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ABSTRACT

The Long Run Impact of Childhood Interracial Contact on Residential Segregation*

This paper exploits quasi-random variation in the share of Black students across cohorts within US schools to investigate whether interracial contact in childhood impacts the residential choices of Whites in adulthood. We find that, 20 years after exposure, Whites who had more Black peers of the same gender in their grade go on to live in census tracts with more Black residents. Further investigation suggests that this result is unlikely to be driven by economic opportunities or social networks. Instead, the effect on residential choice appears to come from a change in preferences among Whites.

JEL Classification: I29, J15, R23

Keywords: residential segregation, social contact, race

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1 Introduction

Racial segregation is a salient and durable characteristic of life in American cities. Even fifty years after the civil rights era, Black-White segregation remains at very high levels. According to the latest figures from the 2020 census, the average White metropolitan area resident lives in a neighborhood that is 9 % Black, while the average Black resident lives in a neighborhood that is 41 % Black (Logan and Stults, 2022). The social and economic consequences range from adverse effects on education and earnings to negative effects on health behavior and outcomes (Ananat, 2011; Logan and Parman, 2017; Niemesh and Shester, 2020; Derenoncourt, 2022). The latter has been tragically highlighted by the COVID-19 pandemic, during which segregated counties in the US experienced above-average death and infections rates (Torrats-Espinosa, 2021).

The literature differentiates between three different causes of Black-White residential segregation: actions to exclude Black people from predominantly White neighborhoods, preference-based self-selection of Black people into Black neighborhoods, and White people choosing not to live in neighborhoods with higher shares of Black residents (e.g. Cutler, Glaeser, and Vigdor, 1999; Boustan, 2011). The empirical evidence suggests that the latter is one of the most important factors in explaining the persistence of Black-White segregation in the US (Crowder, 2000; Boustan, 2010; Shertzer and Walsh, 2019). Card, Mas, and Rothstein (2008) document a substantial heterogeneity in segregation dynamics over time and across regions, and find this to be correlated with Whites' racial attitudes. Yet little is known about the mechanisms behind this relationship, or the extent to which preferences can be changed to reduce residential segregation.

This paper addresses this research gap and investigates whether exposure of Whites to Black peers at a young age can impact residential racial segregation. In particular, we analyze how plausibly exogenous variation in a White student's school cohort affects residential location choices later in life. The data used comes from the National Longitudinal Survey of Adolescent Health (Add Health) which, for a nationally representative sample of adolescents, provides information on the race of all students in their school and then surveys them at various points over the next twenty years. This allows us to exploit idiosyncratic variation in grade composition within schools, a methodology first proposed by Hoxby (2000) that has since been widely used to identify causal peer effects.[1] We provide several tests giving evidence that the variation used is good as random and uncorrelated with other variables that might influence residential choices.

The main contribution of this paper is then to demonstrate that the racial composition of students' school cohorts impacts residential location choices later in life. We find that White individuals who were in grades with more Black students of the same gender in 1994-95 are more likely to live in neighborhoods with more Black residents in 2016-18. The magnitude of the effect implies that going from the average of 8 percent Black students of the same gender in the grade to 10 percent increases the share of Black residents in one's neighborhood two decades later by almost 0.4 percentage points, which is 5 percent of the mean. The results are robust to several modifications of the model, including the introduction of grade-school and tract fixed effects.

A priori, these results could be driven by three distinct channels: economic opportunities, social networks, and racial preferences. We provide several pieces of evidence which speak against economic opportunities being a major force behind our results. We find no effect of cohort racial composition on individual education and labor market outcomes, nor do we detect any impact on other neighborhood characteristics such as average income or property value. We further document that our results are unlikely to be driven by friendships and social ties formed in school nor by the preferences of partners in interracial relationships. Instead, it appears our results are likely to be shaped by changes in racial attitudes. Consistent with this, we find positive effects of exposure to Black peers on White adults' stated liberalness and the likelihood of interracial partnership, and find that exposure to Blacks in school changes the relationship between neighborhood racial composition, neighborhood satisfaction, and moving decisions. In particular, our estimates suggest that, for those exposed to a greater share of Black peers in schools, there

[1] See, for example, Bifulco, Fletcher, and Ross (2011); Lavy, Paserman, and Schlosser (2012); Carrell, Hoekstra, and Kuka (2018); Patacchini and Zenou (2016); Merlino, Steinhardt, and Wren-Lewis (2019); Fruehwirth, Iyer, and Zhang (2019).

is a reduction in patterns associated with 'White flight' (Schelling, 1971; Boustan, 2010). This is consistent with interracial contact changing Whites' attitudes towards mixing with Blacks (Williams, 1947; Allport, 1954).

Our paper therefore not only contributes to the literature on residential segregation, but also to that on the impact of interracial contact. Increasing evidence finds that contact between groups can change attitudes and influence behavior (e.g. Corno, La Ferrara, and Burns, 2019; Carrell, Hoekstra, and West, 2019; Bazzi, Gaduh, Rothenberg, and Wong, 2019; Mousa, 2020; Lowe, 2021; Bursztyn, Chaney, Hassan, and Rao, 2021; Boucher, Tumen, Vlassopoulos, Wahba, and Zenou, 2021). Yet there is limited evidence on whether such contact can have important behavioral impacts a long time after the contact has occurred. Exceptions include Merlino et al. (2019), who find that contact with Black people influences Whites' romantic partners, Billings, Chyn, and Haggag (2021), who find that it reduces registration with the Republican party, and Schindler and Westcott (2021), who find a reduction in far-right voting. We add to this literature by demonstrating an impact on an important economic decision decades after contact occurred.

The remainder of the paper is organized as follows. Section 2 describes the data set and estimation strategy, and provides evidence in favor of our main identification assumption. In Section 3, we present our benchmark results and several robustness checks. Section 4 interprets our empirical findings and discusses potential channels at play. Finally, Section 5 concludes and briefly discusses policy implications.

2 Data and estimation strategy

2.1 Data

We use data from the National Longitudinal Survey of Adolescent Health (Add Health).[2] The survey selected 80 nationally representative high schools and 54

[2]The Add Health project was designed by J. Richard Udry, Peter S. Bearman, and Kathleen Mullan Harris, and funded by a 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

feeder schools in the US 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 including family background, health behaviors and friendships. This in-home survey was administered to around 20,000 students, who then constituted the base sample for the subsequent waves, administered in 1996 (Wave 2), 2001-02 (Wave 3), 2008-09 (Wave 4), and 2016-18 (Wave 5).

In a first step, we derive information about school peers using all respondents of the in-school survey. As this is basically a census of students, using this data minimizes measurement error in constructing our main independent variables, i.e., the shares of students in peer groups who are Black.[3] We consider three alternative groups of peers, which we refer to as cohorts: all those in the same grade, those of the same sex in the same grade, and those of opposite sex in the same grade.

Our analysis then uses the contextual data for Wave 5 provided by Add Health to retrieve our main dependent variable, that is, the share of Blacks in the census tract of the respondent's residence in Wave 5.[4] This is estimated by Add Health using the American Community Survey and linked to all geolocated individuals interviewed in Wave 5. We also make use of other information provided by the Wave 5 survey including the respondent's education, labor market outcomes, and other tract characteristics.

We focus our attention on White students since they constitute the majority group, which is of primary interest when considering racial attitudes toward minorities. The relatively small number of students of other racial groups limits our

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[3]In the in-school survey, students self-report the race they identify with, with a small percentage citing more than one race. In this paper, the Black share is defined as the share of students who identify themselves as Black only.

[4]Census tracts are small geographic areas: they generally have between 1,500 and 8,000 people, with an optimum size of 4,000 people each. They are commonly used to present information for small towns, rural areas, and neighborhoods, and hence they provide us with a measure of local segregation. To give an idea, in the US there are about 74,000 census tracts.

ability to draw robust inference on whether they are affected differently. Of the total available sample of White respondents for which we have location data in Wave 5, we were unable to match 420 respondents with information on their school cohort. This leaves us with a total of 7,095 individuals, spread across 434 school cohorts and 840 peer groups of the same grade and same gender.

In terms of attrition, Bifulco et al. (2011) and Merlino et al. (2019) find no evidence that attrition in Wave 4 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 our Wave 5 sample or the probability of not responding to the first request to participate in Wave 5. Additionally, our results are robust to using sample weights and are improved by including individuals who didn't respond at the first request to participate in Wave 5. See Appendix Table A9 for more details.

Summary statistics of the main variables we use in our analysis are reported in Table 1. For individuals in our sample, the mean share of Blacks in the census tract in Wave 5 (our main outcome variable) is around 8%. Interestingly, the standard deviation is higher within schools than between them, suggesting that it is reasonable to look for factors which determine this outcome using within-school variation. In contrast, it should be noted that the variation in Grade Black share within schools is relatively low, being about 1.5 percentage points for the both gender measure and 2.5 percentage points for the same gender measure.[5] This means that we are unable to look at impacts of very large changes in grade Black shares in percentage point terms, but, given that the standard deviation is between 20-30% of the mean, it is substantial enough to generate important variations in exposure.

2.2 Estimation strategy

Directly regressing residential segregation on cohort composition may produce biased results since cohort composition is likely to be correlated with several (possibly

[5] Note that one reason these standard deviations are low is that a number of schools in our sample have no Blacks in any grade, and hence a standard deviation of zero. We keep individuals in these schools in our analysis, but since these schools do not contribute directly to our main results results are extremely similar when we remove them.

