The Impact of Interethnic Contact in Schools on Managers' Hiring Decisions*

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Abstract

This paper analyzes whether interethnic contact in childhood shapes managers' hiring behavior. Exploiting quasi-random variation in the share of same-gender non-Western migrant peers across cohorts within Danish primary schools, we use administrative employer-employee data to show that such contact leads managers to hire significantly more migrants from this group later in life. Exploring several mechanisms, we find evidence most consistent with a change in managers' attitudes toward migrants. To test this directly, we link the same school records to the European Social Survey and show that childhood contact reduces prejudice and increases support for immigration in the broader population.

JEL classifications: I29, J15, J61, J71.

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1 Introduction

Faced with structural changes and demographic aging in OECD countries, the labor market integration of migrants and their descendants is becoming increasingly important (OECD, 2023). Yet substantial evidence documents ethnic and racial discrimination in recruitment processes (e.g., Evsyukova® et al., 2024), representing a persistent barrier to the economic inclusion of migrants (Oreopoulos, 2011; Neumark, 2018). While some work has examined how features of the recruitment process affect these barriers (Behaghel et al., 2015; Berson et al., 2020), there has been little attention in the economics literature to the individuals involved in the recruitment process—i.e., the managers within the hiring firms.[1] In particular, we know little about which individual-level factors influence managers' propensity for in-group favoritism in hiring.

Our paper addresses this research gap by investigating whether contact with migrants in childhood shapes the hiring behavior of managers. To obtain exogenous variation in childhood contact, we exploit idiosyncratic differences in cohort composition between managers who attended the same primary school.[2] We focus on a period in Denmark when switching schools was difficult and, in addition to exploiting variation across cohorts, we leverage within-cohort variation across genders. This approach is motivated by evidence that children, especially at the ages we study, predominantly form close friendships with same-gender peers, suggesting that these peers may be especially influential for attitude formation and future choices.[3]

[1] Glover et al. (2017) show that variation in bias across managers within a firm is an important determinant of employee performance, but they do not examine recruitment behavior.

[2] This approach builds on the methodology proposed by Hoxby (2000), which has been widely used to identify peer effects (see, e.g., 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; Cattan et al., 2025; Bailey et al., 2026).

[3] Gender segregation among primary school friendships is well-documented: children spend up to 10 times more of their free-time with same-gender peers than with opposite gender peers (Mehta and Strough, 2009), 77% of Danish children's friendships are same-gendered, and children report that it is easier to discuss important issues within such friendships (Bagues and Zinovyeva, 2025). In the US, only the racial composition of same-gender peers shapes friendship groups and behavior (Merlino et al., 2019, 2024). Finally, outgroup information is most effective at creating empathy when it is relatable, and the dimensions of relatability vary strongly by gender (Andries et al., 2026).

We begin by presenting balance and clustering tests, which indicate that the variation we exploit is effectively as good as random. We focus on contact with non-Western migrants, who constitute the largest share of the migrant population in our context and face significant labor market disadvantages and discrimination in Denmark (Liebig, 2007; Dahl and Krog, 2018). We find no evidence that school-based contact with non-Western migrant peers affects the likelihood of becoming a manager, ruling out selection into the manager population.

Our main finding is that managers hire more non-Western migrants when they had more contact with non-Western migrants of the same gender in school. The effects are economically meaningful—a manager whose same-gendered school cohort contained at least 10% migrants hires around 23% more migrants than average. Using an event-study design focusing on within-plant variation and centered on the timing of manager changes, we find the effect to be immediate and persistent.

Our results are robust across a wide range of alternative specifications, including controls such as peers' parental background and school-year fixed effects, among others. To confirm our causal interpretation, we implement an instrumental variables approach that assigns managers to schools based on their childhood residential location, yielding results very similar to our baseline.

We then investigate the mechanisms underlying our main result, and find compelling evidence that childhood contact with migrants induces a persistent change in attitudes toward migrants. To isolate this channel, we first examine and rule out a range of alternative explanations. We find no significant effects on managers' educational performance, income, or choice of residential location. The employees they recruit also do not vary according to other observable characteristics, such as their age, education, or pre-recruitment earnings. We also rule out a social network explanation: the effects are not driven by migrants who attended school at the same time as the hiring managers, they are equally strong for hirings occurring far from the manager's schooling location, and they persist for hires made more than two decades after school completion—patterns inconsistent with the effects being driven by personal connections formed in school.

Exploring variation in impacts across migrant groups yields several results consistent with a general reduction in discrimination towards non-Western migrants.

The effects are strongest when we consider contact with and hiring of migrants from Muslim-majority countries—a group that faces particularly high levels of discrimination in European labor markets (e.g., Adida et al., 2010). Moreover, contact with non-Western migrants from one set of countries impacts hiring from other non-Western groups: for instance, contact with Turks in school increases the hiring of non-Western migrants not from Turkey or its bordering countries. The effects are also specific to non-Western migrants: we find no impacts on the recruitment of Western migrants, consistent with such migrants facing much less discrimination.

To provide direct evidence of a change in attitudes, we draw on an additional data source: nine waves of the European Social Survey. Linking survey respondents to the administrative school data and focusing on individuals in the same cohorts as our managers, we apply a similar identification strategy to study long-run attitude formation. The childhood contact that shapes hiring behavior also produces measurable changes in attitudes and beliefs in the broader population: Danes who had more contact with migrant peers in school exhibit significantly lower prejudice in adulthood—they are more likely to have migrant friends, view immigration as beneficial, and support more liberal immigration policies. We find no impact on general trust or confidence in political institutions, suggesting that contact does not affect broader political orientations. Together, these findings provide direct evidence that early interethnic contact shapes long-run attitudes and support the hypothesis that these changes drive the hiring effects we document.

Our paper contributes to the literature on the labor market integration of migrants, which has documented that migrants in OECD countries often experience worse employment outcomes than natives (e.g., Bratsberg et al., 2014; Hermansen et al., 2025). The literature has identified several characteristics of migrants as crucial factors for successful integration, such as language skills (Bleakley and Chin, 2004), legal status (Adda et al., 2025) and citizenship (Govind, 2021). 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 (Foged et al., 2024), geographic dispersal policies (Damm, 2009), and legal employment opportunities (Fasani et al., 2021). However, few papers have examined the role of managers in determining whether migrants

get hired, despite their significant impact on migrants' labor market integration in their function as gatekeepers. Exceptions are Åslund et al. (2014), Giuliano et al. (2009) and Bandiera et al. (2009), who find that migrant managers are more likely to hire migrants. Our findings complement this literature by showing that contact with migrants in early life can help to mediate in-group biases in hiring behavior by non-migrant managers.

We also contribute to the literature on the consequences of interethnic and interracial 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), recent studies have started to analyze long-term behavioral consequences such as the formation of interracial relationships (Merlino et al., 2019), attitudes to ethnic minorities (Achard et al., 2024), political partisanship (Schindler and Westcott, 2021), political identity and development (Bazzi et al., 2019; Kok et al., 2025) and residential choices (Merlino et al., 2024). Yet there is no evidence on whether such contact has economically meaningful effects on labor markets. We fill this gap by providing, to the best of our knowledge, the first causal evidence on the long-run impact of ethnic contact on hiring behavior—an outcome directly relevant for the labor market integration of migrants. Combining administrative data with survey evidence, we further show that these behavioral patterns are mirrored in persistent differences in attitudes toward migrants.

Finally, we make an important contribution to the ongoing discussion on the generalization of intergroup contact effects (Mousa, 2020; Lowe, 2021; Alrababah et al., 2021). A recent meta-analysis of experimental studies by Lowe (2025) shows that the positive effects of contact often remain specific to the individuals met. Our observational setting with contact sustained over years of shared schooling and outcomes measured up to decades later reveals a different picture: such contact with non-Western peers in childhood positively influences managers' hiring behavior toward other migrants both within and across ethnic groups.

The remainder of the paper is organized as follows. Section 2 provides background information about migrants 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. Section

6 concludes the paper.

2 Background

This section reviews Denmark's immigration history and primary school system, which informs our analysis of later labor market behavior.

2.1 Immigration to Denmark

Until the mid-20th century, Denmark was primarily a country of emigration. This pattern changed in the 1960s, when substantial inflows of labor migrants arrived from non-Western countries, particularly Turkey, Pakistan, and Yugoslavia. The guest worker program ended after the oil-price shock in 1973, but immigration continued through family reunification and refugee inflows, peaking during the civil war in the former Yugoslavia in 1993–1994 (Liebig, 2007).

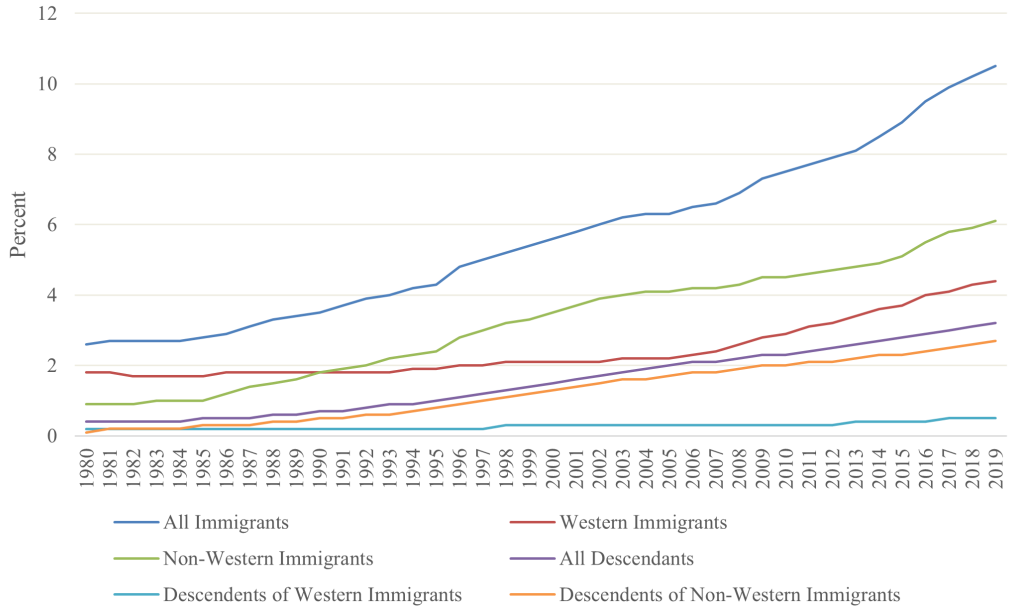
As illustrated in Figure 1, the share of non-Western migrants in Denmark increased steadily from the late 1980s onward.[4] As a result, migrant presence in Danish schools since the mid-1980s has mainly involved children of non-Western origin. In the labor market, non-Western migrants account for approximately 2 to 8% of the Danish workforce in our period of interest (1996–2019) and consistently face worse outcomes than native-born workers (Tranæs and Zimmermann, 2004). The labor market position of migrants has therefore been central to public debate and policy in Denmark. Experimental evidence shows that discrimination remains an important barrier: Dahl and Krog (2018) document significantly lower callback rates for applicants with Middle Eastern-sounding names.

Overall, Denmark's immigration trajectory closely mirrors broader trends across Western Europe—non-Western migrant populations grew substantially from the mid-1990s onward and consistently faced worse labor market outcomes than native-born workers—suggesting that our findings are likely to generalize beyond the Danish context (OECD, 2023). Further details on Denmark's immigration history are

[4] We follow Statistics Denmark's official classification of migrants by origin (see Appendix Table A1).

provided in Appendix A.1.

Figure 1: Share of migrants in Denmark



Notes: This figure plots migrants and descendants as a share of the Danish resident population, separately for Western and non-Western origin, from 1980 to 2019. Definitions follow Statistics Denmark. Data source: Statistics Denmark.

2.2 The Danish School System

Compulsory schooling in Denmark's public primary schools (Folkeskole) spans nine years (grades 1–9, ages 6–16) (Jørgensen, 2017). Public primary schools are administered by municipalities and are free of charge. We restrict our sample to cohorts entering primary school before the 1989 Act on the Folkeskole introduced free school choice. Prior to this, students automatically attended the public school assigned to their home address limiting the scope for parental sorting to drive variation in cohort composition. In our study period, the vast majority of students attended public schools: in 1989, 89.7% of students completed primary school in public schools, and the share in private schools grew only modestly until the 2000s.

Although school switching is relatively uncommon, transitions from public to private schools are concentrated late in compulsory schooling: roughly 87% of

moves occur during eighth or ninth grade (Undervisningsministeriet, 2003). We therefore define school cohort composition as the share of migrant-origin students in an individual's grade cohort, measured at the start of eighth grade. This timing limits endogenous changes driven by selective moves, better reflects cumulative peer contact over the first 8–9 years of schooling, and is the first point at which grade-level information is consistently observed in our data. We show below that our conclusions are unchanged when excluding private school students or using alternative definitions of peers. Further institutional details are provided in Appendix A.2.

3 Data and Estimation Strategy

3.1 Data

Our empirical analysis relies on longitudinal Danish administrative register data covering the entire population. Provided by Statistics Denmark, these registers allow for high-quality, consistent tracking of individuals over time via anonymized personal identification numbers. We use registers from 1977–1996 to measure managers' primary-school cohort composition and registers from 1996–2019 to observe subsequent labor-market outcomes and hiring decisions.

The Compressed Student Register (KOTRE) covers all individuals enrolled in eighth and ninth grade in Danish primary schools since 1977. For our analysis, we restrict to cohorts observed in eighth grade up to 1996, i.e., cohorts that began primary school before the 1989 Folkeskole reform introducing free school choice.

We construct the universe of employer-employee matches from 1996 to 2019 using the Integrated Database for Labor Market Research (IDA), creating a complete labor market history for all individuals. Managers are defined as employees whose occupational code begins with the digit denoting managerial positions, following Ronchi and Smith (2026). We exclude self-employed and owners as administrative records do not allow us to distinguish owners who actively manage day-to-day operations, including personnel decisions, from those who are primarily passive. We define a hire in plant p as occurring when an individual is observed working at p in

year t but was not employed at the same plant or firm in any of the preceding five years. All hires at a given plant in year t are then assigned to each full-time manager employed at that plant in that year.[5] Since our preferred specification includes plant fixed effects, we assign each manager to a particular plant—the one at which they served as manager the longest—and include only hires made at that plant.

Demographic characteristics, including immigration status and country of birth, are taken from the Population Register (BEF) and the Immigration Register (IEPE). Following Statistics Denmark's classification (see Appendix Table A1), we define non-Western migrants as people either born in a non-Western country or those Danish-born without any Danish parents and with a non-Western migrant mother.[6] We use this definition to construct two central variables: the non-Western share within each school-cohort (separately for girls and boys), and, for each manager, the share of hires that are non-Western migrants.[7]

The cohort-level migrant share serves as a meaningful proxy for interpersonal contact in school, given that the institutional structure ensures a tight link between cohort composition and daily student interactions. Classes are capped at 28 students, and class assignments are stable (Appendix A.2). As a result, even small cohort shares translate into a non-trivial probability of having migrant classmates with whom students interact daily over several years.

