Journal of Public Economics 239 (2024) 105242

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

The long run impact of childhood interracial contact on residential segregation[☆]

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ARTICLE INFO

Keywords:
Residential segregation
Social contact
Race

ABSTRACT

This paper exploits quasi-random variation in the share of Black students across cohorts within US schools to investigate whether childhood interracial contact impacts the residential choices of Whites when they are adults. 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.

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 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 for the Black population 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 infection rates (Torrats-Espinosa, 2021).[1] The literature differentiates between three different causes of racial 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 et al., 1999; Boustan, 2017; Aliprantis et al., 2022). 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; Davis et al., 2023). Card et al. (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 reduce 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

☆ We would like to thank participants at AFSE 2023, EALE 2022, EEA 2023, EMUEA 2022, ESPE 2022, Verein fuer Socialpolitik 2022 and seminars at Göttingen, ICM, Pittsburgh, PSE, Utrecht and ZEW; two anonymous referees and the editor Danny Yagan for valuable comments and feedback. This project has benefited from support by Research Foundation Flanders-FWO (G029621N), the French National Research Agency (ANR-20-CE41-0014-01 & ANR-17-EURE-0001) and the National Bank of Belgium (2022 Mécénat). This research obtained IRB approval by the Ethics Committee for the Social Sciences and Humanities of the University of Antwerp (main affiliation of Merlino up to 08/2022). We thank Bradford Morbeck for excellent research assistance. The usual disclaimers apply.

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[1] The literature has highlighted several factors through which segregation can negatively affect socio-economic outcomes of Blacks including, among others, access to and quality of public schooling and healthcare, increased crime, reduced property values, and poor environmental conditions (see, among others, Boustan, 2011; Chetty et al., 2016; Torrats-Espinosa, 2021; Akbar et al., 2022; Derenoncourt, 2022).

https://doi.org/10.1016/j.jpubeco.2024.105242
Received 8 December 2023; Received in revised form 2 September 2024; Accepted 17 September 2024
Available online 11 October 2024
0047-2727/

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.[2] We provide several tests giving evidence that the variation we use is as good as random and uncorrelated with other variables that might influence residential choices. Moreover, while in most schools in our sample there is enough exogenous variation in the share of Blacks to provide reliable estimates, it is likely to be too small to trigger 'flight' of White parents moving their children away from the school.

The main contribution of this paper is 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 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 Blacks in one's neighborhood more 20 years later by almost 0.4 percentage points, which is 5 percent of the mean. The effect that we find occurs in between people's 20s and 40s, a period where the vast majority of our sample change census tract. The result is therefore driven by people choosing to live in census tracts with more Blacks, rather than differing propensities to stay in Blacker tracts. The results are robust to several specifications, such as the introduction of grade-school and census tract fixed effects, ensuring that the results do not simply reflect preexisting patterns of residential segregation.

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 for those in interracial relationships. Instead, it appears our results are likely to be shaped by changes in racial attitudes. Consistent with this, we find that exposure to Black peers increase White adults' stated liberalness and the likelihood of interracial partnership. Moreover, it reduces the chance that White respondents interviewed by Black interviewers display discomfort during the interview. Therefore, our study highlights that interracial contact and friendship between students in school can help to change racial attitudes without producing any kind of negative externalities. This can happen by modes including self-reflection regarding one's own racial identity and bias, racial decategorization, changes in role models, and increased empathy and emotional connections (Tropp and Wright, 2001; Pettigrew and Tropp, 2008).

Our analysis also suggests that interracial exposure in childhood is able to translate into a reduction of White flight behavior later in life. In particular, our effect is particularly strong for Whites in neighborhoods which are considered as having a large potential for White flight behavior—i.e., those with a share of Blacks around 10 to 20%. Furthermore, individuals who have been exposed to Blacks in school exhibit a less noticeable negative correlation between the concentration of Black residents and both neighborhood satisfaction and social interaction with neighbors. Therefore, we document that interracial exposure in school can mitigate patterns associated with 'White flight' (Schelling, 1971; Boustan, 2010; Bayer et al., 2022).

Our paper contributes to the literature on the determinants of residential segregation (Massey and Denton, 1993; Charles, 2003; Boustan, 2011). In particular, we highlight how social contact with Blacks in high school can in the long run help to reduce the propensity of Whites to avoid living in Black neighborhoods. In doing so, we complement a body of related work examining how White parents react to changes in the overall racial composition of schools (e.g., Baum-Snow and Lutz, 2011). In particular, our study differs by analyzing the impact of quasi-random within-school variation, which is generally unobservable by parents when choosing a school or residential location.

We also contribute to the literature that finds that contact between groups can change attitudes and influence behavior (e.g., Dobbie and Fryer, 2015; Carrell et al., 2019; Mousa, 2020; Boucher et al., 2021; Bursztyn et al., 2021; Lowe, 2021; Corno et al., 2022). In particular, in line with Allport (1954)'s theory, social contact with minority groups has a positive effect on attitudes of the majority if this happens in situations characterized by equal status, cooperation, common goals, and support by social and institutional authorities, as in the context of schools. Yet there is no evidence on whether such contact can have important behavioral impacts on residential choices a long time after the contact has occurred.[3] The decision regarding where to live holds economic significance given its profound implications for health, educational opportunities, well-being and outcomes in the labor market (Bayer et al., 2008; Chyn and Katz, 2021). In our specific context, location choices of Whites have an additional impact as they can influence the extent of racial residential segregation, thereby affecting the well-being of Blacks.

The remainder of the paper is organized as follows. Section 2 describes the data. In Section 3 we present the estimation strategy and provide evidence in favor of our main identification assumption. In Section 4, we present our benchmark results and several robustness checks. Section 5 interprets our empirical findings and discusses potential channels at play. Finally, Section 6 concludes and discusses policy implications.

2. Data

We utilize data from the National Longitudinal Survey of Adolescent Health (Add Health) which covers 80 nationally representative high schools and 54 feeder schools in the US.[4] The initial phase of the survey involved administering a questionnaire to all students enrolled in grades 7–12 during the academic year 1994–95. This self-administered in-school survey included approximately 90,000 students and collected information on basic socio-demographic information, such as gender and race, social behavior and friendships. In particular, it asked students to nominate the five closest female and male friends. Another sample of students was then interviewed at home and asked several detailed questions on topics including family background, health behaviors and friendships. This in-home survey was administered to around 20,000 students (of whom around 13,000 are White), who then

[2] See, for example, Bifulco et al. (2011), Lavy et al. (2012), Patacchini and Zenou (2016), Carrell et al. (2018), Fruehwirth et al. (2019) and Merlino et al. (2019). We also exploit idiosyncratic variation in gender shares across grades within a school in a similar manner to papers such as Lavy and Schlosser (2011) and Hill (2015).

[3] The economic literature on long run effects has so far focused on interracial relationships (Gordon and Reber, 2018; Merlino et al., 2019) and political behavior (Billings et al., 2021; Schindler and Westcott, 2021; Polipciuc et al., 2023). While these outcomes are important, interracial relationships are a relatively rare phenomenon, and an individual's vote has no direct economic impact on themselves.

[4] 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 Health, Carolina Population Center, 123 W. Franklin Street, Chapel Hill, NC 27516-2524 (Add Health@unc.edu). No direct support was received from grant P01-HD31921 for this analysis.

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).

The Wave 1 in-school survey is basically a census of students, so we use it to construct our main independent variables, i.e., the shares of students in peer groups who are Black.[5] We consider three alternative groups of peers: all those in the same grade, those of the same sex in the same grade, and those of opposite sex in the same grade.

We then use the Wave 5 survey to retrieve our main dependent variable: the share of Blacks in the census tract of the respondent's residence in 2016–18.[6] This information is retrieved by Add Health matching the location of individuals in Wave 5 with the American Community Survey. 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.

As we are interested in the racial attitudes of the majority group towards minorities, our sample contains only White students.[7] This is for two reasons: first, according to Allport (1954), social contact is expected to have an impact on the majority rather than the minority, and, second, the sample of students of other racial groups is small, so we cannot draw robust inference. Of the 8061 Whites interviewed in Wave 5 we have location data for 7520 respondents, of whom we were unable to match 420 with information on their school cohort.[8] This leaves us with a total of 7090 individuals, spread across 126 schools, 434 school cohorts, and 840 peer groups of the same grade and same gender.

In our sample, there is no systematic relationship between one's cohort Black shares and attrition, measured by the probability to be in the Wave 5 sample or the probability of not responding to the first request to participate in Wave 5. Our results are also robust to using sample weights, and are improved by including individuals who did not respond at the first request to participate in Wave 5. See Table A.1 for more details.[9]

Table 1 reports the summary statistics of the main variables of our analysis.[10] 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 (.11 vs .079, respectively), 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 1.6 percentage points for the both gender measure and 2.5 percentage points for the same gender measure.[11] In this sense, our data has two advantages. First, given that the within-school standard deviation is between 20%–30% of the mean, the variation in grade Black share within schools is substantial enough to generate important variations in exposure. Second, while we are unable to look at impacts of very large changes in grade Black shares in percentage point terms, the focus on small variations has the advantage that they are unlikely to trigger drastic behavioral responses by parents, such as changes in residence or schools to avoid contact with minorities. Another general advantage of our data set is that it provides a nationally representative sample of adolescents in school. The grade Black share among all individuals in our sample is 15%, which corresponds to the population share of Blacks in school aged 16 (own calculations with data from the 1990 Census). The fact that the grade Black share among White students, reported in Table 1, is substantially lower reflects patterns of spatial and school sorting.

Finally, Table 1 documents that our sample is well suited to study residential segregation as respondents are characterized by a high degree of mobility, not only immediately after finishing school, but also later in life. More than 90% of respondents moved between Wave 3, when respondents were 18 to 26 years old, and Wave 5, when respondents were 33 to 43 years old. These were not primarily short-distance moves within the same neighborhood, as half of the moves had a distance of more than 20 km.

3. Identification and methodology

Cohort composition is likely to be correlated with several (possibly omitted) variables that impact residential choices, such as 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. Hence, directly regressing residential segregation on cohort composition is likely to produce biased results.

We overcome this issues by exploiting within school variation in the share of Black students across cohorts. The idea is that the differences between the average school composition and their child's specific cohort (which is not observed at the time of enrollment) does not affect school choice.

3.1. The empirical model

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

$$Y_i = \alpha_0 \; ShareBlack_{gs} + I_{gm} + I_{sm} + \epsilon_i, \tag{1}$$

where $ShareBlack_{gs}$ is the share of Blacks in grade g in school s in Wave 1, I_{gm} are grade-gender fixed effects, I_{sm} are school-gender fixed effects, and ϵ_i is the error term.[12]

The gendered racial composition of grades plays a significant role in our analysis. Hence, we estimate a second regression model exploiting the gendered racial composition of grades. The corresponding regression equation is the following:

$$Y_i = \alpha_1 \; ShareBlack^{same}_{gs} + \alpha_2 \; ShareBlack^{opp}_{gs} + I_{gm} + I_{sm} + \epsilon_i, \tag{2}$$

5 In the in-school survey, students self-disclose their racial identity, with a minority reporting affiliations with more than one race. In the context of this paper, the Black share refers specifically to the share of students who exclusively identify themselves as Black.

