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Creating Birds of Similar Feathers: Leveraging Similarity to Improve Teacher–Student Relationships and Academic Achievement

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When people perceive themselves as similar to others, greater liking and closer relationships typically result. In the first randomized field experiment that leverages actual similarities to improve real-world relationships, we examined the affiliations between 315 9th grade students and their 25 teachers. Students in the treatment condition received feedback on 5 similarities that they shared with their teachers; each teacher received parallel feedback regarding about half of his or her 9th grade students. Five weeks after our intervention, those in the treatment conditions perceived greater similarity with their counterparts. Furthermore, when teachers received feedback about their similarities with specific students, they perceived better relationships with those students, and those students earned higher course grades. Exploratory analyses suggest that these effects are concentrated within relationships between teachers and their "underserved" students. This brief intervention appears to close the achievement gap at this school by over 60%.

Keywords: adolescence, brief interventions, field experiment, matching, motivation, similarity

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Humans foster social connections with others as a fundamental, intrinsic social motivation—we are hard-wired to be social animals (Lieberman, 2013; Ryan & Deci, 2000). Those who more successfully relate to others experience a broad constellation of positive outcomes ranging from greater happiness (Gilbert, 2006) to superior health (Taylor et al., 2004). Children who thrive typically cultivate positive relationships with parents, peers, and teachers (Wentzel, 1998). Even for adolescents, achieving positive teacher–student relationships (TSRs) is an important outcome in its own right and may catalyze important downstream benefits (Eccles et al., 1993).

Thus, for those who study positive youth development, schooling, and social motivation (e.g., Bronk, 2012; Pintrich, 2003) the topic of improving TSRs sparks tremendous interest. One promising approach might leverage individuals' perceptions of similarity as a means to promote a sense of relatedness. Numerous basic social psychological texts underscore some version of the basic message that "likeness begets liking" (Myers, 2015, p. 330). Similarity along various dimensions (style of dress, background, interests, personality traits, hobbies, attitudes, etc.) connects to a wide array of relationship-related outcomes (such as attraction, liking, compliance, and prosocial behavior) in scores of studies (Cialdini, 2009; Montoya, Horton, & Kirchner, 2008).

The theory behind the promise of this approach is that interacting with similar others supports one's sense of self, one's values, and one's core identity (Montoya et al., 2008; Myers, 2015). In other words, as an individual interacts with similar others, she reaps positive reinforcement in the form of validation. For instance, imagine a ninth grade student enrolling in high school in a new town. As she encounters peers who also value religion, enjoy sports, participate in math club, and aspire to attend college, she learns that her values and beliefs are socially acceptable within her

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new community. Continuing to affiliate with these individuals will reinforce a perception that her values and beliefs have merit. Conversely, her peers who eschew religion, think sports are silly, ridicule math club, and see no point in college will cast doubt on the values and beliefs that lie at the core of her identity. Spending time with these students will not be reinforcing. In this way, similarity acts as a powerfully self-affirming motivator (Brady et al., in press) in the context of friendships and close relationships.

Unfortunately, a fundamental problem arises in using similarity to improve relationships: people either share something in common or they do not. Thus, scholars can develop experimental manipulations of similarities, but these interventions typically rely upon fictitious similarities (e.g., Burger, Messian, Patel, del Prado, & Anderson, 2004; Galinsky & Moskowitz, 2000). While these studies enable causal inferences to be made, the fictitious nature of the similarities minimizes their utility for real-world interventions. On the other hand, numerous correlational studies have identified real similarities between individuals in real relationships and have shown that these similarities correspond with improved relationship outcomes (e.g., Chen, Luo, Yue, Xu, & Zhaoyang, 2009; Gonzaga, Campos, & Bradbury, 2007; Ireland et al., 2011). However, the correlational nature of these studies precludes causal inferences from being made. Thus, how scholars might successfully leverage real similarities to improve real-world relationships, such as TSRs, remains a vexing challenge.

In this study, we test the effects of an intervention that potentially mitigates these trade-offs. Specifically, we experimentally manipulate *perceptions* of veridical similarities as a means to try and improve TSRs between ninth graders and their teachers. In addition to examining TSRs as a key outcome, we note that these relationships have shown robust associations with consequential student outcomes (McLaughlin & Clarke, 2010). Thus, we also test whether the intervention affects students' classroom grades. To our knowledge, this is the first experimental study to use actual similarities as a means to improving real, ongoing relationships.

Similarity and Relationships

Of the research connecting similarity and interpersonal relationships, two main types of studies proliferate: those that have fabricated similarities for the sake of experimental manipulations and those that have investigated actual similarities. Both types of studies have enhanced scientific understanding of the importance and potency of similarity in relationships. Across both the experimental and correlational approaches, two notable themes emerge.

First, the content of the similarities associated with improved relationship outcomes covers an impressively disparate array of topics. For example, experimentally manipulating the similarity of names appears to boost liking and compliance. One researcher bolstered return rates on a questionnaire by using names on a cover letter that were similar to respondents' own names (Gamer, 2005). In a series of primarily correlational studies, Mackinnon, Jordan, and Wilson (2011) found that students who are physically similar to one another (e.g., both wearing glasses) will tend to sit next to one another in class. Using both experimental and correlational approaches, Boer et al. (2011) found that shared music preferences helped foster closer social bonds between people.

Although few scholars have explored the idea of using similarities to improve relationships in education, some have examined whether students perform better academically when their teacher shares their ethnicity. For instance, Dee (2004) found significant positive effects on test score outcomes for Black students who were assigned to Black teachers and for White students who were assigned to White teachers. Although he does not examine TSRs, he does hypothesize that trust and role-modeling may be crucial mechanisms in explaining his findings.

