The Impact of Childhood Inter-Ethnic Contact on Managers' Hiring Decisions*

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Abstract

This paper analyzes whether inter-ethnic contact in childhood affects the hiring behavior of managers. To identify the causal effect of exposure, we exploit quasi-random variation in the share of immigrant students across cohorts within Danish schools. Using administrative employer-employee data, we find that being exposed to more immigrant peers of the same gender in school leads Danish managers to hire more immigrants later in life. Exploring a variety of mechanisms, we find evidence most consistent with a change in managers' attitudes toward immigrants.

JEL classifications: I29, J15, J61, J71.

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1 Introduction

Faced with structural changes, demographic aging, and worsening labor shortages in OECD countries, the labor market integration of immigrants and their descendants is becoming increasingly important (OECD, 2023). Yet, there are many barriers to their integration, from differences in human capital and referral networks to residential segregation and labor market discrimination. An important mediator of these effects is managers, as they screen applicants and decide who to hire. Evidence suggests that managers tend to hire workers of the same origin (Bandiera et al., 2009; Giuliano et al., 2009; Åslund et al., 2014). However, little is known about which moderating factors can alleviate these biases, which is crucial for policy design.

Our paper addresses this research gap by investigating whether exposure to ethnic diversity in childhood shapes the hiring behavior of managers. For this purpose, we examine how plausibly exogenous variation in a student's school peer group influences their hiring choices later in life. To do so, we use longitudinal Danish administrative register data on primary school completion from 1976 to 1996 and hirings from 1996 to 2019. For each manager, the data provides us with information on the immigrant background of all classmates and employees they hired during their career. To overcome selection bias, we exploit idiosyncratic variation in cohort composition within schools, a methodology proposed by Hoxby (2000) that has been extensively utilized for the purpose of discerning peer influences.[1] Doing so, we find that managers are more likely to hire non-Western immigrants if they were exposed to children of non-Western origin during school. This relationship also holds when we include plant fixed effects and analyze the hiring behavior of managers within the same plant over time. This finding remains robust across several sensitivity checks, including adjustments for sector and municipality trends.

In terms of mechanisms, we do not find any evidence of effects on managers' educational performance or labor market outcomes. We also do not find any evidence that managers live in areas characterized by high concentrations of non-Western

[1] See, for instance, Bifulco et al. (2011); Lavy et al. (2012); Patacchini and Zenou (2016); Carrell et al. (2018); Fruehwirth et al. (2019); Merlino et al. (2019, 2024).

immigrants. While we cannot rule out that changes in managers' social networks may explain the results, several pieces of evidence point against this. First, we find that the effect is as large when we look at hirings made over twenty years after the manager has left school, when the importance of school-based social networks is likely to be diminished. Second, we find exposure to immigrants from Turkey (the biggest group during the schooling period) leads to an increase in the hiring of immigrants from other countries, even if we exclude all countries that border Turkey. The most likely remaining mechanism is therefore that managers more exposed to immigrants in school change their attitudes related to the hiring of immigrants. Consistent with this, we find that our results are driven by exposure to and the recruitment of immigrants from Muslim majority countries, a group of immigrants that is particularly likely to face discrimination in the European labor market (see, among others, Adida et al., 2010; Fernández-Reino et al., 2022).

Our paper contributes to the literature on the labor market integration of immigrants, which has documented that immigrants in OECD countries often experience worse employment outcomes than natives (see, among others, Åslund and Rooth, 2007; Bratsberg et al., 2014; Brell et al., 2020). The literature has identified several characteristics of immigrants as crucial factors for successful integration, such as language skills (Chiswick, 1991; Bleakley and Chin, 2004), legal status (Kossoudji and Cobb-Clark, 2002; Bahar et al., 2021) and citizenship (Steinhardt, 2012; Felfe et al., 2020). Several studies have also examined the influence of the host society, emphasizing the significance of economic conditions upon arrival (Åslund and Rooth, 2007), the availability of language training (Lochmann et al., 2019), geographic dispersal policies (Damm, 2009), and legal employment opportunities (Fasani et al., 2021). However, few papers have examined the role of managers in the hiring process, despite their significant impact on migrants' labor market integration in their function as gatekeepers. On the one hand, they might use discriminatory hiring practices that penalize migrants (Dahl and Krog, 2018). On the other hand, they might favor immigrants. In particular, previous literature has found that managers with a migratory background are more likely to hire someone with a similar background (Åslund et al., 2014). Additionally, as referrals are an important tool for migrants to overcome informational asymmetries (Dustmann et al., 2016),

native managers hiring some migrants can lead to more integration over time. Our findings complement this literature by demonstrating that exposure to immigrants in early life can help to mediate in-group biases in hiring behavior by non-immigrant managers.

In addition, we make a contribution to the literature on the consequences of inter-ethnic and inter-racial contact. While the early literature in economics has focused on the short-term impact on attitudes in the majority group (Marmaros and Sacerdote, 2006; Boisjoly et al., 2006; Camargo et al., 2010), recent studies have started to analyze long-term behavioral consequences such as the formation of interracial relationships (Gordon and Reber, 2018; Merlino et al., 2019), political partisanship (Schindler and Westcott, 2021; Polipciuc et al., 2023) and residential choices (Merlino et al., 2024). We enrich this recent literature by providing, to the best of our knowledge, the first causal evidence on the long-run impact of ethnic exposure on hiring behavior.

The remainder of the paper is organized as follows. Section 2 provides background information about immigrants in Denmark and the Danish school system. Section 3 presents our data and estimation strategy. Our main results are presented in Section 4, followed by a discussion of potential mechanisms in Section 5.

2 Background

2.1 Immigrants in Denmark

Until the middle of the 20th century, Denmark was, like other European countries, mainly characterized by emigration. The few immigrants who came to Denmark were overwhelmingly of Danish origin who returned from Northern European countries and the US (Matthiessen, 2009). During the second half of the century, the situation altered, and Denmark experienced demand-driven net immigration. Since the 1960s, Denmark received substantial inflows of labor migrants from non-Western countries. In the case of Denmark, these so-called "guest workers" mainly originated from Turkey, Pakistan, and Yugoslavia. As in other European countries, the guest worker policy came to an end with the oil-price shock in 1973 (Liebig,

2007).

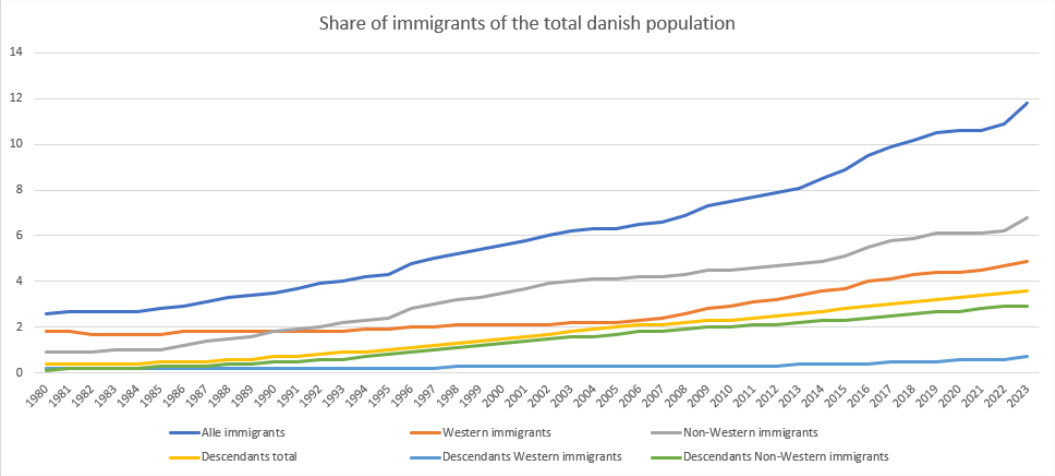
In subsequent years, immigration from guest-worker countries continued on a lower level via family reunifications until the restrictive Aliens Act was implemented in 2002 (Matthiessen, 2009). In addition, Denmark experienced an increasing inflow of refugees since the early 1980s, which peaked during the civil war in the former Yugoslavia in 1993 and 1994 (Bauer et al., 2004; Liebig, 2007). As a result, in 2008 the ten largest immigrant groups (in descending order) had their origins in the following countries: Turkey, former Yugoslavia, Iraq, Lebanon, Pakistan, Somalia, Iran, Vietnam, Afghanistan, and Sri Lanka (Matthiessen, 2009).

