice, Restricted-Choice, No-Choice).

		Effort (Cost) Conditions	Choice (Control) Conditions
Experiment 1:	Effort Varied, No-Choice	Low-Effort, High-Effort	No-Choice
Experiment 2:	Choice Varied, Low-Effort	Low-Effort	No-Choice, Restricted-Choice
Experiment 3:	Effort Varied, Choice Varied (without Free-Choice)	Low-Effort, High-Effort	No-Choice, Restricted-Choice
Experiment 4:	Effort Varied, Choice Varied (with Free-Choice)	Low-Effort, High-Effort	No-Choice, Restricted-Choice, Free-Choice

Table 1: Chart of conditions in each experiment.

Experiment 1: Variable Effort, Constant No-Choice Game

Experiment 1 used trials of either Low- or High-Effort, while holding the control factor constant with only No-Choice trials in order to test the effects of different levels of effort costs on preference and performance. In this experiment we further sought to establish baseline preference and performance levels for Low- and High-Effort trials in the absence of choice. In line with our first hypothesis, we predicted that participants would show an increased preference for and enhanced performance on Low- compared to High-Effort trials.

Experiment 1 Method

Thirty-seven participants were recruited; however, due to a programing error, data were not collected for one participant. Thus, the final sample included thirty-six participants (16 females; mean age = 22.1 years, *SD* = 7.33). To control for base preferences for blaster colors, participants were randomly assigned to one of four counterbalanced sets of stimuli (see Figure 1). Two sets were composed of only the green blasters and two sets were composed of only orange. Within all four sets of blaster stimuli, the color-value (lightness vs. darkness) distinguished the levels of effort (Low-Effort, High-Effort) in a counterbalanced fashion, with two exemplar blasters in each effort condition. In a single block there were 16 No-Choice trials: eight Low-Effort and eight High-Effort. Blocks repeated six times across the game, resulting in a total of 48 No-Choice, Low-Effort trials and 48 No-Choice, High-Effort trials.

Experiment 1 Results

Preference: Figure 3A shows the change in preference for Low- and High-Effort blaster cues, as rated on a seven point Likert-type scale, from before to after participants played the Variable Effort, Constant No-Choice Game. Pre-game preference ratings for Low- and High-Effort blasters were not significantly different ($t(35) = -1.43$, $p = .162$). Preference ratings for Low-Effort blasters increased from an average pre-game rating of 4.08 ($SD = 1.55$) to an average post-game rating of 5.32 ($SD = 1.51$), while for High-Effort blasters, preference ratings decreased from an average pre-game rating of 4.67 ($SD = 1.49$) to an average post-game rating of 2.82 ($SD = 1.67$). For both Low- and High-Effort conditions, the change in preference was significantly different from zero (both $ps < 0.005$). Additionally, the change in preference for Low- compared to High-Effort blasters differed significantly ($t(35) = 5.09$, $p < 0.0001$), suggesting that the difference between Low- and High- effort requirements was sufficiently learned.

Performance: Figure 3B shows success rates for Low- and High-Effort trials in the Variable Effort, Constant No-Choice Game. While the success rates were high for both conditions, success for Low-Effort trials ($M = 97.1\%$, $SD = .04\%$) was higher than for High-Effort trials ($M = 95.2\%$, $SD = .08\%$) and this difference approached significance ($t(35) = 1.99$, $p = .054$).

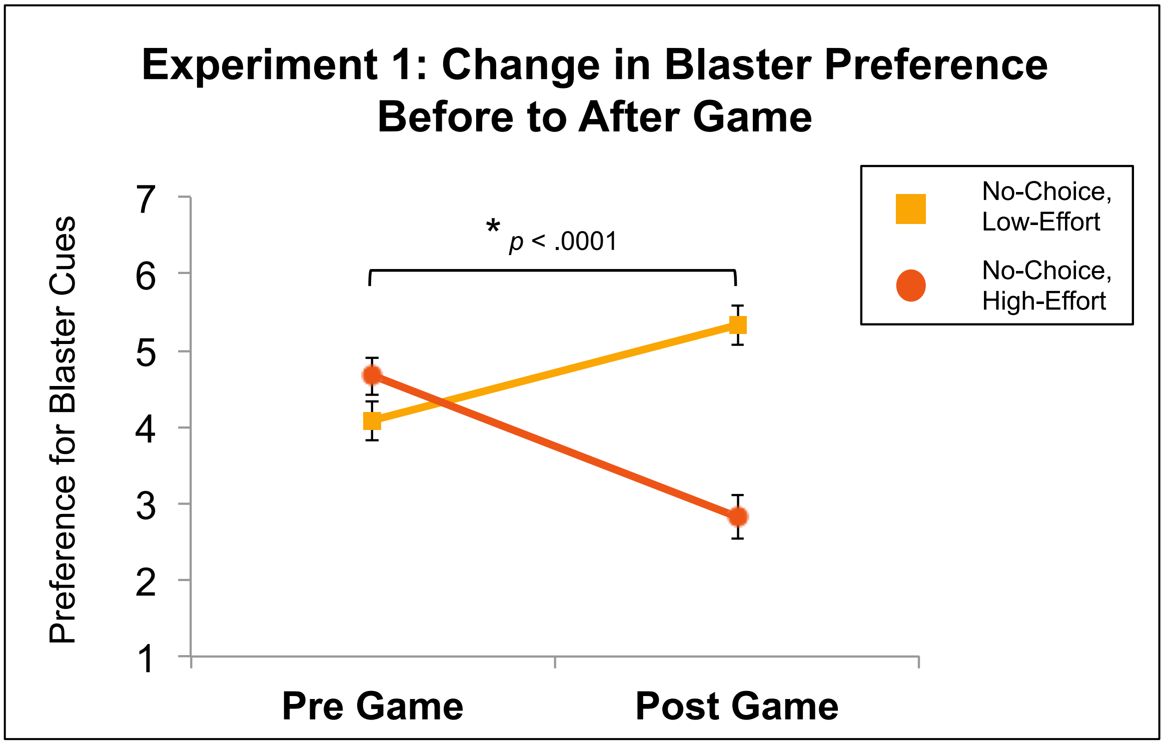


Figure 3A: Change in preference for conditions in Experiment 1: the Variable Effort, Constant No-Choice Game. The change in preference from before to after the game for Low- compared to High-Effort blasters was significantly different ($p <$ 0.0001).

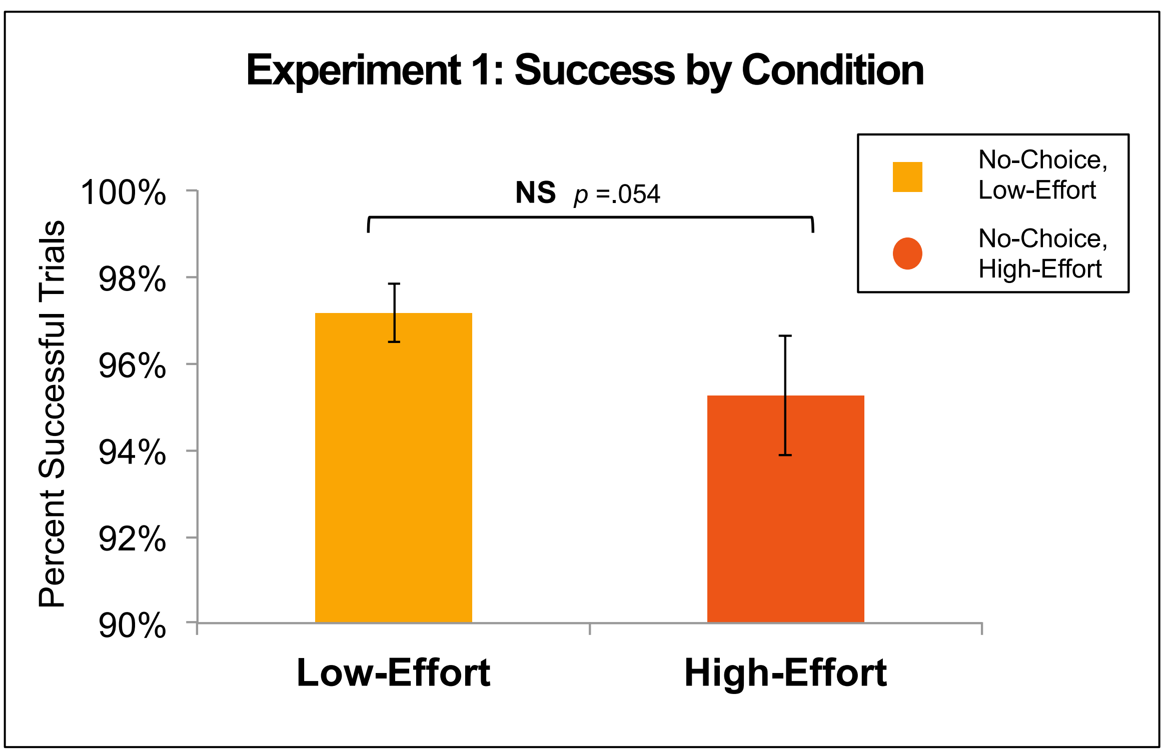


Figure 3B: Success rates for conditions in Experiment 1: the Variable Effort, Constant No-Choice Game. The difference in success rates for Low- and High-Effort blasters approached significance (p = .054). All error bars represent standard error of the mean.

Experiment 1 Discussion

In this experiment in which only effort costs were varied, we sought to establish baseline levels of preference for and performance on Low-Effort and High-Effort conditions when there was no control over effort costs. Based on effort discounting theory, which holds that the value of an outcome is discounted by its associated effort requirement, we hypothesized that participants would show enhanced preference and performance for Low- compared to High-Effort trials. These hypotheses were supported, as evidenced by strong preference for Low-Effort blasters. While high success rates across conditions confirmed that both the Low and High levels of effort were achievable for subjects, success rates were marginally higher in the Low-Effort condition although this difference did not reach significance. Thus, results from subsequent experiments can be interpreted in light of participants preferring Low- to High-Effort and showing a modest, albeit non-significant, boost in performance in the Low-Effort condition.

Experiment 2: Variable Choice, Constant Low-Effort Game

Experiment 2 used Low-Effort trials of either Restricted- or No-Choice to test the effects of different levels of control on preference and performance, with effort level held constant. In this experiment we sought to establish baseline preference and performance levels for Restricted- and No-Choice trials in the context of Low-Effort alone. Importantly, the Restricted-Choice condition did not grant any actual control over the effort requirements linked to blaster selection, as the options offered in the Restricted-Choice condition were always two blasters within the same effort category (Low-Effort in this experiment). However, control is often inferred even when individuals actually possess none (Clark, *et al.,* 2009; Davis, *et al.*, 2000; Langer, 1975; Wegener & Wheatley, 1999). To further promote this tendency of presuming personal control, three features were implemented including: (1) giving participants no explicit information regarding the choice and effort conditions, (2) subtle mapping of blaster cues onto choice and effort contingencies that had to be learned through experience, and (3) drawing effort requirements from a range (in this experiment, the Low-Effort range of 11 to 20 presses) so that effort was somewhat ambiguous. While these features were implemented across all experimental conditions, we expected that the ambiguity created would particularly facilitate perceptions of control in the Restricted-Choice condition. Thus, in line with our second general hypothesis, we predicted that participants would show enhanced preference and performance for Restricted- compared to No-Choice trials, even though the Restricted-Choice condition provided no actual means for reducing effort costs. On the other hand, if participants (accurately) perceived that the Restricted-Choice condition offered no actual control over effort costs, then we would not expect to see enhancements in preference and performance (and possibly even detriments) for the Restricted- relative to the No-Choice condition (Moller, *et al.*, 2006).

Experiment 2 Method

Thirty-four participants were recruited and completed the experiment (17 females; mean age = 21.1 years, *SD* = 5.09). To control for base preferences for blaster colors, participants were randomly assigned to one of four counterbalanced sets of stimuli (see Figure 1). Two sets were composed of only the light colored blasters (light green and light orange) and two sets were composed of only the dark colored blasters (dark green and dark orange). Within all four sets of blaster stimuli, the color-hue (green vs. orange) distinguished the choice conditions (Restricted-Choice, No-Choice) in a counterbalanced fashion, with two exemplar blasters in each choice condition. The Restricted-Choice condition was implemented by offering a choice between two blasters of slightly different colors that both required Low-Effort. Importantly, choice in this condition did not confer any control over effort costs. In a single block there were 32 Low-Effort trials: 16 Restricted-Choice and 16 No-Choice. Blocks repeated three times across the game, resulting in a total of 48 Restricted-Choice, Low-Effort trials and 48 No-Choice, Low-Effort trials.

Experiment 2 Results

Preference: Figure 4A shows the change in preference for Restricted- and No-Choice blaster cues from before to after participants played the Variable Choice, Constant Low-Effort Game. Pre-game preference ratings for Restricted- and No-Choice blasters were not significantly different ($t(33) = -0.39$, $p = .699$). Across both the Restricted- and No-Choice conditions, preference ratings decreased from similar average pre-game ratings of 5.07 (SD = .92) and 5.16 (SD = 1.14) to similar post-game ratings of 4.66 (SD = 1.04) and 4.75 (SD = 1.38), respectively. For both conditions, the change in preference did not significantly differ from zero (Restricted-Choice: ($t(33) = -1.80$, $p = .081$); No-Choice: ($t(33) = -1.60$, $p = .119$) and there was no difference in the pre- to post- change in preference between the Restricted- and No-Choice conditions; from before to after the game, preference for both conditions decreased approximately .06 of a point on a seven point Likert-type scale.

Performance: Figure 4B shows success rates for Restricted- and No-Choice trials in the Variable Choice, Constant Low-Effort Game. Participants performed similarly in the two conditions, successfully completing 95% (SD = .08%) of No-Choice trials and 94.2% (SD = .10%) of Restricted-Choice trials ($t(33) = 0.93$, $p = .360$).

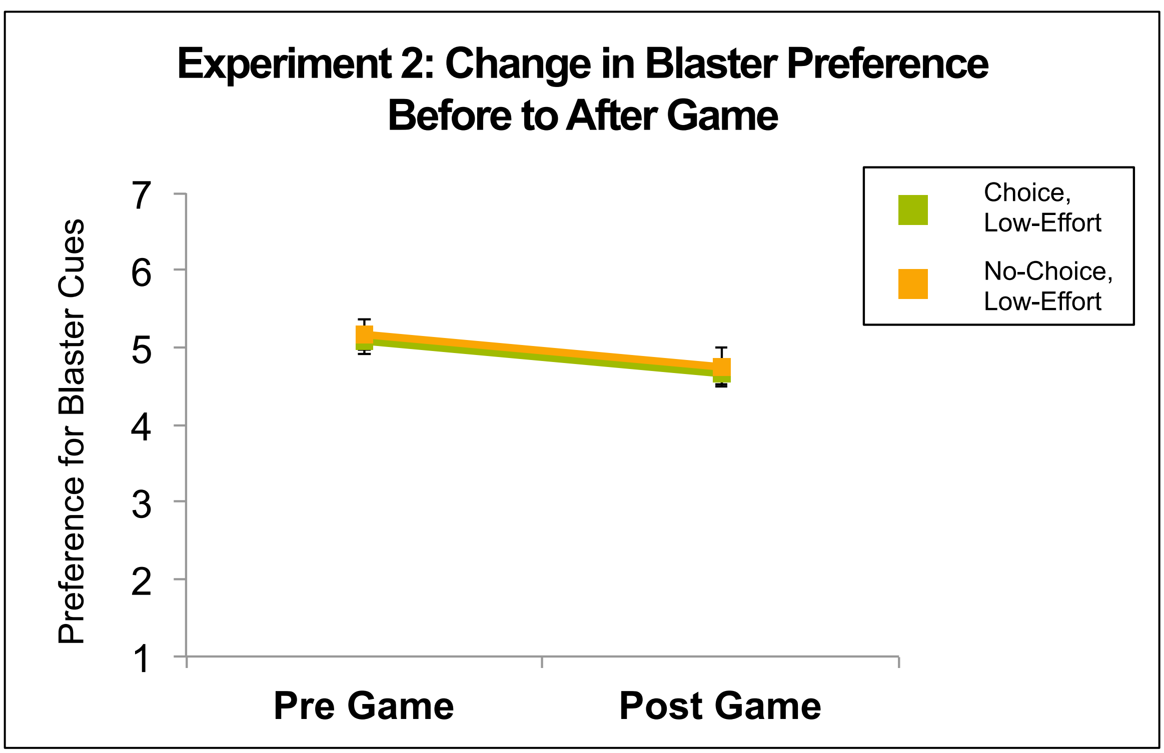


Figure 4A: Change in preference for conditions in Experiment 2: the Variable Choice, Constant Low-Effort Game. There was no difference in the change in preference from before to after the game for Restricted- compared to No-Choice blasters.

