More than Just Friends? School Peers and Adult Interracial Relationships

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This paper investigates whether interracial contact in childhood impacts adult romantic relationships. We exploit quasi-random variation in the share of black students across cohorts within US schools. We find that more black peers of the same gender lead whites to have more relationships with blacks as adults. While we do not find impacts on labor market outcomes, there are significant effects on reported racial attitudes. Furthermore, an increase in meeting opportunities is unlikely to explain the increased interracial relationships, since the effect is persistent across time, space, and social networks. Overall, interracial contact during childhood has important long-term behavioral consequences.

I. Introduction

Interracial marriage rates are important indicators of social integration and the health of race relations (Fryer 2007). It may therefore be concerning that

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in the United States the marriage rate between blacks and whites is low—according to the 2015 American Community Survey (Ruggles et al. 2015), only 7.8% of married blacks intermarry with whites. Such assortative matching is likely to have important implications for labor market outcomes (Pencavel 1998), intergenerational income mobility (Chadwick and Solon 2002), and income inequality (Greenwood et al. 2014). Racial preferences appear to play an important role in explaining this sorting (Wong 2003; Fisman et al. 2008; Hitsch, Hortaçsu, and Ariely 2010). Yet little is known about what determines these racial preferences or to what extent they are influenced by individuals' experiences.

Social interaction has long been postulated as a potential means of reducing racial prejudices (e.g., Williams 1947; Allport 1954). Indeed, recent studies have shown that white students and teachers exposed to a greater number of black students adjust their stated attitudes or choose to interact more frequently with blacks in schools (Boisjoly et al. 2006; Marmaros and Sacerdote 2006; Dobbie and Fryer 2015; Carrell, Hoekstra, and West 2015). Baker, Mayer, and Puller (2011) find evidence, however, that this effect may be limited, since in their study exposure does not appear to impact students' broader social networks. Moreover, these papers study the impact on stated attitudes or limited interactions, such as emailing or sharing a dorm. Hence, it is yet to be demonstrated that such social interactions affect major life decisions, such as marriage and cohabitation.

This paper investigates whether exposure to racial diversity at a young age partly explains assortative matching by race. In particular, we explore how plausibly exogenous variation in a white student's school peer group influences the romantic relationships that they later undertake as an adult. To do so, we use the National Longitudinal Survey of Adolescent Health (hereafter, Add Health), which collects information on the race of all students within surveyed schools in the United States and then more than a decade later surveys a sample of these students on their romantic partners. These data allow us to exploit idiosyncratic variation in grade composition within schools, a methodology pioneered by Hoxby (2000) and widely used to iden-

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tify causal peer effects (Sacerdote 2014).[1] A number of tests confirm that the variation we use is as good as random and uncorrelated with other variables that might influence adult relationships. Moreover, we show that a higher share of black students in a grade stimulates diversity in social interactions both within and outside the classroom.

The main contribution of this paper is to provide evidence that the racial composition of students' school cohorts impacts romantic relationships later in life. This is not simply the result of students having more potential black partners in school, since the peer groups that impact adult relationships are students of the same sex in the same grade. The importance of same-sex peers is consistent with young people forming closer friendships with individuals of their own gender (Kalmijn 2002). The magnitude of the effect is important—going from the average of 8% blacks of the same gender in the cohort to 10% would increase the probability of dating a black as an adult by approximately 0.6 percentage points, which is 13% of the mean. The result survives several robustness checks, including the introduction of grade school fixed effects. Furthermore, we find no evidence that our results are driven by measurement error in the way outlined in Angrist (2014). We therefore conclude that school racial composition has an important impact on adult interracial relationships.

We then give evidence that the most likely mechanism behind this impact is a change in racial preferences or attitudes. First, we find significant effects on reported attitudes in several waves of the survey. Second, we document evidence suggesting that an increase in indirect meeting opportunities—that is, meeting a partner through school friends—is unlikely to play a major role. In particular, if our result stemmed mainly from increased meetings with blacks through school-based social networks, we would expect it to be stronger for those relationships formed in school, at a younger age, and geographically closer to school. We find no evidence for such a differential impact. Finally, we show that any impact of cohort racial composition on educational performance or labor market outcomes would unlikely be large enough to explain our measured effect. Overall, therefore, our results suggest that racial diversity in schools impacts individuals' attitudes or beliefs, which in turn affect their decisions regarding relationships.

We proceed in the following way. Section II details the data set and estimation strategy, and it provides evidence for the validity of our main identification assumption. In Section III we analyze the extent to which a higher black share in a given cohort increases interracial exposure and friendship.

[1] See, e.g., Bayer, Hjalmarsson, and Pozen (2009), Bifulco, Fletcher, and Ross (2011), Lavy, Paserman, and Schlosser (2012), Carrell, Hoekstra, and Kuka (2016), and Patacchini and Zenou (2016). The Add Health data have been used to study peer effects in a number of papers, including Calvó-Armengol, Patacchini, and Zenou (2009) and Fletcher, Ross, and Zhang (2013).

In Section IV we then present the benchmark results before proceeding to undertake a number of robustness checks, including adding additional controls, looking for bias driven by measurement error, and considering alternative specifications. We then investigate our results further in Section V in an attempt to shed light on the mechanisms that may potentially be driving the result. Finally, Section VI concludes, discusses policy implications, and makes suggestions for future research.

II. Data and Estimation Strategy

A. Data

We use data from Add Health.[2] The survey selected 80 nationally representative high schools and 54 feeder schools in the United States and first gave a questionnaire to all students in the schools in grades 7–12 in 1994–95. This in-school survey was self-administered and collected basic information from around 90,000 students, including their gender and race. Within each school a sample of students was then interviewed at home and asked many detailed questions on topics such as family background, health behaviors, friendships, and romantic relationships. This in-home survey was administered to around 20,000 students, and these students formed the sample for the following waves, administered in 1996 (wave 2), 2001–2 (wave 3) and 2008–9 (wave 4).

In a first step, we use all students in the in-school survey to construct information about school peers. In particular, we construct our main independent variables, the shares of students in peer groups who are black.[3] We consider three alternative peer groups: the cohort of all students in the same grade, students of opposite sex in the same grade, and students of same sex in the grade. A key advantage of using the in-school sample is that it is close to a census of students within the grade, and hence we reduce measurement error in cohort composition differences.

Our analysis then uses the wave 4 in-home survey to measure outcomes in terms of relationships. Within this survey, respondents were asked to give

[2] The Add Health project was designed by J. Richard Udry, Peter S. Bearman, and Kathleen Mullan Harris and was funded by grant P01-HD31921 from the National Institute of Child Health and Human Development, with cooperative funding from 23 other federal agencies and foundations. Special acknowledgment is due Ronald R. Rindfuss and Barbara Entwisle for assistance in the original design. Persons interested in obtaining data files from Add Health should contact Add Health, Carolina Population Center, 123 West Franklin Street, Chapel Hill, NC 27516-2524 (addhealth@unc.edu). No direct support was received from grant P01-HD31921 for this analysis.

[3] In the in-school survey, race is self-reported and students could define themselves as being of more than one race. In the analysis that follows the black share is defined as the share of students who defined themselves as black only. For romantic partners, individuals can only report one race. We consider alternative definitions of race in the robustness checks (app. sec. C5).

basic information, including race, on a list of current and past romantic partners. This list included their current or most recent romantic partner as well as any person who they had been married to, had lived with for more than 1 month, or had had a relationship with that resulted in pregnancy. Since this information is collected 12–13 years after wave 1, when respondents finished high school, the vast majority of the respondents' partners are not part of the original sample. We then construct our two dependent variables: a binary variable indicating whether an individual reported any black partners of any gender and the share of an individual's reported romantic partners who are black.[4]

We focus our attention on white students, since whites are the majority group and this is of primary interest when considering racial views. The relatively small number of blacks and students of other racial groups limits our ability to draw robust inference on whether they are affected differently. For most of our analysis we focus on the set of white students who were interviewed and assigned sample weights in wave 4, of which there are 9,353.[5] Of this sample we were unable to match 405 respondents with information on their school cohort, and we dropped a further 69 for whom we observe less than five students in the in-school survey of the same gender.[6] This leaves us with a total of 8,879 individuals, spread across 421 school cohorts and 818 peer groups of the same grade and same gender.

In terms of attrition, Harris (2013) finds that attrition bias in wave 4 is negligible for demographic, behavioral, health, and attitudinal variables after study estimates were adjusted with final sampling weights. Moreover, Bifulco, Fletcher, and Ross (2011) find no evidence that attrition is correlated with minority shares within cohorts. In our sample, there is no systematic relationship between one's cohort black shares and the probability to be in waves 3 or 4 (cols. 1 and 2 of table C3). We discuss in more detail the robustness of our results to attrition in Section IV and provide additional tests in appendix section C2.

Summary statistics of the main variables we use in our analysis are reported in table 1, along with other characteristics that help to describe our sample. We report the estimated population mean of a range of variables

[4] Since data on each partner's race are reported by the respondent, one concern is that this may be misreported in a way that is influenced by exposure to blacks in school. In app. sec. C5, we show that this is unlikely to be driving our results by focusing on partners whose race is also classified directly by the interviewer.

[5] The in-home surveys sampled students with unequal probability, and we therefore use sampling weights in our analysis. For more details on the Add Health data, see Chen and Chantala (2014). We test that our results are not being driven by respondents with large sample weights in app. sec. C2.

[6] This is done in order to reduce noise stemming from the extreme values of our independent variable that these observations produce. Results are robust to the inclusion of these observations.

Table 1
Summary Statistics

	Mean	Within-School SD	Between-School SD	*N*
Main variables:				
Any black partners	.046	.19	.04	8,879
Share of black partners	.03	.13	.029	8,696
Grade black share, both genders	.083	.012	.12	8,879
Grade black share, same gender	.082	.019	.12	8,879
Other wave 1 variables:				
Age	16	1.2	1.2	8,879
Female	.52	.5	.075	8,879
Hispanic	.14	.19	.26	8,879
Family income	50	38	24	7,073
Grade size	225	28	159	8,879
Grades in school	3.9	0	1.3	8,879
In middle school	.21	0	.41	8,879
In high school	.59	0	.49	8,879
Lives in urban area	.45	.18	.42	8,795
Region = Northeast	.18	0	.39	8,879
Region = Midwest	.3	0	.46	8,879
Region = South	.36	0	.48	8,879
Region = West	.16	0	.37	8,879
Other wave 4 variables:				
Age	29	1.2	1.2	8,879
Number of recorded partners	1.8	1.3	.24	8,879
Number of cohabiting partners	1.4	1	.24	8,879
Number of marriages	.63	.56	.18	8,696
Attended college	.67	.44	.14	8,878
Employed	.66	.47	.07	8,875

along with the estimated population standard deviations both between and within schools. Variable definitions are given in appendix A.

The relative scarcity of interracial relationships is immediately apparent—less than 5% of our sample report having had a relationship with a black person. Whereas the average students' cohort is 8% black, only 3% of their reported partners are black. The average within-school standard deviation in the grade black share is around 1.3 percentage points. If we restrict individuals' cohorts to be only those students of the same gender, this standard deviation increases to 1.9 percentage points.

Individuals range between the ages of 11 and 21 when surveyed in wave 1, with 21% attending a middle school and 59% attending a high school.[7] In

[7] We define a middle school as one containing no grades higher than grade 9 and a high school as one containing no grades lower than grade 9. Among those schools that contain both grades 8 and 10, three schools show abnormally large increases in the number of students between the two grades (above 100%). In the analysis, we follow a conservative approach, splitting each of these schools in two. Our results are, however, robust to not splitting these schools.

wave 4, individuals are between 24 and 34 years old and report 1.8 romantic partners on average.