The dominance of Non-European immigrants is apparent in Figure 1, which shows the development of the immigrant population in Denmark over time. We thereby differentiate in our analysis not only between non-Western and Western immigrants, but also between first-generation immigrants who were born abroad and children of immigrants born in Denmark. The steady increase in the share of the immigrant population since the late 1980s is mainly due to non-Western immigrants and their descendants. The latter is driven by higher fertility rates of non-Western immigrants (Bauer et al., 2004). For example, in the period 2002-2006, the fertility rate among non-Western immigrants was 2.34, while it was only 1.75 among Danish women and 1.5 among Western immigrant women (Matthiessen, 2009). Finally, Figure 1 documents that, since the enlargement of the European Union in 2004, Denmark experienced a growing share of non-Western immigrants driven by inflows of labor migrants from the new Central and Eastern European member states.

As a result of the described trends, when analyzing immigrant exposure in Danish schools in the 1980s and 1990s, it is almost exclusively about exposure to children of non-Western immigrants. In other words, the immigrant children to whom the Danish managers in our analysis were exposed in school were primarily children of non-Western origin.

In the labor market, non-Western immigrants account for approximately 6% of the Danish workforce in our period of interest (2008-2019). Overall, immigrants are characterized by a relatively weak attachment to labor markets and a high welfare dependency. Among descendants from non-Western countries, employment is

Figure 1: Share of Immigrants in Denmark



higher than for their parental generation, though still lower than for Danes without a migration background (Schultz-Nielsen, 2001; Schultz-Nielsen and Constant, 2004; Bauer et al., 2004; Matthiessen, 2009). The labor market situation of immigrants has continuously received a lot of attention in public debates and has led to several controversial political discussions and decisions (Dahl and Krog, 2018; Jakobsen et al., 2019). Given this situation, it is rather surprising that there is limited evidence about the extent of penalties in the Danish labor market for migrants compared to those of other European countries. A noteworthy exception is a recent study by Dahl and Krog (2018), which uses a field experiment to document that applicants with Middle Eastern-sounding names have significantly lower callback rates than applicants with Danish-sounding names. This is consistent with studies for other European countries that show that in particular immigrants from non-European countries face substantial penalties in the hiring process (Carlsson and Rooth, 2007; Kaas and Manger, 2012; Lippens et al., 2023). We will analyze whether exposure during childhood is able to reduce in-group biases among Danish managers.

2.2 The Danish school system

In the following, we will describe the main features of the Danish primary school system for the sample cohorts completing primary school between 1976 and 1996. The Danish primary school system remained largely unchanged during this period, with compulsory schooling spanning nine years from grade 1 (6-7 years old) to grade 9 (15-16 years old) (Jørgensen, 2017).[2] After completing primary school, students completed compulsory education, and they may either choose to enter the labor market, enroll in high school, or pursue vocational training.

All children entering school age in Denmark are automatically assigned to a specific public primary school based on their home address, with the right to admission to their designated school. Each Danish address is linked to a specific public school. The 1989 Act on the Folkeskole provided the parents with a free choice of school within their local community (Retsinformation, 1990). For this reason, we limit our analysis to cohorts who completed elementary school up to 1996, i.e., who started public school before the act became effective. Municipalities administer the public primary schools, which are free of charge, and parent co-payment is prohibited by law. Funding for individual schools is not determined by local tax revenues alone, as extensive transfers between municipalities are implemented to mitigate financial inequalities. Each municipality has the authority to determine the number of primary schools and establish school district boundaries (Retsinformation, 1990). The school district is typically defined by the geographical distance to the school and is rarely regulated due to, for instance, demographic changes among the population.

The public school has always been the main choice for Danes: for example, in 1989, 89.7 percent of the students completing primary school did so in a public school. The share of students completing primary education in private schools slightly increased over the sample period, but it is only from the 2000s that Denmark experienced a boom in private school uptake. Private schools served as the

[2] An additional but non-compulsory introductory year, grade 0, was introduced in 1979, which gained widespread popularity and became mandatory in 2009. Furthermore, a non-compulsory grade 10 was offered as an alternative for students who were not ready to continue within the educational system. The curriculum is basically identical as the one in 9th grade and it does not give any additional prerequisites when applying to the next educational options (Jørgensen, 2017).

only alternative option for the assigned public school (Education, 2002).[3]

3 Data and estimation strategy

3.1 Data

The empirical analysis in this paper relies on longitudinal Danish administrative register data that covers the entire population of Denmark. Statistics Denmark provides the data from multiple registers, ensuring high-quality and consistent data on the Danish population. Individuals can be linked across registers and years using personal identification numbers anonymized by Statistics Denmark. We use data from 1976 to 2019: we use data up to 1996 to build managers' primary school cohorts, and use years from 2008 to 2019 to recover their hiring decisions.

Moreover, we construct the universe of employer-employee matches from 2008 to 2019 using the Integrated Database for Labor Market Research (IDA).[4] This allows us to create a complete labor market history for all individuals. We use workers' occupational codes to identify managers in each firm-plant.

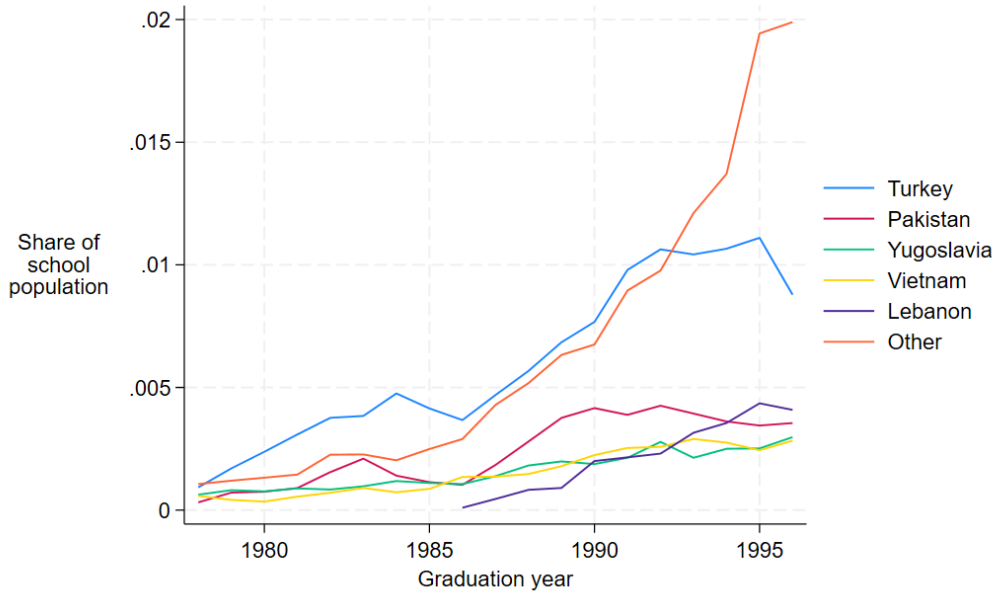
More precisely, using the Compressed Student Register (KOTRE), the study identifies every individual who has completed a Danish primary school from 1976 to 1996, i.e., those students who entered primary school before the 1989 Act on the Folkeskole came into place. The students and employees are then matched to form the final sample of individuals for analysis. In addition, demographic information, including immigration status and birthplace, is matched using the population register (BEF) and the immigration register (IEPE). When considering Non-Western immigrants in school, we define them as foreign-born children who immigrated to Denmark before 1997 and native-born children with an immigrant parent who entered Denmark before 1997. We then calculate the proportion of immigrants in the manager's cohort, as well as their share among new recruits in their establishment.

