New Evidence on Self-Affirmation Effects and Theorized Sources of Heterogeneity from Large-scale Replications

Paul Hanselman, Christopher S. Rozek, Jeffrey Grigg,
and Geoffrey D. Borman

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Author Note

Paul Hanselman, University of California, Irvine
Christopher S. Rozek, University of Chicago
Jeffrey Grigg, Johns Hopkins University
Geoffrey Borman, University of Wisconsin-Madison

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Address correspondence to Paul Hanselman, University of California, Irvine, 3200 Education Irvine, CA 92697-5500.
Contact: paul.hanselman@uci.edu

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Abstract

Brief, targeted self-affirmation writing exercises have recently been offered as a way to reduce racial achievement gaps, but evidence about their effects in educational settings is mixed, leaving ambiguity about the likely benefits of these strategies if implemented broadly. A key limitation in interpreting these mixed results is that they come from studies conducted by different research teams with different procedures in different settings; it is therefore impossible to isolate whether different effects are the result of theorized heterogeneity, unidentified moderators, or idiosyncratic features of the different studies. We addressed this limitation by conducting a well-powered replication of self-affirmation in a setting where a previous large-scale field experiment demonstrated significant positive impacts, using the same procedures. We found no evidence of effects in this replication study and estimates were precise enough to reject benefits larger than an effect size of 0.10. These null effects were significantly different from persistent benefits in the prior study in the same setting, and extensive testing revealed that currently theorized moderators of self-affirmation effects could not explain the difference. These results highlight the potential fragility of self-affirmation in educational settings when implemented widely and the need for new theory, measures, and evidence about the necessary conditions for self-affirmation success.

Keywords: values affirmation, replication, stereotype threat, intervention, achievement gap, scale-up, middle school

New Evidence on Self-Affirmation Effects and Theorized Sources of Heterogeneity from Large-scale Replications

One potentially promising approach to reducing persistent racial/ethnic achievement gaps is to tackle their social-psychological dimensions, including the negative consequences of stereotype threat and other identity threats in school. Because identity threats have detrimental consequences for marginalized groups in many academic settings (Steele, Spencer, & Aronson, 2002), such approaches can have substantial impacts. For instance, brief reflective writing exercises conducted in school settings can provide large and lasting benefits for theoretically-threatened groups, such as African American and Hispanic middle-school students (Cohen, Garcia, Purdie-Vaughns, Apfel, & Brzustoski, 2009; Sherman et al., 2013), women in a college physics course (Miyake et al., 2010), and first-generation college students (Harackiewicz et al., 2014).

How robust are these effects? Although benefits of seemingly simple interventions suggest great potential, researchers caution that these techniques are “not magic” (Yeager & Walton, 2011). By their nature, the interventions target specific interactions between individuals and their social context and, therefore, critical differences in intervention delivery, individual students, or social contexts may lead to substantial variability in effectiveness. As a result, one must gauge the impact of these interventions in diverse settings and, to the extent that there are meaningful differences in effects, assess whether theorized moderators explain these differences. If heterogeneous effects follow theoretically predictable patterns, then these interventions have a clear role in improving educational outcomes and reducing achievement gaps. However, if heterogeneity remains unpredictable, then the immediate value of these interventions is less clear.

Theorized heterogeneity also complicates the fundamental enterprise of independent replication, which is increasingly recognized as necessary to build firm scientific understanding in psychology as in other fields (Ioannidis, 2005; Ioannidis, 2012; Pashler & Harris, 2012). If the impacts of social-psychological interventions depend on seemingly subtle differences in delivery, individuals, and social contexts, then discrepant replication results may reflect predictable differences in effectiveness across diverse settings. On the other hand, mixed results may be due to unpredictable study-specific differences, such as unrecognized moderators or sampling variation. This distinction is especially difficult to disentangle when studies are conducted by different investigators and with different populations in different contexts. As a result, initial replication efforts of affirmation interventions in educational settings—which demonstrate success (e.g. Sherman et al., 2013), challenges (e.g. Kost-Smith et al., 2012), and failure (e.g. Dee, 2015)—raise questions about both the size and variability of these effects when implemented broadly. In particular, do theorized moderators explain differences in self-affirmation benefits? This study provides unique evidence on this question by reporting on a new large-scale test of self-affirmation effects and comparing these results to a previous effort in the same setting.

Self-affirmation: Theory and Promise

This study is informed by theories of social identity threats, which create particular challenges for members of marginalized social groups in school (Steele et al., 2002). For instance, Black and Hispanic students are subject to *stereotype threat* in academic settings, in which they face the threat of conforming to or being judged by negative stereotypes about their racial/ethnic group (Steele & Aronson, 1995). The experience of stereotype and other identity threats leads to poorer academic performance through a variety of psychological responses,

including stress, anxiety, and vigilance (Schmader, Johns, & Forbes, 2008), and may contribute to longer term disengagement and a "downward spiral" of performance (Cohen & Garcia, 2008). Since these stereotype threats uniquely apply to groups subject to negative academic stereotypes, they may account for portions of the widening of racial achievement gaps in school.

Stereotype threats are pernicious because students are affected by virtue of membership in a marginalized group (regardless of whether or not they endorse a negative stereotype, as long as they are aware of it), and broad social stereotypes are difficult to change. Instead, the goal of many social-psychological interventions is to reduce the harm that existing threats cause by shifting how students view themselves and/or their social world (Wilson, 2011). The example we consider is a set of brief writing exercises that ask students to reflect on meaningful personal values, such as family, friends, music, or sports. Following their initial presentation (e.g. Cohen, Garcia, Apfel, & Master, 2006; Cohen et al., 2009; Sherman et al., 2009), we refer to these activities as *self-affirmation* interventions throughout this paper, reflecting the goal to allow students to "reaffirm their self-integrity" (Cohen et al., 2006, p. 1307). Similar interventions have also been described as "values affirmation" (e.g. Cook, Purdie-Vaughns, Garcia, & Cohen, 2012; Harackiewicz et al., 2014; Shnabel, Purdie-Vaughns, Cook, Garcia, & Cohen, 2013).

Self-affirmation interventions are believed to restore an individual's sense of worth in the face of threats related to social identity, thus mitigating detrimental stress responses (Steele, 1988). Because individual identities are complex, individuals "can maintain an overall self-perception of worth and integrity by affirming some other aspect of the self, unrelated to their group" (Sherman & Cohen, 2006, p. 206). Threats to academic identity experienced by minority members in school can be muted by focusing attention on other specific aspects of identity (Critcher & Dunning, 2015; Sherman & Cohen, 2006; Steele, 1988; Walton, Paunesku, &

Dweck, 2012). Reflection on important values provides a psychological buffer against the full brunt of detrimental stereotype threats in school, and because of the potentially recursive nature of threat and poor performance, subtle buffering early on may lead to substantial benefits over time (Cohen & Garcia, 2008; Cohen et al., 2009; Taylor & Walton, 2011; Walton, 2014).

Geoffrey Cohen and his colleagues have developed these theoretical ideas alongside specific classroom writing activities to promote self-affirmation via reflection on important values. Each activity takes 15-20 minutes and is conducted by classroom teachers several times during the school year; the timing emphasizes critical moments such as the beginning of the school year and potentially stressful evaluative milestones. Consistent with theoretical expectations, these activities did not significantly impact White students' academic performance, who likely experienced relatively little academic identity threat (Walton & Cohen, 2003). However, the effects on grade point average for 7th grade African American and Hispanic students were substantial and persistent (Cohen et al., 2006; Cohen et al., 2009; Cook et al., 2012; Sherman et al., 2013). Remarkably, the benefits of the intervention reduced the racial achievement gap in the targeted course by 40% (Cohen et al., 2006, p. 1307), which suggests great potential for this approach to address educational disparities that are associated with identity threat processes.

What mediates these effects? Critcher and Dunning (2015) presented recent laboratory evidence for an "affirmation as perspective" model, in which self-affirmations "expand the contents of the working concept—thus narrowing the scope of any threat" (p. 4). Working concept refers to the salient identities that make up one's self-concept in consciousness at any point in time. When aspects of identity are threatened, working self-concept tends to constrict, amplifying the negative experiences of that threat. However, if a broader working concept is

maintained, then threats associated with a specific aspect of identity are less salient. It stands to reason that self-affirmation in school expands the contents of self-concept for students subject to academic stereotypes, thus reducing attention to the threat and muting the stress responses that lead to poorer performance.

Empirical tests of mediators in middle school settings have been mixed. Cook et al. (2012) reported impacts of self-affirmation on Black students' level and variability of sense of belonging in school, which indicate effects on students' construal of their social environments, but the authors argued that these effects are "not a mechanism in the sense of mediation" (p. 483). Similarly, Sherman et al. (2013) reported impacts on higher levels of construal and a more robust sense of social belonging, while Cohen et al. (2006) reported decreases on a measure of cognitive activation of racial stereotype, yet neither found evidence that these effects mediated the impact of self-affirmation. Shnabel et al. (2013) found that writing about social belonging mediated some of the self-affirmation benefits; however, Tibbetts et al. (in press) did not replicate this result in another setting and instead found that writing about independence mediated some of the affirmation benefits.

The self-affirmation writing exercises have been implemented in at least four middle school field settings beyond the original one. Figure 1 summarizes both the positive impacts from early field trials within three schools (Cohen et al., 2006; Sherman et al., 2013) and smaller and sometimes non-statistically significant estimates in large-scale, multi-school replications (Borman, Grigg, & Hanselman, 2016; Dee, 2015).[1] The latter are well-powered studies

[1] The summary presented in Figure 1 should be viewed as an informal account of previous self-affirmation impacts in middle school settings. A formal and more expansive meta-analysis will certainly be useful in the future as more independent evidence emerges, but our specific purpose in collecting these estimates was to provide context for the current study. We therefore focus only on studies in middle schools that report self-affirmation effects on overall GPA relative to an alternate activity. These criteria rule out studies at other levels (e.g., Miyake et al., 2010),

conducted by independent research teams, and their results raise questions about the fundamental sources of variability in self-affirmation effects. Unfortunately, many features of the research settings varied in these studies and little implementation information is available to isolate the impact of specific differences. For instance, the study conducted by Dee (2015) illustrates multiple potentially relevant changes across research efforts. For one, it was conducted in schools with substantial minority student populations; these are contexts where self-affirmation may be less effective (Hanselman, Bruch, Gamoran, & Borman, 2014). For another, it recruited an unusually representative sample of students (a 94% consent rate), which could account for dampened impacts if the students not typically included in other studies benefit less from the intervention. These preliminary results suggest the need for more precise consideration of where, for whom, and under what conditions self-affirmation is beneficial.

Theoretical Moderators of Self-Affirmation Effects

Psychological theory posits that self-affirmation is beneficial in specific circumstances (Cohen & Sherman, 2014; Yeager & Walton, 2011), highlighting the need to identify the necessary and sufficient "preconditions" for its benefits in educational settings (Cohen et al., 2006). Null results emphasize this point, since existing theory provides post hoc explanations but not clear insight into when, where, and why self-affirmation might not have worked (e.g., see Harackiewicz, Canning, Tibbetts, Priniski, & Hyde, in press). And of course if moderators were well understood, then studies would likely not have been fielded in such unsuccessful contexts.

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those that consider other outcomes (e.g., Cook et al., 2012, Study 1), and those without a non-self-affirmation control group (e.g., Cook et al., 2012, Study 2). Similarly, we omit the study by Bowen, Wegmann, and Webber (2013) because reported values do not include an overall estimate of impacts on GPA (that study reports offsetting impacts on initial GPA and slope over time; inspection of their Table 3 and Figure 1 suggests this study would contribute a small negative effect on overall GPA to our summary if included). We include detailed information about the source of represented estimates in Appendix Table A1.

In surveying potential self-affirmation moderators, the literature points to three relevant domains: features of the delivery of the activities, individual characteristics of the participating students, and aspects of the social context. First, specific features of the delivery of the brief self-affirmation intervention are hypothesized to be necessary for students to benefit. For example, Critcher, Dunning, and Armor (2010) found that self-affirmation exercises were only effective when introduced before a threat or before participants became defensive in response to a threat, which suggests that it is important to implement self-affirmation exercises before stressful events in school in order to short-circuit negative recursive cycles (see also Cohen & Garcia, 2014; Cook et al., 2012). Qualities of presentation that shape how students perceive the writing activities—such as making participants aware that exercises are beneficial (Sherman et al., 2009) or externally imposing affirmation (Silverman, Logel, & Cohen, 2013)—may mute self-affirmation benefits. Conversely, researchers have argued that the activity is most beneficial when presented as a normal classroom activity (Cohen & Sherman, 2014; Purdie-Vaughns et al., 2009) and when promoting specific types of writing (e.g., Shnabel et al., 2013). Finally, the type of control group used has also been suggested as an implementation-based moderator of the effects of self-affirmation. The typical control group, which asks students to write about non-important values, has the potential to undermine students' confidence if they write about activities in which they have low ability whereas other control writing prompts, which are more neutral or open-ended, might allow control participants to spontaneously affirm themselves (McQueen & Klein, 2006).

Second, numerous individual difference variables have been hypothesized to make students more vulnerable to stereotype threat and thus moderate the effects of self-affirmation, including identifying with a negatively stereotyped group, being knowledgeable about self-

relevant negative stereotypes, and caring about doing well in school (Aronson, Lustina, Good, Keough, & Steele, 1999; Cohen & Sherman, 2014; Shapiro & Neuberg, 2007). Therefore, while all negatively stereotyped minority students might be helped by self-affirmation, subgroups that are even more highly negatively stereotyped, such as Black males (Eagly & Kite, 1987; Purdie-Vaughns & Eibach, 2008; Sidanius & Pratto, 1999) or the lowest-achieving minority students (Cohen et al., 2009), might benefit most from self-affirmation.

Finally, context variables are hypothesized to moderate self-affirmation benefits. Social characteristics, such as group composition and environmental cues, influence the behavior and performance of stereotyped students (Dasgupta, Scircle, & Hunsinger, 2015; Inzlicht & Ben-Zeev, 2000; Murphy, Steele, & Gross, 2007). The effectiveness of self-affirmation approaches depends on the identity threats "in the air" in a particular setting (Steele, 1997), and the hypothesized recursive benefits are theorized to depend on relatively rich learning environments for threatened students to take advantage of as they are buffered from perceived threats (Cohen & Sherman, 2014). Because self-affirmation is theorized to disrupt stereotype threat processes, settings in which threats are more likely to be experienced may provide the greatest opportunity for benefits. For instance, while self-affirmation reduced gender disparities in performance in an introductory college physics course (Miyake et al., 2010), it was not beneficial in introductory science settings in which gender gaps and stereotype threat were not present (Lauer et al., 2013). Theory and empirical evidence also suggest that minority students attending schools in which their group is poorly represented and in which there are large racial achievement gaps benefit most from self-affirmation (Cohen & Garcia, 2014; Hanselman et al., 2014).

In summary, psychological theory posits moderators of self-affirmation effects in several domains, but evidence for specific moderators is limited because the data to test these theories

are lacking, especially in applied educational settings. This means that mixed evidence of self-affirmation benefits may be due to theorized variation in how the activities were delivered, individual characteristics, or social contexts. In particular, very little is known about how to translate theorized constructs and laboratory manipulations into measures of the relevant moderating features as they occur in applied settings. Moreover, it is impossible to isolate specific relevant differences between the independent field trials to date, which have been conducted in different contexts with different populations and different procedures. Nonetheless, interrogating potential moderators is key to assessing both the underlying theory of self-affirmation and its likely practical impact. To the extent that a priori hypotheses predict heterogeneity, these results would confirm theory and point to where these strategies have the most potential to improve student outcomes. On the other hand, it is possible that mixed self-affirmation results are not explained by currently theorized moderators, which would imply the need for greater and more specific inquiry into the necessary conditions for success.

A New Self-affirmation Replication Study

Given variable evidence of impacts in applied settings, we tested the effects of brief, in-class self-affirmation writing exercises for 7th grade students on subsequent academic outcomes in a new double-blind randomized experiment in a sample of over 1200 students in one Midwestern school district. We sought to learn whether similar benefits could be attained in a different setting, both in terms of geographic location and scale of implementation.

