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Does Self-Control Improve With Practice? Evidence from a 6-Week Training Program

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Abstract

Can self-control be improved through practice? Several studies have found that repeated practice of tasks involving self-control improves performance on other tasks relevant to self-control. However, in many of these studies, improvements after training could be attributable to methodological factors (e.g., passive control conditions). Moreover, the extent to which the effects of training transfer to real-life settings is not yet clear. In the present research, participants (N = 174) completed a 6-week training program of either cognitive or behavioral self-control tasks. We then tested the effects of practice on a range of measures of self-control, including lab-based and real-world tasks. Training was compared to both active and no-contact control conditions. Despite high levels of adherence to the training tasks, there was no effect of training on any measure of self-control. Trained participants did not, for example, show reduced ego depletion effects, become better at overcoming their habits, or report exerting more self-control in everyday life. Moderation analyses found no evidence that training was effective only among particular groups of participants. Bayesian analyses suggested that the data was more consistent with a null effect of training on self-control than with previous estimates of the effect of practice. The implication is that training self-control through repeated practice does not result in generalized improvements in self-control.

Keywords: self-control, self-regulation, intervention, ego depletion, self-control training

Does Self-Control Improve With Practice? Evidence from a 6-Week Training Program

Self-control, or the ability to control thoughts, behaviors, and feelings, seems to be important for success in most areas of life (De Ridder, Lensvelt-Mulders, Finkenauer, Stok, & Baumeister, 2012). During the past 15 years, much of the research into self-control has been inspired by the strength model, which draws the analogy between self-control and a physical muscle (for a review, see Muraven & Baumeister, 2000). This model proposes that, just as using a muscle leads to temporary fatigue, exerting self-control leads to temporary reductions in self-control performance; a phenomenon that has been termed ‘ego depletion’ (Baumeister, Bratslavsky, Muraven, & Tice, 1998). However, the strength model also suggests that if self-control is repeatedly exerted over time (interspersed with periods of rest), then the opposite effect should occur. In other words, just as a muscle grows stronger with exercise, so self-control should improve over time with practice (Muraven, 2010a; Muraven, Baumeister, & Tice, 1999).

The ego depletion effect has been the subject of hundreds of empirical tests, extensive analysis of mediating and moderating factors, and much lively theoretical debate (e.g., Hagger, Wood, Stiff, & Chatzisarantis, 2010; Inzlicht & Schmeichel, 2012; Carter, Kofler, Forster, & McCullough, 2015). However, the strength model’s predictions about the long-term effects of exerting self-control have received less attention. If practicing self-control improves subsequent self-control, as has been suggested by several studies (e.g., Muraven, 2010a; Muraven et al., 1999), then self-control training could benefit people facing everyday self-regulatory struggles such as controlling emotions, breaking bad habits, and overcoming impulses. Yet, there are many things that we do not yet know about the effects of training self-control through practice (see Berkman, in press; Inzlicht & Berkman, 2015; Inzlicht, Schmeichel, & Macrae, 2014, for discussion). For example, how reliable are training effects? Can we be sure that the observed improvements are the result of practicing self-control? Perhaps most importantly from an applied perspective, does training self-control indeed influence real-life outcomes that depend on self-control? The present research sought to address these unanswered questions.

What Do We Already Know About the Effectiveness of Self-Control Training?

Table 1 provides an overview of prior studies of self-control training. Studies investigating the effect of self-control training typically ask one group of participants to perform a task requiring self-control over a period of weeks, while another group performs either no task, or a task that does not require self-control. The performance of the two groups is then compared on a subsequent self-control task, usually different to the one that was trained. A wide range of tasks has been used to train self-control, such as using one's non-dominant hand, developing and executing a personalized study or exercise program, completing the Stroop task, squeezing a handgrip, or performing a logical reasoning task. The effect of training has also been assessed using a wide range of tasks, such as tolerating pain, inhibiting aggressive inclinations or behavior, ignoring a distracting video while performing a visual tracking task, solving anagrams, or holding a handgrip. The common feature of all of these tasks is that they are believed to require self-control.

How effective are these interventions in improving self-control performance? Hagger et al. (2010) meta-analyzed the findings of studies that measured the effects of training on ego depletion (i.e., performance on the second of two sequential tasks involving self-control). Across 9 tests, taken from 7 published papers, they observed that training significantly reduced ego depletion, with an overall effect size of $d_+ = 1.07$. This suggests that practicing self-control is an extremely effective intervention. To put this effect size in context, it places the effect of self-control training on ego depletion at roughly the 95^{th} percentile in terms of both the average effect size of psychological interventions (Lipsey & Wilson, 1993) and the average effect size observed in social psychology (Richard, Bond, & Stokes-Zoota, 2003). Effect sizes from individual studies ranged from medium (d = 0.48, Hui et al., 2009) to extremely large (d = 8.59, Oaten & Cheng, 2006a).

However, close inspection of previous findings suggests that there is variation in the effect of training across studies. For example, some studies found effects of training on outcomes relevant to self-control only after participants had exerted self-control on an initial task (Finkel, DeWall, Slotter, Oaten, & Foshee, 2009; Gailliot, Plant, Butz, & Baumeister, 2007; Oaten & Cheng, 2007). Other studies obtained effects of training only among subgroups of participants – typically those with a predisposition to perform worse on the self-control task, such as participants with higher levels of trait aggression (Denson, Capper, Oaten, Friese, & Schofield, 2011; Gailliot et al, 2007). Finally, some studies found no improvements after training on outcomes relevant to self-control. For example, Muraven et al. (1999) found that trying to improve mood, a task that requires self-control, led to worse performance on a measure of self-control when compared with control groups. Bertrams and Schmeichel (2014) found that participants who spent a week practicing logical reasoning subsequently performed worse than a control group on an initial anagram task, and performed no better than the control group on the same task when depleted. Other unpublished studies have also observed null effects of training (e.g., Davisson, 2013; Klinger, 2013).

It is also important to exercise caution when interpreting these findings, for a number of reasons. One of these reasons is that the majority of studies on self-control training have used passive control groups; in other words, they compared the effect of practicing self-control to the effect of doing nothing (see Table 1). This is not an ideal design for testing the effects of an intervention, as any improvements in the trained group are potentially attributable to factors other than training itself. For example, participants may improve because they are treated differently from the control group, because they have different expectations about their improvement, or because they believe that the experimenter has different expectations about their improvement. In other words, any improvements in self-control in the trained group may be attributable to Hawthorne effects, placebo effects, or demand characteristics (see Shipstead, Redick, & Engle, 2012, for similar criticisms of studies training executive function).

This problem is compounded when self-control performance is assessed using subjective measures that are especially susceptible to such influences (Greenwald, Spangenberg, Pratkanis, & Eskenazi, 1991; Shipstead et al., 2012), and when adherence to the training task is not assessed, which makes it difficult to confirm that completion of the training task is responsible for any observed improvements in the trained group. Although most previous studies asked participants to keep records of task performance, the methods used may not have reliably assessed adherence (e.g., paper diaries submitted after training; see Table 1), and rates of adherence generally were not reported, presumably because these records were designed more to motivate adherence than to measure it. The upshot of these methodological decisions is that the effect of training on self-control performance may have been over-estimated in previous studies.

It is also possible that the data in published studies represents only a subset of the total data on the effect of self-control training. Evidence from various sources suggests that statistically significant findings are more likely to be published, both within studies (e.g., selective reporting of variables with positive effects; Franco, Malhotra, & Simonovits, 2016), or across studies (e.g., studies with null effects are less likely to be published; Franco, Malhotra, & Simonovits, 2014). Both factors would mean that the published literature overestimates the true effect of self-control training. A recent meta-analysis by Inzlicht and Berkman (2015) attempted to quantify the influence of publication bias on the self-control training literature by providing a 'p-curved' estimate of the training effect, employing an analysis which uses the distribution of p values in a set of studies to estimate the true size of the underlying effect. Findings suggested that after correcting for possible publication bias, the true effect of training could be as small as $d_+ = 0.17$. This analysis suggests that the effect of training may not be as robust as previously thought. However, as corrections for publication bias can only estimate the impact of missing data on the observed effect, the only way to establish the true effect is through adequately-powered empirical research (Inzlicht & Berkman, 2015). In sum, variability in previously reported training effects, methodological confounds in previous studies, and the possibility of publication bias all converge to indicate the need for research that establishes the reliability of training effects using rigorous methodology.

Does Training Generalize Beyond Ego Depletion Effects?

