A CRITICAL EXAMINATION OF THE EGO-DEPLETION EFFECT: CAN YOU VS. WILL YOU ENGAGE IN EFFORTFUL SELF-REGULATION?

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ABSTRACT

A CRITICAL EXAMINATION OF THE EGO-DEPLETION EFFECT: CAN YOU VS. WILL YOU ENGAGE IN EFFORTFUL SELF-REGULATION?

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Self-regulation refers to people's ability to regulate their own thoughts and actions, and override them when necessary. Measures of self-regulation have been observed to correlate with a number of important outcomes, including academic achievement and job performance (Cleary, 2006; Gailliot, 2008; Mischel, Shoda, & Peake, 1988). Nevertheless, there is still debate about how best to conceptualize self-regulation. Does self-regulation draw on ability or motivation?

This debate has been discussed, in recent years, in the context of a finding referred to as the *ego-depletion effect*. This effect has typically been demonstrated using a two-task procedure (e.g., Muraven, Tice, & Baumeister, 1998). In a *depletion condition*, participants perform a task that presumably places a high demand on self-regulation, whereas in a *control condition*, participants perform a *non-regulatory* task. All participants then complete a demanding regulatory task. In a number of studies, participants in the depletion condition have performed worse on the second task than participants in the control condition.

One prominent explanation of ego-depletion is the *resource depletion account*. The basic idea of this account is that self-regulation draws on a limited resource that becomes even more limited after engaging in self-regulation; this negatively affects subsequent self-regulatory behavior. Thus, according to this account, ego-depletion reflects an *inability* to continue regulating effectively.

There are several limitations of the resource account of ego-depletion and alternative explanations are possible. Specifically, a *motivation account* suggests that ego-depletion reflects an *unwillingness* to perform the second task rather than an inability to do so. That is, it could be that individuals who have previously exerted more effort may be less inclined to exert effort thereafter without sufficient motivation.

In this dissertation, I conducted five studies to investigate this possibility. In the first study, I performed a meta-analysis and found a relationship between the depletion effect and ratings of effort and difficulty on the first task. In the second study, I found a depletion-like effect after two initial non-regulatory tasks that required different levels of effort exertion. In the third and fourth studies, I found depletion-like effects using motivation manipulations—even when, across conditions, participants' resource was presumably depleted to the same degree. In the fifth study, I investigated whether the role of effort and motivation extended to a three-task conservation procedure (see Muraven, Shmeuli, & Burkley, 2006). While I did not find evidence for conservation, I did find some evidence that motivation continues to impact performance after two demanding regulatory tasks. Together, these findings suggest that motivated effort allocation has a significant impact on ego-depletion. Specifically, the results suggest that individuals are less inclined to exert effort after prior exertion. If sufficiently motivated, however, participants can and will exert effort.

Dedicated to Dad—the lesson you taught us most, the power of work, kept us all going when you were gone—and to Mom—the lessons you teach us most, the power of love and service, keep us living for the right reasons.

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CHAPTER 1: INTRODUCTION

Self-regulation has been defined as the ability to direct one's thoughts and actions (Muraven, Tice, & Baumeister, 1998) and can be used to control emotions, drives, impulses, appetites, performance, and attention (Vohs & Baumeister, 2004). Measures of self-regulation correlate with important outcomes such as relationship satisfaction, academic achievement, and job performance (Cleary, 2006; Funder & Block, 1989; Funder, Block, & Block, 1983; Gailliot, 2008; Mischel, Shoda, & Peake 1988). Specific examples of self-regulatory failures include binge eating, impulse buying, unprotected sex, illicit drug use, gambling, violence, and procrastination (Baumeister & Heatherton, 1996).

The Depletion Effect

The purpose of this dissertation was to investigate the nature of so-called *ego-depletion* in self-regulation. This behavioral phenomenon, hereafter referred to as the *depletion effect*, has typically been demonstrated using a two-task procedure. In a *depletion condition,* participants perform a task presumed to place a high demand on self-regulation, whereas in a *control condition,* participants complete a non-regulatory task. All participants then complete a demanding regulatory task. The depletion effect refers to the finding that performance on the second task is significantly worse in the depletion condition than in the control condition (for a review, see Baumeister, Vohs, & Tice, 2007). In general, the two tasks last 5 to 10 minutes each, indicating this effect occurs over a relatively short time span (Hagger, Wood, Stiff, & Chatzisarantis, 2010).

One paradigm used in research on the depletion effect involves emotion regulation. For example, in some studies, participants have watched an emotionally evocative movie clip (e.g., a comedy act). In the depletion condition, participants are instructed to suppress or exaggerate their emotional reaction to the movie clip, whereas in the control condition, they simply watch the movie clip. In studies involving this paradigm, performance decrements have occurred in depletion conditions relative to control conditions on tasks designed to measure persistence (Baumeister, Bratslavsky, Muraven, & Tice, 1998; Gailliot et al., 2007), physical endurance (Muraven et al., 1998), reasoning (Schmeichel, Vohs, & Baumeister, 2003), fluency (Schmeichel, Demaree, Robinson, & Pu, 2006), color-word interference (Inzlicht & Gutsell, 2007), and working memory (Schmeichel, 2007).

Another paradigm involves attention regulation. For example, in some studies, participants have examined a passage of text (Baumeister et al., 1998). In the depletion condition, participants must find all of the Es within the text, but only cross off Es that satisfy a given rule, whereas in a control condition, participants cross off all of the Es. In studies involving this paradigm, performance decrements occurred in depletion conditions relative to control conditions on tasks designed to measure persistence (Baumeister et al., 1998; Tice, Baumeister, Shmueli, & Muraven, 2007) and attention regulation (Muraven, Shmueli, & Burkley, 2006).

In paradigms such as these, the intended distinction between a self-regulatory and non-regulatory initial task has been the involvement of response inhibition and suppression (Muraven et al., 1998). The Stroop task, one of the most widely studied inhibition tasks (MacLeod, 1991), has been utilized in depletion effect studies. In this task, individuals identify what color a color-name appears in when the color and color-name match (low inhibition) or mismatch (high inhibition). When used as the initial task, participants who completed high inhibition trials subsequently persisted less on unsolvable puzzles relative to participants who solved low inhibition trials (Gailliot et al., 2007; Webb & Sheeran, 2003). When used as the second task, performance was reduced on high inhibition Stroop trials following tasks that involved regulating attention (Gailliot et al., 2007), suppressing emotion (Inzlicht & Gutsell, 2007), and counting backwards by 7s while standing on one leg (Webb & Sheeran, 2003).

To summarize, the depletion effect has occurred using a wide variety of tasks, where researchers often manipulate one form of self-regulation with the first task (e.g., emotion regulation) and then test another form of self-regulation with the second task (e.g., attention regulation). Tasks have involved factors ranging from suppressing emotions and thoughts, to resisting temptation, to persisting at difficult problems, to enduring a physical task, to reasoning and problem solving, and to working memory. In a recent meta-analysis of depletion effect studies, the averaged corrected standardized mean difference for performance on the second task was $d^{+} = 0.62$, representing a medium-to-large effect size (Hagger et al., 2010).

Due to the impact of self-regulatory successes and failures on the individual and society, it is important to understand why depletion effects occur (Baumeister & Heatherton, 1996). The specific question motivating this research was whether depletion effects occur because people are *unable* to engage in self-regulation, or whether they are *unwilling* to do so. A *resource account* of depletion effects implies individuals are incapable of self-regulation following a demanding self-regulatory task. By contrast, a *motivation account* implies individuals choose how much effort to allocate to self-regulation.

The Resource Depletion Model

The resource depletion model of self-regulation has three main assumptions. The first assumption is that all self-regulatory thoughts or actions rely on a domain-general mental resource (Muraven & Baumeister, 2000). In other words, any type of regulatory action (e.g., emotional or attentional) will draw on the same resource. The second assumption is that the resource is *limited* (Baumeister et al., 1998). This assumption leads to the prediction that the ability to self-regulate at any given moment is determined by the amount of the resource available, and whether that amount is sufficient to meet regulatory demands. If demands on the resource are greater than the supply available, then performance suffers (Just, Carpenter, & Miyake, 2003).

The final assumption is that the self-regulatory resource has *limited strength*. That is, the resource is not only finite, but is also expendable. Baumeister and colleagues described this assumption in terms of a muscle analogy: If a person uses a muscle strenuously for some period, it becomes fatigued and is left in a state of reduced capacity (see Muraven et al., 1998). If a person uses that same muscle immediately afterward to do another task, performing that task is harder and less effective than if the muscle had been at rest.

Stemming from the muscle analogy, there are two additional hypotheses of the resource depletion model (Muraven et al., 1998). The *recovery hypothesis* is that the resource returns to baseline capacity following a period of rest. The *training hypothesis* is that practicing regulatory behaviors increases one’s ability to self-regulate effectively. The benefit of training is not limited to the specific behavior practiced but applies to the general use of the resource.

To test the recovery hypothesis, Tyler and Burns (2008) manipulated the amount of time between the first task and the second task (i.e., 1, 3, or 10 minutes). The dependent measure on the second task, handgrip duration, did not change significantly for control participants across the time interval conditions. However, Tyler and Burns did find that handgrip duration varied as a function of time interval in the depletion condition. That is, relative to a baseline measure, handgrip duration was longest in the 10-minute condition, shorter in the 3-minute condition, and still shorter in the 1-minute condition. Tyler and Burns argued that as the interval between tasks increased, participants' regulatory resource recovered to a greater extent, allowing them to hold the handgrip longer. In a second study, Tyler and Burns found a reduced depletion effect for participants who relaxed during a 3-minute interim period between the first and second task, relative to control participants who simply waited during that period. Additionally, Oaten, Williams, Jones, and Zadro (2008) found that the depletion effect was reduced after a 45-minute break, although within the parameters of their study, the reduction was limited to individuals low in social anxiety.

To test the training hypothesis, Muraven (2010) administered a single self-regulatory task, the stop signal task. In this task, participants pressed a key to indicate whether a square appeared on the right- or left-hand side of a computer screen; when a tone sounded, however, participants were instructed to withhold their response to the stimuli. Then, during a two-week interim, participants assigned to training conditions practiced self-regulation (e.g., avoiding sweets or practicing handgrip endurance), while participants assigned to control conditions practiced non-regulatory activities (e.g., simple arithmetic). Finally, participants completed the stop signal task a second time.

From the first testing session to the second, Muraven found that participants' stop signal performance improved more in the training conditions than in the control conditions. Using the typical two-task procedure in testing sessions, other researchers found a reduced depletion effect in training participants relative to control participants (Gailliot, Plant, Butz, & Baumeister, 2007; Muraven, Baumeister, & Tice, 1999). Oaten and Cheng (2006a, 2006b, 2007) found similar results using training periods that lasted several months. In these studies, participants engaged in study, exercise, or financial monitoring programs as part of their regulatory training. Proponents of the resource depletion model have argued that training reduces susceptibility to the depletion effect by strengthening the capacity or stamina of the regulatory resource (Muraven et al., 1999).

The resource depletion model offers one explanation of the depletion effect. That is, in the typical two-task procedure, the initial regulatory task in the depletion condition expends participants' resources, whereas the initial non-regulatory task in the control condition does not. Participants in depletion conditions perform worse on the second task than participants in control conditions because, for a period, they cannot regulate as effectively. According to this resource account, the ability to regulate falters in the short-term, but this ability recovers with time and even increases through training. Additionally, the depletion effect occurs across varying forms of self-regulation because the regulatory resource is domain-general (Baumeister et al., 1998; Muraven et al., 1998).

Limitations of the Resource Depletion Model

Though the resource depletion model provides an account of the depletion effect and offers several additional hypotheses, this model has several limitations. First, as others have noted, resource models are difficult to falsify and are based on circular reasoning (e.g., Alberts, Martijn, Greb, Merckelbach, & de Vries, 2007; Hirst & Kalmar, 1987; Navon, 1984; Wickens, 1980). Navon (1984) suggested that resource models may be useful, as long as there is an explanatory mechanism that provides testable predictions about the resource. However, Baumeister et al. (1998) acknowledged that the resource depletion model is limited because the nature of the resource is unknown. Self-regulation presumably involves many processes (Wegner, 1994), and it is unclear which process defines the resource and drives the depletion effect. When choosing tasks that involve a high level of self-regulation, most researchers choose tasks that require participants to override some response. However, there is no direct empirical evidence showing that response inhibition is the driving mechanism underlying the regulatory resource.

Second, while I have reported some support for the additional hypotheses of the resource depletion model, this support is limited and deserves some qualifying statements. A few studies showed that the depletion effect was reduced partially after a 3-minute break and more so after a 10-minute or 45-minute break (Oaten et al., 2008; Tyler & Burns, 2008). However, most depletion studies have included a 2-5 minute break between the first and second task when participants have filled out manipulation check questionnaires (Baumeister et al., 1998; Muraven et al., 1998). Even with this break, the depletion effect has occurred in many cases. In a study by Tice et al. (2007), participants in a depletion condition had a 5-minute break without any intervening tasks or manipulation checks, but still demonstrated performance decrements on the second task relative to control participants. Muraven and Baumeister (2000) speculated that the resource recovers more slowly than it is used. If this is the case, then the small break between tasks may be insufficient to fully overcome the depletion effect. This would mean that the brief interim period in most studies attenuates the depletion effect.

However, findings of a recent meta-analysis by Hagger et al. (2010) on the depletion effect suggest this may not be the case. In this meta-analysis, the interim period was included as a potential moderator of the depletion effect. Since very few studies have indicated the exact length of the interim period, Hagger et al. coded the interim period into the following categories: no activities reported, participants completed questionnaires, or participants completed filler tasks/rested. Hagger et al. predicted the depletion effect should be larger in studies that reported no activities since the chance for recovery should be less in these cases. Instead, they found the opposite: the depletion effect was significantly smaller in the studies that reported no activities during the interim compared to studies that had questionnaires, fillers, or a break. Hagger et al. indicated that crude coding and interrelationships among moderators may have affected this finding. Overall, these mixed results suggest that it is inconclusive whether recovery occurs in the manner proposed by the resource depletion model.

Evidence supporting the training hypothesis is more consistent. In their meta-analysis, Hagger et al. (2010) found that training has a large and significant effect, such that practicing self-regulation has reduced the degree of depletion that participants experience. One drawback of the training studies is that they do not address the underlying mechanism driving the training effect. Training may improve the efficiency of the resource or it may actually increase the capacity of the resource (Muraven et al., 1999).

Alternatively, an interesting result from one of the training studies suggests that individuals may benefit from training for another reason. Muraven et al. (1999) examined the size of the depletion effect before and after a two-week period and found mixed results. For several training conditions, or those who practiced self-regulation, the size of the depletion effect was smaller after training than before, as predicted. For other training conditions, the size of the depletion effect did not differ after training. Unexpectedly, the control condition, or those who did not practice self-regulation, showed a large increase in the size of the depletion effect after the interim period.

Muraven et al. (1999) decided to examine the influence of effort in these results by assigning participants to high, low, and no effort groups based on their reports of training activities. Those who exhibited high effort showed reduced effects of depletion relative to the control condition. Those in the low effort and no effort groups did not differ from the control condition. Thus, those who engaged more in the training regimens were the ones who benefited the most and showed reduced effects of depletion (but see Muraven, 2010).

To summarize, the resource account may be inadequate to fully explain the depletion effect. The resource depletion model is limited because the nature of the resource is ill defined, which makes it difficult to develop falsifiable hypotheses. Mixed support for the additional hypotheses associated with the model also limits the resource account. Furthermore, as I will discuss next, alternative accounts of the empirical results are possible.

An Alternative Account of the Depletion Effect: Motivated Effort Allocation

According to the resource account, differences in the ability to regulate explain the depletion effect. Alternatively, differences in emotion or motivation could explain the effect. For example, participants in depletion conditions could experience negative mood changes after completing the first task, which could then cause them to perform worse on the second task relative to control participants. However, researchers have successfully ruled out emotion as a driving factor in the depletion effect. Almost all depletion studies have included a measure of mood and arousal in between the first and second task and there have typically been no differences in positive or negative affect or arousal after the first task (Baumeister et al., 1998; Muraven et al., 1998; Webb & Sheeran, 2003). In cases where differences in mood or emotion emerged, including the variable as a covariate rarely altered the results (e.g., Alberts et al., 2007).

While emotion does not seem to influence the depletion effect, a motivation account has not been sufficiently explored. Such an account can be thought of in terms of a general principle known as the *law of least effort*. As Eisenberger (1992) stated, “performance evolves toward the minimal amount of energy expenditure required for reinforcement” (p. 249). Rats demonstrated this by exerting the minimum effort required to push levers for reinforcement and choosing the least effortful behavioral sequence that led to a reward (Brener & Mitchell, 1989; Lewis, 1965; Solomon, 1948). When two responses resulted in similar reinforcement, rats chose the less effortful response (Lewis, 1965; Solomon, 1948).

Cognitive Miser and Motivated Tactician

The law of least effort extends to human behavior as well. Fiske and Taylor (1984) introduced the concept of *cognitive miserliness* that suggested people often use mental short cuts when processing information to exert minimal effort. Consistent with this idea, research on judgmental biases showed that humans make decisions about their environment using heuristics (Kahneman & Tversky, 1973). Other areas of research indicate that people tend to use short cuts such as schemas, scripts, stereotypes, and categorical judgments when perceiving the world (Operario & Fiske, 1999). According to the cognitive miser view, using these short cuts is automatic and part of normal processing, even though they sometimes lead to errors.

Later, Fiske and Taylor refined the cognitive miser view and introduced the concept of the *motivated tactician* (Fiske, 1992; Fiske & Taylor, 1991). The basic idea of this concept is that people behave in miserly ways because they perform to a level that is *good enough* for current goals and intentions. In other words, people will only give as much effort as a given situation justifies (Brehm & Self, 1989). Fiske (1992) also suggested that people do not engage in shortcuts if they are sufficiently motivated to process information in other ways. Fiske (2004) cited research where activation of stereotypes depended on several motivation variables such as temporary goals, attitudes, values, and the immediate context. As an example, Ebenbach and Keltner (1998) found that people in power were less motivated to judge people accurately (e.g., their judgments were stereotypical), whereas people who dislike conflicts were more motivated to make accurate judgments.

Similarly, Hockey (1997) proposed that people have a range of effort they typically exert. If circumstances call for additional effort, they can exert more effort or reduce their performance standard; generally, people choose the latter. Meijman (1997) suggested that choosing the path of least effort is beneficial for health; that is, making high effort a habitual response has maladaptive effects similar to those of chronic stress because both activate the same physiological systems. Ultimately, it appears efficient to make decisions to exert the least amount of effort for the greatest amount of gain.

Lower effort processing can actually be quite useful and accurate. When faced with making decisions with limited time and knowledge, people must often “take the best” and easiest strategy. Gigerenzer and Goldstein (1996) used computer simulations to test fast and frugal algorithms of inferential reasoning against classical rational modes of reasoning and found that these two modes were comparable in terms of speed and accuracy. Some results even favored the fast and frugal algorithms.

As cognitive misers and motivated tacticians, people are generally inclined to use low effort strategies and processing. There are both psychological and physiological benefits for doing so (Gigerenzer & Goldstein, 1996; Meijman, 1997). However, people can increase effort when motivated (e.g., Ebenbach and Keltner, 1998).

Sequential and Continuous Task Performance

The cognitive miser and motivation tactician concepts are useful in explaining behavior when there are sequential demands on effort. Kivetz and Zheng (2006) used these concepts to explain how people balance hard work and rewards or relaxation. They found that individuals who had invested greater effort on an initial task were more likely to choose the easier or more gratifying of two options on a second task. For example, after a more effortful first task, individuals were more likely to choose a second task that involved watching movies than one that involved self-evaluation. These authors argued that individuals use hard work as a way to justify or entitle self-gratification.

Similar trade-offs occur in the workplace. When people must perform cognitively demanding tasks sequentially or continuously, performance suffers, a phenomenon known as *after-effects* (Broadbent, 1979). In many after-effects studies, researchers have manipulated cognitive demand between experimental and control conditions. Participants in experimental conditions consistently reported higher fatigue compared to controls (Lorist et al., 2000; Schellekens, Sijtsma, Vegter, & Meijman, 2000; van der Linden, Frese, & Meijman 2003a; van der Linden, Frese, & Sonnentag, 2003b; van der Linden, Massar, Schellekens, Ellenbroek, & Verkes, 2006). More important, self-report measures indicated that experimental participants, relative to control participants, were less willing to exert effort and even resisted exerting effort (van der Linden & Eling, 2006; van der Linden et al., 2003a). Experimental participants also reported an aversion to task continuation (Boksem, Meijman, & Lorist, 2005; Lorist et al., 2000).

