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titled
The Effect of Reduced Self-Control Resources on Risk Preferences Depends on Task Characteristics
by
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Submitted to the Graduate Faculty as partial fulfillment of the requirements for the Doctor of Philosophy Degree in Psychology

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An Abstract of

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Previous exertions of self-control can affect subsequent risk preferences usually resulting in risk-seeking tendencies. A few studies, however, have found that reductions in self-control resources (i.e., “ego depletion”) can decrease risk-taking resulting in relative risk-aversion compared to non-depleted controls. In efforts to explain these contradictory results, three experiments examined whether certain task features predicted when ego depletion would lead to increased or decreased risk-taking. Across different variations of the Columbia Card Task (CCT), the ego depleted risked less than their non-depleted counterparts when task features promoted feelings of perceived control (Experiments 1-3). Manipulations aimed at reducing perceived control eliminated this depletion effect and descriptively reversed the trend, such that the ego depleted were risking more than the controls. Ego depletion also affected some aspects of participant’s information use on the CCT. While both the ego depleted and the non-depleted were sensitive to changes in gain amount and probability, the ego depleted were insensitive to changes in loss amount (Experiment 1). Experiment 2 also showed that trait self-control moderated the ego depletion effect under conditions of low, but not high perceived control. Results of
Experiment 3 suggested that the previous inconsistencies in the literature were not due to differences in risk-taking tasks requiring experienced-based learning. Together these results provide a more nuanced account for the effect of ego depletion on risk preferences and offer potential explanations for previous inconsistencies reported in the literature.
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List of Abbreviations

BART ....................Balloon Analogue Risk Task
BAS ......................Behavioral activation system
BIS .......................Behavioral inhibition system
CCT .....................Columbia Card Task
Exp ......................Experiment
$M$ ........................Mean
SD ........................Standard Deviation
SCS ......................Self-Control Scale
S-W ......................Shapiro-Wilks
List of Symbols

\(\alpha\) ........Alpha level
\(\eta^2_p\) .........Partial eta squared effect size

\(b\) ........Unstandardized beta weight
\(d\) ........Cohen’s d effect size
\(F\) ........Test statistic for ANOVA or regression
\(p\) ........Probability of obtaining a test statistic assuming the null hypothesis is true
\(r\) ........Pearson statistic that assesses the relationship between two variables
\(t\) ........Test statistic associated with analysis
Chapter One

Limited Self-Control Resources and Risk-Taking

Our capacity to regulate our thoughts, emotions, and behaviors is extremely adaptive and helps us attain our goals. This capacity for self-control is associated with several positive outcomes; for example, increased self-control predicts better physical and mental health, more financial stability, and less substance abuse and criminality even after controlling for socioeconomic status and intelligence (Moffitt et al., 2011; Tangney, Baumeister, & Boone, 2004). These results, in particular the latter associations, suggest that self-control can serve as a buffer against engaging in certain risky behaviors. These studies join others showing that decreased self-control (or increased impulsivity) is associated with problem gambling, drug use, juvenile delinquency, and risky sexual behavior (Fuentes, Tavares, Artes, & Gorenstein, 2006; White et al., 1994; Zuckerman & Kuhlman, 2000).

While greater trait self-control consistently predicts less risk-taking, the impact of state self-control on risk-taking is less consistent. State self-control can change throughout the day resulting in occasions where an individual may lack the capacity or motivation to exert self-control. An abundance of research has shown that people are particularly vulnerable to lapses in self-control after having recently regulated their behavior or resisted temptation (for review see, Baumeister, Vohs, & Tice, 2007; for meta-analysis see, Hagger, Wood, Stiff, & Chatzisarantis, 2010). This phenomenon whereby previous exertions of self-control impair performance on subsequent self-control tasks is called the “ego depletion” effect (Baumeister, Bratslavsky, Muraven, & Tice, 1998).
Under the state of ego depletion, one’s willingness to accept risks seems to change compared to a non-depleted state in which minimal effort has been spent exercising self-control. The precise direction of these changes in risk preference, however, has been inconsistent with some studies reporting that ego depletion causes risk-seeking (Appendix A) and others showing it increases risk-aversion (Appendix B). The purpose of the present work is to examine the conditions under which ego depletion is likely to decrease or increase risk-taking relative to non-depleted controls. The present chapter will first provide a summary of the empirical and theoretical developments in the ego depletion literature and then focus on how ego depletion affects risk preferences.

Typically, the ego depletion effect is obtained using a two task paradigm in which participants perform two unrelated tasks both relying on self-control. Evidence of an ego depletion effect is found if a group who initially exercised self-control (i.e., the ego depleted) performs differently (usually worse) on a subsequent task compared to a group who exercised minimal self-control (i.e., the non-depleted). For example, Muraven, Tice, and Baumeister (1998) found that participants who enhanced or suppressed their emotional reactions to a film clip quit sooner on a handgrip endurance test than participants who just watched the film. Similarly, individuals who engaged in a thought suppression task persisted less on unsolvable anagrams relative to individuals who did not restrict their thoughts.

According to the strength model, acts of self-control, such as thought suppression, emotion regulation, and physical endurance, consume the same general, finite resource leaving people more susceptible to lapses in self-control on subsequent tasks (Muraven et al., 1998). Thus in the context of the two task paradigm, the first task draws energy from
a limited resource pool leaving less energy for the second task and increases the chances of a failure in self-control. Ego depletion effects have been demonstrated in a variety of domains affecting cognitive processes and behaviors, such as executive functioning, task persistence, health-related behaviors, and decision making (Hagger, Wood, Stiff, & Chatzisarantis, 2009; Schmeichel, 2007).

While the strength model of self-control has garnered substantial support over the past fifteen years, it has not gone unchallenged. An increasing number of studies have found several ways of counteracting ego depletion effects that are not readily explained by the limited resource account. For example, people who expect or believe that exercising self-control will not impair subsequent attempts at self-control show no depletion effects (Job, Dweck, & Walton, 2010; Martijn, Tenbult, Merckelback, & Dreezens, 2002). Similarly, people’s perception of their depletion state is more influential than their actual depletion state (Clarkson, Hirt, Chapman, & Jia, 2011; Clarkson, Hirt, Jia, & Alexander, 2010). The introduction of humor or expressing one’s core values (i.e., self-affirmation) following a depleting task is yet another way to eliminate ego depletion effects (Schmeichel & Vohs, 2009; Tice, Baumeister, Shmueli, & Muraven, 2007). These studies suggest that the strength model requires some qualifications.

In an attempt to incorporate these findings, Inzlicht and Schmeichel (2012) propose a process model of ego depletion. According to this model, ego depletion effects are not due to the draining of an abstract reservoir of self-control resources, but instead result from shifts in motivational and attentional states. They assert that ego depletion decreases motivation to exercise self-control and increases desire for immediate gratification. Simultaneously, it also biases attention toward reward and impairs people’s
ability to monitor discrepancies between desired and obtained outcomes. The consequences of these shifts in motivation and attention is that the ego depleted lack the desire to control themselves, feel more inclined to act on impulse, and attend more to rewards and less to signals indicating the need for control.

Building upon this process model, the present work examined how ego depletion affected attentional and motivational processes in decisions involving risk and uncertainty. Several of the hypotheses tested in Chapters 2-4 were derived from the process model with the aim of understanding the underlying mechanisms driving ego depletion effects. The subsequent sections review how the availability of self-control resources can affect our judgments and decisions in riskless and risky choice contexts. Although the literature shows that ego depletion certainly influences decision making, it lacks studies investigating the underlying processes; the present work sought to fill this gap in the literature.

**Impact of Ego Depletion on Decision Making**

The availability of self-control resources has been shown to have implications for decision making. For example, Vohs and Faber (2007) showed that ego depletion increased impulse spending. In this study, participants completed either a depleting or non-depleting initial task and subsequently were given $10 that they could keep or use toward purchasing some products in a laboratory shopping simulation. The products were generally inexpensive ranging in type from food to novelty items (e.g., mugs, playing cards). Provided this unanticipated buying opportunity, depleted participants purchased more items, spent more money, and reported being more tempted than
controls. Depletion has also been shown to increase preference for smaller, immediate rewards over larger, delayed rewards (Vohs, Baumeister, & Schmeichel, 2012).

In addition to increasing impulsivity, ego depletion is hypothesized to increase reliance on simple, intuitive processing as opposed to effortful, deliberative processing. Consistent with this hypothesis, ego depleted individuals evidenced more pronounced decision biases attributed to simple, intuitive processing (i.e., attraction and reference-dependence effects), but less bias on decision problems (i.e., compromise effect) requiring more effortful, trade-offs (Masicampo & Baumeister, 2008; Pocheptsova, Amir, Dhar, & Baumeister, 2009). Additional evidence shows that given the option to choose between two equally attractive alternatives, depleted individuals are more likely than non-depleted controls to defer their decision for a later time. This choice deferral is arguably a simpler alternative that does not require making difficult trade-offs.

The decision making process, in and of itself, also has been shown to reduce self-control resources. That is, after repeatedly making choices, people are more likely to select a more expensive, affectively-rich product (Bruyneel, Dewitte, Vohs, & Warlop, 2006). Likewise, ego depletion (via repeated choice) leads to decreased persistence on a variety of tasks demanding self-control, such as pain tolerance and problem solving (Vohs et al., 2008). Thus, similar to previous ego depletion studies, the same task can serve as a manipulation or measure of self-control resources. This supports the theory that these tasks diminish resources from and require access to a general resource pool.

The Effect of Ego Depletion on Risk Taking

As mentioned previously, empirical studies examining the effect of ego depletion on risk-taking have obtained contradictory results. Appendices A and B show that most
studies report that ego depletion increases risk-taking, but several show that it decreases risk-taking. Unfortunately, there have been very few attempts at explaining these inconsistencies. The aim of the present research is to identify and manipulate variables potentially responsible for these inconsistencies to determine when ego depletion is most likely to increase or decrease risk-taking.

When surveying the extant research in this area, one quickly notices the variety of operational definitions of risk-taking. While these measures may all assess components of risk taking, they have different task characteristics that could explain the inconsistent results. Thus, the plan of the present work was to analyze these previous studies in an effort to discover possible task characteristics that could moderate the ego depletion effects. The following section will first briefly review how risk preferences have been measured and then describe how ego depletion effects may depend on the characteristics of the given risk-taking measure.

**Measuring risk-taking.** Expert and lay persons agree that individuals differ in their willingness to accept risks with some preferring minimal risks (i.e., the risk-averse), whereas others tolerate higher risks (i.e., the risk-seeking). These individual differences in risk attitude are believed to determine an individual’s actual risk preference in a given situation. Normative models, such as expected value theory and expected utility theory, conceptualize risk attitude as an enduring characteristic that directly predicts risk preferences. More recent research, however, has revealed that the relationship between risk attitude and risk preference is complex and determined by individual and situational factors, such as affective states, the particular risk domain (e.g., health vs. financial risks), and other task characteristics (For review see, Weber, 2010). To gain a better
appreciation for the differences between these two perspectives, a more detailed
discussion of each perspective will follow in turn.

Expected value theory and its later revision, expected utility theory\(^1\), contend that
decision-makers should treat risky options as distributions of possible outcomes and
select the option with the highest expected value. To calculate the expected value of each
alternative, one multiplies the value of each outcome \((v)\) by its probability of occurring
\((p)\). The decision to purchase renter’s insurance is a commonplace example where
expected value theory can be applied. Imagine you have a choice of paying $200 for a
renter’s policy that covers $10,000 in personal property losses or paying nothing and
accepting the 1% chance of losing your personal property valued at $10,000 to some peril
(e.g., theft, fire, etc.). According to expected value theory, one should not purchase this
insurance policy because the expected value of the gamble (i.e., \(-$10,000 \times .01 = -$100\))
is higher (or less costly) than the expected value of the policy (i.e., \(-$200 \times 1.0 = -$200\)).

Expected value is the first moment of a risky option’s distribution (akin to the
mean), whereas variance is the second moment and refers to the average variability of an
outcome distribution. Prospects with higher variance, such as the above option to forgo
renter’s insurance, result in more unpredictable and extreme outcomes on average,
whereas a prospect with lower variance has more predictable outcomes (Weber, 2009).
Variance is often used to define risk with less variance representing safer or less risky
decisions (Schonberg, Fox, & Poldrack, 2011). For example, a gamble offering a 50%}

\(^1\) Expected value theory posits that people share an objective value for an outcome and linearly weight
outcome and probability information. In contrast, expected utility theory acknowledges that the subjective
value of an outcome (especially those non-monetary in nature) can vary from individual-to-individual and
result in non-linear utility functions.
chance of losing $200, although equivalent in expected value to the previous insurance
gamble, is less risky than the insurance gamble.

Although risk attitude is summarized using descriptive labels such as risk-seeking
and risk-aversion, it actually refers to the shape, specifically the curvature, of an
individual’s utility function according to expected utility theory. The utility function
maps objective wealth onto subjective utility. Utility functions can be concave indicating
diminishing subjective utility as objective wealth increases. For example, a change in
wealth from $10,000 to $30,000 is associated with a greater increase in subjective utility
than a change from $50,000 to $70,000 even though the absolute change is the same.
One consequence of this diminishing sensitivity to objective increases in wealth is the
frequently observed risk aversion for coin toss gambles (e.g., 50% chance of winning
$100 or nothing). For gambles such as this one, a risk-averse person would report
receiving $45 for sure as equivalent to playing the gamble even though the sure amount is
less than the expected value of the gamble (i.e., $50). Utility functions can also be
convex resulting in people valuing the gamble more than its expected value and
becoming risk-seeking.

Although this conceptualization of risk attitude is parsimonious, it suffers from at
least two major limitations. First, researchers have had limited success predicting risk
preferences in naturalistic risk-taking contexts using an individual’s risk attitude
(Schonberg et al., 2011; Weber, 2010). The second limitation is that expected value
theory neglects important variables that have been shown to influence risk preferences
besides the curvature of one’s utility function. The following will discuss how
psychological models of risk-taking have sought to address these limitations.
Given the apparent prediction gap between risk attitude and risk preferences, psychologists have developed alternative risk-taking measures with more ecological validity. One type of measure uses risk-taking scenarios derived from real-life situations and asks people to indicate the likelihood they will engage in a given risk behavior. The Kogan-Wallach Choice Dilemmas Questionnaire (CDQ; Kogan & Wallach, 1964) and the Domain-Specific Risk Attitude Scale (DOSPERT; Weber, Blais, & Betz, 2002) are two examples of this measure. For the CDQ, participants read elaborate decisions scenarios about a character and report the lowest probability of success needed for them to recommend the riskier of two options. For example, one scenario involves choosing to stay with one’s current job that provides an adequate salary and lifetime job security or switch to a new job with higher salary, but uncertain job security. The DOSPERT measures people’s likelihood to engage in a variety of risky behaviors spanning five domains: financial (e.g., investing 10% of your annual income in a new business venture), ethical (e.g., passing off somebody else’s work as your own), social (e.g., disagreeing with an authority figure on a major issue), health/safety (e.g., driving a car without wearing a seat belt), and recreational risks (e.g., bungee jumping off a tall bridge). The DOSPERT has achieved moderate success in predicting real world risk behaviors (Weber, 2010).

