



Refugees and the educational attainment of natives: Evidence from Norway

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ABSTRACT

Increases in immigration raise a range of challenges for schools. Existing research primarily investigates the impact of immigrants on native test scores and demonstrates mixed results. Using Norwegian register data and narrow within-school, within-family comparisons, we demonstrate negative effects of refugees on native math performance, and no effect on English or Norwegian performance. These effects are concentrated amongst groups of refugee children who themselves face the greatest educational difficulties, while other groups have zero to positive effects. Our results suggest a need for targeted policy aimed at helping immigrant children most likely to face educational difficulties, while suggesting caution in generalising results from specific refugee episodes.

1. Introduction

There has been a recent rapid increase in immigration into Europe, including refugees and asylum seekers. At the height of the immigration wave in the mid-2010s approximately one million refugees and asylum seekers came to Europe. Norway has experienced a particularly dramatic increase in immigration coming from an historically low base. While only 3.5 percent of the total population of Norway in 1990 were immigrants (and 5.3% in 2000), by the beginning of 2022 they accounted for approximately 15% of the total population. In recent years, the share of refugee arrivals in total immigration has been as high as 30% (2016). As we discuss later this share becomes substantially higher if families of refugees are also included.

Rapid increases in the immigrant population have the potential to generate a range of social challenges and one particular focal point is education and school systems. Reflecting this, there is a growing body of literature in the US and Europe that examines the effect of immigrants in schools. This literature focuses primarily on the impact of immigrants on the educational attainment of native students. This research reports mixed evidence. For instance, [Ballatore, Fort and Ichino \(2018\)](#), [Frattini and Meschi \(2019\)](#) demonstrate marked negative effects of increases in exposure to immigrant classmates on native Italian students in schools and in vocational training, respectively. Both present evidence that these negative effects are concentrated amongst low income students. [Tonello \(2016\)](#), also for Italy, demonstrates zero to small negative effects in Italian Junior High Schools, with some evidence that these become larger (more negative) with higher immigrant shares. Large

negative effects have also been found for Denmark ([Jensen & Rasmussen, 2011](#)). While earlier Norwegian evidence suggests that non-European immigrant peers lead to higher native dropout from secondary schooling ([Hardoy & Schone, 2013](#)), later evidence suggests no effect ([Hardoy et al, 2018](#)). Evidence from Austria suggests no-effect on native grade retention and track choice ([Schneeweis, 2015](#)). Other earlier cross-European evidence suggests negative, but small, effects ([Brunello & Rocco, 2013](#)). Evidence from the Netherlands suggest a worsening of the learning environment associated with greater immigration shares but no effect on test scores except for recent arrivals ([Ohinata & Van Ours, 2013](#); [Bossavie, 2020](#)). At the same time, existing US evidence has at times demonstrated positive effects of immigration on native educational attainment ([Hunt, 2017](#); [Figlio, Giuliano, Marchingiglio, Özek & Sapienza, 2021](#)), or negative effects only when immigrant students have limited English aptitude ([Diette and Uwaifo Oyelere, 2014](#)). Corresponding UK evidence demonstrates no causal effect of non-English speakers on the school performance of native students ([Geay, McNally & Telhaj., 2013](#)).

One issue with interpreting and generalising these findings is the sheer diversity of immigrant groups both within and across countries. While the papers summarised above explore, and demonstrate a range of heterogeneous effects across immigrants groups, one fundamental issue is that economic immigrants and refugees are likely to differ in a range of ways likely to influence own educational performance, their interactions with native students within the school and classroom, and ultimately any effect on native educational attainment. In the paper most closely related to ours, [Figlio and Özek \(2019\)](#) examine how a dramatic increase

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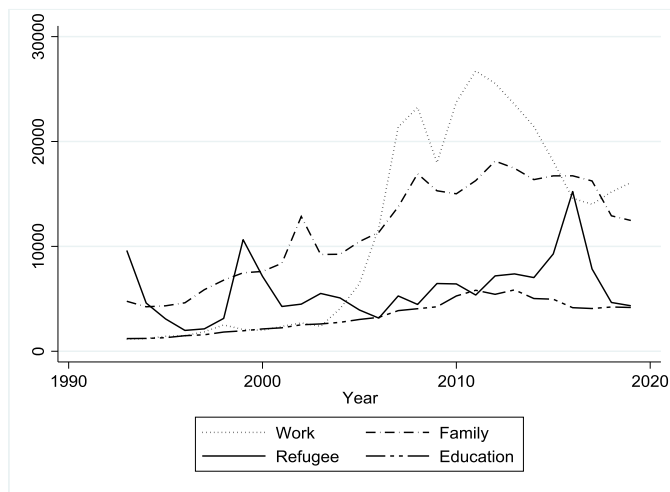


Fig. 1. Immigration flows into Norway 1995–2019.

Notes: Fig. 1 provides flows of immigrants into Norway by reason for immigration. Source Statistics Norway (2021), Immigrants by Reasons for Immigration.

in the exposure of native students in Florida to a particular immigrant group, Haitian refugees following the earthquake in 2010, influenced native tests scores. They highlight the fact that the effect of refugees on incumbent students is likely to be substantially different to that of other immigrants. This reflects both the nature of refugee migration which might involve sudden, and highly disruptive, movements against the background of conflict, natural disasters or other shocks, and the fact that these immigrants often have characteristics that make them very dissimilar to the native population. Despite this, they demonstrate no adverse effect of this inflow of refugees on native student educational performance. Other recent US research demonstrates zero to positive effects of refugee exposure on native educational performance. Morales, 2021 utilises within school across grade variation in refugee shares for an urban district in the state of Georgia. They demonstrate positive effects of refugee exposure on math, and zero average effects on English in the state of Georgia. Van der Werf (2021) utilises geographic variation in the resettlement patterns of South-East Asian refugees in the US following the end of the Vietnam War. They demonstrate zero to small positive effects of US students' exposure to these refugees.

We return to this issue using administrative data for Norway. Our main approach focuses on the effect of refugee class composition on the test scores of Norwegian primary school students. Unlike, for instance Figlio and Özek (2019), our focus is on children from refugee families who arrived in the country across different times and from different regions. These families, as we demonstrate, are distributed across the country and across schools. This focus has both advantages and disadvantages. For instance, when combined with our ability to distinguish between immigrant groups, this allows us to explore a range of heterogeneity in the effects of refugee spillovers. As discussed below, these are important, and point to large variations in spillovers across different refugee groups. Beyond this, we explore mechanisms in ways not previously possible in the literature. These, we argue, are important contributions as appropriate policy responses, such as targeted school interventions, may differ across immigrant types and also according to the source of spillovers. Likewise, the educational impacts of changes in immigration policy, or changes in immigration flows, may also depend on these.

At the same time, we recognise the potential for disadvantages related to this approach in terms of challenges to causal identification. These include non-random location and schooling decisions of immigrants, and potential native mobility responses to immigration. Our main approach is to adopt very narrow points of comparison such as

exploiting within sibling, and within school, variation in exposure to immigrant classmates, and in doing so the register data we use allows for precise estimates of the parameters of interest. Beyond this, and as we describe further in the paper, Norway provides an ideal setting to examine this issue due to a range of institutional features which first leads to refugee families being spread geographically across the country and across schools, and also a range of factors that reduce subsequent mobility of both refugees and Norwegian families. Again, this is discussed in more detail later but these include a range of factors such as the central government allocation of refugees to municipalities across Norway which limits (at least initial) endogenous sorting, an absence of school choice which when combined with high home ownership amongst Norwegian families makes school changes highly costly and infrequent, a negligible private school sector, strict social progression within schools, through to factors such as that refugees will typically learn a local dialect of the Norwegian language which may make relocation more difficult. Finally, there are range of other institutional features that make within school grade sorting less likely to be a source of bias. Most notably, there is no ability streaming in Norwegian primary schools and sorting classes on the basis of gender, ability or ethnicity is explicitly not allowed in Norway (Ministry of Education and Research, 2021) Together this allows us to retrieve estimates of the effect of refugee students on native educational outcomes that we argue can be interpreted causally. Nonetheless, we explore in detail the potential for factors such as non-random mobility to influence our results.

In summary, our estimates show marked negative impacts of refugee shares on native students' mathematics performance in primary school, but no overall effect on their Norwegian or English language performance. When exposed to higher refugee shares than their siblings who attended the same school, children perform markedly worse on average in mathematics. As we discuss later, the pattern across these three subject areas likely reflect the key features of how these subjects, and how refugees in these classes, are taught. Of particular note, refugee students may be often be taught separately or receive different material in Norwegian classes fundamentally altering their interactions with native students. While, math is taught in Norwegian with no additional resources for refugee, or other children, with language difficulties. We demonstrate no effect of non-refugee immigrant classmates across any of these test score outcomes. Our main results are robust to a range of additional potential confounders and in particular potential mobility responses to changes in refugee shares appear unlikely to be driving our results.

