

# Who Benefits Most from a Same-Race Mentor? Optimal Matching in Big Brothers Big Sisters\*

Brachel Champion<sup>†</sup>, Zachary Szlendak<sup>‡</sup>, and Corey Woodruff<sup>§</sup>

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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 Big Brothers Big Sisters matches, we estimate a selection-on-observables model that is motivated by the matching heuristic used by the youth's case worker and balance of pre-match observables across same-race status. We find that on average youth in same-race/ethnicity mentoring relationships had no significant improvements compared to those in cross-race matches. However, Black and Hispanic youth who were assigned a same-race/ethnicity mentor had slightly higher self-perceived school ability and improved attitudes concerning risky behaviors. In contrast to previous work, we find little evidence that race/ethnicity-matching improves grades or expectations for future educational attainment. These results imply that when minority mentors are in short supply, matching on race or ethnicity at the expense of another desirable trait may not lead to improved youth development.

*JEL:* H51, I13, J26

*Keywords:* Youth mentoring, race congruence, matching, child development

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<sup>†</sup>Corresponding author: Department of Economics, University of Colorado Boulder, 256 UCB, Boulder, CO 80309; [brachel.champion@colorado.edu](mailto:brachel.champion@colorado.edu)

<sup>‡</sup>Institute for Defense Analyses; [zszlenda@ida.org](mailto:zszlenda@ida.org)

<sup>§</sup>Amazon, Inc; [corey.woodruff@colorado.edu](mailto:corey.woodruff@colorado.edu)

## 1 — Introduction

In 2014, an estimated 4.5 million youth in the U.S. were in structured, one-to-one mentoring relationships (Bruce & Bridgeland, 2014). Youth mentoring programs are held in high regard for their positive impacts on the participants' development. Experimental evaluations of mentoring programs have shown that mentored youth have better social-emotional skills (Grossman & Tierney, 1998) and increased likelihood of completing high school and enrolling in post-secondary education relative to non-mentored youth (Falk, Kosse, & Pinger, 2020; Rodriguez-Planas, 2012).<sup>1</sup> However, the mechanisms that produce effective one-to-one youth mentoring are less clear. Mentors may increase youth development by modeling positive emotional behavior (Hayes, Castonguay, & Goldfried, 1996), refining critical thinking skills (Radziszewska & Rogoff, 1991; Vygotsky, 1978), influencing attitudes towards risky behaviors (Beier, Rosenfeld, Spitalny, Zansky, & Bontempo, 2000), or relating advice based on a shared background. In particular, sharing a racial or ethnic identity is hypothesized to build stronger emotional bonds that lead to more positive interactions. Positive impacts of race-matching on youth outcomes has been well documented in a classroom context (Dee, 2004; Egalite, Kisida, & Winters, 2015; Harbatkin, 2021), but how important is a shared identity in a youth mentoring relationship?

To answer this question, we estimate the impact of assigning a same-race/ethnicity mentor on the social, emotional and academic outcomes of the universe of youth participating in the Big Brothers Big Sisters of America (BBBS) mentoring program. BBBS is the largest youth mentoring organization in the U.S., supporting over 135,000 one-to-one mentoring relationships in 2019 across 200 local agencies in all 50 states. BBBS has been shown to improve perceptions of scholastic ability, risk attitudes, and self-esteem of participants compared to non-mentored youth (Grossman & Tierney, 1998; Herrera, Grossman, Kauh, & McMaken, 2011). It is hypothesized that the success of the BBBS program is in part due to assigning a mentor of the same race or ethnicity. Furthermore, many of the youth participating in BBBS and their parent(s) express preferences for a mentor of the same race or ethnicity. But there is often a mismatch between the supply of minority youth (Littles) and minority mentors (Bigs). In 2018, 72% of youth in the BBBS program were minorities, while only 32% of volunteers were non-white.<sup>2</sup> As a result, BBBS must

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<sup>1</sup> DuBois, Portillo, Rhodes, Silverthorn, and Valentine (2011) provide a thorough review of the literature evaluating youth mentoring programs.

<sup>2</sup> Based on authors' calculations.

choose how to allocate a scarce supply of minority mentors to mentees. Therefore, it is important to understand which outcomes are most improved by race/ethnicity-matching, and for which youth this premium is largest.

It is not clear that same-race matching should be preferred to cross-race. Rhodes, Reddy, Grossman, and Maxine Lee (2002) provides a detailed explanation of the potential social and cultural costs and benefits of both same- and cross-race mentor matching. Having a mentor that shares your racial or ethnic background can promote trust and aid in establishing a relationship. Mentors of a different racial or ethnic background may not be able to adequately support the youth in the challenges they face. Having successful role models of similar racial background may also empower youth — especially of minority heritage — to achieve higher levels of success themselves. On the other hand, cross-race matches can still be valuable and lead to many of the same positive outcomes as same-race matches. Racial diversity in mentoring 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.

The observed variation in race matching among completed matches comes from two sources. First, there is variation across local agencies in the relative supply of minority Bigs. This variation across agencies is in part generated by neighborhood characteristics that could directly influence child outcomes. Second, there is race-matching variation within an agency that arises from case workers curating matches. Our discussions with BBBS administrators lead us to conclude that matches are made largely on the basis of race, gender, shared interests and geographic proximity between the Big and the Little, conditional on the availability of an eligible Big. Ex-ante, there is no reason to prefer either source of variation. The cross-agency variation is not subject to idiosyncratic case worker selection bias, but may be driven primarily by contextual effects that are correlated with youth outcomes. On the other hand, within-agency variation generated by case workers is likely uncorrelated with local characteristics but may be correlated with unobserved youth characteristics. Section 2 outlines the within-agency matching process that leads to our preferred match-level selection-on-observables model. The within-agency variation in race-matching is plausibly exogenous conditional on observed match characteristics because case workers must balance all the dimensions of potential matches when selecting a Big to assign. Key to our analysis, we use the rich set of controls available in the data to show the pre-match characteristics of the Littles and Bigs as well as Littles' outcomes do not differ substantially

between same- and cross-race matches. However, if unobserved Big or Little characteristics affect both the probability of same-race assignment and growth in the Little's outcomes then estimates of same-race effects would be biased. To address match-level unobservables, we estimate a similar model at the agency level that avoids bias from endogenous match formation, but at the cost of potentially introducing bias from contextual effects. Results are similar across both models which suggests that bias from either source of variation is minimal.