The Original Study

The original self-affirmation study in a middle school setting was first reported by Cohen et al. (2006), with supplemental analyses elsewhere (Cohen et al., 2009; Cook et al., 2012; Shnabel et al., 2013). We replicated the procedures in the original experiments as described

below. Cohen and his colleagues originally reported several substantively important features of self-affirmation intervention on student outcomes: substantial persistent benefits for "negatively stereotyped" students (African American and Hispanic students) on Grade Point Average; significantly higher benefits for low-performing African American students; an improved trend in grades throughout the year; and no benefits for European American students. Our primary focus was on the first finding, representing the highly policy-relevant main impact of the intervention on negatively stereotyped groups. The impact for African American students ranged from 0.21 to 0.34 GPA points across individual experiments and across courses (Cohen et al., 2006, p. 1308).

The Previous Independent Replication in the Current Research Setting

The immediate precedent for the current self-affirmation replication is the study reported by Borman et al. (2016). That study was the first successful independent replication of the benefits of self-affirmation benefits in middle schools. The researchers reported statistically significant benefits for "potentially threatened" students (Black and Hispanic) on 7th grade GPA across all schools in the district. Like the original study, term-specific GPA data revealed a less negative trend for potentially threatened students in the self-affirmation condition, and no benefits for "not potentially threatened" students (White and Asian). Some results deviated from the original patterns. For one, the impacts were smaller, with an impact of 0.065 cumulative GPA points; the confidence interval for this estimate was (0.001, 0.128), which excludes all impact estimates from the original study. The authors speculated that this difference may have been at least partially related to the challenges of implementing at scale. Also, the replication found no evidence of an interaction between the intervention and prior achievement. In supplemental analyses, researchers reported that the treatment benefits in this scale-up were

concentrated in a subset of schools hypothesized to have the most threatening environments for potentially threatened groups, based on the numerical presence and relative academic standing of these students (Hanselman et al., 2014).

The Current Study

The current study was designed to replicate both the original self-affirmation study (Cohen et al., 2006) and the previous successful independent replication (Borman et al., 2016). Three key features of this design provide unique insights into the effects of self-affirmation in educational settings. First, procedures followed those in the original study, including intervention materials, as we detail below. The study therefore is an example of a well-powered "close" replication of the effects of self-affirmation for potentially threatened groups in middle school (Brandt et al., 2014). Moreover, given the scale of the research, the study contributes important evidence about the general promise of these interventions to improve minority students' achievement.

The second key feature of the study is that it was conducted in the same setting as a previous randomized trial of self-affirmation, in the same district and schools, by the same research team, with the same research protocols. In the current study, we ask whether these middle school scale-up results were replicated, and we use comparisons across studies to test theorized sources of heterogeneity. Since features of the study corresponded closely to those in the previous one (Borman et al., 2016; see Table A2 for a summary), the *within-setting* comparisons across the two studies allow for much more specific tests of moderation than comparisons between settings. A recent precedent for such a within-setting comparison is provided by Harackiewicz et al. (in press), who found different affirmation effects in a college

setting and discussed several potential explanations for the difference. We exploit a similar pattern to conduct comprehensive tests of theorized sources of heterogeneity.

A third contribution of this study is that we collected information on self-affirmation implementation, including qualitative features of students' responses to the exercises. These data provide an unprecedented picture of the experience of the self-affirmation activities when they are implemented in an entire school district. And, in combination with information about individual student characteristics and features of the social context, this information supports unique tests of the theorized sources of heterogeneity.

Building on the unique empirical features of this research, we addressed three sequential research questions. Our first question was: (1) what was the effect of the self-affirmation intervention in the new large-scale implementation? Because we found no evidence of benefits, we asked: (2) were estimated effects substantively and significantly different from the impacts for the students from a previous study in the same setting? Given meaningful and detectable differences, we finally asked: (3) why was the same intervention seemingly beneficial for targeted students in one implementation but less so in the next?

The third research question is the most theoretically important, but it also is the most challenging. To preview our approach, we drew on the theory underlying the design of the interventions to conduct a series of tests of potential explanations for differences in effects across studies. Based on hypothesized moderators of the impacts of self-affirmation, these explanations fall into three broad classes: characteristics of implementation, individuals, and social context. We then conducted a series of empirical tests of these potential explanations to assess which, if any, explained the differences in experimental impact estimates.

Method

The Large-scale Self-Affirmation Studies

All data were generated or collected as part of two randomized trials of self-affirmation writing activities among 7th grade students. The research was conducted through a partnership with the school district, which recognized large racial achievement gaps and was interested in strategies to improve the performance of minority students. District administrators provided support to the project, and principals at all 11 regular middle schools agreed to participate. Given this support, study implementation involved researchers (who provided training and activity materials), school learning coordinators (who coordinated the site-specific logistics, including scheduling), and teachers (who implemented the activities in their classrooms). The involvement of educators in diverse roles approximated how the exercises would be likely to be implemented if adopted as a universal district initiative.

Throughout this paper we refer to the first study, conducted with 7th grade students in 2011-2012, as "cohort 1" and the second study, conducted in 2012-2013, as "cohort 2." The focus of this paper is on the new evidence on self-affirmation effects provided by cohort 2; no results from this study have been reported previously. In order to compare results across the two studies, we also conducted new analyses of participants in cohort 1, including documenting impacts in 8th grade. We therefore detail aspects of both the new study (cohort 2) and the previous one (cohort 1).

The general outline of both studies was similar, as follows: Research activities began in the summer with parallel contact at each of district's 11 middle schools. After confirming authorization from the principal and identifying an appropriate setting for the writing exercises with each school's learning coordinator, research staff provided a training session for the 7th

grade instructional teams at each school. During the 30-minute training session, a member of the research staff introduced the study as research about 7th grade students' experiences, beliefs, and social-emotional learning. The researcher described the mechanics of implementation and reviewed the teacher implementation script. Teachers administered the writing exercises during normal class time with materials provided by the research team and the completed exercises were returned to the research team for recording. After the school year, the district provided administrative data, including transcript and demographic information. No study activities were conducted after the 7th grade year, but additional administrative data on 8th grade performance were collected after the following year.

Below we highlight the core features of the intervention, with a focus on similarities and differences between the two studies. Appendix Table A2 provides a summary.

Self-affirmation Intervention and Implementation

The self-affirmation intervention procedure followed Cohen et al. (2006). Seventh grade students completed a short (15-20 minute) writing prompt as part of normal class activities several times during the school year. We identified four time points for the self-affirmation writing interventions. These provided a consistent template for the district, but scheduling varied according the formative assessment dates in individual schools. The time points were: (1) at the start of the school year, in the week prior to formative fall standardized assessments, (2) in November, in the week prior to the state's standardized achievement test for accountability purposes, (3) in the winter, in the week prior to a midyear language skills formative assessment, and (4) in the spring, in the week prior to the final formative assessment of the year. Based on the evidence that self-affirmation exercises are most effective earliest in the school year (Cook et

al., 2012), we provided school officials with the option of omitting the winter exercise to reduce logistical challenges; four schools did so for cohort 1 and two did so for cohort 2.

The activities were administered by teachers in the classroom using scripts provided by the original research team. 45 teachers were involved in cohort 1, 44 were involved in cohort 2, and 33 were consistent across both studies; teacher changes reflected exits from the school, re-assignments, and looping (teachers moving grades along with students). The intervention activities were completed in a classroom setting determined by the school's learning coordinator to be the most appropriate for the writing exercises: in Language Arts classes at seven schools and homeroom period at four (constant across both cohorts). Homeroom periods were abbreviated classes with non-academic curricula, including activities related to socio-emotional standards. In either case, exercises were implemented among all 7th graders in these regular classrooms by their classroom teachers.

The activities were packets of 3-4 pages with prompts and spaces for individual writing responses. They were identical on the cover sheet, which included the student's name. On subsequent pages the exercises varied by randomly assigned condition (for consented students; all non-consented students, including newly enrolled students without a personalized packet, completed the procedural/neutral control prompts). The treatment condition, following the original study, prompted students to reflect on values (such as friends, family, music, or sports) that were important to them. The precise format of the treatment exercise varied throughout the year to avoid repetition. There were two randomly assigned control conditions: one focused on values, in which students are asked to select least important values from the same list presented to treatment students and explain why they may be important to someone else, and a second devoted to various procedural writing prompts, such as describing summer activities or

explaining how to open a locker (we refer to these prompts as “neutral,” as they do not explicitly concern values). The latter control branch was introduced after the first administration in the cohort 1 study, so all control students in the first cohort received the “Least Important Values” prompt for the first exercise. Because we found no evidence of differences between control conditions in either cohort nor evidence that these differences explain differential impacts, we combined both control groups in our main analyses.

Individualized packets were prepared for every student in the district based on classroom rosters and distributed to teachers ahead of implementation. The priority in implementation procedures was to promote an environment in which students engaged in the genuine self-reflection about aspects of identity that is hypothesized to lead to self-affirmation benefits. One implication, following previous research, is that activities were to be conducted as a normal part of classroom activity; this point was stressed in the teacher training and implementation scripts. However, the fact that teachers implemented the activities independently in their own classrooms created challenges for documenting precise features of implementation, as we discuss below.

We also instructed teachers to avoid representing the activities as evaluative, to avoid reference to external research, and to avoid presenting the activities as beneficial. These guidelines were based on theory and empirical evidence (Cohen & Sherman, 2014; Silverman et al., 2013), with the caveat that there is little existing guidance about how these features translate into best practice for teachers in established educational settings. For instance, anecdotal feedback from teachers highlighted some tension between these theoretical ideals and integration into classroom activities. For many students and some teachers, the medium of the activities—a personalized packet completed individually—led to a default perception of the activities as a test or assessment. We made efforts to mitigate these perceptions. For instance, previous studies have

distributed activities in individual envelopes. In initial planning, we found this to be well outside the norm of classroom activities in the current setting, and instead used a collated packet of papers with a cover sheet to mask differences across conditions.

Some teachers also reported questions from students along the lines of: “if this isn’t graded, why do I have to do it?” One response was for teachers to justify the activities as part of a research study. Recognizing the potential for such deviations from instructions, researchers never described the project to teachers in terms of stereotypes, identity, or self-affirmation. Instead, researchers emphasized that the study concerned the thoughts and opinions of middle school students. Therefore, to the extent that teachers presented or justified the activities as part of a research project, they communicated that students’ responses were valued, which we expected would encourage expressive self-reflection.

Comparison to Original Study

In the context of replication, it is important to be clear about key similarities and differences in protocol, subjects, and context. This is particularly true for interventions in applied school settings, where procedures must be sensitive to local conditions and can shift over time due to logistical constraints or contextual appropriateness. Previous self-affirmation interventions highlight this point: Sherman et al. (2013) reported creating simplified versions in a setting with many English Language Learners, and even in the original setting, the experimental protocols (including the number of exercises, and instructions for choosing important values) shifted between years (Cohen et al., 2006).

The current study set out to replicate the original research (i.e., Cohen et al., 2006) as closely as possible at a larger scale in a new setting. Intervention materials—student exercises and teacher implementation instructions—were provided by the original research team. The

fielded activities correspond most closely to Experiment 2 reported by Cohen et al. (2006)—circling important values instead of marking most and least important—and the simplified version employed by Sherman et al. (2013). Timing followed the original experiments, prioritizing a first administration as early in the school year as possible and spacing additional implementations throughout potentially stressful periods later in the school year.

The original study included three to five 7th grade implementations, depending on experiment (Cohen et al., 2009); we fielded three or four (depending on school) in both cohorts. In contrast to the original studies, we did not field implementations in 8th grade; a maximum of four implementations was feasible in the current context, and we prioritized the earliest activities. The original study also administered a student survey at the beginning and end of the 7th grade academic year. The survey addressed students' "self-perceived ability to fit in and succeed in school" (Cohen et al., 2009, p. 401). We conducted a similar survey at the beginning and end of the 7th grade school year for cohort 2. In this respect, the cohort 2 study was more similar to the original research than cohort 1, when no surveys were administered.

The original study was conducted in a single school, described as "middle- to lower-middle-class families at a suburban northeastern middle school whose student body was divided almost evenly between African Americans and European Americans" (Cohen et al., 2006, p. 1307). The current context included students in 11 Midwestern middle schools in a single district. Overall student 7th grade enrollments in the district were 45% White, 25% Black, 19% Hispanic, and 10% Asian. Based on the original finding that results were consistent when non-Asian minority students were combined as "potentially stereotyped," we combined Black and Hispanic (including multiracial) students in preferred analyses. Across the 11 schools, the share of potentially threatened students ranged from 19% to 81%. As in the original study, the

intervention was provided to students independently by teachers in their classrooms, with materials provided by the research team. The original study was conducted with 3 teachers. The current study (cohort 2) was conducted with 44 teachers in 77 classrooms.

Our analyses include only administrative outcomes. It was not feasible to collect the more detailed outcome measures of the original study, including teacher gradebooks and a race activation task at the end of grade 8 (Experiment 2) or grade 7 (Experiment 1). However, we collected state standardized achievement test results, which were not considered in the original research.

Fidelity

Previous research provides little specific guidance on how to identify or measure the most relevant aspects of self-affirmation implementation, but the anecdotal challenges that teachers reported in implementing the activities in their classrooms highlight the need for more attention to these issues in applied settings. We considered several indicators of fidelity. One indicator is whether students responded to the writing prompts. By that standard, fidelity was quite high in both cohort 1 and cohort 2. In terms of basic exposure to the assigned materials, 88-95% of students completed the assigned activity for each administration. Student absences from class accounted for the majority of non-completion, while less than 1% of students in each administration completed a non-assigned packet due to administrative errors (such as a roster change).

We also coded the content of all students' responses, distinguishing between responses that showed clear evidence of self-affirming reflection and those that did not. Each response was coded independently by two trained coders who were blind to the experimental condition. A response was coded as self-affirming if it met three criteria: (1) the student wrote about

themselves, (2) the response identified a listed "value" from the experimental prompt, and (3) the text expressed either the importance of the value (for example: "My family is the most important thing to me because…") or that they are "good in" the valued domain (example: "I'm good at drawing."). Inter-rater agreement was above 80% in both cohorts, and discrepant cases were resolved with the guidance of a core research team member. Based on those measures, fidelity to treatment was high in both cohorts, with 98.0% of treatment students providing at least one response reflecting self-affirming reflection, and 95.8% doing so during the first two exercises of the year.

Although our study is unprecedented in the scale at which we have documented fidelity in self-affirmation writing exercises, we acknowledge that it is possible for more subtle aspects of implementation to have failed in ways that we could or did not observe. Teachers' independent actions in the classroom, as discussed above, provide one example. Educational research has highlighted the organizational mechanisms that buffer teachers' practice from external demands (Weick, 1976) and the role of individual teachers' sense-making in shaping how reforms are enacted in the classroom (Coburn, 2004). We therefore gathered additional evidence with a teacher survey conducted at the end of each school year. These responses should be interpreted with caution for several reasons: we obtained reports from the teachers of only 56.0% of students (46.1% for cohort 1 and 64.2% for cohort 2), the items were retrospective reports (6 months on average after the fact), and it is unknown whether these (or any) teacher behaviors are critical to self-affirmation success. Nevertheless, these data complement other implementation measures and provide a preliminary window into teachers' administration of the activities.

Teacher responses supported the anecdotal reports discussed above, suggesting that the presentation of the exercises was not always as directed. Teachers of 31.1% of students reported describing the writing exercises as being part of a research study, and teachers of 20.3% of students reported describing the activities as "good for" students. These deviations may have detracted from the effectiveness of the self-affirmation activities, but we do not know how they compare to previous studies, since prior research has not reported systematically on teacher administration.