Another type of assessment uses behavioral measures of risk-taking that tend to be more engaging and share characteristics of actual risk taking. The Balloon Analogue Risk Task (BART) is one behavioral measure in which participants receive a certain amount (e.g., one cent) for each pump they deliver to a virtual balloon. With each successive pump, the chances of the balloon popping and losing all of one’s money for
the round increases. Participants can continue inflating the balloon until they decide to collect the money they have earned thus far or until the balloon pops. The BART involves dynamic (as opposed to static) risk in which the expected value and variance change as each additional action is taken, much like real-world risks. Dynamic tasks also typically provide feedback and the probability of loss increases with each sequential choice. Importantly, participants are not informed about the probability of popping the balloon; thus participants must learn it through experience.

The Iowa Gambling Task (IGT) is another behavioral measure that requires participants to learn the reward contingencies. During the IGT, participants select cards from four decks with particular reward and punishment schedules. Unbeknownst to the participant, two of the decks consist of large rewards, but even larger punishments resulting in a net loss (if continually sampled), whereas the other two decks consist of smaller rewards and punishments resulting in a net gain. Previous studies show that neurologically intact adults proceed through distinct learning phases starting with sampling of both decks to ending with primarily sampling from the net gain deck (Bechara & Damasio, 2005). The IGT and BART tend to be more engaging and affective than answering (hypothetical) decision scenarios or deciding between pairs of gambles (in efforts to calculate utility functions). Because risky decisions often involve affect and incremental feedback, this may be one reason why the IGT and BART have had more success predicting everyday risk behaviors, such as drug use and stealing (Buelow & Suhr, 2009; Lejuez et al., 2002). Predicting risk preferences using risk attitude alone is also likely to fall short because risk-taking is a complex construct depending on many
separate processes whose interaction may change depending on the domain or context of the risk behavior.

For this reason, descriptive models of risk, such Prospect Theory (Kahneman & Tversky, 1979), have sought to identify additional variables that influence risk preference previously ignored by expected value theory. For example, expected utility theory does not predict risky-choice framing effects whereby risk preferences regarding two objectively equivalent options change when the options are described in terms of gains or losses (Tversky & Kahneman, 1986). Demonstrations of this framing effect can involve endowing participants with $600 and having them decide whether to: 1) keep $200 [lose $400] for sure or 2) enter a lottery with a 33% chance of keeping $600 [losing $0] and a 67% chance of keeping $0 [losing $600]. Participants reading the gain frame usually select the former sure-thing option, while participants reading the loss frame (contained in the brackets) tend to select the latter risky option.

Another contextual variable that influences risk preferences is the manner in which the risk information is acquired. Risk information can be conveyed in a descriptive summary as presented thus far (e.g., Option 1: $3 for sure or Option 2: 80% chance of winning $4 and 20% chance of $0), or it can be learned through experience by having participants sample different options and receive outcome information; the IGT and BART are two examples of experienced-based tasks. Typically though, experienced-based paradigms present participants with two buttons on a computer screen that they repeatedly choose from to reveal outcome information. Through this sampling process, people learn the outcome distribution of each option. When considering Options 1 and 2 above, description-based participants tend to select $3 for sure over the gamble, thereby
exhibiting the typical risk aversion found with high-probability gambles in the gain domain. In contrast, most experience-based participants select the gamble, Option 2. Across several other binary gambles (in the gain and loss domain) a consistent pattern emerged suggesting that decisions from experience tend to underweight rare events (e.g., 20% of $0). This underweighting on the part of the experience-based group can result in selecting the risky option more often (as when deciding between Options 1 and 2), or selecting the risky option less often than the description group (e.g., Option 3: $3 for sure or Option 4: 10% chance of $32 and 90% of $0). This apparent “experience-description gap” reveals an interesting tendency in which people overweight small probability events in decisions from description, but underweight the same events in decisions from experience (Hertwig, Barron, Weber, & Erev, 2004).

**Task analysis of ego depletion studies.** The above summary of various risk taking measures is useful when analyzing the differences among the studies presented in Appendices A and B. While these appendices divide previous studies by whether ego depletion increased or decreased risk taking, they can also be organized by the type of risk measure used. Studies examining the effect of ego depletion on risk taking have used static and dynamic games of chance as well as self-report, scenario-based risk taking measures. It is the contention of this proposal that the contradictory results are at least partially due to the type of risk measure employed.

In terms of self-reported risk-taking, ego depletion consistently increases preferences for risky actions. This tendency toward risk-seeking behavior has been obtained with the Choice Dilemma Questionnaire and the DOSPERT. For example, depleted individuals were more likely to say they would engage in a variety of risky
behaviors on the DOSPERT, such as drinking excessively at a public party, engaging in unprotected sex, driving a motorcycle without a helmet, and betting a day’s income in a poker game (Fischer, Kastenmüller, & Asal, 2012). Similarly on the CDQ, depleted individuals were willing to accept lower odds of success for a risky option than non-depleted individuals (Freeman & Muraven, 2010).

Although the pattern is not as consistent as the self-report measures, depletion generally increases risk taking on behavioral measures, especially tasks requiring decisions from experience. For example, depleted individuals risked more in the BART choosing to inflate the virtual balloon more than non-depleted individuals (Freeman & Muraven, 2010). Using a different decision from experience task, Molet et al. (2012) found that depleted individuals were more likely to select a low probability, high-payoff option that was lower in expected value than a high probability, low-payoff option. In this study, participants first equally sampled two gambling options for 20 trials. Option 1 offered a lower net payoff (i.e., a variable ratio schedule with a 20% chance of 10 points and an 80% of 0 points), whereas Option 2 offered a higher net payoff (i.e., a variable ratio schedule earning 3 points on average). After this forced sampling procedure, participants freely selected one of the two options for 20 trials. During this free selection period, depletion increased preference for the lower net payoff that resembled a reinforcement schedule for slot machines and lotteries.

The effect of ego depletion on the Iowa Gambling Task (IGT) is more mixed. De Langhe, Sweldens, Van Osselaer, and Tuk (2008) found that depletion facilitated performance; that is, the ego depleted chose more from the net gain decks and less from the net loss decks. This group difference, however, disappeared after the 40th trial of the
IGT (100 total trials). Reporting essentially the opposite results, Goodwin (2012) found that depletion impaired performance on the IGT especially toward the end of the task. Unfortunately, Goodwin’s study does not report sufficient details to analyze possible procedural differences.

The effect of depletion on choices in static gambles or lotteries has been inconsistent as well. During these tasks, participants select between two gambles or simply decide to purchase tickets to have an opportunity to win some jackpot. A majority of the studies that report increased risk aversion among depleted individuals (3 out of the 4) have measured risk taking using choices in chance gambles (Carr & Steele, 2010; Kostek & Ashrafioun, 2013; Unger & Stahlberg, 2011). In contrast, only 4 out of 8 studies showing that depletion increases risk-seeking have done so (Bruyneel, Dewitte, Franses, & Dekimpe, 2009; Imhoff, Schmidt, & Gerstenberg, 2013; Inzlicht & Kang, 2010; Schmeichel, Harmon-Jones, & Harmon-Jones, 2010).

Offering a possible explanation for these inconsistencies, Unger and Stahlberg hypothesized that the effect of ego depletion depends on the characteristics of the risky decision problem. Specifically, Unger and Stahlberg (2011) argued that the effect of ego-depletion depended on the extent to which the risk taking task evoked feelings of responsibility and perceived control over the outcomes of a decision. According to this hypothesis, ego depletion will result in decreased risk taking when decision tasks produce these feelings and increased risk taking when the tasks do not. In support of this prediction, they note that studies framing a decision as a gamble or lottery—which they argue evoke less responsibility and control—generally find increased risk taking among the ego-depleted relative to non-depleted controls (Bruyneel, Dewitte, Franses, &
Dekimpe 2009; Freeman & Muraven, 2010; Inzlicht & Kang, 2010). Conversely, Unger and Stahlberg found evidence of decreased risk taking on an investment scenario that presumably evoked increased feelings of responsibility and control.

To increase feelings of control and responsibility, Unger and Stahlberg created an investment scenario. Participants imagined they were a manager of a company deciding which of four countries they would choose to invest in for a new manufacturing site. To assist them with their decision, participants read information about each country’s political stability, infrastructure, relative cost, and profit potential. These written descriptions were composed such that participants had to make trade-offs, but ultimately there were two safer options and two riskier options. Finally, participants were provided a pay-off matrix with the potential outcomes and their associated probabilities listed.

Across three experiments, ego depleted individuals were more likely to choose the safer, lower risk countries than the non-depleted. These results suggest that framing the task as an investment scenario is sufficient to increase feelings of responsibility and control, and in turn, increase risk-aversion among the depleted.

Although consistent with their prediction, these results have several limitations preventing a definitive conclusion that personal responsibility and control explain the previous inconsistencies on resource depletion. One limitation is that Unger and Stahlberg did not run a control condition in which the same gambles were presented, but removed from the above investment context. If framing of the risk task (i.e., a gamble versus an investment) is simply responsible for the contradictory results, then presenting the same pay-off matrix as gambles should yield the opposite effect. A second limitation is that they were unable to verify via manipulation checks whether the investment
scenario actually increased personal responsibility or control relative to a control group.

A third limitation is that they used actual countries (i.e., Germany, Poland, China, and Vietnam) which could have introduced pre-existing prejudices unrelated to the main manipulation. In fact, Germany was a safe option and presumably the residing country of many of the participants who attended the University of Mannheim. Consequently, the observed depletion effect may reflect enhanced in-group favoritism as suggested by previous studies (e.g., DeWall, Baumeister, Gailliot, & Maner, 2008; Govorun & Payne, 2006) and not risk aversion.

Lastly, Unger and Stahlberg never clearly distinguished between control and responsibility. For definitional purposes, control will be defined as “probability alterability” (Goodie, 2003). In other words, control refers to one’s ability to change the likelihood of obtaining a positive or negative outcome while engaging in an activity. Control varies along a continuum. Roulette or a coin flip, for example, offers no degree of control because the probability of winning is determined by chance and nothing can be done (without cheating) to improve one’s chances. In contrast, trivia games or tasks involving a skill component offer more control because with practice one can improve one’s chances of success. While control refers to a contingency between one’s actions and the resulting outcome, responsibility describes whether a person is held accountable for his or her actions. That is, can one be blamed or commended for the consequences of an action that could affect the self or others? One way to manipulate responsibility is to make the decision-maker more or less accountable to others, which refers to social responsibility.
This distinction is important because previous research shows that control and responsibility can have different influences on risk perception and risky decision making (Anderson & Galinsky, 2006; Charness & Jackson, 2009; Nordgren, Van Der Pligt, & Van Harreveld, 2007; Pahlke, Strasser, & Vieider, 2013). For example, increases in (perceived) control generally lead to greater willingness to accept risk (e.g., Davis, Sundahl, & Lesbo, 2000; Goodie, 2003). In contrast, contexts with high social responsibility tend to produce decreased risk-taking (Charness & Jackson, 2009; Pahlke et al., 2013).

Consistent with Unger and Stahlberg’s assertion, their economic scenario appears to manipulate both control and responsibility. Concerning responsibility, participants assuming the role of the company manager presumably feel some degree of social responsibility in that their decision could affect employees and shareholders. This increased concern over social responsibility may explain why the ego depleted became more conservative. In terms of control, participants, having pored over all the contextual information about each country, may have felt there was some degree of skill involved in choosing the optimal country. Ultimately though, this information was extraneous because all outcomes were determined by chance based on the pay-off matrix presented at the end. Nonetheless, participants could have perceived that this task required some skill, and thus, they had some modicum of control. Consistent with this interpretation, there is a rather large literature showing that people can overestimate the extent to which games of chance involve skill (e.g., Langer, 1975). Under non-depleted conditions, we would expect that this increased perceived control would increase preferences for the riskier options. However, it is unknown whether increases in perceived control have the
same effect on risk preferences under the state of ego depletion. Because control and responsibility were confounded in Unger and Stahlberg’s economic scenario, Experiments 1 and 2 sought to disentangle the two so that we could better understand when ego depletion is likely to lead to decreased risk-taking.
Chapter Two

Experiment 1: Evidence that Ego Depletion Decreases Risk-Taking

The present study sought to address some of the limitations of Unger and Stahlberg (2011) as discussed in Chapter 1 and examine the effect of ego depletion on an alternative risk-taking task that is more akin to a gambling game instead of an investment scenario. A gambling task was preferred because it related more closely to previous work on trait impulsivity and risk-taking, and it could avoid some of the idiosyncrasies of Unger and Stahlberg’s investment scenario. Further improving upon Unger and Stahlberg’s design, Experiment 1 also only focused on increasing feelings of control to test whether the ego depleted would risk less than the non-depleted on a task with only the appearance or illusion of control. This would suggest that control alone was sufficient to decrease risk-taking among the depleted.

To increase perceived control, Experiment 1 borrowed manipulations from the illusion of control literature that have been shown to inflate people’s expectancy for success in games of chance. Demonstrations of the illusion of control rely on introducing certain task characteristics that lead people to feel their actions are influential in producing some desirable, but ultimately chance-determined outcome (Langer, 1975; Thompson, 1999). For example, people who choose their own lottery ticket or roll a die themselves are more confident of winning than those who are assigned a ticket or have someone else roll the die (Dunn & Wilson, 1990; Wohl & Enzle, 2002). Similarly, people wager more in a game of chance involving cards with familiar symbols as opposed to unfamiliar ones (Bouts & Van Avermaet, 1992). These examples illustrate
how incorporating elements of choice, active involvement, and familiarity can cause people to overestimate the amount of control they exert over chance outcomes.

For Experiments 1-3, the Columbia Card Task (CCT; Figner, Mackinlay, Wilkening, & Weber, 2009) was selected because one could manipulate task characteristics, such as choice and active involvement, that have been shown to increase perceived control. Furthermore, the CCT is a dynamic risk-taking task that shares similarities to real-world risks in that the chances of loss increase the more people engage in the task. For the CCT, participants turn over a desired number of cards from an array with the potential of winning or losing points. During each round, participants can read about the value and the number of gain and loss cards, but they do not know where the loss or gain cards are located in the array of 32 cards. Participants select which cards they would like to turn and then see the outcome. For each gain card revealed, participants earn the specified gain points (i.e., 10 or 30), but if a loss card is selected, they lose points (i.e., -250 or -750) and the round terminates. As participants turn over more cards, the risk of turning a loss card increases. Thus, the number of cards turned on each trial is a measure of risk-taking.

It was expected that choice over which card to turn and the participant’s active role in the game (as opposed to the experimenter playing for them) would promote an illusion of control. With these task characteristics giving participants a sense of increased control, the ego depleted were expected to turn over fewer cards on the CCT and thus take fewer risks than the non-depleted.