An advantage of our setting is the ability to explore the heterogeneity of refugee children. Most notably, we demonstrate marked differences in the negative effects on mathematics according to the region of origin of refugees. While we demonstrate some role for observable characteristics, these patterns survive attempts to control for different characteristics of these refugee students. This is important when comparing our results to the recent US literature on the impact of refugees (particularly Figlio & Özek (2019) and Van der Werf (2021)). Like these papers, we demonstrate (imprecise) positive effects on math from exposure to refugee from particular regions of origin (in our case Asia), but at the same time we show large negative effects from other groups. These broadly follow patterns of underperformance across these different refugee groups. We do not suggest that this reconciles our evidence with this US evidence, but it does highlight the need to be careful with the extrapolation of findings from specific refugee events. More generally, our results suggest the need for policy targeted at specific immigrant groups within schools, who themselves face difficulties, and may generate negative spillovers on other students.

2. Background, institutional details and data

2.1. Background and institutional details

Immigration to Norway has increased considerably over the last

decades. Prior to the 1990s, 2.1. 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 over the past 3 decades (for example increasing from 2,400 entrants in 1993 to 26,700 in 2011, but down to 16,000 in 2019.), and with changes in the countries of origin. This includes a shift away from immigrants from western European and Nordic countries to more from eastern Europe. While Norway has a history of taking refugees, the number of refugees and their share of immigration has increased substantially over time such that by 2018 refugees made up 12.5% of the immigrant inflow (and as high as 30 % in 2016 with the large inflow of Syrian refugees). Fig. 1 provides an illustration of immigrant inflows by type over time.

Partly reflecting patterns of conflict abroad, these refugee inflows experience peaks. For instance, 15,231 individuals came to Norway as refugees in 2016 (along with around 16,000 coming for family reunion). Likewise, there have been substantial variations in regions of origin overtime. Most notably there have been spikes in refugees following the Balkan conflict in the 1990s; an Iraqi, Afghani and Somalian refugee wave in the early 2000s, and more recently the spike in refugees from Syria in 2015 to 2017. Refugees to Norway are on average younger than the Norwegian population in general and this leads to refugees being over-represented among school age children.

Norway, like other Scandinavian countries, exercises a range of controls over the settlement patterns of refugees within the country. The legal framework regulating the treatment of refugees, asylum seekers and family reunions is decided at the national level. The UDI (Norwegian Directorate of Immigration) processes applications for protection, family reunion and residence. When a refugee is granted residence the Directorate of Integration and Diversity (IMDi) allocates them a place to live. This occurs in two steps. In the first step the IMDi approaches municipalities to see if they are willing to accept refugees. These municipalities then indicate if and how many they are willing to take. There are financial transfers attached to accepting refugees and an expectation that it is part of the municipalities responsibilities to take refugees subject to factors such as housing capacity. Not all municipalities are necessarily approached every year as this is dependent on refugee inflow. But as an example, in 2014, all municipalities were asked to settle refugees. In the next step IMDi requests that municipalities take a specific number of refugees who have given housing needs (i.e. family size, age of children if any etc). In principal, municipalities can decline to take these refugees on the basis of specific issues such as a lack of suitable available housing (although municipalities often enter the private rental market to find housing so this is not necessarily binding in practice), or a lack of available personnel to help settle refugees, for example health workers with the right competence for refugees with specific health needs. In principle, refugees also have the right of refusal of settlement in a given municipality, but this has severe financial consequences. For instance, they lose their economic support for the 5-year settlement process (Tønnesen & Andersen, 2019).

The refugees that the government offer to municipalities are not entirely random. There are some criteria that the IMDi follows. Examples of such criteria are the capacity to settle refugees fast, labor market and educational opportunities, representativeness of the refugee pool (the same municipalities should not always receive refugees with the most demanding needs) and relatives in the same region. But importantly, municipalities cannot decide which refugees that they will take. For example, they cannot specify gender, family structure, ethnicity etc. In fact, all municipalities should settle a group of refugees that is representative of the current inflow of refugees to Norway. Later we examine whether there is evidence that refugees from similar backgrounds are more likely to be settled in the same municipalities and attend the same schools. This model of settlement has been constant since 2002, however the system prior to this was in practice very similar (Friberg & Lund, 2006).

Municipalities are funded on a per refugee basis in the order of

830,000 Norwegian Kroner (approx. \$US95,000) over the 5 year introductory program, with additional funding for the provision of adult Norwegian language training and social studies.¹ Refugees themselves are part of the Norwegian social security system and receive, for instance, free housing, access to health care and social security payments as per other Norwegians. While there is some variation in the settlement programmes, a core component is mandatory Norwegian language training. This is intensive and in the order of at least 450 learning hours. These occur in municipality run centres and will typically occur in the local dialect. Norwegian has numerous distinct dialects (along with 2 distinct written forms)², which while following the same grammatical structure, vary in their linguistic distance from each other and mutual understanding across dialects can be difficult even for Native Norwegians. This may further reduce mobility of adult refugees.

Additionally, municipalities are given extra resources through the national municipal income system, based on the number of children and how many of these are immigrants. Some of these additional resources may be given to schools, but the resource allocation model varies between municipalities and there exist no national guidelines in how these resources should be allocated. However, schools are committed to offer refugees and other immigrants Norwegian teaching and along with some teaching in their mother language.

There are no overarching rules on where municipalities provide housing for refugees, but they are given municipal housing where the location is typical a function of current capacity. In practice there is a lot of dispersion of refugee settlement both across and within municipalities, and as we demonstrate later, this manifests itself in very few schools with marked concentrations of refugee students. After several years in the first municipality, it is possible for refugees to move elsewhere, and internal migration from rural communities to larger population centers, particularly into the greater Oslo area, is common. Importantly, we can demonstrate that our main results remain if we exclude Oslo and other major cities of Norway where one might think that this endogenous sorting is most concentrated and problematic.

In our period of analysis (and since 1997) school is compulsory for children aged 6-16 in Norway. All compulsory schooling is free. Primary schools and elementary schools are run and financed by the municipalities. There were about 430 municipalities in Norway in the time period we analyse. There is no ability tracking system during compulsory schooling. In fact, it is not allowed by law to organise classes by gender, ability or ethnicity. This prohibits, for instance, the grouping of refugee children into specific classes (Ministry of Education and Research, 2021).

While a small number of municipalities have free school choice, in practice all students go to their local school with other children from their area. Schools have an obligation to take children from their local catchment area. There are also strict rules which enforce that children attend their local school. Home ownership rates in Norway are very high, and there is often a lack of a 'deep' rental market for family homes. As a result, changing between state run schools often involves buying and selling housing. The number of private primary schools is very low and in our period of analysis less than two percent of Norwegian primary school children attend private schools. Moreover, these are concentrated in the large cities.

2.2. Data

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, in 8th grade, and in 9th grade only in Norwegian and mathematics. We focus on 5th grade

¹ More detailed information is provided at <https://www.imdi.no/tilskudd/tilskudd-2020/integreringstilskudd/>

² A third set of Sami languages are not typically learnt by refugees.

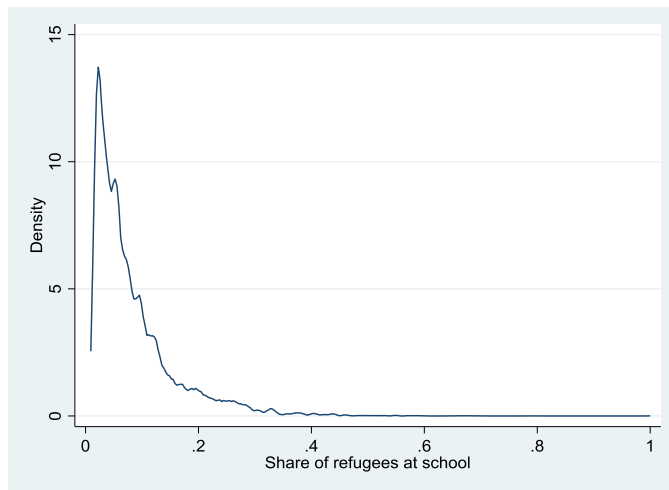


Fig. 2. The distribution of the share of refugees across schools and time, 2007–2015.

Notes: Fig. 2 reports the kernel density estimates of the distribution of shares of refugees in school-years for those observations where at least one refugee was present.

scores (corresponding roughly to age 11). This primarily reflects the fact that shortly prior to the 8th grade test students change from primary to lower secondary school. Hence, there is substantially reshuffling of classes, schools and peers.³ While we provide estimates for all three subjects, there are important differences across these that influence the interpretation of the results, and leads us to focus primarily on mathematics results. First, refugees and other students from non-Norwegian backgrounds are often taught Norwegian separately to the main class or, at the minimum, given different learning materials. This fundamentally changes in-class interaction in this subject making it difficult to interpret spillover effects which could, as an example, combine disruption effects with, in effect, smaller class sizes for Norwegian students in Norwegian language classes. There are also substantially higher patterns of test exemption for refugee children in Norwegian. This is done principally on the basis of limited knowledge of the Norwegian language. Overall patterns of exemption are reported in Table A1.

