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Immigrant peers and English language skills

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Abstract

Does the exposure of children to immigrant peers with proficient language skills increase foreign language acquisition? We examine this in the context of Norway, where all students are taught and assessed in English from the beginning of compulsory school. We use this, combined with recent increases in immigration, to investigate the effect of exposure to English-speaking peers on the English language skills of Norwegian children. Exposure increases these skills, has no effect on other educational attainment, and does not appear to reflect general peer quality effects or other confounding factors. The effects are evident only within same-gender peers and for children with initially above-average English proficiency. This suggests important roles for social interaction and baseline language competencies.

Keywords Immigration · Language skills · Educational attainment

JEL Classification J15 · I21

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1 Introduction

Foreign language skills are a focus of many education systems. This reflects several motives, ranging from a view that multilingualism has a positive effect on cognitive skills, through to prioritising internationally used languages. It is generally believed that exposure to proficient speakers of a language is important and provides one of the motivations behind a variety of student exchange programmes at the school level and beyond. Yet, in practice very little is known about the effects of this exposure, particularly for younger children. It is this gap that this paper seeks to fill.

The lack of evidence in this area reflects difficulties in identifying the effects of exposure. For example, while Spanish in the US or French in the UK are often prioritised, selection into these subjects is still typically voluntary (or at least alternative languages are offered and available) and there is a lack of universal testing of skills. The Nordic countries are potentially instructive as they prioritise English from an early school age. For example, in Norway, the focus of this paper, all children take English from school entry. Importantly for our purposes, English is also universally tested using standardised central exams from a relatively early age. This, coupled with the recent changes in immigration patterns in Norway and high-quality register data, provides an opportunity to examine the effect of exposure to English-speaking background (ESB) immigrant children on the language skills of native Norwegian children.

Exposure to ESB immigrant school peers could influence language attainment in a variety of potential ways. For example, from an educational production function perspective (Todd and Wolpin 2003), peers with language competence could represent additional, domain-specific, educational inputs within the classroom. At the same time, the use of English outside of the classroom may complement the language skills learned by students in the classroom. Critically, this type of informal learning is likely to be conditional on the existing English language skill levels of native students. Irrespective of the channel, any learning effects are likely to be more pronounced in specific critical periods for acquiring language skills (Bleakley and Chin 2004; 2010).

Our main approach is to estimate the effect of school-year variation in English-speaking background immigrant children on the English attainment of Norwegian children. We control for shares of immigrant children such that our identifying assumption is that, holding immigrant shares constant, whether a given immigrant student is from an ESB background (or not) is as good as random from the perspective of their native school colleagues. Our main result is that Norwegian primary school children exposed to English-speaking peers exhibit higher educational attainment in English. We demonstrate that this is not a broader peer-ability effect reflecting, for instance, the positive selection of English-speaking background immigrant children in general. For example, exposure to immigrant children who are proficient in English has a zero effect on native students' Norwegian language and math attainment.

Our empirical approach leaves room for a range of other potential biases that may undermine the interpretation of our estimates. A set of concerns relates to

the mobility of Norwegian children. The nature of our results and setting mean that this sorting would have to solely reflect English ability and/or interest, rather than, for instance, wider academic ability. Nonetheless, we explore the potential role of mobility in a range of ways, including school choice, holding prior ability constant, and using within-sibling comparisons. An additional set of challenges relates to our measure of the language background of immigrant children. In our main approach, we use data, primarily from the CIA World Factbook, to categorise countries of origin by English language usage as a first language. We explore the robustness of our results to utilising alternative approaches based on the prior English proficiency of immigrant children.

A question is, how do these spillovers occur? We provide evidence demonstrating that these peer effects occur solely within same-gender peers, and only for Norwegian children with prior competence in English. Together, this suggests that exposure to English language-speaking peers represents an important determinant of language acquisition, but that this is conditional on base language competency and social interactions that are likely to occur outside of classrooms.

Our paper is related to several areas of research. First, there is now a large literature on whether immigrant children peers affect the core skills of native children (see, for instance, Figlio and Özek 2019; Bossavie 2020; Green and Iversen 2022; Morales 2022; and Figlio et al 2024, amongst many others). The main focus of this literature is the potential of exposure to immigrant children to have deleterious effects on the core skills, typically math and/or literacy, of native children. This literature presents mixed results. For example, research on exposure to immigrant peers demonstrates marked negative effects on native student educational attainment in Italy (Ballatore et al 2018; Frattini and Meschi 2019) and Denmark (Jensen and Rasmussen 2011), smaller effects in cross-country European evidence (Brunello & Rocco 2013), no effects in England (Geay et al 2013), and in some US studies positive effects (Hunt 2017; Figlio et al 2024). A smaller set of research focuses specifically on exposure to immigrants from a refugee background, where any negative effects may be expected to be larger. These again show effects ranging, for example, from negative effects on mathematics in Norway (Green and Iversen 2022) to zero effects in Florida (Figlio and Özek 2019). We take this as a starting point and adopt a similar identification approach but focus on a particular domain where immigrant exposure may potentially have positive effects, namely the effect on native students' English language skills from exposure to English-speaking immigrant peers.

A broader, related literature examines the labour market effects of bilingualism. For example, Cappellari and Di Paolo (2018) demonstrate that a policy shift to bilingual education (Spanish and Catalan) in Catalan schools led to increases in the returns to education, and these effects are concentrated amongst non-Catalan background students. Ginsburgh and Prieto-Rodriguez (2011) show that proficiency in a foreign language, primarily English, has marked effects on individual earnings in a number of European countries. Similarly, Stöhr (2015) finds large labour market returns to high-level English skills in Germany. Recently, Strazzeri et al (2025) demonstrate only muted effects of early English training in Switzerland on educational progression but find some deleterious effects on students in the lower educational track. Our paper contributes to this literature in that we show that increased

diversity, in terms of students' language background, during schooling can lead to higher, labour market-relevant, language skills of natives.¹

A related and large literature examines the effects of immigrants' acquisition of host country language skills on their own later outcomes (Chiswick and Miller 1995; Dustmann and Van Soest 2001; Bleakley and Chin 2004, 2008, 2010). In this paper, we demonstrate that immigrants can themselves influence the language skills of host-country children. This literature demonstrates critical periods where language is most readily acquired by children. This is relevant to the interpretation of our results, which come from exposure between the ages of approximately 10 and 14. This is at the latter part of the critical period and hence effects could be larger if exposure occurred at earlier ages.