We use the universe of BBBS matches from 2010 to 2018 to estimate the causal effect of assigning a same-race/ethnicity Big (hereafter referred to as "same-race" for brevity) on the Little's social, emotional and academic outcomes after a year of mentoring. On average, we find that Littles matched to a same-race Big do not have significantly higher growth in outcomes relative to those in cross-race matches. However, minority Littles experience some improvements when paired with a Big of the same race: Black and Hispanic Littles in same-race matches experience small but statistically significant improvements in their risk attitudes relative to their cross-race counterparts, and Black Littles had higher self-perceived school ability. On the other hand, Hispanic Littles in cross-race matches had higher grades in reading, social studies and science, and Black Littles were more likely to identify a non-guardian mentor after a year of cross-race mentoring. 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 seem to impact the Little. These results imply that race-matching is an important dimension for youth mentoring organizations to consider when targeting the social and emotional development of minority youth. However, when the supply of minority mentors is scarce, matching on race at the expense of other important traits may not produce the fastest growth in youth. It may even be the case that for some youth cross-race mentoring has even higher benefits.

We add to the literature on youth mentoring by identifying the causal effect of race-matching on the outcomes of all youth participating in BBBS during 2010-2018. Grossman and Tierney (1998) and Herrera et al. (2011) randomly assigned youth to receive BBBS mentorship and found they had improved self-perceptions of scholastic ability, risk attitudes, and self-esteem relative to those who did not receive a mentor. In both randomized control trials, the sample size was small relative to overall scale of BBBS (about 1,100 youth participated in each study) and neither examine the impacts by the type of Big 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 Big was not randomized among the treated group. Hence, it is not clear if same-race mentoring increases Littles' development at a faster rate than cross-race mentoring or improves outcomes relative to non-mentored youth. We extend youth mentoring by considering the universe of BBBS participants and provide quasi-experimental evidence of the impacts of race-matching that accounts for the non-random assignment of minority Bigs.

We also contribute to the literature on racial congruence by isolating the indirect effects of an adult mentor on the youth's self-perceived social, emotional and academic abilities. Much of the existing work is focused on educational contexts, particularly K-12 schooling. Harbatkin (2021) outlines how student outcomes are affected through "active teacher effects" and "passive teacher effects". In the first case, teachers may leverage their authority (either consciously or unconsciously) to assign lower grades or dole out harsher punishments for misbehavior to students of a different race (Bates & Glick, 2013; Dee, 2005; Ouazad, 2014). At the same time, teachers may have a passive influence on the student by appealing to their motivations and self-confidence in daily interactions, and this effect may have differential impacts by racial congruence (Van Ewijk, 2011). In a youth mentoring setting such as BBBS, the adult mentors have little to no formal authority or evaluative power over the youth, allowing us to attribute the estimated effects to the passive channel. In contrast, studies that examine the effect of same-race teacher assignment on test scores (Dee, 2004; Egalite et al., 2015; Harbatkin, 2021) or career path choice (Kofoed & McGovney, 2019) cannot disentangle the passive effects from the active effects of race-matching.

The rest of the paper is structured as follows. Section 2 outlines the BBBS program, the process of matching Big to Little and the real-world constraints that produce variation in race matching that is plausibly exogenous after conditioning on a set of observed covariates. Section 3 describes the data on the universe of BBBS matches during the 2008-2018 period used. Section 4 formalizes the empirical strategies used to identify the causal effect of race-matching on Littles' outcomes. Section 5 displays the results of estimating these models and discuss their implications. Finally, Section 6 concludes.

## 2 — Big Brothers Big Sisters of America

The Big Brothers Big Sisters of America (BBBS) organization is a volunteer-based, one-on-one youth mentoring organization that pairs a youth mentee (Little) and an adult mentor (Big). Founded in 1904, BBBS now has over 200 agencies across all 50 states and is currently the largest youth mentoring organization in the United States, supporting over 135,000 matches in 2019 alone. The two largest programs that BBBS operates are “community-based” (CB) and “site-based” (SB) 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 BBBS office. In contrast, CB matches spend one-on-one time in their community in activities like playing games at a park, attending a sporting event, or visiting a museum. BBBS expects Bigs to become a role model for their Little by consistently spending time together (e.g. 3-4 times a month). Bigs are encouraged to plan activities that foster a friendship with their Little, such as going for a walk or playing a board game. BBBS does not expect Bigs to invest large amounts of money in their Little or spend time tutoring. The goal for the relationship is to inspire the Little through positive interactions and “quality time.”

Potential Bigs and Littles apply for the program at their local BBBS agency. Littles are typically between the ages of 8 and 13 when entering the program, and can stay enrolled through age 18. Once applied, an assigned case worker learns their preferences for a Big and administers a baseline survey. Bigs are usually 21 years or older, must pass a background check, and complete an in-person interview with their BBBS case worker. After the interview is complete, the Big enters the pool of available mentors. Bigs are assigned to a Little by the case worker based on a variety of criteria that can include gender, race, shared interests, and travel time. Once assigned, the Big and Little meet and mutually agree to form the match. If a match is successful, the local agency administers a follow-up survey every twelve months the match survives. Either party may end the match at any time, though the case worker provides continual support throughout to avoid early terminations. In our sample (described in detail in Section 3), matches last for approximately 2-3 years (mean: 34.93 months, median: 29.20 months).

