Running head: REPEATED-MEASURES EGO DEPLETION

Development of a Within-Subject, Repeated-Measures Ego Depletion Paradigm:

Inconsistent Results and Future Recommendations

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Abstract

The ego-depletion effect is currently under scrutiny for low replicability, possibly reflecting the limited statistical power available in between-subject designs. In response, we created a within-subject, repeated-measures ego-depletion paradigm that repeatedly alternated depletion and recovery manipulations. Each manipulation was followed by measuring subjective fatigue, mood, and self-control performance. Across twelve studies ($N = 754$), participants reliably reported having less energy and lower mood after depleting manipulations compared to after recovery manipulations. Depletion manipulations did not consistently affect behavioral self-control, although the small effect was meta-analytically significant ($d = .045$). Furthermore, unintended fatigue and practice effects occurred over the course of the paradigm, systematically interfering with the intended depletion effects. We recommend that depletion research take advantage of within-subject designs without using repeated-measures.

Keywords: ego depletion; self-control; methods; within-subject; self-regulation

Over the last twenty years, experimental psychology has amassed over six hundred studies showing that people who have just engaged in a difficult self-control task become fatigued, and subsequently perform poorly at a subsequent self-control task (Cunningham & Baumeister, 2016; Hagger, Wood, Stiff, & Chatzisarantis, 2010). This phenomenon, termed ego depletion (Baumeister, Bratslavsky, Muraven, & Tice, 1998), has traditionally been studied using a between-subject, sequential task paradigm. One group of participants completes a difficult self-control task, while the other group of participants completes an easy version of a similar task; afterwards, both groups complete an unrelated self-control task and the performance of the two groups are compared. This design has been used with a vast variety of depleting tasks to induce fatigue, and a variety of second tasks to measure the impact of fatigue (Hagger et al., 2010). Although hundreds of studies have found differences between these groups – and while these findings are theoretically similar to studies of mental fatigue in other fields (Boksem, Meijman, & Lorist, 2005; Saxby, Matthews, Warm, Hitchcock, & Neubauer, 2013) – recent failures to replicate the effect and evidence of substantial publication bias have shaken confidence in the very existence of the phenomenon (Carter, Kofler, Forster, & McCullough, 2015; Hagger et al., 2016; Lurquin et al., 2016; Xu et al., 2014).

Here, we present the results from one potential approach to address the methodological weaknesses that currently exist in the study of ego depletion. We summarize a series of studies using a within-subject, repeated-measures ego depletion design, and follow with recommendations for the study of ego depletion going forward.

Reliability and Effect Size of Ego Depletion

The existence of the ego depletion effect has recently been questioned on the grounds of failures to replicate (Blain, Hollard, & Pessiglione, 2016; Hagger et al., 2016; Lange & Eggert,

2014; Lurquin et al., 2016; Xu et al., 2014), and underpowered studies, potential questionable research practices, and evidence of publication bias (Carter & McCullough, 2013, 2014). In particular, a large-scale preregistered replication recently found no evidence of the depletion effect across 2141 participants in 23 labs (Hagger et al., 2016). While other publication-bias free estimates of ego depletion exist, they too conclude that, at best, previous measures of the ego depletion effect size are greatly overstated. Various unbiased estimates of the effect size vary from zero (Carter et al., 2015) to Cohen's $d = 0.17$, 0.24 , or 0.25 (Carter et al., 2015; Inzlicht, Gervais, & Berkman, 2015; Tuk, Zhang, & Sweldens, 2015). The ego depletion effect is also likely moderated by individual differences and characteristics of the tasks (Cohen, 1992; Judd, Kenny, & McClelland, 2001), contributing to heterogeneity in the effect size.

Assuming that depletion occurs in some contexts and under some conditions (thus potentially accounting for some non-replications), the ego depletion effect size is almost certainly substantially smaller than previously thought. Given a true but small effect size of ego depletion, current methodologies may not be statistically powerful enough to practically study depletion in a laboratory setting. Between-subject designs, particularly with small sample sizes, tend to be low powered and susceptible to inflated false positives due to flexibilities in statistical analyses (Simmons, Nelson, & Simonsohn, 2011).

Large sample sizes are required for sufficient statistical power, yet many ego depletion studies have been published with fewer than 20 participants per cell (Bertrams, Baumeister, Englert, & Furley, 2015; Gailliot, Baumeister, et al., 2007; Gailliot, Schmeichel, & Baumeister, 2006; Inzlicht & Kang, 2010; Schmeichel, Vohs, & Baumeister, 2003; Vohs et al., 2008). For example, if the true effect size of depletion is Cohen's $d = 0.25$, then between-subject designs with 20 participants per cell should result in statistically significant results only 12% of the time.

With severely underpowered experiments, most studies would fail to find significant results even in the case of a true existence of a depletion effect. On the other hand, sufficiently large sample sizes may become unfeasible; if ego depletion's true effect size is $d = 0.25$, a between-subject design would require 253 participants per cell to have 80% power.

Given the problem of underpowered studies and the likely unfeasibility of asking depletion researchers to collect over 200 participants per cell (Gervais, Jewell, Najle, & Ng, 2015), we sought to develop an alternative depletion paradigm that is adequately powered, yet not necessitating an unfeasible number of participants. To do so, we took advantage of the known power benefits of within-subject, repeated-measures designs (May & Hittner, 2012; Snijders, 2005). We were motivated to create a new depletion paradigm to address the replication problems plaguing the traditional between-subject sequential design. We sought an alternative experimental design, with increased statistical power, to improve the reliability of ego depletion experiments. Specifically, we wanted to increase the reliability of the ego depletion effect by inducing the effect in a within-subject, repeated-measures design.

Within-Subject and Repeated Measure Designs

Within-subject designs, particularly when coupled with repeated-measures designs, have both advantages and disadvantages. Beneficially, repeated-measures designs allow for more data to be collected and are compatible with multilevel modelling, resulting in significantly higher statistical power (May & Hittner, 2012; Snijders, 2005). Increasing statistical power means that researchers can be more confident in their results because an increasing proportion of significant results will reflect true effects instead of false positives – an increased sensitivity. Second, within-subject designs allow for the removal of between-subject variation due to individual differences. Ego depletion has been shown to be moderated by individual differences, such as

trait self-control (Gailliot et al., 2006) and willpower theories (Job, Dweck, & Walton, 2010), making it more important to control for between-subject variation.

