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WHO BENEFITS MOST FROM A SAME-RACE MENTOR? EVIDENCE FROM A NATIONWIDE YOUTH MENTORING PROGRAM

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WHO BENEFITS MOST FROM A SAME-RACE MENTOR? EVIDENCE FROM A NATIONWIDE YOUTH MENTORING PROGRAM [*]

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ABSTRACT:

We identify the impacts of assigning a mentor of the same race or ethnicity on the social, emotional and academic development of youth relative to assigning a mentor of a different race or ethnicity. Using the universe of matches from a nationwide youth mentoring program, we document that a rich set of pre-match observables are balanced across same-race/ethnicity match status. We find that Black and Hispanic youth assigned a same-race/ethnicity mentor had slightly faster growth in self-perceived school ability and attitudes toward risky behaviors after twelve months of mentoring, relative to cross-race matches. On the other hand, cross-race matched Hispanic youth had improvements in course grades and cross-race matched Black youth were more likely to report having a "special adult" in their life. In contrast to previous work on race-matching, we do not find improvements in grades or expectations for future educational attainment. Given that racial/ethnic minority mentors are often in short supply, these results imply matching on race or ethnicity at the expense of other desirable traits is not necessarily optimal.

JEL Classification: H51, I13, J26
Keywords: youth mentoring, race congruence, matching, child development

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1 — INTRODUCTION

In 2014, an estimated 4.5 million youth in the U.S. were in structured, one-to-one mentoring relationships (Bruce and Bridgeland 2014). Youth mentoring programs are held in high regard for their positive impacts; mentored youth have better social-emotional skills (Grossman and Tierney 1998), have higher high school completion rates and are more likely to enroll in post-secondary education relative to non-mentored youth (Rodríguez-Planas 2012, Falk et al. 2020), which have long-term impacts that differ by race (Chetty et al. 2019).[1] The bond between mentor and mentee is thought to be stronger when the pair share a racial or ethnic identity (Sanchez and Colón 2005), and mentoring organizations often prioritize racial/ethnic-congruency when assigning a mentor (Rhodes et al. 2002). While the benefits of race-matching have been well-documented in the classroom (Egalite et al. 2015, Dee 2004, Harbatkin 2021), the effects of assigning a same-race or ethnicity social mentor on youth outcomes is less clear.

To answer this question, we estimate the impact of assigning a mentor of the same race or ethnicity (hereafter referred to as "same-race" for brevity) on the social, emotional and academic outcomes using the universe of youth participating in a large, nationally available mentoring program during 2010-2018. The program supported over 135,000 one-on-one, social mentoring relationships in 2019 across 200 local agencies in all 50 states. The program is intended to develop the social-emotional skills of youth by pairing an adult mentor who can role model positive non-cognitive skills which influence later life economic outcomes (Heckman and Rubinstein 2001, Heckman and Karapakula 2019). Administrators and staff often prioritize racial or ethnic matching when choosing which mentor to assign youth in hopes of improving the length, quality and impacts of the relationship. But there is a mismatch in the supply of racial or ethnic minority youth and mentors: in 2018, 72% of youth in the program were racial/ethnic minorities, compared to only 32% of mentors.[2] As a result, youth mentoring organizations must choose how to allocate a scarce supply of racial or ethnic minority mentors to mentees. Therefore, it is important to understand which outcomes are most improved by racial or ethnic matching, and for which youth this premium is largest.

[1] DuBois et al. (2011) provide a thorough review of the literature evaluating youth mentoring programs.
[2] Based on authors' calculations.

It is not theoretically clear that same-race matching should be preferred to cross-race matching. Rhodes et al. (2002) provide a detailed explanation of the potential social and cultural costs and benefits of both same- and cross-race mentoring. The authors hypothesize that having a mentor who shares a racial or ethnic background can promote trust and aid in establishing a relationship. Furthermore, accomplished role models of a similar background may empower youth to achieve higher levels of success themselves, such as pursuing a college education. On the other hand, cross-race matches may bridge cultural and social gaps and challenge cultural beliefs, or foster a sense of community between racial groups. Furthermore, when same-race mentors are in short supply, a cross-race match may be better for the youth than no match at all.

Indeed, the literature on race-matching in youth mentorship has found a range of impacts. Rhodes et al. (2002) find that boys had declines in scholastic competence and self-worth relative to non-mentored boys, but that same-race matches experienced less of a decline than cross-race matches. Girls in same-race matches likewise reported slower declines in their perceptions of the value of school and self-worth than cross-race matched girls. However, the authors also found several benefits to cross-race matches. Youth were less likely to report initiating alcohol when placed in cross-race matches. In addition, youth in cross-race relationships reported that they were more likely to talk to their mentors when distressed and more often described their mentor as providing unconditional support. Finally, parents of youth in cross-race matches were more likely to report the relationship improved their child's peer relationships, that the mentor tried to build on the youth's strengths, and that the mentor took their child places they wanted to go.

In our study, the observed variation in same-race match status comes from two sources. First, there is variation across local agencies in the relative supply of racial/ethnic minority mentors. This variation is likely correlated with neighborhood characteristics, such as school quality, that could directly influence youth outcomes. Second, there is variation within agency, in which case managers could potentially allocate youth with the greatest growth potential to same-race matches. Key to our analysis, we show that a rich set of baseline outcomes and pre-match characteristics of youth and mentors are similar between same- and cross-race matches. This substantially reduces the concern that our estimates are biased by either source of potential unobserved heterogeneity.

Our results indicate that assigning a same-race mentor improves outcomes for some youth.

Black and Hispanic youth in same-race matches experience small but statistically significant improvements in their attitudes toward risky behaviors relative to their cross-race counterparts, and Black youth had higher self-perceived scholastic ability. On the other hand, Hispanic youth in cross-race matches had higher grades in reading, social studies and science, and Black youth were more likely to report having a "special adult"[3] in their lives after a year of cross-race mentoring. Race-matching increases the length of the match for White and Hispanic youth but does not improve the quality of match as measured by the case manager. Lastly, same-race mentors assigned to Black and Hispanic youth are less likely to end the match by moving away, but more likely to lose contact with their mentee.

Our findings support the theory that having a same-race mentor improves the self-esteem and confidence of certain youth, perhaps by sharing a background that creates a stronger relationship. But there are also benefits to cross-race mentoring that may complement the areas where same-race mentoring does not impact youth, such as course grades. These results imply that race-matching is an important dimension for youth mentoring organizations to consider when targeting the social and emotional development of racial/ethnic minority youth or the length of a match. However, when the supply of racial/ethnic minority mentors is scarce, matching on race at the expense of other important traits may not produce the fastest growth among other outcomes. It may even be the case that cross-race mentoring has higher marginal benefits than same-race mentoring for some youth.

We add to the literature on youth mentoring by estimating the effect of race-matching on the outcomes of all youth participating in the program during 2010-2018. Grossman and Tierney (1998) and Herrera et al. (2011) randomly assigned youth to receive mentorship from the Big Brothers Big Sisters (BBBS) program during 1991-1993 and 2004 and found mentored youth had improved self-perceptions of scholastic ability, attitudes toward risky behaviors, and self-esteem relative to youth in the comparison group. In both studies, the sample sizes of about 1,100 youth are small relative to the overall scale of BBBS and neither examine the impacts by the type of mentor assigned. Rhodes et al. (2002) use the experimental variation in Grossman and Tierney (1998) to estimate the effect of race-matching, but the race of the mentor was not randomized among the treated group. Hence, it is not clear if same-race mentoring increases

[3] Defined in the survey as "a non-guardian adult who [youths] often spend time with."

youth development at a faster rate than cross-race mentoring or improves outcomes relative to non-mentored youth. Furthermore, the effects of race-matching may be quite different today as discussions of the importance of racial diversity have become more frequent in public discourse. We extend this literature by examining the universe of youth participants in a national youth mentoring program across a wider and more recent time frame and provide quasi-experimental evidence of the impacts of race-matching that accounts for the non-random assignment of racial or ethnic minority mentors.

We also contribute to the literature on racial congruence by isolating the so-called "passive effects" of an adult mentor on the youth's self-perceived social, emotional and academic abilities. Much of the existing work focuses on educational contexts, particularly K-12 schooling, where student outcomes are affected through "active teacher effects" and "passive teacher effects" as described in Harbatkin (2021). In the first case, teachers may leverage their evaluative authority—either consciously or unconsciously—to assign lower grades or dole out harsher punishments for misbehavior to students of a different race (Bates and Glick 2013, Dee 2005, Ouazad 2014). At the same time, teachers may have a passive, non-evaluative influence on the student by appealing to their motivations and self-confidence in daily interactions, which may have differential impacts by racial congruence (Van Ewijk 2011). In the youth mentoring program we study, the adult mentors have little formal authority or evaluative power over the youth, allowing us to attribute the estimated effects to the passive, non-evaluative channel. In contrast, studies that examine the effect of same-race teacher assignment on test scores (Harbatkin 2021, Egalite et al. 2015, Dee 2004) or career path choice (Kofoed and McGovney 2019) cannot disentangle the passive effects from the active effects of race-matching.

2 — Background

The youth mentoring organization we study operates a volunteer-based, one-on-one youth mentoring program that pairs a youth mentee and an adult mentor. With over 200 local agencies across all 50 states, the program is accessible in most areas and most youth who apply are eligible to participate. This program supported over 135,000 matches in 2019, making it one of the largest youth mentoring programs in the U.S. Within the organization, matches are either "site-based"

(SB) or "community-based" (CB) mentoring. SB matches are typically organized at a specific location—such as a workplace, school, or community center—and tend to have more structure imposed on them by the local agency. In contrast, CB matches spend time in their community doing activities like playing games at a park, attending a sporting event, or visiting a museum. Mentors are expected to become a role model for their youth by consistently spending time together (e.g. 3-4 times a month) and are encouraged to pick activities that foster a friendship with their youth. The program does not expect mentors to invest large amounts of money in the youth or spend time tutoring. Rather, the goal for the relationship is to inspire the youth through positive interactions and "quality time."