Table 1: Summary statistics

	Mean	Within school s.d.	Between school s.d.	N
Main variables				
Share of census tract Black, Wave 5	.082	.11	.079	7090
Share of census tract Black, Wave 1	.055	.063	.12	7034
Grade Black share, both genders	.08	.016	.19	7090
Grade Black share, same gender	.079	.025	.19	7090
Other Wave 1 variables				
Age	16	1.1	1.4	7090
Female	.56	.47	.14	7090
Hispanic	.13	.19	.23	7090
Family income ($000's)	52	34	25	5705
Grade size	224	24	132	7090
Grades in school	4.1	0	1.2	7090
In middle school	.22	0	.49	7090
In high school	.59	0	.5	7090
Lives in urban area	.46	.17	.43	7031
Region = Northeast	.18	0	.41	7090
Region = Midwest	.31	0	.43	7090
Region = South	.34	0	.49	7090
Region = West	.17	0	.36	7090

omitted) variables that impact residential choice—not least, the composition of the population that lives nearby the school. Moreover, self-selection of individuals into schools is problematic, as parents who are more inclined to live in Blacker neighborhoods may choose to enroll their kids in schools with a larger share of Black students.

In order to control for these factors, we exploit variation in the share of Black students across cohorts within an individual school. In other words, we assume that parents select a school for their kids independently of the differences between the average school composition and their child's school specific cohort (which is not observed at the time of enrollment). To implement our identification strategy, we estimate the following regression equation:

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

where $ShareBlack_{cs}$ 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. As we show below, the gendered racial composition of grades plays a significant role in our analysis. Hence, we split school and grade fixed effects by gender. Controlling for grade essentially also controls for respondents' age at the time of the Wave 5 interview. Standard errors are clustered at the school level.[6] Our main dependent variable Y_i is the share of the population living in the same census tract as the respondent in Wave 5 that is Black.

In our regressions, we first define a cohort as those students who are in the same grade within the school in Wave 1. We subsequently split each grade in two groups, considering separately those students of the opposite gender and those of the same gender. The idea is that peers of the same gender may influence individuals' behavior more if this is the group with which they are most likely to interact, which we will test for by regressing measures of interaction as dependent variables.

2.3 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. The idea is that while families might choose which school to send their kids to based on the average racial composition of the school, the differences between the average school composition and their child's school specific cohort do not play a role. We test three implications of this identification assumption.

First, we perform several balancing tests. In other words, we test whether within-school variation in the share of Black students is correlated with predetermined individual level variables. In particular, we regress a range of predetermined student characteristics on the Black share of their peer group controlling for school-gender and grade-gender fixed effects. For each characteristic, we perform two different balancing tests: first, regressing it on the Black share of students in each grade, and then simultaneously regressing on the Black share of students of opposite and same sex in each grade. We show in Table 2 the results of some of these balancing tests on the main sample we use in our analysis—results are very similar when we use samples relevant to supplementary regressions. The results support

[6] 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, Athey, Imbens, and Wooldridge (2017) for a discussion. Results are robust to clustering at the school-grade level.

our main identification assumption. In particular, only two of the predetermined variables, grade size and language spoken at home being different from English, are significantly different from zero at the 0.10 level, and only in some of the tests. We believe the correlation with these variables to be spurious; however, we control for them in all of our regressions.[7]

Second, we test for non-random 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-gender fixed effects. However, we need to take into account that each individual is present in many others' peer groups but not their own (Guryan, Kroft, and Notowidigdo, 2009). We therefore perform several tests designed to address this issue, including those proposed by Guryan et al. (2009) and Caeyers and Fafchamps (2016). More details can be found in Appendix B. Overall, none of the tests rejects random clustering. We also find no evidence that children who switch out of schools with high Black shares are less likely to live in Black neighborhoods later on. 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 plot the distribution of differences in the Black shares between grades. We find the distribution to be very symmetric, which is consistent with differences across grade being as good as random.

Finally, the variation in the share of Black students across grades may be partly affected by the end of court-ordered desegregation orders which occurred during this time. Lutz (2011) show that the expiration of court oversight led to signifi-

[7] Additionally, we run regressions like those reported in Table 2 for a comprehensive set of pretreatment student characteristics available in Add Health and observe how many coefficients are significant at the 5 percent level. Of the 86 variables, 9 % are significant when regressed on the both gender Black share, 6 % when regressed on the same gender Black share, and 6% when regressed on the opposite gender Black share, consistent with the Black shares being distributed quasi-randomly. We also use these variables together to predict the Wave 5 tract Black share, and find no significant correlation between this predicted Black share and our Wave 1 cohort Black shares when we control for school-gender and grade-gender fixed effects.

Table 2: Balancing tests for cohort composition measures

		Independent variable:		
	(1) N	(2) Grade Black share, both genders	(3) Grade Black share, opp. gender	(4) Grade Black share, same gender
Age	7,090	0.0191	-0.113	-0.0846
		(0.440)	(0.264)	(0.297)
Parent is Black	6,350	0.0441	0.00399	0.0543
		(0.0269)	(0.0355)	(0.0486)
Share of census tract Black	7,034	0.0102	0.0329	-0.00589
		(0.0851)	(0.0613)	(0.0588)
Share of census block Black	7,030	0.00335	0.0374	-0.0164
		(0.0976)	(0.0635)	(0.0816)
Grade size	7,090	125.8*	72.61*	59.76
		(74.95)	(39.33)	(43.77)
Share same gender	7,090	0.0215	0.0180	-0.0693
		(0.0701)	(0.0428)	(0.0478)
Born in USA	7,090	0.00679	0.0643	-0.0303
		(0.0836)	(0.0514)	(0.0628)
Lives with both biological parents	6,326	0.0871	0.165	-0.0447
		(0.359)	(0.216)	(0.245)
Number of older siblings	7,081	-0.481	0.0745	-0.492
		(0.748)	(0.498)	(0.435)
Years of parental schooling	6,816	1.254	1.233	0.0601
		(1.191)	(0.746)	(0.823)
Log of family income	5,650	0.611	0.422	0.0594
		(0.524)	(0.334)	(0.356)
Home language is not English	7,090	0.143	0.0201	0.145*
		(0.0970)	(0.0641)	(0.0760)

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. Coefficients in each row are from two separate regressions: the first where the variable in the first column is regressed on the overall grade Black share, and the second and third where the variable is regressed on the same gender and opposite gender Black shares simultaneously. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

cant 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 variation in cohort composition and Wave 1 neighborhood Black shares. This strongly suggests that the independent variables of interest is not systematically driven by changes in the residential location of pupils, nor by changes in its racial composition.

3 Main results

Before analyzing the impact of grade racial composition on residential choices, we look at whether a more diverse student population in school translates into close social contact. Indeed, 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 however could react to differences in composition by avoiding people with different background, leading to *de facto* segregation in schools. This would occur, for example, if they form very segregated friendship networks (Currarini, Jackson, and Pin, 2009; Mele, 2017). It is therefore important to test this assumption using information on contact provided in the Add Health data.

Table 3: Impacts of grade shares on childhood exposure and friendship

Dependent variable:	Share of classmates Black				Has Black friend			
	in Wave 1		in Wave 2		All friends		Closest friends	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grade Black share, both genders	0.407^{**}		0.296^{*}		0.167		0.115^{*}	
	(0.157)		(0.165)		(0.105)		(0.0640)	
Grade Black share, same gender		0.200^{**}		0.288^{***}		0.206^{***}		0.129^{**}
		(0.0911)		(0.0994)		(0.0781)		(0.0630)
Grade Black share, opposite gender		0.191^{*}		0.0276		-0.0186		0.00166
		(0.0994)		(0.0769)		(0.0771)		(0.0598)
Observations	2629	2629	2058	2058	7090	7090	7090	7090
Adjusted R^2	0.924	0.924	0.917	0.918	0.019	0.020	0.031	0.032
Dep. var. mean	0.086	0.086	0.085	0.085	0.015	0.015	0.008	0.008

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$ * $p < .10$, ** $p < .05$, *** $p < .01$

Table 3 reports two results indicating that a higher Black share in a grade increases social contact with Blacks. In columns (1) and (3), we show that more Blacks in a grade within a school translates into a higher share of classmates who are Black in school both measured at Wave 1 in 1994-95 and at Wave 2 in 1996.[8] Note that, while positive, the coefficient is significantly different from 1, suggesting that there is some segregation across classes within schools, possibly related to tracking or subject choice. In columns (5) and (7), we show that more Blacks in a grade also translates into a higher share of nominated friends and closest friends who are Black in school in Wave 1. Columns (2), (4), (6) and (8) then show that these results are generally driven by Black peers of the same gender as the respondent. These results are in line with those reported in Merlino et al. (2019) and are consistent with the broader literature that shows young people form closer friendships with individuals of their own gender (McPherson, Smith-Lovin, and Cook, 2001; Kalmijn, 2002; Soetevent and Kooreman, 2007). Correspondingly, we find that 70% of closest friends in Wave 1 are of the same gender, and 76% of closest friends who are in the same grade are of the same gender.[9]

Table 4 reports the main result of the paper: more exposure to Blacks in school has an impact on long-term residential choices. In particular, column (1) shows that individuals who were in grades with more Black students in 1994-95 are more likely to live in neighborhoods with more Blacks in 2016-18. Column (2) then shows that this effect is driven by Black peers of the same gender, in line with the results related to exposure shown in Table 3.