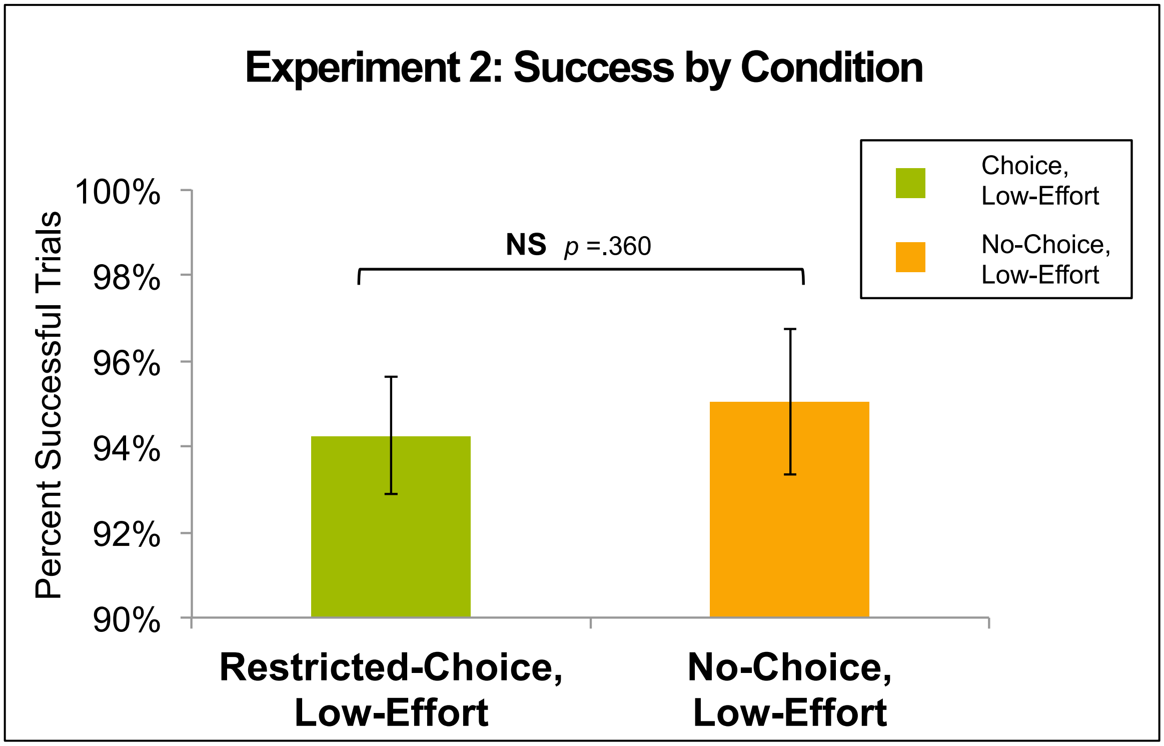


Figure 4B: Success rates for conditions in Experiment 2: the Variable Choice, Constant Low-Effort Game. There was no difference in success rates for Restricted- and No-Choice blasters (p = .360). All error bars represent standard error of the mean.

Experiment 2 Discussion

This experiment employed conditions of No-Choice and Restricted-Choice for the purpose of determining the impact of no control and illusory control, respectively, on preference for and performance on Low-Effort trials. Given that control is often perceived even when there is none and given design features that were incorporated to facilitate subjective inferences regarding personal control, we hypothesized that participants would show enhanced preference and performance for Restricted- compared to No-Choice trials, in line with our second general hypothesis. However, this hypothesis was not supported, as there were no meaningful or significant differences between the two choice conditions. Across both the Restricted-Choice and No-Choice conditions, preference for blasters decreased very slightly and to the same degree (approximately .06 of a point on a seven point Likert-type scale), but changes in preference in these two conditions were neither significantly different from one another nor significantly different from no change in preference. Similarly, there was no significant difference in success rates between the two conditions. Thus, the provision of illusory control appeared to have no substantial impact on participants' preference or performance in this experiment where effort levels were held constant. While previous studies have found positive motivational effects of choices offering only illusory control (e.g., Cordova & Lepper, 1996; Tricomi, *et al.,* 2004), many such studies involved choices in a context of rewarding outcomes or intrinsically motivating situations. However, there is evidence that a context of costs may reduce the positive effects of choice (Fleming, *et al.,* 2010; Gourville & Soman, 2005; Iyengar & Lepper, 2000). For example, a series of experiments by Leotti and Delgado (2011; 2014) demonstrated that a loss, compared to a gains, context can diminish the desirability of exercising personal control via making choices. As effort is weighed as a cost in the decision making process in much the same manner as monetary losses (Botvinick, *et al.,* 2009; Kurniawan, *et al.,* 2010), it is possible that in the context of this task, effort costs overshadowed the potential motivational benefits of the Restricted-Choice condition. Our results are consistent with work by Moller and colleagues (2006) who also examined choice in the context of effortful tasks and found no performance benefits from choices that limited the expression of personal control. Thus, the results from the current experiment suggest that when choice is limited and offers only illusory control, an effortful context may undermine potential motivational benefits often associated with choice.

Experiment 3: Variable Choice and Effort, Without Free Choice Game

The Variable Choice and Effort, Without Free Choice Game used Restricted- and No-Choice trials of both Low- and High-Effort to test the combined influence of different levels of control and cost factors on preference and performance in a 2x2 design. Within this experiment we examined hypotheses two and three: that preference and performance ratings would favor conditions where greater control is perceived, and that low control but high effort would have the combined influence of producing lower preference and performance. As in Experiment 2, the Restricted-Choice condition did not confer any control over effort costs, as the blaster options offered in Restricted-Choice were always within the same effort category (e.g., a choice between two High-Effort blasters). Thus, the perception of control was free to vary, while actual control was held constant between the two conditions and was effectively zero. Although there was no effect of choice on preference or performance in Experiment 2 where Restricted-Choice was tested in the context of Low-Effort costs, we predicted that the addition of a High-Effort condition might elicit a positive motivational effect from the Restricted-Choice condition. We specifically hypothesized that the contrast effect of having two levels of effort might increase the salience of personal control, as participants tried to exercise control to avoid the higher effort costs (in line with participant preference for Low-Effort established in Experiment 1). As with all of the experiments, the choice and effort contingencies were obscured such that actual levels of control over effort costs and how these contingencies mapped onto blaster cues were somewhat ambiguous. For example, effort cost requirements (number of presses) for the two conditions were drawn from ranges that were consecutive to one another (Low-Effort 11-20 presses; High-Effort 21-30 presses), thus making effort costs somewhat difficult to characterize. Such ambiguities in the task left a margin of uncertainty for participants to make inferences regarding the degree to which Restricted-Choice afforded control over effort costs. We predicted that participants would infer control in the Restricted-Choice condition and, thus, show enhanced preference and performance for Restricted-Choice compared to No-Choice trials, in line with our second general hypothesis. In-line with hypothesis three, we also specifically anticipated that the additional effort requirements of the High-Effort condition might undermine motivation, especially as high effort costs intersected with low levels of personal control. Thus, we predicted that any decrements in motivational outcomes would be observed in the No-Choice, High-Effort condition, which combined the lowest level of personal control and the greatest effort costs.

Experiment 3 Method

Thirty-three participants completed the experiment (13 females, 2 other gender; mean age = 21.7 years, *SD* = 4.83). To control for base preferences for blaster color, participants were randomly assigned to one of four counterbalanced sets of stimuli. The color-hue category (green vs. orange) distinguished levels of choice (Restricted-Choice, No-Choice) and the color-value category (lightness vs. darkness) distinguished levels of effort (Low-Effort, High-Effort); within these constraints, conditions were fully counterbalanced across color categories, creating four sets of blaster stimuli with two exemplar blasters in each of 2x2 conditions (see Figure 1). Across a single block there were 16 Restricted-Choice trials (eight Low-Effort and eight High-Effort) and 16 No-Choice trials (eight Low-Effort and eight High-Effort). Blocks repeated four times across the game resulting in a total of 32 Restricted-Choice, Low-Effort trials; 32 Restricted-Choice, High-Effort trials; 32 No-Choice, Low-Effort trials; and 32 No-Choice, High-Effort trials.

Experiment 3 Results

Preference: Figure 5A shows the change in preference for all choice and effort conditions from before to after participants played the Variable Choice and Effort Without Free Choice Game. Bonferroni-corrected pairwise t-tests of pre-game preference ratings did not reveal any significant differences among the four conditions. Across choice conditions, preference for Low-Effort increased from pre-game (Restricted-Choice, Low: M = 4.18, SD = 1.27; No-Choice, Low: M = 4.03, SD = 1.66) to post-game (Restricted-Choice, Low: M = 5.29, SD = 1.22; No-Choice Low: M = 5.15, SD = 1.47) and preference for High-Effort blasters decreased from pre-game

(Restricted-Choice, High: M = 4.56, SD = 1.42; No-Choice, High: M = 4.45, SD = 1.7) to post-game (Restricted-Choice, High: M = 3.38, SD = 1.54; No-Choice, High: M = 3.32, SD = 1.49). Across all conditions, changes in preference were significantly different from zero (all ps < .005). A 2x2 ANOVA of the change in preference data, indicated a main effect of effort (F(1,32) = 37, p <.0001), such that the increase in preference for Low-Effort blasters was significantly different (using the Bonferroni-corrected significance threshold of p = .025) than the decrease in preference for High-Effort blasters for both the Restricted-Choice (t(32) = 4.82, p < .0001) and No-Choice (t(32) = 4.92, p < .0001) conditions. However, there was no main effect of choice (F(1,32) = .022, p = 0.882), as the Restricted-Choice and No-Choice conditions (collapsing across effort conditions) showed virtually no difference in this experiment (t(32) = -.15, p = .882). There was no interaction of the choice and effort conditions in the before to after game preference change (F(1,32) = .003, p = 0.957).

Performance: Figure 5B shows success rates for all conditions in the Variable Choice and Effort Without Free Choice Game. Success rates were high (above 95%) for all four conditions (Restricted-Choice, Low-Effort: M = 97.9%, SD = .03%; Restricted-Choice, High-Effort: M = 96.2%, SD = .05%; No-Choice, Low-Effort: M = 98.4%, SD = .03%; No-Choice, High-Effort: M = 95.3%, SD = .08%). A 2x2 ANOVA indicated a main effect of effort (F(1,32) = 10.679, p = .003), such that success rates for Low-Effort blasters were significantly greater than success rates for High-Effort blasters for the No-Choice (t(32) = 2.77, p = .009) condition and this difference for the Restricted-Choice condition approached the Bonferroni-corrected significance threshold of p = .025 (t(32) = 2.18, p = .037). However, there was no main effect of choice (F(1,32) = .181, p = .674), as the Restricted-Choice and No-Choice conditions (collapsing across effort conditions) showed virtually no difference (t(32) = .425, p = .674). There was no interaction of the choice and effort conditions (F(1,32) = 1.267, p = .269).

As we had an a priori hypothesis that the greatest motivational decrements would be observed in the No-Choice, High-Effort condition, we also examined performance differences in this condition relative to the other conditions. While the decrement in performance in the No-Choice, High-Effort condition was significant when compared to both No-Choice, Low-Effort (as already stated) and Restricted-Choice, Low-Effort (t(32) = -2.81, p = .008), the decrement was not significant in comparison to Restricted-Choice, High-Effort (t(32) = -.88, p = .385).

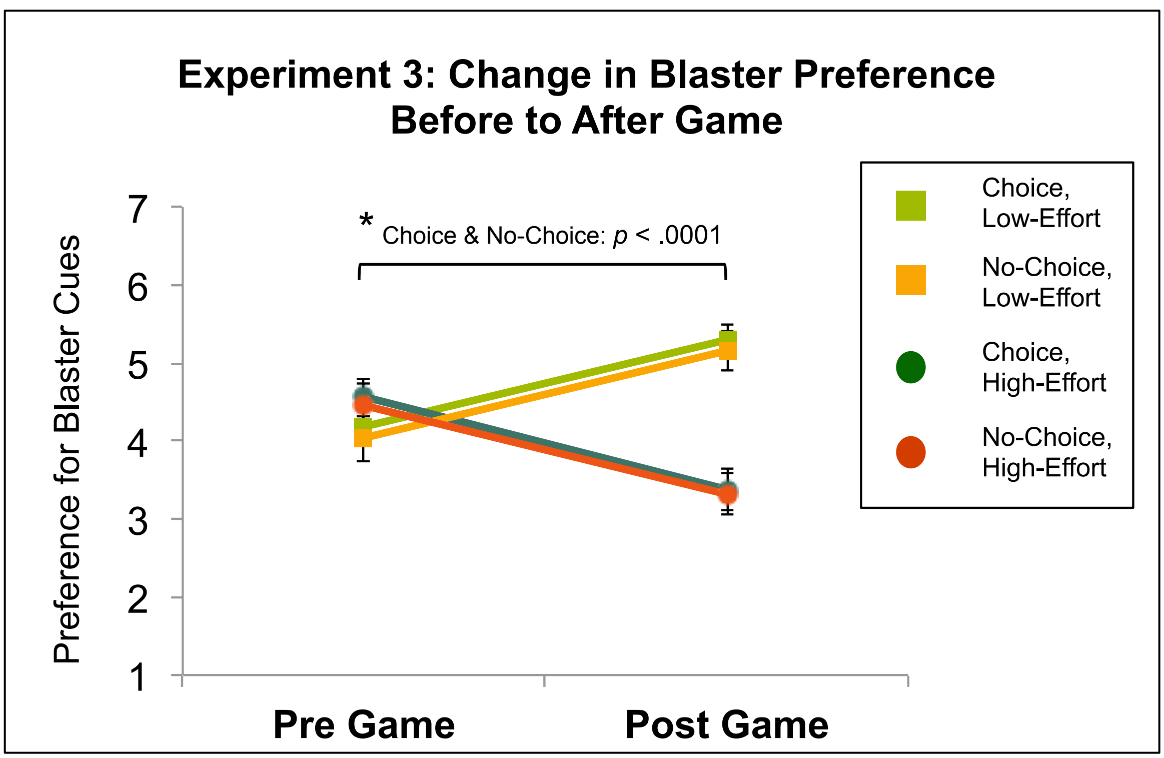


Figure 5A: Change in preference for conditions in Experiment 3: the Variable Choice and Effort Without Free Choice Game. A 2x2 ANOVA indicated a main effect of effort (p <.0001), such that the increase in preference for Low-Effort blasters was significantly different than the decrease in preference for High-Effort blasters for both the Restricted- and No-Choice conditions (both p's < .0001) .

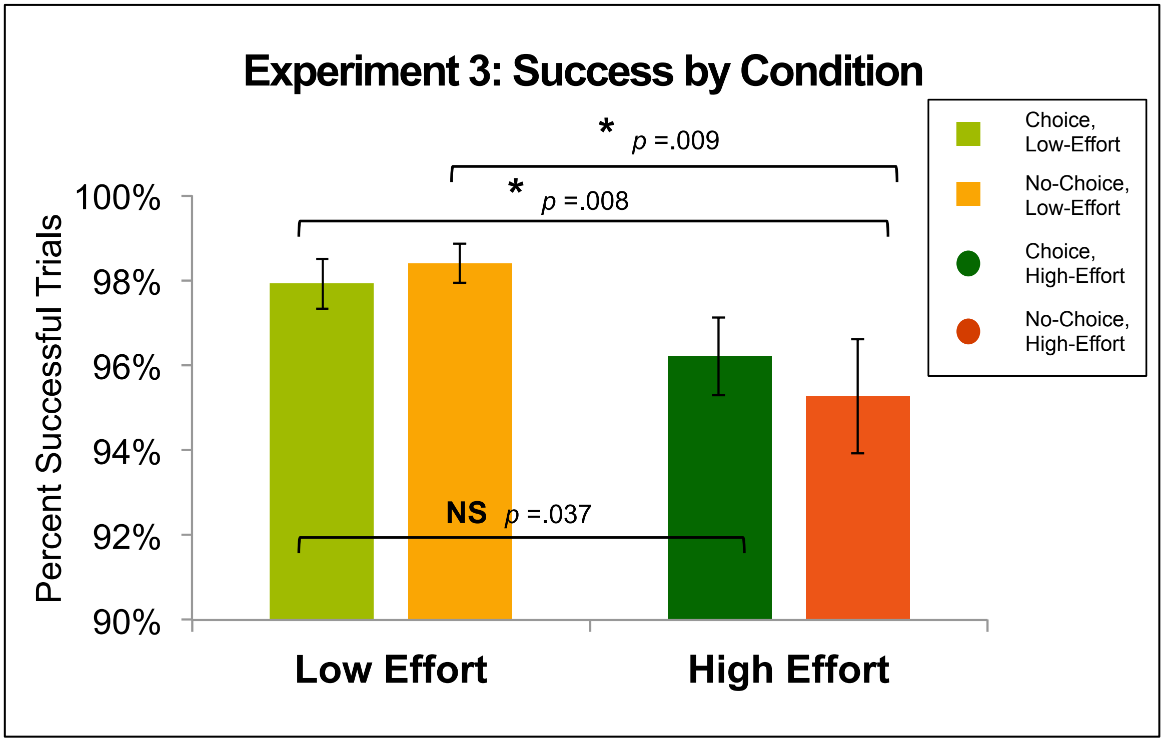


Figure 5B: Success rates for conditions in Experiment 3: the Variable Choice and Effort Without Free Choice Game. A 2x2 ANOVA indicated a main effect of effort (p = .003), such that success rates for Low-Effort blasters were significantly greater than success rates for High-Effort blasters for the No-Choice condition (p = .009) and this difference approached the Bonferroni-corrected significance threshold of p = .025 for the Restricted-Choice condition (p = .037). Due to our a priori hypothesis, we also demonstrated that success rates in the No-Choice, High-Effort condition were significantly lower than in the No-Choice, Low-Effort (p = .009) and Restricted-Choice, Low-Effort (p = .008) conditions. All error bars represent standard error of the mean.