Aside from questions about the true size of the training effect, another limitation of previous research on self-control training is that studies have typically employed lab-based measures as outcomes, such as persistence in solving anagrams (Bertrams & Schmeichel, 2014; Gailliot et al., 2007) or performing a visual tracking task under distraction (Oaten & Cheng, 2006a, 2006b, 2007). With the exception of Oaten and Cheng's (2006a, 2006b, 2007) questionnaire measures of self-control behavior and Muraven's (2010b) study of the effects of self-control training on smoking cessation, all of the evidence to date concerning the effects of self-control training has been based on participants' performance in laboratory settings. Whether performance gains on such tasks translate into improvements in self-control outside the laboratory is largely an open question. Self-control training has the potential to help people to overcome their everyday self-regulatory challenges, but this potential can only be realized if improvements in self-control transfer from the trained task to the struggles that people experience in their everyday lives, and not simply to other experimental tasks.

Evidence from studies of executive function training suggests that we should not automatically expect such transfer. Executive function training studies investigate whether or not it is possible to train cognitive abilities such as working memory and inhibition through practice. This type of training is conceptually similar to self-control training, because executive functions are thought to subserve self-regulation (Hofmann, Schmeichel, & Baddeley, 2012). However, it is distinct in that researchers in these two areas are typically interested in different outcomes (improvements in cognitive abilities vs. improvements in performance on unrelated tasks that require self-control). Researchers studying the effects of training executive functions have highlighted the importance of ensuring that training-related improvements do not merely reflect task-specific learning, but indicate genuine improvement in the underlying ability being trained – which should then generalize to unrelated tasks that draw upon this ability. Enriquez-Geppert, Huster, and Herrmann (2013) distinguished between different levels of generalization: modality transfer (improvements on the same task used in training, but using new stimuli), near transfer (improvements in other tasks targeting the same domain as the training task), far transfer (improvements in tasks targeting another domain), and meta-cognitive transfer (improvements in "everyday behavior, health, or overall quality of life," p. 4). While there is some controversy about the extent to which executive function training generalizes, highly-powered studies comparing the effects of training working memory with active control conditions have observed little generalization beyond near transfer (e.g., Redick et al., 2013), and a recent meta-analysis concluded that there was "no convincing evidence" for generalization beyond near transfer

(Melby-Lervåg & Hulme, 2013, p. 270). In other words, trained participants demonstrate improvement in the trained ability and on closely related tasks, but do not seem to reliably improve on other tasks that are supposedly related to the trained ability.

Given the theoretical and conceptual overlap between executive function training and self-control training, it is puzzling that studies training self-control and studies training executive functioning have observed such different effects. Typically, self-control training studies have found that effects generalize to both similar and dissimilar tasks (e.g., avoiding sweets for two weeks appears to improve both participants' ability to avoid cigarettes and their performance on a computer-based cognitive task; Muraven, 2010a, 2010b). If training executive functioning is conceptually similar to training self-control, why would one type of training generalize widely while the other does not? One possibility is that the disparity is due to differences in the methods used to investigate the effects of training, rather than to differences in the underlying effects of training. Specifically, studies investigating the effects of training executive functions, which typically find that effects do not generalize, tend to have active control conditions and stringent measures of adherence and training performance (e.g., Harrison et al., 2013; Redick et al., 2013). In contrast, studies investigating the effects of training self-control, which typically find that effects do generalize, tend to have passive control conditions and fail to measure adherence or training performance. Thus, it is possible that the wide-ranging benefits observed after self-control training could be attributable to confounds such as Hawthorne effects, placebo effects, and demand characteristics. Clearly, there is a need for research that assesses the extent to which training self-control leads to generalized improvements in self-control using methodologically rigorous designs.

The Present Research

As we have seen, the evidence supporting the effectiveness of self-control training may be less robust than it initially appears. In particular, the use of passive control conditions, reliance on laboratory-based measures of self-control, variability in observed effects, and possible publication bias each offer grounds for caution in concluding that self-control can be improved through practice. The primary goals of the present research were, therefore, to (a) investigate the effects of self-control training using a rigorous methodology that rules out alternative explanations for any observed effects, and (b) examine the presence and reliability of the effects of training across various measures of self-control. We conducted this assessment using appropriate control conditions, randomization checks, and measures of adherence; and examined the effects of training on a wide range of outcome measures in both laboratory and field settings.

A secondary goal was to gain a deeper understanding of the nature of self-control training effects. There are many unanswered questions about exactly how, why, and when practicing self-control leads to improvements in self-control. For example, a distinction has been drawn between practicing behaviors that require self-control, such as avoiding colloquialisms, and engaging in tasks that train an underlying cognitive ability thought to subserve self-control, such as training inhibition via the Stroop task (Berkman, Graham, & Fisher, 2012). Previous studies have generally assumed that these training tasks will have equivalent effects on self-control outcomes (either using them interchangeably, or combining them; e.g., Hui et al., 2009), but this assumption has yet to be tested. Similarly, the literature to date is largely silent about the mechanisms underlying the effects of training, and about whether training is more effective for some people than for others. The broader literature on self-control suggests some plausible candidates as mediators and moderators: For example, trait levels of self-control, beliefs about self-control, and executive functions have each been shown to predict self-control outcomes in everyday life (e.g., Tangney, Baumeister, & Boone, 2004; Job, Walton, Bernecker, & Dweck, 2015; Hofmann, Schmeichel, et al., 2012). Thus, assessing these variables at baseline and tracking whether and how they change over the course of training may help us to identify both the mechanisms underlying self-control improvement, and the particular groups that might experience greater improvements. Thus, to investigate the processes underlying self-control training, we included pre- and post-training measures of these potential mediators and moderators, and trained self-control using both cognitive and behavioral tasks.

Method

Participants

Participants were undergraduate and postgraduate students at a UK university, recruited via email. Figure 1 shows the flow of participants through the study. Of the 185 participants who began the study, 174 participants (59% female) completed their assigned training program and attended their follow-up assessment (i.e., there was a 6% dropout rate). Eighty-nine participants were randomized to the two training groups (cognitive training, n = 45; behavior training, n = 44), and 85 were randomized to the two control groups (active control, n = 45; no-contact control, n = 40).

Power Analyses

For each dependent measure, our key aim was to assess the effect of self-control training (i.e., the effect of practicing self-control vs. not practicing self-control on the outcome of interest). As such, the critical comparison was between the participants in the training groups (who exerted self-control), and participants in the control groups (who did not exert self-control).

Therefore, power calculations were based on the ability to detect differences in performance between these two groups.

Power analyses based on the effect size estimate of $d_+ = 1.07$ from Hagger et al.'s (2010) meta-analysis of training effects on ego depletion indicated that 40 participants would be required, split between the training and control conditions, to achieve 90% power (two-tailed). However, because this estimate relates to only one of our dependent measures (the ego depletion effect), and because concerns have been raised over whether this figure overestimates the true effect size (e.g., Inzlicht & Berkman, 2015), we based our power calculations on 90% power to detect a medium-sized effect ($d_+ = 0.50$) between participants who received versus did not receive training (two-tailed). This power analysis indicated that 172 participants were required in total (86 training and 86 control). This sample size also affords 80% power to detect the average effect size observed across social psychological phenomena ($r = .21$, equivalent to $d_+ = 0.43$; Richard et al., 2003), and is nearly 3 times larger than the average sample size in previous studies of the effects of self-control training (see Table 1).[1]

Training Paradigm

We assessed the effectiveness of both a behavioral training task and a cognitive training task in two separate conditions, following recommendations that researchers should employ 'single-domain' training (Berkman et al., 2012). The behavioral training condition involved the task used most commonly in previous research, namely, using one's non-dominant hand for all daily activities (see Table 1). The cognitive training condition required participants to perform the Stroop task and the stop-signal task; both tasks involve inhibition (Miyake et al., 2000), the component of executive function that is most closely related to the prototypical definition of self-control (i.e., overriding unwanted impulses; Hoffmann, Schmeichel, et al., 2012).

We included two control conditions that allowed us to control for, and assess the impact of, confounding variables (e.g., the amount of contact that participants had with the experimenter, effort invested in the study, and expectations about improvement). Participants in the active control condition undertook sets of math and linguistic problems each day (difficult tasks that involve effort and persistence, but – unlike the tasks designed to train self-control – do not require participants to inhibit responses), whereas participants in the no-contact control condition completed an online questionnaire each week about cognitive failures.