In contrast, all students are taught mathematics together, there is no ability streaming and there are no additional / different resources for children from a non-Norwegian background. An additional important point is that the language of instruction of mathematics is Norwegian, all materials are in Norwegian, as are the tests. English teaching provides an interesting case. It is possible in some cases that refugee and immigrant students may have superior English language skills to young Norwegian children due to greater home exposure to the language. A further complication with English is that due to technical issues there was no English test in 2011. Together this makes it both hard to interpret the English results, but also the lack of test in 2011 is problematic especially in our most demanding within school, within sibling models.

We standardise test scores to mean 0 and standard deviation of 1 for each year. Our population of analysis is all Norwegian fifth graders for 2007 to 2015 inclusive⁴, except for a very small number of Norwegian students who are exempted from the tests for other reasons such as special educational needs. This provides nine cohorts of between 50 000 and 60 000 students every year. We observe in which grade students are in within a given school and year, but not their class. Hence all measures of composition are at the school year level. Norway also follows a strict system of social progression within schools. There is no grade retention

and in general children (including those with special needs) are expected to follow school with children of the same age. This means that any refugee or immigrant spillover effects we estimate do not reflect the substantial age differences between native and immigrant children that can be present in many other settings. With this said, we later explore whether there is a possible role for delayed school starting to affect our results. In the analysis that uses family fixed effects, and compares siblings, we drop singleton observations leaving us with a total estimating sample of 204,058. As we demonstrate, the underlying results, without family fixed effects, are unaffected if estimated on this smaller sample.

These test score data is merged with individual information and family information from Statistics Norway. An important feature of these data is the availability of family identifiers that make it possible to identify siblings. In addition, the family information includes parental education, income amongst other standard family background variables. Information on schools such as enrolment, school type and other characteristics of the schools, are drawn from an administrative system (Grunnskolen informasjonssystem, GSI). This information is collected annually. In addition, we observe a range of information regarding students from an immigrant background. Of importance is the information on reasons for immigration which we use to assign children's immigration type. We observe if an immigrant came to Norway as a refugee, asylum seeker, for family reunion, education or for work. Within the family there will be a focal individual. We exploit this information on parental immigrant status to identify children who are refugees or from refugee backgrounds. 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.⁵ This aims to capture, for instance, the 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. This leaves another category of other immigrant children as those who have at least one parent born overseas who originally came to Norway for work or education. This covers a heterogeneous group of, essentially, economic immigrants from a variety of countries, but for instance approximately 20% of economic immigrants are from Denmark and Sweden, with substantial shares from the UK, US and approximately 5% from Poland.

Appendix Table A2 provides descriptive statistics on the key variables in our analyses. Immigrants in general gain lower test scores than Native students, but this is particularly marked for children from a refugee background. Refugee children perform markedly worse across all test scores than other immigrants and native students. On average non-immigrant Norwegian 5th grade students are in school grades where 3.8% of students are refugees and 4.2% are other immigrants. To provide more information Fig. 2 provides kernel density estimates of the distribution of the share of refugees across grade-levels. To aid presentation this is only presented for classes with at least 1 refugee in the grade-level. This excludes just under half of our school-year observations where no refugees were present. This demonstrates that while most of these remaining year observations have small numbers of refugees there do exist some higher shares. Our main estimates provide the linear effect of the share of refugees but in further estimates we investigate potential non-linearities and investigate robustness to excluding schools with high shares of refugees.

As our empirical approach relies on within school variation over time, Fig. 3 provides further illustrative evidence on the degree of within school variability in refugee shares over time. We display school minimum and maximum refugee shares at grade 5 across our period of analysis. As our data contains 2500 schools, we show this for only the 100 bottom schools who have at least one refugee in year 5 in our period

³ An additional issue is that we do not observe grade composition for grades 6 and 7.

⁴ National testing of year 5 students was first introduced in 2007.

⁵ In practice, these children (if not unaccompanied refugees) only have parents who were born overseas, i.e. they rarely have one Norwegian parent. The very small minority with Norwegian parents, including those who were adopted, are classified as Norwegian.

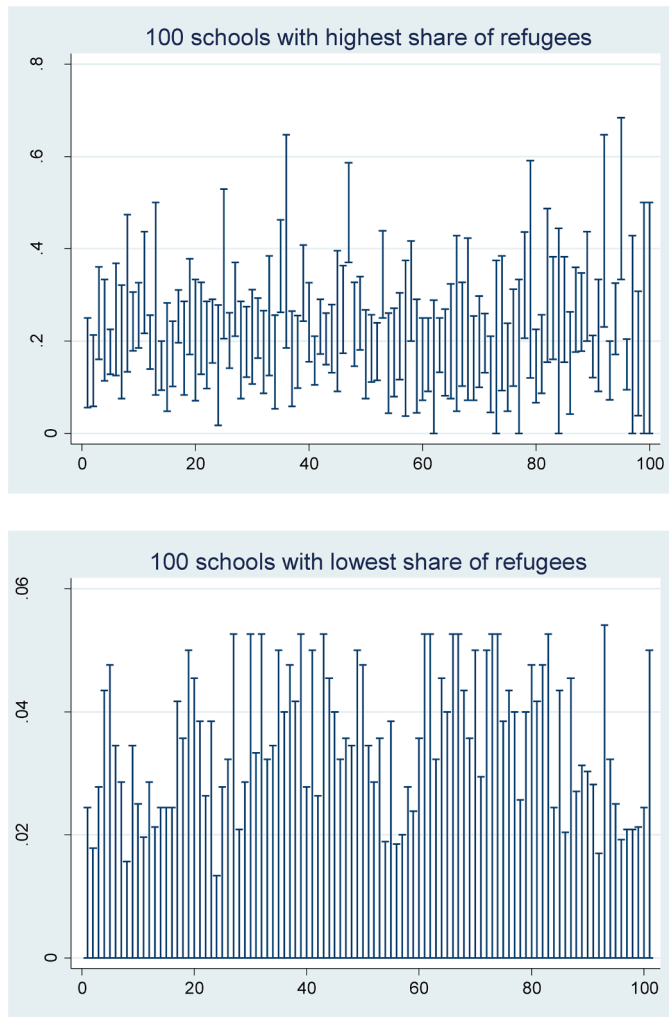


Fig. 3. Within school variation in refugee shares, 100 schools with the highest average share of refugees and the 100 schools with the lowest average share of refugees, 2007–2015.

Notes: These figures report the within school-year variation in refugee shares across the period of analysis. In the top panel this is reported for the 100 schools with the highest average share of refugees. In the bottom panel this is for the 100 schools with the lowest average share of refugees but that had refugee students at some point during the period of analysis.

of analysis, and for the top 100 schools in terms of average refugee shares in our period analysis. The key takeaway from these two panels is that there is substantial within school across time variation in refugee shares across the distribution of schools who have refugee students.

Finally, to provide further evidence on both the distribution of children from a refugee background and how this changes in our period of analysis, Fig. 4 provides two maps which report the share of children from a refugee background in year 5 as a proportion of all students in year 5 in the municipality for 2007 and 2015, respectively. Due to the geography of Norway these are, in turn, split into subsections, the Northern parts of Norway which start just north of Trondheim, and the southern part which also include Oslo, Stavanger and Bergen. These plots demonstrate 3 points. First, refugee children are spread across the whole country, with some exceptions. There are few refugee children in some remote Northern parts of Norway and central Mountain areas, all of which are in general sparsely populated. The spread reflects the policy of distributing refugees across the country, while the lack of refugees in certain very sparsely populated areas reflects limited support resources in these areas (as described above). Second, there are concentrations in some major urban areas, and this motivates us to examine the robustness

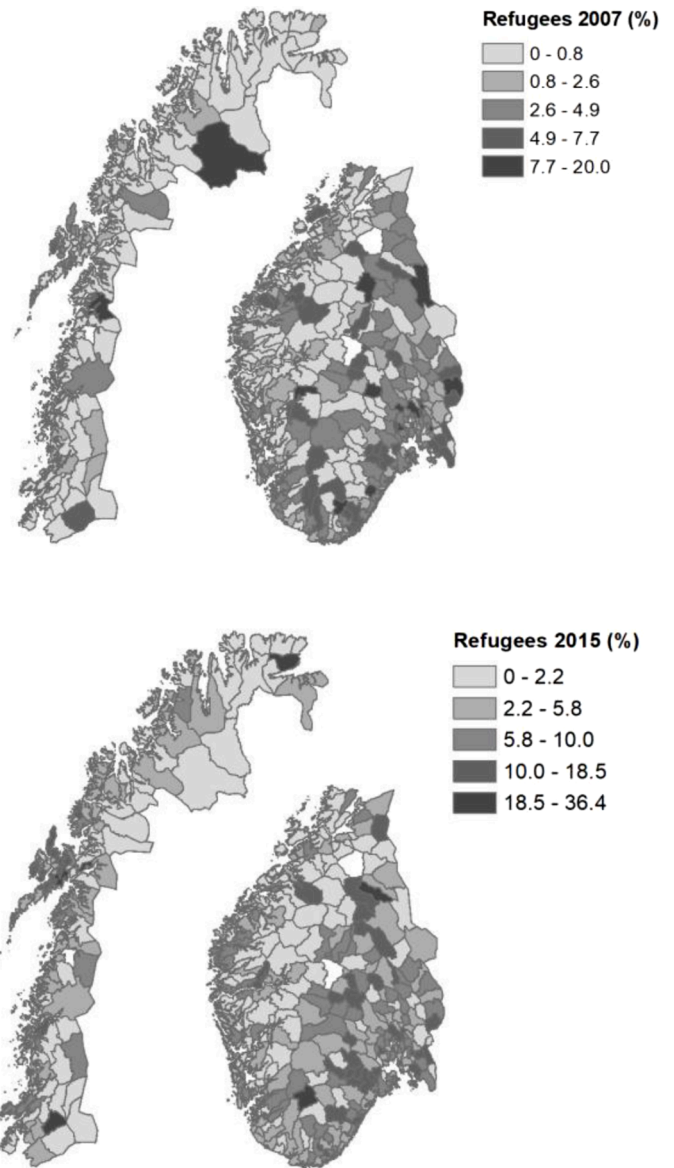


Fig. 4. Share of Refugee Students (year 5) as a proportion of all year 5 students in the municipality, Norway 2007 and 2015.