Figure 1 presents a version of our main result in a graphical fashion by plotting the relative share of Blacks in the (Wave 5) neighborhood of Whites against the relative share of Blacks in the (Wave 1) same gender cohort. The figure depicts a positive relationship which can be interpreted as follows: individual who are in a grade with more Black students of their gender with respect to their school average,

[8]Note that data on classes taken is only collected for a subset of schools, substantially reducing our sample. The in-school survey is only conducted in Wave 1 and therefore all grade Black shares are measured in 1994-95.

[9]Note also that our results are compatible with the existence of homophily in friendship found by Currarini et al. (2009) and Fletcher, Ross, and Zhang (2020). While that measure of homophily compares realized friendships with each group's share in the population of pupils, here we are interested in whether more diversity in the classroom implies more contact with Blacks in an absolute sense.

Table 4: Results on residential segregation in Wave 5

	Black share in census tract, Wave 5		Black share > 10%, Wave 5		Black share > 20%, Wave 5	
	(1)	(2)	(3)	(4)	(5)	(6)
Grade Black share, both genders	0.189**		0.588*		0.427**	
	(0.0746)		(0.309)		(0.190)	
Grade Black share, same gender		0.194***		0.454**		0.415**
		(0.0565)		(0.197)		(0.159)
Grade Black share, opposite gender		0.0109		0.219		0.00913
		(0.0557)		(0.286)		(0.108)
Observations	7090	7090	7090	7090	7090	7090
Adjusted R^2	0.188	0.189	0.149	0.149	0.141	0.141
Dep. var mean	0.0819	0.0819	0.253	0.253	0.118	0.118

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

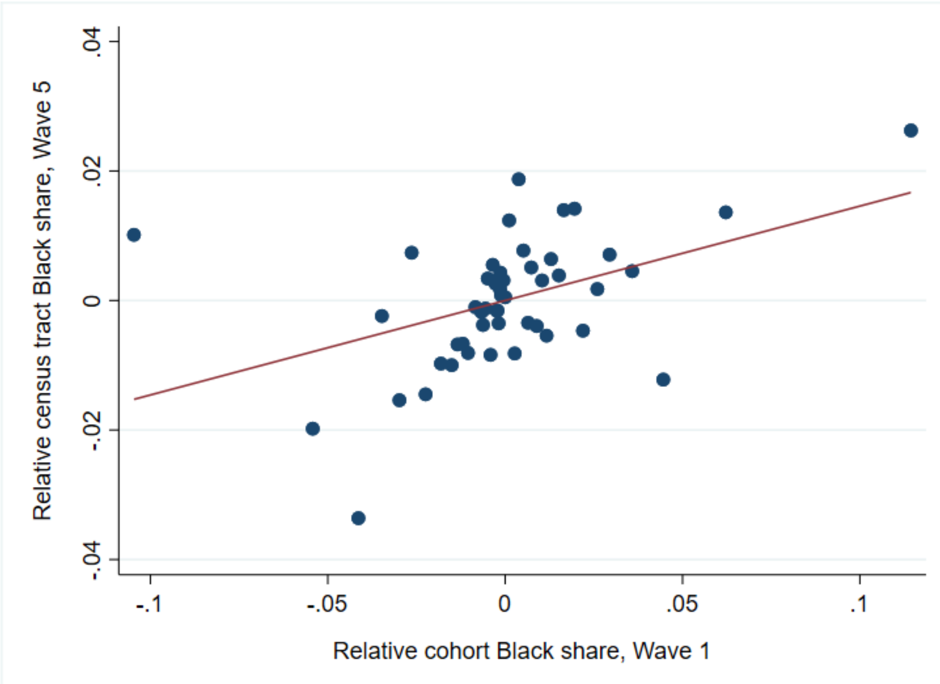
also end up living in Blacker neighborhoods in Wave V than their schoolmates.

In terms of magnitude, the point estimate in column 2 of Table 4 implies that going from the average of 8 percent Blacks in the same gender cohort to 10 percent (an increase of around one within-school standard deviation) would increase the share of Blacks in one's neighborhood in Wave 5 by almost 0.4 percentage points, which is 5 percent of the mean.

To better understand the nature of these findings, we construct dummy variables that take the value one if the individual resides in a neighborhood where the share of Blacks in 2016-18 is above a certain threshold. We then use the variables corresponding to the 10 % and 20 % thresholds as dependent variables in columns (3) to (6). The significant coefficients suggest that a large part of the main result is being driven by pushing White individuals to choose neighbourhoods over these thresholds. This is particularly interesting in light of the findings of Card et al. (2008) confirming Schelling (1971)'s theory and according to which the tipping points above which Whites leave a neighborhood range from 5% to 20%.

To elaborate further on this point, we plot in Figure 2 the results of regressions where a dummy variable is constructed for different values of the share of Blacks

Figure 1: Correlation of relative shares, same sex cohort

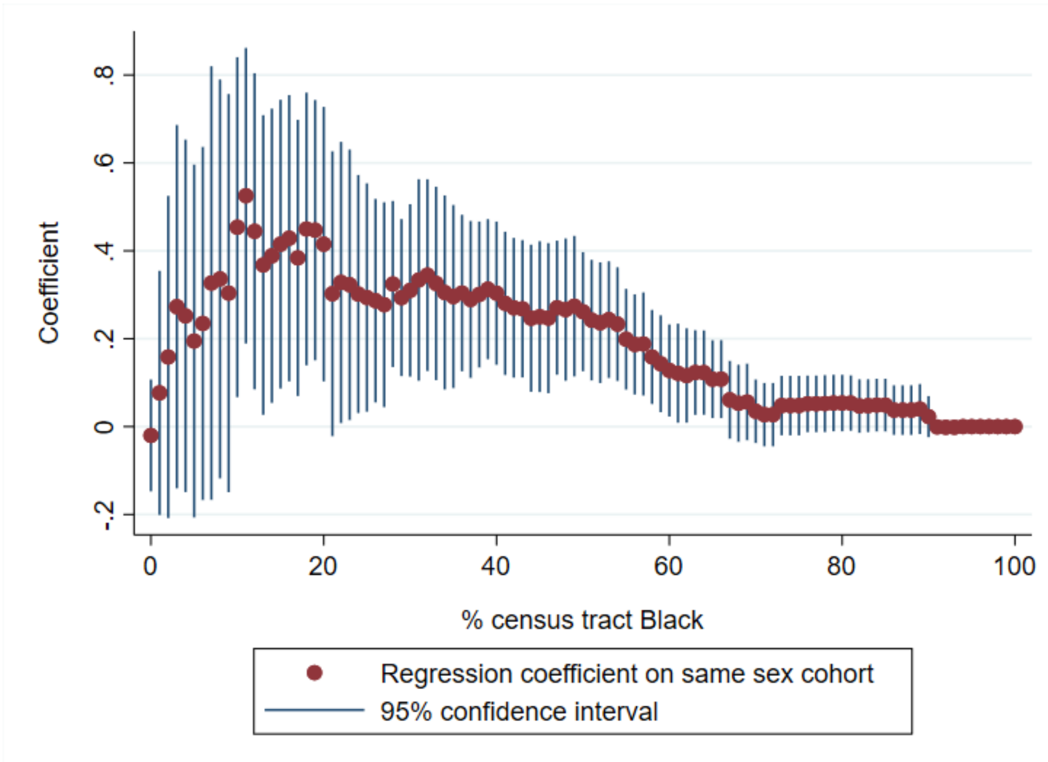


Notes: The figure plots a binned scatter plot of the relative share of Blacks in the Wave 5 neighborhood against the relative share of Blacks in the Wave 1 same gender cohorts. A cohort's relative Black share is the share within the cohort minus the minus the median of this variable among those in our sample who attended the same school. Individuals are binned into 50 bins of equal size according to their relative cohort Black share in Wave 1.

in the neighborhood in Wave 5, ranging from to 0 to 100%. The figure shows an inverted U-shape pattern, indicating that the effect is particularly strong for values around 10 to 20%, even though the average Black share in our sample is only 8%. This suggests that the impact of social contact is particular relevant for Whites that might live in neighborhoods which would be considered as having a large potential for White flight behavior.

Table 5 provides evidence of the robustness of our preferred specification, namely column (2) in Table 4. We report this result again in column (1) of Table 5 and then add in various sets of controls to observe how our coefficient of interest changes. In column (2), we include several individual controls measured in Wave 1, including family income, mother's education, and the Black share of the census tract. We explicitly show the coefficient on the census tract Black share in the table as it could be interesting to compare its magnitude with our identified effect. Doing so, shows

Figure 2: Impact across the distribution



Notes: The figure plots OLS coefficients and 95% confidence intervals of the same gender grade Black share when we use as an outcome variable a dummy that takes the value one if the respondent lives in Wave 5 in a neighborhood with more than x% Blacks (for different values of x, represented on the horizontal axis). The regressions control for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects.

that the coefficient of an individual's census tract Black share has less than half of the size of our main coefficient of interest. Column (3) additionally includes other characteristics of the Wave 1 cohort, including the share of the same gender cohort whose mother attended college and the share born in the US. 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, 2019).

We can additionally control for a number of unobservables by introducing school trends and other fixed effects. In column (4), we control for school-specific trends, and in column (5) for school-grade fixed effects. The most demanding specification is probably that of column (6), where we additionally include fixed effects for the tract of residence in Wave 1. Note that, there are on average 25 census tracts within a school. By including census tract fixed effects, we are controlling for any difference in the residential area from which students are drawn. Indeed, neighborhood characteristics when young have been shown in the literature to be correlated with residential preferences in adulthood (Dawkins, 2005). The results reported in Table 5 show that the coefficients are relatively stable in these specifications, if not slightly stronger.