The matching process begins with potential mentors and youth applying for the program at their local agency. Youth are typically between the ages of 8 and 13 when entering the program, and can stay enrolled through age 18. Youth (or their guardians) submit an application that includes basic demographic information as well as information on their preferences for a mentor. Once they have applied, an assigned case manager administers a baseline survey that covers the youth's attitudes towards school, relationships and risky behaviors. Mentors are usually 21 years or older, must pass a background check, and must complete an in-person interview with their case manager. After the interview is complete, the adult enters the pool of available mentors. Mentors are assigned to a youth by the case manager based on a variety of criteria that can include gender, race, shared interests or background, travel time between the two, and the preferences stated by either party. Once assigned, the mentor and youth meet and mutually agree to form the match. If the match is successful, the local agency administers a follow-up survey identical to the baseline survey every twelve months the match survives. Either party may end the match at any time, though the case manager provides continual support to avoid early terminations. In our sample (described in detail in Section 3), matches typically last for 2-3 years (mean: 34.93 months, median: 29.20 months).

It is common for youth and their parent(s) to request a mentor of the same gender, and many request a mentor of the same race.[4] However, the youth's preferences are constrained by the available supply of mentors of a given race or gender. Figure 1 shows that, though almost

[4]We assume youth reveal their true preferences since requesting a mentor with certain qualities is costless to the youth and does not affect the probability of being matched overall.

all matches are same-gender, a majority of mentors in our sample of matches are White (69%) while a majority of youth are Black or Hispanic (66%). In addition to balancing preferences, case managers face a limited supply of racial or ethnic minority mentors. Figure 2 shows the fraction of mentors and youth participating in a match within a local agency, separated by race. A point above the plotted 45 degree line implies mentors of that race are relatively overrepresented at their agency compared to youth, while a point below the diagonal line implies underrepresentation and a point on the line shows that the proportions of mentors and youth of a particular race are balanced. Figure 2 shows that in our sample, often a majority share of mentors are White while a majority share of youth are Black or Hispanic within a given agency.[5]

3 — DATA

The data contain the universe of matches between 2010 and 2018. We observe information on the race, gender and age of every mentor and youth that participated in the program. Also included are measures of the youth's socioeconomic status (number and type of guardians in the household, on free/reduced lunch) and the mentor's educational attainment. Most importantly, the data also include a survey which is administered to every youth upon entering the program (hereafter referred to as the baseline survey) and then again every twelve months that the match continues (hereafter referred to as the follow-up survey). The survey includes 33 Likert scale questions ranging from 1-4 regarding the youth's perception of social relationships, their abilities and performance in school, and their plans for future educational attainment. These 33 questions are aggregated into eight summary scores by taking the average of the components. The eight summary measures are social acceptance, school ability, attendance, grades, education expectations, risk attitudes, parental trust, and special adult. We further group these eight measures into three topic areas: school experience, education and social experience.

The school experience group is comprised of social acceptance, school ability, and truancy. The social acceptance score is derived using the social competence subscale from the Perceived Competence Scale for Children (PCSC, Harter (1982)) which measures the youth's perceptions of

[5] Anecdotally, the director of the local agency informed us that the supply of mentors is often mismatched with the supply of youth: most mentor applicants are White and/or female and most youth applicants are racial or ethnic minorities and/or male.

their friendships (e.g. "I have a lot of friends."). The school ability score is a shortened version of the cognitive competence module from the PCSC and measures the youth's self-perceived academic ability (e.g. "I am very good at my schoolwork", and "I feel that I am just as smart as other kids"). Attendance is the average days late to school and absent as reported by the youth.[6]

The education group contains two outcomes: grades and education expectations. Grades is the average letter grade (mapped from F-A to 1-5 correspondingly) the youth received in mathematics, reading or language arts, social studies, and science. Education expectations contains three questions regarding their prospective educational attainment: how likely they are to (1) finish high school, (2) go to college, and (3) finish college.

Lastly, the social experience group includes risk attitudes, parental trust, and special adult. Risk attitudes was adapted from the Peer Pressure Inventory (PPI) developed by Brown et al. (1986) and measures the youth's perceptions of whether certain risky behaviors are acceptable among kids their age. These behaviors include using tobacco, drugs and alcohol, truancy, and misbehavior (e.g. hitting someone and breaking rules in school). Parental trust is an abbreviated version of the Inventory of Peer and Parental Attachment questionnaire (IPPA, Armsden and Greenberg (1987)) which measures the youth's perceptions of their relationship with their parents (e.g. "My parents respect my feelings."). Lastly, special adult is a single dummy indicating whether the youth feels that they have a non-guardian adult in their life who is a role model to them.

In almost all cases, youth experience growth in their outcomes on average. Table A1 shows the means of the eight outcomes were higher at first follow-up than their baseline values, except in the case of attendance and grades. These two were slightly lower after a year of mentoring, though the difference is not statistically significant. Of particular importance, the proportion of youth who identified a special adult in their life at follow up was approximately thirty percentage points higher than the baseline fraction. The definition of special adult describes a non-guardian mentor with whom the youth has a close connection, suggesting that the program is highly effective in assigning mentors who are able to bond with their youth. Although we cannot empirically test the growth in these outcomes against a counterfactual youth who did not receive mentoring, these changes over time suggest that the participant outcomes are trending upward during their first twelve months in the program.

[6] Frequencies are categorically binned 1-4, i.e. 1=no absences, 2=1-2 absences, etc.

We restrict our sample to the set of successful CB matches that lasted at least one year. Our analysis examines the change in youth outcomes at the first follow-up survey relative to their baseline survey. Hence, we omit any matches that do not have a baseline survey. This excludes all matches that began prior to 2010 since the case managers did not begin administering a baseline survey until that year. We exclude any matches that do not have at least one follow-up survey. It is possible that matches that did not last through the first follow-up survey differ in some important ways from those that did last. Table A2 tests for differences between matches that lasted through the first follow-up and those that did not. We do not find evidence that these matches differ in any important ways based on observable characteristics of either the mentor or the youth. Lastly, we consider only CB matches because the matching process for SB programs tend to vary more by state, SB matches tend to end earlier, and youth enrolled in SB programs were not balanced across same-race status. The resulting sample includes 29,532 matches. We do not observe any information on proposed matches that failed to form. Many agencies operate a waiting list for youth who were not able to be matched, so we assume that the probability of a youth rejecting a proposed match is low.

4 — METHODOLOGY

Because mentors are assigned to youth by a case manager, the variation in same-race status is non-random. However, agencies face shortages in the supply of racial or ethnic minority mentor applicants which restricts the case manager's ability to be overly selective when assigning a mentor to the youth. In our sample, often a majority of mentors are White while a majority of youth are Black or Hispanic (Figure 1). This within-sample stylized fact coincides with the experience of staff we spoke to: the pool of mentor applicants is predominantly White while the pool of youth applicants is predominantly racial or ethnic minorities.[7] Families often express preferences for a mentor who is the same gender and race as the youth. Additionally, they commonly ask to be matched with a mentor that shares interests or has a particular level of education. These requirements further restrict the pool of eligible mentors that fulfill the requests of the youth. Case managers must consider both the youth's preferences as well as characteristics

[7] Specifically, our contact at the local agency estimated that about 80% of mentors applicants were White, while 80% of youth applicants were racial or ethnic minorities.

that increase the quality of a match, such as geographic proximity. Figure 1 shows that there is significant variation in same-race status, despite the priority placed on race-matching.

These constraints faced by case managers when choosing a mentor to assign a youth motivate a selection-on-observables empirical strategy. Conditional on the demographics and socioeconomic status of the youth and mentor, the probability of case managers assigning a same-race and otherwise-eligible mentor is plausibly exogoneous. Table 1 shows the average values of the baseline survey and characteristics of youth and mentor by same-race status. The difference in most outcomes characteristics are small but statistically significant due to the large number of observations. For this reason, Imbens and Wooldridge (2009) recommend using a normalized difference in means test to account for statistically significant differences that arise simply due to large sample sizes. The authors suggest that normalized differences of less than 0.25 in absolute value indicate no significant difference between the two groups. The largest normalized difference we observe is in free-reduced lunch status (-0.12) but is still insignificant by this definition. These results show that same- and cross-race matches are observably similar in both their baseline outcomes and characteristics of the youth and mentor. However, case managers may assign same-race status based on unobservable characteristics. Any relevant unobserved variables would need to be correlated with both the growth in youth outcomes and the probability of same-race status while also uncorrelated with the rich set of baseline outcomes and match characteristics to threaten the internal validity of our results. Still, we cannot directly test for this selection on unobservables so we estimate specifications at both the match- and agency-level to combat this possibility. After controlling for demographic and socioeconomic characteristics of the mentor and youth in each, the remaining variation in same-race status is plausibly exogenous, though each approach is susceptible to different types of bias. We describe the assumptions behind both models and the associated threats to identification below.

4.1. Match-Level Estimation

We first identify the impacts of assigning a same-race mentor on the outcomes of the youth at the match-level. Specifically, we estimate the equation

$$Y_{iat}^{F} = \alpha + \beta SameRace_{iat} + \eta Y_{iat}^{B} + \Gamma' YouthChars_{iat} + \Theta' MentorChars_{iat} + \delta_t + \varepsilon_{iat} \quad (1)$$

for match i at agency a in year t. $SameRace_{iat}$ equals one if the mentor and the youth in match i are of the same race,[8] and Y_{iat}^{F} denotes the youth's outcome at the follow-up survey. We control for the initial value of the outcome variable, Y_{iat}^{B}, to account for any baseline differences among youth. $YouthChars_{iat}$ is a vector of the youth's race, age, gender, free/reduced school lunch status, and single parent home status. $MentorChars_{iat}$ is a vector of the mentor's race, age, gender, and educational attainment. δ_t are year fixed effects that account for any trends in outcomes over time. Finally, we cluster the standard errors at the agency level to allow for dependency in the error term among matches within an agency.

To interpret β in Equation 1 as the causal effect of a same-race match, it must be the case that whether the youth was matched with a same-race mentor is as good as randomly assigned conditional on the observable characteristics of both the youth and the mentor. Because of the heuristic approach to matching described in Section 2, we include controls for the youth's age, gender, and socioeconomic status and the mentor's age, gender and educational attainment to account for the decision criteria used by the youth's case manager in selecting a mentor. To the extent that these controls are correlated with other relevant characteristics, they may also proxy for qualities of the match that we do not observe but that the case manager does, such as personal interests mentioned during the interview.

