

RUNNING HEAD: SELF-REGULATION TRAINING

Self-regulation training

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## Introduction

In light of the myriad benefits conferred by self-regulation that are described elsewhere in this volume, a natural question for scientists and laypeople alike to ask is whether or not self-regulation can be improved with training. A good deal of research and theorizing has been done to answer this question, and the purpose of this chapter is to review the aspects of that literature that are most pertinent to self-regulation and are indicative of the direction of the field as I see it. The plan is to review key theoretical models that describe how self-regulation might come to be strengthened or improved with training, and then turn to the empirical literature that attempts to test those theoretical ideas. Next, I'll review some of the key conceptual issues that emerge when thinking about whether and how self-regulation can be improved with training, and attempt to provide a clearheaded analysis of how it would be most fruitful for researchers to think about those issues. Finally, I'll close by describing an emerging approach that grounds self-regulation training in neuroscientific evidence, thereby resolving some of the issues that have been problematic in previous work.

But, before all that, I must first set the scope of the chapter by defining how I'll be using the terms self-regulation, training, and improvement. Definitions in this area can be thorny, so my goal for the present purpose is to offer reasonable and broad ones that will allow us to move forward briskly to the question at hand without getting stuck for too long in the underbrush.

### **What do I mean by self-regulation?**

Self-regulation is defined here as the process of purposefully directing one's actions, thoughts, and feelings toward a goal (Carver & Scheier, 2011). A goal is a cognitive construct that specifies an intended outcome, typically one that is relatively long in duration and wide in scope compared to immediate or hedonic goals. For example, "quit smoking" is a goal that might

require self-regulation for a smoker because it would compete or conflict with the immediate goal to “smoke a cigarette”. My working definition, then, is that self-regulation is the capacity to enact psychologically distant goals in favor of psychologically proximal ones. It is noteworthy that in this definition self-regulation includes not only overriding or inhibiting prepotent responses (typically referred to as self-control or inhibitory control, an avoidance action) but also biasing behavior toward desired responses, usually in the face of prepotent alternative responses or mere inertia (sometimes referred to as goal striving, an approach action).

Self-regulation under this definition is a broad construct in both content and temporal scope. It involves directing behavior toward a goal in a way that endures over time and also accommodates other ongoing goals and the constraints of everyday life. In a lab, a smoker may be given a choice between smoking one or zero cigarettes; but in vivo, that person can smoke, or engage in a substitute behavior, or distract him/herself, or tough it out, or even smoke two cigarettes instead of one. In my definition, self-regulation is the process of guiding one’s behavior, moment to moment, day after day, in a way that generally promotes progress toward a goal or set of goals, and as such necessarily plays out in a multitude of ways depending on contextual factors and momentary constraints. Self-regulation thus requires a range of skills including self-control, planning, and other executive functions, but, importantly, is not limited to just those skills (Inzlicht, Berkman, & Elkins-Brown, in press). Successful self-regulation also requires the motivational capacities to want and enjoy behaviors in line with the goal, and to prioritize it above competing goals, tempting alternatives, or simply doing nothing. Above all, self-regulation toward a goal requires an overarching mechanism to enable individuals to attend to, evaluate, and enact a diverse range of goal-consistent behaviors across a variety of contexts and for an enduring period of time.

*What is the role of executive function in self-regulation?*

It is intuitive that there would be a relationship between self-regulation and “executive function”, which refers to a suite of high-level cognitive processes including working memory, attentional control, and inhibitory control. All else being equal, people who are smart, mentally flexible, and facile with their mental resources would seem to be more likely to reach their goals. Indeed, there is some evidence that this is the case, such as the fact that people with higher (versus lower) levels of working memory capacity spontaneously engage in emotion regulation following negative feedback (Schmeichel & Demaree, 2010; Schmeichel, Volokhov, & Demaree, 2008). I have no doubt that better, faster, or more efficient executive functioning confers an advantage when it comes to self-regulation.

Nonetheless, executive function is not the same as self-regulation. It is neither necessary nor sufficient for goal attainment: people with poor working memory reach their goals sometimes, too, and people with exquisite inhibitory control have been known to fall short. More critically, the near-conflation between executive function and self-regulation in the literature (see, for example, Hofmann, Friese, & Roefs, 2009) has led to an outsize view of the role of cognitive function in goal attainment relative to other pertinent factors such as motivation. This pattern is also apparent in the literature on self-regulation training. Therefore, in the sections that follow, I will be careful to draw a distinction between studies that target executive function per se and those that target self-regulation more broadly, and to focus on the latter.