Sample

Because the study was administered in regular classrooms, all students in these classrooms completed some form of individual activity during implementation. However, students were only participants in the study (i.e., they were randomized to experimental condition, had data collected, and were included in analyses) if they assented and their parents consented. All seventh grade students in all 11 regular middle schools in the Midwestern school district were recruited to participate at school registration days (attended by the vast majority of parents and students) at the end of summer and with follow-up at the start of the school year. In the cohort 1 study, we received consent and assent for 63.6% (1048/1648) of the population; for cohort 2 the number was 72.8% (1269/1722), reflecting improved recruiting efforts. Study participants were individually randomly assigned to the experimental group with randomization blocked by school.

Because attrition was low, even into 8th grade, we analyzed a consistent full cases sample. We dropped 9.0% of cases overall due to missing data/attrition: 2.6% of cases were missing data on covariates we included in models for precision, an additional 4.4% had no transcript data in 8th grade, and 2.1% more were missing standardized testing outcomes. The extent of attrition

overall and the individual sources of attrition were statistically equivalent across experimental condition (cohort 1: 10.6% treatment and 10.2% control, χ^2=0.03, df=1, p=0.86; cohort 2: 7.5% and 8.1% attrition, respectively, χ^2=0.14, df=1, p=0.71); overall attrition was higher for cohort 1 than cohort 2 (10.4% vs. 7.8%, χ^2=4.75, df=1, p=0.03). To the extent that differential attrition contributed to possible differences between cohorts, it would have operated (along with differences in recruiting) through different types of individuals being included in the two analytic samples, which we addressed explicitly (see "Individual Student Differences" Results section).

Measures

All student demographic information was derived from district administrative records. Our primary individual demographic variable was an indicator for students' potential susceptibility to social identity threats relating to academic performance in school, which we operationalized as African American or Hispanic racial/ethnic group membership. We treated multiracial students as potentially susceptible to racial identity threat because they are likely to identify with or be perceived as a member of a marginalized group, but results were similar when these students were excluded (see Figure 3, Panel C). To the extent that administrative racial/ethnic group membership misrepresents susceptibility to social identity threats, our impact estimates may have been attenuated, but similarly so for both cohorts.

To increase the precision of the self-affirmation treatment effect estimates, we included additional baseline student characteristics in our preferred specification for impact models. These included pre-treatment (grade 6) achievement outcomes and binary indicators for female, limited English proficiency status, receipt of special education services, and eligibility for free or reduced price lunch, which we included as a proxy for family economic resources. Results were substantively similar when we excluded these covariates (see Figure 3 and Appendix Figure A1).

In some models, we restricted the sample to schools with relatively low proportions of Black and Hispanic students and relatively large prior achievement gaps for those students, both of which serve as proxies for more potentially threatening school contexts. Following previous research, we created a binary indicator for potentially threatening school contexts, defined as schools with below average numbers of Black and Hispanic students and above average prior racial achievement gaps (Hanselman et al., 2014).

Our ultimate interest was students' academic performance. The primary outcomes, following previous research in the self-affirmation literature, were students' overall grade point average (GPA) in grade 7 and grade 8. GPA reflects overall academic performance across all academic subjects and was recorded on a 4-point scale. Results were robust to focusing on only core academic courses, which corresponded closely to overall GPA (correlations of 0.98-0.99 in each grade). We gave grade 8 GPA conceptual priority, as it was the only grade point average measured entirely subsequent to the full treatment regime.

In supplementary analyses, we assessed treatment effects on a standardized academic assessment, the Wisconsin Knowledge and Concepts Examination (WKCE) tests in mathematics and reading. During the study period, WKCE tests were administered for state accountability purposes in November of grade 7 and grade 8. Although the grade 7 tests were administered relatively early in the course of the intervention, the second exercise explicitly targeted the potentially high stress week prior to WKCE testing, making effects on this early outcome worthy of consideration.

Experimental Balance

Table 1 reports descriptive statistics and tests of baseline experimental equivalence for each cohort, both overall and within the subset of potentially threatened (Black and Hispanic)

students. The sample was majority White, but included a substantial number of potentially threatened students in each cohort (reported numbers include multi-racial students). Pre-treatment differences between the treatment and control group were substantively small (generally less than 0.1 standard deviations) and not statistically significantly different, suggesting that randomization was successful in yielding comparable groups.

Analyses

All analyses were based on intention-to-treat estimates of the effect of self-affirmation, which assess the impact of assignment to the treatment group and therefore reflect the policy-relevant impacts of providing the self-affirmation (Borman, 2002). We calculated effects overall and within theoretically relevant subgroups. Estimates were based on the following general multilevel model of treatment effects:

$$Y_{ij} = \beta_0 + \beta_1(Treatment_i) + \beta \boldsymbol{X_i} + \eta_j + \varepsilon_i \qquad (1)$$

In this model, Y_{ij} is the observed outcome for student *i* in school *j*, $Treatment_i$ is the randomly assigned self-affirmation treatment status for student *i*, $\boldsymbol{X_i}$ is a vector of pre-treatment covariates (grade 6 outcome, gender, limited English proficiency, special education, and free lunch eligibility), η_j is the residual component for school *j*, and ε_i is the residual for student *i*. Because the treatment was randomly assigned to each student, β_1 provides an unbiased estimate of the effect of the self-affirmation intervention without additional controls, but we included a pretreatment achievement measure and additional covariates, $\boldsymbol{X_i}$, to increase the precision of this estimate.[2]

[2] Some previous research has highlighted self-affirmation effects on achievement trajectories. These trends are especially helpful in characterizing the decline of minority students' achievement relative to majority students. We focus only on impacts on outcomes at single points in time here for two reasons: (a) our substantive interest is (variability in) the ultimate benefits of the intervention among potentially threatened students, which is best captured by overall impacts, and (b) given baseline equivalence, impacts on overall outcomes are analogous to impacts on

Within this basic framework, we conducted specific analyses to explore potential differences between the two studies, including alternate outcomes and estimates for theoretically relevant sub-groups. Many of our analyses tested for differences in effects between cohort 1 and cohort 2 by estimating cohort-by-treatment interactions in pooled models with all observations, and we also estimated overall effects with the pooled data. We provide additional details for specific analyses as we present the results below.

Results

Estimated Impacts of Self-Affirmation

The raw pattern of results for the new study of self-affirmation (cohort 2) for the focal outcome (Grade Point Average) is presented in the right panel of Figure 2. As expected, there were no effects of the intervention on the performance of Asian and White students, who are not hypothesized to be subject to the same types of identity threats in school as are the other groups. Potentially threatened groups (Black and Hispanic) performed worse overall, but the differences between treatment and control groups were similarly small in both 7th and 8th grade. To estimate treatment effects as precisely as possible for this targeted group, we used multilevel models of the self-affirmation intervention, controlling for pre-treatment student characteristics. Estimates for all outcomes were negative, but none were statistically different from zero (Table 2). The GPA effect in grade 7 was approximately zero (d=-0.002), while the effect in grade 8 was nominally negative (d=-0.072). Because the sample was quite large, these null results rule out (at the 0.05 significance level) impacts of 0.10 standard deviations or greater on GPA in grades 7

(linear) trends. Estimates from longitudinal growth models were substantively similar to those presented here but less precise.

and 8.[3] Results for standardized achievement outcomes were similar. Concerning our first research question, therefore, we found no evidence of treatment benefits for the targeted population in the new study.

Although not our primary focus, we also tested three additional findings reported by Cohen et al. (2006). First, we found no evidence of greater benefits of the intervention for potentially threatened students; the estimated interaction pointed in the opposite direction in our preferred specification but was not significantly different from zero (p=0.15). Second, we found no evidence of differential effectiveness by prior academic performance. Following the procedures described by Cohen et al. (2006), we created tercile groups based on 6th grade GPA, within the potentially threatened and potentially non-threatened groups. We failed to reject the null hypothesis that treatment impacts were equivalent across all three groups (p=0.20). We also found no evidence of differential impacts by prior achievement among White and Asian students (p=0.73). Finally, we tested for evidence of an improved trajectory of performance throughout the year. Considering students' grades in each of the four terms of the school year, we tested for an interaction between treatment and term. GPA declined by 0.05 GPA points per term on average among Black and Hispanic students, but there was no difference by experimental condition (p=0.77).

Comparing Self-affirmation Effects across Studies

The results above led us to ask whether the null effects in the current study (cohort 2) differed from those in the previous research in the same setting (cohort 1). A first question was whether the benefits observed previously (Borman et al., 2016) were detectable in the year

[3] The 95 percent confidence interval for the self-affirmation effect on overall grade point average in grade 7 was (-0.047, 0.165) for cohort 1 and (-0.088, 0.083) for cohort 2. The intervals for grade 8 were (0.015, 0.282) and (-0.192, 0.047).

following the intervention. We analyzed data from the subset of students from the prior study with valid observations in grade 8, using parallel procedures to those above (estimates summarized in Table 2).[4] We found that self-affirmation group students received significantly higher grades in 8th grade (d=0.152), bolstering the interpretation that the intervention led to detectable increases in academic performance for African American and Hispanic students. However, when we combined cases across studies, we did not find a statistically significant average self-affirmation treatment effect (grade 7: $p = 0.54$, grade 8: $p = 0.58$).

To address our second research question, we estimated the difference between self-affirmation impacts for cohort 1 and cohort 2 by pooling data from both samples and including cohort interactions with all covariates. We found that in several cases the null effects for cohort 2 were distinguishable from comparable effects for cohort 1. For the primary outcome, 8th grade GPA, the standardized cohort 2 estimate was small and negative (d=-0.072), while the cohort 1 estimate was positive (d=0.152), and we could reject the null hypothesis that effects were equal ($p = 0.013$).[5] We also found statistical evidence of differences between the treatment effects across cohorts for the two supplementary mathematics state test score outcomes ($p = 0.037$ in Grade 7, $p = 0.023$ in Grade 8), although only the grade 8 mathematics cohort effect difference would be statistically significant if the Bonferroni correction for multiple comparisons was applied to both estimates in this mathematics domain.

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[4] These analyses differed from previous reported by considering only students with grade 8 information for all outcomes. The main implication was that the reanalyzed results were less precise, and therefore provided more conservative tests of statistical significance. The pattern of results across grade 7 matched those reported by Borman et al. (2015)—positive benefits for GPA and mathematics achievement and smaller negative impacts on reading—although none of these were statistically significant in the reduced sample (see Table 2).

[5] Appendix Table A3 presents all estimates from pooled models of treatment effects in both cohorts. These models suggest general similarity between cohorts in the associations between covariates and outcomes (fewer significant interactions than would be expected by chance). There is also suggestive evidence that the control group was higher achieving in cohort 2 in GPA and mathematics, conditional on grade 6 scores, but none of these differences are significant at the 0.05 level.

These results were robust across different specifications of the treatment effects model. In addition to our preferred specification, which included the full set of individual control variables, we also estimated impacts in models with no covariates and with controls only for the pre-treatment outcome measure. Figure 3 summarizes results of these three specifications (represented by symbol shapes) for the focal group and comparison (Black/Hispanic students, combined control; Panel B1), as well as for alternate comparisons testing theorized moderators (discussed in the corresponding sections below). Appendix Figure A1 presents comparable results for grade 7 overall grade point average. In all cases, results were substantively robust across all covariate specifications, although predictably less precise for the models omitting the alternate control cases.

To summarize results to this point, the two studies provided diverging pictures of the impacts of the self-affirmation intervention on Black and Hispanic students' academic outcomes. For cohort 1, benefits in GPA persisted in the academic year following the intervention. For cohort 2, however, we found no evidence of benefits of the intervention. Moreover, we rejected the null hypothesis that impacts were equal in both studies, despite being conducted in the same research setting. These results motivated our final research question: do the currently theorized moderators of self-affirmation explain the differences in treatment effects across the two cohorts? In the remaining sections, we focus on the primary outcome measure, grade 8 GPA, and assess potential explanations for the decline in treatment effects from cohort 1 to cohort 2.

Differences in the Delivery of Self-affirmation: Intervention Design

Research projects, like educational practice, evolve over time for pragmatic reasons. For instance, in previous self-affirmation studies, investigators adjusted the frequency and content of intervention exercises as they were implemented across successive cohorts and in new settings

(Cohen et al., 2009; Sherman et al., 2013). In the current study, two design changes between the first and second cohort created differences in the delivery of the self-affirmation activities that potentially explain differential impacts: a shift in comparison group activities for one of the four exercises and a pre-intervention survey, which was added in the second study.

First, a randomly selected half of the control group was assigned a different first exercise in the cohort 2 study, compared to cohort 1. All control students were assigned the original control activity in cohort 1, which directed students to select values that were unimportant to them and write about why these values may be important to someone else. Half of the control group did the same in cohort 2, but half was randomly assigned to an alternate control activity for exercise 1 that asked students to write about what they did over the summer. Alternate control conditions were added in response to reported struggles of some students with the original "least important values" control activity. The alternate control writing prompt was modeled after typical classroom free-writing prompts, and was administered to non-consented students in both years. This prompt is "neutral" in the sense that it does not explicitly refer to values, but students could, potentially, write self-affirming responses (see "Student Experiences" section below). A random half of the control group in both cohorts completed a comparable alternate activity for exercise 2, which asked students to describe how to complete a procedural task, such as how to open a locker.

To assess whether this modification in the control regime contributed to different intervention impacts, we focused on the randomly selected half of the control group in both cohorts that received exactly the same sequence of exercises, which directly followed the original design (Cohen et al., 2006). These estimates are presented in Figure 3 in subpanel 2 for each sample (labeled "Original Control"). The cohort-by-treatment interaction estimates were

substantively unchanged in these analyses, though less precise owing to the smaller sample size, implying that the slight procedural change does not explain the drop-off in impact in the second cohort. Since we found no evidence of differences between the two control groups, we pooled both groups for all reported analyses, unless noted otherwise.

A second design change for the second cohort was the administration of a 15-20 minute survey by researchers in classrooms in the first week of school. Interaction with research team members was similar for both studies because, for cohort 1, researchers visited classrooms during this time to collect student assent forms. In both assent (cohort 1) and survey (cohort 2), researchers did not connect these overt research activities with the writing exercises, the first of which was administered on average one week later. Students were told in both cases that the study was interested in their thoughts and opinions as middle school students. The survey included items about individual characteristics (e.g., locus of control, self-complexity, and social belonging) but omitted any specific reference to racial identity, stereotypes, or self-affirmation, which might have primed students to experience identity threats.

It is theoretically possible that survey prompts about social-psychological constructs like social belonging could change how students respond to the self-affirmation exercises. Although we could not directly assess whether the inclusion of the survey accounted for lower benefits for cohort 2, this explanation is unlikely for two reasons. First, to explain the decline in our setting, prior surveys would needed to have muted the treatment contrast (such as by inoculating treatment students from self-affirmation benefits), but the original large and persisting impacts were found in the presence of a pre-survey (Cohen et al., 2009). Based on this result, we might have expected the largest benefits for cohort 2. Second, the prior surveys were distinct from the self-affirmation exercises, fielded on a different day by the researchers, instead of teachers, and

not explicitly linked to the exercises. Therefore social psychological responses activated by the survey would have to persist over time and remain relevant for a separate task. While future research is necessary to test whether such prior prompts modify self-affirmation benefits, we note that if such brief, distinct stimuli moderate self-affirmation impacts, then there are many other school experiences that are also likely to matter. If true, the effects of the self-affirmation intervention would be extremely difficult to predict a priori.

Differences in the Delivery of Self-affirmation: Student Experiences

One potential explanation for heterogeneity in treatment effects between the two studies is a decline in the quality of students' experience of the activities related to implementation. Although formal and informal procedures were consistent, the hypothesized psychological processes may be sensitive to subtle changes in delivery (Yeager & Walton, 2011), and it is possible that small changes in classroom procedures had large consequences for effectiveness. For instance, if teachers presented the materials differently in the second cohort, then fewer students may have engaged in genuine self-reflection. As discussed in the "Fidelity" section, no direct observations of classroom implementation were collected (the activities were intended to be part of regular classroom activities and not to be associated with research). Instead we conducted three indirect tests of implementation differences as explanations for differential benefits between cohorts: changes in theorized features of implementation, changes in implementing teachers, and changes in students' written responses to the intervention.