One possible threat to identification is that youth are assigned to same-race matches based on some unobserved match characteristics that are correlated with growth in outcomes. Because youth often fill out the baseline survey prior to being matched with a mentor, case managers may be more likely to allocate same-race mentors to the youth with lower or higher baseline values of certain outcomes.[9] For example, if case managers expect youth with higher baseline scores to benefit more from a same-race mentor our estimates would be positively biased. Table 1 shows that youth matched to same-race mentor have similar baseline outcomes on average to those in cross-race matches. Additionally, case managers may assign youth to same-race matches based on their own demographics or socioeconomic status, or those of the mentor. Table 1 also shows no significance differences in the demographics and socioeconomic status of youth and mentors

[8] In the case of multi-racial individuals, we use the first listed race as their primary identity for defining a race-match. For individuals in the "other" category, we determine race congruence using the included subcategories (Asian, Pacific Islander and American Indian) rather than the mentor and youth both being in the "other" category.

[9] In some cases the youth fills out the baseline survey after a potential mentor has been identified but before the initial meeting.

by match type.

Absent from our match-level specification are agency fixed effects which absorb much of the variation in same-race status, leaving only within-agency variation. In doing so, our results may suffer from time-invariant bias from two sources: region-specific demographic composition and idiosyncratic agency behavior.[10] We account for region-specific factors that might bias our results through the inclusion of other characteristics of the mentor and youth. To the extent that these characteristics correlate with the socioeconomic or demographic characteristics of the local community, these covariates act as a proxy for the unobserved determinants of youth's growth. We describe this strategy in detail in Section 4.3. Finally, we test the robustness of our results to the inclusion of agency fixed effects and find similar effects across both models (see Section 5.1 for a fuller discussion).

It is worth noting that participation in the program is voluntary, so there is likely selection into who enrolls in the program. For that reason, it is important to note that we can only unbiasedly estimate the relative effects of twelve months of mentoring on outcomes for youth in same-race matches relative to those cross-race matches among youth who elected to enroll in mentoring. We cannot say what the effect of same-race mentoring is relative to a counterfactual youth who did not receive any mentoring.[11]

4.2. Heterogeneity by Youth Race

We re-estimate Equation 1 interacted with dummies for the race of the youth to determine which youth are most affected by race-matching. The heterogeneous treatment effect model is

$$\begin{aligned} Y_{iat}^{F} = \alpha &+ \beta_B SameRace \times Black_{ia} + \beta_W SameRace \times White_{ia} \\ &+ \beta_H SameRace \times Hispanic_{ia} + \beta_O SameRace \times Other_{ia} \\ &+ \eta Y_{iat}^{B} + \Gamma' YouthChars_{iat} + \Theta' MentorChars_{iat} + \delta_t + \varepsilon_{iat} \end{aligned} \tag{2}$$

[10] Agency fixed effects would account for the former as agencies serve the local community within a certain geographic distance.

[11] For estimates of the impact of youth mentoring enrollment see Grossman and Tierney (1998), Herrera et al. (2011), Park et al. (2017), Rodríguez-Planas (2012).

where the control set and fixed effects are identical to the average treatment effect model. The only exception is the omission of the mentor's race dummies which are collinear with the youth's race dummies and $SameRace$-youth's race dummy interactions. In this model, β_k, $k = B, W, H, O$, is the effect of assigning a same-race mentor relative to a cross-race mentor for a youth of race k.

4.3. Agency-Level Estimation

To abstract away from any match-level bias, we estimate the agency-level analog of Equation 1 which takes the form

$$\overline{Y}^F_{at} = \alpha + \beta\overline{SameRace}_{at} + \eta\overline{Y}^B_{at} + \Gamma'\overline{YouthChars}_{at} + \Theta'\overline{MentorChars}_{at} + \delta_t + \nu_{at} \quad (3)$$

where $\overline{Y}^F_{at}$ is the mean of the outcome variable in the follow-up survey at agency a in year t. $\overline{SameRace}_{at}$ is the fraction of matches in agency a in year t that are same-race, $\overline{Y}^B_{at}$ is the agency-year mean of the outcome variable in the baseline survey, and $\overline{YouthChars}_{at}$ and $\overline{MentorChars}_{at}$ are agency-year means of the youth and mentor characteristics included in Eq. 1, respectively.

In order to interpret β in Equation 3 as the causal impact of a higher percent of same-race matches, the proportion of observed same-race matches at an agency in any particular year must be as good as randomly assigned, conditional on the included observables. While the agency-level regression model does not suffer from match-level endogeneity concerns, it is susceptible to bias from local economic or social factors such as differences in school spending or the demographic composition which informs the pool of potential mentors and youth. Controlling for the average baseline youth outcome combats bias from social factors, while the proportions of youth receiving free/reduced lunch, youth in a single-parent home, and mentors in each educational attainment bin control for socioeconomic factors. Lastly, we include the proportion of mentors and youth in each race category which controls for local demographic trends over time.

A second, related concern is that the matching heuristic used by agencies is correlated with both the fraction of same-race matches as well as the average outcome at the first follow-

up survey. If, for example, agencies with a higher ratio of racial/ethnic minority mentors to racial/ethnic minority youth more often match promising youth with same-race mentors, our results would be biased. The causal interpretation of Equation 3 rests on the assumption that the fraction of same-race matches is uncorrelated with unobserved agency-specific characteristics.

We similarly estimate Equation 2 at the agency-level by replacing all of the variables with their agency-level means. To estimate the heterogeneous treatment effects at the agency-level, we include the proportion of same-race matches where both mentor and youth are Black, White, Hispanic or in the Other category. We discuss the results of both the estimated average and heterogeneous treatment effects at the match- and agency-levels in the next section.

5 — Results

5.1. Match-Level Results

Panel A of Table 2 contains the results of estimating Equation 1. In the match-level regressions, being matched with a mentor of the same race does not appear to improve youth outcomes relative to being matched with a mentor of a different race. The lack of significant impacts can be interpreted in several ways. It may simply be the case that there are no relative benefits to being matched with a mentor of the same race. However, the literature on race-congruency suggests there are positive effects of both same- and cross-race matching (Rhodes et al. 2002). By averaging the effects into a single estimate, we may be masking important heterogeneity.

Panel B contains the results of estimating a model where the same-race indicator variable is interacted with a set of indicators for the youth's race. As the literature suggests, it may be the case that mentoring improves school outcomes for racial/ethnic minority youth when paired with a mentor of the same race. Our results show this to be true in some instances. Black youth in same-race matches saw a 0.0345 point improvement ($p < 0.05$) in their self-perceived school ability relative to those who were mentored by a cross-race mentor. This estimate is small relative to the average baseline school ability among cross-race matched youth (2.9360), but represents a 23.37% larger rate of growth compared to the average youth assigned a cross-race mentor (0.0345/0.1476). Asian, Pacific Islander and American Indian youth in same-race matches had better attendance after a year of mentoring (0.1649, $p < 0.001$), a stark contrast to the average

growth rate of -0.0191 among cross-race matches. Lastly, we find no effects of race-matching on the youth's self-perceived social acceptance for any race category.

Contrary to the existing literature on race-congruency, we find few impacts of race-matching on educational outcomes of youth. Black youth matched to Black mentors had slightly lower grades (-0.0034) but this effect is imprecisely estimated. Hispanic youth in same-race matches had slightly lower grades (-0.0473, $p < 0.05$) relative to those in cross-race matches. This could be due to spending extended time with a mentor whose primary language is English. Currie and Thomas (1999) show that the Head Start preschool program had larger impacts on standardized test scores for Hispanic children, and particularly Hispanic children from households where the primary language spoken was Spanish. Unfortunately, we cannot test this directly since we do not observe the youth's language ability or the primary language spoken at home. However, Table 3 shows that the effect on Hispanic youths' grades is driven by their scores in reading, social studies and science, with no effect on math grades. The affected grades are in courses that have a higher marginal return to increased English language ability (e.g. reading comprehension). These results combined with the fact that mentors rarely spend time tutoring their youth provide suggestive evidence of an English language ability mechanism driving the cross-race effects on Hispanic youths' grades. Youth in the Other category had significantly higher math grades (0.1789, $p < 0.05$) as a result of being race-matched, but the effect on overall grades is insignificant. Lastly, we find no effects of race-matching on the youth's expectations for educational attainment.

Turning to the outcomes related to the youth's social interactions, we find a 0.0107 point improvement in the risk attitudes of Black youth matched with a Black mentor relative to those in cross-race matches (21.53% increase, $p < 0.05$). We observe a similarly sized effect of 0.0153 for Hispanic youth (30.78% increase, $p < 0.01$) and no statistically significant effects for White youth or youth in the Other category. We do not observe any statistically significant effects of race-matching on parental trust. Notably, the small relative decrease in the likelihood that a youth in a same-race match reports having a special adult in their life appears to be driven by Black youth. Because these effects can be interpreted as the relative impact of having a same-race mentor compared to a mentor of another race, these results suggest that Black youth may benefit along some dimensions from having a non-Black mentor.

We test the robustness of these results with two additional specifications. In the first, we

include agency fixed effects to account for any unobserved agency-specific, time-invariant omitted variables that are correlated with the outcomes of the youth and the probability they are assigned a same-race mentor. For example, some agencies may be relatively more adept at identifying same-race mentors who will have a greater impact on the youth through a unique screening process. Although all agencies report to the national office, they are allowed to deviate slightly from organization-wide policy to better serve the local community. The drawback of using agency fixed effects is that they may overfit the model by accounting for a common matching heuristic shared by all case managers at a particular agency. Table 4 shows the results of estimating Equations 1 and 2 with agency fixed effects. The point estimates are similar to those in Table 2 though attenuated. While the within-agency estimator is not susceptible to agency-specific bias, it seems to absorb much of the useful identifying variation from estimating the same-race effect across agencies.

