

Ruhose, Jens

Research Report

Microeconometric Analyses on Economic Consequences of Selective Migration

ifo Beiträge zur Wirtschaftsforschung, No. 61

Provided in Cooperation with:

Ifo Institute – Leibniz Institute for Economic Research at the University of Munich

Suggested Citation: Ruhose, Jens (2015) : Microeconometric Analyses on Economic Consequences of Selective Migration, ifo Beiträge zur Wirtschaftsforschung, No. 61, ISBN 978-3-95942-000-6, ifo Institut - Leibniz-Institut für Wirtschaftsforschung an der Universität München, München

This Version is available at:

<https://hdl.handle.net/10419/167358>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



61

ifo Beiträge
zur Wirtschaftsforschung

Microeconomic Analyses on Economic Consequences of Selective Migration

Jens Ruhose

ifo Institut

Leibniz-Institut für Wirtschaftsforschung
an der Universität München e.V.

Herausgeber der Reihe: Hans-Werner Sinn
Schriftleitung: Chang Woon Nam

61

ifo Beiträge
zur Wirtschaftsforschung

**Microeconometric Analyses
on Economic Consequences of
Selective Migration**

Jens Ruhose

Bibliografische Information der Deutschen Nationalbibliothek

Die Deutsche Nationalbibliothek verzeichnet diese Publikation
in der Deutschen Nationalbibliografie; detaillierte bibliografische
Daten sind im Internet über
<http://dnb.d-nb.de>
abrufbar

ISBN-13: 978-3-95942-000-6

Alle Rechte, insbesondere das der Übersetzung in fremde Sprachen, vorbehalten.
Ohne ausdrückliche Genehmigung des Verlags ist es auch nicht gestattet, dieses
Buch oder Teile daraus auf photomechanischem Wege (Photokopie, Mikrokopie)
oder auf andere Art zu vervielfältigen.

© ifo Institut, München 2015

Druck: ifo Institut, München

ifo Institut im Internet:
<http://www.cesifo-group.de>

Preface

This volume was prepared by Jens Ruhose while he was working at the Ifo Institute. It was completed in December 2014 and accepted as a doctoral thesis by the Department of Economics at the University of Munich. It includes four self-contained chapters that contribute to the understanding of the performance of migrants in the host society and the impact of migrants on the citizens of these host societies. Given a large increase in the migrant population of many developed countries, it is important to know both how migrants integrate into the host countries and how immigration affects natives.

The introduction in Chapter 1 of the volume summarizes three studies of the author that deal with explanations for migrant selection and the importance of migrant selection to labor force quality. The chapter also contains a discussion of empirical methods in migration economics. Chapter 2 quantifies non-pecuniary migration costs that are incurred due to selective migration along cultural traits. The results give a lower-bound estimate of psychic migration costs, which are economically substantial. Chapter 3 focuses on the school system as a tool for integrating migrants. The chapter shows that comprehensive schooling, that is, the abolition of early between-school tracking based on ability, might not be an appropriate way to foster the integration of migrants, albeit children from the least integrated families are disadvantaged in early tracking systems. Chapters 4 and 5 cover the effect of immigration on the host societies along two dimensions: native income mobility and crime. Chapter 4 looks at the impact of immigration on the intergenerational income mobility of natives. A priori, it is unclear whether immigration should increase or decrease the importance of socioeconomic background for the success of native children. The chapter documents that the presence of migrants mitigates the importance of family background by nudging children to choose occupations that are complementary to the skills and occupations of migrants. Chapter 5 investigates the impact of immigration on crime. The current perception in the literature is that migrants do not substantially increase crime. However, by looking at a group of immigrants with low occupational and language skills, but who were granted immediate citizenship upon arrival, the chapter shows that migrants can have a substantial increasing effect on the crime rate. The chapter also contributes by showing that preexisting regional economic conditions matter in the migrant–crime relationship

Keywords: Immigration, migrants, selection, migration costs, culture, internal migration, psychic cost, educational inequalities, educational tracking, differences-in-differences, intergenerational income mobility, U.S. metropolitan areas, instrumental variables approach, crime, allocation policy.

JEL-No.: D51, F22, I21, I28, J15, J31, J61, J62, K42, O15, R23.

Acknowledgements

This thesis represents the final product of my PhD studies. Numerous people helped me along the way. While an exhaustive listing of all people who supported me in writing this thesis would go well beyond the scope of this page of acknowledgments, I want to take the opportunity to name a few to whom I am particularly thankful.

First and foremost, I would like to thank my supervisor Ludger Wößmann for his constant guidance, advice and support during the last years. I am also grateful to Alexander M. Danzer for being my second supervisor. Furthermore, I thank Gabriel Felbermayr for joining the dissertation committee.

Special thanks are attributed to my coauthors Oliver Falck and Alfred Lameli (Chapter 2), Guido Schwerdt (Chapter 3), and Marc Piopiunik (Chapter 5) for their collaboration, constant help and advice, and patience. I would also like to thank Eric A. Hanushek, Tim Krieger, Nicolai Netz, Matthias Parey, Laura Renner, Fabian Waldinger, and Ludger Wößmann who are coauthors in other projects for their helpful comments, inspiring ideas and continuous encouragements. I am especially indebt to Eric A. Hanushek who invited me to work with him together at Stanford University for half a year; I have greatly benefited from numerous conversations and discussions. I also owe many thanks to my current and former colleagues at the Ifo Center for the Economics of Education for inspiring conversations and helpful suggestions. In particular, I want to thank Francesco Cinnirella, Nadine Fabritz, Oliver Falck, Franziska Kugler, Philipp Lergetporer, Constantin Mang, Marc Piopiunik, Laura Schilz, Martin Schlotter, Ruth Maria Schüler, Guido Schwerdt, Benedikt Siegler, and Simon Wiederhold for their outstanding commitment and exceptional engagement.

I also received support, comments, and suggestions from many other friends and colleagues that unfortunately cannot all be mentioned here. However, special thanks go to Arek Florczak, André Hübner, Kevin Hölscher, and Karin Meier who were awesome flatmates and became best friends.

Last, but definitely not least, I owe my deepest gratitude to my parents, Hannelore and Hubert, my sister, Andrea, and my brothers, Chris and Jörn, for their unconditional love and support throughout my life.

