Original Article

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

Inconsistent Results and Future Recommendations

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Abstract: Ego depletion is 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 12 studies (N = 754), participants reliably reported having lower energy and mood after depleting manipulations compared to after recovery manipulations. Depletion manipulations did not consistently affect behavioral self-control, although the depletion 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 takes advantage of within-subject designs across multiple sessions to avoid spillover effects between measurements.

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

Over the last 20 years, experimental psychology has amassed over 600 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 is compared. This design has been used with a vast variety of depleting tasks to induce fatigue, and an equally vast variety of second tasks to measure the impact of fatigue (Hagger et al., 2010). Although hundreds of studies have found differences between these groups, 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 approach to address the methodological weaknesses in the current study of ego depletion. We summarize a series of studies using a within-subject, repeated-measures 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 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), underpowered studies, 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 2,141 participants in 23 labs (Hagger et al., 2016). While other publication-bias free estimates of depletion exist, they too conclude that, at best, previous measures of the depletion effect size are overstated. Various unbiased

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estimates of the effect size vary from zero (Carter et al., 2015) to small Cohen's $d = 0.17$, 0.20, 0.24, or 0.25 (Carter et al., 2015; Garrison, Finley, & Schmeichel, 2017; Inzlicht, Gervais, & Berkman, 2015; Tuk, Zhang, & Sweldens, 2015), to moderate $d = .48$ or .49 (Dang, 2018; Dang, Liu, Liu, & Mao, 2017). The depletion effect is also likely moderated by individual differences and task characteristics, contributing to heterogeneity in the effect size (Cohen, 1992; Judd, Kenny, & McClelland, 2001).

Assuming that depletion occurs in some contexts and under some conditions (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 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 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 $d = 0.25$ (Carter & McCullough, 2013), 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 without requiring 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).

Within-Subject and Repeated-Measures Designs

Within-subject designs, particularly repeated-measures designs, have advantages and disadvantages. Beneficially, repeated-measures designs allow for more data to be collected and multilevel modeling to be used, resulting in more 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 (increased sensitivity). Within-subject designs also allow for the removal of between-subject variation from 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-measures 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 found a number of recovery manipulations that may counteract the depletion effects. For example, self-affirmation manipulations, where participants write about a personally meaningful value (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, receiving a gift, smoking a cigarette, or listening to classical music also cancel depletion effects (Heckman, Ditre, & Brandon, 2012; Shmueli & Prochaska, 2012; Tice, Baumeister, Shmueli, & Muraven, 2007; Tyler & Burns, 2008). Egan, Clarkson, and Hirt (2015) found that positive affect restored performance after a depleting task, and further found that the restoration was explained by people's expectations of recovery. The number of independent replications of this effect suggests that, while the mechanism may not be entirely understood, inductions of positive affect after the previous depleting task can return participant's performance to baseline levels. Notably, these studies find that performance after recovery manipulations are statistically indistinguishable from baseline control conditions.

To address the problem of spillover between the depletion and non-depletion 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 depletion blocks and 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.

We believe repeated-measures designs are compatible with any theoretical model of depletion that allows for fast recovery from depletion. Both limited resource and motivational accounts of depletion acknowledge that motivational factors can nearly instantly moderate the depletion effect (e.g., Baumeister & Vohs, 2007; Job et al., 2010), and updated resource theories suggest that most laboratory-based depletion effects are more likely due to changes in the motivation to conserve resources, rather than due to a total lack of available resources (Baumeister, 2014; Baumeister & Vohs, 2016). Brief repeated depletion and recovery manipulations should theoretically be effective whether depletion is entirely a motivational process (Berkman, Hutcherson, Livingston, Kahn, & Inzlicht, 2017; Francis & Inzlicht, 2016; Inzlicht & Schmeichel, 2012; Kurzban, Duckworth, Kable, & Myers, 2013), or whether depletion is due to small variations in an underlying physical resource, amplified by a strong motivation to conserve the resource (Baumeister, 2014; Baumeister & Vohs, 2016). As such, the within-subject, repeated-measures paradigm does not test or compare particular theoretical models. The paradigm is compatible with any theory of depletion that accounts for moderation by motivational factors.

Overview of Studies

We first describe the basic repeated-measures paradigm structure, introduce our main depleting and recovery manipulations, and outline how this paradigm differs from the standard, sequential task design. We then describe the general procedure, the self-control dependent variables, and analysis plan. Next, we present meta-analytic results on how the paradigm reliably affects self-reported fatigue and mood, and less reliably affects behavioral self-control performance. 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 unsuccessful attempts to change the manipulations used in the paradigm.

