



Does successfully changing personality traits via intervention require that participants be autonomously motivated to change?

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ABSTRACT

A growing body of research suggests that personality traits can be changed through intervention. Theorists have speculated that successful interventions may require (1) that participants autonomously choose which traits they change and (2) that they be deeply invested in the change process. The present studies tested these propositions by examining whether interventions to change conscientiousness and emotional stability can be successful when (1) participants are randomly assigned traits to change or (2) they are naïve with respect to the intervention's target trait. Results indicated that participants could be randomly assigned to change conscientiousness—even if they were unaware that the intervention was targeting conscientiousness. In contrast, interventions targeting emotional stability were effective only if participants both (1) autonomously chose to work on emotional stability and (2) received an effective intervention. These findings have practical implications for designing interventions—and they suggest that different traits may develop via different processes.

1. Introduction

Personality traits are linked to a huge gamut of consequential life outcomes, including relationship quality, occupational success, physical and mental health, and even mortality (Ozer & Benet-Martínez, 2006; Roberts et al., 2007). Moreover, traits can and do change (e.g., Bleidorn et al., 2018; Lucas & Donnellan, 2011). Taken in conjunction, these findings have led to growing interest in whether it is possible to change personality via psychological interventions (e.g., Allemand & Flückiger, 2017; Hudson, 2021; Magidson et al., 2014). To that end, emerging research suggests that certain interventions can, in fact, spur growth in some traits (e.g., Hudson et al., 2019; Hudson & Fraley, 2015; Roberts, Luo, et al., 2017).

Despite these promising findings, the precise ingredients that contribute to successful personality interventions remain poorly understood. In this article, I focus on two highly interrelated ingredients. Namely, theorists have speculated that trait-change interventions may depend on individuals being autonomously motivated to change (e.g., Hennecke et al., 2014; Hudson, 2021; Hudson & Fraley, 2017; Magidson et al., 2014; Roberts, Hill, et al., 2017). Thus, successful interventions may require (1) that participants choose which traits they change (e.g., Deci & Ryan, 2000), and also (2) that they be actively invested in changing the target traits (e.g., Lodi-Smith & Roberts, 2007). However, few studies have directly tested these propositions. For example, prior

interventions explicitly targeting personality have typically let participants select which traits they wished to change (e.g., Hudson et al., 2019).

Accordingly, the goal of the present studies was to isolate interventions from participants' motives by testing whether interventions can be effective (1) if participants are assigned traits to change or (2) if participants are naïve regarding the target traits (and thus are not actively invested in the intervention's true aims). Therefore, these studies shed light on whether autonomous motivation to change is necessary—or alternatively, whether merely “going through the motions” (i.e., adhering to an intervention without investment in its goals) might be sufficient. All said, these studies both elucidate mechanisms underlying successful trait-change interventions and inform broader theories of personality development.

2. Adult personality development

A large body of research shows that personality traits can and do change (for a meta-analysis, see Roberts et al., 2006). For example, as people get older, they tend to become more agreeable, conscientious, and emotionally stable (Lucas & Donnellan, 2011; Roberts & Mroczek, 2008; Soto et al., 2011). These changes are thought to occur due to both biological maturation (Bleidorn et al., 2009; Briley & Tucker-Drob, 2014), as well as the impact of life experiences (e.g., Bleidorn et al.,

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2018). For example, committing to a career or romantic partnership is associated with increases in conscientiousness or emotional stability, respectively (Hudson et al., 2012; e.g., Hudson & Roberts, 2016; Lehnart et al., 2010).

Modern theories generally agree that environmentally-driven personality change results from a complex interplay between people’s patterns of thoughts, feelings, and behaviors; their motives; their identities (i.e., how they see themselves); and their biology (e.g., Burke, 2006; Hutteman et al., 2014; Lodi-Smith & Roberts, 2007; Roberts, 2018; Roberts & Jackson, 2008; Roberts & Wood, 2006; Wrzus & Roberts, 2017). For example, one of the predominant theories of personality development, the Neo-Socioanalytic model, specifies that individuals experience trait change when *both* (1) their state-level patterns of thoughts, feelings, and behaviors are modified over an extended period of time *and* (2) those individuals are motivated to incorporate the new patterns into their identities (Lodi-Smith & Roberts, 2007; Roberts & Wood, 2006).

Thus, from a Neo-Socioanalytic perspective, committing to a career is associated with gains in conscientiousness because (1) workplaces serve as strong, consistent presses to behave in a conscientious fashion (e.g., show up on time, perform high-quality work) and (2) individuals are motivated to incorporate increased conscientiousness into their identities (e.g., because they want the traits associated with being a “good employee”) (Hudson & Roberts, 2016; Lodi-Smith & Roberts, 2007). Over sufficiently long periods of time (perhaps as short as six weeks; Roberts, Luo, et al., 2017), the new, conscientious patterns of behavior become learned, automatized, and habitual—and perhaps even encoded into individuals’ biology (e.g., through changes to the nervous system or epigenome) (e.g., Hennecke et al., 2014; Roberts, 2018; Roberts & Jackson, 2008; Tucker-Drob & Briley, 2019).

Critically, as depicted in the left-hand panel of Fig. 1, from this perspective, psychological investment in the change process is necessary, as it encourages individuals to update their identities to reflect their new repertoire of thoughts, feelings, and behaviors (i.e., to begin to see themselves as more conscientious) (Burke, 2006; Lodi-Smith & Roberts, 2007; Roberts & Wood, 2006). The alternative is that non-invested individuals may psychologically resist trait change by refusing to incorporate state-level changes into their identities (e.g., “My workplace forces me to be more organized, but that’s not who I truly am;” Wrzus & Roberts, 2017). Thus, to summarize, from a Neo-Socioanalytic perspective, trait growth occurs when individuals both (1) change their patterns of thoughts, feelings, and behaviors over

sufficiently long periods of time, and (2) they are motivated to accept the new cognitive, affective, and behavioral patterns into their identities (Lodi-Smith & Roberts, 2007; Roberts & Wood, 2006; Wrzus & Roberts, 2017).

2.1. Does trait development require that individuals be motivated to change?

In contrast to the Neo-Socioanalytic model, other personality development theories suggest that motives are not necessary to trait development. For example, as depicted in the right-hand panel of Fig. 1, the Sociogenomic model implies that chronic state-level cognitive, affective, and behavioral changes have the potential to coalesce into enduring trait growth via biological mechanisms—irrespective of individuals’ desires (Roberts, 2018; Roberts & Jackson, 2008). Thus, for example, an individual who is forced by his/her workplace to regularly engage in elevated levels of conscientious behaviors would be expected to eventually increase in trait conscientiousness—even if s/he was not motivated to change or invested in doing so.

Nevertheless, from a Sociogenomic perspective, desires for change may still *catalyze* trait development. For example, people who want to change certain traits may be motivated to volitionally change relevant state-level thoughts feelings, and behaviors—which, if maintained, may lead to trait growth (e.g., Allemand & Flückiger, 2017; Hennecke et al., 2014; Hudson et al., 2019; Hudson & Fraley, 2015, 2017). Thus, motives may not be *necessary per se* to the process of trait change; instead, motives may simply represent one of many potential presses that can spur new state-level thoughts, feelings, and behaviors.

Empirical findings generally support the idea that motives influence personality development. Indeed, a mega-analysis of 12 studies found that people tend to change in ways that align with their desires (Hudson et al., 2020). For example, people who want to become more extraverted tend to actually increase in extraversion at a faster rate than their peers who do not wish to change. However, prior studies have not attempted to disambiguate whether motives and investment are *necessary* for trait change (as implied by the Neo-Socioanalytic model), as opposed to whether they simply *catalyze* trait change (as implied by the Sociogenomic model). For example, prior studies have not examined whether behavioral modifications *in and of themselves* might lead to trait growth, even in the absence of goals to change.