[3]Private primary schools receive public funds covering on average 75 percent of their costs and parents then cover the rest. The system is similar to the voucher schools in the US system. The schools are free to set their tuition fee and there is typical a sibling's discount. The price varies a lot across the private schools, and in 1990 it was around 100-200€ a month per child.

[4]Employer-employee matches for this period are inconsistent with earlier data, meaning that we rely on post-2008 data to correctly identify true hirings.

Figure 2 shows the share of non-Western immigrant groups in managemers' cohorts, defined by the year of elementary school completion. The strong positive trend closely resembles that of the full population in Figure 1. In addition, the figure highlights the important role of Turkish immigrants, who constituted the largest single group in all cohorts. Finally, the figure documents that the concentration of immigrants in school was rather low in all observed cohorts. Even in the later cohorts of the 1990s, when Danish schools experienced a much higher concentration than in the 1980s due to increased immigration and refugee inflows, the average immigrant share never exceeded 2%. This is also evident in Figure A1 in Appendix A, which shows that more than 95% of our sample had an immigrant share of less than 8% in their elementary school cohort, with more than 80% having no immigrants in their cohort at all.

Figure 2: Share of Immigrants in Completion Year



In our final sample, we focus on all Danish managers without a migration background who completed a Danish primary school between 1976 and 1996 and who are working in establishments with less than 250 employees and less than 5 man-

Table 1: Summary Statistics

	(1)	(2)	(3)
	N	Mean	Within school standard deviation
Year completed primary education	40,804	1985	5.07
Female	40,804	0.30	0.46
School cohort size	40,804	66.3	27.0
Migrant share in school cohort, same gender	40,804	0.012	0.039
Migrant share in school cohort, opp. gender	40,804	0.012	0.039
Number of hirings	40,804	35.6	62.3
Migrant share of hirings	40,804	0.047	0.091

agers.[5] By doing so, we cover over 40,000 managers from around 1,300 primary schools and working in approximately 30,000 establishments in our final sample.

In Table 1 we present summary statistics of the main variables used in our analysis. It documents a significant gender imbalance, with 70% of managers being male—a pattern also documented by Smith et al. (2021). We will consider this dimension in our empirical analysis from multiple perspectives. The table further shows that the average year a manager completed primary education, when their immigrant share is measured, is 1985, with an average cohort size of 66. In these cohorts the average Danish manager had a non-Western immigrant share of 1.2%, both for the same and opposite gender. However, there is substantial variation within schools with an average within-school standard deviation of 4 percentage points. This variability can be attributed, in part, to the substantial growth in the percentage of immigrants and their descendants over time, especially after 1985, which is the average primary school completion year in our sample, as depicted in Figures 1 and 2. However, a large part can also be attributed to the fact that our average cohort is around 66 students, meaning that cohorts with close to the average share have either 0 or 1 immigrant of each gender.

Finally, the table also documents that our data set is well suited for addressing our research question, as the average manager has more than 35 hires, and around 5% of all hires involved a migrant. From Table A1 in Appendix A, we can see

[5] We remove large establishments because we are unlikely to have a good assessment there of who is responsible for hiring decisions. For the same reason, we also remove mangers in establishments where there are more than four managers present.

that around 46 % of managers hired a migrant at some point during their career as a manager. This table also shows that managers do not differ from the rest of the population in many dimensions—most importantly, in their exposure to immigrants in school.

3.2 Estimation strategy

A naïve regression of hiring behavior on immigrant concentration in school is likely to produce biased estimates for two reasons. First, cohort composition in school might be correlated with potentially unobserved factors that influence the hiring decisions of managers, such as the social and ethnic environment of the school. Second, the choice of school could be influenced by parental preferences. Depending on their preferences for ethnic diversity, parents may choose to send their children to schools with a low or large share of immigrants. Self-selection into schools may also happen along other dimensions that are likely to be correlated with the share of immigrants in school, such as income.

We circumvent these problems in two ways. First, we focus on pupils who entered school before the implementation of the 1989 Act on the Folkeskol, which introduced free choice of school within the local community. Second and most importantly, we exploit variation in the gendered share of immigrant students across cohorts within an individual school. In other words, we assume that differences between the average school composition and a child's school-specific cohort in a school are unknown to parents at the time of the school decision, and therefore do not matter for parental school choices. This is particularly compelling as we focus on the gendered composition of each cohort. Indeed, in line with previous literature (Merlino et al., 2019, 2024), we show below that the relevant peers in our context are non-Western migrants in the same cohort and of the same gender as the managers.

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

$$Y_i = \beta_1 \, ShareImmig_{gcs} + \beta_2 \, ShareImmig_{ocs} + \beta_3 \, female_i + I_t + I_s + X_{st} + \varepsilon_i, \quad (1)$$

where the main dependent variable Y_i is the share of the immigrants among all hirings a manager i made during his/her career. $ShareImmig_{gcs}$ is the share of non-Western immigrants of the same gender as the managers within their cohort c in school s, $ShareImmig_{ocs}$ is the corresponding share of non-Western immigrants of the opposite gender, $female_i$ is a dummy that takes value one if manager i is female, I_t and I_s are year and school fixed effects respectively, X_{st} are school-specific controls and time trends to take into account the evolution of migrant shares in schools in our sample period,[6] and ε_i is a random error term. Standard errors are clustered at the school level.

We also undertake some analyses at the plant-year level, which allows us to see how plants' recruiting dynamics change over time. Here the estimating equation is very similar to the one above, but contains a time component:

$$Y_{py} = \beta_1\ ShareImmig_{gcs} + \beta_2\ ShareImmig_{ocs} + \beta_3\ female_i + I_t + I_s + I_y + X_{st} + \varepsilon_i, \tag{2}$$

where Y_{py} is the share of immigrants among hirings made by plant p in year y. The characteristics on the right hand side - i.e. the school immigrant shares and various controls - are those of the manager i who is present in plant p in year y. In most cases we only have one manager in the data for a given plant, but when we have more than one manager we use the characteristics of the most senior manager - that is, the manager who has been in the plant the longest or, where this is equal, the one with the highest earnings.

3.3 Identification assumption

Our identification strategy rests on the assumption that variation in the gender-specific ethnic cohort composition within schools is as good as random once we control for year and school fixed effects and school-specific time trends. The basic idea is that differences between the average school composition and a child's

[6] Specifically, we control for the share of non-Wester migrants in all cohorts up to 5 years before and after the manager's primary school completion year. Additionally, we add two sets of school specific time trends for each school: one for cohorts from 1976 to 1987, and another from 1988 to 1996. These variables allow us to better capture the rapid evolution of the share of non-Western immigrants in Denmark starting from the mid-80s that is apparent in Figure 1.

school specific cohort, which are unobservable to parents at the time of enrollment, do not play a role in parental school choices. There are two implications of this assumption we can investigate.

First, we can run a balancing test in which we check whether within-school variation in the gendered share of non-Western immigrants is correlated with predetermined variables at the individual level. In particular, we regress a range of predetermined parental and background characteristics on the share of non-Western immigrants in their gender-specific peer group, while controlling for year and school fixed effects and school-specific time trends.[7] The corresponding results, displayed in Table 2, are in favor of our main identification assumption, as only one of the predetermined variables, the migrant share in the parents' workplace, is significantly different from zero at the 10% level. Moreover, we do not find any evidence that cohort size and gender share are correlated with the share of non-Western immigrants in a cohort.

An advantage of our data is that we can redo the same exercise for all Danes who completed a Danish primary school between 1977 and 1996, irrespective whether they became manager or not. The corresponding results are reported in Table B. Reassuringly, we find also in this case with more than observations only in one case a significant coefficient which provides additional support for our main identification assumption.

Second, we can test for non-random clustering of immigrant students across cohorts within schools. A simple and intuitive way to do this is to regress the immigrant share of the same gender on the immigrant share of the opposite gender. Doing so, we do not discover any significant relationship between the cohort distribution of non-Western immigrant students of the same and opposite genders (Table 2, last row). As we expect that any pattern of self-selection would affect the opposite and same gender immigrant shares in the same way, we interpret the result as additional support in favor of our assumption that the cohort composition within schools is quasi-random.