In the second specification, we drop any matches where the mentor or youth had two races listed to address measurement error in same-race status. In our sample, there are 1,210 mentors and 3,585 youth who listed two races and thus we cannot be certain of their same-race status. For example, the same-race status between a White, non-Hispanic mentor and a White, Hispanic youth is not obvious. The youth's perception of their own same-race status likely depends on whether they primarily identify as White or Hispanic. Another possibility is that youth may experience an intensity of same-race effects along the race-congruence continuum. In this case, the White, Hispanic youth described above may benefit from having a White, non-Hispanic mentor more than a mentor of a completely different race, but less than having a White, Hispanic mentor. We take a conservative approach to addressing this ambiguity in same-race status by dropping any matches where either party was multi-racial. Table B1 shows the results of estimating the same-race effect on the subsample of matches where both mentor and youth listed a single race and thus their same-race status is certain. The point estimates are remarkably similar to those in Table 2 but with larger standard errors given the considerable loss of power from dropping around 4,500 observations. From this table we conclude that the effect of race-matching is not driven by multi-racial matches for whom we are less certain of their true perception of same-race status.

5.2. Agency-Level Results

Table 5 contains results from estimating Equation 3. These results are robust to match-level bias but are susceptible to any contextual effects not captured by our controls for mentor and youth demographics and socioeconomic status. The agency-level results are qualitatively similar in most cases when scaled by the percent of same-race matches at an agency. Unless bias from geographic variation is the same direction and magnitude as case manager selection bias, these results would not persist across specifications. In Panel A, we find increasing the proportion of same-race matching within an agency has no statistically significant impacts on youth' outcomes except in the case of grades. An additional one percent of pairs being race-matched would increase the average grades score by 0.002109 (0.2109/100, $p < 0.05$). When scaled by the average proportion of same-race matches within an agency, the magnitudes of the estimates in Panel A are similar to those in Table 2.

In Panel B, we include the proportion of same-race matches within each race category of the youth. This allows us to examine the impacts of increasing the proportion of same-race matches within youth race subgroups. Although statistically insignificant, we find similarly positive effects on school ability for Black youth. The significant effect of race-matching on grades is driven by White youth in same-race matches: increasing the proportion of White-White matches by one percent leads to a 0.004698 increase in the average grade of White youth ($p < 0.01$). At the agency level, we find that increasing the share of race-matched Hispanic youth increases education expectations by 0.002314 points ($p < 0.05$). Lastly, we find improvements in risk attitudes among Black youth and youth in the Other category, but no effects on parental trust or the prevalence of youth reporting a special adult.

Taken together, Tables 2 and 5 show that race-matching generates modest improvements in the youth's self-perceptions and problem behavior, primarily among Black and Hispanic youth. We find minor improvements in course grades and no impacts on educational expectations, unlike the broader literature on race-congruency. This is perhaps unsurprising given that the program is focused on social mentoring. Mentors often spend time bonding with their youth in leisurely activities rather than academic activities such as tutoring. Indeed, the organization described their program as primarily impacting the social and emotional of development of

the youth. The lack of findings for educational outcomes implies that pure role-modeling may not be the primary driver of race-congruence effects on tests scores, high school completion or college enrollment for racial or ethnic minority youth. Outside of the classroom, race-matched mentoring appears to have the highest marginal impacts on the non-cognitive outcomes of youth.

5.3. Effects on Match Length and Quality

Although we do not find large effects of race-matching on the socio-emotional development or educational expectations of youth after a year of mentoring, it is possible that being assigned a same-race mentor improves the length and quality of interaction between youth and mentor. Column 1 of Table 6 shows that race-matching slightly increases the total length of the relationship. White youths' matches lasted two months longer ($p < 0.01$) and Hispanic youths' last an additional 1.41 months ($p < 0.01$) as a result of race-matching. On the other hand, cross-race matches in the Other category ended about five months later than same-race matches ($p < 0.05$). We do not find any effect on the length of the match in months for Black youth.

The average match length for our sample was around 34 months, but the distribution of match length is skewed right (see Figure A1). Thus we estimate the effect of race-matching on the number of follow-up surveys conducted.[12] While our analytic sample consists of matches who completed at least one follow-up survey, approximately half of matches had additional follow-up surveys. Column 2 shows the average cross-race match lasted about two follow-up surveys, roughly equivalent to 24 months. The effects of race-matching on number of follow-up surveys correspond in sign and significance to those in Column 1: the average number of surveys was unaffected for Black youth, increased slightly for White and Hispanic youth and decreased for Asian, American Indian, and Pacific Islander youth. Columns 3 and 4 further break down the number of surveys into exactly two and three or more, where about a half of the sample is evenly distributed. We find that Black youth who were race matched were two percentage points more likely to last till the second follow-up ($p < 0.05$), while youth in the Other category were six percentage points less likely to last as long relative to their cross-race matched counterparts ($p < 0.05$). Only White youth were more likely to have filled out three or more follow-up surveys

[12] Each follow-up survey is administered roughly twelve months apart.

as a result of race-matching (+3pp, $p < 0.05$).

Columns 5-7 show the effect of race-matching on the likelihood of case managers flagging the match for concerns it may end prematurely.[13] We find no effect of race-matching for any race group on the probability a match has been flagged for either moderate or high concerns at the end of twelve months. However, we note that about two-thirds of matches were instead marked as "None". With an average match length of 34 months, it may be the case that the level of concern begins to rise after the first follow-up survey. It is difficult to assign causality to estimates on later levels of match concern since about half our sample ended the match before the second follow-up.

Another measure of match quality is the reason the match was terminated. Either the youth or mentor may terminate the match for a variety of reasons.[14] Case managers may reasonably hypothesize that assigning a same-race mentor would reduce the probability a match is terminated prematurely. Although most matches are closed because the mentor moves away and cannot continue meeting with the youth.[15] Table 7 shows the effect on same-race matching on the reasons for match closure for the subsample of matches that are no longer active. Panel A, Column 1 shows that while 9% of matches end because the youth moves away, assigning a same-race mentor does not affect the probability of this. White youth in same-race matches were more likely to report ending the match because they felt incompatible (+1pp, $p < 0.001$, Panel A, Column 2) or because they graduated high school (+3pp, $p < 0.001$, Panel A, Column 4), though combined these represent less than 10% of matches. Having a same-race mentor did not reduce the probability of a youth ending the match because of time constraints, losing contact with their mentor or losing interest in the program. Panel B shows the effects of race-matching on the reasons mentors initiated the match closure. Column 1 shows that same-race mentors of Black, White and Hispanic youth were less likely to end the match by moving away, relative to cross-race mentors. Black mentors were less likely to end their match due to time constraints (-2pp, $p < 0.05$, Panel B, Column 2), but were more likely to lose contact with their assigned youth along with Hispanic mentors (+4pp, $p < 0.001$ and +4pp, $p < 0.01$, Panel B, Column 4). Race-matching had no effect on the probability the mentor ended the match because they felt

[13] Not meeting as regularly as expected is a primary cause for concern.
[14] The local agency in some rare cases will terminate the match if they are concerned with the mentor's behavior.
[15] Often this is the case among mentors who are enrolled in university and eventually graduate.

incompatible with their youth, though only 3% of mentors overall reported this as the reason for match closure.

In summary, same-race matching seems to slightly increase the length of matches for White and Hispanic youth, but does not increase the probability the case manager reports the match is in good standing. Race-matching also does not reduce the probability that the youth terminates the match early for undesirable reasons (incompatibility, lost contact or interest), though these represent a small proportion of match terminations. Lastly, race-matching decreases the likelihood a mentor ends the match by moving away, but increases the probability the mentor loses contact with Black and Hispanic youth.

6 — Conclusion

In this paper, we estimated the effect of same-race mentorship relative to cross-race mentorship on the outcomes of youth who participated in mentoring for at least twelve months. We found that youth who were assigned a same-race mentor had almost no improvements relative to those assigned a cross-race mentor, on average. It is possible that both same- and cross-race mentoring have positive impacts for certain youth and negate each other when averaged across the entire sample. Heterogeneity analysis by the race of the youth revealed this is somewhat the case. Same-race matching improved self-perceived school ability for Black youth, truancy for youth in the Other category, and risk attitudes for both Black and Hispanic youth. On the other hand, Hispanic youth in cross-race matches had slightly higher grades after a year of mentoring, and Black youth in cross-race matches were more likely to identify a special adult in their life. It may also be the case that race-matching improves race-relevant outcomes. For example, youth in same-race matches may have better self-perceptions of their race or ethnicity, may have a more postive racial or ethnic identity or may better cope with experienced racial or ethnic discrimination. We cannot conclude that race-matching is not an important determinant for such outcomes as we do not observe them in the data.

Youth mentorship has been shown to have significant positive effects on a range of outcomes for children, and race-congruence is believed to be an important determinant of this success. We contribute to the literature on race-congruence by showing there are potential benefits to both

same- and cross-race matching. Furthermore, when full race-matching is not feasible, organizations must choose how to allocate the scarce supply of eligible mentors to youth. We showed that certain groups benefit from race-matching along different dimensions. This heterogeneity in the same-race premium as well as identifying the scenarios when cross-race benefits outweigh same-race benefits are critical for understanding how to efficiently allocate racial/ethnic minority mentors in the presence of supply constraints. Our results suggest that policy makers in areas with higher proportions of racial and ethnic minorities, should consider the additional benefits of policies that lessen these supply constraints for youth mentoring programs. For example, the State of Colorado offers tax credits equal to 50% of donations to the BBBS program.[16]

Although we identified for whom same-race mentoring is most impactful, more research is needed to understand the mechanisms behind these effects. Race-congruence seems to impact youth's self-perceptions more than academic performance or attitudes towards adults. This suggests that mentorship improves the youth's self-confidence but may not impact their academic skill or perceptions of authority. This is not unexpected as mentors in community-based matches rarely spend time helping their youth study or complete homework. In addition, we focused on the first twelve months of mentoring. It may be the case that affecting the youth's academic ability or worldview takes more than one year, and further research is needed to estimate the causal impacts on longer term outcomes. Finally, we are not able to study the impact of same-race matching in mentoring relative to no mentoring whatsoever because our data consist only of successful matches. More work is needed to make credible claims about the level effects of race-congruence compared to non-mentored youth.