All participants worked on their assigned training program for six weeks (longer than the typical duration of training; Table 1). Participants with daily training tasks (i.e., all participants except those in the no-contact control condition) completed their tasks five days per week, from Monday to Friday. Participants either completed their tasks online (cognitive training, active control) or completed a daily online measure of task adherence (behavioral training), using a link provided to them by email, which enabled us to assess adherence for all conditions. At the end of each online session, participants completed items assessing their perceptions of the training task (the extent to which it required self-control, effort, and motivation). Participants completed these items again post-training, and also rated their perceived improvement in self-control (among rating their perceived improvement in various other skills and abilities, which served as distractor items). Further details about the training protocol for each condition are provided in the Supplemental Materials.

Dependent Variables

We examined the effects of training on a broad range of outcomes relevant to self-control. Inspired by Enriquez-Geppert et al. (2013), we classified our self-control measures as representing ‘near transfer’ to similar tasks (e.g., performance in a laboratory-based ego depletion paradigm), 'far transfer' to other conceptually-related tasks (e.g., intentional control of behavior), and 'meta-cognitive transfer' to important real-life outcomes (e.g., wellbeing). Figure 2 shows the dependent variables in each category. An overview of our measures is provided below, and further details about all dependent variables are provided in the Supplemental Materials.

Near transfer: Performance under ego depletion. As in previous research, we assessed the effect of training on the ego depletion effect (i.e., the extent to which performance on a self-control task was influenced by previous exertion of self-control). To provide a strict test of performance under depletion, we employed the 'severe depletion' paradigm (Vohs, Baumeister, & Schmeichel, 2012), in which participants completed a series of four consecutive tasks that each required self-control. Our key measure of ego depletion was persistence on a hand-grip task, measured before and after this set of depleting tasks.

Far transfer: Self-control behavior in the lab. Studies to date have tended to assess the effects of training on self-control via performance or persistence on demanding or tedious tasks. Another way to assess self-control in a laboratory setting is by simulating a real-life self-control dilemma (cf. Denson et al., 2011). As such, the present research measured participants' performance of two behaviors that have been shown to depend upon self-control resources, but have not yet been tested as training outcomes; eating chocolate and displaying prejudice. Chocolate consumption was measured during a task presented to participants as a measure of consumer decision making, and prejudice was measured using an unobtrusive test developed and validated by Webb (2011), in which participants could take advice from Asian or White targets.[2]

We tested for main effects of training on these behaviors, but also considered the hypothesis that training might improve self-control only among participants with a predisposition to engage in that behavior (cf. Govorun & Payne, 2006, Friese, Hofmann, & Wänke, 2008). We measured these predispositions by assessing implicit attitudes towards both targets (chocolate, Asian people) using Implicit Association Tests (IATs; Greenwald, McGhee, & Schwartz, 1998) and used these measures to assess whether training effects were moderated by implicit attitudes. We also assessed several other individual differences that might influence performance of these behaviors: namely, explicit attitudes, motivation to respond without prejudice, and dietary restraint (see Supplemental Materials for details).

Far transfer: Intentional versus habitual control of behavior. Research indicates that there is a substantial gap between people's stated intentions to act and their subsequent actions (Sheeran, 2002). This gap is especially large when people want to modify habitual behaviors (Ouellette & Wood, 1998; Webb & Sheeran, 2006), which are particularly difficult to inhibit. As self-control influences the extent to which individuals can overcome habits and enact intentions (De Ridder et al., 2012; Neal, Wood, & Drolet, 2013), it is possible that self-control training could reduce habitual control, and increase intentional control, over behaviors. We assessed this hypothesis by asking participants to complete measures of habit strength, intentions, and behavior for a wide variety of behaviors before and after training, including both behaviors that would usually be considered good habits (e.g., studying, tidying, eating fruit and vegetables) and bad habits (e.g., gossiping, drinking alcohol, skipping lectures). If self-control training helps people to translate good intentions into action, we might expect to observe not only main effects of training on the incidence of positive and negative behaviors and future intentions to engage in those behaviors, but also stronger prediction of behavior by intentions and weaker prediction by habits among trained participants.

Meta-cognitive transfer: Self-control behaviors in everyday life. To assess whether the effects of training transferred to real-life self-control efforts, we assessed the performance of everyday behaviors involving self-control by asking participants to complete daily reports on their performance of seven behaviors, and to provide specific details about either the frequency (e.g., number of alcoholic drinks) or duration (e.g., time spent watching TV or playing video games) of each behavior (for a retrospective measure of similar behaviors, see Oaten & Cheng, 2006a, 2006b). We also measured variables that have been shown to relate to behavioral enactment in studies using experience sampling (Hofmann, Baumeister, Förster, & Vohs, 2012; Hofmann, Vohs, & Baumeister, 2012), such as the strength of desire to perform each behavior. Finally, in keeping with the idea that successful self-control involves not only the effortful inhibition of unwanted behaviors but also the enactment of desired behaviors (cf. Fujita, 2011), we asked participants to report the amount of time that they spent engaging in behaviors such as exercising and studying daily, and to report their perceived success in achieving health, academic, and relationship goals each day.

Meta-cognitive transfer: Well-being. Greater self-control is associated with improved well-being (e.g., De Ridder et al., 2012) and training self-control might therefore be expected to enhance well-being. The present research operationalized well-being in terms of changes from pre- to post-training in positive and negative emotions and well-being, and included a measure of life satisfaction after training. Participants also completed measures concerning their use of emotion regulation strategies before and after training, as strategy use could mediate any effects of training on well-being. In particular, we hypothesized that self-control training may increase the resources available to employ strategies such as reappraisal (cf. Urry & Gross, 2010), a highly effective means of managing emotions (Webb, Miles, & Sheeran, 2012).

Potential mediators and moderators of training effects.

To discover more about how training works and for whom it works best, the present research measured three individual difference variables: trait self-control, implicit theories about willpower and temptation (i.e., the extent to which participants believe that willpower is a limited resource that can be depleted by resisting temptations), and executive function (assessed using three tasks measuring the components of inhibition, shifting, and updating; Miyake et al., 2000). All of these variables were measured both before and after training, as they constitute both potential moderators (pre-existing differences between participants could influence the effectiveness of training) and potential mediators (changes in these variables over time could help us to understand the mechanisms underlying training effects on self-control). Additional individual differences (impulsivity, regulatory focus, and conscientiousness) were also measured at baseline in order to confirm the success of our randomization procedure.[3]

Procedure

The study was presented to all participants as an investigation of whether "brain training" could enhance cognitive function. The rationale for the measures and training tasks did not mention self-control (e.g., the stated purpose of the behavioral training task was "to strengthen visuo-motor co-ordination, an ability that is often inhibited in people who have a cognitive impairment"). Participants could receive up to £65 (approximately $100) for participating in the study, depending on their performance, and could also earn entries into prize draws.

After random assignment to conditions, participants completed baseline assessments online. All participants then began their assigned training task on the same date in order to minimize variability due to time-of-semester effects. After completing six weeks of training, participants completed online assessments, a 7-day self-control diary, and attended the laboratory in person to complete follow-up assessments (detailed information about the timing of each assessment in relation to the training period can be found in the Supplemental Materials). Figure 3 shows the structure of the study.

Results

Randomization Checks

To establish whether participants in the training and control conditions were equivalent at baseline, we tested for group differences on all baseline measures relevant to self-control (see Supplemental Materials for descriptive statistics for all variables). Trained participants had higher levels of trait self-control at baseline, $t(172) = 2.24$, $p = .03$, $d_+ = 0.34$, and marginally higher levels of conscientiousness, $t(172) = 1.95$, $p = .05$, $d_+ = 0.30$, all other $ps > .08$. We therefore conducted all analyses of the effects of training on outcomes both with and without these variables as covariates; unless otherwise stated, the inclusion of these covariates did not alter our observed effects.

Manipulation Checks

To ensure that participants completed the training program to which they had been assigned, we assessed the number of sessions that participants in each condition completed. Participants in the cognitive training condition completed an average of 28 training tasks out of 30 ($M = 28.44$, $SD = 1.84$, range 23-30), participants in the behavioral training condition completed an average of 29 out of 30 reports concerning use of their non-dominant hand ($M = 29.18$, $SD = 1.65$, range 23-30), and participants in the active control condition completed an average of 29 tasks out of 30 ($M = 28.53$, $SD = 1.77$, range = 22-30). There were no differences between these conditions, $F(2, 131) = 0.43$, $p = .65$. Participants in the no-contact control condition completed an average of 4.70 out of 6 questionnaires ($SD = 1.91$, range = 0-6).

