

# The long run impact of childhood interracial contact on residential segregation

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## Abstract

This paper investigates whether interracial contact in childhood impacts residential choices in adulthood. 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 in a grade induces whites to live in blacker census tracts more than 20 years after exposure. We do not find any effect on labor market outcomes or other neighborhood characteristics, suggesting the most likely mechanism is a change in preferences of respondents.

*JEL classifications:* 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, 55% of African Americans in metropolitan regions live in neighborhoods where they are over-represented (Logan and Stults, 2021).<sup>1</sup> The negative social and economic consequences of this residential segregation have been documented by several works, and range from adverse effects on education and earnings to diametrical effects on health behavior and outcomes (Boustan, 2011; Ananat, 2011; Niemesh and Shester, 2020). The latter has been tragically highlighted by the COVID-19 pandemic, during which segregated counties in the US have been experiencing above-average death and infections rates (Torrats-Espinosa, 2021).

The literature differentiates between three different causes of black-white residential segregation: collective white actions to exclude blacks from white neighborhoods, such as discrimination in housing markets, preference-based self-selection of blacks into black neighborhoods, and outmigration of whites from neighborhoods with higher shares of blacks (Cutler, Glaeser, and Vigdor, 1999; Boustan, 2011). The empirical evidence suggests that the latter has been the most decisive factor for the emergence and 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 causal 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 blacks at a young age can impact residential racial segregation. In particular, we analyze how plausibly exogenous variation in a white student's school peer group affects residential location choices later in life. The data used comes from the National Longitudinal Survey of Adolescent Health (Add Health), which provides information on the race of all students in covered schools and on selected characteristics of their residence during adulthood. This allows us to exploit idiosyncratic variation in grade composition within schools, a methodology first proposed by Hoxby (2000) and that has since then widely been used to identify causal peer effects (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). We provide several tests giv-

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<sup>1</sup>This is measured by the so-called index of dissimilarity. It is the most common used measure of segregation and captures at the city level how much two groups are evenly spread among census tracts.

ing 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 individuals who were in grades with more black students of same gender in 1995 are more likely to live in neighborhoods with more blacks in 2016-18. The magnitude of the effect implies that going from the average of 8 percent blacks of the same gender in the cohort to 10 percent 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. The results are robust to several modifications of the model, including the introduction of grade-school and tract fixed effects.

Our results could be driven by two distinct channels: economic opportunities and 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. We further document that our results are unlikely to be driven by the preferences of partners in interracial relationships. Instead, it appears the results are driven by moving decisions made between ages 25 and 43. We document that, during this period, whites in blacker neighbourhoods move more during this period - consistent with 'white flight' dynamics observed in the literature (Schelling, 1971; Boustan, 2010). Yet this relationship is reduced for those who were exposed to a greater share of blacks in schools. This is consistent with interracial contact changing whites' attitudes towards mixing with blacks (Williams, 1947; Allport, 1954).

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).<sup>2</sup> The survey selected 80 nationally representative high schools and 54

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<sup>2</sup>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

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 use all students in the in-school survey to collect comprehensive information about school peers. Indeed, 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. Using this data, we construct our main independent variables, i.e., the shares of students in peer groups who are black.<sup>3</sup> We consider three alternative groups of black peers: all those in the same grade, those of 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. 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, in this case, blacks. The relatively small number of students of other racial groups limits our 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.

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<sup>3</sup>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 to be black only.