[16]This policy recommendation relies on the assumption that the additional resources would increase the number of racial/ethnic minority mentors, either through matches lasting longer (i.e. from greater support) or from having more racial/ethnic minority mentors enter the program (i.e. more flexibility in training and potentially recruitment efforts). Source: https://www.colorado.gov/pacific/sites/default/files/Income35.pdf, Accessed April 6th, 2021.

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TABLES AND FIGURES

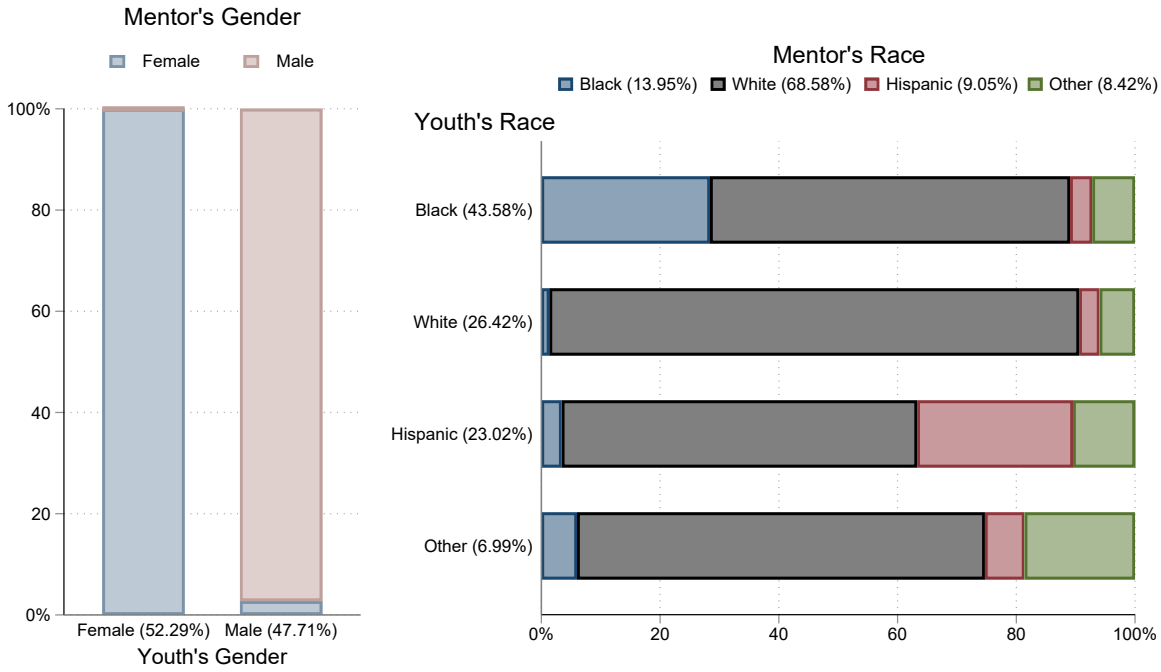

FIGURE 1 — PERCENT OF MATCHES BY MENTOR/YOUTH RACE AND GENDER

Notes: Left panel shows the percent of youth by gender matched to female and male mentors. Percent of full sample that are female or male youth shown in parentheses. Right panel shows the percent of youth by race matched to a mentor of a certain race. Percent of the full sample of youth that are of each race are shown in parentheses along the vertical axis. Percent of the full sample of mentors that are of each race are shown in parentheses in the legend. The Other category includes Asian, Pacific Islander, and American Indian. Within the Other-Other cell, same-race is defined using the associated subcategories. The height of each colored portion of the bar shows the proportion of youth matched to a male or female mentor, by the gender of the youth.

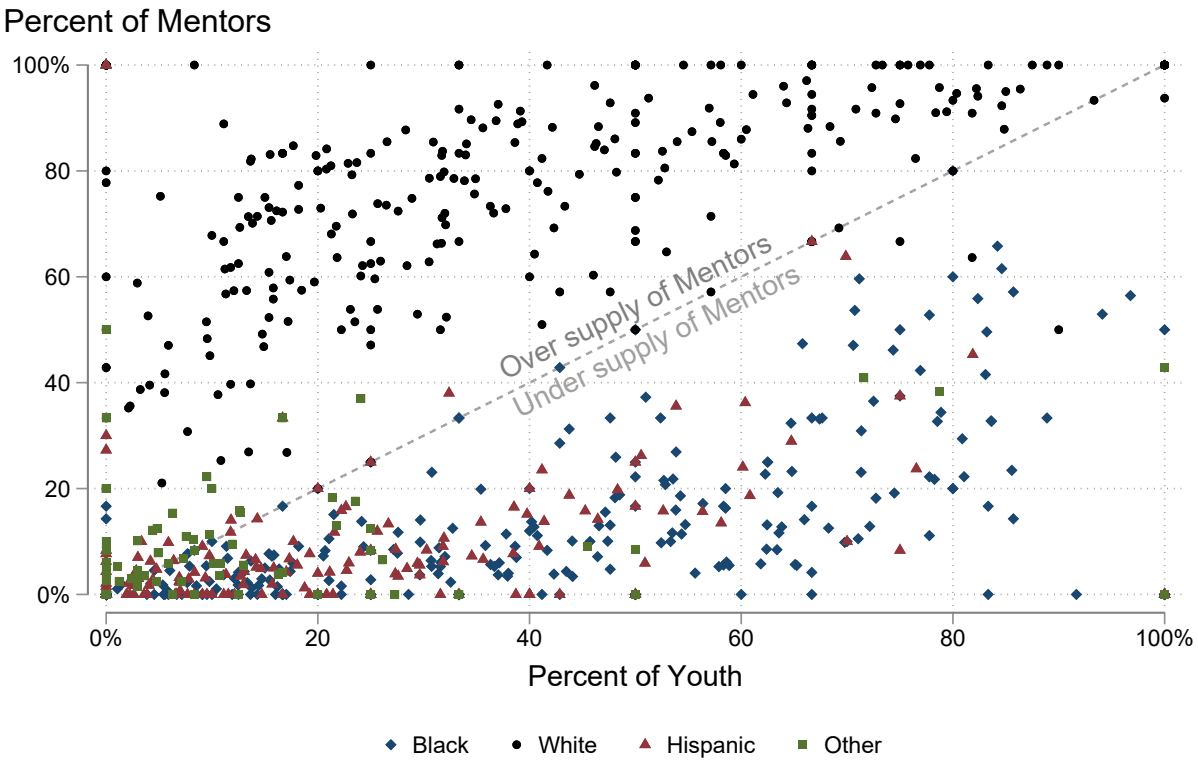

FIGURE 2 — VARIATION IN RACIAL COMPOSITION OF MENTORS AND YOUTH BY AGENCY

Notes: Each point represents the proportion of mentors and youth of a particular race within a local agency. The upper diagonal represents all of the agencies where the proportion of mentors in a particular category is greater than the proportion of youth in the same category, and vice versa. The Other category includes Asian, Pacific Islander and American Indian, but not necessarily the relative proportion of the subcategories.

TABLE 1 — BALANCE OF BASELINE CHARACTERISTICS BY SAME-RACE STATUS

	Same-Race			Cross-Race			Difference		
	Mean	SD	N	Mean	SD	N	Mean	T-stat	Mean/SD
Baseline Survey									
Social Acceptance	2.84	0.65	12,427	2.91	0.62	16,530	-0.07	-9.92	-0.08
School Ability	2.92	0.60	12,443	2.94	0.59	16,565	-0.01	-2.07	-0.02
Attendance	2.91	0.82	12,527	2.91	0.85	16,665	0.00	0.30	0.00
Grades	3.70	0.80	12,414	3.72	0.77	16,507	-0.02	-2.21	-0.02
Education Expectations	3.55	0.67	12,564	3.62	0.62	16,761	-0.07	-9.62	-0.08
Risk Attitudes	3.85	0.26	12,507	3.85	0.27	16,689	-0.00	-0.33	-0.00
Parental Trust	3.57	0.58	12,545	3.61	0.55	16,715	-0.04	-6.24	-0.05
Special Adult (=1)	0.56	0.50	12,397	0.59	0.49	16,471	-0.03	-4.97	-0.04
Youth Characteristics									
Male (=1)	0.47	0.50	12,653	0.48	0.50	16,879	-0.01	-1.28	-0.01
Age	11.25	1.83	12,649	11.15	1.83	16,867	0.10	4.66	0.04
Free-Reduced Lunch (=1)	0.74	0.44	12,653	0.81	0.39	16,879	-0.07	-14.35	-0.12
Single-Parent HH (=1)	0.67	0.47	12,653	0.70	0.46	16,879	-0.02	-4.39	-0.04
Two-Parent HH (=1)	0.21	0.41	12,653	0.20	0.40	16,879	0.01	2.44	0.02
Family Income	28082.32	22545.42	7,556	23857.21	18571.58	10,144	4225.11	13.65	0.15
Mentor Characteristics									
Male (=1)	0.46	0.50	12,653	0.47	0.50	16,879	-0.01	-1.16	-0.01
Age	37.89	12.08	12,652	36.48	11.15	16,878	1.41	10.35	0.09
Less than High School (=1)	0.01	0.09	12,653	0.01	0.07	16,879	0.00	3.16	0.03
High School Graduate (=1)	0.06	0.24	12,653	0.04	0.20	16,879	0.02	8.25	0.07
Some College (=1)	0.22	0.42	12,653	0.17	0.38	16,879	0.05	10.59	0.09
Associate Degree (=1)	0.07	0.25	12,653	0.05	0.21	16,879	0.02	6.53	0.05
Bachelor's Degree (=1)	0.43	0.50	12,653	0.51	0.50	16,879	-0.07	-11.94	-0.10
Advanced Degree (=1)	0.21	0.41	12,653	0.23	0.42	16,879	-0.02	-4.22	-0.04

Notes: Means, standard deviations and sample sizes are calculated from the analytical sample of formed matches by same-race status. Same-race status is defined using the specific race recorded for the mentor and youth. In the case of multi-racial individuals, the first listed race is used for matching. All outcomes shown are the baseline values. The last three columns are the difference in means across groups, the T-statistic of the difference and the standardized difference, respectively. The standardized difference, Mean/SD, is the difference in means divided by the standard deviation of the difference (see Imbens and Wooldridge (2009)).

