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Journal of human capital, 01 Dec 2023, Vol. 17, Issue 4, pages 557 - 592

- ISSN: 19328575
- DOI: [10.1086/726628](https://doi.org/10.1086/726628)
- Publisher: UNIVERSITY OF CHICAGO PRESS; The University of Chicago Press
- Language: English
- URL: <https://www.journals.uchicago.edu/doi/10.1086/726628>

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Syrian Refugees and Human Capital Accumulation of Working-Age Native Children in Turkey

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The arrival of Syrian refugees has significantly changed the labor-market conditions and the relative abundance of different skill groups in Turkey. We examine how the arrival of Syrian refugees affects school enrollment and employment of working-age native children using a difference-in-differences instrumental variable methodology. We find a significant drop in employment, largely due to children shifting from work-school balance to education only. School enrollment rises for boys, especially those with educated parents. However, the rate of girls not engaged in employment or education increases, particularly among those with less-educated parents, but decreases for boys with more-educated parents.

I. Introduction

Forced migration from less-developed countries has reached unseen levels. According to the latest United Nations High Commissioner for Refugees (UNHCR) statistics, the number of forcibly displaced people worldwide is 108.4 million, of which about 29.4 million are refugees (UNHCR 2023a). Of these refugees, 76% are hosted in developing countries, and 70% are hosted in neighboring countries (UNHCR 2023b). Turkey has

We thank Yusuf Emre Akgündüz and Andrew Foster for valuable comments and suggestions. Research reported in this publication was supported in part by the Population Studies and Training Center at Brown University through the generosity of the Eunice Kennedy Shriver National Institute of Child Health and Human Development (P2C HD041020). The usual disclaimer holds.

Electronically Published October 17, 2023.

Journal of Human Capital, volume 17, number 4, winter 2023.

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<https://doi.org/10.1086/726628>

been the top host country since 2015 and currently hosts 3.7 million refugees. A vibrant recent literature focuses on quantifying the effects of the refugees on the social and economic outcomes of host countries.¹ Our paper contributes to this literature by studying the impact of Syrian refugees on the human capital accumulation of working-age native children (15–17-year-olds) in Turkey.

The arrival of refugees affects the human capital accumulation of working-age native children in two main ways. First, it affects their employment outcomes. The entry of refugees into the labor market might displace some native children out of employment, as reported for native adults working in the informal sector in Turkey,² and some children could choose to quit because of potentially falling wages and rising returns to education as unskilled labor becomes more abundant.³ In this case, native children would accumulate fewer years of labor-market experience at a given age. Second, the arrival of refugees affects native children's schooling decisions by changing the returns to schooling and the opportunities in the labor market.

The setting of our study provides important substantive advantages to examine this question. While many refugee groups worldwide live in refugee camps and have limited contact with natives, Syrian refugees in Turkey overwhelmingly live in urban areas. In addition, they participate in the labor market in large numbers.⁴ Moreover, because of their demographic differences, the refugees significantly change the age, education, and gender mix of the Turkish labor market.⁵ Therefore, by altering the returns to different skill sets, the refugees generate strong incentives for native children to change their schooling decisions. Our middle-income-country setting is appropriate for studying the refugee impact on enrollment and employment outcomes of working-age children because the school enrollment rate of working-age children is not as high as those rates in developed countries, whereas their employment rate is higher. In 2011, before the refugees' arrival, among 15–17-year-old children,

¹ See, e.g., Maystadt and Verwimp (2014), Alix-Garcia and Bartlett (2015), Calderón-Mejía and Ibáñez (2016), Ruiz and Vargas-Silva (2016), Ceritoglu et al. (2017), Fallah, Krafft, and Wahba (2019), Aksu, Erzan, and Kırdar (2022), and Aracı, Demirci, and Kırdar (2022) for labor-market effects; Alix-Garcia et al. (2018), Akgündüz and Torun (2020), and Altundağ, Bakış, and Rozo (2020) for local production effects; Alix-Garcia, Bartlett, and Saah (2013) for environmental effects; Alix-Garcia and Saah (2010), Balkan and Tumen (2016), and Depetris-Chauvin and Santos (2018) for price effects; Maystadt and Duranton (2019) for effects on road infrastructure; and Aygün, Kırdar, and Tuncay (2021) and Ibáñez, Rozo, and Urbina (2021) for health outcomes.

² See, e.g., Del Carpio and Wagner (2015), Ceritoglu et al. (2017), Aksu, Erzan, and Kırdar (2022), and Aracı, Demirci, and Kırdar (2022).

³ Akgündüz and Torun (2020) find an increase in natives' employment in jobs that require more abstract tasks, suggesting an increase in returns to education.

⁴ On the basis of the 2018 Turkish Demographic and Health Survey, Demirci and Kırdar (2023) calculate that 61.8% of refugee men aged 18–59 have paid jobs compared with 68.9% of native men.

⁵ Aksu, Erzan, and Kırdar (2022) report that refugees are, on average, younger and less educated and have a higher male-to-female ratio.

71.7% of boys and 69.0% of girls were enrolled in school, and 23.6% of boys and 11.2% of girls were employed.⁶ Finally, another important characteristic of our setting is the high share of working-age children who are neither in employment nor in school or training, that is, the not engaged in employment or education (NEET) group. Among the 15–17-year-old children in 2011, 11.5% of boys and 23.2% of girls were in the NEET group. Hence, it is also critical to understand the refugee impact on the size of this group.

Our setting provides critical methodological advantages as well. First, the magnitude of the refugee influx is tremendous, essentially dwarfing other events that could contaminate the estimates in these studies. Second, the influx takes place quickly, reducing the odds of other major events taking place simultaneously with the event of interest. In contrast, many studies quantifying the effects of migrants in developed countries rely on inflows of migrants taking place over longer periods.

To identify the refugee impact on work and school outcomes of native working-age children, we leverage the variation in the ratio of migrants to natives across regions in Turkey using a difference-in-differences instrumental variable (IV) methodology. The rich microlevel data from the 2004–15 Turkish Household Labor Force Surveys provide detailed information on each individual's demographics, schooling, employment, and parental background. Studying the effect of the refugee shock on natives' school enrollment in Turkey by using a difference-in-differences methodology requires paying careful attention to five major issues: (1) endogenous settlement of the refugees across regions, (2) potential preexisting trends in outcome variables, (3) isolation of the effect of the compulsory schooling policy of 2012 from the refugee effect, (4) potential changes in the supply of educational resources, and (5) uncovering of the distributional effects of the migrant shock. Next, we briefly discuss the importance of each of these issues and explain how we address them.

Because the refugees' location of residence choice is likely to be related to the economic conditions across regions, we use a distance-based instrument to account for the endogenous selection of refugees across regions. This instrument depends on (i) the distance between Syrian provinces and Turkish provinces, (ii) the distance between Syrian provinces and the nearest borders in the four neighboring countries, (iii) prewar population shares of Syrian provinces, and (iv) the total number of Syrian refugees in the four neighboring countries. The fundamental assumption for the validity of our instrument is that the time trends in outcome variables across regions with high and low values of the instrument would have been the same, conditional on region and time fixed effects and a set of covariates, in the absence of the refugee shock. However, preexisting time trends in outcome variables across regions could be correlated with the instrument—violating the key assumption for the instrument.

⁶ The source for these data is the 2011 Turkish Household Labor Force Survey.

Therefore, by using placebo tests, we check the validity of the identification assumption for various specifications that allow different forms of preexisting time trends and choose the ones that satisfy the assumption. Furthermore, we conduct panel event-study analysis for all outcomes, allowing us to check the plausibility of our identification assumption.

In 2012, Turkey increased compulsory education from 8 to 12 years, making high school education mandatory—although there was substantial noncompliance with the policy. Because the timing of the educational reform coincides with the refugee influx, it is essential to isolate the migrant impact from the policy impact. Our paper addresses this concern by accounting for the variation across birth cohorts in the policy exposure in our estimating equations. Another important issue is that the arrival of refugees could alter schooling inputs such as the student/classroom and student/teacher ratios. Moreover, the Turkish government could respond to the arrival of refugees by boosting its investments in the schooling infrastructure in the refugee-hosting regions.⁷ Therefore, we also examine how such potential investments and the arrival of refugees alter schooling inputs per student.

Recent research shows that the employment effects of the refugee shock have been heterogeneous across different segments of the Turkish adult population. This is mainly because Turkey has a large informal sector, and the refugees mostly compete with the natives for informal-sector jobs. As a result, the refugee impact on native employment has been negative for the informal-sector workers but positive for the formal-sector workers (Del Carpio and Wagner 2015; Aksu, Erzan, and Kırdar 2022; Aracı, Demirci, and Kırdar 2022). The resulting changes in parental income imply that the refugee effect on school and employment outcomes of working-age children might differ much by the socioeconomic status of their parents. To uncover the distributional effects of the refugee shock, we analyze the effects of the migrant shock separately for different levels of parental education.

We find that while the refugee shock reduces boys' employment, it increases their school enrollment. Quantitatively, a 1 percentage point increase in the migrant/native ratio reduces boys' employment by 0.7 percentage points and increases their enrollment by 0.3 percentage points. For girls, we estimate null policy effects on school enrollment. However, the effect on employment is negative; a 1 percentage point increase in the migrant/native ratio reduces girls' employment by about 0.5 percentage points. The informal sector completely drives the effect on the employment of both boys and girls. The employment effects are larger than those reported for adult natives in the informal sector (see, e.g., Del Carpio and Wagner 2015; Aksu, Erzan, and Kırdar 2022), presumably

⁷ In the context of health infrastructure, Aygün, Kırdar, and Tuncay (2021) find that the Turkish government responded by increasing the number of nurses, pediatricians, and hospital beds in the refugee-hosting regions.

because children are likely to be doing the simplest tasks that newly arrived refugees can easily learn.⁸

These findings are consistent with the arrival of refugees worsening the conditions in the informal labor market and raising the returns to schooling (as formal-sector wages rise in relation to informal-sector wages). The stronger school-enrollment impact for boys is also in line with the larger rise in returns to schooling for men than for women after the arrival of refugees and with the future labor-market returns being more important on average for males because of their much higher rate of participation in the labor force. Our findings regarding the heterogeneity of the refugee effects show that the positive refugee effect on boys' enrollment is stronger for those who have more-educated parents. This finding is consistent with the theoretical expectation that higher earnings in the formal sector for more-educated parents (resulting from the positive impact of refugees on employment and wages there) reduce the marginal utility of their children's earnings and increase the demand for education as a consumption good.