Additionally, in Table B1 in Appendix B, we present the same balancing tests

[7] Note that some of these variables are available only for respondents whose parents where in the sample. As a result, the sample size is smaller in some regressions.

Table 2: Balancing and Random Clustering Tests

	(1)	(2)	(3)	(4)
	Migrant share, same gender	Migrant share, opp. gender	Dep. var. mean	N
Father in register	–0.0019	0.022	0.99	40,804
	(0.033)	(0.029)		
Mother in register	–0.0047	0.0041	1.0	40,804
	(0.0060)	(0.0041)		
Father's age	–0.47	–0.33	29	38,299
	(1.3)	(1.2)		
Mother's age	–1.9	1.1	26	40,339
	(1.2)	(1.2)		
Parents attended higher education	–0.0061	–0.0056	0.19	40,797
	(0.078)	(0.082)		
Parents income percentile	0.49	4.1	71	40,757
	(3.9)	(4.3)		
Parents income percentile missing	0.012	–0.0031	0.0012	40,804
	(0.010)	(0.0050)		
Migrant share, parents' workplace	–0.013	–0.016*	0.0065	22,970
	(0.013)	(0.0084)		
Migrant share, address	0.0059	–0.029	0.0074	29,541
	(0.026)	(0.026)		
Migrant share, municipality	–0.00057	–0.0010	0.021	29,541
	(0.0047)	(0.0037)		
School cohort size	–3.9	–2.1	66	40,804
	(3.4)	(3.1)		
Share cohort same gender	–0.0017	0.013	0.51	40,804
	(0.026)	(0.026)		
Migrant share, opp. gender	0.0024		0.012	40,804
	(0.023)			

Notes: The table reports OLS estimates controlling for year and school fixed effects and two sets of school-specific time trends. Coefficients in the first and second column of each row are from a regression where the variable indicated in each line is regressed both on the same gender and opposite gender shares of non-Western immigrants, respectively. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

on the whole working age population in Denmark in our sample period. Here, we do not find any of the predetermined variables being significant. This supports the conclusion that the allocation of migrants of a given gender across cohorts in a given school was essentially random before the introduction of free choice of schools in Denmark.

Table 3: Probability of Being a Manager

	(1)	(2)	(3)	(4)	(5)
LHS variable	Being a manager in our sample	Log of number of recruitments	Migrant share in plant municipality	Migrant share in firm sector	Migrant share in plant before manager joined
Migrant share, same gender	–0.0013	0.053	–0.0031	0.017	0.020
	(0.0066)	(0.32)	(0.013)	(0.013)	(0.022)
Migrant share, opp. gender	–0.0068	0.065	–0.0023	–0.015	0.0029
	(0.0061)	(0.31)	(0.011)	(0.015)	(0.025)
R^2	0.012	0.10	0.25	0.14	0.14
Dep. var. mean	0.038	2.9	0.067	0.046	0.029
Number of schools	1,374	1,314	1,313	1,297	1,304
N	1,083,890	40,804	39,960	30,229	35,112

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, and two sets of school-specific time trends. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

4 Main results

Before analyzing the impact of cohort ethnic composition on hiring behavior, we analyze whether exposure to immigrants in school affects the chance of becoming a manager. The corresponding results are reported in column (1) of Table 3 and are based on the full sample including the total Danish population who completed a Danish primary school between 1976 and 1996. We do not find any evidence that exposure to immigrants in school increased or decreased the chances that someone became a manager later in life. We also do not find that those who became managers hired more people nor worked in firms based in municipalities with a higher share of non-Western immigrants if they were exposed to migrants in school (columns 2 and 3). Additionally, there is no evidence that exposure to immigrants in school affected managers' selection into sectors or firms with a higher share of non-Western immigrants (columns 4 and 5).

Our main results are reported in Table 4. They document that exposure to immigrant children in primary school positively impacts the likelihood of hiring immigrants once one is a manager later on in life. In particular, we find in column (1) that managers who were in cohorts with more non-Western immigrant children of the same gender hire more non-Western immigrants than those with little exposure to immigrants in school. The effect is robust, exploiting differences in hiring be-

havior between managers within the same plant by adding plant fixed effects to our model (column 2). This is our preferred specification that we use throughout the rest of the analysis. Moreover, it is noteworthy that we do not find any significant gender differences among managers in the hiring behavior of immigrants. [8]

We then create a binary variable indicating whether a class migrant share of the same or opposite gender is larger than 5%. Given that the average class size is around 20 students, this corresponds to a sort of extensive margin, i.e., of having at least one non-Western immigrant in a class. The results in column (3) show that this extensive margin significantly affects a manager's hiring decisions as well (and, in particular, when they are of the same gender as the manager).

The pattern does not change when we add to our sample immigrant managers, of whom those of non-Western origin are per se more likely to hire non-Western immigrants (column (4)). The latter is consistent with the role of ethnic networks, which have been shown to be an important aspect of hiring decisions by managers with an immigrant background (Åslund et al., 2014).

Results in column (5) show that exposure also matters at the extensive margin, as a higher share of immigrants of the same gender increases the likelihood of hiring at least one immigrant when being a manager.

In terms of magnitude, our benchmark estimate in column (2) implies that going from the average of 1.2 percent non-Western students of the same gender in the cohort to 5 percent (an increase of around 1 within-school standard deviation) increases the share of non-Western immigrants hired by almost .23 percentage points, which is 4.9 percent of the mean.

To better understand the results of column (3) and the share of migrants that is driving the results, we construct dummy variables that take the value of one if the manager had a share of non-Western migrants in their cohort above x%, with x ranging from 0 to 25. In general, we lack power to detect important non-linearities, with the point estimates not significantly different from one another. The gradually

[8] In Appendix C we present additional results regarding the heterogeneity of the effects. In particular, Table C1 shows that the effect is statistically equal in schools in which the migrant share was above or below the threshold of 6%, for firms above and below the median size of 37 employees, for female and male managers, for managers who left school more or less than 20 years earlier, and for hirings that happened before or after 2012.

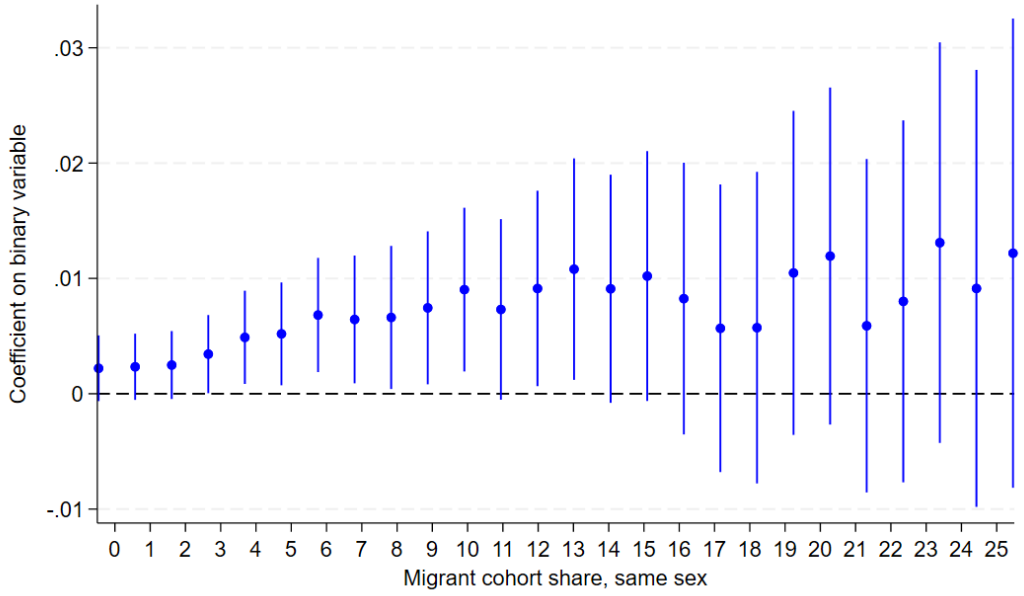
Table 4: Baseline results

	(1)	(2)	(3)	(4)	(5)
LHS variable	Share of recruitments that are non-Western migrants				Hired any immigrants
Non-Western migrant share, same gender	0.082***	0.060***		0.053**	0.21*
	(0.029)	(0.021)		(0.021)	(0.11)
Non-Western migrant share, opp. gender	–0.0035	–0.0014		–0.011	0.067
	(0.029)	(0.020)		(0.021)	(0.12)
Migrant share $\geq$ 5%, same gender			0.0051**		
			(0.0023)		
Migrant share $\geq$ 5%, opp. gender			–0.0011		
			(0.0022)		
Is Western migrant				0.022*	
				(0.013)	
Is non-Western migrant				0.047***	
				(0.014)	
Plant FEs	N	Y	Y	Y	Y
R^2	0.12	0.78	0.78	0.78	0.70
Dep. var. mean	0.047	0.047	0.047	0.048	0.46
Number of schools	1,314	1,314	1,314	1,314	1,314
N	40,804	40,804	40,804	41,122	40,804