There are challenges, however, to within-subject, repeated measure designs. Many dependent variables are not conducive to repeated measures; performance on some self-control tasks may improve with practice, or the accuracy of the measure may depend on participants being naïve. Within-subject designs might also increase demand characteristics, as participants are necessarily exposed to both conditions and could intentionally choose to act differently in different conditions, such as by exaggerating the differences between conditions (Charness, Gneezy, & Kuhn, 2012). Finally, there are concerns about carry-over or order effects. By definition, ego depletion continues to affect subsequent tasks – in the original sequential ego depletion design, the self-control task done at Time 1 then interferes with a second task done at Time 2. To allow a non-depletion control measurement to occur at any time after a depletion measurement, a repeated-measure design must ensure that participants can first fully recover from being depleted and return to baseline.

Theoretical Assumptions

A within-subject, repeated-measures ego depletion design necessarily has obstacles, in that, by definition, depletion carries over from one task to another. To design a repeated-measures paradigm, it must be possible to return participants to their baseline, non-depleted state repeatedly and, ideally, relatively quickly. Previous research has suggested that a number of recovery manipulations can counteract the effects of depletion. Specific activities done after the initial depleting task seem to accelerate the recovery process. For example, self-affirmation manipulations, where participants write about a value that is personally important to them (Sherman & Cohen, 2006), have been shown to rejuvenate participant's self-control abilities

back to normal levels (Schmeichel & Vohs, 2009). Positive mood inductions, such as watching a humorous video or receiving a gift, also cancel depletion effects (Shmueli & Prochaska, 2012; Tice, Baumeister, Shmueli, & Muraven, 2007). Being instructed to relax while listening to classical music for three minutes also eliminates the depletion effect (Tyler & Burns, 2008). Most of these recovery activities seem to increase positive affect and many are perceived as being relaxing or rejuvenating.

To address the problem of spillover between the depletion and non-depleting blocks in our within-subject design, our non-depleting recovery blocks were not only non-depleting but were rejuvenating. Following research finding that relaxing and positive mood manipulations hasten people's return to baseline following depletion, the proposed depletion paradigm used short pleasant videos to facilitate recovery from depletion. This design increased the contrast between the depletion blocks and the non-depletion blocks, and aimed to prevent the depletion manipulations from carrying over across the entire experiment. Because positive mood inductions are loosely associated with increased susceptibility to biases and less deliberative thinking (Clore & Huntsinger, 2007; Forgas, 2013; Pham, 2007), the same directional effects as depletion, this recovery manipulation should not conflate our measurement of depletion on task performance – if anything, it might reduce the observable depletion effect. Contrasting depletion blocks with positive videos should thus be a conservative measure of depletion, since measurements of self-control performance are expected to be slightly harmed by the recovery blocks (reduced deliberative thinking due to positive mood) but even more harmed by the depletion blocks.

Different theoretical models of self-control might make different hypotheses about the feasibility of a repeated-measures paradigm. For example, a limited resource model that relies on

the presence of a physical resource (Gailliot, Baumeister, et al., 2007) would predict a longer refractory period for depletion, since time is presumably required for the resource to be replenished. In contrast, motivational models, such as the shifting-priorities process model (Francis & Inzlicht, 2016; Inzlicht & Schmeichel, 2012; Inzlicht, Schmeichel, & Macrae, 2014), opportunity-cost models (Kurzban, Duckworth, Kable, & Myers, 2013), and perhaps even the most recent revision of the resource model (Baumeister, 2014) are compatible with faster recovery from depletion, as well as faster implementation of depletion. Although the within-subject, repeated-measures paradigm is not intended to test theoretical models against each other, the paradigm assumes a motivational model. Motivational models, which are based on dynamically changing priorities, choices, and preferences (e.g., Berkman, Hutcherson, Livingston, Kahn, & Inzlicht, in press) can accommodate fast depletion and recovery since they are not restrained by the presumably slow replenishment of physical resources.

Overview of Studies

We first outline the basic repeated-measures paradigm structure, introduce our main depleting and recovery manipulations, and describe the general procedure: all studies followed very similar experimental designs, procedures, and analyses. We then present the meta-analytic results on how the paradigm reliably manipulated self-reported fatigue and mood, with participants reporting feeling more fatigued and less pleasant after depletion manipulations and progressively more so as the study continued. Next, we present the results from the four studies that used the same dependent measure, the Cognitive Estimation Task (CET), and three subsequent studies that used three other self-control dependent variables. Across studies, the results on the dependent variable are mixed and not as reliable as hoped. Next, we discuss our mixed attempts to separate the effects from the “depleting” and “recovery” components of the

paradigm. In the final section of the results, we briefly report two failed attempts to change the paradigm by using a different depleting manipulation (restricted writing) or a different recovery manipulation (self-affirmation). For all studies, additional results, materials, and data are available online at https://osf.io/rh2gv/?view_only=962335c0214d447fba4e8fad303c6f11 (*blinded link for peer-review only*).

Paradigm Structure

The within-subject repeated measure structure consists of multiple blocks, half of which are depletion blocks and half recovery blocks (Figure 1). All studies, except for Study 1, strictly alternated between the recovery and depletion blocks (Study 1 blocks were assorted randomly, with up to three consecutive blocks of a single type). Each block began with a short depleting or recovery manipulation, equivalent to the “Time 1” task in the standard, sequential depletion paradigm. Participants then indicated current mood and energy levels using two visual analogue scales: one indicated mood from ‘pleasant’ to ‘unpleasant’, and one indicated energy from ‘energized’ to ‘fatigued’. A block finished with a quick self-control dependent variable, equivalent to a “Time 2” task of the standard paradigm. In a single experiment, a participant completed between 6 and 22 blocks.

Figure 1. Two blocks of the within-subject, repeated-measures paradigm, showing both the depletion manipulation condition and the recovery manipulation condition. Studies repeated this pattern between 3 and 11 times, for a total of 6 to 22 blocks. The self-control measurement varied across studies (see Table 1), but was most commonly the Cognitive Estimation Task (CET).

Depleting and Recovery Manipulations

For our standard depleting manipulation, we chose the add-3 task (Kahneman, Tursky, Shapiro, & Crider, 1969), an effortful timed task that uses both working-memory and cognitive processing. Participants are shown four random numbers for one second each, and must store the numbers in working memory and add three to each of the numbers (1 becomes 4, 9 becomes 2, etc.). They then have a four-second response window to type their answer. This task is extremely effortful, as measured both by subjective report of effort (and, seen below, self-reports of

fatigue) and large pupil dilation while performing the task (Kahneman et al., 1969), making it a good candidate to induce ego depletion. Although there is no explicit inhibitory component to this task, self-control is required to maintain items in working memory (e.g., Schmeichel, 2007) and to stay focused and persevere on the task. Similarly effortful tasks, such as sustained attention tasks, also cause decreasing performance such that the more effortful the task, the greater the performance decrement – these decrements are explained using a similar resource model to the original model of ego depletion (Pattyn, Neyt, Henderickx, & Soetens, 2008; Thomson, Besner, & Smilek, 2015). All studies used 90 seconds of this add-3 task for the depleting manipulation, except for Study 9, which intentionally tests a different depletion manipulation.