Microeconometric Analyses on Economic Consequences of Selective Migration

Inaugural-Dissertation
zur Erlangung des Grades
Doctor oeconomiae publicae (Dr. oec. publ.)
an der Ludwig-Maximilians-Universität München

2014

vorgelegt von
JENS RUHOSE

Referent: Prof. Dr. Ludger Wößmann
Korreferent: Prof. Dr. Alexander M. Danzer
Promotionsabschlussberatung: 13. Mai 2015

Contents

1	Introduction	1
1.1	Selective Migration	5
1.2	Selective Migration and Economic Development	11
1.3	Causal Inferences in Migration Economics	14
1.4	Outline of the Thesis	17
2	The Cost of Migrating to a Culturally Different Location	23
2.1	Data	26
2.1.1	Historical Dialect Distance between German Counties	26
2.1.2	Internal Migration in Germany	27
2.2	Empirical Strategy	32
2.2.1	Identification	32
2.2.2	Estimation Setup	35
2.3	Results	36
2.3.1	Wage Premium for Overcoming Dialect Distance	36
2.3.2	Robustness Checks	39
2.3.3	Effect Heterogeneity	48
2.3.4	Persistence of Wage Premium	53
2.4	Conclusion	55
	Appendix	56
3	Does Early Educational Tracking Increase Migrant-Native Achievement Gaps? Differences-In-Differences Evidence Across Countries	59
3.1	Data	62
3.1.1	International Large-Scale Student Assessments	62
3.1.2	Defining Early Educational Tracking	63
3.1.3	Analytical Sample	65
3.2	Empirical Strategy	70
3.2.1	Cross-Sectional Model	70
3.2.2	Differences-In-Differences Model	71
3.3	Results	73
3.3.1	Results from the Cross-Sectional Model	73
3.3.2	Results from the Differences-In-Differences Model	78
3.4	Robustness Checks	80
3.4.1	Different Country Samples	80
3.4.2	Different Tracking Definitions	82
3.4.3	Different Survey Matches Definitions	83
3.4.4	Anticipation Effects	84
3.5	Effect Heterogeneity	85

3.6	Conclusion	88
	Appendix	90
4	Immigration and Intergenerational Income Mobility: Evidence from U.S. Metropolitan Areas	95
4.1	Immigration and Income Mobility of U.S. Citizens across the United States	98
4.2	Causal Effect of Immigration on Income Mobility of U.S. Citizens	104
4.2.1	Regression Setup and Baseline Control Variables	104
4.2.2	Conditional Correlation Results	105
4.2.3	Identification Strategy	108
4.2.4	Instrumental Variables Results	112
4.2.5	Robustness Checks	116
4.3	Why Does Immigration Increase Income Mobility of U.S. Citizens?	119
4.3.1	Personal Income and Immigration	120
4.3.2	Mechanism	125
4.3.3	Other Mechanisms	131
4.4	Conclusion	132
	Appendix	134
5	Immigration, Regional Conditions, and Crime: Evidence from an Allocation Policy in Germany	143
5.1	Ethnic German Immigrants and the Allocation Policy	147
5.2	Data	152
5.3	Empirical Model	155
5.4	Results	158
5.4.1	Main Results	158
5.4.2	Robustness Checks	163
5.4.3	Effect Heterogeneity	168
5.5	Conclusion	171
	Appendix	172
6	Summary and Discussion	177
	References	184

List of Tables

2.1	Summary Statistics	30
2.2	Dialect Distance and Post-Migration Wages	37
2.3	Robustness Checks	41
2.4	Adding More/Other Pre-Migration Wages	42
2.5	Placebo Treatment	44
2.6	Non-Linearities in Distance	46
2.7	Alternative Dialect Measures	47
2.8	Effect Heterogeneities	49
2.9	Two-Time Mover Analysis	52
2.10	Persistence of Wage Premium	54
A2.1	Source-County Fixed Effects	57
A2.2	Geographic Distance and Non-Indexed Post-Migration Wages	58
3.1	Tracking Status of Countries	64
3.2	Summary Statistics – Test Scores	68
3.3	Summary Statistics – Demographics and Socioeconomic Background	69
3.4	OLS Estimation – Secondary School	74
3.5	OLS Estimation – Primary School	77
3.6	Differences-In-Differences Estimation – Baseline Results	79
3.7	Robustness Checks	81
3.8	The Role of Language Spoken at Home	87
3.9	Subgroup Analysis	88
A3.1	Migrant Achievement Gaps in Primary School in TIMSS 1995	90
A3.2	Country List by Matched Survey Waves	91
A3.3	OLS Estimation – Secondary School by Survey	92
A3.4	Piecewise Deletion of Countries	93
4.1	Summary Statistics	102
4.2	Immigration and Intergenerational Income Mobility	106
4.3	National Birthplace Shares 1930 – 2010	109
4.4	Baseline IV Results	114
4.5	Robustness: Further Controls	117
4.6	Robustness: Different Specifications	118
4.7	Income, Education, and Immigration	122
4.8	Immigration and Occupational Employment Shares	127
4.9	Occupational and Educational Adjustments as Mechanisms	130
A4.1	Data Sources of Baseline Analysis	135
A4.2	Data Sources of Further Controls	136
A4.3	Further Summary Statistics	137
A4.4	Occupational Employment Shares	138

A4.5	Robustness: Testing Personality Traits and Attitudes	139
A4.6	Potential Other Mechanisms	140
A4.7	Other Outcomes: Family Income	141
A4.8	Other Outcomes: School Resources	142
5.1	Summary Statistics	153
5.2	Effect of Ethnic German Immigrants on Total Crime	159
5.3	Effect of Ethnic German Immigrants on Crime Subcategories	162
5.4	Testing for the Effect of Lagged Crime Rates on Current Ethnic German Inflow Rates	164
5.5	Alternative Model Specifications	167
5.6	Effect Heterogeneity	169
A5.1	Determinants of Ethnic German Immigrant Inflows into Counties	173
A5.2	Analysis Sample	173
A5.3	Effect of Ethnic German Immigrants on Total Crime with County-Specific Party Majorities	174
A5.4	Including Spatially Lagged Crime Rates	175