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, and two sets of school-specific time trends. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

increasing may coefficients indicate, however, that our results are not driven by simply having one immigrant in the cohort, but perhaps rather by having at least one immigrant of the same gender in one's class (which will happen in expectation at the 10 % threshold). The relatively small share of students with immigrant shares higher than 10 % (see Figure A1), however, makes it hard to interpret what happens after this point.

Figure 3: Impact across the Distribution



In what follows, we examine the extent to which our results may be due to omitted variable bias by adding more control variables. The corresponding results are presented in Table 5. In column (1), we include several individual characteristics of managers such as parental income, education and age. The fact that our coefficient of interest is almost identical to the one from the benchmark specification (column (2) of Table 4) suggests that our result is not being driven by unobservable characteristics of managers which are correlated with the added controls (Altonji et al., 2005; Oster, 2019).

To control for potentially unobserved relevant factors at the local level, we add in columns (2) and (3) sector and municipality specific trends, respectively. Re-

Table 5: Robustness of the Baseline Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Additional controls	+ Sector trends	+ Municipality trends	+ Pre-recruitments plant migrant share	Excluding Copenhagen	Migrant share weighted by years in school	Including large plants
Migrant share, same gender	0.056***	0.057***	0.060***	0.076***	0.071***		0.018**
	(0.021)	(0.021)	(0.021)	(0.023)	(0.024)		(0.0087)
Migrant share, opp. gender	–0.0053	–0.0074	–0.0037	0.0041	0.013		–0.0013
	(0.020)	(0.020)	(0.020)	(0.024)	(0.023)		(0.0092)
Pre-recruitments plant migrant share				–0.040*			
				(0.023)			
Weighted migrant share, same gender						0.065**	
						(0.026)	
Weighted migrant share, opp. gender						–0.015	
						(0.022)	
R^2	0.78	0.79	0.79	0.79	0.78	0.78	0.75
Dep. var. mean	0.047	0.047	0.047	0.047	0.046	0.047	0.056
Number of schools	1,313	1,313	1,313	1,293	1,225	1,313	1,342
N	40,392	40,364	40,227	32,841	38,676	40,392	94,780

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, two sets of school-specific time trends, and plant fixed effects. The additional controls included in the regression whose results are presented in column (1) are: parents' higher education , parents' income percentile, whether the parents income percentile is not observed, single mother or father, father's and mother's age, school's neighborhood immigrant share and gender composition of the school cohort. Standard errors (in brackets) are clustered at the school level. * p $<$ .10, ** p $<$ .05, *** p $<$ .01

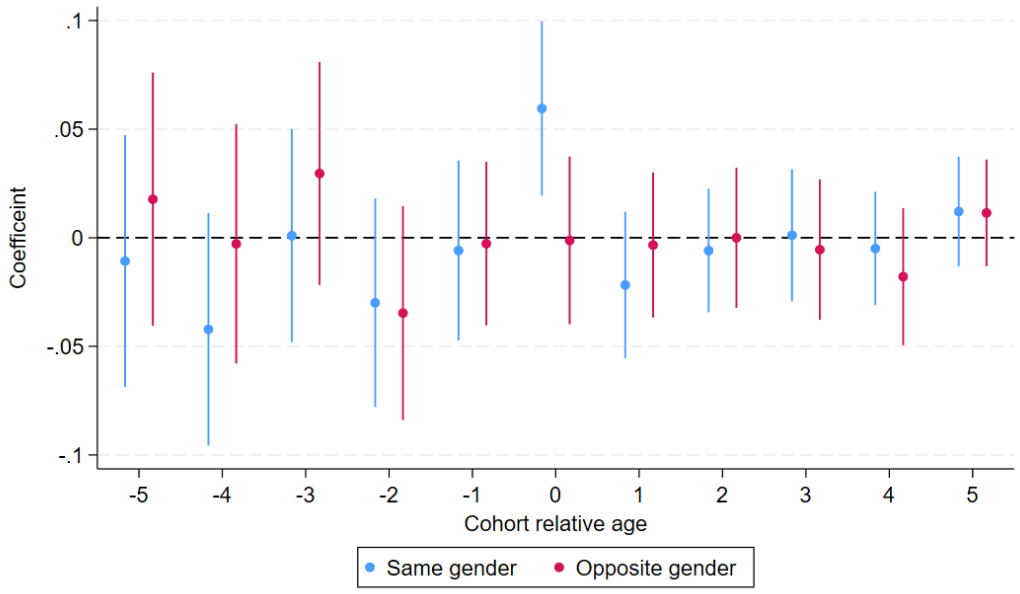
assuringly, the corresponding estimate is again very close to our coefficient from the baseline specification. Next, it could be that our effects are simply due to self-selection of certain managers in firms that would hire many migrants in any case, even if we do not find any evidence of managers' selection in these firms in Table 3. To check for this, we control for the initial immigrant share in a plant (column (4)). We find that the initial immigrant share in a firm is negatively correlated with the current share of immigrant hires, while the coefficient of interest is statistically not different from the benchmark estimate. Overall, the stability of our estimates in Table 5 suggests that unobserved omitted variables are unlikely to affect our results.

This conclusion is also supported by the fact that the opposite gender share in all specifications displayed in Table 5 is insignificant, as most variables of concern that are correlated with the same-gender grade immigrant share would also be correlated with the opposite-gender grade immigrant share. We can apply a similar reasoning by incorporating in our regression the immigrants shares in cohorts relatively close to that of the individual, and regress them against the share of recruits that are non-Western immigrants. The corresponding results are shown in Figure 4. Reassuringly, we find that only the coefficient of the same cohort is significantly different from zero.

To check whether our results could be driven by schools in Copenhagen that are characterized by a higher immigrant concentration than the rest of the country, we provide in column (5) of table 5 estimates when we exclude Copenhagen schools; reassuringly, the coefficient of interest barely changes. The same holds true in column (6) when we weight the immigrant cohort share by giving more weight to immigrants who spent a longer time in school. In other words, our immigrant exposure measure increases with each additional year an immigrant remains in a given cohort. Finally, we expand in column (7) our sample to include managers from establishments with 250 or more employees and those with more than four managers. In most of these cases, we cannot reliably determine who is responsible for hiring decisions. Nevertheless, the estimate remains significant and positive. However, as expected, the effect size is significantly smaller than in the main sample, as this expansion introduces noise and increases measurement error.