Our final sample includes all Danish managers without a migration background who were in grade 8 of a Danish primary school between 1977 and 1996 and work in plants with fewer than 250 employees and 4 managers or less.[8] This yields 49,680 managers from around 1,300 primary schools working in approximately

[5] When more than one full-time manager is employed at a plant in a given year, the same hire is assigned to each of them. To show that this multiple assignment does not drive our findings, Appendix Table C7 reports alternative specifications using stricter manager definitions, including one that retains only a single manager per plant-year, selected by rank, earnings, and tenure. The results remain similar and, if anything, are slightly stronger under this stricter assignment rule.

[6] In the remainder of the paper, we use the term migrants to refer to non-Western migrants, unless explicitly noted otherwise.

[7] Appendix Figure B1 shows the share of non-Western migrants in managers' school cohorts. The positive trend mirrors that of the full population in Figure 1, with Turkish migrants consistently the largest single group.

[8] We exclude large plants and plants with many managers because we cannot identify which manager is responsible for hiring decisions, and so including such plants will increase noise.

Table 1: Summary Statistics

	(1)	(2)	(3)
	N	Mean	Within school standard deviation
Birth year	49,680	1971	5.02
Female	49,680	0.29	0.45
School cohort size	49,680	66.6	27.1
Migrant share in school cohort, same gender	49,680	0.012	0.038
Migrant share in school cohort, opp. gender	49,680	0.012	0.039
Number of hirings	49,680	34.0	59.5
Migrant share of hirings	49,680	0.047	0.091

Notes: This table reports summary statistics for the manager analysis sample. *Within-school standard deviation* is computed across cohorts within the same primary school. *Migrant share of hirings* summarizes the share of hires that are non-Western migrants by manager.

15,000 plants or establishments.

Table 1 presents the summary statistics of the main variables. Managers are predominantly male (71%), consistent with Smith et al. (2021), and completed primary school on average in 1985, with cohort sizes of around 66 students. The average share of non-Western migrants within gender-by-cohort cells in these cohorts was 1.2%, both for same- and opposite-gender students. Appendix Figure B2 shows that over 95% of cohorts had less than 8% migrants, and more than 80% had none. However, there is substantial variation, with an average within-school standard deviation of almost 4 percentage points. This reflects both the growth of migrant populations over time and the small cohort sizes, where one or two migrants can generate large percentage differences within a gender group.[9]

The table also shows that our data is well-suited for the analysis: the average manager has 34 hires, with 4.7% involving a migrant. Roughly 45% of managers hired a migrant at some point (Appendix Table B1). Managers do not differ from the broader population in key dimensions, including contact with migrants in school.

[9] Although there has been substantial immigration to Denmark since the 1990s, even in 2020, more than 70% of Danish students were not exposed to non-Western migrant shares exceeding 10%, a threshold we later use in our analysis.

3.2 Estimation Strategy

A naïve regression of hiring behavior on migrant 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, school choice may reflect parental preferences. For example, parents with particular views on ethnic diversity or different socioeconomic backgrounds may select into schools with systematically different migrant shares.

To address these concerns, we exploit within-school variation in the gender-specific share of non-Western migrant students across cohorts. 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 and parents are even less likely to know or act on the gender-specific migrant share of a cohort.

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

$$Y_i = \beta_1 \, ShareMig_{gcs} + \beta_2 \, ShareMig_{ocs} + \gamma_c + \delta_s + F_i + X_{sc} + \varepsilon_i, \qquad (1)$$

where the main dependent variable Y_i is the share of migrants among all workers hired by manager i during their career. $ShareMig_{gcs}$ is the share of non-Western migrants among students of the same gender g as manager i in cohort c in school s, $ShareMig_{ocs}$ is defined analogously for students of the opposite gender, F_i is an indicator equal to one if manager i is female, γ_c and δ_s are cohort and school fixed effects respectively, X_{sc} are school-specific controls and time trends to take into account the evolution of migrant shares in schools in our sample period,[10] and ε_i is

[10] Specifically, we control for the share of non-Western migrants in all cohorts up to 5 years older or young than the manager's cohort. 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 migrants in Denmark starting from the mid-80s that is apparent in Figure 1.

a random error term. Standard errors are clustered at the school level.

We also conduct analyses at the plant-year level, which enables us to study how hiring dynamics within plants change over time. The specification closely follows the baseline equation but incorporates a time dimension:

$$Y_{py} = \beta_1 \, ShareMig_{gcs} + \beta_2 \, ShareMig_{ocs} + \gamma_c + \delta_s + \lambda_y + \mu_m + F_i + X_{sc} + \varepsilon_{py}, \quad (2)$$

where Y_{py} is the share of migrants among workers hired in plant p in year y. The characteristics on the right-hand side—i.e., the school migrant shares and various controls—are those of the manager i who is in plant p in year y.[11] Instead of plant fixed effects, we here include year fixed effects, λ_y, and 'manager change' fixed effects, μ_m. Essentially, we define a 'manager change' to occur when the identity of the most senior manager in a plant is different from that of the most senior manager in the previous year—e.g., after the previous manager left and/or a new one arrived. We then assign plant-year level observations to the nearest observed change in managers. In most cases, we only have one manager change in the data for a given plant, so in these cases, this is effectively a plant fixed effect. But when we have more than one manager change, this essentially treats the two changes as if they were in two different plants.

3.3 Identification Assumptions

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 key idea is that deviations between a school's average migrant composition and the realized composition of a specific cohort are not observable or predictable to parents at the time of enrollment and therefore do not influence school choice.[12] This as-

[11] When we have more than one manager in a plant, we use the characteristics of the most senior manager—that is, the manager who has been in a plant the longest or, where this is equal, the one with the highest earnings.

[12] An alternative identification strategy uses Denmark's 1986 refugee dispersal policy (see Damm, 2009). However, this approach is less suited to our research question. Focusing only on refugees would capture a limited subset of early interethnic contact, as interactions with other migrant groups may also influence later hiring behavior. Additionally, restricting the analysis to dispersed refugees

sumption has two testable implications.

First, we examine whether within-school variation in the gender-specific migrant share is balanced with respect to predetermined characteristics. If this variation is quasi-random, it should be uncorrelated with predetermined individual characteristics. We test this by regressing a range of parental and background variables on the share of non-Western migrants in an individual's gender-specific peer group, controlling for year and school fixed effects, as well as school-specific time trends—i.e., estimating Equation (1) with these variables as the dependent variable.[13]

The results in Table 2 support our identification assumption. Only one coefficient—emigration before primary school completion—is significant at the 10% level.[14] We do not find any evidence that cohort size and gender share are correlated with the share of non-Western migrants in a cohort.

An advantage of our data is that we can replicate this analysis for the full population who completed a Danish primary school between 1977 and 1996, irrespective of whether they became managers or not. The corresponding results are reported in Appendix Table C1. This pattern is similar: only one coefficient is significant. Taken together, these findings support 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.

Second, we examine whether there is evidence of non-random clustering of migrant students across cohorts within schools. If migrant allocation reflected systematic sorting, one would expect clustering across genders within cohorts. We therefore regress the same-gender migrant share on the opposite-gender migrant share within cohorts. We find no significant relationship, and the same holds in the full-

would significantly reduce statistical power, given the small number of managers exposed to this policy within our observation window.

[13] Some parental variables are observed for certain years, leading to smaller samples in those specifications.

[14] Our results remain robust when controlling for pre-completion emigration status as well as for all other covariates listed in Table 2. We also run an additional test where we put the same-gender cohort migrant share on the left hand side, and all predetermined characteristics on the right hand side, along with our standard set of controls. Conducting a joint F-test that the coefficients on the predetermined characteristics are all equal to zero gives us a p-value of 0.68.

Table 2: Balancing Tests

	(1)	(2)	(3)	(4)
	Migrant share, same gender	Migrant share, opp. gender	Dep. var. mean	N
Mother in register	–0.0045	0.0024	1.0	49,680
	(0.0053)	(0.0037)		
Father in register	0.0052	0.0068	0.99	49,680
	(0.029)	(0.025)		
Parents income percentile	1.5	2.0	71	49,624
	(3.7)	(3.9)		
Parents income percentile missing	0.012	–0.0055	0.0011	49,680
	(0.0085)	(0.0043)		
Parents attended higher education	–0.016	–0.019	0.20	49,672
	(0.070)	(0.069)		
Parents' age	–1.1	–1.0	28	49,356
	(1.1)	(1)		
Parents are members of church	–0.0048	–0.017	0.92	35,479
	(0.067)	(0.064)		
Migrant share, municipality	–0.0027	0.00079	0.018	49,495
	(0.0029)	(0.0028)		
Migrant share, postcode	–0.0011	0.0045	0.017	48,458
	(0.0067)	(0.0052)		
Migrant share, address	0.0083	–0.014	0.0064	49,496
	(0.021)	(0.023)		
Emigrated before grade 8	–0.052*	0.039	0.015	49,680
	(0.028)	(0.032)		
School cohort size	–3.3	–2.9	67	49,680
	(3.3)	(2.9)		
Share of cohort female	0.018	–0.0034	0.51	49,680
	(0.024)	(0.024)		

Notes: The table reports OLS estimates controlling for cohort and school fixed effects, two sets of school-specific time trends, and for the share of non-Western migrants in all cohorts up to 5 years older or younger than the individual's cohort. 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. Non-self explanatory variables are defined as follows: *higher education* indicates having completed a bachelor's degree or higher; *Members of church* indicates being a paying member of the Danish state church; *postcode* denotes sub-municipal area codes (approximately 1,100 in Denmark); and *address* is a unique building identifier. Coefficients close to zero support the assumption that within-school cohort-to-cohort variation in migrant shares is approximately as-good-as random. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

population sample (columns (1) and (2) of Appendix Table C2). Since any parental sorting mechanism would likely affect both gender-specific shares similarly, the absence of correlation provides additional support for quasi-random cohort composition within schools.

We further implement two formal tests proposed by Guryan et al. (2009) and Caeyers and Fafchamps (2024), which assess whether an individuals' characteristics —i.e., migrant background—are correlated with those of their peers while correcting for the mechanical bias that arises when peer groups exclude the individual. Appendix Table C2 reports the results using the full population of schoolchildren (excluding younger siblings). Column (3) presents the naïve specification, while columns (4) and (5) apply the respective bias corrections. Across all specifications, the relevant coefficient is small and statistically insignificant, further supporting the view that variation in migrant shares across cohorts can be treated as good as random.

Another key assumption underpinning our identification strategy is that contact with migrants in school does not systematically influence natives' later decisions to become managers. To address this concern, we analyze whether contact with migrants in school affects the chance of becoming a manager using the full sample of the total Danish population who were in grade 8 of a Danish primary school between 1977 and 1996. The corresponding results are reported in column (1) of Table 3. We do not find any evidence that contact with migrants 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 migrants if they were in contact with migrants in school (columns (2)-(3)). Additionally, there is no evidence that contact with migrants in school affected managers' selection into sectors or firms with a higher share of non-Western migrants (columns (4)-(5)), though the point estimates are economically sizable and suggest the possibility of some sorting into such firms. This pattern is consistent with our broader interpretation and motivates our preferred specification which includes plant fixed effects, as well as one of our robustness checks where we control for the pre-existing workforce composition.

Table 3: Probability of Being a Manager

	(1)	(2)	(3)	(4)	(5)
LHS variable	Being a manager in our sample	Log of number of hires	Migrant share in plant municipality	Migrant share in firm sector	Migrant share in plant before manager joined
Migrant share, same gender	–0.0081	0.24	–0.0038	0.018	0.024
	(0.0073)	(0.27)	(0.013)	(0.011)	(0.020)
Migrant share, opp. gender	–0.0029	0.094	0.0041	–0.0024	–0.0033
	(0.0066)	(0.27)	(0.011)	(0.012)	(0.020)
R^2	0.015	0.085	0.23	0.13	0.12
Dep. var. mean	0.046	2.8	0.067	0.044	0.028
Number of schools	1,317	1,317	1,312	1,306	1,312
N	1,080,924	49,680	42,843	36,168	41,145

Notes: The table reports OLS estimates controlling for gender, cohort, and school fixed effects, two sets of school-specific time trends, and for the share of non-Western migrants in all cohorts up to 5 years older or younger than the manager's cohort. This table tests whether school-cohort exposure predicts selection into management, number of recruitments, or into more migrant-intensive jobs/locations. Column (1) uses the full population of individuals completing Danish primary school in the relevant cohorts; other columns use the manager sample (as indicated by the table). Small and statistically insignificant coefficients indicate that exposure does not predict becoming a manager, hiring volume, or sorting into higher-migrant establishments/sectors/locations. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

4 Results

Our empirical analysis is organized as follows. First, we present the main estimates of the effect of migrant contact on hiring behavior. Next, we assess the robustness of our findings to alternative specifications. Finally, we examine the timing and persistence of the observed effects within plants.

4.1 Main Results

Our main estimates are presented in Table 4. They document that contact with children with a migrant background in primary school positively affects managers' likelihood of hiring migrants later in life. Specifically, column (1) reveals that managers who were in cohorts with more non-Western migrant children of the same gender hire more non-Western migrants than those with less contact with migrants in school. Contact with more non-Western migrants of the opposite gender does not have a significant influence. This is consistent with same-gender peers being the relevant margin of contact: children at the ages we study predominantly form

Table 4: Baseline results

	(1)	(2)	(3)	(4)	(5)
LHS variable	Share of hires that are migrants				Hired any migrants
Migrant share, same gender	0.10***	0.081***		0.076***	0.25**
	(0.027)	(0.019)		(0.020)	(0.10)
Migrant share, opp. gender	0.016	0.0026		–0.0071	0.11
	(0.025)	(0.019)		(0.020)	(0.10)
Migrant share $\geq$ 10%, same gender			0.011***		
			(0.0034)		
Migrant share $\geq$ 10%, opp. gender			0.00046		
			(0.0033)		
Is Western migrant				0.014	
				(0.010)	
Is non-Western migrant				0.044***	
				(0.012)	
Plant FEs	N	Y	Y	Y	Y
R^2	0.10	0.73	0.73	0.74	0.67
Dep. var. mean	0.047	0.047	0.047	0.048	0.45
Number of schools	1,317	1,317	1,317	1,317	1,317
N	49,680	49,680	49,680	50,070	49,680

Notes: The table reports OLS estimates controlling for gender, cohort, and school fixed effects, two sets of school-specific time trends, and for the share of non-Western migrants in all cohorts up to 5 years older or younger than the manager's cohort. The unit of observation is the manager. Columns (1)–(4) use as the dependent variable the share of non-Western migrants among all hires made by the manager over their observed career; column (5) uses an indicator for whether the manager hired at least one non-Western migrant. The key regressors are the cohort shares of non-Western migrants among same-gender and opposite-gender classmates in the manager's primary school. Column (2) adds plant fixed effects (preferred specification); column (3) replaces cohort shares with indicators for cohort migrant share $\geq$ 10%; column (4) expands the sample to include migrant managers and adds manager migrant-status indicators. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

close friendships with same-gender peers, making these peers particularly influential for attitude formation and future choices (Mehta and Strough, 2009; Bagues and Zinovyeva, 2025; Merlino et al., 2019, 2024).[15] These effects are robust to the inclusion of plant fixed effects, which compare hiring behavior across managers within the same plant (column (2)). This is our preferred specification and we use it throughout the rest of the analysis.