The repeated measures paradigm requires a recovery period, so that participants' self-control can be returned to normal control levels. Although extended breaks can rejuvenate people (Oaten, Williams, Jones, & Zadro, 2008; Tyler & Burns, 2008), previous research has found that active relaxation or positive videos can speed up the process, and shorten the recovery period down to as little as three minutes (Tyler & Burns, 2008). We chose to use positive videos as our recovery condition, as previously used to diminish the effects of ego depletion (Shmueli & Prochaska, 2012; Tice et al., 2007). All studies but two used the same videos from the set of eleven videos (Study 6 used a different but comparable set of videos; Study 10 tests a different recovery manipulation, a self-affirmation task). The positive videos were consistently between 60 and 90 seconds long, with an average time of 75 seconds. Some studies, but not all, matched the length of recovery time to the length of the depletion time.

General Procedure

The following studies consist of a mixture of studies done in-lab with introduction to psychology students, and studies done online with Mechanical Turk workers. Introduction to psychology students were generally given course credit (or \$10), while M-Turk workers received between \$0.50 and \$2.50 depending on the length of the study. For in-lab studies, up to five participants were seated at separated desks with individual computers, and conducted the studies programmed in MediaLab and DirectRT. Online studies were conducted with Qualtrics.

Power analyses were conducted before experiments to inform our sample sizes. The within-subject and repeated measures design allowed for smaller samples; power analysis suggested that a small-medium effect size ($d = .3$; data collected in 2013) with 22 measurements and a within-subject correlation of 0.5 (MacPherson et al., 2014) would have 80% power with only 36 participants. Studies had between 30 and 143 participants, with an average of 63 participants per study. Samples for each study are available in Table 1, along with a summary of the other variations in the experimental paradigm across studies.

Table 1

Summary Characteristics of Each Study

Study	Participants	N	Self-Control Measurement	Recovery Manipulation	Depletion Manipulation	# of Blocks
1	Undergraduates	44	CET	Videos	Add-3 Task	22
2	M-Turk	68	CET	Videos	Add-3 Task	12
3	Undergraduates	40	Flanker	Videos	Add-3 Task	18
4	M-Turk	60	Anchor Effect	Videos	Add-3 Task	12
5	Undergraduates	72	Solvable Anagrams	Videos	Add-3 Task	16
6	University/Community ¹	143	CET ¹	Videos ¹	Add-3 Task	6
7a	M-Turk	30	Anchor Effect	Neutral Questions	Add-3 Task	12
7b	M-Turk	31	Anchor Effect	Videos	Neutral Questions	12
8a	M-Turk	71	CET	Videos	Add-3 Task	6
8b	M-Turk	72	CET	Videos	Neutral Questions	6
9	M-Turk	62	CET	Videos	Restricted Writing	8
10	M-Turk	61	CET	Self-Affirmation	Add-3 Task	8

Note. The most common dependent variable (DV) was the Cognitive Estimation Task (CET), based off of Shallice & Evans (1978). The effortful add-3 task was based off of Kahneman et al. (1969), and the videos were positively valenced with various enjoyable content, often involving music.

¹ Data from Study 6 was collected by a separate group of researchers and used different but comparable CET questions and videos. Participants in Study 6 also each completed the repeated-measures depletion paradigm multiple times over the course of a larger longitudinal study (see Miles, Lin, Francis, & Inzlicht, in prep). For this analysis, we include only data from the first time that the participants did the repeated-measures paradigm.

Participants first provided informed consent, and then gave their current mood and energy levels. In some studies, they also completed an individual difference measure at the beginning, such as the Implicit Theories of Willpower Scale (Job et al., 2010). To indicate initial mood and energy levels, participants were presented with numberless scales modelled after visual analogue scales (Crichton, 2001); the mood scale was anchored with ‘pleasant’ at the top (or right) and ‘unpleasant’ at the bottom (or left) and the energy scale anchored with ‘energized’ and ‘fatigued’, and participants chose the point on the line which most closely represented their current state. These mood scales went from 1-100 for online participants, and 1-12 for in-lab participants (transformed to 1-100 for consistent analyses). Participants then performed a variable number of practice trials of the depleting add-3 manipulation, to learn the task – online participants completed between 1 and 6 trials (performance-based) and in-lab participants completed either 5 or 10 trials (performance-based). The experiment then began, with each participant completing some number of alternating recovery and depleting blocks (see Table 1). Some studies had individual difference measures at the end of study, and then all participants received a full debrief and payment.

Analysis

For each independent study, we conducted two primary tests². First, we determined if the paradigm influenced people’s subjective fatigue levels (and mood), by creating a multi-level model for each. We tested whether mood and fatigue were affected by three fixed effects: (i)

² We conducted additional analyses where relevant – for example, a separate model tested for moderation by an individual difference measure, willpower beliefs (Job et al., 2010). See Supplemental Materials for these analyses.

condition, depleting versus recovery, (ii) the block order, to distinguish earlier from later blocks, and (iii) the interaction between condition and block order. For each random intercept model, participant was the random factor. When we found no significant interaction terms, we re-conducted the models with only the main effects of block and condition, and report the results of these models below. Next, we looked to see if the three same paradigm characteristics – block condition, block-order, or their interaction – affected the main dependent measure, the self-control task conducted at the end of each block. These new multilevel models were identical to the first models, other than the new dependent variable. If there was a significant condition-by-block interaction, we conducted tests of simple effects for the first and sixth block of the study (West, Aiken, & Krull, 1996). The sixth block was chosen because the shortest studies had only six blocks.

Unstructured multi-level models were done in R, using the mixed modelling function ‘lmer’ from package lme4 version 1.1-10 and the ‘pamer.fnc’ wrapper from the package LMERConvenienceFunctions, version 2.10 to estimate degrees of freedom (most conservative values used), F-values and p-values (Tremblay & Ransijn, 2013). Simple effects of interactions (West et al., 1996) were likewise done with *lmer*. Full analysis code is available on the Open Science Framework (https://osf.io/rh2gv/?view_only=962335c0214d447fba4e8fad303c6f11). Within-subject predictor variables (self-reported mood and energy) were person-centered, while between-subject variables, like willpower lay theories, were grand-mean centered. Effect sizes were calculated as semi-partial R^2 (Edwards, Muller, Wolfinger, Qaqish, & Schabenberger, 2008), equivalent to partial eta-squared (η^2_{partial}).

