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Immigrant peers in the class: responses among natives and the effects on long-run revealed preferences[a]

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Abstract

We investigate whether exposure to immigrant peers at school affects natives' future interactions with ethnic minorities. Identification is based on variation in immigrant exposure across cohorts within school catchment areas in Sweden. We document that natives respond to immigrants by changing school and develop an IV strategy that accounts for such endogenous responses. Our results show that minority exposure at the extensive margin increases the probability that natives form inter-ethnic romantic partnerships, which is suggestive of altered preferences for interacting with immigrants. We also find that minority exposure affects women's educational choices and family formation decisions in a family-oriented direction.

Keywords: contact hypothesis, peer effects, intermarriage
JEL-codes: J12; J15; I2

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Table of contents

1 Introduction

Recent migration flows from non-Western countries to Europe have intensified the discussion around effective integration policy (ILO 2016; OECD 2016). Most of the relevant academic literature focuses on the effects of immigration on labor market outcomes of natives, the economic integration of immigrants (see e.g. Dustmann et al. 2017 and Peri 2016 for overviews), and the opportunities and school outcomes of native and immigrant children (Gould, Lavy, and Paserman 2004; Gould, Lavy, and Daniele Paserman 2009; Geay, McNally, and Telhaj 2013; Figlio and Özek 2019). In the long run, however, the development of countries with large immigrant minorities will also depend on attitudes, preferences and behaviors among natives.

In this paper, we return to Allport's (1954) 'contact hypothesis' which states that inter-group contact across race or ethnic groups can reduce majority group prejudice. We are interested in whether childhood exposure to ethnic minority peers in school affects natives' attitudes towards the minority group. This question is key to understanding the long-run development of societies that host large immigrant minorities, and informative for policy areas such as refugee placement and student-school assignment rules. The setting is Sweden, a country where 19 percent of the population is foreign-born, i.e., immigrants. This group is predominantly made up of refugees and family members of refugees, today originating primarily from the middle-East (Iran, Iraq and Syria) (SCB 2019). The immigrant population thus represents a non-trivial ethnic minority.

To assess natives' attitudes towards ethnic minorities, we focus on long-run outcomes that are observed in register data and capture revealed preferences for interacting with minority groups. Our main outcome of interest, observed in mid-life, is inter-ethnic family formation. As suggested by Kalmijn (1998) and Fryer (2007), integration in the most intimate spheres of life, such as cohabitation and residential neighborhood, are true reflections of the majority's attitudes towards minorities, and shows that members of different groups accept each other as social equals. We complement this outcome by also studying how childhood exposure to minorities affects the ethnic composition of the schools to which individuals send their children. That is, does the experience of having immigrant peers in school affect parents' revealed preferences for their own children's interactions with immigrants at school? In addition to capturing revealed preferences for interacting with minority group members, these outcomes constitute broad measures of the joint integration of immigrants and natives in a society, which reflect inter-group contact opportunities and social and economic integration. For example, exposure to immigrant peers in school could affect human-capital related outcomes and thereby alter both future academic paths and career paths, as well as family-formation decisions. We therefore carefully explore the mechanisms that explain our findings, in order to understand whether they are a likely result of a change of attitudes or should be attributed to other factors. We rely exclusively on register data which allows us to study observed behavior, and therefore circumvent the problem of misreporting bias in survey-based studies on attitudes.

The paper studies a population of native-born students and exploits idiosyncratic variation in exposure to school-cohort peers with non-Western origin in grade 4–9, building on a strategy first introduced by Hoxby (2000).[1] Because of the strict enforcement of residence-based school assignments in Sweden in the early 1990s, we can use within-school variation across cohorts in the *expected* share of non-Western peers as an instrument for actual shares in grade 9, where expected shares are calculated based on students' residential address at age 10, 6 years before graduation. This method allows us to alleviate concerns that natives respond to immigrant inflows by changing school or moving to a new neighborhood (Dustmann and Preston 2001), and at the same time exploit variation in peer composition over a period of six years (grades 4–9), which implies a significant treatment duration. Expected peer composition represents peers in the catchment area students belonged to at age 10 (grade 4), before potential endogenous moves in response to immigrant inflows in the remaining six years of compulsory education. Importantly, exposure at age 10–15 is particularly relevant in this context since early adolescence is a key period for attitude and identity formation (Backes and Backes 2019).

Our empirical strategy, similarly to Merlino, Steinhardt, and Wren-Lewis (2019), relies on the theoretical assumption that social contacts among young students mostly occur within gender, i.e., girls (boys) interact more with other girls (boys) than with boys (girls).[2] Our specification therefore includes exposure to same- vs. opposite sex minority

[1] Many papers have followed the strategy introduced in Hoxby (2000), see for example Lavy and Schlosser (2011); Lavy, Silva, and Weinhardt (2012); Carrell, Hoekstra, and Kuka (2018); and Cools, Fernández, and Patacchini (2019).
[2] Intra-gender friendship is more common than inter-gender friendship according to studies by McPherson and Smith-Lovin (1987) and Kalmijn (2002).

peers as separate regressors. In addition, we explore the extensive and intensive margins of exposure, since variations in the share of minority peers might have very different impacts on attitude formation depending on the baseline exposure level. When students have no prior experience of minority students, exposure might lead them to update their beliefs, while additional exposure in immigrant-dense environments is less likely to have such an impact.

We find a positive effect of immigrant exposure in school on future inter-ethnic romantic partnership formation. The result is only present at the extensive margin, indicating that individuals with little previous interactions with the minority group are the ones affected. This result is corroborated by our analysis of the school choices that treated individuals later make for their own children. Specifically, we find that childhood exposure to non-Western peers at the extensive margin increases the likelihood that individuals place their children in schools with a higher share of non-Western students. These results are driven by women only and are not exclusively explained by exposure to immigrant girls.

Our findings are in line with two recently published US-based studies about long-term effects of peers of opposite race during school on biracial relationships: Merlino, Steinhardt, and Wren-Lewis (2019) find a positive effect of being exposed to black peers in school on white American's future self-reported interracial romantic relationships and Shen (2018) finds that interracial births is more common among black women who have grown up in a less race segregated school district. We can confirm their findings in a very different context (ethnic groups in Europe), but also show that they extend to other types of behavior.

Our study makes two additional contributions. First, we exploit rich Swedish register data to shed light on mechanisms. We focus on human capital outcomes, which are informative of altered educational paths that might have impacted marriage market opportunities. In addition, we study peer networks and direct links between exposed children and their future partners (e.g., whether they attended the same school). We find that natives exposed to immigrant peers have a lower probability of attending an academic track in high school, and an increased probability of having a child, suggesting that exposure to peers with non-Western origin alters both career paths and family formation decisions. We find that students' GPA is unaffected, and we find no effect on the probability of having a partner from the same school, nor on the share of immigrants in the field of study that the students choose or in the residential neighborhood where they live as adults. Taken together, these results show that exposure to minority peers has altered women's educational choices and family formation decisions in a more family-oriented direction, but there is no evidence that the composition of their future peer networks, that is, the supply of potential minority partners, has changed. This result is consistent with a change of attitudes, but not conclusive when it comes to prejudice towards the minority group. A possible interpretation is that natives incorporate more family-oriented values from immigrant families, and at the same time become more inclined to have close interactions with people of non-Western origin.

Second, our empirical strategy removes bias from endogenous reactions to immigrant exposure by exploiting expected variation based on default school assignments. Using variation at the catchment area level, we can explicitly address whether parents of treated children react by moving their child to a new school or by moving to a new residential area. We find empirical support for white flight-type behavior: when the share of immigrant students of the same (not opposite) gender increases on the intensive margin, parents are more likely to move their children to another school or change residential address. However, parents do not seem to react when immigrant exposure increases at low baseline levels. These findings are in line with the tipping-point literature (Card, Mas, and Rothstein 2008; Böhlmark and Willén 2020).

The key contribution of this paper is to establish a causal relationship between exposure to immigrant peers in childhood and revealed preferences for social interactions with immigrants in the future. In addition, we are able to both address and take into account concerns that natives respond to immigrant inflows by changing school or moving to a new neighborhood, which have been raised in earlier papers (see e.g. Dustmann and Preston 2001). Considering recent migration trends in Europe, it is important to understand how interactions between majority and minority groups (i.e., natives and immigrants) affect integration processes. Evidence from black-white interactions in the U.S. might not be informative for the European setting. In particular, immigrant-native relations in Europe are characterized by religious and language barriers, and the norms that maintain endogamy appear to be strong (Kalmijn and Van Tubergen 2010).

The remainder of the paper is organized as follows. Section 2 gives a brief background to the literature about the contact hypothesis, section 3 describes the main outcome of interest, inter-ethnic partnership and how it has evolved over time. Section 4 presents data and descriptive statistics, and section 5 lays out the details of the empirical strategy. Section 6 presents the results and section 7 concludes the paper.

2 Earlier literature

The origins of prejudice and negative attitudes towards minority groups is a central theme in the social sciences, and of increasing policy importance in Western economies that have experienced large inflows of migrants from non-Western countries. Prejudice against minorities may lead to segregation and slow down migrants' integration in the host country.[3] Contact theory however states that attitudes can be altered under certain conditions: interpersonal contact across race/ethnic groups can reduce prejudice if characterized by equal status within the contact context, common goals, intergroup cooperation, support from authorities, and personal interactions (Allport 1954). We believe that compulsory school fulfills these criteria and thereby offers a relevant setting for testing the contact hypothesis.

Empirically, the predictions from the contact hypothesis are hard to verify. It is difficult to disentangle the effect of social interactions across ethnic groups from potential selection, i.e., the reason for why they have interacted in the first place. Paluck, Green, and Green (2018) survey the most recent studies that rely on random assignment and find that despite positive effect sizes, for several reasons, the jury is still out when it comes to the contact hypothesis. Studies with good precision tend to indicate the smallest effects and publication bias might lead to underreporting of non-significant results; studies on racial/ethnic prejudice tend to generate weak effects; and none of the reviewed studies focuses on attitudes of adults above age 25.[4]

Several of the most relevant studies of the contact hypothesis rely on evidence from random assignment of roommates/dormmates at university campuses, and show that white students exposed to greater numbers of black students express more favorable

[3] Several studies provide evidence that immigrants face discrimination in the labor market, see e.g. Carlsson and Rooth (2007) and Eriksson, Johansson, and Langenskiöld (2017).

[4] The review by Paluck, Green, and Green (2018) is a follow-up of Pettigrew and Tropp's (2006) influential meta-analysis, which summarized mainly studies based non non-random variation in group exposure.

attitudes towards blacks and interact more with black students during or closely after the exposure period (Boisjoly et al. 2006; Marmaros and Sacerdote 2006; Camargo, Stinebrickner, and Stinebrickner 2010; Dobbie and Fryer 2015; Corno, La Ferrara, and Burns 2019; Carrell, Hoekstra, and West 2019).[5] However, this altered behavior seems to have limited, if any, effect outside the university environment (Baker, Mayer, and Puller 2011; Marmaros and Sacerdote 2006). Inter-group contact has also been found to affect attitudes between groups defined by socioeconomic status: Rao (2019) shows that rich Delhi students randomly exposed to poor classmates are less likely to discriminate against poor students, and more likely to socialize with them.

Few earlier studies address the effects of inter-group contact on attitudes and behavior in the very long run. One recent exception is Billings, Chyn, and Haggag (2021) who show that minority exposure in school affects long-run political preferences. When it comes to intermarriage as a proxy for attitudes, the contact hypothesis has some indirect support: Romano (2004) studies veterans who have experienced inter-racial interaction in their military service and shows that both black and white veterans have higher rates of intermarriage than non-veterans. Merlino, Steinhardt, and Wren-Lewis (2019) study the effect of racial diversity in school on assortative mating by race and find that a larger share of black students in grade 7–12 stimulates diversity in social interactions both within and outside the classroom. Importantly, this also increases the probability of interracial romantic relationships 10 years later. Gordon and Reber (2018) and Shen (2018) study the effect of court-ordered school desegregation of black and white children in the US during the 1970s on biracial births by exploring the variation in segregation across counties over time. Gordon and Reber (2018) focus on white women and conclude that the positive raw correlation between biracial births and school desegregation disappears when controlling for endogenous migration patterns among whites. Shen (2018) focuses on black women and shows that among those, school desegregation had no effect on mobility, and she therefore concludes that school desegregation actually increased biracial births among black women. From these studies we draw two conclusions: endogenous mobility is a caveat important to handle, and biracial/inter-ethnic births is a relevant outcome to study. The present paper is conceptually influenced

[5] Additional studies that investigate contact theory in similar settings include Finseraas and Kotsadam (2017) and Scacco and Warren (2018).

by these US-based studies, but with the important value added of studying a European context with focus on interactions between immigrants and non-immigrants. In addition, our register data allow us to investigate in more detail several alternative mechanisms that can explain the effect of immigrant peer exposure on the probability of ending up in a mixed-ethnic partnership.