Our analysis of further outcomes, particularly combining work and school and being neither in employment nor in education or training (NEET status), allows us to understand the above employment and enrollment effects better. We find that the arrival of every 10 refugees pushes 3 boys from work to school and 4 boys from combining school and work to school only, whereas it does not increase their NEET incidence. However, an important result is that the arrival of refugees decreases the NEET incidence among boys who have more-educated parents; the arrival of every 10 refugees eliminates the NEET status of 3 boys in these households. On the contrary, the arrival of refugees raises the NEET incidence among girls; the arrival of every 10 refugees pushes 3 girls into NEET status. Moreover, the increase in NEET girls results completely from girls who have less-educated parents. Hence, for the human capital accumulation of working-age children, the arrival of refugees has been relatively more negative for girls than for boys, particularly for girls who have less-privileged backgrounds.

Our paper contributes to the literature about the migrant impact on native children's school enrollment and the migrant impact on native employment. Unlike previous papers, our study examines migrant impact on both school enrollment and employment of children and on the share of NEET children. Most of the studies about the migrant impact on schooling outcomes of young natives have been in the context of developed countries. Only a few recent papers (Assaad et al. 2018; Tumen 2018) examine the impact of refugees on schooling outcomes of young natives in less-developed-country settings. Our paper is closely related to that of Tumen (2018), who studies the effect of Syrian refugees on

⁸ In fact, Aksu, Erzan, and Kırdar (2022) find a stronger displacement effect of the refugee shock on younger workers in the informal sector.

the high school enrollment rate of natives in Turkey. However, our study differs in several important ways—including the outcomes we study, the methodology, and the results. In the next section, we provide a detailed comparison of our paper to that of Tumen (2018), and we also review the related literature.

II. Related Literature

A sizeable literature examines the effects of immigrants on the educational attainment and performance of natives in the context of developed countries. Early studies focus on the effects of immigrants on school enrollment and completion rates of natives. Betts (1998) and Hoxby (1998) find a negative effect of immigrants on the educational attainment of natives in the United States. However, these studies only partially account for the endogenous settlement of immigrants across regions. More recently, McHenry (2015) and Hunt (2017) instrument for immigrants' residential sorting and find an overall positive effect on natives' school attainment. Brunello, Lodigiani, and Rocco (2020) use an IV strategy to show that an increase in immigration of low-skilled labor causes polarization in human capital accumulation in Italy: the shares of natives who have less than high school education or who have college education both increase. Van der Werf (2018) finds precise zero or small positive effects on native children's test scores and educational attainment. Gould, Lavy, and Paserman (2009) study the long-term effects of exposure to immigrant peers on the academic outcomes of Israeli elementary-school children. They find a negative effect on high school matriculation rates that is driven mainly by disadvantaged students. Shih (2017) finds that foreign students increase domestic students' enrollment in US universities by reducing domestic students' cost of attending college. In addition, a related strand of the literature shows that native adults in developed countries respond to immigration by upgrading their skills, changing their occupations, and increasing their school attainment (Orrenius and Zavodny 2015; Gunadi 2018; Llull 2018; Basso, Peri, and Rahman 2020; Ransom and Winters 2021; Bächli and Tsankova 2023).

Few studies examine the effects of refugees on the schooling of native youth in less-developed countries. Assaad et al. (2018) show that the refugee influx does not affect the educational attainment of native youth in Jordan. Tumen (2018) studies the effect of Syrian refugees on the high school enrollment rate of natives in Turkey. Our study differs from that of Tumen (2018) in several ways. First, we analyze school enrollment jointly with labor-market outcomes, which sheds light on the mechanisms for the increased school enrollment among boys. Second, we account for the potential effects of the 2012 compulsory schooling policy. Third, our findings on the distributional effects of the refugee shock are the opposite of those of Tumen. While we find that boys in families that have high parental education increase their school enrollment, Tumen finds that

the effect occurs for boys in families that have low parental education. We argue that our findings are more in line with the findings of the previous literature, which reports more beneficial refugee effects on labor-market outcomes of more-educated parents—implying a drop in the marginal utility of their children’s potential earnings. Fourth, the magnitudes of our estimates are substantially different; Tumen’s IV estimates imply that a 10 percentage point rise in the refugee ratio increases the school enrollment of 15–17-year-old boys by 9 percentage points, whereas we find a 3 percentage point increase. Our analysis also provides important clues as to why our findings are much different from those of Tumen (2018). We provide the details of this analysis in section VII, where we revisit the related literature after presenting our results.

Our paper is also related to the literature about the effect of refugees on the employment rates of young natives. In contexts of low-skilled immigrants, we might expect the labor-market outcomes of young natives to respond more because young natives are nearly substitutes for immigrants. In fact, Smith (2012) finds a negative effect of low-educated immigrants on the employment rate of high school students in the United States. Similarly, Glitz (2012) finds that the employment of young native workers in Germany is more affected by the inflow of ethnic Germans than by the employment of prime-aged workers. In the context of refugees, Olivieri et al. (2022) report that young and low-educated Ecuadorian workers are adversely affected by the arrival of Venezuelan migrants. Even in contexts of higher-skilled immigrants, young natives might be more affected because it is easier not to hire a young person than to fire an existing older worker. In fact, Aydemir and Kırdar (2017) and Cohen-Goldner and Paserman (2006) find a stronger effect of higher-skilled immigrants on young natives. On the other hand, Winter-Ebmer and Zweimüller (1999) find small or no detrimental effect on unemployment of young native workers in the context of immigration in Austria, and Neumark and Shupe (2019) report a minor role of immigration in the decline of teen employment in the United States since 2000.

Finally, our paper relates to the recent literature on the effect of Syrian refugees on labor-market outcomes of natives in Turkey. The findings of this literature agree on certain results but not all. Ceritoglu et al. (2017) report more negative findings of the refugee shock: falling informal employment, increasing unemployment, and falling labor-force participation rate and job-finding rates among natives. At the other extreme, Cengiz and Tekgüç (2022) do not find a negative effect even on informal employment of natives. Del Carpio and Wagner (2015), Aksu, Erzan, and Kırdar (2022), and Aracı, Demirci, and Kırdar (2022) find a negative effect on informal employment but a positive effect on formal employment and zero overall effect on men’s employment but a negative effect on women’s employment. Aksu, Erzan, and Kırdar (2022) also find a positive effect on wages in the formal sector and show that not accounting for pre-existing trends in this variable—as done in the other papers—hides this

fact because the pretreatment formal-wage growth was much higher in the control regions.

III. Background Information

A. *Syrian Refugees in Turkey*

Syria's civil war has displaced millions of Syrians since its inception in March 2011. As a frontline state, Turkey began receiving refugees from Syria in April 2011. The number of Syrian refugees in Turkey increased from about 8,000 in 2011 to more than 2.5 million by the end of 2015. Among all countries in the world, Turkey has hosted the highest number of refugees since 2015. Ferris and Kirişçi (2016) report that most refugees stated that they left Syria for security reasons and chose Turkey as their destination because of the ease of transportation.

The Turkish government gave "temporary protection" status to the Syrian refugees in October 2011. The Turkish Disaster and Emergency Management Authority (TDEMA) was tasked with setting up camps for the refugees near the border. As more and more refugees arrived, the Turkish Directorate General for Migration Management (TDGMM) was established in October 2014 and made responsible for the registration and overall coordination of the refugees. Over time, refugees moved out of the camps and settled in regions of their choice. In fact, according to the TDGMM (2016), only about 10% of Syrians in Turkey lived in refugee camps at the end of 2015. In terms of demographic characteristics, compared with natives, refugees are on average younger and less educated and have a higher male-to-female ratio (Aksu, Erzan, and Kırdar 2022; Demirci and Kırdar 2023).

Before the enactment of Law 8375 by the Turkish government in January 2016, Syrians under temporary protection could have work permits only under certain conditions and with certain restrictions. Only 7,351 work permits were issued to Syrians before 2016, mostly to those who started a business. Hence, most Syrians have been employed in the informal sector—where they have no social security coverage and could be paid below the minimum wage.

B. *Employment and School Enrollment of Working-Age Children in Turkey*

The minimum employment age in Turkey is 15 according to Labor Law 4857 passed in 2003. The Turkish Household Labor Force Survey (THLFS) shows that 11.2% of girls and 23.6% of boys were employed among 15–17-year-olds in 2011, before the refugees' arrival. However, most of these youth were employed in the informal sector; only 16% of girls and 15% of boys were formally employed. Several indicators show that the work attachment of these young workers was high. First, the majority were not agricultural family workers; in fact, 69% of boys and 51% of girls were wage workers. In addition, 68% of girls and 83% of boys

worked full-time, and 62.5% of girls and 71% of boys were employed in permanent jobs (not temporary or seasonal).

All children (including the refugee children under temporary protection) are entitled to free public education in Turkey. Years of compulsory schooling in Turkey was set at 8 years in 1997 and 12 years in 2012. However, imperfect compliance exists—especially with the latter policy passed in 2012. Moreover, the latter policy also allows distance education. According to the 2011 THLFS, before the arrival of Syrian refugees and the 2012 compulsory schooling reform, 70.4% of 15–17-year-olds were in school. According to the 2013 THLFS, after the arrival of Syrian refugees and the 2012 compulsory schooling reform, 77.6% of 15–17-year-olds were in school. Hence, significant noncompliance with the compulsory schooling policy exists. The THLFS allowed a distinction between distance education and in-class education until 2013. According to this, 71.4% of 15–17-year-olds received in-class education in 2013 compared with 66.9% in 2011.⁹

Finally, we discuss refugee children's schooling because their presence could cause overcrowding in schools. The school enrollment rate of Syrian children was low in the early years after their arrival. On the basis of statistics from the Ministry of National Education, Kırdar, Koç, and Dayioğlu (2021) report that the enrollment rate of Syrian refugee children was only 30.4% in the 2014–15 school year.¹⁰ In addition, the high school enrollment rates of Syrian refugee children have been significantly below those at the primary-school and middle-school levels.¹¹ In the early years of the conflict, Syrian children in Turkey were mostly educated in Temporary Education Centers (TECs), which were established specifically for them and followed the curriculum of Syrian schools in the Arabic language of instruction. Starting in the 2014–15 school year, Syrian children could enroll in public schools. However, Kırdar, Koç, and Dayioğlu (2021) report that among the refugee children in school, only 17.4% in the 2014–15 school year and 20.0% in the 2015–16 school year were enrolled in public schools. Therefore, for the period of our analysis (2004–15), we do not expect the enrollment of refugee children at high schools to have an important direct effect on native high-school-age children via competition for resources. However, our empirical analysis checks the potential effect of refugees via this channel.