6 This is the finest level of aggregation available in Add Health. Besides, note that census tracts are small geographic areas: they generally have between 1500 and 8000 people, with an optimum size of 4000 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.

7 To minimize selection in terms of racial identity, we use race as assigned by the interviewer, which is available only for respondents of the in-home survey. In this sample, 18 students self-declare to be of mixed race. Dropping them from the sample does not change the results we present in Section 4.

8 Moreover, we drop 10 White students who we were not able to match to in-school data with more than one cohort.

9 This is in line with the findings of Bifulco et al. (2011) and Merlino et al. (2019), who find no evidence that attrition in Wave 4 is correlated with minority shares within cohorts.

10 Additional summary statistics can be found in Appendix B.

11 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 are extremely similar when we remove them.

12 In the data, high schools are usually separated from their feeder school; when this is not the case, we impute them as separate if the transition to high school, typically grade 9, induces large variations in cohort size. This avoids treating them as variations in within school grade compositions.

Table 1
Summary statistics.

	Mean	Median	Within school s.d.	Between school s.d.	N
Main variables					
Share of census tract Black, Wave 5	.082	.035	.11	.079	7090
Share of census tract Black, Wave 1	.055	.011	.063	.12	7034
Grade Black share, both genders	.08	.025	.016	.19	7090
Grade Black share, same gender	.079	.024	.025	.19	7090
Other Wave 1 variables					
Age	16	16	1.1	1.4	7090
Female	.56	1	.47	.14	7090
Hispanic	.13	0	.19	.23	7090
Family income (000's)	52	45	34	25	5705
Grade size	224	182	24	132	7090
School size	820	677	0	515	7090
Grades in school	4.1	4	0	1.2	7090
In middle school	.22	0	0	.49	7090
In high school	.59	1	0	.5	7090
Lives in urban area	.46	0	.17	.43	7031
Region = Northeast	.18	0	0	.41	7090
Region = Midwest	.31	0	0	.43	7090
Region = South	.34	0	0	.49	7090
Region = West	.17	0	0	.36	7090
Moving related variables					
Moved house between Waves 1 and 5	.93	1	.25	.12	7060
Moved house between Waves 3 and 5	.91	1	.26	.15	5845
km between Wave 1 and Wave 5 location	349	23	670	294	7060
km between Wave 3 and Wave 5 location	340	21	699	277	5845

where $ShareBlack_{gs}^{same}$ is the share of Blacks of the same gender as the respondent in grade g in school s in Wave 1, $ShareBlack_{gs}^{opp}$ is the share of Blacks of the opposite gender to that of the respondent in grade g in school s in Wave 1, and the other terms are as in (1). 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. In this model, the within school variation is driven by idiosyncratic changes in the gender and racial composition of the population in the school district.

Since we have gender-specific cohort shares, we also 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.[13]

Our main dependent variable Y_i is the share of the population that are Black living in the same census tract as the respondent in Wave 5.

3.2. Identification assumption

Our methodology is valid under 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 specific cohort in that school do not matter since they are unobservable and unknown to parents at the time of the school choice decision.[14] We test some implications of this identification assumption.

First, we perform several balancing tests. In other words, 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 our main identification assumption: only two of the predetermined variables in table, grade size and language spoken at home being different from English, are significantly different from zero at the 10 percent 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.[15]

One concern may be that we lack power to detect small correlations between our main treatment variables and observables which influence future residential location choice. Hence, we combine all of our predetermined variables used in the balancing tests to predict the Wave 5 tract Black share of each individual using an OLS regression, always controlling for grade-gender and school-gender fixed effects. We then test for whether this predicted value is correlated with any of our treatment variables in the final row of Table 2 and find no significant correlation.

Second, the race of a student should be uncorrelated with that of their peers after controlling for school-gender fixed effects. We perform several tests for non-random clustering of Black students across grades within schools, including those proposed by Guryan et al. (2009) and Caeyers and Fafchamps (2016), that take into account that each

[13] We cluster standard errors at the school level since students are sampled using a two-stage process in which first a sample of schools are selected—see Abadie et al. (2022) for a discussion. Results are robust to clustering at the school-grade level or the school-gender level.

[14] There could be selection after children start in a school if White students with a high grade Black share are more likely to change school. We exploit Wave 2 of the Add Health survey, which interviewed students a year after Wave 1, and do not find any evidence of school changes being driven by grade Black share. See Appendix C for more details.

[15] Summary statistics of these variables are presented in Table B.1. Additionally, we run regressions like those reported in Table 2 for a comprehensive set of pre-treatment 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.

Table 2
Balancing tests for cohort composition measures.

	(1)	(2)	(3)	(4)
	N	Independent variable:		
		Grade Black share, both genders	Grade Black share, opp. gender	Grade Black share, same gender
Age	7090	0.0191	−0.113	−0.0846
		(0.440)	(0.264)	(0.297)
Parent is Black	6350	0.0441	0.00399	0.0543
		(0.0269)	(0.0355)	(0.0486)
Share of census tract black	7034	0.0102	0.0329	−0.00589
		(0.0851)	(0.0613)	(0.0588)
Share of census block black	7030	0.00335	0.0374	−0.0164
		(0.0976)	(0.0635)	(0.0816)
Grade size	7090	125.8*	72.61*	59.76
		(74.95)	(39.33)	(43.77)
Share same gender	7090	0.0215	0.0180	−0.0693
		(0.0701)	(0.0428)	(0.0478)
Born in USA	7090	0.00679	0.0643	−0.0303
		(0.0836)	(0.0514)	(0.0628)
Lives with both biological parents	6326	0.0871	0.165	−0.0447
		(0.359)	(0.216)	(0.245)
Number of older siblings	7081	−0.481	0.0745	−0.492
		(0.748)	(0.498)	(0.435)
Years of parental schooling	6816	1.254	1.233	0.0601
		(1.191)	(0.746)	(0.823)
Log of family income	5650	0.611	0.422	0.0594
		(0.524)	(0.334)	(0.356)
Home language is not English	7090	0.143	0.0201	0.145*
		(0.0970)	(0.0641)	(0.0760)
Predicted Wave 5 tract Black share	7090	−0.0148	−0.0141	0.00458
		(0.0142)	(0.0112)	(0.0147)

Notes: The table reports OLS estimates controlling for 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. * $p < .10$, ** $p < .05$, *** $p < .01$.

individual is present in many others' peer groups but not their own. More details can be found in Appendix C. 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 as good as random.

Third, if Black shares were driven by higher dropout rates for Blacks, we should observe systematically lower Black shares in later grades. In Appendix C, we plot the distribution of differences in the Black shares between grades, and find it to be very symmetric. This is consistent with quasi-random variation.

A fourth concern is that the end of court-ordered desegregation orders in the early nineties that led to significant changes in racial composition of school districts (Lutz, 2011) explains the variation in the share of Black students across grades we observe. However, during the time in which we study exposure in schools (Wave 1, 1994/1995) most of the court-ordered desegregation plans were still in place. Based on a representative survey of schools collected by Rossell and Armor (1996), only 6 out of 130 school districts had court-ordered desegregation plans dismissed these before 1996 (Lutz, 2011). Consistent with this, only 1.7% of our sample lives in a school district where a court dismissed the desegregation order between 1989 and 1995, and hence it would be at most an extremely small part of the variation we use which stems from the dismissal of desegregation plans.

However, the within-school variation used could be potentially driven by schools that experienced large changes in racial makeup for different reasons, such as redrawing of school districts, changes in catchment areas or the construction of a large building complex. We address this aspect in Appendix D and provide evidence in Table D.1 that it is very unlikely that at our results are driven by such cases.[16]

Additional support comes from Table 2 which shows that there is no significant correlation between variation in cohort composition and Wave 1 neighborhood Black shares. This strongly suggests that changes in the independent variables of interest are not systematically driven by changes in the residential location of pupils, nor by changes in its racial composition.

Given these results, we conclude that the variation in Black students across grades within schools is as good as random and is driven by idiosyncratic changes in the gender and racial composition of birth cohorts in school districts.

4. Results

4.1. Exposure and contact

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 more exposed 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 et al., 2009; Mele, 2017, 2020). It is therefore important to test this assumption, and we can do so using information on contact provided in the Add Health data.

Table 3 reports results indicating that a higher Black share in a grade increases social contact with Blacks. In columns (1) and (2) we

[16] We also document in Appendix D in Fig. D.1 that our results remain consistent and the coefficient of interest changes minimally when we drop one school at a time. Therefore, we can rule out the possibility that potential concurring shocks affecting the racial composition happening in a single school are the main drivers of our findings.

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

Dependent variable:	Share of Black classmates		Has at least one Black student among set of					
			Classmates		Closest classmates		Friends	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grade Black share, both genders	0.407*** (0.153)		0.796** (0.371)		0.419 (0.294)		0.167 (0.105)	
Grade Black share, same gender		0.200** (0.0886)		0.438** (0.204)		0.467** (0.210)		0.206*** (0.0781)
Grade Black share, opposite gender		0.191* (0.0967)		0.373** (0.177)		−0.0561 (0.130)		−0.0186 (0.0771)
Observations	2623	2623	2635	2635	2623	2623	7090	7090
Adjusted R^2	0.924	0.924	0.824	0.824	0.137	0.144	0.019	0.020
Dep. var. mean	0.085	0.085	0.546	0.546	0.012	0.012	0.015	0.015

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

show that more Blacks in a grade within a school translates into a higher share of classmates who are Black.[17] Importantly, in columns (3) and (4) we observe that these changes in cohort shares have a significant impact on whether or not individuals share a class with *any* Black student, consistent with most of our sample coming from schools with relatively low Black shares in general. In columns (5) and (6), we restrict to a much smaller set of classmates—those who the student in question spends the most class time with (typically just one person). Here again there is a significant effect, but only of the share of Blacks among those of the same gender, which is not surprising given that almost all such classmates are of the same gender. Similarly, in columns (7) and (8) we note that more Blacks in a grade also translates into a higher share of Blacks being nominated as friends, but again only if they are of the same gender. These results are consistent with the broader literature that shows young people form closer friendships with individuals of their own gender (McPherson et al., 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.[18]

An important point highlighted by these results is that, although the variation we have is simply driven by random variation, it is enough to generate large impacts in interactions with Blacks for some White students. In other words, rather than thinking of a 10% increase in the grade Black share as uniformly increasing everyone's contact with Blacks by 10%, it is likely that some share of the White students experience a large increase in exposure. This may be the change, for instance, in going from having no Black classmates to having one. For example, the coefficient of column (3) of Table 3 implies that a 2.5 percentage point increase in the share of same gender Blacks in one's grade increases the probability of having at least one Black classmate by 2 percentage points. As a result, it is reasonable to hypothesize that there could be a large impact on the attitudes and behavior of those most effected by the change in cohort composition.