This is an important lack of knowledge for two reasons. First, on a basic-science level, understanding the role that motives play in

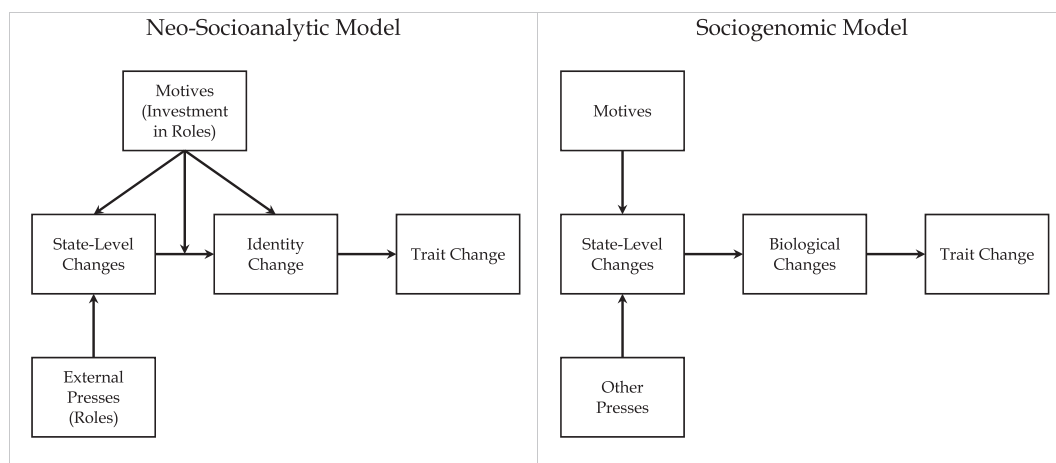


Fig. 1. Note. Role of motives in the process of trait change in the Neo-Socioanalytic and Sociogenomic models. This Figure vastly oversimplifies each model’s hypotheses for the sake of more clearly emphasizing their differences. In the Neo-Socioanalytic model, motives (e.g., to conform with role-based expectations) are crucial to shaping changes in both state-level thoughts, feelings, and behaviors and identity changes. Motives also determine whether state-level changes are integrated into individuals’ identities. Ultimately, identity change leads to trait change. In contrast, in the Sociogenomic model, state-level changes lead to trait-level changes irrespective of individuals’ motives. However, motives can catalyze state-level changes.

personality development can refine theories of how and why traits change. Moreover, on an applied level, understanding whether motives are necessary to trait growth has important implications for the development of interventions designed to change personality traits. For example, must interventions be guided by *participants'* goals, or can *interventionists* select which traits to change (e.g., policymakers might be interested in interventions to increase conscientiousness in the general population)?

2.2. Interventions to change personality traits

Based on theory and findings that personality traits are responsive to environmental factors, scholars have recently taken a growing interest in whether it is also possible to change traits via psychological interventions (e.g., Allemand & Flückiger, 2017; Hennecke et al., 2014; Hudson, 2021; Hudson & Fraley, 2015, 2017; Magidson et al., 2014; Roberts, Hill, et al., 2017). Such interventions provide an elegant way to test theories. For example, several longitudinal interventions have supported Sociogenomic claims that chronic state-level changes can spur enduring trait growth (Carnelley & Rowe, 2007; Gillath et al., 2008; Hudson et al., 2019; Hudson & Fraley, 2015, 2018). Beyond theory, to the extent that interventions can successfully change traits, they may hold applied utility for enhancing important life outcomes, such as relationship satisfaction, well-being, or a host of other criterion variables (Carnelley & Rowe, 2007; Hudson & Fraley, 2016a; Roberts, Luo, et al., 2017).

2.2.1. How do trait-change interventions work?

Generally, trait-change interventions are based on principles drawn from popular personality development theories, such as the Neo-Socioanalytic or Sociogenomic models (see Hudson, 2021; Hudson & Fraley, 2017; Magidson et al., 2014; Roberts, Hill, et al., 2017). Accordingly, most trait-change intervention frameworks suggest that successful interventions require that participants (1) change their thoughts, feelings, and behaviors until the new patterns become learned, automated, and habitual; and (2) be motivated to incorporate the new cognitive, affective, and behavioral patterns into their identities (e.g., Hennecke et al., 2014; Hudson, 2021; Hudson & Fraley, 2017; Magidson et al., 2014; Roberts, Hill, et al., 2017). Consequently, trait-change interventions generally focus on attempting to modify participants' thoughts, feelings, and behaviors on a regular basis to align with target traits. For example, an intervention designed to increase conscientiousness should encourage participants to regularly engage in highly conscientious behaviors (Hudson, 2021; Hudson et al., 2019; Hudson & Fraley, 2015; Magidson et al., 2014; Roberts, Hill, et al., 2017).

2.2.2. Are trait-change interventions efficacious?

Relatively few studies have tested interventions explicitly designed to change personality traits (for an overview, see Hudson, 2021). Nevertheless, at least two emerging lines of evidence suggest that trait-change interventions can be effective. First, several studies have found that personality traits change in response to interventions targeting other constructs. For example, one study found that cognitive training leads to increases in openness (Jackson et al., 2012). Other studies have found that personality traits appear to change in response to mindfulness or social skills training (Krasner et al., 2009; Oei & Jackson, 1980; Piedmont, 2001). Similarly, in a quantitative review of more than 200 studies, clinical interventions (e.g., therapy) were associated with increases in especially extraversion and emotional stability (Chow et al., 2017; Roberts, Luo, et al., 2017).

A second line of evidence comes from interventions more explicitly designed to change personality traits (Hudson et al., 2019; Hudson & Fraley, 2015; Stieger et al., 2021). In these studies, participants were asked which traits they would like to change and were subsequently administered a weekly behavioral change intervention for roughly four months. Participants in the intervention groups were instructed to

pursue weekly goals for pulling their thoughts, feelings, and behaviors into alignment with their desired traits. For example, someone who wanted to become more extraverted might attempt goals similar to, "invite a few friends to lunch," or "go to a coffee shop and meet someone new." In these studies, successful completion of weekly goals was associated with greater growth in the target traits—at least for extraversion, conscientiousness, and emotional stability, and perhaps agreeableness. Specifically, two studies found that individuals who adhered to an intervention to change extraversion, conscientiousness, or emotional stability increased approximately 0.30–0.40 SDs in the respective trait across 15–16 weeks, as compared with their peers who did not (Hudson et al., 2019; Hudson & Fraley, 2015). A different study found that participants who completed an intervention to increase extraversion, agreeableness, conscientiousness, or emotional stability tended to grow anywhere from approximately 0.50 to 1.00 SDs in the respective trait across three months (Stieger et al., 2021).

Thus, taken collectively, the emerging literature suggests that interventions can help people change their personality traits—except openness to experience and perhaps agreeableness—at least across short periods of time, such as four months. These interventions generally operate by helping participants to pull their thoughts, feelings, and behaviors into alignment with desired personality traits.

2.3. Do trait-change interventions require that participants be motivated to change?

Although it appears that interventions can facilitate trait growth in some traits, the precise ingredients that contribute to successful interventions are not well-understood. The present studies focus on elucidating two such highly-interrelated ingredients: Theorists have suggested that successful interventions may require (1) that participants autonomously choose which traits they change and (2) that they be deeply and actively psychologically invested in changing the target traits (Hudson, 2021; Hudson & Fraley, 2015, 2017; Magidson et al., 2014; Roberts, Hill, et al., 2017). With respect to the former, individuals who freely make an autonomous, self-determined choice to change traits are likely more motivated than individuals who do not make such a choice (Deci & Ryan, 1985, 2000). In contrast, attempts to dictate which traits participants change may lead to participant reactance and undermine intervention success (Hudson, 2021).

With respect to the latter, intervention efficacy may depend on participants being deeply motivated to change and actively invested in doing so for at least two reasons. First, people who want to change in ways that align with the intervention's goals may be more likely to adhere to the invention (i.e., actually change their behavior) (Hudson, 2021). Second, individuals who are invested in changing the target traits may be more willing to update their identities in ways that align with the intervention's aims (Lodi-Smith & Roberts, 2007; Roberts & Wood, 2006). For example, a person who wants to increase in conscientiousness may more readily begin to see him- or herself as more conscientious as his or her state-level patterns of thoughts, feelings, and behaviors change across the course of an intervention. In contrast, individuals who are not invested in changing the target traits may resist incorporating any new cognitive, affective, and behavioral patterns into their identities. Such individuals may instead generate external attributions for any state-level changes that they experience during the intervention (e.g., "I'm not actually more conscientious, I'm just faking it for this study"), which may undermine trait growth (Wrzus & Roberts, 2017).

3. Overview of the present studies

The present studies were both 16-week, intensive longitudinal designs that used experimental methods to separate participants' motives to change from the goals of a trait-change intervention. Both studies used an intervention developed by Hudson and colleagues (2019). In their study, participants chose which big five traits they wished to work

on changing. Subsequently, participants selected behavioral goals each week that would pull their thoughts, feelings, and behaviors into alignment with the desired traits. In their study, completing more numerous goals led to greater growth in extraversion, conscientiousness, and emotional stability.

In both of the present Studies 1 and 2, participants were administered Hudson and colleagues' (2019) intervention. However, in Study 1, participants were randomly assigned to change either conscientiousness or emotional stability. Thus, participants had no choice in which trait the intervention targeted, creating some degree of independence between their motives (e.g., free choice) and the intervention's aims. Study 2 more thoroughly bifurcated participants' motives from the intervention. Specifically, participants were asked to nominate whether they would like to work on changing conscientiousness or emotional stability. Participants were subsequently randomly assigned either to receive an intervention targeting their desired trait, or to unknowingly receive an intervention targeting the trait *that they did not choose*. Thus, Study 2 elucidates whether adhering to an intervention can spur growth in the target trait, even when participants are naïve as to which trait is being targeted (and thus are not deeply and explicitly psychologically invested in changing the true target trait).

