A "Win-Win Exercise"? The Effect of Westward Migration on Educational Outcomes of Eastern European Children*

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Nathan I. Hoffmann

Department of Sociology

University of California, Los Angeles

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Abstract

Since the end of the Cold War, millions of migrants from Eastern Europe have sought better opportunities in Western European countries, yet few studies have assessed the impact of such moves on these migrants' children. In the aim of isolating a "treatment effect" of migration on educational outcomes, this study analyzes Programme for International Student Assessment (PISA) scores from 2012, 2015, and 2018 for adolescents born in twelve Eastern European countries and living in eight Western European countries. It employs propensity-score matching within a homeland dissimilation framework, comparing immigrants' outcomes on reading, math, and science assessments to similar stay-at-homes in their countries of origin. In unadjusted comparisons to their counterparts who remained behind, migrant children attain lower scores across all three subjects. Once immigrant children are matched to non-immigrants with similar propensities to migrate, the disparity for math scores disappears, while those for reading and science remain. Disparities are wider for adolescents who come from within the EU, migrate at older ages, or speak a foreign language at home. This paper indicates the need for policymakers and educational administrators to better handle the negative academic effects that migration can have on children from within Europe.

Keywords: Immigration, Education, Europe, Causal Inference, Dissimilation

*Replication code for this paper is available from the author's website, <https://nathanhoffmann.com/>.

Introduction

On October 13, 1999, at the European Parliament in Brussels, President of the European Commission Romano Prodi impassionedly argued for welcoming a dozen new European Union (EU) member states just ten years after they had emerged from behind the Iron Curtain. He highlighted the “immense economic, political and cultural benefits” sure to follow for citizens in both existing and new member states, emphasizing that “[i]t is, in the end, a ‘win-win’ exercise” (European Commission 1999). Since the subsequent enlargements in 2004 and 2007, millions of migrants from Eastern Europe have sought better opportunities in wealthier countries in the West (Kahanec and Zimmermann 2010).¹ For example, in 2020 Romania had over 3 million citizens living in other EU countries, Poland 1.5 million, and Bulgaria 750 thousand, constituting large proportions of these Eastern countries’ small populations (Eurostat 2021). Nearby non-EU countries have also sent many migrants, including over 2 million from Russia (European Commission 2019).

Much scholarly attention has focused on the labor outcomes of these Eastern European migrants in Western Europe (e.g. Galgóczi, Leschke, and Watt 2013; Zaiceva and Zimmermann 2008). Little research, however, has focused on school-age Eastern European migrants in Western Europe. In addition to being of interest to immigrants themselves, outcomes for this “1.5 generation” are important to the long-term economic and social health of countries (Kasinitz et al. 2008), with education, historically a “great equalizer” (Bernardi and Ballarino 2016), standing apart as an especially important measure. Have the economic forces encouraging parents to move West resulted in net educational gains or losses for the children who accompany them?

The dominant scholarly approach to immigrant education stems from the traditions of assimilation and integration (Bashi Treitler 2015; Schneider and Crul 2010): studies commonly take the native-born as the reference group, with smaller gaps between them and immigrants heralded as evidence of less disadvantage or better integration of the latter (Drouhot and Nee 2019; Waters and Jiménez 2005). This work is useful for policymakers occupied with the incorporation of newcomers, but the migrants themselves may have different concerns: has migrating allowed them to secure a

¹The 2004 enlargement involved the eight Central and Eastern European countries of the Czech Republic, Estonia, Latvia, Lithuania, Hungary, Poland, Slovakia and Slovenia as well as Cyprus and Malta. In 2007, Bulgaria and Romania joined as well, followed by Croatia in 2013. The members of the EU before 2004 include Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, Sweden and the United Kingdom.

better life for their children?

If we want to assess educational gains or losses from the perspective of immigrants themselves, we should investigate how their outcomes diverge from their non-migrant counterparts (“stay-at-homes”) in their countries of origin (Zuccotti, Ganzeboom, and Guveli 2017). Jiménez and Fitzgerald (2007) designate this the “homeland dissimilation” approach: whereas assimilation research tracks how immigrants become more similar to the native-born, studies of dissimilation examine the process of immigrants becoming different from those they left behind in their country of origin. FitzGerald’s (2012) “comparativist manifesto” calls for more migration scholars to study dissimilation, but so far its investigation has been rare (for exceptions, see Baykara-Krumme 2015; Guveli et al. 2017).

This paper harnesses estimated propensity scores and matching (Imbens and Rubin 2015; Rosenbaum and Rubin 1983) to estimate the effect of migration from Eastern to Western Europe on children’s educational outcomes.² I analyze data from the Programme for International Student Assessment (PISA), which the Organisation for Economic Co-operation and Development (OECD) uses to assess 15-year-olds’ reading, math, and science skills every three years in dozens of countries worldwide (Schleicher 2019). This paper analyzes scores from 2012, 2015, and 2018 for children born in Albania, Estonia, Poland, Romania, Russia, and the seven Former Yugoslavian countries. Out of a sample of 115,349 Eastern European children, 1,110 live in the Western European countries of Austria, Denmark, Finland, Germany, Luxembourg, the Netherlands, Switzerland, and the United Kingdom, with the remainder residing in their countries of origin.