Table 5: Robustness analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Grade Black share, same gender	0.194***	0.195***	0.181***	0.196**	0.263***	0.291***
	(0.0565)	(0.0511)	(0.0515)	(0.0822)	(0.0980)	(0.104)
Grade Black share, opposite gender	0.0109	0.00861	0.00910	-0.0148		
	(0.0557)	(0.0518)	(0.0525)	(0.0631)		
Census tract Black share, wave 1		0.0892**	0.0884**	0.0954***	0.0846**	
		(0.0358)	(0.0356)	(0.0358)	(0.0357)	
Extended controls		Y	Y	Y	Y	Y
Extended cohort controls			Y	Y	Y	Y
School trends				Y		
School-grade FE					Y	Y
Tract FE						Y
Observations	7090	7090	7090	7090	7078	6564
Adjusted R^2	0.189	0.203	0.203	0.207	0.185	0.187

Notes: The table reports OLS estimates. The dependent variable is the Black share of the Wave V census tract population. Benchmark controls included in all columns are grade size, language spoken at home in Wave 1, 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 US and the individual's age at Wave 5. 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 US, and the share the same gender. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Since we have strong evidence that our variation in cohort same gender Black share is quasi-random and race is generally not measured with error, selection bias or measurement error is unlikely to be a problem here. This point is discussed further in Appendix C.

Some individuals surveyed in Wave 1 are not part of the final sample as they were not interviewed in Wave 5, and hence one may be concerned that this attrition impacts the results. In Appendix A, we show that this is unlikely to be the case. First, we show that, in our sample, the Black share of one's same gender cohort is not related to attrition. Furthermore, our results are robust to taking into account survey weights provided by Add Health for panel analysis on Waves 1 and 5, which control for attrition based on observables. We also show that our results are improved by the inclusion of individuals who didn't initially respond to the Wave 5 survey, which is comforting if we believe they may be more like non-responders than the rest of the sample.

Another concern is that, since our identification is driven by small quasi-random variation across cohorts, our results may be driven by some other aspect of the cohort which is correlated with the Black shares. We test for this in two ways. First, we construct over two hundred 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. In doing so, we obtain a distribution of the t-statistic of the different coefficients. Figure D5 in Appendix D clearly shows that the t-statistic of our coefficient of interest is an outlier on the right tail of this distribution. Second, we perform ten thousand placebo regressions in which we assign students to cohorts within their school at random. Plotting the distribution of coefficients, we note that the true coefficient is clearly an outlier as it is larger than almost all of the placebo coefficients (see Figure D6 in Appendix D). We can therefore conclude that it is very unlikely that our results are driven by chance or correlation with other characteristics of school cohorts.

In Appendix E, we investigate some subsample splits and interactions to further investigate the nature of our results. The estimates show that the coefficient of interest does not significantly differ by gender, and we find nos significant interactions when interacting our coefficient of interest with the school Black share, within-school friendship segregation, the Republican vote share in the school county, the share of students residing in urban areas, or the grade size reveals. This is likely to be the result of a lack of power rather than strong evidence for a homogeneous effect. One area we do find a significant difference in is region, with a significantly smaller effect for the subsample of schools in the North East region. One potential explanation for this is that this is the region where, in our sample, within-county Black-White segregation appears smallest, but this result should be interpreted with caution given that the survey is not representative at the regional level. Finally, in Table E15 we do not find any evidence that the result is driven by Blacks of a particular 'type'—e.g. those scoring grades above average. This speaks in favor of a general impact of exposure which is independent of any individual characteristics of Blacks. This result is noteworthy as studies on older individuals—i.e., in college—found the impact of minority exposure to vary with the ability of minority students (e.g. Carrell et al. (2019). Again, however, lack of power prevents us from concluding that such an effect couldn't be present here.

In the next section, we turn to exploring the mechanisms behind our findings exploiting the richness of the Add Health data.

4 Investigating mechanisms

The literature on residential segregation has emphasized one major factor that could explain our results: racial preferences (Boustan, 2011). In the context of our paper, there are two additional potentially relevant mechanisms: economic opportunities and residential choices of friends or partners. In the remainder of this section, we will review the various mechanisms to qualify our results and discuss potential drivers.

4.1 Economic Opportunities

Some studies have found that an increased share of Black students in school can worsen the educational achievement for their peers (Hoxby, 2000; Hanushek, Kain, and Rivkin, 2009; Billings, Deming, and Rockoff, 2014). This may translate in the long run into worse labor market outcomes. This would then limit one's ability to move to more amenable neighborhoods, which are more expensive and characterized by relatively fewer Black residents.

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 criminal activity (as recorded by being arrested or incarcerated). The results of these regressions are presented in Table 6. The coefficients on the Black shares are always insignificant. This is consistent with Bifulco et al. (2011) and Merlino et al. (2019), who do not find any impact of minority shares on these outcomes in Waves 3 and 4. Hence, there is no support for the hypothesis that contact with Blacks in school translates into sufficiently lower opportunities which induce changes in residential location due to financial constraints.

Table 6: Other outcomes related to education, employment, and criminality

	Average test score (1)	Attended college (2)	Employed (3)	Log earnings (4)	Ever arrested (5)	Ever incarcerated (6)
Grade Black share,	0.384	-0.0318	-0.00897	0.687	0.131	0.0355
same gender	(0.337)	(0.172)	(0.119)	(0.804)	(0.178)	(0.121)
Grade Black share,	-0.0690	-0.00445	0.209	-0.163	0.0346	-0.0605
opposite gender	(0.414)	(0.180)	(0.160)	(0.616)	(0.205)	(0.133)
Observations	7003	7090	7090	6762	6998	6992
Adjusted R^2	0.107	0.0870	0.0501	0.0317	0.0830	0.0620
Dep. var mean	2.890	0.643	0.843	10.20	0.278	0.114

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. The dependent variables are all measured in Wave 5. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Another way to test for this hypothesis is to look at the neighborhood characteristics in Wave 5. If treated individuals are more likely to live in Blacker areas because of financial constraints, we should expect their neighborhoods to be worse than others along an array of other dimensions such as population density, average income, poverty rates, unemployment, or the share of inhabitants with a college degree. Table 7 finds no evidence that exposure to Blacks in school has an impact on any of these characteristics of one's (tract-level) neighborhood. It therefore appears unlikely that our result is driven by changes in the economic opportunities available to Whites.

Table 7: Other tract characteristics

	Log pop. density (1)	Log of median income (2)	Poverty rate (3)	Unemployment rate (4)	Share college degree (5)	Log of median property value (6)
Grade Black share, same gender	0.119	-0.0241	0.0304	0.0144	0.0614	-0.0602
	(1.026)	(0.194)	(0.0486)	(0.0139)	(0.0732)	(0.325)
Grade Black share, opposite gender	-1.434	0.0730	-0.000717	0.00112	-0.0150	-0.295
	(0.909)	(0.162)	(0.0356)	(0.0178)	(0.0783)	(0.350)
Observations	7090	7088	7089	7090	7090	7090
Adjusted R^2	0.330	0.231	0.182	0.121	0.227	0.330
Dep. var mean	6.038	11.065	0.117	0.055	0.318	12.239

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. The dependent variables are all taken from the American Community Survey and linked to Wave 5 Add Health data. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

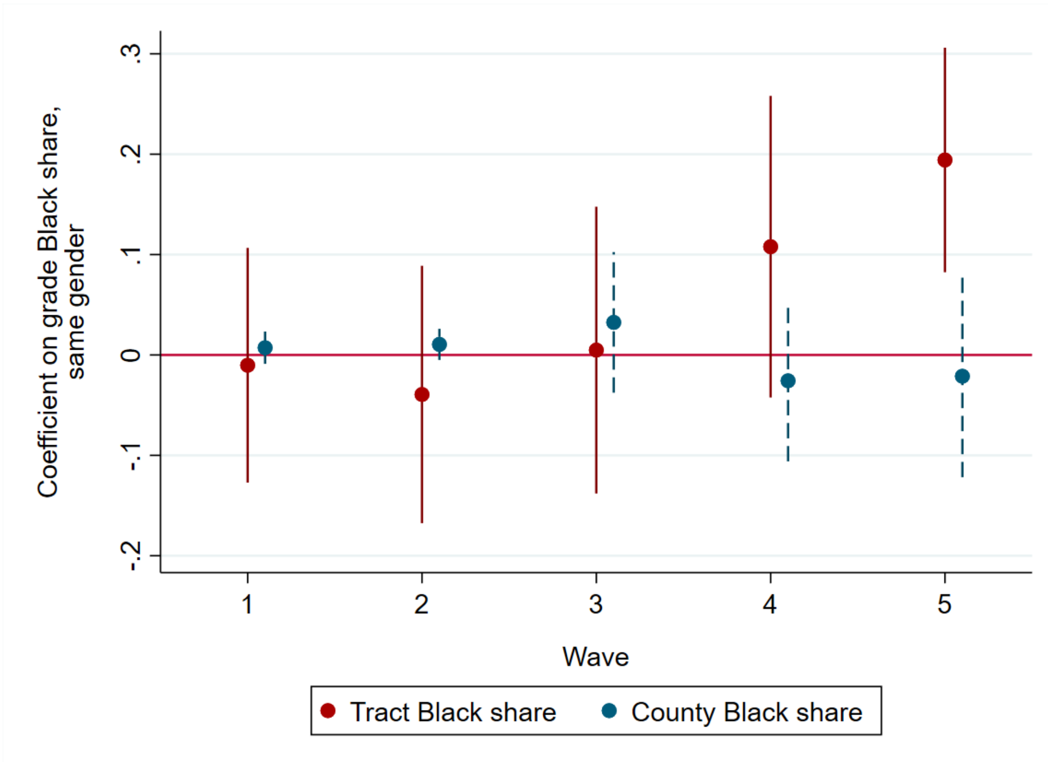
4.2 Social Networks and Partners

An alternative explanation is that the effect we find on residential segregation is driven by social networks, in particular through the residential choices of Black friends made in school. One way to test for this mechanism is to analyze how the effect varies over time and space. The idea is that social connections formed in school tend to weaken over time and space, because as time passes by and individuals move further away, they tend to see each other less. As a result, if the main mechanism behind our results were related to friendships and social ties formed in school, we should expect our results to be stronger when the respondent is closer in time and space to the exposure to Blacks in school.