Experiment 3 Discussion

This experiment employed conditions of No-Choice and Restricted-Choice across both Low-Effort and High-Effort trials to determine the combined impact of varying both costs and control over costs on preference and performance in a 2x2 design. While the Restricted-Choice condition did not confer any actual control over effort costs, experimental contingencies were ambiguous so that participants might infer control (though illusory) when given a Restricted-Choice and, thus, show enhanced motivation in this condition. While participants in Experiment 2 did not show motivational benefits related to the provision of Restricted-Choice, we hypothesized that varying the level of effort across two conditions might create a direct contrast effect, making the choice condition more salient in Experiment 3. Thus, in line with our second general hypothesis, we anticipated increased perceptions of control when participants were given a choice and, therefore, enhanced performance and preference for the Restricted- compared to No-Choice conditions. However, this hypothesis was not supported. Only the effort, but not the choice condition had an effect on preference and performance. Replicating results from Experiment 1 and supporting our first general hypothesis, participants in Experiment 3 both preferred and were more successful on Low-Effort compared to High-Effort trials. Replicating results from Experiment 2, the Restricted-Choice compared to the No-Choice condition did not significantly affect preference and performance. Thus across Experiments 2 and 3, there was no evidence that the mere provision of choice, conferring at most illusory control, had any impact on participants' preference or performance. These findings are in line with studies demonstrating that mere choice, devoid of opportunities for personal control, does not enhance motivation (Moller, *et al.*, 2006; Reeve, *et al.*, 2003). Further, these results suggest participants may have experienced the Restricted-Choice condition, which offered options that were only superficially different, as limiting the opportunity to express control. Evidence suggests when individuals experience conditions as *controlling* their behavior rather than providing them with an opportunity for control, motivation is undermined (Deci & Ryan, 1987; Patall, *et al.,* 2008; Pittman, *et al.,* 1980).

While our second general hypothesis was not supported, performance results are consistent with our third general hypothesis, which posited that the greatest motivational deficits would occur when the lowest levels of personal control met the highest effort costs. While we did not observe a differential effect between the choice conditions related to increasing effort requirements, it is possible that both effort conditions required an effort cost beyond some threshold at which the potential motivational benefits of choice are undermined, particularly a choice only offering illusory control. Thus, the null effect of choice in this experiment may be due to both low levels of personal control (in both the Restricted- and No-Choice conditions) and due to effort costs (across both effort conditions). Finding that participants were least successful in the No-Choice, High-Effort condition further supports our third hypothesis that a lack of control and a context of increased costs may exert a conjoint influence to undermine motivation. To further parse the effects of personal control and effort costs on the motivational consequences of choice, future studies might need to examine the effects of differing levels of control across a greater range of effort costs including a no effort condition. Nonetheless, results from the current study suggest that in the context of effort requirements, a choice conferring, at most, illusory control may not sufficiently bolster perceptions of personal control or override decision-related costs to enhance motivational outcomes.

Experiment 4: Variable Choice and Effort, With Free Choice Game

The Variable Choice and Effort, With Free Choice Game was similar to Experiment 3, but introduced a new choice condition, Free-Choice, in which participants were given a choice between one Low- and one High-Effort blaster and were allowed to freely choose which they preferred to use. Thus, Experiment 4 used Free-, Restricted- and No-Choice trials of both Low- and High-Effort to test the combined influence of different levels of personal control and effort costs on preference and performance. As in Experiment 2 and 3, the Restricted-Choice condition did not offer any actual control over effort costs, however, the Free-Choice condition actually did confer control over effort costs as participants learned to map blaster cues to their associated effort requirements. Given that the Restricted-Choice condition in Experiments 2 and 3 did not enhance either preference for blaster cues or performance on associated effort trials, we sought to determine whether the provision of Free-Choice would increase perceptions of control to enhance motivational outcomes for freely chosen effort trials. In line with our second general hypothesis, we generally predicted that motivational outcomes would be enhanced correspondent to the level of control conferred by the choice condition (Free-Choice > Restricted-Choice > No-Choice). However, the three choice conditions could not be fully dissociated for the preference data, because the Free- and Restricted-Choice conditions utilized the same set of blaster cues and preference was calculated via pre- and post- game ratings of the cues (see Method for further details). Therefore preference could only be determined for the collapsed Choice condition (encompassing both Free- and Restricted-Choice). Thus for preference data, we hypothesized that Choice trials would be preferred over No-Choice trials. Furthermore, as the only difference between the two conditions comprising the collapsed Choice condition was whether the blasters options presented were from the same (Restricted-) or different (Free-) effort categories, we further anticipated that perceptions of control elicited by Free-Choice trials would generalize to the Restricted-Choice trials as well. Therefore, for the performance data, we hypothesized that in addition to enhanced performance in the Free-Choice condition, the Restricted-Choice condition would also be associated with enhanced motivation (compared to the Restricted-Choice condition in Experiments 2 and 3) due to the context of Free-Choice. Additionally, in line with our third general hypothesis, we also predicted that any observed deficits in motivational outcomes would occur at the intersection of the lowest levels of personal control (only No-Choice in this experiment) and the highest effort costs (High-Effort).

Experiment 4 Method

Thirty-three participants were recruited; however one participant was excluded due to not following directions. Thus, the final sample included thirty-two participants (22 females; mean age = 21.7 years, *SD* = 4.21). To control for base preferences for blaster color, participants were randomly assigned to one of four counterbalanced sets of stimuli. The color-hue category (green vs. orange) distinguished No-Choice from Choice (Free- and Restricted- combined) and the color-value category (lightness vs. darkness) distinguished levels of effort (Low-Effort, High-Effort); within these constraints, conditions were fully counterbalanced across color categories, creating four sets of blaster stimuli (see Figure 1). Across a single block there were 16 No-Choice trials (eight Low-Effort and eight High-Effort) and 16 Choice trials. Within the Choice trials, there were eight Restricted-Choice trials (four Low-Effort and four High-Effort) and eight Free-Choice trials (in which participants could freely choose Low- or High-Effort). Blocks repeated four times across the game resulting in a total of 32 No-Choice, Low-Effort trials, 32 No-Choice, High-Effort trials, at least 16 Choice, Low-Effort and at least 16 Choice, High-Effort trials, plus an additional 32 Free-Choice trials (with effort level dependent on participants' choices).

Experiment 4 Results

Preference: Figure 6A shows the change in preference for all choice and effort conditions from before to after participants played the Variable Choice and Effort With Free Choice Game. Bonferroni-corrected pairwise t-tests of pre-game preference ratings did not reveal any significant differences among the four conditions. Across both choice conditions, preference for Low-Effort blasters increased from pre-game (Choice, Low: M = 4.52, SD = 1.45; No-Choice, Low: M = 5.13, SD = 1.43) to post-game (Choice, Low: M = 5.73, SD = 1.31; No-Choice, Low: M = 6.02, SD = .68) and preference for High-Effort blasters decreased from pre-game

(Choice, High: M = 4.63, SD = 1.41; No-Choice, High: M = 4.72, SD = 1.63) to post-game (Choice, High: M = 2.63, SD = 1.12; No-Choice, High: M = 2.75, SD = 1.45). Across all conditions, changes in preference were significantly different from zero (all p's < .005). A 2x2 ANOVA of the change in preference data, indicated a main effect of effort ($F(1,31) = 46.98$, $p < .0001$), such that the increase in preference for Low-Effort blasters was significantly different (using the Bonferroni-corrected significance threshold of $p = .025$) than the decrease in preference for High-Effort blasters for both the Choice ($t(31) = 6.87$, $p < .0001$) and No-Choice ($t(31) = 5.06$, $p < .0001$) conditions. However, there was no main effect of the choice ($F(1,31) = .30$, $p = .59$) and no interaction of the choice and effort conditions in the before to after game preference change ($F(1,31) = .44$, $p = .511$).

Performance: Figure 6B shows success rates for all conditions in the Variable Choice and Effort With Free Choice Game. Success rates were high (above 93%) across all six conditions (Free-Choice, Low-Effort: M = 96.2%, SD = .08%; Free-Choice, High-Effort: M = 95.7%, SD = .11%; Restricted-Choice, Low-Effort: M = 97.3%, SD = .06%; Restricted-Choice, High-Effort: M = 95.5%, SD = .08%; No-Choice, Low-Effort: M = 95.8%, SD = .07%; No-Choice, High-Effort: M = 93.6%, SD = .08%). The 3x2 ANOVA including all three choice conditions and both effort conditions did not reveal any significant differences among the conditions ($F(1,31) = .233$, $p = .793$).

We also decided to analyze the performance data in a 2x2 fashion with Free-Choice trials excluded. This analysis was performed for several reasons: (1) we wanted to perform an analysis comparable to that in Experiment 3; (2) treating Free-Choice and Restricted-Choice as separate conditions may not have accurately reflected participants' experience of the game as both types of choice were instantiated utilizing the same set of blaster cues and thus may have been indistinguishable to participants; (3) collapsing Free- and Restricted-Choice into a single Choice condition was not appropriate as the Free-Choice condition involved a higher percentage of Low-Effort trials than the two other choice conditions (in which Low- and High-Effort were balanced); and (4) average success scores were computed from fewer data points in the Free-Choice, High-Effort condition as participants tended not to choose High-Effort blasters when given a Free-Choice and three subjects never chose a High-Effort blaster during a Free-Choice trial. Thus, performance was also analyzed in a 2x2 fashion in which Free-Choice trials were excluded. While this analysis paralleled the one performed in Experiment 3, the key difference is that the Free-Choice condition may have still exerted an influence on the context in which participants experienced Restricted-Choice in Experiment 4.

The 2x2 ANOVA with factors of Restricted- and No-Choice and Low- and High-Effort revealed a main effect of effort ($F(1,31) = 5.57$, $p = .025$). While success rates for Low-Effort blasters were greater than success rates for High-Effort blasters, post hoc pairwise comparisons did not reach significance for either the Choice ($t(31) = 1.72$, $p = .095$) or the No-Choice ($t(31) = 1.94$, $p = .062$) conditions.

Further, the 2x2 ANOVA revealed a main effect of choice ($F(1,31) = 6.64$, $p = .015$), such that success rates for the Choice condition were significantly greater than success rates for the No-Choice condition for High-Effort ($t(31) = 2.43$, $p =$

.021), while this difference was not significant for Low-Effort ($t(31) = 1.35$, p = .187). There was no interaction of the choice and effort factors ($F(1,31) = .13$, p = .725).

As we had an a priori hypothesis that the greatest motivational decrements would be observed in the No-Choice, High-Effort condition, we also examined performance differences in this condition relative to other conditions. While the decrement in performance in the No-Choice, High-Effort condition was significant when compared to Choice, High-Effort (as already stated) and Choice, Low-Effort ($t(31) = -4.27$, $p < .005$) conditions, this decrement in performance did not reach the Bonferroni-corrected significance threshold of $p = .017$ when compared to the No-Choice, Low-Effort ($t(31) = -1.94$, $p = .062$) condition.

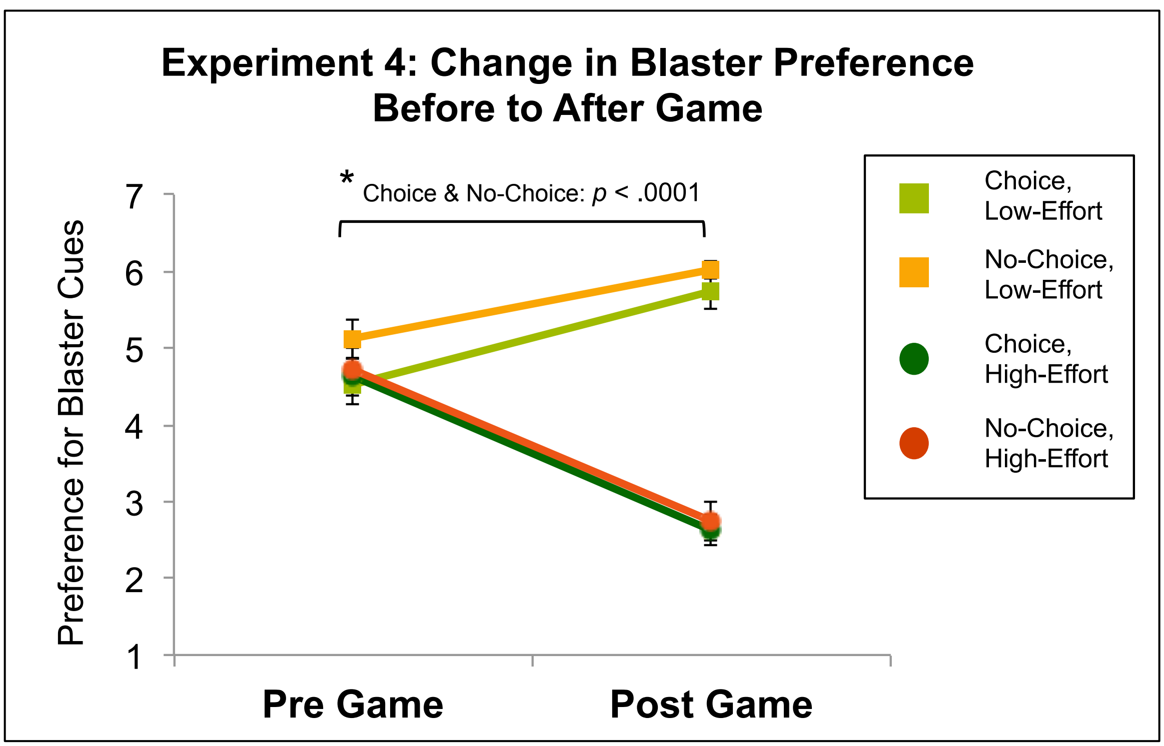


Figure 6A: Change in preference for conditions in Experiment 4: the Variable Choice and Effort With Free Choice Game. A 2x2 ANOVA indicated a main effect of effort (p <.0001), such that the increase in preference for Low-Effort blasters was significantly different than the decrease in preference for High-Effort blasters for both the Choice and No-Choice conditions (both p's < .0001) .

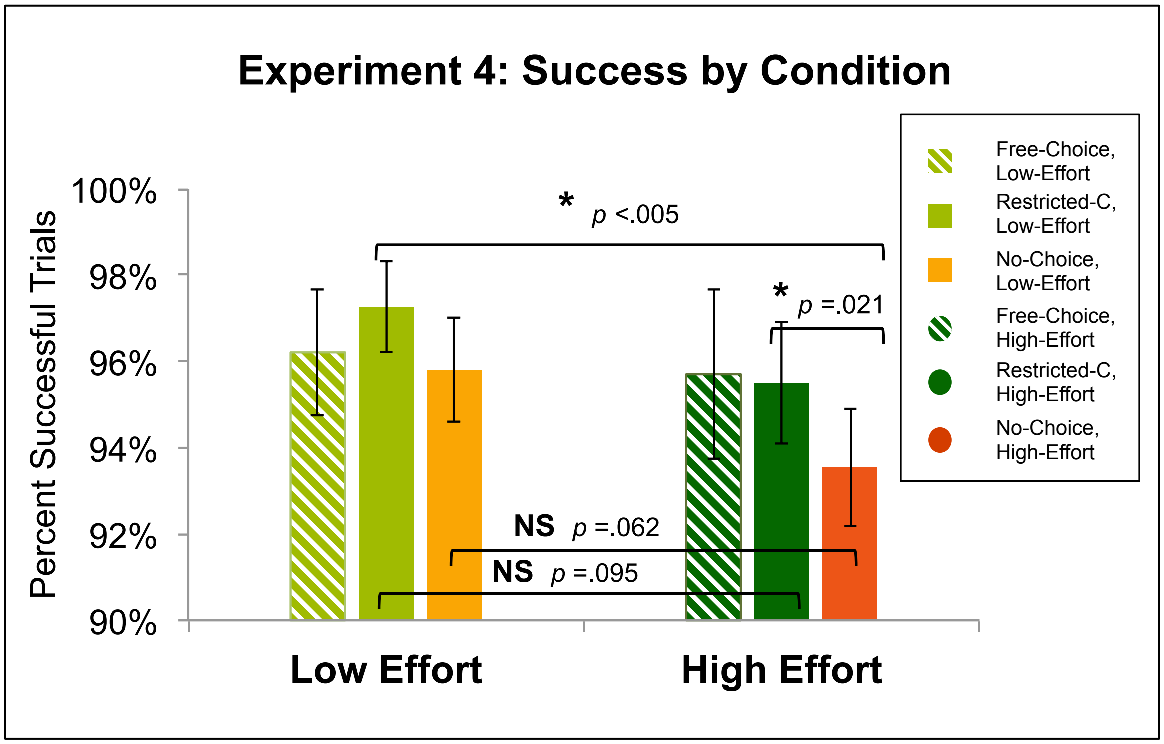


Figure 6B: Success rates for conditions in Experiment 4: the Variable Choice and Effort With Free Choice Game. While the 3x2 ANOVA did not produce significant differences, a 2x2 ANOVA excluding the Free-Choice condition, produced a main effect of effort (p = .025). However, pairwise comparisons probing the main effect of effort did not reach significance for either the Choice (p = .095) or the No-Choice (p = .062) conditions. The 2x2 ANOVA also produced a main effect of choice (p = .015), such that success rates for the Choice condition were significantly greater than success rates for the No-Choice condition for High-Effort (p = .021), while this difference was not significant for Low-Effort (p = .187). Due to our a priori hypothesis, we further demonstrated that success rates in the No-Choice, High-Effort condition were significantly different than in the Restricted Choice, Low-Effort (p < .005) condition. All error bars represent standard error of the mean.