TABLE 2 — SAME-RACE IMPACTS ON YOUTH'S FOLLOW UP OUTCOMES AT THE MATCH LEVEL

	School Experience			Education		Social Experience		
	Social Acceptance (1)	School Ability (2)	Attendance (3)	Grades (4)	Education Expectations (5)	Risk Attitudes (6)	Parental Trust (7)	Special Adult (=1) (8)
Panel A: Average Same Race Effect								
Same Race	-0.0088	-0.0016	0.0128	0.0145	-0.0182	0.0068	-0.0125	-0.0135
	(0.0120)	(0.0160)	(0.0168)	(0.0170)	(0.0128)	(0.0039)	(0.0104)	(0.0077)
Panel B: Same Race Effect by Race of Youth								
Same Race × Black	-0.0054	0.0345*	0.0238	-0.0033	0.0111	0.0107*	0.0038	-0.0272***
	(0.0093)	(0.0138)	(0.0195)	(0.0159)	(0.0121)	(0.0053)	(0.0120)	(0.0060)
Same Race × White	-0.0004	-0.0143	-0.0264	0.0243	-0.0052	-0.0075	-0.0304	-0.0061
	(0.0211)	(0.0185)	(0.0292)	(0.0292)	(0.0234)	(0.0063)	(0.0162)	(0.0117)
Same Race × Hispanic	-0.0218	-0.0165	0.0407	-0.0473*	-0.0178	0.0153**	-0.0219	-0.0050
	(0.0146)	(0.0195)	(0.0279)	(0.0240)	(0.0178)	(0.0055)	(0.0142)	(0.0113)
Same Race × Other	-0.0190	0.0369	0.1649***	0.1181	-0.0156	0.0179	0.0057	-0.0142
	(0.0457)	(0.0807)	(0.0468)	(0.0821)	(0.0556)	(0.0133)	(0.0313)	(0.0276)
Baseline Mean of Cross-Race	2.9138	2.9360	2.9052	3.7190	3.6228	3.8543	3.6102	0.5916
Average Growth of Cross-Race	0.1656	0.1476	-0.0191	-0.0272	0.0474	0.0497	0.0969	0.2961
N	28,601	28,630	28,890	28,270	29,111	28,861	28,948	28,571

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses are clustered at the agency level. All regressions include controls for the youth's baseline outcome, gender, race, free/reduced lunch status, and single-parent home status, mentor's gender and race, as well as fixed effects for the youth's age at follow up, mentor's age and education at follow up, and calendar year. Panel B omits the controls for mentor's race to avoid collinearity with the interaction terms and the include controls for youth's race. Baseline mean of cross-race is the mean of the outcome at baseline among the cross-race group.

TABLE 3 — SAME-RACE IMPACTS ON YOUTH'S INDIVIDUAL COURSE GRADES

	Math	Reading	Social Studies	Science
	(1)	(2)	(3)	(4)
Same Race × Black	-0.0085	0.0037	0.0010	-0.0129
	(0.0204)	(0.0189)	(0.0179)	(0.0234)
Same Race × White	0.0283	0.0007	0.0203	0.0406
	(0.0358)	(0.0377)	(0.0342)	(0.0432)
Same Race × Hispanic	-0.0172	-0.0542*	-0.0713*	-0.0795*
	(0.0307)	(0.0268)	(0.0286)	(0.0335)
Same Race × Other	0.1789*	0.0696	0.0623	0.1420
	(0.0899)	(0.0925)	(0.1007)	(0.0817)
Baseline Mean of Cross-Race	3.6379	3.7509	3.6647	3.8120
Average Growth of Cross-Race	-0.0297	-0.0143	0.0307	-0.0907
N	29,180	29,161	28,669	28,824

Notes: $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$. Standard errors in parentheses are clustered at the agency level. All regressions include controls for the youth's baseline outcome, gender, race, free/reduced lunch status, and single-parent home status, mentor's gender, as well as fixed effects for the youth's age at follow up, mentor's age and education at follow up, and calendar year. Baseline mean of cross-race is the mean of the outcome at baseline among the cross-race group.

TABLE 4 — MATCH LEVEL EFFECTS WITH AGENCY FIXED EFFECTS

	School Experience			Education		Social Experience		
	Social Acceptance	School Ability	Attendance	Grades	Education Expectations	Risk Attitudes	Parental Trust	Special Adult (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Average Same Race Effect								
Same Race	-0.0090	-0.0042	0.0013	-0.0108	-0.0155	0.0019	-0.0112	-0.0193***
	(0.0072)	(0.0093)	(0.0116)	(0.0112)	(0.0082)	(0.0026)	(0.0075)	(0.0054)
Panel B: Same Race Effect by Race of Youth								
Same Race × Black	0.0011	0.0125	-0.0187	-0.0081	-0.0078	0.0005	0.0004	-0.0306***
	(0.0082)	(0.0106)	(0.0165)	(0.0178)	(0.0120)	(0.0033)	(0.0102)	(0.0062)
Same Race × White	-0.0003	-0.0043	-0.0186	0.0082	0.0042	-0.0032	-0.0297	-0.0046
	(0.0213)	(0.0176)	(0.0271)	(0.0304)	(0.0241)	(0.0063)	(0.0166)	(0.0122)
Same Race × Hispanic	-0.0268	-0.0311	0.0296	-0.0387	-0.0329*	0.0069	-0.0245	-0.0053
	(0.0168)	(0.0186)	(0.0185)	(0.0212)	(0.0167)	(0.0050)	(0.0138)	(0.0111)
Same Race × Other	-0.0477	-0.0308	0.1284**	0.0548	-0.0698	0.0056	0.0011	-0.0266
	(0.0350)	(0.0342)	(0.0474)	(0.0480)	(0.0371)	(0.0108)	(0.0256)	(0.0211)
Baseline Mean of Cross-Race	2.9138	2.9360	2.9052	3.7190	3.6228	3.8543	3.6102	0.5916
Average Growth of Cross-Race	0.1656	0.1476	-0.0191	-0.0272	0.0474	0.0497	0.0969	0.2961
N	28,601	28,630	28,890	28,270	29,111	28,861	28,948	28,571

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses are clustered at the agency level. All regressions include the controls listed in Table 2 as well as agency fixed effects. Baseline mean of cross-race is the mean of the outcome at baseline among the cross-race group.

TABLE 5 — SAME-RACE IMPACTS ON AVERAGE YOUTH FOLLOW UP OUTCOMES AT THE AGENCY LEVEL

	School Experience			Education		Social Experience		
	Social Acceptance (1)	School Ability (2)	Attendance (3)	Grades (4)	Education Expectations (5)	Risk Attitudes (6)	Parental Trust (7)	Special Adult (=1) (8)
Panel A: Average Same Race Effect								
Same Race	0.0078	0.0502	0.0152	0.2109*	0.0319	0.0316	0.1187	0.0253
	(0.0640)	(0.0825)	(0.0817)	(0.0987)	(0.0646)	(0.0175)	(0.0653)	(0.0393)
Panel B: Same Race Effect Within Race of Youth								
Same Race × Black	-0.0215	0.0740	0.0050	-0.0030	0.0865	0.0454*	0.0211	-0.0279
	(0.0568)	(0.0636)	(0.0834)	(0.0749)	(0.0641)	(0.0182)	(0.0536)	(0.0399)
Same Race × White	0.0573	0.0138	-0.0129	0.4698**	-0.1116	0.0335	0.1600	0.1059
	(0.1196)	(0.1549)	(0.1331)	(0.1581)	(0.0946)	(0.0317)	(0.1071)	(0.0747)
Same Race × Hispanic	0.0582	0.1275	0.1641	0.0393	0.2314*	0.0396	0.1100	0.0398
	(0.0979)	(0.1161)	(0.1363)	(0.1471)	(0.0929)	(0.0312)	(0.1018)	(0.0585)
Same Race × Other	-0.1316	0.2482	-0.0408	0.3979	0.4793	0.1357*	0.2561	-0.0872
	(0.2230)	(0.2661)	(0.2570)	(0.2993)	(0.2506)	(0.0611)	(0.2057)	(0.1240)
Baseline Mean of Outcome	2.9649	3.0281	2.8957	3.6471	3.5782	3.8923	3.6574	0.8916
Average Growth	0.1433	0.1172	-0.0195	-0.0056	0.0606	0.0358	0.0604	0.3258
Fraction of Same-Race Matches	0.5069	0.5066	0.5066	0.5020	0.5046	0.5046	0.5043	0.5055
N	1740	1743	1745	1735	1746	1742	1745	1739

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors in parentheses. All regressions include controls for the average baseline score of all youth at the agency; the within-agency fraction of youth that are: male, in each race category, in each age year bin, on free/reduced lunch, and live in a single-parent home; the within-agency fraction of mentor's that are: male, in each race category, of each age, in each educational attainment bin; and calendar year fixed effects. Panel B omits the controls for the fraction of mentors in each race bin to avoid collinearity with the interaction terms and the fraction of youth in each race bin. Baseline mean is the mean of the outcome at baseline. Fraction of same-race matches is the average proportion of matches within an agency that are same-race.