**What do I mean by training?**

As foreshadowed in the previous section, studies have varied quite a bit in the level of specificity of their training target. Some studies have sought to improve the quite narrow construct of proactive attentional control (a presumptive component of cognitive control)

whereas others have targeted broad constructs such as self-regulation or goal attainment. In light of the complex relationship between the narrower and broader constructs in this area, it is logical to focus this review specifically on the topic of self-regulation training. More to the point, I will define training for the purpose of this chapter to refer to programs that, through a variety of means, aim to increase self-regulation in the broader sense of the term. Therefore, the sizable literatures on executive function training in the narrow sense (working memory, attentional control, etc.) will be glossed over to the extent that they do not examine the degree to which the training causes changes in other skills besides the focal skill that promote self-regulation.

### **What do I mean by improvement?**

It would seem like an odd question to ask—to improve simply means to get better, right? The issue here is to differentiate training, which I define as protocol that actually improves self-regulation, from learning or practice effects, which improve only the focal task. People generally get better at any task they do over and over again: that's a practice effect. But the construct self-regulation extends beyond any one task, so genuine improvement in self-regulation is reflected in performance increases in other tasks that index self-regulation but were unpracticed during the training: that's a training effect. A carry-over from one (practiced) task to another (unpracticed) is also called generalizability or training transfer. Sometimes people will even distinguish between near and far transfer in recognition of the fact that the distance between tasks in conceptual space varies. For example, if training in the 100 yard dash to improved your time on the 200 meter sprint, that could be called near transfer; if the same training improved your maximum bench press weight, that might be called far transfer.

Most of the time that people talk about improving self-regulation, they're referring to far transfer in the sense that they expect the training to generalize beyond the task or set of tasks

used in the training to other, novel tasks. Few people would be interested in self-regulation training if it only meant getting better at the Stroop task or the cold-pressor challenge. Instead, the promise of self-regulation training lies in its ability to generalize to other skills that we care about as much or more than the training task, but that are difficult to improve in other ways, such as kicking a bad habit, eating better, or exercising more. Throughout this chapter, I draw a distinction between practice effects, near transfer, and far transfer, and emphasize theories and studies that focus on far transfer. As we will see, the question of what kind of transfer constitutes successful training and how to achieve it is a topic of ongoing and vigorous debate in the training literature.

### **Theoretical models of self-regulation training**

The first step in designing a program to train self-regulation is to articulate a theoretical model for how self-regulation works, and ideally one that provides a point of leverage within that model that can be targeted for behavioral or psychosocial intervention. I won't belabor the first clause given the excellent coverage of theories of self-regulation in this volume. Instead, I'll briefly review a few theoretical models of self-regulation with an emphasis on their stance on how self-regulation might be improved through training.

#### **The strength model**

The most prominent model of self-regulation training in the social psychology literature is the strength or resource model (Baumeister, Vohs, & Tice, 2007). According to this model, self-regulation in all domains draws upon a common, limited resource. Though acts of self-regulation deplete the resource in the short run, consistent self-regulation engagement can strengthen the resource in the long run. A distinct feature of the strength model is its characterization of the resource as one that is shared across domains, in other words, that various

forms of self-regulation all draw from the same well. This model therefore lends itself nicely to the possibility of domain-general improvement in self-regulation if the resource itself can be strengthened or enlarged.

A useful way to evaluate models of self-regulation training is to look at their ability to adapt for various populations and to scale to larger samples. The strength model's view of training is inherently flexible and thus easily adaptable. Any activity that draws upon the common self-regulatory resource is a potential candidate to be used for training. If someone has trouble regulating his emotions, then he might instead train on a behavioral task; if someone has problems controlling her substance use in a particular context, then she could practice regulating her emotions in another one. Scientists have now identified a number of ways that self-regulation tasks could be delivered en masse via computers or mobile devices. According to the strength model, even those digital forms of self-regulation could be used for practice and thereby increase self-regulatory resources in other domains.

### **Motivational models**

Other theoretical models of self-regulation emphasize the motivation to engage in self-regulation rather than the raw ability to do so. They focus on the *will*, rather than the *way*, as it were, as their leverage point. In this category I include self-determination theory (Deci & Ryan, 1985; Ryan & Deci, 2000), self-concordance theory (Sheldon & Houser-Marko, 2001), self-affirmation and other consistency theories (Festinger, 1957; Steele, 1988), and valuation-based models of self-regulation (Berkman & Inzlicht, under review). The common theme across these models is that goals that are, or perceived to be, closely related to one's self (meaning one's sense of identity, self-concept, core values, and so forth) are more likely to be pursued robustly and ultimately achieved than are goals that are motivated from without. The specific pathways

by which closeness-to-self enacts its effects on goals vary across the models, and include mechanisms such as intrinsic vs. extrinsic motivation, self-concordancy, subjective value, and ego consistency. Though the details vary, all of these models share the assumption that some goals are more closely linked with the self than others, and are thereby endowed with particular importance because of the fundamental need for a coherent and positive sense of self (Crocker & Major, 1989; Crocker & Wolfe, 2001).