To explore the time dimension, we plot in Figure 3 our main coefficient of interest on the same gender grade Black share across different waves of the survey. Moreover, we distinguish between census tracts and counties. The first interesting result to report is that the effect of school diversity on residential choices in census tracts emerges between Waves 3 and Wave 5, i.e., many years after exposure and not right after leaving high school. This pattern is not consistent with the idea that people chose their residential location to stay closer to their high-school friends, who happened to be more likely to be Blacks for individuals more exposed to Blacks in school.

The second notable pattern is that we do not find a statistically significant ef-

Figure 3: Impact on tract and county Black shares over time



Notes: The figure plots reports OLS coefficients and 95% confidence intervals of the same gender grade Black share from regressions where the dependent variable is the share of Blacks in the census tract (in red) and county (in blue). Regressions control for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects.

fect for counties at any point in time. This is not surprising as long-distance moves across counties and state are primarily driven by job related reasons (Molloy and Smith, 2019; Ning, Molloy, Smith, and Wozniak, 2022) for which local racial composition should not matter. For moves within counties, which are dominated by housing and family motives (Molloy and Smith, 2019; Ning et al., 2022), we instead find that racial composition plays a role in later waves.

Finally, it is noteworthy that exposure to Blacks during childhood does not affect residential choices immediately after school (Wave 3) when location changes often reflect educational choices or the transition into the labor market. Instead, we find exposure to matter most in Wave 5 when respondents are between 33 and 43 years old. This age group belongs to the so-called category of "family age" adults for whom location changes are often driven by family-related motives such as marriage, children, or schooling (DeWaard, Johnson, and Whitaker, 2019). Consistent with this interpretation, in Appendix F we find the correlation between tract Black share and stated liberalness to be strongest in Wave 5, suggesting that it is at this point of the life-cycle when attitudes are most likely to play an important role in residential choices.

To explore the geographical dimension, we analyze whether our effect is significantly different for those who have moved further away from their school location. Doing so, we find in column (1) of Table 8 that the effect of exposure does not vary with the distance moved between Wave 1 and Wave 5. Again, this appears inconsistent with the idea that our result is driven by a desire to be close to school friends.

An alternative possibility is that residential choices could be due to the preferences of Black partners of White respondents. This possibility could conceivably contribute to the main result since we have shown in previous work that social contact with Blacks in school translates into a higher probability of having an interracial relationship later on in life (Merlino et al., 2019). However, column (2) of Table 8 does not support this view, as we do not find evidence that the effect differs between respondents who have no Black partner and those with a Black partner in Wave 5. Moreover, given that having a Black partner is relatively rare, this mechanism would unlikely be able to explain a large share of our main result.

Table 8: Results relating to social networks and preferences

	Cenus tract Black share, wave 5 (1)	Cenus tract Black share, wave 5 (2)	Stated liberal-ness index (3)	Has Black partner, wave 5 (4)	N'hood satis-faction index, wave 2 (5)	Log of km moved, waves 3-5 (6)
Grade Black share, same gender (S)	0.298*	0.185***	0.742*	0.180**	0.0287	-0.587
	(0.151)	(0.0589)	(0.444)	(0.0902)	(0.437)	(1.519)
Grade Black share, opposite gender (O)	0.0578	0.0236	-0.171	-0.0314	0.0542	0.760
	(0.0947)	(0.0534)	(0.364)	(0.0758)	(0.468)	(0.974)
Log of km moved, waves 1-5 (D)	0.00471					
	(0.00351)					
S × D	-0.0258					
	(0.0355)					
O × D	-0.0103					
	(0.0208)					
Has Black partner, wave 5 (P)		-0.0651				
		(0.0630)				
S × P		0.125				
		(0.456)				
O × P		-0.0886				
		(0.904)				
Relative tract Black share (R)					-6.529***	8.203***
					(0.635)	(0.915)
S × R					11.66***	-20.86**
					(3.992)	(8.092)
O × R					7.007	1.512
					(5.446)	(7.336)
School FEs × D	Y					
School FEs × P		Y				
School FEs × R					Y	Y
Observations	7060	7090	7090	7090	5330	5843
Adjusted R^2	0.25	0.22	0.11	0.04	0.09	0.09
Dep. var mean	0.08	0.08	-0.00	0.02	0.00	3.52

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. The variables labelled D, P, and R are interacted with this set of controls - coefficients reported for these variables are therefore the marginal effects at the sample means. The relative tract Black share (R) is the share of census tract residents that are Black (measured in Wave 2 in column 5 and Wave 3 in column 6) minus the median of this variable among those in our sample who attended the same school. The stated liberalness index is constructed from three variables related to how liberal a person declares themselves to be - see Section 4.3 for details. The neighborhood satisfaction index is constructed using seven questions related to an individual's neighborhood - see Appendix G for more details and results using the individual components. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

4.3 Racial Preferences

Having ruled out these alternative channels, the remaining likely potential mechanism is a change in preferences. Unfortunately there are no direct measures of racial attitudes in the Add Health survey. However, in Table 8 we analyze some variables that could be attributed to changes in attitudes.

First, although racial attitudes are not measured directly, there are some measures which we might believe are correlated with racial attitudes. In Waves 4 and 5, for instance, respondents are asked whether they consider themselves politically liberal. Since these waves occur at times when race was a potentially salient political issue, it is possible that part of people's responses to these questions may be impacted by their attitudes towards Blacks.[10] In Wave 3, respondents were asked whether race is an important factor within a romantic relationship, which again could be correlated with more general attitudes towards Blacks. To increase power, we combine these three measures of stated liberalness into an standardized index using inverse covariance weighting and put it as the dependent variable in our baseline regression in column 3 of Table 8. In column 4 of this table, we also test whether more exposed individuals are more likely to have a Black partner, which again is likely to be correlated with attitudes towards Blacks. Consistent with Merlino et al. (2019), we find significant positive impacts on both of these outcomes.

As a further test of whether changes in preferences are a consistent mechanism for the impact we find, we can explore whether there is any impact of school exposure on outcomes related to White flight. This concept describes the phenomenon that Whites are more likely to move out of neighborhoods when there are more Blacks living there, and has been found to be an important determinant of residential segregation (Reber, 2005; Card et al., 2008; Lee, 2017). In columns 5 and 6, we therefore explore the correlation between a neighborhood's relative Black share and two outcomes related to White flight.[11] Consistent with the process of White

[10]Wave 4 was undertaken at a time when Obama was running for and then became the first Black US president, while Wave 5 took place in 2016-2018 when racial issues were at the center of Trump's presidential campaign (Henderson, 2016).

[11]The relative tract Black share is defined as the Black share of the census tract where an individual lives in the relevant wave, minus the median census tract Black share of others in our sample from the same school. We use this relative share as a proxy of how Black a neighborhood is com-

flight, we find that Whites in neighborhoods with a higher Black share are less satisfied with their neighborhood and typically move further between Waves 3 and 5 (when the impact of school contact on residential choices emerges). Most interestingly, the interactions of same gender Black share and the relative tract Black share in both columns show a strong negative sign. Hence, we find that these White flight patterns are weaker for people who had more Blacks of the same gender in their school grade. In other words, for Whites more exposed to Blacks at school, their neighborhood satisfaction and subsequent moving behavior is less negatively correlated with the neighborhood Black share.

Altogether, these findings support the interpretation that individuals who had more contact with Blacks in school are more likely to live in racially mixed neighborhoods due to a change in racial preferences.

5 Conclusions

In this paper we have analyzed how variation across White students' school peer groups affects residential location choices in adulthood. We exploit idiosyncratic variation in grade composition within schools, and we provide several tests supporting the assumption that the variation used is as good as random. We then show that a greater share of Blacks within White students' school cohorts in 1994-95 leads them to reside in neighborhoods with more Blacks in 2016-18. This result is driven by Black peers of the same gender as the respondent, who we show individuals are likely to have more interactions with than those of the opposite gender.

Our findings suggest that economic opportunities, partner preferences in interracial relationships, and social networks are unlikely to be major forces behind these results. Indeed, we find no effect of cohort racial composition on individual education and labor market outcomes, nor on neighborhood characteristics such

pared to other neighborhoods where the individual could most likely move to. Note that the number of observations in these columns is smaller than the full sample since in column 5 we restrict to those who responded in Wave 2 (the latest wave in which these questions were asked), and in column 6 we restrict to those on which we have the Wave 3 location information. The neighborhood satisfaction index is constructed using seven questions related to an individual's neighborhood - see Appendix G for more details and results using the individual components.

as average income, crime, or property value. Instead, the most likely mechanism behind our results is a change in racial preferences of respondents.

With respect to policy, our analysis suggests that being exposed to Black students in school can translate into a reduction of White flight behavior, which is an important driver of racial segregation in the US (Boustan, 2010; Shertzer and Walsh, 2019). Therefore, policies aiming to increase racial diversity in schools could help to reduce racial segregation and its negative welfare effects among future generations. An interesting question would be to understand whether such policies, which may result in larger changes in Black shares, lead to similar effects to those found in this paper.