Paradigm Structure

The within-subject repeated-measures structure consists of multiple blocks, half of which are depletion blocks and half of which are 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 each 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. 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.

Depleting and Recovery Manipulations

For our depleting manipulation, we chose the add-3 task (Kahneman, Tursky, Shapiro, & Crider, 1969), an extremely effortful timed task that requires working-memory and attention control. Participants are shown four random numbers for 1 s each, and must store the numbers in working memory and add three to each of the numbers (1 becomes 4, 9 becomes 2, etc.), before typing in their answer. This task is extremely effortful, according to both subjective report of effort and measurements of pupil dilation (Kahneman et al., 1969), making it effective at inducing depletion (Milyavskaya, Inzlicht, Johnson, & Larson, in press). All studies used 90 s of the add-3 task for depleting blocks, except for Study 9, which used a restricted writing task.

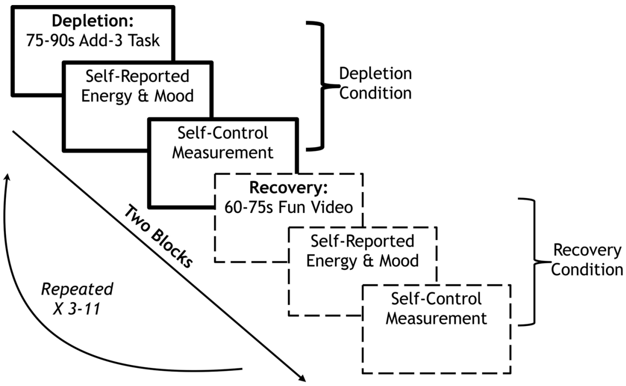

Figure 1. Two blocks of the within-subject, repeated-measures paradigm, showing both the depletion manipulation condition and the recovery manipulation condition. Studies repeated this pattern between 3 and 11 times, for a total of 6–22 blocks. The self-control measurement varied across studies (see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

The repeated-measures paradigm requires a recovery manipulation, so that participants' self-control can be returned to normal control levels. We used positive videos as our recovery manipulation, as they have been previously found to cancel the effects of depletion (Shmueli & Prochaska, 2012; Tice et al., 2007; Tyler & Burns, 2008). All but two studies 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, self-affirmation). The positive videos were between 60 and 90 s long, with an average time of 75 s. Some studies, but not all, matched the length of recovery manipulation to the length of the depletion manipulation.

Comparison With Standard Paradigm

Although this repeated-measures paradigm substantially differs from the traditional two-task depletion paradigm in design, we suspect it still captures the same fundamental phenomenon. The central premise of depletion is that "one act of volition will have a detrimental effect on subsequent volition" (Baumeister, et al., 1998, p. 1252), that initial usage of self-control is followed by poorer self-control. As such, the critical condition is the depletion condition. A non-depletion control condition is necessary as a point of comparison to establish reduced performance, but the precise characteristics of the control condition are not critical to the phenomenon of depletion. The most important characteristic of the control condition is that the control manipulation does not *improve* performance on the subsequent task. A control condition that improves subsequent task performance would invalidate a paradigm, since between-condition comparisons would no longer be solely attributed to depletion weakening performance. On the other hand, a control condition that may *worsen* performance on the second task could not explain a difference between poorer depletion-condition performance and a relatively better control-condition performance, and so would not invalidate depletion findings, although it would bias the depletion effect size (making the effect size appear artificially smaller).

Previous research finds either that positive mood inductions have no effect on subsequent self-control tasks (e.g. Martin & Kerns, 2011) or that, if anything, positive mood inductions can slightly worsen cognitive performance (Clore & Huntsinger, 2007; Forgas, 2013; Pham, 2007). Because we cannot foresee our recovery manipulation *improving* self-control performance, the recovery manipulation cannot account for a depletion effect found in this repeated-measures paradigm. We further suspect that the rejuvenating manipulations in this repeated-measures paradigm would not artificially lower the effect size substantially – previous uses of positive videos to recover from depletion have returned participant performance to be indistinguishable from baseline (Heckman et al., 2012; Tice et al., 2007).

The second difference between the within-subject paradigm and the traditional paradigm is the length of the depleting task. There is no theoretical reason for a minimum length of a task – in fact, manipulating tasks of a similar length (e.g., 1 min of Stroop; Halali, Bereby-Meyer, & Meiran, 2014) have been previously used to induce depletion. Depletion still could be a dose-dependent state, where more previous use of self-control may lead to relatively more depletion, and less previous use of self-control may lead to less depletion. A meta-analysis found only a marginally significant relationship between duration of the depleting task and the magnitude of the depletion effect (Hagger et al., 2010), but both limited resource and motivational theories would likely predict a continuum of depletion's effect size based on the degree of depletion. Even with dose-dependency, the repeated-measures paradigm should still be able to detect depletion, given sufficient statistical power to detect what may be a reduced effect size.