3 Inter-ethnic families and the origin of non-native students

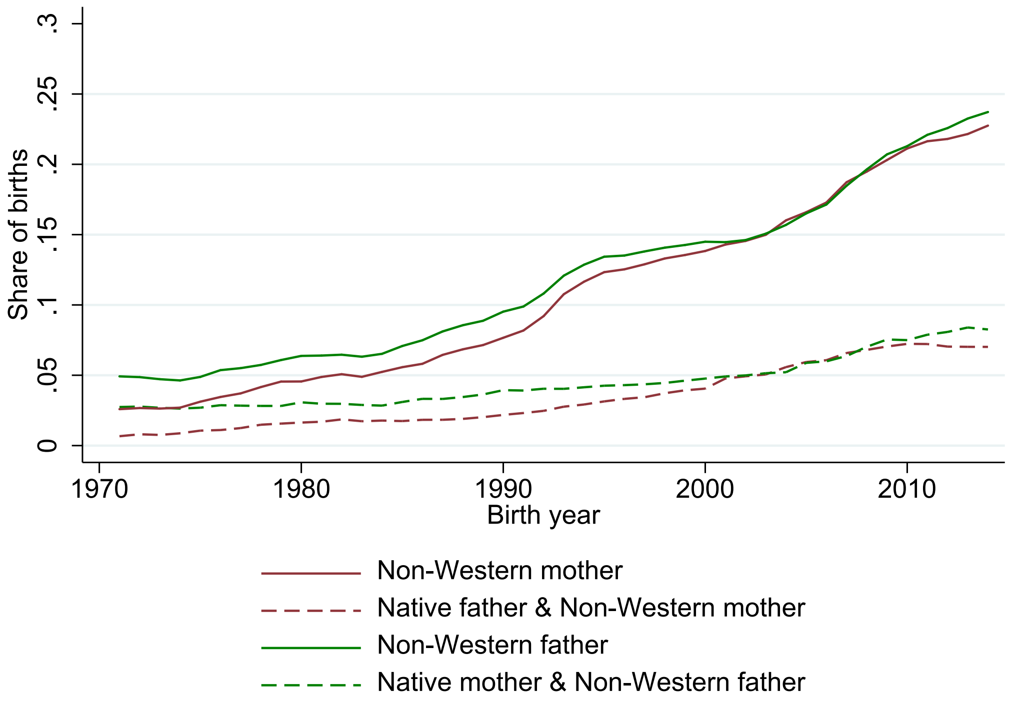
In this section we take a closer look at inter-ethnic partnership formation in Sweden today and over time. We also present descriptive statistics of the origins of non-Western students attending Swedish compulsory schools in our observation window.

Ethnic homophily, i.e., the principle that contact is more frequent among individuals with similar ethnic background, is present in most countries and cultures.[6] With regard to contact between natives and immigrants, Sweden is no exception.

Figure 1 shows inter-ethnic births and births to mothers and fathers with non-Western background, respectively, as shares of all births. We define inter-ethnic births as births to one native parent (born in Sweden with both parents Swedish-born), and one parent with non-Western background (born in a non-Western country or born in Sweden with both parents born in a non-Western country). Each observation has been weighted by the inverse of the total number of births of the mother to account for differential fertility patterns across groups over time. In 1970, about 5 (2.5) percent of all newborns had a father (mother) with non-Western background. In a little less than half of those cases, the other parent had similar background, as indicated by the difference between the solid and the dashed lines. With time, the population with non-Western background has grown. So has the share of inter-ethnic births, but not in proportion to the increase of the former, as illustrated by the widening gap between the dashed and solid lines. Relative to all births to parents with non-Western background, inter-ethnic births have thus become less common. This is likely explained both by an increased supply of in-group partners among non-Western immigrants, and compositional changes of the migrant population.

[6] Ethnic origin is one of the strongest bases for the more general concept of homophily (McPherson, Smith-Lovin, and Cook 2001).

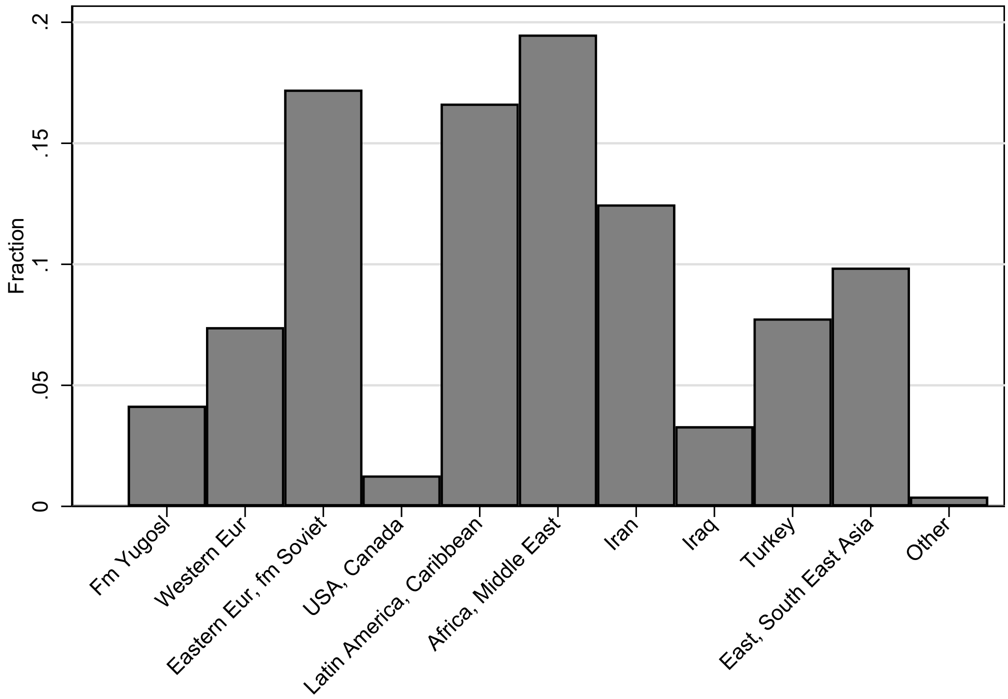
Figure 1 Share of all births in Sweden with non-Western mother/father or mixed-ethnic parents



Source: Own calculations based on all births in Sweden observed in the Swedish multi-generation register. A person is considered as having native background if they were born in Sweden to parents who were also born in Sweden. Non-Western background is defined as being born in a non-Western country or having parents who were born in a non-Western country.

Figure 2 reports the origins of foreign-born students attending Swedish compulsory schools in 1991–1994. These are the students to whom the native students in our analysis sample are exposed (but who are not themselves included in the analysis sample). At the time, non-native children in Swedish schools originated primarily from Latin America, Africa/the Middle East, and Eastern Europe/former Soviet. Although the composition of origin countries is somewhat different among more recent immigrants, there is still overlap with some of the most recent immigrant groups from North Africa and the Middle East.

Figure 2 Country of origin among parents to non-native origin students in grade 9, 1991–1994



Note: Adoptees and immigrants from other Nordic countries are excluded here.

4 Data and descriptive statistics

The analysis is based on Swedish universal registers covering all residents aged 16–74 in Sweden from year 1990–2016. We link parents to their children through a multi-generation register including all births in Sweden.

The population of interest is native students (born in Sweden to parents also born in Sweden) graduating from compulsory school in 1991–1994, which we observe in the compulsory school graduation register (grade 9, at age 16). We cannot observe students in other grades. To this population we merge the relevant treatment (exposure to non-Western peers), outcomes and background characteristics. The observation window of cohorts graduating 1991–1994 is chosen because i) for earlier cohorts we cannot observe residential neighborhood at age 10, and ii) for later cohorts we cannot observe completed fertility. However, for our purposes it is also useful to limit the analysis to a few cohorts since we can assume that they all face the same marriage market.

Treatment variables. The treatment variables are defined as the share of all girls (boys) in the school cohort with non-Western origin, that is, born in a non-Western country, or born in Sweden to two non-Western parents.[7] This definition ensures that interactions between our population (natives) and peers of non-Western origin are strictly inter-ethnic, i.e., we do not allow individuals with mixed origin to enter the definitions. We will refer to non-Western origin students as either non-Western, immigrant or minority students.

We observe our treatment, gender-specific shares of non-Western students at the school and cohort level in grade 9, when we observe students' school assignment at graduation.[8] Additionally, we construct our identifying variation by calculating the shares of girls and boys with non-Western origin in the catchment area (of the 9^{th} grade school) each student belongs to at age 10. We cannot directly observe catchment areas in our data, but we can reconstruct them using detailed geographic information on residential areas. Appendix B describes in detail how the catchment areas and default schools are determined.

Outcome variables. Our main outcome variable of interest is inter-ethnic partnership. It is defined as a dummy variable taking the value 1 if the individual has a child with an individual of non-Western origin, i.e., forms a mixed-ethnic family, and 0 otherwise. We aim to capture completed fertility, and we observe all births up until 2017 when our observed cohorts are 39–42 years old. An alternative is to study inter-ethnic marriage. In our Swedish population 79 percent of the natives have a child and 50 percent are married. Among those who have a child 57 percent are also married, and 13 percent among those who are married do not have a child. Thus, married couples are a subsample of couples having a child together, and we therefore prefer inter-ethnic family formation as the main outcome. In Appendix A, we also present results when we combine inter-ethnic marriage and inter-ethnic family formation as an outcome, and the results are (as expected) similar to the baseline.

We argue that inter-ethnic romantic partnership is an outcome that captures revealed preferences for homophily – but we cannot rule out that it also may be affected by other mechanisms. In order to test alternative mechanisms that may introduce a causal relationship between exposure to peers with a different origin and the formation of ethnically mixed families, we explore a number of different fertility and human capital related outcomes: to have a child, to have a child with a partner who attended the same

[7] Western countries include Western Europe, the UK, the US, and Canada.
[8] The grade 9 register does not include classroom indicators therefore treatment is defined at the cohort level.

school (+-2 cohorts), 9th grade GPA (standardized within cohort), attending an academic track in high school (as opposed to a vocational track or not starting high school directly), and college attendance. We also measure the share of individuals of non-Western origin in the residential area where individuals live as adults, and in the level/field of education chosen by age 30.[9]

Finally, we also study the share of immigrant students in the school the treated individuals choose for their children. For this analysis, we use a sub-set of our original sample, consisting of individuals who have school-aged children. Today (in contrast to the situation when the treated cohorts where in school themselves) there is school choice in Sweden, and approximately 28 percent of all 9th grade students attended a different school than their default public school in 2017 (Holmlund, Sjögren, and Öckert 2019). When parents are allowed to choose, their attitudes towards ethnic minorities may imply that they are more or less inclined to send their children to schools with many minority children. The share of immigrants in their children's school can therefore reflect attitudes towards minorities.

Background characteristics. Students' family background is characterized by mother's and father's years of schooling, mother and father having a professional degree (medicine, engineering, law or economics), and parents' long-run earnings, measured when the student is in grade 9 (16 years old). We also control for month of birth, mother's and father's age at childbirth, number of siblings and birth order.

Our population of interest, natives observed in the grade 9 register 1991–1994, consists of 311,973 individuals. After dropping observations with missing information on family background the sample is reduced to 272,972 observations, which implies attrition by 13 percent. However, within this sample, the share of immigrant peers in grade 9 is representative: this share is calculated before dropping observations with missing background variables.

Appendix Table A1 presents summary statistics of all variables used in the regression analysis for the population of native students. The share of peers with non-Western origin in grade 9 is on average 4 percent. The average number of girls/boys in a school cohort is around 60, implying a cohort size of 120 and about 4–5 classes per grade. In terms of

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[9] Residential areas are defined by SAMS – Small areas for market statistics. Statistics Sweden has divided Sweden into about 9000 SAMS units with about 700 - 2700 inhabitants in each.

our main outcome variable, we observe that 6.9 percent of natives form an inter-ethnic family.

5 Empirical strategy

Our identification is based on variation in peer group composition across cohorts within schools, a strategy first presented by Hoxby (2000). The idea is to estimate a relationship like the following using the population of native-born students:

$$y_{igst} = \alpha + \beta_s S\_NW_{sgt} + \beta_o O\_NW_{sgt} + f(N\_g_{st}) + f(N\_b_{st}) + \delta_{sg} + \theta_{tg} + \gamma Pop\_NW_{igct} + \varepsilon_{igst} \quad (1)$$

where y_{igst} represents the outcome of native-born individual i of gender g, attending school s, belonging to cohort t; S_NW_{sgt} and O_NW_{sgt} represent the share of same-gender (*S*) and opposite-gender (*O*) peers at the school-cohort level that are of non-Western origin; and $f(N\_g_{st})$ and $f(N\_b_{st})$ are second-order polynomials in the number of girls and boys in the school-cohort, which effectively control for cohort size and allow us to identify effects of the share of non-Western peers net of variations in student numbers. δ_{gs} and θ_{gt} represent school-gender and cohort-gender fixed effects, and Pop_NW_{igct} controls for the share of non-Western neighbors (aged 16–64) in the residential neighborhood where the student lives. This means that β_s and β_o are identified by exploiting within-school, across cohort variation in the shares of non-Western origin students, net of general trends in the immigrant population in the student's neighborhood. A causal interpretation of β_s and β_o relies on the assumption that the within-school across cohort variation in the share of minority students is (conditionally) uncorrelated with the error term of the outcome regression.

There are two main concerns with estimating the effects of exposure to peers using equation (1): i) variation across years might not be idiosyncratic, but correlated with the error term, e.g., if the student composition in schools is trending over time in a way that is correlated with treatment (in our application e.g. due to white flight), and ii) the variation is a snapshot of the composition of a school cohort at a given time and does not necessarily capture long-term exposure. This is a particularly relevant concern when studying exposure to immigrant students – if many immigrants observed in a given school-cohort and year have recently immigrated, the observed share in that year will not be representative of long-term exposure.[10][11]

Below, we present our strategy for solving both challenges. The basic intuition of our strategy is to define our identifying variation as exposure to minority students at age 10, in the catchment area of the grade 9 school that students are *expected to attend* given their address at age 10. The peer group observed at age 10 follows the student for the remaining 6 years of compulsory education, but may be subject to student mobility both in the native population potentially "reacting" to exposure by moving or changing schools, and by in- and out-mobility among immigrants. By using this measure, we exploit variation in "expected" long-term exposure to non-Western peers before potential endogenous mobility due to the ethnic composition of the school cohort. It also allows us to explicitly address the question of endogenous mobility by studying the reactions (i.e., school mobility or residential mobility) of exposed native families.

