The Effects of Immigrant Concentration in Schools on Adolescent Crime

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ABSTRACT

Despite a growing literature on immigrant concentration in school and children's educational outcomes, the effects on crime has received surprisingly little empirical focus. This study addresses the effect of immigrant peer exposure on adolescent criminal behavior among immigrant and native male students using Norwegian administrative data. Our sample covers eighteen full cohorts in their final grade of compulsory education followed to their early twenties (476,352 students; 13,521 school-cohorts; 1,011 schools). Results show a moderate positive correlation between immigrant share and crime rates at the school-cohort level (Pearson's r=0.225, p<0.001). While this between-school relationship is reduced in regressions controlling for school fixed effects and student background characteristics, we find that native students in cohorts with more immigrant peers within the same school have slightly higher propensities to commit crimes. These effects are concentrated among students with low-educated parents. For immigrant students, we do not find any adverse immigrant peer effects.

Key words: crime; immigration; ethnic segregation; peer effects; school fixed effects

INTRODUCTION

The consequences of large-scale immigration from low-income origin countries to rich, advanced countries is Europe and North America is high on the political agenda in many countries (Coleman, 2006, Alba and Foner, 2015, Waters and Pineau, 2015). A key aspect of this is concern for increased immigrant concentration in residential areas characterized by relative social deprivation, leading to out-migration among natives, an acceleration of place-based inequalities, and adverse effects of school segregation affecting students of both minority and majority background (Crowder, Hall and Tolnay, 2011, Schwartz and Stiefel, 2011, Rathelot and Safi, 2014, Wessel *et al.*, 2017). However, these debates often have a limited basis in empirical evidence, and several recent studies on the effects of immigrant concentration in schools on educational outcomes indicates modest reason for concern. However, this does not rule out concern for effects on youth delinquency as the literature on how school and residential concentration of immigrants affects crime is less established.

While the relationship between immigration and crime has a long history in the social sciences (e.g., Hart, 1896, Taft, 1933), the current state of knowledge provides mixed evidence on the magnitude and direction of the immigration-crime link (Bell and Machin, 2013, Ousey and Kubrin, 2018). One might expect that immigrant students will have heightened crime propensities compared to natives because they, for example, experience relative socioeconomic disadvantages, face (structural) discrimination, and, for the foreign-born, may have experienced childhood traumas that lead them to commit more crimes, and that this delinquent behavior could spill over to their fellow schoolmates of both ethnic minority and majority background. While the literature on immigrant peer effects and crime is limited, several studies find marked peer effects on crime and educational outcomes from exposure to school and neighborhood contexts characterized by socioeconomic deprivation, high rates of delinquency, or other forms of problem behavior (e.g., Haynie, Silver and Teasdale, 2006, Carrell and Hoekstra, 2010, Fletcher, 2010, Zimmerman and Messner, 2011, Damm and Dustmann, 2014). By contrast, higher crime rates in schools with high immigrant concentrations could reflect the sorting of both native and immigrant families with less socioeconomic resources and children with higher crime propensities into the catchment areas of these schools (Sobel, 2006, Hällsten, Szulkin and Sarnecki, 2013).

A growing literature has analyzed the causal relationship between school immigrant shares and educational outcomes among their students in Norway and other immigrant-receiving Western countries (e.g., Gould, Lavy and Paserman, 2009, Jensen and Rasmussen, 2011, Schwartz and Stiefel, 2011, Geay, McNally and Telhaj, 2013, Ohinata and van Ours, 2013, Conger, 2015, Hermansen and Birkelund, 2015). A recurring result here is that peer effects of exposure to immigrant schoolmates often are very small or nonexistent once student sorting is adequately addressed. David Card (2013: F279) argues that findings from this literature "suggests that concerns over the negative consequences of immigrant classmates for native academic achievement may be overblown," although "there may be some effect on other aspects of the school environment, such as bullying or crime." The link between immigrant peer concentrations in school and student criminal behavior has not yet been systematically addressed; our study contributes to closing this central gap in the literature.

In this study, we present, to the best of our knowledge, the first empirical evidence on the effects of immigrant concentration in schools on students' later propensity to commit crime during adolescence. We use high quality Norwegian administrative data on 18 full cohorts of male students in the final grade of compulsory education (at age 16, graduating in 1992–2009) followed up with recorded crime information into their early twenties (16–21 years). The most central challenge to estimating the causal effect of immigrant peer concentration on student outcomes is nonrandom sorting across schools and their residential catchment areas. To address this selection issue, we exploit the within-school, across-cohort variation in immigrant peer composition to explore the effects on adolescent criminal behavior. We report tests validating the assumptions of our identification strategy and the robustness of our results. We focus on the effect of immigrant peer exposure in the final grade of compulsory schooling on students' criminal behavior during late adolescence—a link that has not been addressed in the existing literature. Moreover, we examine whether immigrant peer effects on adolescent crime vary between natives and immigrants and by parental education and employment status, immigrant

concentration in school, and residential mobility. Furthermore, we observe peer environments in school before early school leaving can occur and in an educational system without formal ability tracking and where school attendance is based on residential location.

PRIOR RESEARCH ON IMMIGRANT PEER EFFECTS

Ever since the Coleman et al. (1966) report research on the importance of school context and peer effects has been high on the agenda in the social sciences. Despite a growing number of empirical studies on the consequences of immigrant concentration in schools for children's educational outcomes, the potential link between exposure to immigrant peers and later-life criminal behavior has not yet been addressed in detail. The literature addressing educational outcomes has, however, reach varied conclusions.

Using a large immigrant influx into Israel in the 1990s, Gould et al. (2009) exploit the cross-cohort, within-school variation in immigrant concentration to study the effect of exposure to immigrant peers, and find a substantial, long-term adverse effect on high school dropout rates and high school matriculation among native Israeli students. After instrumenting for the city-level immigrant concentration, Jensen and Rasmussen (2011) find negative effects of immigrant concentration on the achievement scores of both native and immigrant students in Danish lower secondary schools. In a comparative study of within-school effects of immigrant peers on native students' standardized test performance, Seah (2014) reports a positive influence of immigrant peers in Australia, no effect in the United States, and a negative effect on natives in Canada. In the United States, Schwartz and Stiefel (2011) found that students in New York City schools attending cohorts with higher shares of foreign-born peers had slightly lower achievements. Conger (2015) uses administrative data on public high schools in Florida to show that students in cohorts with higher numbers of foreign-born peers have similar or slightly better educational achievements compared to students in cohorts with fewer immigrants within the same school. Hunt (2012) found that an increase in the immigrant share in the population increased the likelihood of high school graduation among native-born black students in the United States. Similarly, Brunello and Rocco (2013) and Geay et al. (2013) find small or no negative spillover effects of immigrants on natives, and Ohinata and Van Ours (2013) and Schneeweis (2015) find large negative effects of immigrant concentration on the educational outcomes of immigrant students in the Netherlands and Austria, respectively, but no significant effect on natives. In Sweden, Brandén et al. (2018) find that native and immigrant students' average grade achievements are relatively unaffected by peer effects related to higher concentrations of immigrant students in lower-secondary schools, but they do a find nontrivial negative effects on the eligibility requirement for continuation into upper-secondary school among the poorest achieving students.