Second, even the most trivial similarities can lead to positive sentiments toward another person. Laboratory experiments informing participants that they and another participant share: a preference for Klee versus Kandinsky paintings (Ames, 2004), the tendency to over- or underestimate the number of dots on a computer screen (Galinsky & Moskowitz, 2000), or purported similarity in fingerprint patterns (Burger et al., 2004), have all enhanced relationship-related outcomes. Correlational studies show comparably surprising findings. For example, people who have similar initials are disproportionately likely to get married (Jones, Pelham, Carvallo, & Mirenberg, 2004).

Despite their contributions, these two approaches to studying the connections between similarity and relationships leave two important gaps in our knowledge. First, this work leaves open the crucial scientific question of whether real similarities cause improved outcomes in real relationships. Certainly, the preponderance of this experimental and correlational evidence, generalized across so many types of similarities—including ones that seem especially unimportant—suggests that this causal association should exist. However, without direct experimental evidence, some doubt remains.

A second gap in our knowledge is particularly salient for educational practitioners. Without some way to leverage real similarities between individuals within a classroom, the associations between similarity and relationship outcomes have limited practical applications. Car salespeople may be well-served by suggesting that they too enjoy camping, golf, or tennis if they notice tents, clubs, or rackets in the trunk of your car (Cialdini, 2009). However, teachers who lie about what they share in common with individual students will likely be found out over the course of an ongoing relationship (to say nothing of the ethically dubious nature of this tactic). One could argue that teachers might leverage similarity by learning what students have in common with each other and assigning them to collaborative groups with like-minded classmates. However, it seems important for schools to socialize students to work effectively with those from different backgrounds. In summary, as compelling and robust as the similarity-relationship research is, important scientific and applied gaps plague our understanding of and ability to leverage these associations.

Teacher–Student Relationships and Student Outcomes

In addition to healthy relationships as an important outcome in their own right (Leary, 2010), TSRs matter because they are associated with a broad array of valued student outcomes including: academic achievement, affect, behavior, and motivation. As McCombs (2014) concludes from a series of studies she conducted, "What counts and what leads to positive growth and development from prekindergarten to Grade 12 and beyond is caring relationships and supportive learning rigour" (p. 264).

Many studies have shown that students with better TSRs tend to achieve more highly in school (Cornelius-White, 2007; Roorda, Koomen, Split, & Oort, 2011). For example, Wentzel (2002) found that middle-school students' perceptions of their teachers on rela-

tional dimensions such as fairness and holding high expectations predicted students' end-of-year grades. Estimated effect sizes of TSRs on achievement range from $r = .13$ to $.28$[1] for positive relationships at the secondary level (Roorda et al., 2011).

With respect to students' affect toward school, students in classes with more supportive middle school teachers have more positive attitudes toward school (Roeser, Midgley, & Urdan, 1996; Ryan, Stiller, & Lynch, 1994) and their subject matter (Midgley, Feldlaufer, & Eccles, 1989). Conversely, middle school students who lack a bond with their teacher are more likely to disengage or feel alienated from school (Murdock, 1999). Cornelius-White's (2007) meta-analysis showed that TSRs were correlated with students' satisfaction with school ($r = .44$).[2]

Associations between TSRs and students' behavior include findings that middle school students more willingly pay attention in class when they think their teacher cares more (Wentzel, 1997). On the other hand, adolescents who perceived more disinterest and/or criticism from their teachers were more likely to cause discipline problems (Murdock, 1999). Cornelius-White's (2007) findings show that more positive student perceptions of their TSRs corresponded with increased student participation ($r = .55$) and attendance ($r = .25$), and decreased disruptive behavior ($r = .25$).

Studies of TSRs and student motivation follow similar patterns. Adolescents' perceptions of teacher support and caring predict student effort as reported by both teachers (Goodenow, 1993; Murdock & Miller, 2003) and students (Sakiz, Pape, & Hoy, 2012; Wentzel, 1997). Meta-analyses (Cornelius-White, 2007; Roorda et al., 2011) show that TSRs are associated with motivation ($r = .32$) and secondary school engagement ($r = .30$ to $.45$).

Of this array of important outcomes, we chose to focus on students' classroom grades. Among the associations between TSRs and these outcomes, we felt grades were (arguably) the most consequential for students' futures—potentially affecting advancement/retention decisions, tracking, graduation, college placement, and additional, important outcomes.

Scientific Context of the Study

In striving to contribute to the scientific theories linking similarity and relationships, we structured the study to learn whether the causal associations between similarity and relationships found in laboratory studies generalized to real, ongoing relationships. Furthermore, if successful, our intervention would have important applications for classrooms. Specifically, it would offer a tangible example of how similarities might be leveraged to actually improve relationships in the classroom. Simultaneously, we hoped to evaluate the effects of our intervention as rigorously as possible in a naturalistic setting and to err on the side of being conservative in the inferences we made from our data.

We evaluated our intervention using a 2×2 design and focusing on multiple classrooms during a single class period. Through this design, each individual within every teacher–student dyad was randomly assigned to receive feedback (or not) from a "get-to-know-you" survey. Specifically, students were randomly assigned to either learn what they had in common with one of their teachers (i.e., students in the "student treatment" group), or not learn about similarities with their teacher (i.e., students in the "student control" group). Teachers found out what they had in common with about half of their students in the focal class (i.e., students in the "teacher treatment" group) but not with the other half (i.e., students in the "teacher control" group). Thus, all randomization occurred at the student level.