Our identification strategy relies on the assumption that variation across cohorts, within the catchment areas where children reside at age 10, mimics a random allocation. This is a strong assumption and the remaining concern regarding identification is that despite exploiting the expected peer composition, treatment is still correlated with the error term. This could be the case if neighborhood inflows of immigrants are correlated over time (before and after age 10) and native families gradually have responded to such trends before age 10, thus creating differences in the composition of natives within schools over time in our sample. Although our specification controls for general trends in the immigrant population, and we provide robust evidence that treatment is uncorrelated with a range of observed background characteristics, we cannot fully rule out such compositional changes in terms of unobservable characteristics. As a robustness test, we therefore also include catchment area-specific linear trends. Below, we describe our empirical strategy in more detail.

[10] Exposure time has been shown to be of particular importance when studying how neighborhood characteristics affect child outcomes, see e.g. Chetty, Hendren, and Katz (2016a).

[11] A third relevant critique to the method is raised by Angrist (2014). When using peer averages (i.e., "leave-out-mean") as the treatment variable there is a negative mechanical correlation between own and peer characteristics. In our setting the population of interest (natives) is strictly divided from the peers (non-Western origin) who are potentially affecting them. This follows the solution proposed by Angrist (2014), and has been implemented in, e.g., Angrist and Lang (2004) and Imberman, Kugler, and Sacerdote (2012).

5.1 Identifying reduced-form effects of exposure in the catchment area

We exploit the strict enforcement of residence-based school assignment rules and assign students to the default 9th grade school which is determined by the student's address through catchment areas (CA). Students are assigned to CA:s based on their residential location at age 10, six years before leaving compulsory school after 9th grade[12]. Next, we calculate the shares of non-Western origin students in the CA:s when students are aged 10, in order to capture exposure duration of at least 6 years (i.e. the remaining 6 years of compulsory school). The identifying variation thus comes from cohort variation in the age-10 and gender-specific student population residing within a catchment area, ignoring subsequent in-and out-mobility from the CA.

The reduced-form effects of long-term exposure to non-Western peers are then estimated using an equation similar to equation (1), but defined at the CA level:

$$y_{igct} = \alpha + \beta_s S\_NW_{cgt} + \beta_o O\_NW_{cgt} + f(N\_g_{ct}) + f(N\_b_{ct}) + \delta_{cg} + \theta_{cg} + \gamma Pop\_NW_{igct} + \varepsilon_{igct} \quad (2)$$

where subscript *c* represents the CA of the 9th grade school, that students are expected to attend based on their residential address at age 10.

For the population of interest in our study, the possibility to opt out from the CA school was limited. Students were automatically assigned to their default public school (choice between public schools was introduced in July 1994 and was not an option to our cohorts), and on average only 1 percent of the compulsory school graduates in our sample attended an independent (private) school.[13] The strict enforcement of catchment areas means that exposure at the CA level is a relevant treatment definition.

We use the CA-level variation for two different analyses. First, we investigate whether treatment induces students (or their parents) to react by changing school or by moving to a new catchment area. This type of reaction is known from the tipping-point literature and is essential for understanding the mechanisms through which exposure to minority peers might affect long-term outcomes.[14] Second, we estimate the reduced-form effect of CA-exposure to non-Western peers on long-term outcomes. The latter is an intent-to-treat (ITT) estimate, which reflects the "total" effect of age 10-exposure, including mediating

[12] It should be noted that it was common to change schools between 6th and 7th grade in Sweden at the time. We cannot observe such changes, but a common scenario would have been that all 6th graders from a number of smaller schools were assigned to start 7th grade in a bigger school in the local area.

[13] See Böhlmark and Lindahl (2015).

[14] For a discussion relevant for Sweden, see Böhlmark and Willén (2017).

channels such as school/residential mobility or other reactions. The ITT can be different from the treatment-on-the-treated (TOT) we aim to capture for two different reasons. First, age 10-exposure in the CA will differ from actual school exposure due to non-systematic in- and out-mobility, and if this measurement error is random, we expect the ITT to be attenuated in comparison to the TOT. Second, if parents with certain characteristics or preferences systematically react to immigrant exposure by moving to other areas, the ITT is not necessarily informative of the effects on those who take up treatment. Therefore, we also use an IV specification to gauge the magnitude of the effect on those actually treated.

5.2 Instrumental variable specification estimating effects of school exposure

To identify the TOT we use (conditionally exogenous) exposure observed in the catchment area, to create an instrument for actual (observed) 9th grade exposure at the school.

Consider S_NW_{igct} and O_NW_{igct} representing the shares of same- and opposite gender peers of non-Western origin in the catchment area *c* of individual *i* of gender *g*, cohort *t*. Let the *expected shares* at the gender-school-cohort level be represented by

$$E\_S\_NW_{gst} = \frac{\sum_{i=1}^{N_{gst}} S\_NW_{igct}}{N_{gst}};\ E\_O\_NW_{gst} = \frac{\sum_{i=1}^{N_{gst}} O\_NW_{igct}}{N_{gst}};$$

where N_{gst} is the number of students in the gender-school-cohort. For a given population in school cohort *s* and *t*, $E\_S\_NW_{gst}$ and $E\_O\_NW_{gst}$ thus represent the average over individual students' exposure in the catchment area they belonged to at age 10. As such, expected shares are informative of the student composition prior to any endogenous response to immigrant inflows after age 10, and in addition represent variation in long-term exposure (6 or more years) of non-Western peers.

Our instrumental variables strategy, which follows Black, Devereux, and Salvanes (2013), uses these expected shares as instruments for the actual school-peer composition. In other words, we use the overlap between the intended variation (the one in the student's expected school aggregated over all students in school *s*) and the variation in the school the student actually attends.

In the first stage, we regress S_NW_{gst} and O_NW_{gst} (the shares at the school level, see equation 1) on the expected shares and all other control variables, and use the predicted values in the second stage:

$$y_{igst} = \alpha + \beta_s^{IV}\widehat{S\_NW}_{gst} + \beta_o^{IV}\widehat{O\_NW}_{gst} + \delta_{gs} + \theta_{gt} + f(N\_g_{st}) + f(N\_b_{st}) + \gamma Pop\_NW_{igct} + \varepsilon_{igst} \quad (3)$$

where $\widehat{S\_NW}_{gst}$ and $\widehat{O\_NW}_{gst}$ are the predicted values from the first stage, and β_s^{IV} and β_o^{IV} are the IV coefficients of interest, indicating how exposure to non-Western peers affect the outcome.

The exclusion restriction implies that the instrument should affect outcomes only through interactions that arise because children attend the same school. Using expected shares – based on residential location – as instruments for actual school shares, we therefore need to make the assumption that the CA-level variation does not affect the outcomes of interest beyond the exposure that arises within schools. We believe that exposure to peers in the neighborhood can affect outcomes directly, but this type of effect is unlikely to be cohort-specific: when neighborhood children interact, they do not necessarily separate by birth year. The cohort-specific variation that we exploit only affects peer groups that are defined by age and gender, such as the interactions in the classroom at school. The only alternative type of age-specific interaction that we find plausible is leisure (e.g. sport) activities organized in the local area. Although the latter channel is likely to be of limited importance, we cannot fully rule out the possibility that the exclusion restriction is violated, and present three different specifications for full transparency. To sum up, we present the following specifications to estimate the effect of exposure to non-Western peers:[15]

1) Reduced form estimates using cohort variation at the CA-level (*equation 2*). This specification gives us the ITT estimate of exposure to non-Western peers through grades 4–9.
2) Reduced form estimates using expected variation (expected shares based on CA:s observed at age 10) at the 9th grade school.
3) IV-estimates where actual school exposure in 9th grade is instrumented with expected shares (based on CA:s observed at age 10). This specification gives us

[15] In all specifications, we use standard errors that are clustered at the level of the fixed effects (i.e., catchment area or school).

the effect on those actually treated (Local average treatment effect – LATE) (*equation 3*).

We expect the ITT to be lower (in absolute value) than the LATE, since the former is based on variation from all students in the CA, not only from the compliers (those attending the same school as the immigrant students). The difference can occur both because of endogenous responses and random in- and out-mobility from the CA.

5.3 Mechanisms and model specifications

Before turning to the results, we discuss and motivate our model specification. More explicitly, we address two different margins of immigrant peer exposure.

Firstly, the importance of exposure to immigrant peers of the same gender. This approach assumes that young people form close friendships with individuals of their own gender (Kalmijn 2002). Same-gender interactions are also the mechanism behind the results in Merlino, Steinhardt, and Wren-Lewis (2019). They find that white US students exposed to black classmates of the same gender are more likely to enter mixed-race romantic relationships in the future. We investigate this mechanism by allowing the effect to vary depending on whether the immigrant peers are of the same or the opposite gender as the native student.

Secondly, non-linear effects. Swedish schools are ethnically segregated, and the opportunity for up-dating beliefs about the minority group (and as a consequence changing the number of future interactions with the group) may be different depending on the baseline number of minority students among peers in the classroom. In order to capture such non-linear effects, we make a distinction between the extensive and the intensive margin: the former is intended to capture variation from 0 to 1 immigrant peers in the school-cohort, while the latter is intended to capture variation from at least one to more than one immigrant in the school-cohort. Technically, we estimate a linear spline with a knot at the mean (0.03 percent), corresponding to the 70th percentile in the distribution of immigrant shares at the school cohort level. This means that on average, variation below the knot corresponds to moving from 0 to 1 non-native student in the class, while variation above the knot refers to additional immigrant peers in classes that already had at least one immigrant.

6 Results

In this section we present the effects of immigrant exposure in school on both short-, intermediate- and long-run outcomes. We start by presenting a balancing test based on model specification 1, i.e., at CA-level with CA-fixed effects. Thereafter, we present the results on endogenous reactions among natives, i.e., we investigate whether native students change school or move when the share of immigrant peers increases in their catchment area. Finally, we present results on the main outcome of interest and on different complementary outcomes which help us to disentangle mechanisms and facilitate the interpretation of our findings.

6.1 Balancing tests

An implicit test of the identifying assumption is to regress the identifying variation (the share of non-Western peers) on predetermined student characteristics in model specification (2).[16] We replace the outcomes with the treatment variables, using separate specifications for variation below and above the knot at 0.03 in the spline function. Table A2 in Appendix A presents the results from such balancing tests. The first column shows the associations between the share of same-gender immigrant peers below the knot and observed pre-determined characteristics, while the second shows the corresponding associations for exposure above the knot. Columns 3 and 4 show results for the share of opposite-sex non-Western peers. Table A2 uses the exact definitions of covariates that are later used in our empirical analyses.

All the estimates in front of the covariates capturing students' background characteristics are (conditionally) small in magnitude, suggesting no correlation with the variations used for identification. However, a few of them are statistically significant, although the estimates are close to zero. To give an idea of their economic significance, Table A3 in Appendix A presents the results from a slightly different model, in which the education dummies are replaced by continuous variables of numbers of years of post-compulsory education, and the quadratic form of those.[17] In this model a few estimates turn out statistically significant, but important to note is that there is no general pattern

[16] That is, conditioning on catchment area * gender f.e., cohort * gender f.e. and number of male and female (native and non-native) students in the cohort at catchment area level. In addition, we also condition on the share of immigrants (all ages) in the residential neighborhood.

[17] The reason for including dummy variables for different education levels in the main specifications is to get a more flexible model specification. However, Table A3 allows us to assess the magnitude of the estimates in a more straightforward way.

across model specifications: for example the indicator for father's elite education turns out statistically significant in specification 2 but not in specification 1, 3 and 4. This observation suggests a spurious correlation rather than selection on observables. The interpretation of this estimate suggests that if a native student's father has an elite education[18] (conditioning of years of education) the share of immigrants of the same gender decreases on the intensive margin with 0.000255 percentage points. We neglect this association given that it is not economically meaningful and conclude that the balance test supports our identification strategy of idiosyncratic variation in the share of immigrants at the different margins. We also note that the p-values of F-tests of all the covariates do not indicate that the covariates are jointly significant.

Our conclusion from these balancing tests is that our preferred model specification is without individual background covariates, but as a robustness check we also present specifications where covariates are included. To further validate the identifying assumption, we provide additional evidence in Section 6.4 that our results have not occurred by chance, by simulating the distribution of treatment effects under random allocation of minority shares.

6.2 Natives' reactions to immigrant peers in school

Native reactions in response to rising immigrant shares – in terms of changing school or moving – could be the result of negative attitudes, but also of rational behavior if school quality decreases when many low performing students enter the school. For the treated cohorts, graduating in 1991–1994, residence-based school assignments were binding but exceptions were made based on special circumstances. Changing school was therefore an outcome that required active parents who could convince school principals to make exceptions.[19] Alternatively, parents could move to a different catchment area in order to gain access to another school. Based on the tipping-point literature our *a priori* assumption is that the effect is larger on the intensive margin, i.e., when the number of immigrants in the class exceeds one.

[18] That is, a professional degree such as medical doctor, lawyer or engineer.
[19] Legislation prior to 1994 opened up for school choice only when it was considered practically and economically feasible, which was up to the municipality to decide (Government bill 1990/91:18). There were no general rules governing school choice and the decision was at the discretion of municipality administrators and principals. In practice, there was little school choice at this time, see e.g. Holmlund, Sjögren, and Öckert (2019).