B. Estimation Strategy

We cannot simply regress dating behavior on cohort composition since cohort composition is likely to be correlated with a range of other omitted variables that impact dating behavior—not least, the composition of the population that lives nearby the school. Moreover, self-selection of individuals might further bias results if those who are more inclined to date blacks choose to enroll in schools with a larger share of black students.

To control for these omitted variables, we exploit variation in the share of black students across cohorts within an individual school.[8] In other words, we assume that families do not select schools based on the differences between the average school composition and their child's school-specific cohort and that these differences are not correlated with other important omitted variables.

To implement our identification strategy, we estimate the following regression equation:

$$Y_i = \alpha \mathit{ShareBlack}_{cs} + I_{gm} + I_{sm} + \varepsilon_i, \tag{1}$$

where $\mathit{ShareBlack}_{cs}$ is the share of blacks within cohort c in school s, I_{gm} are grade-gender fixed effects, I_{sm} are school-gender fixed effects, and ε_i is a random error term.[9] We split school and grade fixed effects by gender since much of our analysis uses gender-specific cohort shares, and we are concerned about systematic differences in cohort shares across gender at the school and grade level. Note that by controlling for grade we are essentially also controlling for respondents' age at the time of the wave 4 interviews. Standard errors are clustered at the school level.[10]

Our main dependent variable, Y_i, is whether an individual reports in wave 4 at least one relationship with a black partner. This embeds the idea that contact with blacks might affect the probability of a first interracial relationship. This "extensive margin" is probably the most affected by attitudes toward

[8] Schools with no black students therefore do not affect our results. To examine the characteristics of the schools that contribute most to our results, table D1 provides summary statistics for schools that have within-school variation in the black cohort share above the median.

[9] An alternative specification would use the number of blacks in the cohort rather than the black share. In most of our analysis we use the black share, since we believe this more likely to be quasi-random, but we consider the alternative specification in table C6.

[10] We cluster standard errors at the school level since students are sampled using a two-stage process in which first a sample of schools are selected; see Abadie et al. (2017) for a discussion. Results are also robust to clustering at the school cohort level.

blacks, but it may also be the case that contact with blacks impacts the "intensive margin" of how many relationships an individual has with black people. Indeed, a number of closely related studies, such as Boisjoly et al. (2006), Marmaros and Sacerdote (2006), Camargo, Stinebrickner, and Stinebrickner (2010), and Baker, Mayer, and Puller (2011), use shares as dependent variables, either exclusively or in addition to the extensive margin. Hence, we also estimate equation (1) with the dependent variable being the share of individual i's reported romantic partners who are black. Both models are therefore similar to the specification typically used in the peer effects literature, with an important difference. Since we focus only on the impact on whites, compositional changes have an impact on interracial relationships, and hence we are not concerned by the critique that such linear models limit potential policy implications.

In our regressions, we start by considering an individual's peer group as the cohort of students in the same grade within the school. We then split grades in two, considering separately those students of the opposite gender and those of the same gender. On the one hand, we may expect opposite peers to have the largest impact on romantic relationships, as this group forms a pool of potential partners since most students are attracted to individuals of opposite gender.[11] On the other hand, same-gender peers may be more important if this is the group from which close friends are most likely to be drawn.

C. Identification Assumption

Our methodology relies on the assumption that variation in cohort composition within schools is as good as random once we control for grade-gender fixed effects. We can test three important implications of this assumption.

First, we can test whether within-school variation in the share of black students is correlated with predetermined individual level variables—a type of balancing test. In particular, we regress a range of predetermined student characteristics on the black share of their peer group while controlling for school-gender and grade-gender fixed effects. Each characteristic is regressed first on the black share of students in the whole grade and then simultaneously on the black share of students of the opposite and same sex in each grade. We show results in table 2 for this exercise undertaken on the main sample we use in our analysis; results are very similar when we use samples relevant to supplementary regressions. The results support our main identification assumption—only one of the predetermined variables, grade size, is significantly different from zero at the 10% level. Although we believe the correlation with grade size to be spurious, we control for this variable in all of our regressions.[12]

[11] Approximately 5% of our sample report a same-sex partner in wave 4.

[12] Indeed, when we run a multivariate regression of cohort black shares on these variables, an F-test does not reject that all of the coefficients are zero. Additionally,

Table 2
Balancing Tests for Cohort Composition Measures

		Independent Variable		
	N	Grade Black Share, Both Genders	Grade Black Share, Opposite Gender	Grade Black Share, Same Gender
Age	8,879	−.690	−.0373	−.693
		(.564)	(.314)	(.438)
Parent is black	7,890	−.00616	−.0116	−.000805
		(.0361)	(.0272)	(.0315)
Share of census tract black	8,799	−.0457	.0414	−.0845
		(.114)	(.0914)	(.0687)
Share of census block black	8,792	−.0578	.0460	−.0974
		(.0990)	(.0802)	(.0879)
Grade size	8,879	151.5*	72.65*	76.41*
		(76.61)	(38.25)	(40.52)
Gender ratio in grade	8,879	−.210	−.293	.0361
		(.155)	(.258)	(.207)
Born in the United States	8,879	−.0207	.0364	−.0208
		(.137)	(.0774)	(.105)
Lives with both biological parents	7,875	.363	.188	.139
		(.380)	(.208)	(.286)
Number of older siblings	8,866	.0918	.00252	.0848
		(.706)	(.465)	(.475)
Years of parental schooling	8,492	.507	.445	−.133
		(1.532)	(.849)	(1.095)
Log of family income	6,998	.207	.0818	.112
		(.743)	(.519)	(.526)
Home language is not English	8,879	.00457	.0397	−.0126
		(.102)	(.0657)	(.0785)

NOTE.—Each coefficient is from a separate regression where the variable in the first column is regressed on one of the three specified independent variables, with controls including race, grade-gender fixed effects, and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.
* $p < .10$.

Second, we can test for nonrandom clustering of black students across grades within schools: if variation is as good as random, then the race of a student should be uncorrelated with that of their peers once we control for school fixed effects. As noted by Guryan, Kroft, and Notowidigdo (2009), however, one cannot test for this by simply regressing an individual's race on that of their peers because each individual is present in many others' peer

we run regressions like those reported in table 2 for all pretreatment student characteristics available in Add Health and observe how many coefficients are significant at the 5% level. Of the 86 variables, 3.3% are significant when regressed on the gender black share, 6.7% when regressed on the same-gender cohort black share, and 4.9% when regressed on the opposite-gender black share, consistent with the black shares being distributed quasi-randomly.

groups but necessarily not their own. We therefore undertake a number of tests designed to avoid this problem, including those proposed by Guryan, Kroft, and Notowidigdo (2009), Stevenson (2017), and Caeyers and Fafchamps (2016). Details on these tests and results can be found in appendix B. Overall, none of the tests reject random clustering, and we therefore conclude that the distribution of blacks after controlling for fixed effects is consistent with quasi-random variation.

Third, we investigate whether differences in black shares across grades are symmetric. If changes in grade black share were driven by blacks dropping out disproportionately, then we might observe that black shares were systematically lower in later grades. In appendix B we therefore plot the distribution of differences in the black shares between grades. We find the distribution to be very symmetric, consistent with differences across grade being as good as random.

Finally, the variation in the share of black students across cohorts may be partly affected by the end of court-ordered desegregation orders that occurred during this time. Lutz (2011) shows that the expiration of court oversight led to significant changes in racial composition, but these changes are not correlated with other trends, and hence this is not a threat to our identification. Moreover, table 2 shows that there is no significant correlation between our variation and neighborhood black shares, suggesting that our variation is not being driven by changes in the areas students are taken from or changes in the racial composition of those areas.

III. Exposure and Friendship

Our empirical strategy relies on the implicit assumption that a higher share of blacks in a school cohort implies that white students are exposed more to black students. Students may, however, be somewhat segregated within schools if they get assigned to different classes or form different friendship networks (Currarini, Jackson, and Pin 2009). It is therefore instructive to test this assumption before undertaking our main analysis. In this section, we therefore assess the impact of the share of blacks within cohorts on several measures of exposure. In particular, we examine several smaller peer groups with which students are likely to have a substantial amount of contact.

For a subsample of the population, the Add Health data provide information on how much class time each student overlaps with each other student in waves 1 and 2. For these students, we can thus construct a set of classmates with whom they took at least one course. These classes are much smaller than grades, with the median student having 44 classmates. Columns 1–4 of table 3 show the results when we estimate equation (1) using the share of blacks among each student's classmates as the dependent variable, which has a mean value of 0.09 in our sample. The results confirm that a higher share of blacks in the grade leads to more black classmates. Note that our measure of

Table 3
Impact of Cohort Black Shares on Exposure

	Share of Black Classmates				Average Black Share of Same Grade Students in Same Club	
	In Wave 1		In Wave 2			
	(1)	(2)	(3)	(4)	(5)	(6)
Grade black share, both genders	.347**		.347**		.210***	
	(.133)		(.158)		(.0305)	
Grade black share, same gender		.153**		.221***		.115***
		(.0721)		(.0834)		(.0233)
Grade black share, opposite gender		.180**		.131		.0856***
		(.0842)		(.0855)		(.0146)
Observations	3,788	3,788	2,922	2,922	7,032	7,032
Adjusted R^2	.935	.935	.923	.923	.549	.546

NOTE.—The table reports ordinary least square estimates controlling for grade size, grade-gender fixed effects, and school-gender fixed effects. Standard errors (in parentheses) are clustered at the school level.
** $p < .05$.
*** $p < .01$.

same-gender cohort black share, which is constructed using wave 1 data, has a significant impact on classmates in wave 2, confirming that there is persistence in exposure over time.

The Add Health in-school survey also collects information on each student's school-sponsored extracurricular activities, such as sports teams and language clubs. For each student for whom we have data on at least one club in which they participated, we construct the share of blacks among students of the same grade who undertake each activity and then average this across the activities that the student is involved in. Columns 5 and 6 of table 3 report the results of estimating equation (1) with this as the dependent variable. The positive and significant coefficients indicate that a higher share of black students in an individual's cohort increases the share of blacks among peers with whom they undertake extracurricular activities.

We hence conclude that a higher share of black students in a grade increases the exposure to black peers, at least for the subsample of students for whom we have information on academic or extracurricular networks. It is natural then to ask whether this exposure results in intensified interracial social interactions. This is not obvious since there are racial biases both in preferences and in meeting opportunities (Currarini, Jackson, and Pin 2009; Fletcher, Ross, and Zhang 2013). Since Add Health collects information on friendships, we can directly test for an impact of cohort black shares on the share of school friendships in wave 1 that are with black students. In particular, we regress various measures of interracial friendship on the grade black shares using the specification given in equation (1).

The results are displayed in table 4. In columns 1–4 our dependent variable is a binary variable indicating whether an individual nominates any black student as a friend, while in columns 5–8 it is the share of a student's

Table 4
Impact of Cohort Black Shares on Friendships

	Dependent Variable: Any Black Friends				Dependent Variable: Share of Black Friends			
	All Friends		Closest Friends		All Friends		Closest Friends	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grade black share, both genders	.0679		.0782		.0746		.0961	
	(.0929)		(.0585)		(.0797)		(.0685)	
Grade black share, same gender		.144**		.116**		.119**		.125**
		(.0583)		(.0446)		(.0597)		(.0554)
Grade black share, opposite gender		−.0667		−.0268		−.0336		−.0123
		(.0571)		(.0390)		(.0458)		(.0431)
Observations	11,700	11,700	11,700	11,700	9,961	9,961	9,961	9,961
Adjusted R^2	.016	.017	.013	.014	.038	.039	.027	.028

NOTE.—The table reports ordinary least square estimates controlling for grade size, grade-gender fixed effects, and school-gender fixed effects. Standard errors (in parentheses) are clustered at the school level.
** $p < .05$.

nominated friends who are black.[13] In each case, we consider two sets of friends—in columns 1, 2, 5, and 6 we include all reported friends, while in columns 3, 4, 7, and 8 we focus on closest friends, that is, those friends with whom the student reports having the most contact during the week.