Meta-Analysis. To conduct meta-analyses across the studies (Goh, Hall, & Rosenthal, 2016), we analyzed the raw data across studies using three-level hierarchical models. Meta-

analysis using raw data, instead of using summary statistics, allows for increased power and ability to examine moderators, and is thus preferable when the raw data is readily available (Cooper & Patall, 2009; Curran & Hussong, 2009). The three-level hierarchical models were structured with observations nested within participant, nested within study, and were also conducted using the lmer package. Estimated R^2 was calculated with *r2glmm* R package using the Kenward-Roger approach (Jaeger, 2016). When meta-analyses were conducted across studies with different dependent variables, the dependent variable was first standardized separately for each study.

Dependent Variables

As seen in Table 1, we used four different self-control measures as the dependent variables across studies. These measures were repeatedly administered to participants at the end of each block (between 6 and 22 times), after the recovery or depletion manipulations, and after the brief manipulation checks (self-reported state). All selected self-control measures were modified to fit into a repeated-measures design by ensuring that the measure took no longer than two minutes (and generally much less than that). We attempted to select measures that would not be susceptible to practice effects or to mood inductions. Most of the studies used the Cognitive Estimation Task (CET), one study used two-minute blocks of an inhibitory reaction time task (the flanker task), and one study looked at performance on sets of solvable anagrams. All of these dependent variables had been previously used as dependent measures in ego depletion paradigms (CET in Schmeichel et al., 2003; solvable anagrams in Baumeister et al., 1998; inhibition reaction time tasks in Inzlicht & Gutsell, 2007). The remaining two studies use the anchoring effect heuristic, which, to our knowledge, had not been previously used in ego depletion experiments.

CET. Our most common self-control measures were sets of three (or four) CET questions, averaged to one measure (Bullard et al., 2004). The cognitive estimation task is a measure of executive functioning and problem-solving (Shallice & Evans, 1978) that can be administered quickly and without practice effects (MacPherson et al., 2014). Participants are asked to estimate answers to quantitative questions to which the answers are not commonly known, such as “How old was the oldest figure skater to win an Olympic medal”. The ambiguity of the questions means that performance on the cognitive estimation task does not depend on specific crystallized knowledge (Della Sala, MacPherson, Phillips, Sacco, & Spinnler, 2004). Although originally a clinical tool, the cognitive estimation task has also been used in non-clinical populations as a measure of ego depletion, where depleted participants have been shown to come up with unreasonable and extreme estimates more often than non-depleted participants (Schmeichel et al., 2003). This may be because participants who are unable or unwilling to self-regulate do not appropriately monitor the quality of their answers and are more likely to write down the first guess that comes to mind, regardless of the quality.

Participants answers were transformed into absolute z-scores based on a distribution of normal answers previously collected from a group of 138 online participants – thus, higher scores on the CET indicate poorer performance (see Supplemental Materials).

Flanker Task. Study 3 used 2-minute long flanker tasks (Eriksen & Eriksen, 1974), which were comprised of 16 compatible and 16 incongruent trials displayed in a random order. The flanker task requires participants to respond to a central stimulus (the letter H or S) in a stimulus array by pressing the appropriate button on a button-box. There are two types of trials: compatible trials present the target letter surrounded by four flanking letters of the same type (e.g., SSSSS) and incongruent trials present the target letter surrounded by the opposite letter

(e.g., SSHSS). Incongruent trials are more difficult, are associated with longer reaction times and more errors, and require greater inhibitory control of pre-potent responses (Eriksen & Eriksen, 1974; van Steenbergen, Band, & Hommel, 2009). For each flanker trial, the participant focuses on a fixation cross before the trial begins, then the flanking letters appear on the screen alone for 100ms, and lastly the central target letter appears with the flanker for 250ms. Participants must suppress their response to the flanking letters to appropriately respond to the target by pressing the associated button within the 1500s response window.

Depletion has been previously found to decrease performance on cognitive reaction-time tasks such as the Stroop task (Inzlicht & Gutsell, 2007; Johns, Inzlicht, & Schmader, 2008), generally by increasing error rates or slowing reaction times on high-conflict trials, specifically. Importantly, positive mood inductions have been previously shown to not affect performance on the flanker task (Martin & Kerns, 2011).

Performance on the flanker task can be analyzed a number of ways, including by examining error rates or reaction times. As our primary dependent measure (used in the meta-analyses), we measured the number of errors on incongruent trials. More errors on incongruent trials is a sign of poorer performance, particularly when not accompanied by increased reaction times that can signal a time-accuracy tradeoff. Because of the limited response window and a relatively high proportion of errors (15.5% of incongruent trials), error rate was a viable measure of performance. As a secondary dependent measures, we analyzed the reaction times for incongruent trials, controlling for reaction time on congruent trials. Although not included in the aggregated results tables or meta-analyses, results with reaction time are available in the Supplemental Materials.

Solvable Anagram Task. For our fifth study, the paradigm's dependent variable was the number of anagrams solved in a restricted time frame. Previous studies have found that participants who are depleted successfully complete fewer anagrams than control participants (Baumeister et al., 1998; Chow & Lau, 2014; Gailliot, Plant, Butz, & Baumeister, 2007; Gordijn, Hindriks, Koomen, Dijksterhuis, & Van Knippenberg, 2004). Researchers have explained this effect by suggesting that self-control is necessary for perseverance, and perseverance on individual anagrams results in a greater number solved overall. The explanation of perseverance has been supported by other experiments that show depleted participants spend less time attempting to solve unsolvable puzzles (Baumeister et al., 1998). We chose this dependent variable both because of its frequent use in the field (Hagger et al., 2010) and because we could easily modify the task to be repeatedly administered in 90 seconds increments (see Supplemental Materials).

For the aggregated result tables and the meta-analyses, we presented the number of correctly solved anagrams as the primary dependent variable. This measure of performance is commonly used (e.g., Baumeister et al., 1998; Boucher & Kofos, 2012) and is the most face-valid measure of performance for the solvable anagrams task. Other variables – such as time spent per anagram – may diverge based on different participants' strategies towards the tasks (e.g., seeing many anagrams and solving only the easy ones quickly, versus seeing few anagrams and solving each of them slowly), and thus may not linearly map on to performance. While not included in the aggregated results, results with these secondary dependent variables are available in the Supplemental Materials.