List of Figures

1.1	Stock of Migrants as Share of Total Population by World Region, 1990–2013	2
1.2	Emigration Odds for Primary-Educated and Tertiary-Educated Individuals by Source Country, 2000	6
1.3	Predicted Earnings of Migrants and Non-Migrants	9
1.4	Genetic Distance and Migrant Selection, 2000	10
1.5	Development Accounting Results Across U.S. States, 2007	13
2.1	Dialect Distance – The Case of Worms	28
2.2	Added-Variable Plot of Dialect Distance and the Post-Migration Wage	40
2.3	Sample of Two-Time Movers	51
A2.1	Geographic Distance and Dialect Distance	56
3.1	Average Migrant-Native Achievement Gaps in Primary and Secondary School	67
3.2	Matching of Survey Waves	73
4.1	Mean Child Income Rank by Parent Income Rank	100
4.2	Intergenerational Rank-Rank Slope Across U.S. Metropolitan Areas	101
4.3	Share of Foreign-Born Across U.S. Metropolitan Areas	103
4.4	Predicted and Observed Share of Foreign-Born in 1980	111
A4.1	Predicted and Observed Share of Foreign-Born in 1980, including Outliers	134
5.1	Annual Inflow of Ethnic German Immigrants to Germany	148
A5.1	Crime Trends by State	172

Chapter 1

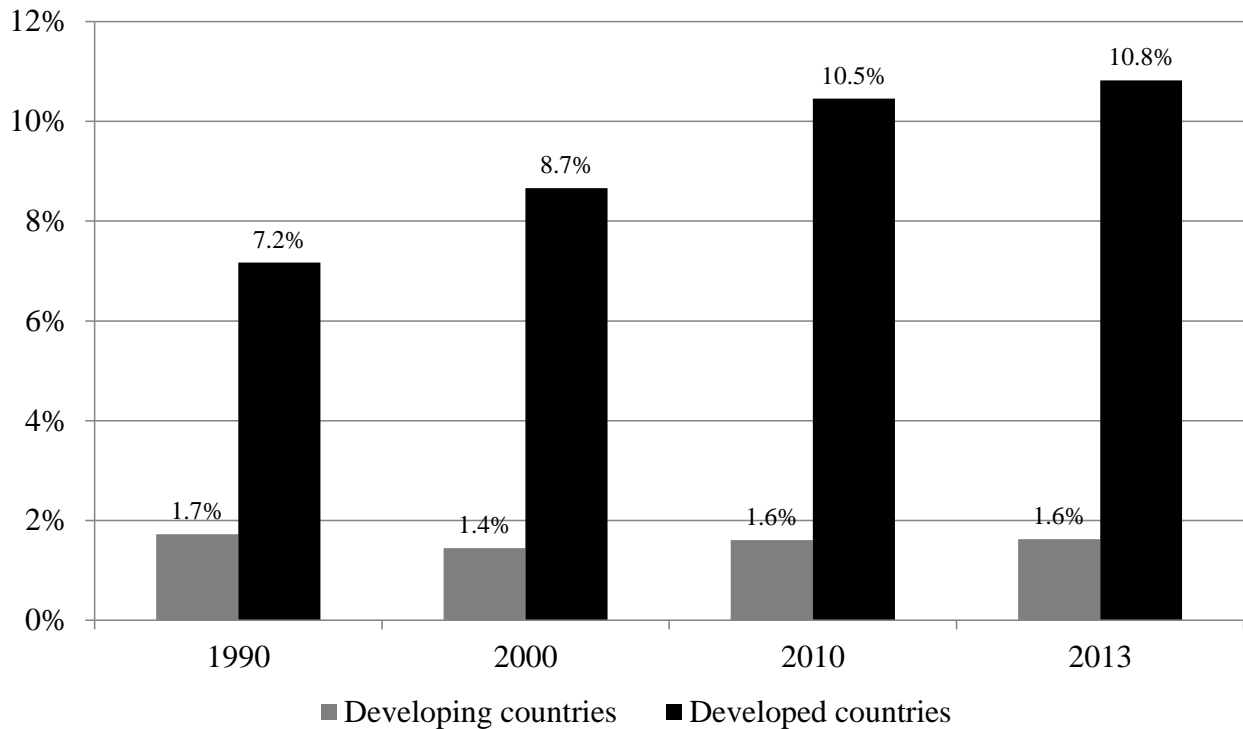
Introduction

Migrants make up a sizeable fraction of the population of developed countries. Figure 1.1 shows the distribution of first-generation migrants in the developing and developed world. The share of migrants out of the total population in developed countries increased from 7.2 percent in 1990 to 10.8 percent in 2013, an increase of 50 percent in only two decades. Moreover, this statistic does not take into account people who were born in the country in which they live or have citizenship there, but have at least one parent who was born abroad (second-generation migrants). German Census data show that 35 percent of children below age five have a migration background (Ruhose, 2013*a*). In the United States, 32 percent of the children below age five had a migration background in 2010 (own calculations based on Ruggles et al. (2010)). The aggregated figures, however, mask a very uneven distribution of migrants across regions within countries because migrants are more likely to settle in agglomerations (Bartel, 1989). For example, U.S. cities such as Miami (76 percent), Los Angeles (64 percent), San Francisco (57 percent), and New York (53 percent) have substantially higher shares of children below age five with a migration background than do other regions of the country (own calculations based on Ruggles et al. (2010)). The same situation prevails in Germany, that is, migrants tend to cluster in the bigger cities (Ruhose, 2013*a*).

Given these large migrant populations in many developed countries, it is important to know both how migrants integrate into the host countries and the effects of immigration on natives. A fair amount of research into these issues has been conducted¹, including work on the labor market

¹ See Borjas (1994), Friedberg and Hunt (1995), LaLonde and Topel (1997), Borjas (1999), Card (2009), Dustmann, Glitz and Frattini (2008), and Dustmann and Glitz (2011) for overview articles.

Figure 1.1
Stock of Migrants as Share of Total Population by World Region, 1990–2013



Notes: The figure shows first-generation migrants as a percentage of the current population by world region. Depending on the data source, numbers include only foreign-born migrants or foreign citizens. Data come from United Nations, Department of Economic and Social Affairs, Population Division (2013). *Developing countries:* Africa, Asia (except Japan), Latin America and the Caribbean plus Melanesia, Micronesia, and Polynesia. *Developed countries:* Europe, Northern America, Australia/New Zealand, and Japan.

effects on natives of immigration² and investigations into how migrants perform in the labor market compared to natives³.