In terms of magnitude, our benchmark estimate in column (2) implies that going from the average of 1.2% non-Western students of the same gender in the cohort to

[15]This does not imply that interactions with migrants of the opposite gender are irrelevant. As our identification strategy leverages within-school variation, our estimates capture only the marginal effect of additional contact relative to the school-specific average. Thus, we identify the impact of incremental differences in contact with migrants of the same and opposite gender within the same cohort, above and beyond the impacts of broader contact with other migrants within the school.

5% (an increase of 1 within-school standard deviation) increases the share of non-Western migrants hired by approximately 0.31 percentage points, which is around 6.6% of the mean.

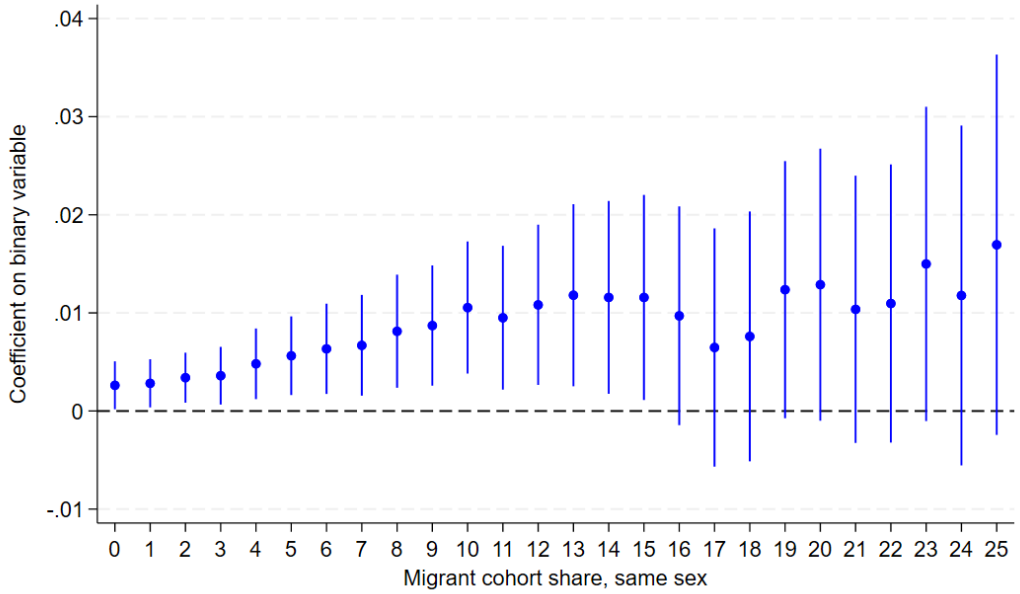
We then construct a binary variable indicating whether a class migrant share of the same or opposite gender is larger than 10%. Given that the average class size is around 20 students, this corresponds to having at least one non-Western migrant of the same gender in a class in expectation. Using this indicator to calculate the magnitude of the effect, a manager whose same-gender cohort contained at least 10% migrants hires around 23% more non-Western migrants than average. Importantly, this 10% threshold remains highly relevant in the contemporary Danish context: even in 2020, over 70% of Danish students are not exposed to non-Western migrant shares exceeding this level. This prevalence underscores the external validity and broad applicability of our findings.

In column (4), we include migrant managers in the sample and show that, consistent with Åslund et al. (2014), non-Western migrant managers are also more likely to hire migrants. This provides us with a measure to compare the magnitude of our result from column (3) —having at least 10% of migrants in one's same-gendered cohort closes the gap in hiring behavior between Danish and non-Western migrant managers by around a quarter.

Finally, results in column (5) show that contact matters at the extensive margin also for hiring, as a higher share of migrants of the same gender increases the likelihood of hiring at least one migrant when being a manager.

To better understand the results of column (3) and the share of migrants driving them, we construct dummy variables that equal one if the manager's cohort share of non-Western migrants exceeds x%, with x ranging from 0 to 25. Figure 2 depicts the estimates using these binary treatment variables. Overall, we lack power to detect important non-linearities. The gradually increasing coefficients may nevertheless suggest that the results are not driven simply by having a single migrant in the cohort, but rather by having at least one migrant of the same gender in one's class (which in expectation occurs around the 10% threshold). However, the relatively small number of students with migrant shares above 15% (see Appendix Figure B2) makes it difficult to draw conclusions for cohorts with higher migrant shares.

Figure 2: Impact across the Distribution



Notes: Each point reports the coefficient from an OLS regression of the manager's non-Western hiring share on an indicator for whether same-gender cohort share exceeds the threshold on the x-axis (x%), using the baseline controls (i.e., as in column 2 of Table 4). The estimates are already positive and statistically significant at the extensive margin (i.e., 0%). The upward pattern indicates that hiring effects are stronger at higher levels of migrant contact, consistent with greater exposure being more likely to lead to significant contact. Vertical bars show 95% confidence intervals based on standard errors clustered at school levels.

The hiring patterns do not differ significantly between male and female managers, so we present the results pooled rather than separately by manager gender. Appendix Table C3 provides additional evidence of similar effects across genders; contact with same-gender non-Western migrant peers in school significantly increases the likelihood of hiring both male and female non-Western migrants later in life, with effect sizes that are similar.[16]

[16] Appendix Table C4 shows that the effect is statistically similar for male and female managers, for schools in both small and large municipalities, for small and large cohorts, and across plants of different sizes and with few or many managers. The only statistically significant heterogeneity arises with respect to the share of migrants in the firm's sector: the effect is primarily driven by firms operating in sectors that employ a higher share of migrants.

4.2 Robustness

We now assess the robustness of our main findings across a range of alternative specifications.

First, we examine to what extent 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. Moreover, we include two additional gender-specific peer group measures. The first is the share of same-gendered students in a given cohort with higher educated parents. The second variable is the average percentile of same-gendered peers' parents in the income distribution. In both cases, we do not find any significant effects on managers' hiring behavior.[17]. 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 driven by unobservable characteristics of managers and their peer groups that are correlated with the added controls (Altonji et al., 2005; Oster, 2019).

Another concern is that the effects reflect self-selection of certain managers into firms that would hire many migrants regardless of manager's contact. To address this, column (2) controls for the plant's initial migrant share. We find no significant relationship between a firm's initial migrant share and its subsequent migrant hiring, given that we are controlling for plant fixed effects. More importantly, the coefficient of interest is not statistically different from the benchmark estimate. Table 3 showing no statistically robust evidence of sorting further corroborates this conclusion, and we investigate this further in Section 4.3.

To account for potentially unobserved local factors, columns (3) and (4) include sector- and municipality-specific trends, respectively. Column (5) instead controls for unobserved, time-varying school characteristics by including school-year fixed effects. Across these specifications, the estimated coefficient remains very close to the baseline.

Overall, the stability of the estimates in Table 5 suggests that unobserved omitted variables are unlikely to affect our results. This conclusion is further supported

[17]For brevity, we report in each case only the coefficients for same-gender cohort shares.

Table 5: Robustness of the Baseline Results

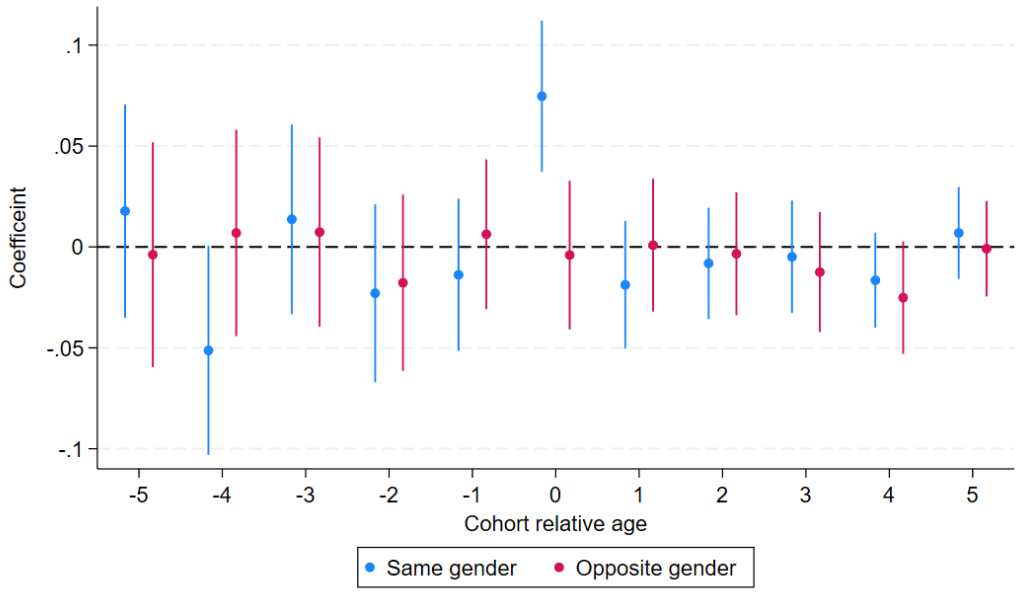
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Extra controls	Previous plant migrant share	Sector trends	Local trends	School-year Fixed Effects	Excluding Copenhagen	Only normal public schools	Using school IV
Migrant share, same gender	0.084***	0.086***	0.078***	0.079***	0.10**	0.090***	0.079***	0.089***
	(0.020)	(0.021)	(0.019)	(0.019)	(0.050)	(0.022)	(0.020)	(0.033)
Migrant share, opp. gender	0.0027	0.0089	0.00042	0.0029		0.029	0.0014	0.033
	(0.020)	(0.024)	(0.019)	(0.019)		(0.021)	(0.021)	(0.031)
Parent higher-ed share, same gender	–0.0027							
	(0.0071)							
Avg. cohort parent inc., same gender	0.00011							
	(0.00013)							
Pre-recruitments plant migrant share		–0.024						
		(0.022)						
Dep. var. mean	0.047	0.046	0.047	0.047	0.046	0.046	0.046	0.047
Number of schools	1,317	1,306	1,317	1,317	1,297	1,225	1,141	1,221
N	49,680	38,853	49,644	49,528	44,947	47,619	43,707	45,871

Notes: The table reports OLS estimates controlling for gender, cohort and school fixed effects, two sets of school-specific time trends, plant fixed effects, and the share of non-Western migrants in all cohorts up to 5 years older or younger than the manager's cohort. The dependent variable is the manager's share of hires that are non-Western migrants, as in the preferred baseline specification with plant fixed effects (column 2 of Table 4). Each column re-estimates the preferred model under the modification described in the column header. Column (1) includes controls for parental higher education, parental income percentile (and a missing-income indicator), single-parent household, parents' ages, the immigrant share in the school's neighborhood, and the gender composition of the school cohort. Column (2) includes the plant's non-Western migrant share in the year before the manager joins; the sample excludes plants not observed in the pre-join year. Column (3) includes sector-specific linear trends. Column (4) includes municipality-specific linear trends. Column (7) restricts the sample to regular public schools. Column (8) instruments the manager's cohort migrant shares with those of the school most commonly attended by individuals of similar age living in the same geographical grid cell; in this specification, school fixed effects and trends correspond to the instrument school. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

by the insignificant opposite-gender share in all specifications in Table 5, since variables correlated with the same-gender grade migrant share would likely also correlate with the opposite-gender share.

We can also exploit the structure of our data to provide further evidence that the estimated effects reflect genuine peer contact rather than confounding school-level factors. To do so, we include migrant shares from cohorts close in age to the manager's own cohort and examine whether these predict hiring behavior. The corresponding results are shown in Figure 3. Only the coefficient for the individual's own cohort is statistically different from zero.[18]

Figure 3: Coefficients on Other Cohorts



Notes: This figure plots coefficients from regressions that include migrant shares for cohorts in the same school within a +/- 5 year window around the manager's cohort; the x-axis is cohort distance (0 = own cohort). Estimates are shown separately for same-gender and opposite-gender cohort shares, with 95% confidence intervals based on school- and plant-clustered standard errors. Controls are as in the baseline (i.e., as column (2) of Table 4), except we omit the share of non-Western migrants in all cohorts up to 5 years older or younger than the manager's cohort (since this is a function of the added coefficients). Only the same-gender non-Western migrant share in the manager's own cohort (time 0) is statistically different from zero, indicating that same-cohort, same-gender contact drives the effect rather than contact with other cohorts within the school.

To examine whether the results are driven by Copenhagen schools, which have

[18]This does not preclude the existence of meaningful interactions between students across cohorts. However, because our identification leverages within-school variation, the estimates capture only the marginal effect of increased contact over and above the average cross-cohort contact in each school.

higher migrant concentrations than the rest of the country, column (6) of Table 5 excludes managers who attended primary school in Copenhagen. The coefficient of interest barely changes.

Column (7) focuses on all students attending regular public schools (Folkeskole), which are operated by municipalities responsible for administration, budgeting, school organization, and teacher employment. Notably, the effects are almost identical to those obtained from the full sample, which also includes students attending private schools.[19]

While our identification strategy already addresses the main threats to validity, column (8) provides an additional layer of robustness through an instrumental variable approach. Specifically, we instrument migrant contact by assigning managers not to the school they went to, but the school most commonly attended by peers of similar age (i.e., within two years) who live in the same grid cell. This strategy exploits variation in school composition that is entirely independent of individual school choice, thereby also addressing any remaining concerns about sorting based on migrant shares. The consistency of the IV estimate in column (8) with our benchmark highlights the credibility of our identification strategy.

In addition to the controls included in Table 5, we would also like to control for the characteristics of the people hired. Appendix Table C5 reports results from specifications estimated at the level of the individual new hire, where the dependent variable indicates whether the recruit is a non-Western migrant. At this level, we find a positive and statistically significant effect of same-gender non-Western cohort share on the likelihood of hiring a non-Western migrant, closely mirroring our main findings at the manager level (Table 4). This result is robust to the inclusion of additional controls and occupation fixed effects.