Table 1: Summary statistics

	Mean	Within school s.d.	Between school s.d.	N
<i>Main variables</i>				
Share of census tract black, Wave V	.0818566	.1067597	.0792128	7090
Share of census tract black, Wave I	.054636	.0634313	.117575	7034
Grade black share, both genders	.0797002	.0155908	.192231	7090
Grade black share, same gender	.0792521	.0252516	.189996	7090
<i>Other Wave 1 variables</i>				
Age	16.00944	1.068762	1.372499	7090
Female	.5557752	.4689177	.1358581	7090
Hispanic	.1251059	.1937875	.2293006	7090
Family income	52.32824	33.6119	24.79959	5705
Grade size	223.7108	23.55905	132.1165	7090
Grades in school	4.094041	0	1.211375	7090
In middle school	.2190059	0	.4911923	7090
In high school	.5866987	0	.4968472	7090
Lives in urban area	.4562153	.1659608	.4297206	7031
Region = Northeast	.1832816	0	.4119639	7090
Region = Midwest	.3076814	0	.427618	7090
Region = South	.3407229	0	.4875595	7090
Region = West	.168314	0	.3592762	7090

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 (columns 1 and 2 of Appendix Table B11). Additionally, our results are robust to the introduction of sample weights.

Summary statistics of the main variables we use in our analysis are reported in Table 1.

## 2.2 Estimation strategy

We cannot simply regress residential segregation on cohort composition since cohort composition is likely to be correlated with a range of other omitted variables that impact residential choice—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 live in blacker neighborhoods choose to enroll 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 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:

$$Y_i = \alpha \text{ShareBlack}_{cs} + I_{gm} + I_{sm} + \varepsilon_i, \quad (1)$$

where  $\text{ShareBlack}_{cs}$  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  $\varepsilon_i$  is a random error term. 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 5 interviews. Standard errors are clustered at the school level.<sup>4</sup>

Our main dependent variable  $Y_i$  is the share of blacks living in the same census tract as the respondent in Wave 5. This embeds the idea that contact with blacks might affect residential preferences. Since we focus only on the impact on whites, compositional changes have an impact on residential choices, and hence we are not concerned by the critique that linear models as (1) 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 in Wave 1. We then split grades in two groups, considering separately those students of the opposite gender and those of the same gender. The idea is that same gender peers may be more important if this is the group from which close friends are most likely to be drawn. This is in line with the findings by Merlino et al. (2019).

### 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 parents might choose schools because of their average racial composition, this decision is not affected by differences on the share of black students by gender in the grade their child attends. We test three important implications of this identification assumption.

First, we test whether within-school variation in the share of black students is correlated with predetermined individual level variables—a 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

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<sup>4</sup>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.

the whole grade, and then simultaneously on the black share of students of 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 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 test. We believe the correlation with these variables to be spurious; however, we control for them in all of our regressions.

Second, we can 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 fixed effects. However, one cannot simply regress an individual's race on that of their peers, because each individual is present in many others' peer groups but necessarily not their own (Guryan, Kroft, and Notowidigdo, 2009). We then perform several tests designed to account for this problem, including those proposed by Guryan et al. (2009), Stevenson (2017) and Caeyers and Fafchamps (2016). More details can be found in Appendix C. Overall, none of the tests rejects random clustering. 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 C 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 cohorts 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 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.

Table 2: Balancing tests for cohort composition measures

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

Notes: 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. These OLS regressions include grade-gender fixed effects and school-gender fixed effects. Standard errors (in brackets) are clustered at the school level. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

### 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 this 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 about friendship that is provided in the Add Health.

Table 3 reports two results indicating that a higher black share in a grade increases social contact with blacks. In column (1), we show that more blacks in a grade within a school translates into a higher share of nominated friends who are black in school, i.e., both measured at Wave 1 in 1995. Column (2) then shows that the effect is driven by black peers of the same gender as the respondent. These results are in line with those reported in Merlino et al. (2019), who also show that the share of blacks within cohorts has an impact on several other measures of exposure on a different sample of the Add Health. In particular, the importance of same sex peers is consistent with the literature (Soetevent and Kooreman, 2007; Merlino et al., 2019). The intuition is that young people forming closer friendships with individuals of their own gender (McPherson, Smith-Lovin, and Cook, 2001; Kalmijn, 2002), and hence these are the social contacts that are more relevant in shaping one's attitudes. Note also that our results are compatible with the existence of inbreeding homophily in friendship found by Currarini et al. (2009) and Fletcher, Ross, and Zhang (2013). 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.