For the behavioral and cognitive training conditions, we also analyzed data from the training tasks, which allowed us to determine whether participants performed the training tasks correctly and whether performance improved over time. Findings from these analyses are reported in the Supplemental Materials and supported the idea that participants adhered to the training procedures (we did not exclude any participants on the basis of these analyses).

Perceptions of Training and Beliefs About Its Likely Effects

We first analyzed whether participants in the training and control conditions perceived the tasks as requiring equal amounts of effort and self-control and were equally motivated to undertake training. We analyzed both the overall judgments provided at the end of the study, and daily ratings provided at the end of each training task (by all participants except no-contact controls). Training participants perceived that their tasks required more self-control than control participants, as rated both at the end of training, $t(171) = 3.31$, $p = .001$, $d_+ = 0.50$, and on a daily basis, $t(132) = 3.09$, $p = .002$, $d_+ = 0.56$. Training participants also reported that their tasks required more effort, but only when rated retrospectively, $t(171) = 3.70$, $p < .001$, $d_+ = 0.56$, and not when rated immediately after each task, $t(132) = 0.00$, $p = 1.00$, $d_+ = 0.00$. Follow-up analyses suggested that these effects were driven by participants in the behavioral training group providing higher ratings of effort and self-control than participants in each of the other groups, $ts > 2.83$, $ps < .006$, $d_+ > 0.60$. Participants in the training and control conditions did not differ in their reported motivation to complete the tasks, either assessed retrospectively or daily, $ts < 1.61$, $ps > .11$, $d_+ < 0.30$.

Participants in the training and control conditions differed in the extent to which they felt that training had improved their self-control, $t(171) = 3.58$, $p < .001$, $d_+ = 0.54$. Follow-up comparisons showed that participants who received behavioral training believed that their self-control had improved to a greater extent than did participants in the other three conditions, ts > 2.51, ps < .02, $d_+ > 0.54$, which did not differ from one another, ts < 1.67, ps > .10, $d_+ < 0.36$.

Near Transfer

Our primary test of the effect of self-control training on ego depletion was change in persistence on the handgrip task from pre- to post-depletion (see Table 2). A repeated-measures ANOVA with time (pre- vs. post-test) as a within-participants IV, condition (training vs. control) as a between-participants IV, and handgrip performance as the dependent variable, revealed a significant effect of time, $F(1, 171) = 7.66$, $p = .01$, partial $\eta^2 = .04$, but no significant effect of condition, $F(1, 171) = 3.37$, $p = .07$, partial $\eta^2 = .02$, and no interaction between condition and time, $F(1, 171) = 0.03$, $p = .86$, partial $\eta^2 < .001$. Participants held the handgrip for longer after completing the four self-control tasks (M = 56.96, SD = 49.65) than before doing so (M = 48.14, SD = 45.09), d+ = -0.30, but this change in performance over time did not differ between training and control participants. In other words, we neither observed the standard effect of ego depletion, nor did we find any influence of self-control training on the ego depletion effect.

While the main purpose of asking participants to complete a series of self-control tasks was to increase the likelihood that self-control performance would be subsequently impaired (as Vohs et al., 2012, observed), ego depletion studies typically find that self-control performance suffers after completing only a single self-control task, and previous training studies have found that training attenuates this effect. Thus, we might expect to observe performance differences between trained and control groups not only after the depleting tasks, but also within the series of depleting tasks, and these differences might become more pronounced as participants complete more self-control tasks. We therefore computed standardized performance scores for each of the four tasks, and assessed whether condition influenced performance across the series of tasks. A repeated-measures ANOVA with task (first, second, third, fourth) as a within-participants IV, condition (training vs. control) as a between-participants IV, and performance as the dependent variable showed no effect of condition, $F(1, 171) = 0.22$, $p = .64$, partial $\eta^2 = .001$, and no interaction between condition and task, $F(3, 513) = 0.27$, $p = .85$, partial $\eta^2 = .002$. Thus, consistent with the findings of the effect of training on handgrip performance, these analyses suggest that training did not affect self-control performance. MANOVA also showed no effect of condition on participants' judgments of the amount of effort or self-control required by each of the depleting tasks or how motivated they were to perform the tasks $F(3, 167) = 1.14$, Wilk's $\Lambda = 0.98$, $p = .34$, partial $\eta^2 = .02$.

Far Transfer

To determine whether training influenced the performance of behaviors requiring self-control, we examined our measures of chocolate consumption and prejudice.[4] Participants in the training and control conditions did not eat different amounts of chocolate, $t(167) = 0.47$, $p = .64$, $d_+ = .07$, nor were they more biased toward Asian targets in the advice taking task, $t(156) = 0.01$, $p = .99$, $d_+ = .002$ (see Table 2). We also conducted hierarchical regression analyses to examine whether the association between implicit attitudes and behavior was moderated by training. Implicit attitudes toward Asian people (standardized scores) and a dummy-coded variable for training condition (0 = control groups, 1 = training groups) were entered in the first step of an analysis to predict prejudiced behavior; however, neither variable significantly predicted prejudice ($ps > .79$). At the second step, the interaction term was entered, which was also non-significant, $\beta = -0.05$, $t(154) = -0.45$, $p = .65$. The same analysis with the amount of chocolate eaten as the dependent variable also revealed that neither implicit attitudes nor condition significantly predicted chocolate consumption (ps > .65).[5] At the second step, the interaction term for these two predictors was also non-significant, β = -0.06, t(165) = -0.48, p = .63.

To assess whether training increased participants' intentions to engage in positive behaviors (e.g., studying, tidying, eating fruit and vegetables) and decreased participants' intentions to engage in negative behaviors (e.g., gossiping, drinking alcohol, skipping lectures), we performed a repeated-measures MANOVA on intentions to engage in the 27 measured behaviors. For this analysis, all measures were coded so that higher scores indicated 'good' intentions (e.g., intentions to binge drink were coded such that higher scores represented weaker intentions to engage in the behavior, whereas for studying higher scores indicated stronger intentions). The within-participants IV was time (pre- vs. post-training) and the between-participants IV was condition (training vs. control). There was no effect of condition, F(27, 145) = 1.19, Wilk's Λ = 0.82, p = .26, partial η^2 = .18, indicating that participants in the training and control conditions had similar intentions. There was, however, a significant effect of time, F(27, 145) = 3.88, Wilk's Λ = 0.58, p < .001, partial η^2 =.42, indicating that participants generally reported stronger intentions to engage in good behaviors and avoid bad behaviors at the end, as compared to at the start, of the study. The interaction between condition and time was non-significant, F(27, 145) = 1.24, Wilk's Λ = 0.81, p = .21, partial η^2 =.19, indicating that this positive change in intentions from pre- to post-training was similar in both conditions.

In order to assess whether training changed participants' behavior, we carried out a MANOVA with performance of each the 27 behaviors at post-test as dependent variables and condition (training vs. control) as the between-participants IV. All measures were coded so that positive scores reflected greater performance of the behavior in question. As there were some extreme outliers, all values ±3 SD from the mean for each behavior were replaced with the next most extreme value (resulting in the replacement of 50 values, or 1.1% of the total). There was no effect of condition on behavior, $F(27, 141) = 0.99$, Wilk's $\Lambda = 0.84$, $p = .48$, partial $\eta^2 = .16$, indicating no overall difference in behavior between participants in the training and control conditions.[6]

We then investigated whether habit strength and intentions were differentially predictive of behavior between conditions. Following Danner, Aarts, and de Vries (2008), a measure of the extent to which each behavior was habitual was created by converting the rating for context stability to a -4 to +4 scale, and multiplying this by frequency of past behavior. For each participant, we then computed the correlation between the strength of habits and post-training behavior across the 27 behaviors, as well as the correlation between intentions at baseline and post-training behavior. As correlations are not normally distributed, we tested for differences in the magnitude of these correlations between conditions using Mann-Whitney U tests. These tests showed that there was no significant difference between the training and control conditions in how well behavior was predicted by habits, $U = 3346.50$, $p = 0.28$, or how well behavior was predicted by baseline intentions, $U = 3296.00$, $p = 0.18$. Habits positively predicted behavior among both participants who received self-control training (median correlation; $r = .21$) and participants in the control conditions (median $r = .32$). Likewise, intentions positively predicted behavior among both participants who received self-control training (median $r = .31$) and participants in the control conditions (median $r = .36$). However, the magnitude of the correlations did not differ significantly between participants who received versus did not receive training.