As an important preface, research suggests that most people want to change their personality traits (Baranski et al., 2017; Hudson and Fraley, 2016b; Hudson & Roberts, 2014). Thus, it is likely that most participants across both studies wanted to increase in the target traits. Nevertheless, Studies 1 and 2 provide critical information regarding the efficacy of specific ingredients of trait-change interventions (e.g., can interventionists choose which traits people change?). Moreover, motivation is a broad construct that runs along a spectrum (e.g., Emmons, 1992; Emmons & McAdams, 1991). For example, (1) desiring change, (2) specifically making an autonomous choice to change a trait, and (3) investing in the change process all seem to represent escalating levels of motivation to change oneself (Deci & Ryan, 1985, 2000; Hudson, 2021; Lodi-Smith & Roberts, 2007). Thus, Studies 1 and 2 also provide insight into theoretical claims that high levels of motivation to change (e.g., deep investment) may be necessary (e.g., Lodi-Smith & Roberts, 2007; Roberts & Wood, 2006).¹

4. Study 1

Previous research suggests that interventions can be efficacious in helping participants change personality traits of their own choosing (Hudson et al., 2019; Hudson & Fraley, 2015). The present Study 1 was a 16-week, intensive longitudinal experiment designed to examine whether it is possible to assign participants to change a trait that they did not choose. Participants were randomly assigned at the beginning of the study to work on attempting to increase their levels of conscientiousness or emotional stability. Subsequently, participants received a weekly intervention (Hudson et al., 2019) designed to target the assigned trait.

In both Studies 1 and 2, I focused exclusively on conscientiousness and emotional stability for two reasons. First, due to limited participant availability, it was not feasible to randomly assign participants to five conditions with adequate statistical power to allow precise effect estimates. Second, conscientiousness and emotional stability in particular were chosen because (1) trait-change interventions appear to have the strongest effects on extraversion, conscientiousness, and emotional stability (Hudson et al., 2019; Hudson & Fraley, 2015) and, (2) among these three traits, conscientiousness and emotional stability share the least amount of conceptual overlap (e.g., extraversion and emotional stability share affective components).

¹ The present studies were not preregistered. Datasets containing relevant variables can be found on Open Science Framework (<https://osf.io/bua8c/>).

4.1. Method

4.1.1. Participants

Participants for Study 1 were recruited from psychology courses at several American universities. Students in participating courses were offered the opportunity to complete waves of the study in exchange for extra course credit. To participate, students were required to register a user account on the study website. Participants were instructed to complete one wave of the study per week of the 16-week semester. However, to provide leniency and flexibility, the study website allowed participants to complete waves as frequently as once every five days. Participants who waited longer than seven days between waves were sent automated email reminders to continue the study.

A total of 175 participants provided at least 1 wave of data. The study was run for only one semester; thus, total sample size was determined by enrollment in the participating courses. No data were excluded for any reason. This sample size afforded approximately 80% power to detect average-sized zero-order effects ($r \sim 0.21$ Richard et al., 2003). A Monte Carlo power simulation based on previous effect sizes using this same intervention (Hudson et al., 2019) suggested that the sample size provided approximately 63% power to detect the intervention's effects (i.e., the extent to which the intervention predicted trait growth across time; the two-way interaction in the multilevel model described in the Results section below). At Wave 1, participants were 71% female, with an average age of 20.16 years ($SD = 2.80$). Participants were instructed to select all racial/ethnic groups with which they identified; 49% identified as White, 27% as Asian, 13% as Black, 11% as Hispanic/Latino, 6% as Asian Indian, 2% as Middle Eastern, and 1% as Pacific Islander.

On average, participants provided 10.67 waves of data ($SD = 4.85$), with 167 (95%), 146 (83%), 110 (63%), and 44 (25%) participants providing data at waves 2, 5, 10, and 16, respectively. Attrition analyses revealed that no study variables at Wave 1—whether measured or manipulated—were statistically significantly related to number of waves provided, all $|r|s \leq 0.12$, 95% CI [-0.03, 0.27].

4.1.2. Measures

Personality traits. Participants' personality traits were measured using the 44-item Big Five Inventory (BFI; John & Srivastava, 1999). The BFI contains separate subscales to measure extraversion, agreeableness, conscientiousness (e.g., "I see myself as someone who does a thorough job"), emotional stability (e.g., "I see myself as someone who is relaxed, handles stress well"), and openness to experience. All items were rated on a scale running from *strongly disagree* (1) to *strongly agree* (5). For the purposes of this study, I analyzed only the conscientiousness and emotional stability scales. Items were averaged to form separate composites for conscientiousness (Wave 1 $\alpha = 0.83$) and emotional stability (Wave 1 $\alpha = 0.86$).

4.1.3. Procedure

Every wave, participants provided self-report ratings of their trait levels of conscientiousness and emotional stability.² At the end of Wave 1, participants were told that, as part of the study, we wanted to help them change their personality traits. Participants were randomly assigned by the study website to work on changing either conscientiousness or emotional stability. The manipulation was between-persons (i.e., each participant worked on a single trait for the entire study duration). For example, participants assigned to change conscientiousness read the following instructions:

Research indicates that the vast, vast majority of people want to increase in conscientiousness (e.g., being more thorough, hardworking, responsible, and organized). So, this semester, we'd like to help you become more

² The present study is part of a larger study. Other questionnaires that are not relevant to the immediate study aims (e.g., attachment measures) were also administered each wave.

conscientious.

Similar instructions—albeit referencing emotional stability—were presented for those randomly assigned to attempt to become more emotionally stable. All participants were subsequently administered an intervention (developed and validated by Hudson et al., 2019) in which they were provided a list of prewritten “challenges”—concrete, behavioral goals designed to help them change their traits. Specifically, relevant instructions read:

Research suggests that regularly performing certain behaviors can help you make the changes you desire to your personality traits.

To help you progress toward your goals, we've created “Weekly Challenges” that you can pursue. These weekly challenges are small steps that you can take, which scientific research suggests will help you make your desired changes to your personality.

Following identical procedures to those used by Hudson and colleagues (2019), participants were presented with a list of 50 challenges relevant to their assigned trait. The challenges were all small, concrete, specific behaviors that prior research suggests are effective in catalyzing trait growth (Hudson et al., 2019). For example, prototypical conscientiousness challenges included “organize and clean up your desk,” “when you wake up, make a list of things you would like to accomplish that day,” and “show up 5 min early for a class, appointment, or other activity.” Prototypical emotional stability challenges included “when you are worried about something, write it down,” “when you feel anxious about a decision, make a pros and cons list for both options,” and “identify someone who has hurt you in the past and choose to forgive them” (for the full list of challenges, see the Appendix in Hudson et al., 2019).

Participants were instructed to browse the list of challenges and to accept between 1 and 4 challenges that they wished to attempt to accomplish during the following week. Participants were sent an automated email with a list of the challenges they had accepted. The following week, participants were presented with each of the challenges they had accepted the prior wave. For each individual challenge, participants were asked to rate the number of times they completed the challenge in the past week, on the following scale: *I did not complete this challenge (0), once this past week (1), twice this past week (2), at least three times, but not every single day this past week (3), and every single day this past week (4)*. Thus, there were measures of both the number of challenges participants accepted—and the number of times that participants reported completing each challenge. After reporting on their success in completing the prior week's challenges, participants selected new challenges for the upcoming week.

To summarize the procedure, at Wave 1, participants were randomly assigned to complete challenges pertaining to either conscientiousness or emotional stability. At every Wave, participants first provided ratings of their personality traits, indicated how many times they had completed challenges from the prior week (Wave 2 onward), were reminded of their assigned trait, and accepted new challenges pertaining to the assigned trait. I used these data to examine the extent to which completing challenges (i.e., adhering to the intervention) predicted growth in the relevant personality traits across time.

4.2. Results and discussion

At Wave 1, participants' trait levels of conscientiousness and emotional stability were moderately correlated ($r = 0.19$, 95% CI [0.04, 0.33]). On average, participants accepted 2.40 challenges each week ($SD = 1.23$) and reported completing 3.87 challenges each week ($SD = 2.56$). To be clear, the number of completed challenges is greater than the number of accepted challenges because participants could report completing challenges multiple times (and thus, participants reported completing each accepted challenge an average of 1.61 times). Participants assigned to work on emotional stability did not differ from their peers assigned to work on conscientiousness in terms of weekly number of challenges accepted (unstandardized difference = 0.03, 95% CI

[-0.34, 0.40], $r = 0.01$) or completed (unstandardized difference = -0.48, 95% CI [-1.24, 0.29], $r = -0.09$). Finally, participants' Wave 1 levels of conscientiousness and emotional stability were also uncorrelated with weekly challenge acceptance and completion rates (all $|r|s \leq 0.11$, 95% CI [-0.04, 0.25]).