Research also demonstrated that participants overcame after-effects if they were motivated. Boksem, Meijman, and Lorist (2006) had participants complete a task consisting of seven 20-minute intervals. Before the last interval of the task, participants were told that their performance would be compared to others' and that the best performer would receive a cash reward. Whereas performance began to decline during the previous intervals, performance improved significantly after the motivation intervention. Boksem et al. explained their findings in terms of an effort/reward imbalance. Specifically, they suggested that maintaining performance required a high level of effort. When there was no reason or reward to maintain effort, performance declined. When offered a reward for the last interval, participants were motivated to increase their effort and improved performance. In other words, participants behaved as motivated tacticians.

Overcoming Depletion Effects

Consistent with the motivated tactician concept, Schellekens et al. (2000) suggested individuals avoid the depletion effect as long as they are willing to expend more effort. In fact, several studies showed that individuals overcome the depletion effect through motivation. As examples, Martijn, Tenbult, Merckelbach, Dreezens, and de Vries (2002) demonstrated that when participants were told the depletion effect should not occur, it did not. Muraven and Slessareva (2003) eliminated the depletion effect by telling participants that performance on the second task was beneficial to themselves and others. Furthermore, Alberts et al. (2007) primed persistence before the second task and influenced participants to engage longer in a physical endurance task despite an initial depleting task. Finally, in their meta-analysis, Hagger et al. (2010) combined data across several studies that included a motivation manipulation and found a large effect. That is, depleted participants given motivation strategies performed better on the second task compared to depleted participants with no motivation strategies.

The motivated tactician concept may explain the benefits of self-regulatory training as well. Rather than strengthening the resource in some way, as assumed in the resource depletion model (Muraven et al., 1999), an alternative view is that after practicing self-regulation, individuals increase effort tolerance or endurance. That is, individuals motivate themselves to engage in high effort tasks or strategies for longer periods.

The effects of training may link to research on learned industriousness, which refers to the finding that people adapt to expected levels of effort. When people receive rewards or reinforcement for high effort, they perform accordingly (Eisenberger, 1992). Converse and DeShon (2009) found that the depletion effect occurred after one effortful regulatory task, but not after two. They suggested in a two-task procedure, individuals did not have enough experience to learn the expected level of effort exertion and adapt to that level. Individuals did adapt, however, when performing a three-task procedure.

Muraven et al. (2006) used a three-task procedure and found that people were more likely to conserve (and perform worse) on a current task if they anticipated future self-regulatory demands. Hagger et al. (2010) found a large effect of conservation manipulations across several studies. Essentially, the concept of conservation is an extension of the motivated tactician concept. That is, when people anticipate a need for high effort in the future, they will adjust their current effort to a lower level (e.g., Muraven et al., 2006) unless there is sufficient motivation to continue at the same level (e.g., via reward, Converse & DeShon, 2009).

Overview of the Current Research

To summarize, research on decision making (e.g., heuristics) and workplace performance (e.g., after-effects) provides evidence for miserliness and motivated effort tactics. These concepts readily apply to self-regulation and the depletion effect. Existing research has shown that people can overcome the depletion effect, not by rest as the resource account suggests, but by increasing motivation (e.g., Muraven & Slessareva, 2003). That is, people adaptively adjust their effort in response to external (e.g., rewards and future demands) and internal (e.g., justification) motivation cues. The effects of training may even result from learned industriousness rather than greater resource capacity or stamina (Converse & DeShon, 2009). Overall, this evidence supports a motivation account rather than a resource account of the depletion effect.

The purpose of my dissertation was to further test whether changes in the motivation to exert effort can provide an alternative explanation of the depletion effect. I conducted five studies to investigate this account. In the first study, I performed a meta-analysis examining the relationship between the depletion effect and self-report ratings of effort, difficulty, and fatigue following the first task. In the second study, I examined whether a depletion-like effect could occur after an effortful task that did not require self-regulation. In the third and fourth studies, I examined whether I could create a depletion-like effect among participants objectively depleted to the same degree by using motivation manipulations. In the fifth study, I examined whether the depletion effect is attributable to resource conservation or effort conservation.

CHAPTER 2: STUDY 1- META-ANALYSIS

Study 1 Rationale and Predictions

When asked to rate effort after an initial task, participants in depletion conditions have often indicated the task required more effort than participants in control conditions (Baumeister et al., 1998; Muraven et al., 1998). Participants have also rated the first task in depletion conditions as more difficult than in control conditions (Muraven et al., 1998; Schmeichel, 2007; Vohs & Schmeichel, 2003; but see Schmeichel et al., 2006). When participants reported a personal experience in which they successfully self-regulated, Muraven et al. found they often mentioned exerting significant effort. Furthermore, self-reported fatigue or exhaustion has increased more after an initial depleting task than after an initial control task (Alberts et al., 2007; Baumeister et al., 1998; Muraven et al., 1998; Webb & Sheeran, 2003).

Proponents of the resource account have argued that these manipulation checks corroborate the notion that differences in self-regulatory demands on the first task primarily influence the size of the depletion effect. Alternatively, it seems possible that differences in effort or difficulty demands on the first task primarily influence the size of the depletion effect. That is, performance may drop after the first task not because the first task required self-regulation per se, but rather because it required a high degree of effort. Differences in effort demands may also lead to the proposed domain generality of the resource. In other words, the type of task does not matter so long as the first and second task demand a higher level of effort.

To investigate whether these self-reported differences on the first task related to the size of the depletion effect, I performed a meta-analysis on a subset of published depletion studies. If variables such as effort have a significant impact on the depletion effect, there should be a strong positive relationship between the two. That is, greater differences in effort ratings on the first task should be associated with greater differences in performance on the second task, suggesting that the more effort participants exert initially, the less effort they exert subsequently.

Fortunately, the meta-analysis by Hagger et al. (2010) provided the effect sizes from a large number of depletion studies. These studies used the two-task procedure or similar designs (see Hagger et al. for a complete discussion of search and inclusion criteria). These researchers also provided the effect sizes of commonly used manipulation check questions. Although they showed signs of heterogeneity, Hagger et al. reported that the self-report effect sizes for effort, difficulty, and fatigue were significantly different from zero and were medium to large in magnitude. Effect sizes for other self-report items (i.e., positive affect, negative affect, and self-efficacy) were much smaller and two of them were not significantly different from zero.

Using their supplemental materials, I selected all of the studies that provided the depletion effect size as well as the effect sizes for self-report ratings of effort, difficulty, or fatigue. I predicted the self-report effects would have a positive relationship with the depletion effect. I also predicted that the effort effect would have the strongest relationship with the depletion effect compared to the effects of difficulty and fatigue.

Method

In total, I identified 124 tests of the association between the depletion effect and self-reported effort, difficulty, or fatigue. I removed eight tests where researchers did not predict that a depletion effect would occur.[1] For example, I only included one test from Schmeichel's second study (2007) where participants completed a digit span task for the dependent measure. The task had forward digit span trials, where participants recalled digits in the order given, and reverse digit span trials, where participants recalled digits backwards. Schmeichel predicted a depletion effect on the reverse trials because people must use complex executive functions to recall the digits backwards. He predicted there would be no depletion effect on forward trials because people can use basic memory processes to recall the digits in order. I included the test involving reverse digit trials, but not the test involving forward digit trials. The final data set of 116 tests (effort, *k* = 31; difficulty, *k* = 59; and fatigue, *k* = 26) came from 87 studies reported in 48 research articles.

Results

Analysis Plan

First, with a separate analysis for each, I calculated the sample-weighted average effect size for the depletion effect and for the effort effect, difficulty effect, and fatigue effect. Second, I performed a weighted least squares (WLS) analysis with the depletion effect regressed on the self-report effect. Thus, I performed the WLS analysis three times with effort, difficulty, or fatigue as the predictor variable. In each WLS analysis, I included sample size as a weighting variable.

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[1] The following tests of the depletion effect and self-report effect were removed from the analyses: Schmeichel (2007, Study 2: 1 test where the dependent variable was forward digit span performance); Tyler & Burns (2009, Study 2: 1 test from the 20-minute task duration condition); Wright et al. (2007, Study 1: 2 tests from the low and high relevance conditions that had a low performance standard; Study 2: 1 test from the low relevance condition); Wright, Martin, & Bland (2003, Study 1: 2 tests from the low standard condition); Wright, Stewart, & Barnett (2008, Study 1: 1 test where the dependent task was a non-regulatory math task).

Data Preparation

There are several points to make regarding the data set. First, if studies reported multiple manipulation checks, such as effort and difficulty, I included the effort effect and depletion effect together as one test and the difficulty effect and depletion effect together as another test (e.g., Baumeister et al., 1998, Study 1). I used these separate tests in their respective WLS analysis. However, I included the depletion effect, which was the same for both of these tests because it came from the same study, only once when calculating the sample-weighted average depletion effect.

Second, Hagger et al. (2010) reported a total sample size for each study, but did not report condition *n*s separately. I went back to the original studies to determine the sample size for each condition as precisely as possible. I noticed several cases where the sample size used to compute the depletion effect differed from the sample size used to compute the self-report effect. In most of these cases, the study involved multiple conditions, but not all of them were a test of the typical depletion effect (e.g., in Muraven & Slessareva (2003) some conditions received a motivation manipulation). It appeared that researchers based the depletion effect on a simple main effect, whereas they based the self-report effect on a main effect. When computing sample-weighted average effect sizes, I used the simple main effect condition *n*s for the depletion effect and the main effect condition *n*s for the self-report effect.[2] When conducting the WLS

[2] I have included a table of the *n*s I used in Appendix A as a reference. I did conduct the WLS analyses using the *n*s reported by Hagger et al. (2010). The results differed somewhat, with the *r* decreasing slightly from .263 to .241 in the effort WLS analysis, and increasing moderately from .213 to .265 and from .048 to .111 in the difficulty and fatigue WLS analyses, respectively. As I believe the weights I used were more accurate, as well as more conservative, I discussed and interpreted the results based on those weights.

analyses, I used the smallest combined sample size as the weight for each study. This provided the most conservative estimate of the relationship under investigation.

Third, there were two cases where a single study had more than one between-subjects test of the depletion effect. In Schmeichel's first study (2007), half of the participants received a sentence span task as the dependent measure and half of the participants received an operation span task as the dependent measure. In Tyler and Burns' study (2009), there were three sets of control/depletion conditions based on the length of the first task: 3-minutes, 10-minutes, or 20-minutes. I removed the 20-minute condition from the analyses as specified earlier (see Note 1), but kept the 3-minute and 10-minute conditions. I included a depletion effect from each set of these conditions when calculating the sample-weighted average depletion effect size. However, because researchers reported the main effect of self-report ratings and not a simple main effect, this meant the self-report effect size was the same for both sets of conditions coming from one study. In these cases, I included the self-report effect size only once when calculating the sample-weighted average self-report effect size. I included each set of conditions in the WLS analyses.[3]

[3] I randomly removed one test from each of these two studies, thus eliminating the repetition of a self-report effect size. This removal only affected the difficulty WLS analysis; despite removing only two data points, it resulted in *r* decreasing from .213 to .163. By random selection, one of the data points I removed was in the upper right quadrant of the data spread. If I keep this data point and remove the data point from the other set of conditions in this study, the *r* decreased less dramatically from .213 to .201. If anything, this may suggest that potential outliers exaggerated the relationship between the depletion effect and difficulty ratings.

Sample-Weighted Average Effect Sizes

Effect sizes (*d*) for the second task (i.e., the depletion effect) ranged from 0.12 to 2.60 (k = 90). The sample-weighted average effect size across all of the tests was d_+ = 0.63 with a 95% confidence interval from 0.57 to 0.69. This suggests that differences between conditions on the first task have a medium effect on second task performance. Effect sizes (*d*) for self-reported effort on the first task ranged from 0.06 to 1.89 (k = 31). The sample-weighted average effect size across all of the tests was d_+ = 0.69 with a 95% confidence interval from 0.60 to 0.79. The demands of the initial task had a medium effect on effort ratings (Cohen, 1987). Effect sizes (*d*) for self-reported difficulty on the first task ranged from -0.31 to 3.39 (k = 57). The sample-weighted average effect size across all of the tests was d_+ = 1.03 with a 95% confidence interval from 0.95 to 1.10. The demands of the initial task had a large effect on difficulty ratings. Effect sizes (*d*) for self-reported fatigue on the first task ranged from -0.43 to 1.67 (k = 26). The sample-weighted average effect size across all of the tests was d_+ = 0.56 with a 95% confidence interval from 0.46 to 0.66. The demands of the initial task had a medium effect on fatigue ratings. As expected, since this is a subset of the larger meta-analysis, these sample-weighted average effect sizes are similar to those reported by Hagger et al. (2010) in magnitude.

Weighted Least Squares Analyses

Next, I performed weighted least squares analyses to examine the relationship between self-report effects and the magnitude of the depletion effect. I regressed the depletion effect onto each type of self-report rating while including sample size as a weighting variable. In this context, *r* reflects the effect size between the self-report effect and the depletion effect. These analyses revealed a medium effect of effort on the depletion effect, *r* = 0.263, 90% CI [-0.03, 0.56], a small effect of difficulty on the depletion effect, *r* = 0.213, 90% CI [0.00, 0.43], and no effect of fatigue on the depletion effect, *r* = 0.048, 90% CI [-0.29, 0.39].

Discussion

The meta-analysis revealed a relationship between effort and the depletion effect and between difficulty and the depletion effect. As predicted, the strongest effect emerged when I included effort ratings in the WLS analysis. The confidence intervals for these effects contained zero, but the self-report ratings generally came from a single-item question, with unknown reliability and validity. Even so, I found a potentially important relationship between effort and the depletion effect, a relationship unexplored previously. With better measures and a larger sample of studies, this effect might prove even stronger.

Contrary to my predictions, no relationship emerged between fatigue and the depletion effect. Post hoc, this finding can be interpreted as support for the motivation account versus the resource account. If an inability to perform drives the depletion effect, individuals should be truly fatigued. Greater fatigue differences on the first task should then correlate with an increased depletion effect. This effect did not come out in the analysis. Moreover, it seems unlikely that regulating for 5-10 minutes, the length of most initial tasks, could produce true fatigue.

Instead, it seems more likely that when individuals feel like they exerted significant effort or when they feel that a task was quite difficult, they do not work as hard when they encounter another effortful or difficult task. This pattern of results is consistent with the concept of cognitive miserliness. Overall, this meta-analysis provided evidence that connects the depletion effect to motivated allocation of effort.

CHAPTER 3: STUDY 2- NON-REGULATORY INITIAL TASKS

Study 2 Rationale and Predictions

In previous studies of the depletion effect, regulatory demands may have been confounded with effort demands. As the meta-analysis results suggested, the depletion effect relates to differences in effort exertion on the initial task, not necessarily to differences in regulatory exertion. A few studies have examined this possibility. In Schmeichel's third study (2007), there were three levels of the first task: a low-demand condition, a high-demand/no self-regulation condition, and a high-demand/regulation condition. Participants rated the difficulty of these conditions in a graded fashion. On the second task, only the high demand/regulation condition showed impaired performance. The low demand and high demand/no self-regulation conditions did not differ from each other. As another example, Muraven et al. (1998) found the depletion effect after a task that involved thought suppression, but not after a 3-digit multiplication task, even though participants rated the math task and the suppression task similarly in difficulty. Muraven and colleagues interpreted these results as support for the resource account and argued that the type of task does matter. That is, the depletion effect only occurs after an effortful self-regulatory task, not after an effortful non-regulatory task.

However, the majority of studies on the depletion effect have reported differences in effort and/or difficulty on the first task and researchers have rarely explored performance outcomes after non-regulatory tasks. With this in mind, I designed the second study to explore whether a depletion-like effect would occur using non-regulatory initial tasks that required different levels of effort. According to the resource account, the depletion effect should not occur using non-regulatory initial tasks because they do not expend the regulatory resource. By contrast, according to a motivation account, individuals should be sensitive to effort demands, regardless of task type. I predicted that participants who performed a more effortful task first would perform significantly worse on the second task than those who performed a less effortful task first.

The two non-regulatory initial tasks involved mathematics: a single-digit addition task and a 3-digit multiplication task. Single-digit addition is relatively automatic and less effortful than three-digit multiplication, a task that requires greater attention and which people rarely do by hand. Muraven et al. (1998) argued people solve these types of math problems using standard procedures that do not involve overriding responses; thus, they are non-regulatory tasks. Muraven et al. even used 3-digit multiplication as a control task in one study, whereas I am using it as the more effortful task in this study. For the regulatory second task, I used an anagram task, in which approximately half of the anagrams were unsolvable. Researchers have often used anagram tasks in depletion studies, arguing that they involve effortful regulation because individuals must persist in the face of failure and override the desire to quit (Baumeister et al., 1998; Vohs & Heatherton, 2000).

Method

Participants

Thirty-two undergraduates from the subject pool at Michigan State University participated in this study for partial course credit. Three participants were removed from the low-effort addition condition, one because there was a timing error and two because they completed the provided sheets of math problems before the time limit was up. The final sample (low-effort condition, *n* = 13; high-effort condition, *n* = 16) had a mean age of 19.45 (*SD* = 1.50) and was 72% female. A post hoc power analysis indicated I achieved acceptable power (> .80, alpha = .05, two-tail) to reject the null hypothesis that there would be no difference in performance across conditions (Faul, Erdfelder, Lang, & Buhner, 2007).

Procedure and Materials

Participants completed the study individually in five steps: (a) a timed math task and manipulation check questions, (b) the Brief Mood Introspection Scale (Mayer & Gaschke, 1988; BMIS), (c) an anagram task and manipulation check questions, (d) a demographics questionnaire, and (f) additional manipulation check questions.

Participants were randomly assigned to a low-effort condition or a high-effort condition. The procedure for the two conditions differed only on the initial math task. Participants in the low-effort condition solved single-digit addition problems and participants in the high-effort condition solved 3-digit multiplication problems. Participants in both conditions worked on this task for 8 minutes using paper and pencil, without the aid of a calculator. After the first task, participants answered several manipulation check questions regarding their effort and the difficulty of the math task (see Appendix B for the manipulation check questions used for the tasks in this and subsequent studies). On all manipulation check questions, unless otherwise noted, participants read each question and rated themselves on a scale from 1 to 100. Participants also completed the BMIS to rule out differences in mood or arousal as an alternative explanation for the results (e.g., Muraven et al., 1998). On this questionnaire, participants used a 4-point scale to rate how well mood adjectives described themselves at that moment.

Then, participants in both conditions completed an anagram task in which approximately half of the items were unsolvable, although participants were not aware of this (see Appendix C). The task consisted of a list of 48 anagrams. An experimenter told the participants to solve the anagrams by unscrambling the letters to form a common English word. The experimenter gave an example of an anagram and identified the correct solution. The experimenter instructed the participant to begin working and stated, “When you have decided that you have worked on the task long enough (or when you want to stop), please tell me that you are done.” The experimenter started a stopwatch as soon as the participant began the task and stopped it when the participant quit. Persistence (time on task) was the dependent variable.

After the second task, participants answered several manipulation check questions regarding their effort and the difficulty of the anagram task. Then they completed a demographics questionnaire on which they reported their gender, age, and ACT scores (see Appendix D). I used ACT scores to ensure that performance differences on the anagram task across conditions were not attributable to differences in general cognitive ability. Finally, participants completed a study questionnaire that asked them to provide ratings for the first and second task on several aspects such as task difficulty, motivation to perform, effort required, and effort invested.[4]

[4] In Studies 3-5, participants did not rate effort and difficulty of each task on the final study questionnaire. Thus, I only reported effort and difficulty ratings from Study 2 that were given directly following each task.

Results

Manipulation Checks

Table 1 displays the means, standard deviations, and inferential statistics for the manipulation check questions. After the first task, participants who completed the single-digit addition task indicated that they invested less effort than participants who completed the 3-digit multiplication, $t(27) = -4.32$, $p < .001$, $d = -1.67$. Participants also rated the single-digit addition task as less difficult, $t(27) = -5.33$, $p < .001$, $d = -2.06$, and easier to work on, $t(27) = -5.39$, $p < .001$, $d = -2.08$, than the 3-digit multiplication task.