Overall, the estimates suggest that students have more black friends when there is a greater share of blacks among students of their own gender.[14] This also holds true when we restrict the analysis to just to close friendships. The difference between the impact of same- and opposite-gender black shares is consistent with the psychological and sociological research: social interaction in nonromantic relationships rarely crosses gender lines, especially at an early

[13] The sample used for the regressions presented in table 4 is larger than that in table 2 since we do not restrict the analysis to students who were reinterviewed in wave 4. To assure us that we can make causal inferences from the coefficients in table 4, we also carry out balance tests on this larger sample and find very similar results to those reported in Sec. II. The sample in cols. 5–8 is smaller than that in cols. 1–4 because it is confined to students who have at least one friend we can match in the Add Health data. On average, students have four such friends, with 2% of students nominating at least one black friend.

[14] Our results are compatible with the existence of homophily in friendship found by Currarini, Jackson, and Pin (2009) and Fletcher, Ross, and Zhang (2013). Fletcher, Ross, and Zhang (2013) find that a greater share of blacks in a cohort increases homophily once they control for the direct compositional effect that if there are more blacks in a class, there are more potential matches with blacks. Here instead we are interested in whether more diversity in the classroom implies more contact with blacks in an absolute sense. Our findings in table 4 suggest that the overall effect of both changes in friendship formation and the compositional effect are positive, which is consistent with Fletcher, Ross, and Zhang (2013).

age.[15] This provides extra motivation to divide students' peers by gender in the following analysis.

IV. Main Results

Our main results are provided in table 5. We first report the result of estimating equation (1) when the dependent variable is whether an individual reports in wave 4 having had any black partners. While there is no significant effect when not distinguishing by gender (col. 1), we find that a higher share of blacks of the same gender increases the likelihood of having had at least one black partner as an adult (col. 2). Blacks of the opposite gender do not have any significant influence.

In terms of magnitude, the point estimate implies that going from the average of 8% blacks in the same-gender cohort to 10% (an increase of around 1 within-school standard deviation) would increase the probability of dating a black person by approximately 0.6 percentage points, which is 13% of the mean.[16]

The estimates of our second specification, where the dependent variable is the share of reported partners who are black, are presented in columns 3 and 4 of table 5. While the coefficient on grade black share in column 3 is significant, column 4 shows that the coefficient on the black share of the same-gender cohort is positive and highly significant, whereas the coefficient on the black share of the opposite-sex peer group is insignificantly different from zero, as in column 2. Again, the magnitude of the effect is not trivial: an increase of around 1 within-school standard deviation of black students of the same gender in the cohort would increase the average black share of adult relationships by half a percentage point, which is about 15% of the mean.

The results in table 5 provide evidence that individuals who had a greater share of blacks in their same-gender school cohort have a higher chance to have one or more black partners later in life. The effect is not likely to be driven by students meeting romantic partners in their own grade, since the share of students who report having a partner of the same gender is relatively small (approximately 5%).[17] Rather, the results are consistent with the increase in close social interactions with black students of the same gender pre-

[15] See McPherson, Smith-Lovin, and Cook (2001) and Kalmijn (2002) as well as the references discussed therein.

[16] Ideally we would like to compare the magnitude of our identified effect with the impact of some other variable, but we are not aware of other well-identified factors influencing interracial relationships. In col. 3 of table C6, however, we include the black share of an individual's census block group in wave 1 as a control variable. We find this coefficient to be significant but about half the size of the coefficient on the same-gender cohort black share.

[17] If we exclude those respondents from our sample, the coefficient on same-gender cohort black share increases slightly and the p-value stays exactly the same.

Table 5
Benchmark Results

	Any Black Partners		Share of Black Partners	
	(1)	(2)	(3)	(4)
Grade black share, both genders	.246		.325**	
	(.191)		(.126)	
Grade black share, same gender		.283**		.261***
		(.119)		(.0858)
Grade black share, opposite gender		−.0548		.0586
		(.123)		(.0808)
Observations	8,879	8,879	8,696	8,696
Adjusted R^2	.061	.062	.055	.056

NOTE.—The table reports ordinary least square estimates controlling for grade size, grade-gender fixed effects, and school-gender fixed effects. The dependent variable in cols. 1 and 2 is a dummy variable coded as 1 if a respondent reports having had at least one black partner. In cols. 3 and 4 the dependent variable is the share of romantic partners who are black. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.
** $p < .05$.
*** $p < .01$.

sented in the previous section. Hence, the importance of same-sex peers could be driven by the fact that students typically form closer friendships with students of their own gender, and hence this peer group has the largest impact on post-high-school relationships.[18] Moreover, a student's grade is less likely to be the relevant population for within-school romantic relationships than for friendship. Indeed, in our data a majority of students' within-school friends are in the same grade, but this is true for less than a quarter of within-school romantic partners.[19]

Since it is the composition of same-gender students that is driving our results, from now on we focus on these cohorts. To ease readability and improve the clarity of our argumentation, we focus on the binary interracial relationship measure as our outcome variable of interest. Results pertaining to the share of partners who are black can be found in appendix section C1. Before we explore potential mechanisms behind our result, we test its robustness to several alternative specifications.

We first analyze the extent to which our results may be affected by omitted-variable bias by introducing a series of additional control variables. These results are presented in table 6. Column 1 presents our benchmark result when cohorts are divided by gender, with column 2 introducing a number of control

[18] In line with this interpretation, Soetevent and Kooreman (2007) find that interactions with peers of the same gender are generally much stronger than those with peers of the opposite gender for several academic and nonacademic outcomes.

[19] Further tests looking at opposite-gender black shares in other grades relevant for potential romantic partners do not yield significant results. Potential reasons include the smaller sample for this analysis (i.e., first or last grades must be excluded).

Table 6
Robustness to Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)
Grade black share, same gender	.283**	.306**	.297**	.283*	.302**	.315*
	(.119)	(.119)	(.120)	(.167)	(.147)	(.177)
Grade black share, opposite gender	−.0548	−.0584	−.0432	−.0341		
	(.123)	(.119)	(.120)	(.156)		
Benchmark controls	Y	Y	Y	Y	Y	Y
Extended controls		Y	Y	Y	Y	Y
Extended cohort controls			Y	Y	Y	Y
School trends				Y		
School-grade fixed effect					Y	Y
Tract fixed effect						Y
Observations	8,879	8,879	8,879	8,879	8,879	8,879
Adjusted R^2	.062	.070	.070	.078	.084	.172

NOTE.—The table reports ordinary least square estimates. The dependent variable is a dummy variable coded as 1 if a respondent reports having had at least one black partner. Benchmark controls are grade size, grade-gender fixed effects, and school-gender fixed effects. Extended controls include an individual's religion, birth year, the black share of the census block group, whether an individual lived with a single parent at wave 1, whether an individual had repeated or skipped a grade prior to wave 1, family income, mother's education, whether an individual was born in the United States, and the individual's age at wave 4. Extended cohort controls include the share of the same-gender cohort whose mother attended college, the share whose father attended college, the share Hispanic, the share Asian, the share whose parents were born in the United States, and the grade gender ratio. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.

* $p < .10$.

** $p < .05$.

variables, including the share of blacks within an individual's census block group, his or her family's income, and his or her religion. Column 3 then additionally includes other characteristics of the same-gender cohort, such as the share of the grade who are of the same gender and the share of same-gender peers whose parents attended college. Our coefficient of interest remains almost unchanged, suggesting that our result is not being driven by unobservables correlated with the controls we add (Altonji, Elder, and Taber 2005; Oster 2017).

We can additionally control for a number of unobservables by introducing school trends and other fixed effects. Column 4 adds a trend variable for each school—that is, school fixed effects interacted with grade—that controls for factors such as differential dropout rates among blacks across schools. Column 5 adds school-grade fixed effects, so that we can control for any variable that impacts a particular grade within a school. In this case, our coefficient of interest is identified entirely from the difference in the black shares between genders. This specification therefore allows us to control for the exact level where we would suspect that selection is taking place. Using this more limited source of variation, the coefficient remains significant and of the same magnitude.

Finally, column 6 introduces census tract fixed effects. On average there are 25 census tracts within a school, so including census tract fixed effects further ensures that our results are not being driven by variation in the residential area from which students are drawn. In this regression standard errors are larger than our benchmark due to a reduction in statistical power, but our coefficient moves relatively little and remains significant. Therefore, estimates in table 6 strongly suggest that unobserved omitted variables are unlikely to drive our result.

One potential concern with the methodology of exploiting cohort variation is that results can be driven by selection bias or measurement error, as described by Angrist (2014).[20] Since we have strong evidence that our variation in same-gender cohort black share is quasi-random and race is generally not measured with error, this is unlikely to be a problem in our case. Nonetheless, we check for bias from measurement error in three ways. First, if the cohort black shares were proxying for an individuals' true race, we would expect them to be significant when we replace our dependent variable with predetermined variables correlated with race. Yet in table 2 we can see that the coefficients are insignificant when regressing parental race and two alternative measures of neighborhood black shares. Second, if our result was driven by measurement error in race, the coefficient should fall when we introduce other variables correlated with race, yet we observe little change when we do so. Third, following Carrell, Hoekstra, and Kuka (2016) and Feld and Zölitz (2017), we redo our estimation introducing varying amounts of measurement error. As expected, a greater amount of measurement error leads to results being attenuated to zero and does not bias the coefficient upward. Results from these tests and more details can be found in appendix section C4.

A different concern is that since our identification is driven by small quasi-random variation across cohorts, our standard errors may be inappropriate or our results may be driven by some other aspect of the cohort that is correlated with the black shares. We test for this in two ways. First, we construct more than 200 other cohort shares including, for instance, the share of Hispanics and the share who have college-educated mothers. We enter them into regressions individually in place of our main explanatory variable and record the *t*-statistic. The *t*-statistics of our coefficients of interest in the benchmark results lie at the extremes of the right tails of the relevant distributions of the *t*-statistic resulting from this exercise (see fig. C2 in app. sec. C4). Second, we undertake 10,000 placebo regressions whereby we assign students to cohorts within their school at random. Plotting the distribution of coeffi-

[20] Angrist (2014) also discusses a range of other potential problems with the peer effects literature, but these are mainly not relevant to our context. For instance, nonlinearity is not an issue since our peer characteristic of interest—being black or not—is binary in nature.

cients, we note that the true coefficient is clearly an outlier, as it is larger than almost all of the placebo coefficients (see fig. C3). We can therefore conclude that it is very unlikely that our results are driven by chance or correlation with other characteristics of school cohorts.

Since a number of individuals surveyed in wave 1 are not included in our final sample, we may be concerned that this attrition impacts our results. In appendix section C2 we provide several tests that show no evidence of such an impact. These include showing that in our sample the black share of one's same-gender cohort is not related to attrition and that controlling for being a respondent in wave 3 does not affect our results. Furthermore, our results are relatively insensitive to the way individual wave 4 weights are assigned. Results are also similar when we focus on relationships recorded in waves 1 and 2, where attrition is much less of a concern. Finally, we use the procedure introduced by Lee (2009) to provide bounds of our results accounting for attrition and find that the lower bound is significantly above zero.