IV. Data

Our main dataset is the THLFS. These microlevel surveys are published by the Turkish Statistical Institute (TurkStat) annually on a cross-sectional

⁹ Dayioğlu and Kırdar (2022) report much stronger effects of the 1997 compulsory schooling reform on children's schooling outcomes—which also lowered their employment rates, especially among rural children for whom the schooling impact was the strongest.

¹⁰ This improved significantly over time and reached 59.1% in the 2016–17 school year.

¹¹ Even in the 2020–21 school year, only 39% of high-school-age refugee children were in school compared with about 80% of primary-school-age and middle-school-age refugee children.

basis. The surveys are representative of the Turkish permanent resident population and hence exclude the Syrian refugees under temporary protection. We use all rounds of the THLFS from 2004 to 2015 except for 2012 because data for the geographic distribution of Syrian refugees are not available for this year. The reason for starting from the 2004 round is that there was a major restructuring of the survey in this year, and the reason for stopping with the 2015 round is that there was a dramatic minimum-wage hike (about 30%) in January 2016—which could also affect the schooling and employment outcomes of working-age children.

The sample is restricted to 15–17-year-old children for the following reasons. The lowest age is 15 because the THLFS targets the population aged 15 or above, who are eligible for formal employment. The highest age is 17 because the sample's representativeness drops markedly beyond this age, and our focus is on children living with their parents. According to the 2013 Turkish Demographic and Health Survey (TDHS)—which includes a complete roster of all children ever born and information about whether they are living with their parents—we calculate that 96.1% of 15-year-olds, 92.8% of 16-year-olds, and 90.4% of 17-year-olds live with their families, whereas only 83.9% of 18-year-olds live with their families. A reason for the drop at the age of 18 is college enrollment. Another reason is marriage, especially for the female sample.¹²

The dataset includes information about a rich set of labor-market outcomes and educational attainment. The variable we use for schooling is a binary variable for enrollment, which takes the value of one if a child is enrolled in a school and zero otherwise. The variable for employment status is defined as in the original dataset (which is a labor-force survey that follows Eurostat standards). We also generate a dummy variable for the status of combining school and employment and another for the status of being neither in employment nor in education or training (NEET status). In addition, the data include individual-level characteristics such as age, gender, marital status, and region of residence and household-level characteristics such as the household head's educational attainment, age, and marital status.

We combine the microlevel data and provincial numbers on the ratio of Syrians to natives. The numbers of Syrian refugees come from the Disaster and Emergency Management Presidency of Turkey (AFAD) for 2013 and the Ministry of Interior Directorate General of Migration Management of Turkey for 2014 and 2015.¹³ The numbers of Syrian refugees across provinces in Turkey are unavailable for 2012. Because the number of Syrians in Turkey within a given year varies substantially from month to month in the 2013–15 period, we adjust the variation of Syrians across

¹² While 88.9% of 18-year-old women are never married, 97.2% of 15–17-year-old girls are never married.

¹³ AFAD provides the individual numbers only for provinces that have refugee camps but also reports a total of 80,000 refugees in other provinces. Hence, we distribute these 80,000 refugees among non-camp provinces according to their proportional shares in 2014.

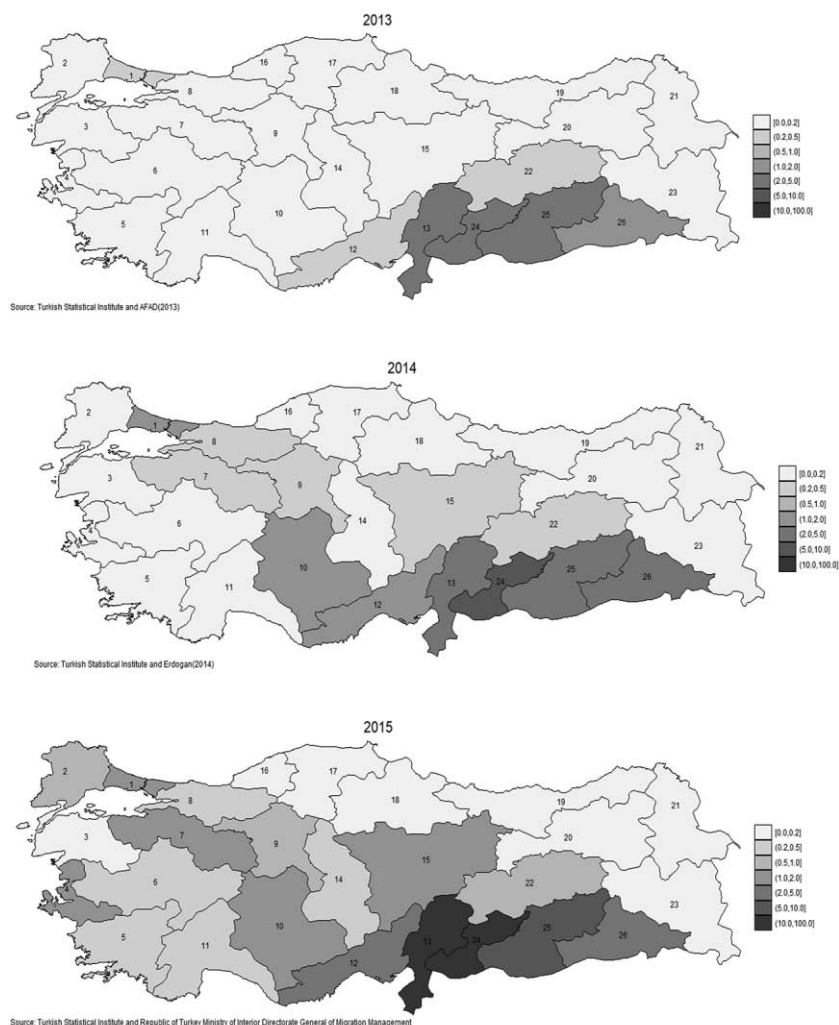


Figure 1.—Ratio of migrants to natives across the 26 NUTS-2 regions, 2013–15. The ratios are multiplied by 100. The number code of each NUTS-2 region is shown on the graph. A color version of this figure is available online.

regions so that it can represent the year average instead of the year end.¹⁴ Using these figures on the number of Syrians and the NUTS-2 region populations obtained from TurkStat, we generate the ratio of Syrian refugees to natives for each Nomenclature of Territorial Units for Statistics,

¹⁴ For each year, we calculate the average value of the monthly numbers of Syrian migrants in Turkey (call this $x(t)$, where t denotes the year) using the time-series data obtained from the UNHCR. Then, we calculate the total number of Syrian migrants in Turkey using the NUTS-2 region data for each year (call this $y(t)$). Finally, we adjust the provincial numbers by multiplying them by $x(t)/y(t)$ to align the sum of regional numbers in each year with the average monthly value for that year.

Level 2 (NUTS-2) region over time. Figure 1 shows the variation in treatment intensity across regions and over time. Table A1 (tables A1–A10 are available online) provides key information on the 26 NUTS-2 regions of Turkey, including the migrant/native ratios for each year.

Panel A in table 1 shows summary statistics for the microlevel data separately for each gender group. The sample consists of individuals aged 15–17. The majority of these working-age children (71% of boys and 63% of girls) are enrolled in school. The percentage of employed children is 22.5% among boys and 10.3% among girls. The majority of employed boys and girls work in the informal sector. The fraction of children who combine work and school is important, which is 6.9% for boys (31% of employed boys) and 2.7% for girls (26% of employed girls). Finally, the fraction of NEET children is remarkable: 13.5% for boys and 29.3% for girls. The compulsory schooling policy affects about 19% of the children in our sample. Panel B of table 1 shows summary statistics for the household heads of the children in our sample. A large fraction of household heads have a primary- or secondary-school degree, which is about 64%. About 16% have no degrees, and about 20% have a high school or university degree.

TABLE 1
SUMMARY STATISTICS

A. Mean Values for Young Individuals			B. Mean Values for Household Heads		
Parameter	Boys	Girls	Parameter	Boys	Girls
School enrolled (%)	71.00	63.16	Age groups (%):		
Employed (%)	22.46	10.28	15–24	.70	.85
Formally employed (%)	3.06	1.47	25–34	2.22	2.55
Informally employed (%)	19.40	8.80	35–44	42.60	41.95
Neither enrolled nor employed (%)	13.45	29.25	45–54	39.84	39.55
Both enrolled and employed (%)	6.91	2.69	55–64	9.69	9.91
Age groups (%):			65+	4.95	5.19
15	33.46	33.21	Married (%)	91.70	91.90
16	33.24	33.44	Education level (%):		
17	33.29	33.35	Illiterate: no degree	15.32	15.87
Married (%)	.99	3.43	Primary or secondary school	64.32	63.95
Education reform (%)	19.13	18.69	High school	13.08	12.92
Relationship (%):			College and above	7.26	7.27
Household head	.13	.08	Employment (%):		
Partner	.00	.69	Formal sector	43.63	42.57
Child	93.28	90.93	Informal sector	27.59	28.35
Child-in-law	.02	1.77	Unemployed	28.78	29.08
Grandchild	4.47	4.40	Gender (%):		
Sibling	.17	.14	Male	89.97	89.45
Other	1.91	1.93	Female	10.03	10.55
Household size	5.49	5.72	Observations	155,500	154,394
Observations	155,500	154,394			

Source.—Data are from the 2004–11 and 2013–15 Turkish Household Labor Force Surveys. Note.—Fractions are rounded to the nearest digit.