4.3. Did completing challenges predict trait growth?

For my primary analyses, I examined whether participants who completed more numerous challenges (i.e., who adhered to the intervention to a greater degree) experienced greater growth in the relevant personality trait. Because (1) I wanted to examine *growth* in participants' personality traits, and (2) I believed that completing challenges week over week should have an accumulating effect on trait growth, I computed the *average number of challenges* each individual participant completed each week across the study's duration. Using the same statistical methods as Hudson and colleagues (2019), I then modeled growth in each trait as a function of average weekly challenges completed.³ For example, the multilevel model (MLM) examining growth in emotional stability (at wave, w , for person, p) as a function of challenges completed was:⁴

$$(\text{Stability})_{wp} = b_0 + b_1(\text{Month})_{wp} + b_2(\text{Average Weekly Stability Challenges Completed})_p + b_3(\text{Month})_{wp}(\text{Average Weekly Stability Challenges Completed})_p + U_p + \varepsilon_{wp}$$

All outcome and predictor variables were standardized across all observations before being entered into the model (see Ackerman et al., 2011), except Time, which was centered on Wave 1 and scaled in months. Thus, the b_1 (Month) coefficient captures monthly growth in the personality traits for people who completed average numbers of challenges (e.g., a coefficient of $b_1 = 0.02$ would indicate that people who completed an average number of challenges [3.87 each week] were predicted to increase 0.02 SDs in the personality trait each month). The b_3 interaction term captures the extent to which completing greater or fewer challenges each week predicted greater or lesser trait growth. A positive interaction term would indicate that people who completed more numerous challenges experienced greater trait growth each month, as compared with their peers who completed fewer challenges.

As seen in Table 1, challenge completion did, in fact, predict monthly growth in conscientiousness ($b_{\text{Month} \times \text{Challenges}} = 0.03$, 95% CI [0.02, 0.05]).⁵ As depicted in the left panel of Fig. 2, this interaction indicates that people who completed high numbers of challenges each week ($z = 1$; 6.43 challenges per week) were predicted to increase 0.03 SDs in conscientiousness each month (95% CI [0.01, 0.05])—amassing to a total cumulative increase of 0.12 SDs across the entire 16-week study duration (95% CI [0.03, 0.20]). In contrast, people who completed low numbers of challenges each week ($z = -1$; 1.31 challenges per week)

³ Alternative models—such as examining the within-person correlation between challenges completed each week and traits—have different assumptions and model different types of change processes. For example, simply modeling within-person variation in traits and challenges (e.g., $[\text{Trait}]_{wp} = b_0 + b_1[\text{Challenges Completed}]_{wp} + U_p + \varepsilon_{wp}$) assumes (1) that there is no cumulative growth in traits, but rather that (2) traits fluctuate up and down each week around a set value, depending on the number of challenges completed (e.g., completing fewer challenges might lead to temporary *decreases* in a trait).

⁴ Although not depicted in the text for simplicity, the models also included the appropriate Time 1 trait and the interaction thereof with Month to control for regression to the mean.

⁵ These analyses used all available data across both conditions and thus considered people who were assigned to work on emotional stability to have completed zero conscientiousness challenges. Nonetheless, models that examined the simple effect of challenge completion on growth in conscientiousness *only among participants assigned to work on conscientiousness*, similar results were found, with slightly larger effect sizes (simple $b_{\text{Month} \times \text{Challenges}} = 0.04$, 95% CI [0.02, 0.07]).

Table 1
Study 1 Growth in personality traits as a function of challenges completed (i.e., intervention adherence).

Predictor	Conscientiousness			Emotional Stability		
	<i>b</i>	95% CI		<i>b</i>	95% CI	
Intercept	-0.02	-0.08	0.05	0.02	-0.05	0.09
Month	0.00	-0.02	0.01	0.01	-0.01	0.03
Challenges Completed	-0.01	-0.09	0.05	-0.01	-0.09	0.06
Month × Challenges Completed	0.03	0.02	0.05	0.00	-0.02	0.01
	<i>s</i> ²	<i>SE</i>		<i>s</i> ²	<i>SE</i>	
Random Intercept	0.08	0.01		0.09	0.01	
Random Slope	0.03	0.005		0.03	0.004	

Note. Ninety-five percent confidence intervals for parameter estimates in bold-face do not include zero. For all statistically significant interaction terms, $p < .001$. All models include the appropriate Time 1 trait and the interaction thereof with Month to control for regression to the mean.

were predicted to decrease 0.04 SDs in conscientiousness each month (95% CI [-0.06, -0.01])—amassing to cumulative decrease of 0.13 SDs in conscientiousness across the semester (95% CI [-0.22, -0.05]).

Directly contrasting with the conscientiousness findings, challenge completion did not predict growth in emotional stability across time ($b_{\text{Month} \times \text{Challenges}} = 0.00$, 95% CI [-0.02, 0.01]). Thus, as depicted in the right panel of Fig. 2, irrespective of the number of challenges completed, people tended to remain relatively stagnant in emotional stability across time ($b_{\text{Month}} = 0.01$, 95% CI [-0.01, 0.03]).

Taken together, these findings suggest an interesting bifurcation: It appears that participants can successfully be assigned to increase in conscientiousness. In other words, it did not matter whether participants chose to attempt to increase in conscientiousness; rather, as long as they adhered to the prescribed intervention and successfully engaged in greater numbers of conscientious behaviors (via completing challenges), they experienced growth in conscientiousness across the study’s duration. This seems to support Sociogenomic claims that behavioral change in and of itself can spur trait growth, even in the absence of other factors such as autonomous free choice and psychological investment in change (Magidson et al., 2014; Roberts & Jackson, 2008). This finding may have implications for the development of future trait-change interventions. For example, it may be possible for policymakers to implement large-scale interventions designed to increase conscientiousness across a general population consisting of individuals with varying levels of motivation to change.

In contrast to conscientiousness, the findings from Study 1 seem to suggest that participants cannot be successfully assigned to increase their levels of emotional stability. Indeed, challenge completion was unrelated to growth in emotional stability. This does not seem to indicate a problem with the intervention itself—which has been shown to be efficacious when properly aligned with participants’ desires (Hudson et al., 2019). Moreover, the null findings for emotional stability do not reflect low adherence to the intervention in Study 1: Participants working on emotional stability accepted and completed equal numbers of challenges, as compared with their peers who were working on conscientiousness. Therefore, these findings seem to indicate that participants’ autonomous free choice—and perhaps the accompanying investment and motivation to work on the trait (Deci & Ryan, 1985, 2000)—is an important component of attempts to change emotional stability. This seems to align with Neo-Socioanalytic accounts of trait development (e.g., Lodi-Smith & Roberts, 2007). Thus, it appears that different traits may develop via different processes and exhibit idiosyncratic responses to intervention attempts.

5. Study 2

In Study 1, I found that it was possible to randomly assign individuals to increase in conscientiousness. Namely, as long as participants incorporated new conscientious actions into their weekly behavioral repertoires (via completing challenges), they tended to experience growth in conscientiousness over time. In contrast, it did not appear to be possible to assign participants to increase in emotional stability. In other words, completing assigned emotional stability challenges did not predict trait growth.

The findings from Study 1 may reflect qualitative differences between conscientiousness and emotional stability (e.g., emotional stability is more affective in nature and thus may be more difficult to change without aligning the participants’ desires with the intervention’s goals)(Vazire, 2010). However, it is also possible that the nuanced pattern of results in Study 1 is attributable to sampling error. Thus, Study 2 was designed to conceptually replicate Study 1. Critically, Study 2 is my only attempt to replicate Study 1 in any fashion as of time of this writing—and thus replication of the findings (or lack thereof) will be informative.

Beyond replicating Study 1, Study 2 also more thoroughly separated participants’ motives from the intervention and probed the mechanisms underlying trait change. Specifically, in Study 2, participants chose whether they wanted to work on increasing in conscientiousness or



Fig. 2. Study 1 Predicted Growth in Conscientiousness and Emotional Stability as a Function of Completing Trait-Relevant Challenges. Note. Participants who completed more numerous conscientiousness challenges experienced greater growth in the trait. In contrast, challenge completion was unrelated to growth in emotional stability. The “high challenge completion” lines are plotted at 1 SD above the mean of challenge completion (6.43 challenges/week). The “low challenge completion” lines are plotted at 1 SD below the mean of challenge completion (1.32 challenges/week).

emotional stability across the duration of the study. Participants subsequently completed the challenges intervention described in Study 1. However, participants were randomly assigned either (1) to receive challenges relevant to their chosen trait (e.g., receive conscientiousness challenges if they elected to work on conscientiousness), or (2) unbeknownst to them, to receive challenges relevant to the trait *they did not choose* (e.g., receive emotional stability challenges if they elected to work on conscientiousness).

This design has several desirable features. First, it provides a conceptual replication of Study 1. Namely, I was able to examine whether participants experienced trait growth as a function of completing challenges relevant to a trait that they did not choose. Based on the findings of Study 1, I expected that participants who unwittingly completed conscientiousness challenges (believing that they were working on emotional stability) should nevertheless increase in conscientiousness. In contrast, I expected that participants who unknowingly completed emotional stability challenges (because they believed they were working on conscientiousness) should *not* increase in emotional stability across time.