Columns (3) and (4) of table 3 report the main result of the paper: more exposure to blacks in school has an impact on long-term residential choices. In particular, individuals who were in grades with more black students in 1995 are more likely to live in neighborhoods with more blacks in 2016-18. In line with the results on friendship in Wave 1, this effect is driven by black peers of the same gender.

In terms of magnitude, the point estimate 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.

Table 4 provides evidence of the robustness of the main results in our preferred specification of column (4) in Table 3, which also reported also in column (1) of Table 4. In particular, column (2) shows the results are robust to the introduction of several individual controls measured in Wave, including the black share of the

Table 3: Results on friendship in Wave 1 and residential segregation in Wave 5

	Share of friends black, Wave 1		Black share in census tract, Wave 5	
	(1)	(2)	(3)	(4)
Grade black share, both genders	0.128*** (0.0494)		0.189** (0.0746)	
Grade black share, same gender		0.169*** (0.0346)		0.194*** (0.0565)
Grade black share, opposite gender		-0.0234 (0.0333)		0.0109 (0.0557)
Observations	6131	6131	7090	7090
Adjusted $R^2$	0.0571	0.0599	0.188	0.189

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

census block group, family income and mother's education. Column (3) further 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 also 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 4 show that the coefficients are relatively stable in these specifications, if not slightly stronger. This suggests that, while opportunity of social interactions in the neighborhood may be important in determining residential preferences (Mouw and Entwisle, 2006; Lee, 2017), interactions in school act through a separate additional channel.

Since some individuals surveyed in Wave 1 are not part of the final sample as they were not interviewed in Wave 5, one may be concerned that this attrition

Table 4: Robustness analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Grade black share, same gender	0.194*** (0.0565)	0.195*** (0.0521)	0.181*** (0.0523)	0.196** (0.0839)	0.257** (0.0986)	0.291*** (0.104)
Grade black share, opposite gender	0.0109 (0.0557)	0.00947 (0.0516)	0.00972 (0.0522)	-0.0127 (0.0629)		
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	0.201	0.202	0.206	0.184	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. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

impacts the results. In Appendix B, 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 observable variables.

In Appendix A, 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, region and the level of segregation at the county level. Furthermore, interacting the coefficient of interest with the school black share, segregation at the school level, the Republican vote share in the school county, the share of students residing in urban areas, and the grade size reveals no sizeable heterogeneity along these dimensions. This is likely to be the result of a lack of power rather than strong evidence for a homogeneous effect.

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 suggests that two factors determine residential choices (Boustan, 2011): opportunity and preferences.

The first potential channel we investigate is economic opportunities. Some studies suggest that an increased share of black students in school may worsen educational achievement for their peers (Hoxby, 2000; Hanushek, Kain, and Rivkin, 2009; Billings, Deming, and Rockoff, 2014). This may translate into worse educational outcomes and, in the long run, into worse labor market performances. This would then limit one's ability to move to more amenable neighborhoods, which are more expensive and characterized by a less black population.

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 criminal activity, as recorded by being arrested or incarcerated. The results of these regressions are presented in Table 5. The coefficient on the black shares is 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. Hence, there is no support for the hypothesis that contact with blacks in school would translate into lower opportunities to move later on because of financial constraints.

Table 5: 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, same gender	0.384 (0.337)	-0.0318 (0.172)	-0.00897 (0.119)	0.687 (0.804)	0.131 (0.178)	0.0355 (0.121)
Grade black share, opposite gender	-0.0690 (0.414)	-0.00445 (0.180)	0.209 (0.160)	-0.163 (0.616)	0.0346 (0.205)	-0.0605 (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, the share of inhabitants with a college degree. Table 6 finds no evidence that exposure to blacks in school has an impact on any of these characteristics of one's (tract-level) neighborhood.