V. Identification Method and Estimation

We employ a difference-in-differences design using the variation in treatment intensity across 26 NUTS-2 regions in Turkey. Our baseline specification is

$$y_{ijt} = \alpha + \beta R_{jt} + X'_{ijt}\Pi + F'_{ijt}\Gamma + \tau P_i + \mu_j + \delta_t + \theta_{jt} + \varepsilon_{ijt}, \quad (1)$$

where y_{ijt} indicates school enrollment or employment status of individual i in region j at time t , and R_{jt} is the ratio of Syrian refugees to natives in region j at time t . Our key parameter of interest is β , which shows the effect of increasing the migrant/native ratio from 0 to 1 on the school enrollment rate of natives. For each individual, our baseline specification includes individual- and family-level control variables, denoted X'_{ijt} and F'_{ijt} , respectively. The individual-level controls include the interaction of age and gender, marital status, and relationship with the household head. The family-level control variables include the household size and the household head's age, gender, marital status, and education. Education categories are (i) illiterate, (ii) no degree, (iii) primary school graduate, (iv) secondary school graduate, (v) high school graduate, and (vi) a college degree or above. Household heads' age groups are 15–24, 25–34, 35–44, 45–54, 55–64, and 65 and above. In addition, our baseline specification includes region (μ_j) and year (δ_t) fixed effects. The error term is shown by ε_{ijt} .

Turkey passed an education reform in March 2012, which increased the duration of compulsory schooling from 8 to 12 years. Although this reform made high school attainment mandatory, imperfect compliance by natives reduced its effectiveness. Because the timing of the arrival of the refugees coincides with that of the reform, it is essential to separate the effects of these two events on educational attainment. Therefore, our baseline specification includes a policy dummy, P_i , which is equal to 1 if the reform affects individual i . Specifically, individuals born in 1998 or later are affected by the policy. Moreover, in a robustness check, we interact the policy dummy with parental background variables, as the bite of the policy might differ across groups.

Finally, θ_{jt} in equation (1) stands for region and time interactions. In particular, we use time trends for the 12 Nomenclature of Territorial Units for Statistics, Level 1 (NUTS-1) regions. We employ these interactions to ensure that differential preexisting time trends across regions do not drive our findings. The fact that the pretreatment period in our data (2004–11) is much longer than the posttreatment period (2013–15) helps us identify the time trends separately from the migrant impact.

We estimate equation (1) using two different samples: (i) the full sample and (ii) a sample restricted to regions of high treatment intensity (where the ratio of migrants to natives is higher than 0.05) and low treatment intensity (where the ratio of migrants to natives is lower than 0.002),

excluding regions of medium treatment intensity.¹⁵ The second sample increases the variation in our variable of interest. In addition, if there is heterogeneity in the migrant impact by its treatment intensity, it allows us to estimate the impact only for regions that have a high dose of treatment.

We use alternative ways of calculating the standard errors. In the estimation of equation (1), because our key variable of interest varies at the region-year level, we cluster the standard errors at this level. In addition, to address potential serial correlation across different years for a given region, we cluster the standard errors at the region level in an alternative estimation. Because we have 26 clusters in our sample, we might face the few-clusters problem. Hence, we also conduct wild-cluster bootstrap to calculate *p*-values (Cameron, Gelbach, and Miller 2008).

A. *Exploiting Distance as an Instrument*

The specification in equation (1) assumes that the ratio of migrants to natives is exogenous. This assumption will be violated if the variation in the migrant/native ratio is correlated with the economic conditions across regions or time. This could happen, for instance, if migrants consider the variation in labor-market conditions across regions as they settle in different areas.

Figure 1 shows the regional variation in the refugee/native ratio in Turkey. As can be seen from the figure, a large fraction of the refugees resided in regions close to the Syrian border in 2013. Although they dispersed toward western regions over time, the majority still resided in the border regions in 2015. Aksu, Erzan, and Kırdar (2022), accounting for several covariates, show that distance to the Syrian border is the most critical determinant of refugee settlement patterns. Several reasons exist for the geographic concentration of refugees in the border regions. First, fleeing the war, Syrians arrived in Turkey via the closest border crossing and settled in the neighboring regions. In addition, the camps established by Turkey were in the provinces close to the border. Even though refugees could move to other provinces over time, their initial settlement patterns created some inertia. This inertia was further strengthened by the policy of the Turkish government of providing health and education services to refugees in the province in which they register—although this policy was not strictly enforced. Finally, residing closer to Syria allows refugees to visit their relatives back in Syria on certain occasions, such as religious holidays.

Hence, we employ the distance-based IV used by Aksu, Erzan, and Kırdar (2022),¹⁶ which is an extension of the instrument used by Del Carpio and Wagner (2015):

¹⁵ We have five high-treatment-intensity regions (Adana, Hatay, Gaziantep, Şanlıurfa, and Mardin regions) and eight low-treatment-intensity regions (Balıkesir, Zonguldak, Kastamonu, Samsun, Trabzon, Erzurum, Ağrı, and Van regions).

¹⁶ This instrument has also been used by Aygün, Kırdar, and Tuncay (2021) and Kırdar, Cruz, and Türküm (2022).

$$I_{n,t} = \sum_{s=1}^{13} \frac{(1/d_{s,T})\pi_s}{(1/d_{s,T} + 1/d_{s,L} + 1/d_{s,J} + 1/d_{s,I})} \frac{T_t}{d_{n,s}}, \quad (2)$$

where $d_{s,X}$ for $X = T, L, J, I$ stands for the minimum distance of Syrian province s to Turkey, Lebanon, Jordan, and Iraq, respectively; π_s stands for the prewar population shares of Syrian provinces; T_t denotes the total number of Syrian refugees in the four neighboring countries; and $d_{n,s}$ is the distance of Turkish region n to Syrian province s . In this instrument, the first ratio adjusts the prewar population shares of Syrian provinces according to their distances from the four neighboring countries. For instance, while the prewar population share of the Aleppo province was 21.6%, we would expect the share of Syrian refugees in Turkey originating from Aleppo to be 42.3% with this formulation because Aleppo is much closer to Turkey than the other three neighboring countries. This instrument essentially distributes the total number of Syrian refugees (not just those entering Turkey) first across countries by distance and then within Turkey by the distance of Turkish regions from Syrian provinces. Therefore, this instrument accounts for the potential endogeneity in the size and timing of the refugees entering Turkey.¹⁷

B. Plausibility of Identification Assumption

The fundamental assumption for the validity of our instrument is that the trends in outcome variables across regions with high and low values of the instrument would have been the same, conditional on region and time fixed effects and a set of covariates, in the absence of the refugee shock. To assess the plausibility of this assumption, we conduct a placebo test. Specifically, we estimate our two-stage least squares (2SLS) specification by using data only for the pretreatment period of 2004–11—after transferring the migrant ratios in 2013, 2014, and 2015 to 2009, 2010, and 2011, respectively. If the key identification assumption holds, we would expect no effect of the counterfactual migrants on our outcome variables, conditional on the region and time fixed effects and the set of covariates.

Table 2 presents the results of the placebo tests by gender for the full (panel A) and restricted (panel B) samples. For each case, we report the results for the baseline specification in columns 1 in each panel and for the specification with preexisting time trends at the NUTS-1 region level in columns 2 in each panel. Columns 1 in each panel show that the

¹⁷ The difference between this instrument and the one used by Del Carpio and Wagner (2015) is that it reweights the population share of Syrian provinces (π_s) according to their distance from all neighboring countries and distributes the total number of Syrian refugees in the neighboring countries instead of the refugees only in Turkey. However, Jordan, Lebanon, and Iraq also received substantial numbers of Syrians. In fact, a tiny fraction of Syrians in Turkey originate from the provinces bordering Jordan and Lebanon. Moreover, the size of the refugee population entering Turkey and the time of its arrival could depend on the relative economic conditions and treatment of refugees in these destination countries.

TABLE 2
PLACEBO TESTS

	A. Full Sample				B. Restricted Sample			
	Boys		Girls		Boys		Girls	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Dependent variable:								
School enrollment	-.199 (.173) [.099]** {.079}	-.012 (.179) [.202] {.964}	.456 (.199)** [.312] {.264}	.239 (.227) [.319] {.617}	-.069 (.182) [.138] {.640}	.097 (.194) [.228] {.740}	.230 (.208) [.320] {.533}	.226 (.250) [.324] {.553}
Employment	.304 (.210) [.212] {.176}	-.002 (.269) [.271] {.991}	.299 (.175)* [.150]** {.058}	-.183 (.181) [.160] {.444}	.284 (.200) [.200] {.257}	-.306 (.272) [.300] {.618}	.343 (.180)* [.196]* {.150}	-.190 (.206) [.155] {.483}
Formal employment	-.065 (.033)* [.029]** {.074}	-.008 (.064) [.048] {.931}	.002 (.027) [.035] {.978}	-.044 (.037) [.050] {.582}	-.052 (.036) [.038] {.218}	-.061 (.050) [.053] {.478}	-.022 (.032) [.033] {.579}	-.029 (.036) [.046] {.645}
Informal employment	.369 (.190)* [.196]* {.105}	.006 (.236) [.250] {.998}	.297 (.165)* [.137]** {.042}	-.139 (.177) [.166] {.477}	.336 (.185)* [.194]* {.168}	-.244 (.241) [.269] {.627}	.365 (.186)** [.206]* {.143}	-.161 (.201) [.149] {.425}
NEET	.028 (.122) [.085] {.745}	-.062 (.169) [.242] {.849}	-.758 (.258)*** [.370]** {.011}	-.200 (.247) [.332] {.638}	-.010 (.135) [.100] {.930}	.157 (.181) [.248] {.763}	-.476 (.278)* [.407] {.314}	-.057 (.290) [.385] {.932}

Enrolled and employed	.133 (.093) [.120] {.253}	-.076 (.128) [.172] {.857}	-.003 (.052) [.064] {.961}	-.144 (.049)*** [.073]** {.206}	.205 (.090)** [.110]* {.119}	-.052 (.143) [.208] {.882}	.096 (.061) [.068] {.287}	-.022 (.041) [.058] {.755}
Observations	111,332	111,332	111,901	111,901	52,460	52,460	53,644	53,644
Controls for:								
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS-2 FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS-1 linear trends	No	Yes	No	Yes	No	Yes	No	Yes