I find that Eastern European adolescents in the West score lower than their stay-at-home counterparts on reading, math, and science. These disparities shrink when immigrants are matched to similar stay-at-homes, but a negative effect of migration remains for reading and science. However, results are moderated by age at migration and country of origin: students who immigrated at younger ages show no disparities, and those from within the EU tend to show greater disparities compared to counterparts in their countries of origin. Students who do not speak the host country language at home also tend to perform worse. Although many researchers have argued for more direct support for immigrant-origin children in European schools (e.g. Alba, Kasinitz, and Waters

²Xu and Xie (2015) employ propensity score matching to estimate a causal effect of rural-to-urban migration in China on a variety of measures of child well-being. Such a matching approach, however, has been underutilized in studies of international migration.

2011; Arikan, van de Vijver, and Yagmur 2017; OECD 2015), this paper is one of the few to implicate children who migrate from within Europe. Europe may have benefited economically from free movement of labor, but children of Eastern European migrants have not necessarily figured into this “win-win exercise.”

Theoretical Background

To understand the academic achievement of immigrants, quantitative scholars often compare immigrants to local populations (often called “natives”). Such comparisons are motivated by theories of assimilation, which view convergence toward the dominant group as inevitable, or at least desirable (Gordon 1964; Warner and Srole 1945). Recently, more nuanced versions of assimilation theory have switched out the dominant group for a mutable “mainstream” (Alba and Nee 2003) or denied the inevitability of assimilation or necessarily positive effects (Portes and Zhou 1993). European scholars, by contrast, more often focus on integration, homing in on the role of government policy and educational institutions when analyzing immigrant academic achievement. But these integration studies also usually maintain a focus on immigrant-native disparities (Thomson and Crul 2007; Vermeulen 2010), and there is debate whether integration and assimilation approaches are really that different (Schneider and Crul 2010, 1145).

Studies of immigrant selection hint at the pitfalls of comparing immigrant children to “natives,” emphasizing immigrants’ relative standing in their countries of origin (Feliciano 2020). Feliciano (2005, 864) makes a compelling argument in the U.S. context that “the selective nature of their migration [...] helps explain educational attainment differences among immigrants’ children,” and that such differences “can partly be attributed to the reproduction of pre-migration class structures in the United States.” An immigrant group may have a low absolute education level but be more highly educated than compatriots who remained in their country of origin (Ichou 2014); this “contextual attainment” (Feliciano and Lanuza 2017) functions as hidden cultural capital that helps their children succeed more than would be expected based on absolute measures.

Although the selection approach begins to take sending-country context into account, it still often fails to highlight the disruptive and transformational aspects of the migration process (Zúñiga and Giorguli Saucedo 2018; Waldinger 2015). Crossing the border is the defining feature of mi-

gration, yet paradoxically it rarely occupies a central place in educational analyses. Reflecting the heritage of the assimilation and integration traditions, studies of immigrant incorporation usually imply the perspective of the native-born: “How similar are immigrants to us?” Rarely do scholars shift the focus and pose the counterfactual, “How would immigrants’ lives have progressed had they not immigrated?” One approach that places this counterfactual at the center of the analysis is Jiménez and FitzGerald’s (2007) “homeland dissimilation” approach. Whereas studies of assimilation chart how immigrants become more similar to the native-born, the dissimilation approach studies how immigrants diverge from co-national stay-at-homes. In his work on Mexican-U.S. migration, FitzGerald (2008) finds that the choice of comparison group starkly alters conclusions about social mobility: whereas Mexican Americans attain lower levels of education than the American mainstream, compared to Mexicans in Mexico they achieve striking upward mobility. Shifting attention to the effects of migration on immigrants and applying the dissimilation approach in other migration contexts may similarly upend conventional wisdom, and yet the framework has rarely been applied.

In FitzGerald’s application, the dissimilation approach still suffers from two shortcomings. First, a focus on migration between only two countries has difficulty teasing apart the relative importance of sending and receiving countries in the dissimilation process. Second, his applied work does not provide an obvious blueprint for applying his approach to quantitative data. In an extension of FitzGerald’s perspective, I overcome these limitations by comparing flows from multiple sending countries to multiple receiving countries and by integrating the dissimilation approach with the statistical framework of causal inference.

From a causal inference perspective, factors need to be at least hypothetically manipulable in order to make sense as causal (Holland 1986). The potential outcomes framework makes this clear: “The key assumption of the model is that each individual in the population of interest has a potential outcome under each treatment state, even though each individual can be observed in only one treatment state at any point in time” (Morgan and Winship 2015, 4). Although randomly assigning some people to migrate and others to remain would be considered unethical by many, such a scenario is at least conceivable [and has happened to some extent in the past; see Abramitzky, Boustan, and Connor (2020)]. On the other hand, conceiving of a native-born child under the alternative “treatment state” of a migrant, as the assimilation tradition implicitly does, does not

capture an effect of migration. To its credit, the assimilation approach pursues a different aim: assessing the pace of integration. But if we want to estimate an effect of migration, it uses an inappropriate baseline. If the goal is to identify a causal effect of migration, the potential outcomes approach suggests a different comparison group: comparable non-migrants *from the country of origin*. Statistical adjustments then allow us to compare like to like. While the dissimilation approach points us toward the home-country comparison group, the statistical insights of causal inference show us how to rigorously isolate the effect of migration from potential confounders.