Regarding the second task, there was a near significant difference in effort investment, $t(27) = 1.98$, $p = .058$, $d = 0.77$. When looking at the *t*-test where equal variances were not assumed, the result for this variable was significant, $p = .044$. Participants in the low-effort addition condition indicated they invested more effort on the anagram task than participants in the high-effort multiplication condition. Otherwise, participants did not differ in difficulty ratings or effort given ratings (*p*s > .20).

Items on the BMIS can be used to create two mood scales: Pleasant-Unpleasant and Arousal-Calm. There were no significant differences between conditions on the BMIS scales or ACT scores (*p*s > .20).

Anagram Persistence and Number Correct

Table 2 displays the means, standard deviations, and inferential statistics for anagram performance. As expected, participants in the high-effort multiplication condition persisted significantly less on the anagrams than participants in the low-effort addition condition, $t(27) = 2.97$, $p < .01$, $d = 1.14$. The difference in number of correct anagram solutions across conditions was non-significant ($p > .96$).

Table 1: Descriptive and Inferential Statistics for Study 2 Questionnaires

	High-Effort Multiplication Condition				Low-Effort Addition Condition						
			95 % CI				95 % CI				
Variable	M	SD	LL	UL	M	SD	LL	UL	$t(27)$ [a]	p	Cohen's d
Math Task Ratings											
Effort Invested	66.44	30.12	51.68	81.20	19.77	27.45	4.85	34.69	-4.32	<.001	-1.67
Task Difficulty	39.94	24.19	28.09	51.79	3.85	2.91	2.27	5.43	-5.33	<.001	-2.06
How hard did you work?	48.94	26.35	36.03	61.85	8.31	6.97	4.52	12.10	-5.39	<.001	-2.08
BMIS											
Pleasant-Unpleasant Scale	7.23	6.08	3.92	10.54	8.67	5.23	5.71	11.63	0.63[b]	.54	0.26
Arousal-Calm Scale	14.92	4.01	12.74	17.10	16.33	3.68	14.25	18.41	0.91[b]	.37	0.38
Anagram Task Ratings											
Effort Invested	79.00	29.27	64.66	93.34	95.69	8.59	91.02	100.00	1.98	.058	0.77
Task Difficulty	94.00	7.16	90.49	97.51	95.85	6.36	92.39	99.31	0.73	.48	0.27
How hard did you work?	90.75	13.38	84.19	97.31	95.92	6.38	92.45	99.39	1.28	.21	0.49
ACT	25.47	2.59	24.16	26.78	23.92	3.73	21.81	26.03	-1.27[c]	.21	-0.51

Note. CI = Confidence Interval; *LL* = lower limit; *UL* = upper limit.

[a] The statistics reported here are the *t*s associated with the test when equal variances were assumed. Although there were cases in which Levene's test was significant, in no case was the *t* associated with equal variances assumed significant when the *t* associated with equal variances not assumed was not significant. [b] These *t*-tests had 23 *df* because some participants did not receive all of the manipulation check questions. [c] This *t*-test had 25 *df* because some participants did not report an ACT score.

Table 2: Descriptive and Inferential Statistics for Study 2 Anagram Performance

	High-Effort Multiplication Condition				Low-Effort Addition Condition						
			95 % CI				95 % CI				
Variable	M	SD	LL	UL	M	SD	LL	UL	$t(27)$ [a]	*p*	Cohen's *d*
Persistence (in minutes)	10.26	4.71	7.95	12.57	18.11	9.23	13.09	23.13	2.97	.006	1.14
Correct Solutions	1.56	1.26	0.94	2.18	1.54	1.33	0.82	2.26	-0.05	.96	-0.08

Note. CI = Confidence Interval; *LL* = lower limit; *UL* = upper limit.

[a] The statistics reported here are the *t*s associated with the test when equal variances were assumed. Although there were cases in which Levene's test was significant, in no case was the *t* associated with equal variances assumed significant when the *t* associated with equal variances not assumed was not significant.

Discussion

Baumeister and colleagues have argued that the depletion effect only occurs after tasks that involve self-regulation and not after non-regulatory tasks (e.g., Muraven et al., 1998). However, in this study, I induced a depletion-like effect using two non-regulatory tasks that differed in terms of effort exertion. Whereas Muraven et al. did not find the depletion effect after a 3-digit multiplication task, I did find a depletion-like effect after a multiplication task when compared to a less effortful single-digit addition task. Specifically, participants persisted less after a multiplication task than after an addition task. Performance did not differ in number correct; however, I expected no difference here because about half of the anagrams were unsolvable and the remaining anagrams were very difficult. These results suggest that researchers have overlooked the impact of effort in previous research. Furthermore, these results are inconsistent with a resource account of the depletion effect, but are entirely consistent with a motivation account of this effect.

CHAPTER 4: STUDY 3- INFLUENCE OF GOAL SETTING

Study 3 Rationale and Predictions

When people act as motivated tacticians, they choose whether to use low- or high-effort strategies. I designed Study 3 to investigate this idea by examining whether performance on a second task in a depletion paradigm changes depending on the motivation to exert effort, even when all participants have completed the same initial task. According to the resource account, if the first task is the same across conditions, and thus expends the regulatory resource to the same degree, then performance on the second task should not differ. By contrast, according to a motivation account, if individuals are sensitive to motivation manipulations, then performance on the second task should depend on how motivated individuals are to expend effort, regardless of prior regulatory expenditure.

Specifically, Study 3 investigated whether performance would increase if participants were motivated to exert more effort. Participants completed the same two tasks. After the first task, I manipulated performance goals. Half of the participants were given a specific, difficult goal for the second task, whereas the other half of the participants were given instructions to do their best.

I based this manipulation on the hypothesis that goals motivate action (Latham & Locke, 1991). Goals can range from vague (e.g., do-your-best) to specific (e.g., complete 32 problems within 10 minutes) and from easy (e.g., perform better than 10%) to difficult (e.g., perform better than 90%). A consistent finding coming out of goal setting research is that individuals given *specific, difficult goals* perform better than individuals given do-your-best instructions (e.g., Lock, Shaw, Saari, & Latham, 1981).

According to Latham and Locke (1991), *do-your-best* instructions lead to different performance outcomes, many of which fall short of a person's best. Research has shown that people given do-your-best instructions evaluate their performance more positively (Kernan & Lord, 1989) and anticipate that they will be satisfied at any level of performance (Mento, Locke, & Klein, 1990). Latham and Lock suggested that people with do-your-best instructions give themselves the "benefit of the doubt" when they evaluate their performance. Specific, difficult goals, on the other hand, set the bar for what constitutes good performance and motivates greater effort exertion than do-your-best instructions.

Accordingly, I predicted that even though resource expenditure after the first task should not differ across participants, performance on the second task would differ depending on what instructions the participants received for that task. That is, I predicted that participants given a specific, difficult goal would be more motivated to continue exerting effort on the second task and would thus outperform participants told to do their best on the second task.

Method

Participants

Seventy-three undergraduates were recruited from the subject pool at Michigan State University to participate in this study for partial course credit. I removed three participants from the analyses (one appeared intoxicated, one did not follow the typing instructions, and one did not follow the typing instructions or the anagram timing instructions). This left a sample of 70 (do-your-best condition, n = 36; specific-difficult-goal condition, n = 34). The final sample had a mean age of 20.17 (SD = 2.13) and was

54% female. A power analysis indicated a sample of this size would have acceptable power (> .80, alpha = .05, two-tail) to reject the null hypothesis that there would be no difference in performance across conditions (Faul et al., 2007).

Procedure and Materials

Participants were randomly assigned to one of two conditions: a do-your-best condition or a specific-difficult-goal condition. Participants completed the study individually in eight steps: (a) a timed typing task and manipulation check questions, (b) the BMIS, (c) a goal manipulation segment, (d) an anagram task and manipulation check questions, (e) filler tasks (this part was conditional, which I explain later), (f) a demographics questionnaire, (g) a vocabulary test, and (h) a study questionnaire. Participants expected the session would last one hour.

An experimenter seated the participant in front of a computer to perform the typing task and read the instructions for the task aloud (see Appendix E for detailed instructions and the text of the typing task). Researchers have used a related paper and pencil task (e.g., Baumeister et al., 1998) and a computerized version of the typing task (Muraven et al., 2006) in prior studies. I adapted the present version to collect key presses and run with an eye-tracking device, which I needed for another study.

The instructions indicated a passage would appear on the screen and the participant needed to type out the passage, including punctuation and capitalization. However, the participant was instructed to type the passage without typing spaces or the letter E. The instructions also indicated that the participant would be unable to see what was typed, but that he or she should try to do his or her best, correct mistakes, and continue typing until told to stop. After 6 minutes had elapsed, the typing task ended and the experimenter asked the participant to fill out manipulation check questions regarding their effort and the difficulty of the typing task. Participants also completed the BMIS.

Then, participants in both conditions completed an anagram task. For this study, 80 moderately easy and solvable anagrams were taken from Gilhooly and Johnson (1978; see Appendix F). Baumeister et al. (1998) used solvable anagrams as their second task and argued that completing solvable anagrams still required self-regulation because participants had to persist at making and breaking combinations of letters in order to come up with the correct solution, despite initial failures. I used these easier, solvable anagrams so the performance goal would be attainable.

The instructions for the anagram task were the same as those used in Study 2, but were presented on the computer. The experimenter read them aloud to the participant. An added section manipulated performance goals. Participants in the do-your-best condition were told, “Do your best to solve as many anagrams as you can.” Participants in the specific-difficult-goal condition were told, “Your goal is to identify at least 50 different words that can be created from the set of anagrams. Please try and reach or surpass this goal.” This goal was the 85th percentile for the number of anagrams participants solved correctly in a separate pilot study (see the results section of Study 3 for more details).

To reduce experimenter error in timing persistence, the computer time-stamped when the participant started and stopped working on the anagrams. Specifically, the participant was instructed to press enter on the computer when starting the task and to press escape on the computer after deciding to stop. The participant completed the task itself with paper and pencil. If the participant had not quit voluntarily when 10 minutes remained in the hour-long session, the experimenter stopped the participant. Several participants who were told to stop were asked whether they understood that they could have quit at any time; all of them indicated that they did. After the anagram task, all participants completed manipulation check questions regarding their effort exertion on the task, the difficulty of the task, and their desire to quit or keep working on the task.

At this point, the experimenter determined how long the participant had been in the study. The informed consent had indicated the session would last one hour and I wanted consistency across participants. That is, I did not want participants' decisions about how long to work on the anagrams to be influenced by whether they could finish the study early. Thus, if more than 10 minutes remained in the hour-long session after participants had quit voluntarily on the anagrams, the experimenter gave the participants filler questionnaires to complete until the 10-minute mark and then went on to the final steps of the study. If there were 10-minutes or less remaining after participants stopped working on the anagrams, they went directly to the final steps of the study.

For the final steps of the study, the experimenter gave the participant a demographics questionnaire. Participants also completed a vocabulary test (Zachary, 1986). For each of 15 target words, the participant selected the closest synonym from a group of four additional words. I used ACT scores and the vocabulary test to ensure that any performance differences on the anagram task across conditions were not attributable to differences in general cognitive ability or verbal ability. The participant also completed a study questionnaire that assessed how the goal manipulation influenced motivation and performance on the anagram task.

Results

Pilot Study

I ran a pilot study for two reasons: (1) to establish a performance goal for Study 3, and (2) to assess whether the goal effect would occur when participants performed the anagram task alone. Thirty-one participants each were assigned to a do-your-best condition and a specific-difficult-goal condition (age $M = 20.21$, $SD = 4.37$; 74% female). In these conditions, participants did not perform the typing task; they just performed the anagram task. Table 3 displays the means and standard deviations for anagram performance in the pilot study.

Using the number of anagrams that participants solved correctly in the pilot do-your-best condition, I computed the 85th percentile of performance, which was 50 anagrams. I subsequently used this number as the goal for the specific-difficult goal condition in the pilot study and in Study 3. For the pilot conditions, independent sample t-tests indicated a significant difference on number correct, $t(60) = 2.16$, $p = .03$, $d = 0.56$, but not on persistence $t(60) = 1.06$, $p = .30$, $d = 0.27$.

Study 3 Manipulation Checks

I performed independent sample t-tests on the manipulation check questions to assess how participants rated the different tasks and how they reacted to the goal manipulation. Table 4 displays means, standard deviations, and inferential statistics for the manipulation check questions. I did not expect differences on manipulation check questions for the typing task because the experimental procedure was the same for both conditions. However, participants in the specific-difficult-goal condition reported that they invested more of their total effort on the typing task than participants in the do-your-best condition, $t(64) = 2.20$, $p < .05$, $d = 0.55$. Participants rated task difficulty, how hard they worked, and how much effort they had to give similarly (ps > .37).

Regarding the anagram task, I expected participants in the specific-difficult-goal condition to rate their effort and work ethic higher than participants in the do-your-best condition. Accordingly, participants in the specific-difficult-goal condition self-reported that they invested more of their total effort than participants in the do-your-best condition, $t(65) = 2.25$, $p = .03$, $d = 0.56$. Otherwise, participants did not differ on ratings of task difficulty, how hard they worked, how much effort they had to give, or their desire to quit or keep working (ps > .19).

I also predicted that participants in the specific-difficult-goal condition would rate themselves as more motivated by the instructions. As expected, these participants self-reported that the goal motivated them to increase their effort, $t(66) = 2.55$, $p = .01$, $d = 0.63$, and persist longer on the anagram task, $t(67) = 2.60$, $p = .01$, $d = 0.63$, than participants in the do-your-best condition. Self-reported differences in whether the anagram instructions motivated participants to perform better did not reach a conventional significance level, $t(67) = 1.76$, $p = .08$, $d = 0.43$.

There were no statistically significant differences across conditions on the vocabulary measure or the BMIS pleasant-unpleasant scale (ps > .26). However, there was a significant difference on the BMIS arousal-calm scale, $t(68) = 2.31$, $p = .02$, $d = -0.56$, and a marginal difference on ACT scores, $t(60) = 1.71$, $p = .09$, $d = -0.44$.

Participants in the do-your-best condition rated themselves as more aroused and reported higher ACT scores than participants in the specific-difficult-goal condition.

Study 3 Anagram Persistence and Number Correct

Table 3 also displays the means and standard deviations for anagram performance in Study 3. I performed independent sample t-tests on persistence time and number correct in order to determine if the goal instructions affected performance on the anagram task. As predicted, participants in the specific-difficult-goal condition solved more anagrams correctly than participants in the do-your-best condition, $t(68) = 2.27$, $p = .03$, $d = 0.55$. Although the direction of the effect remained the same, the difference between conditions did not reach conventional significance for persistence time, $t(68) = 1.81$, $p = .08$, $d = 0.44$. When I included the BMIS arousal-calm scale and ACT scores as covariates, the results for number correct did not change ($p = .03$); the results for persistence time changed slightly with the *p*-value reaching .058. Some participants did not report their ACT scores so these analyses were performed on a subset of the whole sample.

I also computed effect sizes to compare performance in the Study 3 conditions to performance in the pilot conditions. Participants in the Study 3 do-your-best condition persisted less ($d = -0.91$) and solved fewer anagrams correctly ($d = -0.51$) than participants in the pilot do-your-best condition. Participants in the Study 3 specific-difficult-goal condition persisted less ($d = -0.80$) and solved fewer anagrams correctly ($d = -.52$) than participants in the pilot specific-difficult-goal condition. Notably, these decreases were comparable between the two different instruction types.

Table 3: Descriptive Statistics for the Pilot Study and Study 3 Anagram Performance

	Pilot Study				Study 3			
	Do-Your-Best Condition		Specific-Difficult Goal Condition		Do-Your-Best Condition		Specific-Difficult Goal Condition	
Variable	*M*	*(SD)*	*M*	*(SD)*	*M*	*(SD)*	*M*	*(SD)*
Persistence (in minutes)	34.03	(12.11)	37.31	(12.36)	23.87	(10.69)	28.41	(10.30)
Correct Solutions	39.77[a]	(13.77)	46.94	(12.25)	31.75[a]	(17.48)	40.29	(13.63)

[a] Indicates a significant difference for a variable within a study (p < .05).

Table 4: Descriptive and Inferential Statistics for Study 3 Questionnaires

	Do-Your-Best Condition				Specific-Difficult Goal Condition						
			95 % CI				95 % CI				
Variable	M	SD	LL	UL	M	SD	LL	UL	$t(68)$ [a]	p	Cohen's d
Typing Task Ratings											
Effort Invested	83.81	21.26	76.87	90.75	92.35	8.28	89.57	95.13	2.19	.03	0.53
Task Difficulty	72.06	19.11	65.82	78.30	68.00	26.31	59.16	76.84	-0.74	.46	-0.18
How hard did you work?	76.25	18.06	70.35	82.15	71.53	26.08	62.76	80.30	-0.88	.38	-0.21
How much effort did you give?	83.61	18.54	77.55	89.67	83.18	15.84	77.86	88.50	-0.11	.92	-0.03
BMIS											
Pleasant-Unpleasant Scale	5.86	6.98	3.58	8.14	4.88	5.28	3.11	6.65	0.66	.51	-0.16
Arousal-Calm Scale	16.61	4.10	15.27	17.95	14.47	3.60	13.26	15.68	2.31	.02	-0.56
Anagram Task Ratings											
Effort Invested	82.09	21.86	74.85	89.33	91.81	11.51	87.82	95.80	2.25[b]	.03	0.56
Task Difficulty	82.97	15.74	77.76	88.18	77.31	19.80	70.45	84.17	-1.30[b]	.20	-0.32
How hard did you work?	83.86	15.43	78.75	88.97	86.09	12.10	81.90	90.28	0.66[b]	.51	0.16
How much effort did you give?	83.37	20.74	76.50	90.24	87.28	12.75	82.86	91.70	0.92[b]	.36	0.23
How much did you want to stop working?	68.71	28.58	59.24	78.18	61.28	36.71	48.56	74.00	-0.93[b]	.36	-0.23
Did you keep working after you thought about quitting?	75.80	29.16	66.14	85.46	78.25	26.67	69.01	87.49	0.36[b]	.72	0.09

Table 4 (cont'd)

How long did you think about quitting before you did?	55.11	28.94	45.52	64.70	53.84	33.23	42.33	65.35	-0.17[b]	.87	-0.04
How much did you force yourself to keep working?	67.37	26.82	58.48	76.26	74.06	25.25	65.31	82.81	1.05[b]	.30	0.26
Influence of Anagram Instructions											
Did goal motivate you to perform better?	58.34	28.81	48.80	67.88	70.82	30.17	60.68	80.96	1.76[c]	.08	0.43
Did goal increase your effort?	59.03	30.65	48.73	69.33	76.09	24.21	67.95	84.23	2.55[d]	.01	0.63
Did goal influence you to persist longer?	63.66	31.90	53.09	74.23	80.76	21.67	73.48	88.04	2.60[c]	.01	0.63
ACT	25.32	2.98	24.27	26.37	23.87	3.68	22.57	25.17	1.71[e]	.09	-0.44
Vocabulary	9.78	2.82	8.86	10.70	9.12	2.09	8.42	9.82	1.11	.27	-0.27

Note. CI = Confidence Interval; *LL* = lower limit; *UL* = upper limit.

[a] The statistics reported here are the *t*s associated with the test when equal variances were assumed. Although there were cases in which Levene's test was significant, in no case was the *t* associated with equal variances assumed significant when the *t* associated with equal variances not assumed was not significant. [b] These *t*-tests had 65 *df* because some participants did not receive all of the manipulation check questions. [c] These *t*-tests had 67 *df* because some participants did not answer all of the questions. [d] This *t*-test had 66 *df* because some participants did not answer all of the questions. [e] This *t*-test had 60 *df* because some participants did not report their ACT scores.

Discussion

The manipulation check items did not completely follow predicted patterns. Some patterns suggested that participants in the specific-difficult-goal condition were more depleted following the typing task and that participants in the do-your-best condition were more aroused going into the anagram task and potentially had greater cognitive abilities. If anything, this should have given participants in the do-your-best condition an advantage on the anagram task. Nevertheless, the results indicated these differences were not important; instead, the motivation manipulation had the greatest influence on anagram performance. I expected differences on self-report questions on the anagram task, but few differences were significant. However, participants in the specific-difficult-goal condition did indicate that they put in more total effort than participants in the do-your-best condition.