In the spirit of recent recommendations (Cumming, 2014; Simmons, Nelson, & Simonsohn, 2011), we identified six "prespecified hypotheses" before analyzing our data. Specifically, we expected that students in the student treatment group would (a) perceive themselves as more similar to their teachers and (b) report a more positive TSR compared with those in the student control group. For students in the teacher treatment, we expected that, (c) their teacher would perceive these students as more similar, (d) their teacher would rate their TSR more positively, and the students', (e) midquarter grade, and (f) final quarter grade would be higher than students in the teacher control group. As described in the Statement of Transparency in our online supplemental materials, we also collected additional variables and conducted further analyses that we treat as exploratory.

These main hypotheses reflect an underlying logic that by focusing teachers' and students' attention on what they have in common, we will change their perceptions of how similar they are to one another. Congruent with the aforementioned research on similarity, we expect these changed perceptions will lead to more positive relationships between teachers and students. In other words, the core social psychological theory that we are reinforced by our social interactions with similar others (Montoya et al., 2008), will generalize to the educational setting we studied. These more positive relationships, in turn, will cause other downstream benefits for students.

Two explanatory notes about these hypotheses are in order. First, we hypothesized that students' grades would be affected by the teacher treatment (but not the student treatment) based on previous correlational work. Brinkworth, McIntyre, Harris, and Gehlbach (manuscript under review) showed that when accounting for both teachers' and students' perceptions of their TSR, the teachers' perceptions (but not students' perceptions) of the TSR are associated with students' grades. Second, similar studies of brief interventions that have impacted students' grades have found that the effect of the intervention was concentrated within a subpopulation of students, such as Black students (Cohen, Garcia, Apfel, & Master, 2006; Walton & Cohen, 2011), Latino students (Sherman et al., 2013), or low self-efficacy students (Hulleman & Harackiewicz, 2009). However, in the absence of information about which subgroups might react most positively to the intervention, we made no predictions about potential subgroup effects of the intervention.

Method

Participants

We conducted the study at a large, suburban high school in the southwestern United States. We focused on ninth graders because they were just transitioning to high school and might particularly benefit from connecting with an adult in a school where they did not know any authority figures. The students in our final sample ($N = 315$) were 60% female, 51% White, 19% Latino, 11% Asian, 6% Black, and 10% reporting multiple categories or "other." These

[1] This range represents the lower and upper bounds of the confidence intervals across both the fixed and random effects models the authors used.

[2] Cornelius-White (2007) does not report elementary and secondary student results separately for his outcomes.

proportions of different races/ethnicities are similar to the school as a whole (54% White, 20% Latino, 13% Asian, and 10% Black). These students were mostly native English speakers (81%) and came from families where college graduation represented the median educational level of the mothers and fathers (though the range included mothers and fathers who had not attended elementary school to those who completed graduate school).

The teachers in our sample ($N = 25$) were 52% male, 80% White, and 92% native English speakers. These 25 teachers were part of a faculty of 170, 41 of whom taught ninth graders. The mean age of the teachers was 47.5 years old ($SD = 10.42$), and the mean years of experience was 18.0 ($SD = 9.5$). Most teachers (72%) had completed a graduate degree and came from families where 1 year of college represented the median educational level for both their mothers and fathers (though the range extended from those completing fourth grade to those who completed graduate school). Both teachers and students were blind to the purpose of the study.

Measures

Our main measures were borrowed from Gehlbach, Brinkworth, and Harris (2012). Students' perceptions of their degree of similarity to their teachers were assessed through a 6-item scale ($\alpha = .88$), which included items such as "How similar do you think your personality is compared to your teacher's?" Students' perceptions of their TSR were measured with a nine-item scale ($\alpha = .90$) that asked students to evaluate their overall relationship with their teachers, for example, "How much do you enjoy learning from [*teacher's name*]?" To minimize the burden on teachers, we asked them a single item to assess their perceptions of similarity to each student, "Overall, how similar do you think you and [*student's name*] are?" However, they did complete the full parallel 9-item teacher-form of the TSR scale ($\alpha = .86$ for teachers; see the online Appendix for a complete listing of these scales).

We collected midquarter and final quarter grades from student records. Because teachers at this high school have autonomy to decide on the most appropriate way to grade students, this measure represents a combination of homework, quizzes, and other assessments depending upon teachers' individual approaches and the subject matter they teach.

Our exploratory analyses used additional measures. Teachers rated the amount that they interacted with their students by answering, "Compared to your average student, how much have you interacted with [*student's name*] this marking period?" We also collected attendance and tardiness data and (eventually) end-of-semester grades from school records. These measures are listed in the online supplementary materials.

Procedure

The study unfolded over the course of the first marking period at the school. Just before the beginning of the school year, the principal helped our research team recruit as many ninth grade teachers as were interested in participating. In turn, during the first week of school these 27 consenting teachers helped us collect consent forms from their students. Throughout the following week of school, these students and teachers visited their computer lab and completed the initial get-to-know-you survey. We mailed our feedback forms to the school by the middle of the third week of classes. Students ($N = 315$) and 24 teachers then completed these forms over the course of the next 2 weeks. An additional teacher submitted her feedback sheet late (though her students completed their sheets on time); this teacher and her students were retained in the sample. Two teachers and their classes never completed the feedback forms, thereby reducing the final sample size to 315 students and 25 teachers. Midquarter progress grades were finalized at the end of the fifth week of classes. During the eighth and ninth weeks of classes, students and teachers took the follow-up survey. (Because teachers were allowed to take the survey on their own time, some teachers completed the follow up survey up to one month later.) The quarter concluded at the end of the 10th week of classes.

Students and teachers took the 28 item get-to-know-you survey during their first period class. The survey asked teachers and students what they thought the most important quality in a friend was, which class format is best for student learning, what they would do if the principal announced that they had a day off, which foreign languages they spoke, and so on (see Figure 1). From these surveys we composed the feedback sheets that comprised the core of the intervention.