2 Background, institutional details and data

English is compulsorily taught in Norway from an early age (starting in first grade) throughout compulsory schooling (to age 16) and post-compulsory schooling. This reflects a view that it is an essential skill for all children. While other foreign language options are available later in schooling, English remains compulsory throughout. At the same time for many children English is never or rarely spoken outside of school (or even outside of English class). Children's TV and movies are typically dubbed into Norwegian and in practice there is large variation in the English language ability of adult Norwegians. Norwegian remains the main language of everyday family, leisure and work life. For example, recent PISA evidence shows that only 3% of 15 year old Norwegians without an immigrant background have a home language other than Norwegian (OECD 2023).

At the same time, Norway has recently experienced a dramatic increase in immigration from a previously low base. This has included a shift away from earlier patterns which featured substantial immigration from neighbouring countries with similar languages to Norwegian to a broader range of immigrants including from English-speaking countries. Prior to the 1990s, Norway had a very small immigrant population (approx. 3.8% of the total population in 1989), and a large share were European economic immigrants. Economic immigration has increased markedly over the past 3 decades, for example increasing from 2400 entrants in 1993 to 26,700 in 2011, and with changes in the countries of origin. Naturally, economic immigrants are free to choose where they live within Norway.² While there is a greater concentration in major cities, there remains a substantial spread across Norway. Ultimately, we demonstrate that our estimates hold outside of the major cities where sorting may be the most acute.

¹ There is also a long-standing (but not large) literature that seeks to examine the effect of bilingual instruction programs in schools on educational attainment. These find mixed results, ranging from positive effects (Pope 2008), null effects on limited English proficiency students (Chin et al 2013) through to substantial negative effects on core skills attainment, see, for instance, Jepsen (2010) and Anghel et al (2016).

² Recently arrived refugees are not, see for instance Green and Iversen (2022).

There exist several institutional features that are additionally advantageous for our research. In our period of analysis, and since 1997, school is compulsory for children aged 6–16. There is no ability school tracking system in compulsory schooling. Norwegian schools follow a legally defined neighbourhood school system, and only a very small number of municipalities have some exceptions from this law and allow a degree of school choice. This means that Norwegian students go to their local school with other children resident in the same area. The number of private primary schools is very low and in our period of analysis less than 2 percent of Norwegian children attend private primary schools. In practice, a substantial share of these private schools are in the same location, within the same buildings, as local schools that have been recently closed (typically in rural areas) and these schools also enrol students from the local area. Public schools are obliged to take all students from predefined catchment areas that are very infrequently altered, and students must attend their local school. In some cases, primarily within larger cities, municipalities offer introductory classes for newly arrived immigrant children. However, these are offered on a short-term basis (from 6 months to one year) before these students are integrated into their neighbourhood school.

Mobility among young Norwegian students is low. The strict neighbourhood school system means that changing school requires moving house. This is particularly costly in the Norwegian setting due to the very high levels of house ownership. The share of households owning their own home in Norway is approximately 80 percent. In comparison, the share in other Nordic countries and in the UK is closer to 60 percent, and the share is about 50 percent in southern Europe (Aarland and Sørvoll 2021). Hence, changing school typically involves buying and selling property. These features, combined with a lack of school choice, make Norway a good setting to examine the effect of exposure to English-speaking Background (ESB) immigrant children on the language skills of native Norwegian children.

Our data on test scores comes from the Norwegian Directorate for Education (UDIR). Norwegian students are tested in reading in Norwegian, reading in English and mathematics in 5th grade, 8th grade and 9th grade. We focus on 8th grade scores (corresponding roughly to age 14) for a number of reasons. First, these tests occur approximately one month upon entry into middle school (ungdomsskole). This leads us to use grade composition in primary school as the main measure of peer exposure. This combined with earlier test scores allows us to explore alternative approaches to categorising ESB immigrant school children (more below) while also allowing for controls for prior attainment. We observe grade composition of all students in 5th grade. Given the low mobility in Norwegian schools, this provides an informative proxy for grade composition in upper primary school (grades 5 to 7). One complicating factor with the 5 test scores is that, for technical reasons, the English language test was not conducted in 2011.

English is taught in Norwegian schools from the start of primary school (barneskole) which commences in the year that children turn 6. While it is in principle possible to delay school entry, in practice this is very uncommon: approximately 98.6% of school children start in the ‘correct’ year in our period of analysis. There is also a strong emphasis on social progression such that grade retention is extremely

rare. English language forms a compulsory part of instruction across all years of compulsory schooling, and through to the academic track of high school (age 16+).

We standardise test scores to mean 0 and standard deviation of 1 for each year. Our population of analysis is all Norwegian eighth graders for 2010 to 2015 inclusive, except for a very small number of students who are exempted from tests for reasons such as severe special educational needs.³ This provides six cohorts of between 50 000 and 60 000 students every year. We observe in which year students are in within a given school and grade, but not their class. Hence all measures of composition are at the school-year level. In theory, this could introduce an issue if ESB students are clustered in specific classrooms. However, Norwegian law prohibits schools from dividing classes by ethnicity, ability or gender (opplæringsloven § 14–2). Moreover, schools are required to maximise diversity in classrooms. In practice, this means that if there are 2 classes and 2 immigrant-background children, those children should not be placed in the same class. This has come back into focus recently with the arrival of Ukrainian refugees (Udir – Regulations for newly arrived pupils and adults (14.03.2025)).