The design of the CCT also offers two additional benefits. One benefit of the CCT is that one can observe whether participants adjust their gameplay to account for
changes in expected value. Half of the CCT rounds are *risk advantageous* (RA) meaning the normative solution dictates selecting some cards, whereas the other half are *risk disadvantageous* (RD) meaning the normative solution dictates that no cards should be selected. If participants are integrating the probability and outcome information appropriately, they should turn more cards in the risk advantageous rounds than risk disadvantageous rounds. Because ego depletion can impair problem solving and higher-order cognitive processes (Schmeichel, 2007; Schmeichel, Vohs, & Baumeister, 2003), it is plausible that the ego depleted may not adjust their playing to match the optimal solution of a round leading to a Round × Depletion interaction.

Another advantage of the CCT is that by independently manipulating gain, loss, and probability information in a factorial design, one can examine the extent to which individuals attend to each piece of information. This measure of information use provides an additional test of whether the depletion groups differ in their searching of the problem space. Interestingly, Unger and Stahlberg (2011) conducted an experiment that sought to test a similar question. They reasoned that the increased risk aversion among the ego depleted might motivate them to seek additional information about the investment options. If the investment scenario caused ego depleted people to adopt a more cautious strategy, then they might be willing to read more essays about their investment options in an effort to reduce uncertainty and check whether their initial decision was correct. Participants received the investment scenario along with the pay-off matrix, but before making a final decision, they were given 24 titles of essays that provided additional information about the countries. They were told to select up to 10 essays that were then purportedly going to be given to the participant to read. Contrary to their hypothesis,
depletion had no effect on the participant’s willingness to read additional information. In the current study, instead of looking at motivation to search additional information, the CCT allows one to see to what extent participants are attending to the information directly relevant to the task. That is, one can see the extent to which participants integrate gain, loss, and probability. This analysis could provide insight into why the ego depleted were acting more risk-averse.

Overview and Hypotheses

The primary purpose of the first experiment was to determine whether increasing perceived control on the CCT would be sufficient to decrease risk-taking among the ego depleted. Furthermore, this experiment examined to what extent ego depletion affected risk-taking in different contexts (risk advantageous vs. disadvantageous) and amount of information use. For this study, participants arrived in the laboratory and completed either a depleting or non-depleting task (i.e., a perceptual search task) and then completed a mood measure followed by the CCT. The mood measure was included to show that the depleting task was not inducing a more negative or positive mood compared to the non-depleting control task. Mood was not expected to differ between the two versions of the perceptual search task consistent with previous studies on ego depletion (Baumeister et al., 1998). In sum, the following hypotheses were tested during Experiment 1:

**Hypothesis 1.** Under conditions of high perceived control, the ego depleted were expected to risk less (i.e., turn fewer cards) than the non-depleted.

**Hypothesis 2.** Participants would select more cards during RA rounds than RD rounds. The ego depleted would be less sensitive to changes in expected value and, therefore, show less discrimination between the RA and RD rounds.
Hypothesis 3. The ego depleted will be less likely to attend to and integrate probability and outcome information presented during the CCT.

Hypothesis 4. The effect of ego depletion on risk-taking in the CCT should remain after controlling for possible changes in mood.

Method

Participants. A total of 43 undergraduates at the University of Toledo participated in exchange for partial course credit for their Introductory to Psychology course ($M_{age} = 19.33, SD = 2.77; 25$ females).

Materials

Positive affect and negative affect schedule (PANAS). The brief version of the PANAS is a 20-item mood questionnaire that has been used to measure momentary as well as more enduring feelings of positive and negative affect (Watson, Clark, & Tellegen, 1988). Participants read each item and then indicated the extent to which they felt that way right then in the present moment. They responded using a 5-point scale with points labeled: “1- Very slightly or not at all,” “2- A little,” “3- Moderately,” “4- Quite a bit,” and “5- Extremely.” The positive affect (i.e., interested, excited, strong, enthusiastic, proud, alert, inspired, active, attentive, and determined) and negative affect (i.e., distressed, upset, guilty, scared, hostile, irritable, ashamed, nervous, jittery, and afraid) items were intermixed and presented in the same order for all participants. Watson et al. (1988) reported internal consistency reliabilities for momentary affect as $\alpha_{PA} = .89$ and $\alpha_{NA} = .85$.

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2 All three experiments used a convenience sample of undergraduate students introducing the possibility that the results may not generalize to other populations (varying in age or cultural background). This potential limitation would need to be confirmed by future research.
CCT. Each round began with 32 cards presented face down in a $4 \times 8$ grid (see Figure 1). Among the 32 cards were a certain number of gain and loss cards valued at a specified amount; this information (i.e., the gain amount, the loss amount, and the probability of loss) was always displayed at the top of the screen along with the current round total. During each round, participants selected which cards they would like to turn from among the 32 cards and were able to stop selecting cards at any time by clicking the button labeled, “STOP/Turn Over.” For each gain card turned, participants earned the designated amount of points. If they turned a loss card though, they immediately lost the amount of points specified for the round and the round ended. For this version of the CCT, feedback concerning the outcome of each round (i.e., number of gain and loss cards selected) was delayed until the participant chose to stop (see Figure 1). This delayed feedback was chosen to capture participants’ willingness to select as many cards as they would like without being prematurely cut off by turning a loss card as would occur with immediate feedback.

The number of loss cards (i.e., 1 or 3), the value of each gain card (i.e., 10 or 30), and the value of each loss card (i.e., 250 or 750) changed for each round. A factorial combination of these levels resulted in eight different rounds. These eight combinations were repeated three times for a total of 24 rounds. Half of the rounds were risk advantageous in that the normative solution dictated choosing between 4-23 cards (depending on the round) and the other half of rounds were risk disadvantageous meaning the normative solution was to turn zero cards (see Table 1). Performance on this task was not incentivized, but participants were told, “Your objective is to win as many points as possible and avoid losing points.”
*Figure 1.* Screenshots of the computerized CCT. The top image shows the beginning of each round and bottom image shows a round in progress; participants were able to select which cards would be turned but they did not receive feedback until clicking the “STOP/Turn Over” button.
Table 1

*Information Available for each CCT Round with its Corresponding Optimal Solution for Experiments 1 and 2*

<table>
<thead>
<tr>
<th>Experiment 1</th>
<th>Experiment 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Loss Cards</td>
<td>Gain Amount</td>
</tr>
<tr>
<td>1</td>
<td>10</td>
</tr>
<tr>
<td>1</td>
<td>10</td>
</tr>
<tr>
<td>1</td>
<td>30</td>
</tr>
<tr>
<td>1</td>
<td>30</td>
</tr>
<tr>
<td>3</td>
<td>10</td>
</tr>
<tr>
<td>3</td>
<td>10</td>
</tr>
<tr>
<td>3</td>
<td>30</td>
</tr>
<tr>
<td>3</td>
<td>30</td>
</tr>
</tbody>
</table>

*Note.* Shaded cells represent risk disadvantageous (RD) rounds.

**Procedure & Design**

After obtaining informed consent from participants, the experimenter told the participants that they would be completing two unrelated experiments—one concerning individual differences in perceptual search skills and another regarding decision making. During the first experiment, participants completed a two-part perceptual search task followed by the PANAS. The perceptual search task served as the primary manipulation of self-control resources.
During the first part of the perceptual search task, all participants received a sample of text in which they had to cross out every occurrence of the letter ‘e’ as performed by several previous studies (e.g., Baumeister et al., 1998; DeWall, Baumeister, Gailliot, & Maner, 2008; Egan, Hirt, & Karpen, 2012; Muraven, Shmueli, & Burkley, 2006). The purpose of this task was to establish a “habit” which the depleted participants would then have to break in the second part. For the second part, depleted participants crossed out every occurrence of the letter ‘e’ except when: 1) a vowel followed an ‘e’ or 2) a vowel appeared two letters before the ‘e.’ Participants in the non-depleting control condition received the same text, but continued to cross-out every instance of the letter ‘e’ with no additional exceptions.

Participants were given eight minutes to complete each part of the perceptual search task. Immediately after the perceptual search task, participants completed the PANAS and then played the CCT. After completing the CCT, participants answered a series of brief questionnaires asking them about the strategies they used during the CCT and a variety of demographic questions.

The experimental design was a $2 \times 2$ mixed design with Depletion (depleted or control) as the between-subjects variable and Round (risk advantageous or disadvantageous) as the within-subjects variable. Participants were run in individual sessions that lasted approximately 45 minutes. All statistical analyses were conducted using IBM’s Statistical Package for the Social Sciences (SPSS; version 19); Type III Sums of Squares were used for all ANOVAs.
Results

PANAS. The positive affect items and negative affect items were summed separately to obtain a total affect score for each valence (scores could range from a minimum 10 to a maximum of 50). The internal consistency reliabilities for the positive and negative affect scales were $\alpha_{PA} = .88$ and $\alpha_{NA} = .68$. Ego depleted participants ($M = 26.04, SD = 7.90$) and non-depleted controls ($M = 23.79, SD = 6.59$) did not significantly differ in positive affect, $t(41) = 0.99, p = .33, d = 0.31$. Similarly, the ego depleted ($M = 14.5, SD = 5.14$) and controls ($M = 13.63, SD = 3.88$) did not significantly differ in negative affect, $t(41) = 0.61, p = .54, d = 0.19$.

CCT. The average number of cards selected during the 12 RA and 12 RD rounds was submitted to a $2 \times 2$ mixed ANOVA with Round (RA vs. RD) as a within-subjects factor and depletion state (depleted vs. non-depleted control) as a between-subjects factor. The ANOVA returned two significant main effects. As predicted, participants selected more cards during the advantageous ($M = 11.43, SD = 4.67$) than disadvantageous rounds ($M = 8.92, SD = 3.97$), $F(1, 41) = 34.57, p < .001, \eta^2_p = .46$. Also consistent with our hypothesis, depleted participants ($M = 8.89, SD = 3.43$) selected fewer cards than controls ($M = 11.79, SD = 4.33$), $F(1, 41) = 5.96, p = .02, \eta^2_p = .13$. Although the cell means presented in Table 2 trend toward a Round $\times$ Depletion interaction, the interaction was not significant, $F(1, 41) = 2.85, p = .10, \eta^2_p = .07$. Thus, both groups were sensitive to the changes in expected value.

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$^3$ When gender, positive affect, and negative affect were entered as covariates to $2 \times 2$ ANCOVA model, results remained the same. The main effects of depletion and trial type remained significant, and neither the interaction term nor the covariates were significant.
Table 2

Mean Number of Cards Selected and Standard Deviations for Depletion State and Trial Type

<table>
<thead>
<tr>
<th>Depletion State</th>
<th>Advantageous</th>
<th></th>
<th>Disadvantageous</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>n</td>
<td>M</td>
<td>SD</td>
<td>M</td>
</tr>
<tr>
<td>Ego-depleted</td>
<td>24</td>
<td>9.82</td>
<td>4.10</td>
<td>7.98</td>
</tr>
<tr>
<td>Non-depleted</td>
<td>19</td>
<td>13.46</td>
<td>4.65</td>
<td>10.12</td>
</tr>
</tbody>
</table>

Information use. Because the CCT manipulated gain amount, loss amount and probability information orthogonally, an analysis examining whether information use differed between depletion conditions was conducted. To do so, the mean number cards selected for each of the eight unique rounds was submitted to a 2 Probability × 2 Gain Amount × 2 Loss Amount × 2 Depletion mixed ANOVA. Table 3 shows the main effects and interactions of the analysis on information use. The main effects of probability, gain amount, and loss amount were as one might expect; participants turned more cards when: a) there was only one loss card versus three ($M = 11.97$ vs. 8.72), b) the gain cards were worth 30 versus 10 points ($M = 10.74$ vs. 9.86), and c) the loss cards were worth -250 versus -750 ($M = 10.83$ vs. 9.86). Participants attended to all three pieces of information, but probability was the most salient as reflected by the large effect size. The main effect of depletion replicated the previous analysis that separated risk advantageous and disadvantageous rounds.
Table 3

ANOVA Summary Table for Information Use by Depletion Condition

<table>
<thead>
<tr>
<th>Source</th>
<th>SS</th>
<th>F</th>
<th>p</th>
<th>Partial $\eta^2$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Probability (P)</td>
<td>900.84</td>
<td>56.45</td>
<td>&lt;.001</td>
<td>.58</td>
</tr>
<tr>
<td>Gain (G)</td>
<td>54.01</td>
<td>4.42</td>
<td>.04</td>
<td>.10</td>
</tr>
<tr>
<td>Loss (L)</td>
<td>79.95</td>
<td>7.63</td>
<td>.009</td>
<td>.16</td>
</tr>
<tr>
<td>Depletion (D)</td>
<td>708.65</td>
<td>5.96</td>
<td>.02</td>
<td>.13</td>
</tr>
<tr>
<td>P × D</td>
<td>26.34</td>
<td>1.65</td>
<td>.21</td>
<td>.04</td>
</tr>
<tr>
<td>G × D</td>
<td>4.43</td>
<td>0.36</td>
<td>.55</td>
<td>&lt;.01</td>
</tr>
<tr>
<td>L × D</td>
<td>47.54</td>
<td>4.54</td>
<td>.04</td>
<td>.10</td>
</tr>
<tr>
<td>P × G</td>
<td>1.83</td>
<td>0.40</td>
<td>.53</td>
<td>.01</td>
</tr>
<tr>
<td>P × L</td>
<td>1.99</td>
<td>0.45</td>
<td>.51</td>
<td>.01</td>
</tr>
<tr>
<td>G × L</td>
<td>0.71</td>
<td>0.21</td>
<td>.65</td>
<td>&lt;.01</td>
</tr>
<tr>
<td>P × G × L</td>
<td>1.99</td>
<td>0.56</td>
<td>.46</td>
<td>.01</td>
</tr>
<tr>
<td>P × L × D</td>
<td>14.76</td>
<td>3.30</td>
<td>.08</td>
<td>.08</td>
</tr>
<tr>
<td>G × L × D</td>
<td>8.73</td>
<td>2.58</td>
<td>.12</td>
<td>.06</td>
</tr>
<tr>
<td>P × G × D</td>
<td>0.34</td>
<td>0.08</td>
<td>.79</td>
<td>&lt;.01</td>
</tr>
<tr>
<td>P × G × L × D</td>
<td>0.20</td>
<td>0.06</td>
<td>.82</td>
<td>&lt;.01</td>
</tr>
</tbody>
</table>

Note. All main effects and interactions shared the same degrees of freedom (1, 41).