These models are more descriptive than proscriptive, and as such are largely silent on the issue of whether self-regulation can be improved by training or some other means. The models nonetheless can be used to generate predictions about how to improve self-regulation based on how they describe the relationships among their central constructs. For example, if intrinsic goals are more likely to be achieved than extrinsic ones, then an implied intervention to improve self-regulation is to convert an extrinsic goal to an intrinsic one. Despite the intriguing possibilities that arise from the idea of motivational interventions to improve self-regulation, there are very few studies that attempt to test motivational theories of self-regulation in that way.

An interesting contrast between the strength model and motivational models is in their implied intervention targets. In the former, self-regulatory strength is conceptualized to be a property of the individual's general capacity, whereas in the latter, self-regulatory strength is cast as a property of the goal. It follows that training interventions based on the strength model would have effects on a range of goals (because the training target is the individual), whereas interventions based on motivational models would affect one particular goal (because the training target is the goal itself). Perhaps their limited scope accounts for the correspondingly limited enthusiasm for motivation-based self-regulation training interventions in the research literature. A related issue is whether it is even possible to "convert" an extrinsic goal to be

intrinsic or self-concordant or generally more self-related. If not, then a natural target for interventions based on motivational models of self-regulation would not be the goal itself, but rather the process of goal selection. Here, the hypothesis is that self-regulatory success can be increased by electing to pursue goals that are intrinsic or central to one's self in the first place.

### **Cognitive models**

Other theories conceptualize self-regulation training in terms of cognitive factors such as attentional processes, beliefs about oneself, and habit learning. According to these models, self-regulation success is alternately a function of self-efficacy (Bandura, 1991), beliefs about the nature of self-regulatory resources (Molden & Dweck, 2006), or the degree of cognitive association between the goal and relevant environmental cues (Fishbach, Friedman, & Kruglanski, 2003; Gollwitzer, 1999; Mann & Ward, 2007). This class of theories is admittedly broader than the previous two, but it is useful to group cognitive models together for the present purposes because the kinds of interventions they deploy to increase self-regulation are related. Namely, interventions grounded in these cognitive models of self-regulation typically involve cognitive training based in some form of associative learning or conditioning that are broadly aimed at reinforcing associations between certain cognitions and behaviors.

The implied targets for intervention in these models are individual factors, in the case of self-efficacy and beliefs models, and goal-specific factors, in the case of attentional and associative models. Either way, the implied training interventions are more straightforward than in motivational models: Beliefs can be changed through information delivery, and attention and associations can be changed through reinforcement learning. These models have also received limited empirical attention in the empirical literature on self-regulation training despite their

elegant simplicity, though the issue of generalizability across goals and contexts is one that crops up in the existing studies.

### **Summary**

The strength model, motivational models, and cognitive models are contrasting theoretical frameworks for understanding how self-regulation might be improved through intervention. However, from a broad perspective the models all make essentially the same prediction—that self-regulation is not a fixed property of a goal or a person, but rather can be improved through behavioral means—even though they draw on a diverse range of theoretical constructs. It is from that removed distance that the models can be compared in a meaningful and productive way. Allowing each model to operate on its own terms, is there empirical evidence that self-regulation can be improved based on its predicted pathways? In this sense, the self-regulation training paradigms suggested by each of the models can be considered as empirical tests of the models themselves. In the following section, I review the empirical literature on self-regulation training with particular emphasis on what those data can reveal about the theoretical models and on the theoretical issues that emerge across the theories.

### **Empirical evidence for and against self-regulation training**

The number of training studies in the psychological literature is relatively small compared to the overall number of studies on self-regulation, but is still sufficient to serve as the basis for a useful review and synthesis. In fact, it is my impression that the number of training studies is accelerating right now due to the high level of practical importance they could have for large-scale societal problems such as drug use, unhealthy food intake, and other risky behaviors. If that is the case, then right now is the ideal time to take stock of what works and what doesn't in terms of theory-informed self-regulation training programs. The purpose of the present section

is to review a representative if not comprehensive sample of training studies that have their roots in each of the theoretical models described above.

### **The strength model**

There have been around a dozen training studies based on the strength model prediction that practice in one form of self-regulation will generalize to other forms by increasing a general self-regulatory capacity or strength. These studies are listed in Table 1. For the most part, these studies involve randomly assigning participants to practice a common, everyday task that involves self-regulation or control task, and then measuring their performance on another, unpracticed form of self-regulation. For example, two weeks of avoiding contractions and slang in everyday speech reduces the effect of stereotype suppression on anagram persistence (Gailliot et al., 2007), and two weeks of foregoing sweets or practicing a handgrip exercise twice a day increases the likelihood of cigarette smoking cessation (Muraven, 2010a). Table 1 contains further details about the kinds of manipulations and dependent measures that were used, as well as the observed effects.