We may also be concerned that our results are sensitive to the way the race of partners is measured, since this is self-reported by the interviewee. To address this issue we use observations from waves 1–3 where the partners were interviewed, and we use the interviewer's report of the partner's race. Using this alternative sample of partners, we show that same-gender cohort black share still has a significant impact on both the probability and the share of interracial relationships (see cols. 1 and 5 of table C7). Impacts on respondents' reporting of their partner's race is therefore unlikely to be affecting our results.

Finally, we may be concerned that our results are sensitive to the behavior of the linear probability model. We therefore report results from three nonlinear models in appendix section C3. From table C5 we can note that the significance of our coefficient of interest is robust to these alternative estimators.

In appendix section C6, we investigate several subsample splits and interactions to investigate the nature and variation of our results. The estimates show that the coefficient of interest does not significantly differ by gender, school black share, the Republican vote share in the school county, the share of students residing in urban areas, the grade size, or whether the cohort is measured in a middle or high school. This may seem surprising given that the rate of interracial relationships varies substantially across gender and schools, but it is likely to be the result of a lack of power rather than strong evidence for a homogeneous effect. For instance, women are three times more likely to report a black partner than men, but while we cannot reject the hypothesis that the coefficient is the same for men and women, neither can we reject the hypothesis that the coefficient for women is three times as large as it is for men. There is some indication, using the segregation measure of Echenique and Fryer (2007), that the effect is stronger in less segregated schools, possibly suggesting that the grade black share is most likely to have an impact when there is more social mixing between whites and blacks of the same gender. It should be noted, however, that segregation is correlated with many other variables, and therefore we should not overinterpret this interaction.

We also perform the same analysis as our benchmark specification for Hispanics and Asians, but we do not find any statistical effect of the share of students of those groups on subsequent romantic relationships (see app. sec. C5). This suggests that there may be something special with respect to attitudes and social interactions with blacks compared with other minorities, which is consistent with similar findings in the labor market (Hellerstein and Neumark 2008).

We have so far established that students who have more blacks of the same gender in their school cohort go on to have, on average, more relationships with blacks as adults. To our knowledge, the only other paper that has looked at the impact of school peers on adult interracial relationships is the one by Gordon and Reber (2018), who study the effect of school racial desegregation between 1961 and 1985 on subsequent black-white births. Their results are sensitive to the specification of cohort trends, and hence they cannot rule out no effect or a modest positive effect. Our work is complementary to theirs in providing evidence on relationships beyond those that lead to children and by studying a different era and source of variation. Additional data available in Add Health also allow us to provide insight on the potential mechanisms behind the increase in interracial relationships. This is what we explore in the next section.

V. Investigating Mechanisms

There are three mechanisms that could lie behind the result identified in the previous section. First, the effect may be the result of a change in individuals' attitudes. Greater exposure to blacks may change beliefs in line with the contact hypothesis (Williams 1947; Allport 1954). Second, the effect may be the result of increased meeting opportunities. A greater number of blacks in an individual's same-gender cohort may increase the number of blacks in their social network, and through this network they may meet a greater number of potential black partners. Third, the effect may be the result of poorer educational achievement. Various studies suggest that an increased share of black students may worsen educational achievement for their peers (Hoxby 2000; Hanushek, Kain, and Rivkin 2009; Billings, Deming, and Rockoff 2014). This may have knock-on effects on college attendance or employment, which in turn impact individuals' propensity to form romantic relationships with blacks.

Identifying the mechanism at work is important for policy and for understanding the nature of peer effects. In particular, if adult relationships change as a result of a change in attitudes, this suggests cohort composition may impact a broader range of behaviors including discrimination. In this section, we aim to investigate whether evidence from our data is consistent with one or more of these mechanisms.

A. Attitudes

To test whether our result is compatible with the contact hypothesis, we would like to look directly for changes in attitudes regarding race. The Add Health surveys do not ask questions specifically about such attitudes, but in wave 3 respondents are asked to rate how important they think several elements are for a serious committed relationship.[21] One of these elements is "being of the same race or ethnic group." We construct a binary measure of the relative importance of race in a relationship by comparing the rating given to race to the other factors. The coefficient is significant and negative (col. 1 of table 7), indicating that white students who had a greater share of blacks in their same-gender school cohort attach less importance to racial homogeneity within romantic relationships.

We would also like to get a sense as to whether attitudes toward race are impacted beyond the context of romantic relationships. We exploit here the fact that in the wave 1 in-school survey students are asked how much they agree with the general statement "the students at this school are prejudiced." Answers to this question could incorporate any form of prejudice, but for black respondents we can imagine answers should partly reflect the extent to which they feel that nonblack students are prejudiced toward black students. We therefore use the sample of black students in the in-school survey and regress this variable on the cohort black shares. The corresponding estimate in column 2 of table 7 implies that a greater share of blacks of the same gender within a grade indeed leads black respondents to report less prejudice. While this may be because blacks are less likely to report prejudice when they are a larger minority, it is also consistent with a change in the attitudes and behavior of nonblack students.

A third strategy involves examining the degree respondents in wave 4 report themselves to be liberal. In general, changes in racial attitudes may not shift individuals' overall political identification, but wave 4 of the survey was collected in 2008, the year when Barack Obama was first campaigning to become president. In both the Democratic primary and the general election, Obama positioned himself as the most liberal of the widely supported candidates. Hence, it is reasonable to hypothesize that individuals' political identification in 2008 may be particularly correlated with their attitudes toward

[21] Since not all of our main sample was interviewed in wave 3, we have a smaller number of observations for this analysis. In app. sec. C2 we investigate whether this attrition may be biasing our results in an important way, and we do not find any evidence to this effect.

Table 7
Impact of Cohort Black Shares on Attitudes

	Importance of Race in Relationships (1)	Are Students in School Prejudiced? (Sample of Blacks) (2)	Liberalness in Obama Election Year (3)
Grade black share, same gender	−.458**	−.534*	.907**
	(.229)	(.320)	(.399)
Grade black share, opposite gender	.0403	.200	−.0760
	(.263)	(.209)	(.409)
Observations	7,386	9,332	8,336
Adjusted R^2	.070	.067	.080

NOTE.—The table reports ordinary least square estimates controlling for grade size, grade-gender fixed effects, and school-gender fixed effects. Wave 4 cross-sectional weights are used in all regressions. Standard errors (in parentheses) are clustered at the school level.
* $p < .10$.
** $p < .05$.

blacks.[22] We therefore regress declared liberalness in wave 4 on the cohort black shares (col. 3 of table 7). The coefficient is positive and significant for peers of the same gender, suggesting that having more black peers in school makes whites declare themselves as more liberal in 2008. Consistent with this effect being driven by Obama, we do not find a similarly significant impact of same-gender cohort black shares on liberalness reported in wave 3, and indeed results are similar to column 3 when we use the difference in reported liberalness between waves 3 and 4 as a dependent variable.

B. Impact by Distance, Age, and Social Network

Our results so far are consistent with a change in racial attitudes, but it is also possible that the effect we have found is driven by an increase in meeting opportunities that result from more diverse social networks formed in school. Note that our results cannot be due to a direct increase in meeting opportunities in school because the effect we find stems from the race of peers of the same gender. Furthermore, since partners are mostly met after high school, the effect cannot be due to a change in dating competition within schools. We have seen, however, that students with more black peers make more black friends. Hence, it might be that through these friends they then

[22] If we regress wave 4 liberalness on the importance of race in relationships and a dummy for whether the individual ever reports having a relationship with a black person, then we find that both coefficients are significant at the 1% level, even when we control for grade black shares. This suggests a strong correlation between liberalness in wave 4 and attitudes toward blacks. On the other hand, if we regress wave 3 liberalness on these variables, both coefficients are insignificant, suggesting that the 2008 measure of liberalness may be exceptionally correlated with attitudes toward blacks.

meet other black people who become romantic partners. In other words, white students who are more exposed to blacks in school might have a lower meeting bias later on, independent of their racial attitudes.

To test whether meeting opportunities can explain most of the impact on adult romantic relationships, in this section we investigate heterogeneity in our result by distance, age, and social network. The basic idea is that if the result is driven by social networks formed in school, then the effect should be strongest for those relationships formed right after and closest to school. If instead greater romantic interaction with blacks is driven by a change in attitudes, then our results should be similar in all relationships.

To analyze the differential impact by geographical distance, we exploit information provided by respondents on where they live and when, if ever, they moved between US states. First, we use the distance of the physical location of the respondent in wave 4 from their location in wave 1. Second, we use whether the individual resides in wave 4 in the same county of the school they attended. Third, we use the information on whether the respondent lived outside the state they went to school in when they met their partners reported in wave 4. Table 8 then shows the results from interacting each of these measures of distance with the share of black students of the same

Table 8
Impact by Distance from School

	(1)	(2)	(3)
Grade black share, same gender	.277*	.319*	.304**
	(.147)	(.180)	(.147)
Grade black share, opposite gender	−.0919	−.134	−.0286
	(.161)	(.242)	(.160)
Same-sex black share × distance (1,000 km)	−.0444		
	(.166)		
Opposite-sex black share × distance (1,000 km)	.105		
	(.248)		
Same-sex black share × not in school county		−.0419	
		(.247)	
Opposite-sex black share × not in school county		.127	
		(.370)	
Same-sex black share × all partners met out of state			.0984
			(.387)
Opposite-sex black share × all partners met out of state			−.426
			(.439)
Observations	8,831	8,875	8,696
Adjusted R^2	.077	.078	.074

NOTE.—The table reports ordinary least square estimates controlling for grade size, grade-gender fixed effects, and school-gender fixed effects. The dependent variable is a dummy variable coded as one if a respondent reports having had at least one black partner. We also control for the interaction of the relevant distance variable with grade-gender and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.
* $p < .10$.
** $p < .05$.

gender in the school cohort.[23] We control for the interaction of the relevant distance variable with grade-gender and school-gender fixed effects. The number of observations varies slightly, reflecting missing data on the different distance measures. None of the interaction terms are significant, and the coefficient on the same-gender cohort black share does not change substantially. This suggests that the effect of having a higher share of black students in one's school cohort on the probability of interracial dating does not fade with distance from school. Since we expect social networks to be mostly local (especially concerning indirect meeting via friends), this evidence suggests that the meeting opportunities mechanism is unlikely to drive our results.

Social networks are also very likely to deplete over time, especially at a young age. Hence, in columns 1 and 2 of table 9 we compare the effect of the composition of a student's cohort on relationships formed before and after age 23. If the effect is being driven by meeting opportunities stemming from school social networks, it would likely be most important in relationships formed before or just after students left school. This is not what we find, however, with the point estimate being if anything larger for those relationships formed longer after leaving.

As a final test, we use information in waves 1 and 2 on romantic relationships students had during high school. This information is useful because for these relationships we know whether the partners are in the same school or not as well as whether they share mutual friends with their partners. Using these data, we run regressions similar to the benchmark model. We see that there is no evidence that the effect of same-gender cohort black share is strongest for partners within school or those with mutual friends (cols. 3–6 of table 9). Indeed, the effect is significantly larger for relationships with partners not in the same school compared with those in the same school, which may be because black peers directly compete in the dating markets within schools. Overall, these results are inconsistent with the hypothesis that the main mechanism driving our result is an increase in indirect meeting opportunities.

C. Educational Performance

It is reasonable to hypothesize that a student's performance in school may have an impact on the race of their future adult partners. For instance, if worse grades mean that students are less likely to go to college, they may then meet proportionally more black people and as a result be more likely to have a relationship with blacks. Similarly, if students go on to engage in criminal behavior, they may then come into contact with more blacks, and this may impact the race of their partners.

[23] Alternatively, we can split the sample based on these measures of distance and test for differences in the coefficient across the samples. The results of this analysis are very similar (table C10).