Table 6: 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 (1.026)	-0.0241 (0.194)	0.0304 (0.0486)	0.0144 (0.0139)	0.0614 (0.0732)	-0.0602 (0.325)
Grade black share, opposite gender	-1.434 (0.909)	0.0730 (0.162)	-0.000717 (0.0356)	0.00112 (0.0178)	-0.0150 (0.0783)	-0.295 (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. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

Another potential explanation is that the effect we find on residential segregation is not the result of the respondent's preferences, but of those of their partner. This possibility makes sense as 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). The results presented in Table 3 could then just be due to these interracial relationships induced by social contact, as a black partner may have stronger preferences to live in a blacker area than a non-black partner.

We explore the link between our result and cohabiting with a black partner in Table 7. Consistent with (Merlino et al., 2019), we indeed see that individuals who are exposed to more blacks of the same gender in school are more likely to cohabit with a black partner in Wave 5. We then test whether this is likely to be driving our results on residential segregation in two ways. First, we include as a control the variable indicating whether the respondent's current partner is black (column 2). Second, we run the baseline specification on the sample of respondents who do not have a black partner (column 3). The results show that, while having a black partner translates into a stronger effect of social contact against residential segregation, our main results are actually present, and of the same magnitude, for people who do not have a black partner.

Finally, the literature suggests that the persistence of residential segregation, and its exacerbation after the end of court-ordered desegregation, is mostly due to white flight (Reber, 2005; Card et al., 2008; Lee, 2017). In order to understand our results in light of this observation, we then look into the patterns of residential choices of the respondents through time and space in Table 8.

The first interesting result to report from Table 8 is that the effects of school diversity on residential choices we have documented so far actually really emerges

Table 7: Interracial relationships and residential segregation

Dependent variable:	Has black partner	Tract black share	
Sample:	Full sample		Those without black partners
	(1)	(2)	(3)
Grade black share, same gender	0.180* (0.103)	0.180*** (0.0660)	0.185*** (0.0671)
Grade black share, opposite gender	-0.0314 (0.0666)	0.0134 (0.0601)	0.0228 (0.0601)
Has black partner		0.0803*** (0.0147)	
Observations	7090	7090	6938
Adjusted $R^2$	0.035	0.198	0.187

*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 variable in column 1 takes a value of 1 if the individual is cohabiting with a black partner in Wave 5, and zero otherwise. Column 3 restricts to the sample where this variable takes a value of zero. The dependent variable in columns 2 and 3 is the black share in the census tract, as in the baseline regression. Standard errors (in brackets) are clustered at the school level. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

between Waves 3 and Wave 5 (columns (1) to (3) in Table 8). This is particularly interesting as respondents are more likely to have school-aged children in Wave 5 (aged on average 38 years) than in Wave 3 (aged on average 22 years), and a child's school enrolment has been documented to be one of the main drivers of the white flight (Caetano and Maheshri, 2017).

Furthermore, columns (4) to (6) in Table 8 investigate how far individuals move between Wave 3 and 5, when the impact of school contact on residential choices emerge. While column (4) shows that there is no impact on moving distance *per se*, the positive and significant coefficient on the relative tract black share in column (5) means that people who are in blacker neighborhoods in Wave 3 move further between Wave 3 and 5.<sup>5</sup> This is consistent with the white flight hypothesis. Most interestingly, the interaction of same gender black share and the relative tract black share in Wave 3 shows a strong negative sign. Hence, we find that this “white flight effect” is weaker for people who had more blacks of the same gender in their grade in school. This result finds further confirmation in the specification of column (6), where we interact the relative tract black share with school-gender fixed effects and all other controls.

<sup>5</sup>The relative tract black share is defined as the black share of the census tract where an individual lives in Wave 3, minus the median Wave 3 census tract black share of other whites from the same school.