Most relevant to this paper though is how information use interacted with
depletion level. A significant Loss Amount × Depletion interaction revealed that while
the non-depleted group ($M_{250} = 12.65$ vs. $M_{750} = 10.93$) significantly adjusted their risk-
taking as the loss amount increased, the depleted group did not ($M_{250} = 9.01$ vs. $M_{750} =
8.79$), $F(1, 41) = 10.72, p = .002, \eta^2_p = .21$ and $F(1, 41) = 0.23, p = .64, \eta^2_p < .01$,
respectively. This interaction also indicated that the non-depleted only risked more than
the depleted when the loss amount was at its lowest (-250 points) and not at its highest (-
750), $F(1, 41) = 9.20, p = .004, \eta^2_p = .18$ and $F(1, 41) = 2.85, p = .10, \eta^2_p = .07$,
respectively. Interestingly, depletion level did not interact with probability or gain
amount suggesting that both groups were sensitive to changes in this information. Lastly the previous Loss Amount × Depletion interaction was qualified by a marginally significant 3-way interaction that included Probability. Figure 2 shows that the depleted group’s insensitivity to changes in loss amount persisted regardless of changes in probability, all $F$s < 1. In contrast, the non-depleted group showed the most sensitivity to changes in loss amount when the probability of loss was high rather than low, $F(1, 41) = 16.07, p < .001, \eta^2_p = .28$ and $F(1, 41) = 2.87, p = .10, \eta^2_p = .07$, respectively.

![Error bars represent standard errors.](image)

*Figure 2.* Probability × Loss Amount × Depletion interaction indicated that the non-depleted were more sensitive to changes in loss amount and probability than the depleted.
Finally, a regression coefficient analysis (RCA; Lorch & Myers, 1990; Pfister et al., 2013) was conducted to examine information use at the subject-level. For each individual, the number of cards selected was regressed onto three dummy coded variables representing probability (1 loss card = 1, 3 loss cards = 2), gain amount (10 points = 1, 30 points = 2) and loss amount (-250 points = 1, -750 points = 2). The unstandardized beta weights were then compiled for each individual and independent samples t-tests were run to check for group differences. Similar to the group-level analysis, depleted individuals were significantly less sensitive to changes in loss amount than non-depleted individuals (see Table 4).

### Table 4

*Average Unstandardized Beta Weights Obtained by Regressing Number of Cards Selected on Probability, Loss Amount, and Gain Amount*

<table>
<thead>
<tr>
<th>Predictor</th>
<th>Depletion</th>
<th>Non-Depletion</th>
<th>t (40)</th>
<th>d</th>
</tr>
</thead>
<tbody>
<tr>
<td>Probability</td>
<td>M -2.82</td>
<td>M -3.82</td>
<td>1.14</td>
<td>0.35</td>
</tr>
<tr>
<td>Gain Amount</td>
<td>M 0.59</td>
<td>M 1.03</td>
<td>-0.56</td>
<td>-0.17</td>
</tr>
<tr>
<td>Loss Amount</td>
<td>M -0.23</td>
<td>M -1.72</td>
<td>2.07*</td>
<td>0.64</td>
</tr>
</tbody>
</table>

*p < .05.

### Discussion

It was expected that given a gambling task with the trappings of control (i.e., choice and active involvement), the ego depleted would risk significantly less than the

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4 One participant from the depletion condition selected the same amount of cards for all trial combinations resulting in no variability to predict. Therefore, she was excluded from the RCA (N = 42). Excluding this subject for the previous ANOVA model did not change the results.
non-depleted. Consistent with this Hypothesis 1, the ego depleted, on average, selected significantly fewer cards on the CCT than the non-depleted. These results extend Unger and Stahlberg’s hypothesis by showing that perceived control is sufficient and feelings of increased responsibility are not necessary to result in decreased risk-taking among the depleted. In addition, the results remained the same after controlling for possible group differences in positive and negative affect (Hypothesis 4)—two variables not accounted for by Unger and Stahlberg.

Contrary to Hypothesis 2, both groups were sensitive to changes in expected value choosing to risk more during advantageous rounds than disadvantageous ones. These results converge with previous studies showing that ego depletion does not affect sensitivity to changes in expected value (Corser, Prunier, & Jasper, 2014; Unger & Stahlberg, 2011).

Although sensitivity to changes in expected value was spared, ego depletion impaired information use on the CCT. Specifically, ego depleted participants were relatively insensitive to changes in loss amount choosing to risk a similar amount regardless of the loss value. In contrast, non-depleted participants showed signs of using all three pieces of information. One explanation for these results is that the depleted may have lacked the cognitive resources or motivation to integrate all the information after exerting self-control during the perceptual search task. Alternatively, since both loss amounts were relatively large (-250 & -750) compared to the gain amounts (10 & 30), the ego depleted might have found both losses very aversive and therefore were motivated to minimize the chances of incurring such large losses. This latter possibility fits well with
a study conducted by Carr and Steele (2010) who found that under the state of ego depletion people become more loss averse.

Although these results make interesting connections with the extant literature on risk-taking, they are not without their limitations. One limitation of Experiment 1 as well as Unger and Stahlberg’s study is that there were no manipulation checks to verify whether the CCT or the investment scenario elicited feelings of control. Similarly, these experiments lacked a control condition in which participants perceived a low level of control. Thus, Experiment 2 sought to compare the effects of ego depletion under conditions of high versus low perceived control. Furthermore, manipulation check questions were included to verify that the control manipulation was having the intended effect.
Chapter Three

Experiment 2: Manipulating Perceived Control

The primary aims of Experiment 2 were to replicate the observed decrease in risk-taking among the ego depleted found in Experiment 1 and confirm whether changes in perceived control can eliminate or reverse this pattern. To vary the level of perceived control, Experiment 2 again relied on standard manipulations used in the illusion of control literature, such as choice, active involvement, and familiarity (Langer, 1975). Following a depleting or non-depleting perceptual search task, participants played an active or passive variation of the Columbia Card Task in which superficial characteristics of the task were manipulated to induce feelings of high and low perceived control, respectively. In the active CCT, the cards had familiar symbols (see Figure 4), and participants chose which cards they wanted to turn. For the passive CCT, the cards had unfamiliar symbols (see Figure 4) and the experimenter selected which cards were turned. In both conditions, participants did not receive outcome feedback (i.e., whether they selected a losing card) until they finished choosing how many cards they liked to turn. Serving as an addendum to Hypothesis 1, the following prediction was made:

**Hypothesis 1a.** Under conditions of high perceived control (i.e., during the active CCT), the ego depleted were expected to risk less (i.e., turn fewer cards) than the non-depleted, but risk more under conditions of low perceived control (i.e., during the passive CCT). This would support Unger and Stahlberg’s contention that perceived control is an important determinant of depletion effects on risk-taking tasks. Experiment 2 also provided another opportunity to test Hypotheses 2 regarding the effect of ego depletion on expected value sensitivity.
A secondary aim was to explore a potential motivational mechanism suggested by the process model that could explain why ego depletion increases risk-taking under conditions of low perceived control. There is preliminary evidence that ego depletion increases preference for risky options because it increases approach motivation. Supporting the relationship between ego depletion and approach motivation, Schmeichel et al. (2010) showed that ego depletion increased approach motivation as assessed by self-report (Study 1) and behavioral measures (Study 3). Specifically, depleted individuals reported higher incentive sensitivity on the Behavioral Activation System (BAS) scales (Carver & White, 1994) and were more accurate at identifying reward-related cues in a visual search task. Importantly, preference for a low-stake gamble (i.e., $50 for sure versus a 50% chance of winning $100 or $0) was positively correlated with BAS scores, but not scores on a self-control scale (Tangney et al., 2004). Consequently, Schmeichel and colleagues concluded that the increased preference for the risky option was due to increases in approach motivation rather than a reduction in self-control strength. They, however, did not measure both approach motivation and gambling preferences within the same study to show that approach motivation statistically mediated the relationship between ego depletion and increased preference for the risky option. Consistent with Schmeichel et al. (2010), it was hypothesized that:

**Hypothesis 5.** Ego depletion would increase BAS scores suggesting increases in approach motivation. Furthermore, Experiment 2 sought to test whether this increase in BAS scores mediated the expected increase in risk-taking during the low-perceived control CCT.
A tertiary aim of Experiment 2 was to examine how trait self-control correlated with CCT performance. In light of Schmeichel and colleagues’ results, we wanted to confirm that trait self-control was related to the CCT. Furthermore, this provided an opportunity to examine whether ego depletion interacted with trait self-control. Imhoff et al. (2013) found that trait and state self-control interact in several contexts including risk-taking tasks. In their study, the ego depleted selected riskier gambles on the Game of Dice than the non-depleted. Importantly though, individuals with high trait self-control were driving this effect, whereas individuals with low trait self-control showed no effect of ego depletion. Thus, Imhoff and colleagues labeled this as the ironic effects of trait self-control because high self-controllers, who presumably have more self-control resources to draw upon, were actually the worst and most tempted by the risky options.

This study is of particular interest because the direction of the depletion effects on the Game of Dice fit with the perceived control hypothesis of Unger and Stahlberg (2011). During the Game of Dice, participants select one of four mixed gambles that depend on the outcome of a die roll. Two of the gambles are labeled risk disadvantageous (having negative expected values) and the other two are risk advantageous (with expected values equal to zero or greater). The key here is that rolling a die is similar to gambles and lotteries, which generally elicit feelings of low perceived control. By adding trait self-control, Imhoff et al. (2013) introduced the possibility that ego depletion effects depended on not only task features, but also trait self-control. Based on this previous literature, it was hypothesized that:

**Hypothesis 6.** Trait self-control would interact with state self-control under conditions of low perceived control just like Imhoff and colleagues found with the Game
of Dice. That is, depletion effects were expected to be most pronounced amongst individuals scoring high on trait self-control. A similar moderation analysis was conducted for the high perceived control CCT to see if a similar relationship would emerge.

**Method**

**Participants.** A total of 97 undergraduates from the University of Toledo (\(M_{\text{age}} = 19.51, SD = 2.1, 53\) females) participated in exchange for partial course credit.

**Materials**

**Self-control scale (SCS).** Developed by Tangney, Baumeister, & Boone (2004), the brief SCS consists of 13-items, such as “I do things that feel good in the moment but regret later on” and “I’m good at resisting temptation.” Participants indicated the extent to which each item represented them using a 5-point scale anchored by “not at all like me” and “very much like me.” The SCS has been demonstrated to have high test-retest reliability (\(r = .87\)) and internal consistency (\(\alpha = .85\)).

**Behavioral activation system scale (BAS).** Developed by Carver and White (1994), the 13-item BAS scale measures sensitivity to reward and approach motivation. Example items are, “When I see an opportunity for something I like, I get excited right away” and “When I want something, I usually go all-out to get it.” Participants indicated their agreement with each statement on a four-point scale ranging from “strongly agree” to “strongly disagree.” The BAS scale has been shown to have acceptable test-retest reliability (\(\alpha = .69\)).

**Ego depletion manipulation check questions.** Consistent with previous studies using ego depletion manipulations, participants answered four manipulation check
questions about task difficulty, motivation, and fatigue (e.g., Clarkson et al., 2010; Muraven & Slessareva, 2003). The perceived difficulty of the perceptual search task was measured using the following item: “How difficult was the perceptual search task for you?” (1 = very easy, 7 = very difficult). Motivation for the perceptual search task was assessed with two questions: “How important was it for you to do well on the perceptual search task?” (1 = Not at all important, 7 = Very important) and “How motivated were you to complete the perceptual search task?” (1 = Not at all motivated, 7 = Very motivated). One question measured fatigue: “How mentally exhausting was the perceptual search task for you?” (1 = Not at all exhausting, to 7 = Extremely exhausting).

Illusion of control manipulation check questions. Evidence of illusion of control was assessed using a combination of direct and indirect measures some of which have been adapted from previous studies (e.g., Dunn & Wilson, 1990; Langer, 1975). Direct measures explicitly probe for feelings of perceived control, while indirect measures are behavioral manifestations that are consistent with illusory control over chance events (e.g., estimating level of skill, confidence). The first set of control questions were administered after participants read the instructions to the game, but before beginning the game. The indirect measures were: “How confident are you that you will earn a profit (i.e., win more money than you lose) in the card game?” and “How confident are you that you will avoid turning a loss card?” (1 = Not at all confident, 7 = Very confident).

Following the card game, participants answered the following questions: “How much skill do you think is involved in the card game?” (1 = Luck, 9 = Skill), “How responsible were you for the ways in which the rounds turned out?” (1 = Not at all
responsible, 9 = Very responsible), and “To what extent can a player influence whether he or she turns a gain card?” (1 = No influence, 9 = Complete influence). The latter two questions served as direct measures of perceived control.

**CCT.** The computerized CCT described in Experiment 1 was modified to implement the illusion of control manipulation. First, all participants played the game using a physical game board and playing cards. The game board consisted of a framed cork board (46” × 36”) covered with a green felt fabric that was marked with 32 rectangles (approximately 3” × 2”) and arranged in a 4 × 8 matrix (just like the CCT program). Gain, loss, and probability information were displayed on 4” × 6” index cards in a card holder that could be easily viewed by the participant. Two different card decks containing 34 cards each were used to manipulate stimulus familiarity. One deck consisted of standard playing cards (31 black suited and 3 red aces), whereas the other deck was constructed from card stock of similar size to the playing cards. Printed on the back of the cards were several symbols from the Ge’ez script used in several Ethiopian languages (see Figure 3). These were chosen instead of Greek letters—the stimulus choice for Biner, Huffman, Curran, and Long (1998)—because the active Greek life on Toledo’s campus may have increased familiarity with the Greek symbols thereby diluting the illusion of control manipulation. A red or black rectangular sticker appeared on the down-side of the card to indicate loss and gain cards, respectively.
Procedure & Design

After participants read and signed an informed consent form, the experimenter informed participants that they were going to complete two unrelated experiments—one concerning individual differences in perceptual search skills and another regarding decision making. During the first experiment, participants completed the SCS, a two-part perceptual search task, the BAS scale, and the manipulation check questions. The perceptual search task served as the primary manipulation of self-control resources. After completing these questionnaires, participants read the instructions for the Columbia Card Task (see Appendix C) and completed eight rounds of the CCT. Each round had a unique combination of the number of loss cards (i.e., 1 or 3), the value of each gain card (i.e., $1 or $3), and the value of each loss card (i.e., -$10 or -$35). Half of the rounds were risk advantageous where the normative solution was to choose between 19-28 cards and the other half of rounds were risk disadvantageous where the normative solution dictated turning no cards. The gain and loss values were changed to make RA rounds more uniform. In the original task, the optimal solution varies widely from 4-23. By adjusting the values, we expected to reduce the variability in participants’ risk-taking.
The gain and loss amount was also changed to dollars instead of points to make the rewards more concrete and the game more affective; the game, however, was still played for hypothetical money.

During the passive CCT, the experimenter shuffled and dealt the cards after which the participant indicated how many cards they would like the experimenter to turn. Participants playing the active CCT shuffled and dealt the cards as well as decided which cards were turned. Previous research suggests that such active involvement and choice over chance events creates an illusion of control (reference). Thus, the experimental design was a $2 \times 2$ between-subjects design with the following factors: Depletion (depleted or control) and CCT version (active and passive). Following the CCT, participants completed a series of brief questionnaires asking them about the strategies they used during the gambling task and a variety of demographic questions similar to Experiment 1.