Table 9
Impact by Age and Social Network

	Any Black Partners in Wave 4		Any Black Partners in Waves 1 or 2			
	Met before Age 23 (1)	Met after Age 23 (2)	In Same School (3)	Not in Same School (4)	With Mutual Friend (5)	Without Mutual Friend (6)
Grade black share, same gender	.139	.259**	−.00691	.273***	.0686	.221*
	(.113)	(.105)	(.121)	(.0992)	(.145)	(.128)
Grade black share, opposite gender	−.00216	−.00935	.126	−.0172	.0970	.193
	(.0878)	(.0924)	(.184)	(.106)	(.130)	(.191)
p-value, coefficients equal	.4		.02		.37	
Observations	8,879	8,879	8,879	8,879	8,879	8,879
Adjusted R^2	.060	.028	.050	.049	.048	.058

NOTE.—The table reports ordinary least square estimates controlling for grade size, grade-gender fixed effects, and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

To test for this mechanism, we first analyze whether we observe any impact of cohort black shares on average test scores, college attendance, employment, earnings, or recorded crime. The results of these regressions are presented in table 10, and we can see that the coefficient on the black shares is always insignificant. This is consistent with Bifulco, Fletcher, and Ross (2011), who do not find any impact of minority shares on these outcomes.

The insignificance of the results in table 10 may, however, result from a lack of power rather than the absence of any real effects. Indeed, an impact on test scores of the size estimated in Billings, Deming, and Rockoff (2014),

Table 10
Impact of Cohort Black Shares on Educational Performance

	Average Test Score (1)	Attended College (2)	Employed (3)	Log Earnings (4)	Ever Arrested (5)	Ever Incarcerated (6)
Grade black share, same gender	.269	.328	.0823	−.769	−.155	−.0491
	(.397)	(.226)	(.232)	(.498)	(.239)	(.152)
Grade black share, opposite gender	.292	.161	−.0141	−.0961	.336	.0743
	(.384)	(.183)	(.220)	(.372)	(.218)	(.185)
Observations	8,808	8,878	8,875	8,125	8,838	8,873
Adjusted R^2	.110	.119	.037	.113	.093	.079

NOTE.—The table reports ordinary least square estimates controlling for grade size, grade-gender fixed effects, and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.

for instance, is within our 95% confidence interval. We therefore regress our main outcome variable on these measures to come up with an approximate upper bound for the size of this mechanism.[24] Even if we assume that all of the true coefficients are at the upper bounds of the various 95% confidence intervals, we estimate that impacts on educational performance, employment, and earnings can account for no more than 22% of the effect identified in our benchmark. This is consistent with the observation of Sacerdote (2014) that the impact of peers on social outcomes is larger than that on test scores. We therefore conclude that educational performance is unlikely to be an important mechanism in explaining our result.

VI. Conclusions

This paper finds that greater racial diversity in schools significantly impacts the prevalence of interracial adult relationships. The higher the share of black students of the same gender in a grade, the more likely a white student has a black partner during adulthood. Moreover, we provide evidence that this effect is associated with changes in stated attitudes and cannot be explained by increasing meeting opportunities, since it is persistent across time, space, and social networks.

Overall, our results enhance the understanding of how exposure to racial diversity can reduce the degree of assortativity by race in dating and marriage through a change in attitudes. This suggests that policies designed to increase racial diversity in schools could reduce racial prejudices and encourage social integration. There is need for further research, however, since the changes in minority shares that we study are smaller than those typically induced by policy. In particular, future work should further investigate whether potential nonlinearities in the impact emerge when considering larger variations in the share of black students, since these effects are hard to detect using the quasi-random variation we exploit.

Our paper has also highlighted the need to investigate further whether being exposed to racial diversity at school has implications for racial attitudes outside the social sphere. One could imagine that attitudes that impact romantic relationships might also affect discrimination in the labor market or the workplace, but it is also possible that the latter may be more affected by work-related experiences. This is an important question to investigate if we are to understand fully the effects of policies designed to decrease racial discrimination.

[24] Results are available on request. One reason this upper bound is approximate is because the coefficients in this regression are likely to be biased due to omitted variables. We nonetheless use these coefficients because no well-identified impacts of these variables exist, and the direction of this bias is in any case likely to inflate the result.

Appendix A

Variable Definitions

Table A1
Description of Variables

Variable	Wave	Description	Values
Main variables:			
Any black partners	4	Reports any black partners	No = 0, yes = 1
Share of black partners	4	Share of reported partners who are black	[0, 1]
Grade black share, both genders	1	Share of students in an individual's grade who define themselves as black	[0, 1]
Grade black share, same gender	1	Share of students of the same gender as the individual in one's grade who are black (self-defined)	[0, 1]
Other variables:			
Are students prejudiced?	1	Extent to which students agree with statement "the students at this school are prejudiced"	0, …, 4
Average test score	1	Average of most recent grade in math, English, history, and science	[1, 4]
Being liberal	4	Dummy coded as 1 if respondent considers himself or herself as liberal or very liberal	
Earnings	4	Income received from personal earnings before tax	US$
Family income	1	Annual family income of individual	Thousands of US$
Grade size	1	Number of students in individual's grade	9, …, 965
Grades in school	1	Number of grades in individual's school	1, …, 6
Importance of race in relationships	3	Takes a value of 0 if "being of the same race or ethnic group" is ranked as a less important element of a serious relationship than love, fidelity, commitment, and money; takes a value of 1 otherwise	0, 1
In middle school	1	Individual's school has no grades beyond grade 9	No = 0, yes = 1
In high school	1	Individual's school contains no grades before grade 9	No = 0, yes = 1
Living in urban area	1	Respondent lives in an urban area	No = 0, yes = 1
Number of cohabiting partners	4	Number of partners individual cohabited with for at least a month	0, …, 21
Parent is black	1	Parent interviewed in wave 1 (normally resident mother) defines themselves as black	No = 0, yes = 1
Respondent is white	1	Interviewer defines respondent as white	No = 0, yes = 1

Table A1 (*Continued*)

Variable	Wave	Description	Values
School segregation	1	Segregation of blacks in individual's school, by gender, as defined by Echenique and Fryer (2007)	[0, 1]
Share of census block black	1	Proportion of census block group population black	[0, 1]
Years of parental schooling	1	Years of schooling of individual's most educated parent	8,..., 17

Appendix B

Tests for Nonrandom Clustering

We undertake a number of tests that look for evidence of nonrandom clustering of black students within schools. The relevant sample on which to conduct tests of nonrandom clustering is the one we use to construct cohort black shares. Hence, for these tests we use the sample of around 80,000 students who were surveyed in the in-school survey and who are in cohorts containing at least one student present in our main analysis sample.

The intuitive idea behind these tests of nonrandom clustering is that if cohorts are more or less black in some systematic way, then an individual's race will be significantly correlated with that of their peers. Traditionally, this hypothesis would be tested by regressing a dummy variable of whether an individual is black on the black share of the rest of their peer group. However, such a test would typically produce a negatively biased coefficient since individuals' peer groups necessarily exclude the individuals themselves. We thus undertake several tests designed to avoid this exclusion bias.

Caeyers and Fafchamps (2016) derive analytically a formula for the exclusion bias and then show that one can test for nonrandom clustering by transforming the standard test appropriately. In particular, in column 1 of table B1 we use as a dependent variable the "transformed black dummy" $\widehat{Black_i}$, where

$$\widehat{Black_i} = Black_i - bias_{cs} \times ShareBlack_{cs}.$$

Here, $Black_i$ is a dummy taking the value 1 if individual i is black and $bias_{cs} = (N_s - 1)(K_c - 1)/[(N_s - 1)(N_s - K_c) + (K_c - 1)]$, where N_s is the number of students in the school and K_c is the number of students in the cohort. The regression produces an insignificant coefficient and hence does not reject random clustering. In column 2, we carry out a similar test with the grade divided by gender. Coefficients on both peer groups are small and insignificant, consistent with our assumption of quasi-random allocation across grades.

An alternative method for correcting for exclusion bias is proposed by Guryan, Kroft, and Notowidigdo (2009), who suggest controlling for the set of all potential peers. In our case, this involves adding the black share among all other students in the school as a control variable. Results of this test are displayed in columns 3 and 4: again, the coefficients of interest on the cohort black shares are insignificant.

A simple and less formal test is presented in column 5, whereby we regress the male black share on the female black share. The coefficient is insignificant, suggesting that there is unlikely to be important self-selection or omitted variables when it comes to race shares, since most factors that we could imagine influencing the female black share would also simultaneously influence the male black share.

Stevenson (2017) suggests an alternative test for nonrandom clustering: to pick randomly one observation within each cohort and to regress the share of blacks among the rest of the cohort on a dummy for whether the selected individuals are black, along with school-gender and grade-gender fixed effects. In this way, each observation is only present on either the right-hand side or left-hand side in each regression and there is no bias generated. We do this 10,000 times and, using the derived test statistics, obtain a p-value of .61 for the grade black share and a p-value of .50 for the same-gender grade black share. Thus, we are far from rejecting random clustering.

Feld and Zölitz (2017) show that if variation in cohort black shares is systematic, then measurement error should bias the coefficients of our regressions upward; they should be biased downward if the variation is instead quasi-random. In appendix section C4, we test for the impact of measurement error on our results by introducing random error in our measure of race. Consistently with variation in cohort black shares being quasi-random, we find that doing so biases our results toward zero.

As a final test on the randomness of variation in grade black share, we check whether differences in black share across grades are symmetric. If we found that black shares were on average significantly higher for later grades or the opposite, we might worry that the variation stemmed from systematic trends. For instance, if changes in grade black share were driven by blacks dropping out disproportionately, we would observe that on average black shares would fall as we advanced through grades. To examine this, we collapse our data down to the school grade level and calculate the change in black share between each grade and the previous grade. We plot the distribution of this variable in figure B1, and we observe that there is no obvious asymmetry. Indeed, the mean change in grade black share is -0.0000266, while the mean absolute change in grade black share is 0.0262.

Overall, therefore, all of the tests we perform suggest that the variation in black share across grades can be considered as good as random.

Table B1
Tests for Nonrandom Clustering

	Transformed Black Dummy (1)	Transformed Black Dummy (2)	Black Dummy (3)	Black Dummy (4)	Black Share of Males in Grade (5)
Black share of others in grade	.219		.121		
	(.190)		(.500)		
Black share of others of same gender in grade		.00553		−.115	
		(.0981)		(.257)	
Black share of opposite gender in grade		.0291		.0541	
		(.0920)		(.261)	
Black share of others in school			−109.2***	−109.1***	
			(25.83)	(25.63)	
Black share of females in grade					.0636
					(.0799)
Observations	81,638	81,638	81,638	81,638	80,696
Adjusted R^2	.999	.397	.410	.411	.980

NOTE.—The table reports ordinary least square estimates. Controls in cols. 1–4 include grade-gender fixed effects and school-gender fixed effects and in col. 5 include grade and school fixed effects. Regressions reported in this table are run on the wave 1 in-school survey. Standard errors (in parentheses) are clustered at the school level.

*** $p < .01$.

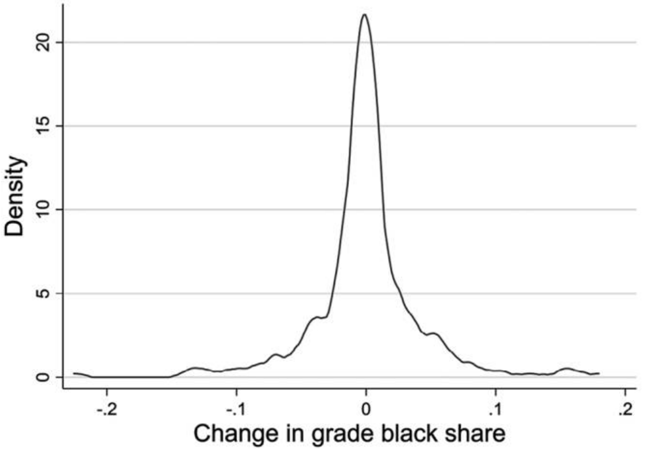

FIG. B1.—Kernel density of change in grade black share.