Other than the practical possibility of carry-over effects (see Theoretical Assumptions, above), there is no inherent reason why depletion should not be studied with a repeated-measures design. A phenomenon as potentially ubiquitous as ego depletion should be detectable in a variety of paradigms, including lab-based tasks such as this repeated-measures task, as well as within daily life (e.g., Hofmann, Vohs, & Baumeister, 2012). By using a variety of paradigms, we can help to disentangle the theoretical construct of ego depletion from its traditionally used paradigm, as well as test the boundary conditions of the phenomenon.

General Procedure

Studies were conducted either in-person, with introduction to psychology students, or online with Mechanical Turk workers. Students received course credit or \$10, while M-Turk workers received between \$0.50 and \$2.50, depending on the length of the study.

Power analysis informed our original sample sizes. The within-subject, repeated-measures design require fewer participants; power analysis suggested that to detect a small-medium effect size (d = 0.30, derived from conservative estimates available in 2013), collecting 22 repeated-measures per participant with a within-subject correlation of 0.5 (MacPherson et al., 2014) would result in 80% power with only 36 participants. Note, however, that the obtained within-subject correlations were often substantially lower than expected (actual ICCs from ρ = .00–.67) – because of this, many of the individual studies were underpowered and so results from individual studies should be examined with caution. Meta-analytic results are provided throughout. Studies had between 30 and 143 participants, with an average N = 63. Samples for each study are available in Table 1, along with a summary of the other paradigm variations across studies.

Participants first provided informed consent and gave their current mood and energy levels (on 1–100 slider or 1–12 scale). In some studies, they also initially completed questionnaires, such as the Implicit Theories of Willpower Scale (Job et al., 2010). 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 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 participants were debriefed and compensated.

Analysis

For each study, we first determined if the paradigm influenced people's subjective fatigue levels and mood, by creating a multilevel model for each. We tested whether mood and fatigue were affected by three fixed effects: (i) condition, depleting versus recovery, (ii) block order, to distinguish earlier from later blocks, and (iii) the interaction between condition and block order. Each model had a random intercept for participant. When we found no significant interaction terms, we reconducted the models with only the main effects of block and condition and report those results. Next, we looked to see if the three same paradigm characteristics – condition, block-order, or their interaction – affected the main dependent measure, the self-control task conducted at the end of each block. 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.

We conducted additional analyses where relevant – for example, a separate model tested for moderation by individual difference measures (see Supplemental Materials).

Unstructured multilevel models were done in R, using the "lmer" function from package lme4 version 1.1–10 (Bates, Maechler, Bolker, & Walker, 2015) and "pamer.fnc" from the package LMERConvenienceFunctions, version 2.10 to estimate degrees of freedom (most conservative values used), F-values, and p-values (Tremblay & Ransijn, 2015). Simple effects of interactions (West et al., 1996) were likewise done with *lmer*. Self-reported mood and energy were person-centered, while between-subject variables, like willpower 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_p).

Meta-Analysis

To conduct meta-analyses across studies (Goh, Hall, & Rosenthal, 2016), we analyzed raw data from all 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 preferred when the raw data is 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 self-control dependent variables, the dependent variable was first standardized separately for each study.

Dependent Variables

As seen in Table 1, we used four different behavioral self-control measures as the dependent variables across studies. These short measures were repeatedly administered to participants at the end of each block (between 6 and 22 times), after the recovery or depletion manipulations and the brief self-reported manipulation checks.

Cognitive Estimation Task

Our most common self-control measure was sets of three (or four) Cognitive Estimation Task (CET) questions, averaged to one measure (Bullard et al., 2004). The cognitive

Table 1. Summary characteristics of each study

Study	Participants	*N*	Self-control dependent 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	Undergraduates*	143	CET*	Videos*	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

Notes. The most common dependent variable was the Cognitive Estimation Task (CET; Shallice & Evans, 1978). The effortful add-3 task was based on Kahneman et al. (1969), and the videos were positively valenced with various enjoyable content, often involving music. *Study 6 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, 2018), although only data from the first occasion is analyzed here.

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 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 CET has been previously used as a measure of depletion, where depleted participants have written down unreasonable and extreme estimates more often than non-depleted participants (Schmeichel et al., 2003; Vohs, Baumeister, & Schmeichel, 2012). To score the CET, participants' answers were transformed into absolute *z*-scores based on a distribution of normal answers previously collected from a group of 138 online participants – higher scores on the CET thus indicate poorer performance (see Supplemental Materials).