TABLE 6 — SAME-RACE IMPACTS ON MATCH LENGTH AND QUALITY

		Follow-up Surveys			Match Concern After 12mos		
	Match Length (mos)	Total	Two (=1)	Three or More (=1)	None (=1)	Moderate (=1)	High (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Same Race × Black	-0.72	-0.05	0.02*	-0.01	-0.00	-0.02	0.02
	(0.54)	(0.04)	(0.01)	(0.01)	(0.04)	(0.01)	(0.04)
Same Race × White	2.00**	0.14**	0.02	0.03*	0.03	-0.02	-0.01
	(0.69)	(0.05)	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)
Same Race × Hispanic	1.41**	0.09**	0.01	0.02	-0.01	0.01	0.00
	(0.53)	(0.04)	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)
Same Race × Other	-4.92*	-0.33*	-0.06*	-0.08	0.07	-0.01	-0.06
	(2.25)	(0.13)	(0.02)	(0.04)	(0.05)	(0.02)	(0.04)
Mean of Cross-Race	33.99	2.04	0.24	0.27	0.66	0.10	0.23
N	28,206	28,206	28,206	28,206	28,206	28,206	28,206

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses are clustered at the agency level. All regressions include the controls listed in Table 2 excluding baseline survey scores. The analytic sample is limited to matches that had at least one follow-up survey, hence Total surveys is at least 1. The outcome in Column 3 is an indicator for filling out exactly two surveys before the match ended, and Column 4 is analogously defined for three or more surveys. Match concern is the level of concern about the match expressed by the case manager.

TABLE 7 — SAME-RACE IMPACTS ON REASON FOR MATCH ENDING

	Moved	Time Constraints	Felt Incompatible	Lost Contact	Lost Interest	Graduated
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Youth Ended Match						
Same Race × Black	-0.01	0.00	0.00	0.01	-0.00	-0.00
	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Same Race × White	-0.01	-0.00	0.01***	-0.01	-0.01	0.03***
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Same Race × Hispanic	-0.00	-0.00	0.00	-0.01	0.01	0.00
	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Same Race × Other	0.01	0.00	-0.01	-0.02	-0.01	-0.01
	(0.05)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Mean of Cross-Race	0.09	0.01	0.01	0.07	0.06	0.07
Panel B: Mentor Ended Match						
Same Race × Black	-0.03**	-0.02*	0.00	0.04***		
	(0.01)	(0.01)	(0.00)	(0.01)		
Same Race × White	-0.04*	0.02	-0.00	0.01		
	(0.02)	(0.02)	(0.01)	(0.01)		
Same Race × Hispanic	-0.07***	0.02	-0.00	0.04**		
	(0.01)	(0.01)	(0.01)	(0.01)		
Same Race × Other	-0.06	0.04	-0.00	-0.03		
	(0.04)	(0.05)	(0.01)	(0.02)		
Mean of Cross-Race	0.24	0.22	0.03	0.12		
N	20,973	20,973	20,973	20,973	20,973	20,973

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses are clustered at the agency level. All regressions include the controls listed in Table 2 excluding baseline survey scores. The analytic sample is limited to matches that were closed at the time the authors received the data. Columns 5 and 6 are blank in Panel B as mentors do not ever report losing interest or graduation as their reason for ending the match.

APPENDIX

ALL APPENDICES ARE FOR ONLINE PUBLICATION

APPENDIX A — APPENDIX FIGURES AND TABLES

TABLE A1 — AVERAGE BASELINE AND FOLLOW-UP YOUTH OUTCOMES BY SAME-RACE STATUS

	Baseline	Follow-up	Diff	Pr(Diff>0)	Obs
School Experience					
Social Acceptance	2.882	3.039	0.158	0.000	29,197
School Ability	2.930	3.074	0.145	0.000	29,162
Attendance	2.907	2.891	-0.017	0.998	29,248
Education					
Grades	3.708	3.686	-0.020	1.000	28,869
Education Expectations	3.591	3.642	0.052	0.000	29,345
Social Experience					
Risk Attitudes	3.853	3.902	0.049	0.000	29,224
Parental Trust	3.590	3.691	0.101	0.000	29,241
Special Adult (=1)	0.579	0.886	0.307	0.000	29,250

Notes: Average survey outcomes at baseline and first follow-up 12mos later are displayed. Pr(Diff>0) is the p-value from an upper-tailed T-test that the average difference between follow-up and baseline is positive.

TABLE A2 — BALANCE BETWEEN MATCHES WITH AND WITHOUT FOLLOW-UP SURVEYS

	Has Follow-up			No Follow-up			Difference		
	Mean	SD	N	Mean	SD	N	Mean	T-stat	Mean/SD
Baseline Survey									
Social Acceptance	2.88	0.63	28,956	2.89	0.65	43,956	-0.01	-1.69	-0.01
School Ability	2.93	0.59	29,007	2.90	0.60	43,977	0.03	5.61	0.03
Grades	3.71	0.78	28,920	3.65	0.80	43,915	0.06	9.86	0.05
Education Expectations	3.59	0.64	29,324	3.55	0.67	44,489	0.04	7.42	0.04
Risk Attitudes	3.85	0.26	29,195	3.83	0.30	44,229	0.02	10.83	0.06
Parental Trust	3.59	0.56	29,259	3.55	0.61	44,342	0.04	9.86	0.05
Attendance	2.91	0.84	29,191	2.85	0.85	44,156	0.05	8.35	0.04
Special Adult (=1)	0.58	0.49	28,867	0.58	0.49	43,774	0.00	0.97	0.01
Youth Characteristics									
Male (=1)	0.48	0.50	30,463	0.45	0.50	44,994	0.03	6.70	0.04
Age	11.20	1.83	29,515	11.46	1.95	44,782	-0.26	-18.17	-0.10
Free-Reduced Lunch (=1)	0.78	0.42	30,463	0.78	0.42	44,994	0.00	0.23	0.00
Single-Parent HH (=1)	0.69	0.46	30,463	0.69	0.46	44,994	-0.01	-1.38	-0.01
Two-Parent HH (=1)	0.20	0.40	30,463	0.19	0.39	44,994	0.01	3.48	0.02
Family Income ($)	25,661	20,469	17,699	25,886	21,484	24,915	-224	-1.08	-0.01
Mentor Characteristics									
Male (=1)	0.47	0.50	30,463	0.44	0.50	44,994	0.03	7.43	0.04
Age	37.09	11.58	29,529	36.34	11.60	44,829	0.75	8.64	0.05
Less than High School (=1)	0.01	0.08	30,463	0.01	0.09	44,994	-0.00	-2.24	-0.01
High School Graduate (=1)	0.05	0.22	30,463	0.07	0.25	44,994	-0.02	-9.49	-0.05
Some College (=1)	0.19	0.40	30,463	0.23	0.42	44,994	-0.04	-13.41	-0.07
Associate Degree (=1)	0.06	0.23	30,463	0.06	0.24	44,994	-0.01	-2.85	-0.01
Bachelor's Degree (=1)	0.48	0.50	30,463	0.44	0.50	44,994	0.04	10.78	0.06
Advanced Degree (=1)	0.22	0.41	30,463	0.20	0.40	44,994	0.02	8.07	0.04
Match Characteristics									
Match Length (months)	35.01	19.91	30,463	15.65	14.99	44,994	19.36	152.15	0.80

Notes: Means, standard deviations and sample sizes are calculated from the sample of formed matches by follow-up status. All outcomes shown are the baseline values. The last three columns are the difference in means across groups, the T-statistic of the difference and the standardized difference, respectively. The standardized difference, Mean/SD, is the difference in means divided by the standard deviation of the difference (see Imbens and Wooldridge (2009)).

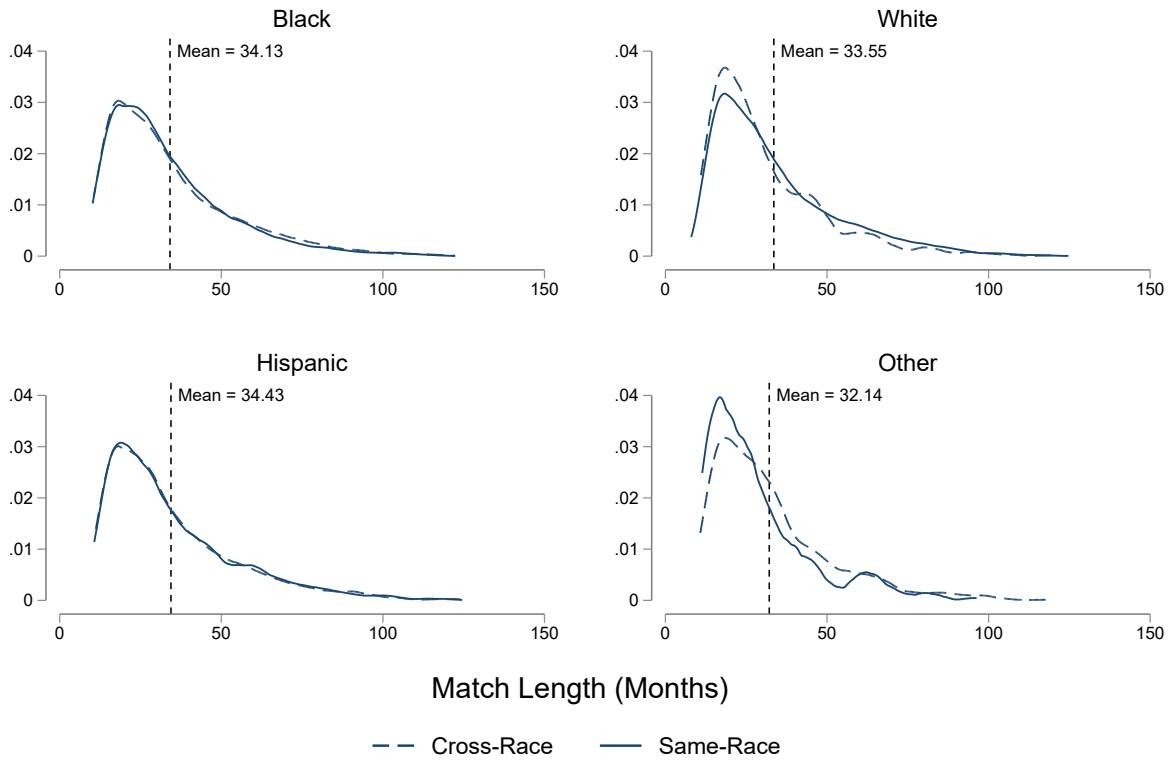

FIGURE A1 — LENGTH OF MATCH BY SAME-RACE STATUS, BY RACE OF YOUTH

Notes: Kernel densities are estimated using an Epanechnikov kernel with a bandwidth of 3. Each set of densities is estimated by race subgroups of the youth for the analytic sample. Dashed vertical lines represent the average match length (in months) for the particular subsample.