Overall, the results of Study 3 support a motivation account of the depletion effect. Specifically, a motivation manipulation shifted performance among participants objectively depleted to the same degree. This indicates that performing an effortful regulatory task did not entirely hinder the ability to perform on the second task. Rather, when participants were more motivated, they solved more anagrams correctly. The effect of the motivation manipulation did not extend to persistence time. Given the nature of the specific, difficult goal, which focused on solving a certain number of anagrams, it makes sense that the largest effect was on number correct.

The comparison between Study 3 conditions and the pilot conditions also support a motivation account of task performance. With no prior task, participants with a specific, difficult goal solved more anagrams correctly than participants simply told to do their best (*d* = .56). In other words, the goal motivated participants to exert more effort on the task. In Study 3, this same pattern emerged even when participants had just completed an effortful regulatory task (*d* = .55). Furthermore, the size of the goal effect within the pilot conditions and within the Study 3 conditions was similar in magnitude. If true depletion was a factor in Study 3 conditions, the goal effect should have been smaller because motivation was competing against decrements in ability.

When compared to the pilot conditions, anagram performance decreased after an effortful regulatory task, but the size of the decrease was comparable for participants in do-your-best or specific-difficult-goal conditions. This suggests that the motivation to exert effort generally declined after a prior task, but the specific, difficult goal sufficiently motivated participants to exert more effort whether there was a prior task or not. In other words, people are miserly when it comes to effort exertion, but that miserliness reflects motivation and not capability.

CHAPTER 5: STUDY 4- INFLUENCE OF PERCEIVED EFFORT

Study 4 Rationale and Predictions

In Study 3, I investigated whether a specific, difficult goal would increase participants' effort exertion, despite equivalent levels of depletion with other participants. In Study 4, I investigated whether greater perceived effort exertion on the first task would decrease participants' effort exertion, despite equivalent levels of depletion with other participants. I manipulated perceived effort using a design similar to that of Kivetz and Zheng (2006).

Kivetz and Zheng (2006) showed that manipulating perceived effort on an initial task influenced subsequent choices. Their first task involved unscrambling sentences. Participants were told that they would be randomly assigned to a particular level of effort; participants in the low-relative-effort condition were told that the number of sentences they would construct could be between 14 and 28 and participants in the high-relative-effort condition were told the number could be between 4 and 18. All participants were told they had been assigned to unscramble 16 sentences. Thus, in the low-relative-effort condition, participants believed they had to exert less effort relative to other participants and in the high-relative-effort condition, participants believed they had to exert more effort relative to other participants.

Kivetz and Zheng (2006) predicted that those in the high-relative-effort condition would be less motivated to continue exerting effort subsequently because they would think they had already exerted enough effort while the opposite would be true for those in the low-relative-effort condition. Indeed, even though all participants unscrambled the same number of sentences, participants in the high-relative-effort condition subsequently chose what Kivetz and Zheng deemed “self-indulgent” rewards more often than participants in the low-relative-effort condition. They argued that even when the first task was the same across conditions, perceptions of effort on that task influenced participants to exert more or less effort subsequently.

In Study 4, I had all participants perform the same initial effortful regulatory task. Afterward, I gave participants false feedback that suggested they either exerted relatively low or high effort on the first task. Then, I had all participants perform a second effortful regulatory task. According to a resource account, the false feedback should not affect performance because of equivalent depletion across participants. According to a motivation account, participants in the high-perceived-effort condition should be less motivated to continue exerting effort and should perform significantly worse on the second task than those in the low-perceived-effort condition.

Method

Participants

Seventy undergraduates from the subject pool at Michigan State University participated in this study for partial course credit. After the study, the experimenter debriefed the participants about the nature of the perceived-effort deception. The experimenter also asked the participants whether they believed the effort feedback at the time that it was given. Participants’ responses were coded as either believing the feedback, partially believing, or not believing. In the analyses for this study, I included only those participants who fully believed the report.[5] This left a sample of 47 (low-perceived-effort condition, *n* = 21; high-perceived-effort condition, *n* = 26). The final sample had a mean age of 19.91 (*SD* = 2.39) and was 81% female. An a priori power analysis indicated a sample of 70 would have 80% power (alpha = .05, two-tail) to reject the null hypothesis that there would be no difference in performance across conditions. A post hoc power analysis indicated the final sample of 47 had 73% power (alpha = .05, two-tail) to reject the null hypothesis that there would be no difference in performance across conditions.

Procedure and Materials

Participants were randomly assigned to one of two conditions: a low-perceived-effort condition or a high-perceived-effort condition. Participants completed the study individually in seven steps: (a) a typing task and manipulation check questions, (b) the BMIS, (c) an effort feedback segment, (d) an anagram task and manipulation check questions, (e) filler tasks (this part was conditional, just like in Study 3), (f) a demographics questionnaire, (g) a vocabulary test, and (h) a study questionnaire. Participants were informed the session would last one hour.

During a session, the experimenter seated the participant in front of a computer. The experimenter calibrated an eye tracker (an Eyelink II at 500 Hz) to the participant's eye movements and informed the participant that the eye tracker would be monitoring

[5] I decided to only include those who believed the effort feedback in the analyses to ensure that the results would be the cleanest test of my hypotheses. It is possible that disbelieving the feedback could have interacted with the motivation to continue to exert effort. Results on the main dependent variables did not differ if I included all participants in the analyses. However, the difference on one manipulation check question, "Did your effort report on the typing task influence you to decrease your effort on the anagram task?" was not significantly different with the full sample, but was with the reduced sample.

pupil diameter throughout the task. Then the experimenter read aloud the instructions for the typing task. The typing task and instructions were the same as those used in Study 3, with an added section instructing the participant not to look down while typing, as this interrupted the eye tracking.

After 6 minutes had elapsed, the typing task ended and the experimenter stated, “Here are two questionnaires for you to fill out. While you are filling them out, I will be putting together a report regarding your pupil diameter during the typing task. The computers are networked so I don’t have to get on your computer to do this.” The experimenter handed the participant a sheet with several manipulation check questions about the typing task and the BMIS. Then, the experimenter went just outside of the study room to another computer that was networked with a printer. The experimenter logged on to the computer, opened a document of the pre-set effort report, and printed two copies of it.[6] The report was set up to print the current date and time. These procedures were implemented to lend credibility to the effort report. After the copies were printed, the experimenter returned to the study room, gave the participant one copy, and kept the other, telling the participant to follow along as the experimenter read the report.

In both conditions, the report began by summarizing scientific research that used pupil diameter as a measure of effort on behavioral tasks. The report then indicated that

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[6] During Study 4, I had technical difficulties with the printer. For 17 participants, it did not connect to the network and failed to print the report. However, the printer made printing-like sounds for 14 of these participants. I had extra copies of the report printed and used these when the printer failed. To ensure that this did not affect participants’ belief of the pupil report, I performed an independent t-test with report belief as the dependent variable and whether the printer worked or not as the grouping variable. The result was not significant ($p > .40$).

the participant’s pupil data had been analyzed as an indicator of his or her effort exertion on the typing task. The report showed a graph with two lines plotted, one representing the participant’s data and one representing other participants’ data for comparison. The report had a written summary of the participant’s data in comparison to others’. The graph and summary were false and intended to deceive participants into thinking they exerted more or less effort relative to other participants. Specifically, in the high-perceived-effort condition, the graph showed that the participant’s pupil diameter was higher than the other participants’ pupil diameter and the summary stated, “The pupil data show that you exerted more effort than the average participant on the typing task. Specifically, you exerted more effort than 81.5% of the other participants” (see Appendix G for a complete copy of the high-effort pupil report). In the low-perceived-effort condition, the graph showed that the participant’s pupil diameter was lower than the other participants’ pupil diameter and the summary stated, ”The pupil data show that you exerted less effort than the average participant on the typing task. Specifically, 81.5% of the other participants exerted more effort than you” (see Appendix H for a complete copy of the low-effort pupil report).

Participants in both conditions then completed the anagram task. In Study 2, 27 of the 48 items on the anagram task were unsolvable. In Study 4, I adapted this list by replacing the unsolvable anagrams with solvable ones. To replace two of the items, I removed one extra letter that had made the anagram unsolvable (e.g., OERIRFPM became OERRFPM: PERFORM). To replace the remaining unsolvable items, I used the Medical Research Council Psycholinguistics database to generate a list of seven-letter nouns that had concreteness and familiarity ratings of 400 or higher (these ratings range from 100 to 700; Wilson, 1998). I randomized this list and selected 25 anagrams to replace the unsolvable anagrams from the original list (see Appendix I). The basic instructions and computer methodology were the same as those used in Study 3.

As in Study 3, the experimenter stopped the participant on the anagrams if he or she was still working on them 10-minutes before the end of the hour-long session. The experimenter asked several of these participants whether they understood that they could quit at any time; all of them indicated that they did. Otherwise, the experimenter followed the same procedure involving filler tasks to ensure that participants were in the study session for approximately one hour.

After the anagram task, the participant answered manipulation check questions regarding the task. The final steps of the study were completed after the filler tasks (if they were administered). The participant completed a demographics questionnaire and the same vocabulary test used in Study 3. The participant also completed a study questionnaire that assessed how the pupil data influenced their effort exertion on the anagrams and whether they thought the pupil report was an accurate reflection of their exerted effort.

Results

Manipulation Checks

I performed independent sample t-tests on the manipulation check questions to assess how the participants rated themselves on different aspects for each task and how they reacted to the effort feedback. See Table 5 for means, standard deviations, and inferential statistics for the manipulation check questions. As predicted, there were no differences in task difficulty, hard work, or effort ratings for the typing task (ps > .77).

Unexpectedly, there were also no significant differences on any self-report questions for the anagram task (ps > .12). Participants rated task difficulty and their effort, work, and desire to do the task similarly.

I also examined participants' self-reported reaction to the pupil diameter feedback. I predicted that participants in both conditions would indicate that the effort reading from the pupil data was accurate and similar to their subjective feeling of effort. However, compared to the high-perceived-effort participants, participants in the low-perceived-effort condition felt that the pupil diameter feedback was not similar to their subjective feeling of effort, $t(44) = -3.79$, $p < .001$, $d = -1.15$, and was not an accurate reflection of their effort, $t(45) = -5.06$, $p < .001$, $d = -1.52$.

However, if the pupil diameter feedback successfully manipulated participants' motivation to exert effort, participants in the high-perceived-effort condition should have been less motivated to perform better on the anagram task. To assess this, participants were asked whether the pupil diameter feedback motivated them to increase their effort on the anagram task and whether the pupil diameter feedback influenced them to decrease their effort. While the difference between conditions was not significant on the first question ($p > .37$), the difference was significant on the second question, $t(45) = -2.04$, $p = .048$, $d = -0.61$. Participants in the high-perceived-effort condition indicated that the pupil diameter feedback influenced them to decrease their effort on the anagram task.

There were no statistically significant differences across conditions on the mood scales, ACT scores, or performance on the vocabulary measure (ps > .41). Thus, mood or verbal ability did not differentially affect performance on the anagram task.

Anagram Persistence and Number Correct

I performed independent sample t-tests on persistence time and number correct to determine if the perceived-effort manipulation affected performance on the anagram task. Table 6 displays means, standard deviations, and inferential statistics for anagram performance. As predicted, participants in the high-perceived-effort condition solved fewer anagrams correctly than participants in the low-perceived-effort condition, $t(45) = 2.57$, $p = .01$, $d = 0.77$. Additionally, high-perceived-effort participants persisted less than low-perceived-effort participants, $t(45) = 2.95$, $p = .005$, $d = 0.89$.

In a separate pilot study with 53 participants, individuals with no prior task or feedback persisted on average 28.80 minutes ($SD = 13.81$) and solved on average 9.72 anagrams ($SD = 5.05$). Compared to this pilot study, performing a prior task decreased the average amount of time that participants decided to persist on the anagrams, but particularly so for participants in the high-perceived-effort condition ($d = -1.31$ versus $d = -0.58$ for participants in the low-perceived-effort condition). In terms of number correct, those in the high-perceived-effort condition solved fewer anagrams ($d = -0.86$) than those in the pilot study, whereas participants in the low-perceived-effort condition solved a comparable number to those in pilot testing ($d = -0.19$).

Table 5: Descriptive and Inferential Statistics for Study 4 Questionnaires

	High-Perceived-Effort Condition				Low-Perceived-Effort Condition						
			95 % CI				95 % CI				
Variable	M	SD	LL	UL	M	SD	LL	UL	$t(45)^{a}$	*p*	Cohen's *d*
Typing Task Ratings											
Effort Invested	90.00	18.62	82.84	97.16	90.19	15.08	83.74	96.64	0.04	.97	0.01
Task Difficulty	84.46	18.42	77.38	91.54	85.81	12.24	80.57	91.05	0.29	.78	0.09
How hard did you work?	86.42	17.90	79.54	93.30	86.38	13.29	80.70	92.06	0.01	.99	0.00
How much effort did you give?	90.54	17.92	83.65	97.43	91.14	11.55	86.20	96.08	0.13	.89	0.04
BMIS											
Pleasant-Unpleasant Scale	7.27	7.89	4.24	10.30	5.76	6.24	3.09	8.43	-0.71	.48	-0.21
Arousal-Calm Scale	16.12	4.10	14.54	17.70	15.52	3.52	14.01	17.03	-0.52	.60	-0.16
Anagram Task Ratings											
Effort Invested	77.62	21.69	69.28	85.96	86.43	16.33	79.45	93.41	1.54	.13	0.46
Task Difficulty	91.00	11.05	86.75	95.25	92.00	9.58	87.90	96.10	0.33	.75	0.10
How hard did you work?	92.08	11.03	87.84	96.32	86.57	17.35	79.15	93.99	-1.32	.19	-0.40
How much effort did you give?	87.31	20.98	79.25	95.37	88.57	13.44	82.82	94.32	0.24	.81	0.07
How much did you want to stop working?	70.96	26.42	60.80	81.12	74.57	26.83	63.09	86.05	0.46	.65	0.14
Did you keep working after you thought about quitting?	82.46	24.11	73.19	91.73	82.43	24.31	72.03	92.83	0.01	.99	0.00

Table 5 (cont'd)

How long did you think about quitting before you did?	69.69	25.61	59.85	79.53	73.81	32.12	60.07	87.55	0.49	.63	0.15
How much did you force yourself to keep working?	73.62	24.65	64.14	83.10	70.52	28.94	58.14	82.90	-0.40	.69	-0.12
Influence of Pupil Diameter Feedback on Anagram Task											
How similar was feedback report and subjective feeling of effort?	80.92	22.96	71.92	89.92	45.86	39.01	29.18	62.54	-3.79[b]	<.001	-1.15
How accurate was the feedback?	89.79	15.31	83.91	95.67	46.57	40.18	29.38	63.76	-5.06	<.001	-1.52
Did feedback increase your effort?	56.54	31.27	44.52	68.56	64.52	29.32	51.98	77.06	0.90	.38	0.27
Did feedback decrease your effort?	28.15	31.21	16.15	40.15	12.52	17.92	4.86	20.18	-2.04	.048	-0.61
ACT	24.83	3.27	23.52	26.14	25.06	3.44	23.42	26.70	0.21[c]	.83	0.07
Vocabulary	9.23	2.69	8.20	10.26	9.86	2.50	8.79	10.93	0.82	.42	0.25

Note. CI = Confidence Interval; *LL* = lower limit; *UL* = upper limit.

[a] The statistics reported here are the *t*s associated with the test when equal variances were assumed. Although there were cases in which Levene's test was significant, in no case was the *t* associated with equal variances assumed significant when the *t* associated with equal variances not assumed was not significant. [b] This *t*-test had 44 *df* because some participants did not answer all of the questions. [c] This *t*-test had 39 *df* because some participants did not report their ACT scores.

Table 6: Descriptive and Inferential Statistics for Study 4 Anagram Performance

	High-Perceived-Effort Condition				Low-Perceived-Effort Condition						
			95 % CI				95 % CI				
Variable	M	SD	LL	UL	M	SD	LL	UL	$t(45)^{a}$	p	Cohen's d
Persistence (in minutes)	13.97	7.16	11.22	16.72	21.57	10.43	17.11	26.03	2.95	.005	0.89
Correct Solutions	6.04	3.13	4.84	7.24	8.76	4.15	6.99	10.53	2.57	.01	0.77

Note. CI = Confidence Interval; *LL* = lower limit; *UL* = upper limit.

[a] The statistics reported here are the *t*s associated with the test when equal variances were assumed. Although there were cases in which Levene's test was significant, in no case was the *t* associated with equal variances assumed significant when the *t* associated with equal variances not assumed was not significant.

Discussion

Manipulation check questions showed no differences between conditions on the typing task. This indicated that participants in both conditions viewed the initial task similarly and put forth the same amount of effort. Participants were depleted to the same degree before they received the pupil diameter feedback and completed the second task.

Regarding the pupil feedback, some participants did not believe the report at the time it was given to them. While initially surprising, this does make sense when looking more closely at the procedure. Participants rated task difficulty, their effort, and how hard they worked on the typing task and then received the pupil diameter feedback. As the typing task was quite challenging, most participants reported that they put forth significant effort and worked hard on the task. Therefore, when the experimenter told low-perceived-effort participants that they did not put forth significant effort, this feedback contradicted the self-report ratings they had just given.

There were no differences on manipulation check questions for the anagram task although I had expected the ratings to coincide with actual performance. However, it is possible that the pupil report changed participants' perception of effort on the anagram task. For example, participants told their effort was above average on the first task may have inflated the anagram task ratings subsequently.

The anagram performance results suggest that participants act as motivated tacticians. Participants told they exerted more effort initially withheld effort on the anagram task and performed worse than participants told they exerted less effort initially. This coincides with the self-report data where high-perceived-effort participants indicated the pupil diameter feedback influenced them to decrease their effort. These results also confirmed the meta-analysis. Study 4 showed that differences in perceived effort led to a depletion-like effect. The meta-analysis showed that differences in self-reported effort related to the size of the depletion effect.

Furthermore, the low-perceived-effort participants performed similarly to the pilot participants who had not had a prior task. Even though participants in the low-perceived-effort condition may have persisted less, they still solved about the same number of anagrams. If performing the anagram task in a depleted state truly incapacitated regulatory abilities, these participants should not have been able to perform as accurately on the anagram task as participants in the pilot study.

These results, together with the results of the previous three studies, suggest that the depletion effect and regulatory performance are often dictated by motivation, rather than the availability of a regulatory resource. If individuals have already exerted significant effort, they are less motivated to continue exerting effort. If initial effort exertion is the same among participants, performance shifts up or down depending on the willingness to continue exerting effort. An individuals' willingness to exert effort reflects many factors, including goals and perceptions of previous efforts.

CHAPTER 6: STUDY 5- CONSERVATION OF RESOURCE VS. EFFORT

Study 5 Rationale and Predictions

Earlier, I discussed research by Muraven et al. (2006) that suggested the depletion effect partly results from resource conservation. Resource conservation is an alternative explanation for Studies 3 and 4. That is, while the first task depleted participants' resource to the same extent, the motivation manipulations may have encouraged some participants (e.g., in the low-perceived-effort condition) to continue using what resources remained, while encouraging other participants (e.g., in the high-perceived-effort condition) to conserve their remaining resources.

Muraven et al. (2006) investigated the concept of conservation across several studies. In some of them, they used the typical two-ask procedure, but manipulated instructions on the second task to make some participants believe there would be a challenging self-regulatory third task. Other participants were led to believe there would be a challenging non-regulatory third task or remained ignorant of a third task. Participants who believed there would be a regulatory third task performed worse on the second task than participants who thought it would be non-regulatory or did not know. Muraven et al. argued that these results supported the resource account's assertion that the depletion effect, and now conservation, depend on whether or not self-regulation is involved in prior or future efforts.

In their final study, Muraven et al. (2006) used a three-task procedure in order to assess how conserving or not conserving affected performance when participants actually completed the third task. After a depleting initial task, participants who anticipated a challenging self-regulatory third task exhibited greater depletion effects on a second task than those who anticipated a challenging non-regulatory third task. In other words, when participants expected the need for future regulation, they were more likely to conserve. Muraven et al. also found a trade-off between performance on the second and third task. If depleted participants conserved on the second task (and performed worse), they performed better on the third task, and vice versa. According to the resource account, depleted participants who conserved on the second task still had enough resources to perform the third task, whereas depleted participants who did not conserve on the second task did not have sufficient resources to perform the third task.