Empirical Studies in Europe

An overwhelming number of studies of immigrant education in Europe operate in the assimilation tradition, focusing on gaps between native-born and immigrants, and few such studies consider Eastern European or white children specifically (exceptions exist for the United Kingdom: see Hoffmann 2018; Strand 2014). Numerous OECD analyses of PISA results show that immigrants tend to perform worse than non-immigrants (OECD 2010, 2015, 2018), with the 2018 results showing especially large gaps in Belgium, Denmark, Finland, Germany, Iceland, the Netherlands, Slovenia, and Sweden (Schleicher 2019). Average gaps tend to be about 50 points, or one half of a standard deviation. Pooling results from 2006 to 2015, one report finds mixed results for Eastern European immigrant children when controlling for SES (OECD 2018, 101). For example, Polish pupils in Germany and Austria are somewhat more likely to attain baseline academic proficiency than similar natives. On the other hand, those from Bosnia and Herzegovina, Croatia, and Romania perform worse in Austria, and Albanians in Switzerland are nearly 40 percent less likely to attain proficiency.

Smaller-scale, qualitative studies also tend to emphasize the disadvantage of Eastern European youth in Western European schools. For example, Moskal (2014) finds that Polish migrant children in Scotland have difficulty transferring their family’s social and cultural capital to a new national context. They also face barriers to inclusion in school, with home-school relations and language learning standing out as important factors (Moskal 2016). Similarly, in Norway, Sadownik (2018) shows that lack of social and cultural capital proves a barrier to the full belonging and participation of Polish children in early childhood education and care. In comparative work on Romanian children

in Italy and Spain, Valtolina (2013) argues that, even while many of these children eventually thrive, they also face linguistic difficulties, struggle to adjust to local culture, and lack close Spanish or Italian friends.

These qualitative studies suggest the importance of taking variation by country of origin and immigrant selection into account, and yet few of the quantitative studies discussed above do so. At the individual level, immigrant students are more likely to excel when they come from highly educated, high-income families that speak the host country language at home (OECD 2010, 2015) and when they migrate at younger ages (Hermansen 2017; Lemmermann and Riphahn 2018). The few analyses that do compare test scores of immigrant children to those of country-of-origin stay-at-homes do not focus on Eastern European children, but they note that immigrants tend to perform more similarly to origin-country than destination-country pupils (Feniger and Lefstein 2014), and yet have somewhat lower scores than the latter (Arikan, van de Vijver, and Yagmur 2017; Dronkers and de Heus 2009).

Eastern European parents who migrate are a selected group: younger and better-educated Eastern Europeans are more likely to intend to migrate (Zaiceva and Zimmermann 2008). Kahanec (2013, 141) reports that migrants from the East tend to be “medium-skilled,” with about a quarter having completed higher education, somewhat higher than their home-country averages. Yet even while they benefit from higher salaries and standards of living than stay-at-homes, they often face significant down-skilling and possible “brain waste” upon arrival in the West (Kahanec 2013, 143), and their low labor market status presents barriers to integration (Verwiebe, Wiesböck, and Teitzer 2014). Gregson et al. (2016, 543) discuss how Eastern European migrants are overrepresented in work characterized by the “four Ds: it is dirty, often demeaning, physically demanding and in some cases, dangerous.” And such migrants are often afflicted by anti-immigrant sentiment (Heizmann and Böhnke 2019), especially in the United Kingdom (Fox, Moroşanu, and Szilassy 2012; Rzepnikowska 2019).

Although none of these studies employs a dissimilation approach comparing Eastern Europeans in West and East, their findings suggest that migration may have both positive and negative repercussions for Eastern European immigrants’ education. Children of migrants to the West enjoy higher standards of living and greater parental cultural capital than their stay-at-home counterparts, both factors well established in promoting academic success (Barone 2006; Martins and

Veiga 2010). At the same time, their parents’ precarious labor situation and a less-than-welcoming society are likely to hinder their educational achievement. The struggle of learning a new language, especially for children who migrate at older ages, is also likely to act as a barrier to learning (OECD 2018, chap. 5). This paper’s matching design aims to net out these competing factors, accounting for selection on pre-migration attributes while isolating the effects of migration.

Data and methods

Data

Data for this study come from the pooled 2012, 2015, and 2018 waves of PISA, an achievement test for reading, math, and science that is administered to 15-year-old students in dozens of countries around the world. I select only students born in Eastern Europe, including both those who migrate to Western Europe and those who stay behind (stay-at-homes). For the migrants, I keep only adolescents whose parents were not born in the country of destination; for example, I exclude children born to British parents in Poland who then migrate to the UK. Furthermore, I select only cases that provide specific countries of origin, rather than an aggregate category such as “Eastern European,” and exclude birth countries with less than 10 immigrants to avoid bias related to limited overlap (Morgan and Winship 2015, 148). This leaves 115,349 complete cases, including 1,110 immigrant children and 114,239 stay-at-homes.³ Countries of origin in the final dataset include Albania, Estonia, Poland, Romania, Russia, and those countries that formerly made up Yugoslavia,⁴ and countries of destination include Austria, Denmark, Finland, Germany, Luxembourg, Netherlands, Switzerland, and the United Kingdom. See Table 1 for sample sizes by countries of origin and residence. Immigrant children are not spread evenly throughout the eight destination countries, in part due to migration patterns, in part due to which immigrant groups countries decide to report in PISA. For example, the only specific Eastern European country that the United Kingdom reports is Poland. For mean scores by country for immigrants and stay-at-homes, see Tables S5 and S6 in

³In order to increase computational efficiency, I initially fit propensity score models to subsets of these data that include all immigrants and a random sample of 20,000 stay-at-homes. Blocking and matching use the full dataset.