4.2. The long run effect on residential choices

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.

Fig. 1 presents 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, also end up living in Blacker neighborhoods in Wave 5 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 give an idea about the size of the effect, note that if we add the census tract Black share in Wave 1, which we expect to be an important factor explaining residential location in adulthood, the corresponding coefficient (reported in column (2) of Table 5) is less than half the size of the coefficient of the same gender Black share in one's grade.

To better understand whether these findings are driven by respondents living in neighborhoods with very few Blacks, we construct dummy variables that take the value of one if the individual resides in a neighborhood where the share of Blacks in 2016–18 is above a threshold of 10% and 20%. Using these variables as dependent variables in columns (3)-(6) yields once again significant estimates which are larger in size than those using the full sample. We conclude that a large part of the main result is being driven by pushing White individuals to choose neighborhoods 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%. Indeed, these results show that social contact with Blacks has an effect precisely for Whites living in neighborhoods whose share of Blacks is within this range, i.e., those who would have been most likely to flight.

To further elaborate on this point, we plot in Fig. 2 the results of regressions using binary outcomes that capture different degrees of Black neighborhood concentration. For this purpose, we constructed a set of dummy variables, each of which takes the value of one if the respondent lives in Wave 5 in a tract with more than x% Blacks, with x ranging from to 0 to 100. The figure shows an inverted U-shape pattern. The point estimates start to become significant around the threshold

[17] Data on all classes attended by pupils was collected for a subset of high schools, substantially reducing the sample. However, this information has two advantages: it is based on school transcripts, rather than being self-reported, and it reflects exposure to a broader group of fellow students than friends. We define two people as classmates if they took any classes together. We use outcomes in Wave 1, but results are very similar if use those in Wave 2. Moreover, results on friends remain very similar if we restrict to 'closest' friends, i.e., those they report having most contact with. Results available upon request.

[18] Note also that our results are compatible with the existence of homophily in friendship found by Currarini et al. (2009), Mele (2020) and Fletcher et al. (2020). While 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** (0.0746)		0.588* (0.309)		0.427** (0.190)	
Grade Black share, same gender		0.194*** (0.0565)		0.454** (0.197)		0.415** (0.159)
Grade Black share, opposite gender		0.0109 (0.0557)		0.219 (0.286)		0.00913 (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. * $p < .10$, ** $p < .05$, *** $p < .01$.

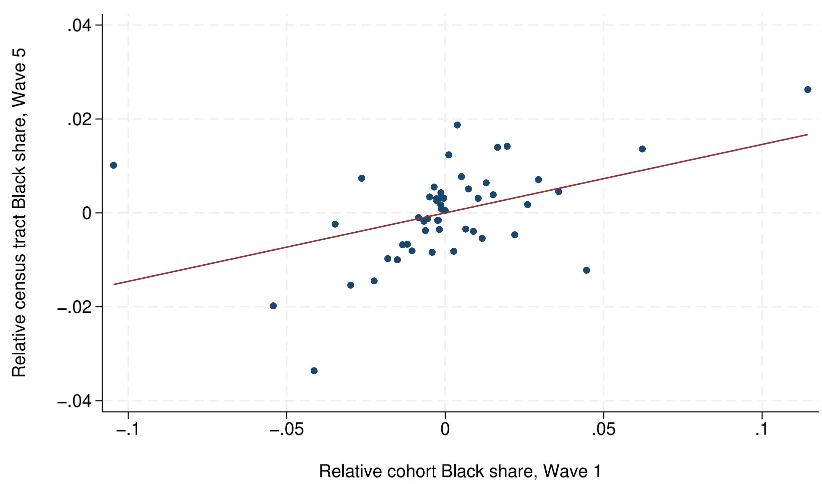

Fig. 1. Correlation of relative shares, same sex cohort. *Notes:* The figure plots a binned scatter plot of the relative share of Blacks in the Wave 5 neighborhood against the relative share of Blacks in the Wave 1 same gender cohorts. A cohort's relative Black share is the share within the cohort minus 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.

of 10% and decrease in size if Black neighborhood concentration is larger than 20%. This indicates that the effect is particularly strong for neighborhoods with a Black Share of 10 to 20%. Interestingly, the effect is also still present in neighborhoods up to and beyond the point when Blacks start to become the majority. Overall, the figure highlights 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.

4.3. Robustness

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 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. 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 et al., 2005; Oster, 2019).

We can additionally control for a number of unobservables by introducing school trends and other fixed effects. In column (4), we include school-specific trends that control for potential factors that systematically could vary across schools such as differences in Black dropout rates. In column (5) we include school-grade fixed effects to control for any factors or shocks that influence a particular grade within a school. Doing so, implies that our coefficient of interest is identified exclusively from the difference between the shares of blacks between genders. 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. Hence, 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). Hence, this specification ensures that our findings are not about the persistence of residential segregation, but rather about social contact with Blacks reducing it. The results reported in Table 5 show that the coefficients are relatively stable in these specifications, if not slightly stronger.

The insignificance of the opposite gender share in our baseline regression suggests that our results are unlikely to be driven by omitted variables, since most variables of concern that are correlated with the same-gender grade Black share would also be correlated with the opposite-gender grade Black share. We can extend the same logic by adding to our regression the Black shares in cohorts relatively close to that of the individual, and regress them against the share of Blacks in census tract in Wave 5. Fig. 3 reports the coefficients of these regressions. We see that only the coefficient on the same cohort is significant. This figure also shows that results are similar when the dependent variable is having a Black friend in Wave 1, consistent

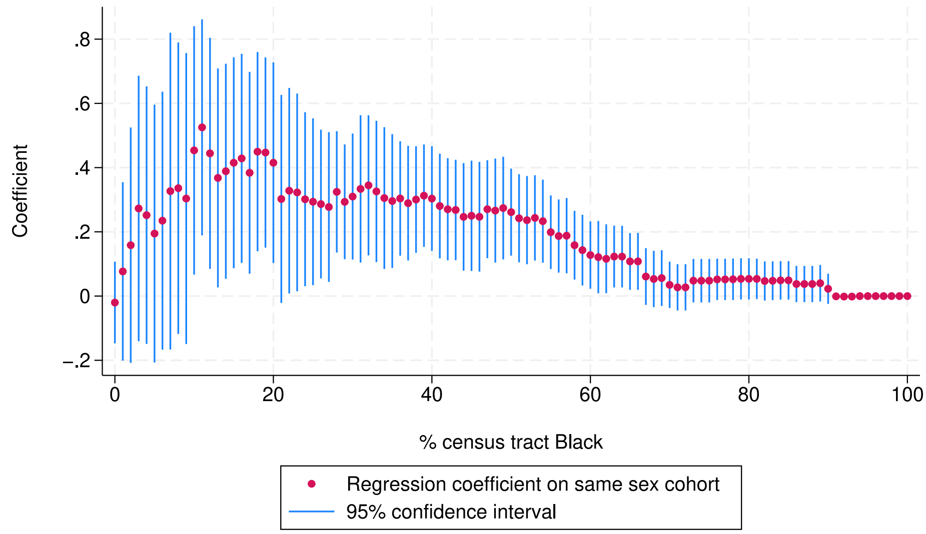

Fig. 2. Impact across the distribution. *Notes:* The figure plots OLS coefficients and 95% confidence intervals of the same gender grade Black share when we use as an outcome variable a dummy that takes the value one if the respondent lives in Wave 5 in a neighborhood with more than x% Blacks (for different 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.

Table 5
Robustness analysis.

	(1)	(2)	(3)	(4)	(5)	(6)
Grade Black share, same gender	0.194*** (0.0565)	0.195*** (0.0511)	0.181*** (0.0515)	0.196** (0.0822)	0.263*** (0.0980)	0.291*** (0.104)
Grade Black share, opposite gender	0.0109 (0.0557)	0.00861 (0.0518)	0.00910 (0.0525)	−0.0148 (0.0631)		
Census tract Black share, Wave 1		0.0892** (0.0358)	0.0884** (0.0356)	0.0954*** (0.0358)	0.0846** (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. * $p < .10$, ** $p < .05$, *** $p < .01$.

with this cohort being the most important in generating substantial contact.[19]

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 D, which provides estimates from several alternative model specifications.

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 did not initially respond to the Wave 5 survey, which is comforting if we believe they may be more like non-respondents than the rest of the sample. Finally, in column (6) of Table D.1, we show that our result is robust to removing individuals who are in grade 12 in Wave 1, since we may be concerned that variation in Black share measured here is a function of differential dropout rates.

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

[19] Note that, at least for students in high school, there is some contact across grades, as documented in the data on who takes courses with whom. The results in Fig. 3 suggest that such contact is not sufficient to generate friendships or changes in residential location, which may be because the exposure in such classes is less important than exposure in elementary or middle school when classes are typically single grades. Alternatively, it may result from there being homophily on grade in a similar way to there being homophily on gender.

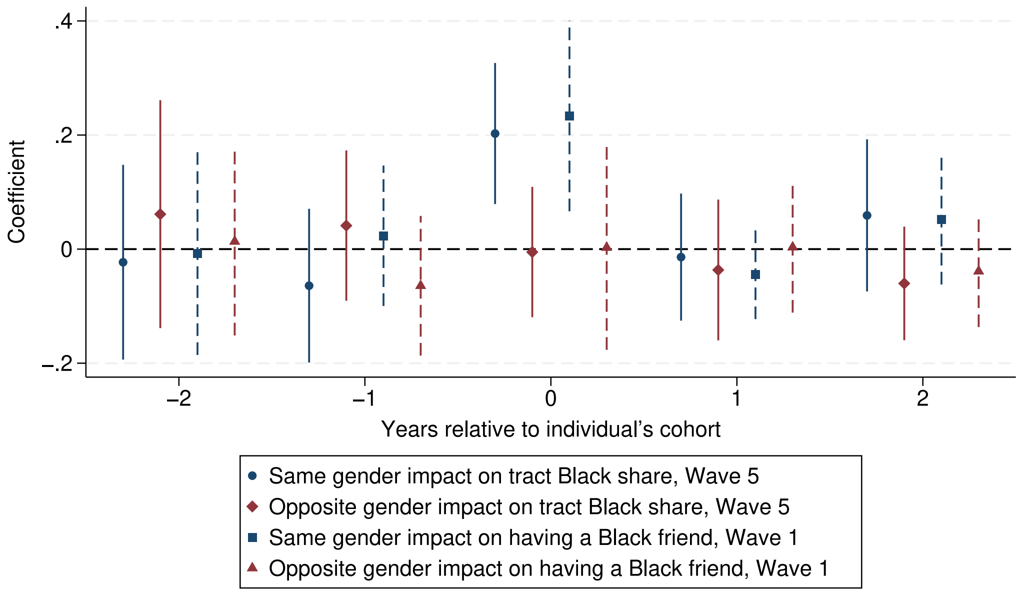

Fig. 3. Coefficients on other close cohorts.