INSERT TABLE 1 AROUND HERE

This set of studies has a number of notable features and limitations. The strengths of these studies include the variety of tasks deployed for training (ranging from journaling to squeezing a handgrip to practicing good posture and mood regulation) as well as for the outcome variables (including cognitive tasks, cold pressor tolerance, and urges toward intimate partner violence), and a good deal of geographical diversity of the labs involved. Their weaknesses include the limited sample sizes (ranging from 38 to 122), the uniformity of the duration (all

exactly 2 weeks except for 3 studies that I discuss further below), and the lack of longer-term follow-ups to assess durability of the training. There is also considerable variation in the quality of the control groups used, ranging from relatively tight controls of participants who exerted regular effort and even reported on their self-control attempts (Muraven, 2010a, 2010b) to wait-list participants who engaged in no alternative treatment (Gailliot et al., 2007).

On the whole, these studies provide evidence that practicing one form of self-regulation increases performance in at least one other. A meta-analysis (Hagger et al., 2010) of seven of the studies listed in Table 1 found the average effect size to be quite large,  $d = 1.07$ , and a more recent meta-analysis that statistically corrected for publication bias suggested that the average effect across all 13 studies is  $d = 0.60$  (Inzlicht & Berkman, under revision). This evidence provides reason to be optimistic that self-regulation training might work, and that it potentially works quite well.

However, even without conducting a formal meta-analysis, there appears to be a good deal of heterogeneity in the effects. (And, indeed, the Hagger meta-analysis of seven of the studies found statistical evidence of heterogeneity among the effects). The effect sizes range from a modest 0.1 to an astounding 8.59! Three of the largest effect sizes, which are up to an order of magnitude larger than most of the others, come from a series of studies by Oaten and Cheng (2006a, 2006b, 2007), all of which involved training for 2 months. Several features of these studies lead me to believe that they do not actually test self-regulation training. First, the main dependent measure is a visual tracking task that does not appear elsewhere in the literature, is not validated, and does not appear to require self-regulation. Second, the training protocols involved study skills training (2006a), gym attendance (2006b), and self-guided financial monitoring (2007), none of which require more self-regulation than many people exert on a daily

basis. Indeed, the descriptions of the protocols are insufficiently detailed to assess whether participants engaged self-regulation at all. For example, the only detail about the physical activity training is “Exercise programmes were tailored to suit the individual by gym staff, and included aerobic classes, free-weights, and resistance training” (Oaten & Cheng, 2006b; pp. 719). Third, the estimates of the size of the ego depletion effect (which were provided along with the estimates for the training effects) are also outliers relative to other, more reliable estimates of the effect size such as those derived from meta-analyses, suggesting that the measures used in these three studies may not have tapped the same construct as the one assessed by other studies in the strength model literature.

When these three studies are eliminated from the sample, the bias-corrected effect size of self-regulation training shrinks to  $d = 0.17$ ,  $CI = -0.07, 0.41$  (Inzlicht & Berkman, in press). This number is small but still positive, and more in keeping with the size of an effect that one might expect to be caused by 14 days of regular but nonetheless moderate self-regulation exertion. It simply doesn't stand to reason that watching one's language or using one's nondominant hand for a few weeks would increase self-regulatory ability across domains by a full standard deviation. If that were the case, then anyone who went on an extended vacation with the in-laws or had to wear a sling after a sports injury would be noticeably more self-regulated as a result—dubious hypotheses, both.

Even supposing a modest but positive effect, these results bring up a number of interesting theoretical questions. First, self-regulation training studies that are grounded in the strength model are inherently ambiguous with regard to the nature of the resource that is being trained because the dependent variable is, by design, qualitatively different from the variable targeted by the training task. What exactly is the “resource” that handgrip strength and Stroop

performance both require? Second, what do the participants think they are doing during training and during the dependent measure assessments? The answer is not provided in most of the studies, but the question is quite relevant in light of the facts that self-regulation (and self-control and willpower) are socially and personally desirable, and, as reviewed below, that beliefs about one's own capacities in a domain can influence performance in that domain. And, third, is a set of questions about the real-world implications of these results. Do the effects endure across time? Do they scale with increasing intervention dosage? Are their effects even more powerful during critical developmental periods such as early childhood? These are important questions, and I will return to them in the sections that follow.

### **Motivational models**

A number of theoretical models suggest that self-regulation could be improved by increasing goal-directed motivation. At present, psychologists face a dearth of tools to easily boost motivation, but there is some evidence in support of the motivation idea. This section briefly reviews the relevant literature on “motivation training,” such as it is, with particular emphasis on studies testing its effect on self-regulation.

Several studies provide support for the idea from self-determination theory that autonomy or choice will increase motivation to engage in self-regulation, mostly in the health domain. For example, a brief, physician-delivered physical activity intervention designed to increase autonomy about choices related to physical activity and relatedness with the physician about the decisions (two key constructs in self-determination theory) led to increases in physical activity up to 13 weeks following the intervention (Fortier, Sweet, O'Sullivan, & Williams, 2007). Similarly, a cigarette smoking cessation intervention delivered in person by a health counselor that focused on building autonomy about the choice to quit smoking increased autonomous

motivation and, thereby, reduced smoking at one- and six-month follow-up points relative to a treatment-as-usual control (Williams et al., 2006). Like many randomized controlled trials, these studies have impressive and enduring effects, but use a multipronged “shotgun” intervention approach similar to motivational interviewing (Markland, Ryan, Tobin, & Rollnick, 2005) that makes the precise mechanisms difficult to pinpoint.