Experiment 4 Discussion

This experiment employed conditions of No-Choice, Restricted-Choice and Free-Choice across both Low-Effort and High-Effort trials to determine the combined impact of varying both costs and control on preference and performance. The design of Experiment 4 was similar to Experiment 3, except that while half of the Choice trials were Restricted-Choice trials (split evenly between Low- and High-Effort), the other half of the Choice trials were Free-Choice, in which participants were offered a choice between a High- and a Low-Effort blaster and were allowed to freely choose which they preferred. Given that the Restricted-Choice condition in Experiments 2 and 3 did not enhance preference or performance, we sought to determine whether the provision of Free-Choice, a choice that did actually confer control over effort costs, would enhance motivational outcomes. In line with our second general hypothesis, we predicted that outcomes would be enhanced correspondent to the level of control conferred by the choice condition. While preference data did not support this hypothesis, it did replicate the findings from Experiments 1, 2 and 3, with an increase in preference for Low-Effort cues, a decrease for High-Effort cues, and no apparent effect of Choice (representing both Free- and Restricted-Choice) on preference.

Performance data, however, did demonstrate main effects of not only effort but also choice. While the 3x2 analysis of success rates did not yield significant effects, this may have been due to these two choice conditions being indistinguishable to participants as they were both represented by the same set of blaster cues. As we wanted to perform an analysis that was comparable to that in Experiment 3 and remove any bias resulting from the Free-Choice condition having a higher percentage of Low-Effort trials than either of the other choice conditions, we also analyzed performance data in a 2x2 fashion, excluding Free-Choice trials.

Replicating results from Experiments 1 and 3, and supporting our first hypothesis, participants performed significantly better on Low- than on High-Effort trials.

Importantly, results from this analysis also supported our second hypothesis that motivational outcomes would be enhanced correspondent to the level of control conferred by the choice condition. Experiment 4 was the only experiment to offer actual control over effort costs and the only experiment in which participants performed better on the effortful trials in which they were given a choice compared to those in which they were not. Further, the positive motivational effect of choice was present even when Free-Choice trials were excluded. Thus, even when the direct influence of the Free-Choice trials, along with the lower effort advantage conferred by Free-Choices, was removed from the analysis, the motivational effect of personal control uniquely offered in this experiment still had a positive impact on the experience of the illusory control condition, enhancing choice-linked performance in what was a previously unmotivating condition.

Thus, in contrast to Experiments 2 and 3, in which Restricted-Choices were offered alone and there was no positive effect of choice, the results of Experiment 4 suggest that the provision of occasional free choices may be sufficient to provide motivational benefits in an effortful, and otherwise uncontrollable context. While participants were not more successful on Free-Choice trials than on Restricted-Choice trials, this may have been due to both the Restricted- and the Free-Choice trials utilizing the same set of blaster stimuli. Given the subtle differences between the two Choice conditions, it is possible that participants may not have explicitly distinguished between Free- and Restricted-Choice conditions, and rather, perceived all the choices as conferring some control over effort costs.

In line with our third hypothesis, and consistent with results from Experiment 3, we again observed the lowest levels of performance in the No-Choice, High-Effort condition, supporting the notion that low levels of personal control and high levels of costs may combine to undermine motivation (Fleming, *et al.,* 2010; Iyengar & Lepper, 2000; Moller, *et al.*, 2006; Patall, 2012; Reeve, *et al.*, 2003).

While choosing significantly enhanced performance, preference remained unaffected by the provision of choice. It is possible that despite perceptions of control positively affecting performance on immediately subsequent trials, this apparent motivational effect was not integrated across trials to create a stronger preference for cues associated with choice. It is also possible that effort costs undermined the preference for having a choice but not choice-linked performance. Future research might investigate possible dissociable effects of perceptions of control on performance and preference and how these effects may dynamically interact with one another.

General Discussion

Across four experiments we tested the hypotheses that motivational outcomes would be: (I) enhanced when lower compared to higher effort was required, (II) enhanced correspondent to the level of control conferred by choice, and (III) diminished when the lowest levels of personal control intersected with the highest effort requirements. These hypotheses were generally, although not always specifically supported. Our first hypothesis was premised upon effort discounting theory, which holds that effort decreases the utility of related outcomes (Botvinick, *et al.,* 2009; Kurniawan, *et al.,* 2013). In line with effort discounting, we hypothesized that participants would both prefer and perform better when lower effort was required. This hypothesis was supported for both preference and performance data across all experiments in which effort was varied (1, 3, 4), even though the time allowance for the key pressing requirement was calibrated across all levels of effort such that the requisite effort was achievable in both conditions.

Our second hypothesis, that motivational outcomes would be enhanced according to the level of control offered by the choice condition was generally supported, although the threshold for perceived control was not what was predicted. Much research has demonstrated the motivational benefits of personal control and even of illusory control (Alloy & Abramson, 1979; Leotti, *et al.,* 2010, 2015; Langer, 1975; Langer & Rodin, 1976; Rodin & Langer, 1977; Ryan & Deci, 2000). Thus, we predicted that our Restricted-Choice condition, which provided only illusory control, would be sufficient to enhance motivational outcomes. However, our results indicated that illusory control alone may not be sufficient to enhance motivation in a context of effort-based decision making. In experiments 2 and 3, in which Restricted-Choice was the only choice offered to participants, no beneficial effects of choosing were observed in terms of preference or performance. In experiment 4, however, when Restricted-Choice was offered in a context of Free-Choice, with the two conditions subtly and likely unnoticeably mixed, Restricted-Choice led to positive effects on performance. This positive effect of illusory control in the context of real control was demonstrated despite Free-Choice trials (and thus the lower effort advantage conferred by Free-Choice) being removed from the analysis. Thus, while illusory control alone was not enough to motivate performance improvements linked to choosing, illusory control in the context of real control did boost performance.

Based on evidence that low levels of personal control and heightened costs associated with a decision may combine to produce a particularly damaging coalition, our third hypothesis predicted that we would observe the largest decrements in motivation when factors of personal control were at their lowest and costs were at their highest. This hypothesis was largely confirmed in both of the two experiments that tested the combined effects of differing levels of control and cost together (Experiments 3 and 4). While the factors of choice and effort in these experiments did not produce a formal interaction effect, our a priori hypothesis led us to probe the pairwise comparisons. Across both experiments, the No-Choice, High-Effort condition was associated with significantly lower levels of performance than in other conditions, suggesting that low personal control and a context of high costs lead to poorer motivational outcomes. Together the pattern of results across all of our experiments provides support for our overarching hypotheses, suggesting that control provides benefits to motivation while costs undermine these benefits and that there may be an interplay between control and cost factors to influence motivational consequences in effort-based decision making.

While considerable research has demonstrated the motivational benefits of simply making choices even when choices do not confer actual control (Bhanji, & Delgado, 2014; Bhanji, *et al.,* 2016; Cockburn, *et al.,* 2014; Murayama, *et al.,* 2015;

Murty, *et al.,* 2015; Patall, *et al.,* 2008), our results suggest that deriving positive effects from illusory control may be a fragile effect that may be easily unraveled by factors such as effort costs. This is consistent with other work suggesting there may be a framing effect for decisions such that choice may lose its desirability and advantageous features in the context of high costs, such as effort costs (Fleming, *et al.,* 2010; Gourville & Soman, 2005; Iyengar & Lepper, 2000; Kool, *et al.,* 2010; Leotti & Delgado, 2014; Samuelson & Zeckhauser, 1988; Vohs, *et al.,* 2008). Thus the present research attempted to disambiguate how control and cost factors affect the motivational consequences of choosing, across differing levels of personal control, within an effortful task. Consistent with the work of Moller and colleagues (2006) and Reeve and colleagues (2003) who empirically assessed the role of personal control in the motivational effects of choice, we also found that while mere choice devoid of opportunities for personal control did not enhance motivation, choices that provide opportunity for actual control did. Our study extends previous work by exploring not only the motivational effects of choices offering differing levels of control but also how degrees of control may interact with varying levels of effort to influence motivation. Our study demonstrated that in an effortful context, illusory control alone may not be sufficient to overcome effort costs to boost motivation. Instead, given the same high cost context, it may be necessary to have choices that provide actual control in order to motivate performance in the face of effort costs. Furthermore, the positive performance benefit of real personal control appeared to generalize to performance on trials linked to choices that did not actually provide control, demonstrating that conditions of illusory control can be contextualized by opportunities for free choice to enhance the motivational effects of illusory control when it was otherwise not associated with increased motivation.

The present study also had several limitations. First, while we directly manipulated personal control across our choice conditions, no self-report manipulation checks were included and, thus, we can only infer that it was indeed perceptions of personal control that led to the observed motivational effects. Future work should include self-report measures to ensure that perceptions of personal control match corresponding manipulations of control. Second, while evidence that the worst rates of performance occurred under conditions of highest effort and lowest personal control, does suggest that effort costs undermine the positive benefits of making choices, a no-effort condition would have allowed us to directly contrast the effects of choice within and without a context of effort. To more directly parse the effects of personal control and effort costs, future studies should utilize a broader range of effort costs including a no-effort condition. Third, our results can only go so far towards answering the question of whether the effect of choice as a motivational event in and of itself is impacted by factors of control and cost. While our results directly demonstrated a difference between No-Choice and Choice conditions, we only indirectly demonstrated a difference between a Choice condition offering no actual control (Experiment 3) and a Choice condition that occasionally offered control (Experiment 4). Future research could try to parametrically modulate personal control in decision making scenarios to directly demonstrate a point at which personal control might mediate between the positive versus negative effects of making a choice.

The current study also has significant implications for the study of personal control and effort and their applications in psychiatry, counseling, motivation science and education. For example, deficits in perceiving personal control and exerting effort have been implicated in a range of psychiatric conditions from depressive disorders to schizophrenia (Maeda, *et al.,* 2013; Späti, *et al.,* 2015; Treadway, *et al.*, 2012; 2015) and both personal control and effort allocation are highly relevant to education science (Katz & Assor, 2006; Patall, 2013). While our study provided evidence that a context of effort costs may negate the benefits of making a choice, future research should explore whether the cognitive effort demanded by more computationally difficult choices (e.g., Kool, *et al.,* 2010) undermines the positive benefits of choice. Additionally, given that developmental stage and psychopathology can impact willingness or ability to expend cognitive effort in decision making (Leykin, *et al.,* 2011; Luke, *et al.,* 2012; Westbrook, *et al.,* 2013;), future research could examine how the altered decision costs for these groups influence the motivational outcomes of choice.

While the beneficial effects of personal control have been repeatedly demonstrated across domains from performance on simple tasks and educational activities to coping with stress (e.g., Bhanji, *et al.,* 2016; Murayama, *et al.,* 2015; Murty, *et al.,* 2015; Patall, 2013), many research studies operationalize personal control via low-cost, simple choices that may or may not offer actual control. Given that effort is a ubiquitous requirement for nearly all goal achievement, and given that differing levels of control may be required to enhance motivation in the face of increasing effort costs, it is important to examine the conjoint effects of control and cost factors in decision making, particularly given that in some circumstances (e.g., therapy, educational interventions) effort costs may necessarily be high and personal control may be difficult to enact. Our results shed light on the subtleties of how these factors may interact, suggesting that when costs are high, mere choice and illusory control alone may not suffice to enhance motivation. Rather, under effort requirements, opportunities to exert real control over the situation may be necessary to boost motivation. At the same time, our results suggest that conditions of illusory control may be transformed by even intermittent occasions of actual control, suggesting that perceptions of control rather than complete and total personal control may be sufficient to motivate.

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Attributions:

HS-T & ET conceptualized the project. HS-T programmed the experiments, oversaw data collection, and analyzed the data. HS-T drafted the manuscript. ET & JAR provided critical feedback on the manuscript.

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MANUSCRIPT 1

SUPPLEMENTARY MATERIAL

Supplementary Method

Choice and Effort Conditions

The full set of conditions included two levels of effort costs (Low-Requirement and High-Requirement, as defined by the number of key presses required) and three different levels of control provided across choice conditions (Free-Choice, Restricted-Choice, and No-Choice, as defined by the level of control over effort costs conferred by the blaster options offered). In the No-Choice condition, a single blaster was offered individually without another option. In the Free- and Restricted-Choice conditions, two blasters were offered for participants to choose between. The key difference between these two choice conditions was that in the Free-Choice condition (Experiment 4 only), participants were offered a choice between one High- and one Low-Requirement blaster and were allowed to freely choose which they preferred to use, while in the Restricted-Choice condition, participants were offered a choice between two different colored blasters in the same effort category (e.g., a choice between two High-Requirement blasters), such that their choice actually had no effect on the amount of effort required to fill the charge bar. Thus, the Restricted-Choice condition offered only illusory control. Subtle blaster color categories represented the choice and effort conditions, with two similarly colored exemplar blasters in each category (See Figure 1). Having two exemplar blasters per condition allowed implementation of the Restricted-Choice condition with a choice between two blasters from the same effort category. Low-Requirement blasters required a random number of presses between 11 and 20 to fill the effort bar, while High-Requirement blasters required a random number of presses between 21 and 30.

All experimental choice and effort contingencies were imbued with some ambiguity in order to facilitate subjective inferences regarding perceptions of control. Specifically, three design features were implemented: (1) no explicit information was given to participants about the effort levels or the nature of the choice conditions; rather, participants' only information about experimental conditions was acquired through playing the game; (2) blaster cues representing choice and effort conditions were organized into subtle color categories and the mapping of choice and effort contingencies onto the color scheme was also not explicitly stated; and (3) effort requirements (number of presses) were randomly drawn from ranges that were only subtly different between the Low- and High-Requirement conditions so that effort contingencies were somewhat uncertain. We expected that these ambiguities would facilitate subjective inferences regarding the level of control conferred by the choice conditions, particularly in the Restricted-Choice (illusory control) condition.

Task

See Figure 2 for a representative schematic of a single trial within the tasks. At the beginning of each trial, a blaster cue screen presented blaster options for the trial (two options for Choice conditions or a single blaster for the No-Choice condition) was displayed for an unlimited time until the participant responded to activate the indicated blaster. Next, a jittered fixation was displayed before the "charge bar" appeared so that participants could not exactly predict the charge bar onset, thus minimizing preemptive first presses. When the effort period began at the appearance of the charge bar, participants were to begin making fast repetitive key presses until the charge bar was filled. The charge bar was the same size for Low- and High-Requirement trials but filled at different proportions, depending on a pre-set number of presses required on a given trial. The number of required presses was not explicitly indicated to participants but could be implicitly estimated from the proportion of the bar filled with each press.

In order to standardize subjective difficulty, the task was individually calibrated to each participant's performance on a pre-game training task (Knutson, *et al.,* 2000; Mangels, *et al.,* 2006), such that success rates were high across all conditions. The specific time allotted for the charge bar on a given trial was determined by the formula $c_t = r(p_t)(1.1) + (r_t)(1.1)$, where c_t is allotted charge time, r is the required number of presses for a given trial, and r_t and p_t are the participant's average pre-game reaction time to make an initial key press, and the participant's average pre-game time between successive presses, respectively. Average individual pre-game reaction time values (both r_t and p_t) were both multiplied by 110% so that the task would be challenging but not impossible for participants. If the participant successfully completed the required number of presses in the allotted time, then the blaster "fired" and a "pow" symbol was displayed for 500ms, followed by an alien with an "X" on its head, displayed for 1250ms, indicating the alien was successfully "blasted." If the required presses were not completed in the allotted time, then a "No" symbol was displayed for 500ms, followed by an alien icon, displayed for 1250ms, indicating the alien escaped.

Stimuli Sets

All eight blasters were used in each experiment, either with all eight used within each participant (Experiments 3 and 4) or with a set of four stimuli used within each participant, counterbalanced such that all eight stimuli were used between participants (Experiments 1 and 2). See Figure 1 for an example of the blaster stimuli sets used in each experiment. Across all experimental stimuli sets, color-hue category (green vs. orange) represented choice conditions and color-value category (lightness vs. darkness) represented effort conditions; within these constraints, conditions were fully counterbalanced across color categories, such that for each experiment, conditions were mapped onto color categories to create four versions of blaster stimuli, used across participants. For example, in Experiment 3 light green blasters represented Choice, Low-Requirement for one participant's game and No-Choice, High-Requirement for another participant's game. Thus, we aimed to minimize the chances of differences (both within an experiment and between experiments) resulting from preference for one color blaster over another. In experiments in which a condition was not used (Choice in Experiment 1; High-Requirement in Experiment 2), one respective color category was excluded from each of the stimuli sets used in that particular experiment (e.g., in Experiment 1, only green *or* orange blasters were used for an individual participant's set of stimuli) The specific mapping of condition to color category that was used in each experiment is detailed in the respective experiment.

Supplementary Results

Potential Effects of Learning

The task used in the present study was designed to minimize the role of learning such that learning the choice and effort cues would not be necessary for successful performance. Several aspects of the design minimized the role of learning in achieving successful performance: (1) participants were given substantial training on how to fill the "effort bar" before the task began (although they were not trained on the choice and effort cues), (2) the effort bar was displayed on screen during the trial showing participants in real-time how quickly they were filling the bar (3) the amount of time allotted for filling the bar across all conditions was determined by the same formula. Nevertheless, it is possible that participants learned the cues at different rates and that this affected success rates across the different conditions.

Our task was not designed to measure learning, and it is not possible to attribute unsuccessful trials to participants not having learned the effort contingencies because we cannot rule out the possibility that participants were simply not motivated to complete the trial. Thus, it might not be possible to assess learning on a trial-by-trial basis. However, we can address the possibility that learning, rather than motivation, may have influenced the differential performance rates across the conditions. To examine this possibility, we explored success rates across blocks (Fig S1) and within block 1, comparing the first 16 trials to the second 16 trials (Fig S2). Figure S1 shows that, for all effort conditions in all experiments, success rates were above 95% in the first block and either remained constant or diminished across successive blocks. Similarly, Figure S2 shows that, for all effort conditions in all experiments, success rates were well above 95% even in the first 16 trials of the games. For all effort conditions in all experiments, except for the High-Requirement condition in Experiment 1, success rates diminished from the first 16 trials of the game to the second 16 trials of the game. In the High- Requirement condition in Experiment 1, success rates increased slightly from 97.6% (SD = 7.2%) in the first 16 trials of the game to 98.3% (SD = 4.4%) in the second 16 trials of the game, however, this difference was not significant ($t(36) = -.627$, $p = .535$). From this pattern of results we can infer that participants learned how to successfully complete trials in all conditions very early in the games and that diminishing success rates across successive blocks were not due learning.