First, we noted three theoretically important features of the self-affirmation writing intervention design: that activities are administered during targeted times of potential stress, especially early in the school year (Cook et al., 2012; Critcher et al., 2010), that activities are not explicitly presented as externally imposed (Silverman et al., 2013), and that activities are not

presented as being beneficial to students (Sherman et al., 2009). We documented that that these features of implementation did not vary (or improved) between cohorts. With respect to timing, 91% of classrooms for cohort 1 administered exercise 1 prior to the targeted first formative standardized assessment of the year, and 81% administered exercise 2 prior to the state standardized testing. The comparable numbers in cohort 2 were 91% and 97%, respectively. Based on retrospective self-reports from teachers provided at the end of the school year, we also found more faithful implementation for the second cohort. In cohort 1, 31.1% of students were taught by a teacher who reported describing the activities as "good for" them, while 42.2% were taught by a teacher who reported explaining the activities as connected to a research study. Both figures improved for cohort 2: 13.9% for "good for" instructions and 24.6% for mention of a research study. With the caveats outlined in the "Fidelity" section, these reports show no indication of poorer implementation in cohort 2. In other words, while imperfect delivery of the exercises may explain some of the attenuation of self-affirmation effects, these features did not explain the difference in effects between the two studies here.

Second, we considered whether changes in implementing teachers accounted for the decline in benefits. Due to staffing changes, 77% of the Black and Hispanic students in cohort 1 and 60% in cohort 2 completed the exercises with a teacher who implemented in both studies. If teacher fatigue with the study adversely affected implementation, then impact declines should have been largest among the "both-cohort" teachers. Conversely, if unique cohort 1 teachers were especially effective, the declines should have been be largest among "single-cohort" teachers. We found no evidence for either hypothesis (see Appendix Table A4). Treatment by cohort interactions were substantively equivalent in both sub-populations (-0.196 grade points

for the both-cohort teachers; -0.188 for the single-cohort teachers) and these interactions were statistically indistinguishable from one another ($p = 0.99$).

Finally, we tested whether students' written responses differed across the two cohorts of the study. While features of the written responses are imperfect proxies for the desired self-reflection, they provide an indication of whether the quantity or quality differed across cohorts. The two most basic measures of overall engagement were comparable in both studies: exercise completion and words written. A high proportion of students completed the activities, ranging from 85-95% (Table A5, Column 1). Completion did not differ by experimental condition or cohort. In supplementary analyses, we found that completers tended to have higher prior GPA than non-completers—no other baseline covariate predicted completion—but this difference was not distinguishable between cohorts.

The relative length of students' responses was consistent across cohorts too, after accounting for variation due to differences in prompts over time (Columns 2 and 3). The only treatment-control difference between cohorts was in mean words written for exercise 1 (Panel A), and this was completely explained by the randomly assigned "neutral" comparison group; students were more prolific when writing about their summer (in cohort 2) than about an unimportant value. Comparing students with the same, "original" prompts (Column 3), there were no cohort differences. By these measures, basic engagement with the activities was consistent across the two cohorts.

Analyses of the qualitative measure of students' responses to the exercises (introduced in the "Fidelity" section above) implied that treatment caused students to engage in much higher

rates of affirmation across all exercises in both studies.[6] The estimates are based on linear probability models, so the coefficient of 0.709 (Table A5, Panel B, Column 4) implies that the chance of affirmation writing was 71 percentage points higher in the treatment group in cohort 1 for exercise 2. The interaction coefficient (0.0796) implies that this treatment effect was actually higher in the second cohort, at a significance level of $p < 0.1$. Exercise 1 was again an exception, but the difference was solely explained by the modifications to the control group (see Column 5). Not surprisingly, the control group in cohort 2, including students who wrote about their summer, was more likely to write affirming statements, which others have noted is a risk in choosing that type of comparison activity (Cohen, Aronson, & Steele, 2000). Even so, treatment impacts on self-affirming writing were greater than 40 percentage points (0.427=0.721-0.294) in the second cohort overall.

On balance, analyses of implementation features, consistent teachers, and direct measures of intervention responses did not support the hypothesis that declines in implementation quality could explain lower benefits for cohort 2. In particular, responses to the exercises were strong overall, and comparable between cohorts. These results cannot rule out the possibility of differential psychological responses to the exercises in the two implementations, which deserves attention in future research. However, for this possibility to be true, the association between key psychological responses and the desired features of students' written responses must have changed between cohorts. The more parsimonious explanation is that declines in implementation did not account for lower effectiveness.

[6] Treatment effects are muted in exercise 3 for both cohorts because overall impacts include several schools that opted out of this exercise, and therefore students had no opportunity to engage in affirmation.

Individual Student Differences

The success of social-psychological interventions depends fundamentally on individual characteristics. Self-affirmation is only hypothesized to help students who are subject to identity threat, and students may also differ in how they respond to the specific reflective writing activity. Meaningful individual differences between cohorts could have resulted from sampling variability and/or because the second cohort study sample was larger, including 36% more potentially threatened students (449 vs. 331 in cohort 1), and different in terms of mean individual characteristics (see Table 1), due to more successful recruitment. We used three strategies to test for individual-level explanations of cohort differences: effects in theoretically sensitive subgroups, observable differences between the two cohorts, and the plausible influence of unobserved heterogeneity.

One implication of theorized moderation of self-affirmation benefits by individual characteristics is that results should be consistently stronger, and therefore less variable across cohorts, in subpopulations where academic stereotype threats are hypothesized to be most salient. We tested effects in two such subpopulations: students identified as only Black or Hispanic (excluding multiracial students), who may identify more strongly with a stereotyped identity, and Black/Hispanic Males, who may be subject to the most acute general academic stereotypes in middle school (Purdie-Vaughns & Eibach, 2008). Results are summarized in Panels C and D of Figure 3. Contrary to the individual difference hypotheses, differential effects across cohorts were similar in both of these subpopulations, even though lower precision in the male subgroup led similar size differences to be statistically insignificant.

We also tested all observed individual student characteristics as explanations of cohort differences. For individual characteristics to explain the decline in treatment effects, differences

between the two samples must have been related to treatment effect heterogeneity. We did find some descriptive differences between studies (see Table 1): the sample for cohort 2 had more female students (52.6% vs. 49.8%; $p = 0.03$), lower 6th grade GPAs on average (2.78 vs. 2.85; $p = 0.11$), and more students eligible for free or reduced price lunch (85.1% vs. 80.1%; $p = 0.07$). However, we found no statistically significant interaction between treatment and individual characteristics (grade 6 grade point average, gender, English proficiency, or Special Education designation) in either cohort, suggesting little opportunity for individual observed characteristics to explain different treatment effects. Not surprisingly, when we re-weighted individual cases in each cohort to balance populations in terms of each of these observable characteristics (for instance, giving greater weight to poor students in cohort 1, who were relatively under-represented in that sample), the effect estimates in each cohort were substantively unchanged (see Table A7).

More generally, we gauged how large total (including unobservable) sub-population differences would need to be to explain the different estimates between the two cohorts, assuming that individual-level treatment effects were constant over time. We considered a thought experiment in which the population was composed of two types of students: strong self-affirmation responders that benefit most from the intervention (type A), and weak self-affirmation responders that benefit least (type B). Assuming the boundary case that the cohort 1 Black/Hispanic sample was populated solely by strong responders, then an estimate of the average impact for this type of student (d_A) on grade 8 GPA is 0.152. Assume the cohort 2 sample was comprised of a mixture of students of type A and B, with the effects for type B students (d_B) unknown. The total impact in cohort 2 would then be an average of the two type-specific effects, weighted by the share of teach type (p_A and p_B, respectively):

$$d_{cohort\ 2} = p_A(d_A) + p_B(d_B)$$

Based on the total effect estimate in cohort 2 (-0.072) and the fact that the proportions of type A and type B students sum to 1, this implies:

$$d_{cohort\ 2} = -0.072 = (1 - p_B)(0.152) + p_B(d_B)$$

Rearranging algebraically:

$$d_B = \frac{.224}{p_B} + (.152)$$

The implication of this inverse relationship between the share and effect size for weak-responders is that cohort 2 null effects could only be explained by very large shares of weak-responders or by substantially negative effects for these students. For instance, if only the surplus students in cohort 2 (25%) were weak responders, then the effect of the intervention among this population of students must have been -0.74 (=-.224/.25 + .152) to explain the total cohort 2 impact; if half of the cohort 2 population was the second type of student, then effects for this group would need to be -0.30 (=-.224/.5 + .152).[7] Since such drastic changes in the underlying population and such large negative effects of the intervention are not plausible, it is unlikely that differences in the underlying student populations explain cohort differences.

Changes in Social Context

Social-psychological interventions are also theoretically sensitive to features of the social environment in which they are implemented (Yeager & Walton, 2011). Since the studies for both cohorts were conducted in the same classrooms, schools, and district, we expected there to be relatively small differences in the relevant social conditions that students experienced across

[7] Similar calculations using the upper bound of the 95% confidence interval for the treatment effect in cohort 2 results in necessary effects for the new student population of -0.29 as a 25% share of cohort 2 and -0.07 as a 50% share.

cohorts. This intuition was not directly testable, as there are no definitive measures of the relevant contextual features, but we assessed several indirect indicators of contexts that may be meaningful. We considered the demographic characteristics of the school population, differences in aggregate achievement, and school-specific impact estimates.

Previous research using data from the cohort 1 study suggested that school contexts moderated the self-affirmation treatment effect on 7th grade outcomes, with the greatest benefits in schools with low minority populations and large prior achievement gaps (Hanselman et al., 2014). In new analyses (summarized in the Figure 3, Panel E), we found that larger than average treatment benefits in these schools in cohort 1 persisted into 8th grade; however, self-affirmation benefits were no more consistent across cohorts in the population of "High Threat" schools, suggesting that context moderation does not explain the overall decline.

In addition, we considered whether shifts in demographic context of all students in the school (conceptually and empirically distinct from individual characteristics of the study samples discussed above) plausibly explained the difference in effects between cohorts. We found no evidence of this possibility, primarily because student characteristics did not change substantially between studies. One proxy for broad context differences related to academics and racial/ethnic identity is sub-group academic achievement and achievement gaps, which were similar for both cohorts and consistent with historic patterns (Figure A2). At the school level, racial/ethnic cohort composition was similar in both cohorts, while achievement gaps, which are one proxy for a racialized academic school environment, were consistently large (Figure A3). Moreover, controlling for either school-level racial/ethnic composition or prior achievement gaps did not alter the core treatment-by-cohort interaction estimate, suggesting that these documented school characteristics did not account for the decline in treatment effects in the second cohort.

Finally, we estimated school-specific impacts for Black and Hispanic students using data from both cohorts to assess whether patterns were consistent across these local contexts. Effects in most schools were similar or slightly lower for the second cohort (Figure A4), suggesting general consistency in lower impacts in cohort 2. However, dramatic changes from positive estimates for cohort 1 to negative estimates for cohort 2 were apparent in two schools (labeled points 5 and 11 in Figure A4). These differences may have been due to either drastic consequential changes in the local context or sampling variation. The latter is a more parsimonious explanation in light of the consistent demographic context discussed above, post hoc qualitative checks (which revealed no substantial year-to-year differences at these schools), and the implausibly large magnitude of the point estimate of the interaction for these schools (0.4-0.5 standard deviations).

To assess whether individual schools drove the overall results, we re-estimated pooled treatment effect models omitting each of the 55 unique pairs of schools in the study (see Figure A5). The main results—small positive effects for cohort 1, slightly negative effects for cohort 2, and therefore a consequential interaction—held in all omitted samples. One school (11) stood out as an extreme case: omitting this school reduced the interaction effect by 20-40% (depending on which additional school was also omitted), while the range for all other omitted pairs estimates was within 15% of the overall estimate. Subsamples that excluded school 11 exhibited greater similarity in estimates across cohorts (smaller interactions) due mostly to smaller estimated benefits for cohort 1, but also due to somewhat smaller estimated negative effects for cohort 2. On the whole, while a single school contributed the most to the decline in effectiveness between cohorts, the differences were meaningfully large without it.

Classroom and district context features may also have contributed to the difference in treatment effects across cohorts. However, we did not have strong a priori predictions about the importance of features at either level. To the extent that individual teachers shape the relevant features of the classroom environment, the similarity in effects for consistent and inconsistent teacher populations (reported above) suggests a small role for these factors. At the district level, even substantial system-wide events are especially difficult to connect theoretically to differences in the treatment effect. For instance, there was notable political and civic unrest during the study surrounding legislation limiting public sector unions, rhetoric surrounding teachers' work, and school closures due to teacher protests. Schools in the district were closed for four days in February during the cohort 1 study, and the associated gubernatorial recall election occurred in June between the two self-affirmation studies. We do not have strong theoretical predictions about whether these events translated to differences in school environments that moderated self-affirmation effects, but it seems unlikely that the unrest and missed days of regular schooling were critical to intervention success in cohort 1. More generally, this example highlights that if self-affirmation effects are sensitive to context changes such as public debate about education then they are fundamentally fragile in the sense that relevant critical conditions are difficult to diagnose, and more importantly, to anticipate.

Discussion

The replication results reported in this paper provide new evidence concerning two fundamental questions about the potential of self-affirmation interventions to improve academic performance and close achievement gaps (Cohen et al., 2006; Yeager & Walton, 2011): 1) Are there benefits of self-affirmation interventions for academic performance in middle school? and 2) Can we identify the necessary and sufficient preconditions for self-affirmation success? The

large-scale replication results reported here, coupled with extensive post hoc tests of heterogeneous effects, provide disconfirming evidence on both counts: we found no effects of the intervention for cohort 2, and we found no evidence that moderators from existing theory explained why this result differed from those in a previous study in the same setting. These results rule out important hypotheses about self-affirmation effects, both in terms of the magnitude of benefits and the sufficiency of theorized moderators, which refines our understanding of both fundamental questions. In closing, we elaborate these specific contributions, highlighting the unique evidence provided by this multi-cohort large-scale replication and implications for future research.

Are there benefits of self-affirmation interventions at scale for academic performance in middle school?

An important contribution of this paper is that it reports on a new large-scale replication of the promising self-affirmation writing interventions introduced by Cohen et al. (2006). Comprehensive null results from this experiment provide no evidence of self-affirmation benefits, and the precision of the impact estimates rules out benefits that are as large as one third the size of those reported by Cohen et al. (2009). Like the recent replication by Dee (2015), our results suggest that self-affirmation has at best modest benefits for minority students when implemented at a large scale. Unlike that study, however, the current failure to replicate cannot be plausibly attributed to idiosyncratic features of the research site or procedures, because a similar prior replication in the same setting did find benefits (Borman et al., 2016). In this paper, we reported persistent intervention benefits for the prior cohort and documented similarity in implementation measures across cohorts, including features of students' written responses.

It is important to point out that low statistical power is only a likely explanation for the null results in cohort 2 if the true effect of the intervention was smaller than estimated for cohort 1 and much smaller than in initial studies (Cohen et al., 2009; Sherman et al., 2013). Using the post hoc power calculations suggested by Gelman and Carlin (2014), we investigated the power of our cohort 2 study design for a range of true effect sizes (Figure 4). If the true benefit of self-affirmation on grade 8 GPA was 0.30, similar to the initial study, then our power was above 0.99. If the true effect was 0.15, as estimated for cohort 1, then power was 0.68. However, if the true effect size was 0.07, the average across the studies summarized in Figure 1, then this study had only a 21% chance of detecting an effect and a type II inferential error was to be expected.