Meta-Cognitive Transfer

The information that participants provided in the diary was used to compute the amount of money spent on non-essential items over the 7 days, the number of unhealthy snacks and meals eaten, the total number of minutes that participants spent engaging in negative behaviors related to self-control (namely, internet use, social networking, socializing, and watching TV), and the total number of minutes that participants spent engaging in positive behaviors related to self-control (namely, exercising, studying and doing household chores). Across all behaviors, a total of three outliers (> 3 SD above the mean) were replaced with the next most extreme value. Data for each of the behaviors was positively skewed, such that most participants performed the behaviors relatively infrequently. A square root transformation was therefore performed on each variable to improve normality. A MANOVA on these four variables found no effect of condition on behavior, $F(4, 168) = 0.53$, Wilk's $\Lambda = 0.99$, $p = .71$, partial $\eta^2 = .01$.

The amount of alcohol that participants reported consuming was extremely skewed (participants reported consuming no alcohol on 78% of days within our sample, and 38% of participants did not drink alcohol on any day during the week). Therefore, chi-square was used to assess whether training condition influenced the likelihood that participants drank on more than one day during the diary period (43% of participants drank on more than one day). There was no significant effect of condition, $\chi^2(1, N = 174) = 2.22$, $p = .14$. Similarly, condition did not influence perceived success at achieving health goals, relationship goals, or academic goals, $F(3, 169) = 1.32$, Wilk's $\Lambda = 0.98$, $p = .27$, partial $\eta^2 = .02$.

As no group differences in behavior were observed, we did not conduct mediation analyses to investigate whether participants' ratings of their desire to perform each of the different behaviors, whether they wanted to control the desire, experienced conflict between the desire and other goals, the extent to which they attempted to resist the desire, and the extent to which they took action to reduce the desire mediated the effect of training. Indeed, consistent with the lack of effects of training on behavior, a MANOVA with condition (training, control) as a between-participants IV and scores for each of these five questions as the dependent variables confirmed that there was no overall effect of condition on these ratings, $F(5, 167) = 0.30$, Wilk's $\Lambda = 0.99$, $p = .91$, partial $\eta^2 = .01$.

To examine the effect of training on well-being, a MANOVA with time (pre- vs. post-training) as a within-participants IV, condition (training vs. control) as a between-participants IV, and positive emotion, negative emotion, and overall well-being as the dependent variables revealed a multivariate effect of time, $F(3, 169) = 30.66$, Wilk's $\Lambda = 0.65$, $p < .001$, partial $\eta^2 = .35$. Follow-up univariate tests revealed that participants reported similar levels of overall well-being ($p = .98$) but fewer negative and fewer positive emotions ($ps < .001$) at the end of the study as compared to the beginning. There was also a significant effect of condition, $F(3, 169) = 2.81$, Wilk's $\Lambda = 0.95$, $p = .04$, partial $\eta^2 = .05$, but no interaction between condition and time, $F(3, 169) = 1.14$, Wilk's $\Lambda = 0.98$, $p = .33$, partial $\eta^2 = .02$, indicating that participants in the training conditions had higher overall well-being than participants in the control conditions ($p = .004$), but that this effect was not driven by an increase in well-being from pre- to post-training. The effect of condition was no longer present when covarying for baseline differences in trait self-control and conscientiousness, $F(3, 167) = 1.83$, Wilk's $\Lambda = 0.97$, $p = .14$, partial $\eta^2 = .03$, however. Life satisfaction was measured at follow-up only: Participants in the training conditions reported being more satisfied with their lives than participants in the control conditions, $F(1, 171) = 5.49$, $p = .02$, partial $\eta^2 = .03$, but again, this effect was no longer present when covarying for baseline differences in trait self-control and conscientiousness, $F(1, 169) = 3.44$, $p = .07$, partial $\eta^2 = .02$. Multivariate analyses also indicated no effect of condition on daily reports of positive and negative emotional experiences, $F(2, 170) = 1.17$, Wilk's $\Lambda = 0.99$, $p = .31$, partial $\eta^2 = .01$. Overall, these findings suggest pre-existing differences in well-being between participants in the training and control conditions, but provide no evidence for changes in well-being as a function of training.[7]

Effects of Training on Potential Mediator Variables

Levels of trait self-control, implicit theories about willpower, implicit theories about resisting temptation, and scores on each of the measures of executive function were entered into a repeated-measures MANOVA with time (pre-training vs. post-training) as a within-participants IV and condition (training vs. control) as a between-participants IV.[8] There was an overall effect of time, $F(6, 159) = 21.04$, Wilk's $\Lambda = 0.56$, $p < .001$, partial $\eta^2 = .44$, but no significant effect of condition, $F(6, 159) = 1.71$, Wilk's $\Lambda = 0.94$, $p = .12$, partial $\eta^2 = .06$, and no interaction between condition and time, $F(6, 159) = 0.48$, Wilk's $\Lambda = 0.98$, $p = .83$, partial $\eta^2 = .02$. These findings indicate that participants tended to improve on these measures over time but that the improvements were similar in the training and control groups.[9]

Did Training Work For Some Participants But Not Others?

To investigate the hypothesis that training might only be effective among some participants (e.g., people lower in relevant abilities might benefit more from it), we conducted a series of hierarchical regression analyses to examine the interaction between the relevant baseline variables (measures of trait self-control, implicit theories about willpower, and executive function) and training (control = 0, training = 1) in predicting each fo the key dependent variables in Table 2. Due to the large number of tests undertaken, we controlled the family-wise error rate using the Holm-Bonferroni correction (Holm, 1979). Using this correction, none of the variables significantly moderated the effect of condition on outcomes (without this correction, 7% of these analyses reached significance, similar to chance level; we report the raw analyses in the Supplemental Materials). We conclude that the hypothesis that training particularly benefited specific groups of people is rejected.

How Confident Can We Be in Our Null Findings?

The analyses reported thus far suggest that repeated practice of tasks involving self-control did not improve self-control. Compared to participants who did not receive self-control training, trained participants did not show reduced ego depletion effects, and were no better at intentionally controlling their behavior or exerting self-control in everyday life. To assess whether our findings provide conclusive evidence for a null effect of self-control training, we undertook Bayesian analyses. A non-significant effect in traditional null hypothesis testing could indicate either the absence of a true effect or that the data are not sensitive enough to detect a true effect. Bayesian analyses allow conclusions to be drawn about whether non-significant results provide conclusive support for the null hypothesis, relative to a pre-existing theory (Dienes, 2011).

The outcome in our study that maps most closely onto previous research, and for which we have a prior estimate of the expected effect size, is the impact of training on ego depletion. We therefore used the estimate of the effect of training on ego depletion reported by Hagger et al. (namely, $d_+ = 1.07$) as the basis of our experimental hypothesis. This estimate was transformed to Fisher's *Z* to ensure a normal distribution (using the formulas described in Lipsey & Wilson, 2001) and represented the experimental hypothesis using a half-normal distribution with a mean of zero and this estimate as the standard deviation (see guidelines in Dienes, 2011). The Bayes factor indicates the relative strength of the evidence for the null hypothesis versus this experimental hypothesis, given the data. Bayes factors below 0.3 indicate substantial evidence for the null hypothesis, Bayes factors above 3 indicate substantial evidence for the experimental hypothesis, and a Bayes factor between 0.3 and 3 indicates that the data are unable to differentiate the two hypotheses (Jeffreys, 1939/1961).

As the present research included multiple measures of self-control performance under depletion, our analysis compared participants in the training conditions with participants in the control conditions on a combined score representing performance across the second, third and fourth self-control tasks and change in handgrip performance from pre- to post-depletion (i.e., performance on all tasks after the initial exertion of self-control). The Bayes factor was 0.23, which indicates that our data are more consistent with a null effect of training on ego depletion than with the experimental hypothesis. Bayes factors for individual tasks ranged from 0.11 (performance on the second self-control task) to 0.33 (performance on the fourth self-control task), indicating relatively consistent evidence for the null hypothesis across the series of tasks. We can therefore conclude that our data are more consistent with the null hypothesis than with Hagger et al.'s effect size estimate and that our data are sufficiently sensitive to distinguish between these hypotheses.