On these feedback sheets, we listed either five things students had in common with their teacher (in the student treatment group[3]) or five commonalities the students shared with students at a school in another state (in the student control group). Each teacher received five items that they had in common with each student who was among those randomly selected into the teacher treatment group (i.e., half of the participating students from the teacher's first period class). Teachers were informed that in the interest of providing prompt feedback, we could not provide reports on their remaining first-period students (the teacher control group). The five similarities were chosen based on an approximate rank ordering of the similarities that had seemed to be most important for generating perceptions of similarity from the pilot test in the previous year (see the Statement of Transparency for more on the pilot test). Students and teachers responded to a series of brief questions on their feedback sheets such as, "Looking over the five things you have in common, please circle the one that is *most surprising to you*." Our hope was that by completing these questions on their feedback sheets, students and teachers would more deeply consider and better remember their points of commonality with one another. Current copies of the measures and materials are available from the first author upon request.

Results and Discussion

Prespecified Hypotheses

As detailed in our Statement of Transparency (see the online supplemental materials), we prespecified six hypotheses (Cumming, 2014). Specifically, we anticipated that (compared with those in the student control group) students in the student treatment group (a) would perceive more similarities and (b) a more positive TSR with their teacher. Compared with those in the teacher control group, we hypothesized that teachers would perceive students in

[3] We generated five similarities for all but one teacher–student pair—a dyad where only four similarities were present after matching their get to know you surveys. This dyad was retained in our analyses.

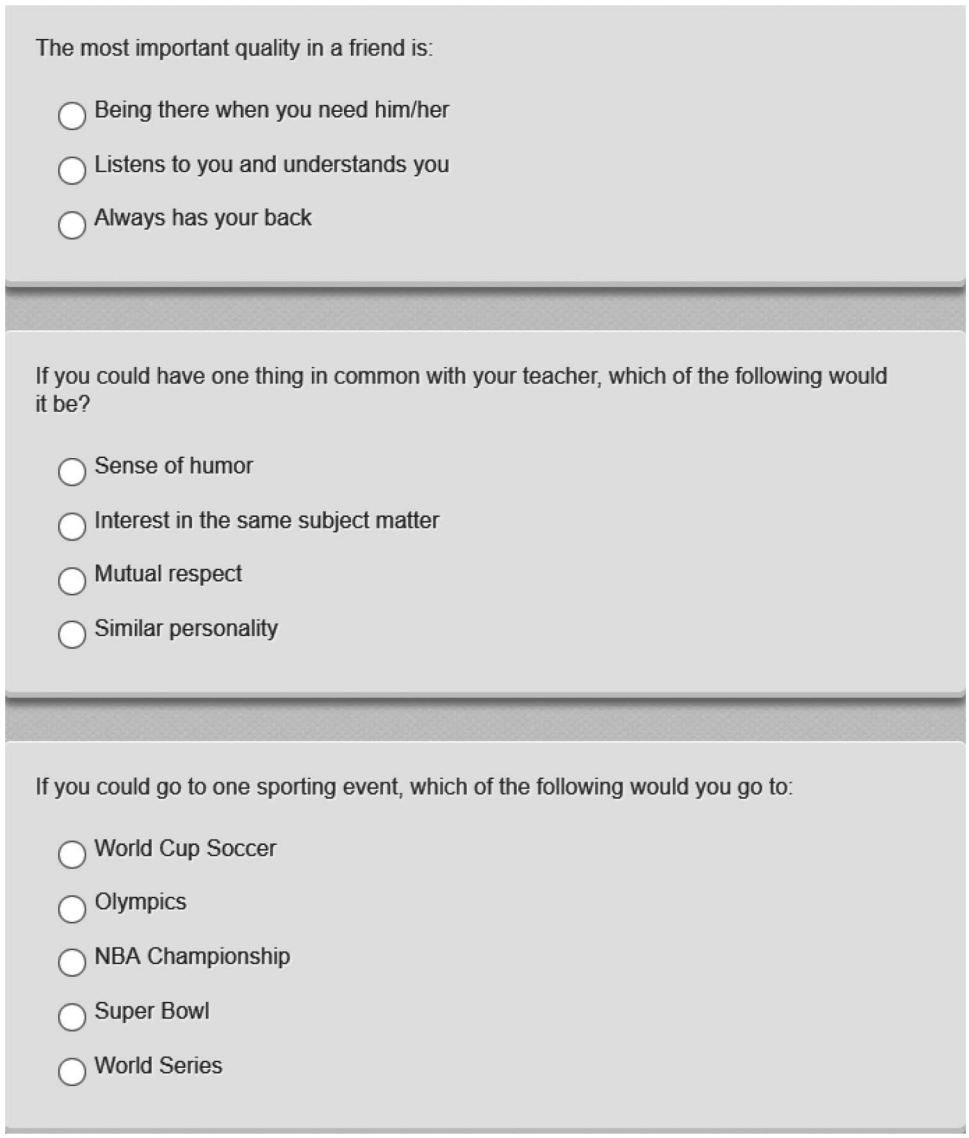

Figure 1. Screen shot of the get-to-know-you survey.

the teacher treatment group as (c) being more similar, and (d) teachers would develop a more positive TSR with these students. Finally, we expected that the students in the teacher treatment group would earn (e) higher midquarter and (f) higher end-of-quarter grades than their counterparts in the teacher control group.