These power calculations highlight a more general possibility: the true impacts of these brief self-affirmation interventions may be positive but relatively small when implemented at scale and across heterogeneous contexts. As Bryk, Gomez, and Grunow (2011, p. 130) observe, "the history of educational innovation is replete with stories that show how innovations work in the hands of a few, but lose effectiveness in the hands of the many" (see also: Schneider & McDonald, 2006). This could be true for self-affirmation due to implementation challenges or differential effects across contexts. If so, then even very large field trials, such as the one conducted by Dee (2015) and the current study, are underpowered and unlikely to detect effects reliably. An important corollary implication, if the true effect size is small, is that significant estimates in individual trials are expected to overstate the magnitude of the effect by a substantial amount (Gelman & Carlin, 2014). If the true effect size is 0.07, then statistically significant results from the current design would over-state this effect by a factor of 2.2 in expectation.[8]

[8] Note that if the same scenario (true effect of 0.07) were true for the previous study (cohort 1), then our results (estimated significant effect of 0.15) would make the correct inference about the existence of a positive effect

The plausible magnitude of self-affirmation effects is a crucial consideration for future work in this field, including implications for study design. If the true self-affirmation effect size for Black and Hispanic students when implemented on a large scale is 0.07, then we are aware of no studies with adequate power to reliably detect the effect, and statistically significant published results are likely to overstate the true impacts. The practical importance of such a small effect may be debatable, but from a policy perspective even a small benefit at scale could justify the negligible cost of this intervention. For instance, the benefits of the Tennessee STAR class size reduction experiment have been estimated to be 0.07 standard deviations in student reading achievement per $1,000 in per-pupil expenditure (Borman & Hewes, 2002, p. 258). A comparable benefit for brief self-affirmation activities, which are orders of magnitude less costly, would be very valuable for educators and policymakers. Therefore, more precise evidence about even potentially small effects of self-affirmation are needed. However, we recognize that more effective implementation of self-affirmation activities may be more expensive, especially if it requires dynamic guidance from a dedicated "psychological engineer" (Yeager & Walton, 2011). If this approach proved successful, then policy implications would then depend on the trade-off between greater benefits and costs.

Can we identify the necessary and sufficient preconditions for self-affirmation success?

A second key contribution of this paper is our detailed analysis of the differential effects of self-affirmation in two large-scale studies conducted in the same research setting. The results are puzzling in their lack of definitive explanation for differences, but they are informative because they demonstrate variation that cannot be explained by the moderators of self-

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but overstate the magnitude of this effect by approximately the amount expected by a significant effect for this study design.

affirmation benefits that have been proposed in the literature (see summary in Table 3). Our general conclusion is that the current hypotheses about variation in self-affirmation effects are insufficient to explain the potentially subtle moderators of impacts. We highlight three specific and related implications of the results.

First, our analyses demonstrate the value of tests of moderators to assess theory about where, and ultimately how, specific interventions are successful. The tests conducted here provide strong, if indirect, evidence about hypothesized differences due to implementation, individual, and context characteristics. Our assessment of individual differences is notable in this regard. Even though we did not directly measure all potential individual difference moderators, we calculated that the offsetting negative impacts of self-affirmation required for an individual difference moderator to explain the cohort differences were too large to be plausible. As a result, theorized differences in individuals across the two cohorts are unlikely to explain the heterogeneous results. In addition, our tests of moderators draw on the analytic leverage provided by a within-research site comparison across multiple cohorts and on the collection of relatively detailed implementation data, including students' written responses. This demonstrates the value of replication over time within a consistent research setting.

At the same time, unexplained variability highlights the need for additional inquiry into the implementation of these activities in diverse educational settings. Our attention to teachers' delivery of the activities and students' responses in large-scale implementations provides a first step in measuring variation in the implementation of self-affirmation exercises, but more work is needed to identify the necessary components for success. One insight from the scale-up effort reported here is the potential tension between fidelity to the scripted intervention and adaptation to local classrooms. At scale, teachers are unlikely to have close, long-standing relationships

with researchers, and they are likely to respond to this tension in different ways. Some responses may have undercut the potency of the intervention, even though they did not preclude benefits in cohort 1 and they did not seem to explain the different results in cohort 2. One future direction could be to remove teachers from delivery through computerized implementation. However, the protocol might alternatively be modified to include teachers more fully. Our anecdotal interactions suggest that teachers would implement much more organically if they were allowed to read students' responses. Future research could explore implications for implementation and effectiveness.

Second, our results point to the need to develop the theory and evidence about how and where self-affirmation works. Because we tested a comprehensive list of proposed moderators of self-affirmation and failed to explain the variation in our findings between cohorts, we conclude that the current cadre of moderators offered by the literature is insufficient. Future experimental studies are needed to robustly assess the existing theorized moderators, and it may be that current theory needs to expand to incorporate new potential explanations for self-affirmation effects.

Our results call more attention to the overall lack of empirical evidence about moderators of self-affirmation effects, which makes it difficult to judge whether theory testing or expansion is the more crucial next step for the field. For example, there is little relevant data and few studies assessing whether awareness about the benefits of self-affirmation, one of the best substantiated potential influences, moderates the effectiveness of the intervention. Sherman et al. (2009) is frequently cited for this point, but this paper only shows a correlational relationship between awareness and affirmation effects on task performance. More research is needed to isolate to what extent this and other theorized components contribute to effectiveness.

Moreover, the unique challenges that arise at scale highlight the need for future research to consider the necessary and sufficient conditions of self-affirmation in applied settings. Our results point to two important avenues in future research: measures of features of implementation and variations in protocol. First, future research needs to develop systematic measures of implementation. This may include videos or observations of classrooms or, alternatively, getting more detailed information from classroom teachers soon after implementation in the form of interviews or surveys. Similarly, administrative data offer imperfect proxies for the social context in which self-affirmation takes place. School climate instruments, including measures of overt and subtle forms of bias and discrimination, should be tested as more direct indices of context. A stronger measurement component would allow researchers to assess how potentially relevant environmental changes, such as the political unrest that occurred during the research reported here, did or did not translate into differences in schools.

Another suggestion for future self-affirmation research in applied settings is to experiment with features of the delivery of the intervention. For instance, researchers might contrast computerized delivery (Paunesku et al., 2015), which may help standardize the delivery of the intervention, to delivery by classroom teachers who, alternatively, may play important roles if their students believe that the values-affirming exercises are coming from them. If teacher-based delivery is employed, our experiences suggest that teacher protocols are an important area to focus on, since even with a script individual teachers may implement materials differently. By systematically varying these protocols, future research should consider how different instructions affect the activities being presented as beneficial, and whether this explains differential benefits.

Third, our unexplained heterogeneity results imply practical limitations of self-affirmation as a tool to improve student performance and close achievement gaps. The proposed efficacy of brief social-psychological interventions to improve educational performance is specific, requiring tailoring the right kind of program to the right kind of students in the right kind of social environment (Walton, 2014; Yeager & Walton, 2011). If variability in impacts cannot be predicted with the information available to educators, then the practical value of these interventions is unclear. That said, short self-affirmation writing exercises in the classroom remain a virtually costless approach to potentially addressing some of the racial disparities in school. Students often participate in broadly similar writing activities in the classroom during the school day, and targeted self-affirmation activities are unlikely to negatively impact students. The impacts may well be positive, but they are likely small, and our results suggest that challenges remain in predicting where exactly, and therefore how widely, the potential benefits of self-affirmation writing activities will extend.

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Table 1. Descriptive Statistics and Experimental Balance by Study, Overall and for Potentially Threatened Students (Black/Hispanic)

	Cohort 1					Cohort 2				
Sample Variable	Mean	C Mean	T Mean	Std Diff. (C-T)	p	Mean	C Mean	T Mean	Std Diff. (C-T)	p
All Students	[939]	[465]	[474]			[1170]	[580]	[590]		
Female	0.502	0.520	0.483	0.075	0.253	0.499	0.498	0.500	-0.003	0.953
Potentially Threatened	0.353	0.357	0.348	0.019	0.776	0.384	0.367	0.400	-0.067	0.250
American Indian	0.039	0.047	0.032	0.080	0.218	0.032	0.028	0.036	-0.046	0.434
Asian	0.106	0.092	0.120	-0.090	0.168	0.142	0.147	0.137	0.027	0.650
Black	0.183	0.163	0.203	-0.101	0.122	0.230	0.209	0.251	-0.100	0.086
White	0.757	0.768	0.747	0.049	0.456	0.702	0.712	0.692	0.045	0.443
Limited English Proficiency	0.144	0.159	0.129	0.087	0.184	0.170	0.167	0.173	-0.015	0.798
Free/Reduced Lunch	0.411	0.413	0.409	0.007	0.910	0.463	0.459	0.468	-0.018	0.753
Grade 6 GPA	3.27	3.28	3.27	0.009	0.896	3.19	3.21	3.18	0.042	0.477
	(0.64)	(0.65)	(0.63)			(0.67)	(0.68)	(0.67)		
Grade 6 WKCE Math	525.3	522.2	528.4	-0.108	0.098	516.8	515.0	518.6	-0.071	0.227
	(57.5)	(57.8)	(57.1)			(51.7)	(51.1)	(52.3)		
Grade 6 WKCE Reading	510.8	508.0	513.6	-0.100	0.127	504.8	505.0	504.5	0.009	0.872
	(56.4)	(56.7)	(56.0)			(57.1)	(57.4)	(56.9)		
Black/Hispanic Students	[331]	[166]	[165]			[449]	[213]	[236]		
Female	0.489	0.512	0.467	0.091	0.410	0.566	0.568	0.564	0.009	0.923
Potentially Threatened	1	1	1			1	1	1		
American Indian	0.112	0.133	0.091	0.132	0.231	0.082	0.075	0.089	-0.050	0.595
Asian	0.009	0.006	0.012	-0.064	0.560	0.020	0.028	0.013	0.110	0.244
Black	0.520	0.458	0.582	-0.248	0.024	0.599	0.568	0.627	-0.120	0.203
White	0.568	0.584	0.552	0.066	0.548	0.519	0.521	0.517	0.008	0.930
Limited English Proficiency	0.293	0.343	0.242	0.221	0.044	0.294	0.300	0.288	0.027	0.775
Free/Reduced Lunch	0.801	0.819	0.782	0.094	0.395	0.851	0.864	0.839	0.070	0.461
Grade 6 GPA	2.85	2.83	2.87	-0.061	0.583	2.78	2.75	2.80	-0.076	0.420

	(0.65)	(2.83)	(0.61)			(0.65)	(0.63)	(0.66)		
Grade 6 WKCE Math	491.3	488.9	493.8	-0.092	0.406	486.1	482.6	489.3	-0.149	0.114
	(53.1)	(55.2)	(51.1)			(44.7)	(44.7)	(44.6)		
Grade 6 WKCE Reading	477.9	475.9	480.0	-0.076	0.490	471.9	471.3	472.4	-0.021	0.823
	(53.3)	(51.9)	(54.8)			(52.1)	(52.5)	(51.9)		

T = Treatment, C = Control, Std Diff. = Treatment-control in standardized units, p = p-value for test of the null hypothesis that the difference (C-T) is equal to zero.
Standard deviations in parentheses; sample sizes in brackets
Notes: Racial/ethnic indicators are not mutually exclusive and do not sum to 1 across groups. This table includes multiracial and White Hispanic students with potentially threatened students, as in our main specifications.

Table 2. Standardized Self-affirmation Treatment Impact Estimates for Black and Hispanic Students

	Cohort 1 (N = 331)		Cohort 2 (N = 449)		p-value for
Outcome	Estimate	SE	Estimate	SE	Difference
GPA, Grade 7	0.062	0.057	-0.002	0.043	0.363
GPA, Grade 8	0.152	0.070	-0.072	0.058	0.013
WKCE Mathematics, Grade 7	0.072	0.059	-0.085	0.047	0.037
WKCE Mathematics, Grade 8	0.101	0.070	-0.080	0.044	0.023
WKCE Reading, Grade 7	-0.034	0.069	-0.005	0.055	0.737
WKCE Reading, Grade 8	-0.030	0.071	-0.005	0.056	0.781

SE = Standard Error; GPA = Overall grade point average; WKCE = Wisconsin Knowledge and Concepts Examination

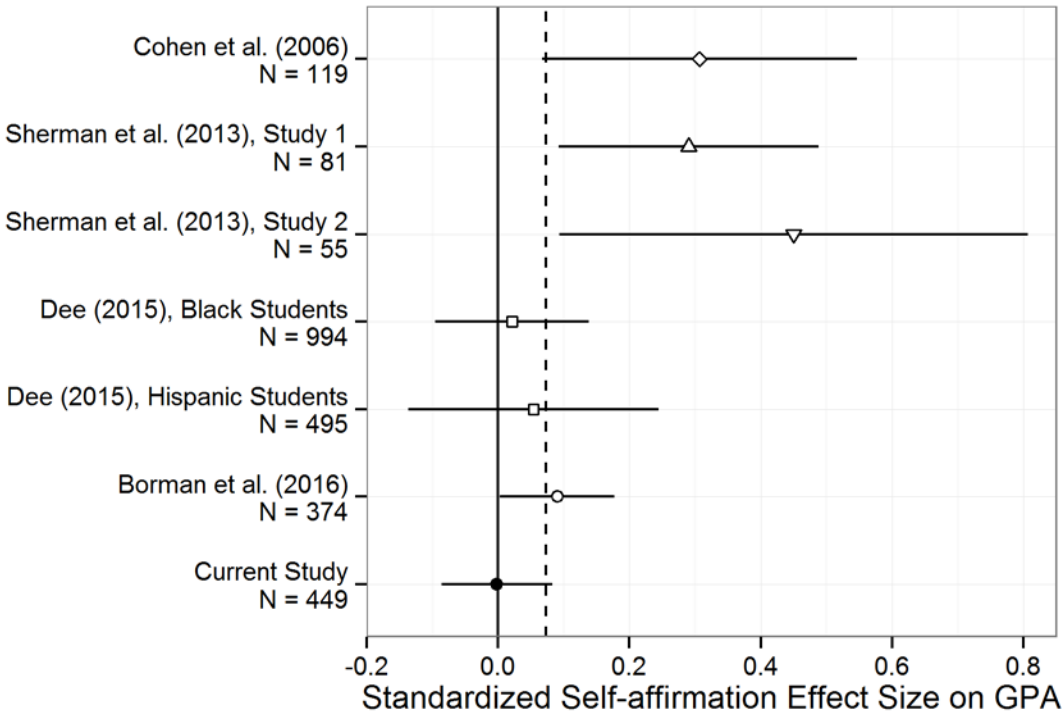
Note: All estimates are based on models including controls for pre-treatment measures of the outcome and baseline student characteristics (gender, special education status, Limited English Proficiency designation, and eligibility for free or reduced price lunch). See Table A3 for full pooled model results.

