Does Willpower Mindset Really Moderate the Ego-Depletion Effect?:

A Preregistered Direct Replication of Job, Dweck, and Walton's (2010) Study 1

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Abstract

This article reports a preregistered study in which we attempted to directly replicate an influential study on the ego-depletion effect conducted by Job, Dweck, and Walton (2010, Study 1, $N=60$). As in the original study, participants ($N=187$) performed a self-control task (Stroop color-word interference task) after performing the control or depletion version of a letter-cancellation task. Despite extensive analyses, we failed to reproduce Job et al.'s (2010) key findings: (a) a significant main effect of ego depletion (i.e., worse Stroop performance in the depletion condition than in the control condition) and (b) a significant moderation of this ego-depletion effect by individual differences in willpower mindset (i.e., primarily individuals holding the belief that willpower is limited demonstrate the ego-depletion effect). These results suggest that the willpower-mindset moderation effect proposed by Job et al. (2010) may not be as robust as initially suggested or may be applicable under more circumscribed situations.

Keywords: ego depletion; willpower mindset; self-control; direct replication; open science

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Since its initial demonstration (Baumeister, Bratlavsky, Muraven, & Tice, 1998), the ego-depletion effect—a decrement in self-control performance subsequent to performing another self-control task—has become widely known not only among researchers but also among the general public (Baumeister & Tierney, 2011). A meta-analysis published less than a decade ago (Hagger, Wood, Stiff, & Chatzisarantis, 2010) reported a moderate effect size for this effect ($d=.62$), suggesting that it is a robust phenomenon.

The last several years, however, have witnessed major challenges to this effect on both conceptual and empirical grounds (e.g., Friese, Loschelder, Gieseler, Frankenbach, & Inzlicht, 2018; Lurquin & Miyake, 2017), especially questioning its replicability. First, more recent meta-analyses suggested that, due to the well-known publication bias and questionable research practices such as *p*-hacking (Wolff, Baumann, & Englert, 2018), the actual effect size might be much smaller (Carter, Kofler, Forster, & McCullough, 2015) or even nonexistent (Carter & McCullough, 2014). Some replication failures have also started to appear in peer-reviewed journals (e.g., Lurquin et al., 2016; Xu et al., 2014). Most notably, a recent preregistered study of the ego-depletion effect involving 23 laboratories ($N=2,141$) revealed an overall effect-size estimate of only $d=.04$ (Hagger et al., 2016).

This article reports a new preregistered study of an influential finding about the ego-depletion effect reported by Job, Dweck, and Walton (2010, Study 1; hereafter, *JDW2010*). In this study, 60 participants completed a questionnaire assessing their implicit theories about whether willpower is limited or not (hereafter, *willpower mindset*). They were then randomly assigned to the depletion or control condition and performed the respective versions of a letter-

cancelation task. As the outcome task, participants performed a manual-response version of the color-word Stroop task. JDW2010 reported two key findings: (a) a significant overall ego-depletion effect (more Stroop errors in the depletion condition) and (b) a significant moderation of this effect by willpower mindset (only individuals with a limited-willpower mindset demonstrated depletion).

This study is worthy of a rigorous direct replication for several reasons. First, it is one of the most influential studies on ego depletion (764 citations on Google Scholar on 10/24/2018). Second, Job and her colleagues' subsequent studies have demonstrated that the influence of willpower mindset on self-control might have even broader consequences (Francis & Job, 2018). Perhaps most remarkably, Savani and Job (2017) reported that Indian participants, who as a group believe that exerting self-control is energizing, even exhibit a reverse ego-depletion effect (exerting self-control *improves* subsequent self-control performance).

Theoretically, JDW2010's finding challenges a dominant account of the ego-depletion effect, the strength model of self-control (Baumeister, Vohs, & Tice, 2007). This model postulates that self-control relies upon a finite domain-general resource and that exerting self-control depletes this resource supply, thereby impairing subsequent self-control performance. JDW2010's work suggests that one's willpower mindset, not the limited supply of self-control resources per se, may be the more critical variable underlying the ego-depletion effect.

Before future research can be firmly built upon JDW2010's novel findings, however, it is essential to establish its replicability. As articulated by Simons (2014), "direct replication by other researchers is the best (and possibly the only) believable evidence for the reliability of an effect" (p. 76). Such direct replication efforts are especially needed here, because, as will be noted later, JDW2010 analyzed the Stroop data unconventionally. Specifically, JDW2010

conducted a multilevel (HLM) logistic-regression analysis on the accuracy of an individual trial on several variables, including some theoretically-unmotivated covariates like age and trial number, possibly without properly coding participants as a random variable (Schimmack, 2016). It thus seemed important to establish that JDW2010's findings are not tied to the way the data were originally analyzed.

We conducted a preregistered direct replication of JDW2010's Study 1. Although we made some changes to strengthen the study (e.g., administering manipulation checks, using a vocal-response version of Stroop, adding a second extension block for Stroop), these procedural changes were expected to be minor enough to not substantially alter the basic pattern of results. Like JDW2010, we tested two primary hypotheses: (a) the overall ego-depletion effect should be significant and (b) this effect should be moderated by willpower mindset. We also evaluated whether a likely correlate of willpower mindset—trait self-control—would demonstrate a similar moderating influence.

Method

We report below how we determined our sample size, all data exclusions, and all manipulations and measures included in this study. The preregistration document is available on the Open Science Framework (OSF) at [xxx].¹ Where relevant, we note any deviations from the preregistered procedures and data-analytic plans (for a summary of those deviations, see **Section 1** of the supplement). In addition, all relevant experimental materials, data files, and data-analysis scripts (R codes) are also available at the project OSF site.

Participants

A total of 194 students participated in the study ($n=97$ in each condition). They were

¹ This private view-only link is currently made available to the editor and reviewers only. The link will be replaced with a public link upon acceptance of the manuscript.

recruited from the human-subject pool of the Department of Psychology and Neuroscience at the University of Colorado Boulder and either received partial course credit or payment for their participation. Participants were randomly assigned to either the control or depletion condition. Prior to the study, we created a randomized list of condition assignments for the first 50 participants and repeated this step for each subsequent 50 participants.

Of the 194 participants, seven met one of the preregistered exclusion criteria and hence were excluded from the analyses:

- not finishing the experiment for various reasons, such as (a) an experimenter ending a session early when a participant failed to follow instructions ($n=1$ in the control condition), (b) computer crashes occurring in the middle of the main Stroop task ($n=1$ in the control condition and $n=3$ in the depletion condition), and (c) participants not completing key portions of questionnaires ($n=2$ in the depletion condition); and,
- being a nonnative English speaker ($n=0$)

Thus, the final sample consisted of 187 participants: $n=95$ in the control condition (60 women) and $n=92$ in the depletion condition (42 women).

Our preregistration document also included three additional exclusion criteria: (a) missing more than one “catch” item included in the questionnaire survey; (b) correctly guessing at least one of the primary hypotheses of the study; and (c) being outside the range of 18 and 30 years old (an exclusion criterion used in Hagger et al.’s (2016) multilab study, in which Carruth and Miyake participated). We did not apply any of these exclusion criteria to our dataset for the following reasons.

The first exclusion criterion was irrelevant, because we ended up not including any catch items in the questionnaires. Second, we decided against applying the second criterion because it was difficult to definitively determine who guessed the study hypotheses correctly. Because of the questionnaires and manipulation-check ratings administered in this study, a fair number of participants, not surprisingly, mentioned concepts like “self-control,” “willpower,” “fatigue,” and

“motivation” in their descriptions of the study purpose. No participants, however, were aware of two experimental conditions (depletion vs. control) or raised the possibility of the study testing the moderating influence of willpower mindset on Stroop. For this reason, we decided not to exclude any participant’s data on the basis of this criterion.

Finally, we decided against applying the age-based exclusion after learning that four participants chose not to provide their age (all in the depletion condition) and that three participants outside this age range were close to the upper-cutoff age (31 years old, $n=2$; 32 years old, $n=1$; all in the control condition). These seven participants would have to be excluded according to the preregistered plan, but their data were otherwise complete. Moreover, there is no reason to think that 31/32-year-old individuals would be radically different from 30-year-old individuals. Thus, we opted to include these seven participants’ data in the analysis. Statistical conclusions remained the same, however, even if those participants were excluded ($N=180$; $n=92$ in the control condition and $n=88$ in the depletion condition).

Sample Size and Power Considerations

The original JDW2010 study used 30 participants per condition ($N=60$). Because of the way the data were analyzed and reported in that study, however, it was not possible to derive effect-size estimates for the main effect of depletion or the moderation effect. Also, at the time of the preregistration (10/18/2016), there were no other published studies that administered the same willpower-mindset measure and reported its moderating influence on the ego-depletion effect. Thus, as specified in the preregistration, we decided to follow what we had done before in our previous preregistered ego-depletion study (Lurquin et al., 2016) and collect data from 100 participants in each condition.

Because we fell just short of this sample-size goal ($N=187$), we also conducted post-hoc

power analyses using G*Power for the primary preregistered analyses (referred to as the fixed-effects modeling below). With the current sample size, the effect size for the main effect of ego depletion we could detect with 80% power was $d=.365$. Similarly, the effect size for the hypothesized moderation effect (i.e., an interaction between willpower mindset x depletion manipulation) with 80% power was $f^2=.042$, which corresponds to $d=.412$.

Materials and Procedure

Carruth (the lead author of this article) and undergraduate research assistants ran experimental sessions, and they all closely followed an experimenter procedural script. With the exception of Carruth, the experimenters were all blind to the hypotheses of the study.

Participants were tested individually in a quiet room, and the testing session lasted 45–50 min. As we have done before (Hagger et al., 2016; Lurquin et al., 2016), the experimenter was present in the room during the entire session and sat discretely on the opposing side of a filing cabinet (blocking any view of the participant) while the participant completed the questionnaires and the letter-cancellation task. During the Stroop task, however, the experimenter sat behind the participant so that they could record error trials on a scoring sheet.

The overall procedure for the current study is schematically illustrated in **Figure S1** in the supplement. Although the procedure followed that used by JDW2010, we provide this figure to make clear the time course of administering manipulation-check measures and different types of trials across blocks.

The questionnaires and manipulation-check ratings were administered via Qualtrics. The Stroop task (both baseline and main blocks) were programmed with the PsyScope software (Version 1.2.5) and were run on an Apple computer (Mac OS X 10.6.4), using a button box capable of serving as a voice key.

Questionnaires. Participants first completed two questionnaires: (a) the Beliefs about the Willpower Scale (JDW2010) and (b) the full 36-item version of the Self-Control Scale (Tangney, Baumeister, & Boone, 2004). We had originally planned to include another measure (the Sensitivity to Ego-Depletion scale), but we dropped it to minimize redundancy.