It is common for Littles and their parent(s) to state a preference for same-gender matches, and many request a Big of the same race. However, the Little’s preferences are constrained by the available supply of Bigs of a given race or gender. Figure 1 shows a majority of Bigs in our sample of matches are white (69%), while a majority of Littles are Black or Hispanic (66%); though almost

all matches are same-gender. This suggests that Littles are willing to accept a Big that does not exactly match their stated preferences.<sup>3</sup> In addition to balancing preferences, case workers face a limited supply of non-white Bigs. Figure 2 shows the fraction of Bigs and Littles participating in a match within a local BBBS agency, separated by race. A point in the upper diagonal of the plot implies Bigs of that race are relatively overrepresented at their agency compared to Littles, while a point in the lower diagonal implies underrepresentation, and a point on the plotted 45 degree line shows that the proportions of Bigs and Littles of a particular race are balanced. Figure 2 shows that in our sample, often a majority share of Bigs are white while a majority share of Littles are Black or Hispanic within a given agency.<sup>4</sup>

### 3 — Data

Our data contain the universe of BBBS matches between 2008 and 2019. The data were supplied by BBBS, and contain information on the race, gender and age of every Big and Little that participated in BBBS mentorship in the United States. Also included are measures of the Little's socioeconomic status (number and type of guardians in the household, on free/reduced lunch) and the Big's educational attainment. Most importantly, our data set also includes the Youth Outcome Survey (YOS) which is administered to every Little upon entering the program (hereafter referred to as the baseline survey) and then again every year (12 months or school year for community- and school-based<sup>5</sup> matches, respectively) that the match continues (hereafter referred to as the follow-up survey). The YOS includes 33 categorical response questions regarding the Little's outlook on their social experience, education and school experience. These 33 questions are aggregated by BBBS into summary scores by taking the average of the component scores. BBBS generated eight summary scores: social acceptance, school ability, truancy, grades, education expectations, risk attitudes, parental trust, and special adult.<sup>6</sup>

The social experience group is comprised of social acceptance, school ability, and truancy.

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<sup>3</sup> We assume Littles reveal their true preferences since requesting a Big with certain qualities is costless to the Little and does not affect the probability of being matched overall.

<sup>4</sup> Anecdotally, the director of the local BBBS agency informed us that the supply of Bigs is often mismatched with the supply of Littles: most Big applicants are white and/or female and most Little applicants are minorities and/or male.

<sup>5</sup> School-based matches are a subset of site-based matches.

<sup>6</sup> YOS also includes questions on juvenile arrest but we omit these outcomes due to lack of variation; most Littles in our sample were never arrested.

The social acceptance score measures the Little's perceptions of their friendships (e.g. "I find it hard to make friends"). The school attitudes measures the Little's perceptions of their scholastic ability (e.g. "I am very good at my schoolwork", and "I feel that I am just as smart as other kids"). Truancy is the Little's self-reported school absences and days late for school.<sup>7</sup>

Education contains two outcomes: grades and education expectations. Grades is the average letter grade (mapped from F-A to 1-5 correspondingly) the Little received in mathematics, reading or language arts, social studies, and science. Education expectations contains three questions regarding their prospective educational outcomes (e.g. "How sure are you that you will finish high school?").

Lastly, social experience includes risk attitudes, parental trust, and special adult. Risk attitudes measures the Little's perceptions of whether certain risky behaviors are "okay". These behaviors include using tobacco, drugs and alcohol, truancy, and misbehavior (hitting someone and breaking rules in school). Parental trust measures the Little's perceptions of how often their parents respect their feelings, accept them as they are, and try to be understanding when the Little is angry. Lastly, special adult is composed of a single dummy indicating whether the Little feels that they have a non-guardian adult who "does a lot of good things" for the little.

Our sample includes the set of successful CB matches that lasted at least one year. Our analysis examines the change in YOS scores at the first follow-up survey relative to their baseline survey. Hence, we omit any matches that do not have a baseline survey. This amounts to removing any matches that began prior to 2010 since BBBS did not begin administering a baseline YOS survey until 2010. We exclude any matches that do not have at least one follow-up YOS administered. It is possible that matches that did not last through the first follow-up survey differ in some important way from those that did last. Table A1 tests for differences between matches that lasted through the first follow-up, and those that did not. 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. We do not find evidence that these matches differ in any important ways based on observable characteristics of either the Big or the Little. 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 were not balanced across same-race status. The resulting sample includes about 29,000 matches. We do not observe

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<sup>7</sup> Frequencies are categorically binned 1-4, i.e. 1=no absences, 2=1-2 absences, etc.



any information on proposed matches that failed to form. Many agencies operate a waiting list for Littles who were not able to be matched, so we assume that the probability of a Little rejecting a proposed match is low.

## 4 — Methodology

Because Bigs are assigned to Littles by a case worker, the variation in same-race matches is likely non-random. In reality, BBBS agencies face severe shortages in the supply of minority Big applicants which restricts the case worker's ability to be overly selective when assigning a Big to the Little. Figure 2 shows the fraction of Bigs and Littles at a local BBBS agency that are in each race category. In our sample, a majority of Bigs at an agency are often white while a majority of Littles are often Black or Hispanic. This within-sample stylized fact coincides with the experience of case workers we spoke to: the pool of Big applicants is predominantly white while the pool of Little applicants is predominantly people of color.<sup>8</sup> As mentioned previously, families often express preferences for a Big who is the same gender and race as the Little. Additionally, they commonly ask to be matched with a Big that shares interests or has a particular level of education. These requirements further restrict the pool of eligible Bigs that fulfill the requests of the Little. Our data support this conclusion. Figure 1 shows that almost all matches are same-gender but there is significant variation in same-race status. Case workers face the difficult challenge of balancing the stated preferences of the Little with other dimensions of the match that increase the expected match length such as geographic proximity.