Comparing labor market outcomes, this literature finds that migrants in developed countries perform worse than natives, even after several years of residence. There are three major reasons for this finding. First, migrants are disadvantaged because, typically, the host country's language is not their mother tongue and research shows that language skills are key to integration and success in the labor market.⁴ Second, most migrant groups are less educated and therefore work in less rewarded occupations than natives.⁵ Thus, much of migrants' lack of labor market integration

² See, e.g., Card (1990, 2005); Pischke and Velling (1997); Friedberg (2001); Borjas (2003); D'Amuri, Ottaviano and Peri (2010); Glitz (2012); Dustmann, Frattini and Preston (2013).

³ See, e.g., Chiswick (1978); Borjas (1985, 1993); LaLonde and Topel (1991); Orcutt and Dowhan (2002); Card (2005); Lubotsky (2007); Algan et al. (2010); Kim (2013).

⁴ See, e.g., Chiswick and Miller (1995); Bleakley and Chin (2004, 2010); Dustmann, Machin and Schoenberg (2010).

⁵ Recent studies show that newly arriving immigrants suffer also from occupational downgrading upon arrival, meaning that they are working in occupations that do not match their formal qualifications (e.g., Eckstein and Weiss, 2004; Dustmann and Preston, 2012; Dustmann, Frattini and Preston, 2013).

can be explained by their socioeconomic background.⁶ The unfavorable labor market position of migrant parents also partly explains why children with a migration background have lower education levels than native children in almost all European countries.⁷ The third main reason for migrants' relatively poor performance, albeit one not covered in this thesis, is discrimination against migrants in school (Dee, 2005; Van Ewijk, 2011; Sprietsma, 2013) and in the labor market (Bertrand and Mullainathan, 2004; Carlsson and Rooth, 2007; Kaas and Manger, 2011).

Institutional arrangements, such as the school system, can facilitate (or hinder) the educational integration of migrants (Hao and Pong, 2008). Chapter 3 contributes to the literature on the first and second argument from above by examining the effect on migrant children of one type of institutional arrangement—early educational tracking. Early educational tracking involves sorting children into different school tracks according to their (perceived) ability relatively early in their school careers. This institutional arrangement is particularly interesting because it has been shown to generally increase educational inequality (e.g. Hanushek and Woessmann, 2006). In a cross-country analysis, we do not find that early tracking affects migrants more than natives, but we do find substantial negative effects from early tracking for children from less integrated families, where degree of integration is measured by the language spoken at home.

As mentioned above, economics of migration research is not only concerned with migrants' performance, but is also interested in the effects of migrants on natives, with a chief focus on labor market effects. A widespread public fear is that a larger labor supply will decrease wages and/or increase unemployment of natives (Scheve and Slaughter, 2001). This fear is legitimate when labor markets are perfectly competitive and workers are perfectly substitutable (Borjas, 2013; Cahuc, 2014). However, the evidence strongly suggests that natives and migrants are not perfect substitutes (Ottaviano and Peri, 2012; Manacorda, Manning and Wadsworth, 2012) and that natives (on average) benefit from migrants through gains in productivity due to occupational specialization made possible by comparative skill advantages of migrants and natives (Ottaviano and Peri, 2006; Peri and Sparber, 2009, 2011*b*; Peri, 2012). Chapter 4 contributes to this strand of the literature by not only looking at the effects of immigration on the income of natives at different ages, but also by analyzing the effects of immigration on the intergenerational income mobility of natives, that is, the correlation between parental and child income. When looking at various metropolitan areas in the United States, I find that the intergenerational income mobility of U.S. citizens is higher in

⁶ See, e.g., Borjas and Bronars (1991); Heath, Rethon and Kilpi (2008); Kristen and Granato (2007); Levels and Dronkers (2008); Ruhose (2013*b*).

⁷ See, e.g., Nielsen et al. (2003); Van Ours and Veenman (2003); Bauer and Riphahn (2007); Schnepf (2007); Schneeweis (2011); Dustmann, Frattini and Lanza (2012).

those areas home to more migrants. This finding is fully explained by immigration-induced changes in occupational structure, which leads to a productivity enhancing specialization of natives and migrants into occupations based on their comparative skill advantages.

Immigration also affects the host society, for example, through changes in schooling, fertility, innovative activity, and crime.⁸ The criminal behavior of immigrants is an especially important concern in many countries. Residents in most OECD countries are more worried about immigrants increasing the crime rate than they are about immigrants taking jobs away from natives (Bianchi, Buonanno and Pinotti, 2012). Fitzgerald, Curtis and Corliss (2012) report that almost 60 percent of Western Europeans think that immigration increases crime, whereas only about 38 percent think that migrants are a threat to the labor market.

Despite this concern, the crime effects of immigration have received relatively little attention in the extant literature. The few studies that identify causal effects from immigration on crime find that migrants are *not* a major driver of crime rates (Chalfin, 2013; Bell, Fasani and Machin, 2013; Bianchi, Buonanno and Pinotti, 2012). Chapter 5 contributes to this nascent field by investigating the crime effects of a particular group of immigrants in Germany—ethnic Germans who came to Germany from Eastern Europe after the fall of the Iron Curtain. In contrast to previous studies, we find strong county-level crime impacts from these relatively large migrant inflows. We argue that this result is explained by the characteristics of these ethnic Germans, who had very poor language and occupational skills. Importantly, the migrants in our setting were granted citizenship upon arrival, which decreased the costs of engaging in crime because one chief sanction for doing so—expulsion—was not an option.

The literature reviewed above already makes clear that the integration of migrants and their effects on natives crucially depend on the skills migrants bring to the host country. It is therefore important to understand migrants' location decision process because this has direct implications for the skill structure of the internal and international migration flow. Especially in the case of international migration, understanding the selection mechanism will help in the design of optimal immigration policies. Furthermore, knowing the skill structure and motives of migrants could change people's attitudes toward immigration. From an econometric viewpoint, understanding the mechanics of selective migration is useful in approaching the endogeneity problem that arises in measuring causal effects of migration.