While the CET has been used less frequently than other depletion measures, it is one of few tasks that can be administered repeatedly without practice effects (MacPherson et al., 2014) and that has been established in the literature (e.g., has been used to detect depletion in more than one publication). Furthermore, a meta-analysis suggests that standardized tests may be the best types of tasks to detect depletion (Carter et al., 2015); while the CET is not an average standardized test, it conceptually relies on similar processes (Schmeichel et al., 2003).

Flanker Task

Study 3 used 2-minute flanker tasks (Eriksen & Eriksen, 1974), 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. Trials may be incongruent (e.g., SSHSS) or congruent (e.g., SSSSS). As our primary dependent measure (used in the meta-analyses), we measured the number of errors on incongruent trials. Results for reaction times are presented in the Electronic Supplementary Material, ESM 1.

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 specifically on high-conflict trials. Importantly, positive mood inductions have been previously shown to not affect performance on the flanker task (Martin & Kerns, 2011).

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; Gordijn, Hindriks, Koomen, Dijksterhuis, & Van Knippenberg, 2004). 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-second increments (see ESM 1 for details, and for time-on-task results).

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?".

The anchoring effect has not previously been used as a depletion dependent variable, but there is reason to think anchoring should be affected by ego depletion. Other heuristic biases are strengthened when people are depleted (Pocheptsova, Amir, Dhar, & Baumeister, 2008) or under cognitive load (Frederick, 2005). Anchoring, however, seems to work opposite from other heuristics – manipulations that strengthen other heuristic biases instead *decrease* the anchoring effect (Bodenhausen, Gabriel, & Lineberger, 2000; Englich & Soder, 2009). Anchoring seems to rely on the deliberative process of recalling anchor-relevant information; when someone spends more time thinking about the anchor value, the anchor value has more and more influence on one's final estimate (Bodenhausen et al., 2000; Strack & Mussweiler, 1997). In other words, weakening deliberative processing seems to decrease the anchoring effect. Given that depletion decreases deliberative processes (Pocheptsova et al., 2008), depletion should decrease the anchoring effect. Against intuitions, then, we thus hypothesized that ego 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).

Because the anchoring task has not previously been used as a measure of ego depletion, studies using this dependent measure (Studies 4 and 7) should not be used to validate the repeated-measures paradigm. However, as shown below, the results of Study 4 closely parallel both the meta-analytic results and the results of studies using the more commonly-used flanker and solvable anagram tasks (Studies 3 and 5).

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 [1–100 scale; $B = 6.85$, $t(4{,}847) = 16.62$, $p < .0001$]. Participants also reported being in a more unpleasant mood after the depleting manipulation compared to after the recovery manipulation [$B = 7.79$, $t(4{,}852) = 17.76$, $p < .0001$]. 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 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 s answering easy survey questions (such as their number of siblings, number of bedrooms, 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.

Participants also consistently reported increasing fatigue and worsening 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 (answering easy survey questions; Table 2). Regardless of the content of the paradigm, participating in the experiment increased subjective fatigue and decreased subjective mood across time.

Effects on Self-Control Measures

Depletion Condition Main Effects

Seven studies contrasted the add-3 depleting manipulation against the video recovery manipulations, using the CET, flanker task, anagram task, or anchoring task to measure self-control. Of these, only Study 8a (CET) found poorer performance on depletion blocks compared to recovery blocks, at the midpoint of the study (e.g., at the average block). Study 1 and Study 2 (both CET) found marginal effects in the same direction ($B = .07$ and $.05$ z-scores, $p = .06$ and $.03$). None of these other studies had significant main effects of condition at the midpoint (ps > .14), although each condition effect was in the predicted direction (see Table 3).

When we analyzed all studies collectively (as a three-level hierarchical model), there was a small significant condition effect at study midpoints, where the depletion

Table 2. Effects of depletion condition and block on subjective energy and mood

	Condition effect (Depletion vs. Recovery)				Block effect (Across Experiment)			
	Energy		Mood		Energy		Mood	
Study	*B* (*SE*)	R^2 [95% CI]	*B* (*SE*)	R^2 [95% CI]	*B* (*SE*)	R^2 [95% CI]	*B* (*SE*)	R^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 (0.12)	.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 [.042, .067]**	**7.80 (0.44)**	**.061 [.049, .074]**	**−0.89 (0.05)**	**.057 [.045, .069]**	**−0.94 (0.05)**	**.057 [.045, .070]**
	(Depletion vs. Neutral)							
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]
	(Recovery vs. Neutral)							
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]