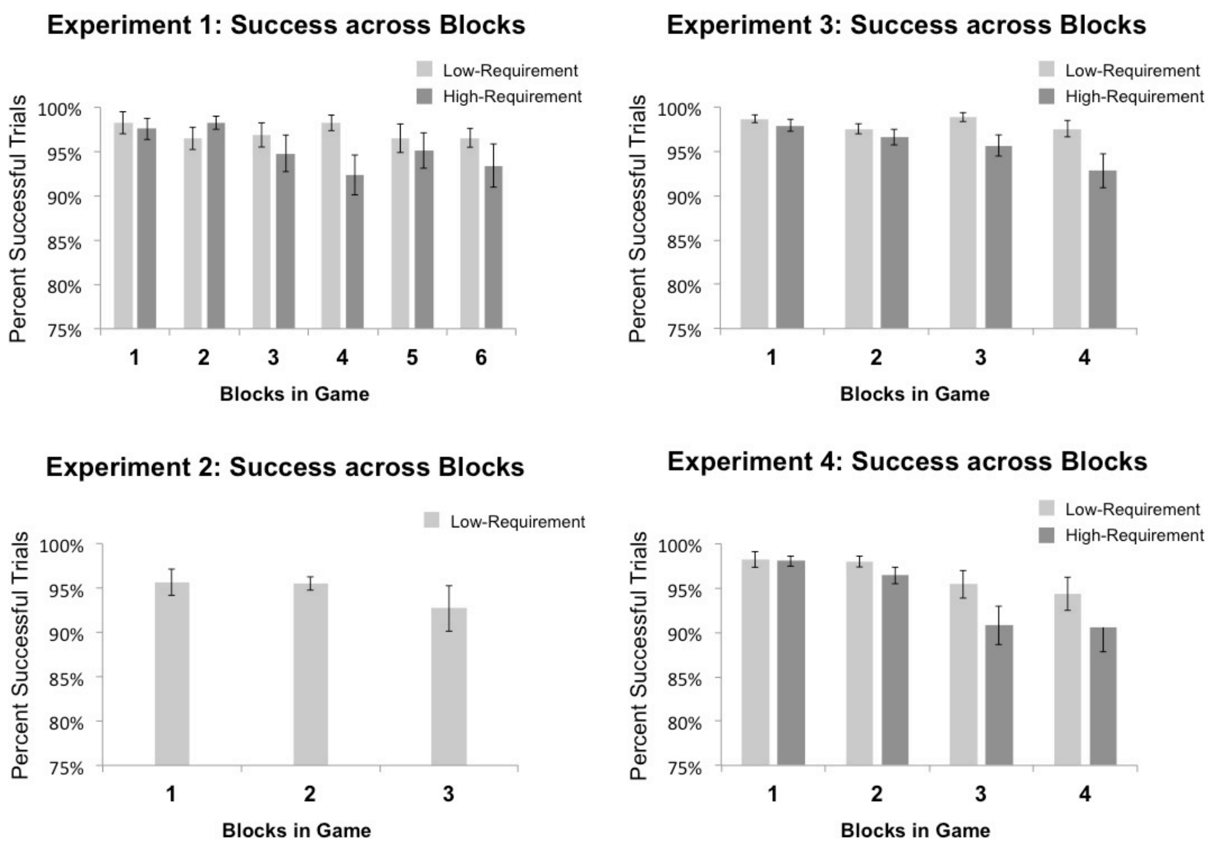

Figure S1:
Success rates across blocks of the game for all experiments.

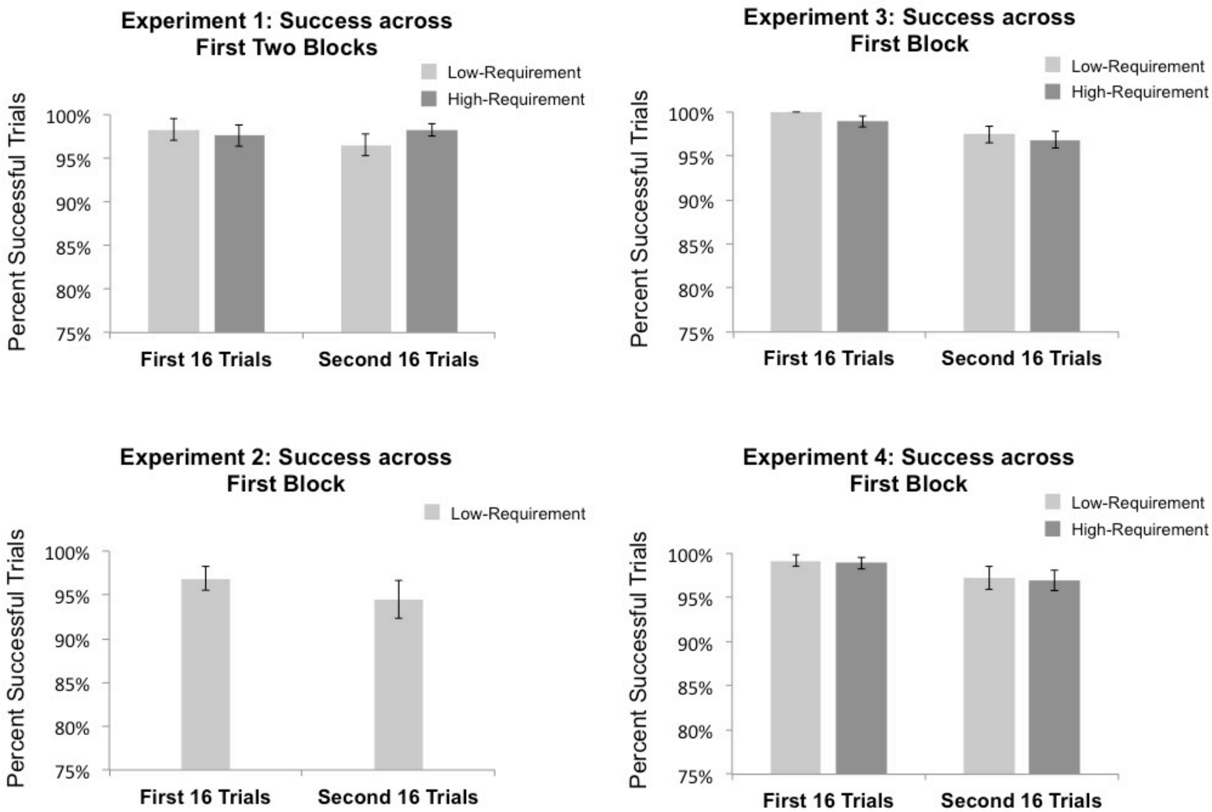

Figure S2:
Success rates across the first and second 16 trials of the game for all experiments.

Supplementary References

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MANUSCRIPT 2

Choice bias: An objective measure of personal control responsivity

Abstract

Previous research strongly suggests that anhedonia is characterized, not only by diminished responsivity to reward, but also by deficits in responsivity to opportunities to exert personal control. However, research in this domain is limited by a lack of objective, behavioral measures of personal control responsivity. To address this, the current paper presents the Choice Bias Task, which uses signal detection methods to elicit a behavioral bias towards opportunities to exert perceived control via choice. Taking a transdiagnostic approach, the Choice Bias Task was administered to a convenience sample of undergraduate students. In line with previous research demonstrating the reinforcing properties of choice (as a proxy for personal control), participants showed a Choice Bias, or an overall bias towards gaining choice opportunities. Additionally, in line with predictions, Choice Bias was associated with measures of reward responsivity and anhedonia suggesting that the Choice Bias Task offers a novel tool to measure responsivity to perceived control and objectively characterize appetitive personal control deficits found in anhedonia.

Introduction

Perceived control, the general belief that one can exert influence to gain desired outcomes, is one of the most influential constructs in psychology and has been studied extensively across decades of research (Leotti, Iyengar & Ochsner, 2010; Reich & Infurna, 2016; Ryan & Deci, 2017). At the trait-level, a strong sense of control is thought to be essential for wellbeing and is robustly associated with better mental health (Ghane, Sullivan-Toole, DelGiacco & Richey, 2019; Leotti, et al., 2010; Reich & Infurna, 2016; Ryan & Deci, 2000). Diminished perceptions of control, on the other hand, have been observed across a range of internalizing psychopathologies (e.g., Benson, et al., 2014; Cloitre, et al., 1992; Ghorbani, et al., 2008; Hofmann, 2005; Mirowsky & Ross, 1990; Stapinski, Abbott & Rapee, 2010) and may represent a general psychological vulnerability for anxiety and mood disorders (Barlow, 2000, 2004). Thus, the construct of personal control appears to have great potential to provide insight into internalizing pathologies. In particular, deficits in personal control show a strong relationship with anhedonia, a cluster of psychiatric symptoms that includes reduced reactivity to rewards and a lack of motivation and positive affect. Currently, however, the anhedonic phenotype is not well characterized in relation to deficits in appetitive personal control.

Improved clinical phenotyping is critical for the advance towards precision medicine and the shortcomings of categorical diagnoses for psychopathology are increasingly recognized (Insel, 2017; Torous, Onnela & Keshavan, 2017). In response, there has been a shift towards conceptualizing psychiatric dysfunction transdiagnostically, where symptoms represent a pathological degree of deviance along a continuum of human functioning within a given domain (Adam, 2013; Insel, et al., 2010; Krueger & Eaton, 2015). Despite extensive research demonstrating that perceived control is a meaningful functional dimension of mental health and illness, this knowledge has stalled along the path to therapeutic intervention. Further, the construct of perceived control has been neglected in recently developed dimensional taxonomies of psychopathology (e.g., Kotov, et al., 2017; National

Institute of Mental Health, 2020). Nevertheless, strong evidence substantiates the link between deficient reward responsivity and diminished responsivity to personal control, calling for progress in characterizing personal control deficits within the anhedonic phenotype.

First, personal control is integrally related to, and appears to amplify, reward processes, which themselves have been identified as core constructs relevant to mental illness (PVS Work Group, 2011). There is robust correspondence between perceptions of control and reward responsivity, which may lie, at least partially, in the reward-related properties of perceived control itself (Leotti, Cho & Delgado, 2015; Ly, et al., 2019). Leotti and colleagues have argued that exerting control by making choices is inherently desired and reinforcing (Leotti, et al., 2010), and this supposition has been largely supported across a number of studies that utilize the provision of choice in an experimental context to induce a sense of control (e.g., Leotti & Delgado, 2011; Patall, Cooper & Robinson, 2008; Wang & Delgado, 2019). These studies have shown that making even simple choices, and thus enacting a personal sense of control, enhances reward circuitry activation both when anticipating a choice (Fujiwara, et al., 2013; Leotti & Delgado, 2011; 2014) and when receiving an outcome linked to an individual's own choice (Mühlberger, et al., 2017; Tricomi, Delgado & Fiez, 2004). Further, individuals who report a stronger sense of control show increased reward circuitry activation when given the opportunity to make a choice (Wang & Delgado, 2019). Together, this work provides strong evidence that perceived control, via personal choice, can augment different aspects of reward processing, increasing the value of rewards associated with choice.

Second, the relation between reward- and control-related processes may be bi-directional, as evidence has also demonstrated that reward (and positive valence processes more generally) enhance perceptions of control and desire for control. For example, reward-sensitive individuals show greater recruitment of cognitive control-related neural regions in response to making a choice, suggesting trait-level reward sensitivity invigorates responsivity when there is an opportunity for personal control (Cho, Smith & Delgado, 2016). Further, the presence of reward information increases the sense of control in ambiguous action-outcome tasks, while loss information attenuates the sense of control (Aarts, et al., 2012; Dewey, Seiffert & Carr, 2010; Gentsch, et al., 2015; Takahata, et al., 2012; Yoshie & Haggard, 2013; for review see Gentsch & Synofzik, 2014). Additionally, individuals who experienced greater pride in response to exerting control showed an increased preference for control, even when control imposed a monetary cost (Stolz, et al., 2019).

Third, there is a distinct, transdiagnostic relationship between perceived control and anhedonia. Disorders with prominent anhedonic features, including depression, social anxiety, and schizophrenia (Kashdan, 2007; Whitton, Treadway & Pizzagalli, 2015), not only show deficits in behavioral and neural response to rewards (Huys, et al., 2013; Whitton, Treadway & Pizzagalli, 2015), but also exhibit disruptions in the sense of control across multiple measurement modalities (Alloy & Abramson, 1979; Cloitre, et al., 1992; Franck, et al., 2001; Hofmann, 2005; Maeda, et al., 2013; Msetfi, Murphy & Simpson, 2007; Romaniuk, et al., 2019; Späti, et al., 2015). Further, evidence spanning multiple disorders suggests diminished perceived control is *specifically* related to symptoms of anhedonia. In schizophrenia, a disorder that often, but not always presents with anhedonia (negative symptoms), reduced perceptions of control (or reduced self-efficacy, a closely related construct) were associated with the anhedonic phenotype specifically (Cassar, Applegate & Bentall, 2013; Grant & Beck, 2009; Maeda, et al., 2013). Another study found that anhedonia, but not general depressive symptoms, mediated the relationship between perceived stress and attenuated perceptions of control (Bogdan, et al., 2012). Positive emotionality and perceptions of control are also correlated in the general population; in a large, nationally representative sample of U.S. adults, low perceived control had a specific association with diminished positive affect, regardless of self-reported anxiety symptoms (Ghane, et al., 2019). Further, among young, healthy female participants, experimental induction of depressed and elated mood produced increased external and internal perceptions of control, respectively (Natale, 1978).

Forth, research has begun to delineate certain mechanistic links between perceived control and reward processes. For example, studies using formal quantitative models to examine the effects of personal control on reward processes have shown that making a choice boosts the expected value of associated rewards and increases positive prediction errors (Cockburn, Collins & Frank, 2014; Wang & Delgado, 2019), two neurocomputational parameters linked to reward wanting and reward seeking (Berridge, 2007; Pessiglione, et al., 2006), specific aspects of reward processing thought to be at the core of anhedonic dysfunction (Thomsen, 2015; Treadway & Zald, 2011). Additionally, reward processes and perceived control rely on congruous neural circuitry (Stolz, et al., 2019; Wang & Delgado, 2019), and evidence further suggests that increased perceptions of control, like heightened trait-level reward responsivity, is associated with increased tonic levels of striatal dopamine (Aarts, et al., 2012; Declerck, et al., 2006a; 2006b; Kayser, et al., 2015; Vassena, et. al., 2019). Together, this work suggests that perceiving control and processing rewards are instantiated by overlapping neurobiological substrates, which are putatively compromised in anhedonia (Thomsen, 2015; Treadway & Zald, 2011). While the correspondence between control- and reward-related deficits is apparent, the extent to which disrupted personal control characterizes anhedonic phenotype(s) remains unknown and specific mechanisms linking perceived control to anhedonia remain largely theoretical (MacAulay, et al., 2014). As such, elucidating control- and reward-related processes, and their interactions, is likely to inform causal pathways in the etiology and/or maintenance of anhedonia, particularly given evidence suggesting reciprocal relationships between control and reward.

While there is theoretical promise for the study of personal control to inform reward-related psychological dysfunction, such research is currently limited by the measurement tools available. Personal control is generally measured via self-report, while objective behavioral measures remain lacking, leaving appetitive personal control at the process-level relatively unexamined. In the domain of reward responsivity, process-level, behavioral measures have provided critical insight into the multi-dimensionality of reward response and improved understanding of the anhedonic phenotype (Case & Olino, 2020; Chevallier, et al., 2016; Olino, 2016; Rizvi, et al., 2017; Thomsen, Whybrow & Kringelbach, 2015). In particular, the signal detection-based probabilistic reward task by Pizzagalli and colleagues (2005) has shown great success in characterizing and predicting anhedonia- and depression-related symptom trajectories (Pizzagalli, et al., 2005; 2008; Vrieze, et al., 2013). Similarly, task-based measures of behavioral responsivity to perceived control may increase the granularity of the personal control construct, enable research into how personal control processes relate to other cognitive and affective processes, and further enrich models of anhedonic phenotypes and symptom prognosis.

Thus, we aimed to develop an objective, behavioral measure of responsivity to perceived control. To do this, we modeled our task on Pizzagalli and colleagues' signal detection task, which utilizes probabilistic monetary rewards to elicit individual differences in reward responsivity, as measured by a decisional bias towards a stimulus most often paired with monetary reward (Pizzagalli, et al., 2005). This original task will hereafter be referred to as the 'Monetary Bias Task'. In our adapted task, the 'Choice Bias Task', the monetary rewards used in the original task are replaced with the abstract reward of choice and non-rewarded outcomes are replaced with no-choice. More specifically, in the Choice Bias Task, the choice outcomes present a choice between two stimuli (which had been previously associated with monetary reward) and the no-choice outcomes present a single (previously reward-associated) stimulus, and participants select the outcome stimuli as appropriate. Importantly, receiving a choice, rather than a no-choice, outcome in this task did not confer participants any possibility of earning a greater amount of money. Instead, based on previous research demonstrating that the opportunity to make a choice is inherently rewarding (Leotti, et al., 2010; Leotti &

Delgado, 2011; Ly, et al., 2019), in the Choice Bias Task, the choice *itself* served as a reinforcer.