⁸ See studies by Gould, Lavy and Paserman (2009); Hunt (2012); Cascio and Lewis (2012); Ohinata and van Ours (2013) for schooling, Furtado and Hock (2010) for fertility, Kerr and Lincoln (2010); Hunt and Gauthier-Loiselle (2010) for innovative activity, and Chalfin (2013); Bianchi, Buonanno and Pinotti (2012); Bell, Fasani and Machin (2013) for crime.

The process of migration—whether internal or international—is anything but random. People have specific reasons for, first, choosing to migrate, and, second, the location chosen. In economics, the decision to move and where to move is modeled as a cost-benefit analysis, which means that people move if and only if the benefits from migration are greater than the costs (Todaro, 1969; Harris and Todaro, 1970). Benefits and costs can both be either pecuniary (monetary) or non-pecuniary (psychic costs) (Sjaastad, 1962; Schwartz, 1973). In Chapter 2, we quantify the psychic costs of migration by using administrative panel data from Germany, combined with historical dialect differences between German counties as a proxy for cultural differences. We find that internal migrants in Germany trade off increases in wage gains from migration for cultural familiarity with the destination region.

Section 1.1 takes a detailed look at several issues related to selective migration and provides a brief overview of some of my own work on this topic (Parey et al., 2014; Krieger, Renner and Ruhose, 2014). Section 1.2 discusses the results of Hanushek, Ruhose and Woessmann (2014), which show that selective migration plays a role in explaining certain macroeconomic phenomena, such as economic development, within destination countries. Section 1.3 contains a discussion of microeconomic methods that can be used to identify causal effects in migration economics. An outline of the thesis is presented in Section 1.4.

1.1 Selective Migration

Understanding selective migration is important for understanding the effects of migration writ large. This section takes a closer look at selective migration, first, by discussing the relevant literature and then by presenting the results of two of my own papers that provide explanations for why selective migration occurs (Krieger, Renner and Ruhose, 2014; Parey et al., 2014).⁹

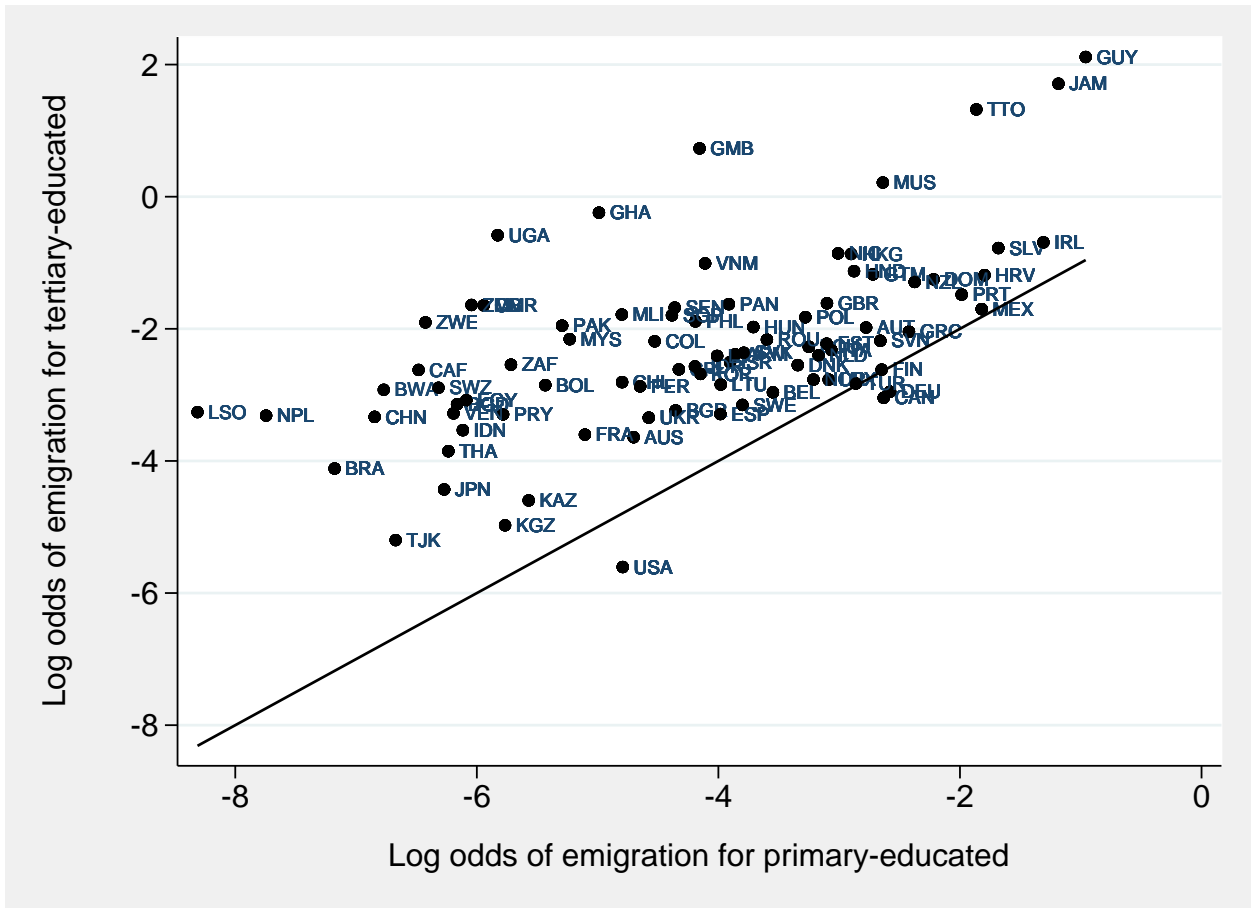
Figure 1.2 plots the odds of migration by primary-educated people against the odds of migration by tertiary-educated people for many countries in the year 2000 (Grogger and Hanson, 2011). With a few notable exceptions, such as Germany, Canada, and the United States, all countries show that

⁹ A field of research closely related to that of selective migration is the work on *brain drain* and *brain gain* (Commander, Kangasniemi and Winters, 2004). This literature deals with how migration impacts sending (especially developing) countries. Based on the presumption that it is mainly the best and brightest who emigrate, the early literature concluded that emigration implies a negative externality for those left behind (Grubel and Scott, 1966; Bhagwati and Hamada, 1974; Miyagiwa, 1991; Haque and Kim, 1995). However, the availability of better data has turned this conclusion on its head and the literature now strongly emphasizes the existence of a brain gain for the sending country (Mountford, 1997; Stark, Helmenstein and Prskawetz, 1997; Vidal, 1998; Beine, Docquier and Rapoport, 2001, 2008; Batista, Lacuesta and Vicente, 2012). This thesis, however, does not deal with the impact of emigration on sending countries but instead focuses on the effects of migration in receiving (mostly developed) countries.

tertiary-educated people are much more likely to emigrate than the primary-educated—indicated by higher relative log odds of emigration for the tertiary-educated (see also Chiswick, 1999). A related implication from the figure is that destination countries can generally expect to receive relatively high-skilled individuals.