Appendix C

Supplementary Results and Robustness Checks

C1. Using Share of Black Partners as the Dependent Variable

In the benchmark results presented in table 5 we used two alternative dependent variables—a binary variable indicating whether an individual reported any black partners and a continuous variable measuring the share of reported partners who are black. While the tables in the main text focus on the specification using the binary variable, we now present results using the share of partners who are black. Results in subsequent sections of the appendix are then reported for both dependent variables. Table C1 reports the results when we run the same regressions as reported in tables 6 and 8 with the alternative dependent variable. Table C2 then reports the results when we run the regressions shown in table 9. In both tables, we see that results are very similar to those obtained using the binary dependent variable.

Table C1
Robustness to Controls and Interaction with Distance Variables When the Dependent Variable Is Share of Black Partners

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Grade black share, same gender	.261***	.275***	.257***	.212**	.160	.204	.280***	.309**	.284***
	(.0858)	(.0852)	(.0854)	(.101)	(.122)	(.152)	(.0956)	(.125)	(.104)
Grade black share, opposite gender	.0586	.0570	.0603	.0495			.0465	.107	.0916
	(.0808)	(.0810)	(.0833)	(.0872)			(.102)	(.153)	(.0958)
Same sex black share × distance (1,000 km)							−.0679		
							(.139)		
Opposite sex black share × distance (1,000 km)							.0469		
							(.191)		
Same sex black share × not in school county								−.143	
								(.155)	
Opposite sex black share × not in school county								−.175	
								(.205)	
Same sex black share × all partners met out of state									−.0917
									(.257)
Opposite sex black share × all partners met out of state									−.330
									(.295)
Benchmark controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Extended controls		Y	Y	Y	Y	Y			
Extended cohort controls			Y	Y	Y	Y			
School trends				Y					
School-grade fixed effect					Y	Y			
Tract fixed effect						Y			
Observations	8,696	8,696	8,696	8,696	8,696	8,696	8,648	8,692	8,696
Adjusted R^2	.056	.062	.063	.074	.074	.158	.080	.068	.065

NOTE.—The table reports ordinary least square estimates. The dependent variable is share of partners who are black. Benchmark controls are grade size, grade-gender fixed effects, and school-gender fixed effects. Extended controls include an individual's religion, birth year, the black share of the census block group, whether an individual lived with a single parent at wave 1, whether an individual had repeated or skipped a grade prior to wave 1, family income, mother's education, whether an individual was born in the United States, and the individual's age at wave 4. Extended cohort controls include the share of the same-gender cohort whose mother attended college, the share whose father attended college, the share Hispanic, the share Asian, the share whose parents were born in the United States, and the grade gender ratio. In cols. 7–9 we also control for the interaction of the relevant distance variable with grade-gender and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.

** $p < .05$.

*** $p < .01$.

Table C2
Impact by Age and Network When the Dependent Variable Is Share of Black Partners

	Black Share of Wave 4 Partners		Black Share of Wave 1 and 2 Partners			
	Met before Age 23 (1)	Met after Age 23 (2)	In Same School (3)	Not in Same School (4)	With Mutual Friend (5)	Without Mutual Friend (6)
Grade black share, same gender	.313**	.257**	.133	.513***	.170	.316
	(.136)	(.126)	(.218)	(.154)	(.220)	(.199)
Grade black share, opposite gender	.0798	−.00476	.420	−.0132	.0802	.290*
	(.130)	(.115)	(.274)	(.224)	(.180)	(.150)
p-value, coefficients equal	.4		.02		.37	
Observations	5,603	5,764	4,894	3,874	4,122	5,472
Adjusted R^2	.058	.055	.138	.125	.088	.152

Note.—The table reports ordinary least square estimates controlling for grade size, grade-gender fixed effects, and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

C2. Robustness to Attrition and Weighting

Since a number of individuals surveyed in wave 1 of the survey are not included in our final sample, we may be concerned that this attrition impacts our results. In this section we therefore undertake a number of tests to examine whether there is any evidence of attrition bias.

We begin by taking the sample of students interviewed in wave 1 and estimate equation (1) where the dependent is a dummy indicating whether the student is in our wave 3 sample. The results of this regression are presented in column 1 of table C3, and we note that the coefficient is insignificant. Column 2 presents the results of a similar regression where the dependent variable is a dummy for the student being in our wave 4 sample, and again we see that the coefficient is insignificant. These results suggest that there is no systematic relationship between an individual's cohort black shares and their probability to be in wave 4.

To provide further evidence that our results are not driven by attrition, we carry out a number of further tests. In column 3 of table C3, we simply take our benchmark regression and add a control variable indicating whether a respondent was interviewed in wave 3. The variable is negative and significant, suggesting that those interviewed in wave 3 are less likely to have black partners than those who were not, but this relationship appears orthogonal to our results—the coefficient on the same-gender cohort black share barely changes compared with the benchmark.

To mitigate against attrition problems, our analysis has used the probability weights constructed by Add Health. We may, however, be concerned that our results are being driven by individuals to whom Add Health assigns high weights in wave 4 of the survey. To test for this, column 4 removes those individuals with weights in the highest 10%. We observe that the coefficient on the same-gender cohort black share is significant and slightly larger than that in our benchmark regression. In column 5 we carry out the benchmark regression without using any probability weights, and we observe that our result is still significant at the 10% level. We therefore conclude that our results are not being driven by individuals to whom Add Health assigns high probability weights.

An alternative way of testing whether our results are driven by attrition is to look at relationships recorded in other waves of the survey. In column 6 of table C3 we therefore use as our dependent variable whether a respondent of the in-home survey has ever had a relationship with a black person outside of school in wave 1 or 2.[25] We find that the same-gender cohort black share has a significant impact on the probability of dating a black person, providing further evidence that our result is not being driven by attrition in wave 4 of the survey.

As a final test we calculate bounds on our result accounting for attrition using the method developed by Lee (2009). The bounds constructed by Lee (2009) aim to provide worst-case scenarios by taking extreme assumptions about attrition. Intuitively, the sample is trimmed to achieve balance between the treatment and control groups, removing alternately the highest and lowest values of the dependent variable. This provides bounds for the treatment effect on the assumption that the effect of treatment on attrition is monotonic—in our case, that students in grades with higher black shares are less likely to be in wave 4 than those in grades with low black shares.

The method developed by Lee (2009) is designed for situations where individuals are subject to a binary treatment and exogeneity is not conditional on control variables. To provide relevant bounds, we therefore need to adapt our specification. To do so, we first construct an independent "treatment" variable, which takes a value of 1 if an individual's cohort had a higher black share than the average cohort within the school and 0 otherwise. In column 1 of table C4 we undertake our benchmark regression with this alternative variable, and we obtain similar results to our benchmark—students in cohorts with a higher same-gender cohort black share than average are significantly more likely to have a black partner.

Columns 2 and 3 of table C4 then provide bounds on our result when we trim the sample manually in the spirit of Lee (2009). In particular, in columns 2 and 3 we drop observations from our control group so that the proportion treated in our final sample is the same as the proportion treated in

[25] We focus on relationships outside of school because changes in the number of blacks of the same gender affects the degree of competition for dating blacks within the school.

the original wave 1 sample. To calculate a lower bound on our result in column 2, we drop those observations that contribute most to a positive correlation between the dependent and treatment variable, that is, those observations that have the smallest residual when we regress the dependent variable on the control variables. In column 3 we instead drop those observations in the control group with the highest such residual.

Trimming the sample manually in this way allows us to keep a specification close to our benchmark. As a robustness check, we also implement Lee's methodology precisely. To do so, we use as our dependent variable the residual when we regress our base dependent variable (i.e., "any black partner" or "share of black partners") on our set of controls (grade size, grade-gender fixed effects, and school-gender fixed effects). Column 4 of table C4 presents the results when we regress this variable using ordinary least squares on our treatment variable. Columns 5 and 6 then display upper and lower bounds as calculated using the Stata command leebounds produced by Tauchmann (2014). Columns 7–12 of table C4 then carry out the same analysis using the share of black partners rather than the binary variable.

The results in table C4 suggest that our results are not significantly upward biased due to attrition: in all cases, the lower bound is significantly above zero. Moreover the 95% confidence interval for the treatment effect as calculated by Imbens and Manski (2004) does not include zero. Overall, therefore, we conclude that attrition is unlikely to be able to explain the effect we find.

Table C3
Robustness to Attrition and Weighting

	In Wave 3 Sample (1)	In Wave 4 Sample (2)	Any Black Partners in Wave 4 (3)	(4)	(5)	Any Black Partners in Wave 1 or 2 (6)
Grade black share, same gender	.185	−.0397	.290**	.402**	.214*	.191**
	(.127)	(.135)	(.119)	(.154)	(.115)	(.0776)
Grade black share, opposite gender	−.00706	−.0986	−.0496	.0197	.0820	.0771
	(.168)	(.168)	(.123)	(.101)	(.103)	(.103)
In wave 3 sample			−.0174**			
			(.00801)			
Weights	None	None	Wave 4	Wave 4 excluding top 10%	None	Wave 1
Observations	11,700	11,700	8,879	8,014	9,141	11,026
Adjusted R^2	.036	.033	.063	.055	.045	.040

Note.—The table reports ordinary least square estimates controlling for grade size, grade-gender fixed effects, and school-gender fixed effects. Standard errors (in parentheses) are clustered at the school level.
* $p < .10$.
** $p < .05$.

Table C4
Lee Bounds Analysis

	OLS	Manual Trimming Lower Bound	Manual Trimming Upper Bound	OLS	Stata Command Lower Bound	Stata Command Upper Bound
	Base Dependent Variable: Any Black Partners					
	(1)	(2)	(3)	(4)	(5)	(6)
Main variables:						
Above-average same-gender black share	.0163**	.0129*	.0538***	.0129***	.0105**	.0541***
	(.00686)	(.00743)	(.00650)	(.00469)	(.00502)	(.00418)
Grade black share, opposite gender	−.0561	−.0396	.00274			
	(.124)	(.128)	(.101)			
Benchmark controls	Y	Y	Y			
Observations	8,879	8,596	8,596	8,879		
	Base Dependent Variable: Share of Black Partners					
	(7)	(8)	(9)	(10)	(11)	(12)
Main variables:						
Above-average same-gender black share	.0114**	.0108**	.0339***	.00935***	.00772**	.0355***
	(.00467)	(.00500)	(.00447)	(.00338)	(.00363)	(.00303)
Grade black share, opposite gender	.0584	.0596	.0321			
	(.0826)	(.0837)	(.0640)			
Benchmark controls	Y	Y	Y			
Observations	8,696	8,431	8,431	8,696		

NOTE.—The dependent variable in cols. 1–3 is whether an individual reports any black partners, and in cols. 4–6 it is the residual when this variable is regressed on grade size, school-gender fixed effects, and grade-gender fixed effects. The dependent variable in cols. 7–9 is the share of partners who are black, and in cols. 10–12 it is the residual when this variable is regressed on grade size, school-gender fixed effects, and grade-gender fixed effects. Lower and upper bounds in cols. 2, 3, 8, and 9 are obtained by trimming the sample of those who were in a grade with an above-average black share based on the value of the residual and then undertaking ordinary least squares (OLS). Lower and upper bounds in cols. 5, 6, 11 and 12 are calculated using the methodology of Lee (2009) via the Stata command leebounds produced by Tauchmann (2014). Standard errors (in parentheses) are clustered at the school level in cols. 1–3 and 6–8. Benchmark controls are grade size, school-gender fixed effects, and grade-gender fixed effects.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

C3. Alternatives to the Linear Probability Model

We have used a linear probability model throughout the paper for simplicity and ease of interpretation. We may, however, be concerned that the model might not perform well in a situation where the dependent variable contains many zeros and relative few ones. To test the robustness of our result to alternative model specifications, table C5 presents the results of our benchmark estimate when we use three nonlinear regression models: probit, logit, and Poisson. From the table, we can note that the significance of our coefficient of interest is robust to these alternative models. We therefore conclude that our results are not driven by the behavior of the linear probability model.