Table 8: Impact on moving behavior

	Change in census tract black share			Log of km moved, Waves 3-5		
	Wave 1 to Wave 5 (1)	Wave 1 to Wave 3 (2)	Wave 3 to Wave 5 (3)	(4)	(5)	(6)
Grade black share, same gender (S)	0.205*** (0.0586)	0.0259 (0.0567)	0.179*** (0.0693)	-1.195 (1.129)	-0.349 (1.140)	-0.589 (1.196)
Grade black share, opposite gender (O)	-0.0141 (0.0565)	0.0902* (0.0543)	-0.0868 (0.0662)	0.653 (1.078)	0.777 (1.081)	0.761 (1.128)
Relative tract black share, Wave 3 (R)					3.737*** (0.394)	
$S \times R$					-13.66*** (4.684)	-20.47** (9.354)
$O \times R$					0.636 (4.705)	1.556 (9.429)
FEs $\times$ R						Y
Observations	7034	5797	5843	5843	5843	5843
Adjusted $R^2$	0.0970	0.0633	0.0236	0.0637	0.0787	0.0888
Dep. var mean	0.0274	0.0231	0.00423	3.523	3.523	3.523

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 variable in column 1 is the difference in the census tract black share where an individual lives in Wave 5 (as measured at Wave 5) - i.e. the baseline outcome measure - minus the census tract black share where an individual lived in Wave 1 (as measured in Wave 1). The dependent variables in columns 2 and 3 are defined similarly. The dependent variable in columns 4 to 6 is the the logarithm of one plus the number of km an individual moved between Waves 3 and 5. The relative tract black share (R) is defined as the black share of the census tract where an individual lives in Wave 3, minus the median Wave 3 census tract black share of other whites from the same school. Standard errors (in brackets) are clustered at the school level. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

## 5 Conclusions

In this paper, we analyze how variation in a white student's school peer group 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 the racial composition of students' school cohorts not only increases interracial friendship in school, but it also induces people to reside in neighborhoods with more blacks in 2016-18. This finding is driven driven by black peers of the same gender as the respondent, and is robust to several modifications of the model, including the introduction of grade-school and tract fixed effects.

Economic opportunities and partner preferences in interracial relationships are unlikely to be a major force behind our results. Indeed, we find no effect of racial composition in a students' school cohort on individual education and labor market outcomes, nor on neighborhood characteristics, such as average income or crime. Instead, the most likely mechanism behind our results is a change in preferences

of respondents which translates into a reduction of white flight behavior for people who had more blacks of the same gender in their grade in school.

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## Appendix A 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 (4) of Table 3.

We first run the same regression for different subsample. The results and the P-value of the test comparing the coefficients on the different samples are reported in Table A9. These show that there are no significant difference across genders, region or segregation at the county level.

Furthermore, Table A10 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 is significant.

Table A9: Subsample splits

	Gender		Region				County segregation	
	Female	Male	North-east	Mid-west	South	West	Low	High
<i>Dependent variable: Any partners black</i>								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grade black share, same gender	0.226*** (0.0785)	0.168 (0.118)	-0.0280 (0.115)	0.220 (0.190)	0.250*** (0.0875)	0.366* (0.199)	0.118 (0.0759)	0.264*** (0.0698)
Grade black share, opposite gender	0.117 (0.0756)	-0.145 (0.0989)	-0.0319 (0.121)	-0.144 (0.158)	0.102 (0.0770)	0.0894 (0.242)	-0.0303 (0.0731)	0.0256 (0.0665)
P-val, coefs equal	.68		.38				.26	
Observations	3942	3148	1298	2179	2413	1192	3500	3590
Adjusted $R^2$	0.199	0.179	0.0566	0.106	0.186	0.0867	0.272	0.105
Dep. var mean	0.0820	0.0817	0.0545	0.0602	0.122	0.0706	0.0884	0.0756