Notes. All effects are significant at $\alpha < .01$ except for when noted with *ns* (nonsignificant) or with *(.01 < *p* < .05). Non-standardized beta and standard error are based on a 1–100 slider scale. The "All" row (bolded) calculates effects using all raw data from the preceding 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.

condition was associated with generally poorer self-control compared to the recovery condition [$t(4,866) = 2.78$, $p = .005$; 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, to $d = .045$, although the condition effect continued to be meta-analytically significant at study midpoints [$t(5,713) = 2.07$, $p = .038$; Table 3]. The effectiveness of the condition manipulations, however, was moderated by significant condition by block interactions (below; Figure 2).

Block Effects and Interactions with Block

In many studies, 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 block, $F(1, 623) = 36.88$, $p < .001$], presumably due to either boredom or fatigue accumulating without being sufficiently counteracted by the recovery videos. On the other hand, Study 5 participants improved on the anagram task as the experiment progressed [$B = .11$ additional solutions per block, $F(1, 1{,}076) = 21.92$, $p < .001$], 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 that contrasted add-3 with video blocks, $t(4,855) = 1.92$, $p = .055$, and a statistically significant but extremely small block effect across the nine studies, $t(5,713) = 2.01$, $p = .044$, where performance generally worsened across the course of the experiments.

Unfortunately, these inconsistent block effects did not 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 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, $B = .105$, $SE = 0.034$, $t(5,817) = 3.05$, $p < .001$] but was weakened as the study progressed [by block six, $B = .045$, $SE = 0.022$, $t(5,817) = 2.04$, $p = .04$]. In every case – even in Study 8a, where the depletion effect was only significant at the end of the study, instead of at the beginning – 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. In general, the condition manipulations were more effective earlier in the

Table 3. Depletion condition and block effects on self-control task performance

	Effect of condition (depletion effect) at study midpoint		Block effect (mean performance change per block)		Condition by block interaction		ICC
Study (dependent variable [DV])	*B* (*SE*)	R^2	*B* (*SE*)	R^2	*B* (*SE*)	R^2	ρ
1 (CET)	0.117 (0.078)†	.003	0.021 (0.006)***	.015	−0.006 (.012)^{ns}	.000	.158
2 (CET)	0.056 (0.029)†	.005	−0.002 (0.004)^{ns}	.000	−0.024 (0.008)**	.011	.172
6 (CET)	−0.077 (0.093)^{ns}	.001	0.095 (0.026)***	.018	−0.211 (0.052)***	.022	.091
8a (CET)	0.123 (0.050)*	.020	0.060 (0.011)***	.096	0.073 (0.021)**	.037	.000
Above 4 CET	**0.042 (0.036)^{ns}**	**.001**	**0.021 (0.004)****	**.009**	**−0.018 (0.004)†**	**.002**	**.129**
3 (Flanker)	0.007 (0.054)^{ns}	.000	0.062 (0.010)***	.055	−0.022 (0.010)*	.007	.670
4 (Anchor)	0.037 (0.021)†	.005	−0.011 (0.006)†	.005	0.014 (0.006)*	.008	.002
5 (Anagrams)	0.053 (0.087)^{ns}	.000	−0.113 (0.019)***	.030	−0.087 (0.019)***	.018	.560
Above 7 Studies (*z*-score DVs)	**0.065 (0.023)***	**.002**	**−0.0052 (0.0027)†**	**.000**	**−0.027 (0.005)*****	**.004**	**.251**
9 (CET, restricted write)	0.024 (0.044)^{ns}	.001	0.000 (0.010)^{ns}	.000	0.038 (0.019)^{ns}	.009	.081
10 (CET, self-affirmation)	−0.156 (0.069)*	.012	0.011 (0.015)^{ns}	.002	0.005 (0.030)^{ns}	.000	.325
Above 9 Studies (*z*-score DVs)	**0.045 (0.022)***	**.001**	**−0.0054 (0.0027)***	**.000**	**−0.027 (0.005)*****	**.003**	**.243**

Notes. For *B* values, positive values correspond to worse performance on the depletion blocks, or worsening performance across the course of the study. Aggregated results (bolded) are for (i) the four Cognitive Estimation Task (CET) 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 Studies 9 (restricted writing) and 10 (self-affirmation). 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. Interclass correlations (ICCs) state the proportion of variance explained by the "participant" random factor. ***$p < .001$; **$p < .01$; *$p < .05$; $^{\dagger}p < .10$.