Appendix Table C6 presents the effect of migrant contact across alternative measurement specifications. In column (1), we reassign individuals to school cohorts based on their year of birth, rather than the observed 8th-grade class. Reassur-

[19]The similarity between the public-only and full-sample estimates is partly mechanical: private-school students make up only about 12% of the full sample. By a simple back-of-the-envelope calculation, this small difference implies an effect of approximately 0.096 in private schools. By contrast, if the effect in private schools were zero, the full-sample coefficient would fall to around 0.07, which is inconsistent with the data.

ingly, this alternative classification, highlighted in the recent work by Ciccone et al. (2025), yields similar results. Column (2) shows that our baseline results are robust when dropping school fixed effects and time trends in the estimation model. In column (3), we weight observations by the number of hires per manager to reduce the influence of those with limited hiring activity; this again produces an estimate that is considerably larger than our benchmark estimate. Column (4) expands the sample to include larger plants or establishments (more than 250 employees) and those with 5 managers or more. Since hiring responsibility is much less clearly assigned in these cases, the estimated effects are smaller, but remain positive and significant. In column (5), we weight migrant cohort shares by the duration migrants remained in the cohort: using migrants' year of arrival in Denmark, we assume they enter the cohort in that year and weight their contribution by the remaining years in the cohort. In other words, our migrant contact measure increases with each additional year a migrant remains in the cohort. Lastly, column (6) excludes schools with no migrants, as well as individuals who have no migrants in the five preceding or following cohorts. The estimates remain robust across these two specifications and, if anything, tend to be larger.

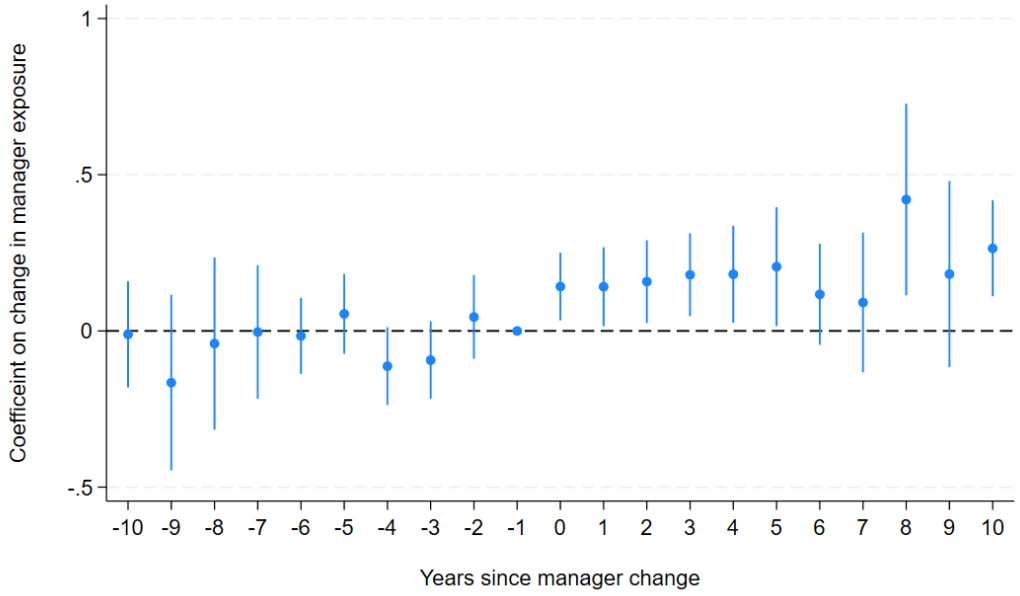
Lastly, we test whether our results are sensitive to alternative definitions of who is a manager in a given plant in a given year (Appendix Table C7). We replicate the preferred specification using four alternatives: (1) restrict to individuals coded as top managers whenever one is present; (2) keep at most one manager per plant-year, selected by rank, then earnings, then tenure/experience; (3) additionally include owner-managers recorded as employers; and (4) follow Acemoglu et al. (2022), using occupation and job hierarchy (selecting the highest-paid if multiple candidates). Across all definitions, the coefficient on same-gender migrant contact remains positive and statistically significant, while the opposite-gender coefficient remains small and indistinguishable from zero.

4.3 Timing of the Impact

Our preferred specification exploits within-plant variation in manager contact to identify changes in hiring patterns over time. To explore the dynamics of this

change, we use a plant-year level specification with manager-change fixed effects, as specified in equation (2).[20] In particular, we interact the change in the manager's contact measure with indicator variables for the years before or after the change. Figure 4 plots the coefficients of these interaction terms, showing that the effect emerges in the year of appointment and persists for up to ten years, though not all yearly estimates are statistically significant. We also find no evidence of pre-trends, indicating that managers with higher contact with non-Western migrants are not systematically hired during broader shifts in firm hiring patterns.

Figure 4: Timing of impact of an appointed manager on new hires



Note: This event-study figure plots coefficients on interactions between the change in the manager non-Western migrant contact measure and event-time indicators relative to the year of a manager change (0 = appointment year). Observations are at the plant-year level, with the controls as in our baseline (column 2 of Table 4) relating to the (most senior) manager in the plant at that year. The absence of pre-trends and the emergence of persistent effects at event time 0 support a causal interpretation tied to the appointed manager. Vertical bars show 95% confidence intervals based on standard errors clustered at school levels.

While the lack of pre-trends is reassuring, we cannot entirely rule out the possibility that the change in manager is part of a broader simultaneous change in hiring strategy at the firm which itself leads to more migrant hires. However, this would re-

[20]Results from estimating this equation assuming static effects (column (1) of Appendix Table C9) give very similar results to the manager-based baseline.

quire three conditions to hold: (i), the change in strategy is focused predominantly on an increase in hiring migrants, since we observe no impact on either the total number of hires or their other observable characteristics (see Figure 5); (ii), the traits that make a manager particularly suited to hiring or managing migrants are sufficiently observable to the plant, allowing firms to base their selection of managers on these traits; and (iii), these traits are sufficiently important to the firm's objectives to partially determine manager selection, despite the fact that, even for managers with a high level of contact, it is rare that more than 10% of the hires are migrants. Taken together, we view the conjunction of these three conditions as implausible, and thus consider it unlikely that our results are driven by endogenous manager selection into plants.

We can also use the plant-year level specification to distinguish between manager changes that increase, decrease, or leave contact unchanged. Interestingly, we see effects of changes in both directions: migrant hiring increases when a manager with greater childhood contact replaces one with less contact, and decreases when the reverse occurs. This symmetry suggests that the effect reflects the incoming manager's own attitudes and behavior, and helps rule out asymmetric time-varying confounders, such as secular upward trends in diversity hiring.

5 Mechanisms

There are four key mechanisms that could explain our findings: educational performance or labor market outcomes, residential sorting of managers, referral networks formed in school, and a reduction in prejudices and beliefs about migrants. We examine each in turn and find that the evidence points most strongly to the last: a persistent change in attitudes. We rule out the first three channels before presenting direct evidence on attitude change using the European Social Survey.

5.1 Education, Earnings and Sorting

One potential mechanism is that contact with migrants affects managers' own educational performance or labor market outcomes, which in turn shapes their hiring

behavior. Although results are mixed, some studies have shown that contact with migrants in school can generate negative effects for natives, as migrant 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).[21] If individuals perform worse at school as a result of migrant peers, they might then go on to hold different management roles or recruit in a different way, potentially resulting in an increase in migrant hiring.

To consider this channel, we first investigate whether contact with non-Western migrants affects educational attainment and income at age 40 among Danes without a migrant background. Columns (1) and (2) of Table 6 report corresponding results and show that both coefficients of the migrant cohort share are insignificant. To address concerns about statistical power, we replicate the analysis on the full population of Danes in the same cohorts and again find no evidence of impacts (see Appendix Table C8). These results suggest that negative peer effects on students' educational performance are unlikely to drive our findings.

This interpretation is supported when we examine a broader set of characteristics of managers' new hires. If early contact with non-Western migrants captured systematic differences across managers—for example, in the types of vacancies they fill or their screening processes—we would expect their recruits to differ along observable dimensions such as education, pre-hire earnings, gender, or age. Figure 5 shows no such pattern: estimated effects on these characteristics are small and statistically indistinguishable from zero. Importantly, we also find no effect on the migrant share in the recruit's occupation, suggesting that managers with more migrant contact are not merely shifting hiring toward occupations with higher migrant representation; rather, contact affects who is hired, not which jobs are filled. Finally, the last two outcomes show no indication of worse post-hire performance or match quality: neither log earnings five years after hiring nor the probability of remaining employed at the establishment changes in response to greater childhood contact. Taken together, these results indicate that the only systematic change is an

[21] Empirical evidence on migrant peer effects in schools shows both negative (e.g., Jensen and Rasmussen, 2011) and positive (e.g., Figlio et al., 2023) effects; see Frattini and Meschi (2019) for a discussion.

Table 6: Other outcomes, managers

	(1)	(2)	(3)	(4)	(5)	(6)
LHS variable	Completed higher education	Income percentile at 40	Migrant share in municipality at 40	Migrant share in postcode at 40	Migrant share at address at 40	Had migrant partner
Migrant share, same gender	–0.031	3.4	0.013	0.027	0.012	0.037
	(0.11)	(5)	(0.013)	(0.018)	(0.024)	(0.027)
Migrant share, opp. gender	0.15	–6.2	–0.010	–0.016	–0.011	0.0039
	(0.10)	(4)	(0.013)	(0.017)	(0.022)	(0.036)
R^2	0.11	0.10	0.30	0.22	0.11	0.082
Dep. var. mean	0.40	87	0.065	0.060	0.015	0.016
Number of schools	1,317	1,310	1,305	1,305	1,305	1,317
N	49,680	47,181	43,463	43,454	43,463	49,680

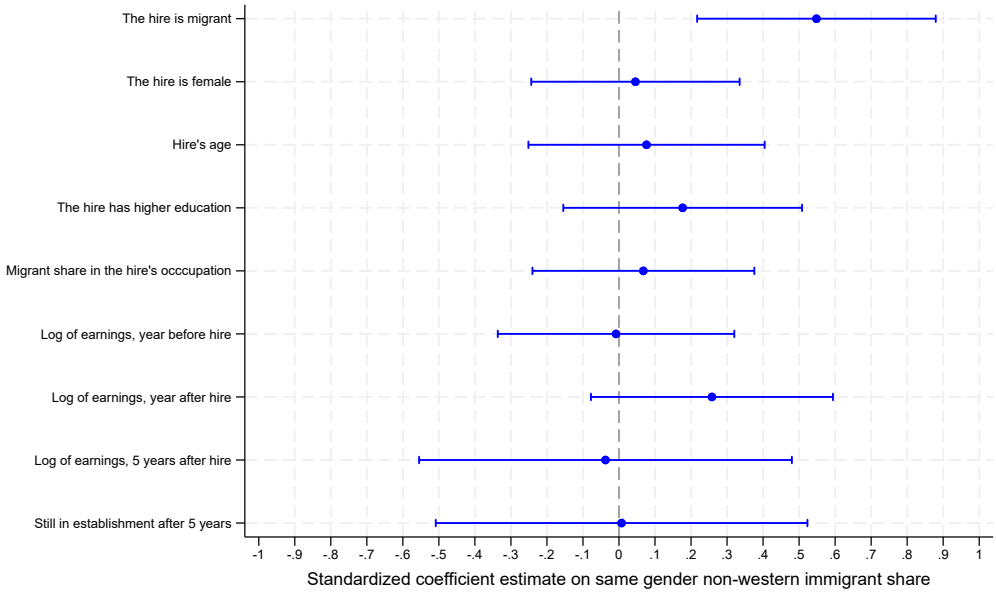
Notes: The table reports OLS estimates controlling for gender, cohort, and school fixed effects, two sets of school-specific time trends, and the share of non-Western migrants in all cohorts within a +/- 5 year window of the individual's cohort. The table tests whether migrant contact in school has any significant impact on later educational and labor market outcomes of managers without an migrant background. Dependent variables (by column) capture managers' own educational attainment, income rank at age 40, residential migrant exposure at different geographic levels at age 40, and partnering outcomes, as labeled. Near-zero coefficients indicate that migrant contact primarily affects hiring behavior rather than managers' own human capital accumulation or later-life residential/partner sorting. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

increase in the share of migrants among new hires.

An alternative explanation could be that managers move to places where there are more migrants, 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 migrants and the geographical concentration in the region where the manager resides at the age of 40. This holds whether we assess migrant concentration at the municipal level (column (3)) or at the sub-municipal level within municipalities at the postcode or address level (columns (4) and (5)). Therefore, we conclude that it is very unlikely that the migrant cohort share has an impact on the residential choices of managers that is sufficiently large to explain our main result.[22] Finally, we do not find any significant

[22]Focusing on the full population rather than managers only, we do find some evidence that school contact with non-Western migrants influences the share of migrants in the municipality and postcode of residence (Appendix Table C8). This effect, however, is too small to be the main driver of our results—i.e., the results suggest that individuals who have contact with 1 percentage point more migrants in school live in areas with around 0.0075 percentage points more migrants, which is unlikely to explain a change in hiring ten times this magnitude. Moreover, since our main results control for plant fixed effects, they effectively control for the location of the plant, and such a place-

Figure 5: Characteristics of New Hires



Notes: Each point shows the coefficient on the manager's same-gender school-cohort is from a separate regression where the dependent variable is the listed hire characteristic (as labeled). Non-self explanatory variables are defined as follows: *higher education* indicates having completed a bachelor's degree or higher and *Hire's occupation* is measured at a 6-digit occupation level variable. Contact increases the likelihood that the hire is a non-Western migrant, while other observable hire characteristics change little. Horizontal bars show 95% confidence intervals based on standard errors clustered by managers' primary school.

impact of the same-gender cohort non-Western migrant share on the likelihood of having a partner of non-Western origin (column (6)).[23]

5.2 School-Based Referral Networks

Another potential explanation is that managers who had more contact in schools hire more migrants because they maintain friendships with former migrant classmates and recruit through these personal connections (e.g., Galeotti and Merlino, 2014). To investigate this mechanism, we analyze heterogeneity in both migrant contact and hiring outcomes.

First, we use the plant–year analysis described above to examine how the effects vary with the timing of hires. Column (4) of Appendix Table C9 shows that the effect is at least as strong for hires made more than twenty years after the manager left school, compared with those made earlier. This contrasts with a network-based explanation, which would predict the strongest effects immediately after school, when school-based networks are most relevant. The effect also does not vary with the distance of the plant municipality from the school municipality (see column (7) of Appendix Table C4). This provides further evidence against social networks being the primary mechanism underlying our results, as one would expect the effect to be stronger in closer proximity to schools if network effects were at play.