**Results**

**Perceptual search task performance.** The second part of the perceptual search task was graded for commission and omission errors. To screen for compliance with the task directions, the distribution of errors for each depletion group was inspected. Two participants from the non-depleted group were considered outliers because they scored over three standard deviation units above the mean in the number of commission errors ($z = 5.45$) or omission errors ($z = 3.17$). Two participants from the depletion condition were
labeled as outliers for having a large number of commission errors \((z = 3.22; z = 3.33)\).

In total four participants were excluded from subsequent analyses \((n = 93)\). \(^5\)

**Manipulation checks.** In addition to excluding the four outliers mentioned above for their poor performance, two additional participants were excluded because they did not comply with the instructions; one accidentally moved onto the BAS and post-depletion questionnaires before completing the perceptual search task. Accordingly, this participant was excluded for analyses involving the BAS and manipulation check questions, but included for the remaining analyses. The other participant, who was randomly assigned to the depletion condition, drank coffee with sugar after the perceptual search task; because ingesting sugar is known to reduce ego depletion effects (Gailliot et al., 2007; Molden et al., 2012), this participant was excluded from all remaining analyses. It was hypothesized that the depleted participants would rate the task as more difficult and more mentally exhausting than control participants, but the groups should not differ in their motivation to complete the perceptual search task. As predicted, Table 5 shows that depleted participants reported that the task as more difficult and marginally more exhausting than the non-depleted participants. Furthermore, the two groups were equally motivated to complete the task and thought it was equally important to do well on the task (see Table 5).

\(^5\) The same grading procedure was conducted for perceptual search tasks collected during Experiment 1, but no outliers were found.
Perceived control. The confidence scores of nine participants were not recorded because the experimenter forgot to administer the questionnaire with the two confidence questions. This reduced the sample to 83 participants (4 as perceptual search outliers, 1 coffee drinker, and 9 missing data). Responses to the two confidence measures assessed prior to playing the CCT were moderately correlated \( (r = .34) \) and therefore were averaged to create an average pre-confidence score. If the instructions alone were sufficient to produce an illusion of control, then participants who were able to deal and select their cards should report higher confidence than participants lacking this control. A 2 CCT Version \( \times 2 \) Depletion ANOVA on the pre-confidence scores returned no significant main effects or interaction. Thus, prior to playing the CCT, the active CCT group \( (M = 3.66, SD = 1.23) \) did not differ from the passive CCT group \( (M = 3.95, SD = 0.96) \) in confidence, \( F(1, 79) = 1.34, p = .25, \eta^2_p = .02 \). The depletion manipulation did not affect confidence scores, \( F(1, 79) = 0.003, p = .96, \eta^2_p < .01 \), and did not interact with the version of the CCT, \( F(1, 79) = 0.79, p = .42, \eta^2_p = .01 \).
Post-perceive control questions. All 92 participants were included in this analysis. The luck vs. skill, responsibility, and influence items were moderately to highly correlated ($\alpha = .70$), therefore they were combined to form a single measure of perceived control and submitted to a 2 CCT Version $\times$ 2 Depletion ANOVA. None of the effects were significant, all $F$’s(1, 88) $< 1$, $p$s $> .36$, $\eta^2_p \leq .01$.

Risk-taking on the CCT. The average number of cards selected across the eight CCT rounds was positively skewed ($z = 4.55$) and significantly non-normal, Shapiro-Wilks (92) = .93, $p < .001$. Applying a square root transformation to each round’s distribution of scores and then averaging these transformed scores corrected for this non-normality, $z = 2.36$, S-W (92) = .98, $p = .08$. Using this square root transformation, the average the number of cards selected for the four RA and four RD rounds were submitted to a 2 Depletion $\times$ 2 CCT Version $\times$ 2 Round mixed ANOVA with the first two factors as between-subjects variables and round (RA vs. RD) as a within-subjects variable.

Similar to Experiment 1, both depletion groups were expected to turn more cards on the RA rounds than the RD rounds. Most importantly, a significant Depletion $\times$ CCT Version interaction was hypothesized. Specifically, depleted individuals were expected to risk more than controls during the active CCT (under conditions of low perceived control), but risk less than controls during the passive CCT (under conditions of high perceived control). All inferential statistics were based on analyses conducted with the square root transformation with the descriptive statistics displaying the original units.

Consistent with Experiment 1, participants turned more cards during risk advantageous rounds ($M = 11.04$) than risk disadvantageous rounds ($M = 6.91$), $F(1, 88) = 88.24$, $p < .001$, $\eta^2_p = .50$. Moreover, this effect of round type did not change
depending on the depletion condition evidenced by a non-significant Round × Depletion interaction, $F(1, 88) = 0.18, p = .68$, $\eta^2_p < .01$. In other words, both groups were equally sensitive to changes in expected value similar to Experiment 1.

Critically though, the ANOVA returned a significant Depletion × CCT Version interaction, $F(1, 88) = 4.48, p = .04$, $\eta^2_p = .05$. Depleted individuals ($M = 7.51$) selected fewer cards than non-depleted individuals ($M = 10.09$) under conditions of high perceived control; descriptively, this difference was reduced under conditions of low perceived control and the trend reversed such that the non-depleted ($M = 8.59$) turned fewer cards than the depleted ($M = 9.32$). Tests examining the simple effects of depletion within each version of the CCT revealed that the depleted risked significantly less than the non-depleted in the active CCT, but this difference was eliminated in the passive CCT, $F(1, 88) = 5.52, p = .02$, $\eta^2_p = .06$ and $F(1, 88) = 0.42, p = .52$, $\eta^2_p < .01$, respectively. The remaining main effects and interactions were not significant: Depletion main effect, $F(1, 88) = 1.45, p = .23$, $\eta^2_p = .02$; CCT version main effect, $F(1, 88) = 0.07, p = .79$, $\eta^2_p < .01$; Round × CCT Version, $F(1, 88) = 0.05, p = .82$, $\eta^2_p < .01$; and Round × CCT Version × Depletion, $F(1, 88) = 0.10, p = .75$, $\eta^2_p < .01$.

**Changes in approach motivation.** Recall that Schmeichel et al. (2010) found that following a depletion manipulation of suppressing emotions, participants scored significantly higher on the BAS scale compared to non-depleted controls. The 13 BAS items ($\alpha = .86$) were summed so that higher scores reflected lower approach motivation. The distributional shape of BAS scores for the non-depleted and depleted groups were examined using standardized skewness values and the Shapiro-Wilks test. Together these values indicated that both distributions were positively skewed and significantly non-
normal, non-depleted: \( z = 2.75, S-W (46) = 0.93, p = .008 \); depleted: \( z = 2.76, S-W (45) = 0.94, p = .02 \). A natural log transformation of the raw scores was applied to correct the non-normality and was used for the subsequent independent samples \( t \)-test. With the transformation, lower BAS scores still reflected higher approach motivation. Consistent with Schmeichel et al. (2010), the depleted \((M = 25.33)\) reported marginally higher approach motivation than the non-depleted \((M = 27.30), t(89) = 1.56, p = .06, \) one-tailed, \( d = .33 \).

**Relationships among trait self-control, BAS, and CCT.** Schmeichel et al. (2010) found that ego depletion increased risky choices in a small-stakes gamble and attributed it to increases in approach motivation rather than lapses in self-control. In support of this view, they found that betting behavior was positively correlated with BAS scores, but not trait self-control. To check whether risk-taking on the CCT was related to self-control, trait self-control scores \((\alpha = .82)\) were correlated with the square root transformation of the number of cards selected across all eight rounds. Results indicated that higher trait self-control was significantly associated with less risk-taking on the CCT, \( r (89) = -0.23, p = .03 \). In contrast, BAS scores were not correlated with CCT performance in either the passive or active version conditions, \( r (44) = -.18, p = .23 \) and \( r (43) = .07, p = .63 \), respectively. As a result, the planned mediational analysis assessing whether approach motivation mediated the relationship between ego depletion and CCT performance was not conducted.

**Ego depletion effect moderated by trait self-control.** Recall that Imhoff et al. (2013) found that ego depletion affected individuals scoring high on trait self-control, but not those scoring low on trait self-control. This moderation effect was observed for the
Game of Dice, a risk-taking task with low perceived control. We predicted that trait self-control would moderate the ego depletion effect on the passive CCT that promotes low perceived control. Using PROCESS (Hayes, 2013), two moderation analyses (for active and passive versions of the CCT) were conducted in which the square root transformation of the number of cards selected was regressed onto ego depletion condition, trait self-control (both mean-centered), and their interaction.

For the passive CCT (low perceived control), the overall regression model was significant, $R^2 = .17, F(3, 42) = 3.93, p = .01$. Critically, the Depletion × Trait Self-Control interaction was significant ($b = .06, SE = .02, p = .01$), while the effects of trait self-control and depletion were not significant, $b = -.003, SE = .01, p = .78$ and $b = .11, SE = .16, p = .48$, respectively. Figure 4 (left pane) plots the nature of this interaction, and tests of conditional effects confirm that ego depletion increased the number of cards selected for high self-controllers ($b = .55, SE = .21, p = .01$), but not moderate or low self-controllers, $b = .11, SE = .16, p = .48$ and $b = -.33, SE = .26, p = .20$, respectively. Consistent with Imhoff and colleagues, the effect of ego depletion on risk-taking depended on trait self-control and affected individuals high on trait self-control.

A similar moderation model using data from the passive CCT (high perceived control condition) was marginally significant, $R^2 = .14, F(3, 42) = 2.81, p = .051$. In contrast to the previous analysis, the Depletion × Trait Self-Control was not significant (see Figure 4 right pane), $b = -.0004, SE = .02, p = .98$. Instead, the variation in risk-taking was explained by two marginally significant effects of depletion condition and trait self-control, $b = -.34, SE = .21, p = .13$ and $b = -.02, SE = .01, p = .09$, respectively.
Thus, depletion and increases in trait self-control were both associated with decreased risk-taking in the active CCT.

![Graph showing the effect of ego depletion on risk-taking in the passive and active CCT](image)

**Figure 4.** Trait self-control moderates the effect of ego depletion on risk-taking in the passive CCT (left), but not in the active CCT (right).

**Discussion**

According to Hypothesis 1a, an interaction between CCT version and depletion condition was expected with ego depletion leading to increased risk-taking during the passive CCT (low perceived control condition) and decreased risk-taking during the active CCT (high perceived control condition). Consistent with Experiment 1, the ego depleted risked less than the non-depleted during the active CCT. Descriptively, this pattern reversed with the passive CCT, such that the ego depleted tended to risk more than the non-depleted controls. Thus, Hypothesis 1a was partially confirmed.
Following up on a study conducted by Schmeichel et al. (2010), the present work also tested whether approach motivation could explain why ego depletion sometimes leads to increased risk-taking. In their study, they found that ego depletion increased approach motivation (i.e., BAS scores) and increased preference for a risky option versus a sure-thing option. They hypothesized that the effect of ego depletion on the risk preference was mediated by increased approach motivation. In the current study, ego depletion increased approach motivation, but this effect was much smaller than the effect reported by Schmeichel et al. (2010). Furthermore, approach motivation was unrelated to CCT performance and therefore could not mediate the effect of ego depletion on risk-taking in the CCT. Overall then, Hypothesis 5 was partially confirmed in that ego depletion increased approach motivation, but BAS scores were unrelated to the CCT.

One criticism may be that Experiment 2 did not have sufficient power to detect the effect of approach motivation on CCT performance. Using a much larger sample ($n = 489$), recent research, however, has also failed to find a relationship between BAS scores and CCT performance (Buelow, 2014). The lack of correlation between the CCT and BAS scores probably reflects the domain-specific nature of risk-taking (Figner & Weber, 2011). Nevertheless, the mediational role of approach motivation is still a very viable hypothesis that future research should address with the understanding that choice of the risk-taking task is important.

Experiment 2 also examined whether trait self-control could explain some of the variance in risk-taking on the CCT. In support of Hypothesis 6, individuals who scored high on a self-report measure of self-control were the most affected by the ego depletion manipulation while playing the passive version of the CCT. As a result, depleted high
self-controllers risked significantly more than non-depleted high self-controllers. This depletion effect was not present among moderate and low self-controllers. This ironic effect of trait self-control whereby depleted high self-controllers are most tempted by risk is consistent with Imhoff et al. (2013) who found a similar effect on the Game of Dice, a gambling task much like other risk-taking tasks labeled as low perceived control.6 Extending these results, we found that trait self-control does not moderate the ego depletion effect on the active CCT. These results provide a more nuanced account of ego depletion effects on risk-taking. They also suggest that some of the previous inconsistencies could be due to the task characteristics or the proportion of high and low self-controllers in a sample.

Although Experiment 2 advances our understanding of when ego depletion is likely to produce decreased risk-taking and who is most likely affected by ego depletion manipulations, it is not without its limitations. One limitation is that despite using standard manipulations to elicit the illusion of control, none of the manipulation check questions asking about control showed the expected effect. That is, there were no differences between the active and passive versions of the CCT in terms of confidence or degree of control they felt that had over the outcomes of CCT. It is still possible that the perceived control manipulation worked while participants played the game, but the effect was not strong enough to appear on the indirect and direct measures. Previous research

6 One might argue that the apparent “ironic” effect could be due low self-controllers exhibiting a floor effect. It is plausible that low self-controllers have a lower absolute level of self-control resources available regardless of depletion state. As a result, they may show less change in response to a depletion task than high self-controllers who have more absolute resources. In other words, high self-controllers may have more to lose and therefore show a more pronounced depletion effect.
on the illusion of control offers some explanations as to why the manipulation check questions failed to detect a control effect.

First, the nature of the gambling task (e.g., single-shot versus multi-shot) has been shown to moderate the illusion of control effect. For example, Koehler, Gibbs, and Hogarth (1994) found that the illusion of control effect is present when betting on the outcome of a single chance outcome, but vanishes with repeated plays of the same gamble. Technically, the CCT in Experiment 2 should be construed as eight single-shot gambles because each round was a unique combination of gain amount, loss amount, and probability of loss. Interestingly, previous research has yielded an illusion of control effect using similar multiple, unique single-shot gambles (Budescu & Bruderman, 1995). However, because there is minimal variation among the CCT rounds (only two levels of each factor vary), participants may have perceived the collection of CCT rounds as a multi-shot gamble (i.e., the same gamble repeated multiple times), which would reduce the illusion.

Second, other situational and individual difference variables have been shown to influence the illusion of control effect. Regarding individual differences, illusion of control effects tend to emerge among only individuals scoring high in desire for control (Burger, 1986; Burger & Cooper, 1979). It is possible that an illusion of control effect is present, but it may only be exhibited among individuals with a strong motivation for control. Unfortunately, we did not measure desire for control in this study, but future research could pursue this interaction effect. Situationally, some evidence suggests that turning people’s attention to the objective probabilities of chance games eliminates the illusion of control (Bouts & Van Avermaet, 1992). During the CCT, the probability of
losing changed from round-to-round making the chance component more salient; this may have weakened the illusion of control effect. The multi-trial nature of the CCT combined with the probability switching may have been sufficient to dilute the illusion of control effect, especially on the direct and indirect questions probing for control.