Table 1 shows estimates of reactions among native students due to an increased share of immigrant peers in the CA-cohort. The outcomes are the probability of changing school, i.e., not attending the expected school in grade 9 according to the residential address at age 10, and the probability of changing residential address (moving outside the CA) between grade 4 and 9, in addition to changing school. We capture possible reactions over a period of 6 years between observed treatment at age 10/grade 4, and the end of compulsory school in grade 9. We present estimates of the share of immigrants of the same and the opposite gender, on the extensive and the intensive margin, respectively. We do this without covariates (specifications 1 and 2) and with covariates (specifications 3 and 4). As a robustness check, we also replace the control for the immigrant share in the CA with linear CA- and gender-specific trends (specifications 5 and 6).

In line with our *a priori* hypothesis, Table 1 columns 1–4 show that on the intensive margin, same-gender peers with immigrant background cause an increase in both school moves and home moves among natives. The estimates are robust to adding demographic background controls, suggesting no selection on observables. Interesting to note is also that the corresponding effects with respect to peers of the opposite gender are close to zero and statistically insignificant.

With respect to the estimates on the extensive margin, we find negative, but not statistically significant estimates from same-gender immigrant peers, and no effects of exposure to opposite-gender peers. Thus, it seems like parents react to immigrant peers but only on the intensive margin, and primarily if they are of the same gender as their own child. This result is somewhat surprising, but in line with the literature suggesting that social interactions at this age occur mostly within gender (see Currarini, Jackson, and Pin (2009) and Fletcher, Ross, and Zhang (2013)), and consistent with parents reacting to the minority children that they observe and hear about through same-gender interactions.[20]

The intensive margin estimates suggest that an increase in the share of same-gender immigrant peers of one within-CA standard deviation (an increase of about 1 immigrant student in a class that already had at least one minority student) increases the probability of changing school among natives with about 0.47 percentage points over a mean of 14

[20] Same-gender inter-racial interactions is also the main mechanism behind the result presented in Merlino, Steinhardt, and Wren-Lewis (2019).

percent, corresponding to an increase of 3 percent.[21] The corresponding effect on home move is of about the same magnitude. The intensive margin response is in line with the "tipping point" literature, which shows that natives do not react to low minority shares, but start leaving neighborhoods when minority shares reach a somewhat higher threshold (Card, Mas, and Rothstein 2008).[22]

Table 1 Estimates of reactions: reduced form effects of exposure to non-Western peers in the catchment area

	(1)	(2)	(3)	(4)	(5)	(6)
	School move	Home move	School move	Home move	School move	Home move
Ext. margin <0.03 & same gender	-0.206*	-0.096	-0.191	-0.083	-0.166	-0.133
	(0.119)	(0.081)	(0.118)	(0.080)	(0.130)	(0.099)
Int. margin >=0.03 same gender	0.159**	0.122**	0.156**	0.119**	0.168**	0.095
	(0.071)	(0.052)	(0.070)	(0.049)	(0.075)	(0.066)
Ext. margin <0.03 & opp gender	-0.094	-0.024	-0.085	-0.011	0.097	0.127
	(0.118)	(0.078)	(0.118)	(0.077)	(0.129)	(0.100)
Int. margin >=0.03 &opp gender	-0.001	0.011	0.002	0.012	-0.040	-0.008
	(0.069)	(0.048)	(0.068)	(0.047)	(0.073)	(0.063)
Outcome mean	0.146	0.101	0.146	0.101	0.146	0.101
Observations	272,965	244,948	272,965	244,948	272,965	244,948
CA f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Year f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Background controls	No	No	Yes	Yes	Yes	Yes
CA linear trend instead of the control for share of immigrant all ages in the CA	No	No	No	No	Yes	Yes

Note: Home move is defined as changing residential address and not attending the expected school in the CA. The model (across specification 1-4) includes controls for the number of female and male students in the school cohort within the catchment area as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

In general, mobility can be explained by gentrification and decline of residential areas. We have an increase in the share of immigrants over time that may overlap with such processes. Moreover, these aggregated trends are probably not equally distributed across CAs, and in some areas they may turn up as positive or negative shocks to the neighborhood. Although it is unlikely that such trends are gender-specific and confound our results, we present alternative specifications controlling also for linear trends for each catchment area. We cannot simultaneously control for both the share of immigrants (all

[21] The within-CA standard deviation at the same-gender extensive margin is 0.03 (see Table A1).
[22] Böhlmark and Willén (2017) estimate that the tipping point in Swedish metropolitan areas occurs at an immigrant share of around 18 percent.

ages) every year, as well as linear trends over a short time period, since it will remove all variation of interest. Adding CA-specific linear trends to the model (and excluding the share of immigrants in the neighborhood) gives even stronger results on school move, suggesting that gentrification or decline of residential areas are not driving this result. On home move, on the other hand, the corresponding estimate (on the intensive margin) decreases with linear CA trends (column 6 in comparison to column 4). We conclude that although gentrification and/ or decline of residential areas may play a role, the estimated effects on school move seems to be driven by the gender-specific ethnic composition in the school.

6.3 Accounting for endogenous reactions

So far, we have documented that native students react when the share of same-gender immigrant peers in the same CA and cohort increases on the intensive margin, i.e. when the increase occurs in a CA that already had some ethnic minority students. This flight occurs between grades 4 and 9 and implies that the students we observe in grade 9 is a selected sample, at least when the immigrant share increases on the intensive margin. This finding thus motivates our IV strategy, in which we mitigate the problem with endogenous responses by instrumenting the observed share of immigrant students in the school cohort with the expected share based on students' CA:s at age 10. Our identifying variation thereby covers immigrant exposure over several years, as opposed to being based on just one cross-sectional observation, which has been the approach in previous studies using within-school variation for estimating peer effects (Hoxby 2000; Lavy and Schlosser 2011; Lavy, Silva, and Weinhardt 2012; Cools, Fernández, and Patacchini 2019). We believe that isolating exposure over several years is a strength of our approach, as previous studies have shown that exposure duration is key to isolating effects of childhood environment on children's future outcomes (Chetty, Hendren, and Katz 2016b; Chetty and Hendren 2018).

As discussed previously, we present estimates from three different models throughout. Two of these are reduced form models, and the third is a 2SLS model. The first model (RF CA) exploits variation in the share of immigrants in the CA (observed at age 10) and includes CA-fixed effects. This model estimates the effect of an increase in the share of non-Western peers of the same age and gender in the residential area that corresponds to a catchment area. The second model (RF SCH) is the reduced form model of the IV, which includes school-fixed effects (observed in grade 9) and estimates the effect of expected exposure (over grades 4-9) to immigrant peers among the natives enrolled in the school in 9th grade. Estimates from this model capture the effects of expected exposure among the 9th grade students but does not scale the estimates by the first stage of the IV. Finally, in the third model, the observed share of immigrants in the school-cohort is instrumented with the expected share given by students' CA in a two-stage procedure. The estimates from this model capture the effect on the treated, i.e., among those whose expected exposure is overlapping with actual exposure. In Appendix A, Table A4, we present the first stage estimates from regressions of the observed share in grade 9 on the expected shares at the different margins.

An additional assumption for the IV method to identify the average treatment effect for the compliers (those who attend the school in the CA) is the monotonicity assumption, i.e, the instrument affects all individuals in the same direction. In our setting, an increase in expected exposure should not be associated with a reduction in exposure at any school, or for any group of students. In order to shed light on the validity of this assumption, we have estimated the first stage separately in different deciles of parents' combined income distribution. If the monotonicity assumption is fulfilled, the first stage estimates should be positive for all subgroups. In Figure A1 in Appendix A, we present the results from this exercise, which give us no reason to question the monotonicity assumption.

6.4 Effects on mixed-ethnic partnership

Table 2 presents results on our outcome of main interest: the probability of having a future immigrant partner. We hypothesize that exposure at the extensive margin is more important for individuals' attitude formation and belief updating. Given previous results based on US data (Merlino, Steinhardt, and Wren-Lewis 2019) we expect potential effects to result from same-gender interactions rather than opposite-gender interactions. We present three specifications, where each is presented both without individual demographic controls (columns 1–3) and with these controls (columns 4–6). Before discussing the estimates, we emphasize that our results are robust to adding individual demographic background variables, which implies that we do not have selection on observable characteristics. However, adding them improves precision in some of our regressions.

Table 2 Effects of exposure to non-Western peers on the probability of ending up in a mixed-ethnic partnership

	1	2	3	4	5	6
	RF CA	RF SCH	IV	RF CA	RF SCH	IV
	Immigrant partner (having a child with this partner)					
Ext <0.03 & same gender	0.107*	0.142*	0.283*	0.107*	0.144**	0.288**
	(0.062)	(0.073)	(0.144)	(0.062)	(0.073)	(0.145)
Int >=0.03 same gender	-0.072**	-0.030	-0.035	-0.071**	-0.030	-0.035
	(0.031)	(0.039)	(0.043)	(0.031)	(0.039)	(0.043)
Ext <0.03 & opp gender	-0.051	-0.024	-0.041	-0.049	-0.023	-0.040
	(0.062)	(0.071)	(0.142)	(0.062)	(0.071)	(0.142)
Int >=0.03 &opp gender	0.033	0.015	0.023	0.033	0.018	0.026
	(0.032)	(0.038)	(0.043)	(0.032)	(0.038)	(0.043)
Outcome mean	0.0693	0.0693	0.0693	0.0693	0.0693	0.0693
Demographic background controls	No	No	No	Yes	Yes	Yes
Observations	272,965	272,971	272,969	272,965	272,971	272,969
R-squared	0.014	0.014	0.000	0.016	0.016	0.000

Note: All models (across specification 1-4) include controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

We find that an increase in exposure to immigrant peers of the same gender increases the probability of having a partner with non-Western origin later in life. This effect, which is only present at the extensive margin, is visible in all specifications. The estimates increase in magnitude when we move from column 1 (4) to column 3 (6), which is a pattern consistent with our *a priori* expectations. The IV estimates are larger in magnitude since they scale the estimates with the first stage, and thus estimate the treatment on the compliers.

The estimates from columns 2 and 5 imply that an increase in the fraction of same-gender peers by 3 percent (i.e., going from 0 to 1 immigrant student in a class of 30), increases the probability of entering an inter-ethnic partnership by 0.4 percentage points, or 6 percent in relation to the outcome mean.[23] To further understand these results, we have estimated the models separately by gender. It turns out that the observed effect is mainly driven by native females (these results are presented in Table A5 in Appendix A) and within this group the effects should not only be attributed to same-gender interactions.

[23] 0.03*0.144=0.004.

We also observe a negative effect on the intensive margin of having immigrant peers of the same gender in the catchment area (models 1 and 4). It is important to remember here that on this margin we also find reactions in terms of natives changing schools and residential addresses. Thus, we cannot rule out that the observed effect is explained by natives moving to contexts where there are fewer immigrant peers. In the models capturing school variation in immigrant peers (models 2 and 3), the corresponding estimates are smaller and statistically insignificant.

We provide two robustness tests of this analysis. First, we have investigated whether our results are sensitive to the definition of the outcome variable 'mixed-ethnic partnership'. In Appendix Table A6 we replace the outcome with definition which includes both inter-ethnic family formation and inter-ethnic marriages. That is, we have broadened the outcome to also include childless inter-ethnic marriages (only 13 percent in our population are married without having a child). The estimates become somewhat larger with this alternative outcome definition.

Second, we use a simulation exercise, described in Athey and Imbens (2017) and applied in Cools, Fernández, and Patacchini (2019), to study the likelihood that the results could have occurred by chance. We calculate the likelihood of obtaining the observed treatment effects by chance by generating randomness in the exposure to immigrant peers. We do this by re-assigning to each school and cohort a random share within the actual treatment distribution within each school of all four immigrant exposure margins of interest: extensive and intensive exposure of both same and opposite gender. We repeat this procedure 1,000 times and run the full RF-specification from Table 2, keeping all other variables at their true levels. The distributions of the estimated coefficients are shown in Figure A2. The blue vertical line in each graph indicates the estimated treatment effect we obtained in Table 2, column 2 (RF-specification without controls). As can be seen in the figure, the estimated coefficient of immigrant exposure of the same gender at the extensive margin is larger in absolute value than any of the randomization-based estimates, providing evidence that this is unlikely to have occurred by chance. The vertical lines that represent the estimated treatment effects for the other margins are within the distribution of randomization-based estimates and could hence occur by chance. To sum up, we find that exposure to immigrant peers in compulsory school increases the probability of ending up in a mixed-ethnic family later in life, and this result is driven by native girls. The effect size is about half of the effect of exposure to black peers on future interracial romantic relationships in the US (Merlino, Steinhardt, and Wren-Lewis 2019).[24]

6.5 Alternative explanations

The inflow of immigrants to schools can affect natives in many ways. On the one hand, if immigrants have poor language skills and limited education backgrounds, teachers might have to adapt teaching to their level, or direct more of their time to this group. This could have negative consequences for natives' human capital formation and future education and career paths.[25] On the other hand, natives' relative performance position in the class is likely to improve, which could be beneficial for future educational outcomes.[26] Such dynamics could in turn alter partner choice and family-formation decisions. It is important to consider these potential mechanisms as they are helpful for understanding the effect of immigrant exposure on future partnership formation. In the following sections, we therefore examine whether exposure to immigrant peers affects short- and long-run human capital related outcomes, the likelihood of having a child (family formation), and the share of immigrants in future residential neighborhoods. We also consider whether there is a mechanical effect through the direct network, i.e., whether the future partner went to the same school.