The real world constraints faced by case workers when deciding which Big to assign a Little motivates a selection-on-observables empirical strategy. Conditional on the race of the Little, the probability of matching a same-race and otherwise-eligible Big depends on the local supply of applicants. If the pool of eligible minority Big applicants is large, the probability of being assigned a same-race Big is high and case-workers would have the freedom to match on other unobservable characteristics, potentially biasing our results. This does not seem to be the case (see Figure 2), however we only observe the Bigs who were matched which represents a subset of the pool of applicants. If the local demographic composition of potential Bigs is endogenous to the growth in Little's outcomes, or if the probability of an eligible Big applying to BBBS is correlated with the

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<sup>8</sup> In particular, our contact at the local BBBS agency estimated that about 80% of Bigs applicants were white, while 80% of Little applicants were people of color.

observed characteristics of Littles in the area, our results would be biased. Table 1 shows that same- and cross-race matches are observably similar at baseline which suggests there is little selection bias induced by the case worker. However, we cannot directly test for selection on unobservable match characteristics. To combat this possibility, we estimate specifications at both the match- and agency-level. After controlling for demographic and socioeconomic characteristics of the Big and Little in each, the remaining variation in same-race status is plausibly exogenous, though each approach is susceptible to different types of bias. We describe these methods and the threats to identification below.

#### 4.1. Match-Level Estimation

We first identify the individual impacts on Littles of race-matching by estimating the effect of being assigned a same-race Big at the match-level. Specifically, we estimate the equation

$$Y_{iat}^F = \alpha + \beta \text{SameRace}_{iat} + \eta Y_{iat}^B + \Gamma' \text{LittleChars}_{iat} + \Theta' \text{BigChars}_{iat} + \alpha_t + \varepsilon_{iat} \quad (1)$$

for match  $i$  at agency  $a$  in year  $t$ .  $\text{SameRace}_{iat}$  indicates whether the Big and the Little in match  $i$  are of the same race,<sup>9</sup> and  $Y_{iat}^F$  denotes the value of the outcome variable at the Little's first follow-up survey. We control for the initial value of the outcome variable,  $Y_{iat}^B$ , to account for any baseline differences among Littles.  $\text{LittleChars}_{iat}$  is a vector of the Little's race and age, as well as measures of socioeconomic status, and  $\text{BigChars}_{iat}$  is a vector of the Big's race, age, and educational attainment. Finally, year fixed effects are included to account for any trends in outcomes over time.

To interpret  $\beta$  in Equation 1 as the causal effect of a same-race match, it must be the case that whether the Little was matched with a same-race Big is as good as randomly assigned, conditional on the observable characteristics of both the Little and the Big. Because of the heuristic approach to matching described in Section 2, we include controls for the Little's age and socioeconomic status and the Big's age and educational attainment to account for the decision criteria used by the Little's case worker in selecting a Big. 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 worker does, such as personal interests mentioned during the interview.

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<sup>9</sup> 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 Big and Little both being in the "other" category.

One possible threat to identification is that Littles are assigned to same-race matches based on some unobserved match characteristics that are correlated with growth in YOS outcomes. Because Littles often fill out the baseline YOS prior to being matched with a Big, individual case workers may be more likely to allocate same-race Bigs to the Littles with higher baseline values of certain outcomes.<sup>10</sup> If case workers expect Littles with higher YOS scores to benefit more from a same-race Big our estimates would be positively biased. Table 1 shows the means and standard deviations of Little's YOS values by match type, the mean difference in scores, the T-statistic of the difference, and the normalized (sample size independent) difference. We find no evidence that Littles with higher baseline YOS scores are more likely to be matched with a same-race Big. Additionally, case workers may assign Littles to same-race matches based on their own demographics or socioeconomic status, or those of the Big. Table 1 also shows no significance differences in the demographics and socioeconomic status of Littles and Bigs 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. Excluding agency fixed effects potentially introduces time-invariant bias from two sources: region-specific demographic composition and idiosyncratic agency behavior.<sup>11</sup> We account for region-specific factors that might bias our results through the inclusion of other characteristics of the Big and Little. 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 Little's growth. In the latter case, some agencies may be more adept at developing successful same-race matches than others by following a set of idiosyncratic best practices in matching or case management. Indeed, local agencies have the freedom to deviate slightly from BBBS policies, possibly to the advantage of Littles in same-race matches. Although our discussions with the local agency did not lead us to believe that there were deviations from organization-wide policy that are correlated with the growth among Littles, we address this potential bias more systematically with an agency-level empirical model. We describe this strategy in detail in Section 4.3.

It is worth noting that participation in the BBBS program is voluntary, so there is likely selection into who enrolls in the program. For that reason, it is important to point out that we

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<sup>10</sup> In some cases the Little fills out the baseline YOS after a potential Big has been identified but prior to the initial meeting.

<sup>11</sup> Agency fixed effects would account for the former as offices serve the local community within a certain geographic distance.

can only unbiasedly estimate the relative effects of twelve months of mentoring on outcomes for Littles in same-race matches relative to those cross-race matches, *among youth who elected to enroll in BBBS mentoring*. We cannot say what the effect of same-race mentoring is relative to a counterfactual Little who did not receive any mentoring.<sup>12</sup>

#### 4.2. Heterogeneity by Little Race

We re-estimate Eq. 1 interacted with dummies for each race of the Little in the match to determine which Littles are most affected by race-matching. The heterogeneous treatment effect model is

$$\begin{aligned} Y_{iat}^F = & \alpha + \beta_B \text{SameRace} \times \text{Black}_{ia} + \beta_W \text{SameRace} \times \text{White}_{ia} \\ & + \beta_H \text{SameRace} \times \text{Hispanic}_{ia} + \beta_O \text{SameRace} \times \text{Other}_{ia} \\ & + \eta Y_{iat}^B + \Gamma' \text{LittleChars}_{iat} + \Theta' \text{BigChars}_{iat} + \alpha_t + \varepsilon_{iat}. \end{aligned} \quad (2)$$

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

#### 4.3. Agency-Level Estimation

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

$$\bar{Y}_{at}^F = \alpha + \beta \overline{\text{SameRace}}_{at} + \eta \bar{Y}_{at}^B + \Gamma' \overline{\text{LittleChars}}_{at} + \Theta' \overline{\text{BigChars}}_{at} + \alpha_t + \nu_{at} \quad (3)$$

where  $\bar{Y}_{at}^F$  is the mean of the outcome variable in the follow-up survey at agency  $a$  in year  $t$ .  $\overline{\text{SameRace}}_{at}$  is the fraction of matches in agency  $a$  in year  $t$  that are same-race,  $\bar{Y}_{at}^B$  is the agency-year mean of the outcome variable in the baseline survey, and  $\overline{\text{LittleChars}}_{at}$  and  $\overline{\text{BigChars}}_{at}$  are agency-year means of the Little and Big characteristics include in Equation 1, respectively.