Muraven et al.'s (2006) studies lend some support for the regulatory-specific nature of the depletion and conservation effect. However, they did not address whether the conservation effect is truly the conservation of a resource or whether it is simply motivated conservation of effort. Even though the conservation concept introduces a motivation element to the resource account, the underlying assumption remains the same: after prior regulatory exertion, individuals operate with reduced resources. Thus, the ability to regulate becomes increasingly limited for those who continue to expend regulatory resources (e.g., like depleted participants who did not conserve in Muraven et al.). If, however, the effects are motivational in nature, there should be no limits on performance for depleted individuals who do not conserve.

I designed Study 5 to investigate whether it is more likely that individuals conserve a limited, expendable resource or whether individuals simply conserve effort. Although it was not a direct replication, I used the same three-task procedure as Muraven et al.'s Study 4 (2006), where participants performed a typing task, a Stroop task, and then an anagram task. On the first task, all participants received the same instructions. When explaining the second task, half of the participants were told to expect a challenging self-regulatory third task and half of the participants did not expect a third task at all. Then, before the third task, there was a motivation manipulation where half of the participants received a specific, difficult goal and half of the participants received do-your-best instructions.

According to the resource account, participants who did not know about the third task (i.e., low-conservation condition) should not hold back effort on the second task. Thus, with *doubly depleted* resources, they should unavoidably perform worse on the third task. No amount of motivation should enable these participants to perform at the same level as those who did conserve on the second task (i.e., high-conservation condition). If, however, no real reduction in resource availability occurs, participants who did not conserve should still be capable of exerting a high level of effort and of performing just as well or better on the third task than those who did conserve, as long as they are sufficiently motivated to do so (e.g., when given a specific, difficult goal).

Another key to this design is that I used the same list of anagrams from Study 3. This allowed me to examine performance on this task not only in the context of conservation in this study, but also in the context of when the task was performed after just one task and when it was performed with nothing prior. That is, this provided a way to compare performance on the anagram task when participants had do-your-best instructions or a specific, difficult goal and were presumably at full capacity, depleted, or doubly depleted. I know from Studies 3 and 4 that performance declined to a certain degree when comparing participants who performed the anagram task by itself to participants who performed the anagram task after a regulatory task. The results from this study allowed me to assess whether performance continued to decline for doubly depleted participants.

Method

Participants

Participants were 118 undergraduate students from the subject pool at Michigan State University who received partial course credit for participation. Six participants were removed from all analyses. Four of these participants did not understand or follow instructions on the Stroop task. One participant did not start typing on the first task as directed and only typed four out of the six minutes. One participant's computer malfunctioned, disrupting the study procedure and data recording. This left a sample of 112 (high-conservation/do-your-best goal condition, n = 29; high-conservation/specific-difficult goal condition, n = 29; low-conservation/do-your-best condition, n = 26; low-conservation/specific-difficult goal condition, n = 28). The final sample had a mean age of 20.36 (SD = 3.18) and was 51% female.

Some participants were not included in all analyses. Four participants were removed from analyses relating to the anagram task because they did not follow the instructions or the computer malfunctioned. Two participants were removed from the anagram persistence analysis because the computer timing was inaccurate. Additionally, there were some cases where participants did not answer all of the manipulation check questions.

Procedure and Materials

Participants were randomly assigned to one of four conditions: a high-conservation/do-your-best condition, a high-conservation/specific-difficult-goal condition, a low-conservation/do-your-best condition, or a low-conservation/specific-difficult-goal condition (see Table 7 for a layout of the design). Participants completed the study individually in seven steps: (a) a timed typing task and manipulation check questions, (b) a conservation manipulation segment, (c) a Stroop task and manipulation check questions, (d) an anagram task and manipulation check questions, (e) a demographics questionnaire, (f) a vocabulary test, and (g) a study questionnaire. Participants were informed the session would last one hour.

Table 7: Design of Study 5

Condition	1st Task	2nd Task	3rd Task
High-Conservation/ Do-Your-Best	Typing Task	Stroop Task w/ Expectation of Task 3	Anagram Task w/ Do-Your-Best Instructions
High-Conservation/ Specific-Difficult	Typing Task	Stroop Task w/ Expectation of Task 3	Anagram Task w/ Specific, Difficult Goal
Low-Conservation/ Do-Your-Best	Typing Task	Stroop Task w/ No Expectation of Task 3	Anagram Task w/ Do-Your-Best Instructions
Low-Conservation/ Specific-Difficult	Typing Task	Stroop Task w/ No Expectation of Task 3	Anagram Task w/ Specific, Difficult Goal

Participants were seated in front of a computer. Unlike the previous studies, I designed this study so that participants guided themselves through the majority of the tasks by reading instructions on the computer. I made this change in order to improve the design of the study and eliminate the possible effects of experimenter bias. Before getting started, the experimenter explained to the participant that the computer would guide them through the study and told the participant to read and follow all of the instructions carefully. The experimenter encouraged the participant to contact the experimenter with any questions not answered by the computer instructions.

Additionally, the experimenter told the participant that the computer instructions would direct him or her to contact the experimenter at certain points to receive further instructions.

After running several participants with this format, I found that more participant errors occurred. For example, some participants used the wrong keys to respond during the Stroop task or pressed certain keys during the anagram task that interrupted persistence timing. To reduce participant error, I added more instructions at the beginning of the study, such as pointing out certain keys the participant would use. After these instructions, the experimenter left the room, but listened to make sure the participant got started on the first task.

The first task was the same typing task used in Studies 3 and 4. The basic instructions were the same, with some instructions added to facilitate the new participant-guided procedure. Specifically, the instructions gave the participant an example of what the typing should look like (e.g., If they saw, "The big red tree." they should type, "Thbigrdtr.") and described different types of errors (e.g., typing spaces or E's, skipping over other characters, or typing other characters not in the paragraph). After 6 minutes elapsed, the typing task ended and the participant answered manipulation check questions regarding their effort and the difficulty of the task. The participant entered their ratings for the questions using numbers on the keyboard and pressing enter.

The computer then presented instructions for the Stroop task:

> In the color-word identification task, you will see a color-word appear in the middle of the screen and will need to correctly identify the COLOR of ink that the word is

written in. Your response will either be blue, green, or red. These responses correspond, respectively, to the 1, 2, and 3 keys on the number pad of the keyboard. They have been labeled B for blue, G for green, and R for red for your convenience. For example, if you saw the following: blue (the word was printed in red), the correct answer would be, "Red," so you would press the key labeled "R" on the number pad. At the beginning of each trial, you will see a fixation cross, followed by a word. Once you make your response, another fixation cross will appear, followed by another word, and so on. Research has shown that color-word identification is challenging because it requires you to suppress or override the automatic tendency to read the color-word in order to identify the color of ink the word is printed in.

For participants in the low-conservation conditions, these instructions were prefaced with, "You will complete a color-word identification task next." For participants in the high-conservation conditions, these instructions were prefaced with, "There are two more tasks to complete. You will complete a color-word identification task next, followed by an anagram task," and were followed with these additional instructions, "After performing the color-word identification task, you will complete an anagram task. This task will also require your ability to suppress or override thoughts and intentions. In order to correctly solve the anagrams, you should expect to work hard at overriding impulses while working on them." Thus, participants in the low-conservation conditions did not know about a third task, whereas participants in the high-conservation conditions knew about a third task and knew that it would be challenging in the same way that the Stroop task was challenging. I adapted these instructions from Muraven et al.'s fourth study (2006). They were designed to influence high-conservation participants, relative to low-conservation participants, to withhold effort on the Stroop task because they knew another challenging regulatory task would immediately follow.

Following these instructions, the participant completed the Stroop task, which included 80 trials. Afterward, the participant answered manipulation check items regarding the difficulty of the task, effort on the task, conservation on the task, and anticipation of a future task. The participants also compared their effort on the Stroop task to their effort on the typing task; responses ranged from 1 (*a lot more effort on the typing task than the Stroop task*) to 5 (*a lot more effort on the Stroop task than the typing task*). To assess participants' comprehension of the conservation manipulation, the participant chose out of five options what task they thought they would complete next (1 = *Don't know*, 2 = *Color-word identification task*, 3 = *Anagram task*, 4 = *Typing task*, or 5 = *Maze task*). I anticipated that high-conservation participants would select the anagram task most often, while the low-conservation participants would indicate they did not know most often.

After the Stroop manipulation check questions, the computer provided instructions for the anagram task. These instructions, the same as those used in Studies 3 and 4, explained how to solve the anagrams and told the participant to press enter when ready to begin and to press escape when done. Additionally, half of the participants from the low- and high-conservation conditions were given do-your-best instructions for the task and half were given a specific, difficult goal for the task (i.e., 50 anagrams). Then the computer directed the participant to contact the experimenter. The experimenter handed the sheets of anagrams to the participant and reminded them to press enter before starting. The experimenter then left the room while the participant worked on the anagrams with paper and pencil. When the participant pressed enter, there was a reminder on the screen about the participant's condition-specific instructions (i.e., complete a goal or do-your-best) and the quitting instructions (i.e., press escape when you feel you have worked on the task long enough). When the participant decided to stop working on the anagrams and pressed escape, the computer guided them through manipulation check questions regarding the difficulty of the task, effort on the task, and how the option of quitting affected effort and the desire to quit.

Following that, the computer directed the participant to contact the experimenter. The experimenter gave the participant the demographics questionnaire, the vocabulary test, and a study questionnaire that assessed how the anagram instructions affected effort and motivation on the task. The participants also compared their effort on the anagram task to their effort on the Stroop task; responses ranged from 1 (*a lot more effort on the Stroop task than the anagram task*) to 5 (*a lot more effort on the anagram task than the Stroop task*).

After the participant completed these questionnaires, the experimenter assessed how long the participant had been in the study. If there were more than 5 minutes remaining in the hour-long session, the experimenter had the participant complete filler questionnaires until the 5-minute mark and then the experimenter debriefed the participant. If there were 5-minutes or less remaining, the experimenter went directly to debriefing the participant. This methodology changed somewhat from Studies 3 and 4. In these studies, participants completed fillers and then completed the last set of questionnaires. In this study, participants completed the last set of questionnaires and then did fillers. I changed this to keep the ratings on the final study questionnaire as close to the final task as possible to reduce the possibility that participant ratings would change by completing the filler tasks themselves or by the passage of time.

Results

Manipulation Checks

I performed 2 (Conservation: high, low) X 2 (Goal Type: do-your-best, specific-difficult) ANOVAs on several variables as manipulation checks. Table 8 displays means and standard deviations for manipulation check questions administered following the typing task. Since there were no manipulations on the first task, I predicted no differences across conditions. Consistent with this prediction, none of the main effects or interactions were significant for total effort exerted, task difficulty, hard work, effort given, degree of self-control, or fighting an urge (ps > .11).

Table 8: Descriptive Statistics for Study 5 Manipulation Check Questions on the Typing Task

Variable	Do-Your-Best Conditions				Specific-Difficult Goal Conditions			
	Low-Conservation Condition		High-Conservation Condition		Low-Conservation Condition		High-Conservation Condition	
	M	(*SD*)	*M*	(*SD*)	*M*	(*SD*)	*M*	(*SD*)
Effort Invested [a]	89.73	9.58	89.59	10.60	86.04	17.27	90.79	13.89
Task Difficulty	68.96	22.94	69.66	21.80	65.11	22.83	70.21	23.01
How hard did you work?	73.88	21.85	78.55	16.32	71.57	23.01	78.79	18.92
How much effort did you give?	84.92	16.60	85.00	12.54	74.89	25.22	83.03	21.42
Self-Control	84.92	14.81	86.38	13.56	83.71	12.71	79.79	19.72
Fight Against an Urge	76.27	23.95	86.52	18.01	78.21	26.93	81.28	21.08

Note. [a] Levene's test of equality of error variances was significant ($p < .05$) for this question.

Table 9 displays means and standard deviations for manipulation check questions administered following the Stroop task. I predicted that participants in the high-conservation conditions would rate effort-type questions lower and rate self-control- and conservation-type questions higher than participants in the low-conservation conditions. I did not predict a main effect of goal type or any interactions because the goal manipulation had not taken place at the time participants answered these questions.

However, there were main effects of goal type and conservation on the self-control question, $F(1, 106) = 5.63$, $p = .02$, partial $\eta^2 = .05$, $d = 0.46$, and $F(1, 106) = 3.94$, $p = .05$, partial $\eta^2 = .04$, $d = 0.39$, respectively. Participants in the do-your-best conditions indicated the task required more self-control than participants in the specific-difficult-goal conditions, and participants in the high-conservation conditions rated self-control requirements higher than participants in the low-conservation conditions. The Conservation x Goal Type interaction was significant for the item, “How difficult was the Stroop task?” $F(1, 106) = 5.00$, $p = .03$, partial $\eta^2 = .05$. The pattern of means indicated that for specific-difficult-goal conditions, participants in the high-conservation condition rated the Stroop task as more difficult than those in the low-conservation condition ($d = 0.32$), whereas the opposite held for the do-your-best conditions ($d = 0.33$).

There was a marginally significant effect of conservation on how much participants had to fight an urge on the Stroop task, such that participants in the high-conservation conditions indicated they had to fight an urge more than participants in the low-conservation conditions, $F(1, 106) = 3.73$, $p = .056$, partial $\eta^2 = .03$, $d = 0.38$. There was also a marginally significant main effect of goal type for total effort exerted; participants in the do-your-best conditions rated their total effort exertion higher than participants in the specific-difficult-goal conditions, $F(1, 107) = 3.67$, $p = .058$, partial $\eta^2 = .03$, $d = 0.37$. Participants compared their effort on the Stroop task to their effort on the typing task (ratings could range from 1 to 5, with 1 meaning more effort on the typing task and 5 meaning more effort on the Stroop task). Participants generally indicated they put more effort into the typing task, overall $M = 1.79$, $SD = 1.07$; there were no significant differences across conditions (ps > .48). All other analyses on the Stroop manipulation check questions (i.e., hard work, effort given, and conservation) were also non-significant (ps > .10).

Following the Stroop task, participants indicated what task they thought they would perform next. Participants were given five options, one being, “Don’t know,” and another being, “Anagram task.” Of the participants in the low-conservation conditions, who should not have known what task came next, 56% indicated they did not know what the third task would be and 19% indicated they thought it would be an anagram task. Of the participants in the high-conservation conditions, who had been told what task came next, 85% indicated they thought it would be an anagram task. Thus, participants appeared to comprehend the conservation manipulation instructions.

Table 9: Descriptive Statistics for Study 5 Manipulation Check Questions on the Stroop Task

Variable	Do-Your-Best Conditions				Specific-Difficult Goal Conditions			
	Low-Conservation Condition		High-Conservation Condition		Low-Conservation Condition		High-Conservation Condition	
	M	(*SD*)	*M*	(*SD*)	*M*	(*SD*)	*M*	(*SD*)
Effort Invested [a]	87.35	18.85	89.24	15.80	78.25	25.02	81.11	31.70
Task Difficulty	56.04[b]	28.33	42.34	24.57	42.61	28.58	52.78	30.28
How hard did you work?	63.23	28.67	54.21	26.93	47.96	31.18	57.78	30.99
How much effort did you give?	69.35	28.45	69.52	25.69	56.79	32.94	67.74	31.85
Self-Control	71.96[cd]	27.81	74.69	23.06	52.36	30.39	69.96	25.78
Fight Against an Urge	54.73	33.18	62.62	27.29	46.11	32.10	60.81	30.07
Typing to Stroop Effort Comparison	1.69	0.97	1.66	1.01	1.89	1.17	1.74	1.13
Conserving Energy	16.85	24.37	13.52	20.95	18.07	22.97	11.30	19.88
Thinking About Next Task [a]	14.35	25.33	7.79	13.68	16.18	21.91	12.07	20.69

Note. Lower averages on the, “Typing to Stroop effort Comparison” mean participants rated their effort on the typing task higher compared to their effort on the Stroop task.

[a] Levene’s test of equality of error variances was significant ($p < .05$) for this question. [b] Indicates a significant Goal Type X Conservation interaction ($p < .05$). [c] Indicates a significant main effect of goal type ($p < .05$). [d] Indicates a significant main effect of conservation ($p < .05$).

Table 10 displays means and standard deviations for manipulation check questions administered following the anagram task. I predicted that participants in the specific-difficult-goal conditions would rate effort-type questions higher and rate desire-to-quit-type questions lower than participants in the do-your-best conditions. I also predicted that participants in the high-conservation conditions would rate effort-type questions higher and rate desire-to-quit-type questions lower than participants in the low-conservation conditions. The results indicated that goal type and conservation interacted with several anagram self-report questions.

There was a significant interaction for the question, “What percentage of your total effort did you invest in the anagram task?” $F(1, 102) = 4.78$, $p = .03$, partial $\eta^2 = .05$. There was a near significant interaction for the question, “How hard did you have to work at the anagram task?” $F(1, 102) = 3.28$, $p = .073$, partial $\eta^2 = .03$. The pattern of means indicated that for the specific-difficult-goal conditions, participants in the high-conservation condition rated their effort/work higher than participants in the low-conservation condition (d = 0.60/0.34), whereas the opposite held for the do-your-best conditions (d = 0.36/0.38).

There were significant interactions for the following desire-to-quit-type items: How long did you think about quitting the anagram task before you actually did? $F(1, 102) = 4.26$, $p = .042$, partial $\eta^2 = .04$; and How much did you force yourself to work on the anagram task as long as you did? $F(1, 102) = 6.81$, $p = .01$, partial $\eta^2 = .06$. There was a near significant interaction on the question, “Did you force yourself to keep working on the anagram task after you first thought about quitting?” $F(1, 102) = 3.60$, p

=.06, partial η^2 = .03. The pattern of means indicated that for the specific-difficult-goal conditions, high-conservation participants rated these items lower than low-conservation participants (*d* = 0.55, 0.70, and 0.40 for the, “Think to quit,” “Force to work as long as you did,” and, “Force to work after thinking about quitting questions,” respectively). The opposite pattern held for the do-your-best conditions (*d* = 0.26, 0.31, and 0.35, respectively). All other analyses on the anagram manipulation check questions (i.e., task difficulty, effort given, self-control, fight an urge, and desire to stop) were non-significant (*p*s > .09).

Table 10 also displays means and standard deviations for manipulation check questions administered in the final study questionnaire. These questions assessed how the anagram task instructions affected participants’ motivation and effort. I predicted participants in the specific-difficult-goal conditions would rate these questions higher than participants in the do-your-best conditions. This was the case: participants in the specific-difficult-goal conditions indicated the instructions/goal on the anagram task motivated them to perform better, $F(1, 102) = 26.33$, $p < .001$, partial η^2 = .21, *d* = 1.02, increase their effort, $F(1, 102) = 26.88$, $p < .001$, partial η^2 = .21, *d* = 1.03, and work harder, $F(1, 102) = 19.56$, $p < .001$, partial η^2 = .16, *d* = 0.88.

Participants compared their effort on the anagram task to their effort on the Stroop task (ratings could range from 1 to 5, with 1 meaning more effort on the Stroop task and 5 meaning more effort on the anagram task). Participants generally indicated they put slightly more effort into the anagram task, overall *M* = 3.93, *SD* = 1.31; there were no significant differences across conditions (*p*s > .76).