If the effect size estimate from Hagger et al.'s (2010) meta-analysis was influenced by confounding factors as well as training effects, or if publication bias resulted in the omission of studies with null effects from their overall estimate, then the effect size used as the basis of our Bayesian analysis could overestimate the strength of prior evidence for training effects. Indeed, as discussed earlier, a recent paper estimated that the actual effect of training might be small ($d_+ = 0.17$, Inzlicht & Berkman, 2015). Given uncertainty about what can be concluded from past data, we also performed a Bayesian analysis to test whether our data supported this smaller estimate of the effect of training, using the same method described above. The Bayes factor for this revised analysis was 0.93, which indicates that our data are consistent with both a true effect of $d_+ = 0.17$ and with the null hypothesis (i.e., $d_+ = 0.00$), and cannot differentiate these two possibilities (indeed, demonstrating a between-groups effect size of $d_+ = 0.17$ with 80% power would require 429 participants per group, one-tailed). Thus, our data cannot confirm whether self-control training truly has a null effect, or whether its effects are merely so small that hundreds more participants would be required to reliably demonstrate them.[10]

Discussion

The present research involved a rigorous and comprehensive test of the effects of two types of self-control training on a wide variety of outcomes relevant to self-control. There was good evidence that participants engaged with the training tasks and that performance on the tasks improved during the 6-week training period. However, we found no evidence that training led to any improvements in self-control. We could not replicate the substantial effect of self-control training on ego depletion observed in a previous meta-analysis (Hagger et al., 2010). In fact, Bayesian analyses suggested that our findings provided substantial support for the null hypothesis relative to this prior estimate of the training effect size. Furthermore, we did not obtain any evidence that training generalized to performance of behaviors involving self-control in either the laboratory or the field, to intentional versus habitual control of behavior, or to well-being. Overall, the present findings suggest that practicing self-control does not improve participants' performance on non-trained self-control tasks.

Integrating the Present Findings with Previous Research

Differences from previous work on self-control training. Our results stand in contrast to previous studies that have observed effects of training on performance under ego depletion (as reviewed in Hagger et al., 2010) and generalization of training effects to everyday behaviors involving self-control (Oaten & Cheng, 2006a, 2006b). Given that the present research assessed training effects using both a much larger sample and improved methodology as compared to previous studies, we have reason to believe that our null findings more accurately reflect the true effect of training on self-control. We note here that, while participants in our behavioral training condition did not differ from participants in the other groups on objective measures of self-control, they nevertheless reported believing that their self-control had improved, suggesting that participants may sometimes believe that training has improved their self-control even when it has not. This observation may shed light on the processes underlying the positive effects observed in previous studies; namely, that they may be a function of participants' expectations of the likely effects of training. Motivational accounts of self-control failure (e.g., Inzlicht & Schmeichel, 2012) may also help to explain why participants might show group differences in performance in the absence of true increases in the trained ability. Most previous research on self-control training lacks the design features necessary to disentangle the effects of beliefs, expectations, and motivation from the actual effects of practice.

The present research also moved beyond the outcomes assessed in previous studies to examine the effects of self-control training across a much broader range of dependent measures, all of which had established associations with self-control and thus should have benefitted from training, according to the strength model (Muraven & Baumeister, 2000). Our consistent finding that training did not influence these outcomes suggests that previous studies may have overestimated both the effectiveness of self-control training and the extent to which effects generalize beyond the lab.

Similarities with previous work on other types of training. The null effects of training observed in the present study are consistent with research in other areas and, in particular, with work on executive function training. Although some studies have observed that training executive functions can influence conceptually-related cognitive outcomes such as fluid intelligence (e.g., Jaeggi, Buschkuehl, Jonides, & Perrig, 2008), these studies often share similar limitations to those examining the effects of self-control training (e.g., a lack of active control conditions; Shipstead, Redick, & Engle, 2010). Studies comparing executive function training with active control conditions have tended to find that training leads to improvements only on tasks that are very similar to the trained task, with no evidence for generalization beyond those tasks to other cognitive abilities (e.g., Dougherty, Hamovitz, & Tidwell, 2015; Harrison et al., 2013; Redick et al., 2013). In the few studies that have assessed the effects of executive function training on self-control outcomes, there is also a lack of consistent evidence for transfer. Working memory training has been found to reduce alcohol consumption (Houben, Wiers, & Jansen, 2011), but does not appear to decrease rumination (Onraedt & Koster, 2014; Wanmaker, Geraerts, & Franken, 2015). There is evidence that performing a single inhibition task can temporarily influence behaviors such as snacking, alcohol consumption, and gambling, but no evidence that training inhibition over time has long term effects on these behaviors (e.g., Verbruggen et al., 2013; Allom, Mullan, & Hagger, in press).

This failure to observe transfer from an intervention to conceptually related outcomes is also common in other domains. For example, researchers have often observed that interventions targeting clinical problems, such as cognitive bias modification for anxiety or self-management for ADHD, do not generalize across symptoms, responses, or settings (e.g., Barry & Haraway, 2005; Beard, 2011; Corrigan & Basit, 1997; Evans, Axelrod & Sapia, 2000). Similarly, in educational contexts, it is widely recognized that students may not transfer learning from one situation to another, leading McKeough, Lupart, and Marini (2013) to comment that “researchers have been more successful in showing how people fail to transfer learning than they have been in producing it" (p. vi). In short, the overall pattern of findings observed across these different literatures is consistent with the findings obtained in the present research – namely, that self-control training did not generalize to untrained outcomes.

Implications for the ego depletion effect. The present findings also have implications for the other key prediction of the strength model – that exerting self-control temporarily reduces self-control performance. In contrast to previous studies that have used hand-grip persistence as a measure of self-control and found that performance suffers under depletion (as reviewed in Hagger et al., 2010), we found that performance on the handgrip task improved following the exertion of self-control (i.e., a negative effect of depletion, d+ = -0.30). This finding is consistent with observations that self-control performance can improve when individuals are required to exert self-control for sustained periods of time (Converse & DeShon, 2009; Dang, Dewitte, Mao, Xiao, & Shi, 2013) and lends support to recent empirical and meta-analytic work which has concluded that the published record is likely to have overestimated the magnitude of the ego depletion effect – perhaps due to publication bias – with the true effect of ego depletion likely to be either small or nonexistent (Carter et al., 2015; Hagger et al., in press). Taken together, it is becoming clear that people do not inevitably falter under high self-regulatory demands, but that whether performance suffers or benefits may depend upon factors such as motivation and beliefs about self-control (e.g., Hopstaken, van der Linden, Bakker, & Kompier, 2014; Bernecker & Job, 2015).

Is Training Inhibition the Right Way to Improve Self-Control?

Although the central idea of programs designed to train self-control involves improving peoples' ability to inhibit a dominant response, self-control involves more than just the effortful inhibition of impulses (Fujita, 2011) and, as such, there may be multiple ways to improve this skill (Inzlicht, Legault, & Teper, 2014). Indeed, there is emerging evidence that people with higher levels of self-control may not actually be better at inhibiting impulses or spend more time doing so, which implies that the ability to inhibit responses is not in fact responsible for the higher levels of success and well-being experienced by people with good self-control. Indeed, Hofmann, Baumeister, et al. (2012) found that people with high trait self-control reported resisting fewer temptations, and Imhoff, Schmidt, and Gerstenberg (2014) found a negative correlation between trait self-control and the frequency with which participants actively engaged in self-control. Taken together with recent findings which suggest that trait self-control might be associated with reduced experience of temptation, rather than increased control of temptation (Hofmann, Baumeister, et al., 2012), and related evidence that people who are good at self-control may actively avoid temptation (Ent, Baumeister, & Tice, 2015), these findings call into question the idea that inhibition training should necessarily result in improved self-control outcomes. If the goal of self-control training is to train the abilities and skills that are possessed by people with high levels of self-control, then this goal may be better accomplished by training people to proactively avoid temptation rather than to reactively inhibit temptation. Inzlicht et al. (2014) also offer a number of other promising suggestions for improving self-control that go beyond the conception of effortful inhibition, such as changing goal appraisals and responding to self-control failures with acceptance.

Limitations and Future Directions

Our study differed from previous work in that participants trained for six weeks as compared to a modal training period of just two weeks in previous research. One potential limitation, therefore, is that this longer-than-average training period could have resulted in selection bias – perhaps only participants who are already relatively high in self-control were willing to sign up for a lengthy study, and these participants were least likely to benefit from training. We do not believe, however, that this bias characterizes our study, or that selection bias could be responsible for our null effect, for two principal reasons. First, our participants scored no higher on measures of trait self-control than participants in previous studies. Two previous studies of self-control training assessed participants' trait levels of self-control at baseline using the Brief Self-Control Scale (BSCS; Bray et al., 2015; Sultan, Joireman, & Sprott, 2012). Our participants' scores on the BSCS were not significantly different from those observed in either of these studies ($ts < 1.45$, $ps > .15$) and did not differ from the original norms reported by Tangney et al. (2004), $t(778) = 0.57$, $p = 0.57$. Second, levels of trait self-control did not moderate the impact of training on any of the outcome measures in the present research, suggesting that pre-existing differences in trait self-control did not influence the effectiveness of training. These observations suggest that it is unlikely (i) that selection bias is a serious problem in the present research, or (ii) that selection bias could account for the difference in findings between our study and previous research. We do, however, acknowledge that research in this area has almost exclusively involved educated, student participants (see Table 1), and tests with more representative samples are overdue.