In Norway, prior research generally find small or no effects of immigrant peer concentration on student outcomes in both lower secondary schools (Hermansen and Birkelund, 2015) and upper secondary schools (Fekjær and Birkelund, 2007, Hardoy and Schøne, 2013, Hardoy, Mastekaasa and Schøne, 2017). For example, Hermansen and Birkelund (2015) who also focuses on peer effects in lower-secondary schools finds that native students are not affected by immigrant concentration in their cohort while immigrant students in cohorts with more immigrant peers have slightly higher propensities to complete upper-secondary education by their early twenties.

A recent literature review on immigration and crime (Ousey & Kubrin 2017) indicates that overall, the crime-immigration association is negative, but the findings vary considerably across studies. A more serious concern is the relative lack of studies with research designs that handles sorting well. Thus, despite initial encouraging results, the findings are not conclusive.

INSTITUTIONAL SETTING IN NORWAY

The empirical analysis focuses on Norway, which constitutes an interesting case due to the combination of strong welfare institutions and a large and ethnically diverse immigrant-origin population. Norway has experienced large-scale immigration over several decades, comparable to

many other European immigrant-receiving countries (Brochmann and Kjeldstadli, 2008, Dustmann and Frattini, 2013). By early 2018, immigrants and their local-born children made up 17.3% of the total population in Norway (Statistics Norway, 2018). The relative size of the Norwegian immigrant population is comparable to countries such as Sweden, the Netherlands, Germany, France, the United Kingdom, and the United States (OECD, 2017). Recent non-European immigration to Norway started with the arrival labor migrants from Pakistan, Turkey, and Morocco around 1970. After a moratorium on unskilled labor migration from outside the Nordic region was introduced in 1975, immigration was restricted to family reunification for persons already in Norway and, since the late 1970s, a rising number of refugees and asylum seekers from countries such as Vietnam, Chile, Iran, Iraq, Somalia, and the former Yugoslavia (Brochmann and Kjeldstadli, 2008).

While adult migrants often experience considerable earnings disadvantages and declining employment rates over the life course, children of immigrants often experience considerable upward socioeconomic mobility in Norway (Bratsberg, Raaum and Røed, 2014, Hermansen, 2016, Hermansen, 2017). However, immigrant children grow up in households with high rates of child poverty compared to the native majority population and this overrepresentation has increased during past 10-15 years (Galloway *et al.*, 2015). Despite this, the educational gaps between native-born children of immigrants and the majority population has steadily narrowed since the early 2000s, but similar educational improvements over time has not been found for childhood immigrants born abroad (Bratsberg, Raaum and Røed, 2012). Despite generational progress, children of immigrants are overrepresented among persons registered with offenses compared to both adult migrants and the native majority in Norway (Andersen, Holtsmark and Mohn, 2017)—which is a pattern also documented in the United States (Morenoff and Astor, 2006). Further, levels of ethnic residential segregation is moderate and comparable to or slightly lower than levels found in other countries in Western Europe (Musterd, 2005, Rogne *et al.*, 2018). In these respects, the Norwegian case is representative of European countries that has seen rapidly growing immigrant populations in recent decades.

The Norwegian comprehensive school system is mandatory and publicly funded. Since 1997, compulsory education has consisted of 10 years of schooling from age six. However, for the cohorts we consider who graduated up to 2007, students started at age seven and compulsory education was split between primary schools (grades 1–6) and lower-secondary schools (grades 7–9).[1] Local municipalities run these comprehensive schools; there is no formal tracking by ability during these grades; school attendance is based on place of residence and the rules specifying that students attend the school in their local catchment are in general strictly enforced. Comprehensive schools in Norway often receive targeted resource transfers according to specific needs and the composition of the student body, which implies that school with many students from disadvantaged family backgrounds have low student-teacher ratios (Hægeland, Raaum and Salvanes, 2005) and schools serving many immigrant children tend to have more teaching assistants for special needs students (Hægeland, Kirkebøen and Raaum, 2009). Norwegian schools are characterized by modest between-school variation in student test achievements and parental socioeconomic resources (OECD, 2016).

More generally, Norwegian welfare-state society is characterized by low levels of economic inequality, a low rate of child poverty, and comparatively high rates of intergenerational social mobility (Esping-Andersen and Myles, 2011, OECD, 2015, UNICEF, 2016). High-quality basic services are offered to all residents, and immigrants have full coverage in healthcare services and other social security benefits. Local municipalities provide subsidized and high-quality childcare services to children from age one until school start. Overall, these institutional features are likely to reduce inequality in living standards between children in native and immigrant families.

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[1] Since the cohort graduating in 1997 they officially graduated from the tenth grade due to the abovementioned changes in the grade structure.

DATA AND METHODS

We use matched panel data on students and schools from Norwegian administrative registries made available for research by Statistics Norway. A system of personal identifiers enables the linkage of various administrative registries, children to their parents, and students to their school of graduation. The data set includes information on eighteen entire student cohorts (1992–2009) in the final grade of Norwegian compulsory education (at age 16). Small schools are likely to be of a special kind or serve students with special needs. We therefore exclude students graduating from small schools, defined as schools cohorts with fewer than 20 graduating students. We also exclude a very small number of students who graduate from compulsory education more than one year before or after the norm of graduating at age 16. As boys commit the great majority of adolescent crimes (i.e., felonies), our focus is solely on male students. With these restrictions, our final sample consists of 476,352 students nested within 13,521 school-cohorts and 1,011 schools.