Our data also allows us to investigate the timing and persistence of the newly

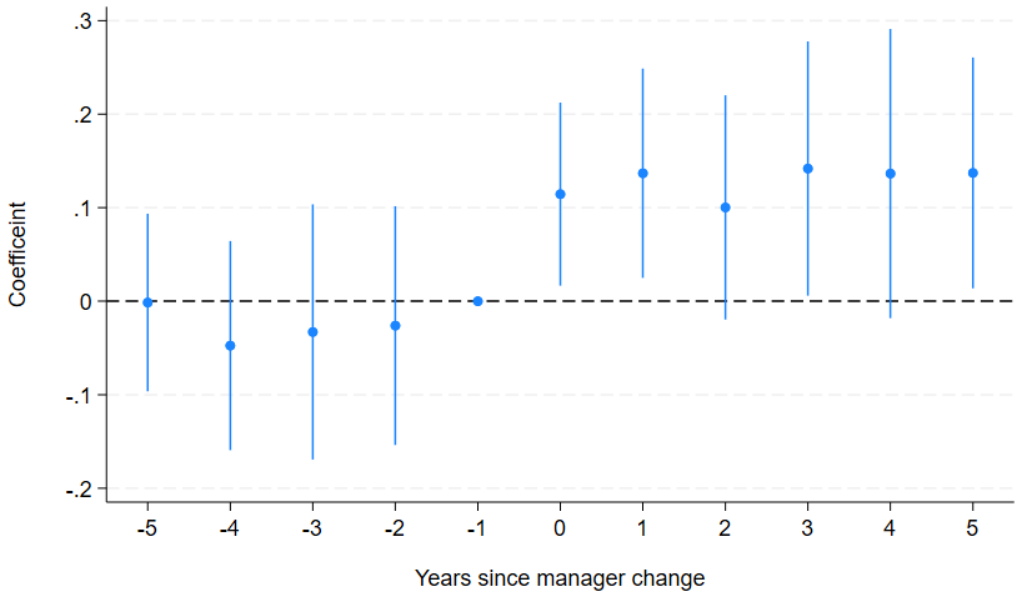
Figure 4: Coefficients on Other Cohorts



appointed manager that had at least one migrant of the same gender in their cohort on the share of non-Wester migrants among recruits. For this, we move to the plant-year level estimation using equation 2, weighting observations so that each manager contributes equally to the regression in order to make our results as comparable to the manager-level regression as possible. Results from this baseline estimation give very similar results to the manager-based baseline (see column 1 of Table C4). To then look at the dynamic impacts of a change in managers, we divide our observations into a set of 'manager changes', where we assign plant-year level observations to the nearest observed change in managers. We then run the regression with 'manager change' fixed effects, and replace the simple exposure measure - i.e. the $ShareImmig_{gcs}$ of the manager in place in plant p in year y - with the change in exposure - i.e. the $ShareImmig_{gcs}$ of the new manager minus that of the old manager - interacted with indicator variables relating to the years before or after the manager change.

Figure 5 displays the coefficients on these interaction terms, which shows that the effect appears as soon as the manager is appointed and it is persistent. Moreover,

Figure 5: Timing of impact of an appointed manager on new hires



we don't see any evidence of potential pre-trends, suggesting that it is not the case that more exposed managers are hired when the firm is in the process of a more general change in it's hiring patterns. We can further break this result down by distinguishing between changes where the manager change led to having a more exposed manager, a less exposed manager, and a similarly exposed manager. This result (in column 2 of Table C4) shows that both types of change impact immigrant recruitment, but the impact of moving to a more exposed manager is substantially greater in magnitude. One possible explanation for this is that the positive impact of an exposed manager on recruitment has a persistent effect even after they leave a plant because of recruitment decisions they made.

5 Potential Mechanisms

There are four key mechanisms that could explain our findings: peer effects in school on educational performance or labor market outcomes, social networks formed during schooling, residential choices of managers, and a reduction in prejudices and

beliefs about immigrants. In the remainder of this section, we examine these mechanisms to refine our results and discuss potential drivers.

First, the effect could be the result of negative peer effects in school. Although results are mixed, some studies have shown that exposure to immigrants in school can generate negative effects for natives as immigrant children often lack native language skills and are more likely to have parents with a low socio-economic background (e.g. Gould et al., 2009; Ballatore et al., 2018).[9]

Table 6: Other outcomes, managers

	(1)	(2)	(3)	(4)	(5)	(6)
LHS variable	Completed higher education	Income percentile at 40	Immigrant share in postcode at 40	Immigrant share in parish at 40	Immigrant share in municipality at 40	Had immigrant partner
Migrant share, same gender	–0.033	3.2	0.030	0.031	0.0096	0.045
	(0.12)	(5.7)	(0.020)	(0.022)	(0.014)	(0.029)
Migrant share, opp. gender	0.10	–7.0	–0.016	–0.024	–0.0054	–0.021
	(0.12)	(4.7)	(0.018)	(0.021)	(0.014)	(0.039)
R^2	0.13	0.12	0.24	0.22	0.32	0.10
Dep. var. mean	0.38	86	0.060	0.053	0.064	0.016
Number of schools	1,314	1,304	1,301	1,301	1,301	1,314
N	40,804	38,615	36,815	36,815	36,869	40,804

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, and two sets of school-specific time trends. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

Hence, we analyze if immigrant exposure in school has any impact on educational and labor market outcomes of native Danes. In particular, we investigate whether the share of non-Western immigrants in a manager's school cohort has an effect on having higher education and on the income percentile at age 40. The corresponding results are reported in columns (1) and (2) of Table 6 and show that both coefficients of the immigrant cohort share are insignificant. The same pattern holds if we run the same regressions on the full sample of all Danes who completed primary school between 1977 and 1996 (see Table C2 in Appendix C).

[9]Overall, the empirical evidence on peer interaction effects of immigrants in school is rather inconclusive as there is evidence of both negative and positive effects of immigrant peers in classroom. For a discussion, see, e.g., Frattini and Meschi (2019).

Therefore, we can conclude that it is unlikely that negative peer effects on the educational performance of students are a driving factor behind our results.

Table 7: Characteristics of New Hires

	(1)	(2)	(3)	(4)	(5)	(6)
LHS variable	Share migrants	Share female	Average age	Share with higher education	Earnings t-1	Earnings t +1
Migrant share, same gender	0.060***	–0.047	0.15	–0.063	11,905	18,927
	(0.021)	(0.042)	(1.1)	(0.043)	(28,149)	(33,930)
Migrant share, opp. gender	–0.0014	0.032	0.39	0.070	–5,512	26,491
	(0.020)	(0.040)	(1.1)	(0.046)	(24,581)	(30,853)
R^2	0.78	0.88	0.86	0.86	0.81	0.84
Dep. var. mean	0.047	1.4	33	0.28	217,325	275,307
Number of schools	1,314	1,314	1,314	1,314	1,314	1,309
N	40,804	40,804	40,804	40,804	40,800	38,207

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, and two sets of school-specific time trends. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

This interpretation of these findings is corroborated when we look at different characteristics of the new hires of managers. Indeed, if managers who had interaction with non-Western migrants in primary school were very different from other managers, we would expect the pool of workers they hire to be different along other characteristics, such as their educational level, earnings, gender (as female workers tend to be clustered in lower-paying occupations and establishments, see Bayard et al., 2003), or age. However, the results presented in Table 7 show no significant effects beyond those on the share of migrants of the new hires.

An alternative explanation could be that managers move to places where there are more immigrants, and hence the pool of potential recruits changes. The findings presented in Table 6 reveal that there is no statistically significant correlation between the cohort's proportion of non-Western immigrants and the geographical concentration in the region where the manager resides at the age of 40. This holds true regardless of whether we assess immigrant concentration at the municipal level (column (5)) or at the sub-municipal level within municipalities (columns (3) and

(4)).[10] When we focus on the whole population instead of only on managers, we find some evidence of an effect of exposure to non-Western migrants in school on the share of non-Western migrants in the municipality where the managers live, somewhat consistent with the findings of Merlino et al. (2024) for the US (see Table C2 in Appendix C). Therefore, we conclude that it is very unlikely that the immigrant cohort share has an impact on residential choices of managers that is sufficiently large to explain our main result.

The third potential explanation is that the effect may be the result of social networks formed in school. The idea is the following. After exposure in school to children of non-Western origin, future managers may have many friends from this group; as many jobs are allocated within one's social network (e.g. Galeotti and Merlino, 2014), treated managers are more likely to hire immigrants. To shed light on this potential mechanism, we analyze heterogeneity in both exposure to different types of migrants and hiring outcomes in our empirical study.