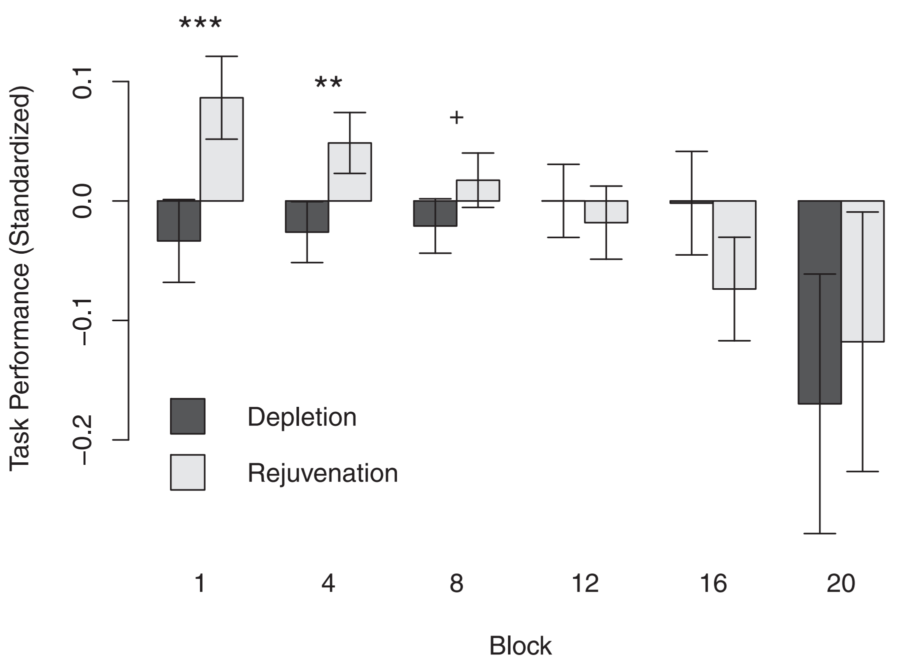

Figure 2. The across-study effect of condition on the performance dependent variable (standardized) is shown for every fourth block. The effect of condition decreased across the experiment (as blocks progressed) – although the depletion effect was evident at the beginning of the paradigm, it was no longer 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* = 5,715, Block 20 *df* = 731). ***$p < .001$; **$p < .01$; $^{\dagger}p < .10$.

experiment (meta-analytic effect size for depletion of d = 0.12 at the first block), and faded as the study progressed (e.g., d = 0.07 by block 4 and d = 0.04 by block 6; Figure 2). The progressive weakening of the condition effect largely negated any potential benefit of having multiple repeated-measures blocks.

No Mediation by Self-Reported Fatigue or Mood

Could self-reported energy or mood mediate the small effect of condition (depletion) on self-control task performance? Self-reported fatigue was generally not related to performance on the self-control measures. Across all studies, fatigue was only significantly correlated with task performance in one study, Study 7a, which contrasted the depletion condition with the neutral condition, instead of the recovery condition, $F(206) = 7.27$, $p = .01$. All other studies had no relationship between self-reported energy and the self-control dependent variables ($p > .11$). In an analysis with all nine datasets, neither self-reported fatigue nor self-reported mood was associated with the standardized self-control dependent variables (fatigue: $t(4{,}694) = .87$, $p = .38$; mood: $t(4{,}694) = 1.23$, $p = .22$). The lack of relationship between self-reported fatigue and task performance 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 possible conflation between the effects of the depleting manipulation and the effects of the recovery manipulation. To address this, Study 7 and Study 8b employed a neutral manipulation, where participants spent 90 s answering basic questions about themselves (such as age, number of siblings, favorite color) or doing easy sorting tasks (e.g., sorting words based on capitalization). Half of participants alternated between the neutral manipulation blocks and the depleting add-3 manipulation blocks (Study 7a), while the other half of participants alternated between the neutral blocks and the recovery video blocks (Study 7b). Study 7 used the anchoring task as the dependent variable, while Study 8b used the CET.

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 showed significantly different degrees of anchoring bias based on condition – the neutral condition had the largest anchoring effect (most deliberative processing), while the recovery condition, like a positive mood induction, had a smaller anchoring bias [less deliberative processing; $F(1, 187) = 5.04$, $p = .03$]. Others have also found positive mood inductions to decrease the anchoring bias, relative to sadness inductions (Bodenhausen et al., 2000; Englich & Soder, 2009). Across studies, then, the anchoring effect was largest in the neutral condition, followed by the recovery condition, and then by the depletion condition. Thus, the condition effect of Study 4 was likely weakened by the effect of the recovery condition on mood; the depletion effect of Study 4 cannot be explained for by the recovery manipulation's effect on anchoring.