Anchoring Effect. Anchoring is the phenomenon where an arbitrary provided value influences people's numeric guesses, and people will guess numbers closer to that provided

value (Furnham & Boo, 2011; Jacowitz & Kahneman, 1995; Strack & Mussweiler, 1997). For example, people who are first asked, “Does the President make more or less than \$500 000?” and subsequently asked “How much money does the President make?”, will give higher salary estimates than people who are first asked, “Does the President make more or less than \$90 000?”. Prior research had not examined the anchoring effect under a state of depletion, but research done on other heuristics suggests that they are likely to be affected by depletion. Many heuristic biases are strengthened when people are happy (Clore & Huntsinger, 2007) or depleted (Pocheptsova, Amir, Dhar, & Baumeister, 2008), or under cognitive load (Frederick, 2005). Anchoring, however, seems to be the exception – manipulations that strengthen other heuristic biases instead decrease the anchoring effect (Bodenhausen, Gabriel, & Lineberger, 2000; English & Soder, 2009). Anchoring may rely on a deliberative process of recalling anchor-relevant information and spending more time thinking about the provided anchor value, resulting in increased influence of the anchor (Bodenhausen et al., 2000; Strack & Mussweiler, 1997). We thus hypothesized that depletion would reduce the deliberative process and result in a reduced anchoring effect.

The magnitude of the anchoring effect was calculated according to Jacowitz & Kahneman (1995), where larger values correspond to larger anchoring effects (see Supplemental Materials).

Results

Reliable Change in Self-Reported Fatigue and Mood

The within-subject, repeated measures paradigm reliably resulted in differential self-reported energy and mood levels between the recovery and depletion conditions (Table 2).

Across all applicable studies, participants reported being more fatigued after the depletion block

compared to after the rejuvenation blocks (1 to 100 scale; $B = 6.85$, $t(4847) = 16.62$, $p < .0001$).

Participants also reported being in a more unpleasant mood after the depleting manipulation compared to after the recovery manipulation ($B = 7.79$, $t(4852) = 17.76$, $p < .0001$). Within-subject fluctuations of fatigue and mood were generally highly correlated, $r = .75$ (all studies, $df = 7307$, 95% $CI = [.74, .76]$).

The effect of condition on subjective energy and mood seemed to largely rely on the contrast between both the depletion and the recovery conditions, and was not clearly driven by one manipulation or the other. Two studies (7b and 8b) alternated between the recovery video manipulations and a neutral manipulation where participants spent 90 seconds answering easy survey questions (such as their number of siblings, number of bedrooms in their house, etc.) Only one of these studies found that the recovery blocks resulted in higher subjective energy and pleasant mood than the neutral blocks, and the effect size was noticeably smaller than the effect size in the standard eight studies (Table 2). Study 7a instead alternated between the add-3 blocks and the same neutral manipulation and also did not find significant condition effects (mood $t(208) = 1.63$, $p = .10$; energy $t(208) = 1.14$, $p = .25$). Based on this evidence, it seems that both the depletion manipulation and the rejuvenation manipulation contributed to the overall condition effect on subjective feelings.

The paradigm also consistently resulted in an increase of fatigue and a worsening of mood across the duration of the experiment (Table 2). These block effects were still significant in versions of the paradigm that contrasted either the recovery condition or the depletion condition with a neutral condition (which consisted of answering easy survey questions; Table 2). Regardless of the content of the paradigm, participating in the experiment seems to increase subjective fatigue and decrease subjective mood as time went on.

Table 2

Effects of Depletion Condition and Block on Subjective Energy and Mood

Study	Condition Effect (Depletion vs. Recovery)				Block Effect (Across Experiment)			
	Energy		Mood		Energy		Mood	
	<i>B</i> (SE)	<i>R</i> ² [95% CI]	<i>B</i> (SE)	<i>R</i> ² [95% CI]	<i>B</i> (SE)	<i>R</i> ² [95% CI]	<i>B</i> (SE)	<i>R</i> ² [95% CI]
1	7.92 (1.17)	.047 [.024, .077]	10.55 (1.20)	.077 [.047, .112]	-0.79 (0.09)	.074 [.045, .108]	-0.74 (0.09)	.062 [.036, .095]
2	6.24 (0.95)	.055 [.028, .091]	8.20 (1.06)	.075 [.043, .115]	-1.39 (0.14)	.121 [.081, .028]	-1.47 (0.15)	.110 [.072, .154]
3	10.95 (1.20)	.116 [.075, .165]	5.41 (1.09)	.037 [.014, .071]	-0.51 (.12)	.029 [.009, .060]	-0.65 (0.11)	.055 [.026, .093]
4	7.99 (1.19)	.064 [.033, .104]	9.88 (1.35)	.076 [.042, .118]	-1.22 (0.17)	.071 [.038, .112]	-1.36 (0.20)	.069 [.037, .110]
6	2.92 (0.67)	.025 [.008, .053]	3.71 (0.78)	.030 [.010, .059]	0.92 (0.20)	.029 [.010, .058]	0.49 (0.23)*	.006 [.000, .023]
8a	9.20 (1.44)	.118 [.059, .190]	9.62 (1.72)	.093 [.041, .160]	-1.96 (0.31)	.112 [.055, .184]	-1.36 (0.38)	.041 [.009, .094]
9	10.08 (1.24)	.136 [.082, .199]	13.73 (1.42)	.181 [.121, .247]	-1.09 (0.27)	.037 [.010, .079]	-1.55 (0.31)	.055 [.021, .103]
10	6.24 (1.54)	.038 [.010, .081]	6.84 (1.57)	.044 [.014, .089]	-2.94 (0.34)	.152 [.096, .217]	-3.45 (0.34)	.193 [.132, .260]
All	6.86 (0.41)	.054	7.80 (0.44)	.061 [.049, .074]	-0.89 (0.05)	.057	-0.94 (0.05)	.057 [.045, .070]
<i>(Depletion vs. Neutral)</i>								
7a	.03 (1.90) ^{ns}	.000 [.000, .024]	.62 (1.97) ^{ns}	.000 [.000, .026]	-2.06 (0.41)	.107 [.042, .195]	-3.74 (0.43)	.266 [.174, .364]
<i>(Recovery vs. Neutral)</i>								
7b	3.27 (1.76) ^{ns}	.017 [.000, .068]	2.80 (2.08) ^{ns}	.009 [.000, .052]	-1.66 (0.39)	.081 [.025, .163]	-2.29 (0.46)	.107 [.042, .195]
8b	3.66 (1.26)*	.027 [.003, .072]	5.05 (1.34)	.044 [.011, .098]	-0.62 (0.28)*	.016 [.000, .055]	-1.08 (0.29)	.042 [.009, .095]

Note. All effects are significant at alpha < .01 except for when noted with *ns* (non-significant) or

with * (.01 < p < .05). Non-standardized beta and standard error are based on a 1 to 100 slider

scale. The “All” row calculates effects using all raw data from the above columns, using a

hierarchical model with observations nested within participants within study. Study 5 is absent

from the table since it did not ask the mood or energy questions after the condition manipulation.