This test score data is merged with individual information and family information from Statistics Norway. The family information includes parental education, income and a range of other standard family background variables. Information on schools, such as enrolment, school type and other characteristics of the schools, is drawn from an administrative system (Grunnskolen informasjonssystem, GSI). This information is collected annually. We lack information on teachers at a grade level, but it is worth noting that there are typically no specialist English teachers at this age group. We observe a range of information regarding students from an immigrant background. Of importance is the information on reasons for immigration and source country. We observe if an immigrant came to Norway as a refugee, asylum seeker, for family reunion, education, or for work. Our approach is to assign refugee status to a child if they or either of their parents entered Norway originally as a refugee or asylum seeker. This aims to capture, for instance, the relatively common case in Norway where the first entrant was a refugee but where the other parent and/or the child themselves entered for the purposes of family re-union. Other immigrant children are those who have at least one parent born overseas who originally came to Norway for work or education. Our focal group is Norwegian-born children who have two parents who were also born in Norway.

Importantly, we observe country of origin, and this is our initial way of determining the language background of children. Specifically, we assign a student to being from an English-speaking background if they or at least one of their parents (who they live with in Norway) is from one of the English-speaking countries specified in Appendix A1. We use data, primarily from the CIA World Factbook, to define English-speaking countries as those having more than 60% with English as their first language. These children are, in practice, drawn from the group of non-refugee immigrant children. Naturally, our approach is likely to generate measurement error.

³ National testing was first introduced in 2007. Our main empirical approach relies on observing students in both 8th and 5th grade. This leads to 2010 being our first 8th grade cohort.

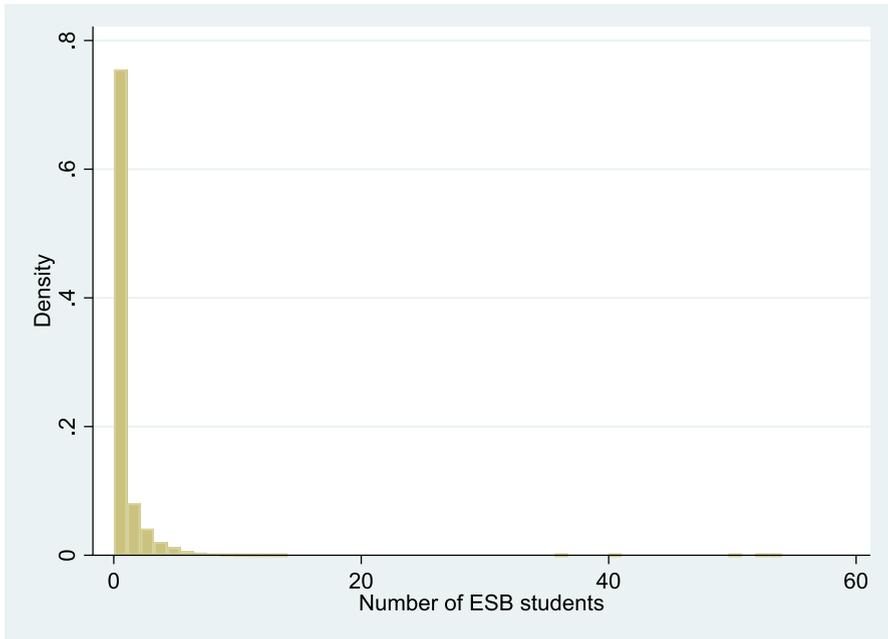


Fig. 1 Distribution of ESB students across school grades

In our results, we explore a range of alternative classification approaches where the main message is that our results are robust to these. Appendix Table 2 provides descriptive statistics on the key variables in our analyses split according to the immigrant status of children.

On average, non-immigrant Norwegian 5th grade students are in school grades where 3.8% of students are refugees and 4.2% are other immigrant background children. Figure 1 displays the distribution of ESB students across school-grade observations. This figure displays all Norwegian schools in our observation period. In our estimation sample, we exclude a small number of international schools which often are English language schools and have, as a result, very high shares of ESB students. This leaves only three schools that, at any time, have more than 10 ESB students in a grade. Nonetheless, we later provide robustness tests to additionally excluding these schools.

To investigate balance, Table 3 provides estimates of differences in key characteristics and school environment of Norwegian students exposed to different levels of ESB peers. These come from separate regressions with school fixed effects included which, as described below, matches the variation we use. While there are some statistically significant differences in characteristics, our view is that these largely reflect the precision of our estimates, and these differences are not in practice meaningful. For example, Norwegian kids with 3 or more ESB students in their grade have mothers who earn approximately 3700 NOK per year (\$US 345) more before tax, and fathers who earn 7962 NOK more (\$US 739). These are very small effects,

only about 1 percent of annual average income in Norway or about $\frac{1}{4}$ of the average annual wage increase in Norway. Their mothers, on average, are 1 percentage point more likely to have a degree (on a base of 0.49). There are some small differences in grade composition, for example, the mean share of refugees in the class is 0.3 percentage points lower for those Norwegian students with 3 or more ESB students in their grade. These differences are unlikely to generate differences in educational attainment. Nonetheless, we acknowledge that we cannot rule out sorting. As a result, we adopt a number of approaches aimed at exploring if sorting is likely to be consequential for our results.

Figure 2 provides information on differences in English, Norwegian and mathematics test score performance according to whether students are Norwegian, English-Speaking Background (ESB) Immigrants, Non-English-Speaking Background (NESB) immigrants, or children from a refugee background. The first panel clearly demonstrates that the average ESB immigrant outperforms all other students in English tests and are, in general, substantially over-represented amongst the highest performers. There is little difference between native and NESB students. A key question is whether these differences likely reflect overall ability or socio-economic differences across these student groups, for instance, the differences in parental background observed in Table 2. Here the plots for both mathematics and Norwegian are informative. The mathematics plot demonstrates some indication that ESB immigrant children outperform native children, while NESB children perform worse. A similar picture is revealed from the plot of Norwegian scores. These suggest little difference in average performance between ESB and Native performance in areas other than English. In English, ESB students perform particularly better. Nevertheless, we later explore the potential for spillovers in other subjects.