6.5.1 Direct links to the school

Table 3 presents results from regressions that investigate whether immigrant exposure affects the probability to find a partner that attended the same school, either belonging to the same cohort or two cohorts above or below.[27] All estimates referring to the extensive margin are small and statistically insignificant. We can thereby conclude that the positive effect on the probability of having an immigrant partner discussed in the previous section cannot be explained by direct links to the school.

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[24]Merlino, Steinhardt, and Wren-Lewis (2019) find that an increase of one within-school standard deviation in the share of black peers in US schools (grade 7-12) increases the probability of dating a black person 10 years later with about 0.6 percentage points, corresponding to a 13 percent increase.

[25] See Gould, Lavy, and Paserman (2004), Geay, McNally, and Telhaj (2013) and Figlio and Özek (2019) for papers that study effects of immigration on native students' educational outcomes.

[26] See e.g. Murphy and Weinhardt (2020) for a study on the importance of relative position.

[27] Important to remember is that the variation we use for identification is one immigrant peer in the same school and cohort in comparison to none. Thus, if the partner stems from the same school but not the same cohort, that is an indication on that links to the school are important, but still the driving mechanism must be the variation from the same school and cohort.

The only statistically significant estimate (at the 10 percent level) is found in the reduced form model (RF CA) and refers to the intensive margin and same gender interaction conditional on covariates (column 4). This negative effect could be explained by the native flight as a response to increased exposure to immigrant peers, which we observed on the same margin in Table 1.

Table 3 Effects of exposure to non-Western peers on the probability of having a partner from the same school

	1	2	3	4	5	6
	RF CA	RF SCH	IV	RF CA	RF SCH	IV
	Probability of having a partner from the same school (allowing for two cohorts above and below)					
Ext <0.03 & same gender	-0.003	0.016	0.042	0.000	0.017	0.043
	(0.058)	(0.069)	(0.136)	(0.058)	(0.069)	(0.136)
Int >=0.03 same gender	-0.037	-0.025	-0.025	-0.039*	-0.025	-0.024
	(0.023)	(0.030)	(0.032)	(0.023)	(0.030)	(0.032)
Ext <0.03 & opp gender	0.085	0.049	0.095	0.085	0.050	0.098
	(0.054)	(0.067)	(0.131)	(0.054)	(0.067)	(0.131)
Int >=0.03 &opp gender	-0.025	-0.009	-0.007	-0.024	-0.010	-0.009
	(0.023)	(0.027)	(0.031)	(0.023)	(0.027)	(0.031)
Outcome mean	0.0547	0.0547	0.0547	0.0547	0.0547	0.0547
Demographic background controls	No	No	No	Yes	Yes	Yes
Observations	272,965	272,971	272,969	272,965	272,971	272,969
R-squared	0.017	0.017	0.000	0.021	0.021	0.000

Note: All models (across specification 1-4) include controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

6.5.2 Potential human capital effects

Another possible mechanism through which exposure to an immigrant peer could affect the likelihood of having an immigrant partner is, as previously discussed, through effects on human capital formation and/or grades. In order to investigate this potential mechanism, we have estimated the full models on the following outcomes: final grades (GPA from grade 9), the probability of attending an academic high-school track (compared to attending a vocational track or not attending high-school directly after grade 9), and the probability of attending college. The results are presented in Table 4.

We find no statistically significant effects on final grades referring to the extensive margin. The estimates are also small in magnitude.[28] In terms of the probability of

[28] As an example, moving from 0 to 1 opposite-gender immigrant peer (extensive margin estimate, column 5) is associated with a change in GPA by 0.47 percent of a standard deviation.

attending an academic track in high school we find that exposure to an immigrant peer has a negative effect, on the same margin. This is important since this is the margin on which we find effects on inter-ethnic partnership. An increase from 0 to 1 same-gender immigrant peer, results in a 1.3 percentage point lower probability to enter an academic program (corresponding to a 2.6 percent effect) when we focus on the estimate in column 5. We find no effect on college attendance of same-gender interactions. We do, however, find a positive effect on the probability of attending college referring to the extensive margin and opposite-gender interactions. Although this estimate does not help us in interpreting the effect on inter-ethnic partnership, we note that exposure to an immigrant of the opposite gender appears to affect long-run human capital related outcomes. We have estimated the models separately by gender and the results suggest no clear difference across genders.[29]

Furthermore, on the intensive margin, we find positive effects of exposure to immigrants of the opposite gender on final grades. These estimates turn statistically significant when we add demographic individual covariates (but remain the same in terms of magnitude). We find no corresponding effects with respect to exposure to immigrants of the same gender. These findings suggest an explanation related to dynamics within the classroom. To shed more light on this, we have re-estimated the model on an outcome capturing the final grade rank within each school cohort. It turns out that when immigrants of the opposite gender enter the school cohort, natives' grades become relatively better. This is true for both native males and native females. (The results are presented in Appendix A, Table A8.) If a higher relative position implies a boost for student GPA (either through teacher assessments or individual performance), relative position is a potential explanation for the positive effects on GPA.

The results presented in this section suggest that the dynamics in the classroom may be dependent on both the gender and the ethnic composition at the school. Gender differential effects are not uncommon in the peer effects literature (see e.g. Lavy, Silva, and Weinhardt 2012; Cools, Fernández, and Patacchini 2019). The influence of extensive margin exposure to immigrant peers on native girls' probability of enrolling in an academic track in high school is of particular importance, since we find effects on inter-ethnic family formation on the same margin.

[29] All results estimated separately by gender are presented in Table A7 in Appendix A.

Table 4 Effects of exposure to non-Western peers on human-capital related outcomes

	1	2	3	4	5	6
	RF CA	RF SCH	IV	RF CA	RF SCH	IV
	Final grade (st dev in the grade point distribution)					
Ext <0.03 & same gender	0.079	-0.191	-0.270	0.037	-0.135	-0.156
	(0.229)	(0.274)	(0.542)	(0.212)	(0.258)	(0.509)
Int >=0.03 same gender	-0.130	-0.138	-0.165	-0.115	-0.170	-0.208
	(0.121)	(0.153)	(0.167)	(0.107)	(0.133)	(0.145)
Ext <0.03 & opp gender	0.332	0.266	0.421	0.287	0.157	0.193
	(0.241)	(0.292)	(0.580)	(0.217)	(0.267)	(0.528)
Int >=0.03 &opp gender	0.167	0.239	0.265	0.131	0.295**	0.332**
	(0.113)	(0.148)	(0.164)	(0.104)	(0.134)	(0.148)
Outcome mean	0.0871	0.0871	0.0871	0.0871	0.0871	0.0871
Covariates	No	No	No	Yes	Yes	Yes
Observations	272,965	272,971	272,969	272,965	272,971	272,969
R-square	0.079	0.081	0.010	0.251	0.252	0.004
	The probability of attending an academic high school program					
Ext <0.03 & same gender	-0.345**	-0.459***	-0.877***	-0.346***	-0.427***	-0.807**
	(0.143)	(0.170)	(0.335)	(0.133)	(0.160)	(0.315)
Int >=0.03 same gender	0.020	-0.024	-0.015	0.014	-0.055	-0.054
	(0.068)	(0.084)	(0.091)	(0.063)	(0.075)	(0.082)
Ext <0.03 & opp gender	-0.018	0.068	0.084	-0.044	0.010	-0.039
	(0.148)	(0.179)	(0.354)	(0.136)	(0.164)	(0.325)
Int >=0.03 &opp gender	0.017	-0.028	-0.037	0.024	0.015	0.013
	(0.066)	(0.085)	(0.094)	(0.062)	(0.076)	(0.084)
Outcome mean	0.495	0.495	0.495	0.495	0.495	0.495
Covariates	No	No	No	Yes	Yes	Yes
Observations	262,061	262,067	262,065	262,061	262,067	262,065
R-squared	0.064	0.065	0.003	0.191	0.191	0.001
	The probability of attending college					
Ext <0.03 & same gender	-0.022	-0.144	-0.242	-0.055	-0.126	-0.201
	(0.121)	(0.148)	(0.291)	(0.114)	(0.138)	(0.271)
Int >=0.03 same gender	-0.025	-0.051	-0.043	-0.019	-0.069	-0.067
	(0.061)	(0.077)	(0.083)	(0.055)	(0.069)	(0.074)
Ext <0.03 & opp gender	0.294**	0.282*	0.540*	0.277**	0.237	0.442
	(0.125)	(0.158)	(0.314)	(0.115)	(0.144)	(0.287)
Int >=0.03 &opp gender	0.041	-0.042	-0.044	0.028	-0.006	-0.004
	(0.062)	(0.078)	(0.086)	(0.056)	(0.073)	(0.080)
Outcome mean	0.391	0.391	0.391	0.391	0.391	0.391
Covariates	No	No	No	Yes	Yes	Yes
Observations	262,061	262,067	262,065	262,061	262,067	262,065
R-squared	0.064	0.065	0.003	0.191	0.191	0.001

Note: All models (across specification 1-4) include controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

6.5.3 Family formation

Educational decisions affect career paths, which could in turn (indirectly and directly) affect family formation. We have therefore also estimated our model on the probability of having a child. Table 5 presents the results. We find that the negative effect of same-gender immigrant exposure on entering an academic high school track described above is followed by an increased probability of having a child at the age of 40. The magnitude of the estimated effect is small; moving from 0 to 1 immigrant student in a class of 30 corresponds to a 0.65 percentage point increase in the probability of becoming a parent, over a baseline at 79 percent (column 5). For women, this result is in line with the literature suggesting that low investment in academic education is associated with a higher probability of having a child. There is no clear gender difference in terms of the magnitude of this estimate, but it is only statistically significant for females (see Table A9 in Appendix A). As the effect from the reduced form model (columns 2 and 5 in Table 5) of extensive margin exposure to same-gender immigrants corresponds to a 0.65 percentage points increase in the probability to have a child, and the effect on having an immigrant partner is 0.4 percentage points, it is possible that the increased prevalence of inter-ethnic partnerships is fully explained by childless women that under exposure decide to have children with non-Western origin partners.

Table 5 Effects of exposure to non-Western peers on the probability of having a child

	1	2	3	4	5	6
	RF CA	RF SCH	IV	RF CA	RF SCH	IV
	The probability of having a child					
Ext <0.03 & same gender	0.214**	0.220*	0.438*	0.220**	0.218*	0.435*
	(0.099)	(0.119)	(0.234)	(0.098)	(0.118)	(0.232)
Int >=0.03 same gender	-0.072	-0.021	-0.029	-0.076	-0.021	-0.029
	(0.048)	(0.062)	(0.069)	(0.048)	(0.062)	(0.069)
Ext <0.03 & opp gender	0.011	0.008	0.025	0.011	0.005	0.019
	(0.096)	(0.115)	(0.229)	(0.096)	(0.115)	(0.227)
Int >=0.03 &opp gender	0.046	0.039	0.048	0.045	0.040	0.049
	(0.049)	(0.061)	(0.068)	(0.050)	(0.061)	(0.069)
Outcome mean	0.793	0.793	0.793	0.793	0.793	0.793
Covariates	No	No	No	Yes	Yes	Yes
Observations	272,971	272,969	272,965	272,971	272,969	272,965
R-square	0.024	0.000	0.031	0.031	0.000	0.024

Note: All models (across specification 1-4) include controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

6.5.4 Future opportunities to meet migrants

Changes in career paths following altered educational choices could influence future peer groups defined by the workplace or the type of neighborhood that individuals settle down in, which could in turn affect opportunities to meet immigrants. We investigate future opportunities to meet ethnic minority individuals by studying two additional outcomes: the share of immigrants within the level and field of study that individuals have chosen by age 30, and the share of immigrants in the neighborhoods where individuals live around age 35. Minority shares within levels/fields of education consists of 41 categories and constitutes a broad proxy for exposure both at the education institution and future workplaces.[30] Table 6 and Table 7 present the results. Table 6 shows that there is no causal link between childhood minority exposure and the share of non-Western peers in the level/field of education chosen by age 30. Turning to Table 7, we find no effects of exposure to immigrant peers when we use school variation: all estimates in models 2–3 and 5–6, on all margins, are close to zero. We can thereby conclude that the observed effect on having an immigrant partner does not seem to be explained by increased opportunities to meet immigrants in the future.

We do observe a negative effect on the catchment area level (models 1 and 4) on the intensive margin. On this margin we also find a positive effect on final grades (Table 4), which could mean that exposed students have better opportunities to settle down in more expensive areas, which are in turn associated with a smaller share of immigrants.

[30] The share of immigrants within levels/fields of education and in neighborhoods is calculated within the population aged 20–65.