Second, we distinguish between non-Western hires who appear in our school data as children and those who do not (generally because they immigrated to Denmark later). Table 7 shows a significant effect of contact on hiring for the latter group (column (1)), but no effect for those who were in grade 8 of Danish primary

based mechanism would be inconsistent with the timing of the impact observed in Figure 4 and the insensitivity of our results to the inclusion of local trends in Table 5.

[23]The lack of impact on partnership formation—which is also true in the full sample (Appendix Table C8)—may be largely attributed to the pool of eligible non-Western partners being limited and assortative mating patterns being very pronounced in the population under study. Structural models of intermarriage demonstrate that cultural distance between natives and many non-Western migrant groups substantially reduces the likelihood of forming interethnic unions, even after conditioning on socioeconomic factors (Adda et al., 2025). These constraints severely restrict the practical realization of increased preferences for interethnic unions. In line with this, less than 2% of all Danes—including managers—have ever had an interethnic partnership. Thus, even if preferences shift as a result of contact, actual interethnic partnering remains rare due to both limited partner availability and persistent cultural barriers.

school before 1997 (column (2)). Indeed, the number of non-Western migrants in our hiring data who are also in our school data is relatively small—less than 0.5% of total hires—so their patterns are unlikely to influence the overall results.

Third, we analyze potential impacts of contact with one group of migrants on the hiring of other migrant groups. Column (3) of Table 7 shows that contact with migrants from Turkey—the predominant country of origin for non-Western migrants in our school sample—leads to a higher hiring probability of Non-Turkish migrants. This also holds when we exclude all non-Western migrants from countries that share a common border with Turkey (column (4)). Additionally, we find in column (5) that contact with Non-Turkish migrants in school increases the likelihood of hiring Turks later in life. A similar pattern emerges when we divide non-Western migrants into two groups based on their relative group size in school (see Appendix Table C10). Contact with migrants from the largest groups in a school increases the likelihood of hiring migrants from other countries. Since job referral networks are often structured along ethnic lines (Munshi, 2003), this cross-group generalization is difficult to reconcile with a network-based explanation and instead points to a broader change in attitudes.

5.3 Attitudes towards Migrants

The spillover effects across social networks and migrant groups instead point to another potential mechanism: reduced prejudice toward migrants, consistent with the contact hypothesis of Allport (1954). We first present indirect evidence for this mechanism using heterogeneity in the types of migrants managers were exposed to in school, and then provide direct evidence on attitudes using survey data from the European Social Survey (ESS).

Contact with Western migrants, who are culturally more similar to Danes, does not affect managers' hiring behavior toward either Western or non-Western migrants (columns (1) and (2) of Table 8). In contrast, the strongest effects arise from contact with migrants from predominantly Muslim countries, the largest group of non-Western migrants in Denmark (column (3)). Contact with this group increases the hiring of both migrants from these countries (column (4)) and those from other

Table 7: Evidence for effects across migrant groups

	(1)	(2)	(3)	(4)	(5)
LHS variable	Share of hires that are				
	non-Western migrants not in school data	non-Western migrants in school data	non-Western non-Turkish migrants	non-Western migrants not from Turkey or bordering countries	Turkish migrants
Migrant share, same gender	0.073***	0.0084			
	(0.018)	(0.0077)			
Turk share, same gender			0.11***	0.078***	0.0031
			(0.031)	(0.028)	(0.013)
Non-Turk migrant share, same gender			0.049**	0.051***	0.022**
			(0.020)	(0.018)	(0.011)
R^2	0.73	0.56	0.71	0.70	0.68
Dep. var. mean	0.042	0.0051	0.040	0.034	0.0065
Number of schools	1,317	1,317	1,317	1,317	1,317
N	49,680	49,680	49,680	49,680	49,680

Notes: The table reports OLS estimates controlling for gender, cohort, and school fixed effects, two sets of school-specific time trends, plant fixed effects, and the share of non-Western migrants in all cohorts up to 5 years older or younger than the manager's cohort. The dependent variable is the manager's share of hires belonging to the migrant group indicated by the column header. For column (3)-(5) regressors decompose school exposure into Turkish and non-Turkish same- and opposite-gender cohort shares. Positive effects across multiple groups suggest that exposure reduces barriers to hiring broadly rather than reflecting narrow origin specific favoritism. Standard errors are clustered at school level. * $p < .10$, ** $p < .05$, *** $p < .01$

non-Western countries (column (5)). Distinguishing between first- and second-generation migrants in column (6) reveals similar effects across both groups: contact increases migrant hiring regardless of whether classmates were foreign-born or had foreign-born parents. Finally, we examine whether the effect differs depending on the 'ability' of the migrant children proxied by their later earnings (above vs. below the median). Column (7) shows effects for both groups, with, if anything, stronger impacts for migrants with lower earnings.

Overall, these results are most consistent with changes in attitudes towards migrants as a whole. The largest effects arise for those groups that have been shown to face significant discrimination in European labor markets (Adida et al., 2010; Fernández-Reino et al., 2022). Finding similar impacts for first- and second-generation migrants, as well as for 'high' and 'low' ability, suggests this is not simply a matter of managers learning about migrants' abilities or the practicalities of the immigration process. Instead, the results are more consistent with changes in beliefs about migrant characteristics that shared across these groups—such as ethnicity and culture.

To provide direct evidence on attitudes, we complement the registry analysis with survey data from the European Social Survey (ESS).[24] We use the nine Danish ESS rounds fielded between 2002 and 2018 (Andrade et al., 2024). For each round, Statistics Denmark provides anonymous personal identifiers that allow us to link respondents to the same administrative registers used in the main analysis. We restrict the sample to native Danes who completed primary school in the cohorts covered by our school register and assign to each individual the share of same-gender non-Western migrants in their primary school cohort, constructed exactly as in the baseline design.

Since the ESS sample is too small to focus on managers alone, we use the broader population of Danish school graduates with no migrant background, which also allows us to speak to the generality of the attitude effects beyond the managerial population. From the ESS immigration module, we select questions that capture

[24]The ESS is an academically driven, biennial cross-national survey covering approximately 30 European countries since 2002. It measures attitudes, beliefs, and behavioral patterns using high quality probability samples and harmonized face to face interviews, with a typical national target of at least 1,500 respondents aged 15 and above in each round (European Social Survey, 2025).

Table 8: Results for different migrant categories

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LHS variable	Share of hires that are						
	non-Western migrants	Western migrants	non-Western migrants	migrants from Muslim majority countries	non-Western migrants not from Muslim majority countries	non-Western migrants	non-Western migrants
Migrant share, same gender	0.081***	–0.0067					
	(0.019)	(0.014)					
Western share, same gender	0.019	–0.038					
	(0.041)	(0.033)					
Muslim migrant share, same gender			0.093***	0.062***	0.033**		
			(0.024)	(0.019)	(0.014)		
Non-Muslim migrant share, same gender			0.061*	0.047*	0.0099		
			(0.033)	(0.027)	(0.019)		
1st-gen migrant share, same gender						0.080***	
						(0.022)	
2nd-gen migrant share, same gender						0.084**	
						(0.037)	
Richer migrant share same gender							0.061**
							(0.027)
Poorer migrant share same gender							0.10***
							(0.031)
R^2	0.73	0.75	0.73	0.71	0.68	0.73	0.73
Dep. var. mean	0.047	0.032	0.047	0.027	0.020	0.047	0.047
Number of schools	1,317	1,317	1,317	1,317	1,317	1,317	1,317
N	49,680	49,680	49,680	49,680	49,680	49,680	49,680

Notes: The table reports OLS estimates controlling for gender, cohort, and school fixed effects, two sets of school-specific time trends, plant fixed effects, and the share of non-Western migrants in all cohorts up to 5 years older or younger than the manager's cohort. The dependent variable is the manager's share of hires in the migrant category listed in each column. Each specification relates these outcomes to same-gender school-cohort contact measures for the relevant group. *Richer(poorer)* indicates whether income measured at age 25 are above(below) the sample median. Effects concentrated in non-Western (and more culturally distant) categories are consistent with reduced discrimination rather than a uniform expansion in hiring. Standard errors (in brackets) are clustered at the school level. * p $<$ .10, ** p $<$ .05, *** p $<$ .01

respondents' social contact with migrants and their views about the consequences of immigration and desired immigration levels.

Our estimation strategy mirrors the baseline cohort contact design. For each outcome, we run an OLS regression of the form:

$$Y_i = \beta_1 \, ShareMig_{gcs} + \beta_2 \, ShareMig_{ocs} + \gamma_c + F_i + X_{sc} + \varepsilon_i, \tag{3}$$

where Y_i is the standardized ESS index for individual i. $ShareMig_{gcs}$ is the share of non-Western migrants of the same gender as the individuals within their cohort c in school s, $ShareMig_{ocs}$ is the corresponding share of non-Western migrants of the opposite gender. F_i is an indicator equal to one if individual i is female, and γ_c are cohort fixed effects. To absorb broader migrant inflows at the school level, we also include X_{sc} which is the ±5 year school cohort migrant share. ε_i is a random error term. We cannot include school fixed effects or school trends due to the smaller sample size, but in the Appendix Table C6 we show that our baseline results on hiring decisions are very similar when these controls are removed (column (2)). In addition, even with this smaller-control set, the variation in gender-specific migrant shares that we use is balanced with respect to predetermined characteristics. Specifically, we regress a range of parental and background variables on the share of non-Western migrants in an individual's gender-specific peer group, controlling for cohort fixed effects and the migrant share in adjacent cohorts (±5 years). The coefficients on the same-gender migrant share, reported in Appendix Table D1, are insignificant, which suggests that the residual variation in migrant shares is approximately as-good-as random, supporting our identification strategy.

We report our main results from this analysis in Table 9. In columns (1) and (2), we create two indices that together contain all the immigration-related questions that are asked in each of the nine waves of the survey. The first index contains questions asking about respondents views on migrants' impacts, while the second contains their preferences over immigration policy. In columns (3) and (4) we then focus on two sets of questions which are asked in only one or two waves but we believe are particular relevant to our proposed mechanism. The dependent variable in column (3) is an index of how many friends respondents have who are migrants

Table 9: Results from the European Social Survey

	(1)	(2)	(3)	(4)	(5)	(6)
LHS variable	Believes that migrants have positive impacts	Believes that more migrants should be allowed in	Has friends who are migrants	Doesn't hold prejudices of migrants	Trusts people in general	Trusts political parties
Migrant share, same gender	1.4**	1.4**	3.0*	4.0*	0.41	–0.070
	(0.71)	(0.71)	(1.7)	(2.1)	(0.73)	(0.72)
Migrant share, opp. gender	–0.12	–0.015	0.063	–1.0	0.24	0.40
	(0.66)	(0.66)	(1.7)	(2.7)	(0.68)	(0.67)
R^2	0.045	0.053	0.040	0.068	0.021	0.026
N	3,686	3,651	760	378	3,708	3,698

Notes: The table reports OLS estimates controlling for gender, ESS wave, cohort fixed effects, and the share of non-Western migrants in all cohorts up to 5 years older or younger than the individual's cohort. The unit of observation is an ESS respondent matched to school and cohort information. Dependent variables (column (1) to (4)) are standardized indices capturing contact with migrants and attitudes toward immigration, as labeled. Positive coefficients imply that greater contact with same-gender non-Western migrants in schools is associated with more pro-immigration attitudes. Column (5) and (6) are placebo/general-trust outcomes testing whether childhood contact shapes natives' general political and social views. Sample sizes vary across columns because the ESS questions used to construct the different indices are not asked in all survey waves; each column therefore uses the maximum available sample for that outcome. All variables are standardized. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

or members of a different ethnicity. In column (4), we use an index of questions that were asked in just one wave but directly get at prejudice—i.e., they ask respondents whether they believe other ethnicities are just as hardworking and intelligent as Danish people.[25] In each column, the variable is standardized (such that it has a standard deviation of 1) and given a sign such that a more positive value represents a more positive attitude towards migrants.

The first four columns of Table 9 show that contact with migrants of the same gender in school has a substantial and significantly positive impact on attitudes towards migrants. Moving from a cohort with a 1.2% same-gender non-Western share (the sample mean) to a cohort with a 5% share—roughly a one standard deviation increase in contact—raises the "migrant friends" index by about 0.11 standard deviations, the indices capturing perceived impacts of immigration and preferred inflows by roughly 0.05 standard deviations, and the "doesn't hold prejudices" index by about 0.15 standard deviations. Contact with opposite-gender migrant classmates has a small and statistically insignificant effect in all specifications.

[25]The corresponding question wording, as asked in the ESS, is reported in Table Table D2.

Columns (5) and (6) provide a useful placebo test of whether childhood contact shapes natives' general political and social views, rather than attitudes towards migrants specifically. If contact primarily changed broader societal orientations—such as general trust in others or confidence in political parties—we would expect effects on these outcomes as well. Instead, we find no impact on either of these placebo measures, suggesting that contact affects views on migrant-related topics in particular. This pattern indicates that hiring decisions of managers with more contact with migrants are unlikely to be driven by shifts in general views, but instead primarily by changes in attitudes towards migrants.

Appendix Figure D1 presents item-level coefficients for each of the underlying question separately. This figure contains each of the questions used in constructing the indices displayed in Table 9, as well as additional questions that were only asked in one or two waves. The figure plots the coefficient on same gender cohort contact for each item separately. The composite index at the bottom of the figure, which aggregates all of the items, confirms a systematically more pro immigration stance among individuals who had more non-Western classmates of their own gender.

Taken together, the ESS evidence supports a contact hypothesis interpretation of the hiring results. Individuals who, based on their school cohort, were in contact with more non-Western peers of the same gender are more likely to report migrant friends and to endorse the view that migrants benefit Denmark and that more migrants should be admitted. This is consistent with our finding that individuals who have had more contact with migrants also live in neighborhoods with more migrants decades later (Appendix Table C8), similar to the impact on residential segregation found in Merlino et al. (2024). These attitudinal differences are observed decades after school and in a sample not restricted to managers, suggesting that early interethnic contact in schools leaves a broad and persistent imprint on attitudes toward migrants, which is then reflected in managers' hiring behavior.

6 Conclusions

This paper explores whether contact with migrants in schools during childhood influences the hiring behavior of managers. For this purpose, we exploited idiosyn-

cratic variation in cohort composition within Danish schools using rich administrative data. We then show that managers who were in contact with children of non-Western origin during school are more likely to hire non-Western migrants. This relationship is robust to the inclusion of plant fixed effects and a wide range of alternative specifications.

Regarding the underlying mechanisms, we provide evidence that our results are primarily driven by attitudinal changes toward migrants, as posited by the contact hypothesis of Allport (1954). We rule out alternative explanations such as economic opportunities, residential sorting, and school-based social networks. On the positive side, we demonstrate that contact effects generalize across migrant groups, rather than being limited to those specific groups students initially encountered. Most directly, linking administrative data with nine waves of the European Social Survey, we show that individuals who experienced greater contact with migrant classmates display significantly lower levels of prejudice in adulthood—effects observed decades after school and in the broader population, not just among managers.