As described in the Statement of Transparency, we expected to test these hypotheses through a combination of multilevel modeling (i.e., Hypotheses 3, 5, and 6 when the outcome was a single item) and multilevel structural equation modeling (i.e., Hypotheses 1, 2, and 4 when the outcome was a latent variable). However, our statistical consultant later advised us that the number of teachers (i.e., Level 2 clusters) was inadequate for M*plus* to provide trustworthy estimates for the models using latent variables. Our models for latent variables had more parameters to be estimated than clusters, making multilevel structural equation modeling impossible. Because of this nested structure of our data, we relied on mean- and variance-adjusted weighted least squares for complex survey data Weighted Lease Squares with Mean and Variance adjustment (WLSMV-complex) estimation, using the CLUSTER option in M*plus*. WLSMV-complex, which uses a variance correction procedure to account for clustered data, provides corrected *SE*s, confidence intervals (CIs), and coverage (Asparouhov, 2005). We used full information maximum likelihood to address missing data. The maximum proportion of missing data for any variable was .012. However, we used M*plus*' robust *SE* approach when our outcomes were latent. To evaluate each hypothesis, we regressed the outcome on the condition as described above. Because random assignment produced equivalent groups between both treatment groups and their respective control groups on key demographic characteristics (specifically gender, race, English language status, and parents' educational level), no covariates were used in these analyses. Consistent with Cumming's (2014) recommendation, we

evaluated our hypotheses using 95% CIs to emphasize the range of plausible values for the treatment effect rather than relying on *p* values. In addition, we report standardized β to provide an estimate of effect size (except for grade-related outcomes where the original 0 to 4.0 scale provides meaningful equivalents of an F through an A). We present descriptive statistics in Table 1.

Our results are congruent with the similarity hypotheses (i.e., Hypotheses 1 and 3). Each treatment made students and teachers feel more similar to one another by the end of the marking period (β = 0.33, *SE* = 0.12, CI [0.10, 0.56] for students; and β = 0.33, *SE* = 0.11, CI [0.11, 0.55] for teachers). In other words, we presume that the true standardized treatment effect fell within the range from .11 and .55 (and between .10 and .56 for students), while bearing in mind that the most plausible values are those closest to .33.

By contrast, the students perceived their TSRs to be relatively similar regardless of the condition to which they were assigned (β = 0.09, *SE* = 0.14, CI [−0.18, 0.36]). In other words, we found minimal support for Hypothesis 2. Within the teacher treatment, teachers perceived a more positive relationship with these students (β = 0.21, *SE* = 0.11, CI [0.00, 0.42]). For students in the teacher condition, we found no compelling support for an effect on midquarter grades (β = 0.04, *SE* = 0.10, CI [−0.15, 0.23]). Although the CI does include 0, our point estimate and the range of plausible responses suggests that students in the teacher condition probably earned higher end-of-quarter grades (β = 0.21, *SE* = 0.11, CI [0.00, 0.43]). Figures 1–4 in the online supplementary materials show how the unadjusted means are distributed when the teacher and student conditions are separated into their four unique groupings of the 2 × 2 design.

The first pair of findings shows that the intervention successfully enhanced teachers' and students' perceptions of similarity. On the one hand, the effects do not seem particularly potent—perhaps reflecting only a mildly to moderately strong intervention. On the other hand, students processed their feedback sheets for approximately 15 min before handing them back in, and yet, still perceived themselves as being more similar to their teacher over a month later. Teachers presumably spent even less time on each feedback sheet given that most teachers had several to complete. Thus, while one might argue that the effects of the intervention were weak, this interpretation should be calibrated against the brevity of the intervention and the amount of time that elapsed before the outcomes were collected (Cumming, 2014).

Although the intervention appeared to improve teachers' perceptions of their relationships with students, we do not find compelling evidence that the intervention improved TSRs from students' perspectives. To the extent that this result reflects a genuine difference in the effect of the intervention, one plausible explanation is that teachers view part of their role as needing to foster positive relationships with students. Thus, they are motivated to perceive students whom they view as similar in a positive light. By contrast, students may not feel any particular obligation to form a positive relationship with their teachers. Learning that they share common ground with their teacher may not change their perception of their teacher because ninth grade students typically have no particular motivation to cultivate this social relationship.

Our findings for students' academic achievement seem paradoxical: the intervention appears to show positive effects at the end of the quarter after finding no effects half-way through the marking period. However, we think this apparent paradox results from a logistical issue rather than a finding of substantive interest. In an unfortunate oversight, we finalized our prespecified hypotheses before reviewing the timing of each key aspect of the study. Although the direction of the estimate for students' midquarter grades is the same as the end-of-quarter grades, we suspect that the intervention occurred too close to teachers' grade-submission deadline to have a meaningful effect in most classes. In other words, students may not have had a sufficient opportunity to do enough graded work between the time that they (and their teachers) completed their feedback sheets and the date that midquarter grades were due. As a result, we do not discuss this outcome further. Students' performance on their final quarter grades, by contrast, suggests that the intervention probably caused students' grades to increase. Our point estimate of this increase corresponds to a little less than a fifth of a letter grade.