3 Empirical strategy

Our main estimating equations are variants of the following:

$$A_{ist} = \alpha_0 + \alpha_1 \text{OneESB}_{st} + \alpha_2 \text{TwoESB}_{st} + \alpha_3 \text{ThreeplusESB}_{st} + \alpha_4 \text{Imm}_{st} + \alpha_5 \text{Refugee}_{st} + \beta \mathbf{X}_{ist} + \delta_s + \gamma_t + u_{ist} \quad (1)$$

where A_{ist} is student achievement for individual i at the beginning of grade 8. We regress this on exposure to immigrant peers in the primary school they attended, where practically this means that all relevant immigrant characteristics and individual controls are measured at grade 5 (the previous observation in the relevant registers). Hence, OneESB_{st} is a dummy variable that equals 1 if school s has one ESB student at time t . TwoESB_{st} is a dummy variable that equals 1 if the school has two ESB students at time t , and ThreeplusESB_{st} is a dummy variable that equals 1 if the school has three or more ESB students at time t . We additionally control for Imm_{st} and Refugee_{st} , the share of immigrant children and refugee children, respectively. This means our main estimates are the effect of primary school exposure (grade 5) to immigrant peers on educational attainment upon entry into middle school (start of grade 8). When combined with the inclusion of grade 5 school fixed effects (δ_s)

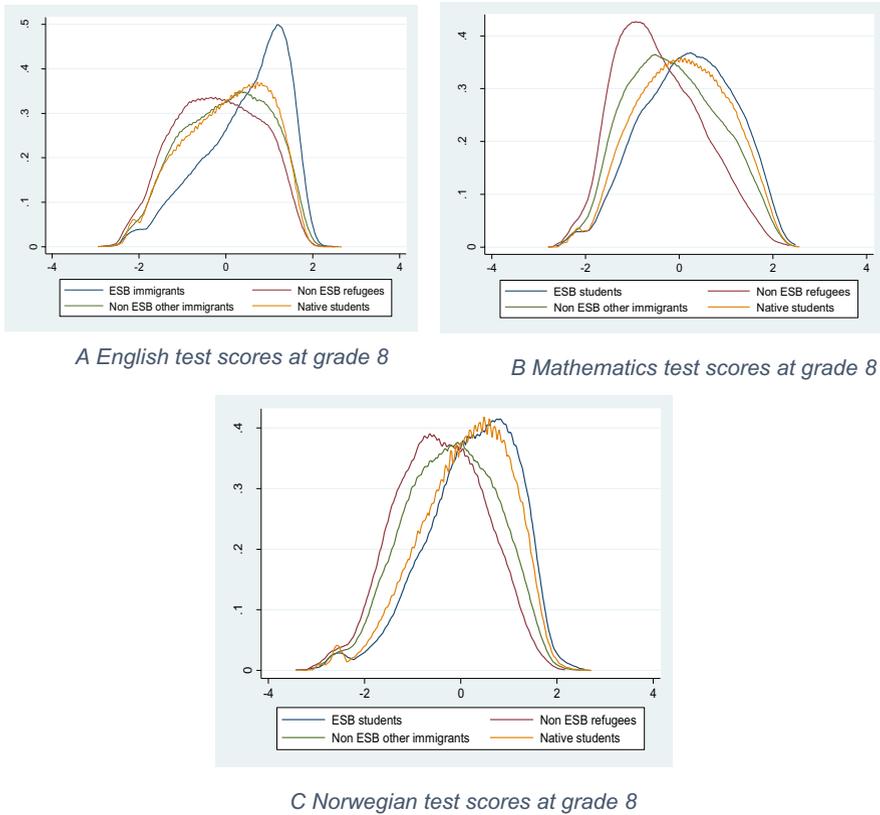


Fig. 2 Kernel density plots of 8th grade test scores

our estimates of interest come from within school-cohort variations in the number of ESB students holding constant the share/number of both immigrant and refugee students. Hence the identifying assumption is that, for a given share of immigrants and refugees in a class, it is as good as random if a given immigrant is from an English-Speaking Background. X_{ist} is a vector of time varying school cohort characteristics and individual student characteristics. γ_t is the year fixed effect, while u_{ist} is an error term. We cluster standard errors at the school-year level. We estimate (1) and all models only for native students: students born in Norway with two Norwegian born parents.

The choice of functional form for ESB is naturally important. As above, our main approach is to consider a non-linear form of the number of ESB-students. This reflects a view that there are likely non-linear effects in exposure, ease of interpretation of estimates, and that the register data allows us to precisely estimate such models. Alternative forms include the number of ESB students or the share of ESB students. We provide initial estimates using the number of ESB students and demonstrate positive effects of exposure using this measure. A primary cost of this

Table 1 The effect of English-speaking background (ESB) peers on English test score performance at grade 8

	(1)	(2)	(3)
Number of ESB students		0.004* (0.002)	
One ESB student			-0.005 (0.007)
Two ESB students			0.023** (0.009)
3 + ESB students			0.023** (0.011)
Share of ESB students	0.076 (0.076)		
Share of refugees	-0.033 (0.087)	-0.031 (0.087)	-0.031 (0.087)
Share of other immigrants	0.065 (0.072)	0.067 (0.072)	0.067 (0.072)
Constant	-0.212*** (0.018)	-0.211*** (0.018)	-0.210*** (0.018)
Observations	206,032	206,032	206,032
r^2	0.120	0.120	0.120

All explanatory variables are measured at 5th grade level. All models include controls for the share of other immigrants and the share of refugees. Other control variables are gender, maternal education level, paternal education level, parental income, live with both parents, number of siblings, birth order and number of students enrolled in the grade. All models have school and year fixed effects. Robust standard errors clustered at the school-year level in parentheses

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

approach is that it is more restrictive in assuming linearity in effects. The same is true for the share of ESBs which has the additional problem of being more difficult to interpret, and where variation can reflect changes in the denominator (number of native students). We also provide estimates using this measure which are positive but not statistically significant at standard levels.