Table 3. Summary of Tested Hypotheses

Hypothesized Explanation for Difference in Effects	**Empirical Tests of Consistency between Cohorts**	**Result**
Different effects due to features of the intervention delivery/implementation		
Providers	Consistent benefits for teachers implementing in both cohorts?	No
	All changes in benefits are due to teachers implementing in both cohorts (due to fatigue)?	No
Control group	Consistent benefits when compared to students in the original control condition?	No
Stealth	Teachers report more violations of protocol in second cohort: describing the activity as externally imposed research?	No
Awareness of Purported benefits	Teachers report more violations of protocol in second cohort: describing the activity as "good for you"?	No
Timing	Intervention more likely to miss key stressful periods in second cohort?	No
Engagement with the prompt	Students complete fewer exercises in second cohort?	No
	Students write fewer words in second cohort?	No
	Impact on self-affirming writing is different in second cohort?	No
Different effects due to individual characteristics		
Racial group	Consistent benefits for all Black and Hispanic students?	No
	Consistent benefits for non-multiracial Black and Hispanic students?	No
Race and gender	Consistent benefits for male minority students?	No
Prior achievement and other administrative characteristics	Consistent benefits when populations are re-weighted across cohorts on observable characteristics?	No
Unobserved receptivity to self-affirmation	Magnitude of different benefits for unobserved populations are plausible?	No
Social context differences		

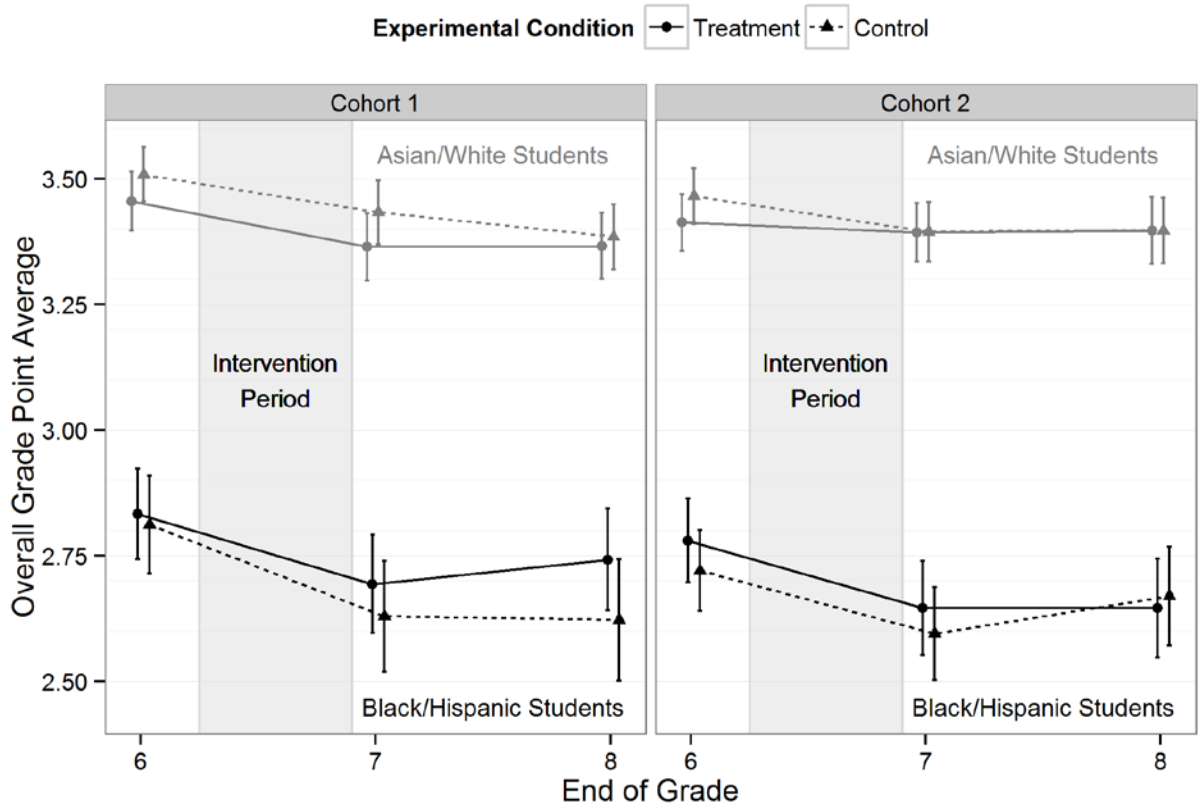
Broad (district) racial and academic climate	Different representation of racial minorities for the second cohort?	No
	Lower racial achievement differences for the second cohort?	No
School racial and academic climate	More consistent benefits in “high threat” schools with few minority students and large gaps?	No
	Differential benefits explained by one or two schools?	No

Figure 1. Estimated Effects of Self-affirmation Writing Exercises on Middle School Grade Point Average



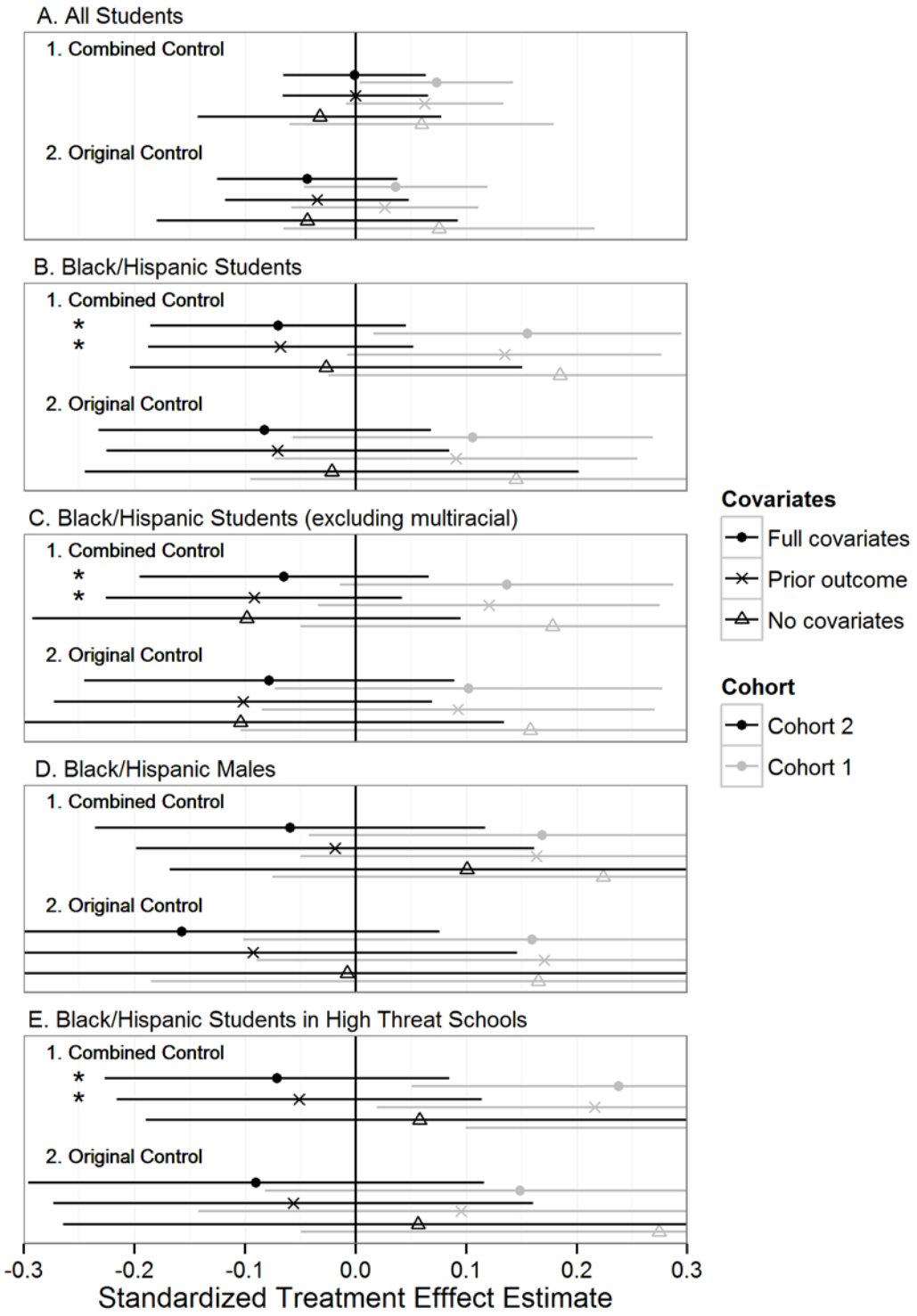
Source: Authors' calculations; see Table A1 for specific references.
Notes: Symbols plot reported effect sizes for potentially stereotyped groups (African American and/or Hispanic students) for the first year of the self-affirmation intervention, and lines represent 95% confidence intervals (+/- 1.96 standard errors). Shapes represent distinct school or district contexts. For instance, Sherman et al. (2013) studies 1 and 2 were conducted in different schools in different states. Dee (2015) reports subgroup results from the same sample of Philadelphia-area schools. The dashed line represents the overall mean effect size (0.07), calculated by weighting individual estimates according to the inverse of their squared standard error. The impact estimates are lower in the large-scale replication studies (Dee 2015, Borman et al. 2016, and Current Study), but these differences could reflect heterogeneous effects across local context, research team, and implementation. This paper investigates two effects observed within the trial conducted in a single school district (represented by circles), for which context and procedures were consistent.

Figure 2. Yearly Grade Point Average (with 95% Confidence Intervals) by Race/ethnicity and Experimental Condition



Notes: Randomly assigned self-affirmation writing interventions were administered throughout the 7th grade year. No effects of the treatment are hypothesized for Asian and White students, who are not subject to general negative stereotypes about academic ability. Raw treatment vs. control differences are statistically different from zero only for Black and Hispanic students in Grade 8 in cohort 1. The treatment benefits in that cohort are statistically different than the small negative effect observed in cohort 2. See Table 2 for standardized estimates and Table A3 for results from a pooled treatment effects model.

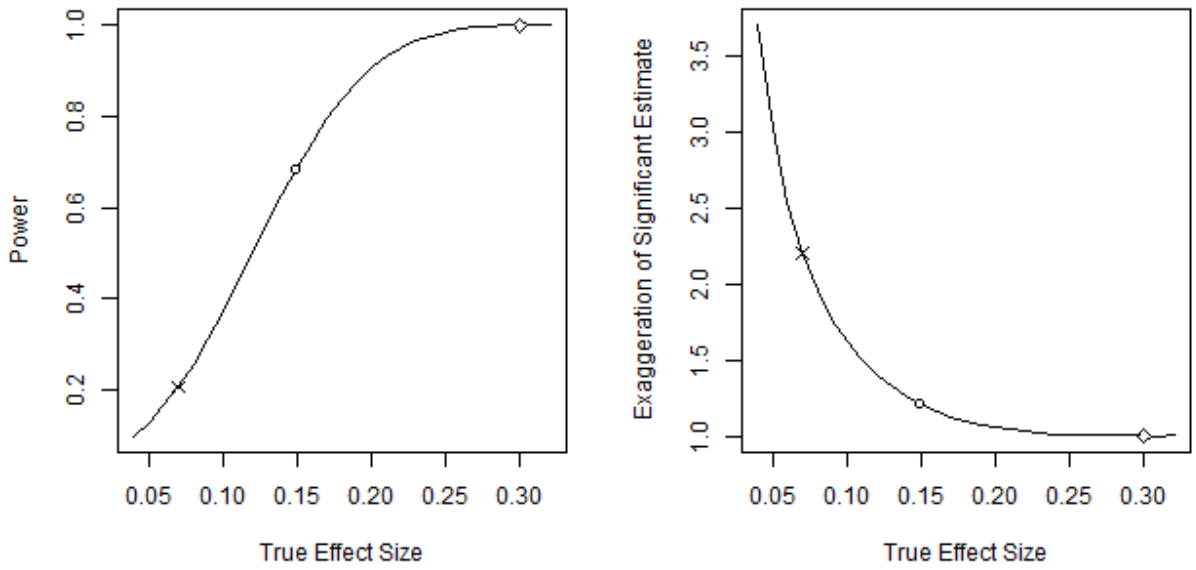
Figure 3. Estimated Self-affirmation Treatment Effects on Grade 8 GPA by Cohort, Sample, Comparison Group, and Included Covariates



GPA = Overall Grade Point Average; CI = Confidence Interval

Note: Each estimate was calculated from a separate multilevel model (students nested within schools) of intention to treat effect of the self-affirmation writing activities. Full covariates specifications include: grade 6 GPA, gender, special education status, Limited English Proficiency designation, and eligibility for free or reduced price lunch. Prior outcome is grade 6 GPA. In the "Original Control" condition, students wrote about a least important value in each of the first two interventions. The "Combined Control" group includes these students as well as those who were assigned at least one writing prompt that did not explicitly mention values. For readability, the displayed range is restricted to effect sizes of absolute value 0.3 or less. Asterisks indicate that the estimated effects are statistically significantly different between cohorts ($p < 0.05$), based on a pooled model. The primary result, reported in Table 2, is the estimate for Black/Hispanic sample with combined control condition and full covariates (Panel B1 circles). Other results assess whether patterns were different for subpopulations and comparisons where self-affirmation benefits are hypothesized to be stronger and more consistent, as described in the text. Because the cohort difference persists across all specifications (although less precise in smaller subsamples), these tests provide no evidence that hypothesized moderators explain the difference.

Figure 4. Power Calculations for Range of True Effect Sizes of Self-affirmation Intervention Effects



Notes: Curves represent power (left panel) and expected exaggeration of a treatment effect estimate significant at the 0.05 level (right panel) for self-affirmation effects in grade 8, given the design for new study (cohort 2) reported here. Calculations are based on the procedure suggested by Gelman and Carlin (2014). Diamonds represent an effect size of 0.3, consistent with the initial study of self-affirmation interventions (Cohen et al. 2006); if true effects are this large, then power is virtually 1.0 and expected exaggeration is minimal. Circles represent the estimated effect size for the first cohort of students (d = 0.15). If the true effect were this large, then cohort 2 power would be 0.68 and expected exaggeration would be 1.21. Xs represent the mean effect size calculated in Figure 1 (d = 0.07). If the true effect were this large, power would be 0.21 and significant values would exaggerate the true effect by 2.22 times on average.

SUPPLEMENTAL MATERIALS INTENDED FOR ONLINE DISTRIBUTION

Appendix

Table A1. Source of Standardized Impact Estimates Displayed in Figure 1

Citation	Grade(s)	N	N Source	D	SE	Impact Source	Additional Notes
Cohen et al. (2006)	7	119	p. 1307	0.31	0.12	"African American students in the affirmation condition earned a higher grade point average (GPA) in these nontargeted courses than did those in the control condition [experiment 1: B = 0.31, t(40) = 2.63, P < 0.02; experiment 2: B = 0.21, t(58) = 1.70, P < 0.10 two-tailed test, P < 0.05 one-tailed test]. Pooling data from both experiments yielded a significant effect [B = 0.23, t(108) = 2.51, P = 0.02]." (p. 1308)	SE calculated from reported t statistic and estimate. Effect size calculated assuming GPA standard deviation of 0.75.
Sherman et al. (2013) Study 1	6,7,8	81	p. 596	0.29	0.10	"Affirmed Latino American students (M = 2.62, SE = 0.06) had a higher GPA than unaffirmed Latino American students (M = 2.40, SE = .06), F(1, 177) = 8.18, p = .005, d = 0.29." (p. 600)	SE calculated from derived t statistic (square root of F statistic) and reported estimate.
Sherman et al. (2013) Study 2	7	55	p. 602	0.45	0.18	"Affirmed Latino American students (M = 2.84, SE = 0.12) had a higher GPA than unaffirmed Latino American students (M = 2.46, SE = 0.11), F(1, 146) = 5.05, p = .026, d = 0.45." (p. 605)	SE calculated from derived t statistic (square root of F statistic) and reported estimate.
Dee (2015): Black Students	7	994	Calculated from Table 2	0.02	0.06	Raw impact estimate reported in Table 5; SD of outcome reported in Table 3.	Overall GPA not reported. Estimate reflects targeted class only.

Dee (2015): Hispanic Students	7	495	Calculated from Table 2	0.05	0.10	Raw impact estimate reported in Table 5; SD of outcome reported in Table 3.	Overall GPA not reported. Estimate reflects targeted class only.
Borman et al. (2016)	7	374	Calculated from Table 1	0.09	0.04	"To illustrate, the estimated interaction term (0.082) and marginal effect (0.065) for the cumulative GPA outcome correspond to effect sizes of 0.11 and 0.09, respectively." (35)	SE is calculated from CI for raw estimate reported in Table A5.
Current Study	7	449	Table 1	0.00	0.04	Table 2	

D = Standardized Treatment Effect, SE = Standard Error, CI = 95% Confidence Interval

Notes: All estimates reflect standardized impacts on grade point average (overall if reported) during the year of implementation. A spreadsheet with all calculations is available upon request.