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. Fig. E.1 in Appendix E 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 Fig. E.2 in Appendix E). 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 F, we investigate some interactions and subsample splits to further investigate the nature of our results. We find no 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. This is likely to be the result of a lack of power rather than strong evidence for a homogeneous effect. We do find however a significant and negative interaction with the share of Blacks in the school being larger than 15%, which is in line with the idea that additional contact is meaningful when the size of the minority is small. Regarding subsample splits, we find significant differences along several dimensions, i.e., the number of observations in a school, school Black share, school segregation and region. The first three results are in line with the interpretation that a marginal increase in Black share significantly increases contact only when there are relative few Blacks, while the last one has to be interpreted with caution, as Add Health is not designed to be representative at a regional level. Finally, in Table F.3 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 could not be present here.[20]

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

5. 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 review the various mechanisms to qualify our results and discuss potential drivers.

5.1. 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 et al., 2009; Billings et al., 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 college attendance, employment, earnings, or criminal activity (as recorded by never having been arrested). We also combine these outcomes into a single index to increase power. The results of these regressions are presented in Fig. 4.[21] The coefficients on the Black shares in these regressions are always insignificant, consistent with Bifulco et al. (2011) and Merlino et al. (2019), who do not find any impact of minority shares on related outcomes in Waves 3 and 4. For comparison, we also include the share of students of the relevant cohort who have a father or a mother who did not graduate from college.[22] These variables are indeed generally negatively correlated with Wave 5 economic outcomes, with the coefficient for mothers

[20] The lack of statistical power hinders us from drawing robust inferences on whether non-White students are affected by exposure to Whites in a similar way to Black students. Using the relatively small sample of all non-White students and regressing the white share in a census tract on the share of white students in the grade yields insignificant estimates.

[21] In these regressions, we add to our baseline specification the cohort share without a college educated mother and an individual's mother's education. Results are very similar when we simply use the baseline specification.

[22] Bifulco et al. (2011, 2014) and Chung (2020) focus on the share of students with college-educated mothers and fathers, and find a positive educational impact, though (Bifulco et al., 2014) finds evidence that this disappears by Wave 4 of the survey.

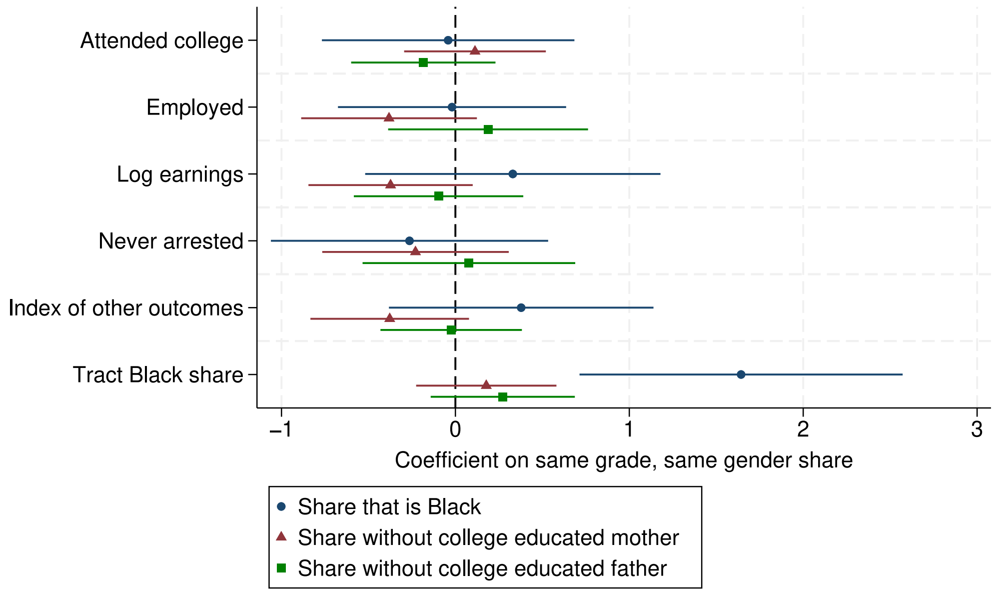

Fig. 4. Other outcomes related to education, employment, and criminality. *Notes:* The figure reports coefficients and 95% confidence intervals of share of students in the same grade and of the same gender who are Black, without a college educated mother or without a college educated father from six OLS regressions controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, school-gender fixed effects and mother's education. The dependent variables are all measured in Wave 5 and are all standardized.

being significant at the 10% level when the index is the dependent variable. However, both coefficients of peers' parental education on the proportion of Black residents in neighborhoods are insignificant and much smaller in magnitude than the coefficient capturing the impact of the cohort's Black share. This suggests that it is very unlikely that the cohort Black share has an impact on economic outcomes sufficiently large to explain our main result.

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. Columns (1) to (6) of Table 6 find no evidence that exposure to Blacks in school has an impact on any of these characteristics of one's (tract-level) neighborhood.[23] To increase power, we combine these standardized measures into a single index, signing each variable so that it is positively correlated with the tract Black share. In column (7), we see that there is no significant impact on this index. Moreover, the coefficient is significantly smaller from that obtained when the (standardized) tract Black share is our dependent variable in column (8).[24] It therefore appears unlikely that our result is driven by changes in the economic opportunities available to Whites.

5.2. 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, individuals tend to see each other less as time passes by and they move further away. 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 Fig. 5 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 (7 years after Wave 1) and Wave 5, (22 years later on average). Hence, the effect is evident not directly after leaving high school, but many years after exposure, and becomes stronger as years go by. Assuming that social connections deplete with time, we would expect the effect to fade over the years if this was the main mechanism. Consequently, this pattern is not consistent with the idea that people chose their residential location to stay closer to their high-school friends.

The second notable pattern is that we do not find a statistically significant effect for counties at any point in time. In other words, we find that exposure to Black peers in school affects residential choices only for moves within, and not across, counties. This is in line with the finding that long-distance moves across counties and states are primarily driven by job related reasons, for which local racial composition should not matter, while those within counties are dominated by housing and family motives (Molloy and Smith, 2019; Jia et al., 2022).

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 et al., 2019). Consistent with this interpretation, in Appendix G we find the correlation between tract Black share and stated liberalness to be strongest in Wave 5,

[23] These results may seem surprising since, in general, census tracts with a higher share of Blacks have different average characteristics. The results are however consistent since, despite an important correlation between these variables, there is also a large amount of dispersion, as the scatter plots in Fig. B.3 show.

[24] There might be compensating differentials by factors such as local amenities, commuting time, access to public transportation, or number of physicians that make these neighborhoods attractive. For example, gentrified "cool" neighborhoods are often more mixed, but are characterized by high property values than other neighborhoods with a similar racial composition. Unfortunately, we cannot test this with the data available in Add Health.

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)	Index (7)	Share Black (8)
Grade Black share, same gender	0.062 (0.53)	−0.058 (0.47)	0.35 (0.57)	0.42 (0.40)	0.34 (0.40)	0.038 (0.37)	0.039 (0.56)	1.60*** (0.47)
Grade Black share, opposite gender	−0.75 (0.47)	0.18 (0.39)	−0.0083 (0.41)	0.033 (0.52)	−0.082 (0.43)	−0.31 (0.36)	−0.27 (0.38)	0.090 (0.46)
Observations	7090	7088	7089	7090	7090	7063	7090	7090
Adjusted R^2	0.330	0.231	0.182	0.121	0.227	0.442	0.192	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. The dependent variables are all taken from the American Community Survey, linked to Wave 5 Add Health data, and standardized. The dependent variable in column (7) is an index of the dependent variables in columns (1) to (6), with them resigned so that they are all positively correlated with the Black share. The dependent variable in column (8) is the (the standardized version of tract Black share. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$.

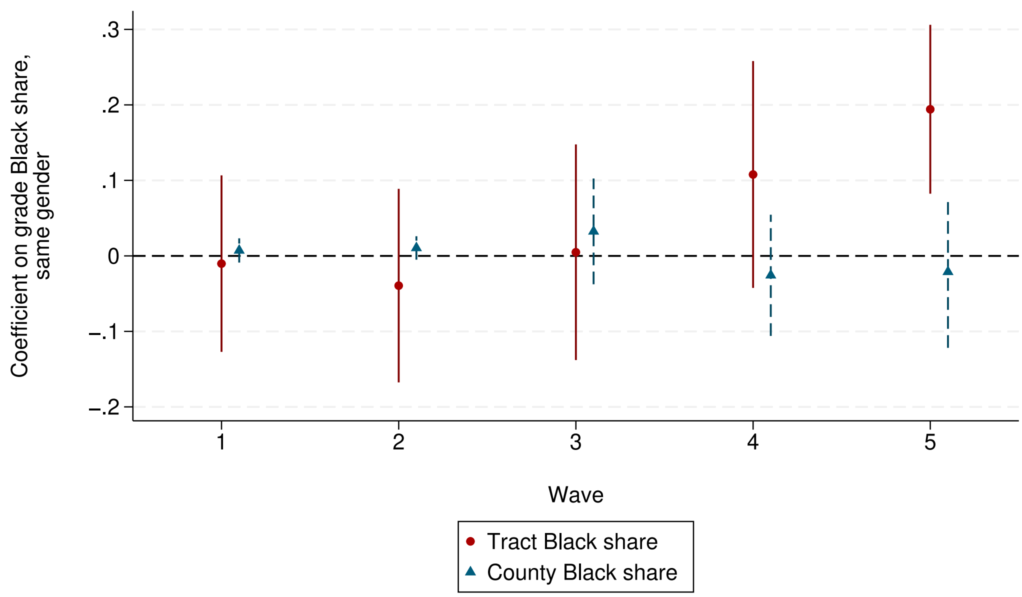

Fig. 5. Impact on tract and county Black shares over time. *Notes:* The figure plots reports OLS coefficients and 95% confidence intervals of the same gender grade Black share from regressions where the dependent variable is the share of Blacks in the census tract (in red) and county (in blue). Regressions control for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and school-gender fixed effects. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

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. The basic idea is that, if our findings were primarily driven by social networks formed in school, then the impact should be strongest for those who still live close to their schools. However, we find in column (1) of Table 7 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 7 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, any contribution to the result would likely be small, since having a Black partner does not mean living in a completely black neighborhood—in our sample, Whites with Black partners live in areas which are 18% Black, compared to 7% for those with White partners. If we were to assume that this difference was the causal impact of having a Black partner, then this would still only explain less than 10% of our baseline result, since the relevant coefficient is slightly lower when we make having a Black partner our dependent variable instead of tract Black share (in column (4) of Table 7).