Variable Measurement

Adolescent crime is measured using a dichotomous indicator of ever being registered for a felony in the age span between 16 and 21 years (yes=1, no=0). Information is based on police records of solved cases where a person has been charged and a judicial decision has been made. This definition is also what is used in the official Norwegian crime statistics (Lyngstad and Skardhamar, 2011).[2] The police records are detailed in that if multiple offences are committed at the same time (e.g. DUI as well as committing a robbery), each offence is recorded. This makes it possible to do further analysis of outcomes by subcategories of adolescent crime (e.g., theft, violence, and drug-related crimes). The police records include also a range of minor offences, but we restrict the analysis to the more serious ones. Until the new penal code took effect in 2015, the Norwegian penal code made a formal distinction between felonies and misdemeanors, where the former is the more serious and have stricter procedural rules. The misdemeanors are largely composed of shoplifting, traffic violations, less serious assaults (without bodily harm), and minor larceny. We restrict the primary analysis to felonies.

We measure peers as the school-level student composition of each student's graduation cohort. Insofar that there is persistence in peer environments, we capture the cumulative effect of peer exposure throughout the enrollment period in lower-secondary schooling. We measure immigrant peer exposure as the school-level proportion of immigrant-origin students (i.e., two foreign-born parents), either born in Norway or abroad. The advantage of such a simple measure of school segregation is that it is easy to compute and understand, while informative for social policy. In later supplementary analyses, we will address variation in the effects of immigrant peer concentration (e.g., variation by region of origin and between foreign-born immigrant peers and Norwegian-born immigrant peers in the second generation).

Out data also includes information on a number of relevant background characteristics of the student and their parents. We control for a comprehensive set of background covariates: immigrant generational status and age-at-arrival intervals, country of origin (for immigrants), gender, whether the child is the first-born of his or her mother, number of siblings, mother's age at birth, whether the child lived in an intact or reconstituted family household, parental education, parental total earnings (log), father's employment, mother's employment, parents' receipt of social welfare transfers, parents' receipt of unemployment benefits, and whether the child had changed neighborhood location during the school-age years. All covariates are measured at age 16 or earlier.

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[2] Suspects are recorded in the charge data regardless of whether they were convicted, which should be seen as an advantage as a fair number of cases do not end in a conviction. Importantly, persons who were arrested and charged in an early stage of a police investigation but subsequently released and no longer considered a suspect in the case will not be included in the charge data.

Empirical Strategy

Our aim is to identify the causal effect of immigrant peer concentration on adolescent crime. Empirically it is difficult to disentangle the influence of peers on their classmates' outcomes due to several inferential obstacles, including (unobserved) selection effects, the reflection problem and common shocks due to shared environments, such as teachers (e.g., Manski, 1993, Sacerdote, 2011, Angrist, 2014). Thus, studies that regress student outcomes on a set of peer characteristics assuming random assignment of students to schools after conditioning on observed covariates are likely to obtain biased estimates of peer effects on students' later-life outcomes (Sørensen and Morgan, 2006).

While our data allows us to control for an unusually wide range of relevant and well-measured peer and student background variables, the primary strength of our data is the panel structure that allows us to observe several adjacent student cohorts within the same schools. This panel structure enables us to reduce bias from unobserved variables at both the level of students and their schools.
By estimating school fixed-effects models which exploits within-school variation in immigrant student composition across adjacent graduation cohort, we aim to break any remaining correlation between immigrant peer concentration and unobserved characteristics of students and their schools (cf. Hoxby, 2000, see for example Jonsson and Mood, 2008, Gould, et al., 2009, Bifulco, Fletcher and Ross, 2011, Lavy, Paserman and Schlosser, 2012, Legewie and DiPrete, 2012, Black, Devereux and Salvanes, 2013). This quasi-experimental approach relies on the identifying assumption that students and their families do not select which schools to attend based on peculiarities of their child's cohort, but rather on the average student composition of the school (Hoxby, 2000). We specify these linear probability models (estimated using OLS regression with school fixed effects) as

$$Y_{isc} = \alpha_s + \gamma \; immigrant_i + \theta \; immigrant \; composition_{sc} + \beta X_i + \delta_c + \varepsilon_{isc} \qquad (1)$$

where i, s, and c are indices for students, schools, and cohorts, respectively. Y_{isc} is the ever-felony outcome; α_s is the school fixed effects; X_i is the set of student background covariates; δ_c is the graduation cohort fixed effects; and ε_{isc} is a student-specific error term.

These school fixed-effects models allows us to examine whether cohort-to-cohort variation in student outcomes is systematically related to cohort-to-cohort variation in immigrant peer composition, thus controlling for all stable school characteristics and nonrandom selection of students into schools. The coefficient of interest, θ, which captures the effect of immigrant student composition, is obtained solely by relying on variation in immigrant student composition across graduating cohorts within the same schools.

Because we observe up to 18 cohorts within the same school, our school fixed-effects models may be biased by (unobserved) within-school trends across cohorts in school quality or student composition. For example, school quality can vary of an extended time period if, say, a school's reputation changes due to an inflow of immigrant students, which, in turn, affects teachers and families' selection into different schools. To adjust for bias from such time-varying school-quality trends, we use a moving average approach that addresses remaining concern for bias (Black, et al., 2013). In a robustness test, we therefor regress the likelihood of committing adolescent crimes by immigrant student composition in the students' cohort, while controlling for the mean immigrant student composition in the students' actual graduation cohort as well as the two adjacent cohorts. By construction, any deviation in peer composition from this three-year mean cannot be due to a linear trend over the 18-cohort period and can thus be treated as idiosyncratic variation. It should, however, be noted that this is a restrictive comparison and a very conservative measure of any true peer effect.

It should, however, be noted that some of the mechanisms through which immigrant student composition might influence student outcomes vary much across cohorts. By relying on within-school variation in cohort composition, any school-wide effects of immigrant student composition will be absorbed by the school fixed effects, and consequently missed in the estimates on student outcomes. Thus, the within-school estimates tell us whether any relationship between immigrant composition and

student outcomes arise from mechanisms related to dynamic changes in immigrant peer exposure across adjacent graduating cohorts within the same school.