⁴For many countries in PISA, former Yugoslavian countries are aggregated into a “Former Yugoslavia” category and further disaggregation is not possible. This is the case for 563 out of 621 cases used in the present analysis. For consistency and statistical power, I use “Former Yugoslavia” for most analyses rather than disaggregating the few cases for which this is possible.

the supplementary material.

Table 1: Sample sizes by country of birth and residence.

Country	Albania	Estonia	Former Yu- goslavia	Poland	Romania	Russia
Austria	0	0	118	19	38	16
Switzerland	33	0	226	0	0	0
Germany	0	0	17	43	0	0
Denmark	0	0	40	0	0	0
Finland	0	133	28	0	0	158
Luxembourg	0	0	189	0	0	0
Netherlands	0	0	3	4	0	0
United Kingdom	0	0	0	45	0	0
<i>Total immigrants</i>	<i>33</i>	<i>133</i>	<i>621</i>	<i>111</i>	<i>38</i>	<i>174</i>
<i>Stay-at- homes</i>	<i>9,262</i>	<i>12,645</i>	<i>61,186</i>	<i>12,341</i>	<i>8,501</i>	<i>10,304</i>

Source: OECD PISA data for 2012, 2015, and 2018.

For outcome variables, I pool the data from the three waves; to match the available data for 2012, I use the first five plausible values for reading, math, and science scores. No student answers every question in PISA exams, and hence these values represent the uncertainty from imputation of scores by the OECD. For any estimate except those indicated in the text, I perform the analysis once for each plausible value for a total of five times. I combine estimates and calculating standard errors using “Rubin’s Rules” (Rubin 1987), as suggested by the PISA Technical Report (OECD 2017, 148). The OECD constructs each score to have a global mean of 500 and standard deviation of 100.

I choose eight variables as candidates for matching based on their likely role in immigrant selection. Mother’s education and father’s education are measured by 6-category ISCED, which I treat as continuous. Composite measures of cultural possessions and home educational resources,

along with age, are also continuous variables.⁵ I standardize the composite measures to have a common mean and standard deviation between the three waves. I also consider three categorical variables: birth country, two-category gender, and whether the child was enrolled in one year or less, more than one year, or no early childhood education and care (ECEC). This last covariate acts a proxy for pre-migration socioeconomic status; since some children may enroll in ECEC only after migration, I test the sensitivity of results to the inclusion of this variable. I could not consider test year as a covariate because not every year has both immigrants and stay-at-homes for a given country, and hence the propensity score model would be overfit. For analyses of moderators, I subset by gender, age at migration, home language environment, origin, and destination.

Analytic Strategy

After settling on stay-at-homes as a suitable comparison group, the next step is to choose subgroup of them that accounts for selection processes. A matching approach allows the comparison of individuals with similar characteristics. Matching has a number of benefits over other methods for estimating treatment effects. As Imbens (2015, 382) argues, “regression methods are fundamentally not robust to the substantial differences between treatment and control groups” that usually occur in observational data. Multilevel models are also not appropriate for this analysis, since the small number of countries may result in biased estimates (Maas and Hox (2005)). In contrast to these approaches, matching allows flexible adjustment of imbalanced distributions in observed covariates, without the strong functional form assumptions of these other other methods (Imbens 2015, 377). Under the assumptions discussed below, matching results in quasi-experimental contrasts between “control” and “treated” groups (Morgan and Winship 2015, 167).

In situations with many covariates, exact matching is often impossible. The propensity score overcomes this: matching on only this variable (if correctly estimated) is as good as matching on all observed covariates (Rosenbaum and Rubin 1983). Formally, a propensity score is the probability “that an individual with specific characteristics will be observed in the treatment group” (Morgan

⁵Cultural possessions include indicators for whether pupils have the following in their home: classical literature, books of poetry, and works of art for 2012, and for 2015 and 2018 those as well as musical instruments and books on art, music, or design. For educational resources, respondents indicate whether their homes contain a desk to study at, a quiet place to study, a computer they can use for school work, educational software, books to help with school work, technical reference books, and a dictionary. These indices were scaled using IRT scaling methodology (see OECD 2014, 312).

and Winship 2015, 118), and its use in observational studies has a long history in social science (Hu and Mustillo 2016). I use logistic regression to estimate the propensity score, which in this case is the probability that a child born in Eastern Europe migrates to Western Europe. As in most studies involving propensity-score matching (PSM), my goal is to estimate the (sample) average effect of treatment on the treated (ATT) – in other words, the effect of migration on test scores for those children who migrate.