5.3. Racial preferences

Having ruled out these alternative channels, the remaining likely potential mechanism driving residential choices of people exposed to Blacks in school is a change in preferences. While there are no direct measures of racial attitudes in the Add Health survey, in columns (3) to (6) of Table 7 we analyze the impact on some variables that we believe are attributable to changes in attitudes.

First, although racial attitudes are not measured directly, there are some measures which are likely to be correlated with those. 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 salient political issue, it is reasonable to think that at least part of people's responses to these questions is impacted by their attitudes towards Blacks.[25] In Wave 3, respondents were asked

[25] 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

Table 7
Results relating to social networks and attitudes.

Dependent variable:	Census tract Black share, Wave 5		Stated liberalness index	Has Black partner, Wave 5	Displays discomfort in Wave 3 interview	
Sample:					Black interviewer	Non-Black interviewer
	(1)	(2)	(3)	(4)	(5)	(6)
Grade Black share, same gender (S)	0.298*** (0.0944)	0.185*** (0.0513)	0.742* (0.444)	0.180** (0.0902)	−0.663* (0.334)	0.0482 (0.229)
Grade Black share, opposite gender (O)	0.0578 (0.0908)	0.0236 (0.0484)	−0.171 (0.364)	−0.0314 (0.0758)	0.0415 (0.262)	0.165 (0.291)
Log of km moved, Waves 1-5 (D)	0.00471* (0.00267)					
S × D	−0.0258 (0.0228)					
O × D	−0.0103 (0.0218)					
Has Black partner, wave 5 (P)		−0.0651 (0.0886)				
S × P		0.125 (0.336)				
O × P		−0.0886 (0.763)				
School FEs × D	Y					
School FEs × P		Y				
Observations	7060	7090	7090	7090	717	4890
Adjusted R^2	0.26	0.22	0.11	0.04	0.02	0.04
Dep. var mean	0.08	0.08	−0.00	0.02	0.20	0.17

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 labeled D and P are interacted with this set of controls—the coefficients reported for these variables are therefore the marginal effects at the sample means. The stated liberalness index is constructed from three variables related to how liberal a person declares themselves to be—see Section 5.3 for details. The measure of discomfort in columns (5) and (6) takes the value of 1 if the interviewer indicates that the interviewed person was either embarrassed, bored/impatient, or not candid. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$.

whether race is an important factor within a romantic relationship, which again is reasonable to assume is correlated with more general attitudes towards Blacks. To increase power, we combine these three measures of stated liberalness into a standardized index using inverse covariance weighting and put it as the dependent variable in our baseline regression in column (3) of Table 7.[26] 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.

Second, we change perspective and use information reported by interviewers after the interview about the respondents behavior. For this purpose, we construct a measure of discomfort displayed in the interview, and then distinguish between those interviewed by a Black versus those interviewed by a White interviewer. The discomfort measure is determined by the interviewers based on how they felt the respondent behaved during the interview.[27] The estimates in column (5) of Table 7 show that racial exposure during childhood decreases the chance that White respondents interviewed by Black interviewers feel uncomfortable during the interview, while we do not find any effect on discomfort when the interviewer is White (column (6)). This pattern is in line with a scenario in which those Whites who were exposed to Black students updated their racial preferences and freed themselves from racial prejudices.

As a further test of whether changes in preferences are a consistent mechanism for our main findings, 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 Table 8, we therefore focus three outcomes related to White flight: neighborhood satisfaction, interaction with neighbors and moving behavior. Consistent with the process of White flight, we find that Whites in neighborhoods with a higher Black share are less satisfied with their neighborhood (column (1)).[28] The results also suggest that Whites in blacker tracts interact less often with their neighbors (column (2)).[29]

2016–2018 when racial issues were at the center of Trump's presidential campaign (Henderson, 2016).

[26] See Appendix G for regressions with the individual index components and discussion of how the index varies with residential location across waves.

[27] The corresponding dummy variable takes the value of 1 if the interviewer indicates that the interviewed person was either embarrassed, bored/impatient, or not candid.

[28] 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. In the Add Health data, proxies of neighborhood satisfaction are not available after Wave 2. However, it would be consistent with the social contact hypothesis for interaction with Black peers to affect attitudes as contact happens—and indeed, as stressed in the introduction, most of the literature focuses on this short term effect. Furthermore, satisfaction in the neighborhood in Wave 2 is a good proxy for respondents' attitudes toward residential segregation, as, at that time, most respondents were still in school and did not decide where to reside themselves. As restrict to those who responded in Wave 2, the number of observations in column (1) is smaller than in the full sample.

[29] The frequency of contact with neighbors captures how often a respondent in the past 12 months got together with any of his/her neighbors to chat or for a social visit.

Table 8
Results relating to White flight behavior.

	Neighborhood satisfaction index, Wave 2 (1)	Frequency of contact with neighbors, Wave 5 (2)	Census tract Black share, Wave 5, if moved (3)	Census tract Black share, Wave 5, if not moved (4)
Grade Black share, same gender (S)	0.0287	−0.479	0.218**	0.0794
	(0.437)	(0.505)	(0.0987)	(0.0952)
Grade Black share, opposite gender (O)	0.0542	0.225	0.0225	0.0311
	(0.468)	(0.511)	(0.0756)	(0.0898)
Relative tract Black share (R)	−6.529***	−1.381***		
	(0.635)	(0.469)		
S × R	11.66***	8.104**		
	(3.992)	(3.884)		
O × R	7.007	6.568*		
	(5.446)	(3.340)		
School FEs × R	Y	Y		
Observations	5330	7002	4425	1922
Adjusted R^2	0.09	0.01	0.14	0.35
Dep. var mean	0.00	2.06	0.08	0.08

Notes: 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. Frequency of contact with neighbors is measured on a scale from 1 to 5. The relative tract Black share (R) is the share of census tract residents that are Black (measured in Wave 2 in column (1), Wave 3 in column (2)) minus the median of this variable among those in our sample who attended the same school. The LHS variable in column (3) is the Black share in the census tract in Wave 5 for the sample of respondents that moved to a different census tract between Wave 4 and 5, while column (4) is for the sample of those who did not. 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 labeled R is interacted with this set of controls in columns (1) and (2)—the coefficients reported for this variable is therefore the marginal effect at the sample means. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Interestingly, both patterns which are associated with White flight are significantly weaker for people who had more Blacks of the same gender in their school grade, as indicated by the interaction between the relative black share and the share of Blacks of the same gender in the grade (indicated by $S \times R$ in the table).[30] In other words, for Whites more exposed to Blacks at school, their neighborhood satisfaction and social interaction with neighbors is less negatively correlated with the neighborhood Black share.[31] Together with the results presented in Table 6, these results strongly suggest that the patterns associated with White flight are significantly 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 social interaction with neighbors are less negatively correlated with the neighborhood Black share.

In order to further understand moving behavior, we estimate our benchmark model separately for those who recently moved and those who did not move between Wave 4 and 5. The results presented in column (3) indicate that exposure to Blacks in school translates into living in Blacker census tracts for respondents who actually move, i.e., they make an active decision to live in Blacker neighborhoods.[32] For the smaller group of respondents who do not move between wave 4 and 5 (column 4), we do not find any significant effect of Black exposure in school.

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

6. Conclusions

In this paper, we analyze 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 support the idea 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 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). This may help to reduce inequalities among the next generation, since children's neighborhoods are shown to be an important determinant of several long-term outcomes such as education, labor market outcomes and crime (e.g., Chetty et al., 2016; Chetty and Hendren, 2018a,b). Therefore, policies aiming to increase racial diversity in schools, such as redesigning school attendance boundaries, could help to reduce racial segregation and its negative welfare effects. If such policies result in large changes in racial compositions, however, they may also trigger more segregation in school (Currarini et al., 2009; Mele, 2020) or changes in school

[30] 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 compared to other neighborhoods where the individual could most likely have moved to.

[31] Given the imprecision of the estimates in Table 8, it is unclear how much exposure to Blacks in childhood would be needed to fully offset the negative influence of the proportion of Blacks in the neighborhood. However, the estimates suggest that the changes in racial composition that we observe in our data are not large enough to do so.

[32] We do not find any impact of cohort Black shares on whether or not respondents move between Waves 4 and 5—results are available upon request to the authors.

choice (Monarrez, 2023). The results in this paper suggest that inducing smaller variations might provide a way to solve the potential trade-offs between school choice in the short-run and long-run behavior. However, more work is needed to better understand these trade-offs. One potential implication of our results is that contact with a relatively small number of Blacks can translate into significant changes in residential behavior later in life. Hence, our results speak in favor of policies that aim at increasing interaction and friendships across racial lines in schools. An example would be introducing cooperative group works, which has to be shown to be effective in stimulating interracial contact (Banks, 2017).

A final 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.

Declaration of competing interest

The authors declare that they have no known competing interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The authors do not have permission to share data.

Appendix A. Attrition

Since a significant fraction of the original sample is not included in our main regressions, it is worth considering whether there is any link between our treatment variable of interest and attrition. In Table A.1, we take as dependent variables indicators for whether, for a given White individual in Wave 1, we observe information on their residential census tract in subsequent waves. In every wave, the coefficients on our terms of interest are insignificant, suggesting that there is unlikely to be a systematic relationship between the treatment variable and attrition in the sample.

We provide some additional tests of robustness to attrition concerns in Table A.2. For reference, we report our baseline results in columns (1) and (2). We then run the same regression, still using the census tract Black share in Wave 5 as dependent variable, on the subset 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 categorized 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 corresponding coefficients reported in columns (3) and (4) are smaller, suggesting that, if those who never respond share characteristics with those who do not respond on the first contacting, then if anything attrition may be biasing our results downwards. To investigate this further, we run two regressions: one on whether someone observed in Wave 1 drops out by Wave 5, and another on whether someone requires a follow-up. Both regressions use all variables from the balancing test in

Table A.1
Attrition across waves.

Dependent variable:	Observe wave 2 census tract Black share		Observe wave 3 census tract Black share		Observe wave 4 census tract Black share		Observe wave 5 census tract Black share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grade Black share, both genders	0.284 (0.177)		0.110 (0.207)		−0.103 (0.210)		−0.275 (0.258)	
Grade Black share, same gender		0.0720 (0.101)		0.0349 (0.140)		−0.0747 (0.117)		−0.109 (0.162)
Grade Black share, opposite gender		0.174 (0.128)		0.107 (0.127)		−0.0511 (0.155)		−0.222 (0.166)
Observations	11 999	11 999	11 999	11 999	11 999	11 999	11 999	11 999
Adjusted R^2	0.279	0.279	0.0390	0.0390	0.0309	0.0309	0.0392	0.0392
Dep. var mean	0.717	0.717	0.731	0.731	0.780	0.780	0.592	0.592

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 is all White individuals in Wave 1 that we can link to data on their grade composition. The dependent variables take the value 1 if an individual we have data on their location in the respective wave. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table A.2
Robustness relating to attrition.