Figure 1.2

Emigration Odds for Primary-Educated and Tertiary-Educated Individuals by Source Country, 2000



Notes: The graph is replicated from Grogger and Hanson (2011). Data come from Docquier, Lowell and Marfouk (2007).

Because migrant selection has important implications for labor force quality in destination countries, it is important to find explanations for the selectivity observed in Figure 1.2. For example, Dahl (2002) shows that internal migrants react strongly to returns to education and amenities across U.S. states. In this framework, selective migration occurs when the benefits and costs of migration vary across population groups with different skill levels. Borjas (1987), a pioneer in the theoretical migration literature, formulates a mechanism of selective migration that depends on the benefits received by high- and low-skilled migrants in different countries. Based on the Roy selection model (Roy, 1951), Borjas argues that migrants who move to countries with a more unequal earnings distribution compared to that of their country of origin should be positively selected, that is, more

skilled than those who do not migrate. The reason is that the migrants' skills are more highly valued in the destination country than in the home country, assuming that the skill (and earnings) distributions of both countries are correlated.¹⁰ Similarly, migrants who move to countries with a more equal earnings distribution than the home country should be less skilled than those who do not migrate because less skilled people can insure against low wages when moving to countries with less dispersed earnings distributions.¹¹

Borjas's seminal paper motivated a great deal of work that attempts to either confirm or refute his hypothesis. Migration between Mexico and the United States in particular has attracted a great deal of interest (Chiquiar and Hanson, 2005; Fernández-Huertas Moraga, 2011; Kaestner and Malamud, 2014). However, the literature attempting to confirm the Borjas model is inconclusive (Parey et al., 2014), chiefly due to lack of good data (as the research needs surveys that track people abroad), legal immigration restrictions, non-comparable skill and earnings distributions between countries, multidimensional skill structures, and different measures of migrant quality.

In our own study, we look at a specific group of migrants: German university graduates (Parey et al., 2014). We exploit a very detailed dataset from the DZHW (*Deutsches Zentrum für Hochschul- und Wissenschaftsforschung, Hannover*) that surveys university students one and five years after graduation, regardless of whether they stay in Germany or go abroad. This allows comparing (predicted) wages of those who moved abroad with those who stayed in Germany. We estimate an extensive Mincer earnings function (Mincer, 1974) for the stayers, which includes age, gender, marriage status, children, subject of study fixed effects, university fixed effects, final school and university grades, and parental background, as well as previous mobility. We use the coefficients from this Mincer regression to obtain predicted earnings for those who went abroad, that is, predictions of how much migrants would have earned if they had stayed in Germany.¹²

¹⁰ This assumption of correlated skill and earnings distributions, however, is unlikely to hold in many settings and especially not in the case of migration from developing to developed countries. For example, looking at migration from Mexico to the United States, as many papers have done, the Borjas model assumes that skills in Mexico and the United States are almost equally distributed within the population.

¹¹ Especially the second prediction of the theory is highly disputed. For example, Chiswick (1999) argues that migration costs should lead to the situation where positively selected migration should be observed overall and that those moving to countries with a more equal earnings distribution should be less positively selected (but not negatively selected). Contradicting predictions, however, may be due to different approaches to measuring labor force quality. Figure 1.2 demonstrates that we can expect positive selection in general if we measure migrant skills by educational attainment. However, this may not be the case if we compare measures of productivity, which include more than just education. For example, in Parey et al. (2014), we find that German university graduates who work abroad are all positively selected in terms of school and university grades. However, when the graduates are compared based on predicted wages, the Borjas model is supported. As another example, Kaestner and Malamud (2014) shows that Mexican migrants in the United States are differently selected according to cognitive skills, education, predicted wages, and health.

¹² We control for selection bias in the Mincer earnings function by employing Heckman (1979) sample correction models. Several identification approaches are used. Our preferred exclusion restriction is based on the introduction

Figure 1.3 plots cumulative distribution functions (cdf) of the predicted earnings for three different groups. The short-dotted line represents the cdf for those who stayed in Germany (Home). According to Borjas's theory, migrants who moved to countries with more unequal earnings distributions than Germany are expected to be positively selected, that is, have higher predicted earnings than those who stayed in Germany.¹³ In contrast, migrants who moved to countries with more equal earnings distributions than Germany are expected to be negatively selected, that is, have lower predicted earnings than those who stayed. The cdf plot reveals that the Borjas model is supported by the data. On the one hand, the cdf for those who moved to more unequal countries is to the right of the stayers' cdf, implying that the predicted earnings of migrants in more unequal countries are indeed higher than the earnings of those who stayed in Germany. On the other hand, the cdf for those who moved to more equal countries is to the left of the stayers' cdf, implying that their predicted earnings are lower compared to those who did not leave.¹⁴

Results are robust to restricting the sample to males and are slightly weaker for women. We also find similar patterns when restricting destination countries to Europe or by excluding the United States. Thus, this descriptive analysis confirms Borjas's theory that selective migration is due to differences in wage distributions. It is interesting that this pattern obtains even within the group of high-skilled individuals. However, on reflection, this is not surprising as legal immigration restrictions should be low or even absent for high-skilled individuals and their skills are likely more transferable.