Table 6 Effects of exposure to non-Western peers on the share of immigrants in level & field of education at age 30

	1	2	3	4	5	6
	RF CA	RF SCH	IV	RF CA	RF SCH	IV
	Share immigrants in level/field of education by age 30					
Ext <0.03 & same gender	-0.009	-0.016	-0.027	-0.011	-0.015	-0.024
	(0.023)	(0.028)	(0.056)	(0.023)	(0.028)	(0.055)
Int >=0.03 same gender	-0.005	0.002	0.001	-0.005	0.001	-0.000
	(0.010)	(0.012)	(0.013)	(0.009)	(0.012)	(0.013)
Ext <0.03 & opp gender	0.013	0.026	0.046	0.013	0.025	0.043
	(0.023)	(0.029)	(0.058)	(0.023)	(0.029)	(0.058)
Int >=0.03 &opp gender	0.009	0.017	0.018	0.008	0.019	0.020
	(0.010)	(0.013)	(0.014)	(0.010)	(0.012)	(0.014)
Outcome mean	0.158	0.158	0.158	0.158	0.158	0.158
Demographic background cotrols	No	No	No	Yes	Yes	Yes
Observations	262,866	262,872	262,870	262,866	262,872	262,870
R-squared	0.035	0.036	-0.000	0.046	0.047	-0.000

Note: All models (across specification 1-4) include controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 7 Effects of exposure to non-Western peers on the share of immigrants in future residential area

	1	2	3	4	5	6
	RF CA	RF SCH	IV	RF CA	RF SCH	IV
	Share immigrants in residential area at age 35					
Ext <0.03 & same gender	-0.004	-0.008	-0.015	-0.003	-0.006	-0.010
	(0.025)	(0.032)	(0.064)	(0.025)	(0.032)	(0.064)
Int >=0.03 same gender	-0.002	-0.015	-0.013	-0.002	-0.015	-0.014
	(0.016)	(0.021)	(0.023)	(0.016)	(0.021)	(0.023)
Ext <0.03 & opp gender	0.023	0.000	0.002	0.024	0.001	0.004
	(0.026)	(0.031)	(0.062)	(0.026)	(0.031)	(0.062)
Int >=0.03 &opp gender	-0.041**	-0.020	-0.020	-0.040**	-0.019	-0.018
	(0.016)	(0.020)	(0.023)	(0.016)	(0.020)	(0.023)
Outcome mean	0.138	0.138	0.138	0.138	0.138	0.138
Demographic background cotrols	No	No	No	Yes	Yes	Yes
Observations	261,175	261,181	261,179	261,175	261,181	261,179
R-squared	0.104	0.110	0.006	0.106	0.111	0.005

Note: All models (across specification 1-4) include controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

6.6 Discussion

We find that being exposed to an immigrant in compulsory school (relative to not being exposed) increases the probability of having a future partner with non-Western origin. This result is driven by native girls at the extensive margin and is not explained by direct links to the school. When considering other potential mechanisms, we find that native girls' interactions with immigrant peers do not negatively affect student performance as measured by the GPA, but do affect their educational choices as girls turn out to be less likely to enroll in an academic track in high school. Girls exposed to an immigrant of the same gender are also more likely to have children by age 40. The latter result could be tied to changes in human capital, as previous literature suggests that for women, low investment in education is associated with a higher likelihood of having children.[31]

It is reasonable to believe that the marriage market is altered when students shift from attending an academic track to a vocational track or not starting high school directly. If enrolling in a vocational track or not starting high school directly increases opportunities to meet immigrants, altered meeting probabilities constitute a potential explanation for the increased likelihood of forming inter-ethnic families in the future. However, when we proxy for meeting opportunities by studying the composition within detailed fields of education and of the future neighborhood, we find no indication that treated individuals end up in environments with higher "supply" of potential immigrant partners.

To sum up, our results show that extensive margin exposure to immigrant peers has effects on several outcomes that capture natives' choices and behaviors. Although the effects on inter-ethnic family formation are consistent with a change of attitudes towards immigrants, they can also be explained by other mechanisms related to educational choices and preferences for family formation more generally. What we can conclude, however, is that the results in Merlino, Steinhardt, and Wren-Lewis (2019) and Shen (2018) appear hold also in a European context where we study relationships between natives and immigrants.

6.7 Ethnic composition in the treated natives' children's school

To further explore outcomes that reflect revealed preferences for interactions with ethnic minorities, we study the school choices that treated individuals make for their own

[31] For a review of the relationship between educational investments and family decisions, see Bertrand et al. (forthcoming).

children. In contrast to the situation when our study population was in school, parents in Sweden today can make an active school choice for their children. Swedish schools are currently more ethnically segregated than the residential areas suggest (Böhlmark, Holmlund, and Lindahl 2016), indicating that schools' ethnic composition is a factor that native parents consider when they choose school for their child. The school's ethnic composition can thus reflect parental preferences for their children's interactions with minority groups. But the composition at the school could also signal preferences for educational quality rather than preferences towards the minority group, and in addition mirrors the neighborhoods that families live in.

In Table 7 we show that extensive margin exposure to non-Western origin peers is unrelated to the composition in the neighborhoods that treated cohorts end up in as adults. It is therefore unlikely that differences in neighborhood composition leads to differences in student composition at their children's schools. Instead, preferences for the peer group, either through attitudes or through school quality concerns, are likely mechanisms.

We study two related outcomes to capture school choice: the share of non-Western students at the school, and whether the child attends an independent school. The latter is relevant since it is one of the margins through which parents can choose to opt out of the public system and affect peer characteristics. We focus on a sub-set of our population who have children that have reached school-age (children aged 7–15). For this analysis we use specification 2 and 3 presented in section 5.3, i.e., we present reduced form estimates using expected variation at the 9th grade school (RF SCH) and IV-estimates (IV). We start by estimating effects both on the extensive and the intensive margin for same- and opposite-gender immigrant exposure. The results are presented in Table A10 in Appendix A. None of the estimates on any margin is large in magnitude or statistically significant, suggesting no effect at all on the school choice for the next generation.

However, since the main results on long-run outcomes are driven by native females, and since same-gender interactions do not appear to be the main mechanism (see Table A5), we also estimate the effects on school choice separately for men and women, and we focus on the extensive and the intensive margins without separating exposure by gender. The idea is to investigate if female natives who have been exposed to one immigrant peer during school choose a more ethnically mixed school for their own child. Table 8 presents the results from this analysis. We find clear evidence that childhood exposure to immigrants – on the extensive margin – makes women more inclined to send their children to schools with a higher share of students with non-Western origin. Interpreting the estimate in column 1, we find that moving from 0 to 1 immigrant peer implies that the share of immigrants at the child's school increases by 0.5 percentage points (or 3.6 percent). We find no effect among men. This finding is in line with our previous results, which showed that exposure at the extensive margin affects women's future educational and family formation choices. We cannot explain our results by direct links through school networks, and we have tentative evidence that treated individuals do not have an increased probability of meeting immigrants later in life. Thus, our results suggest that exposure has affected behavior and that treated individuals have more close contacts with minority group members – a result consistent with (but not conclusive of) a change of attitudes towards the ethnic minority group. An alternative explanation, which does not involve a change of attitudes, is that the minority partners that the treated women now have married have different preferences (than the partners they would have married had they not been exposed), and that this (and not a shift in preferences among native mothers) is driving the results. We cannot disentangle these mechanisms, but conclude that regardless of the mechanism, there is an intergenerational spill-over from mothers' exposure to minority peers to their children's exposure to minorities.

Table 8 Effects of childhood exposure to non-Western peers on parents' school choice

	(3)	(4)	(5)	(6)
	RF SCH	IV	RF SCH	IV
	Share immigrant students in child's school		**Child attends independent school**	
Native females	0.16**	0.33**	-0.10	-0.19
Ext <0.03	(0.066)	(0.137)	(0.203)	(0.420)
	-0.05	-0.04	-0.09	-0.09
Int >=0.03	(0.053)	(0.049)	(0.112)	(0.105)
Demographic background characteristics	Yes	yes	Yes	yes
Outcome mean	0.132	0.132	0.131	0.131
Observations	102,141	102,141	102,145	102,145
Native males				
Ext <0.03	-0.05	-0.09	0.11	0.22
	(0.076)	(0.155)	(0.204)	(0.419)
Int >=0.03	-0.03	-0.03	-0.03	-0.02
	(0.045)	(0.042)	(0.121)	(0.112)
Dem. background characteristics	yes	yes	yes	yes
Outcome mean	0.133	0.133	0.121	0.121
Observations	87,519	87,519	87,522	87,522

Note: All models include controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 (7 and 8) are, separately for mother and father, years of education in 5 categories, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

7 Conclusion

This paper studies the impact of exposure to immigrant peers in school on a large set of short- and long-run outcomes. The main focus of the paper is to investigate whether exposure to immigrant peers affect revealed preferences for interactions with immigrants. We measure preferences for interactions by studying the probability of ending up in mixed-ethnic relationship, and the ethnic composition in the schools that treated individuals choose for their children. These outcomes capture close and long-lasting interactions, and as suggested in the previous literature, indicate that members of majority and minority groups accept each other as equals (Fryer 2007). Using rich register data, we are also able to track long-run educational and family-formation outcomes.

We find that the probability to end up in a mixed-ethnic relationship increases if a native student has been exposed to an immigrant peer in grade 4–9 in compulsory school. The result is mainly driven by native women. This finding is in line with evidence from the US by Merlino, Steinhardt, and Wren-Lewis (2019), who find a positive effect of exposure to black peers on future self-reported interracial romantic relationships and Shen (2018) who find that biracial births increase among black women who have grown up in a less race segregated school district. Our result confirms their findings in a very different context (ethnic groups in Europe), but also shows that it extends to other types of behavior. As a complementary outcome, we also study effects on the ethnic composition in the school chosen for own children (one generation later) and find that native women who have been exposed to an immigrant peer at school tend to put their own children in a more ethnically diverse school.

An important contribution of our paper is that our empirical strategy removes bias from endogenous reactions to immigrant exposure by exploiting expected variation based on default school assignments. Using the expected variation, we can explicitly address whether parents of treated children react by moving their child to a new school or by moving to a new residential area. We find empirical support for white flight-type behavior: when the share of immigrant students of the same (not opposite) gender increases on the intensive margin, parents are more likely to move their children to another school or to move home. However, parents do not seem to react when immigrant exposure increases at low baseline levels. These findings are in line with the earlier tipping-point literature (Card, Mas, and Rothstein 2008; Böhlmark and Willén 2020).

In order to carefully investigate whether our main result can be interpreted as more permanently altered attitudes towards immigrants, as suggested by the contact hypothesis, we have investigated several other potential explanations. We find that natives exposed to immigrant peers have a lower probability of attending an academic track in high school, and an increased probability of having a child, suggesting that exposure to peers with non-Western origin alters both career paths and family formation decisions. We find that students' GPA is unaffected, there is no effect on the probability of having a partner from the same school, and no over-representation of immigrants in the residential neighborhood where individuals live as adults. Taken together, these results show that exposure to minority peers have altered women's educational choices and family formation decisions in a more family-oriented direction. This result is consistent with a change of attitudes, but not conclusive when it comes to prejudice towards the minority group. A possible interpretation is that natives incorporate more family-oriented values from immigrant families.

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Appendix A

Table A 1 Descriptive statistics of independent and dependent variables

	Mean	Standard deviation		
		Overall	Within	Between
A. School variables				
Share immigrant peers in school cohort	0.0408	0.0623	0.0209	0.0774
Share immigrant peers in school cohort same gender	0.0408	0.0650	0.0278	0.0773
Share immigrant peers in school cohort opposite gender	0.0408	0.0651	0.0278	0.0775
Nr girls school cohort	58.74	19.77	7.72	20.30
Nr boys school cohort	61.36	20.41	7.49	21.09
Share immigrant peers in the CA cohort				
Ext<0.03 & same gender	0.0125	0.0133	0.0097	0.0097
Int>=0.03 & same gender	0.0150	0.0445	0.0300	0.0456
Ext<0.03 & opp gender	0.0125	0.0133	0.0097	0.0097
Int>=0.03 & opp gender	0.0150	0.0445	0.0300	0.0453
Expected share immigrant peers in school cohort				
Ext<0.03 & same gender	0.0155	0.0119	0.0072	0.0100
Int>=0.03 & same gender	0.0132	0.0366	0.0146	0.0461
Ext<0.03 & opp gender	0.0155	0.0119	0.0073	0.0100
Int>=0.03 & opp gender	0.0135	0.0373	0.0151	0.0468
Actual share immigrant peers in school cohort				
Ext<0.03 & same gender	0.0166	0.0133	0.0090	0.0106
Int>=0.03 & same gender	0.0242	0.0582	0.0246	0.0710
Ext<0.03 & opp gender	0.0165	0.0133	0.0090	0.0106
Int>=0.03 & opp gender	0.0242	0.0583	0.0246	0.0711

Table A1 Descriptive statistics of independent and dependent variables, contd.

Background characteristics				
Years of schooling father	10.90	2.64	2.51	0.84
Years of schooling mother	11.13	2.37	2.29	0.66
Professional degree father	0.070	0.255	0.252	0.043
Professional degree mother	0.102	0.303	0.302	0.030
Log earnings mother	11.28	2.08	2.05	0.36
Log earnings father	12.29	1.12	1.11	0.20
Birth year mother	3.04	0.97	0.96	0.16
Birth year father	3.52	1.01	1.00	0.14
Birthmonth	6.19	3.36	3.36	0.28
Birth order	1.82	0.88	0.88	0.12
Nr of siblings	2.62	1.04	1.02	0.24
Main outcome variables				
School move	0.146	0.353	0.339	0.103
Home move	0.101	0.302	0.299	0.048
Immigrant partner	0.069	0.254	0.253	0.028
Partner from same school and cohort (+/- 2 years)	0.055	0.227	0.029	0.226
GPA (standardized mean 0, sd. 1)	0.087	0.948	0.936	0.163
Academic secondary school track	0.495	0.500	0.118	0.488
College	0.391	0.488	0.481	0.091
Probability of having a child in mid age (in 2017)	0.793	0.405	0.404	0.036
Share immigrants among neighbours	0.138	0.111	0.105	0.040
Number of observations	272,972			
Outcome variables next generation				
Share of immigrant peers in the child's school	0.133	0.124	0.035	0.120
Prob. for the child to attend an independent school	0.126	0.332	0.060	0.327
Number of observations	203,975			

Note: The shares reported here are without the individual him/her-self as this is the definition we use in the estimations.