	Whole sample		Excluding those requiring follow-up		Whole sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Grade Black share, both genders	0.190** (0.0752)		0.189** (0.0763)		0.0726 (0.126)	
Grade Black share, same gender		0.194*** (0.0565)		0.164*** (0.0530)		0.180** (0.0898)
Grade Black share, opposite gender		0.0128 (0.0564)		0.0483 (0.0554)		−0.100 (0.102)
Sample weights					Y	Y
Observations	7090	7090	6448	6448	7090	7090
Adjusted R^2	0.187	0.188	0.185	0.186	0.245	0.247
Dep. var mean	0.0819	0.0819	0.0811	0.0811	0.0855	0.0855

Notes: Columns (1) and (2) are identical to the columns (1) and (2) of the baseline Table 4 and are provided for reference—please see notes to that table for further details. In columns (3) and (4), we then 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 4 except that observations are weighted using the sampling weights provided by Add Health. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 2 as predictors.[33] In doing so, we find that those not in our sample are likely to have similar characteristics to those requiring a follow-up. It is therefore reasonable to hypothesize that adding those who attrited to our sample would have a similar impact to moving from a regression where these non-first time respondents were not included (i.e., columns 3 and 4 of Table A.2) to our baseline sample (i.e., columns 1 and 2 of Table A.2).

In columns (5) and (6) we return to the full sample, but weight our regressions using the Wave 1-Wave 5 weights provided by Add Health. While standard errors get larger, consistent with their being substantial variation in weights, the estimates of our main coefficient of interest (the same gender grade Black share) do not change in any meaningful way. Overall, we conclude that adjusting for attrition based on observables does not change the results in important ways.

Appendix B. Additional summary statistics

Tables B.1 and B.2 present summary statistics for the variables used in the balance table, measured in wave 1, and in the main analyses split by the number of observations in school. Fig. B.1 then shows the distribution of the Wave 1 same gender Black share for individuals in our sample, while Fig. B.2 illustrates the distribution of the Wave 5 census tract Black share. Fig. B.3 illustrates the correlations

[33] Results are available on request.

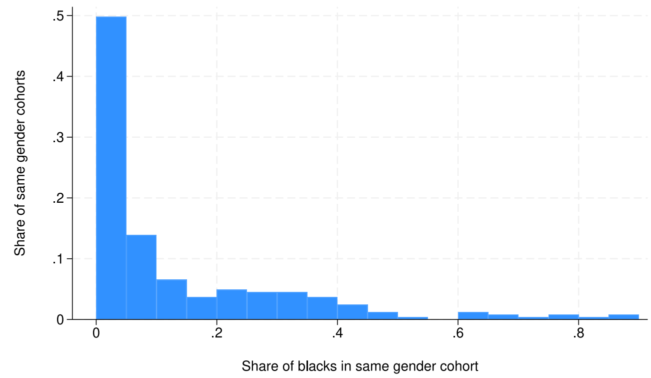

Fig. B.1. Distribution of Wave 1 same gender Black share.

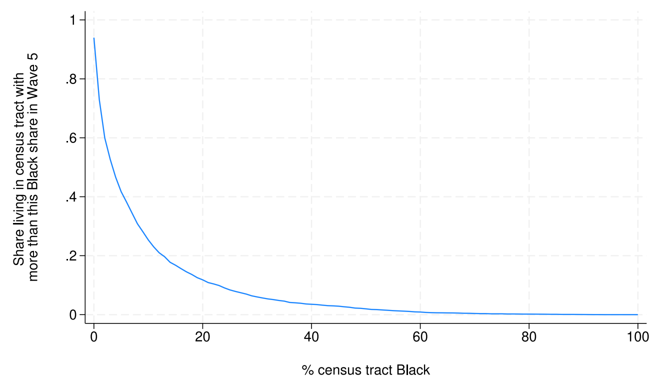

Fig. B.2. Distribution of Wave 5 census tract Black shares in sample.

Table B.1
Summary statistics of variables in balance table.

	Mean	SD	Min	Max	N
Age	16	1.7	12	21	7090
Parent is Black	.0022	.047	0	1	6350
Share of census tract Black	.055	.11	0	.95	7034
Share of census block Black	.047	.11	0	1	7030
Grade size	224	157	2	697	7090
Share same gender	.51	.07	.23	1	7090
Born in USA	.96	.2	0	1	7090
Lives with both biological parents	.61	.49	0	1	6326
Number of older siblings	.77	1.1	0	13	7081
Years of parental schooling	14	2.2	8	17	6818
Log of family income	3.7	.75	0	6.9	5652
Home language is not English	.061	.24	0	1	7090
Predicted Wave 5 tract Black share	.082	.027	−.035	.29	7090

Table B.2
Comparison of schools by number of observations.

	Mean, large schools	Mean, small schools	Within school s.d., large schools	Within school s.d., small schools
Main variables				
Share of census tract Black, Wave 5	.068	.095	.094	.11
Share of census tract Black, Wave 1	.025	.082	.034	.072
Grade Black share, both genders	.043	.11	.01	.017
Grade Black share, same gender	.043	.11	.015	.028
Other Wave 1 variables				
Age	17	15	1.3	1
Female	.55	.56	.5	.46
Hispanic	.13	.12	.17	.2
Family income (000's)	52	52	41	31
Grade size	257	193	32	21
School size	1034	621	0	0
Grades in school	4.5	3.7	0	0
In middle school	0	.42	0	0
In high school	.76	.42	0	0
Lives in urban area	.41	.5	.25	.14
Region = Northeast	.13	.24	0	0
Region = Midwest	.43	.19	0	0
Region = South	.25	.43	0	0
Region = West	.19	.14	0	0
Moving related variables				
Moved house between Waves 1 and 5	.93	.92	.23	.26
Moved house between Waves 3 and 5	.92	.9	.25	.27
km between Wave 1 and Wave 5 location	331	367	659	674
km between Wave 3 and Wave 5 location	318	361	665	709

Notes: This table gives summary statistics for the subsample of the 29 schools in our sample with the most observations and the remaining 97 schools with fewer observations. These 29 large schools contain around 50% of the observations in the small sample.

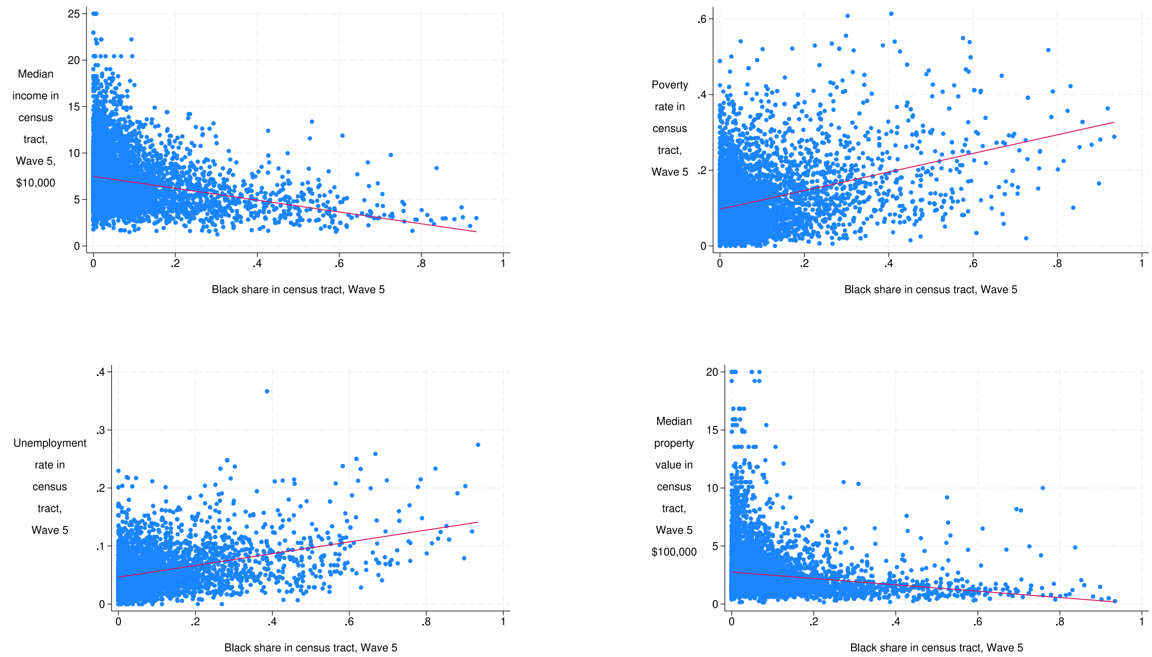

Fig. B.3. Correlations between census tract Black share and other variables. *Notes:* Each point represents a census tract where an individual in our baseline sample lives in Wave 5. The red line is the line of best fit.

between census tract Black share in Wave 5 and other neighborhood characteristics.

Appendix C. 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' $\widehat{Black_i}$ defined as

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

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 C.1 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.

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 controlling for the Black share among all other students in the school in the regression against the Black dummy. Columns (3) and (4) of Table C.1 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 C.1 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. Finally, in column (6), we use the same sample as in our main analysis and regress the grade Black share on the shares in the grades above and below and find no significant correlation.

Next, 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 a disproportionate dropout rate for Blacks. Hence, we plot in Fig. C.1 the distribution of the change in Black share between each grade and the previous grade in each school. The figure displays no obvious asymmetry, and indeed the mean change in grade Black share is −0.0005792. We also plot here the residuals of the grade Black share after we regress on school-gender and grade-gender fixed effects, since this is the variation that is used in giving our main results.

A final check is to compare the actual variation in Black shares within schools with the variation one would expect to see from a standard binomial distribution where every child is chosen randomly to be Black or not based on the average Black share in the school. Plotting this 'expected' within school standard deviation against the observed within school deviation in Fig. C.2 shows that there are few schools with a variation much larger than we would expect. This supports that, as in Goldman et al. (2024), the variation in the Black share within schools we exploit is driven by idiosyncratic changes in the gender and racial composition of birth cohorts in school districts.