PRELIMINARY RESULTS

Validating the Assumptions of the Identification Strategy

Our empirical strategy relies on two key assumptions: (1) that there is meaningful within-school variation in our treatment variable and (2) that the within-school, across-cohort strategy removes systematic sorting by student background characteristics. To begin, Figure 1 shows how native and immigrant students are distributed across schools with varying concentrations of immigrants, while Figure 2 visualizes the school-level trends in immigrant shares across the graduation cohorts we observe. Panel A shows the trends for schools with less than 10% immigrant students at the end of the observation period (i.e., in 2009) and Panel B shows the same trends for schools with 10% or more immigrant students at the end of the period. We see that there is a high degree of across-cohort variation in immigrant shares within schools.

< Figure 1 about here >

< Figure 2 about here >

Table 1 reveals that the majority of variation in immigrant student composition is found between schools and not within schools. Panel A presents the unadjusted overall (between- and within-school) variation in immigrant student shares, while panels B and C presents within-school variation. The distribution in Panel A is calculated for the residuals of regressions of the share of immigrant students while controlling for school and cohort fixed effects, while Panel C presents the residuals from similar regressions that also includes the three-year moving average controls. At the end of lower-secondary schooling, we see that both immigrant students are exposed to higher immigrant shares compared to native students and that they also experience more variation in immigrant peer exposure both overall and within schools. The overall standard deviation of in proportion of immigrant students is 7.1 and 20.5 percentage points among native and immigrant students, respectively. The within-school immigrant peer variation declines to a standard deviation of 3.2 for native students (2.0 after adding moving-average controls) and 6.3 for immigrant students (3.3 after adding moving-average controls). For natives, the within-school variation is about 45% of the overall variation (28% after moving-average controls) and, for immigrants, 31 % of the overall variation (16% after moving-average controls). These within-school deviations in peer composition is the basis for our identification for the immigrant peer effects. Although they may seem small, there is enough within-school variation left to identify the effect of immigrant peers with sufficient precision to determine statistical significance at conventional levels.

< Table 1 about here >

Next, we present evidence from a series of balancing tests where we examine whether student characteristics are systematically related to immigrant student composition across cohorts within schools. The basic notion behind our identification strategy is that, conditional on controls for school fixed effects, variation in peer composition should be "as if random" and should thus not be correlated with predetermined characteristics of the students such as family background characteristics. To examine this issue, Table 2 presents results where we regress the student characteristics on immigrant student composition using an OLS specification controlling for cohort fixed effects (model 1), a specification where we add the school fixed effects (model 2), and a specification where we in addition to the school fixed effects also include the moving-average controls (model 3). While we would expect a relationship in the OLS model using between school variation, this relationship should be strongly reduced in the within-school specifications. However, to the extent that the presence of these observed student background variables in the main specification will reduce bias; these balancing tests provide a worst-case assessment of the situation. As can be seen, many of the student

characteristics are correlated with immigrant student composition in the OLS model for both natives (panel A) and immigrants (panel B). However, when adjusting for school fixed effects and, even more so, when adding the moving-average controls this relationship largely disappears and are, for the most part, rendered statistically not significant even in this very large sample.

< Table 2 about here >

Figure 3 plots the relationship between the share of immigrant students and later adolescent crime rates at the school-cohort level, indicating a slightly higher crime rates in schools with higher proportion of immigrant students (Pearson's $r = 0.225$, $p < 0.001$). When we relate each graduation cohorts deviation from the school mean in immigrant shares and adolescent crime rate, we see that this relationship is very much attenuated (Pearson's $r = 0.018$, $p < 0.05$). We now turn to the analysis of individual-level data, where we include control variables and explore variation across different groups of students.

< Figure 3 about here >

Estimated Immigrant Peer Effects on Adolescent Crime

Table 3 presents preliminary results from linear probability models predicting the likelihood of ever committing a felony in the age span 16–21 years. Column 1 shows the linear relationship between immigrant student composition and adolescent crime while only adjusting for graduation cohort and municipality location. Column 2 introduces adjustments for immigrant generation and geographic origin region. Column 3 adds controls for a broad set of student and socioeconomic family background covariates. In column 4, we add the school fixed effects. Finally, columns 5 and 6 estimate the full model separately for native students and immigrant students. These are the models of central theoretical interest, since they test for the presence of variation in immigrant peer effects between native and immigrant students while strongly reducing the risk for bias caused by unobserved heterogeneity between schools and their student composition.

< Table 3 about here >

In column 1, the coefficient of the fraction immigrant students shows that a one-percentage-point increase in the immigrant share is related to a 0.225 percentage-point increase in the students' propensity to commit crimes during adolescence. Controlling for immigrant background (column 2) reduces this estimate to a 0.155 percentage-point increase, while adding controls for observed student and family background (column 3) reduces this further to a 0.099 percentage-point increase. This estimate is less than half of the unadjusted estimate in column 1. In column 4, we introduce school fixed effects to look at within-school variation in immigrant shares across cohorts and the coefficient for immigrant share is 0.017. However, this estimate is not significant at conventional levels, showing that there is no significant overall within-school relationship between immigrant share and adolescent crime.

When we split the sample by immigrant background in columns 5 and 6, we see a small positive coefficient of fraction immigrant student on crime among natives, which suggests that a one-percentage point increase in the immigrant share is related to a 0.045 percentage-point increase in the likelihood of committing adolescent crimes ($P < .05$). For immigrants there the estimated effect is negative and the point estimate is slightly larger, but the coefficient does not reach statistical significance at conventional levels (b = -0.072, $P > .10$). However, the estimates for natives and immigrants are not statistically significant different from each other, nor from the estimate in model 4.

In sum, these results show that there is a relatively clear clustering of high rates of adolescent crimes in schools with concentrations of immigrant students. However, much of this reflects student sorting on both observed socioeconomic family background characteristics as well as unobserved factors at the level of students and schools. Nevertheless, our results do suggest that there is a non-negligible

adverse immigrant peer effect on adolescent crimes among male native students. For immigrant students, we do not find a corresponding effect and, if anything, graduating from a cohort with many immigrant peers is related to lower crime propensity among minority students.

Robustness checks

In Table 4, we include the moving-average controls to test the robustness of our results. We also estimate models with placebo treatments where we replace the immigrant student composition in the students' own graduation cohort with the immigrant share in the cohorts who graduated the year before and the year after. If our coefficients capture a peer effect, we should expect the estimates for immigrant student composition in the adjacent cohorts to be weaker than the estimate based on the student's actual cohort.

< Table 4 about here >

Table for presents the results separately for native students (panel A) and immigrant students (panel B). In each panel, we first present the baseline school fixed effects estimate (columns 1 and 5). Next, we estimate a model using information on the immigrant student composition in the cohort above and below (columns 3 and 7). Finally, columns 4 and 8 introduce the measures of immigrant student composition in the three cohorts simultaneously.