Note.—Panel A includes the boys and girls in all 26 NUTS-2 regions, while panel B consists of the boys and girls living in the high-treated or low-treated regions. We define five NUTS-2 regions (Adana, Hatay, Gaziantep, Şanlıurfa, and Mardin regions) as high-treated regions where the migrant/native ratio is higher than 0.05 and eight NUTS-2 regions (Balıkesir, Zonguldak, Kastamonu, Samsun, Trabzon, Erzurum, Ağrı, and Van regions) as low-treated regions where the migrant/native ratio is lower than 0.002. The ratios of Syrians to the native population in 2013, 2014, and 2015 are transferred to the years of 2009, 2010, and 2011, respectively. Each cell reports the coefficient estimate from a separate 2SLS regression of the dependent variable on the migrant/native ratio. All specifications control for individual and household-head characteristics as well as region and year fixed effects (FE). Individual-specific control variables include age, relation to the household head, marital status, and household size. Household-head characteristics include six education-level categories, six age categories, and a dummy variable indicating whether the parents are living together. Specification 2, given in even-numbered columns, includes control variables for NUTS-1 time trends, as indicated, to relax the common-trends assumption. Standard errors clustered at the NUTS-2 $\times$ Year level, NUTS-2 region level, and p -values from the wild-cluster bootstrap are given in parentheses, brackets, and braces, respectively. Fractions are rounded to the nearest significant digit.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

identification assumption is violated in the baseline specification for several key outcomes. Specifically, the post-2012 migration ratios are correlated with the school enrollment and the formal and informal employment of boys when using the full sample and with the enrolled and employed status and informal employment of boys when using the restricted sample. Similarly, for girls, the baseline specification fails the placebo test for school enrollment when using the full sample and for employment, informal employment, and NEET when using either sample. In contrast, the specifications in columns 2 in each panel confirm that controlling for the regional trends ensures the validity of the key identification assumption for both samples.¹⁸ Note that the lack of a statistically significant placebo effect in columns 2 in each panel does not simply result from a lack of precision; the estimated coefficient magnitudes are also much smaller than those in columns 1 in each panel. Hence, our preferred specification is the one with region-specific time trends.

We also use a panel event-study design that allows for dynamic lags and leads to the migrant shock to test the validity of our identification assumption. We do this by interacting the instrumented migrant shock for 2015 with the year dummies while omitting a single year (2011) to capture the baseline difference between areas that receive migrants and areas that do not. We check whether the estimated coefficients in the pre-shock period are small and not statistically different from zero, meaning that the instrument does not predict an effect in the pre-shock period. These are given in section VI because this event-study design also provides the estimates for the post-shock period.

Another identification assumption in our framework is that natives do not give a migration response to the arrival of refugees. Aksu, Erzan, and Kırdar (2022) find that the mobility patterns of natives have not responded significantly to the arrival of refugees in an analysis using the same data period (2004–15) and the same geographic level of analysis (NUTS-2 regions). The only exception to this, they show, is that the in-migration of college graduates to the migrant-dense regions increases. These in-migrants are mostly health and education personnel (Aygün, Kırdar, and Tuncay (2021; Aksu, Erzan, and Kırdar 2022)).

VI. Results

We present our main results on the refugee effect in section VI.A. We then present the results on the heterogeneity in the refugee effect by education of household heads in section VI.B. Section VI.C covers an analysis of the potential effects of the arrival of refugees on the schooling infrastructure. Finally, section VI.D presents the results of several robustness checks.

¹⁸ The only exception is for the enrolled and employed girls in specification 2 (given in even-numbered columns) with the full sample.

A. *Main Results by Gender*

We present the ordinary least squares (OLS) and 2SLS estimates (for the full and restricted samples) of the effects of the migrant shock on outcomes of native boys in table 3 and on outcomes of native girls in table 4. A comparison of the OLS and 2SLS results in tables 3 and 4 shows that the OLS and 2SLS estimates display similar qualitative patterns. However, they significantly disagree on the precision and magnitude of some of the estimates. In what follows, we focus on our 2SLS estimates.

Before discussing our results, we note that the first stage for our instrument is strong in our preferred specifications for both boys and girls in tables 3 and 4, having *F*-statistics much higher than that suggested in the literature. Table 3 indicates that the refugee shock increases the school enrollment rate of boys. The precision of the estimates is higher with the restricted sample. With the full sample, the effect on school enrollment of boys is statistically significant at the 10% level when the standard errors are clustered at the NUTS-2 level; however, the statistical evidence vanishes with wild-cluster bootstrap *p*-values. In contrast, with the restricted sample, the evidence of an effect on school enrollment of boys persists even with the wild-cluster bootstrap. The estimates in table 3 imply that the arrival of every 10 refugees drives 3–4 native boys into school.

Table 3 also shows that the arrival of Syrian refugees has a significant adverse effect on the employment of boys. This negative effect stems from the impact on informal employment. Both the magnitude and the statistical significance of these effects are higher with the restricted sample.¹⁹ Quantitatively, the arrival of every 10 refugees drives about 7 native boys out of employment according to the estimates using the full sample. According to the estimates using the restricted sample, the employment displacement of native boys by refugees is about 1 to 1. Displaced native boys used to work in the informal sector.

Our analysis of the effects on both employed and enrolled children and NEET children allows us to understand the above effects on employment and enrollment. According to the estimates for the full sample in table 3, of the seven native boys who are not employed anymore, about four combined school and work before the arrival of refugees; hence, they remain in school without employment. The other three boys who are not employed anymore choose to enroll in school, as the estimates on school enrollment imply. Hence, the fraction of boys neither in school nor in employment does not change; as shown in table 3, the refugee impact on the fraction of NEET boys is virtually null.

¹⁹ The adverse effect on employment is statistically significant only with the standard errors clustered at the region-year level when the full sample is used. However, it is significant also with the standard errors clustered at the region level when the restricted sample is used. The negative impact on informal employment is statistically significant by either way of clustering the standard error for both the full and restricted samples. Moreover, with the restricted sample, the statistical evidence on informal employment persists even when the wild-cluster bootstrap is used.

TABLE 3
EFFECT OF THE MIGRANT SHOCK ON NATIVE BOYS AGED 15–17

	A. Full Sample			B. Restricted Sample		
	OLS	2SLS	Mean	OLS	2SLS	Mean
Dependent variable:						
School enrollment	.192 (.174) [.197] {.596}	.329 (.173)* [.197]* {.215}	.710	.360 (.178)** [.200]* {.280}	.383 (.168)** [.159]** {.093}	.678
Employment	-.365 (.344) [.475] {.742}	-.724 (.381)* [.555] {.661}	.225	-.750 (.365)** [.505] {.201}	-1.193 (.429)*** [.594]** {.286}	.223
Formal employment	.149 (.091) [.129] {.584}	.073 (.110) [.148] {.743}	.031	.135 (.091) [.147] {.476}	.082 (.111) [.154] {.603}	.015
Informal employment	-.514 (.291)* [.405] {.421}	-.797 (.297)*** [.445]* {.427}	.194	-.885 (.313)*** [.420]* {.180}	-1.275 (.339)*** [.462]*** {.005}	.208
NEET	-.097 (.138) [.167] {.610}	-.061 (.156) [.185] {.773}	.135	.048 (.162) [.184] {.801}	.191 (.182) [.196] {.433}	.159
Enrolled and employed	-.271 (.263) [.351] {.833}	-.455 (.324) [.441] {.911}	.069	-.342 (.282) [.409] {.590}	-.619 (.357)* [.498] {.431}	.061
First stage		.102 [.012]***			.104 [.014]***	
F-statistic		70.254			54.719	
Observations	155,500	155,500		74,400	74,400	

Note.—The sample includes the boys aged 15–17 in the 2004–15 Turkish Household Labor Force Surveys (excluding the 2012 version). Panel A includes the boys in all 26 NUTS-2 regions, while panel B consists of the boys living in the high-treated or low-treated regions. We define five NUTS-2 regions (Adana, Hatay, Gaziantep, Şanlıurfa, and Mardin regions) as high-treated regions where the migrant/native ratio is higher than 0.05 and eight NUTS-2 regions (Balıkesir, Zonguldak, Kastamonu, Samsun, Trabzon, Erzurum, Ağrı, and Van regions) as low-treated regions where the migrant/native ratio is lower than 0.002. Each cell reports the coefficient estimate from a separate 2SLS regression of the dependent variable on the migrant/native ratio. All specifications control for individual and household-head characteristics as well as region and year fixed effects. Individual-specific control variables include age, relation to the household head, marital status, and household size. Household-head characteristics include six education-level categories, six age categories, and a dummy variable indicating whether the parents are living together. Each column includes control variables for NUTS-1 time trends to relax the common-trends assumption. Standard errors clustered at the NUTS-2 $\times$ Year level, NUTS-2 region level, and *p*-values from the wild-cluster bootstrap are given in parentheses, brackets, and braces, respectively. Fractions are rounded to the nearest significant digit.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

TABLE 4
EFFECT OF THE MIGRANT SHOCK ON NATIVE GIRLS AGED 15–17

	A. Full Sample			B. Restricted Sample		
	OLS	2SLS	Mean	OLS	2SLS	Mean
Dependent variable:						
School enrollment	-.368 (.278) [.452] {.553}	-.177 (.218) [.303] {.590}	.632	-.335 (.297) [.502] {.568}	-.154 (.231) [.325] {.670}	.549
Employment	-.271 (.214) [.320] {.580}	-.487 (.206)** [.340] {.256}	.103	-.420 (.254) [.368] {.377}	-.692 (.255)*** [.392]* {.158}	.105
Formal employment	-.009 (.046) [.060] {.926}	-.099 (.051)** [.077] {.280}	.015	.036 (.042) [.057] {.643}	-.017 (.042) [.064] {.839}	.007
Informal employment	-.263 (.190) [.278] {.494}	-.388 (.181)** [.293] {.275}	.088	-.456 (.230)** [.319] {.267}	-.675 (.226)*** [.334]** {.084}	.098
NEET	.396 (.198)** [.324] {.310}	.340 (.177)* [.254] {.222}	.292	.545 (.240)** [.414] {.220}	.540 (.226)** [.311]* {.092}	.367
Enrolled and employed	-.244 (.101)** [.141]* {.198}	-.324 (.119)*** [.165]* {.329}	.067	-.210 (.108)* [.161] {.327}	-.306 (.128)** [.187] {.213}	.021
First stage		.101 [.012]***			.103 [.014]***	
F-statistic		71.109			54.476	
Observations	154,394	154,394		74,923	74,923	