These tests of random variation therefore accord with the fundamental assumption behind our identification strategy—i.e., that parents do not 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, does not prevent

Table C.1
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 in grade (6)
Black share of others in grade	0.149 (0.209)		0.00920 (0.413)			
Black share of others of same gender in grade		0.00602 (0.0988)		−0.138 (0.217)		
Black share of opposite gender in grade		0.0208 (0.0927)		−0.0337 (0.233)		
Black share of others in school			−98.69*** (23.15)	−101.8*** (22.75)		
Black share of females in grade					0.0616 (0.0791)	
Black share in grade above						−0.00953 (0.00752)
Black share in grade below						−0.0223 (0.0253)
Observations	81 780	81 778	81 780	81 778	80 837	7090
Adjusted R^2	0.999	0.394	0.395	0.398	0.979	0.976

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

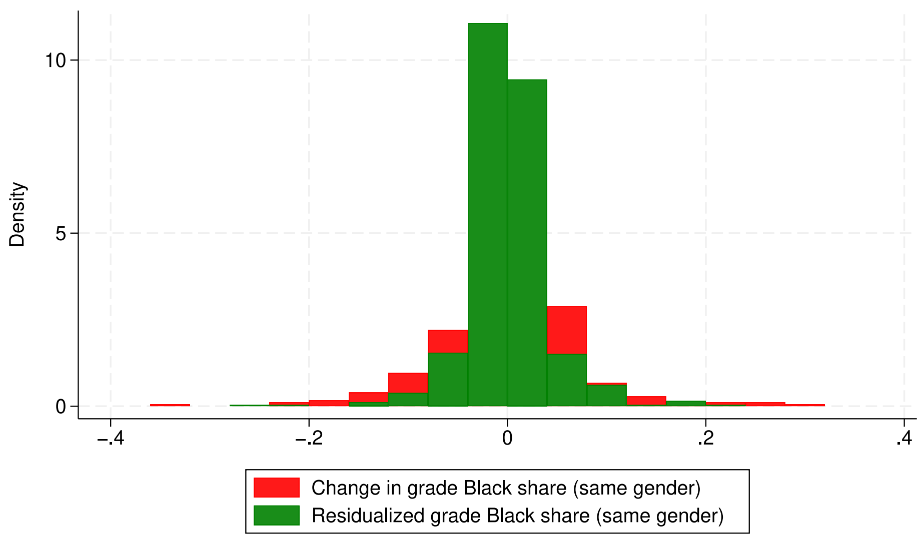

Fig. C.1. Histogram of Black share change and residual.

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 C.2. 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 did not switch school, and indeed those that did not switch are more exposed, it is reassuring that our result is driven by those who did not 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 do not 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.

Appendix D. Alternative specifications

In this appendix we carry out a number of alternative specifications to understand our main result further. First, in columns (1) and (2) of Table D.1 we explore how the inclusion of school-gender FEs impacts the results (with column (1) being our baseline specification). Taking out school-gender FEs increase the coefficients on the grade Black shares. This is consistent with a number of school-level selection issues discussed earlier, such as schools with a greater number of Blacks existing in areas with more Black residents.

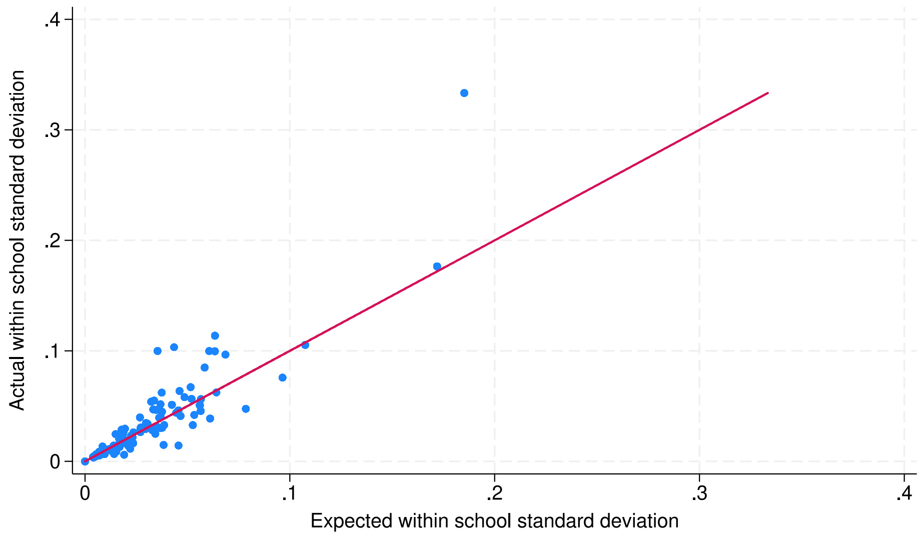

Fig. C.2. Within-school standard deviations in cohort Black shares.

Table C.2
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 (0.00782)	−0.00159 (0.00783)
Grade Black share, both genders × Did not switch school			0.168** (0.0841)	
Grade Black share, both genders × Switched school			0.0896 (0.0932)	
Grade Black share, same gender × Did not switch school				0.164*** (0.0591)
Grade Black share, same gender × Switched school				0.0222 (0.192)
Grade Black share, opposite gender × Did not 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. * $p < .10$, ** $p < .05$, *** $p < .01$.

One 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 do not 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, which we now address.

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 (3) of Table D.1. 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 (4) 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 D.1
Alternative specifications.

Sample:	Full sample				Without a Black parent	Not in grade 12 in Wave 1	Schools with limited variation	Full sample, split schools
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grade Black share, same gender	0.194*** (0.0565)	0.235*** (0.0333)	0.185*** (0.0531)		0.192*** (0.0556)	0.152*** (0.0558)	0.223*** (0.0839)	0.178*** (0.0545)
Grade Black share, opposite gender	0.0109 (0.0557)	0.0944*** (0.0345)	0.0104 (0.0512)		0.00170 (0.0589)	0.0112 (0.0598)	0.0775 (0.0758)	0.0189 (0.0596)
Identifies as Black, Wave 5			0.0984*** (0.0254)	0.101*** (0.0255)				
Block Black share, Wave 1			0.138*** (0.0237)	0.138*** (0.0236)				
Blacks in grade, same gender				0.000771* (0.000449)				
Blacks in grade, opposite gender				−0.000738 (0.000562)				
School-gender FEs	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7090	7090	7090	7090	7076	5966	4025	7062
Adjusted R^2	0.189	0.109	0.201	0.200	0.188	0.186	0.216	0.171
Dep. var mean	0.0819	0.0819	0.0819	0.0819	0.0817	0.0822	0.0795	0.0817

The table reports OLS estimates controlling for grade size, language spoken at home in Wave 1, grade-gender fixed effects, and (except in column (2)) school-gender fixed effects. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$.

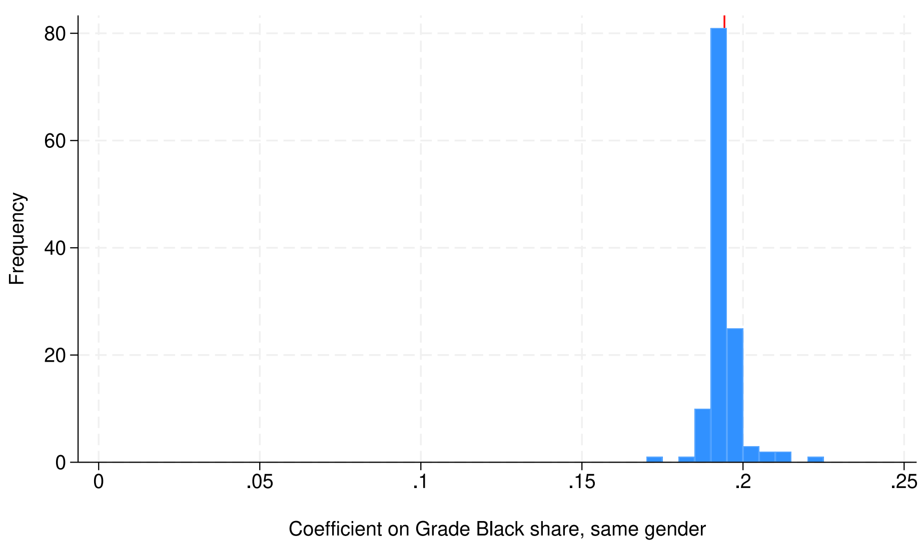

Fig. D.1. Results leaving one school out at a time.

In columns (5) and (6) of Table D.1 we test the robustness of our result to removing two types of individual. In column (4), we remove those who have one parent who reports as Black, while in column (5) we remove individuals who are in grade 12 in Wave 1. This latter regression tests whether our results may be driven by differential dropout of Blacks which is likely to occur in grade 12. In both columns results are similar to our baseline regression.

One might potentially be concerned that our results are driven by schools with large changes in racial makeup which occur, for example, because of redrawing of school districts, changes in catchment areas, or the construction of a large building complex. To take this concern into account, in column (7) of Table D.1, we exclude all schools with a higher variation in the Black Share than expected. We define these as schools having a standard deviation greater than the one from a standard binomial distribution where every child is chosen randomly to be Black or not based on the average Black share in the school (see Fig. C.2). Doing so, we again get a highly significant point estimate, which is even slightly larger in size than when using the full sample of schools. In column (8), we report the results when we instead keep these schools in the sample, but instead 'split' schools in two when they experience a jump in grade Black share above the expected standard deviation. Reassuringly, running our model on this artificially broken-down sample yields an estimate close to our benchmark estimates. We are therefore confident that our results are not driven by abnormal cases of within-school changes in Black shares due to redrawing of school districts, changes in catchment areas or other exceptional events.

Finally, we document in Fig. D.1 that our results remain consistent and the coefficient of interest changes minimally when we drop one school at a time. Therefore, we can rule out the possibility that potential concurring shocks affecting the racial composition happening in a single school are the main drivers of our findings.

Appendix E. 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 (2) 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

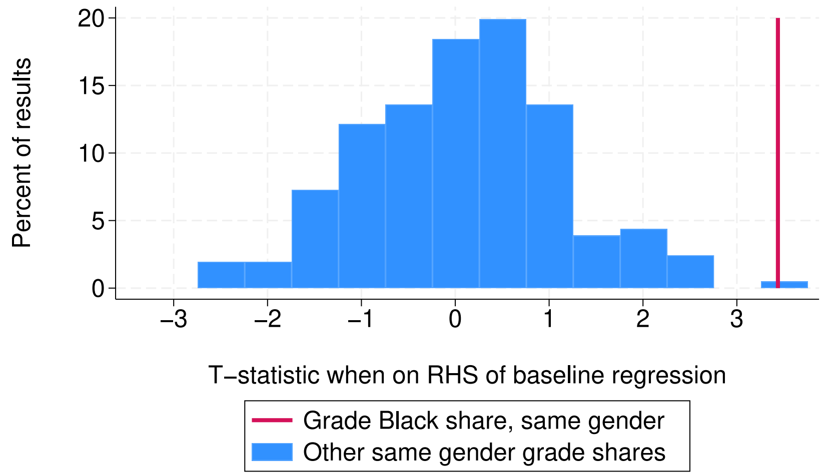

Fig. E.1. Other shares on RHS.