As stated above, identification is at a school-grade level. While average grade size is relatively small, 27 students in our last year of observation, we do not observe exposure to ESB peers within classes. This means our main parameters of interest have an Intention to Treat (ITT) interpretation. To explore the potential for this to understate actual effects of exposure to ESB peers we explore within and across gender peer effect patterns (e.g. the effect of male ESB peers on native male students). This follows from a view that children's interactions are to some extent gendered in the ages we focus on. If language spillovers reflect interactions between students, then we might expect within gender effects to be larger than our main estimates. In a similar vein, the ability to meaningfully interact and learn a language is likely a function of own proficiency in that language. This leads us to explore variants of (1)

Table 2 The effect of English-speaking background (ESB) peers on mathematics and Norwegian test score performance at grade 8

	Math		Norwegian	
	1	2	3	4
Number of ESB students	0.003 (0.002)		0.003 (0.002)	-0.003 (0.007)
One ESB student		-0.001 (0.006)		-0.005 (0.008)
Two ESB students		-0.007 (0.008)		-0.005 (0.008)
3 + ESB students		0.008 (0.009)		0.011 (0.009)
Share of refugees	0.041 (0.083)	0.040 (0.083)	-0.014* (0.077)	0.043 (0.065)
Share of other immigrants	0.014 (0.066)	0.011 (0.066)	0.044 (0.065)	0.038 (0.062)
Constant	-0.182*** (0.017)	-0.180*** (0.017)	-0.282*** (0.016)	-0.280*** (0.016)
Observations	250,975	250,975	243,031	243,031
R-squared	0.169	0.169	0.160	0.160

All explanatory variables are measured at 5th grade level. All models include controls for the share of other immigrants and the share of refugees. Other controls are gender, maternal education level, paternal education level, parental income, live with both parents, number of siblings, birth order and number of students enrolled in the grade. All models have school and year fixed effects. Robust standard errors clustered at the school-year level in parentheses

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

by native students' English language proficiency at grade 5. Again, if initial proficiency has a role to play, we may expect larger estimated effects of exposure to ESB peers.

There are two broad categories of threats to identification. The first is any failure of our key assumption that ESB student exposure is as good as random holding constant immigrant shares. This would include any form of selective ESB enrolment patterns with respect to native students or any differential mobility patterns of native students with respect to ESB peers. As discussed above, the school system and institutional background of Norway reduce many of these concerns. Nonetheless we examine the potential for this to be a salient issue in our robustness section below.

The second category reflects issues of interpretation of our key estimates even if these assumptions hold. ESB students may differ from other immigrant students in ways that more broadly influence the educational performance of their classmates. For instance, even if their assignment to school cohorts is as good as random, we may estimate a broader ESB immigrant effect that does not (just) reflect their differential English language skills. Again, this is difficult to rule out. Our main approach is to estimate analogues of (1) for our two other test scores, mathematics and

Table 3 Heterogeneous effects of English-speaking background (ESB) peers on English test score performance at grade 8

	Boys	Girls	Low educated parents	Highly educated parents
	(1)	(2)	(3)	(4)
One ESB student	0.001 (0.010)	-0.012 (0.0092)	0.004 (0.010)	-0.014 (0.008)
Two ESB students	0.025** (0.013)	0.020* (0.012)	0.027** (0.014)	0.019* (0.011)
3 + ESB students	0.025* (0.014)	0.020 (0.014)	0.034** (0.016)	0.015 (0.012)
Constant	-0.192*** (0.024)	-0.215*** (0.023)	-0.358*** (0.025)	-0.089*** (0.022)
Observations	103,914	102,019	79,889	126,038
R-squared	0.135	0.131	0.067	0.099

All explanatory variables are measured at 5th grade level. All models include controls for the share of other immigrants and the share of refugees. Other controls are gender, maternal education level, paternal education level, parental income, family structure and grade enrolment. All models have school fixed effects. Robust standard errors clustered at the school-year level in parentheses

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Norwegian. If, for instance, ESB students are simply higher quality peers we should observe spillovers across other domains of educational attainment. Beyond this, we explore a range of measurement issues related to the classification of immigrant children, particularly with respect to ESB. While this is important issue to explore, doing so serves to clarify that it is the English language skills of immigrants that are likely driving our results.

4 Results

Table 1 presents initial estimates of (1) for the English test score performance of Norwegian students. All estimates include school fixed effects such that they provide the effect of within school variation in ESB students on the English test scores of Norwegian students. We initially report estimates for the share of ESB students in the grade (column I). The resultant estimate is positive but not statistically significant. Column II instead reports estimates for the number of ESB students in the grade. This has a positive, weakly significant, effect on Norwegian students' English test score performance. A one ESB student increase leads to an average 0.004 of a standard deviation increase in English test score performance. This is small but potentially non-trivial when one remembers that we are estimating average effects over whole school grades.

Table 4 The effect of ESB peers on (I) school mobility (II) English test score performance controlling for grade 5 English test scores and (III) English test score performance with sibling fixed effects

	Attended neighbourhood middle school	Control for past English performance	Sibling FE
	(1)	(2)	(3)
One ESB student	-0.003 (0.004)	0.000 (0.007)	0.015 (0.013)
Two ESB students	-0.007* (0.004)	0.014 (0.009)	(0.022) (0.017)
3 + ESB students	-0.003 (0.004)	0.022** (0.010)	0.043** (0.019)
Fifth grade English performance		0.478*** (0.002)	
Constant	0.828*** (0.009)	0.670*** (0.002)	0.557*** (0.068)
Observations	204,960	204,960	66,461
R-squared	0.127	0.528	0.709

Column (1) reports the effects of ESB students on the likelihood of attending the neighbourhood middle school. Column (2) reports the effects of ESB students on English Performance controlling for 5th Grade Performance. Column (3) reports estimates using sibling fixed effects along with school fixed effects. All explanatory variables are measured at 5th grade level. All models include controls for the share of other immigrants and the share of refugees. Other controls are gender, maternal education level, paternal education level, parental income, family structure and grade enrolment. All models have school fixed effects. Robust standard errors clustered at the school-year level in parentheses

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

One might, however, expect there to be non-linear effects of ESB peers and an advantage of our data is to be able to provide estimates of finer cuts. Column (III) provides estimates where we disaggregate ESB peer exposure to one ESB student, two ESB students, and three or more ESB students. These estimates highlight two points. One, we retrieve substantially larger effects of having at least two ESB students in the school grade on English language performance.⁴ Two, it suggests that the small weakly significant effects reported in column II may reflect the combination of a lack of any effect for having just one ESB, and larger effects for more substantial numbers of ESB peers. This, we argue, is a strength of our register data, the ability to retrieve precise estimates of this form. As a

⁴ We stress again that we do not directly observe peer interactions, hence one could consider these estimates and all that follow as lower bounds of the true effect of actual exposure to ESB peers on English language attainment.

result, in much of our following estimates for English language performance we focus on specification (III).