Notes: These figures report the average refugee shares in year 5 by municipality. This is displayed for the start of our period of analysis (top panel, 2007) and the end of our period of analysis (bottom panel, 2015).

of our results to excluding these areas. Finally, there is time variation in which municipalities have concentrations of refugee children, and it is not the case that areas with the highest concentration in 2007 are also always the high concentration areas in 2015.

3. Empirical strategy

Our main estimating equations are variants of the following:

$$A_{ist} = \alpha_0 + \alpha_1 Ref_{st} + \alpha_2 IMM_{st} + \beta' X_{ist} + \delta_s + \gamma_f + u_{ist} \tag{1}$$

Where A_{ist} is year 5 student achievement for individual i , in school s and at time t . Ref_{st} is the share for refugees in year 5 at schools s and time t . IMM_{st} represents the share of other immigrants in the same cohort, while X_{st} is a vector of time varying school cohort characteristics. δ_s is the school fixed effect and γ_f is the family fixed effect, while u_{ist} is an error term. We cluster standard errors at the school-year level as we observe students only in grade 5, when they take their exams. We

estimate (1) only for native students, i.e. those not classified as a refugee or other immigrant. Hence α_1 and α_2 provide estimates of the effect of refugee, and other immigrant, exposure, respectively on native test score performance.

There are a range of empirical challenges to estimating and interpreting the coefficients of interest from (1). A major issue is the potential for non-random selection of immigrants and refugees into schools and classes. There exist a range of approaches to dealing with these issues. The inclusion of school fixed effects in (1) removes time invariant differences in factors such as school quality that may influence both test score attainment and enrolment patterns of both immigrant and native children. The key parameters are then identified by variations in class composition within schools between cohorts (Hoxby 2000; Gould, Lavy & Paserman, 2009; Hanushek, Kain & Rivkin, 2009). A concern with this approach in our setting is that changes in immigrant shares at schools may lead to mobility responses from native families and students. This may lead to time variation in family background characteristics of native students that we are unable to control for. For instance, in the presence of increasing immigrant flows, so called native flight might occur where better resourced families respond to increases in immigrant concentrations in a school (and locality) via housing and school movement (Betts & Fairlie, 2003; Tumen, 2019).

Our main approach is to include family fixed effects in (1) such that our parameters of interest come from within-family, over-time, variation in immigration concentration between siblings. This, arguably, provides estimates that hold family background and inputs constant. In our most complete specification, we do this in a setting that also includes school fixed effects such that our estimates rely on between sibling within school variation in exposure to different immigration shares. This approach removes many of the obvious sources of bias in our estimation. There remains the potential for other time varying sources of bias both at a school and family level. In the robustness section we explore issues related to possible remaining time-varying sources of bias, with a particular focus on non-random mobility.

4. Results

Table 1 reports estimates of the relationship between immigrant shares and math performance of native students where we build up towards the full specification of (1). We do this to highlight a number of features of these estimates. Column (1) is a simple regression with only year dummies and no other controls. This reveals a small, and not statistically significant, negative correlation between the share of refugees

Table 1

The share of refugees, other immigrants, and the mathematics scores of native students, 5th grade 2007–2015.

	I	II	III	IV
Share of refugee immigrants in the school grade	−0.0604 (0.0594) [−0.003]	−0.169*** (0.0544) [−0.009]	−0.122* (0.0730) [−0.007]	−0.204** (0.0933) [−0.011]
Share of other immigrants in the school grade	0.652*** (0.0505)	0.498*** (0.0444)	−0.0654 (0.0580)	−0.0870 (0.0737)
Observations	383,789	383,789	383,789	204,058
R-squared	0.005	0.119	0.166	0.674
Time Fixed Effects	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
School fixed effects	No	No	Yes	Yes
Family fixed effects	No	No	No	Yes

Controls are gender, maternal education level, paternal education level, and grade enrolment. Column III additional includes birth order number of the child as a series of dummies. Robust standard errors clustered at the school-grade-year level in () parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively. [] provides the effect size of a one standard deviation increase in refugee share.

in the school-grade and native math performance. There is, however, a very large positive relationship between the share of other immigrants and native math performance of approximately 0.65 of a standard deviation. The second column introduces a number of individual and family controls. The effect of other immigrants on native math test score performance remains essentially unchanged, however the effect of the share of refugees becomes sizeable, negative and statistically significant.

As discussed earlier, two main threats to the interpretation of these results are the non-random sorting of immigrants across schools, and any non-random sorting of natives across schools as a result of changes in immigrant shares. Column III reports estimates where we include school fixed effects such that identification comes from within school changes in refugee shares over time. This has some small effect insofar as the effect of refugee shares on test scores is reduced. More noticeable is the dramatic effect on the estimates for the share of non-refugee immigrants. This fits with a view of an, on average, advantaged group who are free, at least via the housing market, to choose schools and are concentrated heavily in the larger cities where student performance is typically substantially higher in Norway. Once this is controlled for, non-refugee immigrant shares have a non-statistically significant effect on native test scores, although these estimates are routinely negative from this point on.

A remaining concern is native mobility responses. Our main approach is to include family fixed effects such that identification comes from within school and within family variation in exposure to immigrants. A side effect of this is that we drop all singleton observations (single children or children without siblings who also attended schooling and sat the 5th grade math exam within our data period). This essentially halves our sample size but also leads to concerns that any changes in coefficients we observe may simply reflect sample selection rather than estimation strategy. In Table A3 we report analogous estimates to Table 1 but where we restrict our sample to non-singleton observations throughout. The main coefficients in these re-estimated models I, II and III on this smaller sample follow the same pattern as the main results. All models with sibling fixed effects include a set of parity dummies (2nd child, 3rd child etc) in order to control for any birth order effects on test scores such as have been demonstrated in previous results for Norway (Black et al, 2005).

While there remains no effect of non-refugee immigrant shares (negative but not statistically significant), there is a substantial increase in the magnitude of the negative effect of refugee shares. This suggests some role for family sorting, but that if anything, there is mobility amongst Norwegian children with lower expected test performance as a result of increases in refugee shares. We explore this issue further later when we examine mobility responses. More generally, this provides some suggestion that a failure to control for this sorting biases the parameter of interest towards zero. This is our preferred specification and all further estimates are based on this within family within school approach unless otherwise indicated. With this said, it should be noted that school fixed effects estimates always provide results of a similar tenor, but with slightly muted effects of refugee shares.

How large are these effects, and how should we interpret them? To aid interpretation all the tables include effects sizes scaled such that they can be interpreted in terms of a one standard deviation change increase in refugee shares on test scores. As an example, the estimates of refugee shares from the sibling model demonstrates that a 1 standard deviation increase in the share of refugees (approx. 5%) reduces a given native students' performance in math by just over 1.0 % of a standard deviation. This in turn translates to an increase in the refugee share by 5 percentage points reducing the math test scores of each Norwegian student in the school grade by on average the equivalent of 3% of an expected year's progress.

Table 2 reports analogous results for Norwegian and English test scores, where for brevity we report estimates from school fixed effects, and school and sibling fixed effects models. The main take-away message is that there is no effect of refugee shares in the classroom on either

Table 2

The share of refugees, other immigrants, and the norwegian and english scores of native students, 5th Grade 2007–2015.