Finally, an additional important question is whether the results extend to other contexts. In Europe, for instance, various migrant communities experience important levels of residential segregation, and it would be interesting to explore whether childhood contact can have similar effects in this alternative setting where cultural differences are arguably larger.

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Appendix A Attrition

Table A9 reports two tests for attrition in our sample. Columns (1) and (2) regress our treatment variables (the share of Blacks in one's grade) against a dummy that takes avalue of one if the respondent of Wave 1 is also present in Wave 5. The fact that all coefficients are insignificant supports the hypothesis that there is no relationship between the treatment variable and attrition in the sample.

In a similar spirit, columns (3) and (4) report the results of the baseline regression on the sample of respondents that could be contacted and responded to the questionnaire on the first attempt of contacting them. In other words, we exclude those who are categorised as being in the Non-Responder Follow-Up (NRFU) part of the Wave 5 survey. If attrition were a driver of our results, we should expect this selected sample to display a stronger effect of exposure to Blacks in high school on their residential choices. However, the coefficients are smaller, suggesting that,if those who never respond share characteristics with those who don't respond on the first contacting, then ifanything attrition may be biasing our results downwards.

Columns (5) and (6) run the same specification as columns (1) and (2) of Table 4, but controlling for the panel Wave 1-Wave 5 weights provided by Add Health. The results are broadly similar, suggesting that adjusting for attrition based on observables does not change the results in important ways.

Table A9: Baseline results with weights and attrition

Dependent variable:	In baseline sample		Wave 5 tract Black share			
Sample:	Wave 1		Wave 5, excl. NFRU		Wave 5, weighted	
	(1)	(2)	(3)	(4)	(5)	(6)
Grade Black share, both genders	-0.275		0.189**		0.0726	
	(0.258)		(0.0763)		(0.126)	
Grade Black share, same gender		-0.109		0.164***		0.180**
		(0.162)		(0.0530)		(0.0898)
Grade Black share, opposite gender		-0.222		0.0483		-0.100
		(0.166)		(0.0554)		(0.102)
Sample weights					Y	Y
Observations	11999	11999	6448	6448	7090	7090
Adjusted R^2	0.0392	0.0392	0.185	0.186	0.245	0.247
Dep. var mean	0.592	0.592	0.0811	0.0811	0.0855	0.0855

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. The sample in columns 1 and 2 are all individuals in Wave 1 that we can link to data on their grade composition. The dependent variable takes the value 1 if an individual is in our baseline sample—i.e. whether we have data on their location in Wave 5. Columns 5 and 6 are identical to the columns 1 and 2 of the baseline table except we exclude from the sample those who are Non-Responder Follow-Ups (NRFU). Columns 5 and 6 are identical to the columns 1 and 2 of the baseline table except that observations are weighted using the sampling weights provided by Add Health. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Appendix B Tests for non-random clustering

In this section, we check for non-random clustering of Black students within schools by means of several tests in the sample used to construct these shares. Hence, we use the sample of around 80,000 students who were surveyed in the in-school survey in Wave 1 and who are in cohorts containing at least one student present in our main analysis sample. This is the relevant sample since it is that used to construct our main explanatory variables (i.e. cohort Black shares) - running these tests on our main analysis sample would not be appropriate since there are no Blacks in this sample.

Intuitively, if the share of Black students varies systematically across cohorts, then an individual's race will be significantly correlated with that of their peers. However, a regression of a dummy variable of whether an individual is Black against the Black share of the rest of their peer group would give a negatively biased coefficient. This is because individuals are not included in their own peer group. In the following, we perform several tests designed to avoid this exclusion bias.

Caeyers and Fafchamps (2016) derive a test for non-random clustering that accounts for the exclusion bias by using as a dependent variable a 'transformed Black dummy' $\widehat{Black_i}$, where

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

where $Black_i$ 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 the number of students in the cohort.

Column 1 of Table B10 reports that the regression produces an insignificant coefficient. In Column 2, we perform the same test using the share of Black students split by gender. Again, the coefficients are small and insignificant. Hence, these results are consistent with the assumption of quasi-random allocation of Black students across grades.

Table B10: Tests for non-random clustering

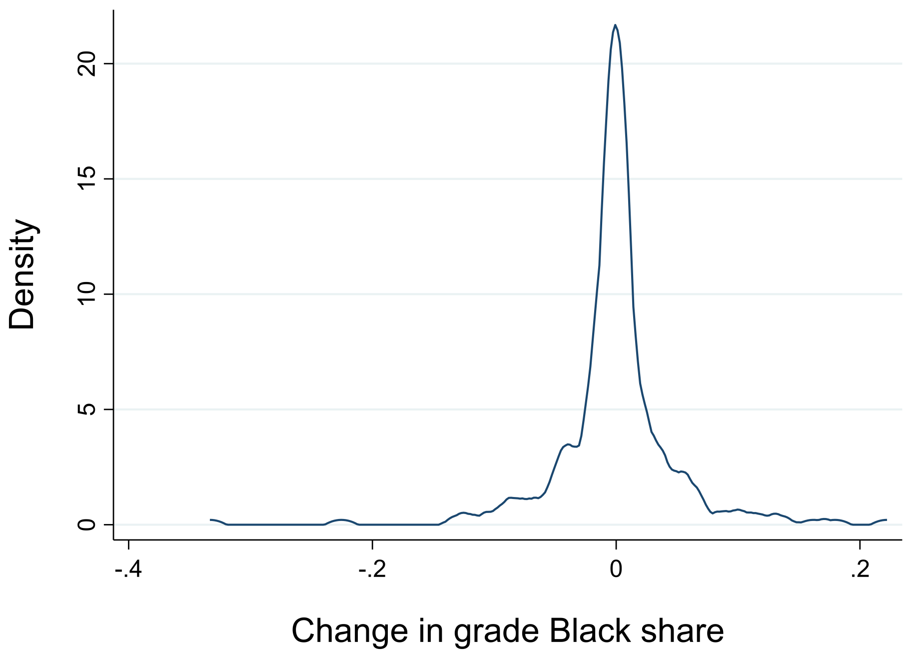
	Transformed black dummy	Transformed black dummy	Black dummy	Black dummy	Black share of males in grade
	(1)	(2)	(3)	(4)	(5)
Black share of others in grade	0.149		0.00920		
	(0.210)		(0.414)		
Black share of others of same gender in grade		0.00602		-0.138	
		(0.0989)		(0.217)	
Black share of opposite gender in grade		0.0208		-0.0337	
		(0.0928)		(0.233)	
Black share of others in school			-98.69***	-101.8***	
			(23.19)	(22.79)	
Black share of females in grade					0.0616
					(0.0792)
Observations	81780	81778	81780	81778	80837
Adjusted R^2	0.999	0.394	0.395	0.398	0.979

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. Regressions reported in this table are run on the Wave 1 in-school survey. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Guryan et al. (2009) propose another test of non-random clustering that removes the exclusion bias by controlling for the set of all potential peers. Basically, this means that we have to control for the Black share among all other students in the school in the regression against the Black dummy. Columns 3 and 4 of Table B10 show that the coefficients of interest on the cohort Black shares are again insignificant.

A simple (less formal) test is to regress the male Black share on the female Black share. The coefficient reported in column 5 of Table B10 is insignificant. As most factors which might influence the female Black share would also simultaneously influence the male Black share, we conclude that self-selection or omitted variables when it comes to race shares is unlikely.

Figure B4: Kernel density of change in grade Black share



Finally, we check whether differences in Black share across grade are symmetric. The idea is that if Black shares were on average significantly higher (or lower) for later grades, the variation might stem from systematic trends due to factors such asdisproportionate dropout rates for Blacks. Hence, we plot in Figure B4 the distribution of the change in Black share between each grade and the previous grade in each school. The figure display no obvious asymmetry, and indeed the mean change in grade Black share is -0.0005792.

These tests of random variation therefore accord with the fundamental assumption behind our identification strategy—i.e. that parents don't select into schools on the basis of the grade-specific Black share once we control for the overall school characteristics. A key rational for this assumption is that, in general, parents are unlikely to know before choosing a school how the composition of a particular grade differs from the school average. This rational, however, doesn't prevent selection occurring after children start in a school, and hence one concern may be that White students may differentially change school as a function of the grade Black share. We would be surprised if this behavior was sufficiently widespread to drive our results, but we can look for evidence of it by exploiting Wave 2 of the Add Health survey, which interviewed students who were below grade 12 in Wave 1 (and who hence would normally have continued in the same school or 'sister school').

Results of this analysis are presented in Table B11. In the first two columns we see that Whites are not significantly more likely to move school when they have a higher share of Blacks in their grade. This suggests that such school moving behavior is unlikely to be widespread. In columns 3 and 4, we test whether there is any evidence that school switching could be related to our outcome of interest by running a regression similar to our baseline. In particular, we now interact grade Black shares with a dummy for whether the individual switched school. Note that the sample size is substantially smaller since we restrict to those who were below grade 12 in Wave 1 and were surveyed in Wave 2. Since most people didn't switch school, and indeed those that didn't switch are more exposed, it is reassuring that our result is driven by those who didn't switch. If selection was driving our results, we would expect to see that students who move out of grades with high Black shares end up living in less Black neighborhoods, since these are the people who might have selected out of their school based on the Black share. We don't see this to be the case—the coefficients on the relevant interactions are positive—suggesting that even if there is some school switching based on grade Black shares, it is unlikely to be large enough to drive our results.