First, we can use the plant-year based analysis to look at heterogeniety according to when the migrants are hired. In column (4) of Table C4, we observe that the effect is just as strong, if not stronger, for recruitments made over twenty years after the manager left school, compared with those made before. This is the opposite of what we would expect with a network based story, where we could reasonably suppose that networks formed in school are likely to be most important in those years just after school completion.

Second, we differentiate between non-Western hires who appear in our school data as children and those who do not, most likely because they immigrated to Denmark after the relevant period. The corresponding results are reported in Table 8. They document a significant effect of exposure on hiring of Non-western immigrants who didn't go to school at the same time as managers in our sample (column (1)), while we do not find any effect for those who completed Danish primary school before 1997. The latter result is driven by the small size of this group, as reflected in the fact that they account for less than 0.5% of total hirings. Given

[10] We identify: (i) the municipality wherein a housing unit is located; (ii) the parish, a local ecclesiastical unit in the Church of Denmark which corresponds to a sub-municipal unit ; and (iii) postcodes.

this low representation, it is unlikely that their hiring patterns significantly shape the overall results. In any case, this pattern strongly speaks against an interpretation that the immediate social networks formed in school are directly driving our results.

Third, we analyze potential impacts of exposure to one group of immigrants on the hiring of other immigrant groups. Estimates in column (3) of Table 8 show that exposure to migrants from Turkey—the predominant country of origin for non-Western immigrants in our school sample—leads to a higher hiring probability of hiring Non-Turkish immigrants. This holds also true when we exclude all Non-Western immigrants from countries which share a common border with Turkey (column 4). On the other hand, we find in column (5) that being exposed to Non-Turkish immigrants in school increases the likelihood to hire Turks later in life. A similar pattern emerges when we divide non-Western immigrants into two groups based on their relative group size in school (see Table C3 in Appendix C). Exposure to immigrants from the largest groups in a school increases the likelihood of hiring immigrants from other countries. These findings further reinforce the evidence that social networks formed in school are not the primary driving force behind the results.

The fourth and last potential mechanism is that the effect could be due to reduced prejudices and beliefs about immigrants in line with the contact hypothesis by Allport (1954). As the data used in this study are register data, we have no direct measure of people's attitudes. Instead, we can exploit the detailed information about the composition of immigrants in in school. Doing so, we can distinguish between Western and Non-Western immigrants, first and second generation immigrant countries, immigrants from Muslim-majority countries and other non-Western immigrants. In doing so, we find in Table C5 in Appendix C that our effects on hiring behavior are driven by exposure to immigrants from predominantly Muslim countries. Moreover, these effects also extend to the hiring of immigrants from non-Muslim majority countries. This pattern speaks against a favoritism toward a specific group and in favor of a general reduction in prejudices and an increased openness to diversity.

Table 8: Evidence for effects across migrant groups

	(1)	(2)	(3)	(4)	(5)
LHS variable	Share of recruitments that are				
	non-Western migrants not in school data	non-Western migrants in school data	non-Turkish migrants	migrants not from Turkey or bordering countries	Turkish migrants
Migrant share, same gender	0.060***	0.00035			
	(0.019)	(0.0080)			
Turk share, same gender			1.7**	0.83	–0.30
			(0.85)	(0.78)	(0.24)
Non-Turk migrant share, same gender			0.032	0.031	0.011**
			(0.023)	(0.020)	(0.0051)
R^2	0.78	0.61	0.78	0.75	0.70
Dep. var. mean	0.042	0.0049	0.047	0.034	0.00065
Number of schools	1,314	1,314	1,314	1,314	1,314
N	40,804	40,804		40,804	40,804

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, and two sets of school-specific time trends. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

6 Conclusions

This paper explores whether exposure to immigrants in schools during childhood influences the hiring behavior of managers. For this purpose, we exploited idiosyncratic variation in cohort composition within Danish schools using rich administrative data. We then show that managers who were exposed to children of non-Western origin during school are more likely to hire non-Western immigrants. This relationship also survives when we include plant fixed effects and a variety of other robustness checks.

While at this stage our results are not conclusive with respect to the mechanism behind these findings, the results are in line with the contact hypothesis of Allport (1954): the higher the share of same-gender pupils of non-Western origin in a cohort, the more likely it is that a Danish manager will hire immigrants of non-Western origin. Our findings suggest that economic opportunities, residential choices of managers, and social networks are unlikely to be the primary drivers of these results. Instead, a likely mechanism is that managers more exposed to immi-

grants in school change their attitudes related to the hiring of immigrants.

Taken at face value, our results have important implications for policymakers as they show that exposure to immigrants in the context of schools can help to increase the labor market integration of immigrants. Policies aiming to increase inter-ethnic contact during childhood in schools and beyond could even help in the long run to reduce labor market discrimination, which is still one of the main barriers to a better integration of immigrants in OECD countries (Zschirnt and Ruedin, 2016; Neumark, 2018; Lippens et al., 2023).

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Appendix A Additional Descriptive Statistics

Figure A1: Distribution of Immigrant Share

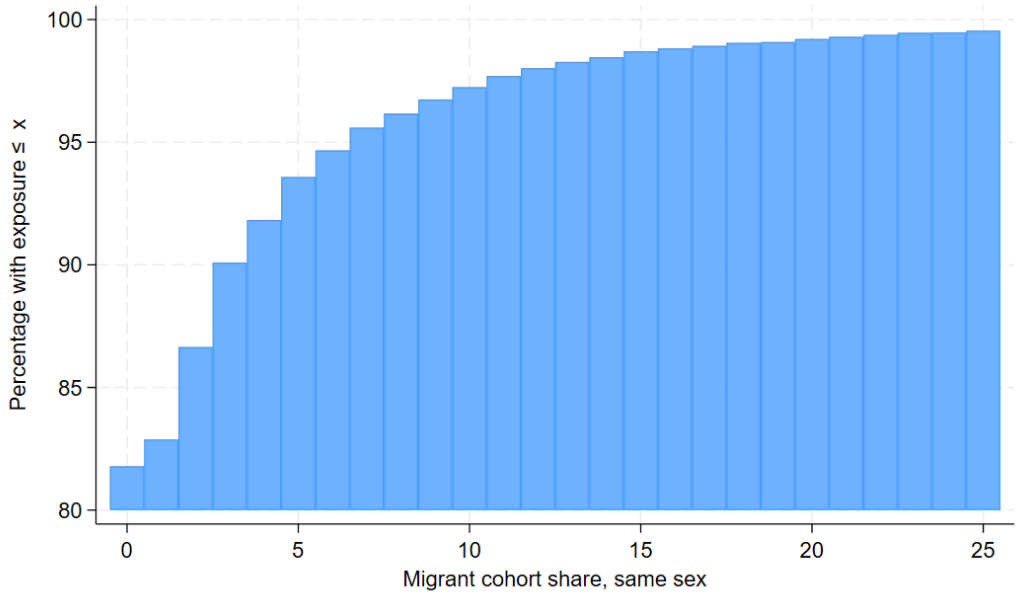


Table A1: Extended Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	Study sample			Full population		
	N	Mean	Within school standard deviation	N	Mean	Within school standard deviation
Female	40,804	0.30	0.46	1.11e+06	0.49	0.50
Year completed primary education	40,804	1985	5.07	1.11e+06	1986	5.39
School cohort size	40,804	66.3	27.0	1.11e+06	64.0	26.2
Migrant share in school cohort, same gender	40,804	0.012	0.039	1.11e+06	0.016	0.047
Migrant share in school cohort, opp. gender	40,804	0.012	0.039	1.11e+06	0.017	0.050
Share of cohort female	40,804	0.51	0.069	1.11e+06	0.50	0.071
Migrant share, municipality	29,565	0.021	0.023	833,939	0.024	0.025
Completed higher education	40,804	0.38	0.49	1.11e+06	0.34	0.47
Income percentile at 40	38,624	86.2	16.8	985,368	75.4	19.8
Mother in register	40,804	1.00	0.023	1.11e+06	1.00	0.057
Father in register	40,804	0.99	0.088	1.11e+06	0.98	0.12
Parents attended higher education	40,656	0.19	0.33	1.10e+06	0.18	0.33
Parents income percentile	40,757	71.2	15.8	1.11e+06	68.7	16.5
Parents income percentile missing	40,804	0.0012	0.034	1.11e+06	0.0042	0.065
Mother's age	40,339	26.3	4.63	1.09e+06	26.4	4.79
Father's age	38,306	28.5	4.67	1.03e+06	28.6	4.78
Migrant share, parents' workplace	23,044	0.0065	0.029	657,934	0.0076	0.031
Employees in plant	40,804	51.7	44.9			
Managers in plant	40,804	2.25	1.28			
Hired more migrants than plant share	35,127	0.22	0.41			
Hired at least one migrant	40,804	0.46	0.50			