Table 10: Descriptive Statistics for Study 5 Manipulation Check Questions on the Anagram Task

Variable	Do-Your-Best Conditions: Low-Conservation Condition *M*	*(SD)*	Do-Your-Best Conditions: High-Conservation Condition *M*	*(SD)*	Specific-Difficult-Goal Conditions: Low-Conservation Condition *M*	*(SD)*	Specific-Difficult-Goal Conditions: High-Conservation Condition *M*	*(SD)*
Effort Invested [a]	86.77[b]	19.49	78.33	27.93	84.85	17.21	93.22	10.29
Task Difficulty	88.62	11.10	81.67	19.52	80.88	17.35	82.07	21.15
How hard did you work?	90.73	9.80	85.11	18.86	81.73	16.49	87.11	15.81
How much effort did you give?	89.62	11.53	86.00	17.03	84.46	11.29	89.33	15.83
Self-Control	55.35	37.30	58.56	36.37	48.77	30.88	45.89	34.59
Fight Against an Urge	31.81	34.65	42.22	35.44	30.31	32.56	32.37	30.14
How much did you want to stop working?	67.54	37.64	63.96	35.50	68.88	31.13	48.85	38.38
Did you keep working after you thought about quitting? [a]	63.38	35.17	74.30	27.90	78.42	26.58	65.00	40.28
How long did you think about quitting before you did? [a]	50.08[b]	35.03	57.78	25.47	61.85	27.81	44.15	36.97
How much did you force yourself to keep working? [a]	60.38[b]	31.81	68.70	22.98	72.62	24.62	50.22	38.93
Did goal motivate you to perform better?	38.84[c]	32.58	34.89	30.96	63.73	26.17	68.46	27.27
Did goal increase your effort?	37.56[c]	30.41	34.26	28.12	65.27	26.75	64.93	30.36
Did goal influence you to work harder?	37.08[c]	32.27	39.78	29.47	60.23	30.17	68.82	29.57
Stroop to Anagram Effort Comparison	4.00	1.20	3.93	1.33	3.85	1.43	3.93	1.51

Note. Higher averages on the, “Stroop to Anagram effort Comparison” mean participants rated their effort on the anagram task higher compared to their effort on the Stroop task.

[a] Levene’s test of equality of error variances was significant (p < .05) for this question. [b] Indicates a significant Goal Type X Conservation interaction (p < .05). [c] Indicates a significant main effect of goal type (p < .05).

Table 11 displays means and standard deviations for the BMIS scales, vocabulary performance, and reported ACT scores. There was a near significant effect of goal type on ACT scores; participants in the specific-difficult-goal conditions reported higher ACT scores than participants in the do-your-best conditions, $F(1, 97) = 3.84$, $p = .053$, partial $\eta^2 = .04$, $d = 0.41$. Otherwise, no other effects or interactions were significant (ps > .10).

Table 11: Descriptive Statistics for Study 5 BMIS, Vocabulary, and ACT Variables

	Do-Your-Best Conditions				Specific-Difficult-Goal Conditions			
	Low-Conservation Condition		High-Conservation Condition		Low-Conservation Condition		High-Conservation Condition	
Variable	*M*	*(SD)*	*M*	*(SD)*	*M*	*(SD)*	*M*	*(SD)*
BMIS								
Pleasant-Unpleasant Scale	5.77	6.29	4.14	6.79	4.36	6.09	4.59	6.36
Arousal-Calm Scale	14.65	3.74	16.28	3.59	16.00	3.77	15.52	3.99
ACT	25.00[a]	3.24	24.81	3.64	26.15	3.11	26.46	4.07
Vocabulary	9.77	2.44	9.71	2.64	10.25	2.03	10.79	2.88

[a] The main effect of goal type for this item was p = .053.

Conservation

I performed a 2 (Conservation: high, low) X 2 (Goal Type: do-your-best, specific-difficult) ANOVA on Stroop task reaction time and accuracy. I used two indicators of reaction time. The first was the average reaction time for correct trials only. The second was the average reaction time for correct trials that were under two seconds. I used this measure to correspond with Muraven et al. (2006); in their fourth study, Stroop trials were removed from analyses if they lasted more than two seconds. Table 12 displays means and standard deviations for Stroop performance indices. I expected to find a main effect of conservation, such that participants in the high-conservation conditions performed worse in terms of reaction time (i.e., slower reaction times) and/or accuracy (i.e., fewer correct trials). Since the goal manipulation had not taken place at this point, I did not expect to find differences across the goal conditions or to find any interactions. Contrary to my predictions, there were no significant effects on the performance indices (ps > .48).

Table 12: Descriptive Statistics for Study 5 Stroop Task Performance

	Do-Your-Best Conditions				Specific-Difficult-Goal Conditions			
	Low-Conservation Condition		High-Conservation Condition		Low-Conservation Condition		High-Conservation Condition	
Variable	*M*	*(SD)*	*M*	*(SD)*	*M*	*(SD)*	*M*	*(SD)*
Accuracy	.97	.03	.97	.03	.97	.03	.98	.02
RT (correct trials only)	774.68	221.49	748.53	146.46	796.84	200.12	752.47	159.92
RT (correct and under 2 seconds)	733.10	172.42	729.76	135.13	759.07	149.36	728.94	142.74

Note. Reaction times are in milliseconds.

Anagram Persistence and Number Correct

I conducted a 2 (Conservation: high, low) X 2 (Goal Type: do-your-best, specific-difficult) ANOVA on anagram persistence and number correct. Table 13 displays means and standard deviations for anagram performance. I expected to find a main effect of goal type, such that participants in the specific-difficult-goal conditions persisted longer and solved more anagrams correctly than participants in the do-your-best conditions. I also expected a main effect of conservation such that the high-conservation conditions would outperform the low-conservation conditions.

The results indicated a significant goal type effect on persistence, $F(1, 102) = 4.57$, $p = .04$, partial $\eta^2 = .04$, $d = 0.42$. Participants in the specific-difficult-goal conditions persisted longer than participants in the do-your-best conditions. While the effect of goal type on number correct demonstrated the same pattern, it did not reach conventional significance, $F(1, 104) = 2.97$, $p = .088$, partial $\eta^2 = .03$, $d = 0.34$. There was no main effect of conservation or an interaction on persistence (*p*s > .19) or number correct (*p*s > .21).

Because there was a near significant main effect of goal type on ACT scores, I performed the ANOVAS again, this time controlling for ACT scores. Some participants did not report their ACT scores so these analyses was performed on a subset of the whole sample. When the dependent variable was persistence, the main effect of goal type was marginally significant, $F(1, 91) = 3.85$, $p = .053$, partial $\eta^2 = .04$, $d = 0.48$. The main effect of conservation remained non-significant ($p = .65$), but the interaction became marginally significant, $F(1, 91) = 3.09$, $p = .082$, partial $\eta^2 = .03$. The pattern of means indicated that for the specific-difficult-goal conditions, participants in the high-conservation condition persisted longer than participants in the low-conservation condition (d = 0.47), whereas the opposite held for the do-your-best conditions (d = 0.28).

When the dependent variable was number correct, the main effect of goal type was not significant (p = .14). The interaction remained non-significant (p = .17), but the main effect of conservation became marginally significant, $F(1, 93) = 3.47$, p = .066, partial η^2 = .04, d = 0.37. Participants in the high-conservation conditions solved more anagrams correctly than participants in the low-conservation conditions.

Table 13: Descriptive Statistics for Study 5 Anagram Performance

Variable	Do-Your-Best Conditions				Specific-Difficult-Goal Conditions			
	Low-Conservation Condition		High-Conservation Condition		Low-Conservation Condition		High-Conservation Condition	
	M	(*SD*)	*M*	(*SD*)	*M*	(*SD*)	*M*	(*SD*)
Persistence (in minutes)	19.14[a]	9.04	18.12	7.42	20.46	8.26	23.65	8.21
Number Correct	31.12	19.00	30.89	17.65	32.67	15.04	40.75	16.99

[a] Indicates a significant main effect of goal type ($p < .05$).

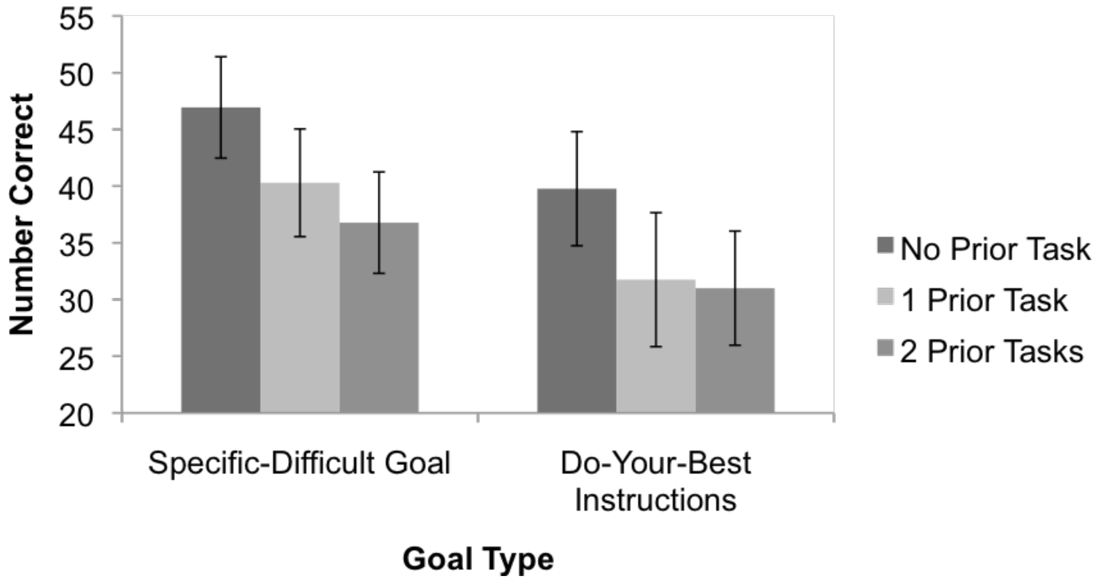
Anagram Performance Comparison

According to the resource account, people become increasingly depleted as they continuously regulate. This predicts that performance should progressively worsen as the number of prior tasks increases, regardless of motivation. According to the motivation account, people become less and less willing to exert effort so performance should generally decline as the number of prior tasks increases. However, motivation should mitigate this decline.

I performed a 3 (Prior Task: none, 1, 2) X 2 (Goal Type: do-your-best, specific-difficult) ANOVA on anagram task performance. I combined the pilot data from Study 3 (no prior task), the data from Study 3 (1 prior task), and the data from Study 4 (2 prior tasks) to perform this analysis. This analysis allowed me to see how performance on the same set of anagrams changed depending on how many tasks had been completed beforehand and what type of instructions were given for the task.

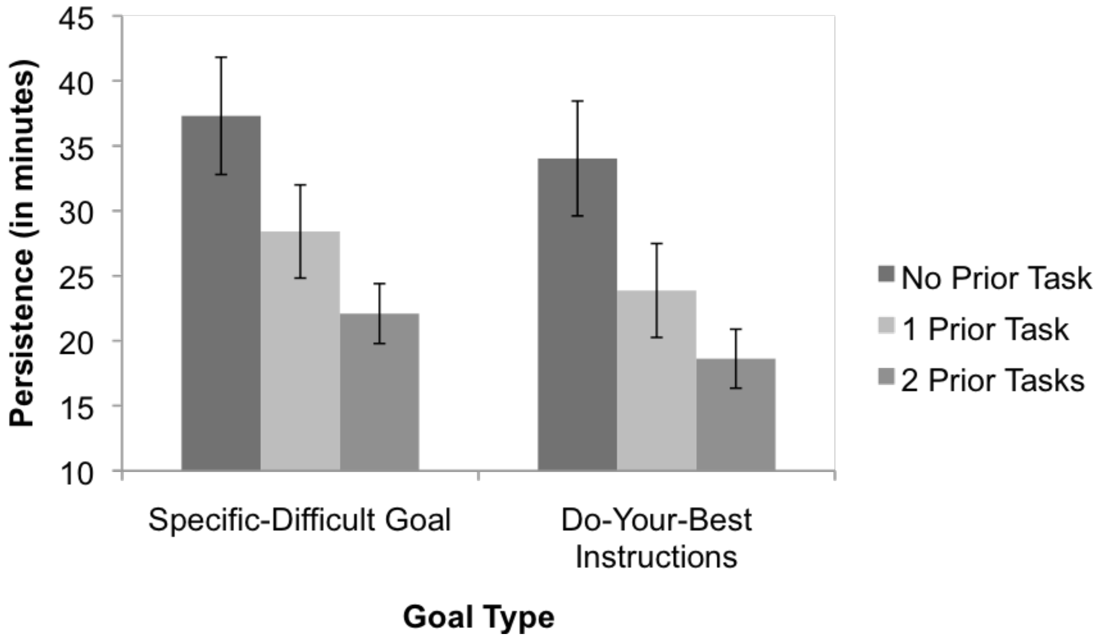
When the dependent variable was number correct, there were main effects of goal type, $F(1, 234) = 11.59$, $p = .001$, partial $\eta^2 = .05$, and prior task, $F(2, 234) = 7.20$, $p = .001$, partial $\eta^2 = .06$ (See Figure 1). Regarding the main effect of goal type, participants in specific-difficult-goal conditions solved more anagrams correctly than participants in do-your-best conditions ($d = .43$). Regarding the main effect of prior task, Tukey post hoc tests indicated participants who had no prior task solved more anagrams correctly than participants who had one prior task ($p = .02$) or two prior tasks ($p = .001$). Performance did not differ between participants who had one or two prior tasks ($p = .70$). The interaction was not significant ($p = .85$).

Figure 1: Number correct on the anagram task by goal type and prior task.



When the dependent variable was persistence, there were main effects of goal type, $F(1, 232) = 7.86$, $p = .005$, partial $\eta^2 = .03$, and prior task, $F(2, 232) = 45.16$, $p < .001$, partial $\eta^2 = .28$ (See Figure 2). Regarding the main effect of goal type, participants in specific-difficult-goal conditions persisted longer than participants in do-your-best conditions ($d = .32$). Regarding the main effect of prior task, Tukey post hoc tests indicated participants who had no prior task persisted longer than participants who had one prior task ($p < .001$) or two prior tasks ($p < .001$). Participants who had one prior task persisted longer than participants who had two prior tasks ($p = .001$). The interaction was not significant ($p = .92$). In both of these analyses, Levene's test for unequal variances was significant.

Figure 2: Persistence on the anagram task by goal type and prior task.



Discussion

Participants rated the typing task similarly in terms of how difficult the task was, how much effort they invested in the task, and how hard they worked on the task. This suggests that participants were objectively depleted to the same degree before the second task. On the Stroop task, participants in the high-conservation conditions rated self-control requirements and the need to fight against an urge higher than participants in low-conservation conditions. This corroborates the greater emphasis on suppression in the task instructions for high-conservation participants. However, high-conservation participants did not indicate that they conserved more or thought about the next task more than low-conservation participants. Nor did high-conservation participants indicate that they withheld effort compared to low-conservation participants. Several manipulation checks on the Stroop task had main effects of goal type or an interaction between goal type and conservation. These effects are not interpretable since the goal manipulation had not taken place.

Furthermore, analyses of performance measures on the Stroop task did not reveal any effects of conservation. There was no evidence that anticipating a future task led participants to conserve resources, and thus perform worse on the Stroop task than participants who had no anticipation of a future task. These results contradicted my predictions. I ran two other versions of this study where participants completed a typing task, a timed anagram task, and then another typing task. I found no evidence for conservation on the second task in either of these cases.

Hagger et al. (2010) reported an averaged corrected standardized effect size of $d^+ = 1.04$ for the conservation effect. Although this effect was quite large, the conservation effect is a relatively new concept in depletion literature. Hagger et al. (2010) only used seven studies from two research articles to meta-analyze the effect. Even though my study was designed as a partial replication of Muraven et al.'s fourth study (2006), I was unable to replicate the conservation effect. Since conservation is a newer aspect of depletion research, known methods of producing the effect are relatively limited and require further investigation.

It is possible that the conservation manipulation in this study was not strong enough to produce the effect. When I conducted Studies 2-4, a steady flow of students signed up for participation; during Study 5, the number of students available in the subject pool was drastically lower and they signed up for participation sporadically. This may have affected the results. In Study 5, participants were primarily guided through the study by computer instructions, whereas in Studies 2-4, participants were primarily guided through the study by the experimenter. This change in procedure may have affected the results. However, I found no conservation in the two other versions of this study I conducted, in which the experimenter guided participants through the study.

It is also possible that individual differences affect whether conservation occurs or not. For example, individuals may differ in how much effort they are normally willing to give and how consistently they exert effort. As another example, individuals may differ in how much they enjoy doing anagrams or other cognitive tasks.

In the future, a very direct manipulation of conservation may be necessary. For example, participants may need explicit instructions telling them that they would be more efficient on a future regulatory task if they conserve effort on the current task. Other participants would not be told about the future task. Then, goal instructions could be manipulated to see if participants who did not conserve can perform as well or better than participants who were explicitly told to conserve.

Concerning the anagram manipulation check items, I had predicted that high-conservation participants would rate effort-type questions higher and desire-to-quit-type items lower than low-conservation participants. If participants had conserved more on the Stroop task, they should have been more willing to continue exerting effort on the anagram task. This pattern of results appeared in the specific-difficult-goal conditions, but was the opposite in the do-your-best conditions.

Participants in the specific-difficult-goal conditions tended to report higher ACT scores than participants in the do-your-best conditions. Even though this effect did not reach the conventional level of significance, I examined anagram performance in two ways: controlling for ACT scores or not. When I did not control for ACT scores, there was a significant main effect of goal type. Consistent with my predictions, participants given a specific, difficult goal persisted longer on the anagrams than participants given do-your-best instructions. The effect of goal type on correct anagram solutions did not reach significance.

When I controlled for ACT scores, the results changed somewhat. For persistence, the main effect of goal type was qualified by the interaction. The results followed the same pattern as the manipulation check items for the anagram task. When high-conservation participants received a specific, difficult goal, they persisted longer than low-conservation participants. When high-conservation participants received do-your-best instructions, they persisted less than low-conservation conditions. For number correct, the main effect of goal type went away and a small effect of conservation emerged. High-conservation participants solved more anagrams correctly than low-conservation participants.

These results suggest that participants generally persisted longer and solved more anagrams if they had a specific, difficult goal. However, this effect may depend on conservation; high-conservation participants performed better when they had a specific-difficult goal. This may suggest that conservation promotes greater effort exertion on subsequent tasks if motivation is high, but promotes continued conservation if motivation is not as high. This pattern is similar to learned industriousness where people continue to exert high effort when it is rewarded. These interpretations are speculative, however, because I did not actually observe conservation on the Stroop task.

The absence of a conservation effect implies that participants were doubly depleted going into the third task. In other words, because performance did not differ across conditions on the first or second task, all participants regulated to the same degree and went into the third task depleted to the same degree. If depletion truly hinders the ability to regulate, performing well after two tasks should be more difficult than after one task. Muraven et al. (2006) found participants who had not conserved on a second task performed worse on a third task. I found that even when participants had presumably not conserved on a second task, I could manipulate some participants to perform better by giving them a goal. This suggests that when participants are doubly depleted, they can continue to exert effort and improve performance.

In an additional set of analyses, I compared participants who presumably performed the anagram task at full capacity (i.e., Study 3 pilot conditions), participants who performed the anagram task depleted (i.e., Study 3 conditions), and participants who performed the anagram task doubly depleted (i.e., Study 5 conditions). Participants who had no prior task persisted the longest and solved more anagrams correctly. Persistence then decreased as the number of prior tasks increased. However, participants who were depleted and doubly depleted solved the same number of anagrams, suggesting that the ability to perform was not exhausted by prior regulatory experience. Additionally, the specific, difficult goal led to better performance across the board. Overall, despite null results for conservation, this study provides evidence that even after two demanding regulatory tasks, participants are able to regulate, particularly when they are more motivated.

CHAPTER 7: GENERAL DISCUSSION

When individuals engage in self-regulation, regulatory performance subsequently suffers. Known as ego-depletion, or the depletion effect, this phenomenon has been demonstrated in many studies. Hagger et al.'s (2010) recent meta-analysis provided further evidence that the depletion effect is medium to large in magnitude. Nevertheless, the phenomenon is not fully understood.

The depletion effect has typically been explained in terms of a resource account. The primary assumptions of the resource depletion model are that self-regulation relies on a domain-general resource, that the resource is limited, and that the resource is expendable (Muraven & Baumeister, 2000). Proponents of the resource account have attributed the depletion effect to an inability to regulate because of diminished resources, or as an attempt to conserve what resources remain. Proponents of the resource account have also argued that the depletion effect will only occur after a task that involves self-regulation.

However, I identified a potential confound in previous studies of the depletion effect. The majority of depletion studies have one initial task that involves self-regulation and one initial task that does not. Individuals rate the task that involves self-regulation as more difficult and more effortful than the task that does not involve self-regulation. Thus, the depletion effect may arise from differences in self-regulation or from differences in task difficulty and effort exertion. In support of the impact of effort, my meta-analysis suggested there is a small relationship between effort ratings on the first task and the depletion effect on the second task. Additionally, I found a depletion-like effect after two initial tasks that presumably did not involve self-regulation. This evidence suggests that motivated allocation of effort poses an alternative account of the depletion effect.