Table A 2 Balancing test of the correlation between the identifying variation and predetermined background characteristics

	(1)	(2)	(3)	(4)
	Share of immigrants:			
	of same gender extensive margin	of same gender intensive margin	of opposite gender extensive margin	of opposite gender intensive margin
D_ed_mother11	-0.000033	0.000090	-0.000067	-0.000207
	(0.000)	(0.000)	(0.000)	(0.000)
D_ed_mother2	-0.000097	-0.000112	-0.000024	-0.000165
	(0.000)	(0.000)	(0.000)	(0.000)
D_ed_mother3	-0.000067	-0.000011	-0.000009	-0.000129
	(0.000)	(0.000)	(0.000)	(0.000)
D_ed_mother4	-0.000023	-0.000022	0.000003	0.000021
	(0.000)	(0.000)	(0.000)	(0.000)
D_ed_mother5	-0.000111*	0.000039	-0.000063	-0.000111
	(0.000)	(0.000)	(0.000)	(0.000)
D_ed_father1	0.000015	0.000112	-0.000006	0.000204
	(0.000)	(0.000)	(0.000)	(0.000)
D_ed_father2	0.000059	0.000057	0.000064	0.000288**
	(0.000)	(0.000)	(0.000)	(0.000)
D_ed_father3	-0.000013	0.000048	0.000026	0.000200*
	(0.000)	(0.000)	(0.000)	(0.000)
D_ed_father4	0.000011	0.000091	0.000028	0.000117
	(0.000)	(0.000)	(0.000)	(0.000)
D_ed_father5	-0.000023	-0.000038	0.000023	-0.000018
	(0.000)	(0.000)	(0.000)	(0.000)

Table A2 Balancing test of the correlation between the identifying variation and predetermined background characteristics, contd.

Professional_father	-0.000079	-0.000266**	0.000012	0.000142
	(0.000)	(0.000)	(0.000)	(0.000)
Professional_mother	-0.000069	-0.000002	0.000022	0.000144
	(0.000)	(0.000)	(0.000)	(0.000)
Loginc_mother	0.000013*	-0.000012	0.000003	0.000016
	(0.000)	(0.000)	(0.000)	(0.000)
Loginc_father	-0.000015	0.000053	0.000030**	0.000051
	(0.000)	(0.000)	(0.000)	(0.000)
Age birth_mother	0.000021	0.000052	0.000011	0.000029
	(0.000)	(0.000)	(0.000)	(0.000)
Age birth_father	-0.000010	-0.000069	-0.000012	0.000008
	(0.000)	(0.000)	(0.000)	(0.000)
Birth month	0.000003	-0.000009	-0.000003	0.000008
	(0.000)	(0.000)	(0.000)	(0.000)
Birth order	-0.000001	-0.000104*	0.000039	-0.000096*
	(0.000)	(0.000)	(0.000)	(0.000)
Number of siblings	-0.000010	0.000073*	-0.000023	0.000043
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	272,965	272,965	272,971	272,971
R-squared	0.647	0.858	0.649	0.860
CA gender f.e.	X	X	X	X
Year gender f.e.	X	X	X	X
Background controls	X	X	X	X
CA gender specific trends	No	No	No	No
Cluster	CA x gender	CA x gender	CA x gender	CA x gender
F-test p-value	0.560	0.590	0.690	0.380
Outcome mean	0.0125	0.0150	0.0125	0.0150

Note: 1) Dummies for education level. Robust standard errors in parentheses. The model includes controls for the number of female and male students in the school cohort within the catchment area as well as the share of immigrants (all ages) in the catchment area. The F-test is based on all the covariates displayed in the table. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A 3 Alternative balancing test of the correlation between the identifying variation and predetermined background characteristics

	(1)	(2)	(3)	(4)
	Share of immigrants:			
	of same gender extensive margin	of same gender extensive margin	of same gender extensive margin	of same gender extensive margin
Ed_mother	-0.000047	-0.000089	-0.000046	-0.000095
	(0.000)	(0.000)	(0.000)	(0.000)
Ed _father	0.000026	0.000043	0.000006	-0.000066
	(0.000)	(0.000)	(0.000)	(0.000)
professional_father	-0.000070	-0.000255**	0.000008	0.000153
	(0.000)	(0.000)	(0.000)	(0.000)
professional _mother	-0.000051	0.000005	0.000047	0.000196*
	(0.000)	(0.000)	(0.000)	(0.000)
Loginc_mother	-0.000005	-0.000011	0.000063	0.000037
	(0.000)	(0.000)	(0.000)	(0.000)
Loginc_father	0.000002	-0.000039	0.000074	0.000287*
	(0.000)	(0.000)	(0.000)	(0.000)
Agebirth_mother	0.000025	0.000064	0.000003	0.000018
	(0.000)	(0.000)	(0.000)	(0.000)
Agebirth_father	-0.000011	-0.000062	-0.000019	-0.000008
	(0.000)	(0.000)	(0.000)	(0.000)
Bith month	0.000003	-0.000009	-0.000003	0.000008
	(0.000)	(0.000)	(0.000)	(0.000)
Birth order	-0.000002	-0.000102*	0.000042*	-0.000092*
	(0.000)	(0.000)	(0.000)	(0.000)
Siblings	-0.000009	0.000074*	-0.000029	0.000035
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	272,965	272,965	272,971	272,971
R-squared	0.647	0.858	0.649	0.860
CA gender f.e.	X	X	X	X
Year gender f.e.	X	X	X	X
Background controls	X	X	X	X
CA x gender-specific trends	No	No	No	No
cluster	CA x gender	CA x gender	CA x gender	CA x gender
F-test p-value	0.800	0.370	0.540	0.120
Outcome mean	0.0125	0.0150	0.0125	0.0150

Robust standard errors in parentheses. The model includes controls for the number of female and male students in the school cohort within the catchment area as well as the share of immigrants (all ages) in the catchment are. The demographic background variables are the one listed in the table and in addition (but not shown) the quadratic form of both mothers' and fathers education level and income. The F-test include all the covariates displayed in the table. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A 4 First stage estimates from a regression of observed immigrant share in grade 9 on expected immigrant share at different margins

	(1)	(2)	(3)	(4)
	Observed share of immigrants in grade 9			
	Same gender		Opposite gender	
	Extensive	Intensive	Extensive	Intensive
Ext. margin <0.03 & same gender	0.528***			
	(0.027)			
Int. margin >=0.03 same gender		0.958***		
		(0.036)		
Ext. margin <0.03 & opp gender			0.526***	
			(0.027)	
Int. margin >=0.03 &opp gender				0.941***
				(0.036)
Observations	168,484	104,484	168,486	104,484
R-squared	0.568	0.893	0.567	0.895
School gender f.e.	X	X	X	X
Year gender f.e.	X	X	X	X
Background controls	No	No	No	No
School-gender-specific trends	No	No	No	No
Cluster	School x gender	School x gender	School x gender	School x gender
Outcome mean	0.00927	0.0283	0.00922	0.0283

Note: The models include controls for the number of female and male students in the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A 5 Effects of exposure to non-Western peers on the probability of ending up in a mixed-ethnic partnership, by gender

	1	2	3	4	5	6
	RF CA	RF SCH	IV	RF CA	RF SCH	IV
	Immigrant partner (having a child with this partner)					
	Males			**Females**		
Ext <0.03 & same gender	0.063	0.095	0.181	0.144	0.180*	0.376*
	(0.079)	(0.095)	(0.195)	(0.092)	(0.107)	(0.208)
Int >=0.03 same gender	-0.077**	0.016	0.012	-0.039	-0.073	-0.076
	(0.036)	(0.049)	(0.053)	(0.047)	(0.059)	(0.065)
Ext <0.03 & opp gender	-0.147*	-0.154	-0.287	0.080	0.184*	0.376*
	(0.079)	(0.095)	(0.183)	(0.093)	(0.104)	(0.218)
Int >=0.03 &opp gender	0.079*	0.045	0.050	-0.010	-0.014	-0.008
	(0.041)	(0.052)	(0.060)	(0.051)	(0.058)	(0.063)
Outcome mean	0.0610	0.0610	0.0610	0.0801	0.0801	0.0801
Demo. background controls	yes	yes	yes	yes	yes	yes
Observations	154,389	154,396	154,393	133,181	133,181	133,181
R-squared	0.012	0.012	-0.000	0.017	0.017	0.000

Note: All models (across specification 1-4) includes controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A 6 Effects of exposure to non-Western peers on the probability of ending up in a mixed-ethnic partnership (either mixed-ethnic family or marriage)

	1	2	3	4	5	6
	RF CA	RF SCH	IV	RF CA	RF SCH	IV
	Immigrant partner (having a child with or being married to this partner)					
Ext <0.03 & same gender	0.097	0.143*	0.290**	0.097	0.145*	0.294**
	(0.063)	(0.074)	(0.148)	(0.063)	(0.074)	(0.148)
Int >=0.03 same gender	-0.077**	-0.053	-0.060	-0.076**	-0.054	-0.060
	(0.032)	(0.040)	(0.044)	(0.032)	(0.040)	(0.044)
Ext <0.03 & opp gender	-0.067	-0.035	-0.065	-0.066	-0.034	-0.064
	(0.063)	(0.074)	(0.146)	(0.063)	(0.074)	(0.146)
Int >=0.03 &opp gender	0.035	0.015	0.025	0.035	0.018	0.028
	(0.033)	(0.039)	(0.043)	(0.033)	(0.039)	(0.043)
Outcome mean	0.0725	0.0725	0.0725	0.0725	0.0725	0.0725
Demographic background controls	No	No	No	Yes	Yes	Yes
Observations	272,965	272,971	272,969	272,965	272,971	272,969
R-squared	0.014	0.014	0.000	0.016	0.016	0.000

Note: All models (across specification 1-4) includes controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A 7 Effects of exposure to non-Western peers on human-capital related outcomes, by gender

	1	2	3	4	5	6
	RF CA	RF SCH	IV	RF CA	RF SCH	IV
	Final grade (st dev in the grade point distribution)					
	Males			**Females**		
Ext <0.03 & same gender	0.247	0.082	0.268	-0.165	-0.383	-0.598
	(0.291)	(0.358)	(0.736)	(0.308)	(0.370)	(0.697)
Int >=0.03 same gender	-0.255	-0.250	-0.269	0.026	-0.079	-0.146
	(0.157)	(0.195)	(0.210)	(0.143)	(0.181)	(0.198)
Ext <0.03 & opp gender	0.185	0.123	0.174	0.356	0.194	0.230
	(0.316)	(0.381)	(0.733)	(0.298)	(0.374)	(0.768)
Int >=0.03 &opp gender	0.002	0.136	0.190	0.277**	0.458***	0.485**
	(0.151)	(0.195)	(0.220)	(0.140)	(0.176)	(0.192)
Outcome mean	-0.0985	-0.0985	-0.0985	0.282	0.282	0.282
Demo. background controls	yes	yes	yes	yes	yes	yes
Observations	139,784	139,790	139,788	133,181	133,181	133,181
R-squared	0.225	0.227	0.003	0.213	0.215	0.005
	The probability of attending an academic high school program					
	Males			**Females**		
Ext <0.03 & same gender	-0.188	-0.428**	-0.837*	-0.506**	-0.435*	-0.815*
	(0.177)	(0.211)	(0.434)	(0.198)	(0.242)	(0.461)
Int >=0.03 same gender	0.012	-0.010	-0.005	0.018	-0.091	-0.100
	(0.089)	(0.106)	(0.114)	(0.090)	(0.106)	(0.115)
Ext <0.03 & opp gender	0.029	0.286	0.488	-0.144	-0.273	-0.609
	(0.178)	(0.219)	(0.418)	(0.205)	(0.246)	(0.508)
Int >=0.03 &opp gender	-0.041	-0.059	-0.076	0.095	0.082	0.080
	(0.083)	(0.101)	(0.116)	(0.092)	(0.114)	(0.121)
Outcome mean	0.462	0.462	0.462	0.532	0.532	0.532
Demo. background controls	yes	yes	yes	yes	yes	yes
Observations	132,670	132,677	132,674	116,167	116,167	116,167
R-squared	0.215	0.216	-0.000	0.169	0.170	-0.000

Note: All models (across specification 1-4) includes controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A7 Effects of exposure to non-Western peers on human-capital related outcomes, by gender, contd.