Lastly, there is evidence suggesting that some of the manipulation check questions tend to yield smaller illusion of control effects and thus, we may have lacked the power to detect these effects. In particular, most of the post-perceived control questions were direct questions and explicitly asked participants whether they controlled the outcomes of the task. Presson and Benassi (1996) found evidence that these questions yield a smaller effect size as opposed to indirect measures assessing confidence or amount wagered (for an exception see, Stefan & David, 2013). For these direct measures, the present study may not have had sufficient power to detect this effect.
Chapter Five

Experiment 3: Decisions from Experience Versus Description

The final experiment explored another task characteristic that could explain previous inconsistencies in the literature on ego depletion and risk-taking. As mentioned in Chapter 1, most of the studies in Appendix A reporting that ego depletion increases risk-taking have used behavioral measures of risk that would be classified as decisions from experience (Freeman & Muraven, 2010; Goodwin, 2012; Molet et al., 2012). In contrast, all but one of the studies reporting increased risk aversion among the ego depleted have used risk measures relying on decisions from description (e.g., Carr & Steele, 2010; Unger & Stahlberg, 2011).

Recall that laboratory tasks requiring decisions from experience do not explicitly provide participants with information about the payoff contingencies; instead they must learn this information by making choices and receiving feedback. The additional learning demand associated with decisions from experience may be particularly difficult for ego depleted individuals. Having exerted self-control resources during the initial task, the ego depleted are likely to have fewer cognitive resources to learn the outcome distributions and to resist the temptation of selecting a larger, although less probable reward. Findings demonstrating that ego depletion adversely affects executive functions (Schmeichel, 2007) and analytic problem solving (Schmeichel et al., 2003) suggest that self-control resources could impact learning on tasks involving decisions from experience.

To examine how the availability of self-control resources impacts experienced-based and description-based decisions, Experiment 3 used two versions of the Columbia Card Task. The description-based version was similar the CCT used in Experiments 1
and 2 in which participants could read the gain, loss, and probability information. In contrast, the experienced-based version of the CCT was designed so that the outcome and probability information were not displayed. Additionally, the cards were no longer displayed in an array as that would reveal probability information; instead, participants only saw one card that was purportedly the next card in a full deck of cards, which consisted of an unknown amount of cards. Participants simply decided whether to continue drawing or to stop and collect their earnings for that round (much like the BART). While participants in the description-based CCT played each round once, experience-based participants were able to sample each CCT round as many times as they would like until they were ready to make a final decision. This final decision was compared to the choices of participants completing the single, description-based task. The dependent variable was the number of cards selected during the final decision round.

**Hypothesis 7.** It was hypothesized that ego depleted participants would choose more cards than non-depleted controls in the experienced-based CCT, but fewer cards than controls in the description-based CCT. Importantly, results for the description-based CCT should replicate the results obtained in Experiment 1 and 2 showing relative risk aversion among the ego depleted.

**Hypothesis 8.** Experiment 3 also offered an opportunity to explore whether the predicted differences on the experienced-based CCT were due to the ego depleted taking fewer samples than non-depleted. The consequence of taking small samples is that participants are simply less likely to obtain a representative sample of the outcome distribution and in turn underweight rare events (e.g., 1/32 chance of turning a loss card on one round of the CCT). Previous research suggests that this under-sampling is one of
the reasons why there is a description-experience gap (Hertwig et al., 2004). It was predicted that the ego depleted would take fewer samples and move to the final decision sooner than the non-depleted controls.

**Method**

**Participants.** University of Toledo undergraduates \((N = 184)\) enrolled in Introductory Psychology participated in exchange for partial course credit.

**Materials**

**Perceptual search task.** The same materials and directions used for Experiments 1 and 2 were included in Experiment 3. Participants were given 6 minutes to complete each part instead of the previous 8 minutes.

**Stroop task.** Consistent with previous depletion studies (e.g., Bruyneel et al., 2009; Muraven, Rosman, & Gagne, 2007; Wallace & Baumeister, 2002), participants completed one of two modified Stroop tasks (Stroop, 1935). Both tasks required participants to identify the color in which a word (i.e., red, yellow, green, or blue) was printed. The non-depleting Stroop task consisted of 80 congruent trials in which the word and font color matched (e.g., the word ‘red’ printed in the color red). In contrast, the depleting Stroop task consisted of a high proportion of incongruent trials in which the color and word mismatched (e.g., the word ‘red’ printed in the color yellow). Additionally, participants in this depletion condition were instructed to indicate the word rather than the color whenever a word appeared in the color blue. This exception occurred on 15 out of the 80 trials. The remaining trials consisted of 45 incongruent trials and 20 congruent trials.
Participants completed 12 practice trials and 80 trials of their respective depletion condition. In both modified Stroop tasks, trial order was completely randomized for each participant. During each trial, a fixation stimulus (XXXXXXXXXX) appeared followed by a word (RED, YELLOW, GREEN, or BLUE) presented in the color red, yellow, green or blue. Participants responded to each stimulus by pressing the j, k, l, or ; computer keys which corresponded to the following colors: red, yellow, green, and blue, respectively. Directly underneath the target stimulus in white font, the words red, yellow, green, and blue appeared in this order to remind participants of the color-to-key mapping. Measures of reaction time and accuracy were recorded.

**Manipulation check questions.** Following the Stroop and perceptual search tasks, participants answered four manipulation check questions similar to Experiment 2. These questions and their respective response scale were as follows: “How difficult were the tasks of Study 1 for you?” (1 = very easy to 7 = very difficult), “How much effort did you put into the tasks of Study 1?” (1 = none at all to 7 = a great deal), “How mentally exhausting were the tasks of Study 1 for you?” (1 = not at all exhausting to 7 = very exhausting), and “How motivated were you to complete the tasks of Study 1?” (1 = not at all motivated to 7 = very motivated).

**BIS/BAS scales.** To be more consistent with the procedure described by Schmeichel et al. (2010), Experiment 3 included measures of behavioral inhibition and activation rather than just the BAS scale like Experiment 2. It seemed plausible that the small effect of ego depletion on approach motivation obtained in Experiment 2 could have been due to these slight differences in measurement. Depletion was not expect to affect BIS scores though because Schmeichel et al. (2010) found no such effect when
using an emotion suppression task. Thus, the purpose of adding the BIS scale was to determine whether the effect of ego depletion on approach motivation was partially driven by methodological differences. The BIS scale (α = .74) consists of 7-items measuring sensitivity to punishment (Carver & White, 1994). Participants indicated the extent to which they agreed with statements, such as “I worry about making mistakes,” and “Criticism or scolding hurts me quite a bit” on a four-point scale ranging from “strongly agree” to “strongly disagree.” The 20 items of the BIS/BAS scales were presented in a random order for each participant.

**Description- and experience-based CCT.** The description-based CCT was essentially identical to the previous active CCT versions used in Experiments 1 and 2. Participants were presented with an array of 32 cards and information regarding the gain amount, loss amount, and probability of loss. Participants turned as many cards as they would like and then pressed the enter key to move to the next round. In contrast, the experience-based CCT presented participants with a single “deck” of cards to sample from which had the same gain-loss contingencies. Participants, however, were not informed about the gain amount, loss amount, or probability of loss. Instead they had to learn this information by sampling the decks. Participants could sample a deck as many times as they would like before moving onto a final round. Prior to playing, all participants received instructions on how to play (see Appendices D and E). In both versions, a chime of a cash register played for each gain card turned and a boing sound (similar to the sound of a spring) played when a loss card was turned. These sound effects were added to make the task more engaging and increase affective reactions.
**Procedure and Design**

Participants signed an informed consent form and were told they would be participating in two separate studies. During the first study, participants completed the depleting or non-depleting versions of the perceptual search task and Stroop task. The order of these tasks was counterbalanced across participants. Two depletion tasks were implemented because the experienced-based CCT required extra time, and there was a concern that participants might recover from only one depletion task midway through the CCT. Increasing the dosage of the depletion manipulation was also expected to enhance some of the effects found in Experiments 1 and 2.

Next, they completed the BIS/BAS scales and manipulation check questions—the order of these was also counterbalanced. The second study began with the instructions for the experience- or description-based version of the CCT (see Appendices D and E) followed by two practice rounds. Subsequently, participants played five rounds of their respective CCT.

Because the sampling feature of the experienced-based CCT required additional time, participants played only five out of the eight rounds from Experiment 2. Specifically, they played two risk advantageous rounds (i.e., RA1: 1 loss card, $1 gain, and -$10 loss and RA2: 3 loss cards, $3 gain, and -$10) and two disadvantageous rounds (RD1: 1 loss card, $1 gain, and -$35 loss and RD2: 3 loss cards, $3 gain, and -$35 loss). Another RA round was included as a rigged trial so that participants turned a loss card upon drawing three cards. The purpose of the feedback was to remind participants that losses were possible. The four remaining rounds were also rigged so that participants could turn the maximum number of cards the deck allowed (i.e., 31 or 29) before lastly
encountering a loss card. These trials were rigged so that we could capture participants’ full range of risk-taking behavior. Participants in the description-based CCT played each round once, whereas participants in the experienced-based CCT had as sampling period followed by a final decision round. The order of the rounds was counterbalanced across participants with exception that the one loss round was always the third round out of the five. Both versions of CCT were presented using DirectRT. The experimental design was a $2 \times 2$ between-subjects design with the following factors: Depletion (depleted or control) and CCT version (description-based or experienced-based).

Lastly, participants completed the same measure of trait self-control as in Experiment 2 and a questionnaire about the CCT. A subset of participants ($n = 128$) also completed 15 trials of the Balloon Analogue Risk-Task (BART) modeled after Lejuez et al. (2002) and Freeman and Muraven (2010). During the BART, participants pumped a virtual balloon receiving one cent for each pump. For each additional pump, there was a chance the balloon would “pop” and the participant would lose what he or she had gained.

**Results**

**Depletion task performance.** For data screening purposes, participants who performed three standard deviation units above or below their respective group mean were defined as outliers and excluded. Like Experiment 2, performance on the perceptual search task was evaluated by calculating the number of omission and commission errors made on the second passage. Seven participants were identified as outliers for committing a high number of either commission or omission errors (3 non-depleted and 4 depleted). Stroop performance was evaluated by analyzing the accuracy or the
percentage of correct responses out of the 80 trials. One additional participant from the non-depletion condition fit the exclusion criterion. Lastly, due to a programming error, one participant completed both versions of the Stroop task and then the non-depleting perceptual search task. This participant and the other eight participants were excluded from all subsequent analyses leaving 86 non-depleted and 89 depleted participants.

Responses to the four manipulation check questions measuring task difficulty, effort, mental exhaustion, and motivation were then analyzed to examine group differences. Consistent with Experiment 2, the ego depleted found the tasks more difficult, effortful, and exhausting than the non-depleted (see Table 6). Both groups were equally motivated during the task.

Table 6.

<table>
<thead>
<tr>
<th>Measure</th>
<th>Depletion</th>
<th>Non-Depletion</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>M</td>
<td>Mdn</td>
</tr>
<tr>
<td>Difficulty</td>
<td>2.79</td>
<td>2.0</td>
</tr>
<tr>
<td>Effort</td>
<td>5.42</td>
<td>6.0</td>
</tr>
<tr>
<td>Exhaustion</td>
<td>2.75</td>
<td>2.0</td>
</tr>
<tr>
<td>Motivation</td>
<td>5.20</td>
<td>5.0</td>
</tr>
</tbody>
</table>

*Note.* Depletion $n = 89$ and non-depletion $n = 86$; *p* < .01, ** $p$ < .001, one-tailed.

**Approach motivation and depletion.** Following the Stroop and perceptual search tasks, 70 non-depleted and 71 depleted participants completed the 13-item BAS scale. These items were summed to form an overall measure of approach motivation with lower scores reflecting higher approach motivation. The distribution of scores for each group was normal based on standardized skewness values and S-W tests, depleted:
S-W (71) = 0.98, \( p = .23 \) and non-depleted: S-W (70) = 0.98, \( p = .49 \). A non-significant Levene’s test indicated that the homogeneity of variance assumption was valid, \( F(1, 139) = 0.001, p = .98 \). Descriptively, the depleted group (\( M = 22.61 \)) reported higher approach motivation than the non-depleted group (\( M = 23.42 \)). However, an independent t-test examining these group differences was not significant, \( t(139) = 0.99, p = .16 \) (one-tailed), \( d = 0.17 \).

**CCT performance.** The primary dependent variable, the average number of cards selected across the four rounds, was shown not to meet the assumptions of normality or homogeneity of variance. Similar to Experiment 2, a square root transformation was applied to each of the rounds and then averaged to form an overall measure of risk-taking on the CCT. Although this transformation alleviated problems with non-normality, it did not eliminate the significant heterogeneity of variance revealed by a Levene’s test, \( F(3, 171) = 5.72, p = .001 \). Consequently, two Mann-Whitney U tests were conducted on the raw CCT scores; one tested for depletion differences in the experienced-based CCT and the other tested for group differences in the description-based CCT.

The descriptive statistics for the four between-subjects conditions are displayed in Table 7. For the experienced based CCT, the depleted and non-depleted groups did not differ, \( U = 861, z = -.37, p = .35 \) (one-tailed), \( r = -0.04 \). Replicating Experiments 1 and 2 though, the non-depleted risked significantly more than the depleted group in the description-based CCT, \( U = 789, z = -1.83, p = .03 \) (one-tailed), \( r = 0.19 \).
Table 7

Average Number of Cards Selected on the Final Round of the CCT by Depletion Condition and CCT Version

<table>
<thead>
<tr>
<th>Condition</th>
<th>Experience CCT</th>
<th>Description CCT</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>n</td>
<td>M</td>
</tr>
<tr>
<td>Depleted</td>
<td>46</td>
<td>7.72</td>
</tr>
<tr>
<td>Non-depleted</td>
<td>44</td>
<td>7.67</td>
</tr>
</tbody>
</table>

Table 7 also shows an experience-description gap in which participants selected more cards in the description-based CCT than the experience-based CCT. A Mann-Whitney U test confirmed that participants were riskier in the description-based CCT, $U = 5,558, z = 5.17, p < .001, r = 0.39$.

For the experienced-based CCT, the number of sampled decks was recorded and then averaged. Table 8 shows the summary statistics for the average number of samples taken by each group. A Mann-Whitney U test revealed that both groups sampled an equal amount, $U = 919, z = -1.12, p = .26, r = -0.12$.