For native students, we first see that the positive effect of immigrant student composition on adolescent crime is robust to the inclusion of moving-average controls (b = 0.053, $P < .10$). Further, we do not find any significant effect of the immigrant student composition in the adjacent cohorts and the magnitude of the estimated coefficients is also small. Assuming that immigrant peer effects primarily reflects schoolmates in the same cohort, these results are as expected. For immigrant students, none of the estimates reaches statistical significance and there is no clear pattern in the estimated coefficients for the adjacent cohorts. Overall, these results provide compelling evidence that the immigrant peer effects we find on native students' adolescent crime propensities actually reflect the influence of classmates in the same cohort.

Subgroup Analyses of Heterogeneous Immigrant Peer Effects

In Table 5, we present estimates for native students and immigrant students where explore whether the estimated effect of immigrant peers varies by parental education (panel A), father's employment status (panel B), level of immigrant concentration in school (panel C), and students' residential mobility during the grades in lower-secondary education (panel D).

< Table 5 about here >

For native students, we see that the immigrant peer effect leading to heightened risk is concentrated among boys from nonacademic families (panel A) and with parents that are not in employment (panel B). There is less variation across schools with high and low immigrant concentrations, as well as between students who had lived in the same residential neighborhood during the years of lower-secondary schooling and those who had moved neighborhoods during this period.

For immigrant students, there is a tendency of larger negative coefficients on adolescent crime (e.g., a deterring effect) for immigrant students from nonacademic families and with fathers not in employment. However, this estimate is only marginally statistically significant in with respect to students with fathers not in employment (b = -0.143, $P < 0.10$). The estimated negative coefficients for immigrant student composition are also larger for immigrant students in schools with more than 10% immigrant students and who lived in the same neighborhood during lower-secondary schooling.

SUMMARY

To sum up, our preliminary results suggest that native male students in cohorts with higher shares of immigrant peers within the same school have slightly higher propensities to commit crimes during the adolescent years. This adverse immigrant peer effect seems to be concentrated among children of native-born parents with low educational attainment and a disadvantaged economic status. For immigrant students, we do not find a similar effect. If anything, immigrant students in cohorts with more immigrant peers are less likely to commit adolescent crimes, but this effect is not significant at conventional levels.

Before the conference, we plan to address these questions in more detail by looking at variation in exposure to different types of immigrant students (e.g., by generation, region of origin, etc.), explore nonlinearities in the effect of immigrant concentration, and variation in effects across different types of felony offenses (e.g., theft, violence, and drug-related crimes).