	Norwegian		English	
Share of refugee immigrants in the school grade	−0.0375 (0.0627) [−0.0020]	0.0138 (0.0886) [0.0007]	−0.0108 (0.0765) [−0.0006]	0.0524 (0.104) [0.0027]
Share of other immigrants in the school grade	−0.0885* (0.0513)	−0.0990 (0.0719)	0.0238 (0.0592)	0.0159 (0.0865)
Observations	374,158	195,397	337,419	153,579
R-squared	0.147	0.660	0.105	0.656
Time Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
School fixed effects	yes	Yes	Yes	Yes
Family fixed effects	No	Yes	No	Yes

Controls are gender, maternal education level, paternal education level, and grade enrolment. Columns II and IV additionally include birth order number of the child as a series of dummies. English tests were cancelled in 2013 and hence there are lower observations numbers for this outcome. Robust standard errors clustered at the school-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively. [] provides the effect size of a one standard deviation increase in refugee share.

of these outcomes. The estimated effects are neither statistically significant, nor large in terms of effect sizes. This pattern remains throughout the rest of our empirical estimates and this leads us often to only report mathematics estimates. For Norwegian this is difficult to interpret for the reasons discussed earlier, children from refugee backgrounds with poor Norwegian language skills are often taught in different groups to the main class, may receive additional help, and may follow a different curriculum to the mainstream Norwegian curriculum. This likely changes the nature of interaction, and hence spillovers, in these classes. English effects are also difficult to interpret for other reasons. For instance, some refugees (but clearly not all or the majority – see [Table A2](#)) may have superior English skills to Norwegian children, but at the same time English is typically taught at this level by teachers who have Norwegian as their native language such that refugee children with both poor English and Norwegian language skills may find these classes difficult. Refugees will typically not receive the same amount of extra resources and help in English classes as in Norwegian classes. Some students with especially serious needs may receive special education in English as well, both native students and immigrants. Analysis of this outcome is further hampered by the fact that English tests were not conducted in 2011. With this said, it is noticeable from the movement in the estimates that there is a degree of selection of non-refugee immigrants into schools that have substantially higher overall test score performance in both Norwegian and English. This fits with the concentration of economic immigrants in the major cities.

5. Robustness

While our empirical approach addresses several obvious threats to identification, there remains the potential for other sources of bias. In this section we adopt a number of approaches aimed at examining these issues.

5.1. Mobility responses

One concern with our strategy is mobility responses. A response to increasing immigrant shares at given schools may be for natives to exit. Our narrow focus on siblings who attend the same school reduces some of the concerns that this biases our main estimates of interest. Nonetheless, we explore this in a number of ways.

First, [Table 3](#) provides a range of estimates of the relationship between refugee shares at the grade 5 and a range of characteristics of the school and grade 5 students. We show these for the raw data, with school

Table 3

Changes in the shares of refugee and observable characteristics.

	Raw	Within school	Within school and family
Education – Mother	−0.131* (0.080)	0.043 (0.077)	−0.026 (0.018)
Education – Father	0.194** (0.087)	−0.015 (0.081)	0.054 (0.034)
Income – Mother (NOK)	−52,009*** (14,133)	−786.1 (12,231)	−19,556* (11,577)
Income – Father (NOK)	−334,381*** (27,611)	8,012 (21,300)	−33,244 (23,478)
School Enrolment	66.43*** (3.43)	0.899 (2.166)	−0.980 (1.867)
Share other immigrant	0.438*** (0.012)	−0.066*** (0.010)	−0.069** (0.009)
Refugees w/high educated parents [#]	0.195*** (0.006)	−0.009 (0.007)	−0.012* (0.007)
Observations	393,461	204,058	204,058

Each cell reports the relationship between the row characteristic and the share of refugees. Column (1) reports the raw correlation, column (2) reports the correlation after introducing school fixed effects, while column (3) reports correlation after additionally including family fixed effects.[#] provides the relationship between the share of refugees who have no parent with a university degree or higher, and the refugee children with at least one parent who has a degree or higher. Robust standard errors clustered at the school-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively.

fixed effects and with school and sibling effects.⁶ The school fixed effects estimates provide within school over time variation in cohort observables as refugee shares change, while adding sibling effects further examines whether these change within family, and within school. These reveal a number of things. First, and perhaps unsurprisingly, within school, within sibling differences dramatically reduces the variation between observable differences as refugee shares increase. Notably, while refugees are more likely to be present in larger schools, this difference disappears in our estimating model. Likewise, there are no differences in a parental education, or fathers' income. There is some difference in average mothers' income across refugee shares, however we stress that these are of a small economic magnitude. For example, a one standard deviation change in refugee share is associated with a 1000 NOK (\$US116) reduction in average mothers' annual income.

Another possible effect of changes in refugee composition at a school are mobility responses of other immigrants, either economic immigrants or other refugees. [Table 3](#) demonstrates some evidence of a change in other immigrant shares as refugee shares changes. Yet, again we stress the small size of this effect. A one standard deviation increase in refugee shares is associated with an approximate 0.38% percentage point reduction in other immigrant shares. This, we argue, is not of a large enough magnitude to meaningfully change class composition.. Reactions of refugees themselves are more difficult to gauge. To gain some idea of whether there are compositional changes within refugee children, we estimated the bivariate relationship between the share of refugee children who have at least one parent with a degree or higher and changes in the share of refugee children who have parents with lower level of education. The magnitude of these estimates are again small, for instance (from column 3) a one standard deviation increase in refugees with lower educated parents leads to a 0.055 percentage point reduction in refugees with highly educated parents.

Despite little evidence of mobility responses by native students in terms of observable characteristics, it is possible that there are changes in the native peer group that we do not detect as refugees shares change. If peer effects are important, then some part of the negative refugee

⁶ The estimates in [Table 3](#) are for the non-singleton data set. The raw differences reported are essentially the same in the full sample.

Table 4

The share of refugees, other immigrants, and the mathematics scores of native students, 5th grade 2007–2015. Changes in the composition of native peers

	Fathers' education	Mothers' education	Fathers' income	Mothers' income	Combined
Share of refugee immigrants in the school grade	−0.176* (0.0936)	−0.175* (0.0944)	−0.211** (0.0948)	−0.184* (0.0938)	−0.182* (0.0954)
Share of other immigrants in school grade	−0.0846 (0.0738)	−0.0817 (0.0738)	−0.0934 (0.0743)	−0.0804 (0.0740)	−0.0931 (0.0745)
Mean of Fathers' Education	0.0238** (0.0100)				0.0212* (0.0110)
Mean of Mothers Education		0.0135 (0.0096)			0.0118 (0.0108)
Mean of Fathers' Income			0.000366 (0.000369)		0.00021 (0.00034)
Mean of Mothers' Income				−0.000668 (0.000696)	−0.00129* (0.00075)
Observations	204,058	204,058	204,058	204,058	204,058
R-squared	0.673	0.673	0.673	0.673	0.673

All controls as per column (III) [Table 1](#) including time dummies, school and family fixed effects. Robust standard errors clustered at the school-grade-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively.

effect may occur through this deterioration in wider peer quality within the school that we do not control for. To investigate this, we estimated a range of models that attempt to control for wider peer composition. These include the average educational levels of non-immigrant classmates (mothers and fathers)⁷ and the average income of non-immigrant classmates' parents. [Table 4](#) reports estimates where we control for these separately, and an additional set of estimates where we control for these together. In no case does this substantially alter our main estimates of interest even as, for example, paternal education is positively related to the test score performance of native students. This suggests little role for wider changes in peer group composition as a main driver of our estimates.

While recent refugee arrivals to Norway are heavily constrained with little choice of residential location, as discussed earlier these constraints become weaker over time. This leads to a concern that our results may be affected by later patterns of non-random location choice. A particular concern is the sorting of refugees into major cities. We re-estimated our main models excluding 5 large cities representing regional centres in Norway in Norway (Oslo, Bergen, Trondheim, Stavanger and Tromsø). The resultant estimates are reported in [Table 5](#). When compared to the baseline estimates, the effects of refugee share on native test score performance is again essentially unchanged.⁸ This makes us more confident that our main estimates are not being driven by larger cities, and specifically, the endogenous sorting of refugees to schools in major cities. Interestingly, the effect of other immigrants is more negative and statistically significant. We also provide estimates solely for schools in these 5 cities, these estimates are very imprecise so caution must be exercised when interpreting them. They do, however, suggest that there are also negative effects of refugees in major cities, and in practice effect sizes are essentially the same across both sets of estimates.

5.2. School choice and neighbourhoods

School choice is very limited in Norwegian schools and the aim is to integrate refugee children quickly into neighbourhood publicly run

⁷ We include these as average ISCED level, but an alternative approach using share of parents with university or higher qualifications yields similar results.

⁸ Related to this, in additional unreported estimates we also dropped all schools-year observations with greater than 30 children in the grade. Again, these are schools outside of large cities, but have the additional feature that these will be predominantly single class per grade. The point estimate of refugee share was unchanged, although it was no longer statistically significant.

Table 5

The share of refugees, other immigrants, and the mathematics scores of native students, 5th grade 2007–2015. Including and excluding large cities.