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 suggests 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 underpowered.

Different Depleting and Recovery Manipulations

In two studies, we changed the content of either the recovery or depleting manipulation, while continuing to use the CET as the dependent variable. Study 9 used a 90-second restricted writing task as the depleting manipulation, instead of the add-3 task (Schmeichel, 2007). For each of the four depletion blocks, the topic and the disallowed letters varied (e.g., describe your room, describe cooking a meal; without using O and S, or T and I). Study 9 still resulted in significant changes to self-reported mood and energy, comparable to other studies (Table 2); having just engaged in restricted writing was associated with significantly more fatigue and unpleasantness, compared to after watching 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, as previously found by Schmeichel & Vohs (2009). At the beginning of the study, participants ranked 11 values according to their personal importance. The most important four values for each participant were each inserted into a self-affirmation exercise, where participants wrote about the specified value for 90 s. This study evoked the same change in self-reported mood and energy as the other studies (Table 2). However, the self-affirmation manipulation worsened people's performance on the CET task, compared to the add-3 task, $F(1, 416) = 5.90$, $p = .02$, $\eta^2_p = .014$. 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 ($p = .84$).

The paradigm continued to affect self-reported mood and energy, regardless of the content of the depletion or recovery manipulations. However, neither alternative manipulation resulted in the expected changes to the CET dependent variable. In fact, brief self-affirmation manipulations slightly hindered people's performance on the subsequent CET task. The ineffectiveness of self-affirmation as a recovery activity is consistent with some previous research: Shmueli & Prochaska (2012) found that positive videos successfully restored people's self-control, while positive writing activities did not. Furthermore, self-affirmation has sometimes resulted in poorer performance and task disengagement, particularly when self-efficacy on a task is low (Vohs, Park, & Schmeichel, 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, but these manipulations did not reliably change performance on the subsequent self-control behavioral measures. Although the depletion effect was meta-analytically significant, the effect size was very small ($d = .045$). This effect size may be consistent with some estimates of depletion effect size in the standard between-subject paradigm (Carter et al., 2015; Hagger et al., 2016; Tuk et al., 2015), but may also not be practically meaningful. This small effect size may not generalize to other depletion paradigms, however the degree of depletion might vary depending on the duration of the previous self-control task, and this effect size was created after exerting self-control for only 90 s.

The paradigm did seem to create a small depletion effect, although depletion was only present at the beginning of the experiments before the accumulation of practice, boredom, or fatigue effects. In four studies, as well as meta-analytically, the intended depletion effect was statistically significant early in the experiment (at block 1, $d = .12$), but faded with time (Figure 2). 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 resistant to boredom and practice effects may be difficult, if not impossible.

Effects on Self-Reported State

Effects on self-reported state were robust across studies. Participants always reported feeling more unpleasant and more fatigued after depleting manipulations, compared to after recovery manipulations. These changes in subjective state are as expected, and might be exacerbated by within-subject demand effects – participants are presumably aware of the purpose of these manipulations and respond to the contrast between conditions. Second, subjective state decreases equally robustly across the course of the experiment, demonstrating that fatigue and negative feelings (perhaps including boredom; Milyavskaya et al., 2018) also accumulate throughout the experiment; perhaps the recovery manipulations, the short enjoyable videos, do not fully return participants to baseline. Promisingly, these changes in subjective state are moderated by willpower theories and self-reported enjoyment of the videos in predicted directions (see Supplemental Materials: 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 behavioral performance, as commonly found in emotion research (Mauss, Levenson, McCarter, Wilhelm, & Gross, 2005) and in the standard depletion paradigm (e.g., Boucher & Kofos, 2012; Hagger et al., 2016).

Spillover Effects

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-lab sessions) all had significant block effects, while only two of the six shorter online studies had significant block effects. The existence of these block effects generally speaks against the use of within-subject, repeated-measures designs. Even after statistically controlling for block effects, these practice effects or accumulating fatigue effects may overwhelm any intended depletion effects.

Statistical Power of the Repeated-Measures Paradigm

How was power affected in this repeated-measures paradigm? The statistical power of repeated-measures designs is affected by three things: the number of observations, the true effect size, and the degree of within-subject correlation. First, designs generally have more power with more observations, either from more repeated measures or more participants. Although many of the above studies did have large numbers of repeated-measures (from 4 to 22 blocks), the interactions with practice or fatigue effects across the experiment meant that only a smaller number of these observations (those blocks at the beginning of the experiments) were useful in detecting a depletion effect.