There is also some promising work based in self-affirmation theory showing that affirming one’s core values can increase self-regulation or related constructs. This work is grounded in the complementary ideas that engaging in self-regulation is often psychologically threatening because it implies that one’s current and past behavior is wrong or bad, and that affirming other valued domains protects the self against that threat, thereby enabling self-regulation in the original domain to occur. This general pattern plays out when, for example, self-affirmation eliminates the ego depletion effect (Schmeichel & Vohs, 2009) or facilitates engagement with health-related messages that otherwise would be threatening or scary (Harris & Epton, 2009; Reed & Aspinwall, 1998). Self-affirmation is not often used as an intervention on self-regulation per se, but its positive effect on several relevant constructs is fairly well established (Cohen & Sherman, 2014). For instance, writing about one’s own core values (as opposed to the values of another person) appears to have lasting effects on educational outcomes among disadvantaged samples (Cohen, Garcia, Purdie-Vaughns, Apfel, & Brzustoski, 2009), on alcohol intake in heavy drinkers (Armitage, Harris, & Arden, 2011), and on medication compliance among people who start out with low rates (Ogedegbe et al., 2012). Though promising, it is important to qualify this summary by noting that evidence for the effects of self-affirmation on self-regulation and behavior change have been mixed in terms of their direction (Vohs, Park, & Schmeichel, 2013) and their durability (Harris & Epton, 2010).

I would be remiss to overlook a final class of “motivation training” interventions that adopts the perspective from economics that all behavior, including self-regulation, is motivated by subjective utility, which is, of course, interchangeable with money. Addiction researchers capitalized on this insight when they developed contingency management treatment, wherein people are paid for abstaining from an addictive substance (Bigelow & Silverman, 1999). A meta-analysis found contingency management to have an effect size  $d = 0.42$  on treatment for alcohol, tobacco, and illicit drugs, which was larger than therapy ( $d = 0.25$ ) and outpatient treatment ( $d = 0.37$ ), and comparable in strength to methadone treatment for opiate use (Prendergast, Podus, Finney, Greenwell, & Roll, 2006). Similarly, “precommitting” to buying more healthy foods at the risk of losing financial incentives is more effective than having the incentives alone (Schwartz, Mochon, Wyper, Maroba, Patel, & Ariely, 2014), though part of the efficacy of this manipulation may be related to loss aversion and not monetary incentives per se. Another line of work relating utility to self-regulation has found that monetary incentives increase persistence at exercise (Cabanac, 1986), endurance on a cold-pressor task (Baker & Kirsch, 1991), and performance on a difficult cognitive task (Boksem, Meijman, & Lorist, 2006). Notably, monetary incentives appear to endure only as long as the payment is available (Volpp, John, Troxel, Norton, Fassbender, & Loewenstein, 2008). Still, as long as the money is there, blunt but effective way to improve self-regulation is by buying it.

### **Cognitive models**

A variety of cognitive models serve up a smorgasbord of relatively simple interventions to increase self-regulation. There are far too many to provide a thorough review in the limited space I have here, so I’ll focus on the three families of interventions that are most prevalent or that I think offer the most promising avenue to cognitive-based self-regulation improvement.

Also, I note that I will omit discussion of interventions aimed at “pure” executive functions such as working memory or inhibitory control for reasons described above. However, good coverage of those topics as they relate to training-related improvement in performance can be found elsewhere (Diamond & Lee, 2011; Melby-Lervåg & Hulme, 2013; Owen et al., 2010).

By far the cognitive manipulation that has the most well documented effects on self-regulation is implementation intentions (Gollwitzer, 1999). Implementation intentions are simple if-then self-statements that create an association between potential hurdles along the path toward goal progress (e.g., *if someone offers me a cigarette at the bar...*) and a concrete plan to overcome them (*...then I will politely decline and state that I am chewing gum*). Interventions based on implementation intentions are alluringly simple, usually involving no more than a single session in which participants brainstorm potential obstacles that could impede goal progress and come up with plans to deal with each (Gollwitzer & Brandstatter, 1997; see the chapter by Gollwitzer in this volume). These kinds of interventions have lasting effects (meta-analytic  $d = 0.65$ ) on a range of goals including personal, academic, health, and consumer-related ones (Gollwitzer & Sheeran, 2006). The effects of implementation intentions can be strengthened further by combining them with a mental contrasting induction, where participants mentally elaborate on the details of the gap between the present state and a desired future outcome (Adriaanse, Oettingen, Gollwitzer, Hennes, de Ridder, & de Wit, 2010; see the chapter by Oettingen in this volume). Counterweighed against the simplicity of implementation intentions is the significant limitation that they are goal-specific, and even hurdle-specific, meaning that they are not expected (and indeed do not) increase self-regulation beyond the focal goal.