Table C5
Nonlinear Specifications

	Dependent Variable: Any Black Partners			Dependent Variable: Share of Black Partners		
	Probit (1)	Logit (2)	Poisson (3)	Probit (4)	Logit (5)	Poisson (6)
Main variables:						
Grade black share, same gender	2.404**	4.637**	3.715**	2.426**	4.657**	4.601***
	(.983)	(1.896)	(1.476)	(.991)	(1.912)	(1.461)
Grade black share, opposite gender	−.701	−1.620	−1.370	−.668	−1.547	.538
	(.920)	(1.723)	(1.341)	(.928)	(1.740)	(1.448)
Observations	8,879	8,879	8,879	8,696	8,696	8,696

Note.—The table present various model specifications that control for grade size, grade-gender fixed effects, and school-gender fixed effects. Standard errors (in parentheses) are clustered at the school level.
** $p < .05$.
*** $p < .01$.

C4. Robustness to Measurement Error

Measurement error in race is not likely to be a concern. However, as discussed in Section IV, if the variation in black shares within schools is not quasi-random, then the estimated coefficients might be biased due to measurement error. One way to test whether our results may be biased by measurement error, suggested by Carrell, Hoekstra, and Kuka (2016) and Feld and Zölitz (2017), is to gradually introduce measurement error into our data and observe how our coefficient of interest changes. In particular, we repeat the following process 1,000 times. First, we generate a new variable that takes a value of 1 with a probability equal to the predicted black share based on school, gender, and grade. Second, we generate a new black dummy variable that takes the observed value with a 99% chance and the random value with a 1% chance. We construct new same-gender cohort black shares based on this dummy and then undertake our standard regression. Third, we repeat this second step for other error levels.

Figure C1 shows the results of this process, where we plot the average coefficient generated as well as the 95% range. We can see that as more measurement error is introduced, the coefficient falls toward zero. This is consistent with our variation being quasi-random and shows that measurement error would bias our results downward rather than upward.

Another way to check for measurement error is to add variables that may be correlated with the measurement error and observe whether our result changes. We therefore add to our benchmark regression two variables that are likely to be correlated with an individual's true race: a dummy for whether the interviewer in wave 4 declares the surveyed individual to be black and the share of the population that are black in the census block group where they live. The results are shown in columns 2 and 3 of table C6, and we include our benchmark regression in column 1 for comparison. Both added variables are positive and highly significant, but the coefficient on the same-gender cohort black share changes little from the benchmark result in column 1. The same holds true when we use the share of black partners as the dependent variable. This further suggests that measurement error is unlikely to be driving our results.

One further technique that has been used to address the concerns of Angrist (2014) is to split the sample between the individuals who may be producing the peer effects from those who are being influenced by them. In columns 4 and 8, therefore, we show that our result holds when we make the main independent variables the number of blacks in their cohort of same and opposite gender rather than their respective share.

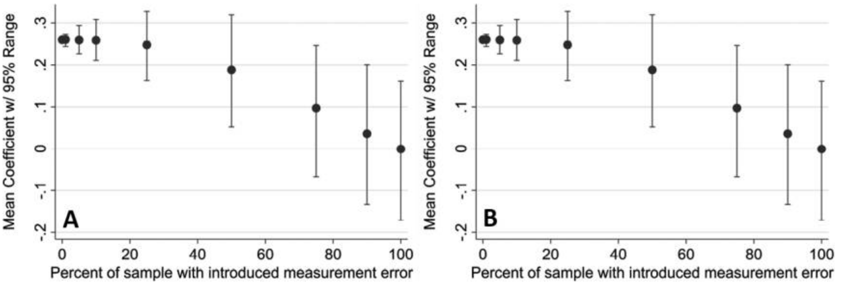

FIG. C1.—Sensitivity of the main result to measurement error in the black variable. *A*, Dependent variable: any black partners. *B*, Dependent variable: share of black partners. The *Y*-axis variable is the average coefficient on same-gender cohort black share from 1,000 regressions where, before each regression, the black dummy variable is replaced with a random value for a share of the sample. This share is indicated on the *X*-axis. A color version of this figure is available online.

Table C6
Robustness to Measurement Error

	Dependent Variable: Any Black Partners				Dependent Variable: Share of Black Partners			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grade black share, same gender	.283**	.282**	.296**		.261***	.258***	.271***	
	(.119)	(.121)	(.119)		(.0858)	(.0854)	(.0849)	
Grade black share, opposite gender	−.0548	−.0612	−.0602		.0586	.0528	.0543	
	(.123)	(.123)	(.125)		(.0808)	(.0822)	(.0801)	
Black dummy—wave 4 interviewer		.257***				.246***		
		(.0838)				(.0897)		
Census block black share			.134***				.105**	
			(.0480)				(.0402)	
Number of blacks of same gender				.00232***				.00125**
				(.000682)				(.000488)
Number of blacks of opposite gender				.000204				−.000197
				(.000707)				(.000515)
Observations	8,879	8,879	8,879	8,879	8,696	8,696	8,696	8,696
Adjusted R^2	.062	.067	.065	.062	.056	.066	.060	.054

NOTE.—The table reports ordinary least square estimates. Controls include grade size, grade-gender fixed effects, and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.
** $p < .05$.
*** $p < .01$.

C5. Other Specifications of Race

Table C7 presents the results when we look at race in different ways. First, in column 1 we use the sample of relationships where the partner was also interviewed: in waves 1 and 2 a number of in-school partners were interviewed as part of the normal in-home survey, and in wave 3 a random subsample of current partners were interviewed. Using this subsample of partners allows us to use a different definition of partner race from that declared by the main interviewee and thus examine the possibility that cohort black shares influences interviewees' reporting of partners' race. The coefficients are significantly different from zero and insignificantly different from our benchmark results, suggesting that any impact on respondents' reporting of partners' race is unlikely to be affecting our results.

In column 2, the black shares are calculated based on those who declare themselves to be black rather than simply those who declare themselves to be black and only black. This distinction might be important since those of mixed race are likely to behave differently from blacks (Fryer et al. 2012). We obtain a very similar result, which is not surprising given the relatively small number of mixed-race individuals. Columns 3 and 4 then look at two other minorities, Hispanics and Asians, and find no significant relationship between their cohort shares and subsequent adult relationships with this groups. This suggests that prevailing attitudes toward interracial relationships with these groups are different.

Finally, columns 5–8 show that similar conclusions can be drawn when we focus on the effect on the share of partners of a given race.

Table C7
Other Specifications of Race

	Dependent Variable: Any Partners				Dependent Variable: Share of Partners			
	Black (per the Interviewer) (1)	Black (2)	Hispanic (3)	Asian (4)	Black (per the Interviewer) (5)	Black (6)	Hispanic (7)	Asian (8)
Grade black share, same gender	.603**				.602**			
	(.264)				(.264)			
Grade black share, opposite gender	.0510				.0388			
	(.220)				(.220)			
Grade black (alternate definition) share, same gender		.252**				.221**		
		(.122)				(.0863)		
Grade black (alternate definition) share, opposite gender		−.129				−.00556		
		(.110)				(.0694)		
Grade Hispanic share, same gender			−.0333				−.0323	
			(.200)				(.136)	
Grade Hispanic share, opposite gender			.217				.133	
			(.219)				(.144)	
Grade Asian share, same gender				−.114				−.106
				(.105)				(.0812)
Grade Asian share, opposite gender				−.0577				−.0229
				(.127)				(.108)
Observations	2,105	8,879	7,614	8,879	2,105	8,696	7,463	8,696
Adjusted R^2	.186	.064	.109	.055	.190	.062	.118	.062

NOTE.—The table reports ordinary least square estimates. Partners in cols. 1 and 5 are all those interviewed in waves 1–3; all other columns concern partners reported in wave 4. Controls include grade size, grade-gender fixed effects, and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.

** $p < .05$.

C6. Subsample Splits and Interactions

In table C8, we present subsample splits that are of interest to understand further any variation in the impact of black students on the probability and the share of adult romantic relationships (first and second panel, respectively). For each split, we present results of a Wald test that the coefficients are identical in the final row.

First, we show that the effect is significant for both male and female students and not significantly different. We also find no evidence of significant differences when we break down the sample between middle and high schools, providing further evidence that our results are not driven by differential dropout rates or students moving between schools. Indeed, when the dependent variable is the share of black partners, the coefficient is significant for both middle and high schools.

The final splits shown in table C8 is by the region the school is located in. The test that all coefficients are equal cannot be rejected at the 10% level, although the effect does appear to be strongest in the Northeast. This may be related to different patterns of segregation or racial attitudes across regions, so we next try to study whether there is a systematic impact across these lines.

To investigate further potential heterogeneity in the effect, in table C9 we interact our independent variables of interest with various variables. This includes the share of blacks within the school, the segregation index proposed by Echenique and Fryer (2007) calculated using the racial composition of same-gender friendship networks, the share of students at the school who are from urban areas, the share of Republican votes in the 1992 presidential election in the county, and the number of students in the grade. We normalize each variable such that the median school has a value of zero. The interaction terms are generally insignificant, although one exception is the interaction with school segregation in column 2. This coefficient suggests that the grade black share has a greater influence when schools are less segregated, possibly suggesting that the grade black share is most likely to have an impact when there is more social mixing between whites and blacks. It should be noted, however, that segregation is correlated with many other variables, and therefore we should not overinterpret this interaction.

Finally, in table C10 we split our sample based on measures of geographical distance as an alternative to the interactions presented in table 8. For three different measures of distance, we present the results first for the sample of students who stayed relatively close to school and then for those who moved farther away. In each case, we note that the coefficients are similar across the two samples, and they are always far from being significantly different. For our preferred binary outcome measure, we find in five of six subsamples a positive significant coefficient of same-gender cohort black share. Overall, the subsample results are in line with table 8 and provide additional evidence that the effect is unlikely to be driven directly by changes in school-based social networks.