Identification of a causal effect in PSM relies on two assumptions (Abadie and Imbens 2006). The first is that, for any migrant child, it is possible to match them to a stay-at-home with a similar propensity to migrate. Since there are over 100 times as many stay-at-homes as migrants in my study, it is not difficult to find stay-at-homes with similar characteristics (and hence estimated propensity scores). The second assumption is that, if the values of the covariates selected for the propensity score model are known, then selection into migration is as good as random – that is, that there are no unobserved confounders. This assumption is more difficult to evaluate. In order to test its robustness, I perform a sensitivity analysis to assess how strong an unobserved confounder would need to be to significantly alter my results (Cinelli and Hazlett 2020). I also check the robustness of my estimates against results from coarsened exact matching (CEM) (Iacus, King, and Porro 2012).

I incorporate two strategies to mitigate bias from possible misspecification of the propensity score estimator, drawing on Imbens on Rubin (2015). First, I use an algorithm to select only the linear, quadratic, and interaction terms that improve logistic model’s fit, based on likelihood-ratio tests. Second, I use a subclassification estimator, averaging ATT estimates from multiple “blocks” in which the propensity score varies little. I also rely on heteroskedastic-robust standard errors (“HC1” in R’s **sandwich** package) from bivariate regressions in each block for my final estimate of the ATT.

Variables used in the propensity score model must be “pre-treatment”: they cannot be influenced by migration itself. Hence I do not consider measures of parental income or wealth, since these are likely influenced by migration and hence would bias estimation of migration’s effect on children’s test scores. I do consider parental education, since most studies indicate that migrants who gain additional education abroad are already among the highly educated of their country of origin (Hansen, Lofstrom, and Scott 2001; van Tubergen and van De Werfhorst 2007).

Results

Descriptive Statistics

The goal of matching is to improve the similarity between the migrants and stay-at-homes. To assess balance before matching, Figure 1 plots variables' means along with 5 percent and 95 percent quantiles of each variable for the immigrant and stay-at-home groups in the pooled PISA data. (The supplementary material contains the full pre-matching balance table as Table S1.) Coverage is excellent for most variables already, however some disparities exist. Contrary to expectations, immigrant parents tend to have slightly *less* education than stay-at-homes. They also score lower on the cultural possessions and home educational environment indices. Matching will help correct for these distributional differences.

Figure 1: Covariate balance for pooled PISA data. Points represent means and bars represent the 90% inner quantile of each covariate distribution.

Figure 2 presents unadjusted difference-in-means estimates by country origin, subtracting the average reading, math, and science scores for immigrants from the scores for stay-at-homes. The error bars show 95 percent confidence intervals. The far right of the figure shows the effect sizes

for all countries of origin. On average, immigrant children achieve lower scores than their stay-at-home counterparts in reading, math, and science, on the order of one-tenth of a standard deviation for math and one-fifth of a standard deviation for reading and science. These estimates vary by country of origin, with Estonia showing the greatest disparities and Albania and Romania showing the smallest. For no country of origin do immigrants attain significantly higher scores than their home-country counterparts. These difference-in-means estimates do not take selection into account, however. Matching will help isolate the effect of immigration from other characteristics that might be correlated with moving.

Figure 2: Difference-in-means estimates for PISA scores by country of birth, comparing immigrants and stay-at-homes, with 95% confidence intervals.

Matching

The first step of the causal analysis is to create a model to estimate propensity scores. I use the algorithm from Imbens and Rubin (2015) described above. I choose to include mother's and father's education *a priori* as linear terms. The algorithm picks five more linear terms: age, birth country,

cultural possessions, early childhood education, and home educational environment. Gender is the only candidate term not chosen. The second part of the selection algorithm chooses four quadratic terms and 12 interactions. The resulting model's coefficients are shown in Table S2 in the supplementary material.

The left panel of Figure 3 plots the density curves of linearized propensity scores for immigrants and stay-at-homes (i.e., the log-odds of immigrating for each group). We see that although there is a fair amount of overlap in the scores between the two groups, the tails of the distributions show some lack of coverage that will need to be corrected. In particular, there are few stay-at-homes with high propensities to migrate. A full balance table is included in the supplementary material (Table S3).

Figure 3: Estimated linearized propensity scores for immigrant and stay-at-home groups. The left panel shows the distribution of scores for the full sample of Eastern European children, whereas the right panel includes scores only for the matched sample.

The next step is to try to improve covariate balance through matching to make a trimmed sample. Since I am interested in the average treatment effect on the treated (ATT), I match each

of the 1,110 immigrant children to one stay-at-home with a similar value of linearized propensity score, without replacement. Using this trimmed sample of 2,220 cases, I apply the same algorithm as above to fit a new logistic model for estimating propensity scores. This time, the algorithm chooses only three linear terms (birth country, ECEC, and mother’s education) and one interaction (between birth country and ECEC). The coefficients for this model are in Table S4.

The estimated linearized propensity scores for the trimmed sample are shown in the right panel of Figure 3. Overlap is very good now between the immigrants’ and stay-at-homes’ linearized propensity scores, which clusters closely around 0. This implies that in the trimmed sample, both immigrants and stay-at-homes have about a 50 percent predicted chance of immigrating, based their covariates. Difference-in-means estimates from this trimmed sample can be more readily interpreted as causal.

The Effect of Migration

Table 2: Matched sample blocked by estimated propensity scores. "Min" and "Max P-Score" show the range of estimated propensity scores within each block. The "n" columns give sample sizes by stay-at-home and immigrant groups, and "Average P-Score" gives corresponding estimated propensity scores. "T-Stat" gives the t-statistic for a difference-in-means test between these two averages.