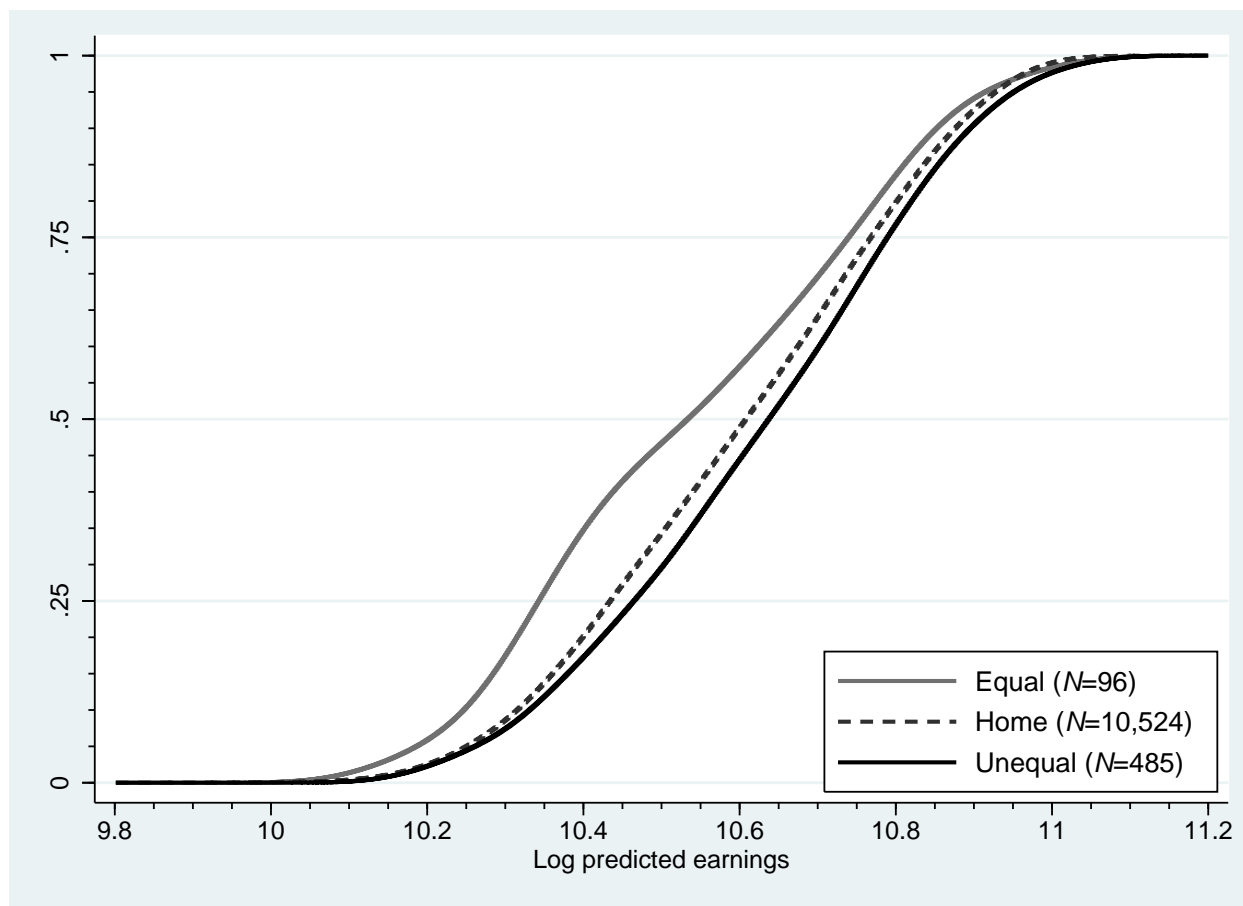
Under the Borjas theory, selective migration occurs due to greater benefits obtained by high-skilled migrants in countries with a more unequal earnings distribution and greater benefits obtained by low-skilled migrants in countries with a more equal earnings distribution. However, migration costs that vary based on education or skill level can also lead to selective migration (Chiquiar and Hanson, 2005). For example, Bauernschuster et al. (2014) show that cultural barriers to migration between German counties, as one example of psychic migration costs and approximated

of ERASMUS (Parey and Waldinger, 2011). However, we also test identification assumptions based on functional form only or using the presence of children before entering the labor market.

¹³ To categorize countries based on their earnings inequality, we constructed our own inequality measures based on net earnings for high-skilled (tertiary-educated) individuals using data from the Luxembourg Income Study and selected additional surveys. For each survey, we computed the 75th/25th earnings ratio among tertiary-educated individuals. To obtain net earnings inequality ratios, we had to transform earnings ratios for countries that report gross earnings only by using OECD average tax rates. The inequality measures are averaged from 1998 to 2010 over available inequality estimates. According to this measure, the United States, France, Poland, Italy, Spain, Japan, Canada, the UK, Austria, Luxembourg, Switzerland, and Belgium show higher earnings inequality, whereas Ireland, Sweden, the Netherlands, Australia, Norway, Finland, and Denmark show lower earnings inequality than Germany.

¹⁴ Kolmogorov-Smirnov tests on stochastic dominance indicate that the differences between the distributions are statistically significant.