One issue is that these ESB effects may simply reflect that these students are in general high ability peers, even while Fig. 2 suggests that they differ primarily in their English language skills. At the same time, these effects on English could reflect other factors. For instance, these students could locate in schools where overall test score performance is generally improving (or avoid schools where they are worsening), or their parents may locate in residential areas which are changing in terms of their demographics in ways that change school test score outcomes. In all cases, this would lead us to expect that changes in the composition of ESB peers should lead to general increases in test score performance across subjects. As an initial step to exploring this Table 2 reports analogous estimates to those reported in Table 1, but for mathematics and Norwegian test performance.⁵

For mathematics (I and II) this reveals no effect of having ESB students as peers. This is true for both the number of ESB students (I) and when we split this according to different numbers of ESB students (II). Note that one alternative reason why we might detect effects is if improvements in English language skills spillover to improvements in mathematics performance. While we cannot explore this, we note again that mathematics is taught wholly in Norwegian making this less likely. We demonstrate similar zero effects for Norwegian language performance (III and IV).

The key point from Tables 1 and 2 is that ESB student peers lead to higher English language performance amongst Norwegian students, and that this effect on learning is domain specific. We detect no effects on mathematics or Norwegian language performance.

One natural question is whether these average effects hide substantial heterogeneity according to student type. Table 3 provides estimates where we split the samples according to demographic differences that are often viewed as conditioning learning outcomes. The improvements to English language skills are quite similar across girls and boys, but there is an indication that exposure to English language speakers is more beneficial for Norwegian children with lower educated parents. This would fit with these children being less likely to be exposed to English in the home.⁶

⁵ Note the lower number for the English results reflect the lack of one cohort due to the cancellation of English tests in one year. Estimating our math or Norwegian models for this smaller sample does not change these results, neither does using the 5th grade math or Norwegian exam information to generate our class composition measures and estimating the English models on the resultant larger sample.

⁶ An important question is whether these effects persist. While we can track students through to their English test scores in 10th grade, we observe fewer cohorts from grade 5 through to grade 10, and only approximately one third of students are randomly assigned to take the English exam (Bensnes 2020; Landaud et al 2022). Table 4 reports estimates of our main specification but where the dependent variable is grade 10 English score. While not statistically significant, the pattern of results for grade 10 English exam performance follows those reported earlier and are of a similar magnitude.

4.1 Robustness

We next seek to explore a range of issues regarding the interpretation of our main results.

The first set of issues regard mobility patterns. For example, do our results simply reflect the fact that Norwegian students who are more motivated or able are more likely to attend school with ESB students? Note again that the pattern of our results means that this would have to solely reflect English ability and/or interest, rather than for instance wider academic ability, and our main estimation approach would require this mobility to occur within schools over time. Nonetheless we consider a number of related issues below. These are: (a) mobility responses of native students to exposure to ESB students; (b) sorting of Norwegian students with higher English language proficiency into school-cohorts with greater ESB numbers; (c) sorting of Norwegian students into schools by family and socio-economic characteristics; (d) the concentration of ESB students in particular schools; and (e) growing ‘internalisation’ of particular neighbourhoods. These are discussed below in turn.

Does exposure to ESB students in grade 5 predict school mobility afterwards? Specifically, we explore whether exposure to ESB peers in grade 5 is predictive of attending a school other than the ‘standard’ middle school that would be expected from a given primary school. We observe this as a given middle school typically draws on a range of feeder primary schools. We report estimates in the first column of Table 4 where the dependent variable is attending the neighbourhood middle school. While there is one weakly statistically significant estimate, we stress the small size of this effect. Being exposed to 2 ESB students, increases the probability

Table 5 The effect of English-speaking background (ESB) peers and English test score performance at grade 8, excluding specific countries of origin

Excluding:	Ireland	UK	US	Australia	New Zealand
	(1)	(2)	(3)	(4)	(5)
One ESB student	−0.005 (0.007)	−0.006 (0.007)	−0.006 (0.007)	−0.005 (0.007)	−0.005 (0.007)
Two ESB students	0.023** (0.009)	0.021** (0.009)	0.020** (0.009)	0.022** (0.009)	0.023** (0.009)
3 + ESB students	0.023** (0.010)	0.020* (0.010)	0.020** (0.010)	0.023** (0.010)	0.023** (0.010)
Constant	−0.210*** (0.017)	−0.215*** (0.017)	−0.212*** (0.018)	−0.211*** (0.0177)	−0.211*** (0.0177)
Observations	206,003	204,241	204,008	205,945	206,018
R-squared	0.120	0.119	0.119	0.120	0.120

All explanatory variables are measured at 5th grade level. All models include controls for the share of other immigrants and the share of refugees. Other controls are gender, maternal education level, paternal education level, parental income, family structure and grade enrolment. All models have school fixed effects. Robust standard errors clustered at the school-year level in parentheses

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 6 Alternative measures of ESB peers and English performance at grade 8

Immigrant grade 5 English score was:	Above average (1)	One St.Dev above average (2)	Two St.Devs above average (3)
One ESB	0.008 (0.007)	-0.003 (0.008)	0.012 (0.008)
Two ESB	-0.002 (0.009)	0.003 (0.012)	0.017 (0.017)
3 + ESB students	0.032*** (0.011)	0.035** (0.016)	0.040* (0.022)
Constant	-0.209*** (0.018)	-0.209*** (0.018)	-0.209*** (0.018)
Observations	206,032	206,032	206,032
R-squared	0.119	0.119	0.120

All explanatory variables are measured at 5th grade level. This table reports results using different methods to calculate the number of ESB immigrants in the class based on their grade 5 English scores. For example, the first column provides the effect of exposure to 1, 2 and 3 or more immigrant students with above-average English test scores in grade 5. All models include controls for the share of other immigrants and the share of refugees. Other controls are gender, maternal education level, paternal education level, parental income, family structure and grade enrolment. All models have school fixed effects. Robust standard errors clustered at the school-year level in parentheses

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

of going to a different school by approximately 3 quarters of one percentage point, while there is no statistically significant effect for more than 2 ESB students.