To better understand our initial pattern of results, we examined whether our intervention might have had differential effects on different subpopulations of students. By fitting a series of multilevel models (for observed outcomes) and models with robust *SE*s

Table 1
Descriptive Statistics for Key Variables in the Study (Unadjusted Mean, SD, and Pearson [r] Correlations)

Variable name	Mean	*SD*	Min.	Max.	Pearson correlations								
					1	2	3	4	5	6	7	8	9
(1) Students' similarity	2.68	0.73	1.00	4.17	—								
(2) Teachers' similarity	2.90	0.91	1.00	5.00	.13	—							
(3) Students' TSR	3.68	0.68	1.00	5.00	.69	.18	—						
(4) Teachers' TSR	3.85	0.55	2.22	5.00	.29	.63	.32	—					
(5) Midquarter grade	3.26	0.99	0.00	4.00	.34	.23	.35	.41	—				
(6) End-of-quarter grade	3.16	1.10	0.00	4.00	.30	.18	.35	.43	.76	—			
(7) Semester grade	2.79	1.11	0.00	4.00	.24	.31	.28	.47	.67	.79	—		
(8) Tardies	0.26	0.66	0.00	9.00	−.13	−.01	−.08	−.05	−.20	−.22	−.13	—	
(9) Absences	1.29	1.61	0.00	5.00	−.15	−.08	−.06	−.16	−.20	−.15	−.10	.15	—
(10) Teacher reported interactions	4.74	1.10	2.00	7.00	.21	.37	.17	.46	.16	.22	.21	−.11	−.10

Note. TSR = teacher–student relationship. *N*s ranged from 275–362.
Correlations are unadjusted for the nesting of students within classrooms. Approximate significance levels are as follows: for |*r*s| ranging from 0 to .12, $p = ns$; for |*r*s| ranging from .13 to .16, $p < .05$; for |*r*s| ranging from .17 to .20, $p < .01$; for |*r*s| .21 and greater, $p < .001$.

(for latent outcomes) in M*plus*, we conducted a series of exploratory analyses on different student subgroups.

Exploratory Analyses

A number of previous studies that use relatively brief, social psychological interventions (Cohen et al., 2006; Hulleman & Harackiewicz, 2009; Sherman et al., 2013; Walton & Cohen, 2011) suggest that certain subgroups of students often benefit disproportionately from the interventions. Specifically, we thought that the school might serve some students better than others, or that there might be a dominant culture at the school that was more inclusive of some students than others. After speaking with the principal about this possibility, he suggested that the White and Asian students were typically well-served by the school, whereas Black and Latino students typically faced more challenging circumstances at home, at school, and throughout their community. Thus, we reexamined our data by analyzing the White and Asian students as a separate group from the remaining "underserved" students. Because these are exploratory analyses, we do not retain the same level of confidence in these findings as our prespecified hypotheses. However, we argue that these results are likely to be instructive for generating future hypotheses (Cumming, 2014).

When fitting our models, we found little evidence for any effects of the intervention on the White and Asian students. We find no particularly compelling evidence that White and Asian students in the student treatment group perceived different levels of similarity with their teachers ($\beta = 0.17$, $SE = 0.15$, CI [−0.13, 0.46]) or felt their relationships to be different ($\beta = -0.12$, $SE = 0.17$, CI [−0.46, 0.21]) compared with those in the student control group. We find a comparable lack of evidence that the intervention affected teachers' perceptions of their similarity to their White and Asian students ($\beta = 0.11$, $SE = 0.16$, CI [−0.20, 0.41]) and teachers' perceptions of their relationships with these students ($\beta = 0.00$, $SE = 0.15$, CI [−0.29, 0.29]). Finally, we find no evidence that the intervention affected White and Asian students' end-of-quarter grades ($\beta = -0.01$, $SE = 0.15$, CI [−0.29, 0.27]).

For the underserved students, the story differed. Underserved students who received feedback about commonalities with their teachers felt much more similar to their teachers ($\beta = 0.56$, $SE = 0.20$, CI [0.18, 0.96]) than their counterparts who did not receive this feedback. It was less clear whether these students felt more positive about their relationships with their teachers ($\beta = 0.39$, $SE = 0.24$, CI [−0.08, 0.86]), though the estimated effect size was moderate and in the expected direction. When teachers received feedback about similarities with their underserved students, they perceived greater levels of similarity with those students compared with their control counterparts ($\beta = 0.56$, $SE = 0.24$, CI [0.08, 1.04]). Similar to the underserved students, it was unclear whether teachers in the treatment group felt more positive about their TSRs with these students ($\beta = 0.43$, $SE = 0.27$, CI [−0.11, 0.96]). Finally, we found some evidence that underserved students' end-of-quarter grades ($\beta = 0.36$, $SE = 0.20$, CI [−0.04, 0.75]) were most likely higher when their teacher received feedback about their commonalities compared with students in the teacher control condition, although the confidence interval does include 0. As depicted in Figure 2, the point estimate for this difference translates into about .4 of a letter grade on a 4.0 scale and corresponds to the difference between a C+/B− versus a B.

Assuming the point estimate approximates the true value of the treatment effect, these effects on grades are substantial. If we compare the White and Asian students with the underserved students in Figure 2, we can estimate the achievement gap between well-served and underserved ninth graders at this school to be approximately .6 of a letter grade. When teachers learned about the similarities that they shared with their underserved students, the achievement gap was reduced by two-thirds to only .2 of a letter grade. This reduction is in line with other relatively brief interventions that have closed the achievement gap. For example, Cohen et al. (2006) report a 40% closure with an even briefer intervention; Walton and Cohen (2011) report a 52% to 79% reduction (depending upon the time period examined) from their more intensive intervention.

Given the potential importance of these differences, we carried out two final sets of analyses. First, to see the extent to which these results persisted over time, we obtained students' grades in their focal class for the full semester. These analyses showed that the effects of the intervention on the underserved students trended in the same direction as the results for students' end-of-quarter grades ($\beta = 0.33$, $SE = 0.22$, CI [−0.11, 0.77]).