Block	Min P-Score	Max P-Score	n		Average P-Score		T-Stat.
			Stay-at- home	Immigrant	Stay-at- home	Immigrant	
1	0.35	0.49	583	524	0.469	0.471	1.49
2	0.492	0.498	33	16	0.492	0.493	0.31
3	0.498	0.498	75	39	0.498	0.498	0
4	0.5	0.504	32	45	0.5	0.5	0.373
5	0.506	0.508	102	148	0.506	0.506	0.165
6	0.509	0.523	145	142	0.514	0.514	-1.26
7	0.525	0.698	140	196	0.58	0.587	1.46

Source: OECD PISA data for 2012, 2015, and 2018.

I use a subclassification estimator for the ATT. As shown in Table 2, the algorithm groups

Figure 4: ATT estimates for the matched sample, including 95% confidence intervals. “Subclass.” indicates whether a subclassification estimator with seven strata was used. “CEM” shows coarsened exact matching estimates. “OLS, full sample” estimates the ATT using linear regression with eight covariates.

the data into seven blocks. The t-statistic for each block shows no significant difference in average propensity score between immigrants and stay-at-homes. This is good news for unbiased estimation.

Finally, I estimate the ATT for reading, math, and science scores. In Figure 4 I display the final results, with and without subclassification. There is little difference between these estimates; we see that compared to the matched control sample of non-immigrant children, immigrant children tend to perform significantly worse in both reading and science. This disparity is somewhat reduced from the unmatched full sample. Based on the subclassification estimator, for reading the gap decreases from -24 to -16, and for science from -20, -13. In math scores, the initial disparity shown in the raw data completely disappears, reducing from -9 to 0; there is no significant gap for math. This implies that selection was driving much of the observed disparities.

In Figure 4 I also include a robustness check of my estimates by performing coarsened exact matching (CEM) (Iacus, King, and Porro 2012). I use the automatic coarsening algorithm from the `cem` package, which matches 456 of the immigrant respondents to 1,972 stay-at-homes. The CEM results tell a similar story: when immigrants are matched to similar home-country counterparts, they perform somewhat worse in reading and science, but no differently in math. The CEM point estimates are somewhat more negative for reading and science than those from PSM.

Lastly, Figure 4 also contains OLS estimates from linear regression on the full, unmatched sample with the eight possible covariates. In this application, the OLS estimates align with the matching results, though with slightly more negative point estimates.

Alternative Specifications

Moderators

Next I assess possible moderators by repeating the propensity score model fitting and matching procedure for subsamples, presented in Figure 5. Note that any comparisons between subsamples are non-causal (Gerber and Green 2012, chap. 9). “Baseline” is the estimate for “PSM & subclass.” from Figure 4. First, ECEC could be a post-treatment variable for children who migrate before the age of 5, so including it could bias results. I repeat the analysis on a subsample excluding such children, while keeping the same control sample (“Age 5+”). I also try the same analysis with the full sample, but excluding early education as a covariate (“No ECEC”). Results for both of these

specifications are substantively the same as in the baseline model, though the difference in science scores under “No ECEC” is only significant at the 10 percent level. In sum, we can conclude that the ECEC variable does not bias estimation.

Figure 5: ATT estimates for alternative specifications. See text for complete definitions.

I also match within separate samples for those who migrate before the age of 10 and those who migrate at age 10 and older (“Under 10” and “10 and older”). Since the exam is administered at about age 15, this set of analyses compares effects for students who immigrated more than five years before the test date to those who arrived in the past five years. Here the results are significantly different: children who migrate under the age of 10 show no significant difference in reading and science scores, and actually perform slightly better in math than their home-country counterparts. The 373 children who migrate at age 10 or later, on the other hand, face much greater disparities. Drawing on Clogg, Petkova, and Haritou (1995, 1276), I perform a two-tailed z-test for the difference in treatment effects between these two groups. I find significant differences for reading, math, and science effects ($p = 0.000$, $p = 0.001$, and $p = 0.000$, respectively).

How much of these age effects are driven by language proficiency? PISA asks students whether

they speak a language other than the test language at home, and proportions do vary somewhat by age at immigration. For those who migrated under the age of 10, 81 percent speak a language other than the host country language at home, whereas 92 percent of those who migrated at age 10 or later do so. I use two subsets to investigate whether home language moderates the effect of immigration. As expected, immigrant children who speak the host country language at home (“Country lang.”, $n = 172$) attain higher scores than those those who speak an international language at home (“Foreign lang.”, $n = 900$), and these differences are statistically significant for reading and science ($p = 0.022$, $p = 0.081$, and $p = 0.018$ for reading, math, and science, respectively).

Next, I subset the sample by gender. For male children, there are no significant disparities between matched immigrants and stay-at-homes. Disparities for female children, on the other hand, are slightly more pronounced than they are for immigrant children overall. This implies that disparities for female children are driving the main results. However, z-tests find no significant differences for these effects at the $\alpha = 5$ percent level ($p = 0.182$, $p = 0.078$, and $p = 0.102$ for reading, math, and science, respectively).

Country effects

The next alternative specifications subset by countries of origin and destination. Do immigrants from some countries show especially great disparities compared to their stay-at-home counterparts? And do immigrants in certain destination countries appear more disadvantaged?