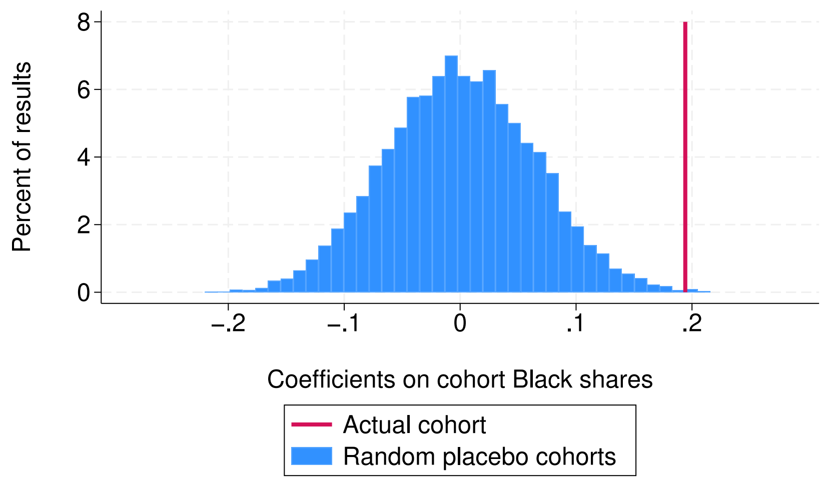

Fig. E.2. Distribution of coefficients from regressions on randomly assigned cohort shares.

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 Fig. E.1. 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 (2) for each assignment of cohort shares and repeat this exercise ten thousand times. This produces a distribution of coefficients, which is reported in Fig. E.2 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.

Appendix F. Heterogeneity

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

A first obvious variable to examine is the school Black share, as we might think that the effect is different in schools with few versus a lot of Black students. In column (1) of Table F.1, we therefore interact our treatment variable with the school Black share. The coefficient is negative, suggesting that the effect is smaller in schools with many Blacks, where marginal increases in Black students do not translate necessarily into additional social contact. The coefficient however is not significant. To check whether this is due to a non-linear effect, in Fig. F.1 we plot the coefficient of same gender grade Black share derived in estimating Eq. (2) when excluding schools where such share is larger than x percent, where x varies between 0 and 100. Hence, the coefficient for $x = 100\%$ is the one of the baseline regression. The figure clearly shows that the coefficient is larger when we exclude schools where such share is larger than 15%.[34] If we interact the share of same gender Blacks in the grade with a dummy that takes value 1 if the school Black share is larger than 15%, we find a negative and significant coefficient. Our interpretation of this finding is that an increase in grade Black share has a significant impact only if it translates into additional contact with Blacks. In schools where there is a large share of Blacks, there is probably already more contact with Blacks, so the effect is smaller in these schools.

[34] Note also that the coefficient is not precisely estimated for small values of x, which is expected given the smaller number of underlying observations.

Table F.1
Interactions.

Interaction term:	School Black share	School Black share >15%	School Black segregation	Republican vote share in 1992	School urban share	Students in grade
Dependent variable: Tract Black share						
	(1)	(2)	(3)	(4)	(5)	(6)
Grade Black share, same gender	0.271** (0.112)	0.350*** (0.115)	0.169 (0.137)	0.185*** (0.0710)	0.210*** (0.0721)	0.237** (0.0941)
Grade Black share, opposite gender	0.113 (0.111)	0.0615 (0.108)	−0.0385 (0.115)	0.0152 (0.0647)	0.00609 (0.0638)	0.102 (0.0910)
Same gender × interaction term	−0.403 (0.422)	−0.235* (0.140)	0.0384 (0.264)	0.202 (0.788)	−0.109 (0.163)	0.000875 (0.000689)
Opp. gender × interaction term	−0.552 (0.430)	−0.104 (0.132)	0.0596 (0.225)	−0.215 (0.854)	0.0763 (0.144)	−0.00000158 (0.000599)
Observations	7090	7090	7022	7050	7082	7090
Adjusted R^2	0.160	0.180	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. * $p < .10$, ** $p < .05$, *** $p < .01$.

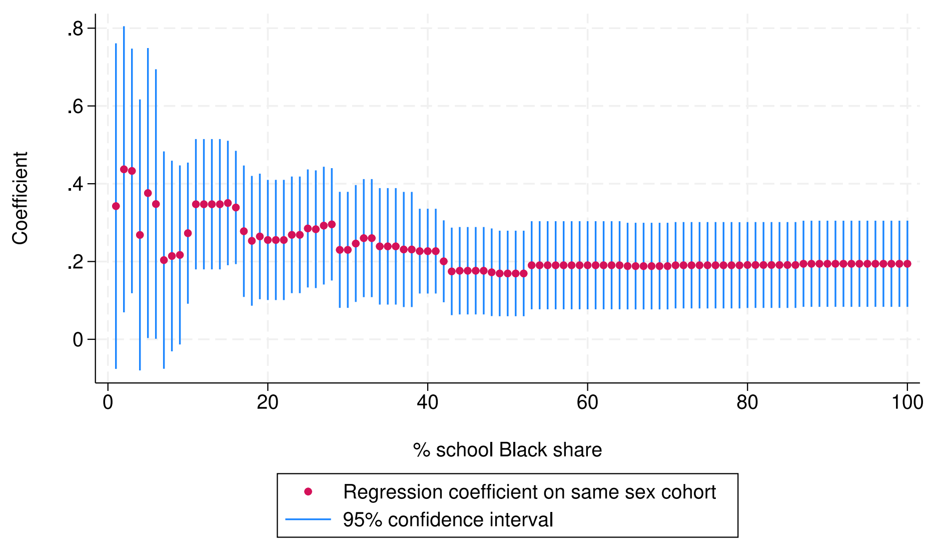

Fig. F.1. Results when excluding schools with larger Black shares. *Notes:* The figure plots OLS coefficients and 95% confidence intervals of the same gender grade Black share in our baseline specification when we exclude schools with a school Black share larger than x%. The value at $x = 100$ is therefore the same as in the baseline, i.e., using the full sample, with the sample we use becoming smaller as we move to the left of the graph.

Columns (3) to (8) of Table F.1 reports the result of interacting the two treatment variables with the level of segregation of the school calculated using the methodology proposed by Echenique and Fryer (2007),[35] 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.

We then run our baseline regression for different subsamples, always including grade-gender and school-gender fixed effects. The results and the p-values of the tests comparing the coefficients on the different samples are reported in Table F.2. Columns (1) and (2) divide the sample by the number of observations in schools, while columns (3) to (6) divide it by region. We find that larger schools have a significantly larger coefficient. One reason could be that larger school have less Black students to begin with (see Table B.2), so that increases in the share of Black students have a larger impact on the probability of interracial contact. This is in line with the findings presented in column (2) of Table F.1 and in Fig. F.1.

Columns (3) to (6) show that schools in the North-East region have 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.[36] 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 result appears significantly larger (at the 10% level) in the set of schools in more segregated counties. Note, however, that given we are measuring county-level segregation with error and

[35] The Spectral Segregation Index proposed by Echenique and Fryer (2007) is calculated recursively using the racial composition of same-gender friendship networks. It measures the connectedness of individuals of the same group counting how many of one's friends are of the same group, how many of their friends are of the same group and so on. It should be noted, however, that this measure of segregation is calculated using friendship networks. These networks are not segregated by cohort, so they are affected by Black shares in grades above and below the respondent's (e.g., see Figure 1 in Currarini et al., 2009). As a result, they are likely to be endogenous, and this segregation measure is correlated with many other variables. This implies that the estimates obtained adding this index should be interpreted with caution.

[36] 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 among the tracts that we observe, using Blacks and non-Blacks as our two groups.

Table F.2
Subsample splits.

	Observations in school		Region				County segregation	
	Below median	Above median	North-east	Mid-west	South	West	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grade Black share, same gender	0.149** (0.0646)	0.357*** (0.0916)	−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	−0.00951 (0.0688)	0.0597 (0.116)	−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	.06		.04				.03	
Observations	3669	3421	1298	2179	2413	1192	4941	2149
Adjusted R^2	0.227	0.0943	0.0558	0.106	0.186	0.0859	0.244	0.0665
Dep. var mean	0.0951	0.0677	0.0545	0.0602	0.122	0.0706	0.0855	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 F.3
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. * $p < .10$, ** $p < .05$, *** $p < .01$.

our sample is not representative at the regional level, this difference between regions should be interpreted with caution.

We also may wonder to what extent the effect depends on the characteristics of the Black children to whom the White children are exposed. 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 F.3. 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) to (6), we therefore split Blacks according to two measures correlated with performance—whether the father is present in the household, and whether their mother went to college. In all of these regressions, we do not 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.

Appendix G. Additional results related to liberalness and neighborhood satisfaction indices

In Tables 7 and 8 in Section 5, we explored the relationship between school exposure, tract Black share, an index of stated liberalness, and an index of self-reported neighborhood satisfaction. The index of stated liberalness is constructed from three components: we separately regress each of them on our variables of interest in Table G.1.

Table G.1
Impact on (non-standardized) components of stated liberalness index.

	Race is not important in relationships, Wave 3	Declared liberalness, Wave 4	Declared liberalness, Wave 5
	(1)	(2)	(3)
Grade Black share, same gender (S)	0.199 (0.217)	0.317 (0.284)	0.378 (0.327)
Grade Black share, opposite gender (O)	0.0542 (0.203)	0.271 (0.231)	−0.317 (0.241)
Observations	5904	6372	7090
Adjusted R^2	0.0529	0.0740	0.0780
Dep. var mean	0.623	−0.989	−2.074

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 variable in column (1) takes a value of 1 if race is declared as being less important than any other aspect of a relationship, and 0 otherwise. The variables in columns (2) and (3) measure liberalness on a 3-point scale, taking the value 1 if the respondent declares to be liberal, −1 if they declare to be conservative, and zero otherwise. (Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table G.2
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 5.3 for details. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table G.3
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 (0.647)	0.146 (0.482)	0.0211 (0.382)	−0.217 (0.499)	0.218 (0.525)	0.912* (0.494)	−0.0556 (0.725)
Grade Black share, opposite gender (O)	−0.280 (0.454)	−0.432 (0.464)	−0.548 (0.385)	0.427 (0.411)	−0.355 (0.485)	0.672 (0.415)	0.797 (0.543)
Relative tract Black share, Wave 2 (R)	−8.081*** (0.639)	−5.402*** (0.637)	−0.339 (0.746)	−3.303*** (0.695)	−4.593*** (0.621)	2.536*** (0.661)	−0.518 (0.724)
S × R	13.95*** (4.017)	4.189 (3.096)	9.358** (3.788)	−1.127 (4.728)	8.345* (4.297)	6.608 (3.988)	9.189 (5.828)
O × R	−3.011 (5.058)	5.648 (4.327)	−2.677 (6.925)	6.863 (4.655)	8.484 (5.963)	0.520 (4.555)	0.695 (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) 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—the coefficients reported for these variables are therefore the marginal effects at the sample means. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$.

In Table G.2, we correlate the index of stated liberalness constructed from these three components 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).

The index of neighborhood satisfaction 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 G.3 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.

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