An additional concern is that there is selection of ESB students into school-cohorts according to the school-cohort variation in the underlying English language proficiency of Norwegian students. This is difficult to address. Our approach is to estimate a variant of (1) where we include the fifth-grade English performance of native students. Doing so holds constant English language attainment at the grade 5 level. This provides a value-added estimate of the effect of exposure to ESB students on native English test score performance (Todd and Wolpin 2003) and is similar to the approach used by the Norwegian Directorate of Education to measure school performance (Utdanningsdirektoratet 2018). It does, however, change the interpretation of our estimates of interest to being the changes in English test score performance due to additional ESB inputs in the period between grade 5 and grade 8. These estimates are reported in the second column of Table 4. Naturally, prior test score performance is a strong predictor of later English attainment. The pattern of effects of exposure remains consistent with earlier results but are somewhat reduced and the effect of exposure to two or more ESB students remains positive but is no longer statistically significant. This may reflect that some of the effects of exposure to ESB students are already captured in grade 5 English test scores. Nevertheless,

Table 7 The effect of same gender ESB peers on English performance at grade 8

	Girls	Boys
	(1)	(2)
Opposite Gender ESB (#)	0.004 (0.04)	-0.005 (0.005)
One Same Gender ESB	-0.007 (0.008)	0.004 (0.009)
2 Same Gender ESB	0.021* (0.013)	0.029** (0.014)
3 + Same Gender ESB	0.016 (0.019)	0.044** (0.019)
Constant	-0.216*** (0.023)	-0.190*** (0.024)
Observations	102,019	103,914
R-squared	0.131	0.135

All explanatory variables are measured at 5th grade level. All models include controls for the share of other immigrants and the share of refugees. Other controls are gender, maternal education level, paternal education level, parental income, family structure and grade enrolment. All models have school fixed effects. Robust standard errors clustered at the school-year level in parentheses

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 8 The effect of exposure to ESB peers on grade 8 English performance according to quartile of own English test score performance at grade 5

	Quartile 1	Quartile 2	Quartile 3	Quartile 4
One ESB student	0.003 (0.012)	-0.016 (0.011)	0.018* (0.0106)	-0.011 (0.010)
Two ESB students	0.011 (0.013)	0.008 (0.014)	0.030** (0.014)	-0.001 (0.012)
3 + ESB students	0.004 (0.016)	0.014 (0.016)	0.058*** (0.016)	0.001 (0.01)
Constant	-0.955*** (0.026)	-0.322*** (0.028)	0.122*** (0.027)	0.746*** (0.024)
Observations	54,154	50,323	52,545	48,525
R-squared	0.117	0.120	0.121	0.134

All explanatory variables are measured at 5th grade level. The estimates reported split native students according to their English test score performance at grade 5. All models include controls for the share of other immigrants and the share of refugees. Other controls are gender, maternal education level, paternal education level, parental income, family structure and grade enrolment. All models have school fixed effects. Robust standard errors clustered at the school-year level in parentheses

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

we view these results as suggesting our main estimates are not driven by this type of school-cohort sorting.

A related concern is that our estimates reflect school sorting of Norwegian students in terms of family characteristics. We address this in the final column of Table 4 by including sibling fixed effects. This model is estimated only for children where we observe at least a pair of siblings, but note our main results are unaffected if we estimate our earlier models on this restricted sample. This approach, when combined with school fixed effects, exploits variation within siblings that attend the same school but in different cohorts. For example, families that move house such that siblings attend different schools, or singleton children, do not contribute to the estimates of our parameters of interest. Naturally, this leads to a substantial increase in the standard errors. Despite this, it is notable that these estimates are positive and of substantial magnitude for any level of exposure. For example, exposure to 3 or more ESB students leads to 0.043 of a standard deviation increase in English attainment of Norwegian students.

A more general concern reflects the concentration of ESB students in specific schools or locations, and that these schools may vary in important unobserved ways that are time-varying. We examine two key issues. One, while we have already excluded the small number of international schools that have very high concentrations of English-speaking students, we further examine whether our results reflect remaining schools with high shares of ESB students. In Table 5 we report estimates where we sequentially remove school-year observations with more than 10 ESB students (I) and more than 5 ESB students (II). Our main results are essentially unaffected by this. Related to this, ESB students are concentrated in the major cities. In Table 6 we investigate excluding, in turn, the 4 biggest cities in Norway where this concentration is most acute, Oslo, Bergen, Trondheim and Stavanger. Our main estimates are unchanged by this. In unreported estimates, we also simply excluded all major cities. While this hurts precision, the estimated coefficients of interest are qualitatively unchanged.

Perhaps schools in areas with increasing amounts of economic immigrants are becoming in general more international. While controlling for other immigrant shares is helpful, we can go further by re-estimating our main models of interest but instead including indicators for an alternative group of economic migrant children. Specifically, we replace our indicators of ESB peers with students from a romance language background.⁷ These students are slightly less prevalent than their ESB counterpart. For instance, 1.47% of students are from these backgrounds compared to 2.26% for ESB immigrants, and there are on average 2.13 romance language students per school-year compared to 2.80 for ESB students. We report these estimates in appendix Table 7. These suggest no effect of romance language students on native student English attainment. There are two implications of these results. First, our main results do not simply reflect growing internalization of a given area/school. Second, it could be the case that Norwegian and immigrant students with weaker Norwegian skills may be more likely to communicate in English which could

⁷ Specifically, all children from France, Italy, Spain, Portugal, Central and South America.

represent a common language. If this does occur, this communication between non-native English speakers does not appear to change Norwegians' English language skills.

Another set of concerns relate to the definition of ESB students. While the patterns in Fig. 2 strongly suggest that we are identifying immigrant students from an ESB background we recognise that we do not directly observe language background of children and that alternative definitions are possible. As a first step, we simply re-estimate our preferred specification changing the distributional cut we use to define ESB. Table 8 presents estimates where instead of classifying ESB as 60% of adults speaking English, we alternately use 50% and 70%. In practice there are few countries around these distributional cuts. For example, increasing to 70% removes only Montserrat and Belize. Doing so does not change our main estimates. Reducing to 50% is more consequential in terms of student numbers characterised as ESB, as Canada (and Surinam) is now included.⁸ The estimates for 2 ESB students becomes slightly smaller, however the effect of three or more students is unchanged.