Effects on Self-Control Measures

Depletion Condition Main Effects. Seven studies contrasted the add-3 depleting manipulation against the video manipulations, using one of the following measures of self-control: CET, flanker task, anagram task, or anchoring task. Of the four studies to use the CET as a dependent variable, only Study 8a found that depletion manipulations resulted in subsequent poorer performance on the CET compared to recovery manipulations at the midpoint of the study (e.g., at the average block), with Study 1 and Study 2 finding marginal effects in the same direction ($B = .07$ and $.05$ z-score units, $p = .06$ and $.03$). The other CET studies (Study 6) had non-significant main effects of condition at the study midpoint (Table 3).

The remaining three studies that contrast the video condition with the add-3 condition used the flanker task (Study 3), the anchoring effect (Study 4), and a solvable anagram task (Study 5) as dependent variables. None of these studies had significant main effects of condition at the midpoint ($ps > .14$), although each condition effect was in the predicted direction.

When we analyzed all of the studies collectively (as a three-level hierarchical model), there was a small significant condition effect at the midpoint of each respective study, where the depletion condition was associated with generally poorer self-control compared to the recovery condition ($t(4866) = 2.78$, $p = .005$; Table 3). The effect size of this difference, however, was extremely small – on average, performance between the two conditions varied by Cohen's $d = .065$. Including the two studies that used alternative depleting or recovery manipulations (see Alternative Manipulations, below) decreased the effect size further, to $d = .045$, although the condition effect continued to be meta-analytically significant at study midpoints ($t(5713) = 2.07$, $p = .038$; Table 3). The effectiveness of the condition manipulations, however, was moderated by significant condition by block interactions (below; Figure 2).

Table 3

Depletion Condition and Block Effects on Self-Control Task Performance

Study (Dependent Variable)	Effect of Condition (Depletion Effect) at Mean Block		Block Effect		Condition by Block Interaction	
	<i>B</i> (<i>SE</i>)	<i>R</i> ²	<i>B</i> (<i>SE</i>)	<i>R</i> ²	<i>B</i> (<i>SE</i>)	<i>R</i> ²
1 (CET)	.117 (.078) ⁺	.003	.021 (.006) ^{***}	.015	-.006 (.012) ^{ns}	.000
2 (CET)	.056 (.029) ⁺	.005	-.002 (.004) ^{ns}	.000	-.024 (.008) ^{**}	.011
6 (CET)	-.077 (.093) ^{ns}	.001	.095 (.026) ^{***}	.018	-.211 (.052) ^{***}	.022
8a (CET)	.123 (.050) [*]	.020	.060 (.011) ^{***}	.096	.073 (.021) ^{**}	.037
All 4 CET	.042 (.036)	.001	.021 (.004) ^{**}	.009	-.009 (.004) ^{ns}	.002
3 (Flanker)	.007 (.054) ^{ns}	.000	.062 (.010) ^{***}	.055	-.022 (.010) [*]	.007
4 (Anchor)	.037 (.021) ⁺	.005	-.011 (.006) ⁺	.005	.014 (.006) [*]	.008
5 (Anagrams)	.053 (.087) ^{ns}	.000	-.113 (.019) ^{***}	.030	-.087 (.019) ^{***}	.018
Above 7 Studies (Standardized DVs)	.065 (.023) [*]	.002	-.0052 (.0027) ⁺	.000	-.027 (.005) ^{***}	.004
9 (CET, restricted write)	.024 (.044) ^{ns}	.001	.000 (.010) ^{ns}	.000	.038 (.019) ^{ns}	.009
10 (CET, self- affirmation)	-.156 (.069) [*]	.012	.011 (.015) ^{ns}	.002	.005 (.030) ^{ns}	.000
Above 9 Studies (Standardized DVs)	.045 (.022) [*]	.001	-.0054 (.0027) [*]	.000	-.027 (.005) ^{***}	.003

Note. All *B* values are presented so that positive values correspond to worse performance on the

depletion trials, or worsening performance across the course of the study. In addition to the

individual study results, we present aggregated results for (i) the four Cognitive Estimation Task

studies that used the add-3 task and video manipulations, (ii) all seven studies which used the

add-3 task and the video manipulations, and (iii) all studies that used an intended rejuvenation

and depleting manipulation, including Study 9 that used restricted writing as the depleting task

and Study 10 that used self-affirmation as the intended recovery task. Study 3's primary

dependent variable is the number of incongruent errors, and Study 5's is the number of anagrams

solved correctly. Studies that used a neutral manipulation (instead of either the depletion or

recovery manipulation) are not included. *** = $p < .001$; ** = $p < .01$; * = $p < .05$; + = $p < .10$

Block Effects and Interactions with Block. Many of the studies had block effects – where performance on the self-control task changed progressively across the course of the experiment. In some studies, such as Study 3 with the flanker task as the dependent variable, participants became progressively worse on the self-control task as the experiment progressed ($B = .06$ additional errors per trial, $F(1, 623) = 36.88, p < .001$), presumably due to either boredom or fatigue that was accumulating without being sufficiently counter-acted by the recovery videos. On the other hand, in Study 5 participants became progressively better on the anagram task as the experiment progressed ($B = .11$ additional solutions per trial, $F(1, 1076) = 21.92, p < .001$), presumably due to practice effects. While five of nine studies (Table 3) had significant block effects, the lack of consistent direction (performance sometimes improving and sometimes worsening) resulted in a marginally significant block effect across the seven studies contrasting add-3 with video blocks ($t(4855) = 1.92, p = .055$) and a statistically significant but extremely small block effect across the nine studies ($t(5713) = 2.01, p = .044$).

Unfortunately, these inconsistent block effects did not always occur independently of the intended condition depletion effects. Instead, the block effects frequently interacted with the condition effect (Table 3). Five studies had significant interactions between condition and block. In four of these five studies, the interaction was such that the depletion effect was larger at the beginning of the study. This was also the pattern found meta-analytically (Figure 2) – the condition effect on the self-control outcome was generally stronger at the beginning of the study (at block one, $B = .105, SE = .034, t(5817) = 3.05, p < .001$) but was weakened as the study progressed (by block six, $B = .045, SE = .022, t(5817) = 2.04, p = .04$). In every case – even in Study 8a, where the depletion effect was only significant at the end of the study – these interactions involved a significant depletion effect existing at some point during the experiment,

either at the beginning or at the end, and no study found a significant condition effect in the opposite direction. In other words, there was no evidence of a “reverse-depletion effect” occurring in any of these nine studies. However, the frequent interactions between condition and block clearly demonstrated one problem with repeated-measures designs. In general, the condition manipulations were more effective earlier in the experiment, and faded as the study progressed (Figure 2). The progressive weakening of the condition effect largely negated any potential benefit of having multiple repeated-measures trials.