Table C8
Subsample Splits

	Gender		School Type		Region			
	Female	Male	Middle School	High School	Northeast	Midwest	South	West
	Dependent Variable: Any Black Partners							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grade black share, same gender	.348*	.199*	.267	.317	1.370**	.205	.137	.479
	(.198)	(.117)	(.179)	(.191)	(.547)	(.255)	(.116)	(.278)
Grade black share, opposite gender	−.0746	−.0345	−.0698	−.0405	.226	−.0873	−.0658	−.212
	(.240)	(.107)	(.190)	(.203)	(.287)	(.347)	(.118)	(.235)
p-value, coefficients equal	.52		.84		.1			
Observations	4,623	4,256	1,882	5,154	1,612	2,690	3,167	1,410
Adjusted R^2	.052	.043	.039	.073	.057	.115	.023	.053
	Dependent Variable: Share of Black Partners							
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Grade black share, same gender	.319**	.186**	.310**	.234*	.844***	.312	.141*	.307
	(.124)	(.0929)	(.135)	(.131)	(.295)	(.248)	(.0788)	(.178)
Grade black share, opposite gender	.0609	.0584	.117	.0813	.416	.145	−.0385	.0612
	(.144)	(.0730)	(.158)	(.118)	(.308)	(.188)	(.0943)	(.172)
p-value, coefficients equal	.36		.67		.1			
Observations	4,537	4,159	1,841	5,048	1,564	2,638	3,111	1,383
Adjusted R^2	.047	.028	.048	.062	.054	.090	.037	.038

Note.—The table reports ordinary least square estimates. Controls include grade size, grade-gender fixed effects, and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

Table C9
Interaction with Regional and School Characteristics

	Interaction Term				
	School Black Share	School Black Segregation	Republican Vote Share in 1992	School Urban Share	Students in Grade
	Dependent Variable: Any Black Partners				
	(1)	(2)	(3)	(4)	(5)
Grade black share, same gender	.513**	.592**	.254**	.222*	.422**
	(.249)	(.239)	(.122)	(.118)	(.205)
Grade black share, opposite gender	.184	−.0679	−.0805	−.107	−.0239
	(.202)	(.256)	(.130)	(.103)	(.146)
Same gender × interaction term	−.892	−.663*	−1.426	.202	.00169
	(.740)	(.390)	(1.123)	(.251)	(.00151)
Opposite gender × interaction term	−.910	−.0506	−1.498	.252	−.00112
	(.726)	(.432)	(1.299)	(.242)	(.00111)
Observations	8,879	8,802	8,828	8,879	8,879
Adjusted R^2	.062	.063	.064	.062	.073
	Dependent Variable: Share of Black Partners				
	(6)	(7)	(8)	(9)	(10)
Grade black share, same gender	.255*	.382***	.248***	.213**	.283**
	(.143)	(.127)	(.0877)	(.0839)	(.138)
Grade black share, opposite gender	.235	.164	.0711	.0122	.110
	(.153)	(.174)	(.0779)	(.0707)	(.117)
Same gender × interaction term	.0264	−.273	−1.374	.150	.000788
	(.516)	(.217)	(.922)	(.186)	(.00112)
Opposite gender × interaction term	−.650	−.242	−.630	.315**	−.00102
	(.455)	(.304)	(.963)	(.152)	(.000819)
Observations	8,696	8,622	8,645	8,696	8,696
Adjusted R^2	.058	.057	.064	.056	.067

NOTE.—The table reports ordinary least square estimates. Controls include grade size, grade-gender fixed effects, and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level. In cols. 5 and 10, the interaction term varies within schools, so we also interact it with school-gender fixed effects.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Table C10
Sample Splits by Distance from School

	Distance Moved		In School County		All Partners Met	
	≤100 km	>100 km	Yes	No	In State	Out of State
	Dependent Variable: Any Black Partners					
	(1)	(2)	(3)	(4)	(5)	(6)
Grade black share, same gender	.283*	.366*	.315*	.282*	.305*	.338
	(.150)	(.219)	(.178)	(.168)	(.158)	(.340)
Grade black share, opposite gender	−.120	.00952	−.138	−.00185	.0454	−.445
	(.183)	(.228)	(.243)	(.206)	(.179)	(.400)
p-value, coefficients equal	.73		.89		.93	
Observations	6,353	2,526	4,670	4,205	6,257	1,554
Adjusted R^2	.062	.117	.069	.090	.079	.151
	Dependent Variable: Share of Black Partners					
	(7)	(8)	(9)	(10)	(11)	(12)
Grade black share, same gender	.295***	.209	.303**	.174*	.302***	.159
	(.107)	(.155)	(.124)	(.104)	(.111)	(.229)
Grade black share, opposite gender	.0690	−.0299	.101	−.0603	.140	−.233
	(.118)	(.206)	(.154)	(.112)	(.109)	(.278)
p-value, coefficients equal			.39		.57	
Observations	6,203	2,493	4,548	4,144	6,257	1,554
Adjusted R^2	.059	.136	.066	.070	.068	.088

NOTE.—The table reports ordinary least square estimates. Controls include grade size, grade-gender fixed effects, and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

C7. Placebo Tests

To address concerns that our standard errors may be inappropriate or our results may be driven by some other cohort characteristic, we undertook two different sets of placebo tests. First, we constructed more than 200 other cohort share variables based on other questions in the in-school survey. The resulting variables include, for instance, the share of the cohort who are Hispanic, the share who live with both of their parents, and the share whose most recent history grade was an A. Figure C2 then plots the t-statistics from the regressions when we enter each of these variables individually into our regression instead of the same-gender cohort black share. The vertical lines represent the t-statistics we obtain in our benchmark, and for both dependent variables they clearly lie at the very right tail of the distribution. Hence, we can conclude that it is very unlikely that our result is driven by chance or correlation with another characteristic of school cohorts.

Our second placebo test involves reassigning students to cohorts randomly so that our measure of same-gender cohort black share is in general not that of their cohort but another within the same school. We then carry out the same regression as in our benchmark 10,000 times to produce a distribution of coefficients, which is displayed in figure C3 alongside the coefficient from our benchmark. As expected, the distributions are centered on zero, and of the 10,000 placebo regressions very few produce coefficients as large as these from our benchmark. In each case, less than 1% of the placebo coefficients are larger than the coefficient found in our benchmark regression. This further confirms that our result is not spurious.

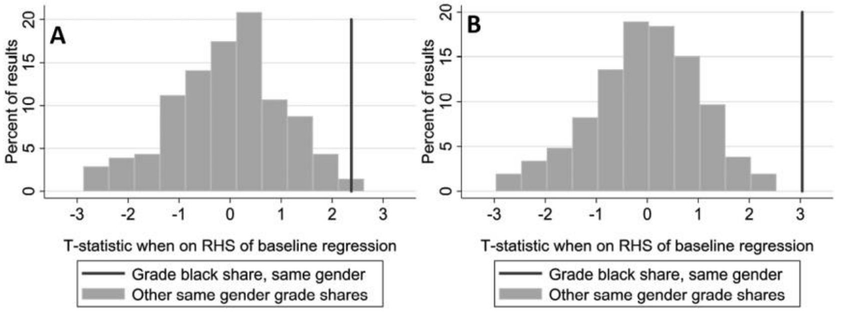

FIG. C2.—Distribution of t-statistics from regressions on other cohort shares. *A*, Dependent variable: any black partners. *B*, Dependent variable: share of black partners. Each t-statistic is taken from a regression where the independent variable is one of more than 200 cohort share variables. The vertical lines are the t-statistic in the benchmark specification, that is, columns 2 and 4 of table 5. A color version of this figure is available online.

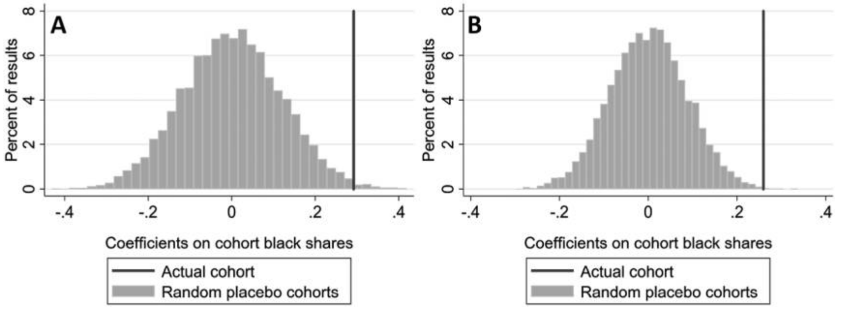

FIG. C3.—Distribution of coefficients from regressions on randomly assigned cohort shares. *A*, Dependent variable: any black partners. *B*, Dependent variable: share of black partners. Each coefficient is taken from a regression where the independent variable is the share of blacks among a randomly chosen cohort within the school. The vertical lines are the coefficients in the benchmark specification (cols. 2 and 4 of table 5). A color version of this figure is available online.

C8. Other Relationship Measures

Table C11 presents the results of making a number of alternative relationship measures the dependent variable in our standard specification. Columns 1–3 focus on binary variables that indicate whether an individual ever married or cohabited with someone, ever had a child, or ever divorced, respectively. Column 4 uses the log of 1 plus the number of partners reported in wave 4.[26] All of the coefficients are insignificant, implying that the shift in the racial composition of relationships does not go along with any change in their number or nature.

Columns 5 and 6 examine the effect on the probability of ever cohabiting or getting married with a black person and the black share of cohabiting and married partners. We find the coefficient to be of similar magnitude to our benchmark. Finally, in the spirit of Gordon and Reber (2018), we see that the share of black peers in school has a positive effect on the probability that white students have a child with a black partner in adulthood (col. 7).

[26] We use the log transformation since the distribution of the number of reported partners is highly skewed, with 90% of respondents reporting three partners or fewer but a few individuals reporting 19 or 20.

Table C11
Other Relationship Measures

	Dependent Variable						
	Ever Married or Cohabited (1)	Ever Had Children (2)	Ever Divorced (3)	ln(1 + Number of Reported Partners) (4)	Ever Married or Cohabited with a Black Person (5)	Share of Married or Cohabiting Black Partners (6)	Ever Had a Child with a Black Person (7)
Grade black share, same gender	−.0822	.0429	.183	.300	.188*	.203**	.207**
	(.227)	(.234)	(.189)	(.233)	(.0987)	(.0956)	(.0897)
Grade black share, opposite gender	−.207	.0343	.181	.0614	−.136	−.0570	−.0434
	(.221)	(.194)	(.168)	(.239)	(.110)	(.0858)	(.0779)
Observations	8,879	8,879	8,879	8,879	8,879	7,579	8,879
Adjusted R^2	.073	.130	.063	.037	.069	.069	.055

NOTE.—Controls include grade size, grade-gender fixed effects, and school-gender fixed effects. Wave 4 cross-sectional weights are used. Standard errors (in parentheses) are clustered at the school level.
* $p < .10$.
** $p < .05$.

Appendix D

Schools with Most Variation in Cohort Black Share

Table D1 provides summary statistics for schools that have within-school variation in the black cohort share above the median. In comparison with table 1, we see that the main difference is the share of black students in the school. We also see that these schools are more likely to be located in the South and have a greater share of students living in urban areas, but there are still many schools in areas outside the South and with rural students that are in this half of the sample.

Table D1
Summary Statistics for Schools with Standard Deviation in Cohort Black Share above the Median

	Mean	Within-School SD	Between-School SD	*N*
Main variables:				
Any black partners	.058	.21	.051	4,330
Share of black partners	.038	.15	.037	4,239
Grade black share, both genders	.15	.022	.14	4,330
Grade black share, same gender	.15	.033	.14	4,330
Other wave 1 variables:				
Age	16	1.2	1.1	4,330
Female	.51	.49	.094	4,330
Hispanic	.24	.23	.35	4,330
Family income	52	43	33	3,356
Grade size	264	33	190	4,330
Grades in school	4	0	1.2	4,330
In middle school	.16	0	.38	4,330
In high school	.65	0	.5	4,330
Lives in urban area	.65	.17	.42	4,290
Region = Northeast	.21	0	.43	4,330
Region = Midwest	.12	0	.34	4,330
Region = South	.49	0	.52	4,330
Region = West	.17	0	.39	4,330
Other wave 4 variables:				
Age	29	1.2	1.2	4,330
Number of recorded partners	1.9	1.4	.28	4,330
Number of cohabiting partners	1.4	1	.28	4,330
Number of marriages	.63	.56	.21	4,239
Attended college	.66	.43	.18	4,330
Employed	.66	.47	.068	4,329

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