	Excluding the 5 largest cities	Only the 5 largest cities
Share of refugee immigrants in school grade	−0.188* (0.104) [−0.010]	−0.209 (0.252) [−0.012]
Share of other immigrants in school grade	−0.159* (0.0903)	0.122 (0.199)
Observations	167,936	36,122
R-squared	0.667	0.679

All controls as per column (IV) [Table 1](#) including time dummies, school and family fixed effects. Robust standard errors clustered at the school-grade-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively. [] provides the effect size of a one standard deviation increase in refugee share.

schools. Nonetheless, municipalities may in some cases organise special teaching for groups of students with special needs in schools other than their neighbourhood school. As an example, refugees can be placed in introductory classes in the first year after they arrive in Norway in a school with more resources for specialised teaching. After this year the children are then placed back into their neighbourhood school. There is no official data on school catchment areas in Norway, and we lack any additional information that explicitly informs us of whether students attend their closest neighbourhood school. However, based on detailed information on every students' neighbourhood ('grunnkrets')⁹, we create proxy catchment areas using information on which schools other, non-immigrant, children in the same area attend. We utilise this to create an alternative version of share of refugees based on only those who we are highly certain go to their neighbourhood school.¹⁰ We view

⁹ There are approximately 14,000 of these neighbourhoods in Norway

¹⁰ Specifically, we observe the neighborhood that students reside in and which school they attend. We pool the 9 years of our data and categorise a school as being a neighbourhood school if at least 90% of students in the area attend it on average across these 9 years. We use this to then characterize whether refugee students attend their neighborhood school. Note this approach will likely lead to misclassifying students as not attending their neighborhood school in cases where, for instance, school catchment areas do not align very well with our residential areas, or a school was shut-down or a new school built during the period.

Table 6

The share of refugees, other immigrants, and the mathematics scores of native students, 5th grade 2007–2015. The role of attending neighbourhood schools.

	Refugees attended neighbourhood school	Controlling for share of refugees who may not be in their neighborhood school	Excluding refugees if uncertain if they attended neighborhood school
Share of refugee immigrants in school grade (and in the neighbourhood school)	-0.247* (0.131) [-0.013]	-0.259** (0.132) [-0.013]	-0.445** (0.223) [-0.012]
Share of refugee immigrants in school grade (uncertain if in the neighbourhood school)		-0.157 (0.120)	
Share of other immigrants in the school grade	-0.0836 (0.0736)	-0.0873 (0.0737)	-0.164 (0.0986)
Observations	204,058	204,058	105,962
R-squared	0.673	0.673	0.679

All controls as per column (IV) [Table 1](#) including time dummies, school and family fixed effects. Robust standard errors clustered at the school-grade-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively. Column I reports our estimates where the share of refugees is only for those refugees where it is highly certain that they attend their closest neighborhood school. Column II introduces an additional control for shares of refugees where we are uncertain that they attend their neighborhood school. Column III excludes all schools where there is an uncertain if any refugees are attending it and it is not their neighborhood school. [] provides the effect size of a one standard deviation increase in refugee share.

this as a very conservative approach likely to often incorrectly characterise refugees as attending a school other than their closest. We then use this information in a number of exploratory ways that are summarised in [Table 6](#). First, we re-estimate our main models where we include only as those refugees attending their neighbourhood school as part of our grade-share variable. In the second column, we additionally include a control for refugee shares where we are not sure if they are attending their neighbourhood school. In the final column we simply exclude from our estimation all school-grade-year observations where there is even one refugee attending who may not be in their neighbourhood school. These results reveal two main points. First, our main estimates do not reflect systematic patterns of non-attendance in neighbourhood schools. Second, attempts to focus on settings where we are more certain about attendance patterns strengthen our main findings. In all cases this leads to markedly larger, negative, effects on native performance. In the most extreme treatment of the problem, our estimated coefficient of interest is more than twice as large in absolute terms as our baseline estimate.

Neighbourhood interactions, beyond school, may also influence school outcomes. These is difficult to isolate as the bulk of students attend their local school in Norway. To explore this, we re-estimated our main model additionally including a covariate to capture neighbourhood refugee composition. The resultant estimates provide some suggestion of a neighbourhood effect in addition to the school spillover effect. This estimate of the grunnskole share is negative but not statistically significant, -0.067 (0.045), while the estimate of school grade refugee share remains -0.170 (0.095) and statistically significant at the 10% level.

Even in cases where refugee children enter and stay in their neighbourhood school a standard concern is that school principals may act strategically to move poor performing and / or disruptive students across grades to, for instance, lessen their impact on other students. In

practice, this is difficult in the Norwegian context. There is no grade retention, there exists a strong emphasis on social progression such that children should attend classes with others of their own age, and principals (or the authorities more generally) do not have the power to move children between grades. Yet, one potential channel is delayed school starting age ([Black et al, 2011](#)). All children in Norway should start compulsory schooling in the calendar year that they turn 6. In practice, 0.8% of Norwegian students in our data start one year later. This rises to 4% for refugee children leading to concerns about selectivity of entry into school. Holding a child back one year can only occur with agreement between parents, school principals and the educational psychological service in the municipal administration. Parents must take the initiative. Together with the local school principal, they ask the educational psychological service to provide an expert assessment. If this assessment supports a late (or early) school starting age, the parents can apply to the school. The decision is then made by the school principal. We explore the potential for this to impact on our estimates of interest by instrumenting refugee share with the share of refugees who should be in a grade based on their birth year. These results are reported in appendix [Table A4](#) and demonstrate that our main results are essentially unaffected by this. There is very high compliance with the school starting age rule so not surprisingly the instrument is highly relevant passing standard thresholds for detecting weak instruments ($F = 73,590$).

5.3. Family differences and siblings

At the heart of our identification strategy are narrow within family, within school, comparisons. The aim of doing this is to hold constant a range of factors that may change in the school environment as refugee shares change. As highlighted earlier, one consequence of this is that identification comes only from non-singleton children. More generally, [Miller, Shenhav and Grosz \(2019\)](#) demonstrate how large families can disproportionately identify the parameter of interest in sibling models. We rely on the size of our data to allow us to re-estimate all of the specifications reported in [Table 1](#) for families of 3 or more children, or those with only 2 children. The point estimates on refugee share where -0.173 (0.105) for 2 child families and -0.273 (0.162) for families with 3+ children.

A related point is that conditional on the number of children in the family, those with closer birth spacing may also do more to identify our main results. Again, we re-estimated our main models separately for different spaced child groupings (2 years apart, 3 years apart, through to 7 years apart). Again, our main results were unchanged. All estimates of refugee share were bounded between -0.179 and -0.322. In the case of larger spacing this additionally means that the children were not in the same school at the time of testing limited the potential for contemporaneous within school spillovers within the family.

Finally, our estimates are only identified by siblings who experience differential refugee shares. This we believe is a less acute problem than in binary treatment settings that are the primary focus of [Miller et al \(2019\)](#), and the most common case in our setting is two siblings neither of whom have any refugees in their class. Nonetheless, we re-estimated our pooled model (column 2 [Table 1](#)) excluding singletons along with sibling pairs experiencing the same refugee share. While this is not a definitive test, these estimates were essentially the same as those reported in [Table 1](#).

5.4. Non-linear effects and concentrations of refugee children

To this point, we have demonstrated robust negative effects of increased refugee shares on native test score performance. While on average refugee children are quite spread across Norwegian schools there is a tail of school grades with high shares of refugees. A concern may be that our results are generated by high refugee share settings that are likely to have unobservable differences that may influence test scores in a range of ways, and where the teacher and class environment

Table 7
Non-linear effects of refugees on the mathematics scores of native students, 5th grade 2007–2015.

	Incl. quadratic term	Number of refugee children
Share of refugee immigrants in school grade	-0.336** (0.157)	
Share of refugee immigrants in school grade ²	0.692 (0.627)	
1 Refugee in class		-0.0118 (0.00918)
2 Refugees in class		-0.0264** (0.0117)
3+ Refugees in class		-0.0368*** (0.0126)
Share of other immigrants in school grade	-0.0878 (0.0747)	-0.0837 (0.0737)
Observations	204,058	204,058
R-squared	0.673	0.671

All controls as per column (III) [Table 1](#) including time dummies, school and family fixed effects. Robust standard errors clustered at the school-grade-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively.

Table 8
The share of refugees and the mathematics scores of native students, 5th grade 2007–2015. Excluding school-grades with high refugee shares.

	Less than 50 %	Less than 40 %	Less than 30 %	Less than 20 %	Less than 10 %
Share of refugees in the school grade	-0.194** (0.0943) [-0.010]	-0.202** (0.0950) [-0.010]	-0.206** (0.0974) [-0.010]	-0.203* (0.111) [-0.008]	-0.330** (0.161) [-0.009]
Share of other immigrants in school grade	-0.0939 (0.0740)	-0.0937 (0.0741)	-0.101 (0.0743)	-0.106 (0.0762)	-0.0952 (0.0830)
Observations	203,005	202,880	202,138	196,950	172,102
R-squared	0.672	0.672	0.673	0.673	0.675

All controls as per column (IV) [Table 1](#) including time dummies, school and family fixed effects. Robust standard errors clustered at the school-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively. [] provides the effect size of a one standard deviation increase in refugee share.

may be atypical. More broadly, any effect of refugee shares on native student performance may be non-linear. [Table 7](#) reports estimates that aim to investigate these issues.

The first column simply adds a quadratic term to the share of refugees. This term is positive but not statistically significant. Nonetheless, when combined with the more negative estimate of share of refugees in the cohort, this could suggest negative effects that are concentrated in low shares of refugees. Column 2 examines this further allowing the effect of refugee numbers to vary over 1 refugee child in class, 2 in the class, or 3 or more. While the effect of 1 refugee is not statistically significant, the pattern of coefficients broadly supports a linear effect of refugee numbers.