Table B11: School switching

	Switched school in Wave 2		Black share in census tract, Wave 5	
	(1)	(2)	(3)	(4)
Grade Black share, both genders	-0.120			
	(0.311)			
Grade Black share, same gender		0.0348		
		(0.136)		
Grade Black share, opposite gender		-0.150		
		(0.174)		
Switched school in Wave 2			-0.00144	-0.00159
			(0.00782)	(0.00783)
Grade Black share, both genders × Didn't switch school			0.168**	
			(0.0841)	
Grade Black share, both genders × Switched school			0.0896	
			(0.0932)	
Grade Black share, same gender × Didn't switch school				0.164***
				(0.0591)
Grade Black share, same gender × Switched school				0.0222
				(0.192)
Grade Black share, opposite gender × Didn't switch school				0.0115
				(0.0721)
Grade Black share, opposite gender × Switched school				0.0707
				(0.195)
Observations	8216	8216	5157	5157
Adjusted R^2	0.561	0.561	0.205	0.205
Dep. var mean	0.0899	0.0899	0.0814	0.0814

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. Standard errors (in brackets) are clustered at the school level. The sample in both columns is restricted to Whites who were below grade 12 in Wave 1 and were interviewed in Wave 2. * $p < .10$, ** $p < .05$, *** $p < .01$

Appendix C Measurement Error

A general concern in studies looking at peer impacts is that measurement error in the independent variable of interest may bias the results upwards (Angrist, 2014). We don't believe this is likely to be a serious issue in our setting given that race is typically measured with much less error than variables such as academic ability. Nonetheless, since race is not an objectively defined variable, there is some potential for what could be thought of as mismeasurement, we investigate this concern in this section.

One 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 surveyed individuals identify themselves as Black, and the share of the population that are Black in the census block where they live in Wave 1. The results are shown in column 2 of Table C12, 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. Another suggestion that has been made to overcome measurement error concerns is to split the sample between the individuals who may be producing the peer effects from those who are being influenced by them Angrist (2014). We do this in column 3 by including the number of Blacks, instead of the share. Even though the variable is likely to be less relevant, we still find a significant effect on our outcome of interest. Overall, these results therefore further suggest that measurement error is unlikely to be driving our results.

Table C12: Measurement error

	(1)	(2)	(3)
Grade Black share, same gender	0.196***	0.187***	
	(0.0566)	(0.0533)	
Grade Black share, opposite gender	0.0111	0.0106	
	(0.0557)	(0.0511)	
Identifies as black, wave 5		0.0983***	0.101***
		(0.0254)	(0.0255)
Block black share, wave 1		0.138***	0.138***
		(0.0238)	(0.0237)
Blacks in grade, same gender			0.000785*
			(0.000453)
Blacks in grade, opposite gender			-0.000755
			(0.000564)
Observations	7090	7090	7090
Adjusted R^2	0.188	0.201	0.200
Dep. var mean	0.0819	0.0819	0.0819

The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Appendix D Placebo Tests

To address concerns that our results may be driven by other cohort characteristics, we perform two different sets of placebo tests. First, we regress the econometric model (1) using as independent variables several same gender cohort shares based on all the appropriate questions included in the in-school survey of Wave 1, i.e., the survey we used to construct the share of Black students in each cohort of the same gender. We constructed over two hundred such variables including, 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. We then record the t-statistics from each regression, and report their distribution in Figure D5. The t-statistics we obtain in our benchmark, indicated by a red line, clearly lies at the very right tail of the distribution. We conclude that it is very unlikely that our result is driven by chance or correlation with another characteristic of school cohorts.

The other placebo test reassigns students to cohorts randomly so that our measure of same gender cohort Black share is that of another random cohort within the same school. We then perform regressions as (1) for each assignment of cohort shares and repeat this exercise ten thousand times. This produces a distribution of coefficients, which is reported in Figure D6 together with the coefficient from our benchmark. The distributions are centered around zero, and the coefficient from our benchmark lies at the very right tail of the distribution. In fact, this is larger than more than 99 percent of the placebo coefficients. This further confirms that our result is not spurious.

Figure D5: Other shares on RHS.

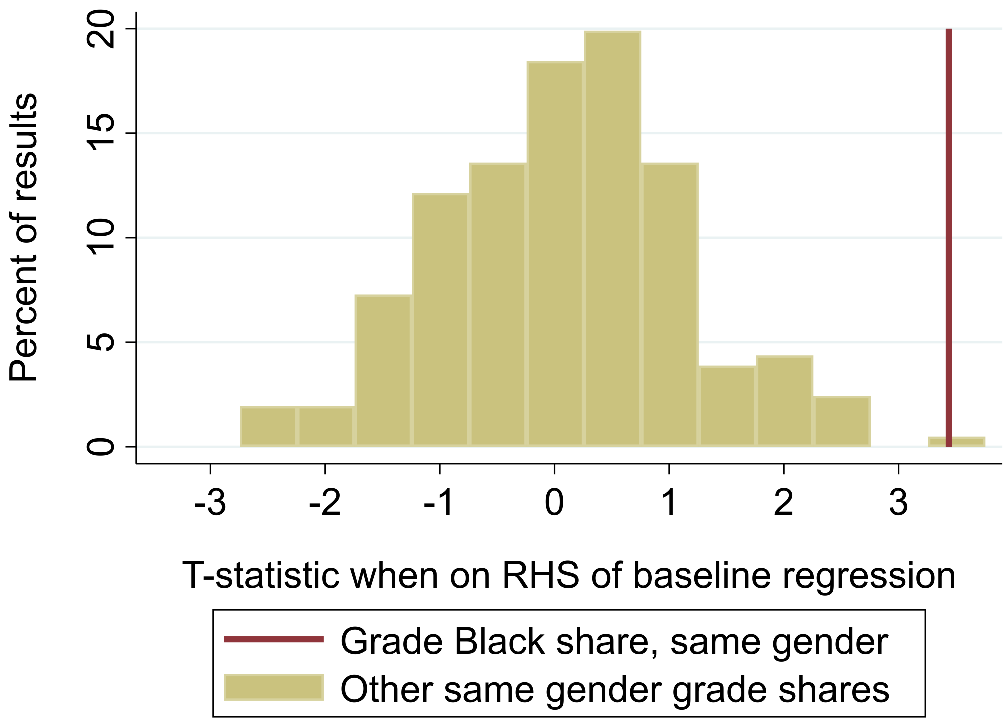
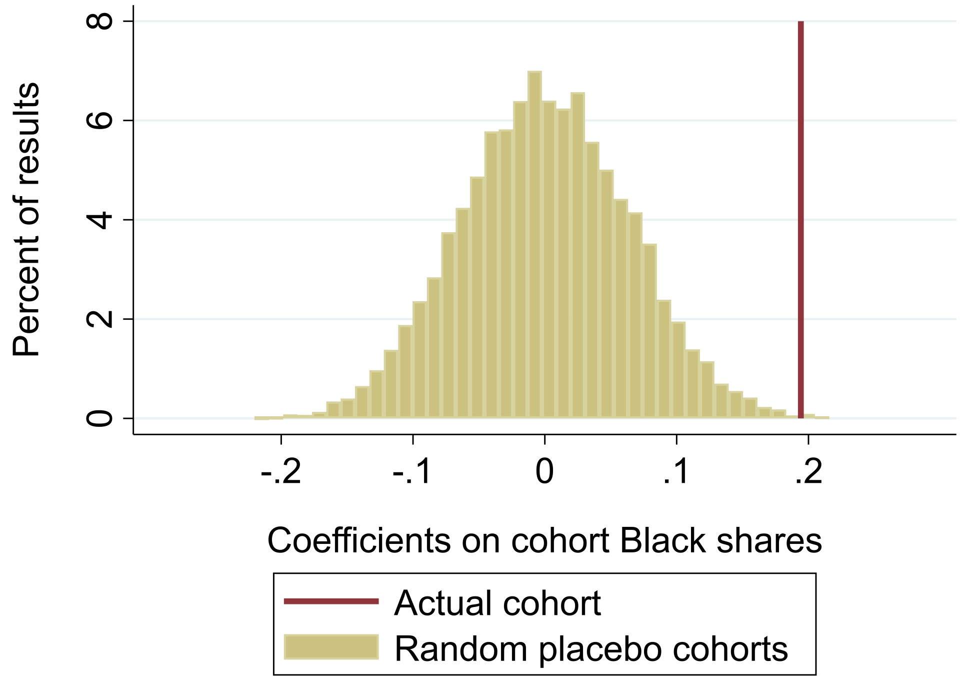


Figure D6: Distribution of coefficients from regressions on randomly assigned cohort shares.



Appendix E Heterogeneity

In this section, we present an investigation of the presence of heterogeneous effects in our sample with respect to our main results presented in column (2) of Table 4.

We first run the same regression for different subsamples. The results and the p-values of the tests comparing the coefficients on the different samples are reported in Table E13. Columns (1) and (2) divide the sample by gender, while columns (3) to (6) divide it by region. While we find no significant differences by gender, we do find that the North-East region has a significantly smaller coefficient than the other regions. One potential explanation for this is that, within our sample, school counties in this region appear less segregated than other regions.[12] To expand on this idea, in columns (7) and (8) we split the sample according to whether the school county has a dissimilarity level above or below .5. Consistent with our intuition, our

[12] We do not have a direct measure of county segregation, but instead estimate it using the tract Black shares in which Add Health respondents (Black or White) live in Wave 5. In particular, we calculate the dissimilarity index amongst the tracts that we observe, using Blacks and non-Blacks as our two groups.

result appears significantly larger in the set of schools in more segregated counties. Note, however, that given we are measuring county-level segregation with error and our sample is not representative at the regional level, this difference between regions should be interpreted cautiously.