Given that the opportunity to exert personal control via choice is generally preferred and rewarding (Leotti, et al., 2010; Leotti & Delgado, 2011; Ly, et al., 2019), we hypothesized that (1) participants would show a general pattern of control-seeking as measured by 'Choice Bias', a decisional bias towards a stimulus most often paired with a choice. Given evidence that choice-linked reward responsivity is diminished in depressed individuals (Romaniuk, et al., 2019) and that perceptions of control are diminished specifically in association with anhedonia symptoms, we hypothesized that (2) Choice Bias would be reduced in relation to higher self-reported anhedonia scores. As previous work has shown that a greater preference for choice and greater subjective value for choice are associated with increased perceptions of control (Wang & Delgado, 2019), we hypothesized that (3) Choice Bias would be associated with self-reported perceived control. Finally, given the putative role of diminished reward learning in anhedonia (Thomsen, 2015), we hypothesized that (4) increased Choice Bias on day one would be associated with a higher baseline Monetary Reward Bias on day two.

Method

Participants and Design.

Informed written consent was obtained from 92 adult undergraduate students who completed either the pilot study or the full study. In both studies, participants completed two experimental tasks, the Choice Bias Task (CBT) and Monetary Bias

Task (MBT) across two days, generally no more than one week apart. As detailed in the respective sections below, participants completed the CBT and MBT in a different order in the different studies. Across both studies, the results presented are primarily those from the CBT, as this task was the focus of the present investigation, however, where appropriate comparisons are made between results from the CBT and MBT. Participants were compensated with course credit plus cash bonuses, ranging from approximately $8—$10, based on earnings from two experimental tasks.

Pilot Study.

Twenty-four participants completed the pilot study in which the MBT was completed on day one and the CBT on day two. Three participants failed the attention check questions within self-report measures and were excluded from analyses, resulting in a final sample of 21 participants (13 women; age 19.14 ± .964 years [mean ± SD]).

Full Study.

Sixty-eight participants completed the full study in which the CBT was completed on day one and the MBT on day two. Six participants failed the attention check questions within self-report measures and were excluded from analyses, resulting in a final sample of 62 participants (39 women; age 19.37 ± 1.13 years [mean ± SD]). Only 57 of the 62 participants in the full study completed the MBT on the second day.

Self-Report Measures.

Across both days of the study, participants completed a battery of self-report measures, including demographics and questionnaires related to perceived control and reward responsivity. The Snaith-Hamilton Pleasure Scale (SHAPS) (Snaith, et al., 1995) was used to assess overall anhedonia and the two subscales of the Temporal Experience of Pleasure Scale (TEPS) (Gard, et al., 2006) were used to assess anticipatory and consummatory domains of reward responsivity. The two subscales of Lachman and Weaver's Sense of Control scale (1998) were used to assess different domains of perceived control: 'Personal Mastery' and 'Perceived Constraints'. The Desirability of Control scale (Burger & Cooper, 1979) was used to assess the extent to which individuals want to be in control, a construct that is distinct from, but related to, perceived control.

Bias Tasks.

Overview of Bias Tasks and CB Pre-Task.

To make the Choice Bias Task (CBT) and Monetary Bias Task (MBT) as comparable as possible, the MBT was slightly modified from the original task (Pizzagalli, et al., 2005; all changes indexed in Supplementary Methods). Trial schematics for the MBT and CBT are presented in Figure 1a and Figure 1b, respectively. Both bias tasks, the MBT and the CBT, presented a difficult perceptual task in which participants make judgments to discriminate between two very similar visual stimuli. Where the MBT presented face stimuli with slightly differing mouth lengths (long vs. short), the CBT presented door stimuli with door handles of slightly different lengths (long vs. short). In both tasks, participants then made a judgment about whether a long or short stimulus was displayed, and correct judgments were probabilistically rewarded. In the MBT, correct judgments were probabilistically rewarded with a monetary bonus of 5¢, while incorrect and correct-non-rewarded trials received an outcome of 0¢.

In the CBT, choice and no-choice served as the rewarded and non-rewarded outcomes, respectively. The choice and no-choice outcomes were implemented via the presentation of the "selection doors" (displayed in Figure 1c). For a choice outcome, two selection doors were offered for the participant to select between. For a no-choice outcome, a single selection door was offered. During the CB Pre-Task (see Figure 1d; described in detail below), participants gained experience with the selection doors, learning to associate them with small monetary outcomes. Participants were instructed to learn what they could about how the selection doors were associated with money because in the main CBT they would see the selection doors again and would earn money associated with the selection doors but would not be able to see the money they were earning. Thus, in both the CB Pre-Task and the CBT, when participants were presented with the selection doors, they made a button press to indicate their selection (choice) or to acknowledge the already selected option (no-choice). Unlike the CB Pre-Task, in the main CBT, monetary outcomes were not displayed following the selection doors. Participants were instructed that, although the monetary outcomes were not being displayed in the main CBT, the money associated with their door selections was nonetheless being totaled and that they would receive all of their earnings at the conclusion of the study. Thus, in the CBT, correct perceptual judgments were probabilistically rewarded with a choice between selection doors, while incorrect and correct-non-rewarded trials received an outcome of a single door, and all door outcomes were purportedly, probabilistically linked to money.

Importantly, in CB Pre-Task, the choice and no-choice conditions were associated with equivalent monetary outcomes, and as such, a choice outcome in the main CBT did not confer participants any possibility of earning more money. Instead, choice provided, at most, an illusory sense of control over monetary outcomes. Thus, while the selection doors were purportedly linked to money in the CBT, choice *itself* was the reinforcer in this task.

In both the MBT and CBT, the two perceptual stimuli (long vs. short) were rewarded asymmetrically, with the “rich” stimulus rewarded more often than the “lean” stimulus. Previous research using the MBT has demonstrated that psychologically healthy participants develop a bias towards the response (the perceptual decision) more frequently paired with a monetary reward. As such, reward bias in the original task represents reward-responsivity, or an individual’s propensity to modulate their decisions in favor of monetary rewards (hereafter, monetary reward bias). In the adapted CBT, reward bias represents control-responsivity, or the extent to which participants modulate their decisions in favor of opportunities to exert perceived personal control via choice (hereafter, Choice Bias).

Task Instructions.

Task instructions can pose a particular challenge in choice preference paradigms. As previously detailed, the CBT offers no actual control over monetary outcomes; however, the Choice Bias effect is, presumably, dependent upon participants developing an illusory sense that their choices grant some control over monetary earnings. Thus, task instructions were designed such that they neither provided false information nor undermined the potential for participants to develop an illusory sense of choice-linked control over monetary outcomes. Further, task instructions were matched across the MBT and CBT to the extent possible. Verbatim instructions for each task are presented in Supplementary Table S1.

CB Pre-Task Details.

Prior to commencing the main CBT, participants first completed the Choice Bias Pre-Task (Figure 1d), in which they gained experience with the selection doors and choice and no-choice conditions that would later appear in the main CBT. The pre-task involved a selection phase in which participants were presented with the selection door options and an outcome phase in which participants received monetary outcomes, purportedly linked to their door selection. The purpose of the pre-task was to familiarize participants with the contingencies between the choice and no-choice conditions and their associated probabilistic monetary outcomes. Importantly, and unbeknownst to participants, the monetary outcomes were equivalent between the choice and no-choice conditions. Verbatim instructions for the CB Pre-Task are presented in Supplementary Table S1, but can be summarized as follows: Participants were instructed that (a) each door would lead to a monetary reward sometimes but not all of the time, (b) they would *not* receive the monetary bonuses displayed during the pre-task, (c) they should use the pre-task to learn about the rewards associated with the different doors because the same doors would appear again in the “full game”, (d) they *would* get to keep all of the money they earned when they played the "full game", however they would not be able to see the money they were earning in the full game.

The selection door stimuli (Figure 1c) used in the pre-task (and later in the main CBT) were comprised of two different door images, which were presented in pairs, with either a choice between the two options (choice condition) or with only one door option available for selection and the other greyed out and unavailable (no-choice condition). For the selection door stimuli, the positions of the doors were fully counterbalanced with every permutation represented within the choice and no-choice conditions.

In the selection phase of the CB Pre-Task, the selection door stimuli were displayed until participants made a button press (1 or 2, representing the door on the left or right) to indicate their selection (choice condition) or to acknowledge the already selected option (no-choice condition). The pre-task consisted of 12 choice trials, in which each of the two choice stimuli repeated six times, and 12 no-choice trials, in which each of the four no-choice stimuli repeated 3 times. Trial order was fully randomized. In the outcome phase, which followed the selection phase, a monetary outcome of either 5¢ or 0¢ was presented. Across the 12 choice trials, each of the two stimuli was rewarded on two random trials. Across the 12 no-choice trials, each of the four stimuli was rewarded on one random trial. Thus, regardless of the selection made, the choice and no-choice conditions were each rewarded four times across the pre-task, so that both conditions were equally associated with 5¢ or 0¢ outcomes. The pre-task lasted ~2 minutes, including the instruction period.

There was no actual contingency in the pre-task between participants' door selections and monetary outcomes. This design feature was chosen for several reasons: (1) the choice and no-choice conditions were modeled on established choice preference paradigms (for further details see Supplemental Methods section on 'Choice Preference Paradigms'), (2) in line with established choice preference paradigms and in order to experimentally isolate the preference for choice itself versus a preference for outcomes linked to choice, the current task utilized choices conferring only illusory, but not actual, control over outcomes (for further explanation see Supplemental Methods section on 'Selection Door Contingency'), (3) past research has demonstrated that personal control is inferred when none exists (Alloy & Abramson, 1979; Langer, 1975; Wegener and Wheatley, 1999), and (4) even choices conferring no actual control over outcomes and conferring no material advantage are preferred over no-choice and have been shown to inflate the value of choice-associated outcomes (Cockburn, et al., 2014; Leotti & Delgado, 2011; Wang & Delgado, 2019).

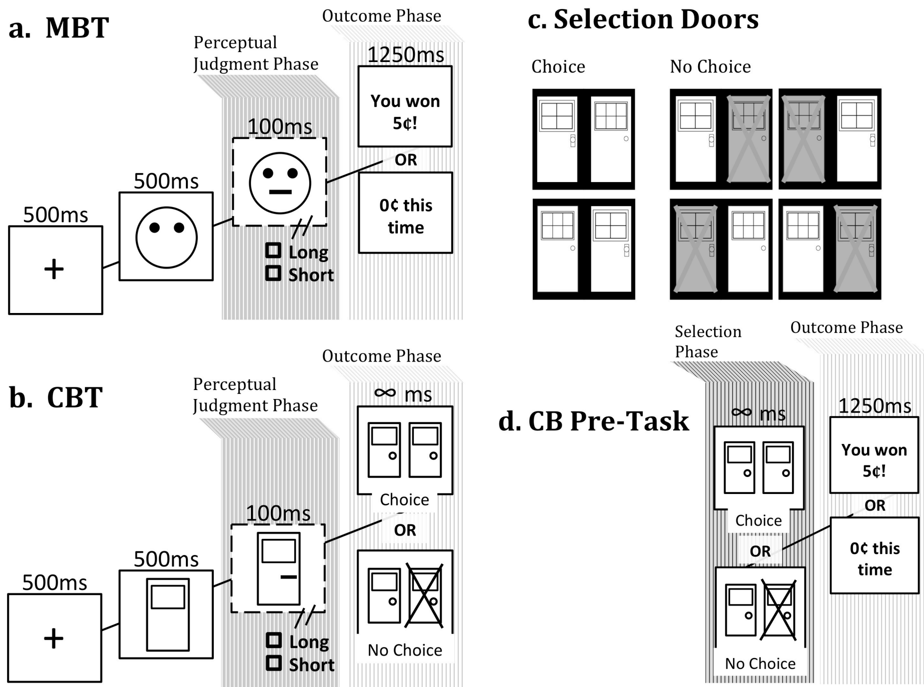

Figure 1. (a) trial schematic for the MBT, (b) trial schematic for the CBT, (c) the selection doors used in the CB Pre-Task (selection phase) and main CBT (outcome phase), and (d) trial schematic for the CB Pre-Task.

Bias Tasks Details.

All computerized tasks were programmed in Inquisit 4 (2015). Aside from reward structure, procedures in the MBT (Figure 1a) and CBT (Figure 1b) were matched to the extent possible. In both the MBT and CBT a 500ms fixation cross was followed by a neutral stimulus (face without mouth in MBT; door without handle in CBT) displayed for 500ms. Next, a perceptual stimulus (long or short mouth in MBT; long or short door handle in CBT) was briefly overlaid on the neutral stimulus for 100ms. In both tasks, the long stimulus was 11mm and the short stimulus was 10mm (Whitton, et. al., 2016). Following presentation of the perceptual stimulus, the neutral stimulus was again displayed until the participant made a response by pressing either key 'o' or key 'p' to indicate their perceptual judgment of the stimulus length, either long or short. The long and short judgments were matched to the 'o' and 'p' key responses in a counterbalanced fashion across participants.

In both tasks, responses were probabilistically rewarded ("You won 5¢!" in MBT, choice between two selection doors in CBT) only when perceptual judgments were correct. However, not all correct judgments were rewarded. Incorrect judgments and correct but non-rewarded trials led to a neutral outcome ("0¢ this time" in MBT, a single selection door in CBT). In the MBT, monetary outcomes were displayed for 1250ms and no response was required from participants. In the CBT, choice and no-choice outcomes were displayed until participants made a button press (1 or 2, representing the door on the left or right) to indicate their selection (choice) or to acknowledge the already selected option (no-choice). Participants were told that the selection doors were linked to monetary outcomes (as they experienced in the CB Pre-Task) but that they would not be able to see the monetary outcomes in the CBT.

At the beginning of both bias tasks, participants were given four training trials. In the first two training trials, participants were explicitly instructed regarding the length of the stimulus (one trial for long; one trial for short). Next, participants were given two training trials in which they had to guess the length of the stimulus before being given explicit performance feedback (one trial for long; one trial for short).

In both bias tasks, the "rich" perceptual stimulus (long or short) was associated with the rewarding outcome (5¢ in MBT; two door choice in CBT) three times more often than was the "lean" perceptual stimulus. A controlled reinforcement procedure was used such that 40 specific trials (30 for the rich stimulus; 10 for the lean) were pseudorandomly marked for potential reward outcomes pending a correct identification of the perceptual stimulus; if participants incorrectly identified the perceptual stimulus on a trial for which reward feedback was due, the reward was conferred on the next correct identification of the same stimulus type. The long and short stimuli were matched to the rich and lean conditions in a counterbalanced fashion across subjects. Both tasks were made up of three blocks of 100 trials each and the lean and rich stimuli were presented equally often in a pseudorandom order with the constraint that no stimulus type was presented more than three times consecutively. In between blocks, there was a 30s rest period before the task resumed automatically. Participants were paid the sum of their total earnings for both bias tasks at the end of the study ($4 for the CBT and up to $6 for the MBT, dependent on performance, for a total of approximately $8.50—$10 for most participants). Each bias task lasted approximately 15 minutes.

Bias Task Data Reduction.

In line with analysis procedures reported by Pizzagalli and colleagues (2005), all data from the bias tasks were cleaned as follows. Any trial with a reaction time under 150ms or over 2500ms was excluded from analyses. Within a given bias task, reaction time data was then log-transformed and, within each subject, any trial with a reaction time that fell outside of ± 3 SD of the subject's mean was excluded from analyses. Based on behavioral models of signal detection (Pizzagalli, et al., 2005; Tripp & Alsop, 1999), two distinct measures, discriminability and response bias, were computed separately from each bias task as detailed below.

Discriminability. Discriminability (DIS) is a measure of an individual's ability to differentiate between the two perceptual stimuli and is thus a measure of task difficulty. Discriminability was computed separately for each task and separately for each of the three blocks in each bias task, according to the formula below. Discriminability was not expected to change significantly across the blocks.

$$\text{DIS} = \log d = .5 * \log\left(\frac{[\text{Rich}_{\text{Correct}} + .01] * [\text{Lean}_{\text{Correct}} + .01]}{[\text{Rich}_{\text{Incorrect}} + .01] * [\text{Lean}_{\text{Incorrect}} + .01]}\right)$$

Response Bias. Response bias (RB) represents the degree of preference for the response more frequently paired with reward (rich condition). Thus, response bias is a measure of an individual's responsivity to rewards. Response bias was computed separately for each task and separately for each of the three blocks in each bias task, according to the formula below. The change in response bias (ΔRB) across the task was defined as ($\text{RB}_{\text{Block 3}}$ - $\text{RB}_{\text{Block 1}}$) and represents the development of a bias for reward across the task.

$$\text{RB} = \log b = .5 * \log\left(\frac{[\text{Rich}_{\text{Correct}} + .01] * [\text{Lean}_{\text{Incorrect}} + .01]}{[\text{Rich}_{\text{Incorrect}} + .01] * [\text{Lean}_{\text{Correct}} + .01]}\right)$$

Response bias in the MBT, or monetary reward bias, represents individual responsivity to monetary reward. Response bias in the CBT, or Choice Bias, represents individual responsivity to perceived control via choice. The primary measure of interest in this study was Δ Choice Bias, or the development of a bias for choice across the task.

Statistical analyses.

While both the pilot study and the full study were used to assess performance on the CBT, due to the greater power in the full study compared to the pilot study, only the full study was used to explore the relationship between Choice Bias and Monetary Reward Bias and the relationship between Choice Bias and self-report measures.