The Beliefs about the Willpower Scale consisted of two subscales: (a) beliefs about strenuous mental activities and (b) beliefs about resisting temptation. Although the original study administered only the first subscale, we administered both subscales because one study reported by JDW2010 (Study 4) used the full scale. The Self-Control Scale, a popular trait self-control measure, was included to examine whether JDW2010's moderation finding may have been driven by a likely correlate of willpower mindset such as trait self-control. Although JDW2010 indicated (in Footnote 4 on p. 1693) that their willpower-mindset measure did not correlate with trait self-control ($r=.17, p>.20$) in their pilot study ($N=65$), it seemed important to evaluate this possibility in a more appropriately sized sample for correlational analysis.

The two questionnaires were combined into a single survey, and the order of item presentation was randomized for each participant. For both questionnaires, participants rated each item on a 7-point Likert scale (1=*strongly disagree*; 7=*strongly agree*). We computed the average rating for each questionnaire and used it as a measure of willpower mindset and of trait self-control, respectively. A higher rating meant a stronger belief that willpower is *nonlimited* or a *higher* level of trait self-control. To avoid a potential problem of confusing participants by using inconsistent rating scales for the two questionnaires, we reversed the rating scale for the willpower-mindset questionnaire so that 1 meant *strongly disagree* and 7 *strongly agree* for both questionnaires. We also changed the rating scale for willpower mindset from a 6-point scale to a 7-point scale to make it comparable to that of the trait self-control measure (1 meant *strongly*

agree and 6 *strongly disagree* in the original willpower-mindset scale). The reliabilities of these measures (Cronbach's α) were all satisfactory: .85 (the first subscale) and .77 (the entire scale) for willpower mindset and .89 for trait self-control.

Baseline Stroop. Before the depletion manipulation, participants completed two brief Stroop trial blocks as baseline measures (not administered in the JDW2010 study). We administered baseline blocks early to familiarize participants with the primary requirement of the Stroop task (saying aloud the color of each stimulus) and to minimize the lag between the two primary tasks (i.e., no need to provide detailed instructions after the administration of the depletion manipulation). Additionally, the experimenter used the baseline blocks to calibrate the sensitivity of the voice key and make necessary adjustments to the microphone.

In the first baseline block (Block 1), strings of 3–5 asterisks (**, ***, or ****) were presented in red, blue, or green on a computer screen in a randomized order. Participants were asked to say aloud the color in which each string of asterisks was presented. There was an initial set of 5 practice/calibration trials before moving on to 22 real baseline trials. In the second baseline block (Block 2), participants received 22 real trials of all congruent stimuli, where the word (*RED*, *BLUE*, or *GREEN*) matched the color it was presented in (e.g., *BLUE* presented in blue). Again, participants were asked to name the color that each word was presented in. Because these short baseline blocks did not include any incongruent trials, they should not have depleted the hypothesized self-control resources.

In all of the Stroop blocks (both baseline and main Stroop blocks), each trial began with a fixation cross (+), presented for 250 ms before the target stimulus appeared. The participant's vocal response to the stimulus terminated its presentation. A fixation cross signaling the beginning of the next trial appeared 750 ms after the participant's vocal response. This cycle

continued until a brief rest period inserted in between blocks was reached.

The RTs were recorded by the program. Using a scoring sheet listing all the correct responses for the entire Stroop trials, the experimenter scored the accuracy of each response as well as noting any voice-key error trials (e.g., trials on which the participant coughed or made some other noise, thus accidentally triggering the start of a subsequent trial).

Depletion manipulation: Letter-cancelation task. The task materials and the script for the letter-cancelation task were obtained from V. Job. We used the same text and instructions as JDW2010, with one change noted below.

The letter-cancelation task consisted of two blocks. In Block 1, participants in both conditions did exactly the same: simply crossing off letter *e*'s on a page of text with a pen. In this “habit-forming” block, participants received the following written instructions:

In this experiment, we are examining factors involved in stimulus identification and detection. The following page contains printed text. In the printed text, we want you to cross out selected text using the following rule: *cross out each letter “e” that appears in the text.*

After reading these instructions, the participant responded to a motivation question, *How motivated are you to do well on this task?*, on a 9-point scale (1=*not at all* to 9=*very*). Following this, the experimenter instructed participants to begin working on the task, started a timer, and then instructed them to stop working after 5 min. The experimenter then administered the following manipulation-check questions, to which participants responded on the same 9-point scale (the underline/italics was used to highlight the dimension inquired by each question):

- How effortful did you find the task?
- How tired do you feel after doing the task?
- How difficult did you find the task?
- How boring did you find the task?
- How frustrated were you while you were doing the task?
- How much effort did you put into the task?

In Block 2, participants in the control condition were given instructions identical to Block 1 and were provided with a fresh page of text to work through (as with JDW2010, the text used for Block 2 was the same as that used for Block 1). In the depletion condition, however, participants received the following more complex instructions:

In the printed text on the following page, we want you to cross out selected text using the following rule: *cross out each letter “e” that appears in the text EXCEPT:*

- *when another vowel (A, E, I, O, or U) follows the “e” in the same word (example: “READ”)* or
- *when a vowel is one letter removed from the letter “e” in either direction in the same word (example: “VOWEL”).*

After the experimenter made sure that participants understood the task requirements for Block 2, participants in both conditions were asked the same motivation question and then worked on Block 2 of the letter-cancelation task for 5 min. After Block 2’s completion, the experimenter provided participants with the same manipulation-check questions administered after Block 1.

We note here that, in the original JDW2010 study, the page of text given to the depletion condition in Block 2 differed from that used in the control condition in visual appearance. Specifically, the text itself was blurred in the depletion condition (but not in the control condition). The rationale for this intentional experimental confounding was not stated in the original study, but we initially interpreted the purpose of this blurring as increasing perceptual demands of some sort in the depletion condition. As explicitly stated by JDW2010, however, the goal of this letter-cancelation task was to “establish a behavioral pattern” in Block 1 and then to require participants in the depletion condition to “inhibit the previously established response” (p. 1687, JDW2010; see also Baumeister & Vohs, 2016, for the same argument). Given this stated purpose, we decided that using a blurred text in the depletion condition was not an integral component of this task (i.e., perceptual degradation of the text would unlikely increase the hypothesized self-control demand of “inhibiting the previously established response”). For this

reason, we opted for not using a blurred version of text in the depletion condition and thereby avoiding an unnecessary experimental confound.

Outcome measure: Main Stroop task. Participants next completed the main Stroop task, which, like the earlier baseline task, also consisted of two blocks. First, as in JDW2010, participants completed a block of mixed congruent and incongruent trials. This replication block (Block 1) was longer than that in the original JDW2010 study (24 congruent and 24 incongruent trials) and consisted of two subblocks of 40 trials each for a total of 80 trials (40 congruent and 40 incongruent trials), with a brief break in between. We increased the number of Stroop trials because the Stroop interference effect is highly susceptible to occasional long RTs, especially at the level of individual participant means. After receiving a brief reminder emphasizing the importance of naming the color and performing 6 practice trials, participants responded to the same motivation question before receiving the real experimental trials.

Of importance, as part of the planned deviations from JDW2010's original procedure, we added a second block of mixed incongruent and neutral (asterisk) trials (Block 2). Again, this extension block consisted of two subblocks of 40 trials each (for a total of 80 trials, split evenly between neutral and incongruent trials). Other than using a different mixture of trial types, the trials were set up in the same way as in Block 1.

We added this extension block because prior research has suggested that the 50/50 mixture of congruent and incongruent trials (as was done in the original study) has been known to elicit a phenomenon known as goal neglect (Kane & Engle, 2003; see also Dang, Björklund, , & Bäckström, 2017), a temporary loss of the task goal (i.e., color naming) from working memory due to the abundance of cues supporting an inappropriate task goal (in this case, word reading, due to the large number of congruent trials for which word reading suffices). In contrast, a 50/50

mixture of incongruent and neutral (asterisk) trials used in Block 2 would make the Stroop task more appropriate as a measure of overriding dominant yet inappropriate responses.

After the completion of Block 2 for the Stroop task, participants responded to the same self-rating questions administered at the end of both blocks of the letter-cancelation task (*effortful, tired*, etc.), although those post-Stroop ratings were not of primary interest. Finally, participants responded to a single open-ended question asking what they thought might be the purpose of the study and provided demographic information (age and gender) before they were debriefed and dismissed.

Data-Exclusion and Data-Transformation Considerations

For the analyses of the baseline and main Stroop data, the calculation of error rates (%) excluded voice-key error trials noted by the experimenter. The analyses of the RT data excluded voice-key error trials as well as actual error trials. Additionally, we excluded an extremely small number of RT trials (.03% of all trials) associated with impossible RTs ($RT < 0$ ms), which likely happened when a participant's vocalization for a particular trial started just about the same time as the appearance of the Stroop stimulus.²

Given that the Stroop data are noisy and often contain outliers (especially in the RT data) and show skewed distributions (both error and RT data), our preregistered plan was to analyze the data with and without transformations and make sure that the results are robust to different transformations. Thus, in addition to using raw error rates and RTs, we also analyzed the data after applying (a) an arcsine transformation for accuracy (specifically, proportion accuracy

² We did not specify in the preregistration that we had intended to exclude from RT analyses the trials with unusually short and long RTs, as we usually do. For this reason, the data reported in this article were conducted without excluding extreme RTs that are unlikely to reflect normal cognitive processes associated with Stroop performance (< 200 ms and $> 5,000$ ms). Removing those rare trials (.09% of all trials), however, did not change the statistical results for either fixed-effects and mixed-effects modeling reported below.

scores, namely “1 minus error rate”) and (b) a logarithmic (natural log) transformation for RTs. For simplicity, the reporting of our results below focuses on the raw error rates (%) and the log-transformed RTs. The conclusions essentially remain the same, however, when the analyses used arcsine accuracy or raw RT data (see **Tables S3A** and **S3B** in the supplement). We had also planned to conduct the RT analyses using Wilcox-Keselman trimming procedure, but, because the raw RT and log RT results converged, we decided to skip this extra RT analyses.

Results

All the analyses reported in this article were conducted with the base R statistical packages, with the exception of mixed-effects models, which were computed using the lme4 package in R. All the relevant data and the annotated R codes used for the analyses are available at our project OSF (see **Section 10** of the supplement for the summary of the data and R code files that we made available).

No analyses of the data were conducted until after all the data collection was completed. An alpha level for statistical tests was set at .05.

Preliminary Analyses

Descriptive statistics. The descriptive statistics for age, the questionnaire measures, and baseline Stroop performance are summarized in **Table S1** in the supplement. Also shown in that table are the results of independent-sample *t*-tests comparing the two conditions.

Random assignment was generally successful, and none of the measures showed significant differences, with the exception of the log RT for Block 2 of the baseline Stroop task for the congruent trials. This small but significant RT difference was unexpected, but the log RT data for the neutral baseline trials did not show such a group difference, thus ruling out the possibility of preexisting general processing-speed differences between the two groups.