More recently, another cognitive manipulation has been suggested that does purport to increase self-regulation writ large. The manipulation targets people’s beliefs about the nature of

their own self-regulatory resources, and is based in Dweck's longstanding theory about the power of lay beliefs (e.g., Molden & Dweck, 2006). The idea is that beliefs about self-regulation, rather than self-regulatory resources per se, are what determine self-regulation performance most of the time, including the case of everyday goal pursuit. The idea is compelling, and the larger theoretical framework is quite powerful, but to date there is only one longitudinal study that shows an effect of self-regulatory beliefs on self-regulatory performance (Job, Dweck, & Walton, 2010, Study 4). In that study, participants who believed their self-regulatory resources were limited, as compared to participants who held unlimited self-regulation beliefs, had a larger number of self-regulatory failures (e.g., greater unhealthy eating and procrastinating). However, this effect only held during finals, a time of high self-regulatory demands, and is correlational because beliefs were not manipulated in that study. Also, a well-powered conceptual replication from my lab failed to show an effect of a longitudinal self-regulatory beliefs manipulation on everyday goal outcomes across 30 days (Miller-Ziegler & Berkman, unpublished data, available from the author by request). It is likely that there is some effect of lay beliefs on self-regulation, but this line of inquiry is still nascent and reliable measures and tools are not yet available.

A final way that cognitive manipulations can influence self-regulation is via identity. The idea is that people's self-concepts can be pushed around to some extent by cognitive factors such as framing or construal. Using this observation as a starting point, psychologists have shown that a "noun-verb" manipulation can improve self-regulation, presumably through a subtle shift in the extent to which the behavior in question is construed as identity-relevant. In one study, phrasing questions about voting intentions in identity-relevant terms (noun: "being a voter") instead of action terms (verb: "voting") increased voting intentions and actual turnout in statewide elections (Bryan, Walton, Rogers, & Dweck, 2011). In another, participants were less likely to cheat by

claiming money they were not entitled to if that behavior was described as a (negative) identity (noun: “being a cheater”) instead of an action (verb: “cheating”; Bryan, Adams, & Monin, 2013). Following this path is a promising direction for the future of self-regulation interventions because it is low-cost, quick to administer, and easily scalable to a broad range of populations and types of desired identities.

### **Summary**

Psychologists have used a variety of means to intervene on self-regulation, with more or less success. At least ten studies have tested the strength model’s prediction that regular practice in one domain of self-regulation has an effect on others, and found moderate support. Other theories predict that self-regulation can be increased through motivational means, but specific interventions that target motivation alone, and via psychological instead of monetary means, have been elusive. In contrast, a variety of cognitive manipulations are available to increase self-regulation, notably including implementation intentions, but most of these target one specific goal and not self-regulation in general.

A number of conceptual issues lurked beneath the surface throughout the foregoing review. In the next section, I dredge them up in an attempt to shed some daylight on what will be critical issues to consider for the next phase of research on self-regulation training.

### **Conceptual issues in self-regulation training**

What follows is an incomplete list of the theoretical issues that face the field. These are not necessarily the ones that are the hardest to tackle or the most relevant to other areas of self-regulation, but those that I believe are most pressing for researchers in this area to consider as we move forward with our self-regulation training interventions.

### **What gets trained?**

One of the biggest conceptual questions regards the intervening mechanisms of the effects: What is actually being trained? This is a deceptively simple question when it comes to a construct, self-regulation, that is measured behaviorally but whose underlying mechanisms are still obscured despite decades of research. The question could be rephrased: when an intervention increases self-regulatory behavior, what contributed to that behavioral improvement? The answer to the question is entirely dependent on your model of what self-regulation is. If you think self-regulation draws upon an unseen resource, then the training expanded the resource; if you think self-regulation is driven by motivation, then the training must have boosted motivation. How can training studies adjudicate among these possibilities, while acknowledging that most of the possible mechanisms (such as motivation or a willpower resource) are unobservable?

I have three modest answers to this question. One is to eliminate the possibility of mere practice effects by mixing up the training tasks, and using different training and dependent measures. This simple option is commonly adopted by studies based on the strength model, but less so in other studies and in other training literatures (e.g., working memory training; see Shipstead, Redick, & Engle, 2012). Another is to measure, or attempt to measure, potential mechanisms of the training, and to include several possible alternatives to your theoretical model. At this early stage of scientific progress in self-regulation training, most researchers are understandably focused on demonstrating effects rather than establishing their mechanisms, though there are some laudable exceptions (e.g., Muraven, 2010a, 2010b, showed that his training effects were not attributable to perceptions of effort or beliefs about the training itself). My final recommendation is to use adequate control groups. A surprising number of studies used a no-contact or wait-list control condition, though again the studies by Muraven are a notable and welcome exception. There is a tradeoff in interventions between the tightness of the control

condition, which, in the extreme, enables the scientist to pinpoint the essential ingredient in the treatment, and the likelihood of finding a difference between the two conditions. An important step that a field must take to mature scientifically is to embrace the former at the cost of the latter.