Note.—The sample includes the girls aged 15–17 in the 2004–15 Turkish Household Labor Force Surveys (excluding the 2012 version). Panel A includes the girls in all 26 NUTS-2 regions, while panel B consists of the girls living in the high-treated or low-treated regions. We define five NUTS-2 regions (Adana, Hatay, Gaziantep, Şanlıurfa, and Mardin regions) as high-treated regions where the migrant/native ratio is higher than 0.05 and eight NUTS-2 regions (Balıkesir, Zonguldak, Kastamonu, Samsun, Trabzon, Erzurum, Ağrı, and Van regions) as low-treated regions where the migrant/native ratio is lower than 0.002. Each cell reports the coefficient estimate from a separate 2SLS regression of the dependent variable on the migrant/native ratio. All specifications control for individual and household-head characteristics as well as region and year fixed effects. Individual-specific control variables include age, relation to the household head, marital status, and household size. Household-head characteristics include six education-level categories, six age categories, and a dummy variable indicating whether the parents are living together. Each column includes control variables for NUTS-1 time trends to relax the common-trends assumption. Standard errors clustered at the NUTS-2 $\times$ Year level, NUTS-2 region level, and *p*-values from the wild-cluster bootstrap at the NUTS-2 level are given in parentheses, brackets, and braces, respectively. Fractions are rounded to the nearest significant digit.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 4 presents the results for girls. Unlike the case for boys, there is no evidence of a positive effect of the refugee shock on school enrollment of girls. In fact, the coefficient estimates are negative in both samples. However, we observe evidence of a negative refugee effect on employment of girls, resulting mostly from the displacement in the informal sector, as for employment of boys.²⁰ The magnitude of the employment effect is somewhat smaller than that for boys. The arrival of every 10 refugees displaces 5 girls according to the estimates using the full sample, and the arrival of every 10 refugees displaces 7 girls according to the estimates using the restricted sample. As for boys, almost all displacement occurs in the informal sector.

Table 4 also shows that the refugee shock decreases the fraction of girls both in school and in employment and increases the fraction of girls neither in school nor in employment (NEET).²¹ According to the estimates based on the full sample, for every 10 incoming refugees, about 3 native girls do not combine school and work anymore, and about 3 native girls are pushed into NEET status. According to the estimates based on the restricted sample, for every 10 incoming refugees, about 5 girls are pushed into NEET status. In other words, the refugee impact has a more detrimental effect on the human capital accumulation of girls than of boys.

Next, we examine the dynamic effects of the refugee shock via an event-study design and using the restricted sample. Analyzing the dynamic effects allows us to check the validity of our identification assumption and observe the time trajectory of the effects. Figure 2 shows the effects for boys. For school enrollment, the coefficients in the pre-shock period are much smaller than those in the post-shock period—which is consistent with our findings in table 3. Although the pre-shock effects for particular years are statistically different from zero, they all have the opposite sign (negative), and an upward jump in the estimated effects is visible after 2012. We observe a similar picture for employment and informal employment. Although the pre-shock coefficients for particular years differ from zero, they all have positive signs. In contrast, the post-shock coefficients have negative signs. In addition, the post-shock coefficients are larger in magnitude and highly statistically significant. For enrolled and employed status, the coefficients in the pre-shock period are small and statistically insignificant. The coefficients in the post-shock period gradually become more negative. In fact, the coefficient for 2015 is highly different from zero but not statistically significant at the 5% level. Finally,

²⁰ The statistical evidence for employment exists only with the standard errors clustered at the region-year level in the full sample, whereas it also exists for the standard errors clustered at the region level in the restricted sample. Statistical significance for informal employment is also stronger in the restricted sample; it persists even when the wild-cluster bootstrap is used.

²¹ When the outcome variable is enrolled and employed, statistical significance exists for both ways of calculating the standard errors in the full sample but only for standard errors clustered at the region-year level in the restricted sample. When the outcome variable is NEET status, statistical significance exists with all methods of calculating the standard errors—including the wild-cluster bootstrap—in the restricted sample.

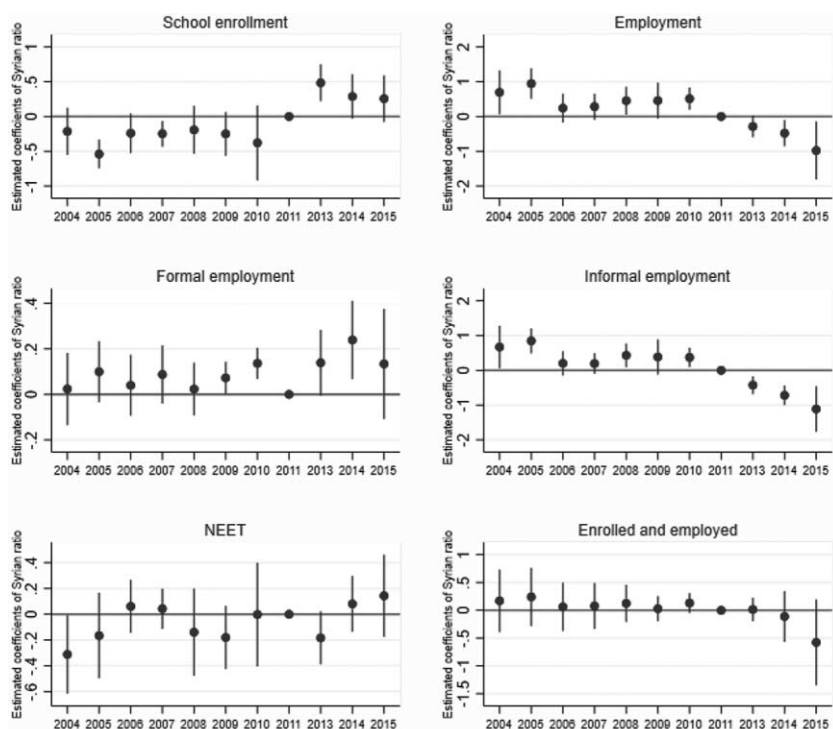


Figure 2.—Dynamic effects for boys in the restricted sample. The sample consists of the boys aged 15–17 and living in either a high- or a low-treatment region in the 2004–15 Turkish Household Labor Force Surveys (excluding the 2012 version). High-treatment regions are five NUTS-2 regions (Adana, Hatay, Gaziantep, Şanlıurfa, and Mardin regions) where the migrant/native ratio is higher than 0.05. Low-treatment regions are eight NUTS-2 regions (Balıkesir, Zonguldak, Kastamonu, Samsun, Trabzon, Erzurum, Ağrı, and Van regions) where the migrant/native ratio is lower than 0.002. The figure plots the estimated coefficients from the leads and lags model of Autor (2003). The vertical bars show the 95% confidence intervals. A color version of this figure is available online.

the formal employment and NEET graphs indicate no evidence of a refugee impact, as in table 3.

Figure 3 displays the event-study results for girls. There is no evidence of a refugee impact on school enrollment of girls; the pre-shock and post-shock coefficients are similar. The results for employment, informal employment, and employed and enrolled status are similar to those for boys. A negative refugee impact exists for all three variables. For the employment variable, while some coefficients in the pre-shock period are statistically different from zero, they are all positive. In contrast, the coefficients for 2014 and 2015 are negative and larger in absolute magnitude. At the same time, the adverse employment effect is less precisely estimated than for boys in figure 2. Nonetheless, the refugee impact on informal employment of girls is sharper. First, the identification assumption is satisfied overall; the coefficients in the pre-shock period

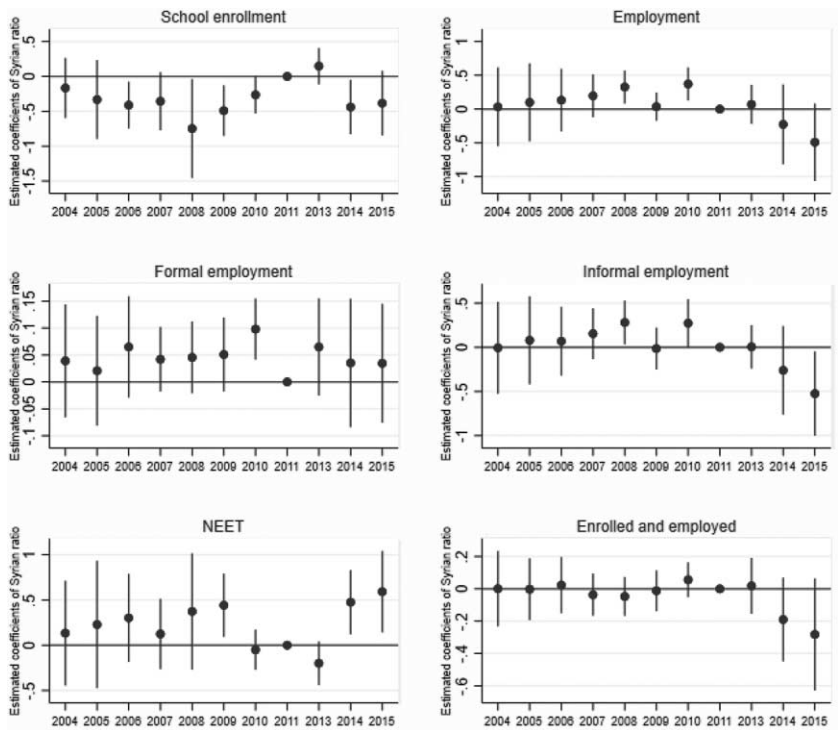


Figure 3.—Dynamic effects for girls in the restricted sample. The sample consists of the girls aged 15–17 and living in either a high- or a low-treatment region in the 2004–15 Turkish Household Labor Force Surveys (excluding the 2012 version). High-treatment regions are five NUTS-2 regions (Adana, Hatay, Gaziantep, Şanlıurfa, and Mardin regions) where the migrant/native ratio is higher than 0.05. Low-treatment regions are eight NUTS-2 regions (Balıkesir, Zonguldak, Kastamonu, Samsun, Trabzon, Erzurum, Ağrı, and Van regions) where the migrant/native ratio is lower than 0.002. The figure plots the estimated coefficients from the leads and lags model of Autor (2003). The vertical bars show the 95% confidence intervals. A color version of this figure is available online.

are small and not statistically significant (except for one that is barely significant). Second, all coefficients in the post-shock period are negative, and the negative coefficient for 2015 is statistically significant. A similar picture exists for employed and enrolled status. The identification assumption is satisfied. Although the post-shock effects are not statistically different from zero, they are negative and much larger in absolute magnitude. Finally, the plot for the NEET status is consistent with the results in table 4. The identification assumption is satisfied overall; it fails only for a single year in the pre-shock period. The coefficients for 2014 and 2015 are positive and statistically significant.