Manipulation checks. We conducted manipulation checks by testing whether the control and depletion conditions differed in the self-ratings provided before (*motivation*) and after each block of the letter-cancellation task (*effortful*, *tired*, *difficult*, *boring*, *frustrated*, and *effort*). As noted earlier, the self-ratings were solicited for both the initial block common to all participants (Block 1) and the manipulation block that differed in the two conditions (Block 2).

We expected no group differences in the *motivation* ratings collected before both blocks, because, at the beginning of Block 2, the control and depletion groups had not diverged yet. More important, although we did not expect any condition differences in the other ratings collected at the end of Block 1 (e.g., *effortful*, *tired*), we expected systematic differences in the ratings collected after Block 2. These expectations were generally confirmed, with the minor exception of the ratings for *boring*. These manipulation-check results are reported in the supplement (see **Section 4** of the supplement, including **Table S2** and **Figure S2**).

Primary Preregistered Analyses: Fixed-Effects Models

We first conducted our preregistered analyses (referred to as *the fixed-effects analysis* here), which treated only participants (but not trials/stimuli) as a random variable. Specifically, for both error and RT data, we first computed mean error rates and RTs for each within-subject condition (congruent vs. incongruent) for each participant. Then, using the Stroop interference score (operationalized as “incongruent minus congruent trials”³) as the dependent measure, we conducted independent-sample *t*-tests to test whether there was any evidence for the ego-depletion effect at the overall group level in each block. This analysis was followed up by regression analyses to test the hypothesized interaction effect between condition (depletion vs. control) and willpower mindset (treated as a continuous variable).

³ There was a typo in our preregistration in this regard: The document incorrectly stated that we would compute the Stroop interference effect by subtracting incongruent trial results from congruent trial results.

Testing the main effect of ego depletion. The condition means as well as Stroop interference scores for the error data (error %) and RT data (log RTs) are presented in **Table 1**. To visually illustrate the data distribution, we also provided the “raincloud” plots for the error and log RT data for both Stroop blocks in the supplement (**Figures S3A–S3E**). As is clear from these plots, the Stroop interference effect was present, especially for the RT data, but, for neither the replication or extension block was there any evidence that the Stroop effect was greater in the depletion condition than in the control condition.

Table 1*Descriptive statistics for Stroop performance measures in Blocks 1 and 2*

	Control			Depletion			<i>t</i>	<i>p</i>
	N	<i>M</i>	<i>SD</i>	N	<i>M</i>	<i>SD</i>		
Stroop Task Block 1								
Error rate for congruent trials (%)	95	0.30	0.98	92	0.56	1.69	1.300	0.195
Log RT for congruent trials	95	6.54	0.16	92	6.56	0.13	0.978	0.329
Error rate for incongruent trials (%)	95	4.77	7.37	92	3.67	3.97	-1.262	0.209
Log RT for incongruent trials	95	6.71	0.16	92	6.72	0.15	0.752	0.453
Difference in error rates (Inc-Cong)	95	4.47	6.97	92	3.12	4.09	-1.620	0.107
Difference in Log RT (Inc-Cong)	95	0.17	0.08	92	0.16	0.07	-0.360	0.719
Stroop Task Block 2								
Error rate for asterisk trials (%)	95	0.69	1.44	92	0.72	1.53	0.136	0.892
Log RT for asterisk trials	95	6.50	0.13	92	6.53	0.13	1.352	0.178
Error rate for incongruent trials (%)	95	3.47	5.32	92	2.99	4.15	-0.681	0.497
Log RT for incongruent trials	95	6.67	0.15	92	6.70	0.15	0.994	0.322
Difference in error rates (Inc-Neut)	95	2.78	5.20	92	2.27	4.33	-0.721	0.472
Difference in Log RT (Inc-Neut)	95	0.17	0.06	92	0.17	0.07	-0.366	0.715

Note. Inc = Incongruent trials; Cong = Congruent trials; Neut = Neutral (asterisk) trials. The *t*-values and *p*-values are results from independent-sample *t*-tests comparing differences between participants in the control condition and participants in the depletion condition.

Block 1. As summarized in the top panel of Table 1, a *t*-test for the Stroop interference effect for the error data failed to reveal a significant difference between the two conditions, $t(185) = -1.620, p = .107, d = -0.239$, 95%CI for the condition difference [-3.014, .296]. Similarly, the condition difference for the Stroop interference effect was not significant for the log RT data,

either, $t(185) = -0.360$, $p = .719$, $d = -0.053$, 95%CI for the condition difference $[-.025, .017]$.⁴

Note that, as illustrated in **Figures S3A** in the supplement, there was one extreme outlier (61.5% error rate) in the control condition (the next highest error rate observed in either condition was about 20%), which might have potentially influenced the t -test results for the Block 1 error data just reported. Even when this outlier was excluded from analysis, however, the condition difference in the Stroop interference effect in error rates ($M_s = 3.92$ vs. 3.12) was still nonsignificant, $t(184) = -1.286$, $p = .200$, $d = -.189$, 95%CI for the condition difference $[-2.047, .431]$.

Block 2. The extension block data showed the same pattern of results. The Stroop interference effect did not differ significantly between the control and depletion conditions, for the error data, $t(185) = -.721$, $p = .472$, $d = -0.106$, 95%CI for the condition difference $[-1.890, .878]$, as well as for the log RT data, $t(185) = -.366$, $p = .715$, $d = -0.053$, 95%CI for the condition difference $[-.023, .016]$.

Testing the moderation effect. We then tested whether willpower mindset would moderate the ego-depletion effect by conducting regression analyses in which the dependent variable (the Stroop interference effect) was regressed onto three variables: condition (control vs. depletion; coded as -1 and $+1$, respectively), willpower mindset (treated as a continuous variable), and the interaction between the two. The results are summarized in **Table 2**.

Block 1. As is clear from Table 2 (top panel), there was no evidence for the hypothesized moderation effect for the RT data in this replication block: The B estimate for the interaction

⁴ Our Block 1 Stroop included substantially more trials (a total of 80, administered in two subblocks of 40 trials each) than JDW2010's version did (a total of 48). Because one could argue that the impact of the depletion manipulation might have faded quickly over the course of the Stroop task, we reanalyzed our data, focusing only on the first 40 trials of the replication block (Block 1) so that the number of Stroop trials in the current study would be approximately the same as that of the JDW2010 study. The results remained the same, however, as reported in **Tables S5, S6A, and S6B** in the supplement.

term was .001, $p=.778$. For the error data, however, the interaction term was significantly different from zero, $B=.875$, $p=.038$, suggesting the moderation of the ego-depletion effect by willpower mindset, as was demonstrated by JDW2010.

Table 2

Results from fixed-effect regression models for Stroop Blocks 1 and 2

	Stroop Interference (Error)				Stroop Interference (Log RTs)			
	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>
Block 1								
Intercept	3.771	0.417	2.948–4.593	<0.001	0.166	0.005	0.156–0.177	<0.001
Condition (C)	-0.674	0.417	-1.497–0.148	0.107	-0.002	0.005	-0.013–0.009	0.736
Mindset (M)	-0.191	0.418	-1.016–0.633	0.648	-0.004	0.005	-0.015–0.007	0.455
C x M	0.875	0.418	0.051–1.699	0.038	0.001	0.005	-0.009–0.012	0.823
Block 2								
Intercept	2.519	0.350	1.829–3.209	<0.001	0.170	0.005	0.160–0.179	<0.001
Condition (C)	-0.238	0.350	-0.928–0.452	0.498	-0.002	0.005	-0.011–0.008	0.721
Mindset (M)	-0.547	0.351	-1.239–0.145	0.120	-0.001	0.005	-0.011–0.009	0.840
C x M	0.287	0.351	-0.405–0.979	0.414	-0.003	0.005	-0.013–0.007	0.558

Note. Stroop interference = the error rate or log RT difference between the incongruent trials and the congruent (Block 1) or neutral/asterisk (Block 2) trials. *B* = Regression parameters, not standardized with respect to the dependent variable. *SE* = Standard errors for *B*. *CI* = 95% confidence intervals for *B* estimates. Condition (between-subjects) was coded as -1 for the control condition and 1 for the depletion condition. Mindset (the first subscale of the willpower mindset measure) was mean-centered and standardized.

A close look at the data, however, suggested that this interaction effect did not qualify as a replication of JDW2010's finding because the directionality of the moderation effect was the opposite of what was expected. Specifically, as illustrated in **Figure S4A** in the supplement, in our data, individuals with a nonlimited-willpower mindset were associated with *lower* error rates among participants *in the control condition* (simple effect: $B= -1.07$, $p=.072$), but not among those in the depletion condition (simple effect: $B=.683$, $p=.250$). Moreover, this significant yet opposite interaction effect was influenced by the extreme outlier in the control condition noted earlier (see **Figure S4B** in the supplement; the outlier is highlighted in that figure). In fact, when this extreme outlier was removed from analysis, the interaction was no longer significant: $B=.444$ (95%CI [-.179, 1.066]), $p=.162$. As these additional results suggest, our primary analyses

for Stroop Block 1 did not yield evidence for the proposal that only individuals with a limited-willpower mindset are susceptible to the ego-depletion effect.

Block 2. There was no evidence for the hypothesized moderation effect for either dependent measure in the extension block. The unstandardized B estimate for the interaction term was not significantly different from zero, for either the error data, $B=.287, p=.414$, or for the log RT data, $B= -.003, p=.558$.

Preregistered follow-up analyses: Replicating JDW2010's analyses. Upon failing to replicate JDW2010's main findings with our fixed-effects analyses, our preregistered follow-up plan was to analyze our data in the same way as JDW2010 did. Briefly stated, JDW2010 focused on incongruent trials only and conducted a multilevel (HLM) logistic-regression analysis at the level of individual trials. The accuracy on a particular trial—binary coding (0 vs. 1) of “correct” vs “incorrect”—was regressed onto multiple trial-level variables (condition [control vs. depletion]; mean-centered trial numbers; and RT for that trial) and participant-level variables (willpower mindset and age, both mean-centered), along with an interaction term for condition and willpower mindset (see JDW2010's description on p. 1687).

We tried to replicate this analysis, but we had difficulty figuring out the actual model tested by JDW2010, primarily because of the high degrees of freedom reported for various terms in the model relative to the number of participants in the study ($N=60$). For example, the t -test result reported for the main interaction term between condition and willpower mindset was “ $t(1433)=6.71$ ” (p. 1687), raising the possibility that JDW2010's HLM analysis might not have properly coded participants as a random variable (Schimmack, 2016), essentially treating all trials in the model (24 incongruent trials x 60 participants = 1,440 total trials) as though they

came from 1,440 unique participants.⁵ Moreover, some exploratory hierarchical models we tried did not converge when RTs were included in the model.

For this reason, we contacted Job and asked for the data-analytic code used for their analysis, but, unfortunately, she no longer had access to them. When Miyake subsequently met with Job in person at a workshop and further discussed the reported analyses, the agreement reached was that the original analysis was suboptimal and that a better and more powerful way to analyze the Stroop data would be to conduct mixed-effects modeling, as Job and her colleagues have been doing recently (Savani & Job, 2017). We thus deviated from our preregistered follow-up analysis plan and conducted mixed-effects modeling instead on our data.