The deeper issue in my mind is theoretical rather than methodological. The dominant models lack specificity regarding exactly how self-regulation actually works. If, in the resource model, self-regulation is the speed of the car and the resource is the amount of gas in the tank, then what is the engine? By what means, in the lay belief model, does a mindset increase physical endurance on a handgrip task? Even newer models (e.g., Inzlicht & Schmeichel, 2012) that explicitly focus on the mechanisms of self-regulation and, by extension, how self-regulation can be improved by intervening on those underlying mechanisms, merely substitute one unobservable process for another. The next generation of models must be more precise in naming the specific, measurable processes that give rise to self-regulated behavior, and in describing the nature of the interactions between them. For example, there is increasing focus on valuation (or subjective utility) as an underlying mechanism through which motivational factors such as reward, effort, and social influence contribute to self-regulation (Botvinick & Braver, 2015; Inzlicht et al., 2014; Kable & Glimcher, 2007). Nonetheless, the fact that the field is awash in interesting and diverse theories of self-regulation, and that there is lively debate among proponents of the various models, is highly encouraging because it means that researchers are actively struggling to refine their theories to keep pace with the emerging evidence.

### **Practical issues: Generalizability, durability, and scalability**

Researchers interested in self-regulation training can learn a great deal about the challenges ahead by looking to related literatures in adjacent fields such as those on working

memory training, from cognitive psychology, and on executive function interventions, from developmental psychology. Because those lines of inquiry have had more time to develop, both theoretically and empirically, they offer us a glimpse into the issues on the horizon and an opportunity to learn from the missteps of others. Three that are imminently on the horizon have to deal with pragmatic questions that we must answer if and when we find ways to effectively increase self-regulation: generalizability, durability, and scalability.

Generalizability refers to the breadth of the effects of a training program. Probably the most appealing feature of the strength model with respect to training is that there is a single, common self-regulation resource, so strengthening it will produce general effects. Other models are almost a mirror image in terms of their potential for generalizability in that they predict focused interventions to affect behavior only in the domains they target. At the societal level, there is a case to be made for both kinds of training strategies: there is likely a large market of individuals who would want to increase their self-regulatory resources across the board; but there is also a market of people who could identify one or two problem areas that could benefit from precise intervention. These might be called “training-to-improve” and “training-to-remediate” populations, respectively. A natural direction for the training literature is to consider the breadth of generalizability that is expected, and that is possible, given a particular model, and to sample from the population of people who are likely to be interested and benefit from training of that breadth.

Durability refers to the length of time for which the training effects will persist. This is entirely unexplored territory for self-regulation training, so I don't have much to say here besides the obvious: durability is critical. Self-regulation training is likely to become little more than an historical curiosity unless we can find a way to use it to produce enduring change. A couple of

the motivational theories explicitly describe the process by which changes in self-regulation can become long lasting (e.g., Cohen & Sherman, 2014, discuss how targeted self-affirmation can trigger an “adaptive cycle”, and Deci & Ryan, 2000, describe a process by which internalized motives become integrated), but most are silent on that issue. I note that, unlike many interventions, some of the training protocols described above involve a “light touch” manipulation and, therefore, might easily be adapted into follow-up or “booster” sessions that could even be delivered remotely (as described in the following paragraph). A self-regulation training protocol doesn’t need to be one-shot to be enduring; it just needs to identify a mechanism to perpetuate its effects.

The highest level of self-actualization that a theory can achieve is for its predictions to be put into practice at a population level. Many, if not all, of us researchers want not only to test our theories but also to use those theories to change people’s lives for the better. Some theories and, in particular, some experimental manipulations, scale-up better than others. For instance, motivational interviewing is relatively effective at increasing intrinsic motivation toward a goal, and thereby increasing self-regulation, but it is highly labor intensive and impractical to deliver on a large scale. In contrast, interventions involving implementation intentions plus mental contrasting have been simplified to a one-page handout and can be self-administered by schoolchildren in a matter of minutes (Oettingen, Kappes, Guttenberg, & Gollwitzer, 2015; see the Oettingen chapter in this volume). Along these same lines, some of the interventions and training protocols described here could be easily ported into digital forms for electronic delivery via computers, tablets, or smartphones, enabling them to reach rural and other populations that are difficult to contact (Berkman, Dickenson, Falk, & Lieberman, 2011). I advocate that theories

be evaluated not just on their ability to proscribe an effective means to improve self-regulation, but also on the ability of the interventions based on those theories to actually reach people.