A motivation account derives from the law of least effort and from the notions that people are cognitive misers and motivated tacticians. While people generally resort to low-effort strategies, effort exertion is adaptive. People will increase or decrease their effort depending on many factors. In my studies, I found that participants increased their effort if they were given specific, difficult goals. I also found that participants decreased their effort if they believed that they had already exerted more effort than others. These performance shifts occurred even when participants had completed the same depleting task. Thus, motivation manipulations had a greater influence on performance than "objective" levels of depletion.

Recent findings suggest that the motivation to conserve influences the depletion effect (Muraven et al., 2006; Tyler & Burns, 2009). That is, individuals perform worse or behave in a depleted manner as a way of preserving their diminishing resource. However, if conservation ties to motivated effort allocation rather than resource preservation, then individuals should be capable of performing well even when they have not conserved, so long as they are motivated. In Study 5, I investigated whether people conserve a resource or conserve effort. While I was unable to detect a conservation effect, I did find that motivation manipulations continued to affect performance, even after two depleting tasks.

Theoretical Implications

The findings from my research are consistent with a motivation account of the depletion effect and suggest that effort and the motivation to exert effort have a large impact on self-regulatory performance. However, as Hagger et al. (2010) pointed out in their meta-analysis, the motivation and resource account may not be mutually exclusive. If this is the case, depletion may occur as the resource account explains, but motivation may moderate this effect. This would suggest that motivation would not always overcome depletion. In other words, people could become depleted to the point that no amount of motivation would improve performance. My research showed that motivation improved performance even after two prior tasks. Further research will have to examine whether this improvement continues beyond two prior tasks.

Nevertheless, my findings showed that performance generally declined as the number of prior tasks increased. Hence, instead of being exclusive, the two accounts may be describing the same resource, albeit differently. According to the resource account, the resource is self-regulatory; by contrast, the resource may be effort. In other words, performance may decline across tasks because of people's unwillingness to continue using their *effort* resources, rather than people's inability to continue using their *regulatory* resources. The key distinctions are that resource account assumes the resource is readily expendable and regulatory specific. The motivation account assumes that the motivation to use effort resources decreases after any effortful behavior. Additionally, the ability to exert effort does not decline; rather, the choice to exert effort changes over time. The resource depletion model focuses on the availability of a resource, whereas the motivation account focuses on decisions about using a resource.

In this sense, depletion effects become the result of dynamic decision-making processes over time. This perspective is not new, but draws on an evolving set of regulatory theories. Many control theories of self-regulation hypothesize that people make decisions to reduce the discrepancy between a current state and a desired goal state; these decisions determine regulatory behavior, effort allocation, and strategies (e.g., Carver & Scheier, 1998). These decisions are thought to rely on economics principles such as expected utility, or combining the value of an outcome with the likelihood of it occurring. One criticism of these theories is that parameters, such as the valence of an outcome, are modeled as static.

In recent years, Vancouver, Weinhardt, and Schmidt (2010) have begun modeling self-regulation by incorporating control theories, decision-making theories, and the theory of multiple-goal pursuit. This theory postulates that people are continuously allocating resources between competing goals over time. Goals compete due to both physical and psychological limitations; when a person chooses to pursue one goal, it inevitably means the individual cannot pursue other goals. This model allows parameters to be dynamic, to parallel how goal priorities, effort toward goals, and so on change over time.

When self-regulation is viewed in terms of dynamic decisions between multiple goals over time, depletion effects become a piece of a larger puzzle. For example, as a participant enters a depletion study, both proximal and distal goals may affect motivation and effort allocation on the administered tasks. Proximally, he or she may have certain goals about the study itself, such as wanting to perform well on the presented tasks. Distally, he or she may have certain goals for the day, such as preparing for an upcoming test. These two goals compete; the participant cannot study for the exam while completing the study. Thus, effort allocation within the study becomes a dynamic decision between multiple goals. In my studies in particular, competing goals create a situation where the participant is constantly deciding whether to quit or keep working on the anagrams.

The concepts of the cognitive miser and motivated tactician easily fit within a dynamic decision-making framework. With such a complex system of goal pursuit, efficiency depends on adaptively adjusting effort according to goal priorities and other factors. Models such as Vancouver et al.'s (2010), that seek to integrate many areas of research, may be the key to understanding self-regulatory behaviors generally, and the depletion effect specifically. However, understanding the role of goal pursuit and effort allocation in the depletion effect will also involve identifying task factors and individual differences that affect motivation.

For example, future research is needed to fully understand the time course of depletion effects. Several components may play a factor such as the length of each task, the length of time between tasks, and the participants' overall expectations of time in the study. The length of initial tasks in most depletion studies is as little as five minutes and typically no more than ten minutes (e.g., Muraven et al., 1998). This means the depletion effect occurs after a relatively short period. If willingness to exert effort or the availability of a resource decrease with time, then depletion effects should be greater after longer initial tasks, but this type of manipulation has rarely been examined. Some research has shown that the length of time between tasks is important and that longer breaks lead to better performance on a second task (e.g., Tyer & Burns, 2008). Expectations also seem to impact regulatory performance; participants performed better when they thought the study was almost over compared to when they expected to continue for some time (e.g., Tyler & Burns, 2009). In my studies, participants were told how long the study would last (e.g., one hour). Some participants may have persisted longer because they knew they would have to remain in the study anyway. Others may have disregarded this information and persisted less with the hopes of leaving the study early.

If time expectations affect regulatory performance, other exogenous factors may influence depletion effects, such as task complexity or task importance. Additionally, factors such as the reward structure within tasks may influence whether motivation increases or decreases across tasks (e.g., Eisenberger, 1992). In my studies, I used several sets of anagrams that differed in terms of solvability and difficulty. These task properties may differentially affect participants' persistence. For example, when participants encounter unsolvable anagrams, they may decide more quickly that effort is futile, and persist less.

Regarding individual differences, many factors may influence regulatory behavior and depletion effects, ranging from personality differences, to cognitive ability differences, to personal preferences, and to life experience. Recent research on regulatory focus, or whether individuals are oriented towards promoting gains or preventing losses, suggested that individuals' motivation to perform is influenced by whether their preferred orientation fits their approach on a current task or goal (e.g., Avnet & Higgins, 2003; Kruglanski et al., 2000). Wan and Sternthal (2008) found that individuals who were more likely to monitor their behavior against a standard were less likely to exhibit depletion effects. Job, Dweck, and Walton (2010) have shown that individuals' beliefs about self-regulation impact their regulatory behavior such that those who believe self-regulation is limited are more likely to behave in a depleted manner than those who believe self-regulation is not limited.

Other individual difference factors that may influence the depletion effect and the motivation to exert effort include work ethic, stress levels, and sleep patterns. According to the resource account, training reduces depletion effects (e.g., Muraven et al., 2010). This training may reflect learned industriousness, or developing the skill to continue exerting effort over time (e.g., Eisenberger, 1992). Individuals may differ in how much self-regulatory training they engage in or how responsive they are to learned industriousness. In my studies, some individuals may have persisted longer because they have trained themselves to continue exerting effort for longer periods of time, even when the desire to quit is strong.

Relating to physiological factors, Hagger et al. (2010) suggested that there are significant differences in ratings of fatigue between control and depletion conditions. In my meta-analysis, I did not find a relationship between self-reported fatigue and the depletion effect, suggesting that any differences were subjective. However, after continued exertion, there may be a point for individuals when true physical fatigue affects regulatory performance.

Further Issues, Limitations, and Future Directions

The present research focused on a limited number of tasks and motivation manipulations. This was beneficial in one sense because it allowed me to determine if the effects of motivation would occur consistently across the narrow set of tasks. However, in order to determine whether the impact of motivation generalizes to the depletion effect at large, future studies should use additional tasks and manipulate motivation and effort in new ways. Future motivation manipulations could include more than two levels in order to investigate whether the motivation to exert effort has a linear relationship with performance. It would also be interesting to see whether individuals are able to estimate how much effort they are willing to give and whether this changes over time. This becomes a question of meta-cognition: how aware are individuals about the dynamic decisions they make to allocate effort among goals?

The current research is also limited because it did not include a direct test of the resource account against the motivation account and did not replicate the typical depletion effect. One step towards distinguishing whether these two accounts are exclusive or work together would be to cross regulation and motivation in a subsequent study. On the first task, participants would complete a regulatory or non-regulatory task. Then, the second task would be the same for all participants, but a motivation manipulation would encourage participants to exert more or less effort. Perceived effort feedback or goal instructions could achieve this kind of manipulation.

This type of study would allow an assessment of how the motivation to exert effort affects performance after a regulatory and non-regulatory initial task. If regulation is critical, as the resource account suggests, performance should be worse after the initial regulatory task compared to the initial non-regulatory task. If motivation plays a role in performance, participants manipulated to exert more effort should outperform participants manipulated to exert less effort. If both motivation and a resource affect performance, there should be an interaction. Participants who performed a regulatory task first should not perform as well as participants who performed a non-regulatory task first, even when they are more motivated. This finding would suggest that motivation was unable to overcome decrements in self-regulatory ability.

Manipulating regulation on a second task, in addition to the manipulations described above, would also contribute to the distinction between a resource and motivation account. According to the resource account, the depletion effect should not occur if the second task does not involve self-regulation. According to the motivation account, the depletion effect should occur regardless of whether the task is regulatory or non-regulatory, so long as both the first and second task are effortful and demanding. With these types of manipulations, researchers can begin to distinguish the role of self-regulatory behavior and effortful behavior in depletion effects.

In the development of a direct test of the resource and motivation account, researchers must address several issues. First, what is the theoretical distinction between a regulatory and non-regulatory task? Because the resource account does not clearly define the nature of the resource, this issue is challenging. The most consistent definition of self-regulation in depletion studies is the ability to override responses, or inhibition (see Muraven et al., 1998). This may be the starting point to distinguish regulatory from non-regulatory tasks. Second, the regulatory and non-regulatory tasks must be equally difficult and effortful. This would ensure that differences in performance are due to inhibitory requirements between tasks and not a confound between effort and regulation. Addressing these issues would involve extensive pilot testing. However, if self-regulation is operationally defined as dynamic decision making amongst concurrent goals, then distinguishing between regulatory and non-regulatory tasks may not be possible.

Regardless of whether the resource and motivation account are mutually exclusive, future research is needed to investigate what mechanism underlies depletion effects. Even with dynamic decision-making processes, task factors, and individual differences affecting self-regulation and the depletion effect, is there a specific cognitive mechanism that underlies this phenomenon? While this question has not been thoroughly examined, some researchers have begun to investigate what specific regulatory process might affect depletion.

Specifically, this research links changes in monitoring processes to depletion. Lorist, Boksem, and Ridderinkhof (2005) examined continuous task performance on a flanker task, in which participants identify a central stimulus when flanking stimuli match or mismatch. Post-error slowing, or the phenomenon that people take longer to respond on trials following an error, decreased as time on task increased. Event-related negativity (ERN), which is associated with performance monitoring in the anterior cingulate cortex, also decreased with time-on-task. Boksem et al. (2006) replicated this finding of reduced post-error slowing on a different task. Several event-related potential components also indicated that with increasing time-on-task, participants were less capable of recognizing and reacting to response conflict.

Inzlicht and Gutsell (2007) also hypothesized that depletion may weaken performance monitoring. To test this hypothesis, they examined ERN in the Stroop task after control and depletion conditions. ERN data indicated that the control condition had greater negativity on incorrect trials than correct trials; there was no difference in negativity on incorrect and correct trials for the depletion condition. This suggested that the conflict monitoring system might have been inefficient in the depletion condition compared to the control condition, or may have been incapable of detecting a discrepancy between intentions and actions.

These studies have examined the depletion effect from a mechanism perspective, with the aim of determining what cognitive processes or functions link directly to the depletion effect. If the resource account and motivation account are not exclusive, this research may be the bridge between the two accounts. The ability to regulate may decline over time, but the mechanism that breaks down may tie to effort allocation. That is, a key component of decision-making, or recognizing discrepancies between current states and goal states, may break down, thus making it difficult for individuals to know where to allocate resources or effort.

While these studies suggest that monitoring processes deteriorate over time, motivation may still have a significant impact on performance. For example, Wan and Sternthal (2008) eliminated conflict monitoring deficits associated with the depletion effect simply by encouraging participants to monitor their performance against a standard. Thus, participants may be able to overcome presumed deficits in cognitive processing, so long as there is motivation to do so.

Conclusion

While my research cannot completely rule out the resource account, it ultimately demonstrates that researchers need to expand their thinking about the potential causes and theoretical underpinnings of depletion effects. Furthermore, my research shows that the ability to self-regulate may not be as limited as the resource account implies. Rather, the motivation to exert effort has a great impact on regulatory behavior in depletion paradigms.

Researchers should continue to explore mechanistic explanations of the depletion effect and integrate this with research concerning the effects of motivation and effort allocation on self-regulatory performance. Additionally, researchers should explore how the depletion effect, cognitive miserliness, and the motivated tactician concept fit into regulatory goal theories (e.g., see Vancouver et al., 2010). As individuals regulate, they must pursue multiple goals simultaneously, and effort allocation between these goals is likely a complex system involving past experience, current contexts, and future expectations. Ultimately, integrating different avenues of research will increase understanding of depletion effects and will broaden scientific understanding of self-regulation.

APPENDICES

APPENDIX A

META-ANALYSIS DETAILS

Table A1: Condition *n*s and Sample Weights for the Focused Meta-Analysis

Author	Study	Depletion Effect		Self-Report Rating Effect		Type of Rating Provided	WLS Sample Weight
		n_c	n_d	n_c	n_d		
Alberts, Martijn, Greb, Merkelbach, & Vries (2007)	2	20	20	40	40	F	40
Baumeister Bratslavsky, Muraven, & Tice (1998)	1	23	23	23	23	D, E, F	46
	3	15	15	15	15	D, E	40
	4	41	41	41	41	E, F	82
Bray, Ginis, Hicks, & Woodgate (2008)	1	23	26	23	26	E, F	49
Burkley (2008)	3	39	39	39	39	E	78
	4	30	30	30	30	E	60
Ciarocco, Sommer, & Baumeister (2001)	1	18	19	18	19	D	37
	2	12	12	12	12	D	24
DeWall, Baumeister, Stillman, & Gailliot (2007)	2	13	14	26	27	D	27
	4	48	49	48	49	D	97
Fennis, Janssen, & Bohs (2009)	3	19	18	19	18	E	37
Finkel & Campbell (2001)	2	23	23	23	23	E	46
Finkel et al. (2006)	1	13	13	13	13	F	26
	2	14	15	14	15	F	29
	3	23	23	23	23	F	46
	4	16	16	16	16	F	32
Friese, Hofmann, & Wanke (2008)	2	33	33	33	33	E, F	66
	3	22	23	22	23	E, F	45
Gailliot & Baumeister (2007)	1	16	16	16	16	D	32
	2	13	14	13	14	D	27
Gailliot, Schmeichel, & Baumeister (2006)	2	9	10	9	10	D	19
Govorun & Payne (2006)		36	36	36	36	D, F	72
Hofmann, Rauch, & Gawronski (2007)		24	26	24	26	D	50

Table A1 (cont'd)

Joireman, Balliet, Sprott, Spangenberg, & Schultz (2008)	3	49	50	49	50	D	99
Legault, Green-Demers, & Eadie (2009)	2	34	34	67	67	F	68
Martijn et al. (2007)		18	19	18	19	F	37
Martijn, Tenbult, Merckelbach, Dreezens, & de Vries (2002)		16	17	16	17	D	33
Muraven & Slessareva (2003)	1	11	11	21	22	D, E	22
	2	21	20	41	41	D, E	41
	3	12	12	23	22	E	24
Muraven, Collins, & Nienhaus (2002)		29	29	29	29	D, E	58
Muraven, Shmeuli, & Burkley (2006)	1	23	23	23	23	E	46
	2	17	17	17	17	E	34
	4	19	19	19	19	E	38
Muraven, Tice, & Baumeister (1998)	1	20	20	20	20	E, F	40
	2	17	17	17	17	D	34
	3	23	26	23	26	D	49
Neshat-Doost, Dalgleish, & Golden (2008)		14	18	14	18	D, F	32
Oikawa (2005)	1	19	19	19	19	F	38
	2	20	20	20	20	F	40
Ostafin, Marlatt, & Greenwald (2008)		40	45	40	45	E	85
Park, Glaser, & Knowles (2008)		28	29	28	29	D, E	57
Pocheptsova, Amir, Dhar, & Baumeister (2009)	2	249	252	94	94	D	188
				95	95	F	190
Schmeichel (2007)	1S1	38	41	69	72	D	79
	1S2	31	31	69	72	D	62
	2S1	29	32	29	32	D	61
	3	30	15	30	15	D	45
	4	33	32	33	32	D	65
Schmeichel & Vohs (2009)	1	29	30	29	30	E	59
	2	12	12	36	36	D	24
Schmeichel, Demaree, Robinson, & Pu (2006)		25	25	25	25	D	50

Table A1 (cont'd)

Schmeichel, Vohs, & Baumeister (2003)	1	12	12	12	12	D	24
	2	18	19	18	19	D	37
	3	18	18	18	18	D	36
Segerstrom & Nes (2007)		20	21	41	42	E, F	41
Shamosh & Gray (2007)		28	29	28	29	E	57
Shmueli & Prochaska (2009)		48	49	50	50	D, E	97
Stewart, Wright, Hui, & Simmons (2009)		21	19	21	19	D, F	40
Stucke & Baumeister (2006)	3	22	23	22	23	E	45
Tyler (2008)	1	16	17	16	17	D	33
	2	15	15	15	15	D	30
	3	15	15	15	15	D, F	30
	4	30	30	30	30	D, F	60
Tyler & Burns (2008)	1	10	10	30	30	D	20
	2	10	10	20	20	D	20
Tyler & Burns (2009)	1	10	10	20	21	D	20
	2S1	10	10	30	30	D	20
	2S2	10	10	30	30	D	20
	3	20	20	40	40	D	40
Vohs & Faber (2007)	1	17	18	17	18	D	35
	2	34	34	34	34	D	68
	3	19	20	19	20	D	39
Vohs & Schmeichel (2003)	3	24	24	24	24	D	48
	4	23	24	24	25	D	47
Vohs, Baumeister, & Ciarocco (2005)	3	15	15	15	15	D	30
	4	30	30	30	30	D	60
	6	14	15	29	29	D	29
	7	16	16	35	36	D	32
	8	16	16	16	16	D	32
Wan & Sternthal (2008)	1	12	13	25	25	E	25
	2	13	14	40	41	E	27
	3	25	25	25	25	E	50
	4	21	21	41	42	E	42
Webb & Sheeran (2003)	1	15	16	15	16	D, E, F	31
	2	14	14	28	29	D, F	28
Wheeler, Brinol, & Hermann (2007)		17	17	64	64	D, E, F	34
Wright et al. (2007)	2S2	23	24	23	24	D	47
Wright, Martin, & Bland (2003)	S2	18	19	18	19	D, F	37

Table A1 (cont'd)

Wright, Stewart, & Barnett (2008)	S1	26	27	53	55	D	53

Note. n_c = sample size of the control condition used to compute the effect size. n_d = sample size of the depletion condition used to compute the effect size. WLS = weighted least squares analysis. D = difficulty rating. E = effort rating. F = fatigue rating.