Beyond replicating Study 1, Study 2's design more explicitly separated participants' motives (i.e., the traits they chose and believed they were working on) from the intervention (the behaviors they were actually implementing via challenge completion). Thus, these data help elucidate processes underlying trait-change interventions. For example, this design allowed me to test whether changing one's behavior can produce trait growth, even in the absence of motivation to change and investment in doing so (or even knowledge of the true target trait). This has implications for whether behavioral change *per se* is sufficient to produce trait growth (per Sociogenomic model)—or whether psychological commitment to changing certain traits is necessary to cement new behavioral patterns into identity and trait change (per the Neo-Socioanalytic model). In a similar vein, this design also afforded the opportunity to isolate the effects of expectations (e.g., does believing that one is working on emotional stability lead to growth in the trait, even when one is, in fact, not engaging in an intervention that should actually increase the trait?). This latter feature of Study 2 has implications for both theory (e.g., do participants' expectations need to align with the goals of the intervention?) as well as potential methodological limitations of intervention studies (e.g., placebo and demand effects).

5.1. Method

5.1.1. Participants

Participants were recruited from psychology courses at several American universities—using identical recruitment procedures to those described in Study 1. As in Study 1, most students were asked to provide 16 total waves of data. Due to academic calendar differences across universities, however, some students were asked to complete only 15 waves. A total of 414 participants provided at least one wave of data. No data were excluded for any reason. This sample size afforded approximately 99% power to detect averaged-sized zero-order effects ($r \sim 0.21$ Richard et al., 2003). A Monte Carlo power simulation based on point-estimates from previous studies using the same intervention (Hudson et al., 2019) suggested that the present sample size afforded approximately 71% power to detect the intervention's effects (i.e., the intervention predicting trait growth). The study was run for only one semester; thus, total sample size was determined by enrollment in participating courses and students' voluntary choice to participate in the study. At Wave 1, the sample was 76% female, with an average age of 20.31 years ($SD = 4.02$). Participants were instructed to select all racial/ethnic groups with which they identified; the racial composition of the sample was 66% White, 19% Asian, 9% Black, 7% Hispanic/Latino, 3%

Asian Indian, 2% Middle Eastern, and 1% Pacific Islander.

On average, participants provided 11.54 waves of data, with 396 (96%), 359 (87%), 300 (72%), and 179 (43%) participants providing data at waves 2, 5, 10, and 15, respectively. Attrition analyses revealed that participants tended to provide more waves of data if they were female ($r = 0.21$, 95% CI [0.12, 0.30]) or higher in conscientiousness, as measured at Wave 1 ($r = 0.22$, 95% CI [0.13, 0.31]). No other variables at Wave 1, including the manipulation, predicted attrition (all $|r|s \leq 0.05$, 95% CI [-0.04, 0.15]).

5.1.2. Measures

Personality traits. Personality traits were measured using the 60-item BFI-2 (Soto & John, 2017). The BFI-2 is an improved version of the BFI that contains 12 items to measure each of the big five personality traits. As in Study 1, I only report findings pertaining to conscientiousness and emotional stability. Items were averaged to form separate composites for conscientiousness (Wave 1 $\alpha = 0.86$) and emotional stability (Wave 1 $\alpha = 0.90$).

5.1.3. Procedure

The procedure for Study 2 was very similar to Study 1, with two major differences. First, in Study 1, participants were randomly assigned to work on changing conscientiousness or emotional stability. In contrast, in Study 2, participants were allowed to freely choose whether they would like to work on changing emotional stability or conscientiousness.

Subsequently, participants were asked to select weekly challenges following identical procedures to those described in Study 1. However, the second major difference in Study 2 was that participants were randomly assigned at Wave 1—in a between-persons fashion—to either (1) receive challenges for the trait they had selected or (2) receive challenges for the trait *they did not select*. Critically, participants were not informed of this manipulation. Thus, for example, someone assigned to the “mismatched challenges” condition who selected conscientiousness would see a list of challenges *actually* designed to help them become more emotional stable. However, the study website did not tell participants in any way that the challenges were designed to target emotional stability instead of conscientiousness. In other words, some participants *thought* they were working on changing a trait of their choice (e.g., conscientiousness), but they were *actually* completing challenges relevant to a different trait (e.g., emotional stability). As a consequence, participants in the “mismatched challenges” condition were not *actively* psychologically invested in changing the intervention's true target trait (Lodi-Smith & Roberts, 2007). Thus, the design of Study 2 explicitly separated participants' motives (e.g., wanting and intending to change emotional stability) from their actual behavior (e.g., performing behaviors that should increase conscientiousness).

Crucially, almost all participants were completely unaware of the mismatch between their desired traits and the challenges. The final wave of the study contained open-ended debriefing questions that asked participants to (1) summarize the study's purpose, (2) guess the study's hypotheses, and (3) describe anything they found suspicious about the study. For the final week of the semester, these debriefing questions were also included on all waves (in attempt to catch students who did not complete all 15–16 waves). A total of 183 participants provided written responses to the debriefing questions. Of these participants, only two questioned the veracity of the challenges. One participant who opted to change conscientiousness but received emotional stability challenges mentioned that the challenges “didn't seem to match up with being more conscientious.” The other participant opted to change emotional stability and actually did receive emotional stability challenges—but s/he believed that the challenges might be attempting to

change his/her attachment style instead of his/her emotional stability. Thus, participants were generally unaware of the “mismatched challenges” manipulation; and participants were equally suspicious of the challenges across experimental conditions.

Thus, to summarize the procedure, at Wave 1, participants chose whether they wanted to work on changing emotional stability or conscientiousness across the study duration. However, approximately half of participants were randomly assigned to unknowingly receive challenges targeting *the trait they did not choose* and thus were naïve to the intervention’s true aims. Every wave (including Wave 1), participants provided ratings of their personality traits, indicated the number of times they had completed each accepted challenge from the prior week, and finally accepted new challenges. I used these data to examine the extent to which completing challenges predicted changes in personality traits across time—as well as the role that motives (which traits participants wanted to change) and the intervention itself (which traits participants were actually enacting) differentially played in facilitating trait growth.

5.2. Results and discussion

Participants’ trait levels of conscientiousness and emotional stability, as measured at Wave 1, were moderately correlated ($r = 0.29$, 95% CI [0.20, 0.37]). Participants were free to choose, at Wave 1, whether they wanted to work on changing conscientiousness or emotional stability across the study’s duration. A total of 247 (60%) participants chose to work on emotional stability—with the remainder electing to work on conscientiousness. Participants were more likely to nominate to work on emotional stability if they were lower in emotional stability ($r = -0.42$, 95% CI [-0.50, -0.34]) or if they were higher in conscientiousness ($r = 0.12$, 95% CI [0.02, 0.21]). This replicates prior research suggesting that people want to increase in desirable traits that they lack (Baranski et al., 2017; Hudson & Fraley, 2016b; Hudson & Roberts, 2014).

On average, participants accepted 2.91 challenges each week ($SD = 1.23$) and reported completing 4.85 challenges each week ($SD = 2.88$). As in Study 1, the number of completed challenges is greater than the number of accepted challenges because participants could report completing challenges multiple times each week (thus, participants reported completing each accepted challenge an average of 1.67 times). Number of challenges completed did not vary as a function of whether participants had nominated to work on emotional stability (unstandardized difference = -0.11, 95% CI [-0.69, 0.47], $r = -0.02$), experimental condition (unstandardized difference = -0.50, 95% CI [-1.07, 0.07], $r = -0.09$), or Wave 1 levels of conscientiousness ($r = 0.09$, 95% CI [-0.01, 0.19]). Participants did, however, complete more numerous challenges if they were more emotionally stable at Wave 1 ($r = 0.15$, 95% CI [0.05, 0.24]).

5.2.1. Analysis strategy

For my primary analyses, I examined whether completing challenges predicted growth in the relevant trait. To be clear, I modeled the extent to which trait growth varied as a function of completing challenges that *actually targeted the trait* (irrespective of which trait participants selected and thus *thought* the challenges were targeting). For example, I modeled the extent to which completing emotional stability challenges predicted growth in emotional stability across time. However, I also included dummy codes for whether the participant was *naïve* that they were working on emotional stability challenges (0 = participant was aware; 1 = participant was naïve). Thus, for example, a participant who chose to work on emotional stability and received emotional stability challenges was *aware* that they were working on emotional stability (naïve = 0). In contrast, a participant who chose to work on *conscientiousness* but was randomly assigned to unknowingly receive emotional stability challenges was *naïve* that they were working on emotional stability (naïve = 1). The precise MLM used (for emotional stability) was:

$$\begin{aligned} (\text{Stability})_{wp} = & b_0 + b_1(\text{Month})_{wp} + b_2(\text{Stability Challenges Completed})_p \\ & + b_3(\text{Month})_{wp}(\text{Stability Challenges Completed})_p \\ & + b_4(\text{Naïve})_p + b_5(\text{Naïve})_p(\text{Month})_{wp} \\ & + b_6(\text{Naïve})_p(\text{Stability Challenges Completed})_{wp} \\ & + b_7(\text{Naïve})_p(\text{Month})_{wp}(\text{Stability Challenges Completed})_p \\ & + U_p + \epsilon_{wp} \end{aligned}$$

Due to how the model is specified, the b_3 (Month)(Challenges) interaction captures the simple effect of completing challenges for participants who were aware of which trait the intervention was targeting. In contrast, the b_7 three-way interaction term captures whether the effect of completing challenges differed for people who were naïve with respect to which trait the intervention was targeting. Thus, the simple two-way (Month)(Challenges) interaction for naïve participants is equal to $b_3 + b_7$.