[Table 8](#) reports estimates which take an alternative approach, and address a related but different issue, are schools with high refugee shares somehow different in a way that is consequential for test score performance? We estimated models where we successively exclude school-grade observations with greater than 50% refugee share progressively all the way through to greater than a 10% refugee share. These results are remarkably consistent and suggest that our estimates do not reflect the effect of settings where refugee shares are high. One might be tempted to also interpret these results as suggesting that increasing the concentrations of refugees has no additional effects on native test score performance, but one must remember the interpretation of these estimates in the presence of school and sibling fixed effects. They are

Table 9
The effect of refugee shares on measures of school inputs.

	Student to teacher ratio	Ordinary instruction hours per student
Share of refugees in the cohort at school	-0.00020 (0.00185)	-0.2914 (0.8034)
Share of other immigrants in the cohort at school	0.00928*** (0.00151)	-2.276*** (0.6561)
Observations	204,058	204,058
R-squared	0.680	0.668

All controls as per column (III) [Table 1](#) including time dummies, school and family fixed effects. Robust standard errors clustered at the school-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively.

unlikely to be informative about, and we lack the statistical power to (for instance) estimate, the effect of changes within school and family from, for instance, a 10% to 50% refugee share.

6. Mechanisms and extensions

A natural question is what mechanisms are likely to generate these negative effects on math we estimate? While we cannot be exhaustive, we are able to explore some potential channels.

First, do time-varying school inputs adjust to changes in refugee shares in ways that could be viewed as compensatory? We examine two measures of school inputs observable to us through the GSI data. Given the markedly lower test score performance for refugee students apparent in [Table A2](#), and their negative impact we demonstrate on native students, one might expect a well-functioning public school system to introduce compensatory inputs.

Specifically, we ask the question what happens to the school inputs of a student who is taught in the same school as their sibling but experiences, for example, a higher refugee share in their grade? Is there any evidence that the school authorities act to introduce additional school inputs? At the same time, additional refugee children may impact on enrolment and the negative effect we are picking up could reflect, at least in part, negative effects of increases in class size.¹¹ We estimate variants of our school and sibling fixed effects model with, as dependent variables, the grade level student to teacher ratio, and the ordinary instruction hour per student (only available for grades 5 to 7 combined). These results are reported in [Table 9](#). We find no evidence that student-teacher ratios are influenced. While negative, again this is not statistically significant, and very small in magnitude. The same is true for instruction hours per student. The lack of any effect, especially compensatory increases in inputs, may provide some hint at why refugee classmates have a negative effect on native student performance. These students face additional challenges in school and demand more attention from teachers, yet we cannot detect any evidence of responses in terms of school inputs, at least those we can observe.

One weakness of our data is that we do not observe information on teachers. A typical concern is that principals could move teachers across classes in response to different class mixes. Note, again that this is an uncommon practice in Norway. However, we re-estimated our main model in (smaller) schools likely to only have one class per grade. The resultant point estimate of refugee share on native math scores was -0.188 suggesting that this form of assignment of teachers to classes are not driving our results.

Next, we conduct a range of analyses which aim to investigate

¹¹ Although we recognize that Norway is a country where precisely zero effects of class size on test scores and other outcomes have been consistently demonstrated (Leuven, Oosterbeek & Rønning, 2008; Falch, Sandsør & Strom, 2017; Leuven and Løkken, 2020).

Table 10

The share of refugees from different regions of origin and the mathematics scores of native students, 5th grade 2007–2015.

	former Yugoslavia	Africa	Asia	Middle East
Share of refugees from...	−0.162 (0.223) [−0.003]	−0.396** (0.166) [−0.009]	0.178 (0.221) [0.003]	−0.355** (0.177) [−0.008]
Share of other refugees	−0.203* (0.101)	−0.108 (0.112)	−0.267*** (0.102)	−0.133 (0.109)
Share of total refugees	0.18	0.31	0.32	0.20
Observations	204,058	204,058	204,058	204,058
R-squared	0.673	0.673	0.673	0.673

All controls as per column (IV) [Table 1](#) including time dummies, school and family fixed effects. Robust standard errors clustered at the school-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively. [] provides the effect size of a one standard deviation increase in the relevant refugee share.

Table 11

The role of parental characteristics and being born in Norway on refugee spillovers.

	All	Former Yugoslavia	Asia	Africa	Middle East
<i>Controlling for Refugees' Average Parental Education, Income, and Share Born in Norway</i>					
<i>Math</i>					
Share of Refugees from...	−0.203** −0.101 [−0.010]	0.341 −0.432 [0.006]	−0.04 −0.299 [−0.001]	−0.322 −0.205 [−0.007]	−0.271 −0.262 [−0.006]
<i>Norwegian</i>					
Share of Refugees from...	0.0138 −0.0886 [0.0007]	0.661 −0.45 [0.011]	0.157 −0.316 [−0.002]	0.184 −0.23 [−0.004]	0.0368 −0.261 [−0.001]
<i>English</i>					
Share of Refugees from...	0.0524 −0.104 [0.0027]	0.279 −0.496 [0.005]	0.345 −0.367 [−0.005]	−0.296 −0.235 [−0.006]	−0.189 −0.29 [−0.004]

All controls as per column (IV) [Table 1](#) including time dummies, school and family fixed effects. Robust standard errors clustered at the school-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively. [] provides the effect size of a one standard deviation increase in the relevant refugee share.

whether refugee characteristics can, in some way, explain our results. As highlighted in the introduction, the existing literature has typically focused on quite broad immigrant categories. We have demonstrated here that the effects of immigrants on native outcomes, at least in our setting, differ markedly between those with a refugee and other immigration background. Naturally, refugees themselves represent a heterogeneous group across a range of dimensions. An important feature of our setting is that they should not differ in terms of their initial location placements across Norway, hence across schools, or more generally in their treatment by Norwegian authorities. An advantage of our setting then is to be able to provide precise estimates across different refugee groups.

We start by disaggregating refugees according to country of origin. We have no priors regarding differential country of origin effects and this is necessarily exploratory, but we do this with a view towards the large variation in immigrant effects highlighted in the existing literature. We divide refugees into four regional categories (Middle East, former Yugoslavia, Asia, and Africa)¹² as together these four regions of origin account for the vast majority (97%) of refugees in Norway. [Table A5](#) provides related descriptive statistics for each of these groups. As can be seen, even amongst these quite aggregated groups, there are marked average differences in family background characteristics, and also test score performance.¹³ We then re-estimated our family fixed

¹² African countries include all countries in Africa. Asian countries include all Asian countries except those defined as “Middle East”. Former Yugoslavia includes Bosnia-Herzegovina, Croatia, Macedonia, Montenegro, Slovenia, Serbia and Kosovo. Middle east is defined as Bahrain, United Arab Emirates, Egypt, Iraq, Iran, Israel, Jordan, Kuwait, Palestina, Lebanon, Oman, Qatar, Syria, Saudi Arabia, Yemen and Turkey.

¹³ Note that these patterns of test score differences remain in simple estimates that control for family background differences and school fixed effects.

effects models allowing for the refugee effects to vary by these regions of origin.¹⁴

We estimate four separate regressions where, in each case, we separate the focal group of refugees from the other 3 groups. This means that in each case we provide an estimate of the effect of the specific refugee group and an estimate pooled for the other three groups. This is primarily for the sake of precision, but a regression where we estimate four parameters, one for each refugee group, results in point estimates that are in essence the same as across the top row of [Table 10](#).

These estimates demonstrate dramatic differences in effects according to region of origin. The average negative impact from the refugee share in class seems to be driven primarily by refugees from African countries and from the Middle East. The coefficient for share of refugees from former Yugoslavia is also negative and similar to our main result, but not statistically significant. While imprecise and not statistically significant there is some suggestion that refugees from Asian countries have a positive impact on the math performance of native students.

Our reading of these results is that they fit with the differences in (for instance) average test score performance of these different refugee groups themselves reported in [Table A5](#). Hence, one interpretation is that students who themselves face academic difficulties are the most likely to be the source of negative peer effects. This in turn fits with evidence on the sources of negative peer effects in schools ([Lavy, Silva & Weinhard, 2012](#)).