Performance on the CBT.

Performance on the CBT was examined within both the pilot study and the full study. Choice Bias was the main variable of interest, however, for completeness, accuracy, reaction time, and discriminability are also reported. For accuracy (percent perceptual judgments correct) and reaction time, repeated measures ANOVAs were run with factors of block (1, 2, 3) and condition (rich, lean). For response bias (Choice Bias) and discriminability, one-way ANOVAs were run with block as the only factor. Significant effects from the ANOVAs were further explored using pairwise t-tests. Effect sizes are reported using partial eta-squared (η_p^2).

Assessment of Response Bias. Choice Bias was the primary variable of interest and was examined in two ways. First, for each block of the CBT, a one-sample t-test was conducted to determine whether Choice Bias was significantly above zero, indicating a preference for choice across the sample. Second, based on previous research using signal detection tasks (e.g., Pizzagalli, et al., 2005), a response bias difference score between block 1 and block 3 was computed to assess the development of a bias for choice (ΔChoice Bias) across the CBT. Also, a response bias difference score between block 1 and block 3 was computed to assess the development of a bias for monetary reward (ΔMonetary Reward Bias) across the MBT.

Comparing Performance on the CBT and MBT. Data from the full study was used to compare response bias and discriminability from the CBT and the MBT. It should be noted here that in the full study, the CBT was always completed on day 1 and the MBT was always completed on day 2, and thus, results comparing performance between the two tasks should be interpreted within this limitation. For the response bias measures (CBT: Choice Bias; MBT Monetary Reward Bias) and the discriminability measures, two-way ANOVAs with factors of task (CBT, MBT and block (1, 2, 3) were conducted.

Comparing Choice Bias in Pilot Study and Full Study.

As the magnitude of Choice Bias varied substantially between the pilot study and the full study, an exploratory, post-hoc analysis was conducted to examine the effect of study on Choice Bias magnitude. The only difference between the two studies was task order across day 1 and day 2 (in the pilot study, MBT was completed on day 1 and CBT on day 2, and this order was reversed in the full study). In order to explore the difference in Choice Bias magnitude between the two studies (and, thus, the effect of task order) Choice Bias was entered into a mixed ANOVA with block as a within subjects factor and study (pilot, full) as a between subjects factor.

Full Study: Choice Bias in Relation to Self-Report Measures.

To assess the relationship between the development of a bias for choice across the CBT and trait-level constructs of interest, ΔChoice Bias was entered into Pearson correlation analyses with self-report measures of reward responsivity and personal control.

Day 1 Choice Bias in Relation to Day 2 Monetary Reward Bias.

Further, ΔChoice Bias on day 1 was entered into Pearson correlation analysis with Block 1 Monetary Reward Bias on day 2 in order to assess whether increased learning about opportunities to exert control via choice across the CBT carried over to promote initial learning about opportunities for monetary reward on the second day.

Results

Pilot Study: Performance on the Choice Bias Task.

Accuracy in CBT.

Accuracy for the rich and lean conditions across the three blocks is plotted in Figure 2a. A two-way ANOVA with factors of block and condition did not show a significant main effect of block [$F(2,40) = .989$, $p = .381$]. As expected, accuracy for the rich stimulus was higher than accuracy for the lean stimulus, however the main effect of condition did not reach significance [$F(1,20) = 2.38$, $p = .139$, $\eta_p^2 = .106$]. There was a significant condition by block interaction [$F(2,40) = 5.60$, $p = .007$, $\eta_p^2 = .219$]. Follow-up pairwise comparisons demonstrated that differences in accuracy for the rich and lean stimuli in each of the blocks was in the expected direction (block 1: [$t(20) = 1.95$, $p = .066$]; block 2: [$t(20) = 1.53$, $p = .142$]; block 3: [$t(20) = 2.55$, $p = .019$]), but this difference was only significant for block 3.

Reaction Time in CBT.

Reaction time for the rich and lean conditions across the three blocks is plotted in Figure 2b. A two-way ANOVA with factors of block and condition showed a significant main effect of block [$F(2,40) = 17.05$, $p < .001$, $\eta_p^2 = 0.46$], however, there was no significant main effect of condition [$F(1,20) = 2.01$, $p = .172$] and no significant interaction [$F(2,40) = 1.29$, $p = .288$]. Follow-up pairwise comparisons across the blocks (collapsing across the rich and lean stimuli) showed a significant decrease in reaction time with each subsequent block (all p's < .01).

Choice Bias.

Choice Bias across the three blocks of the CBT is plotted in Figure 2c. A one-way ANOVA did not show an effect of block [$F(2,40) = .024$, $p = .977$]. As there was an *a priori* hypothesis that Choice Bias would increase across the blocks, pairwise comparisons of Choice Bias between each of the blocks were also conducted and there were no significant differences in Choice Bias between any of the blocks (all p's > .8). However, Choice Bias was above zero in each block and these differences were significant for block 1 [$t(20) = 2.21$, $p = .039$] and block 3 [$t(20) = 2.58$, $p = .018$], but only approached significance for block 2 [$t(20) = 2.01$, $p = .058$].

Discriminability.

Discriminability across the three blocks of the CBT is plotted in Figure 2d. In line with predictions, a one-way ANOVA did not show an effect of block [$F(2,40) = .776$, $p = .467$].

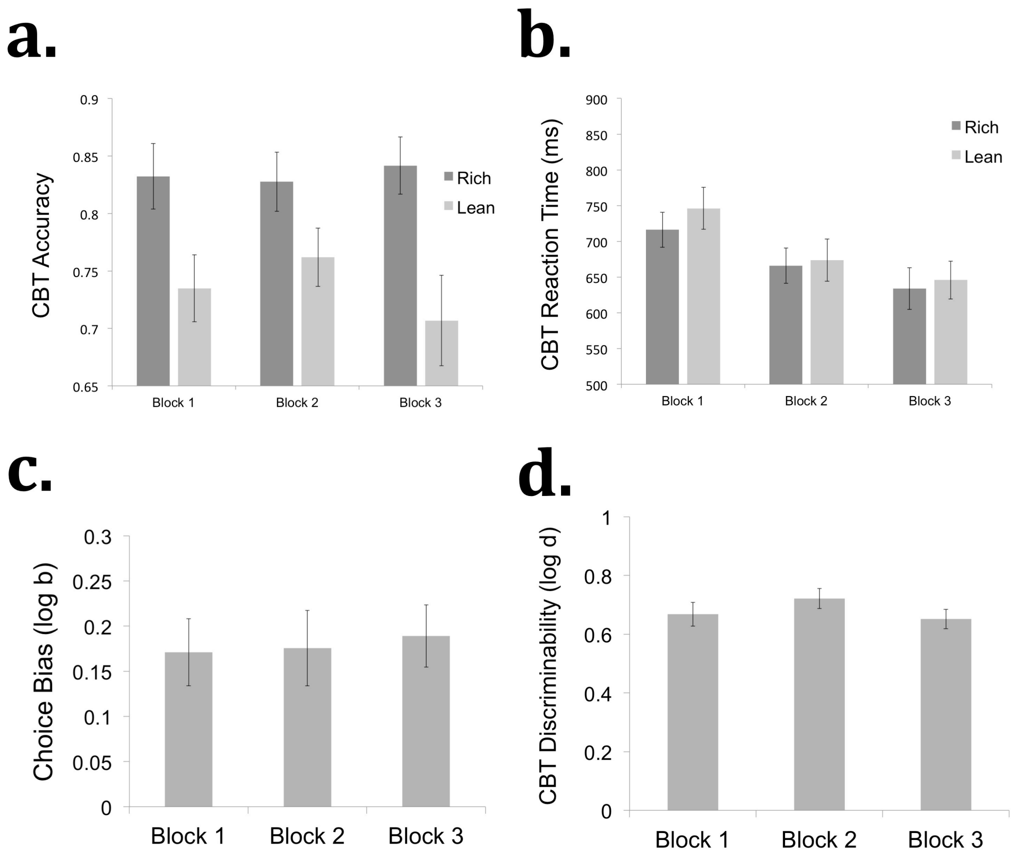

Figure 2. Pilot Study: Overall Results of Choice Bias Task. n = 21 (a) CBT mean accuracy, (b) CBT reaction time, (c) Choice Bias (d) discriminability. Error bars represent standard error of the mean.

Full Study: Performance on the Choice Bias Task.

Accuracy in CBT.

Accuracy for the rich and lean conditions across the three blocks is plotted in Figure 3a. A two-way ANOVA with factors of block and condition did not show a main effect of block [$F(2,122) = 1.38$, $p = .255$] nor an interaction of block by condition [$F(2,122) = .007$, $p = .993$]. As expected, accuracy for the rich stimulus was higher than accuracy for the lean stimulus, however the main effect of condition only approached significance [$F(1,61) = 3.14$, $p = .081$, $\eta_p^2 = 0.05$].

Reaction Time in CBT.

Reaction time for the rich and lean conditions across the three blocks is plotted in Figure 3b. A two-way ANOVA with factors of block and condition showed a significant main effect of block [$F(2,122) = 71.96$, $p < .001$, $\eta_p^2 = 0.54$], however, there was no significant main effect of condition [$F(1,61) = 2.32$, $p = .140$] and no significant interaction [$F(2,122) = .152$, $p = .859$]. Follow-up pairwise comparisons across the blocks (collapsing across the rich and lean stimuli) showed a significant decrease in reaction time with each subsequent block (all p's $< .05$).

Response Bias.

Choice Bias. Choice Bias across the three blocks of the CBT is plotted in Figure 3c (on the left). A one-way ANOVA did not show an effect of block [$F(2,122) = .103$, $p = .902$]. As there was an *a priori* hypothesis that Choice Bias would increase across the blocks, pairwise comparisons of Choice Bias between each of the blocks were also conducted and there were no significant differences in Choice Bias between any of the blocks (all p's > .9). However, Choice Bias was above zero in each block and these differences approached significance for block 1 [$t(61) = 1.90$, $p = .063$] and for block 3 [$t(61) = 1.68$, $p = .098$], but not for block 2 [$t(61) = 1.35$, $p = .181$].

Monetary Reward Bias. Monetary Reward Bias across the three blocks of the MBT is plotted in Figure 3c (on the right). A one-way ANOVA showed a main effect of block [$F(2,112) = 3.88$, $p = .024$, $\eta_p^2 = .065$]. Pairwise comparisons between the blocks showed significant differences in Monetary Reward Bias between block 1 and block 2 [$t(56) = -2.68$, $p = .01$] and between block 1 and block 3 [$t(56) = -2.02$, $p = .048$] but not between block 2 and block 3 [$t(56) = .512$, $p = .61$]. Monetary Reward Bias was significantly above zero in each block (all p's < .05).

Comparing Choice Bias to Monetary Reward Bias. A two-way ANOVA with factors of task and block revealed a significant main effect of task [$F(1,56) = 4.99$, $p = .029$, $\eta_p^2 = .082$], with a significantly higher overall response bias in the MBT (EMM = .195) than in the CBT (EMM = .057). The main effect of block was not significant [$F(2,112) = 2.16$, $p = .12$]. The interaction of task and block was nearly significant [$F(2,112) = 2.98$, $p = .055$, $\eta_p^2 = .051$], suggesting that participants responded to the reward domains in the two bias tasks in different ways. To further probe the interaction effect, the difference in response bias from block 1 to block 3 was computed separately for each bias task to create ΔChoice Bias (-.01 ± .33 [mean ± SD]) and ΔMonetary Reward Bias (.12 ± .46 [mean ± SD]). The difference between ΔChoice Bias and ΔMonetary Reward Bias approached significance [$t(56) = 1.71$, $p = .09$].

Discriminability.

Discriminability in CBT. Discriminability across the three blocks of the CBT is plotted in Figure 3d (on the left). In line with predictions, a one-way ANOVA did not show a significant main effect of block [$F(2,122) = 1.96$, $p = .145$].

Discriminability in MBT. Discriminability across the three blocks of the MBT is plotted in Figure 3d (on the right). In line with predictions, a one-way ANOVA did not show a significant main effect of block [$F(2,112) = .432$, $p = .650$].

Comparing Discriminability in the CBT and MBT. A two-way ANOVA with factors of task and block revealed a significant main effect of task [$F(1,56) = 66.29$, $p < .001$, $\eta_p^2 = .542$], with a significantly higher overall discriminability in the MBT (EMM = .876) than in the CBT (EMM = .544). There was no significant effect of block [$F(2,112) = .651$, $p = .524$] nor significant interaction [$F(2,112) = .637$, $p = .531$].

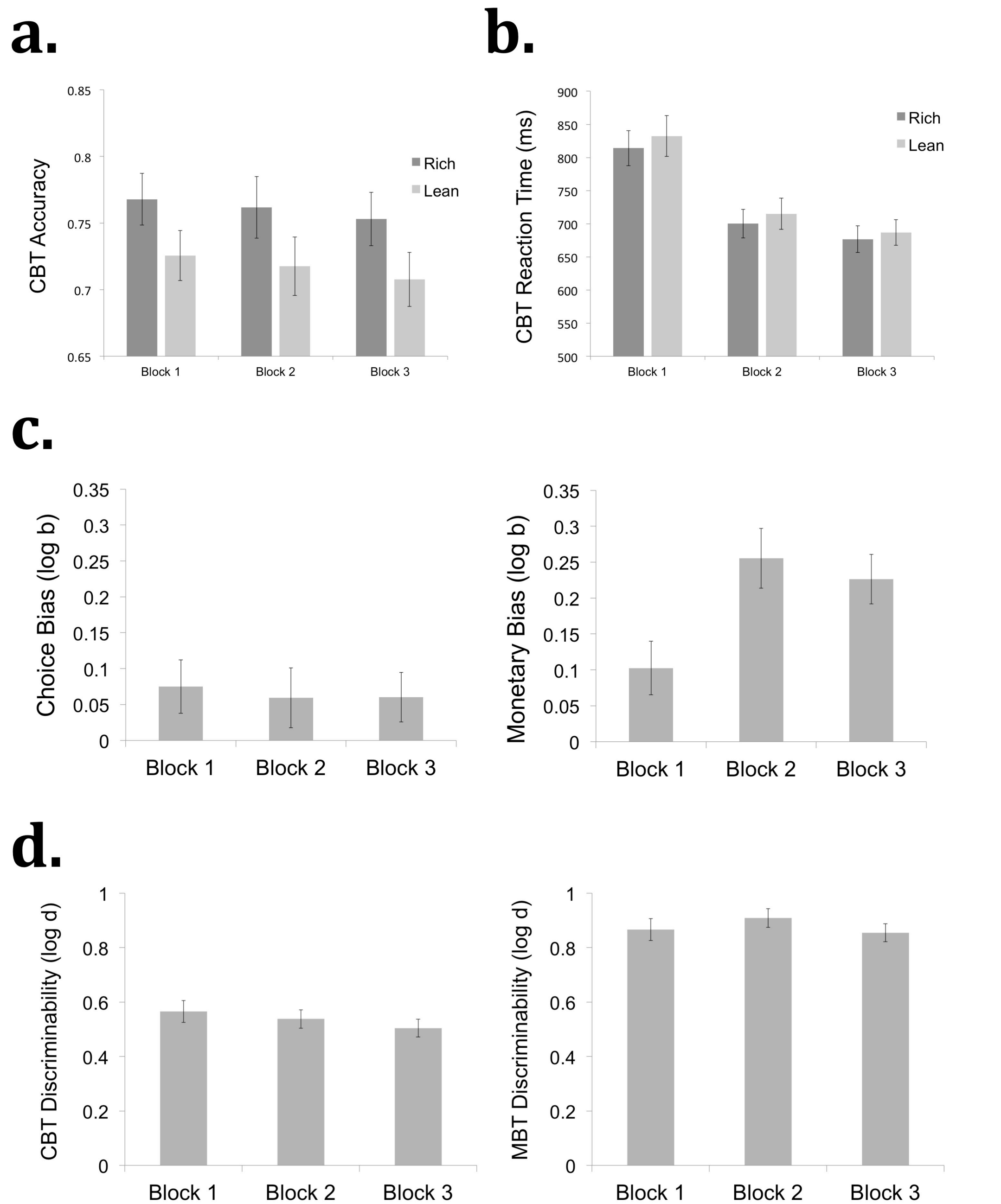

Figure 3. Full Study: Overall Results of Choice Bias Task. CBT: n = 62; MBT: n = 57 (a) CBT mean accuracy, (b) CBT reaction time, (c) Choice Bias (left) and Monetary Reward Bias (right) (d) CBT discriminability (left) and MBT discriminability (right). Error bars represent standard error of the mean.

Comparing Choice Bias in Pilot Study and Full Study.

A mixed ANOVA with block as a within subjects factor and study (pilot, full) as a between subjects factor found no significant effect of block [$F(2,162) = .017$, $p = .983$] and no interaction of block and study [$F(2,162) = .079$, $p = .925$]. However, there was a significant main effect of study [$F(1,81) = 2.88$, $p = .034$, $\eta_p^2 = .034$], with higher Choice Bias (EMM = .179) in the pilot study when participants had prior experience with the MBT and lower Choice Bias (EMM = .065) in the full study when the CBT was the first bias task participants completed.

Full Study: Choice Bias in Relation to Self-Report Measures.

Anhedonia and Reward Responsivity.

SHAPS. As expected, self-reported scores on the Snaith-Hamilton Pleasure Scale (SHAPS) showed a significant negative correlation with ΔChoice Bias [$r(60) = -.265$, $p = .038$] (see Figure 4a), indicating that higher levels of self-reported anhedonia were associated with a relative decrease in Choice Bias across the CBT.