Figure 2. The across-study effect of condition on the performance dependent variable (standardized) is shown for every fourth block. The effect of condition decreased across the

experiment (as blocks progressed) – although the depletion effect was evident at the beginning of the paradigm, it was no longer significant by the 8th block. For each block, only studies which had that block are included (e.g., by the 20th block, only observations from Study 1 remains, since no other study had over 18 blocks), resulting in reduced confidence as the blocks progressed (Block 1 $df = 5715$, Block 20 $df = 731$). *** = $p < .001$; ** = $p < .01$; * = $p < .05$; + = $p < .10$.

Lack of Mediation by Self-Reported Energy or Mood

We next tested whether self-reported energy (fatigue to energized) or mood (pleasant to unpleasant) was predictive of performance on the self-control measures and – if so – whether subjective state could mediate either the effect of condition or of block on the self-control measures. Self-reported fatigue was generally not related to performance on the self-control measures. Across all studies, fatigue was only significantly correlated with self-control measures in one study, Study 7a, which contrasted the depletion manipulation with the neutral questions manipulation instead of the recovery condition $F(206) = 7.27, p = .01$). All other studies had no relationship between self-reported energy and the dependent variables ($p > .11$). In a meta-analytic analysis with all nine datasets, neither self-reported fatigue nor self-reported mood was associated with the standardized self-control dependent variables (fatigue $t(4694) = .87, p = .38$; mood $t(4694) = 1.23, p = .22$). The lack of relationship between self-reported fatigue and performance on the self-control dependent variables meant that self-reported fatigue could not mediate any effects of condition or block on self-control performance.

Separating Effects of the Depleting versus Recovery Manipulations

One limitation of the paradigm design is the conflation between the effects of the depleting manipulation and the effects of the recovery manipulation. It is not clear whether the

condition effects – when they do exist – are due to the add-3 task causing fatigue or the videos causing mood effects. To address this, Study 7 and Study 8b employed a neutral manipulation, where participants spent 90 seconds answering basic questions about themselves (such as age, number of siblings, favorite colour) or doing easy sorting tasks (e.g., sorting words into two boxes depending on if they were capitalized or lowercase). Half of participants would alternate between the neutral manipulation blocks and the depleting add-3 manipulation blocks (Study 7a), while the other half of participants would alternate between the neutral blocks and the recovery video blocks (Study 7b). Study 7 used the anchoring task as the dependent variable, while Study 8 used the CET as the dependent variable.

In Study 7a, using the anchoring task as the dependent variable, participants who completed the neutral and depleting tasks did not show a significant condition effect; condition did not affect energy levels, mood, or the anchoring effect (all $p > .20$). In Study 7b, participants who alternated between neutral and recovery tasks did show significantly different degrees of anchoring bias based on condition – however, the direction of the effect was opposite to the direction of the condition effect when contrasting the depleting and recovery tasks (e.g., opposite in direction to Study 4). The recovery condition, like a positive mood induction, decreased the magnitude of the anchoring bias compared to a neutral condition ($F(1, 187) = 5.04, p = .03$). Others have also found positive mood to decrease the anchoring bias (Bodenhausen et al., 2000; English & Soder, 2009). This difference does not explain the condition effect in Study 4, where the anchoring bias was even smaller in the depletion condition than in the recovery condition.

Study 8b again tested the recovery condition against the neutral condition, to see if the video manipulation alone would change scores on the CET. The condition effect was not significant ($F(1, 310) = .01, p = .90$). This weakly suggest that the video alone is not enough to

cause the condition effects in the previous studies, but given the ultimately small effect size of the condition effect even when contrasting the recovery and depleting conditions, Study 8b was also underpowered.

Different Depleting and Recovery Manipulations

In two studies, we attempted to change either the content of the recovery or depleting manipulations, while continuing to use the CET as the dependent variable. Study 9 used a short restricted writing task as the depleting manipulation, instead of the add-3 task (Schmeichel, 2007). In a continued attempt to keep trials short, participants wrote about a specified topic for 90 seconds, without using two common letters. For each of the four depletion blocks, the specified topic and the disallowed letters varied (e.g., describe your room, describe cooking a meal; O and S, T and I). This version of the task still resulted in significant changes to self-reported mood and energy, comparable to other studies (Table 2): having just engaged in prior restricted writing was associated with significantly more fatigue and unpleasantness, compared to the prior videos (Table 2). However, there was no effect of condition on the CET task ($F(1, 427) = .01, p = .93$), nor did block affect the CET task ($F(1, 427) = 0.34, p = .56$; interaction $F(1, 427) = 1.79, p = .18$).

Instead of using short videos, Study 10 attempted to rejuvenate people through self-affirmation. Self-affirmation has previously been shown to reduce the effects of ego depletion (Schmeichel & Vohs, 2009). At the beginning of the study, participants ranked 11 different values according to their personal importance. The most important four values for each participant were then inserted into the four self-affirmation exercises, where participants wrote about the specified value for 90 seconds. Like the other studies, there continued to be the expected change in self-reported mood and energy (Table 2). However, the self-affirmation

manipulation worsened people's performance on the CET task, compared to the add-3 task ($F(1, 416) = 5.90, p = .02, \eta^2_{\text{partial}} = .014$). This condition effect was opposite in direction to previous tasks. There was no main effect of block ($F(1, 416) = .58, p = .45$) or interaction effect ($F(1, 416) = .04, p = .84$).

The paradigm continued to affect self-reported mood and energy, regardless of the specific content of the depletion or recovery manipulations. However, neither of these alternative manipulations resulted in the expected changes to the CET dependent variable. In fact, brief self-affirmation manipulations may have even hindered people's ability on the subsequent CET task. Self-affirmation has sometimes resulted in poorer performance and task disengagement, particularly when self-efficacy on a task is low (Schmeichel, Park, & Vohs, 2013) – it is possible that in this situation, reminding participants of their values decreased their motivation to persevere on a boring and unrewarding cognitive task, however this is speculative and was not predicted a priori.

General Discussion

Across the series of studies, the repeated-measures paradigm did not consistently perform as a reliable alternative design to measure the effect of ego depletion. The brief depleting and recovery manipulations did significantly change self-reported measures of fatigue and mood in every study; however, these manipulations did not reliably change performance on the subsequent self-control behavioural measures. Although the depletion effect was meta-analytically significant, the effect size was a very small $d = .045$. This small effect size may be consistent with some estimates of the effect size of ego depletion resulting from the standard sequential task between-subject paradigm (Carter et al., 2015; Hagger et al., 2016; Tuk et al., 2015), but may also not be practically meaningful.