Exploratory (Originally Not Planned) Analyses: Mixed-Effects Models

A major advantage of mixed-effects analyses is that those models allow us to control for random variance between participants while also accounting for non-heteroscedasticity in the data. Additionally, mixed-effects models can allow the slopes of trial type (congruent vs. incongruent) to vary between participants and control for nuisance variance associated specifically with individual trials/stimuli, thus increasing the power to detect the hypothesized effects (Judd, Westfall, & Kenny, 2012). Thus, we conducted mixed-effects analyses, modeling both individual participants and trials simultaneously as random variables.

Specifically, we conducted four separate sets of mixed-effects regression models (for errors and log RTs, run separately for Stroop Blocks 1 and 2). For error data, we tested mixed-effects logistic-regression models estimating the odds of making an error (coded as 0 for correct

⁵ The HLM software uses the formula, " $j - p - 1$," to approximate degrees of freedom for between-subjects effects, where j is the number of level-2 observations (participants) and p is the number of parameters in the level-2 equation. Thus, the fact that the reported degrees of freedom (1,433) was higher than the number of participants in the study ($N=60$) seems consistent with Schimmack's (2016) suggestion that JDW2010's HLM model may not have properly nested trials under participants.

trials and 1 for error trials). For log RT data, we tested a mixed-effects linear-regression model estimating log-transformed RTs. Both models included Condition (-1=control vs. 1= depletion) and willpower mindset (z-scored) as between-subjects predictors and Trial Type (-1=congruent trials in Block 1 and neutral trials in Block 2 vs. 1=Incongruent) as a within-subjects predictor. For all models, we estimated, for each participant, random intercepts as well as random slopes for Trial Type (congruent vs. incongruent/neutral). Thus, the mixed-effects analyses we report here are comparable to those conducted by Savani and Job (2017), although we did not include any additional covariates (e.g., age, gender) in our models.⁶ The results of these mixed-effects analyses are summarized in **Table 3**.

Block 1. As shown in Table 3 (top panel), the mixed-effects modeling results are consistent with fixed-effects modeling results reported earlier. For both error and RT data, the effect of Trial Type (i.e., the overall Stroop interference effect) was significant, as expected. More interesting is the finding that, for the log RT (but not error) data, willpower mindset was a significant predictor of the overall Stroop RTs in the expected direction (i.e., a nonlimited willpower mindset was associated with faster RT responses, regardless of the types of trials). In neither analysis for Block 1, however, did willpower mindset interact significantly with Trial Type, Condition (depletion vs. control), or both.

Block 2. The mixed-effects modeling results for the Block 2 data were similar to those of Block 1. Again, as expected, there was clear evidence for the effect of Trial Type (neutral vs. incongruent) for both dependent measures. However, the hypothesized willpower-mindset moderation effect, namely the three-way interaction term (Condition x Mindset x Trial Type),

⁶ To control for random effects of stimuli (e.g., specific combinations of words and colors), we also conducted a set of mixed-effects analyses in which we additionally estimated random intercepts for the character set and color of the stimuli, as was done by Meier and Kane (2013). The results remained the same for these additional models.

was not significant for either error or log RT data. Interesting to note, however, a two-way interaction involving willpower mindset (Mindset x Trial Type) was significant for the error data, odds ratio (OR)=0.796, $p=.013$, suggesting that the limited-willpower mindset was associated with the likelihood of producing larger Stroop interference effects (i.e., greater errors for incongruent trials than for neutral trials). This interaction effect, however, did not involve the ego-depletion manipulation, as predicted by JDW2010.

Table 3

Results from mixed-effect regression models for Stroop Blocks 1 and 2

<i>Predictors</i>	Odds of Incorrect Response				Log RT			
	<i>OR</i>	<i>SE</i>	<i>CI</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>
Block 1								
Intercept	0.003	0.458	0.001–0.008	<0.001	6.632	0.012	6.609–6.655	<0.001
Condition (C)	1.094	0.176	0.775–1.544	0.610	0.010	0.010	-0.010–0.031	0.331
Mindset (M)	1.172	0.172	0.837–1.642	0.356	-0.028	0.010	-0.048– -0.007	0.009
Trial Type (T)	7.827	0.451	3.231–18.958	<0.001	0.083	0.006	0.071–0.095	<0.001
C x M	1.109	0.171	0.793–1.550	0.545	-0.006	0.010	-0.027–0.014	0.544
C x T	0.818	0.160	0.597–1.120	0.210	-0.001	0.003	-0.006–0.004	0.746
M x T	0.896	0.156	0.660–1.216	0.481	-0.002	0.003	-0.007–0.003	0.492
C x M x T	1.080	0.155	0.797–1.462	0.619	0.001	0.003	-0.005–0.006	0.832
Block 2								
Intercept	0.008	0.183	0.005–0.011	<0.001	6.601	0.011	6.579–6.622	<0.001
Condition (C)	0.978	0.109	0.790–1.212	0.842	0.012	0.010	-0.007–0.031	0.213
Mindset (M)	1.216	0.105	0.989–1.494	0.063	-0.017	0.010	-0.036–0.002	0.083
Trial Type (T)	2.346	0.173	1.673–3.291	<0.001	0.085	0.006	0.074–0.096	<0.001
C x M	1.032	0.104	0.841–1.265	0.765	-0.011	0.010	-0.030–0.009	0.283
C x T	0.968	0.096	0.801–1.168	0.732	-0.001	0.002	-0.006–0.004	0.738
M x T	0.796	0.092	0.665–0.953	0.013	-0.001	0.002	-0.005–0.004	0.814
C x M x T	1.035	0.091	0.866–1.237	0.708	-0.001	0.002	-0.006–0.003	0.580

Note. *OR* = Unstandardized odds ratios. *B* = Regression parameters, not standardized with respect to the dependent variable. *SE* = Standard errors for *OR* and *B*. *CI* = 95% confidence intervals for *B* estimates. Condition (between-subjects) was coded as -1 for the control condition and 1 for the depletion condition. Mindset (the first subscale) was mean-centered and standardized. Trial type was coded as (-1 for congruent trials in Block 1 and neutral trials in Block 2 and 1 for incongruent trials in both Blocks 1 and 2). *P* values for binomial logistic mixed-effects models (for the error data) were calculated using Wald's *Z* statistic as estimated by the lme4 package in R. *P* values for linear mixed-effects models (for the log RT data) are calculated by lme4 package in R, based on Satterthwaite's approximations to degrees of freedom.

Secondary (Exploratory) Analyses: Analyses Based on the Trait Self-Control Measure

At the time of preregistration, we hypothesized that individual differences in willpower

mindset would likely be correlated with other individual differences variables, such as trait self-control. In fact, the correlation between willpower mindset (the first subscale) and trait self-control in this study was small but significant, $r(185) = .255, p < .001$. If an individual's willpower mindset is systematically related to his/her perception of how good he/she is at self-control, it seemed possible that any moderation of ego depletion by willpower mindset might be driven primarily by trait self-control, rather than willpower mindset per se.

Because we did not find any evidence for the willpower-mindset-moderation hypothesis in the current study, however, we did not conduct our planned analyses in which the effect of trait self-control would be statistically controlled for to see whether the willpower-mindset-moderation effect would still remain significant. Instead, we conducted a set of analyses to examine whether trait self-control would serve as a significant moderator of the ego-depletion effect. The results of these exploratory analyses (both fixed-effects and mixed-effects analyses) are reported in **Tables S7A** and **S7B** of the supplement. The bottom line is that there was no evidence for such a moderation effect by trait self-control.

We also report in the supplement the results of fixed-effect and mixed-effects modeling that used the full Implicit Theories of Willpower scale, rather than just the first subscale (see **Tables S8A** and **S8B**). Interesting to note, the results based on the full scale produced more statistically significant results in the mixed-effects models (see **Table S8B**) but not in the fixed-effects models (see **Table S8A**), but, again, there was no evidence for either the main effect of ego depletion or the hypothesized moderation of the ego depletion effect by willpower mindset.

Discussion

The current preregistered study failed to reproduce JDW2010's two key findings: the overall ego-depletion effect and its moderation by willpower mindset. As noted by Simonsohn

(2015), evaluating whether replication results are consistent with those of the original study is a complicated matter, but, at the level of statistical significance, our replication results were not in line with those of JDW2010's.

One limitation of this study is that, because of the way the original data was analyzed and reported, we were not able to conduct an a priori power analysis for the hypothesized moderation effect. Thus, the power to detect the hypothesized moderation effect in our preregistered (fixed-effects) analyses was still limited, despite our substantially larger sample size. Important to note, however, two recent preregistered studies, both published after our data collection, also reported a nonsignificant moderation effect of willpower mindset (Dang, Liu, Liu, & Mao, 2017; Garrison, Finley, & Schmeichel, 2018). Although neither was a replication study and hence does not shed direct light on the robustness of JDW2010's original finding per se, the existing body of evidence for the hypothesized willpower-mindset moderation effect on ego depletion is still limited and has not been extended to other laboratories.

One possible reason for our replication failure is that we analyzed the data differently than JDW2010 did. We initially planned and tried to replicate JDW2010's original analysis, but opted for conducting mixed-effects analyses instead, which both Job and we agreed would be more appropriate. Thus, there remains a possibility that JDW2010's original willpower-mindset moderation effect might not be statistically significant if their data were reanalyzed with the same analyses reported here (fixed-effects or mixed-effects modeling) and without including theoretically-unmotivated covariates. If so, this might not be a case of replication failure.

Second, even if the hypothesized moderation effect is real, it might be small and perhaps too difficult to detect reliably, especially using commonly-used self-control tasks. To begin with, the ego-depletion effect itself may not be robust (Frieze et al., 2018; Hagger et al., 2016).

Moreover, though experimentally reliable, the Stroop interference effect is not as robust as a measure of individual differences (Hedge, Powell, & Sumner, 2018). Together with the possibility that the letter-cancellation task may not effectively tap inhibitory-control processes as typically assumed (Lurquin, 2017), it is likely that the hypothesized moderation effect is hard to detect, even with a much larger sample size (see also McClelland & Judd, 1993).

Third, some changes we introduced to strengthen the study may have contributed to the discrepant results. Although some changes (e.g., using a vocal-response version of Stroop) should unlikely matter, others might. For example, assessing willpower mindset and trait self-control at the beginning of the study or administering manipulation-check self-ratings at multiple points might have alerted participants that the study was about self-control or willpower. However, in the two aforementioned ego-depletion studies that also failed to find the moderating influence of willpower mindset, a similar set of questionnaires were administered at the beginning (Dang et al., 2017) or at the end (Garrison et al., 2018) of the respective studies. Moreover, the former study administered manipulation checks, but the latter did not. Thus, these differences cannot readily explain our failure to replicate JDW2010's results.