To push this point further we examine whether we can in some sense account for the differences across regions of origin, and more generally, for refugee spillovers in general. One natural question is whether these

¹⁴ In principle we could provide similar results for economic immigrants, but they typically come from different regions (and countries within these regions) than refugees making comparisons difficult in practice.

patterns reflect differences in observable characteristics of different refugee groups. To investigate this, we re-estimate our models where we, in addition to refugee shares, include shares of key covariates associated themselves with underperformance. This includes average parental education and income levels of mother and father separately. As highlighted earlier, there have been a number of refugee waves into Norway. This has implications for the region of origin effects in particular, as on average these children have been in Norway for differing amounts of time, and this may influence their performance within school amongst other things. This leads us to additionally include a control for the share of refugees born in Norway. As a result, we estimate our main model for all refugees where we include the average parental education, income levels and share born in Norway for refugees additionally. We then estimate variants of the models reported in Table 10, where we additionally include these covariates for the focal refugee group. The aim of this approach is explorative and simply to examine the stability of the main parameters of interest to including these additional controls. The resultant estimates are reported in the top panel of Table 11. The overall pooled estimates suggest that the negative effect of refugee children on native math attainment does not simply reflect characteristics differences. There remains some indication of heterogeneity by regional group. Children from former Yugoslavia have characteristics that, on average, generates negative spillover effects on math performance. Accounting for this, turns this effect positive, but this estimate is very imprecise. While, controlling for characteristics reveals essentially no math spillover effects from Asian refugee children. The point estimates for the other 2 regional groups remain negative, although again we urge caution as we are pushing the data very hard and the results are very imprecise. While explorative, our overall reading is that it is not simply observable differences in refugee characteristics generating the negative refugee spillovers on maths. For completeness, we report equivalent exercises for Norwegian and English. These reveal that the overall zero effects remain once characteristics of refugees are introduced, yet there is some suggestion of positive effects for Norwegian for specific groups. For the reasons discussed earlier, this is difficult to interpret, but could reflect positive spillovers from additional Norwegian language resources due to refugee children in the school-grade.

7. Conclusion

The effects of immigration on a range of outcomes in recipient countries remains highly debated and controversial. Events such as European Migrant Crisis of 2015 bring this into sharper focus. One particular focus of policy debate and research is the impact of immigration, and in particular, immigrant children on the educational outcomes and school experiences of native children. The current research in this area covers a range of countries and provides mixed evidence. The majority of this research does this by examining quite broad categories of immigrants who in practice vary markedly in terms of important characteristics likely to influence their own educational performance and in terms of their reasons for immigration. While recent US research that focuses on refugee spillovers finds zero to small positive effects. This

Table A1

Proportions of students who did not attend exams or where granted an exemption by subject and immigration status. grade 5 national tests, 2007–2015.

	Native students	Refugees	Other immigrants
<i>Math</i>			
Did Not Attend Exam	0.46	1.30	0.99
Exempted	1.96	9.23	6.77
<i>English</i>			
Did Not Attend Exam	0.47	1.17	0.83
Exempted	2.26	10.18	7.45
<i>Norwegian</i>			
Did Not Attend Exam	0.68	1.30	1.17
Exempted	2.65	12.56	10.43

Notes: Table reports the proportion of students who did not sit the respective exams. Calculations authors based on student registry data.

paper returns to this issue in a setting, Norway, which has experienced dramatic increases in immigration, and where we focus on refugee spillovers.

We demonstrate robust negative effects of refugees on the math scores of native primary school children, no effect of other immigrants, and no effect on English or Norwegian performance. We do this by comparing within-sibling within-school variation in exposure to immigrant peers. The negative effects on math are, we argue, of an important magnitude. We subsequently demonstrate that these effects amongst refugees vary markedly, where the negative effects appear to reflect refugee background children who, themselves, face educational difficulties. Other refugee children have an average zero effect on native educational attainment.

Our results are important for several reasons. First, it suggests a role for targeted interventions that are best aimed at schools who enrol refugee children, with a particular focus on educationally disadvantaged refugee children. Second, the pattern of our results across, for instance Math and Norwegian, potentially offer some further indication of this. Even the most disadvantaged refugee group has a zero average spillover effect on Norwegian performance. This possibly reflects the additional resources given to those not from a Norwegian background in Norwegian class (and by extension the lack of these resources in Maths). Finally, our results highlight the need for care in the extrapolation of findings from specific refugee events and groups of immigrants. We show that even within one country and one institutional setting, and among immigrant children from a refugee background, there are large variations in spillovers that range from large and negative, to zero or small and positive.

CRediT authorship contribution statement

Colin Green: Conceptualization, Methodology, Software, Validation, Formal analysis, Investigation, Data curation, Writing – original draft, Writing – review & editing. **Jon Marius Vaag Iversen:** Conceptualization, Methodology, Software, Validation, Formal analysis, Investigation, Data curation, Writing – original draft, Writing – review & editing.

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Appendix

Tables A1, A2, A3, A4, A5

Table A2

Descriptive statistics for key variables, grade 5 students 2007–2015.

	Native students		Refugees		Other immigrants	
	Mean	Std.dev	Mean	Std.dev	Mean	Std.dev
Math Test Scores	0.048	0.983	−0.577	0.986	−0.229	1.04
Norwegian Test Scores	0.054	0.976	−0.614	1.020	−0.406	1.05
English Test Scores	−0.023	0.976	−0.269	1.070	0.034	1.09
Refugee Share	0.038	0.053	0.143	0.107	0.101	0.102
Other Immigrants Share	0.047	0.063	0.134	0.150	0.202	0.172
Grade Enrolment	41.2	22.20	48.4	20.10	47.7	22.00
Parity	1.92	0.97	2.34	1.53	1.84	1.100
Female	0.489		0.493		0.490	
Father's Income	586,254	483,070	264,304	23,001	413,247	369,741
Mother's Income	347,131	240,729	176,689	25,604	214,566	223,872
<i>Father's education level:</i>						
Unknown education	0.010		0.153		0.257	
Primary school	0.000		0.092		0.037	
Lower secondary school	0.154		0.279		0.202	
Incomplete secondary education	0.060		0.036		0.050	
Completed secondary education	0.435		0.202		0.198	
Degree or Higher	0.342		0.225		0.240	
<i>Mother's education level:</i>						
Unknown education	0.000		0.125		0.156	
Primary school	0.000		0.132		0.054	
Lower secondary school	0.134		0.349		0.255	
Incomplete secondary education	0.055		0.028		0.041	
Complete secondary education	0.323		0.202		0.214	
Degree or Higher	0.487		0.167		0.280	
Observations	393,461		22,128		25,085	

All test scores normalised to mean zero for each year observation. Income in 2015 real values.

Table A3

The share of refugees, other immigrants, and the mathematics scores of native students, 5th grade 2007–2015, non-singletons only.

	(1)	(2)	(3)	(4)
Share of refugee immigrants in the school grade	0.0283 (0.0678) [0.0015]	−0.175*** (0.0629) [−0.0090]	−0.115 (0.0849) [−0.0059]	−0.204** (0.0932) [−0.0105]
Share of other immigrants in the school grade	0.663*** (0.0574)	0.479*** (0.0511)	−0.0666 (0.0676)	−0.0870 (0.0737)
Observations	204,058	204,058	204,058	204,058
R-squared	0.003	0.114	0.167	0.674
School fixed effects	no	no	yes	yes
Family fixed effects	no	no	no	yes
Time fixed effects	no	yes	yes	yes
individual and family controls	no	yes	yes	yes

Controls are gender, maternal education level, paternal education level, and grade enrolment. Column III additional includes birth order number of the child as a series of dummies. Robust standard errors clustered at the school-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively. [] provides the effect size of a one standard deviation increase in the relevant refugee share.

Table A4

IV Estimates of the impact of delayed school starting ages, 2007–2015.

	(1)
Share of refugees in the school grade	−0.231** (0.101)
Share of other immigrants in school grade	−0.0882 (0.0737)
School Starting Age	0.922*** (0.005)
Kleibergen-Paap F-Stat	73,590
Observations	204,058
R-squared	0.674
School fixed effects	yes
Family fixed effects	yes
Time fixed effects	yes
individual and family controls	yes

All controls as per column (III) Table 1 including time dummies, school and family fixed effects. Robust standard errors clustered at the school-year level in parentheses. ***, **, and * indicate statistical significance at the 1%, 5% and 10% level, respectively.

Table A5
Selected summary statistics, refugees and region of origin, 2007–2015.

	Refugees from former Yugoslavia	Refugees from Middle East	Refugees from Africa	Refugees from Asia
Math Test Scores	-0.434	-0.593	-0.848	-0.318
Norwegian Test Scores	-0.470	-0.718	-0.763	-0.415
English Test Scores	-0.094	-0.410	-0.430	-0.021
Mother Income (NOK)	217 490	107 747	97 501	160 925
Father Income (NOK)	336 101	198 822	188 446	298 869
<i>Father's education level:</i>				
Unknown education	0.063	0.111	0.258	0.130
Primary school	0.020	0.142	0.085	0.115
Lower secondary school	0.209	0.254	0.287	0.353
Incomplete secondary education	0.055	0.028	0.025	0.053
Complete secondary education	0.450	0.184	0.165	0.157
Degree or Higher	0.207	0.281	0.180	0.192
<i>Mother's education level:</i>				
Unknown education	0.068	0.136	0.163	0.122
Primary school	0.032	0.141	0.186	0.152
Lower secondary school	0.294	0.344	0.372	0.383
Incomplete secondary education	0.037	0.024	0.017	0.044
Complete secondary education	0.375	0.153	0.163	0.169
Degree or Higher	0.193	0.209	0.098	0.129
Observations	4,220	7,351	7,789	4,736

All test scores normalised to mean zero for each year-grade observation. Income in 2015 real values.

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