Unlike what we had hoped, the paradigm did not reliably create depletion effects in individual studies, with the depletion condition effect only statistically significant in one of seven studies (and marginally significant in three more). Furthermore, both practice and general fatigue effects often occurred throughout the course of the experiment, making the repeated-measures component of the paradigm more problematic than helpful. Although the alternating depleting and recovery manipulations did influence self-control performance at the beginning of the paradigm, the manipulations became less effective as the studies progressed. Given the inconsistent effects on the primary dependent variables, along with other limitations inherent to the paradigm (including potential conflation of depletion and mood effects), we do not recommend the regular use of similar brief-manipulation, repeated-measure designs to study the basic ego depletion effect, at least not without substantial modification and improvement.

The most robust findings from the preceding studies were that, first, participants report feeling more unpleasant and more fatigued after depleting manipulations compared to after recovery manipulations. This change in subjective state is to be expected, and might be further increased by the within-subject design creating demand effects – participants are presumably aware of the purpose of these manipulations and respond to the contrast between the conditions. Second, subjective state decreases equally robustly across the course of the experiment, demonstrating that fatigue and negative feelings (perhaps including boredom) also accumulate throughout the experiment. This suggests that perhaps the recovery manipulations, the short enjoyable videos, are not truly returning participants to a baseline state. Promisingly, these changes in subjective state are moderated by willpower theories and self-reported enjoyment of the videos in predicted directions (see Supplemental Materials for details; limited theorists report more subjective fatigue, and those who enjoyed the videos report less decline in mood and

energy across the course of the experiment). However, these changes in subjective state do not directly predict changes in self-control performance. These subjective state reports may be too contaminated by demand and contrast effects to be predictive of ego depletion, or this may be yet another case of self-report failing to map onto behavioural performance, as is commonly found in studies of emotion (Mauss, Levenson, McCarter, Wilhelm, & Gross, 2005) and of ego depletion when using the standard sequential task paradigm (e.g., Boucher & Kofos, 2012; Hagger et al., 2016).

The results from the above studies also demonstrated the near inevitability of undesired carry-over effects, which accumulated across the course of the experiments. Four studies had significant decreases in performance across time (on the CET and the flanker task), and another study had significant improvement in performance across time (the anagram completion task). These block effects were particularly pronounced in longer studies; the three longest experiments (Studies 1, 3, and 5, each conducted over the course of one-hour long in-person sessions) all had significant block effects, while only two of the six shorter studies had significant block effects. The existence of these block effects in general, however, speaks against the use of within-subject, repeated measures designs. Even if controlling for block effects, there is the potential for practice effects or accumulating fatigue effects to overwhelm any depletion condition effects that may exist.

Interestingly, before the accumulation of practice effects, boredom, or fatigue effects, there was evidence of the ego depletion effect. In four studies, as well as meta-analytically, the intended ego depletion effect existed early in the experiment, but faded with time. As seen in Figure 2, meta-analytically the depletion condition effect was significant at the beginning of the study, and then weakened as the studies progressed, due to either accumulating fatigue or

practice effects. After the first six blocks, there was no longer a significant effect of condition, even meta-analytically. Despite attempting to choose tasks that would be less affected by practice effects and that did not rely on participant naivety, the repeated-measures still interfered with the intended condition effects. Finding suitable dependent variables that are not susceptible to the effects of boredom nor practice may be difficult, if not impossible.

The general condition effects may have also been weakened by the brevity of the depletion manipulations. Only 90 seconds of difficult work may be insufficiently long to cause the degree of depletion that may result from the (longer) standard depletion manipulations, which can vary from 2 to 15 minutes. In this case, the increase in power due to having multiple measurements may have been offset by a decrease in power due to studying a weaker (smaller) ego depletion effect. While there may be increased power inherent to some repeated-measures design, this did not equate to more reliable results, at least not with the relatively small samples used.

Recommendations for the Study of Ego Depletion

Within-subject designs can still be a useful tool when studying fatigue or ego-depletion effects. Researchers examining progressive fatigue often successfully use within-subject designs when looking at time-of-day effects (Dai, Milkman, Hofmann, & Staats, 2015) or using experience sampling methods (Hofmann, Vohs, & Baumeister, 2012). Within-subject experimental manipulations of ego depletion may be more successful when the depletion condition and the control conditions are completed across time; for example, Jia (2017) found a within-subject depletion effect when participants came to two sessions, one week apart. Employing within-subject designs, without having multiple repeated measures in the same session, can still substantially increase the power of experimental designs by accounting for

much individual variation. Sessions that are separated across time should not have problems with boredom-related carry-over effects and may be relatively less affected by practice effects and demand effects, compared to having the conditions presented immediately after one another.

Researchers may also want to consider predictions from motivational models of self-control when designing their experiments. For example, some research suggests whether an activity results in ego-depletion or not may depend on whether the activity is autonomous or externally driven (Chow, Hui, & Lau, 2015; Howard, Edwards, & Bayliss, 2016; Moller, Deci, & Ryan, 2006). It may be that, in some cases, both the depletion condition (e.g., a difficult e-crossing task) and a control condition (e.g., an easier, but boring, version of the same task) are externally motivated, and both conditions may equally remove participants feelings of autonomy. If autonomy is a critical factor in the ego depletion effect, then we would predict no difference between conditions in subsequent self-control performance. Other theories suggest that boredom also causes decreases in performance, as people become motivated to do something with purpose (Bench & Lench, 2013) instead of seemingly pointless experimental tasks – thus, there may be a smaller effect when the control condition is also seen as boring. Supporting this, a recent study found that participants who passively view strings of four numbers (boredom condition) report feeling even more fatigued by the end of the task than participants who engage in an effortful addition task (Milyavskaya & Inzlicht, in prep). Ensuring a sufficient contrast between the depletion and control conditions is likely one way to increase the effect size of the ego depletion phenomenon, thereby increasing statistical power without having to increase the sample size.

Conclusion

Brief depleting and recovery manipulations do consistently affect self-reported measures of fatigue and mood, but only affect performance on behavioural measures of self-control

occasionally, with a small, though statistically significant, meta-analytic effect that may not be practically meaningful. While a repeated-measures design had the potential to increase statistical power, due to both its within-subject design and its increased number of observations, this potential was not realized partially because of problems with demand effects, carry-over effects, and conflation between depletion and mood manipulations. Future research should continue to pursue novel and within-subject methods to study ego depletion and fatigue effects, but should ensure that an appropriate length and intensity of the depletion manipulations are maintained.

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