In our exchanges, Job suggested that a critical difference might be our not using a blurry text in the depletion condition for the letter-cancellation task. According to this account, using an unprofessionally-looking text might make participants feel fine to “slack off” on the subsequent task, and this reduced motivation might help magnify the impact of another motivation-related variable, willpower mindset. Although this alternative explanation is worth testing in future high-powered preregistered studies, it remains a conjecture at this point.

Although mindset (especially fixed/growth mindset) has been studied extensively (for a recent meta-analysis, see Sisk et al., 2018), its application to willpower has started only recently.

Given the theoretical importance of JDW2010's proposal, it is important to establish more firmly that the hypothesized willpower-mindset moderation effect is sufficiently large and replicable across multiple labs so that it provides a solid foundation for future theorizing and empirical research on the ego-depletion effect. In this regard, it seems necessary to conduct additional high-powered, preregistered replication studies of JDW2010's original findings.

In the meantime, JDW2010's results should be interpreted cautiously. The impact of willpower mindset on the ego-depletion effect seems substantially less robust than initially suggested and may even be restricted to only specific analyses or situations. More generally, our results also add to the growing body of studies documenting the difficulty of replicating the ego-depletion effect itself.

Author Contributions

N. Carruth and A. Miyake designed the experiment. N. Carruth developed the experimental materials, collected the data, and conducted preliminary data analyses. J. Ramos performed the primary data analyses reported in the manuscript and the supplement, together with A. Miyake, and took the lead for producing the data and data-analytic files provided in the project OSF site. A. Miyake wrote the initial draft of the manuscript with N. Carruth. All the authors discussed the manuscript, contributed to the writing/revision process, and approved the final version for submission.

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Declaration of Conflicting Interests

The authors declare that there are no conflicts of interest with respect to the authorship or the publication of this article.

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Supplemental Materials (SOM-R)

1. A Summary of Deviations from the Preregistered Plans

Below, we summarize all the deviations from the preregistered plans, classified into two categories: (a) deviations in methods and procedures and (b) deviations in data analyses. For some of the entries here, more detailed explanations are provided in the main article. Note that all of the method-related deviations on this list occurred because we completed our preregistration several days before finalizing all the details of the study or because we simply forget to mention some intended changes in the preregistration document. The page numbers below refer to those in the preregistration.

Methods

- Number of trials for the main Stroop (p. 5 & p. 10)
Preregistration document specified 2 blocks with 72 trials each, but the actual study included 2 blocks with 80 trials each.
- Questionnaires (p. 6 & p. 8)
Preregistration specified our plan to administer the susceptibility to ego-depletion questionnaire, but this questionnaire was not administered.
- Questionnaire item presentation (p. 8)
Preregistration specified a single predetermined random order for the questionnaire items, but item presentation was automated to randomize for each participant.
- Catch items (p. 8)
Preregistration specified the inclusion of catch-items in questionnaire, but no catch items were included due to short length of the questionnaires.
- Letter-cancellation task stimuli (p. 9)
The original study (Job, Dweck, & Walton, 2010; hereafter *JDW2010*) used a blurred version of text in the depletion condition in the manipulation block (Block 2), but we administered a nonblurred version of text. This decision was made shortly after we received the materials from V. Job, but we neglected to mention this point in our preregistration document.

Data Analysis

- Exclusion criteria (p. 10)
Preregistration indicated that we would exclude participants outside of the ages 18–30, but we did not apply this exclusion criterion for the reasons outlined in the main article.
- Follow-up statistical tests (p. 10)
Preregistration indicated that our follow-up data-analysis plan was to replicate the exact analyses performed by JDW2010. However, we decided to conduct mixed-effects modeling instead for the reasons explained in the main article.
- Wilcoxon-Keselman trimming (p. 12)
Preregistration specified a plan to use a Wilcoxon-Keselman trimming procedure for the Stroop RT data. However, we decided not to perform this procedure because the other planned RT analyses (raw RT and log RT analyses) led to the same statistical conclusions.

2. Diagrammatic Summary of the Experimental Procedure

The experimental procedure is summarized in the diagram below (**Figure S1**). For the detailed description of the procedure, see the main article.

Figure S1. A schematic summary of the experimental procedure.

3. Descriptive Statistics

The descriptive statistics for age, the questionnaire measures, and the baseline Stroop performance are summarized in **Table S1**, separately for each condition, along with independent-sample *t*-tests for the condition differences.

Table S1

Descriptive statistics for demographic and baseline measures

	Control			Depletion			<i>t</i>	<i>p</i>
	<i>n</i>	<i>M</i>	<i>SD</i>	<i>n</i>	<i>M</i>	<i>SD</i>		
Demographics								
Age	95	19.64	2.81	88 ^a	19.31	1.48	0.998	0.319
Individual difference scales								
Willpower mindset: SMA	95	3.22	1.00	92	3.27	1.01	-0.382	0.703
Willpower mindset: Full Scale	95	3.89	0.76	92	3.98	0.63	-0.875	0.383
Trait self-control	95	4.14	0.69	92	4.18	0.69	-0.394	0.694
Baseline Stroop measures								
Error rate for baseline asterisk (%)	95	0.39	1.60	91 ^b	0.16	1.10	1.164	0.246
Log RT baseline asterisk trials	95	6.36	0.13	91 ^b	6.38	0.12	-1.403	0.162
Error rate baseline congruent trials	95	0.05	0.47	91 ^b	0.05	0.48	-0.030	0.976
Log RT for baseline congruent	95	6.27	0.14	91 ^b	6.31	0.13	-2.181	0.030

Note. SMA = the strenuous mental activity subscale (1=limited willpower, 7=unlimited willpower). Willpower mindset: Full scale was computed by taking an average of the first SMA subscale and the second “resisting temptations” subscale. The *t*-values and *p*-values are results from independent-sample *t*-tests comparing differences between participants in the control condition and participants in the depletion condition.

a. Four participants (all in the depletion condition) did not indicate their ages.

b. The baseline Stroop measure from one participant in the depletion condition was missing because of a computer crash that occurred during this portion of the study (we kept this participant’s data in because the rest of the study went fine).

4. Manipulation-Check Results

In this section, we report the results of our manipulation-check analyses. As noted in the main text, we had different expectations for the *motivation* ratings, collected *before* each block of the letter-cancellation task, and for the other ratings (i.e., *effortful*, *tired*, *difficult*, *boring*, *frustrated*, and *effort*), collected *after* the completion of each letter-cancellation block. Because the non-motivation ratings were more critical as our manipulation checks, we report those results first.

Non-Motivation ratings. The means (*M*s) and standard deviations (*SD*s) of the non-motivation ratings for each block (Block 1 and Block 2) are summarized in **Table S2** below, along with the results of independent-sample *t*-test results.

Table S2

Participant ratings after Blocks 1 and 2 of Task 1, and within-person differences between tasks

	Control			Depletion			<i>t</i>	<i>p</i>
	<i>n</i>	<i>M</i>	<i>SD</i>	<i>n</i>	<i>M</i>	<i>SD</i>		
After Block 1								
Effortful	95	5.77	2.16	92	5.93	2.03	0.543	0.588
Tired	95	4.29	2.19	92	3.50	1.92	-2.635	0.009
Difficulty	95	3.20	1.90	92	3.27	1.72	0.270	0.787
Boring	95	5.72	2.21	92	5.29	2.31	-1.279	0.202
Frustration	95	2.60	1.94	92	2.40	1.77	-0.728	0.468
Effort	95	6.84	1.76	92	7.02	1.70	0.709	0.479
After Block 2								
Effortful	95	5.30	2.35	92	6.95	1.52	5.650	0.000
Tired	95	4.67	2.26	92	4.46	2.07	-0.685	0.494
Difficulty	95	3.41	1.97	92	5.91	1.83	8.988	0.000
Boring	95	5.67	2.52	92	5.47	2.37	-0.575	0.566
Frustration	95	3.00	1.97	92	4.00	2.21	3.271	0.001
Effort	95	6.60	1.94	92	7.24	1.49	2.521	0.013
Rating Difference (Block 2 – Block 1)								
Effortful	95	-0.46	1.31	92	1.01	1.57	6.971	0.000
Tired	95	0.38	1.25	92	0.96	1.30	3.100	0.002
Difficulty	95	0.21	1.30	92	2.64	1.63	11.316	0.000
Boring	95	-0.04	2.06	92	0.17	1.85	0.754	0.452
Frustration	95	0.40	1.45	92	1.60	1.55	5.455	0.000
Effort	95	-0.24	1.11	92	0.22	1.05	2.913	0.004

Note. The *t*-values and *p*-values reported are results from independent-sample *t*-tests examining comparing group differences between participants in the control condition and participants in the depletion condition.

As shown in the top panel of Table 2, the Block 1 ratings did not differ between the two conditions, with the unexpected exception of the *tired* ratings, suggesting that those participants in the control condition as a group reported a higher level of fatigue than in the depletion condition (*M*s=4.29 vs. 3.50) after completing Block 1, although Block 1 was identical for both conditions. More important, the Block 2 ratings (the middle panel of Table 2) showed clear differences between the conditions, but this time with two exceptions. First, the Block 2 ratings for *boring*

after the depletion manipulation did not differ between the two conditions ($M_s=5.67$ vs 5.47 , respectively). In addition, the Block 2 ratings for *tired*, for which we expected to see a condition difference, did not show a significant difference ($M_s=4.67$ vs 4.46).

Because one measure (*tired*) produced unexpected results for both blocks, we conducted a follow-up analysis. Specifically, for each participant and for each rating, we computed a difference between the two blocks (i.e., Block 2 rating minus Block 1 rating) so that the resulting number would indicate a change from Block 1 to Block 2. Specifically, higher difference scores meant a perceived increase in individual ratings (e.g., *effortful*, *tired*) from Block 1 to Block 2. We then compared the resulting difference scores for each rating between the control and depletion conditions. Note that this analysis is essentially the same as conducting a 2 x 2 mixed ANOVA (with condition as a between-subjects variable and blocks as a within-subjects variable) for each rating and testing for the two-way interaction effect (i.e., the effect of block is different for the control and depletion conditions). The results of this comparison are summarized in the bottom panel of Table S2 and are visually illustrated in **Figure S2**.

Figure S2. Mean within-person differences in ratings between Block 1 (the habituation block) and Block 2 (the critical manipulation block) for the letter-cancellation task. Higher bars represent higher ratings after Task 2, relative to Task 1. Error bars represent standard errors.

For all the non-motivation ratings collected after each block, we expected a significant difference between the two conditions, and this is exactly what we observed, with the exception of *boring*, $t(185) = -.754$, $p = .452$. Important to note, although the *tired* ratings produced somewhat unexpected results when the Block 1 and Block 2 ratings were examined separately, this difference-score analysis produced the result consistent with our prediction: An increase in the self-perceived level of fatigue from Block 1 to Block 2 was significantly greater among

participants in the depletion condition (from 3.50 to 4.46) than among those in the control condition (from 4.29 to 4.67).