Table 8

Average Number of Decks Sampled Prior to the Final Round of the Experienced-Based CCT by Depletion Condition

<table>
<thead>
<tr>
<th>Condition</th>
<th>n</th>
<th>M</th>
<th>SD</th>
<th>Mdn</th>
<th>Minimum</th>
<th>Maximum</th>
</tr>
</thead>
<tbody>
<tr>
<td>Depleted</td>
<td>46</td>
<td>1.40</td>
<td>0.50</td>
<td>1.25</td>
<td>1.0</td>
<td>3.0</td>
</tr>
<tr>
<td>Non-depleted</td>
<td>44</td>
<td>1.64</td>
<td>0.84</td>
<td>1.38</td>
<td>1.0</td>
<td>4.5</td>
</tr>
</tbody>
</table>

**Trait self-control and CCT.** Because ego depletion decreased risk-taking on the description-based CCT in Experiments 1-3, an additional analysis was conducted to
examine the relationship between trait self-control and CCT performance in Experiment 3. Identical to Experiment 2, a square root transformation was applied to each round’s distribution and then averaged. This average of the transformed scores was normally distributed for both groups, non-depleted: S-W (44) = 0.95, \( p = .06 \) and depleted: S-W (46) = 0.98, \( p = .52 \). Additionally, the variances between the depletion groups were equal, \( F(1, 88) = 2.41, p = .12 \).

This dependent variable was then regressed onto trait self-control, depletion condition (both mean-centered), and their interaction. The moderation model was not significant, \( F(3, 86) = 1.50, p = .22, R^2 = .05 \). However, depletion fell just short of conventional significance levels mirroring the Mann-Whitney U results, \( b = -.48, SE = .24, p = .051 \). Neither trait self-control nor the interaction term was significant, \( b = -.01, SE = .01, p = .65 \) and \( b = .02, SE = .03, p = .45 \), respectively. Surprisingly, trait self-control was not significantly related to the number of cards turned in the description-based, \( r (88) = -.02 \), or experienced-based CCT, \( r (83) = -.10 \).

**BART.** Of the 128 participants completing the BART, two participants from the depletion group were excluded for their high number of commission errors on the perceptual search task (>3 SD units above the group mean). One non-depleted participant was excluded because he popped every balloon so the adjusted risk-taking measure could not be computed. The dependent variable, the average number of pumps on uncensored trials, was positively skewed for the non-depleted group, but not the depleted group, S-W (62) = 0.94, \( p = .006 \) and S-W (65) = 0.97, \( p = .06 \), respectively. Variances between the two groups was equal, evidenced by a non-significant Levene’s test, \( F(1, 125) = 1.16, p = .28 \). A square root transformation applied to the mean number
of pumps reduced the skew and non-normality, S-W (62) = 0.98, p = .41 and S-W (65) = 0.98, p = .56.

Subsequently, a moderation analysis was conducted using depletion condition and trait self-control (both mean centered) as predictors and the transformed BART scores. The overall model was significant, $F(3, 121) = 3.67$, $p = .01$, $R^2 = .08$. Increased trait self-control was associated with decreased risk-taking on the BART, $b = -.05$, $SE = .02$, $p = .006$. At the same time, the depleted pumped the balloons less than the non-depleted, $b = -.59$, $SE = .29$, $p = .045$. The interaction was not significant, $b = .01$, $SE = .04$, $p = .82$.

Discussion

A review of the literature on ego depletion and risk-taking revealed another task feature that could explain previous inconsistencies. Specifically, studies reporting that depletion increased risk-taking tended to use tasks requiring decisions from experience, while studies reporting decreased risk-taking used tasks involving decisions from description. The CCT was adapted to create two versions—the usual description-based version and an experienced-based version. Similar to Experiments 1 and 2, the ego depleted risked significantly less than the non-depleted on the description-based CCT. Contrary to Hypotheses 7 and 8, ego depletion did not affect risk-taking in the experienced-based CCT or sampling behavior. These results suggest that the description-versus experienced-based distinction does not seem to explain the previous inconsistencies regarding depletion.

Although no depletion differences were obtained in the experienced-based CCT, there was evidence of an “experience-description gap” in which risk preferences changed depending on whether participants had to learn or read about the reward contingencies.
Participants were willing to take more risks in the description-based CCT than the experienced-based CCT. These results at first may seem inconsistent with previous research showing that people tend to underweight rare events during experienced-based tasks and overweight them during description-based tasks (Hertwig & Erev, 2009). Following this research, one might expect that the experienced-based CCT should lead to greater risk-taking because participants might underweight the small loss probability (starting at 3% or 9% and gradually increasing); meanwhile for the description-based CCT, one might expect overweighting of small probabilities and decreased risk-taking.

Subsequent research on the experience-description gap, however, has revealed that not all experience-based tasks are the same and they can lead to different risk preferences. Experienced-based choices have been studied using three paradigms. In the sampling paradigm, participants freely sample two gambles and decide when they would like to move onto the final round; the outcome of this final round determines their final payoff. The experienced-based CCT was modeled after this paradigm. In contrast, during the full and partial feedback paradigms, each outcome counts towards the participants’ payoff and after each choice the outcome of both options are presented (full feedback) or only the outcome of the selected option is shown (partial feedback). In a direct comparison between these three experience-based paradigms, Camilleri and Newell (2011) found that the sampling paradigm led to risk-averse choices, while both feedback paradigms elicited choices that are more consistent with the underweighting of small probabilities. This finding may explain why participants were relatively risk averse in the experienced-based CCT versus the description-based version. Another reason for this pronounced risk-aversion is that the CCT (and BART) are dynamic risk tasks with
much more uncertainty than the monetary gambles used in the typical studies examining the “experience-description gap.”

Experiment 3 also provided an opportunity to examine if trait self-control related to the description-based CCT as was done in Experiment 2. Similar to the results found in the high perceived control CCT, ego depletion predicted decreases in risk-taking on the CCT and this did not interact with trait self-control. Unlike Experiment 2 though, trait self-control was unrelated to CCT performance in Experiment 3.

This discrepancy may very well be due to slight methodological differences between the CCT in Experiments 2 and 3. The primary differences between these two versions is that in Experiment 2 participants played eight live rounds in which they selected which cards to turn, while in Experiment 3 participants played four rounds on the computer and were unable to select which cards to turn; instead they just decided whether to turn a card or to stop. If they turned a card, one of the cards in the array would be turned over—the order/location of these cards was fixed across participants. Importantly in both CCT versions, participants were actively involved in the game more so than the low perceived control group who merely stated the number of cards they wanted to turn. These results suggest that the effect of trait self-control and depletion on risk-taking may be task-dependent—an issue that will be further discussed in the general discussion.

Participants also completed the BART in an attempt to test for convergent results with Freeman and Muraven (2010). In their study, trait self-control and depletion condition independently predicted risk-taking, and there was no interaction. Specifically,
higher trait self-control predicted decreased risk-taking while depletion predicted increased risk-taking.

Consistent with Freeman and Muraven (2010), trait self-control was negatively related to risk-taking in Experiment 3. In contrast though, depletion in Experiment 3 led to decreased risk-taking. This inconsistency could be due to slight procedural differences between these two studies. For example, Freeman and Muraven had participants perform only one depletion task (i.e., attention control) and then play 20 rounds of the BART, which was incentivized. In the present study, participants completed two depletion tasks (i.e., ‘e’ cross-out and Stroop) followed by the CCT and 15 BART trials without incentives.

These methodological differences aside, the BART results contribute to our understanding of ego depletion effects in at least two ways. First, the BART is another experience-based task, and contrary to predictions (Hypothesis 7), the depleted risked less than the non-depleted. This is further support that the direction of ego depletion effects do not appear to depend on the experienced versus descriptive nature of the risk-taking task.

Second, the results suggest that depletion effects may change depending on the number of intervening tasks. In one respect, participants in Experiment 3 should be more depleted having exercised self-control on the Stroop task, the e-cross out, and presumably the CCT. If this were true, then one might expect a larger depletion effect than the one reported by Freeman and Muraven (2010) and especially not an effect in the opposite direction. On the other hand, it is plausible that the decreased risk-taking may reflect a strategy of the ego depleted to conserve their remaining resources. Consistent with this
hypothesis, the ego depleted have been shown to exert less effort during one self-control task when expecting another demanding one (Muraven et al., 2006; Tyler & Burns, 2009). The implications of this conservation hypothesis will be further developed in the general discussion.
Chapter Six

General Discussion

The primary goal of this work was to understand the conditions under which ego depletion should lead to increased or decreased risk-taking relative to a non-depleted control group. A secondary aim, no less important, sought to examine some potential cognitive and motivational mechanisms outlined by the process model (Inzlicht & Schmeichel, 2012) that could explain why ego depletion changes risk preferences. Each of these aims will be considered in turn along with possible limitations and ideas for future research. The last section will discuss the relationship between trait and state self-control as well as how the present work connects to the broader literature on self-control and risk-taking.

Task Features Moderating Ego Depletion Effects

Regarding the first goal, Experiments 1-3 showed that the ego depleted risked significantly less than the non-depleted under conditions aimed at increasing perceived control. In contrast, under conditions expected to reduce perceived control, such as Experiment 2 and previous studies (e.g., Freeman & Muraven, 2010; Molet et al., 2012), the ego depleted became more risk-seeking than the non-depleted. Together these studies suggest that perceived control over the risk-taking task moderates the ego depletion effect. Two related questions that remain are: Why does ego depletion decrease risk-taking? And why does perceived control seem to predict whether ego depletion leads to increased or decreased risk-taking?

One explanation is that the observed decrease in risk-taking among the ego depleted reflects a strategy to avoid further expenditure of self-control. This motivation
is particularly strong under conditions of high perceived control. A similar hypothesis was proposed by Unger and Stahlberg (2011) when they argued that the ego depleted “will refrain from risky options because they fear that their residual self-control does not allow for sufficient coping with the negative feelings (i.e., disappointment or regret) caused by negative outcomes” (p. 30). Here they add that ego depleted individuals are concerned about the possibility of having to exercise more self-control in the form of emotion regulation (e.g., having to cope with negative outcomes). Extending this conservation hypothesis, it is possible that these feelings of regret are more salient and thus more of a concern under conditions of high perceived control. When participants turn the cards themselves, they experience a closer connection between their actions and the outcome possibly triggering self-blame and thoughts, such as “if only I did not select that card, I would have avoided the loss card.” In contrast, under conditions of low perceived control, participants are able to attribute negative outcomes to external forces, such as luck, or in the case of Experiment 2, the researcher who is responsible for turning the cards. In support of this view, a recent study found a link between the perceived controllability of an event and the type of causal attributions made. Specifically, the more participants rated a risky scenario as controllable, the more likely they were to attribute the cause of the outcome to internal rather than external factors (Rickard, 2014).

Future studies though would need to explore this conservation hypothesis more directly. A promising direction might involve manipulating the presence or absence of outcome information similar to other studies on regret theory (e.g., Josephs, Larrick, Steele, & Nisbett, 1992). The prediction is that if outcome information is never revealed, then the ego depleted will become more risk-seeking because there is no potential for
regret or concern about having to regulate future emotions. Another way to test if ego depleted individuals strategically conserve their resources is to manipulate whether they expect to perform a third self-control task. Using a similar manipulation, Muraven et al. (2006) showed that the depleted temporarily perform worse on a second self-control task in efforts to conserve energy for an expected third self-control task. With the expectation of a third task, the depleted may be less likely to resist the temptation of certain gambles. Studies such as these will help us understand the mechanisms driving ego depletion effects and predict when depletion will increase or decrease risk-taking.

One limitation of the above perceived control account is that, according to the manipulation check questions, the high perceived control group failed to report feeling more control over the active CCT than the low perceived group playing the passive CCT. Although several reasons for this null result were proposed in the discussion section of Experiment 2, one implication of this result is that some alternative mechanism (besides perceived control) may explain the CCT Version × Depletion interaction found in Experiment 2. The active and passive versions of the CCT, for example, could have changed participants’ mode of thinking making them adopt a more affective (i.e., risk as feelings) or deliberative (i.e., risk as analysis) decision strategy, respectively (Slovic, Finucane, Peters, & MacGregor, 2004). In fact, the creators of the CCT intentionally created affectively “hot” and “cold” versions of the task to examine the consequences of these two modes of thinking (Figner et al., 2009). The “hot” and “cold” versions of the original CCT are similar to the present work’s active and passive conditions, respectively. Future studies should test if the added perceived control manipulations (i.e., familiarity
and active involvement) are necessary to obtain the CCT Version × Depletion interaction or if varying the “hot” vs. “cold” nature of the risk-taking task is sufficient.

The last task characteristic tested was the role of decisions from experience versus description. Experiment 3 revealed that the experience versus descriptive nature of the risk-taking task does not necessarily predict the direction of the ego depletion effects. On two experienced-based tasks, depletion either had no effect (experienced-based CCT), or it led to decreased risk-taking (BART). Future studies might benefit by manipulating specific features of experience-based tasks as it is clear that not all paradigms share the same features (e.g., Camilleri & Newell, 2011).

**Cognitive and Motivational Mechanisms Underlying Depletion Effects**

In terms of cognitive mechanisms, Experiments 1 and 2 explored how ego depletion affected sensitivity to changes in expected value and information use on the CCT. Both the ego depleted and non-depleted groups changed their game playing by taking more risks during advantageous rounds and fewer risks during disadvantageous ones. Similarly, both groups attended to information regarding the gain amount and probability of loss for each round, but the ego depleted paid less attention to changes in loss amount. Instead, they adopted a conservative approach choosing to turn the same number of cards regardless of whether the loss amount was small or large. In sum, although the ego depleted failed to integrate all the available information, they were still capable of discriminating advantageous from disadvantageous gambles. One interesting implication of these results is that ego depletion may increase the likelihood of using non-compensatory (as opposed to compensatory) strategies. Follow-up studies could confirm whether these results generalize to riskless choices and pinpoint when such non-
compensatory strategies lead to suboptimal decisions by using process tracing software, such as MouseTrace (Jasper & Shapiro, 2002).

Regarding motivation, the process model predicted that increased reward sensitivity might explain why ego depletion sometimes leads to increased risk-taking. Ego depletion increased approach motivation, but approach motivation was unrelated to CCT performance (Experiment 2). The robustness of this former effect between depletion and approach motivation is questionable though because, in Experiment 3, ego depletion did not significantly increase approach motivation. This null result may be partially due to measuring approach motivation with the BAS scale, which is traditionally used as a trait rather than state measure. Future research would benefit from using a more sensitive measure of approach motivation.

While approach motivation might explain instances where ego depletion increases risk-taking on other gambling tasks, it does not account for instances where depletion leads to decreased risk-taking. Such decreases in risk-taking may be due to ego depletion decreasing task engagement. After exerting considerable effort during the first depletion task, depleted participants may want to finish the experimental session quickly and thus rush through the CCT. Additionally, they may feel they satisfied their obligation to the experimenter upon completing the cross-out task.

Data from the present three studies suggests that the evidence for this alternative “disengagement hypothesis” is mixed. Consistent with this hypothesis, the depleted ($M = 18.94$ seconds) spent slightly less time on each round than the non-depleted ($M = 15.84$ seconds) in Experiment 1, $t(41) = 1.94$, $p = .06$, $d = 0.60$. Also the tendency for the ego
depleted to select fewer cards than the non-depleted could be interpreted as increased passivity rather than changes in an underlying risk preference.