Second, in anticipation of trying to understand more about the effect of the intervention, we tested whether the intervention appeared to affect other variables we had collected. In particular, we examined attendance and tardiness data from school records and how much teachers reported interacting with each student compared with the average student. The results from these analyses suggest that the intervention did not affect students' attendance in their focal class (see Figures 4a and 4b in the online supplemental materials). However, the previously noted subgroup differences emerged in how much teachers reported interacting with their students. Specifically, we found no differences by condition in how much teachers interacted with their White and Asian students ($\beta = -0.13$, $SE = 0.16$, CI [−0.43, 0.17]), but they interacted more with those underserved students who were in the teacher treatment condition ($\beta = 0.43$, $SE = 0.16$, CI [0.12, 0.74]).

Conclusion

Our study builds on the robust social psychological research showing that similarity fosters liking and more positive relationships. By experimentally manipulating teachers' and students' perceptions of actual similarities, our study allows for causal inferences to be made about the effects of similarity on real-world, ongoing relationships. Results from our prespecified hypotheses suggest that the intervention alters students' and teachers' perceptions of how much they have in common, benefits TSRs (at least from the teacher's perspective), and likely bolsters students' classroom grades.

A primary theoretical contribution of this work is the demonstration that the causal association between similarity and relationship outcomes found in numerous laboratory studies can generalize to real-life relationships. However, the potential of this intervention to generate broad impact in classrooms is every bit as important. If this approach of connecting students and teachers fosters more positive TSRs (even if the effects are primarily teachers' perceptions of their relationships with certain students), it represents a relatively quick and easy way to improve an important outcome. In addition, if future studies replicate the narrowing of the achievement gap found in this sample, this

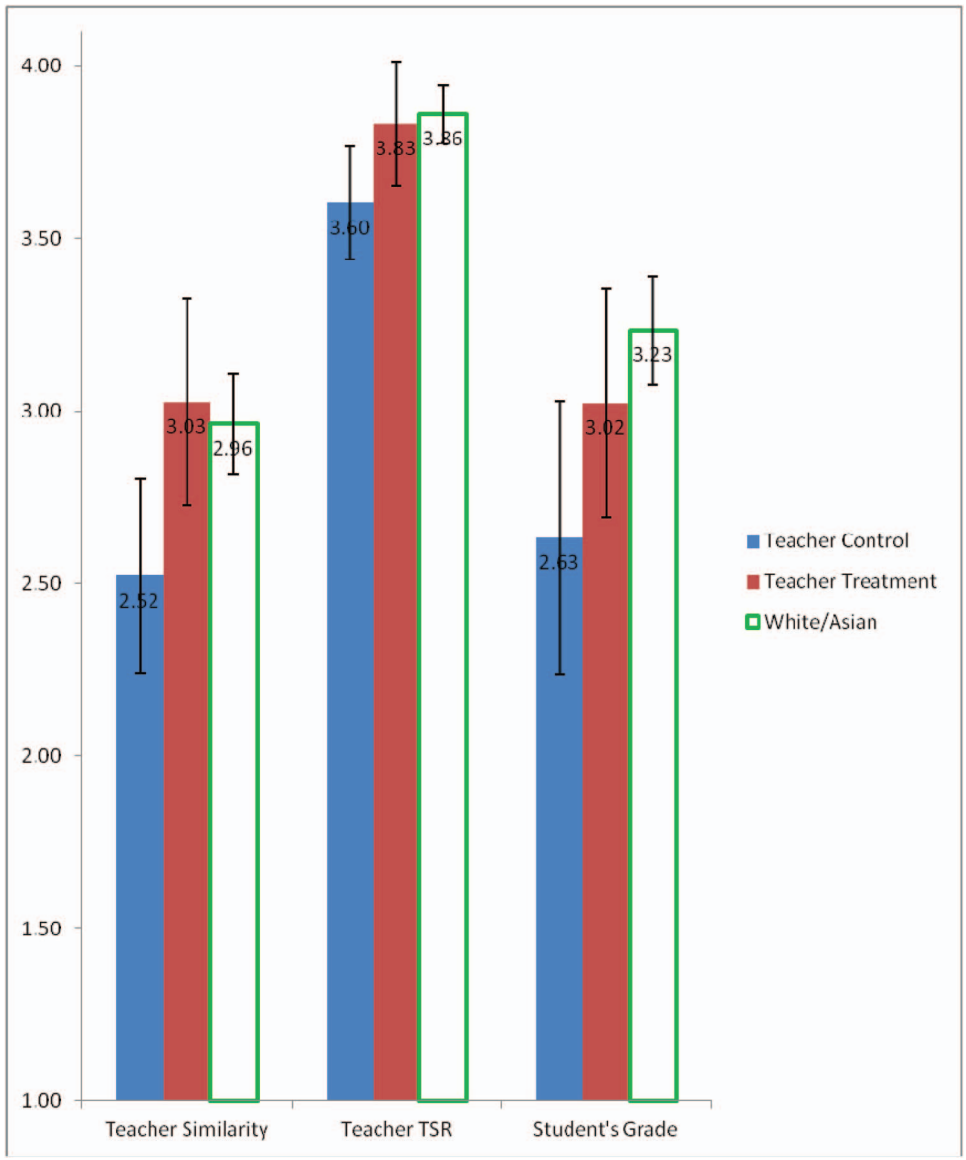

Figure 2. Mean differences and 95% confidence intervals for underserved students by teacher condition in teachers' perceptions of similarity, perception of their teacher–student relationships (TSR), and students' end-of-quarter grades in their focal class. Means for White and Asian students are presented for comparison. The 65% reduction in the achievement gap shown in the right-hand triad of bars corresponds to the difference between less than a B- to a B. See the online article for the color version of this figure.

intervention would be particularly "scaleable" from a policy perspective.