To further confirm the overall effectiveness of the main depletion manipulation, we conducted a one-way MANOVA (not initially planned) that simultaneously took into account all six ratings (*effortful*, *tired*, *difficult*, *boring*, *frustrated*, and *effort*) and used the difference scores for the respective ratings as the dependent measures. The MANOVA results indicated a significant difference between the control and depletion conditions, Pillai-Bartlett trace $V=0.470$ ($df=1$), approximate $F(6, 180)=26.55$, $p<.001$. These results suggest that our manipulation checks were generally successful, even though one of the six ratings, *boring* (arguably the least directly relevant to the hypothesized “depleted” state), did not yield an expected pattern.

Motivation ratings. As illustrated in the procedural figure (Figure S1), the two experimental conditions (control vs. depletion) had not diverged when the motivation ratings were obtained (i.e., at the outset of the habituation block, Block 1, as well as the critical manipulation block, Block 2). Accordingly, there should not be any condition differences for those motivation ratings. Consistent with this expectation, the self-reported levels of *motivation* for performing Block 1 of the letter-cancellation task did not significantly differ in the two conditions ($M=7.57$ and $SD=1.26$ in the control condition and $M=7.51$ and $SD=1.39$ in the depletion condition), $t(185)= -0.462$, $p=.761$. The same was true for the levels of *motivation* before performing Block 2 of the same task ($M=7.12$ and $SD=1.56$ in the control condition and $M=7.04$ and $SD=1.72$ in the depletion condition), $t(185)= -0.301$, $p=.763$. Moreover, the degree of rating changes between Block 1 and Block 2 (operationalized as Block 2 ratings minus Block 1 ratings) did not show any condition differences, $t(185)= -0.840$, $p=.933$.

4. Visual Illustrations for the Stroop Data Used for the Fixed-Effects Modeling

As noted in the main text, our planned (preregistered) analyses of the main Stroop data (both error rates and RT data) are based on the mean scores calculated for each trial type (i.e., congruent and incongruent in Block 1 and neutral and incongruent in Block 2) for each participant. Although Table 1 in the main article provides all the condition means and *SDs*, it is often helpful to see the distributions of the data points visually. For this reason, we provide here the “rain cloud” plots (<https://micahallen.org/2018/03/15/introducing-raincloud-plots/>) of the main Stroop data for both types of dependent measures (error rates and log RTs) for each block (Blocks 1 & 2). Because there was one extreme outlier for the error data for Block 1, we are providing two rain cloud plots for this dataset, one with and one without the extreme outlier.

Here is the summary of the five rain cloud plots presented in this section:

- **Figure S3A:** Block 1 error data with all the data points (including one extreme outlier)
- **Figure S3B:** Block 1 error data without one extreme outlier
- **Figure S3C:** Block 2 error data
- **Figure S3D:** Block 1 log RT data
- **Figure S3E:** Block 2 log RT data

As is clear from the **Figure S3A**, there was one extreme outlier in the control condition, whose error rate for Block 1 was over 60% (61.5%), well above the next highest error rates observed in either of the two conditions (control or depletion). As mentioned in the results section of the main article, this one extreme outlier had some noticeable impact on the statistical results, even though our sample size was almost three times as many as that for the original JDW2010 study.

Figure S3A. The rain cloud plot for the error rate distribution for each participant for Block 1 (the direct replication block). As is clear from this plot, there was one extreme outlier in the control condition (61.5% error rate).

Error data. As shown in **Figures S3B & S3C** below (Figure S3B removes the extreme outlier plotted in Figure S3A), the error data demonstrate a clear general tendency for the floor effect for both Blocks 1 and 2, because the error rates were low in our study. Arcsine-transformed accuracy data showed better distributional characteristics, but this transformation was not able to turn this skewed distribution into a normal distribution (for the results of the analyses based on arcsine-transformed Stroop accuracy data, see **Table S3** in **Section 5** below).

Figures S3B & S3C: The rain cloud plot for the error rate distribution for each participant for Block 1 (Figure S3B, left panel) and Block 2 (Figure S3C, right panel). Note that Figure S3B is the same as Figure S3A, except that one extreme outlier in the control condition is removed.

Log RT data. In contrast, the log RT data are more normally distributed for both blocks (**Figures S3D and S3E**), less skewed than the raw RT data we also analyzed (for the results of the fixed-effects and mixed-effects analyses based on raw RT data, see **Section 5** of this supplement, especially Tables S3 & S4).

Figures S3D & S3E: The rain cloud plot for the log RT distribution for each participant for Block 1 (Figure S3D, left panel) and Block 2 (Figure S3E, right panel).

5. Fixed-Effects and Mixed-Effects Modeling Results Based on Arcsine-Transformed Accuracy Data and Raw RT Data

This section provides the results of our supplementary (but preregistered) analyses based on arcsine-transformed accuracy data and raw RT data for both Stroop Block 1 and Block 2. (Note that Arcsine transformation has to be applied to the proportion correct measure, rather than the error rate or proportion incorrect measure).

We conducted the same fixed-effects and mixed-effects analyses reported in the main text, but with different dependent measures (see **Table S3** for the results of the fixed-effects analyses and **Table S4** for the mixed-effects analyses). The purpose of these additional analyses was to make sure that the results we report are robust to different data transformations. Indeed, the results based on the arcsine-transformed accuracy data and the raw RT data are consistent with those based on the raw error and log RT data reported in the main article (see Tables 2 and 3 in the main article for comparison).

Table S3

Results from fixed-effect regression models using arcsine-transformed accuracy rates and raw RTs.

	Arcsine-transformed accuracy				Raw RTs			
	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>
Block 1								
Intercept	-0.139	0.010	-0.158– -0.120	<0.001	134.467	4.823	124.952–143.983	<0.001
Condition (C)	0.017	0.010	-0.003–0.036	0.090	1.191	4.823	-8.324–10.707	0.805
Mindset (M)	-0.001	0.010	-0.020–0.019	0.942	-2.774	4.835	-12.314–6.766	0.567
C x M	-0.014	0.010	-0.033–0.006	0.171	-1.016	4.835	-10.557–8.524	0.834
Block 2								
Intercept	-0.092	0.010	-0.112– -0.072	<0.001	134.505	4.561	125.507–143.504	<0.001
Condition (C)	0.005	0.010	-0.015–0.025	0.599	0.420	4.561	-8.578–9.419	0.927
Mindset (M)	0.017	0.010	-0.003–0.037	0.100	-2.019	4.573	-11.041–7.003	0.659
C x M	-0.003	0.010	-0.023–0.017	0.757	-2.543	4.573	-11.564–6.479	0.579

Note. Stroop effect (arcsine accuracy) was computed as the arcsine-transformed proportion of correct responses in incongruent trials minus the arcsine-transformed proportion of correct responses in congruent trials (Block 1) or asterisk trials (Block 2). *B* = unstandardized regression parameters. *SE* = standard errors for *B*. *CI* = 95% confidence-intervals for *B* estimates. Condition (between-subjects) was coded as -1 for the control condition and 1 for the depletion condition. Mindset = the beliefs about strenuous mental activity subscale score (between-subjects), which was mean-centered and standardized.

Note that **Table S4**, which presents the results of the mixed-effects analyses, includes only the raw RT data (i.e., no analysis of the arcsine-transformed accuracy data). This is because the mixed-effects analyses of the error data we reported in Table 3 (the left panel) in the main article were logistic-regression analyses focusing on the binary outcomes of the individual trials (correct or incorrect). Given that arcsine transformation must be applied to the proportion correct measure (not at the level of individual trials), it was not possible to conduct mixed-effects analyses for the arcsine-transformed accuracy data. This was the main reason for our decision to report the results of the raw error data in the main article, even though arcsine transformation helps spread out the

scores in the high-accuracy range and generally improves the distributional characteristics of the accuracy data.

Table S4

Results from mixed-effect regression models using raw RTs.

<i>Predictors</i>	Raw RTs			
	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>
Block 1				
Intercept	785.224	9.894	765.832–804.616	< 0.001
Condition (C)	7.325	8.923	-10.164–24.814	0.412
Mindset (M)	-20.467	8.947	-38.003– -2.931	0.022
Trial Type (T)	67.377	4.909	57.757–76.998	< 0.001
C x M	-3.141	8.947	-20.677–14.395	0.726
C x T	0.607	2.413	-4.124–5.337	0.802
M x T	-1.298	2.423	-6.048–3.452	0.592
C x M x T	-0.493	2.423	-5.243–4.257	0.839
Block 2				
Intercept	759.009	9.168	741.041–776.977	< 0.001
Condition (C)	8.824	8.251	-7.348–24.996	0.285
Mindset (M)	-12.471	8.273	-28.686–3.743	0.132
Trial Type (T)	67.356	4.604	58.333–76.379	< 0.001
C x M	-7.861	8.273	-24.075–8.354	0.342
C x T	0.263	2.287	-4.220–4.746	0.908
M x T	-1.085	2.295	-5.583–3.414	0.636
C x M x T	-1.192	2.295	-5.691–3.306	0.603

Note. *B* = unstandardized regression parameters. *OR* = unstandardized odds ratios. *SE* = standard errors for *OR* and *B*. *CI* = 95% confidence-intervals for *B* estimates. Condition (between-subjects) was coded as -1 for the control condition and 1 for the depletion condition. Mindset = the beliefs about strenuous mental activity subscale score (between-subjects), which was mean-centered and standardized. Trial type was coded as (-1 for congruent trials in Block 1 and neutral trials in Block 2 and 1 for incongruent trials in both Blocks 1 and 2). *P*-values are calculated based on Satterthwaite's approximations.

6. Results of the Error and Log RT Analyses Focusing on Only the 1st Subblock of the Main Stroop Block 1 (the Replication Block)

As noted in the method section, Block 1 of the main Stroop task was intended to be the direct replication block. Because we increased the number of trials substantially (40 congruent and 40 incongruent trials in Block 1 of our study vs. 24 congruent and 24 incongruent trials in the original study by JDW2010), one could argue that, perhaps due to decay or dissipation of the depletion effect, the hypothesized effects of ego-depletion and its moderation by willpower mindset might be observed clearly if the analysis focused on the early portion of the Block 1 Stroop trials. Because our Block 1 Stroop trials were administered in two subblocks of 40 trials each, we conducted the same fixed-effects and mixed-effects analyses just for the initial subblock of the Block 1 Stroop trials (a total of 40 trials, which is comparable to the total number of Stroop trials administered in the original study [48]).

The relevant condition means (both the first and second subblocks of Block 1) are summarized in **Table S5** below. As shown in the table, there was no evidence for the larger number of Stroop trials in this study contributed to the absence of the overall ego-depletion effect or its moderation by willpower mindset. The results of the fixed-effects analyses for the first subblock only are summarized in **Table S6A**, and the results of the corresponding mixed-effects analyses are summarized in **Table S6B**.