APPENDIX B — ROBUSTNESS CHECKS

Drop Multi-Race Individuals.—In our sample, 23,811 matches had a youth and mentor who both listed a single race on their application, making the match's same-race status certain. For the remaining 4,431 matches, one or both participants were recorded as "Multi-racial: ([Race 1] and [Race 2])". We used the first mentioned race in these cases to determine same-race status under the assumption the individual would list their primary perceived identity first. To check if this assumption is driving our results, we drop all matches where one or both participants had two races and re-estimate the models on the resulting subsample. The results are listed in Table B1 and are similar to our original results in Table 2.

Controlling for Family Income.—While the youth application form includes a question about total family income, a response is not required and many youth or their parent(s) choose not to answer. About 10,000 of the original 28,601 analytic sample do not have any income information. Although income was appropriately balanced across same-race status (see Table 1), we conduct sensitivity to income in Tables B2-B4. Each table first replicates the baseline results in Table 2 for comparison by outcome group (School, Education, Social). In the second model, we re-estimate the model on the subsample of matches that have valid income information without controlling for income directly to determine if there is selection among the type of matches that chose to report their family income. Lastly, we control for income to test if our estimates of the same-race effect are biased by family income.[17] Results are similar across each model.

[17] Income is reported as a range (e.g. <$10,000, $10,000-$24,999, etc) which we recode to assign the midpoint value of the range for each bin to create a continuous measure of income.

TABLE B1 — MATCH LEVEL EFFECTS DROPPING MULTI-RACIAL INDIVIDUALS

	School Experience			Education		Social Experience		
	Social Acceptance (1)	School Ability (2)	Attendance (3)	Grades (4)	Education Expectations (5)	Risk Attitudes (6)	Parental Trust (7)	Special Adult (=1) (8)
Panel A: Average Same Race Effect								
Same Race	-0.0133	0.0112	0.0118	-0.0130	-0.0006	0.0075	-0.0091	-0.0132*
	(0.0093)	(0.0121)	(0.0150)	(0.0135)	(0.0096)	(0.0040)	(0.0087)	(0.0059)
Panel B: Same Race Effect by Race of Youth								
Same Race × Black	-0.0060	0.0308*	0.0025	-0.0135	0.0019	0.0094	0.0016	-0.0246***
	(0.0113)	(0.0148)	(0.0199)	(0.0173)	(0.0121)	(0.0057)	(0.0127)	(0.0064)
Same Race × White	-0.0174	-0.0309	-0.0272	0.0156	0.0107	-0.0103	-0.0333*	-0.0034
	(0.0230)	(0.0219)	(0.0344)	(0.0324)	(0.0245)	(0.0074)	(0.0163)	(0.0134)
Same Race × Hispanic	-0.0259	-0.0109	0.0300	-0.0534*	-0.0182	0.0132	-0.0179	0.0020
	(0.0170)	(0.0214)	(0.0320)	(0.0271)	(0.0185)	(0.0070)	(0.0131)	(0.0119)
Same Race × Other	-0.0122	0.0503	0.1464**	0.1380	0.0378	0.0110	-0.0026	-0.0018
	(0.0499)	(0.0954)	(0.0504)	(0.0961)	(0.0619)	(0.0166)	(0.0393)	(0.0287)
Baseline Mean of Cross-Race	2.9148	2.9420	2.9066	3.7225	3.6350	3.8562	3.6117	0.5949
Average Growth of Cross-Race	0.1666	0.1452	-0.0113	-0.0331	0.0437	0.0528	0.0972	0.2916
N	24,155	24,141	24,383	23,833	24,553	24,359	24,436	24,101

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses are clustered at the agency level. All regressions include the controls listed in Table 2. Baseline mean of cross-race is the mean of the outcome at baseline among the cross-race group.

TABLE B2 — MATCH LEVEL EFFECTS CONTROLLING FOR FAMILY INCOME - SCHOOL EXPERIENCE

	Social Acceptance			School Ability			Attendance		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Average Same Race Effect									
Same Race	-0.0088	-0.0012	-0.0011	-0.0016	-0.0012	-0.0007	0.0128	0.0199	0.0205
	(0.0120)	(0.0170)	(0.0170)	(0.0160)	(0.0221)	(0.0220)	(0.0168)	(0.0221)	(0.0217)
Panel B: Same Race Effect by Race of Youth									
Same Race × Black	-0.0054	-0.0122	-0.0130	0.0345*	0.0431**	0.0388*	0.0238	0.0284	0.0231
	(0.0093)	(0.0120)	(0.0120)	(0.0138)	(0.0154)	(0.0154)	(0.0195)	(0.0206)	(0.0195)
Same Race × White	-0.0004	0.0160	0.0164	-0.0143	-0.0179	-0.0163	-0.0264	-0.0238	-0.0215
	(0.0211)	(0.0264)	(0.0263)	(0.0185)	(0.0252)	(0.0253)	(0.0292)	(0.0399)	(0.0392)
Same Race × Hispanic	-0.0218	-0.0076	-0.0075	-0.0165	-0.0240	-0.0233	0.0407	0.0653	0.0664
	(0.0146)	(0.0196)	(0.0196)	(0.0195)	(0.0275)	(0.0270)	(0.0279)	(0.0334)	(0.0339)
Same Race × Other	-0.0190	-0.0064	-0.0071	0.0369	0.1209	0.1177	0.1649***	0.1659***	0.1615**
	(0.0457)	(0.0705)	(0.0701)	(0.0807)	(0.1027)	(0.1008)	(0.0468)	(0.0496)	(0.0505)
Model	Baseline	Has Income Info	With Income Control	Baseline	Has Income Info	With Income Control	Baseline	Has Income Info	With Income Control
Baseline Mean of Cross-Race	2.9138	2.9129	2.9129	2.9360	2.9433	2.9433	2.9052	2.9166	2.9166
N	28,601	17,187	17,187	28,630	17,205	17,205	28,890	17,351	17,351

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses are clustered at the agency level. All regressions include the controls listed in Table 2. Baseline mean of cross-race is the mean of the outcome at baseline among the cross-race group.

TABLE B3 — MATCH LEVEL EFFECTS CONTROLLING FOR FAMILY INCOME - EDUCATION

	Grades			Education Expectations		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average Same Race Effect						
Same Race	0.0145	0.0299	0.0308	-0.0182	-0.0050	-0.0046
	(0.0170)	(0.0216)	(0.0216)	(0.0128)	(0.0149)	(0.0148)
Panel B: Same Race Effect by Race of Youth						
Same Race × Black	-0.0033	-0.0052	-0.0115	0.0111	0.0076	0.0044
	(0.0159)	(0.0201)	(0.0204)	(0.0121)	(0.0148)	(0.0151)
Same Race × White	0.0243	0.0412	0.0439	-0.0052	-0.0110	-0.0095
	(0.0292)	(0.0359)	(0.0357)	(0.0234)	(0.0253)	(0.0253)
Same Race × Hispanic	-0.0473*	-0.0574	-0.0561	-0.0178	0.0184	0.0190
	(0.0240)	(0.0295)	(0.0287)	(0.0178)	(0.0187)	(0.0185)
Same Race × Other	0.1181	0.2111*	0.2054*	-0.0156	0.0623	0.0596
	(0.0821)	(0.0953)	(0.0928)	(0.0556)	(0.0598)	(0.0587)
Model	Baseline	Has Income Info	With Income Control	Baseline	Has Income Info	With Income Control
Baseline Mean of Cross-Race	3.7190	3.7432	3.7432	3.6228	3.6355	3.6355
N	28,270	17,007	17,007	29,111	17,463	17,463

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses are clustered at the agency level. All regressions include the controls listed in Table 2. Baseline mean of cross-race is the mean of the outcome at baseline among the cross-race group.

TABLE B4 — MATCH LEVEL EFFECTS CONTROLLING FOR FAMILY INCOME - SOCIAL EXPERIENCE

	Risk Attitudes			Parental Trust			Special Adult (=1)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Average Same Race Effect									
Same Race	0.0068	0.0073	0.0074	-0.0125	-0.0062	-0.0062	-0.0135	-0.0163	-0.0162
	(0.0039)	(0.0049)	(0.0049)	(0.0104)	(0.0124)	(0.0124)	(0.0077)	(0.0112)	(0.0112)
Panel B: Same Race Effect by Race of Youth									
Same Race × Black	0.0107*	0.0167**	0.0157**	0.0038	0.0099	0.0092	-0.0272***	-0.0277***	-0.0284***
	(0.0053)	(0.0060)	(0.0059)	(0.0120)	(0.0127)	(0.0129)	(0.0060)	(0.0074)	(0.0074)
Same Race × White	-0.0075	-0.0084	-0.0080	-0.0304	-0.0305	-0.0302	-0.0061	-0.0154	-0.0152
	(0.0063)	(0.0082)	(0.0081)	(0.0162)	(0.0177)	(0.0178)	(0.0117)	(0.0146)	(0.0145)
Same Race × Hispanic	0.0153**	0.0123*	0.0125*	-0.0219	-0.0199	-0.0198	-0.0050	-0.0017	-0.0016
	(0.0055)	(0.0060)	(0.0061)	(0.0142)	(0.0201)	(0.0201)	(0.0113)	(0.0157)	(0.0157)
Same Race × Other	0.0179	0.0133	0.0125	0.0057	0.0419	0.0414	-0.0142	-0.0202	-0.0207
	(0.0133)	(0.0149)	(0.0146)	(0.0313)	(0.0395)	(0.0394)	(0.0276)	(0.0465)	(0.0462)
Model	Baseline	Has Income Info	With Income Control	Baseline	Has Income Info	With Income Control	Baseline	Has Income Info	With Income Control
Baseline Mean of Cross-Race	3.8543	3.8561	3.8561	3.6102	3.6164	3.6164	0.5916	0.5996	0.5996
N	28,861	17,328	17,328	28,948	17,369	17,369	28,571	17,120	17,120

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors in parentheses are clustered at the agency level. All regressions include the controls listed in Table 2. Baseline mean of cross-race is the mean of the outcome at baseline among the cross-race group.