Speaking against this hypothesis though, both groups found the CCT equally thrilling. In Experiments 1 and 2, participants responded to the statement, “At times during the game I felt a thrill” in which they responded using a 100-point slider bar (or a 1-9 scale in Experiment 2) anchored by “doesn’t apply at all” to “strongly applies.” In both experiments, the depleted (Exp. 1 M = 44.46, Exp. 2 M = 6.02) and non-depleted (Exp. 1 M = 51.47; Exp. 2 M = 6.00) participants found the task equally thrilling, \( t(41) = 0.83, p = .41, d = 0.25 \) and \( t(90) = -0.05, p = .96, d = 0.01 \). Another shortcoming of the disengagement hypothesis is that it would not predict the interaction observed in Experiment 2. According to this hypothesis, the depleted participants should risk less than the non-depleted in both versions of the CCT because a conservative approach (i.e., choosing only a few cards) would be the fastest way to finish the experiment. Future research should try to address this disengagement hypothesis more directly.

**Relationship Between State and Trait Self-Control**

In addition to exploring the “when” and “how” of ego depletion effects, our work connects with previous research (Imhoff et al., 2013) showing that it is useful to consider “who” is most susceptible to these effects. There is accumulating evidence that trait self-control moderates ego depletion effects, but this effect is sometimes inconsistent (Dvorak & Simons, 2009). Most studies show that high trait self-controllers are more resistant to depletion effects presumably because they have access to a larger reserve and therefore do not fatigue as easily. Imhoff et al. (2013), however, reported a surprising set of results showing that high trait self-controllers were more vulnerable to lapses in self-control.
following a depletion task. Experiment 2 yielded this “ironic effect” of self-control in the low perceived control CCT, but for other risk-taking tasks, trait self-control and ego depletion independently predicted performance (e.g., the BART in Experiment 3 and the high perceived control CCT in Experiment 2). Although in Experiment 3, trait self-control was unrelated to the experience- and description-based CCT, \( r = -.10 \) and \( -.02 \), respectively. Together these findings suggest that relationship between ego depletion and trait self-control is slightly more complex and may depend on the type of risk-taking task. It is also worth noting that some of the present results converge with those of a recent doctoral dissertation by Konnikova (2013) who used trait self-control to predict risk-taking in the CCT. Across two studies, she found that high self-controllers risked more on the CCT than low self-controllers. In translating her results to the present study, one can see that high self-controllers (i.e., those who presumably possess more self-control resources—the non-depleted) risked more in the CCT than low self-controllers (i.e., those who presumably have fewer self-control resources—the ego depleted). In other words, the low self-controllers in her dissertation behaved similarly to the ego-depleted in this study, while her high self-controllers behaved more like the non-depleted controls. These parallel results are interesting and accompany another study (see Experiment 2, Freeman & Muraven, 2010) showing that trait and state self-control affect risk-taking similarly.

**Conclusion**

In sum, the primary motivation of these studies was to explain why ego depletion can have opposing effects on risk-taking. Based on previous research, it was hypothesized that perceived control would predict whether ego depletion increased or decreased risk-taking. In support of this hypothesis, ego depletion decreased risk-taking
on a task promoting high perceived control, and this effect was eliminated (and descriptively reversed) under conditions of low perceived control. An additional aim was to examine how ego depletion affected approach motivation in efforts to answer the call to understand the underlying mechanisms driving ego depletion effects (Inzlicht & Schmeichel, 2012). The results of these tests were partially consistent with previous research showing that depletion (descriptively) increases approach motivation (Schmeichel et al., 2010). Analyses of participants’ information use also suggested that the ego depleted risked less because they became more loss averse (Carr & Steele, 2010). Future studies are necessary, however, because the observed increases in approach motivation among the ego depleted were not accompanied by increases in risk-taking on the CCT.

The present work also suggests that both the process and strength models of self-control need to be revised because they cannot account for all the findings in this study. For example, the strength model does not predict an interaction between depletion state and task characteristics. Moreover, it (incorrectly) predicts that ego depletion should always increase risk-taking. On the contrary, the present studies generally found that ego depletion decreased risk-taking. Regarding the process model, only weak support was found for the hypothesis that ego depletion increased approach motivation. Consistent with the process model though, ego depletion affected certain cognitive processes, such as information use on the CCT. Two strengths of the present work are that it helps identify some of the limitations of current self-control models and offers several potential directions for future research.
More broadly, the present work may also relate to the literature on the psychology of poverty. Similar to the ego depletion effects demonstrated in Experiments 1-3, individuals with lower income/wealth tend to be more risk averse (Haushofer & Fehr, 2014). Together these findings suggest that risk aversion may be one consequence of dealing with scarce resources (e.g., money, time, or mental resources) as proposed by Shah, Mullainathan, and Shafir (2012). Additional research though is needed to examine the similarities and differences between ego depletion and poverty.

The present research helps further our understanding of how risk preferences are shaped by the context and the individual. Because people must deal with risk and uncertainty on a daily basis, it is important to understand how situational factors, such as ego depletion and the nature of the risk-taking task, affect people’s decisions. As the present studies have shown, one’s ability to exercise self-control partially depends on the risky situation and one’s enduring traits. The threat of ego depletion increasing risk propensity seems most concerning for high self-controllers or in situations eliciting feelings of low perceived control. In other settings, ego depletion tends to decrease risk-taking, which may be an advantage as it reduces one’s chances of experiencing harm. One drawback though is that this risk aversion could increase the chances of missing some benefits. Determining the appropriate balance between risk and rewards is challenging especially in situations involving uncertainty, but it is encouraging to know that depletion should not compromise our ability to learn through experience.
References


### Appendix A

**Studies Showing Ego Depletion Increases Risk-Taking**

<table>
<thead>
<tr>
<th>Source</th>
<th>Study</th>
<th>Risk-Taking Measure</th>
<th>N</th>
<th>Effect size</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bruyneel et al. (2009)</td>
<td>Study 2</td>
<td>Purchasing Lottery Tickets</td>
<td>68</td>
<td>$d = 0.50$</td>
</tr>
<tr>
<td>Freeman and Muraven (2010)</td>
<td>Exp. 1</td>
<td>Choice Dilemma Questionnaire</td>
<td>70</td>
<td>$d = 0.55$</td>
</tr>
<tr>
<td></td>
<td>Exp. 2</td>
<td>Balloon Analogue Risk Task</td>
<td>46</td>
<td>$d = 0.60$</td>
</tr>
<tr>
<td></td>
<td>Exp. 3</td>
<td>Hypothetically pay $5 to play either: Lottery A: 70% chance of winning $20 or Lottery B: 4% chance of winning $250</td>
<td>91</td>
<td>$d = 0.54$</td>
</tr>
<tr>
<td>Inzlicht and Kang (2010)</td>
<td>Study 2b</td>
<td>Play 20 rounds of a hypothetical gamble. Each round choose either: Option A: $50 for sure or Option B: 50% chance of winning $100 or 50% chance of winning $0</td>
<td>132</td>
<td>$d = 0.40$</td>
</tr>
<tr>
<td>Schmeichel et al. (2010)</td>
<td>Exp. 1</td>
<td>Sensation Seeking Scale</td>
<td>33</td>
<td>$d = 1.00$</td>
</tr>
<tr>
<td></td>
<td>Exp. 2</td>
<td>Vienna Risk Taking Test</td>
<td>30</td>
<td>$d = 0.78$</td>
</tr>
<tr>
<td></td>
<td>Exp. 3</td>
<td>Modified DOSPERT</td>
<td>72</td>
<td>$d = 0.55$</td>
</tr>
<tr>
<td></td>
<td>Exp. 4</td>
<td>Modified DOSPERT</td>
<td>37</td>
<td>$d = 0.69$</td>
</tr>
<tr>
<td>Molet et al. (2012)</td>
<td>Exp. 2</td>
<td>Video game in which participants selected between two options offering two different (but unknown) reinforcement schedules. One option delivered smaller rewards and highest net gain and the other option provided a low probability-high payoff, but lower net gain.</td>
<td>30</td>
<td>$d = 0.75$</td>
</tr>
<tr>
<td>Goodwin (2012)</td>
<td></td>
<td>Iowa Gambling Task</td>
<td>30</td>
<td>$d = 0.77$</td>
</tr>
<tr>
<td>Imhoff et al. (2013)</td>
<td>Study 2</td>
<td>Game of Dice</td>
<td>127</td>
<td>$d = 0.52$</td>
</tr>
</tbody>
</table>
## Appendix B

### Studies Showing Ego Depletion Decreases Risk-Taking

<table>
<thead>
<tr>
<th>Source</th>
<th>Study</th>
<th>Risk-Taking Measure</th>
<th>N</th>
<th>Effect size</th>
</tr>
</thead>
<tbody>
<tr>
<td>De Langhe et al. (2008)</td>
<td>Exp 1</td>
<td>Mixed gamble. Participants endowed with $20 of play money to invest one of two options: invest $1 and keep $1 or invest $1 for a 50% of $2.50. Played 20 rounds and received outcome information after each decision.</td>
<td>27</td>
<td>(d = -0.81)</td>
</tr>
<tr>
<td></td>
<td>Exp 2</td>
<td>Iowa Gambling Task</td>
<td>57</td>
<td>(d = -0.52)</td>
</tr>
<tr>
<td>Carr and Steele (2010)</td>
<td>Exp 1</td>
<td>Decided whether or not to play 6 mixed gambles. Each gamble offered 50% of winning $6 or 50% chance of losing some amount ranging from $1 to $6.</td>
<td>53</td>
<td>Mean (d = -1.3)</td>
</tr>
<tr>
<td></td>
<td>Exp 2a &amp; 2b</td>
<td>Decided between gamble pairs with equal expected utility. One gamble offered higher odds, but lower payoff and the other offered lower odds, but higher payoff.</td>
<td>70</td>
<td>Mean (d = -0.74)</td>
</tr>
<tr>
<td>Unger and Stahlberg (2011)*</td>
<td>Exp. 1-3</td>
<td>Investment Scenario-mixed gamble</td>
<td>81</td>
<td>(d = -0.44)</td>
</tr>
<tr>
<td>Kostek and Ashrafioun (2013)</td>
<td></td>
<td>1/200 chance of winning $200</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*Note.* *Unable to calculate effect size with information provided in publication.*
Appendix C

Card Task Instructions for Low (and High) Perceived Control Conditions

You are now going to participate in a card game. In this game, you will turn over cards to win or lose money.

In each game round, I (you) will shuffle and deal 32 cards so that they are lying face down on the board (no peeking, of course). You will then decide how many of these cards to turn over. Each card is either a gain card (i.e., black) or a loss card (i.e., red); there are no neutral cards. You will know how many gain cards and loss cards are in the deck of 32, and how many points you will gain or lose if you turn over a gain or loss card.

You will indicate the number of cards you want to turn over by (turning the cards horizontally.) telling me a number ranging from 0 to 32. (You can decide to stop selecting cards to turn over at any point. When you are ready to turn them over, you can say, “Stop & Turn Over" the cards.) Then, I (you) will turn over (your chosen cards) that number of cards, one at a time. Every time a gain card is turned over you get the points added to your round total and another card may be turned over. The first time a loss card is turned over, the loss points are subtracted from your current point total and the round immediately ends. Each new round starts with $0; in the end, you will learn the total amount of money won or lost across the rounds. Your goal is to maximize your winnings and minimize your losses.
Appendix D

Instructions for Experienced-Based CCT

Page 1:

Columbia Card Task Instructions

- You are going to play a card game. In this game, you will turn over cards from a card deck to win or lose hypothetical money. Your objective is to win as much money as possible and avoid losing money.

- In each game round, there will be a deck of cards displayed on the computer screen, face down. You will decide how many of these cards to turn over. Each card is either a gain card or a loss card (there are no neutral cards). You will not know how many gain cards and loss cards are in the deck, but you will learn how much money you gain or lose during each round.

- Every time you turn over a gain card you get the specified amount of money added to your round total, and you have the chance to turn over another card. The first time you turn over a loss card, the loss amount is subtracted from your current earnings and the round immediately ends. You can decide to stop turning over cards at any point, as long as you have not yet turned over a loss card.

- You will play from a total of 5 card decks. Press SPACE BAR for further instructions.

Page 2:

Columbia Card Task Instructions

Each round begins with a deck of cards lying face down as shown below. You turn a card by pressing the SPACE BAR on the keyboard. When you want to stop turning cards, press the ENTER key to end the round and collect your earnings.

Press SPACE BAR to continue.
Columbia Card Task Instructions

**Gain Card**: For every gain card you turn, your round total increases by a certain amount.

**Loss Card**: For every loss card you turn over, your round total decreases by a certain amount. Furthermore, the round immediately ends (you cannot turn over additional cards).

After each round, you will choose to play either another round or the final draw. If you play another round, the same deck will be shuffled and you can play again. You will be able to play as many rounds as you would like before moving onto the final draw. If you feel you have sampled enough from a given deck, then choose to go to the final draw. You will then play one final round with the given card deck; **the outcome of this final round will determine your total earnings for the deck**. In sum, you will sample from a deck and then move onto a final draw which will determine your overall earnings.

Press the SPACE BAR to play two practice rounds.
Appendix E

Instructions for Description-Based CCT

Page 1:

Columbia Card Task Instructions

- You are going to play a card game. In this game, you will turn over cards to win or lose hypothetical money. Your objective is to win as much money as possible and avoid losing money.

- In each game round, there will be 32 cards on the computer screen, face down. You will decide how many of these cards to turn over. Each card is either a gain card or a loss card (there are no neutral cards). You will know how many gain cards and loss cards are in the deck of 32, and how many points you will gain or lose if you turn over a gain or loss card. What you don't know is which of the 32 cards that you see face-down are gain cards and which are loss cards.

- Every time you turn over a gain card you get the specified amount of money added to your round total, and you have the chance to turn over another card. The first time you turn over a loss card, the loss amount is subtracted from your current earnings and the round immediately ends. You can decide to stop turning over cards at any point, as long as you have not yet turned over a loss card.

- You will play a total of 5 rounds. Press SPACE BAR for further instructions.
Page 2:

Columbia Card Task Instructions

Each round begins with cards lying face down as shown below. You turn a card by pressing the SPACE BAR on the keyboard. When you want to stop turning cards, press the ENTER key to end the round and collect your earnings.

Press SPACE BAR to continue.

Page 3:

Columbia Card Task Instructions

**Gain Card**: For every gain card you turn, your round total increases by either $1 or $3 depending on the round.

**Loss Card**: For every loss card you turn over, your round total decreases by either -$10 or -$35, depending on the round. Furthermore, the round immediately ends (you cannot turn over additional cards). There will be either 1 or 3 loss cards in different rounds.

The number of loss cards and the money that can be won or lost by turning over a gain or loss card are fixed in each round. This information is always displayed so you know what kind of round you are doing.

Press the SPACE BAR to play two practice rounds.