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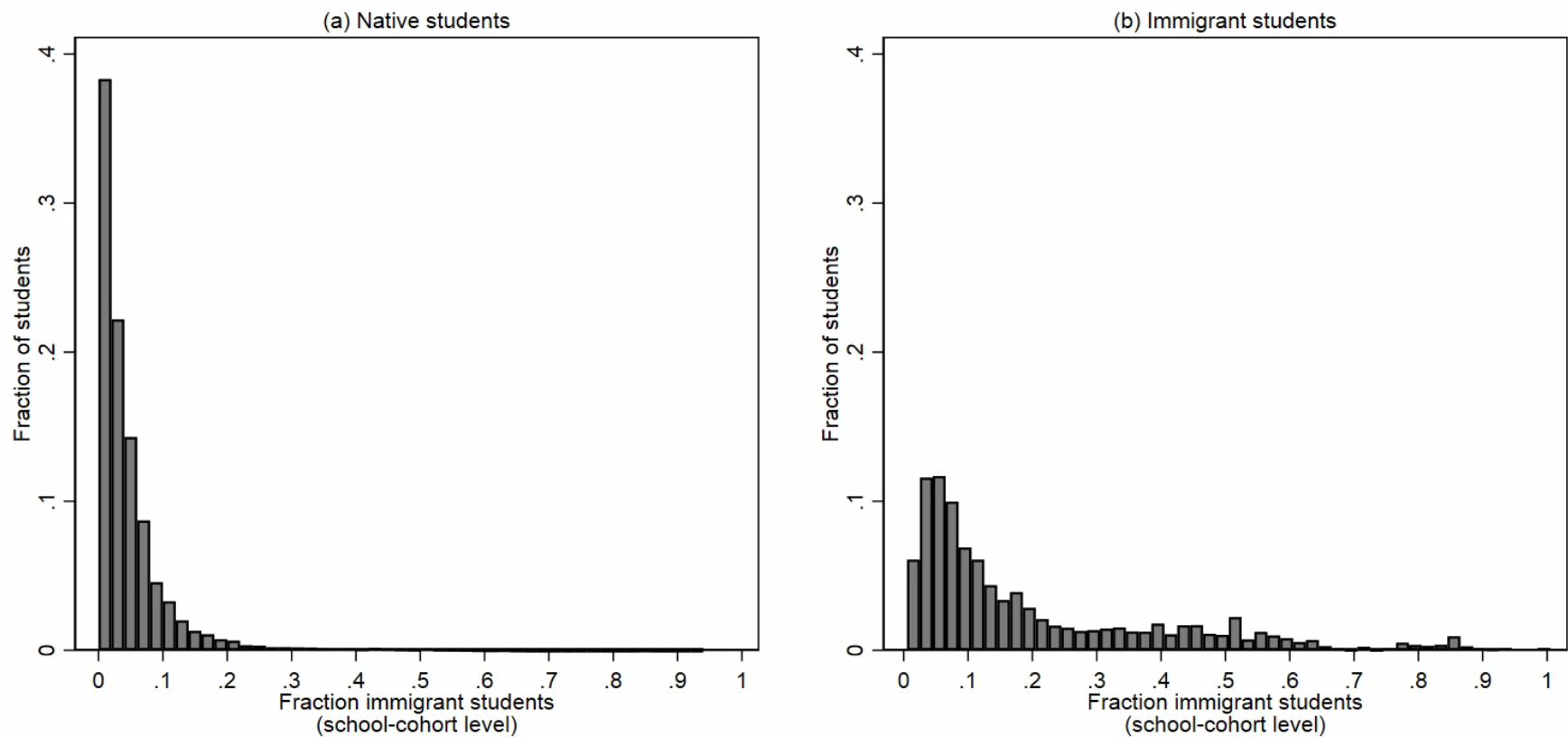
Figure 1. The distribution of native students and immigrant students attending schools with varying immigrant concentrations
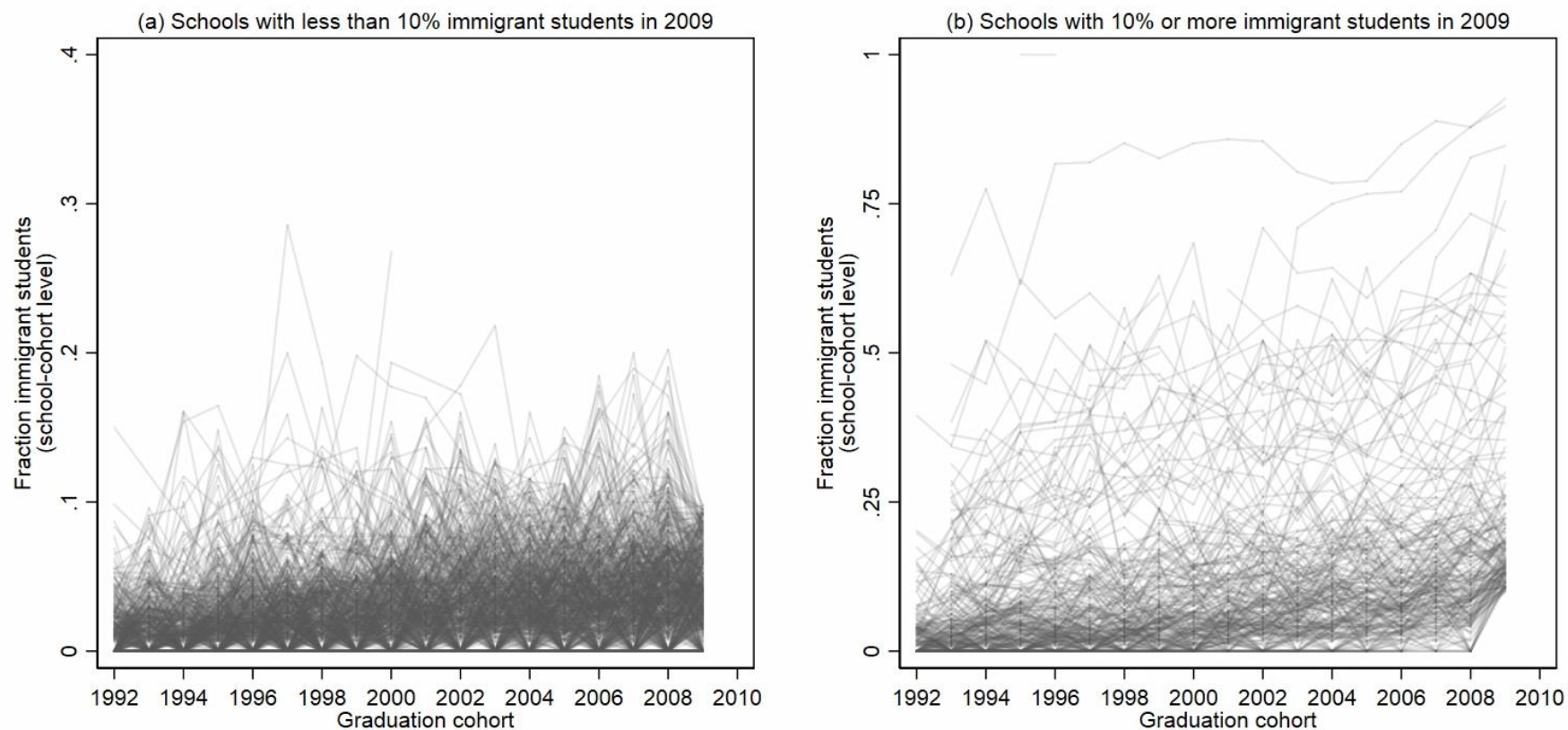
Figure 2. School-level trends in immigrant shares by graduation cohort
Note: Panel A presents trends for schools with less than 10% immigrant students in 2009. Panel B presents trends for schools with 10% or more immigrant students in 2009.
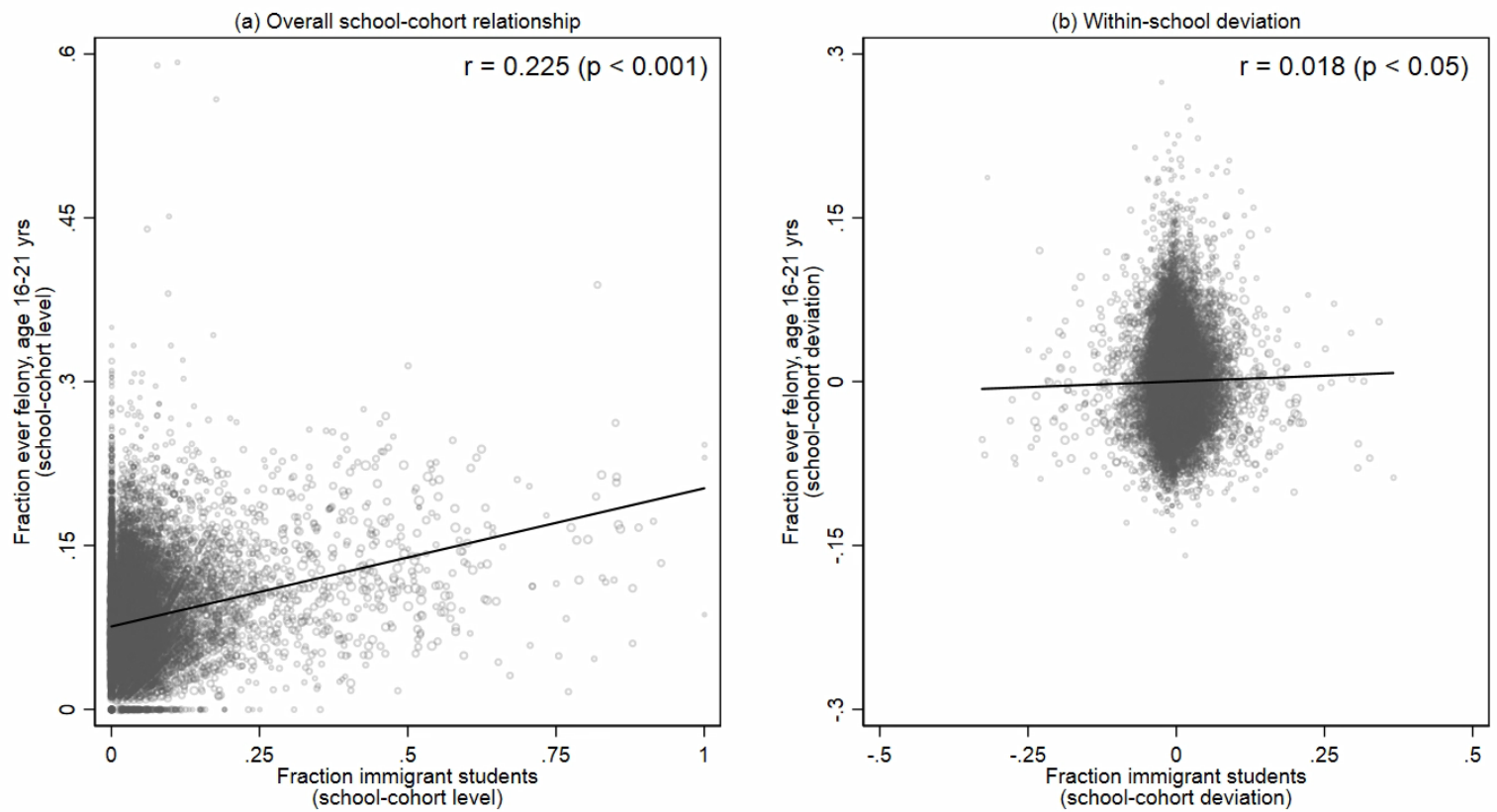
Figure 3. Immigrant student composition at the end of compulsory education and upper-secondary and adolescent crime rates (16-21 years) at the school-cohort level