Table S5

Descriptive statistics for Stroop performance measures for the first subblock of Stroop Block 1

	Control			Depletion			<i>t</i>	<i>p</i>
	<i>n</i>	<i>M</i>	<i>SD</i>	<i>n</i>	<i>M</i>	<i>SD</i>		
Error rate for congruent trials (%)	95	0.38	1.74	92	0.39	1.54	0.016	0.987
Log RT for congruent trials	95	6.54	0.16	92	6.57	0.14	1.261	0.209
Error rate for incongruent trials (%)	95	4.31	7.57	92	2.95	4.28	-1.506	0.134
Log RT for incongruent trials	95	6.70	0.16	92	6.72	0.15	0.799	0.425
Difference in error rates (Inc - Cong)	95	3.93	7.00	92	2.56	4.62	-1.567	0.119
Difference in Log RT (Inc - Cong)	95	0.16	0.08	92	0.15	0.08	-0.757	0.450

Note. The *t*-values and *p*-values are results from independent-sample *t*-tests comparing differences between participants in the control condition and participants in the depletion condition.

Table S6A

Results from fixed-effect regression models for the first subblock of Stroop Block 1

	Stroop Interference (Error)				Stroop Interference (Log RT)			
	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>
Intercept	3.228	0.435	2.369–4.087	<0.001	0.155	0.006	0.143–0.166	<0.001
Condition (C)	-0.679	0.435	-1.538–0.180	0.121	-0.004	0.006	-0.016–0.007	0.459
Mindset (M)	-0.131	0.437	-0.993–0.730	0.764	-0.002	0.006	-0.014–0.010	0.714
C x M	0.597	0.437	-0.264–1.458	0.173	-0.002	0.006	-0.013–0.010	0.789

Note. *B* = Regression parameters, not standardized with respect to the DV. *SE* = Standard errors for *B*. *CI* = 95% confidence-intervals for *B* estimates. Condition (between-subjects) was coded as -1 for the control condition and 1 for the depletion condition. Mindset = the beliefs about strenuous mental activity subscale score (between-subjects), which was mean-centered and standardized.

Table S6B*Results from mixed-effect regression models for the first subblock of Stroop Block 1*

<i>Predictors</i>	Odds of Incorrect Response				Log RT			
	<i>OR</i>	<i>SE</i>	<i>CI</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>
Intercept	0.001	0.711	0.000–0.004	<0.001	6.630	0.013	6.605–6.654	<0.001
Condition (C)	0.952	0.337	0.491–1.842	0.883	0.012	0.011	-0.009–0.033	0.252
Mindset (M)	1.196	0.318	0.642–2.229	0.573	-0.029	0.011	-0.049– -0.008	0.007
Trial Type (T)	23.822	0.706	5.971–95.033	<0.001	0.078	0.008	0.063–0.092	<0.001
C x M	1.105	0.317	0.594–2.057	0.752	-0.005	0.011	-0.025–0.016	0.669
C x T	0.879	0.332	0.458–1.686	0.698	-0.002	0.003	-0.008–0.004	0.462
M x T	0.897	0.312	0.486–1.654	0.727	-0.001	0.003	-0.007–0.005	0.767
C x M x T	1.055	0.312	0.573–1.944	0.863	-0.001	0.003	-0.007–0.005	0.792

Note. *B* = regression parameters, not standardized with respect to the dependent variable. *OR* = unstandardized odds ratios. *SE* = standard errors for *OR* and *B*. *CI* = 95% confidence-intervals for *B* estimates. Condition (between-subjects) was coded as -1 for the control condition and 1 for the depletion condition. Mindset = the beliefs about strenuous mental activity subscale score (between-subjects), which was mean-centered and standardized. Trial type was coded as (-1 for the congruent trials and 1 for the incongruent trials). *P*-values for linear mixed effects models are calculated by lme4 package in R, based on Satterthwaite's approximations to degrees of freedom. *P*-values for binomial logistic mixed effects models are calculated using Wald's *Z* statistic as estimated by the lme4 package in R.

7. Plotting the Significant Interaction Between Condition and Willpower Mindset Observed for the Block 1 Error Data

As noted in the main article and summarized in Table 2, the error data in Block 1 (the direct replication block) of the main Stroop task produced a significant interaction effect between Condition (control vs. depletion) and willpower mindset (treated as a continuous variable). Because this is exactly where JDW2010 observed the hypothesized willpower-moderation effect (i.e., error data in the Stroop block with a 50-50 mixture of congruent and incongruent trials), we graphically plotted the results. The **Figure S4A** below illustrates this two-way interaction effect, but, as elaborated in the main article, this interaction pattern is the opposite of what JDW2010 hypothesized (i.e., among individuals holding a limited-willpower mindset, those in the depletion condition demonstrated a *smaller* Stroop interference effect than those in the control condition).

Moreover, a close inspection of the individual data points revealed that this significant interaction effect was due to one extreme outlier, as shown in **Figure S4B** (circled in blue). When this extreme outlier was removed from analyses, the interaction term was no longer significant, as reported in the main article.

Figure S4. (A) A visual plot of the significant Condition x Mindset interaction observed for the Block 1 error data in fixed-effects modeling (see Table 2 in the main article). The y axis represents the Stroop interference effect, computed as the percentage of errors made in incongruent trials minus the percentage of errors made in congruent trials. The curves around the lines represent 95% confidence intervals. Notably, the significant interaction pattern plotted here was the opposite of what was reported in the original JDW2010 study; specifically, among participants in the ego depletion condition, an unlimited willpower mindset was associated with a higher Stroop interference effect (i.e., worse self-control performance). **(B)** A visual plot of the same interaction that is plotted on the left, but shows all the individual data points. As highlighted in the plot, there was a clear outlier on the top-left corner (also visible in the raincloud plot provided in Figure S3A). When this extreme outlier was removed from analysis, the interaction becomes nonsignificant.

8. Results of Secondary Analyses with the Trait Self-Control Measure

In this section, we present the results of both fixed-effects and mixed-effects analyses we conducted for the trait self-control measure (we used the same models, but, instead of willpower mindset, we included trait self-control). **Table S7A** reports the results of the fixed-effects analyses, and **Table S7B** reports the results of mixed-effects analyses. The descriptive statistics for these questionnaire measures are provided in Table S1 above. As is clear from both Table S7A and S7B, there was no evidence for either the main effect of trait self-control on Stroop performance or the moderation of the ego-depletion effect by trait self-control.

Table S7A

Results from fixed-effect regression models using trait self-control as the target moderating variable.

	Stroop Interference (Error)				Stroop Interference (Log RT)			
	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>
Block 1								
Intercept	3.794	0.421	2.963–4.624	<0.001	0.166	0.005	0.155–0.177	<0.001
Condition (C)	-0.691	0.421	-1.522–0.139	0.102	-0.002	0.005	-0.013–0.009	0.722
TSC	0.408	0.422	-0.425–1.241	0.335	-0.001	0.005	-0.011–0.010	0.919
C x TSC	0.032	0.422	-0.800–0.865	0.939	0.003	0.005	-0.007–0.014	0.539
Block 2								
Intercept	2.524	0.353	1.828–3.220	<0.001	0.170	0.005	0.160–0.179	<0.001
Condition (C)	-0.252	0.353	-0.948–0.444	0.476	-0.002	0.005	-0.011–0.008	0.723
TSC	-0.044	0.354	-0.742–0.654	0.902	-0.002	0.005	-0.011–0.008	0.756
C x TSC	0.090	0.354	-0.608–0.788	0.799	-0.004	0.005	-0.013–0.006	0.460

Note. *B* = unstandardized regression parameters. *SE* = standard errors for *B*. *CI* = 95% confidence-intervals for *B* estimates. Condition (between-subjects) was coded as -1 for the control condition and 1 for the depletion condition. TSC = the trait self-control scale score (between-subjects), which was mean-centered and standardized.

Table S7B*Results from mixed-effect regression models using Trait Self-Control as an independent variable*

<i>Predictors</i>	Odds of Incorrect Response				Log RT			
	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>
Block 1								
Intercept	0.003	0.496	0.001–0.009	<0.001	6.632	0.012	6.609–6.656	<0.001
Condition (C)	1.089	0.178	0.768–1.543	0.634	0.009	0.011	-0.011–0.030	0.375
TSC	1.107	0.182	0.774–1.582	0.578	-0.002	0.011	-0.023–0.019	0.858
Trial Type (T)	8.208	0.490	3.140–21.459	<0.001	0.083	0.006	0.071–0.095	<0.001
C x TSC	1.117	0.183	0.781–1.598	0.544	0.003	0.011	-0.018–0.024	0.765
C x T	0.824	0.164	0.598–1.136	0.237	-0.001	0.003	-0.006–0.004	0.735
TSC x T	1.014	0.168	0.729–1.409	0.936	-0.000	0.003	-0.006–0.005	0.929
C x TSC x T	0.909	0.168	0.654–1.265	0.572	0.002	0.003	-0.004–0.007	0.561
Block 2								
Intercept	0.008	0.185	0.005–0.011	<0.001	6.601	0.011	6.579–6.622	<0.001
Condition (C)	0.981	0.107	0.796–1.210	0.859	0.012	0.010	-0.008–0.031	0.239
TSC	1.011	0.109	0.817–1.251	0.918	0.001	0.010	-0.018–0.021	0.888
Trial Type (T)	2.354	0.175	1.670–3.318	<0.001	0.085	0.006	0.074–0.096	<0.001
C x TSC	1.027	0.109	0.830–1.271	0.804	0.001	0.010	-0.018–0.021	0.889
C x T	0.962	0.096	0.797–1.161	0.685	-0.001	0.002	-0.006–0.004	0.741
TSC x T	0.994	0.098	0.820–1.204	0.948	-0.001	0.002	-0.006–0.004	0.733
C x TSC x T	0.980	0.098	0.809–1.188	0.840	-0.002	0.002	-0.007–0.003	0.452

Note. *B* = Unstandardized regression parameters. *OR* = Unstandardized odds ratios. *SE* = Standard errors for *OR* and *B*. *CI* = 95% confidence-intervals for *B* estimates. Condition (between-subjects) was coded as -1 for the control condition and +1 for the depletion condition. TSC = the trait self-control scale score (between-subjects), which was mean-centered and standardized. Trial type was coded as (-1 for the congruent trials in Block 1 and the neutral (asterisk) trials in Block 2 and 1 for the incongruent trials for both Blocks 1 and 2). *P* values for binomial logistic mixed-effects models (for the error data) were calculated using Wald's *Z* statistic as estimated by the lme4 package in R. *P* values for linear mixed-effects models (for the log RT data) are calculated by lme4 package in R, based on Satterthwaite's approximations to degrees of freedom.

9. Results of Secondary Analyses with the Full Implicit Theories of Willpower Scale

In this section, we present the results of both fixed-effects and mixed-effects analyses we conducted using the full implicit theories of willpower scale. Note that this scale consisted of two subscales: (a) beliefs about strenuous mental activities and (b) beliefs about resisting temptations. Our target study (JDW2010 Study 1) focused on only the first subscale, but one study in that paper (Study 4) used the full scale. For this reason, we also conducted the same fixed-effects and mixed-effects analyses using the full scale.