Our results carry important policy implications. Exposure to migrants in schools can improve labor market integration by reducing in-group favoritism among managers. Importantly, these effects are relevant to current Danish conditions—over 70% of Danish students in 2020 were not exposed to non-Western migrant shares exceeding the 10% threshold we identify as salient. Therefore, policies aiming to increase interethnic contact during childhood in schools could help in the long run to reduce labor market discrimination.

Our findings speak to a range of contexts, though it remains an open question whether the same mechanisms operate in environments with substantially higher levels of migrant concentration. Another promising avenue is to investigate whether early peer contact influences outcomes beyond hiring, such as task allocation, mentoring, promotion, or wages. Linking childhood peer composition to these within-firm outcomes would clarify whether managers with more interethnic contact not only hire more migrants but also treat them differently once employed, offering a fuller picture of how early experiences shape migrant integration.

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Appendix A Additional Background and Context

This section provides additional background information on Denmark's immigration history and primary school system.

A.1 Immigration to Denmark

Denmark experienced demand-driven net immigration from the 1960s onward, with guest workers arriving mainly from Turkey, Pakistan, and Yugoslavia. Following the oil-price shock in 1973, the guest worker program was terminated (Liebig, 2007), but immigration nevertheless continued through family reunification at lower levels until the introduction of the restrictive Aliens Act in 2002 (Matthiessen, 2009). Refugee inflows increased from the early 1980s, peaking during the civil war in the former Yugoslavia in 1993–94. By 2008, the ten largest migrant groups originated from Turkey, the former Yugoslavia, Iraq, Lebanon, Pakistan, Somalia, Iran, Vietnam, Afghanistan, and Sri Lanka (Matthiessen, 2009).

The steady growth in the non-Western migrant population during this period partly reflects higher fertility rates among non-Western migrants relative to Danish and Western migrant women (Tranæs and Zimmermann, 2004; Matthiessen, 2009). Among descendants of non-Western migrants, labor market outcomes are stronger than for the parental generation, though still below those of individuals without a migration background (Tranæs and Zimmermann, 2004; Matthiessen, 2009). Recent evidence shows that children of migrants exhibit lower earnings and weaker labor-market attachment on average, though conditioning on parental socioeconomic characteristics substantially attenuates these differences (Jensen and Manning, 2025).

Table A1: Classification of Western and Non-Western Origin Countries (Statistics Denmark)

Category	**Countries/regions included**
Western	EU member states; Andorra; Australia; Canada; Iceland; Liechtenstein; Monaco; New Zealand; Norway; San Marino; Switzerland; United Kingdom; United States; Vatican City.
Non-Western	Albania; Bosnia and Herzegovina; Belarus; (former) Yugoslavia; Kosovo; North Macedonia; Moldova; Montenegro; Russia; Serbia; (former) Soviet Union; Turkey; Ukraine; all countries in Africa; all countries in South and Central America; all countries in Asia; all countries in Oceania except Australia and New Zealand; and stateless persons.

Notes: This classification follows Statistics Denmark. The paper uses this definition when referring to *Western* and *non-Western* migrant origin.

A.2 The Danish School System

This appendix provides additional details on the Danish primary school system during the sample period (1977–1996).

An additional but non-compulsory introductory year (grade 0) was introduced in 1979, gaining widespread popularity before becoming mandatory in 2009. A non-compulsory grade 10 was also offered as an alternative for students not ready to continue within the educational system; its curriculum is identical to that of grade 9 and provides no additional prerequisites for subsequent educational options (Jørgensen, 2017).

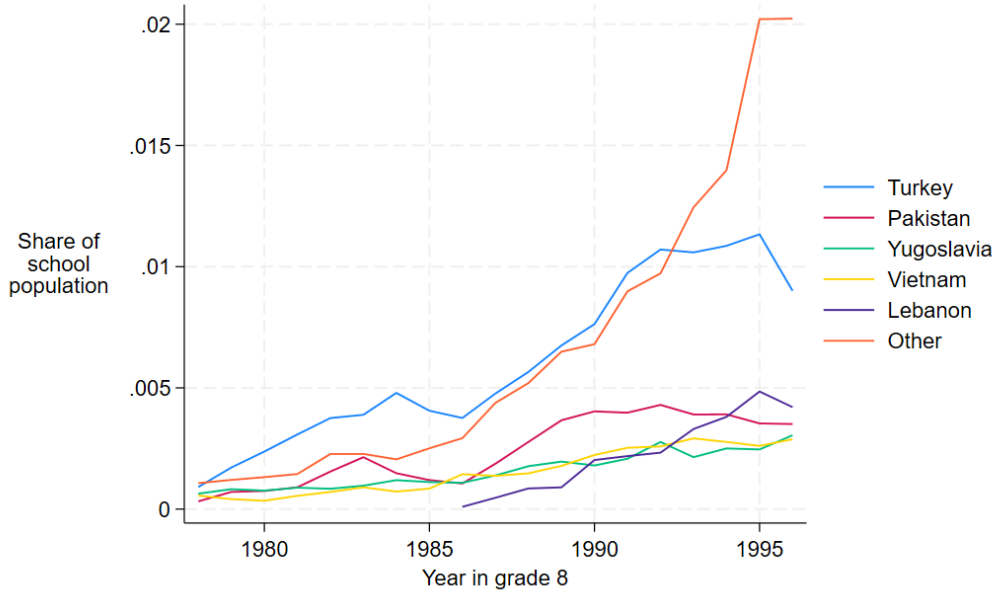
Class composition rules are set out in Danish educational statutes, which require composition to remain constant throughout the school years, prohibiting the separation of students from their original classmates even in the event of school mergers. The regulations stipulate a maximum enrollment of 28 students per class, and principals typically maintain balanced gender ratios. Reassignments are rare, generally limited to exceptional personal or academic circumstances, or for students classified as special needs at institutions lacking appropriate facilities (Retsinformation, 1975, 1991).

Funding for public schools is not solely determined by local tax revenues; extensive transfers between municipalities mitigate financial inequalities. School district boundaries are typically based on geographical distance and rarely adjusted for demographic changes (Retsinformation, 1990). Private schools receive public funds covering on average 75% of their costs, with parents covering the remainder. The share of students completing primary education in private schools increased mod-

estly during the sample period, with sharp growth occurring only from the 2000s. Homeschooling is in principle possible but extremely rare.

Appendix B Additional Descriptive Statistics

Figure B1: Share of migrants in Managers' School Cohorts



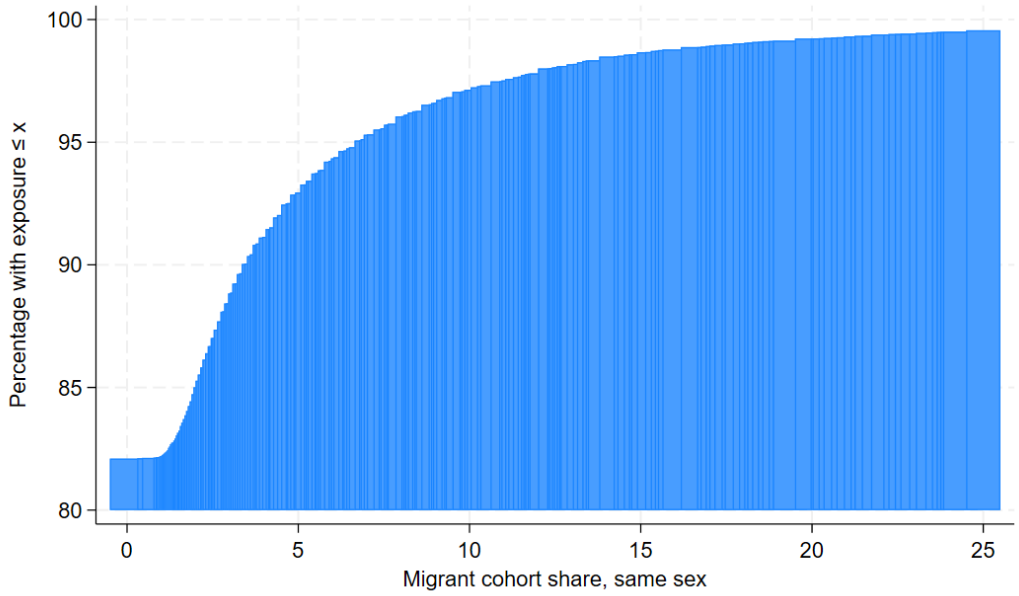
Notes: This figure shows the average cohort composition (share of non-Western migrants) in managers' primary-school cohorts, measured at the start of grade 8. Lines decompose the non-Western share by major origin groups (e.g., Turkey, Pakistan, Yugoslavia, Vietnam, Lebanon).

Table B1: 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	49,680	0.29	0.45	1.11e+06	0.49	0.50
Birth year	49,680	1971	5.02	1.11e+06	1972	5.39
School cohort size	49,680	66.6	27.1	1.11e+06	64.0	26.2
Migrant share in school cohort, same gender	49,680	0.012	0.038	1.11e+06	0.016	0.047
Migrant share in school cohort, opp. gender	49,680	0.012	0.039	1.11e+06	0.017	0.050
Share of cohort female	49,680	0.51	0.069	1.11e+06	0.50	0.071
Migrant share, municipality	49,495	0.018	0.020	1.10e+06	0.021	0.022
Completed higher education	49,680	0.40	0.49	1.11e+06	0.34	0.47
Income percentile at 40	47,187	87.1	16.6	985,368	75.4	19.8
Mother in register	49,680	1.00	0.021	1.11e+06	1.00	0.057
Father in register	49,680	0.99	0.090	1.11e+06	0.98	0.12
Parents completed higher education	49,503	0.20	0.34	1.10e+06	0.18	0.33
Parents income percentile	49,624	71.4	15.8	1.11e+06	68.7	16.5
Parents income percentile missing	49,680	0.0011	0.034	1.11e+06	0.0042	0.065
Employees in plant	49,680	53.1	46.0			
Managers in plant	49,680	2.25	1.30			
Hired more migrants than plant share	41,163	0.45	0.50			
Hired at least one migrant	49,680	0.45	0.50			

Notes: This table reports summary statistics for the study's manager sample and the corresponding full-population cohort. Within-school standard deviation is computed across cohorts within the same primary school. Non-self explanatory variables are defined as follows: higher education indicates having completed a bachelor's degree or higher. All parental variables are measured when the child is 10 years old.

Figure B2: Distribution of migrant Share



Notes: This figure shows the cumulative distribution of the same-gender cohort share of non-Western migrants in the manager sample. The y-axis reports the percentage of managers with shares below the threshold on the x-axis.

Appendix C Additional Figures and Tables

Table C1 mirrors Table 2 in the main text and reports the results of the balancing test for the full sample of individuals who were in grade 8 of Danish primary school between 1977 and 1996, irrespective of their eventual managerial status. In these analyses, we regress a set of parental and background characteristics on the share of non-Western migrants in each individual's gender-specific peer groups. All specifications include year and school fixed effects, as well as school-specific time trends. Of the 26 estimated coefficients, only one is statistically significant. This finding further supports our main result that, prior to the introduction of school choice in Denmark, the allocation of non-Western migrants by gender across school cohorts was effectively random.

In Table C2, we examine whether migrant students are non-randomly clustered within schools using three different tests. We begin with a simple and intuitive test, where we regress the same-gender migrant share on the opposite-gender migrant share within cohorts. The estimated coefficient is statistically insignificant, both in the manager sample (column (1)) and when considering all non-migrant students in the relevant cohorts (column (2)). This absence of correlation provides additional support for quasi-random cohort composition within schools, as any non-random clustering would likely affect gender-specific migrant shares in a similar way.

Next, we regress a dummy variable indicating whether an individual is a migrant on the migrant share of the rest of their peer group. For this analysis, we use the full population of children, including migrants, but exclude younger siblings—both as units of observation and when constructing the migrant share—to avoid mechanical clustering. The corresponding coefficient is reported in column (3), and is negative and insignificant. However, as highlighted by Guryan et al. (2009), this type of regression typically produces a negatively biased coefficient, since our peer groups exclude the individual themselves. Guryan et al. (2009) designed a test to address this bias—the basic idea is to remove the exclusion bias by controlling for the migrant share among all other students in the school. Doing so once again delivers an insignificant coefficient (see column (4)).

An alternative approach to circumvent exclusion bias, proposed by Caeyers and Fafchamps (2024), involves transforming the dependent variable by subtracting the component that is mechanically correlated with peers' outcomes due to the leave-one-out construction. The corresponding result, reported in column (5), yields an insignificant coefficient, providing further evidence in support of quasi-random cohort composition within schools.

Table C3 examines potential gender heterogeneity in the main effect estimated in column (2) of Table 4. For this, we distinguish the analysis by the gender of the hired individuals, breaking down the share of hired migrants into male and female

migrants. The findings indicate that interethnic contact with same-gender peers increases the likelihood that managers later hire both male and female non-Western migrants, with effect sizes that are similar for both groups (columns (1) and (2)). This result persists when we further differentiate by the gender of the manager in columns (3) and (4): the estimated coefficients for the relevant interaction terms are statistically indistinguishable from each other.

Table C4 further examines whether the baseline effect varies along several observable dimensions. First, in column (1), we estimate heterogenous effects based on the manager's gender. Then, in columns (2) to (6), we interact the same-gender cohort migrant share with an indicator for whether the relevant characteristic is above or below the sample median. Specifically, we consider: the share of non-Western migrants in the manager's firm sector (column (2)), the population size of the municipality in which the manager's school is located (column (3)), school cohort size (column (4)), number of employees in the plant (column (5)), and number of managers in the plant (column (6)). Column (7) interacts the same-gender cohort share with the log distance between the manager's plant and their primary school. We report p-values from tests of equality of the interacted coefficients. The only statistically significant heterogeneity arises with respect to the sectoral migrant share: the hiring effect is concentrated among managers working in sectors with above-median migrant employment. All other dimensions yield statistically indistinguishable coefficients. The similar coefficients for male and female managers indicate that both genders are affected in the same way by migrant contact in adolescence, and the absence of a distance gradient provides further evidence against a social network interpretation of the main result.

Table C5 presents results from regressions conducted at the individual recruit level, using the same specification as in our benchmark model (Table 4, column (2)). The dependent variable is a dummy equal to one if the new hire is a non-Western migrant. Consistent with the main analysis at the manager level (Table 4), we find a positive and statistically significant association between the share of same-gender non-Western peers in the cohort and the likelihood of hiring a non-Western migrant. These results remain robust when including controls for new hires' characteristics (column (2))—such as age, gender, and pre-recruitment earnings—as well as occupation fixed effects (column (3)).