5.2.2. Did completing challenges predict trait Growth?

As can be seen in Table 2, participants who completed more numerous conscientiousness challenges tended to experience greater growth in conscientiousness, as compared with their peers who completed fewer challenges (simple $b_{\text{Month} \times \text{Challenges}} = 0.03$, 95% CI [0.01, 0.05]). Moreover, this effect was not moderated by whether or not participants were naïve (versus aware) that they were working on conscientiousness ($b_{\text{Naïve} \times \text{Month} \times \text{Challenges}} = 0.00$, 95% CI [-0.03, 0.03]). Thus, as depicted in Fig. 3, it did not matter whether participants chose to work on conscientiousness or not (and thus whether they were naïve or not); as long as participants were incorporating more new, highly conscientious actions into their behavioral repertoire each week (via completing conscientiousness challenges), they were predicted to experience growth in conscientiousness across time.

In comparison to conscientiousness, the pattern of results was far more nuanced for emotional stability. As can be seen in Fig. 4, the effect of completing emotional stability challenges on trait growth was completely contingent upon whether participants chose to work on emotional stability or not ($b_{\text{Naïve} \times \text{Month} \times \text{Challenges}} = -0.10$, 95% CI [-0.13, -0.07]). As depicted in the left-hand panel of Fig. 4, replicating prior

Table 2
Study 2 growth in personality traits as a function of challenges completed.

Predictor	Conscientiousness		Emotional Stability			
	<i>b</i>	95% CI	<i>b</i>	95% CI		
Intercept	-0.04	-0.14	0.06	-0.02	-0.10	0.07
Month ^a	0.02	-0.00	0.04	0.06	0.04	0.08
Challenges ^{a,b}	-0.02	-0.09	0.11	0.04	-0.03	0.12
Month × Challenges ^{a,b}	0.03	0.01	0.05	0.06	0.04	0.08
Naïve ^c	-0.03	-0.17	0.10	0.02	-0.11	0.16
Naïve ^c × Month	0.00	-0.03	0.03	0.00	-0.03	0.03
Naïve ^c × Challenges ^b	0.01	-0.12	0.14	0.00	-0.12	0.13
Naïve ^c × Month × Challenges ^b	0.00	-0.03	0.03	-0.10	-0.13	-0.07
	<i>s</i> ²	<i>SE</i>	<i>s</i> ²	<i>SE</i>		
Random Intercept	0.09	0.01	0.07	0.01		
Random Slope	0.02	0.003	0.02	0.004		

Note. Ninety-five percent confidence intervals for parameter estimates in bold-face do not include zero. All models include the appropriate Time 1 trait and the interaction thereof with Month to control for regression to the mean. For all statistically significant interaction terms, $p \leq 0.002$.

^a Due to how the model is specified, these are the simple effects for people who were aware of which trait they were changing (e.g., they chose to work on conscientiousness and actually received conscientiousness challenges).

^b Challenges = number of challenges completed.

^c Naïve = participants were unaware they were completing challenges for this domain.

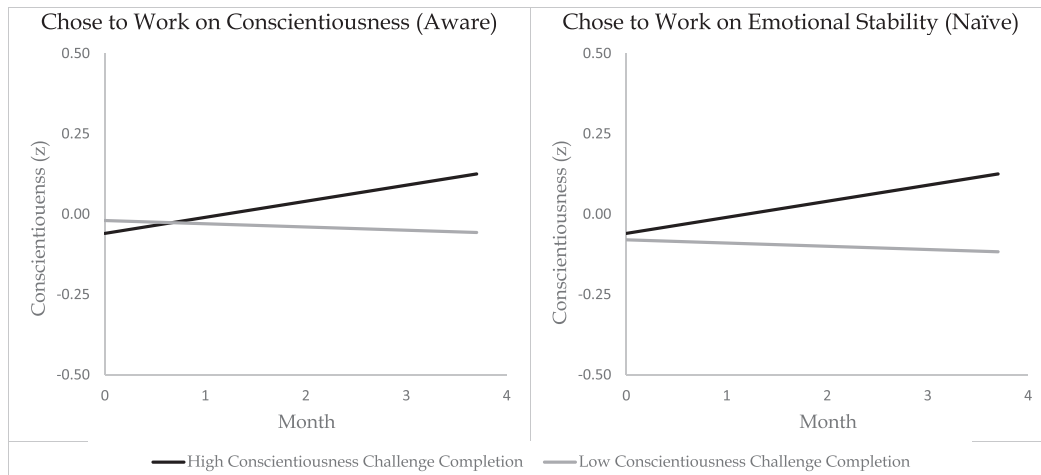


Fig. 3. Study 2 Predicted Growth in Conscientiousness as a Function of Completing Conscientiousness Challenges. Note. The left-hand panel depicts participants who chose to work on conscientiousness and thus knowingly completed conscientiousness challenges. The right-hand panel depicts participants who chose to work on emotional stability and thus did not realize that they were completing conscientiousness challenges. The interactions in the right- and left-hand panels are not statistically different from one another. The “high challenge completion” lines are plotted at 1 SD above the mean of challenge completion (6.43 challenges/week). The “low challenge completion” lines are plotted at 1 SD below the mean of challenge completion (1.32 challenges/week).

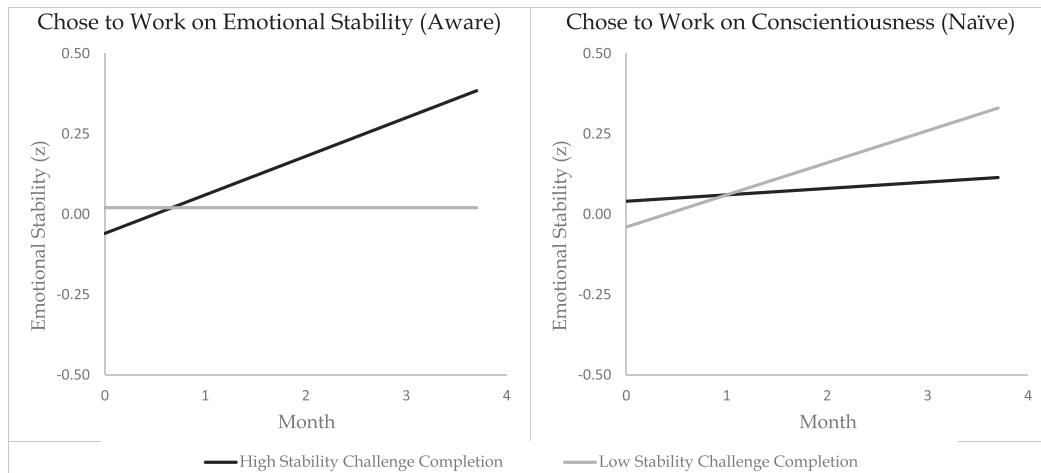


Fig. 4. Note. Study 2 predicted growth in emotional stability as a function of completing emotional stability challenges. The left-hand panel depicts participants who chose to work on emotional stability and thus knowingly completed emotional stability challenges. The right-hand panel depicts participants who chose to work on conscientiousness and thus did not realize that they were completing emotional stability challenges. The three-way interaction and both two-way interactions are statistically significant. The “high challenge completion” lines are plotted at 1 SD above the mean of challenge completion (6.43 challenges/week). The “low challenge completion” lines are plotted at 1 SD below the mean of challenge completion (1.32 challenges/week).

research (Hudson et al., 2019), among participants who chose to work on emotional stability, completing more numerous emotional stability challenges predicted greater trait growth (simple $b_{Month \times Challenges} = 0.06$, 95% CI [0.04, 0.08]). Indeed, participants who chose to work on emotional stability and then completed high numbers of challenges (1 SD above the mean; 7.73 challenges/week) were predicted to increase 0.12 SDs in emotional stability per month (95% [0.10, 0.14])—amassing to a sizable 0.42 SD cumulative increase in the trait over the study’s duration (95% CI [0.33, 0.50]). (Participants who chose to work on emotional stability but then completed low numbers of challenges [1 SD below the mean; 1.97 challenges/week] were not predicted to increase in emotional stability across time, simple $b_{Month} = 0.00$, 95% CI [-0.03, 0.02].) In contrast, for naïve participants, who unknowingly completed emotional stability challenges (because they chose to work on conscientiousness and were randomly assigned to complete emotional stability challenges instead), completing more numerous challenges was actually associated with less growth in emotional stability each month (simple $b_{Month \times Challenges} = -0.04$, 95% CI [-0.06, -0.02]).