Table S8A reports the results of the fixed-effects analyses (see Table S1 above for the descriptive statistics). There was no evidence for the hypothesized moderation effect of willpower mindset on ego depletion even when the full scale was used.

Table S8A

Results from fixed-effect regression models using the combined subscales of the willpower scale as an independent variable.

	Stroop Interference (Error)				Stroop Interference (Log RT)			
	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>
Block 1								
Intercept	3.755	0.420	2.927–4.583	< 0.001	0.166	0.005	0.155–0.177	< 0.001
Condition (C)	-0.673	0.420	-1.502–0.155	0.111	-0.002	0.005	-0.013–0.009	0.732
Mindset Full Scale (<i>M_{FS}</i>)	-0.108	0.430	-0.956–0.740	0.802	-0.001	0.006	-0.012–0.010	0.815
C x <i>M_{FS}</i>	0.625	0.430	-0.223–1.472	0.148	0.004	0.006	-0.007–0.015	0.517
Block 2								
Intercept	2.495	0.350	1.804–3.186	< 0.001	0.170	0.005	0.160–0.179	< 0.001
Condition (C)	-0.233	0.350	-0.924–0.458	0.506	-0.002	0.005	-0.011–0.008	0.741
Mindset Full Scale (<i>M_{FS}</i>)	-0.316	0.358	-1.023–0.391	0.379	-0.002	0.005	-0.012–0.008	0.626
C x <i>M_{FS}</i>	0.493	0.358	-0.214–1.200	0.170	-0.001	0.005	-0.011–0.009	0.854

Note. *B* = unstandardized regression parameters. *SE* = standard errors for *B*. *CI* = 95% confidence-intervals for *B* estimates. Condition (between-subjects) was coded as -1 for the control condition and 1 for the depletion condition. Mindset Full Scale = the average of the two subscales (beliefs about strenuous mental activity and beliefs about resisting temptations) from the implicit theories of willpower (between-subjects), which was mean-centered and standardized.

Table S8B reports the results of mixed-effects analyses. As shown in the table, the mixed-effects analyses produced more significant results than those observed in the analyses reported in the main article that focused on the first subscale only (compare Table S8B with Table 3). In particular, compared to the analysis based on just the first subscale reported in the main article, the full willpower-mindset scale more consistently predicted both error rates and log RTs (i.e., stronger beliefs in unlimited willpower was associated with lower

errors and shorter RTs). Additionally, full-scale willpower mindset significantly moderated the effect of trial type for the Block 2 error data. However, there was no evidence for the hypothesized moderation effect (no three-way interactions).

Table S8B

Results from mixed-effect regression using the full willpower-mindset scale as the target moderating variable.

<i>Predictors</i>	Odds of Incorrect Response				Log RT			
	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>CI</i>	<i>p</i>
Block 1								
Intercept	0.004	0.400	0.002–0.008	<0.001	6.632	0.012	6.609–6.655	<0.001
Condition (C)	1.047	0.175	0.743–1.474	0.794	0.011	0.010	-0.009–0.032	0.283
Mindset Full Scale (M _{FS})	1.389	0.172	0.992–1.947	0.056	-0.029	0.011	-0.050– -0.008	0.008
Trial Type (T)	7.339	0.392	3.404–15.823	<0.001	0.083	0.006	0.071–0.095	<0.001
C x M _{FS}	1.220	0.169	0.875–1.700	0.240	0.000	0.011	-0.020–0.021	0.963
C x T	0.848	0.160	0.620–1.160	0.303	-0.001	0.003	-0.006–0.004	0.741
M _{FS} x T	0.804	0.156	0.593–1.091	0.161	-0.000	0.003	-0.006–0.005	0.860
C x M _{FS} x T	0.951	0.153	0.705–1.284	0.745	0.002	0.003	-0.004–0.007	0.512
Block 2								
Intercept	0.008	0.184	0.005–0.011	<0.001	6.601	0.011	6.579–6.622	<0.001
Condition (C)	0.968	0.109	0.781–1.199	0.765	0.013	0.010	-0.006–0.032	0.183
Mindset Full Scale (M _{FS})	1.251	0.110	1.009–1.551	0.042	-0.021	0.010	-0.041– -0.001	0.036
Trial Type (T)	2.350	0.174	1.672–3.302	<0.001	0.085	0.006	0.074–0.096	<0.001
C x M _{FS}	1.088	0.109	0.878–1.347	0.441	-0.002	0.010	-0.022–0.018	0.830
C x T	0.973	0.097	0.804–1.177	0.778	-0.001	0.002	-0.006–0.004	0.758
M _{FS} x T	0.824	0.097	0.682–0.997	0.046	-0.001	0.003	-0.006–0.004	0.613
C x M _{FS} x T	1.042	0.096	0.862–1.259	0.671	-0.000	0.003	-0.005–0.005	0.890

Note. *B* = unstandardized regression parameters. *OR* = Unstandardized odds ratios. *SE* = Standard errors for *OR* and *B*. *CI* = 95% confidence-intervals for *B* estimates. Condition (between-subjects) was coded as -1 for the control condition and 1 for the depletion condition. Mindset Full Scale = the full implicit theories of willpower scale score (between-subjects), calculated as the average of the two subscales and then mean-centered and standardized. Trial type was coded as -1 for the congruent trials in Block 1 and the neutral (asterisk) trials in Block 2 and 1 for the incongruent trials for both Blocks 1 and 2. *P* values for binomial logistic mixed-effects models (for the error data) were calculated using Wald's *Z* statistic as estimated by the lme4 package in R. *P* values for linear mixed-effects models (for the log RT data) are calculated by lme4 package in R, based on Satterthwaite's approximations to degrees of freedom.

10. A List of Data Files and Annotated Data-Analysis Codes (with R) Available on our OSF Project Site

The data files and R codes that we used for the analyses reported in the main article and in this supplement are available at the project OSF site (currently available for reviewing purposes only, but the URL link will be made public upon acceptance/publication of this article). Specifically, we make the following files available on the OSF site:

Data-Analytic Codes (R Codes)

- **DataPreprocessing.html**
 - HTML file documenting R code for processing raw data to generate processed data files that are properly formatted for the analyses reported in the main article and in this supplement (unless one wants to replicate all the steps we took in our analyses starting with the preprocessing of the raw data files, this file could be skipped).
- **MainAnalyses.html**
 - HTML file documenting R code for analyses detailed in main text (i.e., the codes used for the analyses reported in Tables 1, 2, and 3)
- **SupplementalAnalyses.html**
 - HTML file documenting R code for analyses and figures detailed in the supplemental materials
- **AdditionalAnalyses.html**
 - HTML file documenting R code for additional/alternative analyses briefly mentioned but not detailed in the main text or in this supplement (i.e., no corresponding tables are provided).

All of the files are annotated to facilitate the comprehension of what we did in our analyses. The two most important files are MainAnalyses.html and SupplementalAnalyses.html. The R-code provided in these two files allow interested readers to replicate all the analyses reported in the tables/figures presented in the main article (MainAnalyses.html) and in this supplementary file (SupplementalAnalyses.html).

Data Files

- **LongDataRaw.csv**
 - Raw long-form data file
- **QuestionnaireDataRaw.csv**
 - Raw wide-form data file
- **LongDataProcessedNoExclusions.csv**
 - Processed long-form data file
- **QuestionnaireDataProcessedNoExclusions.csv**
 - Processed wide-form data file
- **LongDataProcessedRTExclusions.csv**
 - Processed long-form data file excluding trials with unusual RTs
- **QuestionnaireDataProcessedRTExclusions.csv**
 - Processed wide-form data file excluding trials with unusual RTs

Note that we provide three types of data files (*raw* data files, *processed* data files used for our main analyses, and the same processed data files that *excluded extreme RTs*), each in two different formats (*long-form* and *wide-form*). We explain how these six files differ from each other below,

but the data files used for most of the analyses reported in the main article and in this supplement are based on the data available in the following two files: LongDataProcessedNoExclusions.csv and QuestionnaireDataProcessedNoExclusions.csv.

To facilitate the understanding of the entries in these data files, we also provide, on our OSF project site, *two codebooks*, one for all the long-form data files (**CodebookforLongForm**) and the other for all the wide-form data files (**CodebookforWideForm**).

Raw data files. The raw data are presented in two formats. The first format is a *long-form* format with *one row per trial* and thus multiple rows for each participant. This data file was a compilation of data outputs from the PsyScope software (for Stroop) and contained primarily information about trial composition (e.g., trial type) and participant performance (e.g., RT). In this data file, the voice-key error trials as well as actual error trials (e.g., incorrectly reading the word for an incongruent trial) are also marked. The second file was a *wide-form* data file, with *one row per participant*. This data file included participants' questionnaire responses (e.g., the responses for the willpower mindset measures) and condition information. Information from both files was required to perform the analyses we conducted for this study, and we used these raw data files to create processed versions of the data files (by running the R code provided in the DataPreprocessing.html file).

Processed data files. We created a processed version of the *wide-form* data file incorporating performance information from the long-form data file (e.g., average error rates or RTs). We used this wide-form data file to perform *fixed-effects* analyses (e.g., the analyses reported in Table 2 of the main article). Note that, because each participant's data are represented in one row in this wide-form data file, this file contains only the mean values (e.g., error rate, RT) for the relevant Stroop conditions (e.g., the congruent vs. incongruent trials for Block 1), calculated by applying the R code contained in the DataPreprocessing.html file. Additionally, we created a processed version of the *long-form* data file incorporating condition and individual difference information from the wide-format data file. This long-form data file was used primarily for our *mixed-effects* analyses (e.g., the analyses reported in Table 3 of the main article).

Both of these processed data files also include a number of transformed (e.g., *z*-scored, log-transformed) variables that were necessary for some of our analyses. The two codebooks provided at the project OSF site explains these transformed variables in the processed data files.

Processed data files without extreme RTs. For each of these processed data files, we also created an almost exact copy that excluded information from trials with reaction times below 200 ms or above 5,000 ms from all calculations. These files were used to confirm that excluding such trials did not qualitatively affect the results of our analyses (this analysis is briefly noted in the Data-Exclusion and Data-Transformation Considerations section of the main article).

Additional notes. All six data files we made available on our OSF site do NOT include the data from seven participants who met our preregistered exclusion criteria (see the Participants section of the main article). Their PsyScope output files are available on the OSF site, however, unless a computer crash during the main Stroop task was the reason for their exclusions.

Finally, although we do not provide separate files that excluded some additional participants (e.g., seven participants who did not indicate their age or were just outside the original age range, one extreme outlier in the Block 1 Stroop task noted in Figure S3A and Figure S4B in this supplement), their participant numbers are clearly indicated in a summary Excel document called **ExclusionSummary** (also available on the project OSF site). With this information, reproducing the analyses that exclude those participants should be straightforward.