Note: Scatter-point circles refer to school-level graduation cohorts. The size of each circle is weighted by the number of students in the cohort.

Table 1. Variation in immigrant student composition before and after removing school fixed effects and trends

	Proportion immigrant students				
	Mean	SD	Min	Max	N
Panel A: Unadjusted variation					
All students	0.058	0.093	0.000	1.000	476,352
Native students	0.049	0.071	0.000	0.927	448,331
Immigrant students	0.205	0.205	0.005	1.000	28,021
Panel B: School FE					
All students	0.000	0.036	-0.323	0.368	476,352
Native students	0.000	0.032	-0.295	0.430	448,331
Immigrant students	0.000	0.063	-0.387	0.313	28,021
Panel C: School FE and moving average					
All students	0.000	0.021	-0.193	0.186	476,352
Native students	0.000	0.020	-0.188	0.193	448,331
Immigrant students	0.000	0.033	-0.173	0.168	28,021

Notes: Panel A provides the overall distribution of immigrant student proportion in our sample. Panel B provides the distribution in residuals from regressions of immigrant student proportion on school fixed effects and graduation cohort fixed effects. Panel C porvides the distribution in residuals from regressions of immigrant student proportion after adding three-year moving average controls in addition to school fixed effects and graduation cohort fixed effects.

Table 2. Balancing tests for immigrant composition in school cohorts

	Panel A. Natives			Panel B. Immigrants		
	OLS	School FE	School FE and moving average	OLS	School FE	School FE and moving average
	(1)	(2)	(3)	(4)	(5)	(6)
Parents' postsecondary degree	0.195***	-0.044†	-0.058	-0.277***	-0.064	-0.097
	(0.010)	(0.024)	(0.036)	(0.013)	(0.047)	(0.076)
Parents' earnings (log)	-0.072*	0.023	-0.125	-3.024***	-0.125	-1.086
	(0.031)	(0.074)	(0.108)	(0.129)	(0.470)	(0.753)
Father employment	-0.141***	-0.018	-0.048*	-0.159***	0.072	0.083
	(0.007)	(0.016)	(0.023)	(0.014)	(0.053)	(0.084)
Mother employment	-0.001	-0.021	0.000	-0.394***	0.041	-0.075
	(0.008)	(0.020)	(0.029)	(0.014)	(0.052)	(0.083)
Parental social welfare	0.108***	-0.005	-0.007	0.102***	0.004	-0.103
	(0.004)	(0.011)	(0.015)	(0.013)	(0.046)	(0.073)
Parental unemployment	-0.032***	0.055***	0.017	-0.041***	0.065†	0.068
	(0.006)	(0.014)	(0.020)	(0.010)	(0.037)	(0.059)
Mother's age at birth	2.288***	0.460†	0.265	0.646**	0.745	2.796*
	(0.107)	(0.255)	(0.373)	(0.198)	(0.727)	(1.167)
Number of siblings	-1.421***	0.139**	0.118	1.094***	-0.065	0.030
	(0.023)	(0.053)	(0.078)	(0.051)	(0.186)	(0.299)
First-born child of mother	0.301***	0.019	0.004	-0.155***	-0.130*	-0.225*
	(0.011)	(0.026)	(0.038)	(0.015)	(0.054)	(0.087)
Intact or reconstituted family	-0.502***	-0.044*	-0.055†	-0.063***	-0.047	-0.045
	(0.009)	(0.022)	(0.032)	(0.013)	(0.048)	(0.076)
Residential stayer	-0.188***	0.016	-0.012	-0.035**	-0.003	-0.024
	(0.008)	(0.018)	(0.026)	(0.013)	(0.049)	(0.078)
N	448,331	448,331	448,331	28,021	28,021	28,021

Notes: The figures in each row are coefficients from a regression that include in addition to cohort compositio measures controls for cohort fixed effects and the student's immigrant background. In column 2, school fixed effects are added to the model. In column 3, three-year moving average controls are added to the model.

† $P < 0.10$; * $P < 0.05$; ** $P < 0.01$; *** $P < 0.001$ (two-tailed tests)

Table 3. Estimated effect of immigrant student composition on adolescent crime among male students in Norwegian lower-secondary school cohorts, 1992-2009.