Table C6 assesses the sensitivity of the main result to six alternative choices in the construction of the key variables. In column (1), we assign individuals to school cohorts based on their year of birth rather than the observed eighth-grade class, following the approach highlighted by Ciccone et al. (2025). Column (2) drops school fixed effects and school-specific time trends from the specification. Column (3) weights observations by the total number of hires per manager, thereby giving greater influence to managers with more hiring activity. Column (4) expands

the sample to include plants with more than 250 employees and those with more than four managers. Since hiring responsibility is much less clearly assigned in these cases, the estimated effects are smaller, but remain positive and significant. In column (5), we weight each migrant's contribution to the cohort share by the number of years they remained in the cohort, using their year of arrival in Denmark as a proxy for cohort entry. Column (6) restricts the sample to schools in which at least one migrant was observed in the five preceding or following cohorts.

Table C7 examines whether the main result is sensitive to the definition of who counts as a manager in a given plant-year. We consider four alternatives to our baseline definition. Column (1) restricts the sample to individuals coded as top managers whenever the plant contains such a top manager. This information is drawn from the occupational classification code (DISCO) provided by Statistics Denmark: specifically, individuals whose first four digits are "1120" before 2010 and "1210" from 2010 onward. Column (2) retains at most one manager per plant-year, selected sequentially by occupational rank, earnings, and tenure (as in our plant-year and hire level specifications). Column (3) expands the sample to include owner-managers recorded as employers in the administrative data.

Column (4) follows the classification in Acemoglu et al. (2022), which identifies top managers using a combination of occupation codes (DISCO) and job hierarchy indicators (PSTILL). A plant's manager is defined as the employee with occupation code 1210 (1120 prior to 2010) "Directors and Chief Executives" and the highest job hierarchy category. If no worker in the plant holds both occupation code 1210/1120 and the highest hierarchy category, managers are defined as those with occupation code 1210/1120 alone. If no worker holds occupation code 1210/1120, managers are defined as those with other managerial occupation codes 1221–1339 (and codes starting with 12, 13, or 14 for the pre-2010 years) and the highest job hierarchy code. Finally, if no worker in the plant holds a managerial occupation code, managers are defined as those with the highest job hierarchy code. If an individual is identified as the manager at plant A in years t and $t+2$ but no manager is observed at plant A in year $t+1$, we assign the same individual as manager in year $t+1$. For plants with multiple employees meeting these criteria in a given year, we define the (top) manager as the one with the highest earnings.

Across all four definitions, the coefficient on the same-gender cohort migrant share remains positive and statistically significant, while the opposite-gender coefficient remains small and indistinguishable from zero. The point estimates are also broadly stable across columns; if anything, column (2) yields a larger coefficient, which is consistent with this definition more directly isolating the single top manager with ultimate hiring responsibility in the plant-year.

Table C8 mirrors Table 6 in the main text but the sample here is the full population of native Danes who completed primary school in the relevant cohorts. The

dependent variables capture educational attainment (column (1)), income rank at age 40 (column (2)), the share of non-Western migrants in the individual's municipality, postcode, and address at age 40 (columns (3)-(5)), and an indicator for having had a non-Western migrant partner (column (6)). For the full population, we find small but statistically significant effects of same-gender cohort contact on the migrant share in the municipality and postcode of residence at age 40, consistent with evidence from the United States reported by Merlino et al. (2024).[26] We find no significant effects on educational attainment, income, or partnership formation with non-Western migrants.

Table C9 presents results from our plant-year level specification, employing manager-change fixed effects as outlined in equation (2). Column (1) reports estimates under the assumption of static effects, which closely mirror those found at the manager level (see Table 4, column (2)). Column (2) replaces same gender migrant share term with an interaction between the difference in this contact measure between the new manager and the predecessor and an indicator variable for the period after the managerial transition. Mechanically, this generates exactly the same results as in column (1), but we include this specification to demonstrate the equivalence before decomposing this interaction more finely as we do in Figure 4. In column (3), we further differentiate the nature of the change brought about by the incoming manager, by focusing on the magnitude of the change and splitting the terms according to the sign of the change. The results indicate that when a new manager had greater contact with same-gender migrant peers in school than their predecessor, the share of non-Western migrant hires increases. Conversely, replacing a manager with someone who had less contact leads to a decrease in hiring of non-Western migrants. Notably, there is no significant effect when the level of migrant exposure remains unchanged following the managerial transition. Finally, column (4) demonstrates that the effect persists—and is at least as pronounced—for hires made more than twenty years after the manager left school, compared to hires made in earlier years.

Table C10 investigates whether contact with the largest migrant group in a manager's school cohort affects the hiring of migrants from other countries. We identify, for each school, the single largest and the two largest non-Western origin groups and construct corresponding cohort shares. In columns (1) and (2), we decompose the dependent variable into the share of hires from the largest non-Western origin group in the manager's school and the share from all other non-Western countries, respectively. In columns (3) and (4), we repeat this exercise using the two largest groups. In both cases, contact with one set of migrant groups significantly increases

[26] As described in footnote 22, this effect is likely too small and incompatible with other results to be an important mechanism behind the changes in managers' hiring behavior.

the hiring of migrants from *other* non-Western countries, while the effect on hiring migrants from the same origin group as in the school cohort is small and statistically insignificant. Since job referral networks tend to be organized along ethnic lines (Munshi, 2003), these cross-group spillovers are difficult to reconcile with a network-based explanation and instead point to a broader reduction in prejudice toward non-Western migrants.

Table C1: Balancing Tests: Full population

	(1)	(2)	(3)	(4)
	Migrant share, same gender	Migrant share, opp. gender	Dep. var. mean	N
Mother in register	0.000067	0.00048	1.0	1,080,924
	(0.0014)	(0.0012)		
Father in register	0.0018	–0.0016	0.99	1,080,924
	(0.0050)	(0.0047)		
Parents income percentile	–0.94	–0.054	69	1,077,015
	(0.66)	(0.61)		
Parents income percentile missing	–0.00055	0.0015	0.0036	1,080,924
	(0.0020)	(0.0019)		
Parents attended higher education	–0.026**	–0.0030	0.17	1,078,630
	(0.013)	(0.012)		
Parents' age	–0.27	–0.037	28	1,070,686
	(0.17)	(0.16)		
Parents are members of church	0.0077	0.012	0.91	808,532
	(0.011)	(0.011)		
Migrant share, municipality	8.3e-06	0.00020	0.021	1,074,015
	(0.00065)	(0.00063)		
Migrant share, postcode	–0.00067	0.00090	0.020	1,051,154
	(0.0014)	(0.0014)		
Migrant share, address	0.0028	0.0049	0.0097	1,074,025
	(0.0048)	(0.0038)		
Emigrated before grade 8	0.0020	0.0051	0.016	1,080,924
	(0.0057)	(0.0054)		
School cohort size	–0.91	–2.4	64	1,080,924
	(1.8)	(1.7)		
Share of cohort female	0.0080	–0.0095	0.50	1,080,924
	(0.014)	(0.014)		

Notes: The table reports OLS estimates controlling for cohort and school fixed effects, two sets of school-specific time trends, and for the share of non-Western migrants in all cohorts up to 5 years older or younger than the individual's cohort. 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. Non-self explanatory variables are defined as follows: *higher education* indicates having completed a bachelor's degree or higher; *Members of church* indicates being a paying member of the Danish state church; *postcode* denotes sub-municipal area codes (approximately 1,100 in Denmark); and *address* is a unique building identifier. Coefficients close to zero support the assumption that within-school cohort-to-cohort variation in migrant shares is approximately as-good-as random. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table C2: Test for random clustering

	(1)	(2)	(3)	(4)	(5)
LHS variable	Migrant share, opposite gender, same cohort		Non-Western migrant dummy		Adjusted non-Western migrant dummy
Migrant share, same gender	0.0033	–0.021			
	(0.022)	(0.017)			
Migrant share of others, same gender, same cohort			–0.00022	0.025	0.029
			(0.018)	(0.018)	(0.018)
Migrant share of others, same gender, same school				–0.99***	
				(0.13)	
R^2	0.71	0.72	0.098	0.10	0.10
Dep. var. mean	0.012	0.017	0.016	0.016	0.016
Number of schools	1,317	1,317	1,317	1,317	1,317
N	49,680	1,080,924	485,416	485,416	485,416

Notes: The table reports OLS estimates controlling for gender, year and school fixed effects, and two sets of school-specific time trends. The unit of observation is the student. The dependent variable in columns (1) and (2) is the opposite gender migrant share in the same cohort. Column (1) reports results for the population of managers, and column (2) for the full population on non-migrants in the relevant cohorts. The dependent variable in columns (3) and (4) is an indicator for being a non-Western migrant. In column (5), the dependent variable is this indicator minus the bias term as calculated by Caeyers and Fafchamps (2024). The sample in columns (3) to (5) is the full population of children, including migrants, excluding younger siblings. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

Table C3: Heterogeneity by migrant gender

	(1)	(2)	(3)	(4)
LHS variable	Share of recruitments that are non-Western			
	male migrants	female migrants	male migrants	female migrants
Migrant share, same gender	0.045***	0.036***		
	(0.014)	(0.013)		
Migrant share, opp. gender	–0.0030	0.0058	–0.0031	0.0060
	(0.013)	(0.013)	(0.013)	(0.013)
Male manager × migrant share, same gender			0.046***	0.040***
			(0.016)	(0.013)
Female manager × migrant share, same gender			0.045**	0.026
			(0.019)	(0.020)
p-val coefs equal	.	.	0.96	0.50
R^2	0.71	0.69	0.71	0.69
Dep. var. mean	0.027	0.020	0.027	0.020
Number of schools	1,317	1,317	1,317	1,317
N	49,680	49,680	49,680	49,680

Notes: The table reports OLS estimates controlling for gender, cohort and school fixed effects, two sets of school-specific time trends, plant fixed effects, and the share of non-Western migrants in all cohorts up to 5 years older or younger than the manager's cohort. The dependent variable is the manager's share of hires that are non-Western male or female migrants, as indicated by column headers. Specifications include the baseline controls and, where shown, interactions between manager gender and same-gender cohort exposure. Standard errors (in brackets) are clustered at the school level. * p $<$ .10, ** p $<$ .05, *** p $<$ .01

Table C4: Heterogeneity of effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Male manager	0.076***						
× migrant share, same gender	(0.023)						
Female manager	0.094***						
× migrant share, same gender	(0.036)						
Migrant sector share low		–0.0063					
× migrant share, same gender		(0.027)					
Migrant sector share high		0.12***					
× migrant share, same gender		(0.024)					
School municipality size low			0.097***				
× migrant share, same gender			(0.037)				
School municipality size high			0.075***				
× migrant share, same gender			(0.023)				
School cohort size low				0.084***			
× migrant share, same gender				(0.022)			
School cohort size high				0.071*			
× migrant share, same gender				(0.039)			
N employees in plant low					0.12***		
× migrant share, same gender					(0.032)		
N employees in plant high					0.053**		
× migrant share, same gender					(0.023)		
N managers in plant low						0.096***	
× migrant share, same gender						(0.030)	
N managers in plant high						0.068***	
× migrant share, same gender						(0.025)	
Migrant share,							0.088***
same gender							(0.029)
Log distance from school							–0.0026
× migrant share, same gender							(0.0094)
p-val coefs equal	0.68	0.00075	0.62	0.78	0.11	0.48	.
R^2	0.73	0.73	0.73	0.73	0.73	0.73	0.73
Dep. var. mean	0.047	0.047	0.047	0.047	0.047	0.047	0.047
Number of schools	1,317	1,317	1,317	1,317	1,317	1,317	1,317
N	49,680	49,680	49,680	49,680	49,680	49,680	48,870

Notes: The table reports OLS estimates controlling for gender, cohort and school fixed effects, two sets of school-specific time trends, plant fixed effects, and the share of non-Western migrants in all cohorts up to 5 years older or younger than the manager's cohort. The dependent variable is the manager's share of hires that are non-Western migrants. Columns report interactions between same-gender cohort exposure and subgroup indicators (as labeled), with p-values testing equality of subgroup effects. *High(low)* indicates above(below) sample median. Standard errors (in brackets) are clustered at the school level. * p $<$ .10, ** p $<$ .05, *** p $<$ .01

Table C5: Recruitee level regressions

	(1)	(2)	(3)
	Baseline specification	+ extra controls	+ occupation fixed effects
Migrant share, same gender	0.13***	0.13***	0.13***
	(0.039)	(0.038)	(0.039)
Migrant share, opp. gender	–0.036	–0.028	–0.030
	(0.041)	(0.041)	(0.041)
R^2	0.16	0.18	0.17
Dep. var. mean	0.047	0.047	0.047
Number of schools	1,329	1,329	1,329
N	634,863	634,813	634,813

Notes: The table reports OLS estimates controlling for gender, cohort and school fixed effects, two sets of school-specific time trends, plant fixed effects, and the share of non-Western migrants in all cohorts up to 5 years older or young than the manager's cohort. The unit of observation is the recruit (new hire), and the dependent variable is an indicator for whether the recruit is a non-Western migrant. Specifications correspond to the baseline recruit-level model and the modifications listed in the column headers. *Extra controls* in column (2) include hire-level indicators for female and completed higher education, hire age, and log earnings in the year prior to hire. 6-digit occupational codes are used in column (3). Positive coefficients imply that managers with greater non-Western immigrant contact in school are more likely to hire non-Western migrants at the individual hire margin. Standard errors (in brackets) are clustered at the school level. * p $<$ .10, ** p $<$ .05, *** p $<$ .01

Table C6: Alternative measurement strategies

	(1)	(2)	(3)	(4)	(5)	(6)
	Alternative cohort assignment	Without school FEs or trends	Weighting by number of hires	Including large plants	Migrant share weighted by time in Denmark	Excluding schools with no migrants
Migrant share, same gender	0.069***	0.087***	0.13***	0.019**		0.094***
	(0.019)	(0.025)	(0.032)	(0.0087)		(0.021)
Migrant share, opp. gender	0.012	0.0079	–0.0053	–0.0017		0.0056
	(0.018)	(0.023)	(0.029)	(0.0092)		(0.022)
Weighted migrant share, same gender					0.086***	
					(0.024)	
Weighted migrant share, opp. gender					–0.0072	
					(0.022)	
R^2	0.73	0.0080	0.94	0.75	0.73	0.76
Dep. var. mean	0.047	0.047	0.059	0.056	0.047	0.052
Number of schools	1,306	1,317	1,341	1,342	1,317	1,164
N	49,125	49,680	1,211,220	94,099	49,680	29,731