TEPS. Further, ΔChoice Bias was positively associated with self-reported reward responsivity on the Temporal Experience of Pleasure Scale (TEPS), showing a significant positive correlation with both the Anticipatory Pleasure subscale [$r(60) = .288$, $p = .023$] (Figure 4b) and the Consummatory Pleasure subscale [$r(60) = .265$, $p = .038$] (Figure 4c).

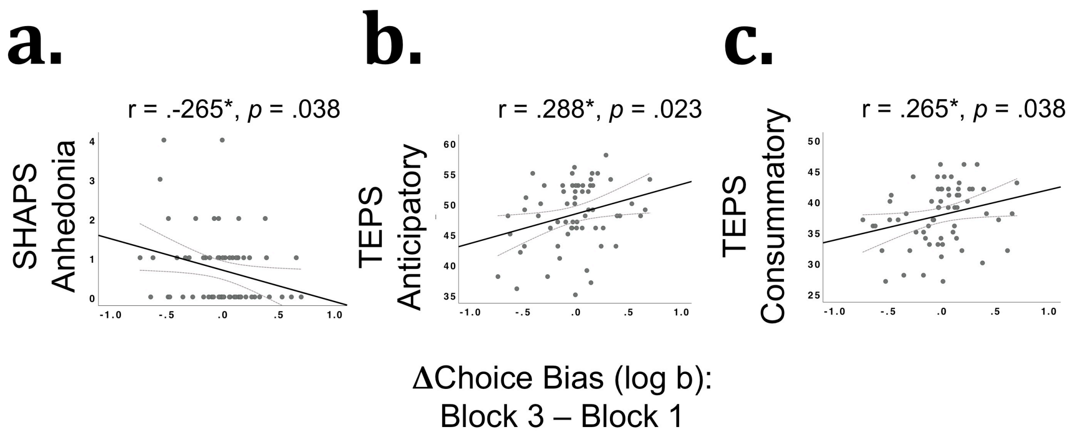

Figure 4. Full Study: Choice Bias and Self-Reported Reward Responsivity.
CBT: n = 62; (a) ΔChoice Bias and SHAPS, (b) ΔChoice Bias and TEPS: Anticipatory Pleasure, (c) ΔChoice Bias and TEPS: Consummatory Pleasure

Perceived Control and Desired Control.

Sense of Control. In line with expectations, ΔChoice Bias showed opposite associations with the contrasting subscales of the Sense of Control scale. Specifically, ΔChoice Bias was positively correlated with the Personal Mastery subscale [$r(60)$ = .158, p = .221] (Figure 5a) and negatively correlated with the Perceived Constraints subscale [$r(60)$ = -.182, p = .156] (Figure 5b). While in the predicted directions, these correlations did not reach significance.

Desirability of Control. Also in line with expectations, ΔChoice Bias showed a positive association with self-reported Desirability of Control [$r(60)$ = .245, p = .055] (Figure 5c), although this correlation did not reach significance.

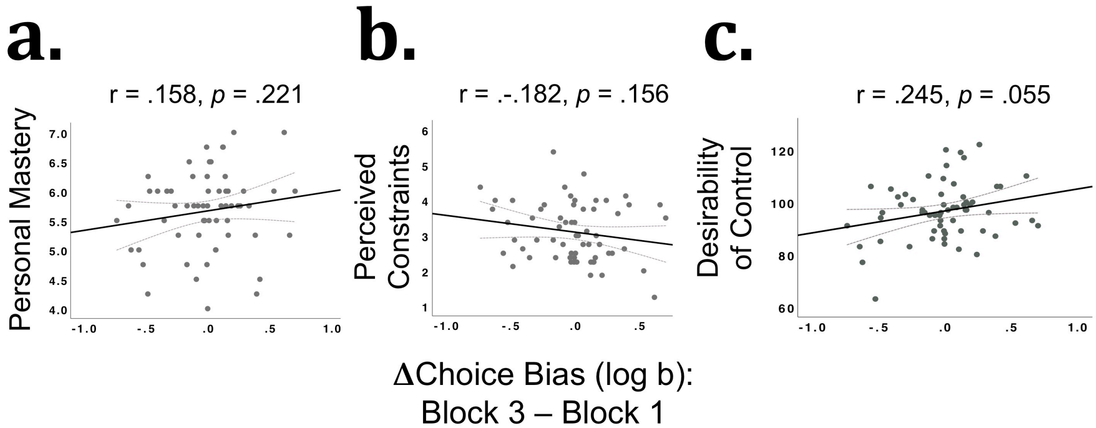

Figure 5. Full Study: Choice Bias and Self-Reported Personal Control.
CBT: n = 62; (a) ΔChoice Bias and SoC: Personal Mastery, (b) ΔChoice Bias and SoC: Perceived Constraints, (c) ΔChoice Bias and Desirability of Control

Day 1 Choice Bias in Relation to Day 2 Monetary Reward Bias.

Figure 6 displays the association between participants' change in Choice Bias across the CBT (ΔChoice Bias) on day 1 and their Block 1 Monetary Reward Bias on the MBT on day 2 [$r(55) = .235$, $p = .079$]. While there was a moderate correlation, it did not reach significance.

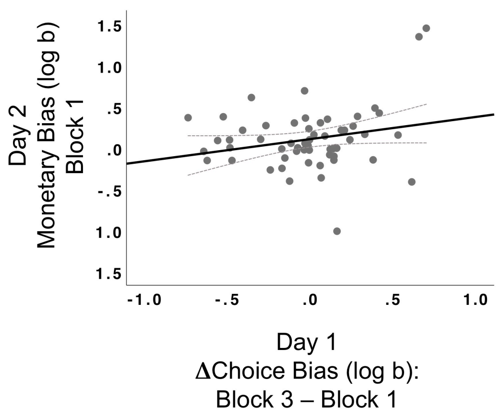

Figure 6. Full Study: n = 57; Day 1 ΔChoice Bias and Day 2 Block 1 Monetary Reward Bias

Discussion

Given theoretical promise for the study of personal control to inform anhedonic phenotype(s) and the inherent limitations imposed by the lack of objective, behavioral measures of personal control, the current study introduced the Choice Bias Task, a novel behavioral measure of responsivity to opportunities to exert perceived control via choice. The Choice Bias Task was based on the signal detection probabilistic reward task by Pizzagalli and colleagues (2005). Where the original task (Monetary Bias Task) measures a decisional bias towards monetary rewards, the current Choice Bias Task measures a decisional bias (Choice Bias) towards the abstract reward of a choice. Preliminary evidence demonstrated that participants show a bias for choice and that this bias is positively associated with reward responsivity and negatively associated with anhedonia, suggesting that the Choice Bias Task offers a novel tool to measure responsivity to perceived control and objectively characterize appetitive personal control deficits found in anhedonia.

As previous research has demonstrated that simple choices are inherently reinforcing in and of themselves (e.g., Leotti & Delgado, 2011; Wang & Delgado, 2019), we hypothesized that participants would show a general pattern of control-seeking as measured by a decisional bias towards a stimulus more frequently paired with a choice between monetary reward cues. There was some support for this hypothesis. Unlike previous research which has found an increasing decisional bias for monetary reward across blocks of the Monetary Bias Task (e.g., Pizzagalli, et al., 2005; 2008), the current study did not find an increase in the bias for choice across the blocks of the CBT in either the pilot study or the full study. However, in both the pilot study and the full study, choice bias was above zero (indicating a bias for the stimulus most often paired with choice) for all blocks, although Choice Bias was only significantly above zero for two of the three blocks in the pilot study and only approached significance in two of the three blocks of the full study. Additionally, Choice Bias was significantly greater in the pilot study compared to the full study. Thus, while there was some evidence that participants developed an overall bias for choice, the evidence also showed that this effect was stronger in the pilot study, suggesting that the bias for choice was more readily developed when participants understood the structure of the task by having had prior experience with the Monetary Bias Task.

The bias for monetary reward was also compared to the bias for the abstract reward of choice, and Monetary Reward Bias was significantly greater. Further, the change in Choice Bias across the task was relatively flat compared to the increase in Monetary Reward Bias across the task and this difference approached significance. Together these findings suggest that participants' decisions were more influenced by monetary rewards than they were by the abstract reward of choice, however these results may also reflect the fact that the Choice Bias Task was completed on the first day while the Monetary Bias Task was completed on the second day, potentially indicating that a greater Monetary Reward Bias was due to participants having more familiarity with the task structure on the second day. The relatively weaker influence of choice on decisions may also be due to the greater uncertainty over monetary rewards in the Choice Bias Task compared to the Monetary Bias Task and/or the additional cognitive effort imposed by the requirement of making a choice in the Choice Bias Task. This explanation is consistent with the idea that the preference, subjective value, and motivational impact of choice is a weak effect that may be undermined by costs such as effort, loss outcomes, or uncertainty (Anderson, 2003; Beattie, Baron, Hershey & Spranca, 1994; Leotti & Delgado, 2014; Patall, 2012; Sullivan-Toole, Richey & Tricomi, 2017). Discriminability was also higher in the Monetary Bias Task compared to the Choice Bias Task, suggesting that participants may have had a more difficult time learning the difference between the perceptual stimuli when rewarded with choice, possibly reflecting the increase cognitive effort costs intrinsically involved in making choices (Vohs, et al., 2008).

Given evidence that responsivity to personal control is diminished in internalizing disorders with prominent anhedonic features (Alloy & Abramson, 1979; Cloitre, et al., 1992; Franck, et al., 2001; Hofmann, 2005; Maeda, et al., 2013; Msetfi, Murphy & Simpson, 2007; Romaniuk, et al., 2019; Späti, et al., 2015), Choice Bias was expected to be reduced in relation to higher self-reported anhedonia scores. In line with expectations, the change in Choice Bias across the task was negatively associated with Snaith-Hamilton anhedonia scores (Snaith, et al., 1995) and was positively associated with self-reported anticipatory and consummatory reward responsivity on the TEPS (Gard, et al., 2006). These results align with evidence of a link between personal control responsivity and reward responsivity in the general population (Ghane, et al., 2019; Ly, et al., 2019) and with evidence suggesting that perceived control is specifically diminished in relation to anhedonia transdiagnostically (Bogdan, et al., 2012; Cassar, Applegate & Bentall, 2013; Grant & Beck, 2009; Maeda, et al., 2013).

To examine task validity, the change in Choice Bias across the task was examined in relation to self-report measures related to the construct of perceived control. The change in Choice Bias was correlated in the expected directions with the subscales of Lachman and Weaver's Sense of Control scale (1998), showing a positive association with personal mastery and a negative association with perceived constraints. However these correlations were not significant and should thus be interpreted with caution. The change in Choice Bias also showed a positive and nearly significant association with the Desirability of Control (Burger & Cooper, 1979), a construct that measures the extent to which individuals want to be in control, which is related to, but distinct from, perceived control, or the extent to which individuals believe they are in control. While there was some evidence for a relation between Choice Bias and self-report measures related to perceived control, this evidence was not significant and, thus, construct validity of Choice Bias will need to be revisited in future research. It may be that Choice Bias shows a more reliable relation with the desire for control than with a individuals' belief that they are in control, particularly given that the Choice Bias Task offers only illusory, but not real, control over monetary outcomes (for a further explanation of this design feature, see Supplemental Methods section on 'Selection Door Contingency').

Given the putative role of diminished reward learning in anhedonia (Thomsen, 2015), the change in Choice Bias across the task was examined in relation to the baseline Monetary Reward Bias on day two. While there was a moderate correlation, it did not reach significance. Thus there was some evidence, albeit rather weak evidence, that control-seeking as defined by Choice Bias may be associated with reduced learning across the domains of personal control-related reward and monetary reward.

There are several major limitations to the current findings. First, while there is some preliminary evidence for a general bias for choice, questions regarding construct validity remain. While associations between Choice Bias and aspects of perceived control were in line with predictions, only the association between Choice Bias and desire for control approached significance. On the other hand, associations between Choice Bias and self-report measures of reward responsivity and anhedonia were significant. Together, these results suggest that responsivity to personal control as measured by the Choice Bias Task may primarily align with affective aspects of control (e.g., wanting control) rather than cognitive aspects of control (e.g., believing oneself to be in control), however future work will need to replicate these findings to establish construct validity. Second, while the full study had greater power and was thus used to compare the effect of money versus the effect of the abstract reward of choice, the Monetary Bias Task was always completed on the second day within this data, and thus, there is an obvious confound in comparing the Choice Bias effect to the Monetary Bias effect. Future research should counterbalance the order of the two tasks in order to properly compare these effects and understand the relative influence of monetary reward compared to the reward of choice. Third, as in the original task (Pizzagalli, et al., 2005), the reinforcer in the Choice Bias Task was confounded with positive performance feedback. In other words, participants were explicitly told that choice would only follow correct perceptual judgments, but that not all correct perceptual judgments would receive a choice outcome. As positive performance feedback has been shown to activate reward responsivity in a similar way to monetary reward (Tricomi, et al., 2006; Tricomi & Fiez, 2008), the current incarnation of the Choice Bias Task does not allow the rewarding effect of positive performance feedback to be dissociated from the rewarding effect of exercising personal control via choice. Despite the confound, this design feature was chosen intentionally because it (1) most closely adhered to the original Monetary Bias Task and (2) was mostly likely to produce an effect related to choice and thus served as an early point of failure for task design (in other words, if the current design "failed" to produce an effect of choice, there would be no need to create additional versions of the task with weaker choice manipulations).

The next version of the Choice Bias Task will address the current limitations in several ways. First, as it appears that Choice Bias was more readily developed in the context of already having had experience with the Monetary Bias Task, the next version of the task will combine these two tasks into a single task, with the different reward modalities (money, choice) presented separately across different blocks of the task and with the different reward modalities associated with distinct sets of perceptual stimuli. Consistent with the idea that choice may only be appetitive in a gains context (Leotti & Delgado, 2014), this task adaptation is more likely to elicit a bias for choice than the current version. Second, in order to be able to compare Monetary Reward Bias to Choice Bias, the order of the monetary reward blocks and choice reward blocks will alternate and will be counterbalanced across subjects in the next version of the Choice Bias Task. Third, in future versions of the task, performance feedback will be uncoupled from choice and no-choice outcomes by first providing performance feedback on every third trial regardless of trial type and then displaying choice and no-choice outcomes. With further refinement, the Choice Bias Task should offer a new tool for measuring individual differences in responsivity to perceived control, which may elucidate a relatively unexplored domain of anhedonia.

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MANUSCRIPT 2

SUPPLEMENTARY MATERIAL

Supplementary Methods

<u>MBT Deviances from Original Task.</u>

The MBT used in the current paper is based on Pizzagalli and colleagues' (2005) probabilistic reward task. To increase comparability with the CBT, the MBT included several modifications to the original probabilistic reward task including:

1. *Monetary Outcomes.* In the original task, following participants' perceptual judgments, only reward outcomes (5¢ outcomes) were displayed, while non-rewarded trials (including both incorrect trials and correct, but not rewarded, trials) simply advanced to the next trial without any outcome display. The presently reported adapted MBT uses the exact probabilistic reward structure of the original task (with correct judgments for the rich stimulus rewarded three times as often as correct judgments for the lean stimulus), however, in a divergence from the original task, outcomes are displayed for every trial, whether outcomes are rewards (5¢) or non-rewards (0¢).

2. *Outcome Duration.* As outcomes were displayed for every trial, we shortened the overall length of the MBT by making outcome durations 1250ms, which is a deviation from the original task in which reward outcomes were displayed for 1750ms.

3. *Task Instructions.* In order to make the CBT and MBT as comparable as possible, task instructions across the two tasks were made as analogous as possible (see Supplementary Table S1 for verbatim task instructions).

Choice Preference Paradigms.

Choice preference paradigms (e.g., Leotti & Delgado, 2011, 2014; Romaniuk, et al., 2018; Suzuki, 1997, 1999) include two conditions: a 'free choice' condition in which participants choose between two options and a 'no choice' or 'forced choice' condition in which participants are given a single option to select. Following the participant's selection (via either free or forced choice), a monetary outcome is delivered. Importantly, monetary outcomes are equated across the choice and no choice conditions, such that an individual's selection cannot impart any additional monetary benefit. Thus, choice in these paradigms offers only an illusory sense of control. Nevertheless, this research has demonstrated that both humans and animals prefer such choices over no choice, even when subsequent outcomes are equivalent across conditions (e.g., Catania, 1975; Leotti & Delgado, 2011; Suzuki, 1997, 1999). Further, such choices have been shown to enhance reward-related response to choice-associated outcomes (Cockburn, et al., 2014; Wang & Delgado, 2019; Tricomi, et al., 2004). Further, it has been argued that choice itself may be rewarding (Leotti, et al., 2010).

Selection Door Contingency.

It has been proposed that the value associated with making choices (choices themselves, independent of associated outcomes) lies in the extent to which the choice confers control, or illusory control, over outcomes (Leotti, et al., 2010; Moller et al., 2006; Patall, 2012;

Reeve et al., 2003; Sullivan-Toole, et al., 2017). In the context of experimental investigation, however, choices that confer actual control over outcomes, make it impossible to disentangle value for choice itself from value for the outcomes choice provides. Therefore, experimental tasks manipulating perceived control via choice have (a) utilized choice conditions that provide illusory control over outcomes rather than actual control and (b) equated outcomes across choice and no choice conditions (e.g., Leotti & Delgado, 2011, 2014; Suzuki, 1997, 1999). Although participants have no actual control over outcomes in these paradigms, choice conditions are preferred, presumably due to participants perceiving choice-conferred control over outcomes.