	All				Natives	Immigrants
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction immigrant-origin students	0.225***	0.155***	0.099***	0.017	0.045*	-0.072
	(0.013)	(0.013)	(0.011)	(0.022)	(0.022)	(0.058)
Immigrant background (ref. = natives)						
Second generation, born in Norway		0.044***	-0.014*	-0.013*	-	ref.
		(0.005)	(0.006)	(0.006)		
Immigrant, arrival ages 0-6 years		0.087***	-0.004	-0.004	-	0.007
		(0.006)	(0.006)	(0.006)		(0.008)
Immigrant, arrival ages 7-12 years		0.114***	-0.027***	-0.026***	-	-0.001
		(0.005)	(0.006)	(0.006)		(0.008)
Immigrant, arrival ages 13-16 years		0.076***	-0.104***	-0.102***	-	-0.063***
		(0.007)	(0.008)	(0.008)		(0.011)
Number of students (log)			0.006**	-0.010*	-0.009*	-0.018
			(0.002)	(0.004)	(0.004)	(0.020)
First-born child of mother			-0.023***	-0.023***	-0.022***	-0.026***
			(0.001)	(0.001)	(0.001)	(0.006)
Number of siblings			0.004***	0.005***	0.005***	0.009***
			(0.001)	(0.001)	(0.001)	(0.002)
Mother's age at birth			-0.005***	-0.005***	-0.005***	-0.002***
			(0.000)	(0.000)	(0.000)	(0.001)
Intact or reconstituted family household			-0.067***	-0.065***	-0.065***	-0.062***
			(0.002)	(0.002)	(0.002)	(0.007)
Parental education (ref. = below upper-secondary education)						
Upper-secondary education completed			-0.027***	-0.028***	-0.029***	-0.008
			(0.002)	(0.001)	(0.001)	(0.008)
Postsecondary degree, BA level			-0.053***	-0.056***	-0.054***	-0.041***
			(0.002)	(0.002)	(0.002)	(0.009)
Postsecondary degree, MA level			-0.069***	-0.074***	-0.070***	-0.082***
			(0.002)	(0.002)	(0.002)	(0.010)
No education registered			0.011*	0.010†	0.008	0.019*
			(0.005)	(0.005)	(0.007)	(0.009)
Parents' earnings (log)			-0.003***	-0.003***	-0.005***	0.000
			(0.000)	(0.000)	(0.001)	(0.001)
Father employment			-0.031***	-0.030***	-0.031***	-0.015†
			(0.002)	(0.002)	(0.002)	(0.008)
Mother employment			-0.012***	-0.012***	-0.010***	-0.017**
			(0.001)	(0.001)	(0.002)	(0.006)
Parents' social welfare			0.095***	0.094***	0.105***	0.051***
			(0.003)	(0.003)	(0.004)	(0.007)
Parents' unemployment			0.023***	0.024***	0.025***	0.003
			(0.002)	(0.002)	(0.002)	(0.007)
Residential stayer			-0.044***	-0.043***	-0.044***	-0.032***
			(0.002)	(0.002)	(0.002)	(0.006)
Intercept	0.105***	0.105***	0.358***	0.433***	0.457***	0.299**
	(0.003)	(0.003)	(0.010)	(0.019)	(0.020)	(0.096)
Country of origin fixed effects	No	No	Yes	Yes	No	Yes
School fixed effects	No	No	No	Yes	Yes	Yes
Moving average controls	No	No	No	No	No	No
R^2	0.006	0.009	0.055	0.050	0.046	0.054
Observations	476352	476352	476352	476352	448332	28020

Note: Linear orobability models estimated using OLS regression. Huber White standard errors in parentheses are robust to within-school clustering and heteroskedasticity All models control for graduation-cohort fixed effects. All models controls for birth cohort fixed effects.

† $P < 0.10$; * $P < 0.05$; ** $P < 0.01$; *** $P < 0.001$ (two-tailed tests)

Table 4. Estimated effect of immigrant student composition on adolescent crime among male students in Norwegian school cohorts using placebo tests and moving average controls

	Natives				Immigrants			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction immigrant-origin students	0.045*	0.053+		0.034	-0.072	-0.120		-0.076
	(0.022)	(0.028)		(0.025)	(0.058)	(0.081)		(0.069)
Fraction immigrant-origin students, t − 1			-0.009	-0.015			-0.087	-0.069
			(0.023)	(0.023)			(0.056)	(0.055)
Fraction immigrant-origin students, t + 1			0.013	0.007			0.076	0.094
			(0.022)	(0.022)			(0.055)	(0.057)
R^2	0.046	0.046	0.046	0.046	0.054	0.061	0.063	0.063
Observations	448,332	448,332	448,332	448,332	28,020	28,020	28,020	28,020
Student and peer covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country of origin fixed effects	No	No	No	No	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Moving average controls	No	Yes	No	No	No	Yes	No	No

Note: Linear orobability models estimated using OLS regression. Huber White standard errors in parentheses are robust to within-school clustering and heteroskedasticity All models control for graduation-cohort fixed effects. All models controls for birth cohort fixed effects.

† $P < 0.10$; * $P < 0.05$; ** $P < 0.01$; *** $P < 0.001$ (two-tailed tests)

Table 5. Subgroup analyses of heterogeneous effects of immigrant student composition on adolescent crime among male students

	Natives	Immigrants
	(1)	(2)
Panel A. Parental education		
Nonacademic families	0.062*	-0.085
	(0.030)	(0.067)
	[271,986]	[20,884]
Academic families	0.021	0.006
	(0.029)	(0.088)
	[176,345]	[7,137]
Panel B. Father's employment status		
Father not in employment	0.121*	-0.143†
	(0.061)	(0.085)
	[45,850]	[15,189]
Father is employed	0.032	-0.053
	(0.022)	(0.063)
	[402,481]	[12,832]
Panel C. Immigrant concentration in school		
Immigrant share at 10% or more	0.054†	-0.099
	(0.031)	(0.064)
	[114,729]	[17,221]
Immigrant share below 10%	0.046	-0.029
	(0.034)	(0.171)
	[333,602]	[10,800]
Panel D. Residential mobility		
Has lived in same neighborbood	0.042†	-0.096
	(0.024)	(0.079)
	[361,160]	[15,767]
Has moved neighborhood	0.054	-0.048
	(0.048)	(0.091)
	[87,171]	[12,254]
School fixed effects	Yes	Yes
Student and peer covariates	Yes	Yes
Country of origin fixed effects	No	Yes

Note: Linear orobability models estimated using OLS regression. Huber White standard errors in parentheses are robust to within-school clustering and heteroskedasticity All models control for graduation-cohort fixed effects. All models controls for birth cohort fixed effects. Number of observations in brackets.

† $P < 0.10$; * $P < 0.05$; ** $P < 0.01$; *** $P < 0.001$ (two-tailed tests)