APPENDIX B

MANIPULATION CHECK QUESTIONS

Study 2

Questions Administered Following the Math and Anagram Task:
How difficult was this task?
(1=Not Difficult at All; 100= Very, Very Difficult)
How hard did you have to work at this task?
(1= Not Hard at All; 100= Very, Very Hard)
How much effort did you have to invest to perform this task?
(1= Not Very Much Effort at All; 100= All of my Effort)

Studies 3, 4, & 5

Questions Administered Following the Typing, Stroop, and Anagram Task
What percentage of your total effort did you invest in the [typing/Stroop/anagram] task?
(1= 1% of my Effort; 100= 100% of my Effort)
How difficult was the [typing/Stroop/anagram] task?
(1=Not Difficult at All; 100= Very, Very Difficult)
How hard did you have to work at the [typing/Stroop/anagram] task?
(1= Not Hard at All; 100= Very, Very Hard)
How much effort did you have to give to perform the [typing/Stroop/anagram] task?
(1= Not Very Much Effort at All; 100= All of my Effort)

Questions Specific to the Anagram Task
How much did you want to stop working on the anagram task?
(1= Not Very Much at All; 100= Very, Very Much)
Did you force yourself to keep working on the anagram task after you first thought about quitting?
(1= Not at All; 100= To a Great Degree)
How long did you think about quitting the anagram task before you actually did?
(1= Not Very Much at All; 100= Very, Very Much)
How much did you force yourself to work on the anagram task as long as you did?
(1= Not Very Much at All; 100= Very, Very Much)

Study 3

Questions Regarding the Goal Manipulation (Given on study questionnaire)
How much did the goal you were given motivate you to perform better on the anagram task?
(1= Not Very Much at All; 100= Very, Very Much)

How much did the goal you were given increase your effort on the anagram task?
(1= Not Very Much at All; 100= Very, Very Much)
How much did the goal you were given influence you to persist longer on the anagram task?
(1= Not Very Much at All; 100= Very, Very Much)

Study 4

Questions Regarding the Pupil Diameter Manipulation (Given on study questionnaire)
How similar was your feeling of how much effort you put into the typing task and the report on your effort from the pupil diameter data?
(1= Not Very Similar; 100= Very Similar)
How accurately did the pupil diameter measure your effort on the typing task?
(1= Not Very Accurately; 100= Very Accurately)
Did your effort report on the typing task motivate you to increase your effort on the anagram task?
(1= Did Not Motivate at All; 100= Motivated Me Very Much)
Did your effort report on the typing task influence you to decrease your effort on the anagram task?
(1= Did Not Decrease Effort at All; 100= Decreased Effort Very Much)

Study 5

Questions Added to those Administered after the Typing, Stroop, and Anagram Task
How much self-control did you have to use to perform the [typing/Stroop/anagram] task?
(Where 1= Not Very Much Self-Control at All and 100= All of my Self-Control)
How much did you have to fight against an urge to perform the [typing/Stroop/anagram] task?
(Where 1= Not Very Much at All and 100= Very, Very Much)

Questions Specific to the Stroop Task
How would you compare your effort on the typing task to the color-word identification task?
Select one:
___________ A lot more effort on the typing task than the color-word task
___________ Somewhat more effort on the typing task than the color-word task
___________ The same amount of effort on the typing and color-word task
___________ Somewhat more effort on the color-word task than the typing task
___________ A lot more effort on the color-word task than the typing task
Were you conserving energy for the next task?
(Where 1= Not Very Much at All and 100= Very, Very Much)
How much did you think about the next task while working on the color-word identification task?
(Where 1= Not Very Much at All and 100= Very, Very Much)

What task do you expect to perform next?
__________Don't know
__________Color-word identification task
__________Anagram Task
__________Typing Task
__________Maze Task

Questions Regarding the Goal Manipulation (Given on study questionnaire)
How much did the instructions/goal you were given motivate you to perform better on the anagram task?

(1= Not Very Much at All; 100= Very, Very Much)

How much did the instructions/goal you were given increase your effort on the anagram task?

(1= Not Very Much at All; 100= Very, Very Much)

How much did the instructions/goal you were given influence you to work harder on the anagram task?

(1= Not Very Much at All; 100= Very, Very Much)

How would you compare your effort on the color-word identification task to the anagram task?
Select one:
__________ A lot more effort on the color-word task than the anagram task
__________ Somewhat more effort on the color-word task than the anagram task
__________ The same amount of effort on the color-word task and anagram task
__________ Somewhat more effort on the anagram task than the color-word task
__________ A lot more effort on the anagram task than the color-word task

APPENDIX C

48 ANAGRAMS FROM STUDY 2

LTEUBLA (bullate)
GROADNE (groaned)
LENPTAE
UOLDIBE
FSNAITE (fainest)
OECARDE
TRAETCR (retract)
MRBTHUE (thumber)
AEDRNOM (madrone)
ARVHTEL
SHMCUEL (mulches)
THATROE
RPSEONH
KECUBEL
RATSIID (diarist)
DNOWIWE
SCUREED (recused, reduces, rescued, secured, seducer)
TNHRCIE (cithern)
LODLANE
NPGRISA (parings, parsing, rasping, sparing)
AICOLST (stoical)
MBYLSOA
ONADESN
ENELGTD (gentled)
OETKPCH
CSEDOLA (solaced)
LEYPSET (steeply)
CABLHED
PLECINA (capelin, panicle, pelican)
OMCNMOT
POSTSGI (spigots)
EMKOONY
GLUAERR (regular)
IIDVEDE
OEFSWLR (flowers, fowlers, reflows)
ROFAVSL (flavors)
CMBHAOT
CTWSIHT
GAWHEIT
TETLELI
EODCMYN
URAHHCC
OERIRFPM
ONTCUESL
LSLIMNOI
SIVTION
LEKHPIC
NORCGEA (acrogen)

Solutions are in parentheses for the solvable anagrams.

APPENDIX D

DEMOGRAPHICS QUESTIONNAIRE

What is your age? _____ years

What is your gender? M F

What is your ethnic background (please circle all that apply)?

a. African-American or Black
b. American Indian or Alaska Native
c. Asian-American or Asian
d. Caucasian (non-Hispanic)
e. Hispanic
f. Native Hawaiian or Other Pacific Islander
Other (please specify): __________________________

What was your total score on the ACT or SAT? _____

* Are you a native English speaker? Yes _____ No _____

*This question was included in the demographics questionnaire for Study 5.

APPENDIX E

TYPING TASK DETAILS

Text from Lentricchia (1990)

There's a little story once told by Wallace Stevens that I have to replot as I retell it. The story (Steven's' and mine) is actually an "anecdote": from the Greek, *anekdota*, meaning unpublished items. More familiarly, in English, a small gossipy narrative generally of an amusing, biographical incident in the life of a famous person whose biography's broad outline has long been a matter of public record. And more: this biography is often—when the famous person is also exemplary—a concentrated representation of the idealized story that a culture would like to tell about itself. Like all anecdotes, then, the one I have in mind can't work as an anecdote unless it somehow tells a story beyond the one it tells. So: an unpublished little story, funny and biographical, apparently stands in for a bigger story, a socially pivotal and pervasive biography which it illuminates—in an anecdotal flash the small story reveals the essence of the larger story and in that very moment becomes exegesis of a public text; the hitherto unpublished items become published. The teller of anecdotes has to presume the cultural currency of that large, containing biographical narrative which he draws upon for the sharp point he would give his anecdote, whose effect is ultimately political: to trigger a narrative sense of community that the anecdote evokes by evoking the master biography. In evoking the master biography, anecdote helps us to remember. And remembrance, so triggered, is the power which sustains, by retrieving, our basic cultural fiction. One day, when he was a little boy, George Washington chopped down a cherry tree in his father's orchard. Americans usually get the point in a hurry; we hardly need to finish the story and deliver its famous moral punchline: the relationship of government and the people who elect it is transparent, sincere because the origin of the USA was honest.
Number of Words in Text: 313
Number of E's in Text: 195
Number of Spaces in Text: 309

Instructions:

Variation for Study 3
In the following task, a passage will fill the screen. You will need to type out this passage, including punctuation and capitalization. However, you must type the passage without typing spaces or the letter E. While you will be unable to see what you type, type as quickly and as accurately as you can. Please do your best; correct any mistakes that you make, and continue typing until you are told to stop.

Variation for Study 4
In the following task, a passage will fill the screen. You will need to type out this passage, including punctuation and capitalization. However, you must type the passage

without typing spaces or the letter E. While you will be unable to see what you type, type as quickly and as accurately as you can. Also, please do not look at your fingers or the keyboard as you type, as this will interfere with the eye tracking. Please do your best; correct any mistakes that you make, and continue typing until you are told to stop.

Variation for Study 5
In the following task, a passage will fill the screen. You will need to type out this passage, including punctuation and capitalization. However, you must type the passage without typing spaces or the letter E. For example, if the passage said the following: The table had three chairs beside it. You would type the following: Thtablhadthrchairsbsidit. While you will be unable to see what you type, type as quickly and as accurately as you can, and avoid making errors. Errors are typing E's or spaces, typing other letters or punctuation that are not in the paragraph, and skipping over other letters and punctuation that are in the paragraph. Please do your best; correct any errors that you make, and continue typing until the computer indicates you are done. As soon as you see the passage, start typing without using spaces or the letter E.

APPENDIX F

80 ANAGRAMS FROM STUDIES 3 & 5

LTIGN (glint)
CNIHF (finch)
CHITP (pitch)
NRCIA (cairn)
FTEIH (thief)
HTIWD (width)
ITRUF (fruit)
TCUON (count)
ADEBL (abled, baled, blade)
EUNDC (dunce)
HMIEC (chime, hemic)
TERDN (trend)
AUGRD (guard)
PHTED (depth)
AVTLU (vault)
OCPHR (porch)
CNHLU (lunch)
NRTOF (front)
JEGUD (judge)
OAJRM (major)
BOTIR (orbit)
SRIVU (virus)
HECPR (perch)
HAENY (hyena)
GIFTH (fight)
FTLRI (flirt)
KASNC (snack)
ORDCW (crowd)
RCNHA (ranch)
ONAPR (apron)
NBIAC (cabin)
HLVOE (hovel)
FIYRA (fairy)
KRTCU (truck)
OWNLC (clown)
IPTLU (tulip)
ONGYA (agony)
FNKIE (knife)
NEIBR (brine)
ZLTWA (waltz)
CRIAV (vicar)
IUNYT (unity)
YAORV (ovary)
HSAFL (flash)
HNDUO (hound)
LCKPU (pluck)
IMENC (mince, nemic)
GITHL (light)
GBRUY (rugby)
CIAHR (chair)
LKCOA (cloak)
HSLAC (clash)
HRCUS (crush)
GBTIO (bigot)
PLIMB (blimp)
KTENO (token)
TRHIM (mirth)
NIRDK (drink)
ACNFY (fancy)
NOEHY (honey)
OFREC (force)
EMCYR (mercy)
HACTM (match)
UTDBO (doubt)
ECRVO (cover)
MACPR (cramp)
YQERU (query)
BNHUC (bunch)
OWAMN (woman)
HTMON (month)
OUSTC (scout)
GEVOL (glove)
LAKPN (plank)
ORHCD (chord)
WSHLA (shawl)
IOGLC (logic)
RETIV (rivet)
TELSY (style)
IJNOT (joint)
CMRBU (crumb)

Solutions are in parentheses.

APPENDIX G

STUDY 4 PUPIL DATA REPORT FOR HIGH-PERCEIVED-EFFORT PARTICIPANTS

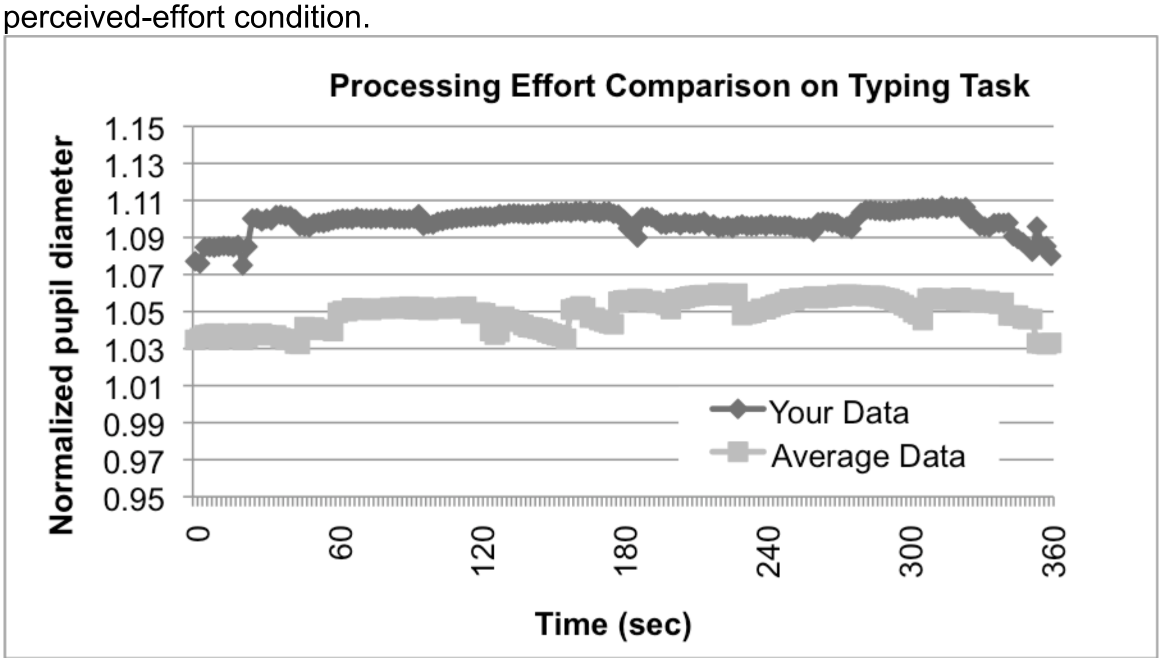
Background on Pupil Dilation:

Dilation of the pupil changes over time. It is commonly known that pupil dilation changes as the amount of sunlight hitting the eye changes. It is also known that dilation changes to reflect processing effort. In psychology, pupil dilation has been studied and used as a reliable physiological measurement of effort in tasks such as mental arithmetic and short-term memory (Hess & Polt, 1964; Kahneman, 1973). Pupil dilation has also been used to measure processing effort during reading comprehension (Just & Carpenter, 1993).

Explanation of Your Pupil Data:

During the typing task you just completed, the eye tracker was set up to measure your pupil dilation. The data from your pupil dilation has been computed as a measure of your processing effort during the task. On the y-axis of the figure below is pupil diameter; pupil diameter is higher when more effort is being exerted. On the x-axis of the figure is time. The figure shows the data relating to your processing effort over the 6 minutes of the task. As a comparison, the figure also shows the average processing effort of participants who have already participated in this study.

Figure A1: False representation of pupil diameter data for participants in the high-perceived-effort condition.



Summary of Your Pupil Data:

The pupil data show that you exerted more effort than the average participant on the typing task. Specifically, you exerted more effort than <u>81.5% </u>of the other participants.

References

Hess, E. H., & Polt, J. M. (1964). Pupil size in relation to mental activity during simple problem solving. *Science, 143,* 1190–1192.

Just, M. A., & Carpenter, P. A. (1993). The intensity dimension of thought: Pupillometric indices of sentence processing*. Canadian Journal of Experimental Psychology, 47,* 310–339.

Kahneman, D. (1973). Attention and effort. Englewood Cliffs, NJ: Prentice-Hall.

APPENDIX H

STUDY 4 PUPIL DATA REPORT FOR LOW-PERCEIVED-EFFORT PARTICIPANTS

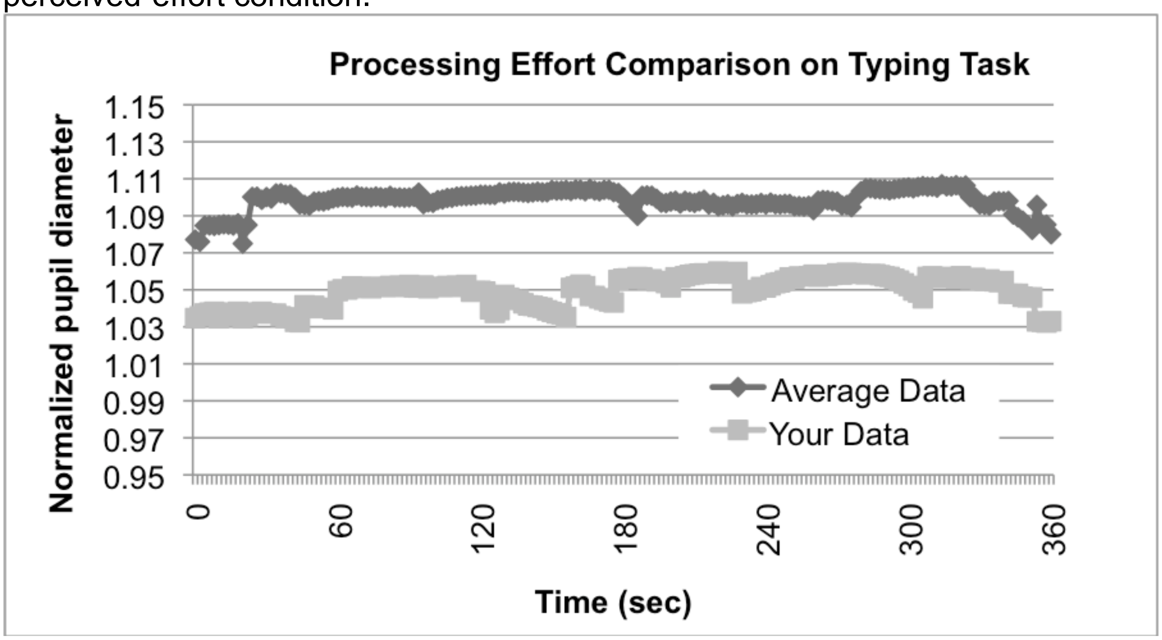
Background on Pupil Dilation:

Dilation of the pupil changes over time. It is commonly known that pupil dilation changes as the amount of sunlight hitting the eye changes. It is also known that dilation changes to reflect processing effort. In psychology, pupil dilation has been studied and used as a reliable physiological measurement of effort in tasks such as mental arithmetic and short-term memory (Hess & Polt, 1964; Kahneman, 1973). Pupil dilation has also been used to measure processing effort during reading comprehension (Just & Carpenter, 1993).

Explanation of Your Pupil Data:

During the typing task you just completed, the eye tracker was set up to measure your pupil dilation. The data from your pupil dilation has been computed as a measure of your processing effort during the task. On the y-axis of the figure below is pupil diameter; pupil diameter is higher when more effort is being exerted. On the x-axis of the figure is time. The figure shows the data relating to your processing effort over the 6 minutes of the task. As a comparison, the figure also shows the average processing effort of participants who have already participated in this study.

Figure A2: False representation of pupil diameter data for participants in the low-perceived-effort condition.



Summary of Your Pupil Data:

The pupil data show that you exerted less effort than the average participant on the typing task. Specifically, 81.5% of the other participants exerted more effort than you.

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Hess, E. H., & Polt, J. M. (1964). Pupil size in relation to mental activity during simple problem solving. *Science, 143,* 1190–1192.

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Kahneman, D. (1973). Attention and effort. Englewood Cliffs, NJ: Prentice-Hall.

APPENDIX I

48 ANAGRAMS FROM STUDY 4

CSEDOLA (solaced)*
OEFSWLR (flowers, fowlers, reflows)*
RIEVNAG (vinegar)
OIOGBYL (biology)
RATSIID (diarist)*
AREEDMR (dreamer)
OERRFPM (perform)**
AICOLST (stoical)*
UDPIDNG (pudding)
SCTFIHA (catfish)
ULCHEKC (chuckle)
TNHRCIE (cithern)*
AFLNNLE (flannel)
NORCGEA (acrogen)*
FSNAITE (fainest)*
EAUQRRT (quarter)
TPAULAS (spatula)
TRAETCR (retract)
AOBCART (acrobat)
EEDSAEW (seaweed)
INAHPIR (hairpin)
CECRNON (concern)
SHMCUEL (mulches)*
POSTSGI (spigots)*
LEYPSET (steeply)*
PLECINA (capelin, panicle, pelican)*
RAECBEM (embrace)
TNETISD (dentist)
RASMDUT (mustard)
LTEUBLA (bullate)*
GROADNE (groaned)*
IZETICN (citizen)
SCUREED (recused, reduces, rescued, secured, seducer)*
LLIMNOI (million)***
AEDRNOM (madrone)*
REHGTIF (fighter, freight, refight)
ELASSME (measles)
PEURTMT (trumpet)
GLUAERR (regular)*
NPGRISA (parings, parsing, rasping, sparing)*
ENELGTD (gentled)*
RSEEPKA (speaker)
ROFAVSL (flavors)*
ETNRPAT (pattern)
EETSRGU (gesture)
IPLSREP (ripples, slipper)
ONTCUSE (contuse)**
CATILPA (capital)

Solutions are in parentheses.
*Signifies solvable anagrams from list in Study 1
**These anagrams were in the list for Study 1 with an extra letter that made them unsolvable (e.g., in the Study 1 list OERRFPM (perform) was OERIRFPM).
***This anagram was solvable in the list for Study 1 (LSLIMNOI: millions); the “S” was removed for this list to make it a 7-letter anagram.

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