Like any study, ours includes a number of limitations that warrant readers' attention. First, the implementation of the various steps of the intervention was imperfect (e.g., a teacher failing to complete the feedback sheets on time, two other teachers responding to the final survey late, etc.). We hope that future studies can remedy these problems and design systems to administer the intervention consistently. However, we also note that implementation of all manner of interventions (new curricula, disciplinary systems, Web portals for parents, and so on) in schools tend to be imperfect. The fact that our intervention was largely effective despite the flaws in execution is an important footnote for practitioners.

Second, our analyses (particularly the exploratory analyses) lacked the statistical power we desired. This caused us to shift to a different statistical approach than the one we had originally planned in our statement of transparency. Our statistical consultant also noted that the multilevel model and clustered *SE* approaches we used, may still result in too many Type-I errors when the number of clusters is small, that is, fewer than 50 (see, e.g., Bertrand, Duflo, & Mullainathan, 2004; Donald & Lang, 2007). To address this potential limitation, we used a wild cluster bootstrap-t (Cameron, Gelbach, & Miller, 2008). As shown in Table 1 in the

appendix, our findings using that approach were generally consistent with those we obtained from our multilevel model and robust *SE* models. Particularly given the emerging hypothesis that the effectiveness of the intervention may be localized to underserved students, future replications should try to obtain substantially larger samples with more clusters across a variety of schools to better evaluate this possibility.

Third, our exploratory findings suggested differences between well-served (White and Asian) and underserved (primarily Black and Latino) students. However, this division of students may mask a more accurate understanding of what moderates the effects of the intervention. For example, we lacked a nuanced measure of socioeconomic status in our data set. Given the correspondence between race and socioeconomic status in this country, we may have actually detected a moderating effect of socioeconomic status that our data masked as a race-based effect. Thus, future studies that can collect a wider array of more precise demographic measures would also be particularly beneficial.

Fourth, the underlying logic of our study describes a story of mediation. Specifically, the effect of our similarity intervention on students' grades may be mediated by teachers' perceptions of their relationships with students. However, recent work has sharpened our understanding of mediation. Proving mediation is a difficult and ongoing journey rather than a succinct set of equations (Bullock, Green, & Ha, 2010) that establish a particular variable as a mediator. Thus, we can only say that our data largely cohere with this mediation story; we do not (and cannot) establish mediation per se within a single study. In the same way that race may be masking a socioeconomic effect that we do not have good enough measures to detect, variables we did not measure may be the fundamental mediators between this intervention and our outcomes. Future research that provides data on other potential mediators (e.g., those not assessed in this study) will also prove tremendously helpful.

Other key future directions emerge out of the results themselves. First, the teacher treatment seemed to yield a greater effect on our outcomes than the student treatment. When teachers learned what they had in common with their students, they felt they had more in common with those students, perceived better relationships with them, and those students seem to have better grades. Although more speculative, it appears that the teacher treatment may primarily affect the underserved students. Thus, one set of future studies might investigate whether the effects of the intervention are really concentrated on teachers and underserved students, or whether this finding varies by context or population. Other studies could investigate whether the intervention might be adapted to improve students' perceptions of the relationship or to make it effective for all students rather than just a subset of students. Additional research might investigate the role of teachers' race and/or the congruence between students' and teachers' race on the effectiveness of the intervention.

Second, although consequential for students' futures, grades have limitations as a key outcome variable. Specifically, they leave substantial ambiguity as to why the effects of the intervention occur—a question that will be especially important for future studies to address. One potential explanation is rooted in interactions. Many teachers may see it as a part of their role to connect with students and form a positive working relationship. Knowing what they have in common with their students provides them with a lever through which they can begin developing this relationship. For a group of predominantly White teachers, learning what they have in common with their underserved students may be critically important. Indeed, we find that teachers report interacting with these students more frequently. From this knowledge and the increased interactions, teachers may connect better with students at an interpersonal level and may be better equipped to connect their subject matter to students' interests. If this scenario transpires, greater learning seems a likely consequence. By contrast, ninth graders (regardless of race) may have little interest in connecting with their teachers or having any more interactions than necessary. They might be much more focused on connecting with their peers during this developmental stage. As a result, the students in this treatment group may find few effects of the intervention beyond greater perceived similarity with their teacher.

An alternative explanation is rooted in perceptual biases. Perhaps teachers typically perceive their students—particularly their underserved students—in stereotypical fashion. However, when they realize several domains in which they share some common ground with these students, the teachers perceive their relationships with these students in a new way—more like members of their own in-group (Hewstone, Rubin, & Willis, 2002). A potential consequence is that teachers might assign these students higher grades as a consequence of perceiving them differently.

Our exploratory analyses suggest that the possibility of perceptual biases will also be an important, challenging area of future investigation. On the one hand, we might expect students, who are welcomed into a classroom where the teacher more frequently interacts with them in positive ways, to attend class more regularly and arrive on time more often. Although we did not find much evidence congruent with this conjecture, there are many factors that affect a student's presence in class.

On the other hand, the perceptual bias story may not be completely congruent with the finding that teachers report interacting more frequently with students in the teacher treatment condition than with their control group peers. In other words, if teachers interact with these students more frequently, then the higher grades may partly be a function of learning. Thus, research that can begin to shed light on the mechanisms—be they teacher–student interactions, teacher perceptions, a combination of both, or other factors—through which this intervention affects these important outcomes of TSRs and grades will be especially fruitful.

In closing, this study shows that (perceptions of) real similarities can be influenced by a brief intervention that affects real relationships in a consequential setting like a high school. Our findings suggest that the improvements in TSRs may, in turn, cause downstream benefits for students' grades. Finally, these results generate strong hypotheses that similar interventions in the future may be effective in helping to close achievement gaps between subgroups of students.

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