Finally, we discuss our findings on the effects of the compulsory schooling policy. The policy increases school enrollment by 4.7 percentage points for boys and 4.9 percentage points for girls. The effects are stronger for children who have more-educated parents. Among boys (girls),

the policy raises school enrollment by 3.9 (3.6) percentage points for those whose household heads' education is below high school but by 7.5 (9.4) percentage points for those whose household heads have a high school or higher degree. Hence, we also check the robustness of our main findings in tables 3 and 4 by the use of a specification in which we allow the effect of the compulsory schooling policy to change by household-head education and children's age. The results in table A2 show that the inclusion of policy dummy interactions reduces the estimated migrant effect on boys' schooling slightly, from 0.329 to 0.277 with the full sample and from 0.383 to 0.337 with the restricted sample. However, the standard errors also grow, and the migrant effect on boys' schooling loses its statistical significance with the full sample but remains statistically significant with the restricted sample (except for the case of wild-cluster bootstrap). The inclusion of policy dummy interactions also decreases the coefficient of the migrant effect on girls' schooling, but no statistical evidence emerges with either the full sample or the restricted sample. Finally, the migrant effects on all other variables, for both boys and girls, change only slightly in terms of both statistical significance and coefficient magnitudes.

B. Heterogeneous Effects by Parental Background

This subsection analyzes the distributional effects of the refugee shock. To do this, we analyze the refugee effect by the household head's education level and employment status (formally employed, informally employed, and unemployed). As discussed in section II, a critical finding of the previous literature is that the arrival of refugees worsens labor-market outcomes for natives who work in the informal sector but improves them for natives who work in the formal sector. Hence, parental income increases for households that have more-educated parents (who are more likely to work in the formal sector) but decreases for households that have less-educated parents (who are more likely to work in the informal sector).

To understand how the refugee impact on children's schooling varies by parental earnings, appendix B (apps. A and B are available online) sketches a simple model in which a child's time is allocated between school and market work. This model shows that the arrival of refugees causes households that have more-educated parents to increase their children's schooling. However, for households that have less-educated parents, the effect is ambiguous. The fall in children's wages and the rise in returns to school (both resulting from the refugee shock) increase children's schooling. In contrast, the fall in parental income (resulting from the refugee shock)—which raises the marginal utility of children's earnings and lowers the marginal utility of the consumption value of schooling—decreases children's schooling.

Table 5 shows our estimates for boys and girls separately for household heads who have or do not have a high school degree. The results in table 5 are based on the full sample. We provide the corresponding results for

TABLE 5
EFFECT OF THE MIGRANT SHOCK ON NATIVE BOYS AND GIRLS AGED 15–17
BY HOUSEHOLD HEAD'S EDUCATION LEVEL, 2SLS ESTIMATES

	A. Boys				B. Girls			
	Less Than High School	Mean	High School and Above	Mean	Less Than High School	Mean	High School and Above	Mean
Dependent variable:								
School enrollment	.198 (.206) [.227] {.459}	.663	.448 (.168)*** [.129]*** {.031}	.894	-.334 (.261) [.349] {.323}	.573	.114 (.212) [.178] {.640}	.864
Employment	-.731 (.415)* [.615] {.684}	.263	-.696 (.292)** [.329]** {.300}	.074	-.559 (.231)** [.374] {.452}	.121	-.217 (.133) [.197] {.734}	.031
Formal employment	.096 (.116) [.155] {.745}	.035	-.096 (.094) [.097] {.527}	.015	-.092 (.056) [.083] {.476}	.016	-.117 (.057)** [.059]** {.261}	.010
Informal employment	-.827 (.334)** [.506] {.422}	.228	-.600 (.224)*** [.253]** {.295}	.059	-.468 (.206)** [.327] {.447}	.105	-.100 (.121) [.158] {.776}	.021
NEET	.035 (.187) [.219] {.899}	.149	-.288 (.168)* [.102]*** {.060}	.076	.518 (.207)** [.277]* {.169}	.335	-.118 (.201) [.180] {.595}	.124
Enrolled and employed	-.498 (.330) [.450] {.895}	.076	-.536 (.317)* [.404] {.750}	.044	-.376 (.133)*** [.178]** {.363}	.029	-.221 (.092)** [.132]* {.540}	.040
First stage	.102 [.012]***		.098 [.011]***		.101 [.012]***		.098 [.010]***	
F-statistic	68.448		83.753		68.709		87.774	
Observations	123,869		31,631		123,229		31,165	

Note.—Panels A and B include the boys and girls, respectively, aged 15–17 in the 2004–15 Turkish Household Labor Force Surveys (excluding the 2012 version). Each cell reports the coefficient estimate from a separate 2SLS regression of the dependent variable on the migrant/native ratio. All specifications control for individual and household-head characteristics as well as region and year fixed effects. Individual-specific control variables include age, relation to the household head, marital status, and household size. Household-head characteristics include six education-level categories, six age categories, and a dummy variable indicating whether the parents are living together or not. Each column includes control variables for NUTS-1 time trends to relax the common-trends assumption. Standard errors clustered at the NUTS-2 $\times$ Year level, NUTS-2 region level, and *p*-values from the wild-cluster bootstrap at the NUTS-2 level are given in parentheses, brackets, and braces, respectively. Fractions are rounded to the nearest significant digit.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

the restricted sample in table A3. Table 5 provides evidence of a positive refugee effect on the school enrollment of boys who have a more-educated household head. This statistical evidence persists even with the wild-cluster bootstrap. While the refugee effect on the school enrollment of boys who have less-educated household heads is also positive, it is smaller in magnitude and not statistically significant at the conventional levels. In other words, the positive impact we find on boys' enrollment in section VI.A is mostly driven by boys in more-educated households. This finding is consistent with the implications of the simple model discussed above; for boys in households that have more-educated heads, the rise in household income decreases the marginal utility of children's current-period earnings and raises the consumption value of children's schooling.

The results on boys' employment indicate a negative effect for both groups by household head's education level.²² In addition, the decrease in employment for both groups is primarily driven by the informal sector. While the statistical significance is higher for boys who have more-educated household heads, the magnitudes of the estimated negative effects are similar. However, given the lower employment rate of boys who have more-educated household heads, the percentage change is much greater for them. A 1 percentage point increase in the migrant/native ratio lowers the informal employment rate by 3.6% for boys who have less-educated household heads and by 10.2% for boys who have more-educated household heads.

Next, we examine the heterogeneity in treatment effects on the fraction of NEET boys by household head's education. We observe no evidence of a change in the fraction of NEET boys in households that have less-educated heads, which is similar to the finding for the total sample of boys. However, the incidence of NEET falls among boys who have more-educated household heads; about three NEET boys who have more-educated household heads are driven into school. The statistical evidence for this effect is robust; it persists even with the wild-cluster bootstrap.

Table 5 shows no evidence of a refugee effect on native girls' school enrollment regardless of parental education, although the negative coefficient for girls who have less-educated household heads is sizeable. The coefficient estimates are more positive for girls who have more-educated household heads, as for boys. Table 5 also shows that the refugee shock decreases the employment of girls who have less-educated household heads, mostly in the informal sector. However, the statistical significance is not robust to alternative ways of calculating the standard

²² This effect is statistically significant only with the standard errors clustered at the region-year level for the sample of less-educated household heads but statistically significant by either method of calculating the standard errors for the sample of more-educated household heads. However, the statistical significance vanishes when the wild-cluster bootstrap is used.

errors.²³ In contrast, the policy effect on the employment of girls who have more-educated household heads is closer to zero.²⁴

Table 5 also shows that for girls who have more-educated household heads, the negative coefficients for the employment effect mostly stem from the decrease in girls' combining school and work. The estimates provide no statistical evidence of an effect on NEET girls or on girls' school enrollment. However, for girls who have less-educated household heads, the arrival of refugees increases the NEET incidence. The arrival of every 10 refugees pushes about 5 girls who have less-educated household heads into the NEET status. In addition, of the six native girls who are not employed anymore, about four are those who used to combine school and work.

As a robustness check, we examine the heterogeneity of the refugee impact by household head's employment status. Table A4 provides the refugee impact for boys and girls whose household head is (i) informally employed, (ii) formally employed, and (iii) unemployed for both the full sample and the restricted sample. This approach allows a more direct check of the implications of the conceptual framework outlined at the beginning of this subsection because the refugee shock changes the marginal utility of children's earnings via its effect on the household head's employment status. However, this also makes the household head's employment status a potentially endogenous variable—unlike the head's education level. Hence, we use it only as a robustness check. Table A4 shows that boys' school enrollment increases more when their household head is formally employed, which is consistent with our findings by education and our conceptual framework. It also implies that boys' employment drops regardless of the head's employment status, which is also in line with our findings by education. Table A4 also indicates that the effects on employment, informal employment, and NEET are stronger for girls who have household heads in the informal sector. This is consistent with the finding that these effects are also stronger when the head is less educated.

C. Potential Effects on Education Inputs

In this subsection, we examine whether the Turkish government responded to the arrival of refugees by investing in the schooling infrastructure and how the inputs into children's schooling, such as student/teacher and student/classroom ratios, changed. Table 6 shows evidence of an

²³ The statistical significance exists only when the standard errors are clustered at the region-year level. The lack of precision in our estimates is likely due to the low fraction of young working females when the sample is broken down by parental background.

²⁴ Table 5 also shows that the formal-sector employment of girls who have more-educated household heads is adversely affected by the refugee shock; however, it is important to note that the mean of this variable, as reported in the final column of panel B, is very low. At the same time, this result is consistent with that of Aksu, Erzan, and Kırdar (2022), who find adverse effects of the refugee shock on adult women's employment in the formal sector.