Table A2. Summary of the Madison Writing and Achievement Project Self-affirmation Randomized Control Trial in Two Cohorts

		Cohort 1	Cohort 2
Overview	7th Grade Year	2011-2012	2012-2013
	Treatment Implementation	Conducted by Language Arts or Homeroom Teachers 3-4 times during year	Conducted by Language Arts or Homeroom Teachers 3-4 times during year
	Experimental Groups	Treatment (50%), Control A (25%), and Control B (25%)	Treatment (50%), Control A (25%), and Control B (25%)
Recruitment	Parental Consent	Collected at school registration days (August) and follow-up via permission slips distributed in school (September)	Collected at school registration days (August) and follow-up via permission slips distributed in school (September)
	Student Assent	Conducted in classrooms at the beginning of the school year (September)	Collected at school registration days (August) and individual follow-up in school (September)
	Consent Rate	63.6%	72.8%
Intervention Details			
Ex 1 (September or October)	Treatment	Students select from a list and write about their important values	Students select from a list and write about their important values
	Control A	"Original" control: students select non-important values and write about their potential importance to someone else	"Original" control: students select non-important values and write about their potential importance to someone else

	Control B	"Original" control: students select non-important values and write about their potential importance to someone else	"Neutral" Control: Students respond to a writing prompt about their summer that does not explicitly mention values
Ex 2 (November)	Treatment	Students select from a list and write about their important values	Students select from a list and write about their important values
	Control A	"Original" control: students select non-important values and write about their potential importance to someone else	"Original" control: students select non-important values and write about their potential importance to someone else
	Control B	"Neutral" Control: students respond to a procedural writing prompt, such as explaining how to open a locker	"Neutral" Control: students respond to a procedural writing prompt, such as explaining how to open a locker
Ex 3 (January or February)	Treatment	Students write free-response about important values	Students write free-response about important values
	Control (A and B)	Students respond to a procedural writing prompt about their morning routine	Students respond to a procedural writing prompt about their morning routine
Ex 4 (April or May)	Treatment	Students write about how a previously selected value is important now	Students write about how a previously selected value is important now
	Control (A and B)	Students respond to a procedural writing prompt about their after-school routine	Students respond to a procedural writing prompt about their after-school routine

Other In Class Activities	In-class assent	Conducted by researchers in classrooms at the beginning of the school year (September) with no specific reference to self-affirmation activities	None
	In-class survey	None	Conducted by researchers at beginning (September) and end (May) of 7th grade with no specific reference to self-affirmation activities

Table A3. Regression Estimates from Pooled Multilevel Models of Treatment Impacts on each Outcome for Potentially Threatened Students (Black and Hispanic)

	(1)	(2)	(3)	(4)	(5)	(6)
	Grade 7 GPA	Grade 8 GPA	Grade 7 Math	Grade 8 Math	Grade 7 Read	Grade 8 Read
Treatment	0.044	0.124*	2.996	4.954+	-2.035	-1.336
	(0.041)	(0.054)	(2.573)	(2.905)	(3.432)	(3.445)
Cohort 2	0.185	0.535*	7.065	8.338	51.166+	11.904
	(0.179)	(0.235)	(23.358)	(26.375)	(28.740)	(28.860)
Treatment * Cohort	-0.046	-0.180*	-6.893*	-8.850*	1.589	0.872
	(0.054)	(0.071)	(3.386)	(3.823)	(4.512)	(4.530)
6th Grade Outcome Measure	0.935*	0.864*	0.722*	0.754*	0.811*	0.750*
	(0.037)	(0.049)	(0.032)	(0.036)	(0.042)	(0.042)
Female	0.089*	0.112*	2.395	-2.074	5.805+	9.925*
	(0.041)	(0.054)	(2.565)	(2.896)	(3.420)	(3.433)
Limited English Proficiency	0.063	0.163*	-2.262	-0.437	-6.495	2.262
	(0.048)	(0.063)	(3.060)	(3.455)	(4.212)	(4.227)
Special Education	-0.012	0.001	-7.653*	-16.141*	-11.394*	-17.482*
	(0.056)	(0.073)	(3.863)	(4.362)	(4.986)	(5.005)
Free/reduced Price Lunch	-0.207*	-0.187*	-6.935+	-10.572*	-18.284*	-17.924*
	(0.057)	(0.075)	(3.594)	(4.058)	(4.766)	(4.772)
Cohort 2 * 6th Grade Outcome Measure	-0.027	-0.086	0.014	-0.018	-0.107*	-0.044
	(0.049)	(0.064)	(0.042)	(0.048)	(0.053)	(0.053)
Cohort 2 * Female	-0.073	0.005	-3.337	1.132	-5.676	-9.961*
	(0.055)	(0.072)	(3.413)	(3.854)	(4.555)	(4.572)
Cohort 2 * Limited English Proficiency	0.008	-0.003	5.383	3.558	6.442	-2.177
	(0.063)	(0.082)	(3.947)	(4.456)	(5.409)	(5.423)
Cohort 2 * Special Education	0.128+	0.074	-7.312	1.176	-4.201	1.942
	(0.074)	(0.098)	(5.013)	(5.660)	(6.592)	(6.617)
Cohort 2 * Free/reduced Price Lunch	-0.019	-0.183+	-6.558	-2.920	2.487	2.110
	(0.078)	(0.103)	(4.891)	(5.523)	(6.479)	(6.503)
Constant	0.082	0.185	160.789*	159.516*	120.567*	159.464*
	(0.148)	(0.189)	(17.710)	(19.997)	(22.613)	(22.689)
Variance Component Estimates						
var(School)	0.036*	0.036*	0.000	0.000	10.671*	5.544

	(0.016)	(0.017)	(0.001)	(0.000)	(10.845)	(8.492)
var(Residual)	0.137*	0.236*	539.207*	687.458*	956.376*	964.651*
	(0.007)	(0.012)	(27.306)	(34.811)	(48.790)	(49.205)
N	780	780	780	780	780	780
Schools	11	11	11	11	11	11

Standard errors in parentheses
+ p<.1, * p<.05

Table A4. Estimates for Treatment Impacts on Grade 8 GPA by Cohort and Teacher Type (both-cohort versus single-cohort) for Potentially Threatened Students (Black and Hispanic)

	(1)	(2)	(3)	(4)
	All Teachers	Both-cohort Teachers	Single-cohort Teachers	All Teachers
Treatment	0.122*	0.109+	0.158	0.168
	(0.055)	(0.061)	(0.120)	(0.114)
Cohort 2	0.149*	0.165*	0.098	0.110
	(0.052)	(0.062)	(0.108)	(0.099)
Treatment * Cohort 2	-0.188*	-0.196*	-0.188	-0.199
	(0.073)	(0.086)	(0.144)	(0.137)
Both-cohort Teacher				0.010
				(0.096)
Treatment * Both-cohort Teacher				-0.060
				(0.130)
Cohort 2 * Both-cohort Teacher				0.066
				(0.118)
Treatment * Cohort 2 * Both-cohort Teacher				0.003
				(0.163)
Grade 6 GPA	0.831*	0.806*	0.871*	0.832*
	(0.033)	(0.040)	(0.060)	(0.033)
Female	0.102*	0.077+	0.155*	0.103*
	(0.037)	(0.044)	(0.071)	(0.037)
Limited English Proficiency	0.152*	0.189*	0.090	0.151*
	(0.042)	(0.051)	(0.075)	(0.042)
Special Education	0.041	0.023	0.074	0.038
	(0.051)	(0.057)	(0.109)	(0.051)
Free/reduced Lunch	-0.272*	-0.259*	-0.293*	-0.272*
	(0.055)	(0.064)	(0.104)	(0.055)
Intercept	0.353*	0.465*	0.275	0.342*
	(0.137)	(0.160)	(0.248)	(0.155)
Variance Component Estimates				
var(School)	0.034*	0.030*	0.051*	0.034*
	(0.016)	(0.016)	(0.031)	(0.016)
var(Residual)	0.242*	0.229*	0.262*	0.242*
	(0.013)	(0.015)	(0.024)	(0.013)
N	744	501	243	744
Schools	11	9	9	11

Standard errors in parentheses
+ p<.1; * p<.05

Table A5. OLS Estimates of Implementation Measures for each Exercise by Treatment Group and Cohort for Black and Hispanic Students

Model	(1)	(2)	(3)	(4)	(5)
Outcome	Returned Assigned Exercise	Words Written	Words Written	Self-affirmation Writing	Self-affirmation Writing
Control Group	Both	Both	Original Only	Both	Original Only
A. Exercise 1					
Treatment (in Cohort 1)	-0.0004	10.94+	9.169	0.721*	0.724*
	(0.0346)	(5.929)	(6.474)	(0.0321)	(0.0367)
Cohort 2 (among control students)	0.0242	13.46*	-1.911	0.330*	0.0431
	(0.0276)	(4.186)	(4.800)	(0.0367)	(0.0543)
Treatment x Cohort 2	-0.0446	-16.38+	-0.630	-0.294*	-0.0105
	(0.0465)	(7.563)	(6.981)	(0.0433)	(0.0476)
N	780	741	586	780	619
Outcome mean	0.931	71.8	69.3	0.587	0.601
B. Exercise 2					
Treatment (in Cohort 1)	0.0354	7.304	15.47*	0.709*	0.669*
	(0.0258)	(5.383)	(6.202)	(0.0423)	(0.0473)
Cohort 2 (among control students)	0.0134	6.652	5.236	0.00792	-0.0436
	(0.0556)	(3.766)	(4.656)	(0.0229)	(0.0446)
Treatment x Cohort 2	0.00480	-4.941	-2.590	0.0796+	0.129*
	(0.0472)	(6.702)	(7.070)	(0.0413)	(0.0568)
N	780	705	561	780	619
Outcome mean	0.879	70.8	67.9	0.442	0.546
C. Exercise 3					
Treatment (in Cohort 1)	-0.0912	14.33	9.952	0.370*	0.334*
	(0.0555)	(9.707)	(11.40)	(0.125)	(0.140)
Cohort 2 (among control students)	0.0952	-1.364	-16.13	-0.0177	-0.0473
	(0.181)	(10.28)	(13.11)	(0.0121)	(0.0263)
Treatment x Cohort 2	0.0800	-6.801	8.553	0.273+	0.297+
	(0.0651)	(11.45)	(14.26)	(0.131)	(0.135)
N	780	464	356	780	619
Outcome mean	0.597(a)	77.6	77.6	0.282	0.354
D. Exercise 4					

Treatment (in Cohort 1)	0.00791	-5.118	-6.270	0.611*	0.601*
	(0.0506)	(6.927)	(7.650)	(0.0386)	(0.0403)
Cohort 2 (among control students)	0.0458	-10.48	-13.11	0.0215	0.0195
	(0.0384)	(8.462)	(11.45)	(0.0219)	(0.0359)
Treatment x Cohort 2	-0.0313	4.243	7.705	0.0795	0.0821
	(0.0725)	(6.838)	(8.874)	(0.0631)	(0.0693)
N	780	695	547	780	619
Outcome mean	0.868	82.4	82.1	0.362	0.451

+ p < .1; * p< .05

(a) Lower exercise completion rates in exercise 3 reflect the inclusion of students in several schools that opted out of this implementation.

Notes: Each panel presents selected results of one of 20 separate Ordinary Least Squares regression models, defined by the model specification listed in the column heading (1-5) for the exercise listed in the row (A-D). All models include indicators for randomization block (school) and intercept (not shown). Standards errors (adjusted for clustering within schools) reported in parentheses.

Table A6. Logistic Regression Estimates of Predictors of Membership in Cohort 1 among Black and Hispanic Students (N=780)

	Coef.	Std. Err.	P-value
Treatment	-0.071	0.152	0.639
Female	-0.327	0.156	0.036
Limited English Proficiency	0.121	0.182	0.507
Special Education	0.136	0.224	0.545
Free/Reduced Lunch	-0.301	0.234	0.198
Grade 6 GPA Quintiles			
2	0.076	0.249	0.761
3	0.233	0.266	0.380
4	0.352	0.290	0.225
5	0.399	0.348	0.252
Grade 6 Math Achievement Quintiles			
2	-0.556	0.262	0.034
3	-0.872	0.294	0.003
4	-0.662	0.313	0.034
5	-0.083	0.367	0.821
Grade 6 Reading Achievement Quintiles			
2	0.344	0.254	0.175
3	0.400	0.282	0.155
4	0.584	0.302	0.054
5	0.033	0.369	0.929
School Indicators			
2	0.508	0.300	0.091
3	0.158	0.339	0.641
4	-0.326	0.440	0.458
5	0.077	0.347	0.825
6	-0.006	0.373	0.987
7	0.669	0.351	0.057
8	-0.061	0.397	0.878
9	0.256	0.331	0.441
10	-0.750	0.385	0.051
11	-0.051	0.332	0.879
Intercept	-0.081	0.446	0.856

Table A7. Ordinary Least Squares Estimates of Self-affirmation Treatment Effects on Grade 8 GPA (4 point scale) for Black and Hispanic Students by Cohort, with and without Weights for Cohort Membership

	Cohort 1	Cohort 2
N	331	449
Unweighted	0.118	-0.056
	(0.055)	(0.046)
Weighted	0.117	-0.041
	(0.058)	(0.044)

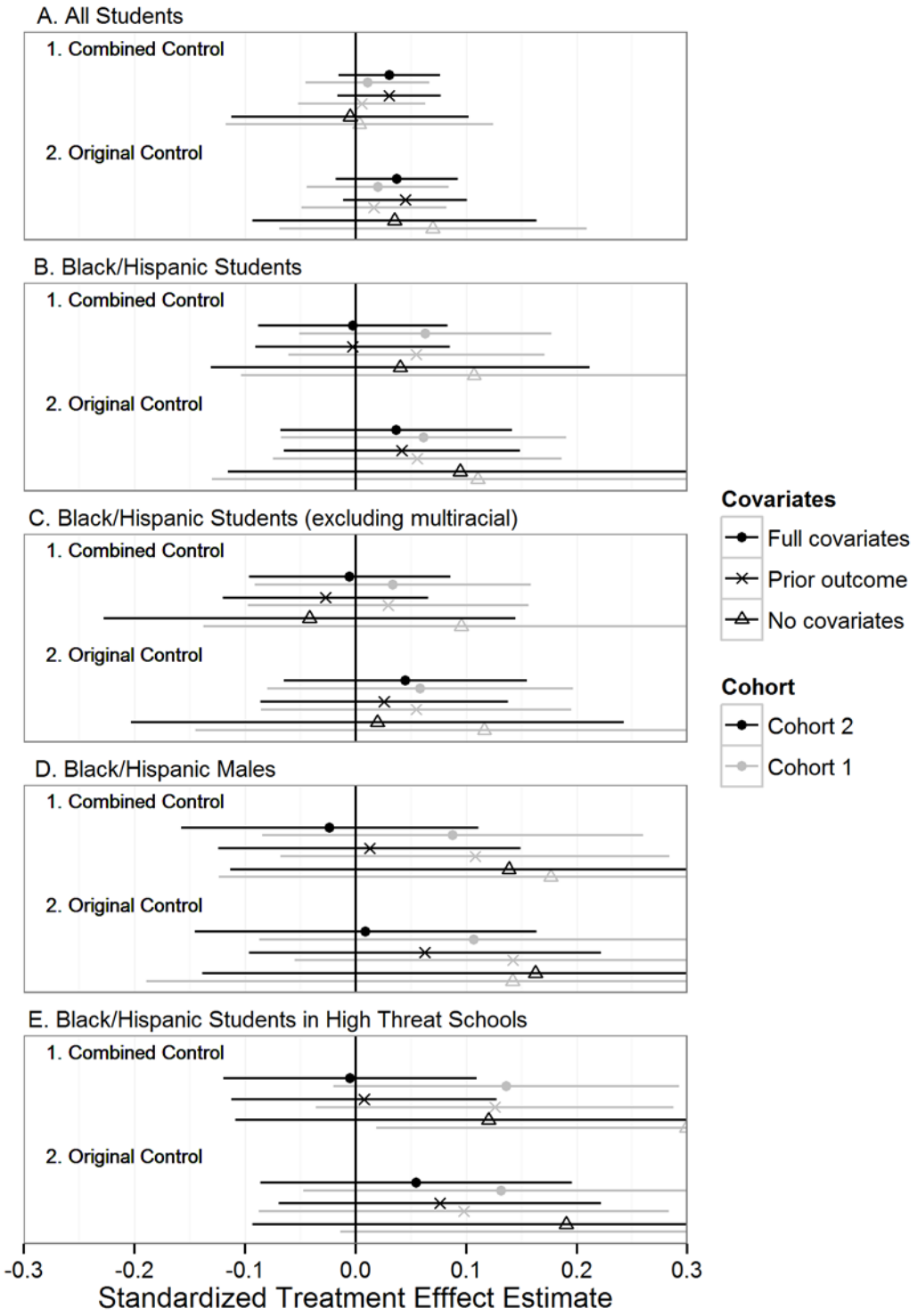
Standard errors in parentheses.