It is also possible that the effects of practicing self-control are nonlinear, which might mean that increasing the length of training does not increase its effectiveness. The time course of training effects, and whether or not they increase as a function of the length of the intervention, are as yet unknown (Berkman, in press). The possibility of non-linear effects is not explicitly considered by the strength model, which appears to assume that more practice should equal more strength. However, an alternative is that participants experience an initial boost from engaging in a new activity (e.g., using their non-dominant hand) which fades over time, perhaps as the activity becomes more automatized, more habitual, and demands less effort. While no study has yet compared the effects of different durations of training on outcomes or studied how the effects of training change over time, one recent study has found that the effects of self-control training wear off relatively quickly once training is complete: Bertrams and Schmeichel (2014) found that practicing logical reasoning for one week influenced ego depletion effects when tested immediately afterwards, but not one week later. Our data do not allow us to test the possibility that training effects were present near the beginning of the training program. However, we would note that self-control training has limited practical value if its effects are so short-lived.

Conclusion

Determining the effectiveness of psychological interventions such as self-control training is important both to further our understanding of the nature of self-control and to answer applied questions about how best to help people to change their behavior. Whereas previous studies have reported improvements in self-control after practice, the present research rectified several methodological problems with previous studies and observed that self-control training did not improve self-control. Given mounting evidence that self-control performance may depend upon more than self-control resources, and that the exertion of self-control does not necessarily impair performance in the short term nor improve it in the long term (Carter et al., 2015; Inzlicht & Berkman, 2015), we suggest that future research could benefit from taking a broader perspective on self-control. Our suggestion is that programs that do more than train people to effortfully inhibit impulses may achieve greater real-world impact.

Footnotes

[1] Inzlicht and Berkman (2015) re-analysed data from studies investigating the effects of self-control training and provided two estimates of the effect size; $d_+ = .17$ and $d_+ = 0.62$. Although these estimates were published after data collection for the present study was completed and so did not contribute to the calculation of the required sample size, it is worth noting that the present sample provides 74% power to test for the averaged of their estimated effect sizes ($d_+ = 0.40$).

[2] We chose this target group because people of South Asian descent represent the largest minority ethnic group in the United Kingdom, where this study was conducted.

[3] The only data collected that are not reported in this paper were measures of spontaneous self-affirmation, positive thinking, and self-esteem. These data were collected in order to help validate a new measure (Harris, Napper, Griffin, Schuez, & Stride, 2016) and the intention was never to analyze these items in relation to training outcomes. Apart from this data, we confirm that we have reported all measures, conditions, data exclusions, and sample size determinations, as per the data reporting guidelines proposed by Simmons, Nelson, and Simonsohn (2012).

[4] Sixteen participants who identified themselves as Asian were excluded from the analysis of the effects of training on prejudice and four participants who reported that they were unable to eat chocolate were excluded from the analyses of the effects of training on chocolate consumption. We also Winsorized one outlying data point (one participant in the cognitive training condition ate 495 grams of chocolate, more than 10 standard deviations above the mean, and we replaced this value with the next most extreme value). Comparisons between the conditions on all baseline measures that might predict differences in these behaviors (i.e., implicit attitudes and explicit attitudes, implicit motivation to control prejudice, explicit

motivation to control prejudice, and dietary restraint) revealed only one significant group difference; participants in the training conditions had more positive implicit attitudes to chocolate than participants in the control conditions, $t(172) = 2.0$, $p = .04$, $d_+ = 0.32$. The null effect of training on the amount of chocolate eaten was identical when controlling for implicit attitudes, $F(1, 166) = .207$, $p = .649$, partial $\eta^2 = .001$.

[5] In contrast to previous findings (e.g., Webb, 2011, Friese et al., 2008), implicit attitudes did not predict behavior in these analyses. One explanation of this finding is that implicit attitudes were measured before training (i.e., at least 6 weeks before the measures of behavior), whereas previous studies measured implicit attitudes and behavior during the same session. Thus, while we can conclude that training did not influence performance of these behaviors, we cannot say that training did or did not help participants to overcome their impulses to engage in particular behaviors, as implicit attitudes and behavior were not associated in either condition.

[6] Analyzing positive and negative behaviors separately also found no effect of condition. Neither performance of positive behaviors such as studying and exercising, $F(14, 156) = 0.96$, Wilk's $\Lambda = 0.92$, $p = .50$, partial $\eta^2 = .08$, nor performance of negative behaviors such as watching TV and overspending, $F(13, 157) = 1.00$, Wilk's $\Lambda = 0.92$, $p = .46$, partial $\eta^2 = .08$, showed effects of condition.

[7] To assess whether these differences in wellbeing masked any effects of training on self-control outcomes, we repeated all analyses of the effects of training on self-control outcomes including baseline well-being as a covariate. No effects that were previously non-significant became significant, or vice versa.

[8] Across the three measures of executive function, a total of 6 responses at baseline and 14 responses post-training contained a high number of errors (over 3SDs above the mean). This

increase suggests that some participants performed the tasks less conscientiously after training. However, the analyses of executive function outcomes were unchanged when these responses were excluded, and none of the analyses of effects of training on self-control outcomes became significant when the participants who had made these responses were excluded, suggesting that this phenomenon did not mask training effects.

[9] We also analyzed the effects of training on outcomes using condition as a four-level variable (behavioral training, cognitive training, active control, no-contact control), as opposed to collapsing across training and control conditions. The results of these analyses did not differ meaningfully from the original analyses; no effects of training became significant, and the effect of condition of well-being became non-significant, $F(9, 407) = 1.63$, Wilk's $\Lambda = 0.92$, $p = .10$, partial $\eta^2 = .03$.

[10] All data reported in this manuscript have been made publicly available via Open Science Framework and can be accessed at https://osf.io/pzum5/

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Table 1

Characteristics of Published Studies on Self-Control Training

Study	Participants	Self-control training task	Duration of training	Control group	Total N	Measure of adherence	Longest follow-up	Dependent measure	Depleting task
Bertrams & Schmeichel (2014)	Undergraduate students	Writing task, involving logical reasoning	1 week	Writing task, no instructions	49	Paper-based, submitted after training	1 week post-training	Anagrams	Typing task
Bray et al. (2014)	Undergraduate students	Squeeze a handgrip	2 weeks	No task	41	Paper-based, submitted after training	Immediately after training	Performance in a physical exercise task	Stroop task
Cranwell et al. (2014, Studies 1 & 2)	University students and staff	Stroop task	4 weeks	No task	62	Online task records	Immediately after training	Handgrip	-
Denson et al. (2011)	Undergraduate students	Use non-dominant hand	2 weeks	No task	70	Online diary, every 2 days; text message, one per week	Immediately after training	Aggressive behaviour in computer task	Anger induction
Finkel et al (2007, Study 5)	Undergraduate students	Use non-dominant hand or modify language	2 weeks	No task	40	Paper-based, submitted after training	Immediately after training	Self-report measure of violent inclinations	Attention control task