**Beware the placebo! Long live the placebo!**

Do training effects amount to nothing more than placebo? Until we start using adequate control conditions as noted above, we may never know. But here's another question that is also worth considering: does it matter? What if merely convincing your participants that an intervention improves self-regulation is all that it takes to make an intervention effective? Placebo treatments have well documented effects on a number of physical processes, and there is strong evidence that placebos could have equal or even greater effects on psychological ones (Shiv, Carmon, & Ariely, 2005). Indeed, several of the self-regulation interventions described here, such as lay beliefs and noun-verb framing, have placebo-like properties insofar as their effects depend on participants' perceptions instead of their capacities. A placebo-based intervention could potentially be made to be generalizable, durable, and scalable. It would be welcome news for public health if self-regulation intervention amounts, essentially, to persuasion.

This is not to say that placebos are panaceas. They are not. They certainly wouldn't liberate us from the obligation of specifying the mechanisms of effects. Claiming that an intervention works because of a placebo effect is much closer to a re-description of the results than to an actual mechanistic explanation. But establishing a placebo effect on self-regulation would substantially advance theory (for example, by contrasting with a capacity explanation) and would be highly appealing from a pragmatic perspective. The greatest challenge would probably be in figuring out a method to separate placebo effects from those attributable to other kinds of beliefs.

Still, all of this is predicated on the existence of some compelling data that, to date, do not exist. The next section provides a blueprint for theory-guided research in this area.

### **Next steps for self-regulation training theory and research**

The idea that self-regulation can be improved with training is as intuitive as it is optimistic. It should be so, and all of us wish that it were. In this chapter, I reviewed theoretical models that explain why and how such training might work, described some of the empirical evidence for and against those models, and discussed sundry conceptual issues that have emerged or soon will as the field moves forward. Throughout, I tried to foresee and discuss the directions that self-regulation training will be going in the coming years. Among these are increased focus on mechanisms, greater theoretical clarity about which mental processes contribute to self-regulation, and more attention to practical issues related to implementation.

Relevant to all of these is an understanding of the neural systems that contribute to self-regulation. This knowledge has implications for training because the rapidly accumulating wisdom about basic brain function can be leveraged to inform interventions to improve self-regulation (Berkman, Graham, & Fisher, 2012). For example, recent research from my lab has shown that cognitive control training causes a shift in key neural regions from later, more reactive, activity to earlier, more proactive, activity (Berkman, Kahn, & Merchant, 2014). This result prompted researchers to attend to aspects of the cognitive control training that turn out to be important but may have gone unnoticed if not for the neural data. As a result, several studies are now underway that test new interventions that were refined based on this information. As this sequence of events illustrates, theory-based interventions often proceed in an iterative manner, with basic science informing intervention work, which in turn prompts further basic research.

There is disparate yet tantalizing evidence that self-regulation can be improved with training. I hope that this review will serve future researchers as a framework to organize the extant evidence and to guide future training interventions based on the lessons we are now beginning to learn.

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**Table 1. Self-regulation training studies based on the strength model**

<b>Study</b>	<b>Self-control training</b>	<b>Training duration</b>	<b>Self-control DV</b>	<b>Total N</b>	<b>t or F</b>
Bertrams & Schmeichel, 2014	Journaling, attending to logical consistency	1 week	Anagrams after self-controlled typing task	49	F = 5.05
Bray et al., 2014	Handgrip practice twice daily	2 weeks	Maximal exertion on a cycling exercise task	41	F = 28.54
Denson et al., 2011	Non-dominant hand use	2 weeks	Noise blast aggression	90	t = 2.15
Finkel et al., 2009	Speech regulation	2 weeks	Intimate partner violence inclination after attentional control	40	F = 4.65
Gailliot et al., 2007, Study 1	Speech regulation	2 weeks	Solving anagrams after stereotype suppression	38	F = 7.02
Gailliot et al., 2007, Study 2	Speech regulation or nondominant hand use	2 weeks	Solving anagrams after stereotype suppression	98	F = 2.08
Hui et al., 2009	Stroop task	2 weeks	Cold pressor tolerance after a concentration task	55	F = 3.11
Muraven et al., 1999	Posture and mood regulation	2 weeks	Handgrip persistence	69	F = 5.57
Muraven, 2010a	Avoiding sweets or handgrip practice	2 weeks	Self-reported cigarette smoking (in smokers trying to quit)	122	F = 5.93
Muraven, 2010b	Avoiding sweets or handgrip practice	2 weeks	Stop-signal task	92	F = 5.04
Oaten & Cheng, 2006a	Study skills program	8 weeks	Visual tracking task during distraction	45	F = 359.98
Oaten & Cheng, 2006b	Physical exercise (aerobic classes and weights)	8 weeks	Visual tracking task during distraction	24	F = 23.80
Oaten & Cheng, 2007	Financial monitoring	4 months	Visual tracking task during distraction	49	F = 48.13