Note: All estimates are based on models including controls for randomization block (school), grade 6 GPA, and baseline student characteristics (gender, special education status, Limited English Proficiency designation, and eligibility for free or reduced price lunch). Weighted models are weighted by the inverse of the estimated probability of inclusion in the cohort. For example, the weights for cohort 2 members are defined as:

$$w_i = \frac{1}{\Pr(Cohort_i = 2)}$$

We estimated the probability of inclusion in a cohort with a logistic regression of group membership on student characteristics, estimates reported in Table A6.

Figure A1. Estimated Self-affirmation Treatment Effects on Grade 7 GPA by Cohort, Sample, Comparison Group, and Included Covariates

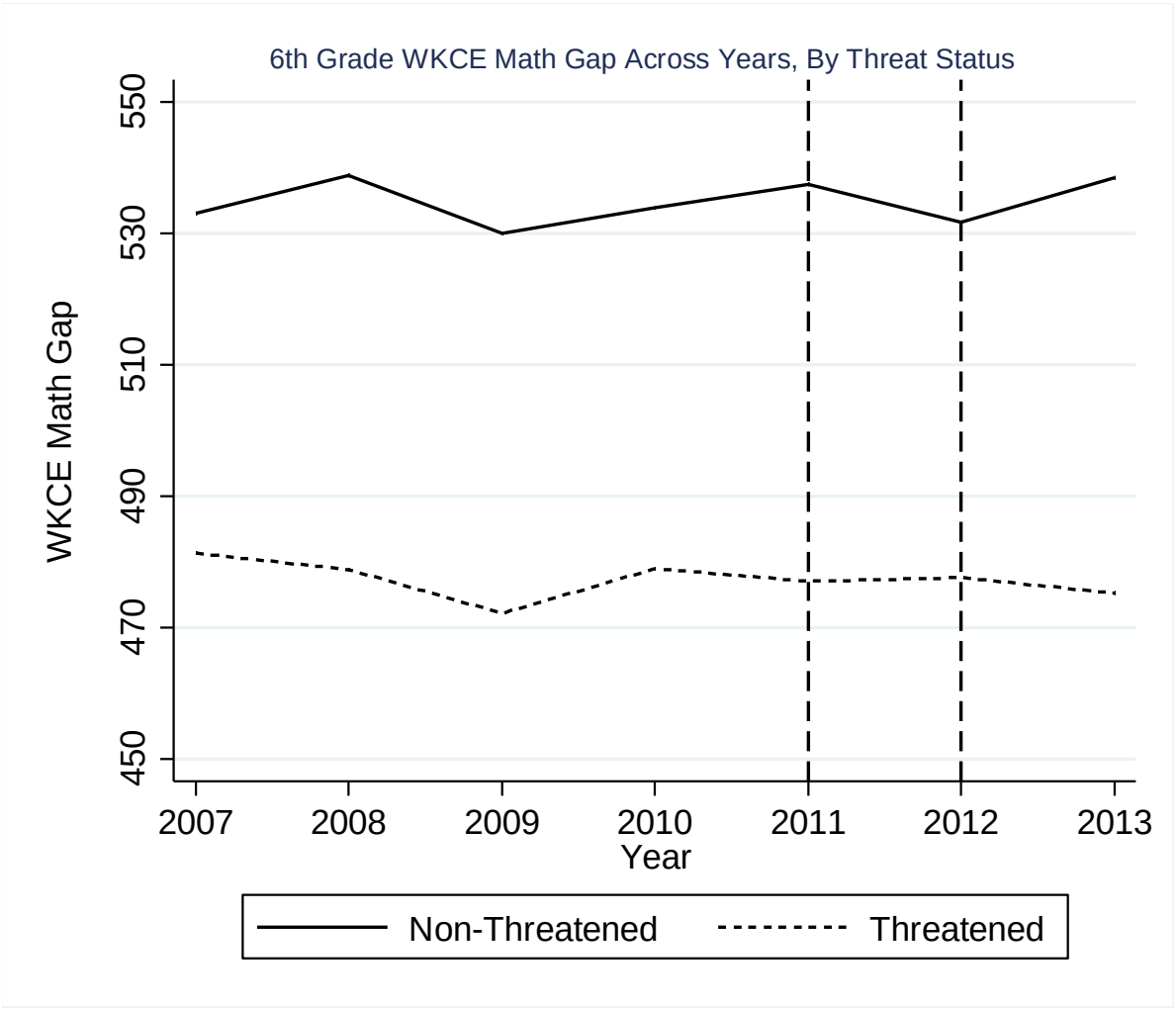


GPA = Overall Grade Point Average; CI = Confidence Interval

Note: Each estimate was calculated from a separate multilevel model (students nested within schools) of intention to treat effect of the self-affirmation writing activities. Full covariates

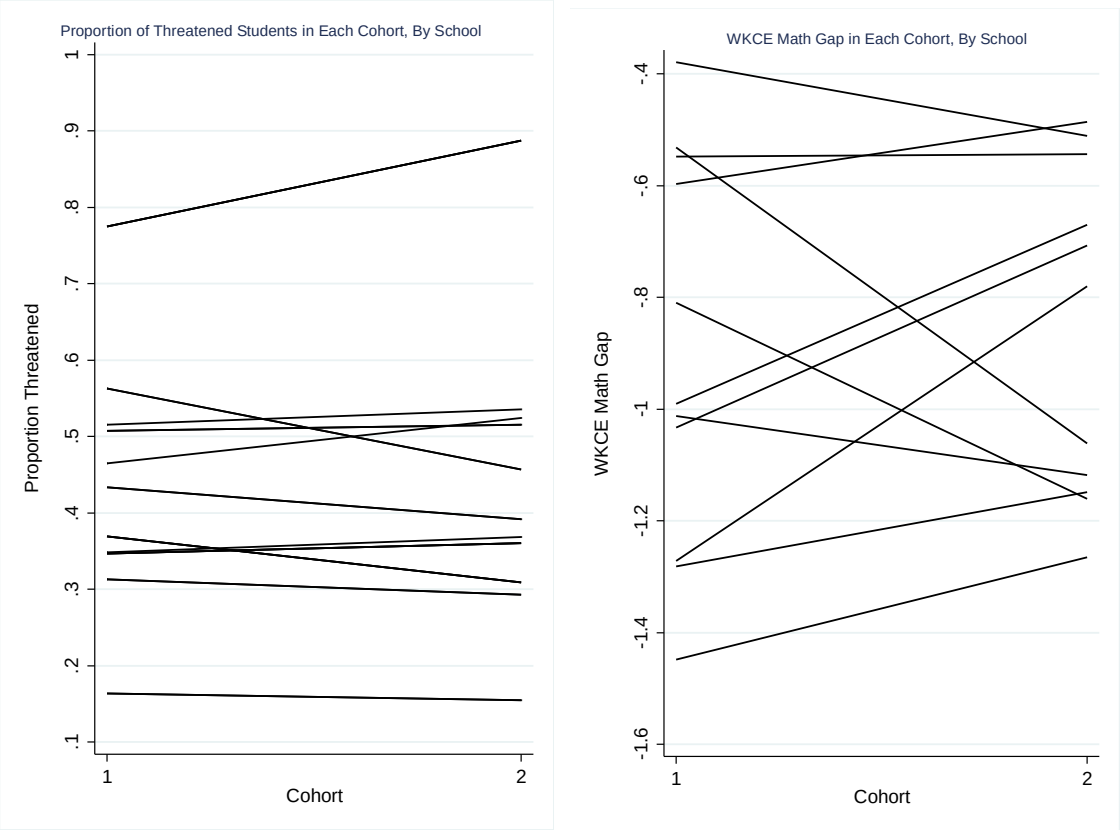
specifications include: grade 6 GPA, gender, special education status, Limited English Proficiency designation, and eligibility for free or reduced price lunch. Prior outcome is grade 6 GPA. In the “Original Control” condition, students wrote about a least important value in each of the first two interventions. The “Combined Control” group includes these students as well as those who were assigned at least one writing prompt that did not explicitly mention values. For readability, the displayed range is restricted to effect sizes of absolute value 0.3 or less. Asterisks indicate that the estimated effects are statistically significantly different between cohorts ($p < 0.05$), based on a pooled model. The main result is the Black/Hispanic sample with combined control condition and full covariates (Panel B1 circles). Other results assess whether patterns were different for subpopulations and comparisons where self-affirmation benefits are hypothesized to be stronger and more consistent, as described in the text. Because the cohort difference persists across all specifications (although less precise in smaller subsamples), these tests provide no evidence that hypothesized moderators explain the difference.

Figure A2. Racial/ethnic Achievement Gap in 6th Grade Mathematics, 2007-2013



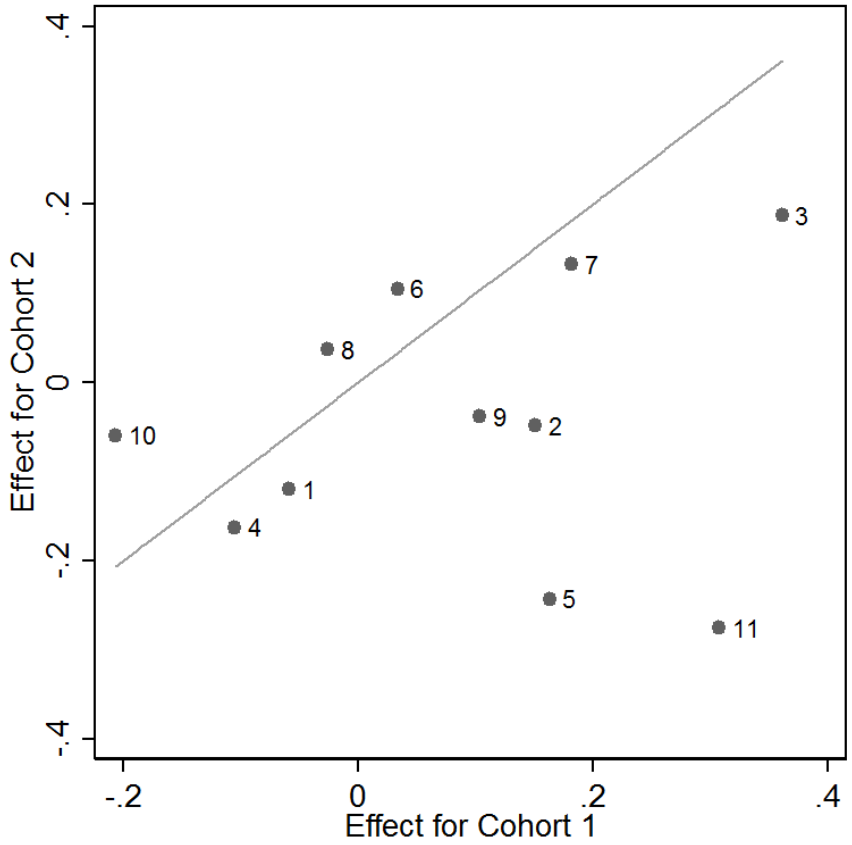
WKCE = Wisconsin Knowledge and Concepts Evaluation
Notes: Non-threatened students include White and Asian students. Threatened students are Black and Hispanic.

Figure A3. Demographic Consistency between the Two Cohorts: Racial Composition and Standardized Achievement Gaps Prior to the Intervention



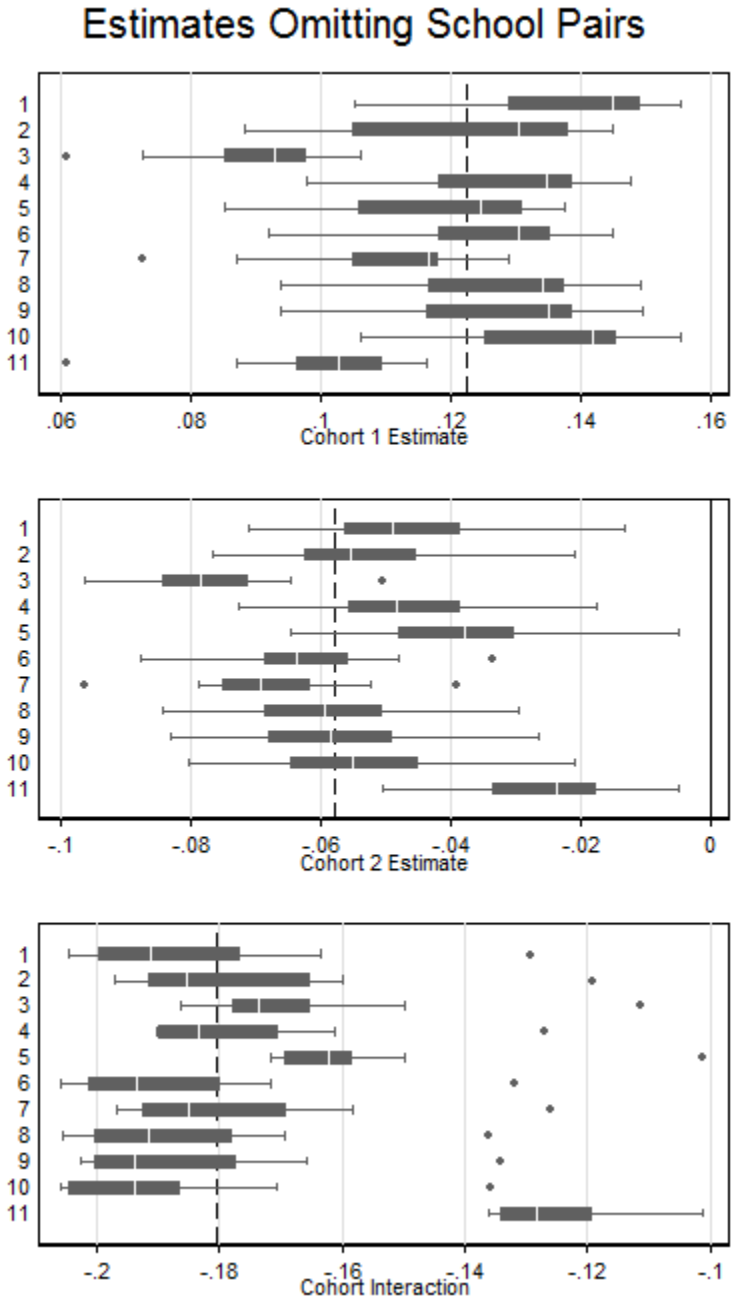
Notes: Each line represents one school. Horizontal slopes indicate no absolute change in the demographic characteristic between each cohort. Racial achievement gaps are calculated as the standardized difference in Wisconsin Knowledge and Concepts Examination (WKCE) mathematics scale scores in 6th grade for Black and Hispanic students compared to Whites and Asians.

Figure A4. Treatment Impact Estimates for Black/Hispanic Students in Cohort 1 and 2 by School



Notes: Each point represents the two cohort treatment effect estimates for grade 8 GPA for a each school (among Black and Hispanic students), including controls for grade 6 GPA, gender, special education status, Limited English Proficiency designation, and eligibility for free or reduced price lunch. The line y=x is plotted, representing equal estimates in both cohorts.

Figure A5. Distribution of Self-affirmation Treatment Estimates in each Cohort and Interaction, Omitting Pairs of Schools



Each boxplot represents the distribution of estimates omitting 10 school pairs, grouped by the 11 schools. The top boxplot in each panel reflects estimates from all pairs omitting school 1 (1 and 2, 1 and 3, …, 1 and 11). The next reflects all estimates omitting school 2 (2and 1, 2 and 3, … , 2 and 11). Note that each pair of schools is therefore represented twice (the 1-2 pair is represented in the distribution for school 1 and for school 2, for instance). The dashed lines represent the overall estimate (omitting no schools).