TABLE 6
TWO-STAGE LEAST SQUARES ESTIMATES OF THE EFFECT OF THE MIGRANT SHOCK
ON EDUCATIONAL RESOURCES ($N = 286$)

Dependent Variable	2SLS	Dependent Variable	2SLS
No. of schools (log)	1.178 (.583)** [.563]** {.210}	Student/school	−371.165 (115.578)*** [109.435]*** {.026}
No. of divisions (log)	.080 (.515) [.744] {.903}	Student/division	−8.657 (11.992) [11.820] {.645}
No. of teachers (log)	−.993 (.550)* [.788] {.315}	Student/teacher	14.258 (6.013)** [9.450] {.255}

Source.—Data are from the Turkish National Education Statistics Yearbooks.
 Note.—The sample includes observations for 26 NUTS-2 regions for the 2004–15 period excluding 2012. Each cell shows the coefficient estimate from a separate 2SLS regression of the dependent variable on the migrant/native ratio. In each regression, we also control for region fixed effects, year fixed effects, and NUTS-1 linear time trends. Open-education students are not included in the sample. Standard errors clustered at the NUTS-2 $\times$ Year level, NUTS-2 region level, and p -values from the wild-cluster bootstrap at the NUTS-2 level are given in parentheses, brackets, and braces, respectively. Fractions are rounded to the nearest significant digit.
 * Significant at the 10% level.
 ** Significant at the 5% level.
 *** Significant at the 1% level.

investment in school construction by the Turkish government. A 10 percentage point rise in the migrant/native ratio brings about an 11.8% increase in the number of schools. On the other hand, there is no evidence of a change in the number of divisions (which determines the average class size) or the number of teachers.²⁵ When we examine the schooling inputs that would influence schooling quality, we find no evidence of a change in the average class size or the student/teacher ratio. Overall, these results provide no evidence of a significant change in schooling inputs.

D. Further Robustness Checks

In a further robustness check, we use the 1-year lagged values of the refugee/native ratio across regions in case it takes some time for the effects on children’s employment and school enrollment to materialize. Table A5 presents these results. All of our qualitative findings hold. However, quantitatively, the estimated effects are somewhat larger for both boys and girls.
 In another check, we assess the sensitivity of our findings to use of a different time period of data. We restrict the data period to 2009–15, instead

²⁵ The rise in the number of schools while the number of divisions remains relatively constant suggests that the number of divisions was reduced per school.

of 2004–15, so that the duration is the same before and after the refugee shock. Note that we prefer a longer pre-shock period because it allows us to better disentangle the refugee effect from the preexisting time trends. We provide the estimates for the shorter data period in table A6. In this case, the results are similar both qualitatively and quantitatively.

We also check whether a particular region drives the results by leaving one of the four (highly treated) border regions out of the sample each time. Table A7 gives the results for boys and girls. Given the number of different regions and samples we analyze, although our findings do not hold for each outcome and sample pair, they hold for most sample and outcome pairs.

VII. Revisiting the Related Literature

In this section, instead of the region-time interactions, we include the logarithm of year-specific distance from the most populated city in each NUTS-2 region to the closest Syrian border crossing as a control variable. In a highly related study, Tumen (2018) takes this approach to account for the potential direct effects of distance on the outcome variables, as in the study by Del Carpio and Wagner (2015). In their identification strategy, the distance-based instrument depends on the annual stock of immigrants, the distance between the 26 NUTS-2 regions in Turkey and 13 provinces in Syria, and the prewar population shares of Syrian provinces. The additional distance variable uses the interactions of the distance of the NUTS-2 regions to the border with year dummies. Hence, the variation in their instrument comes from the distance between the Syrian provinces and the closest Turkish border crossing. However, most Syrian refugees in Turkey originate from a few provinces in northern Syria.

First, we compare the results of the placebo tests under our specification and Tumen's specification. These results are given for boys (girls) in table A8 (A9). Table A8 shows that Tumen's specification yields estimates that have a significant correlation between the post-2012 migration ratio and boys' pre-2012 school enrollment rate and combining of work and school. Moreover, the coefficient estimates using Tumen's specification are in general much larger in magnitude than those in the placebo checks using our specification in table 2. Similarly, for girls, table A9 shows that Tumen's specification yields coefficients that are much higher than those using our specification and fails the placebo test statistically for enrolled and employed status and formal employment. In essence, the placebo checks show that the key identification assumption fails for several outcomes by using Tumen's specification (2018).

Next, we examine how our main estimates with the main sample compare to those using Tumen's specification. Table A8 (A9) provides these for boys (girls). Use of a direct control for the distance variable, as in the study by Tumen (2018), yields a positive effect on boys' high school

enrollment and a negative effect on their employment, as in our estimates. However, the magnitude of the estimates using Tumen's specification is implausibly high. These estimates imply that for every 10 refugees, about 16 native boys enroll in school, and about 25 are driven out of informal employment. Similarly, table A9 reports large effects on girls' employment using Tumen's specification. We attribute this result to the fact that little variation remains in the distance-based instrument after controlling for the distance of Turkish provinces to the border. In fact, Aksu, Erzan, and Kırdar (2022) also find that use of this specification causes implausibly large estimates in their examination of the refugee impact on the adult native population's employment outcomes.

Tables A8 and A9 also report the heterogeneity in the estimates by parental education. Tumen's specification yields a null effect on enrollment of boys from more-educated households but a large and precisely estimated positive effect on enrollment of boys from less-educated households. In fact, these results are consistent with the findings of Tumen (2018). However, they contradict our finding of a stronger positive impact on boys from more-educated households. As discussed earlier, our finding is consistent with the implications of the conceptual framework outlined in appendix B, where falling household income in less-educated households increases the marginal utility of children's current-period earnings and decreases the consumption value of children's schooling.

Finally, table A10 displays how the results regarding the effects on education inputs change when Tumen's specification is used. In fact, the results are substantially different. Table A10 indicates a fall in the number of schools, divisions, and teachers in the refugee-hosting areas—which is possible only if some schools are closed. Consequently, this specification also provides evidence of a significant deterioration in schooling quality via substantial increases in student/teacher and student/classroom ratios. Essentially, the results in tables A8–A10 highlight the danger of using the interactions of year dummies with distance as an additional control variable when the instrument itself is a highly related distance-based variable.

VIII. Conclusion

This paper examines the effect of a substantial inflow of refugees—that significantly changes the skill mix of the hosting country—on the work and school choices of working-age native children. We find a significant decline in native youth's employment in the informal sector after the arrival of Syrian refugees. Because no change in formal employment occurs, total employment also declines. Although employment displacement takes place for both boys and girls, a rise in school enrollment exists only for boys. The arrival of every 10 refugees drives about 3 boys into school. However, the impact on their employment is much stronger;

for every 10 incoming refugees, 7 employed boys are displaced. The displacement of some native boys could be voluntary—given that their school enrollment also increases. The employment displacement effect for girls is somewhat weaker; the arrival of every 10 refugees displaces 5 employed girls. However, unlike boys, none of these girls are driven into school enrollment. Our analysis also reveals that the arrival of refugees has a null effect on the fraction of NEET boys. In contrast, the fraction of NEET girls rises. The arrival of every 10 refugees drives 3 native girls into NEET status. Hence, the refugee impact on the human capital accumulation of girls is more negative than for boys.

The employment displacement effect we estimate for working-age children is larger than that estimated for adults in the informal labor market by Aksu, Erzan, and Kırdar (2022), who use the same rounds of the THLFS and a similar methodology. The strong employment displacement effects we find for native children partly result from the fact that they can combine school and work, unlike adults, and partly from the fact that children are likely to perform the simplest tasks that newly arrived refugees can easily learn.

The refugee impact on school enrollment of working-age boys is stronger for boys who have more-educated parents. This finding is consistent with the findings of the previous literature, which show that the refugee shock has a positive (negative) impact on the labor-market outcomes of more (less) educated parents. Hence, because of the rise in parental income, the marginal utility of earnings falls for children who have more-educated parents, and the demand for children's education as a consumption good increases for more-educated parents. This makes these children more likely to choose school over work compared with children who have less-educated parents. An interesting finding is that the arrival of refugees decreases the fraction of NEET boys in households that have more-educated heads but increases the fraction of NEET girls in households that have less-educated heads.

Our finding that the arrival of refugees pushes some working-age boys but not girls into schooling is consistent with the recent literature on the refugee impact on native adults' employment and wage outcomes. This literature finds that men's formal employment increases at the expense of informal employment, whereas both formal and informal employment decrease among women (Del Carpio and Wagner 2015; Aksu, Erzan, and Kırdar 2022; Aracı, Demirci, and Kırdar 2022). Moreover, wages in the formal sector rise more for men than for women (Aksu, Erzan, and Kırdar 2022). These imply that returns to education increase more for men than for women. In addition, because the gender gap in labor-market participation is large in Turkey, any increase in returns to schooling would matter more for boys than for girls.

Our results also align with the recent literature showing that forced migration brings both opportunities and risks to the host countries. We find that working-age girls—for whom the incidence of NEET increases—are

vulnerable. Moreover, this rise in the NEET incidence is stronger for girls in less-educated families. These girls are driven out of employment but do not enroll in school. Hence, their human capital accumulation process comes to a halt. Our results suggest that the policies that aim to minimize the adverse effects of forced migration on youth in Turkey should target females, particularly those who have less-educated parents. On the other hand, the arrival of refugees also brings opportunities for working-age boys, for whom school enrollment rises. This is particularly the case for boys in more-educated households, for whom the NEET incidence decreases and school enrollment increases.

Our study faces certain limitations. First, because of the modest number of regions over which our key variable of interest varies, some of our findings are statistically suggestive rather than conclusive. Second, because our data cover the period until 2015, our analysis uncovers refugees' short- to medium-term impact on the human capital accumulation of working-age native children. During this period, most refugee children were in TECs, specially designed for them. Mainstreaming of refugee children started in 2014; however, the fraction of refugee children in public schools was still low until 2015. However, this fraction significantly increased afterward. Currently, all refugee children are in public schools. Hence, the long-term impact of refugees on schooling and employment outcomes of working-age native children might differ.

As the number of refugees worldwide increases, it becomes crucial to understand their effects on native populations. The literature on the impact of migrants on the human capital decisions of young individuals has focused on developed countries—where the gap between the educational attainment of natives and that of migrants is acute. We find that even in a middle-income-country context, where educational differences between natives and migrants exist but are smaller, the migrant shock drives native boys into more education. While an important channel in the developed-country contexts is the impact of refugees via in-class interaction and competition with natives, we show that this channel is much less important in our setting and time period. However, the arrival of refugees in Turkey, as with migrants in developed countries, changes the current labor-market opportunities and the future returns to skills for young native individuals. Moreover, in the Turkish case—as the heterogeneity analysis by parental education indicates—another important channel is the impact refugees impose on parental earnings.

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