I first compare migration from EU and non-EU member states. For the main analysis, I had to aggregate former Yugoslavian countries into one category, since this is how these countries are often reported in the data, but here I drop those immigrants without specific country labels and separate the rest into the EU member state of post-2013 Croatia (13) and non-EU pre-2013 Croatia (6), Bosnia and Herzegovina (32), Montenegro (2), and Serbia (5). I group these with the other respective EU and non-EU countries (for totals of 295 and 252 immigrants, respectively) to obtain the “EU” and “Non-EU” estimates in Figure 6. The results show a stark contrast: children who migrate from EU countries attain reading and science scores nearly a half standard deviation below their matched counterparts and math scores one-third of a standard deviation below, but those from non-EU countries show no significant differences from matched stay-at-homes. These differences are significant ($p = 0.004$, $p = 0.002$, and $p = 0.000$ for reading, math, and science, respectively).

Figure 6: ATT estimates by country of origin.

Next, I fit different propensity score models and matching procedures within specific origin countries (Figure 6). The negative differences for immigrant children are most pronounced for those born in Estonia and Poland, compared to home-country counterparts. Math scores for immigrant children born in Former Yugoslavian countries are the only ones significantly higher than those of children who remain behind.

Figure 7: ATT estimates by destination country.

Lastly, I report effects by country of destination, disregarding specific origin country. As shown in Figure 7, immigrant children in Finland attain scores about one-fourth of a standard deviation lower than their home-country counterparts, whereas those in Luxembourg do significantly better. Besides these two countries, causal effect estimates are for the most part not statistically significant, probably due to small sample sizes within individual countries.

Sensitivity Analysis

I next test the robustness of these results to unobserved confounders. Important variables might be correlated with both immigration as well as test scores; for example, highly motivated parents

may be more likely both to migrate as well as to push their children to excel in school, which would bias the effect of migration upward. On the other hand, some families may migrate in order to escape harmful situations, which might adversely affect test scores. This section asks, how strong would an unobserved confounder need to be in order to reduce the effect of immigration on PISA scores to 0 (for reading and science scores) or change an effect to be significant (for math scores)? I use the omitted variable bias (OVB) analysis tools of the **sensemakr** package (Cinelli and Hazlett 2020) to help answer this question.

Figure 8: Contour plot of possible confounders of reading scores. Contour lines represent t-values for the immigration coefficient in an OLS model for reading scores with eight additional covariates and hypothetical levels of confounding. “Unadjusted” shows the t-value of the immigration coefficient with no confounding. “5x mom_ed” shows the t-value of the immigration coefficient after accounting for a hypothetical confounder five times as strong as mother’s education.

This analysis relies on OLS regression models for reading, math, and science. Since the **sensemakr** package cannot handle the standard errors from plausible values, a single regression for the mean plausible value for each score is used. Although these are not identical to the matching estimates (see Figure 4), they tell a similar enough story that sensitivity analysis of the former

can speak to the robustness of the latter. Since mother’s six-point education is one of the more important variables in these models, with a highly significant coefficient of about 7 in each, I use it as a benchmark. I find that an unobserved confounder would need to explain more than 2.2 percent of the residual variance of both immigration and reading scores in order to bring the estimate of immigration down to 0. The equivalent value for science scores is 1.9 percent, while the effect for math scores is already statistically insignificant.

How large is this explained variance relative to the covariates in the model? Figure 8 provides a contour plot for reading scores, showing the effect of a given level of confounding on the t-value of the immigrant coefficient. Each axis shows the partial R^2 association that a hypothetical confounder might have with the treatment (immigration) and outcome (reading scores), and the contours show the resulting t-value for the immigration coefficient. In the OLS model, immigration is highly significant, with a t-value of -7.7. This plot shows that a confounder even five times as strong as mother’s education (`mom_ed`) would only reduce this t-value to -3.7, still significant at the $\alpha = 5$ percent level.

Results are similar for science scores: even a confounder five times as strong as mother’s education would reduce the t-value for immigration only to -2.7. The immigration coefficient in the model for math scores is already insignificant. The OVB tools show that a confounder five times as strong as mother’s education would produce a positive t-value of 2.3. Hence it appears that conclusions from the math score models are the most susceptible to confounding. Although it is not possible to know whether such confounders exist for these data, it seems unlikely that one might be so much stronger than an important variable like mother’s education. We can be fairly confident that the results reported in this paper are robust to moderate levels of confounding.

Discussion and Conclusion

Millions of children in the world today have been swept across international borders by the forces that encouraged their parents to move, yet little research exists evaluating the causal effect of migration on the education of these children. By extending FitzGerald’s “homeland dissimilation” approach (FitzGerald 2012), this paper assesses the effect of migration to Western Europe on the educational outcomes of Eastern European adolescents. On the whole, this study finds a negative

effect on PISA test scores. On average, immigrant children from Eastern Europe residing in Western Europe obtain reading and science scores one-fifth of a standard deviation lower than children in their country of origin and math scores more than one-tenth of a standard deviation lower. When I match immigrant children to similar non-migrant stay-at-homes, these disparities diminish but do not disappear: although math disparities are reduced to zero, immigrant children attain reading and science scores more than one-tenth of a standard deviation below matched counterparts.