Gailliot et al. (2007, Study 1)	Undergraduate students	Modify language	2 weeks	No task	38	Paper based, submitted after training	Immediately after training	Anagrams	Suppress stereotypes
Gailliot et al. (2007, Study 2)	Undergraduate students	Use non-dominant hand or modify language	2 weeks	Pre-post design	98	Questionnaire after training	Immediately after training	Anagrams	Suppress stereotypes
Gailliot et al. (2007, Study 4)	Undergraduate students	Use non-dominant hand	2 weeks	Diary, non-dominant hand use	52	Paper based, submitted after training	Immediately after training	Stroop task	Interaction with outgroup member
Hui et al. (2009)	Undergraduate students	Stroop task and use mouth wash	2 weeks	No task	55	Questionnaire after training	1 week post-training	Pain tolerance, use of floss and toothpaste	Sustained attention task
Muraven, (2010a)	Community sample	Avoid sweets or squeeze a handgrip	2 weeks	Math task or self-control diary	92	Telephone hotline, daily	Immediately after training	Stop-signal task	-
Muraven, (2010b)	Community sample	Avoid sweets or squeeze a handgrip	2 weeks	Math task or self-control diary	122	Telephone hotline, daily	4 weeks post-training	Smoking relapse	-
Muraven et al. (1999)	Undergraduate students	Improve posture or eat fewer sweets or improve	2 weeks	No task	69	Paper-based, submitted after training	Immediately after training	Handgrip	Thought suppression

		mood							
Oaten & Cheng (2006a)	Undergraduate students	Personalized study program	8 weeks	Diary, studying	45	Paper-based, submitted after training	Immediately after training	Visual tracking under distraction, self-reported everyday self-control	Thought suppression
Oaten & Cheng (2006b)	Undergraduate students	Physical exercise	8 weeks	No task	24	Paper-based, submitted after training	Immediately after training	Visual tracking under distraction, self-reported everyday self-control	Thought suppression
Oaten & Cheng (2007)	Undergraduate students	Financial monitoring	4 months	No task	49	Paper-based, submitted once per month	Immediately after training	Visual tracking under distraction	Thought suppression
Sultan et al. (2012, Study 1)	Undergraduate students	Stroop task	2 weeks	No task	33	Online task records	Immediately after training	Impulsive buying scenario	-
Sultan et al. (2012, Study 2)	Undergraduate students	Maintain good posture	2 weeks	No task	145	Online task records	Immediately after training	Impulsive buying scenario	-

Note. Inclusion criteria were that studies (1) asked participants to complete a training task requiring self-control, and (2) assessed subsequent performance on a different measure of self-control (i.e., we excluded studies testing only whether performance in a particular domain improves with practice). Depleting task refers to tasks completed by participants prior to completion of the dependent measure.

Table 2

Key Outcome Measures

	Training (N = 89)	Control (N = 85)
Near transfer		
Handgrip endurance, pre-depletion	53.66 (47.64)[a]	42.42 (41.82)[a]
Handgrip endurance, post-depletion	63.05 (48.45)[a]	50.66 (50.38)[a]
Performance in depleting tasks		
Task 1 (numerical search)	5.36 (3.41)[a]	5.22 (3.95)[a]
Task 2 (vigilance)	383.07 (57.94)[a]	385.08 (48.86)[a]
Task 3 (letter-crossing)	91.64 (38.95)[a]	91.07 (33.96)[a]
Task 4 (math with distraction)	52.01 (18.39)[a]	49.73 (17.00)[a]
Far transfer		
Self-control behavior in the lab		
Bias towards Asian targets (D score)	-0.01 (0.13)[a]	-0.01 (0.13)[a]
Chocolate eaten (grams)	35.43 (28.72)[a]	33.43 (26.88)[a]
Intentional control of behavior		
Correlation between habits and behavior	0.24 (0.24)[a]	0.26 (0.25)[a]
Correlation between intentions and behavior	0.29 (0.22)[a]	0.33 (0.19)[a]
Meta-cognitive transfer		
Daily self-control diary		
Minutes per day spent engaging in negative behaviors	378.80 (166.73)[a]	419.79 (179.23)[a]
Minutes per day spent engaging in positive behaviors	306.12 (147.75)[a]	291.87 (127.85)[a]
Money spent (GBP)	8.12 (7.56)[a]	8.26 (6.35)[a]
Unhealthy meals or snacks eaten	1.93 (1.13)[a]	1.94 (1.03)[a]
Alcoholic drinks consumed	0.55 (0.78)[a]	0.76 (0.92)[a]
Well-being		
Positive emotions (change from pre-post training)	-0.22 (0.39)[a]	-0.24 (0.47)[a]
Negative emotions (change from pre-post training)	-0.26 (0.52)[a]	-0.24 (0.51)[a]
Well-being (change from pre-post training)	0.61 (4.09)[a]	-0.55 (4.69)[a]
Life satisfaction	25.70 (6.76)[a]	23.22 (7.17)[b]

Notes. Means within each row which share the same superscript are not significantly different from one another.

Figure 1

Flow of Participants Through the Study.

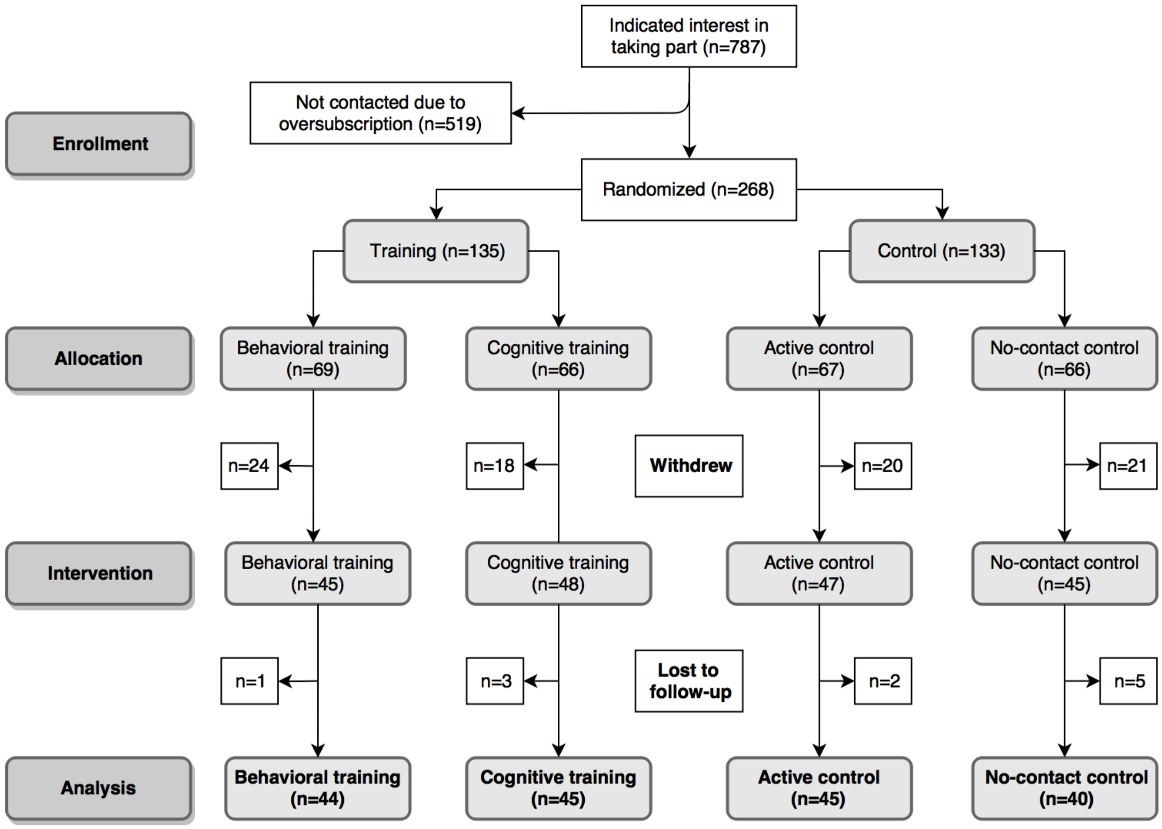

Figure 2

Types and Measures of Training Transfer.

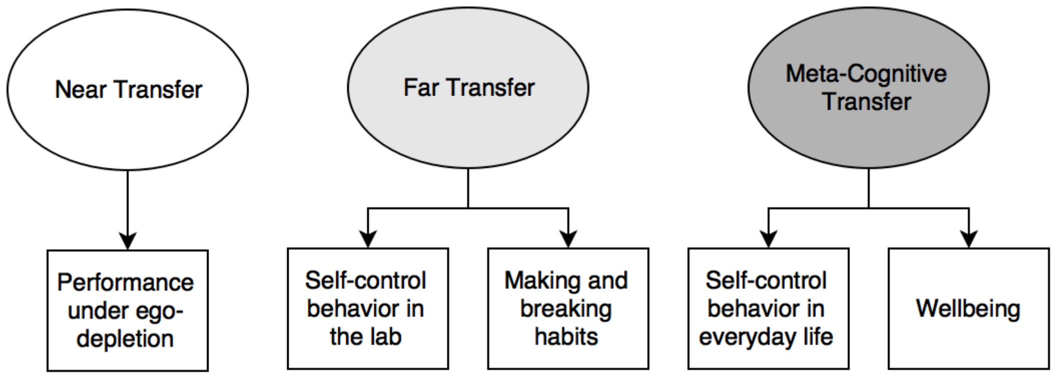

Figure 3

Structure of the Study.