Second, statistical power relies on the true size of the studied effect. In this particular design of a within-subject paradigm, the true depletion effect size may have 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 min. 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.

Third, much of the power advantage of repeated-measures designs is due to statistically accounting for within-subject variation. When a measurement has high reliability within-person, relatively more variation can be explained by knowing the participant, which increases the power to detect the effects of interest. In our *a priori* power analysis, we assumed a within-subject correlation of 0.50. In actuality, within-subject correlations varied substantially between different dependent variables (from $\rho = .00$ to .67; Table 3), while the flanker and anagram tasks had high within-subject correlations, the CET and anchoring tasks had much lower within-subject correlations. Because of this difference, even if the true depletion effect size was 0.30, Study 1 would have only had 52% power (with ICC = 0.1) instead of the 80% power calculated *a priori* (with ICC = 0.5). The lower within-subject correlations, and thus lower statistical power, may be one reason why studies using the CET and anchoring tasks rarely found significant depletion effects. Overall, while there may be increased power inherent to some repeated-measures design, this did not equate to more reliable results or even necessarily more statistical power in this particular design, particularly for the tasks using the CET or anchoring tasks as dependent variables.

While statistical power was ultimately lower than anticipated for individual studies, the meta-analytic results from this paradigm are still informative (Goh et al., 2016). There was reasonable consistency across the studies – the three most highly powered studies (Studies 3, 4, and 6) all had the same significant condition-by-block interaction, which mirrored the meta-analytic interaction. Two of those more highly powered studies (3 and 4) also used common self-control measures, the flanker task and solvable anagrams. The meta-analytic results are thus not representative of only the results from any individual study, nor only of any single dependent variable.

Methodological Contribution

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. The paradigm may have been less statistically powerful than anticipated, due to smaller effect sizes and lower within-subject correlations for some dependent variables. 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-measures designs to study the basic ego depletion effect, at least not without substantial modification and improvement.

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 use within-subject designs when looking at time-of-day effects (Dai, Milkman, Hofmann, & Staats, 2015; Randles, Harlow, & Inzlicht, 2017) or using experience sampling methods (Hofmann et al., 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 across two sessions completed one week apart (cf. Wenzel, Lind, Rowland, Zahn, & Kubiak, 2016). 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.

Depletion may further depend on the length of the prior depleting manipulation. Especially if depletion is a dose-dependent state, experiments with longer and more intensive depletion manipulations may have larger effect sizes and be more statistically powerful. Indeed, some studies have found effects of depletion after long periods of self-control use, but not after shorter periods (Blain et al., 2016). While both this repeated-measures paradigm and other past work do find that short manipulations may create some depletion, longer manipulations and depletion manipulations are likely to be more reliable.

Researchers may also consider predictions from motivational models of self-control when designing their experiments. For example, whether an activity results in 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 the control condition (e.g., an easier e-crossing task) are externally motivated, and may both equally remove participants' feelings of autonomy. If a lack of autonomy is critical for depletion, then we would predict no difference between conditions in subsequent self-control performance. Other evidence suggests that boredom may cause decrease in performance (Milyavskaya et al., 2018), as people become motivated to do something meaningful instead of seemingly pointless experimental tasks (Bench & Lench, 2013) – thus, there may be a smaller depletion effect when the control condition is seen as boring. Ensuring a sufficient contrast between the depletion and control conditions is likely one way to increase the effect size of depletion, thereby increasing statistical power without having to increase sample sizes.

Conclusion

Brief depleting and recovery manipulations consistently affect self-reported measures of fatigue and mood, but only affect performance on behavioral measures of self-control occasionally, with a very small, though statistically significant, meta-analytic effect that might not be practically meaningful. While this repeated-measures design had the potential to increase statistical power, due to its within-subject design and large number of observations, this potential was not realized partially because of problems with demand effects, carry-over effects, low within-subject correlations for some dependent measures, and conflation between depletion and mood manipulations. Future research should continue to pursue novel and within-subject methods to study depletion and fatigue effects, but should consider the length and intensity of depletion manipulations.

Open Data/Materials

Additional results, materials, analysis code, and data are available online at https://osf.io/rh2gv/

Author Contributions

Zoë Francis, Marina Milyavskaya, and Michael Inzlicht designed the studies; Zoë Francis and Hause Lin collected data; Zoë Francis and Marina Milyavskaya conducted analyses; Zoë Francis drafted the original manuscript; all authors edited and approved of the final manuscript.

Electronic Supplementary Materials

The electronic supplementary material is available with the online version of the article at https://doi.org/10.1027/1864-9335/a000348

ESM 1. Text (.docx)
Additional methods and results.

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