These results imply that selection was responsible for much of the gaps in the raw data; parents of immigrant children in this sample are somewhat less educated and have less cultural and educational resources than similar stay-at-homes, and other studies have linked such negative selection to children’s outcomes (Ichou 2014; Feliciano and Lanuza 2017). Yet after matching, disparities remain. Although matching does not account for unobserved differences between migrants and stay-at-homes, sensitivity analysis suggests that the results are robust to confounding. This evidence supports the conclusion that migration leads to worse reading and science scores. It also points to the importance of conceiving of migration at a young age as disruption or dislocation (Zúñiga and Giorguli Saucedo 2018); even if children are moving to a better life, adjustment to a new culture and school can be difficult, especially without adequate institutional support.

This paper represents a departure from the usual assimilation approach to studying immigrant education that takes the local “native-born” as the reference group. Whereas such comparisons are instructive in indicating where the greatest lived disparities occur, they cannot answer another question close to the hearts of many immigrant parents: has immigration been beneficial to their children? By shifting the perspective to the immigrants themselves and processes of dissimilation relative to stay-at-homes, this paper uses a more appropriate method to get at an answer. Although the reading and science gaps exposed here are significant, an assimilation approach comparing Eastern European immigrants and locals suggests much greater disparities (see Figure S1 in the supplementary material). This points to the importance of aligning approach with research question; if the focus is on the effect of migration, the conclusions from assimilation studies may mislead.

The results of this paper contain intriguing heterogeneity. Girls’ scores are stronger drivers of the negative immigration effect in this sample. This is surprising, due to girls’ usual academic success (Buchmann, DiPrete, and McDaniel 2008), and may be due to female stay-at-homes’ relative

success rather than migrants' shortfalls. In addition, children who migrate in early childhood show no significant differences from similar stay-at-homes, but the third of the sample that immigrated at older ages perform substantially worse than matched stay-at-homes on all three subjects. Although the language barrier probably plays some role (OECD 2018, chap. 5), it likely does not completely account for these gaps. Reading shows the greatest disparity, at nearly half a standard deviation. But even math, which relies less on language skills, is one-fifth of a standard deviation lower for more recent migrants than similar stay-at-homes. Although not speaking the host country language at home is associated with lower reading and science scores, recent migrants are only somewhat more likely to fall into this category. Furthermore, schools may bar children without adequate command of the language from taking PISA tests (Dronkers and de Heus 2009, 19). These results strongly suggest that recent migrants struggle in school.

As for conditional effects by country of origin, the negative disparities are most pronounced for intra-EU migration. The lack of negative effects for non-EU immigrants may have three explanations. First, the more selective nature of migration from outside the EU (Razin and Wahba 2015) may not be entirely captured by covariates used for matching, and hence those children from outside the EU may be better equipped with unmeasured educational resources. Second, non-EU European countries already average low PISA scores, so migrants from these countries may experience a "floor effect": there may not be much further such children's test scores can fall. Or, third, due to how the EU defines migrants (Geddes and Scholten 2016), policy aimed at immigrant incorporation may neglect the needs of intra-European migrants. Regarding specific countries of origin, immigrants from Estonia and Poland achieve the lowest compared to matched stay-at-homes. This likely speaks to features of these countries' education systems: in general, children in Poland and Estonia attain higher scores than many Western European countries (OECD 2018).

It is also notable that among the destination countries in this sample, immigrants in Finland show the greatest negative gap from home-country counterparts, while those in Luxembourg achieve the sole positive difference in the sample. These findings may be driven by the compositions of immigrants in samples from these countries: those in Finland are mostly from Russia, and those in Luxembourg are entirely from former Yugoslavian countries. But also, Finland tends to have high overall PISA scores, and Luxembourg low scores. The paradoxical attainment of immigrants in these countries is surprising and merits further study.

Results appear robust for the specific immigrant groups in the countries included in this analysis, but a negative effect of immigration may not extend to other countries. This trade-off of external for internal validity is typical in the tradition of causal inference (Imbens and Rubin 2015, 359). Notably, data are not available for such important destination countries as Italy, Spain, Belgium, and France and the origin country of Ukraine, which limits the generalizability of the findings. Perhaps future waves of PISA will contain more immigration information or more countries, or other internationally comparable tests can fill in the gaps. In addition, although sensitivity analyses suggest that the results are fairly robust to unobserved confounders, future studies may wish to measure possible confounders such as family structure and parental attitudes or motivation.

Despite its historic significance (Favell 2008), migration between EU member states is often not even considered migration, with EU institutions preferring the term “mobility” and reserving “migration” for those who come from outside the EU (Geddes and Scholten 2016, 6). But conceptualizing such long-term movement across borders as migration is perhaps an essential first step toward addressing the disparities that such moves can engender for children in school. These results suggest that Western European countries should focus especially on older migrants and those from within the EU. And although girls tend to surpass boys in school, their greater disparities point to a need for greater support. Finally, even countries with excellent PISA scores overall, such as Finland (Sahlberg 2011), may be letting down immigrants. East-West migration has benefited the economies of countries throughout Europe, but, in order for such movement to be truly a “win-win exercise” for everyone involved, educators and policymakers must provide the resources and guidance to do well in school for the children carried along by these flows.

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No potential competing interest was reported by the authors.

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