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 A10: Interactions

Interaction term:	School black share	School black segregation	Republican vote share in 1992	School urban share	Students in grade
<i>Dependent variable: Tract black share</i>					
	(1)	(2)	(3)	(4)	(5)
Grade black share, same gender	0.271** (0.112)	0.169 (0.137)	0.185*** (0.0710)	0.211*** (0.0726)	0.237** (0.0941)
Grade black share, opposite gender	0.113 (0.111)	-0.0385 (0.115)	0.0152 (0.0647)	0.00544 (0.0642)	0.102 (0.0910)
Same gender x interaction term	-0.403 (0.422)	0.0384 (0.264)	0.202 (0.788)	-0.109 (0.163)	0.000875 (0.000689)
Opp. gender x interaction term	-0.552 (0.430)	0.0596 (0.225)	-0.215 (0.854)	0.0759 (0.144)	-0.0000158 (0.000599)
Observations	7090	7022	7050	7082	7090
Adjusted $R^2$	0.160	0.160	0.167	0.159	0.199

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

## Appendix B Attrition

Table B11 reports two test 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 take values 1 if the 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.

Columns (3) and (4) run the same specification as columns (3) and (4) of Table 3, but controlling for the panel Wave 1-Wave 5 weights provided by Add Health. The results are not affected, suggesting that they do not depend on the way individuals are weighted in the regressions.

Table B11: Baseline results with weights and attrition

Dependent variable:	In baseline sample		Wave V tract black share	
	(1)	(2)	(3)	(4)
Grade black share, both genders	-0.275 (0.258)		0.0726 (0.126)	
Grade black share, same gender		-0.109 (0.162)		0.180** (0.0898)
Grade black share, opposite gender		-0.222 (0.166)		-0.100 (0.102)
Observations	11999	11999	7090	7090
Adjusted $R^2$	0.039	0.039	0.245	0.247

*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 3 and 4 are identical to 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 C Tests for non-random clustering

We undertake a number of tests that look for evidence of non-random clustering of black students within schools. The relevant sample on which to conduct tests of non-random 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 non-random 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 non-random clustering by transforming the

standard test appropriately. In particular, in column 1 of Table C12 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  $K_c$  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.

Table C12: Tests for non-random clustering

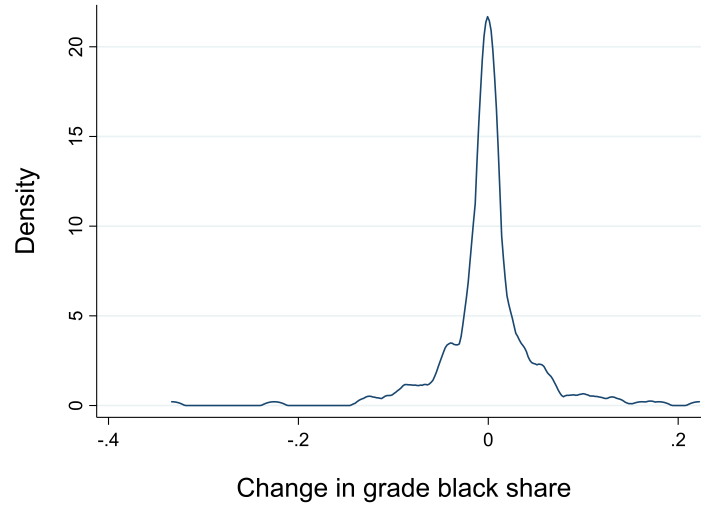
	Transformed black dummy (1)	Transformed black dummy (2)	Black dummy (3)	Black dummy (4)	Black share of males in grade (5)
Black share of others in grade	0.149 (0.210)		0.00920 (0.414)		
Black share of others of same gender in grade		0.00602 (0.0989)		-0.138 (0.217)	
Black share of opposite gender in grade		0.0208 (0.0928)		-0.0337 (0.233)	
Black share of others in school			-98.69*** (23.19)	-101.8*** (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. Controls in columns 1 to 4 include grade-gender fixed effects and school-gender fixed effects, and in column 5 include grade and school 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. \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

An alternative method for correcting for exclusion bias is proposed by Guryan et al. (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 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 which we could imagine influencing the female black share would also simultaneously influence the male black share.

Figure C1: Kernel density of change in grade black share



As a final test on the randomness of variation in grade black share, we check whether differences in black share across grade 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 C1 and we observe that there is no obvious asymmetry. Indeed, the mean change in grade black share is -0.0005792.