In order to interpret  $\beta$  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

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<sup>12</sup> For estimates of the impact of BBBS enrollment see Grossman and Tierney (1998), Herrera et al. (2011), Park, Liao, and Crosby (2017).

bias from contextual factors such as differences in school spending or the local demographic composition which informs the pool of potential Bigs and Littles. Controlling for the average baseline YOS score combats bias from social factors, while the proportions of Littles receiving free/reduced lunch, Littles in a single-parent home, and Bigs in each educational attainment bin control for socioeconomic factors. Lastly, we include the proportion of Bigs and Littles in each race category which controls for local demographic trends.

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 minority Bigs to minority Littles more often match promising Littles with same-race Bigs, 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 Eq. 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 Big and Little 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. The second to last row shows the mean of the dependent variable at baseline among cross-race matches for context. In the match-level regressions, being matched with a mentor of the same race does not appear to impact youth outcomes relative to being matched with a mentor of a different race, with one exception: Littles who were paired with a same-race Big were 1.35 percentage points ( $p < 0.05$ ) less likely to report having a special adult in their life relative to those in cross-race matches. The lack of significant impacts on average imply there are no relative benefits to being matched with a Big of the same race. However, the literature on race-congruency suggests there are positive effects of both same- and cross-race matching. In this case, the effects might cancel out, leading to an overall effect close to zero. A final possibility is that certain youth benefit from having a same-race

match while others do not. In this case, estimating the effects for the full sample might mask important heterogeneity in the same-race effect.

Panel B contains the results of estimating a model where the same-race indicator variable is interacted with a set of indicators for the Little's race. As the literature suggests, it may be the case that mentoring improves school outcomes for minority Littles when paired with a Big of the same race. Our results show this to be true in some instances. Black Littles in same-race matches saw a 0.0345 point improvement ( $p < 0.001$ ) in their self-perceived school ability relative to those who were mentored by a cross-race Big. Asian, Pacific Islander and American Indian Littles in same-race matches were less truant after a year of mentoring (-0.1655,  $p < 0.001$ ). We find no effects of race-matching on the Little's self-perceived social acceptance for any race category.

Unlike the literature on race-congruency, we find few impacts of race-matching on educational outcomes of Littles. Black Littles matched to Black Bigs had slightly lower grades (-0.0034) but this effect is imprecisely estimated. Hispanic Littles 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 Big 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 Little's language ability or the primary language spoken at home. However, Table 3 shows that the effect on Hispanic Littles' 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 Bigs rarely spend time tutoring their Little provide suggestive evidence that improved English language ability is the mechanism behind the cross-race effects on Hispanic Littles' grades. On the other hand, Littles in the Other category saw relatively large improvements in their grades as a result of race-matching (0.1183,  $p < 0.05$ ), and this effect is driven by higher math and science grades (0.1795,  $p < 0.01$  and 0.1420,  $p < 0.05$ , respectively). Lastly, we find no effects of race-matching on the Little's expectations for educational attainment.

Turning to the outcomes related to the Little's social interactions, we find a 0.0106 point improvement in the risk attitudes of Black Littles matched with a Black Big relative to those in cross-race matches ( $p < 0.01$ ). We observe a similarly sized effect of 0.0153 for Hispanic Littles ( $p < 0.001$ ) and no statistically significant effects for white Littles or Littles 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 Little in a same-race match reports having a special adult in their life appears to be driven by Black Littles. Because these effects can be interpreted as the relative impact of having a same-race Big compared to a Big of another race, these results suggest that Black youth may benefit along some dimensions from having a non-Black Big.

## 5.2. *Agency-Level Results*

Table 4 contains results from estimating Equation 3. These results are robust to any match-level bias but are susceptible to any contextual effects not captured by our controls for Big and Little demographics and socioeconomic status. The agency-level results are qualitatively similar in most cases, only scaled by the relative size and match composition of an agency. This lends credibility to the assumption that there is a negligible amount of selection at the match level. Specifically, unless bias from geographic variation is the same direction and magnitude as the 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 Littles' 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,  $p < 0.10$ ). 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 Little. This allows us to examine the impacts of increasing the proportion of same-race matches within Little race subgroups. Although statistically insignificant, we find similarly positive effects on school ability for Black Littles. The significant effect of race-matching on grades is driven by white Littles 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 Littles ( $p < 0.01$ ). At the agency level, we find that increasing the share of race-matched Hispanic Littles increases education expectations by 0.002314 points ( $p < 0.05$ ). Lastly, we find improvements in risk attitudes among Black Littles and Littles in the Other category, but no effects on parental trust or the prevalence of Littles reporting a special adult.

Taken together, Tables 2 and 4 show that race-matching generates modest improvements in the Little's self-perceptions and problem behavior, primarily among Black and Hispanic Littles.

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 BBBS is a social mentoring program. Bigs often spend time bonding with their Little in leisurely activities rather than tutoring. Indeed, BBBS describes their program as primarily impacting the social and emotional development of the Little. The lack of findings for educational outcomes implies that pure role-modeling is not the primary driver of race-congruence effects on tests scores, high school completion or college enrollment for minority youth. Outside of the classroom, race-matched mentoring appears to have the highest marginal impacts on the non-cognitive outcomes of youth.