Table E13: Subsample splits

	Gender		Region				County segregation	
	Female	Male	North-east	Mid-west	South	West	Low	High
Dependent variable: Any partners Black	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grade Black share, same gender	0.226*** (0.0668)	0.168 (0.114)	-0.0280 (0.0829)	0.220 (0.221)	0.250*** (0.0664)	0.366** (0.172)	0.101 (0.0761)	0.332*** (0.0786)
Grade Black share, opposite gender	0.117* (0.0687)	-0.145 (0.0940)	-0.0319 (0.119)	-0.144 (0.154)	0.102 (0.0667)	0.0894 (0.181)	-0.0196 (0.0477)	0.0541 (0.118)
P-val, coefs equal	.66		.04				.03	
Observations	3942	3148	1298	2179	2413	1192	4938	2149
Adjusted R^2	0.199	0.178	0.0550	0.105	0.186	0.0851	0.244	0.0661
Dep. var mean	0.0820	0.0817	0.0545	0.0602	0.122	0.0706	0.0856	0.0736

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. The p-values reported in the row after the regression coefficients are results of testing whether the 'grade Black share, same gender' coefficients are statistically different across the relevant samples. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table E14 then reports the result of interacting the two treatment variables with the school Black share, the level of segregation of the school calculated using the methodology proposed by Echenique and Fryer (2007), the share of Republican votes in 1992 in the Wave 1 neighborhood, the urban share and the total number of students in one's grade. None of the interaction coefficients are significant.

We also may wonder to what extent the effect depends on the characteristics of the Black children which the White children are exposed to. Carrell et al. (2019) find that exposure to high-performing Black students increases White students' propensity to later have a Black roommate, but exposure to low-performing Black students has no such effect. We test for such an effect by splitting our grade Black shares in various ways in Table E15. In columns 1 and 2, we categorize Blacks by how their self-reported grades compare to the class median. While such a specification is close to Carrell et al. (2019), we may however be concerned that self-reported grades are a noisy measure of performance, and indeed many students do not report any grades. In columns 3-6 we therefore split Blacks according to two measures correlated with performance—whether they live with their father, and whether their mother went to college. In all of these regressions, we don't find significant differences between the coefficients on either of the relevant Black shares, though this may of course reflect a lack of power to detect differences.

Table E14: Interactions

Interaction term:	School Black share	School Black segregation	Republican vote share in 1992	School urban share	Students in grade
Dependent variable: Tract Black share					
	(1)	(2)	(3)	(4)	(5)
Grade Black share, same gender	0.271*** (0.102)	0.169 (0.115)	0.185*** (0.0643)	0.211*** (0.0683)	0.237*** (0.0906)
Grade Black share, opposite gender	0.113 (0.0983)	-0.0385 (0.0941)	0.0152 (0.0585)	0.00565 (0.0482)	0.102 (0.0705)
Same gender x interaction term	-0.403 (0.408)	0.0384 (0.191)	0.202 (0.823)	-0.109 (0.157)	0.000875 (0.000572)
Opp. gender x interaction term	-0.552 (0.377)	0.0596 (0.160)	-0.215 (0.642)	0.0763 (0.113)	-0.00000158 (0.000529)
Observations	7090	7022	7050	7082	7090
Adjusted R^2	0.160	0.160	0.166	0.159	0.198

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. Standard errors (in brackets) are clustered at the school level. In column 5 the interaction term varies within schools, so we interact it also with school-gender fixed effects. * $p < .10$, ** $p < .05$, *** $p < .01$

Table E15: Heterogeneity by characteristics of Black peers

Characteristic X	Above-average grades/marks		Mother went to college		Lives with father	
	(1)	(2)	(3)	(4)	(5)	(6)
Grade Black share, Blacks with X=1	0.265*** (0.0999)		0.225*** (0.0660)		0.142 (0.0997)	
Grade Black share, Blacks with X=0	0.160 (0.0996)		0.110 (0.164)		0.239** (0.118)	
Grade Black share, Blacks with X=1, same gender		0.246*** (0.0753)		0.207*** (0.0593)		0.171** (0.0711)
Grade Black share, Blacks with X=0, same gender		0.148 (0.0981)		0.159 (0.148)		0.246** (0.104)
Grade Black share, Blacks with X=1, opp gender		0.0481 (0.0693)		0.0262 (0.0675)		0.00747 (0.0628)
Grade Black share, Blacks with X=0, opp gender		0.0272 (0.123)		-0.00417 (0.121)		-0.00392 (0.0794)
P-val, coefs equal	.44		.51		.54	
P-val, coefs equal (same)		.44		.77		.51
P-val, coefs equal (opp)		.89		.84		.9
Observations	6971	6971	7090	7090	7090	7090
Adjusted R^2	0.192	0.193	0.188	0.188	0.188	0.188
Dep. var mean	0.0818	0.0818	0.0819	0.0819	0.0819	0.0819

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Appendix F Correlation between tract Black share and stated liberalness

In Table F16, we correlate our constructed index of stated liberalness (see Section 4.3 for more details on how this is constructed) with White respondents' tract Black share in each wave, controlling for school-gender fixed effects, grade-gender fixed effects, and the control variables in our baseline regression. Here we can note that there is no significant correlation in the first three waves, but that there is a significant positive correlation in Wave 4 and even more so in Wave 5. We should clearly not take these correlations as causal, but the results are consistent with the idea that attitudes play a larger role in the decision over which neighborhood to live in during later waves. Note that results are very similar if we control for school cohort Black shares or, for Waves 3-5, if we use measures of stated liberalness collected in the relevant wave (results available upon request).

Table F16: Correlation between Black share and stated liberalness over time

	Black share in tract, Wave 1 (1)	Black share in tract, Wave 2 (2)	Black share in tract, Wave 3 (3)	Black share in tract, Wave 4 (4)	Black share in tract, Wave 5 (5)
Index of stated liberalness	0.000832 (0.000993)	-0.0000453 (0.00107)	0.00145 (0.00175)	0.00306* (0.00180)	0.00579*** (0.00146)
Observations	7034	5331	5843	6369	7090
Adjusted R^2	0.524	0.536	0.277	0.208	0.189
Dep. var mean	0.0546	0.0531	0.0744	0.0831	0.0819

Notes: The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. The stated liberalness index is constructed from three variables related to how liberal a person declares themselves to be - see Section 4.3 for details. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Appendix G Construction of neighborhood satisfaction index

In Table 8 in Section 4, we explore the relationship between tract Black share and an index of self-reported neighborhood satisfaction. The index is constructed using responses to a set of seven questions asked in Wave 2. We use all seven questions to avoid a somewhat arbitrary selection. The questions are as follows:

- Do you know most of the people in your neighborhood?
- In the past month, have you stopped on the street to talk with someone who lives in your neighborhood?
- Do people in this neighborhood look out for each other?
- Do you use a physical fitness or recreation center in your neighborhood?
- Do you usually feel safe in your neighborhood?
- On the whole, how happy are you living in your neighborhood?
- If, for any reason, you had to move from here to some other neighborhood, how happy or unhappy would you be?

We standardize answers to each question and code them such that a higher value represents greater satisfaction. We then construct a standardized inverse-covariance weighted index of neighborhood satisfaction using these seven answers (Anderson, 2008). In Table G17 we repeat the regression undertaken in column 6 of Table 8 replacing this index with each of the components. From this, we can note that most of the components are negatively correlated with the relative tract Black share, but this correlation is reduced when individuals are more exposed to Blacks in their cohort.

Table G17: Regressions with neighborhood satisfaction components

	Know people in n'hood (1)	Talked to people on street (2)	People look out for each other (3)	Use rec center in n'hood (4)	Feel safe in n'hood (5)	Happy in n'hood (6)	Would be unhappy if had to move (7)
Grade Black share, same gender (S)	-0.197	0.146	0.0211	-0.217	0.218	0.912*	-0.0556
	(0.647)	(0.482)	(0.382)	(0.499)	(0.525)	(0.494)	(0.725)
Grade Black share, opposite gender (O)	-0.280	-0.432	-0.548	0.427	-0.355	0.672	0.797
	(0.454)	(0.464)	(0.385)	(0.411)	(0.485)	(0.415)	(0.543)
Relative tract Black share, Wave 2 (R)	-8.081***	-5.402***	-0.339	-3.303***	-4.593***	2.536***	-0.518
	(0.639)	(0.637)	(0.746)	(0.695)	(0.621)	(0.661)	(0.724)
S × R	13.95***	4.189	9.358**	-1.127	8.345*	6.608	9.189
	(4.017)	(3.096)	(3.788)	(4.728)	(4.297)	(3.988)	(5.828)
O × R	-3.011	5.648	-2.677	6.863	8.484	0.520	0.695
	(5.058)	(4.327)	(6.925)	(4.655)	(5.963)	(4.555)	(3.768)
Observations	5327	5327	5264	5326	5324	5329	5319
Adjusted R^2	0.116	0.0537	0.0400	0.0614	0.0915	0.0438	0.0329

Notes: The relative tract Black share (R) is the share of census tract residents that are Black (measured in Wave 2 in column 5 and Wave 3 in column 6) minus the median of this variable among those in our sample who attended the same school. The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. The variable R is interacted with this set of controls - coefficients reported for these variables are therefore the marginal effects at the sample means. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$