	The probability of attending college					
		Males			Females	
Ext <0.03 & same gender	-0.213	-0.348*	-0.683	-0.133	-0.067	-0.104
	(0.169)	(0.205)	(0.421)	(0.140)	(0.169)	(0.318)
Int >=0.03 same gender	0.094	0.006	0.010	0.050	-0.007	-0.018
	(0.073)	(0.097)	(0.104)	(0.061)	(0.083)	(0.091)
Ext <0.03 & opp gender	0.064	0.222	0.375	0.153	0.046	0.062
	(0.168)	(0.203)	(0.388)	(0.135)	(0.162)	(0.327)
Int >=0.03 &opp gender	0.022	-0.027	-0.041	0.023	0.087	0.091
Outcome mean	0.245	0.245	0.245	0.134	0.134	0.134
Demo. background controls	yes	yes	yes	yes	yes	yes
Observations	120,740	120,747	120,745	116,167	116,167	116,167
R-squared	0.127	0.127	-0.000	0.085	0.085	0.000

Note: All models (across specification 1-4) includes controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A 8 Effects of exposure to non-Western peers on grade rank within school cohort, by gender

	1	2	3	4	5	6
	RF CA	RF SCH	IV	RF CA	RF SCH	IV
	Grade rank within school cohort					
		Males			Females	
Ext <0.03 & same gender	8.892	2.245	3.792	-29.930**	-19.227	-33.758
	(11.894)	(11.369)	(23.225)	(14.306)	(12.787)	(23.980)
Int >=0.03 same gender	4.656	10.945**	10.440*	16.434**	18.784***	14.906**
	(5.566)	(5.334)	(5.683)	(6.419)	(5.882)	(6.382)
Ext <0.03 & opp gender	-11.996	0.875	-0.780	10.455	9.591	11.549
	(13.191)	(12.681)	(24.520)	(13.555)	(12.328)	(25.491)
Int >=0.03 &opp gender	13.880***	21.053***	20.441***	16.984**	31.581***	32.514***
	(4.485)	(5.068)	(6.001)	(6.813)	(5.640)	(6.178)
Outcome mean	47.76	47.76	47.76	59.91	59.91	59.91
Demo. background controls	yes	yes	yes	yes	yes	yes
Observations	139,784	139,790	139,788	133,181	133,181	133,181
R-squared	0.314	0.351	0.002	0.353	0.408	0.004

Note: All models (across specification 1-4) includes controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A 9 Effects of exposure to non-Western peers on the probability of having a child, by gender

	1	2	3	4	5	6
	RF CA	RF SCH	IV	RF CA	RF SCH	IV
	The probability of having a child					
	Males			**Females**		
Ext <0.03 & same gender	0.247	0.174	0.363	0.195	0.257*	0.493*
	(0.153)	(0.181)	(0.370)	(0.122)	(0.149)	(0.284)
Int >=0.03 same gender	-0.093	-0.031	-0.033	-0.062	-0.005	-0.019
	(0.074)	(0.093)	(0.101)	(0.062)	(0.083)	(0.092)
Ext <0.03 & opp gender	-0.071	-0.003	0.009	0.091	0.023	0.046
	(0.146)	(0.172)	(0.331)	(0.122)	(0.149)	(0.308)
Int >=0.03 &opp gender	0.039	0.024	0.035	0.054	0.049	0.058
	(0.075)	(0.092)	(0.106)	(0.064)	(0.080)	(0.087)
Outcome mean	0.746	0.746	0.746	0.842	0.842	0.842
Demo. background controls	yes	yes	yes	yes	yes	yes
Observations	139,784	139,790	139,788	133,181	133,181	133,181
R-squared	0.018	0.018	0.000	0.017	0.016	-0.000

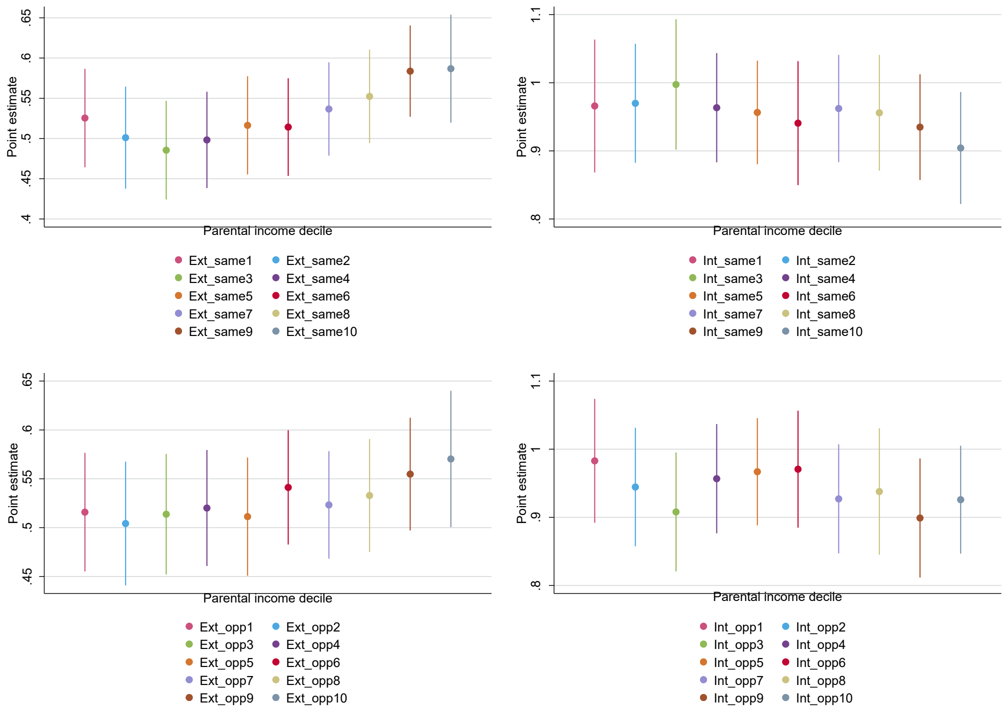
Note: All models (across specification 1-4) includes controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 are, separately for mother and father, years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A 10 Effects of exposure to non-Western peers on the share of immigrant students in the native students' own children's school

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	RF SCH	IV	RF SCH	IV	RF SCH	IV	RF SCH	IV
	Share immigrant students in attended school				**Independent school**			
Ext <0.03 & same gender	0.02	0.06	0.03	0.07	0.09	0.17	0.10	0.18
	(0.040)	(0.079)	(0.040)	(0.080)	(0.116)	(0.229)	(0.120)	(0.238)
Int >=0.03 same gender	-0.04	-0.04	-0.03	-0.03	-0.04	-0.04	-0.01	-0.01
	(0.027)	(0.030)	(0.029)	(0.031)	(0.060)	(0.066)	(0.064)	(0.070)
Ext <0.03 & opp gender	0.02	0.04	0.02	0.05	-0.13	-0.25	-0.15	-0.27
	(0.042)	(0.084)	(0.042)	(0.085)	(0.115)	(0.231)	(0.119)	(0.238)
Int >=0.03 &opp gender	0.00	0.01	0.00	0.01	0.00	0.01	-0.02	-0.02
	(0.025)	(0.028)	(0.026)	(0.029)	(0.063)	(0.069)	(0.066)	(0.072)
	0.02	0.06	0.03	0.07	0.09	0.17	0.10	0.18
Demographic background characteristics	No	No	Yes	Yes	No	No	Yes	Yes
Outcome mean	0.133	0.133	0.132	0.132	0.133	0.133	0.133	0.133
Observations	201,969	201,968	189,660	189,659	201,977	201,976	189,667	189,666

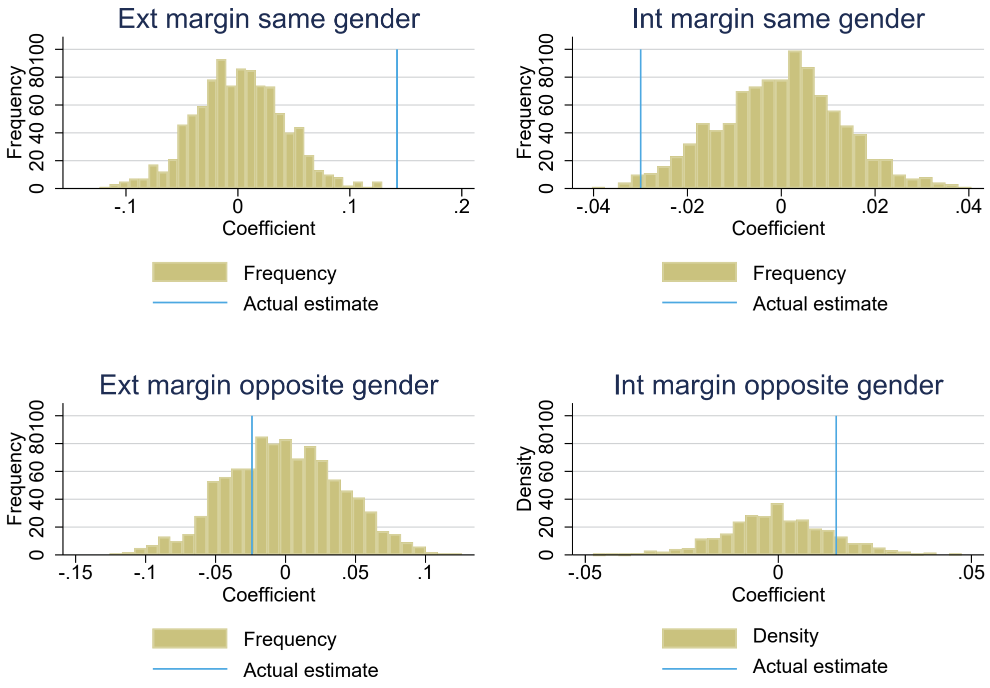
Note: All models include controls for the number of female and male students in the school cohort within the school as well as the share of immigrants (all ages) in the catchment area. The demographic background controls included in specification 3 and 4 (7 and 8) are, separately for mother and father, years of education in 5 categories, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically. Robust standard errors in parentheses are clustered at the level of the fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Figure A 1 Fist stage estimates at the four margins of interest by decile of parents' total income



Note: The models include controls for the number of female and male students in the school as well as the share of immigrants (all ages) in the catchment area, and in addition demographic background controls, separately for mother and father: years of education in 5 categories, indicator for professional degree, log income and log income quadratic, age and student's month of birth, birth order and mother's total number of children linearly and quadratically.

Figure A 2 Randomization-Based Inference of immigrant exposure



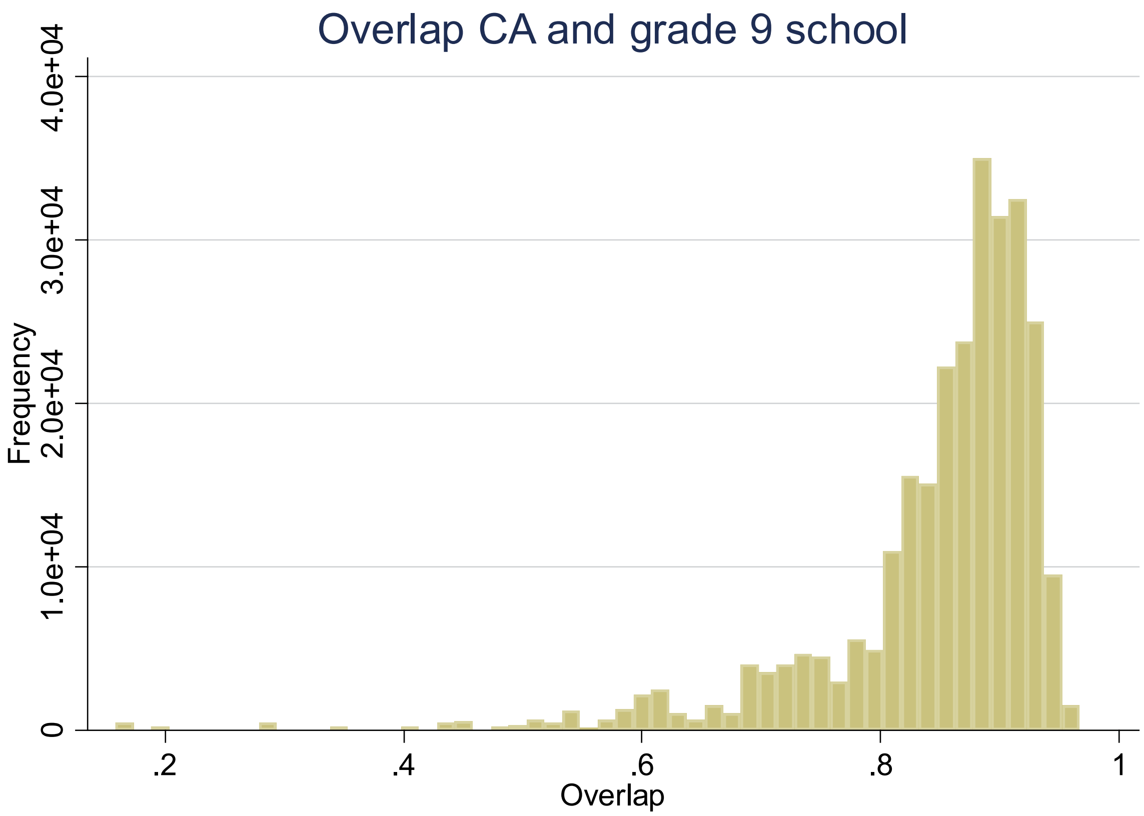
Note: These figures show distribution of coefficients obtained from RF-specification 2 in Table 2 while replacing the immigrant exposure share at the four margins of interest with the value from a random exposure generated in within the actual treatment distribution within each school. Redline represents actual estimates obtained in specification 2 in Table 2.

Appendix B

The construction of CAs and expected schools

First, we identify the 9th grade school that most students within a residential area attend. For each student this is the "default school" that they are assigned to according to their residential address. Figure A3 below shows the share of students who attend the default school in 9th grade (The overlap between CA and grade 9 school), according to their residential address in 4th grade (age 10). The mean and the median are both above 80 percent and indicate i) compliance with the catchment area rule, and ii) limited residential mobility. The CA is defined by grouping all residential areas with the same default school. Using this procedure, about 8,400 residential areas are grouped into 927 CAs. See Holmlund and Böhlmark (2019) for an earlier application and validation of this approach. 4 480 native students attend a school with zero correlation between CA and attended school, and those individuals are dropped.

Figure A 3 Histogram over the share of students in grade 9 who attend their expected school according to their catchment area when aged 10



Note: For 86 schools corresponding to 8.49 per cent of all schools or 1.98 per cent or 6 508 of all students, the overlap is zero and they are excluded from the analysis sample.