Appendix B Additional Balancing Tests

Table B1: Balancing and Random Clustering Tests: Full population

	(1)	(2)	(3)	(4)
	Migrant share, same gender	Migrant share, opp. gender	Dep. var. mean	N
Parents income percentile	–0.69	0.27	69	1,032,713
	(0.77)	(0.69)		
Parents income percentile missing	0.00048	0.0033	0.0036	1,036,445
	(0.0022)	(0.0020)		
Migrant share, parents' workplace	0.0012	0.00049	0.0073	608,330
	(0.0023)	(0.0020)		
Migrant share, address	–0.0023	0.0020	0.010	772,362
	(0.0056)	(0.0046)		
Parents attended higher education	–0.023*	–0.0014	0.17	1,034,232
	(0.014)	(0.013)		
Mother's age	–0.21	0.016	26	1,020,412
	(0.19)	(0.19)		
Father's age	0.025	0.21	29	960,333
	(0.20)	(0.20)		
Mother in register	–0.00056	0.00019	1.0	1,036,445
	(0.0017)	(0.0014)		
Father in register	0.0020	–0.00039	0.99	1,036,445
	(0.0057)	(0.0050)		
Female share	0.0080	–0.0058	0.50	1,036,445
	(0.015)	(0.015)		
Migrant share, municipality	–0.0011	–0.00084	0.023	
	(0.00085)	(0.00079)		
School cohort size	–1.7	–3.2	65	1,036,445
	(2.2)	(2.1)		

Notes: The table reports OLS estimates controlling for year and school fixed effects and two sets of school-specific time trends. Coefficients in the first and second column of each row are from a regression where the variable indicated in each line is regressed both on the same gender and opposite gender shares of non-Western immigrants, respectively. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Appendix C Additional Results

Table C1: Heterogenity of effects

	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.053**					
× migrant share, same gender	(0.026)					
Female	0.071*					
× migrant share, same gender	(0.038)					
Migrant sector share low		–0.0098				
× migrant share, same gender		(0.031)				
Migrant sector share high		0.087***				
× migrant share, same gender		(0.026)				
School municipality size low			0.093**			
× migrant share, same gender			(0.044)			
School municipality size high			0.050**			
× migrant share, same gender			(0.023)			
School cohort size low				0.066***		
× migrant share, same gender				(0.023)		
School cohort size high				0.041		
× migrant share, same gender				(0.046)		
N employees in plant low					0.077**	
× migrant share, same gender					(0.035)	
N employees in plant high					0.047*	
× migrant share, same gender					(0.025)	
N managers in plant low						0.068**
× migrant share, same gender						(0.032)
N managers in plant high						0.053*
× migrant share, same gender						(0.028)
R^2	0.78	0.78	0.78	0.78	0.78	0.78
Dep. var. mean	0.047	.	0.047	0.047	0.047	0.047
Number of schools	1,314	1,314	1,314	1,314	1,314	1,314
N	40,804	40,804	40,804	40,804	40,804	40,804

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, and two sets of school-specific time trends. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

Table C2: Other outcomes, full population

	(1)	(2)	(3)	(4)	(5)	(6)
LHS variable	Completed higher education	Income percentile at 40	Immigrant share in postcode at 40	Immigrant share in parish at 40	Immigrant share in municipality at 40	Had immigrant partner
Migrant share, same gender	–0.0031	–0.83	0.0048	0.0069*	0.0063**	–0.0016
	(0.023)	(0.99)	(0.0033)	(0.0039)	(0.0025)	(0.0064)
Migrant share, opp. gender	–0.016	–0.13	–0.0018	–0.000093	–0.0019	–0.0015
	(0.020)	(0.91)	(0.0030)	(0.0034)	(0.0023)	(0.0060)
R^2	0.070	0.038	0.18	0.13	0.28	0.0068
Dep. var. mean	0.33	76	0.063	0.058	0.066	0.019
Number of schools	1,314	1,312	1,310	1,310	1,310	1,314
N	1,035,984	939,471	880,181	880,181	881,821	1,036,445

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, and two sets of school-specific time trends. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

Table C3: Impacts of biggest migrant groups in school

	(1)	(2)	(3)	(4)
LHS variable	Share of recruitments that are			
	migrants from biggest non-Western migrant group in school	non-Western migrants not from biggest migrant group in school	migrants from two biggest non-Western migrant groups in school	non-Western migrants not from two biggest migrant groups in school
Biggest migrant group in school share, same gender	0.010	0.045		
	(0.0081)	(0.029)		
Other migrants share, same gender	0.018**	0.084***		
	(0.0082)	(0.030)		
Two biggest migrant groups in school share, same gender			0.0021	0.077***
			(0.0082)	(0.026)
Other migrants share, same gender			0.0066	0.070**
			(0.011)	(0.035)
R^2	0.14	0.12	0.14	0.11
Dep. var. mean	0.0038	0.043	0.0049	0.042
Number of schools	1,314	1,314	1,314	1,314
N	40,804	40,804	40,804	40,804

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, and two sets of school-specific time trends. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

Table C4: Regressions at the establishment-year level

	(1)	(2)	(3)	(4)
Observation level	Manager	Senior managers	Plant-year	
Migrant share, same gender	0.096***			
	(0.037)			
Migrant share, opp. gender	–0.026	0.00095		
	(0.040)	(0.043)		
Exposure decreases		–0.096**		
× post-manager change		(0.045)		
Exposure constant		0.00030		
× post-manager change		(0.0016)		
Exposure increases		0.17***		
× post-manager change		(0.044)		
n managers in data low			0.15***	
× migrant share, same gender			(0.043)	
n managers in data high			0.017	
× migrant share, same gender			(0.072)	
$\leq$ 20 years after left school				0.096*
× migrant share, same gender				(0.058)
$>$ 20 years after left school				0.21***
× migrant share, same gender				(0.051)
R^2	0.51	0.56	0.58	0.58
Dep. var. mean	0.049	0.049	0.049	0.049
Number of schools	1,302	1,302	1,302	1,301
N	78,048	77,927	77,880	77,863

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, and two sets of school-specific time trends. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

Table C5: Results for different migrant categories

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LHS variable	Share of recruitments that are							
	non-Western migrants	Western migrants	non-Western migrants	non-Western 1st gen migrants	non-Western 2nd gen migrants	non-Western migrants	migrants from muslim majority countries	non-Western migrants not from muslim majority countries
Migrant share, same gender	0.060***	–0.018						
	(0.021)	(0.015)						
Western share, same gender	0.0064	–0.048						
	(0.042)	(0.038)						
1st-gen migrant share, same gender			0.064***	0.071***	–0.0064			
			(0.025)	(0.021)	(0.014)			
2nd-gen migrant share, same gender			0.048	0.011	0.037			
			(0.038)	(0.030)	(0.024)			
Muslim migrant share, same gender						0.080***	0.054***	0.027*
						(0.026)	(0.020)	(0.016)
Non-muslim migrant share, same gender						0.026	0.017	0.0073
						(0.036)	(0.029)	(0.022)
R^2	0.78	0.78	0.78	0.76	0.73	0.78	0.75	0.74
Dep. var. mean	0.047	0.031	0.047	0.034	0.013	0.047	0.027	0.020
Number of schools	1,314	1,314	1,314	1,314	1,314	1,314	1,314	1,314
N	40,804	40,804	40,804	40,804	40,804	40,804	40,804	40,804

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, and two sets of school-specific time trends. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01