Notes: The table reports OLS estimates controlling for gender, cohort and school fixed effects, two sets of school-specific time trends, plant fixed effects, and the share of non-Western migrants in all cohorts up to 5 years older or younger than the manager's cohort. The dependent variable is the manager's non-Western hiring share. Each column implements the alternative measurement/assignment strategy described in the header. *Alternative cohort assignment* means that cohorts are based on birth year instead of class cohort. *Large plants* are all plants with more that 250 employees. *Time in Denmark* is measured from the immigration year. Consistent estimates across columns indicate that results are not sensitive to how exposure or hiring shares are constructed, while the lower coefficient in column (4) is consistent with each individual manager having little impact on hiring decisions within plants that have more than 250 employees. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table C7: Alternative manager definitions

	(1)	(2)	(3)	(4)
	Only highest ranked managers	Only one manager per plant per year	Including owner managers	As used by Acemoglu et al. (2022)
Migrant share, same gender	0.078***	0.13**	0.071***	0.070***
	(0.023)	(0.050)	(0.019)	(0.019)
Migrant share, opp. gender	0.028	–0.025	0.0044	0.0027
	(0.024)	(0.051)	(0.019)	(0.019)
R^2	0.75	0.73	0.76	0.76
Dep. var. mean	0.047	0.047	0.048	0.048
Number of schools	1,309	1,268	1,320	1,320
N	41,162	22,360	53,504	53,655

Notes: The table reports OLS estimates controlling for gender, cohort and school fixed effects, two sets of school-specific time trends, plant fixed effects, and the share of non-Western migrants in all cohorts up to 5 years older or young than the manager's cohort. The dependent variable is the manager's non-Western hiring share. Each column uses an alternative definition of *manager* (as described in the header). Column (4) follows Acemoglu et al. (2022) manager definitions. Similar coefficients across columns indicate that the main result is not driven by a particular managerial definition. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

Table C8: Other outcomes, full population

	(1)	(2)	(3)	(4)	(5)	(6)
LHS variable	Completed higher education	Income percentile at 40	Migrant share in municipality at 40	Migrant share in postcode at 40	Migrant share at address at 40	Had migrant partner
Migrant share, same gender	–0.016	–1.1	0.0074***	0.0076**	0.011	–0.0024
	(0.021)	(0.96)	(0.0025)	(0.0034)	(0.0067)	(0.0058)
Migrant share, opp. gender	–0.025	0.46	–0.00049	0.00030	0.0072	0.00039
	(0.018)	(0.88)	(0.0023)	(0.0029)	(0.0059)	(0.0055)
R^2	0.071	0.039	0.29	0.18	0.030	0.0068
Dep. var. mean	0.34	76	0.066	0.062	0.024	0.019
Number of schools	1,317	1,316	1,314	1,314	1,314	1,317
N	1,080,445	960,402	862,562	862,416	862,564	1,080,924

Notes: The table reports OLS estimates controlling for gender, cohort, and school fixed effects, two sets of school-specific time trends, and the share of non-Western migrants in all cohorts within a +/- 5 year window of the individual's cohort. The table tests whether migrant contact in school has any significant impact on later educational and labor market outcomes of individuals without an migrant background. Dependent variables (by column) capture individual's own educational attainment, income rank at age 40, residential migrant exposure at different geographic levels at age 40, and partnering outcomes, as labeled. Standard errors (in brackets) are clustered at the school level. * p < .10, ** p < .05, *** p < .01

Table C9: Regressions at the establishment-year level

	(1)	(2)	(3)	(4)
Migrant share, same gender	0.16***			
	(0.044)			
Migrant share, opp. gender	–0.015	–0.015	–0.016	
	(0.047)	(0.047)	(0.047)	
Exposure change × post-manager change		0.16***		
		(0.044)		
Exposure change (if decrease) × post-manager change			–0.12**	
			(0.053)	
No exposure change × post-manager change			–0.0020	
			(0.0016)	
Exposure change (if increase) × post-manager change			0.19***	
			(0.053)	
$\leq$ 20 years after leaving school × migrant share, same gender				0.13**
				(0.063)
$>$ 20 years after leaving school × migrant share, same gender				0.17***
				(0.058)
p-val coefs equal	.	.	.	0.62
R^2	0.58	0.58	0.58	0.59
Dep. var. mean	0.048	0.048	0.048	0.048
Number of schools	1,311	1,311	1,311	1,309
N	80,593	80,593	80,593	80,519

Notes: The table reports OLS estimates controlling for gender, cohort and school fixed effects, two sets of school-specific time trends, fixed effects, and the share of non-Western migrants in all cohorts up to 5 years older or younger than the manager's cohort. The unit of observation is the plant-year. The variable is the difference between the same-gender migrant share of the new manager and that of the old manager. The dependent variable is the share of new hires that are non-Western migrants. Columns implement alternative specifications around manager changes and heterogeneity by time since leaving school. Standard errors (in brackets) are clustered at the school level. * p $<$ .10, ** p $<$ .05, *** p $<$ .01

Table C10: Impacts of largest migrant groups in school

	(1)	(2)	(3)	(4)
LHS variable	Share of recruitments that are			
	migrants from largest non-Western migrant group in school	non-Western migrants not from largest migrant group in school	migrants from two largest non-Western migrant groups in school	non-Western migrants not from two largest migrant groups in school
Largest migrant group in school share, same gender	0.0067	0.064**		
	(0.011)	(0.025)		
Other migrants share, same gender	0.0048	0.090***		
	(0.014)	(0.025)		
Two largest migrant groups in school share, same gender			0.0068	0.072***
			(0.011)	(0.022)
Other migrants share, same gender			–0.012	0.10***
			(0.021)	(0.029)
R^2	0.52	0.71	0.53	0.71
Dep. var. mean	0.0037	0.043	0.0048	0.042
Number of schools	1,317	1,317	1,317	1,317
N	49,680	49,680	49,680	49,680

Notes: The table reports OLS estimates controlling for gender, cohort and school fixed effects, two sets of school-specific time trends, plant fixed effects, and the share of non-Western migrants in all cohorts up to 5 years older or younger than the manager's cohort. The dependent variable is the manager's share of hires from the largest (or two largest) non-Western origin group(s) represented in the manager's school cohort and other non-Western groups, as indicated by column headers. Significant impacts of exposure to some migrant groups impacting the hiring of migrants from other countries speak against narrow origin-specific hiring and toward broader openness. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Appendix D Additional Figures and Tables on ESS Analysis

Table D1 replicates the balancing tests from Table 2 for the subsample of native Danish respondents in the European Social Survey (ESS) whom we can link to administrative school records. Because the ESS sample is considerably smaller than the population of managers ($N \approx 3{,}700$), we cannot include school fixed effects or school-specific time trends. Instead, we control for cohort fixed effects and the share of non-Western migrants in all cohorts within a ± 5 year window of the individual's own cohort. For each predetermined characteristic, including parental income, education, age, church membership, and the migrant share in the individual's municipality, postcode, and address, we regress the variable on the same-gender and opposite-gender cohort migrant shares. The coefficients on the same-gender share are uniformly small and statistically insignificant, supporting the conclusion that the residual variation in gender-specific migrant shares used in the ESS analysis is exogenous.

Table D2 reports precise wording and variable definitions of the questions we used to construct the indexes used in Table 9.

Figure D1 plots the coefficient on the same-gender cohort migrant share from separate OLS regressions for each individual question in the ESS immigration module, as well as for a composite index that aggregates all items. The questions are grouped into the four thematic areas corresponding to the indices reported in Table 9: perceived positive impacts of immigration (area 1), preferred immigration levels (area 2), migrant friendships (area 3), and absence of ethnic prejudice (area 4). Additional questions on migrant-related attitudes and beliefs that were fielded in only a single ESS wave are reported separately. The figure reveals a consistently positive pattern: across nearly all items, individuals who had more same-gender non-Western classmates in primary school report more favorable attitudes toward migrants. The composite index at the bottom of the figure confirms a systematically more pro-immigration stance among individuals with greater childhood contact, reinforcing the interpretation that early interethnic exposure produces a broad and persistent shift in attitudes.

Table D1: Balancing Tests: European Social Survey

	(1)	(2)	(3)	(4)
	Migrant share, same gender	Migrant share, opp. gender	Dep. var. mean	N
Mother in register	0.0015	0.0030	1.0	3,686
	(0.012)	(0.011)		
Father in register	0.070	0.073	0.99	3,686
	(0.078)	(0.073)		
Parents income percentile	–1.6	–2.5	69	3,682
	(11)	(11)		
Parents income percentile missing	–0.019	0.0096	0.0011	3,686
	(0.024)	(0.022)		
Parents attended higher education	–0.063	–0.042	0.19	3,685
	(0.24)	(0.22)		
Parents' age	1.4	1.8	28	3,652
	(3.4)	(3.1)		
Parents are members of church	–0.13	0.52***	0.91	2,732
	(0.19)	(0.18)		
Migrant share, municipality	–0.019	–0.011	0.019	3,669
	(0.012)	(0.011)		
Migrant share, postcode	–0.020	–0.016	0.017	3,590
	(0.014)	(0.013)		
Migrant share, address	–0.030	0.074**	0.0088	3,669
	(0.036)	(0.034)		
Emigrated before grade 8	0.11	0.0084	0.016	3,686
	(0.092)	(0.086)		
School cohort size	–4.0	5.4	64	3,686
	(19)	(17)		
Share of cohort female	–0.043	–0.068	0.50	3,686
	(0.052)	(0.048)		

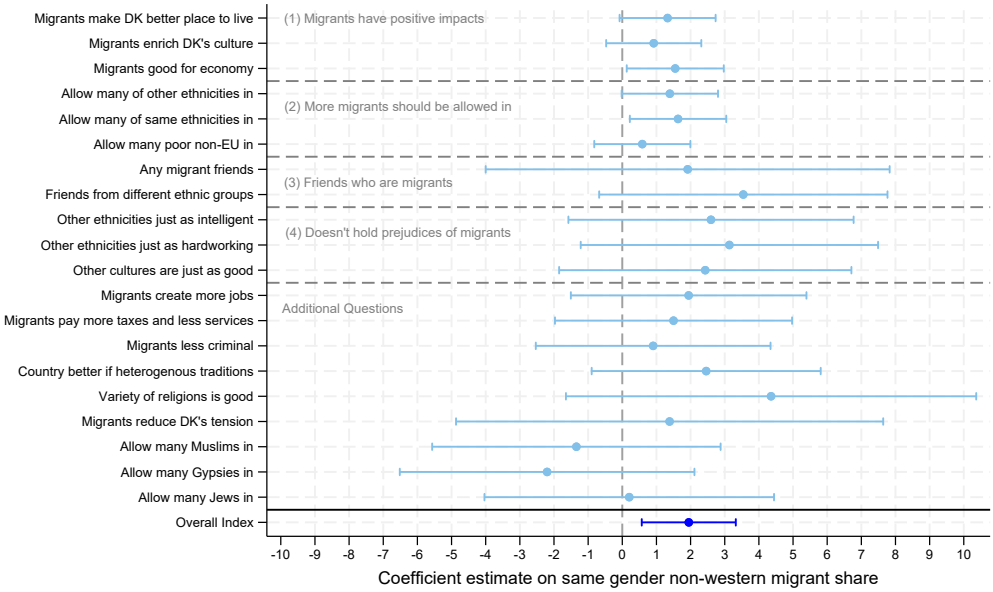
Notes: The table reports OLS estimates controlling for cohort fixed effects and for the share of non-Western migrants in all cohorts up to 5 years older or young than the individual's cohort. Coefficients in the first and second column of each row are from a regression where the variable indicated in each line is regressed both on the same gender and opposite gender shares of non-Western immigrants, respectively. Non-self explanatory variables are defined as follows: *higher education* indicates having completed a bachelor's degree or higher; *Members of church* indicates being a paying member of the Danish state church; *postcode* denotes sub-municipal area codes (approximately 1,100 in Denmark); and *address* is a unique building identifier. Coefficients close to zero support the assumption that within-school cohort-to-cohort variation in migrant shares is approximately as-good-as random. Standard errors (in brackets) are clustered at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table D2: ESS Immigration Attitudes Variables and Question Wording

ESS variable	Question wording	Scale
imwbcnt	Is Denmark made a worse or a better place to live by people coming to live here from other countries?	11-point
imueclt	Would you say that Denmark's cultural life is generally undermined or enriched by people coming to live here from other countries?	11-point
imbgeco	Would you say it is generally bad or good for Denmark's economy that people come to live here from other countries?	11-point
imdfetn	Allow many/few immigrants of different ethnic group from majority in Denmark?	4-point
imsmetn	Allow many/few immigrants of same ethnic group from majority in Denmark?	4-point
impcntr	Allow many/few immigrants from poorer countries outside Europe?	4-point
imgfrnd	Do you have any friends who have come to live in Denmark from another country?	3-point
dfegcf	Do you have any close friends who are of a different ethnic group from most Danish people?	3-point
smegbli	Do you think some ethnic groups are born less intelligent than others?	2-point
smegbhw	Do you think some ethnic groups are born harder working than others?	2-point
smctmbe	Thinking about the world today, would you say that some cultures are much better than others or that all cultures are equal?	2-point
imtcjob	Would you say that people who come to live here generally take jobs away from workers in Denmark, or generally help to create new jobs?	11-point
imbleco	Most people who come to live here work and pay taxes. They also use health and welfare services. On balance, do you think people who come here take out more than they put in or put in more than they take out?	11-point
imwbcrm	Are Denmark's crime problems made worse or better by people coming to live here from other countries?	11-point
pplstrd	How much do you agree or disagree that it is better for a country if almost everyone shares the same customs and traditions?	5-point
vrtrlg	How much do you agree or disagree that it is better for a country if there are a variety of different religions?	5-point
stimrdt	How much do you agree or disagree that if a country wants to reduce tensions it should stop immigration?	5-point
almuslv	To what extent do you think Denmark should allow Muslims from other countries to come and live in Denmark?	4-point
algyplv	To what extent do you think Denmark should allow Gypsies from other countries to come and live in Denmark?	4-point
aljewlv	To what extent do you think Denmark should allow Jewish people from other countries to come and live in Denmark?	4-point
ppltrst	Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people?	11-point
trslplt	How much do you personally trust politicians?	11-point

Notes: This table reports the wording of all European Social Survey (ESS) questions used in the study. Variable names correspond to the ESS questionnaire. *Scale* reports the response scale for each item.

Figure D1: All Questions on Immigration from the European Social Survey



Notes: This figure reports coefficient estimates from separate OLS regressions of each listed ESS immigration-attitude question (and a summary index) on same-gender school-cohort contact with non-Western migrants, with the ESS controls described in the paper. Questions are grouped into four areas corresponding to the indices in Table 9: *(1) Migrants have positive impacts* (column 1), *(2) More migrants should be allowed* (column 2), *(3) Friends who are migrants* (column 3), and *(4) Doesn't hold prejudices of migrants* (column 4). *Additional Questions* capture other migrant-related attitudes and beliefs questions asked in only a single ESS wave. Horizontal bars show 95% confidence intervals based on standard errors clustered by primary school.