## 6 — Conclusion

In this paper, we estimated the causal effect of same-race mentorship relative to cross-race mentorship on the outcomes of youth who participated in BBBS mentoring for twelve months. We found that Littles who were assigned a same-race Big had almost no improvements relative to those assigned a cross-race Big, on average. It is possible that both same- and cross-race mentoring have positive impacts for certain Littles and when averaged across the entire sample negate each other. Heterogeneity analysis by the race of the Little revealed this is somewhat the case. Same-race matching improved self-perceived school ability for Black Littles, truancy for Littles in the Other category, and risk attitudes for both Black and Hispanic Littles. On the other hand, Hispanic Littles in cross-race matches had slightly higher grades after a year of mentoring, and Black Littles in cross-race matches were more likely to identify a special adult in their life.

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 (as is the case with BBBS) organizations must choose how to allocate the scarce supply of eligible mentors to youth. We showed that certain minority 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 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 BBBS.<sup>13,14</sup>

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 Little's self-perceptions more than academic performance or attitudes towards adults. This suggests that, in the context of BBBS, mentorship improves youth self-confidence but may not impact their academic skill or perceptions of authority. This is not unexpected as Bigs in community-based matches rarely spend time helping their Little study or complete homework. In addition, we focused on the first twelve months of mentoring. It may be the case that affecting the Little's academic ability or worldview takes more than one year, and further research is needed to estimate the causal impacts on long-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.

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<sup>13</sup> This policy recommendation relies on the assumption that the additional resources would increase the number of minority Bigs, either through matches lasting longer (i.e. from greater support) or from having more minority Bigs enter the program (i.e. more flexibility in training and potentially recruitment efforts).

<sup>14</sup> <https://www.colorado.gov/pacific/sites/default/files/Income35.pdf>, Accessed April 6th, 2021.

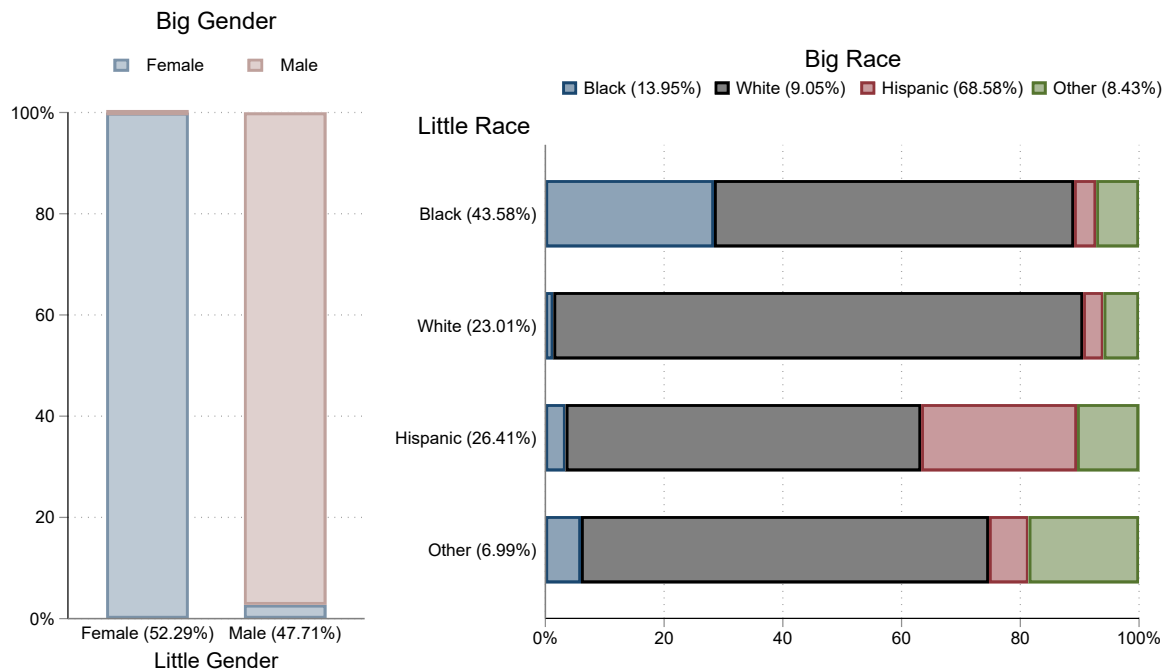
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## Tables and Figures

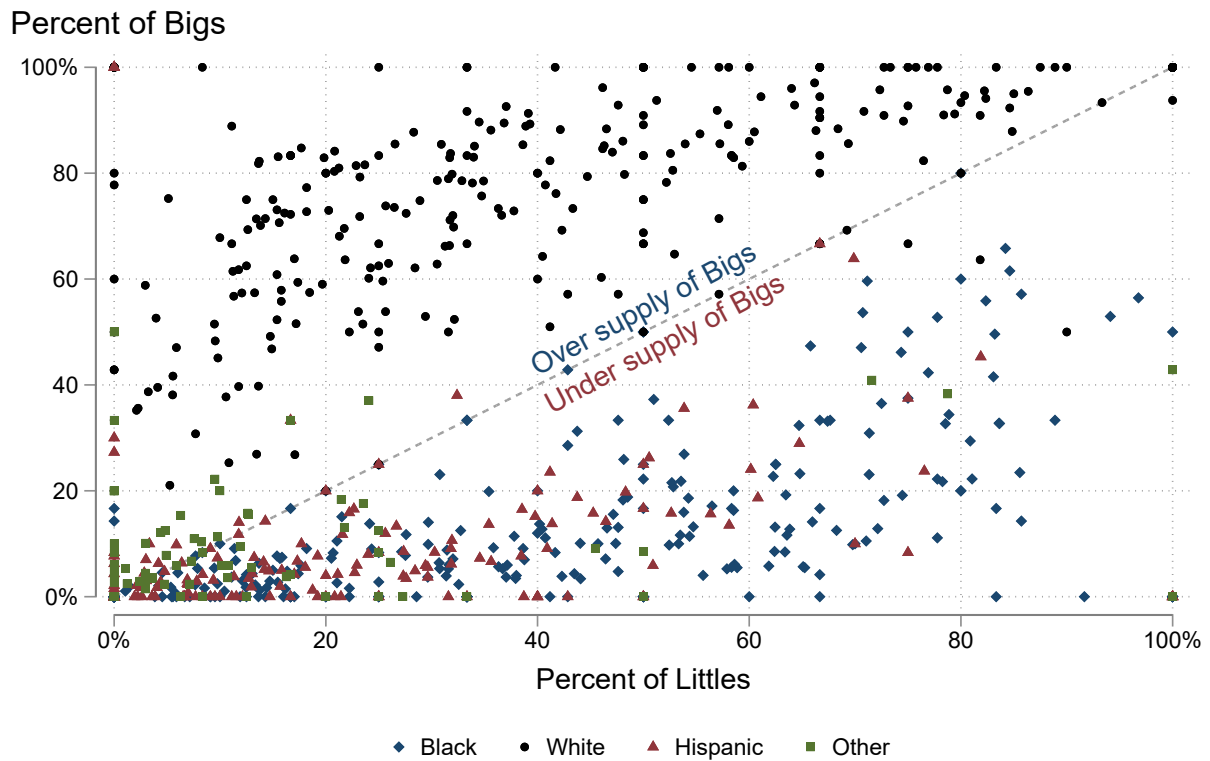
**Figure 1 — Percent of Matches by Big/Little Race and Gender**