A related issue is that while we observe ESB students from a variety of countries of origin, they are (not surprisingly) dominated by a handful of countries. As a result, the effects we observe could reflect idiosyncratic factors with respect to particular groups of immigrant children. Our approach to examining this is to provide estimates of our main model of English achievement where we stepwise remove each of the ESB countries with the largest number of students. These estimates are reported in Table 5. These demonstrate essentially no effect of the removal of any of these countries. This suggests that our effects do not, for instance, reflect the effects of particular ESB immigrant groups, and instead provide the general effect of exposure to ESB peers.

An alternative approach to classifying ESB is to, rather than rely on country of origin, use 5th grade scores in English as an indicator of the language background of immigrant children. Table 6 presents a range of estimates of the effect of ESB peers on native English performance at grade 8 where we use alternative measures derived from immigrant grade 5 tests scores. These are the number of immigrant peers who scored more than average in grade 5 English (column 1), more than one standard deviation above the average (column 2) or more than 2 standard deviations above the average (column 3). The patterns fit with earlier estimates, in all cases exposure to more ESB peers leads to higher native English performance. Moreover, these are increasing in magnitude as the English proficiency (test scores) of ESB peers increases.

4.2 Exploring mechanisms

To this point, we have demonstrated a robust association between exposure to ESB peers and native students' English language attainment. These raise questions

⁸ Our data source had Canada at a seemingly relatively low level of English use. This seems to reflect the particular question in the 2021 census regarding English as the first language. Nonetheless including Canada as an ESB country (such as in the 50% cut) does not meaningfully change our results.

regarding potential mechanisms. While we cannot be exhaustive, we explore two potential channels.

A first question is what do these estimates capture? Do they reflect exposure to additional English language speakers in English classes, or from increased interaction with English language speakers more generally? One way to explore to this is to look at same gender spillovers. The intuition is that for this age group play outside of class is likely still gendered such that same-gender interaction is more frequent. If language spillovers occur outside of the classroom, then one might expect them to be stronger within gender pairs (ESB boys and Native boys, ESB girls and Native girls). In contrast, one might expect any within classroom effects to be more general and have less of a gendered dimension.

To explore this point we re-estimate our main models but split ESB peers according to their gender. We then estimate separate models for native boys and girls. This is data demanding and leads us to estimate our models in a slightly more parsimonious manner focusing on own-gender peers. For example, for *native female* students, we estimate exposure to 1, 2 or 3 or more *ESB female* students. We then include a simple control for the number of male ESB students.

Table 7 reports the resultant estimates. These reveal that the language spillovers appear to operate solely through same-gender groups. There are no cross-gender spillovers, ESB boys do not affect native girls, nor do ESB girls affect native boys. Instead, spillovers occur strictly within gender and are especially strong for boys. We estimate an effect of as high as a 0.044 of a standard deviation increase in average English scores for native boys with 3 or more male ESB peers. These results are suggestive of spillovers occurring outside of the classroom.

A second question is who benefits from exposure to native language speakers? If improved language skills do occur through social interactions outside of the classroom this could be contingent on the language skills of the native student. For instance, it seems unlikely that these types of interactions would improve the skills for those native students initially at a very low level of proficiency. For these students, communication may simply be too difficult or at too low a level.

We explore this by estimating models split by the initial English proficiency of native students at grade 5. Specifically, we estimate separate models by quartiles of grade 5 English language scores. These results are reported in Table 8. These demonstrate the spillovers are concentrated amongst students in the 3rd quartile of English language attainment. This is suggestive of benefits that come from the ability to communicate with English speakers.⁹ A related issue, and a policy concern, is that exposure to native language speakers may discourage those with poor initial English language skills. Table 8 suggests that this does not occur as there are no effects for native students with low levels of initial English proficiency.

⁹ The lack of an effect for the highest performing Norwegian students could reflect ceiling effects in the tests or non-linearities in the peer effects. In unreported estimates, we estimated analogous models to Table 9 but with sextiles instead. Again, we found positive and statistically significant effects for native students who had above average English skills (sextile 4 and 5) but no effect for the top group (sextile 6). While not definitive, this suggests that it may be ceiling effects in the English language test.

5 Conclusion

The role of interactions with, and exposure to, proficient speakers is thought to have an important role in foreign language development. There exists little causal evidence on this, especially for younger children. We focus on the exposure of Norwegian students to English-speaking background (ESB) immigrant peers. While there exists a broader body of work looking at how immigrant peers influence educational attainment of native students, our research departs from this literature by focusing on the effects on the development of English language skills. Our setting is advantageous due to a mix of a universal emphasis on English language acquisition in school and an increase in immigration (from a low base) which included many immigrants (and their children) who have English as their mother tongue.

We demonstrate positive spillovers on native Norwegian students' English skills from exposure to ESB peers. Our estimates suggest effects in the order of a 0.02 of a standard deviation increase in English language test scores at grade 8 for a Native student exposed to 2 or more ESB students in upper primary school. These children are older than is typically thought as the critical period for acquiring languages. This could mean that our estimates are smaller than would be evident for younger children. We see this is an avenue for future research.

A limitation is that we do not observe student interactions and hence our estimates may understate the effects of exposure to native speakers. Our exploration of mechanisms also aims to 'narrow-in' on likely interactions. For example, spillover effects appear wholly concentrated within same-sex pairings, with especially large effects evident between boys. They are also only present for those native students who already have better than average English language skills.

Our view is that our results have likely broad applicability to a range of settings where students learn foreign languages and are exposed to peers who are proficient in this language. More generally, it provides suggestive evidence that there may exist a range of wider effects from immigrant school peers in domains that are typically hard to measure and not a focus of the now very large existing literature in this area. Our view is that this literature would be well served to explore a broader set of outcomes.

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Declarations

Conflict of interest The authors declare they have no conflict of interest.

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