This latter finding in particular provides a conceptual replication of Study 1. In Study 1, I found that trait growth was unrelated to challenge completion when participants did not choose to work on emotional stability—but rather were assigned by the study website to do so. The findings in Study 2 parallel those of Study 1. Participants who did not choose to work on emotional stability (because they explicitly chose to work on conscientiousness instead) did not experience growth in emotional stability as a function of completing emotional stability challenges. Thus, taken together, both Studies 1 and 2 suggest that interventions targeting emotional stability are inert if participants (1) did not explicitly, autonomously choose to work on emotional stability and (2) they are not deeply motivated to change with respect to emotional stability. This supports Neo-Socioanalytic claims that psychological investment is critical to changing trait levels of emotional stability (Lodi-Smith & Roberts, 2007; Roberts & Wood, 2006). These findings also dovetail nicely with common clinical sentiments that psychotherapy is most effective (e.g., in reducing negative affect) when clients are strongly autonomously motivated to improve themselves (e.g., Zuroff

et al., 2007).

To summarize the Study 2 findings thus far, participants who completed conscientiousness challenges experienced growth in conscientiousness across time—irrespective of whether they chose to work on conscientiousness and knew that they were working on changing the trait. In contrast, the effect of completing emotional stability challenges was completely dependent on whether participants chose to work on emotional stability or not. For those who did not choose to work on emotional stability, the challenges were entirely inert—or even *backfired*. In contrast, for those who did choose to work on emotional stability, the challenges appeared to facilitate relatively large amounts of trait growth. These findings collectively parallel Study 1 in suggesting that motivation is largely irrelevant to interventions targeting conscientiousness; as long as participants adhere to the intervention, they will experience trait growth. In contrast, it does not appear to be possible to successfully randomly assign participants to increase in emotional stability.

5.2.3. Do psychological expectations predict trait growth?

One critical question raised by the analyses thus far is the extent to which the findings might be driven by psychological expectations. For example, the emotional stability challenges only led to growth in trait emotional stability when participants both chose to work on emotional stability and were aware that the challenges were targeting emotional stability. This raises the possibility that the interventions might be driven by participants' expectations, rather than the actual behaviors they performed.

To that end, I also analyzed the extent to which participants who *believed* they were working on one trait (e.g., conscientiousness) but completed challenges for a different trait (e.g., emotional stability) experienced changes in the trait they *believed* they were working on, as a function of challenges completed. For participants who chose to work on conscientiousness, unknowingly completing emotional stability challenges did not predict growth in trait conscientiousness (simple $b_{\text{Month} \times \text{Challenges}} = 0.00$, 95% CI [-0.03, 0.02]). Similarly, for participants who chose to work on emotional stability, unknowingly competing conscientiousness challenges did not predict growth in trait emotional stability (simple $b_{\text{Month} \times \text{Challenges}} = -0.01$, 95% CI [-0.02, 0.01]). In other words, completing irrelevant challenges did not predict growth in traits—even in direct spite of participants' expectations (e.g., that the intervention would help them change the trait that they chose).

Thus, these findings align with previous research (Hudson et al., 2019) and suggest that motivation or expectations alone are *not* sufficient to change personality traits. Rather, successfully modifying one's traits appears to require actually engaging in appropriate cognitive, affective, and behavioral changes (e.g., completing trait-relevant challenges). For conscientiousness, autonomous motives and free choice appeared to be irrelevant, as completing challenges, whether knowingly or not, led to greater trait growth. In contrast, the findings in Study 2 suggest that changing emotional stability requires an aligning of *both* individuals' choice/motives *and* an effective trait-change intervention (e.g., completing weekly challenges).

6. General discussion

The present studies were designed to examine two ingredients thought to contribute to the efficacy of trait-change interventions. Namely, theorists have specified that intervention success may hinge upon participants being both (1) free to autonomously choose which traits they are motivated to change and (2) actively psychologically invested in changing the target traits (Allemand & Flückiger, 2017; Hennecke et al., 2014; Hudson, 2021; Hudson & Fraley, 2015, 2017; Magidson et al., 2014; Roberts, Hill, et al., 2017). Accordingly, the present studies tested whether interventions can be effective (1) if participants are assigned traits to change or (2) if participants are naïve as to which traits the intervention is targeting (and thus are not actively

psychologically invested in the intervention's true aims).

6.1. Interventions to change conscientiousness

The results of both studies indicated that participants can be successfully assigned to increase in conscientiousness. Moreover, this was true even if participants were unaware that the intervention was targeting conscientiousness. These findings have both applied and theoretical implications. On an applied level, these results suggest that participants' motives are not particularly relevant to the efficacy attempts to intervene upon conscientiousness (cf. Hudson & Fraley, 2017; Roberts, Hill, et al., 2017). Thus, interventionist-directed approaches, in which participants are instructed to change their conscientiousness, can be effective. Moreover, to be successful, such interventions merely need to change participants' behaviors; working to align the intervention with participants' autonomous choice and psychological investment in the change process does not appear to be necessary (cf. Deci & Ryan, 1985; Lodi-Smith & Roberts, 2007). Thus, interventionists interested in increasing conscientiousness (e.g., researchers, policymakers, perhaps even parents) merely need to incentivize participants to implement new conscientious thoughts, feelings, and behaviors.

Of course, it is important to emphasize that participants in the present studies were college students. Prior research suggests that students are generally receptive to researcher-directed behavioral change suggestions, making intervention attempts relatively easy (e.g., Jacques-Hamilton et al., 2018; McNeil et al., 2010). It is possible that other populations (e.g., older adults, less-educated individuals) may not be as amenable to such requests. Thus, policymakers or researchers targeting non-college-aged adults may need to incentivize adoption of new behaviors. Nevertheless, to the extent that participants engage in conscientious state-level behaviors—for whatever reason—trait-level change should follow.

Applied implications aside, the present findings also help elucidate broader theories of how conscientiousness develops. Namely, it appears that individuals do not need to be psychologically invested in the process of becoming more conscientious in order to experience growth in the trait (cf. Lodi-Smith & Roberts, 2007; Roberts & Wood, 2006). Instead, conscientiousness appears to operate in a relatively straightforward Sociogenomic fashion: chronic state-level changes in conscientiousness translate into trait-level growth, irrespective of individuals' motives to change or investment in/awareness of the process (Roberts, 2018).

6.2. Interventions to change emotional stability

In contrast to the conscientiousness findings, both Studies 1 and 2 robustly found that participants could *not* be successfully assigned to increase in emotional stability. Indeed, participants were likely to experience growth in emotional stability only if they *both* (1) autonomously chose to work on emotional stability *and* (2) received an effective intervention. In isolation, neither component was sufficient. Those who chose to work on emotional stability but received an inert intervention (that actually targeted conscientiousness) did not experience increases in emotional stability. Similarly, those who were assigned to increase in emotional stability—without explicitly autonomously choosing to do so—also did not experience growth in the trait. Critically, these null effects were not attributable to intervention adherence. Participants assigned to work on emotional stability completed equal numbers of weekly challenges as did participants who freely chose to work on the trait.

Thus, these findings seem to suggest that autonomous choice and psychological investment in the change process are critical to changing emotional stability (Deci & Ryan, 1985, 2000; Lodi-Smith & Roberts, 2007; Roberts & Wood, 2006). This seems to support a Neo-Socioanalytic model of development for emotional stability: State-level changes alone are not sufficient to promote growth in emotional

stability. Instead, state-level changes must be accompanied by autonomous motivation to change emotional stability—and likely deep psychological investment in the process of doing so (Lodi-Smith & Roberts, 2007; Roberts & Wood, 2006). This dovetails nicely with clinical observations that psychotherapies are most effective in reducing negative affect (e.g., anxiety, depression) when individuals are autonomously motivated to work on improving themselves (e.g., Zuroff et al., 2007).

Why does emotional stability function differently from conscientiousness? As one potential possibility, some scholars have noted that emotional stability is somewhat qualitatively different from the remaining four traits (e.g., Paulhus & Vazire, 2007; Vazire, 2010). Namely, extraversion, agreeableness, conscientiousness, and openness to experience each have large externally-observable behavioral components. For example, extraverts behave in a gregarious fashion, flocking to others and assuming a more dominant and vivacious role in social interactions. In contrast to these primarily outwardly expressed traits, emotional stability predominantly refers to internal affective states (Goldberg, 1993).

Contrasting with behaviors, which are relatively straightforward to volitionally modify (e.g., with perhaps some effort, most individuals can presumably clean their homes; with perhaps some anxiety, most people can presumably meet someone new at a coffee shop), emotions are difficult to alter via pure volition alone (e.g., “just stop feeling sad”). Indeed, research suggests that people generally use indirect strategies to regulate their affect, including avoiding emotionally evocative stimuli or participating in activities that alleviate negative feelings, such as exercise or socializing (e.g., Gross, 1998). Similarly, cognitive behavioral therapies oftentimes target emotions indirectly by encouraging clients to directly change their thought patterns, rather than their emotions *per se* (e.g., Goldin et al., 2012).