*Notes:* Left panel shows the percent of Littles by gender matched to female and male Bigs. Percent of full sample that are female or male Littles shown in parentheses. Right panel shows the percent of Littles by race matched to a Big of a certain race. Percent of the full sample of Littles that are of each race are shown in parentheses along the vertical axis.

Percent of the full sample of Bigs 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, race-congruence is defined using the associated subcategories. The height of each colored portion of the bar shows the proportion of Littles matched to a male or female Big, by the gender of the Little.

**Figure 2 — Variation in Racial Composition of Bigs and Littles by Agency**



*Notes:* Each point represents the proportion of Bigs and Littles of a particular race within a local BBBS agency. The upper diagonal represents all of the agencies where the proportion of Bigs in a particular category is greater than the proportion of Littles 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 — Same- vs Cross-Race Match Balance Table**

	Same-Race			Cross-Race			Difference		
	Mean	SD	N	Mean	SD	N	Mean	T-stat	Mean/SD
<b>Youth Outcomes Survey</b>									
Social Acceptance	2.84	0.65	12,427	2.91	0.62	16,533	-0.07	-9.90	-0.08
School Attitudes	2.92	0.60	12,443	2.94	0.59	16,568	-0.01	-2.06	-0.02
Education Expectations	3.55	0.67	12,564	3.62	0.62	16,764	-0.07	-9.60	-0.08
Grades	3.70	0.80	12,414	3.72	0.77	16,510	-0.02	-2.22	-0.02
Risk Attitudes	3.85	0.26	12,507	3.85	0.27	16,692	-0.00	-0.33	-0.00
Parental Trust	3.57	0.58	12,545	3.61	0.55	16,718	-0.04	-6.22	-0.05
Truancy	2.09	0.82	12,527	2.09	0.85	16,668	-0.00	-0.31	-0.00
Special Adult (=1)	0.56	0.50	12,397	0.59	0.49	16,474	-0.03	-4.96	-0.04
<b>Little Characteristics</b>									
Male(=1)	0.47	0.50	12,653	0.48	0.50	16,882	-0.01	-1.28	-0.01
Age	11.25	1.83	12,649	11.15	1.83	16,870	0.10	4.67	0.04
Free-Reduced Lunch (=1)	0.74	0.44	12,653	0.81	0.39	16,882	-0.07	-14.34	-0.12
Single-Parent HH (=1)	0.67	0.47	12,653	0.70	0.46	16,882	-0.02	-4.39	-0.04
Two-Parent HH (=1)	0.21	0.41	12,653	0.20	0.40	16,882	0.01	2.45	0.02
<b>Big Characteristics</b>									
Male (=1)	0.46	0.50	12,653	0.47	0.50	16,882	-0.01	-1.15	-0.01
Age	37.89	12.08	12,652	36.49	11.15	16,881	1.41	10.34	0.09
Less than High School (=1)	0.01	0.09	12,653	0.01	0.07	16,882	0.00	3.17	0.03
High School Graduate (=1)	0.06	0.24	12,653	0.04	0.20	16,882	0.02	8.25	0.07
Some College (=1)	0.22	0.42	12,653	0.17	0.38	16,882	0.05	10.59	0.09
Associate Degree (=1)	0.07	0.25	12,653	0.05	0.21	16,882	0.02	6.53	0.05
Bachelor's Degree (=1)	0.43	0.50	12,653	0.51	0.50	16,882	-0.07	-11.94	-0.10
Advanced Degree (=1)	0.21	0.41	12,653	0.23	0.42	16,882	-0.02	-4.24	-0.04
<b>Match Characteristics</b>									
Match Length (mos)	34.87	19.79	12,653	34.98	19.83	16,882	-0.11	-0.48	-0.00

*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 Big and Little. 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 Little's Follow Up Outcomes at the Match Level**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	School Experience			Education		Social Experience		
	Social Acceptance	School Ability	Truancy	Grades	Education Expectations	Risk Attitudes	Parental Trust	Special Adult
<b>Panel A: Average Same Race Effect</b>								
Same Race	-0.0088	-0.0015	-0.0128	0.0145	-0.0181	0.0068	-0.0125	-0.0135*
	(0.0116)	(0.0110)	(0.0154)	(0.0151)	(0.0114)	(0.0040)	(0.0102)	(0.0067)
<b>Panel B: Same Race Effect by Race of Little</b>								
Same Race × Black	-0.0055	0.0345***	-0.0238	-0.0034	0.0112	0.0106**	0.0038	-0.0271***
	(0.0107)	(0.0102)	(0.0150)	(0.0142)	(0.0098)	(0.0034)	(0.0095)	(0.0066)
Same Race × White	-0.0004	-0.0143	0.0264	0.0243	-0.0051	-0.0075	-0.0304	-0.0061
	(0.0215)	(0.0196)	(0.0266)	(0.0266)	(0.0214)	(0.0070)	(0.0176)	(0.0110)
Same Race × Hispanic	-0.0218	-0.0165	-0.0406	-0.0473*	-0.0178	0.0153***	-0.0219	-0.0050
	(0.0148)	(0.0150)	(0.0217)	(0.0203)	(0.0154)	(0.0044)	(0.0140)	(0.0096)
Same Race × Other	-0.0190	0.0375	-0.1655***	0.1183*	-0.0150	0.0179	0.0062	-0.0142
	(0.0353)	(0.0370)	(0.0487)	(0.0485)	(0.0378)	(0.0103)	(0.0308)	(0.0230)
Baseline Mean of Cross-Race	2.9136	2.9359	2.0949	3.7190	3.6227	3.8543	3.6100	0.5915
N	28,604	28,633	28,893	28,273	29,114	28,864	28,951	28,574

*Notes:* \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . Robust standard errors in parentheses. All regressions include controls for the Little's baseline outcome, gender, race, free/reduced lunch status, and single-parent home status, Big's gender and race, as well as fixed effects for the Little's age at follow up, Big's age and education at follow up, and calendar year. Panel B omits the controls for Big's race to avoid collinearity with the interaction terms and the include controls for Little'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 Little’s Individual Course Grades**

	(1)	(2)	(3)	(4)
	Math	Reading	Social Studies	Science
Same Race × Black	-0.0087 (0.0192)	0.0036 (0.0183)	0.0009 (0.0188)	-0.0130 (0.0192)
Same Race × White	0.0283 (0.0355)	0.0007 (0.0352)	0.0203 (0.0351)	0.0406 (0.0352)
Same Race × Hispanic	-0.0173 (0.0269)	-0.0542* (0.0257)	-0.0713** (0.0260)	-0.0795** (0.0264)
Same Race × Other	0.1795** (0.0616)	0.0696 (0.0637)	0.0624 (0.0664)	0.1420* (0.0655)
Baseline Mean of Cross-Race	3.6380	3.7510	3.6648	3.8120
N	29,183	29,164	28,672	28,827

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



**Table 4 — Same-Race Impacts on Average Little Followup Outcomes at the Agency Level**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	School Experience			Education		Social Experience		
	Social Acceptance	School Ability	Truancy	Grades	Education Expectations	Risk Attitudes	Parental Trust	Special Adult
<b>Panel A: Average Same Race Effect</b>								
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)
<b>Panel B: Same Race Effect Within Race of Little</b>								
Same Race × Black	-0.0216	0.0740	-0.0050	-0.0030	0.0864	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	0.1101	0.0399
	(0.0979)	(0.1161)	(0.1363)	(0.1471)	(0.0929)	(0.0312)	(0.1018)	(0.0585)
Same Race × Other	-0.1316	0.2483	0.0407	0.3980	0.4794	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	2.8227	2.9119	2.0845	3.6496	3.5174	3.8568	3.5969	0.5655
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 Littles at the agency; the within-agency fraction of Littles 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 Big's that are: male, in each race category, in each age year bin, in each educational attainment bin; and calendar year fixed effects. Panel B omits the controls for the fraction of Bigs in each race bin to avoid collinearity with the interaction terms and the fraction of Littles 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 A1 — 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
<b>Youth Outcomes Survey</b>									
Social Acceptance	2.88	0.63	28,959	2.89	0.65	43,955	-0.01	-1.71	-0.01
School Attitudes	2.93	0.59	29,010	2.90	0.60	43,976	0.03	5.60	0.03
Education Expectations	3.59	0.64	29,327	3.55	0.67	44,488	0.04	7.40	0.04
Grades	3.71	0.78	28,923	3.65	0.80	43,914	0.06	9.86	0.05
Risk Attitudes	3.85	0.26	29,198	3.83	0.30	44,228	0.02	10.83	0.06
Parental Trust	3.59	0.57	29,262	3.55	0.61	44,341	0.04	9.84	0.05
Truancy	2.09	0.84	29,194	2.15	0.85	44,155	-0.05	-8.34	-0.04
Special Adult (=1)	0.58	0.49	28,870	0.58	0.49	43,773	0.00	0.96	0.01
<b>Little Characteristics</b>									
Male(=1)	0.48	0.50	30,466	0.45	0.50	44,993	0.03	6.69	0.04
Age	11.20	1.83	29,518	11.46	1.95	44,781	-0.26	-18.17	-0.10
Free-Reduced Lunch (=1)	0.78	0.42	30,466	0.78	0.42	44,993	0.00	0.23	0.00
Single-Parent HH (=1)	0.69	0.46	30,466	0.69	0.46	44,993	-0.01	-1.38	-0.01
Two-Parent HH (=1)	0.20	0.40	30,466	0.19	0.39	44,993	0.01	3.47	0.02
<b>Big Characteristics</b>									
Male(=1)	0.47	0.50	30,466	0.44	0.50	44,993	0.03	7.42	0.04
Age	37.09	11.58	29,532	36.34	11.60	44,828	0.75	8.64	0.05
Less than High School (=1)	0.01	0.08	30,466	0.01	0.09	44,993	-0.00	-2.25	-0.01
High School Graduate (=1)	0.05	0.22	30,466	0.07	0.25	44,993	-0.02	-9.49	-0.05
Some College (=1)	0.19	0.40	30,466	0.23	0.42	44,993	-0.04	-13.42	-0.07
Associate Degree (=1)	0.06	0.23	30,466	0.06	0.24	44,993	-0.01	-2.85	-0.01
Bachelor's Degree (=1)	0.48	0.50	30,466	0.44	0.50	44,993	0.04	10.78	0.06
Advanced Degree (=1)	0.22	0.41	30,466	0.20	0.40	44,993	0.02	8.08	0.04
<b>Match Characteristics</b>									
Match Length (mos)	35.01	19.91	30,466	15.65	14.99	44,993	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).