Thus, the affective nature of emotional stability and the indirect methods required to target it through intervention (e.g., emotion regulation strategies) may necessitate a higher level of motivation, intentionality, and psychological investment from participants to change the trait. For example, emotion regulation strategies such as exercise (e.g., Cooney et al., 2014), journaling (e.g., King, 2002), expressing gratitude (e.g., Emmons & McCullough, 2003), or seeking social support (e.g., Collins & Feeney, 2000) may be effective in reducing state-level negative affect only if individuals construe and utilize those activities as a method for controlling negative emotions. In sum, the affective nature of emotional stability may cause it to function differently from the remaining four traits in the context of intervention attempts.

Irrespective of why emotional stability functioned differently from conscientiousness, the present studies offer at least two implications for interventions designed to change emotional stability. First, such interventions may be effective only for individuals who explicitly and autonomously want to increase in emotional stability (Deci & Ryan, 1985). Thus, it may not be possible for interventionists (e.g., researchers, policymakers) to direct participants to become more emotionally stable. Nevertheless, it may be possible for interventionists to work to align participants' desires with the aims of the intervention (Hudson, 2021; Hudson & Fraley, 2015, 2017). In other words, interventionists may be able to convince participants that growth in emotional stability is desirable and consequently find success in helping participants change.

A second implication is that emotional stability interventions appear to require that participants be aware that emotional stability is the target trait—and that they be actively invested in changing the trait. Thus, unlike conscientiousness, it seems that interventionists cannot recommend contextless behavioral changes for emotional stability (e.g., journal about negative feelings, count your blessings) and expect them to spur trait growth. Instead, the present findings suggest that participants need to be actively working alongside the intervention to explicitly change their levels of emotional stability.

6.3. Methodological implications

In addition to the applied and theoretical implications discussed above, Study 2 in particular shores up some methodological limitations of prior intervention work (Hudson et al., 2019; Hudson & Fraley, 2015, 2016a). Namely, interventions in which participants chose which traits they would like to change inherently have some level of demand characteristics and potential placebo effects. Thus, it is possible that participants in an intervention targeting emotional stability, for example, might perceive illusory changes to their traits because they *believe* an intervention will help them change.

The present Study 2 in particular casts doubt on the notion that intervention-driven trait growth is attributable to these types of demand or placebo effects. Namely, in Study 2, participants chose which trait they would like to change—but some participants were randomly assigned to unknowingly receive an intervention targeting a different trait (e.g., some participants who elected to work on conscientiousness received an intervention targeting emotional stability, and vice versa). The vast majority of participants were unaware of this manipulation and expressed no suspicion (despite many participants describing detailed, albeit incorrect, hypotheses as to the study's true purpose).

The results of Study 2 suggested that, among these participants, expectations had no effect. For example, participants who *believed* they were working on conscientiousness—but in reality were not—did not experience growth in conscientiousness. Similarly, participants who *thought* they were working on emotional stability—but in reality were not—did not experience growth in emotional stability. Thus, participants in Study 2 were *not* reporting illusory growth in traits as a function of perceived demand or their own expectations. Consequently, the results of Study 2 should be reassuring to future intervention attempts; interventions appear to produce trait growth that does not seem to be attributable to demand or participants' expectations.

6.4. Other implications, limitations, and future directions

The single biggest implication of the present studies is that personality traits can, in fact, be changed through intervention—at least over a period of four months. Thus, these studies bolster an emerging literature describing successful interventions to change personality traits (Carnelley & Rowe, 2007; Gillath et al., 2008; Hudson et al., 2019; Hudson & Fraley, 2015, 2018; Jackson et al., 2012; Krasner et al., 2009; Oei & Jackson, 1980; Roberts, Luo, et al., 2017). Moreover, these studies extend this body of knowledge by suggesting that people can be assigned to change conscientiousness—whereas they cannot be assigned to increase in emotional stability. These findings suggest that different traits develop via different processes and respond differently to intervention attempts. This has important implications for the creation of future interventions targeting these two traits—and it also helps elucidate theories of how and why personality traits change across time. Collectively, these findings suggest that scholars may need to consider idiosyncrasies in how individual traits develop—rather than formulating omnibus theories that treat all five traits equivalently.

That said, one limitation of the present studies is that—due to practical constraints on the research process—I only explored interventions to change two traits. Thus, it remains unclear whether the remaining three traits (extraversion, agreeableness, and openness) function similarly to conscientiousness or to emotional stability. There is good theoretical rationale to expect that emotional stability is qualitatively different from the other four traits (Vazire, 2010)—and thus extraversion, agreeableness, and openness should function similarly to conscientiousness. Moreover, some empirical evidence tentatively reaffirms that traits such as agreeableness or openness function similarly to conscientiousness, in that they change in response to interventions even when participants did not explicitly choose to work on agreeableness or openness (Jackson et al., 2012; Krasner et al., 2009). In contrast, other studies have found mixed evidence as to whether

interventions can be effective in targeting agreeableness or openness at all. For example, one recent smart phone app-based intervention was successful in helping people change their levels of agreeableness (Stieger et al., 2021), whereas other very similar web-based interventions did not produce statistically significant effects for agreeableness or openness (Hudson et al., 2019; Hudson & Fraley, 2015). Nevertheless, the idea that extraversion—and perhaps agreeableness and openness—function similarly to conscientiousness (and thus emotional stability is qualitatively different) should be tested by future research with interventions explicitly designed to target the remaining three traits.

Irrespective of these issues, the fact that personality can be changed through intervention may have important implications for life outcomes. Personality traits are linked to a wide gamut of valued criterion variables, such as health, well-being, and even mortality (Ozer & Benet-Martínez, 2006; Roberts et al., 2007). Consequently, trait-change interventions may have the potential to enhance these important outcomes. For example, Hudson and Fraley (2016a) found that attaining desired increases to most of the big five traits predicted gains in well-being. However, one limitation of the present studies is that I did not include measures of outcome variables that growth in conscientiousness and emotional stability should theoretically be expected to influence (e.g., academic performance, health). Future research should collect measures of such criterion variables and test whether they are responsive to intervention-driven personality trait growth.

Another implication of the present studies is that personality trait growth reported in the context of intervention attempts does not appear to be an artifact of demand or placebo effects. Indeed, Study 2 separated the traits participants *believed* they were working on from the traits the intervention was actually targeting. Participants did *not* experience trait growth as a function of which traits they *thought* the intervention was targeting. That said, it is still possible that demand characteristics played a role in these studies. For example, participants may have figured out that the interventions were targeting a different trait than what was selected, but they may have been hesitant to admit that they were suspicious during debriefing. Moreover, these studies were further limited in that they relied exclusively on self-report data. Different measures of personality traits (e.g., self-report, observer-report) have differing strengths and limitations (Paulhus & Vazire, 2007). Future intervention research should collect multiple types of personality reports—such as observer reports, which are not as susceptible to demand—to corroborate across multiple methods that true trait change is occurring.

A related limitation of the present studies is that they were relatively short—approximately four months in duration. Thus, it remains unclear (1) how much interventions can change personality traits and (2) how long intervention-driven changes can endure. One recent review of more than 200 studies found that personality traits change in response to clinical interventions (e.g., psychotherapy) and the changes can endure for up to years after the cessation of treatment (Roberts, Luo, et al., 2017). However, the impact of therapy on personality was maximized within a short period of time (several weeks) and quickly tapered off thereafter. Of course, psychotherapy is not primarily designed to change personality traits. Thus, it remains possible that larger changes over longer timespans may be possible with interventions expressly designed to change traits. Future research should explore this possibility using extended longitudinal designs (e.g., that span several years). Future studies should also test whether, akin to psychotherapy, interventions explicitly targeting personality traits can produce trait growth that endures over the span of multiple years—as opposed to trait growth that cyclically reverts after the cessation of the intervention (e.g., Biesanz et al., 1998).

One final limitation of the present studies is that both samples consisted of college students—and were predominantly female. Thus, it is possible that the findings would not generalize to other populations, such as older adults, males, or less-educated individuals. For instance, it may be the case that older adults might experience greater difficulty in changing their traits than do younger adults. Alternatively, it is possible

that people's personalities are similarly responsive to environmental factors, such as interventions, irrespective of age (Briley & Tucker-Drob, 2014). Therefore, future research should attempt to replicate these findings in more diverse samples.

7. Conclusion

The present studies contribute to a growing empirical consensus that personality traits can be changed through intervention. But do successful trait-change interventions require that participants be autonomously motivated to change? The present studies suggest that the answer is “it depends.” For conscientiousness, autonomous motivation and free choice appear to be irrelevant. So long as participants change their behavior, trait change will follow. In contrast, it appears that participants must, in fact, be autonomously motivated to change for emotional stability interventions to be effective. All said, these findings provide practical guidance for the development of trait-change interventions and suggest that different traits may be liable to different developmental processes. Future research should continue to explore trait-change interventions and explicitly test the ingredients that contribute their efficacy.

8. Author Note

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Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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