Figure 1.3
Predicted Earnings of Migrants and Non-Migrants



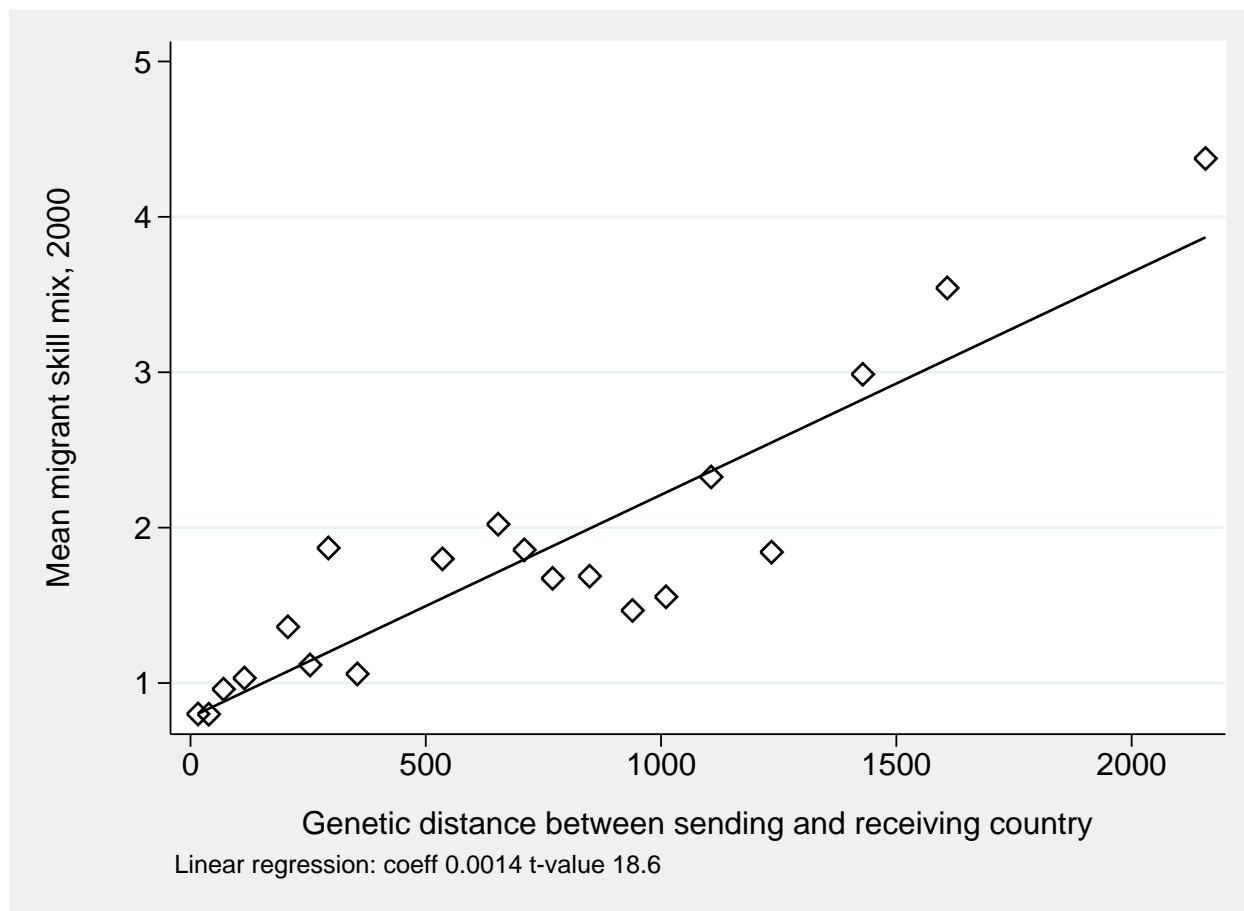
Notes: The figure plots cumulative distribution functions (kernel smoothed) of predicted earnings for three groups of individuals: migrants who moved to countries with more *equal* earnings distributions than Germany, non-migrants who stayed in Germany (*home*), and migrants who moved to countries with more *unequal* earnings distributions than Germany. The graph is from Parey et al. (2014).

by historical dialect differences, are less relevant for high-educated internal migrants; it is easier for these to gather information about potential destinations and employ their skills in different companies, industries, and regions.¹⁵

Whether cultural barriers are also important for migrant selection internationally is the subject of a study by Krieger, Renner and Ruhose (2014), who examine whether the cultural distance between two countries, approximated by the genetic distance between the two countries, can explain migrant selection. Figure 1.4 shows the relationship between genetic distance and migrant selection. The figure was constructed by binning genetic distance into 20 equally sized bins and

¹⁵ Cultural differences, most often approximated by trust, religion, or ethnic origin, can also explain other biases in economic exchange such as trade and investment (Guiso, Sapienza and Zingales, 2009; Felbermayr and Toubal, 2010), income differences (Spolaore and Wacziarg, 2009), knowledge diffusion (Jaffe, Trajtenberg and Henderson, 1993; Comin, Dmitriev and Rossi-Hansberg, 2012), and migration (Belot and Ederveen, 2011; Mayda, 2009).

Figure 1.4
Genetic Distance and Migrant Selection, 2000



Notes: The migrant skill mix is the ratio of the log odds of emigration for tertiary-educated individuals over the log odds of emigration for primary-educated individuals. Thus, values bigger than 1 indicate that the receiving country can expect relatively more tertiary-educated migrants than primary-educated migrants from the sending country. For the nonparametric binned scatterplot, we bin genetic distance into 20 bins of equal size and compute for each bin the mean migration skill mix. The figure is from Krieger, Renner and Ruhose (2014). Coefficients and t-statistics are obtained from OLS regressions on the bilateral country data. Data on migrant stocks by education level are from Docquier, Lowell and Marfouk (2007) and the genetic distance data are from Spolaore and Wacziarg (2009).

computing for each bin the mean migrant skill mix, that is, the ratio of the log odds of emigration by tertiary-educated people divided by the log odds of emigration by primary-educated people. The positive slope indicates that higher genetic distance is associated with more positively selected migrant flows. The relationship survives an extensive set of control variables (including geographic and linguistic distance controls), destination and origin country fixed effects, and instrumental variable estimations with genetic distance in the year 1500 as the instrument for current genetic distance. Additionally, we find that the relationship is nonlinear, suggesting that cultural (genetic) distance plays a significant role only between countries with above-median genetic distance.

This evidence indicates that selective migration is not only due to observable factors (such as earnings) but also to unobserved factors (such as cultural traits). Therefore, identification strategies in the economics of migration (see Section 1.3) need to take selective migration into account.

1.2 Selective Migration and Economic Development

From the above discussion, it is clear that selective migration can substantially alter the composition, and thereby the quality, of the workforce. Evidence shows that the human capital of the workforce is important for economic development (Hanushek and Woessmann, 2008, 2012*a,b*; Schoellman, 2012; Gennaioli et al., 2013; Manuelli and Seshadri, 2014) and that failing to improve the skills of children is associated with substantial economic loss in the future (Hanushek and Woessmann, 2011*a*).¹⁶ Hanushek and Woessmann (2008) demonstrate that when measuring human capital, it is important to distinguish between the *quantity* and the *quality* of education because children do not all acquire the same skills within a given year across different school systems. The quantity of education is often measured by completed years of education and can be easily inferred from Census data. However, the quality of education is much harder to detect. One approach is to use test scores from large-scale international achievement tests such as PISA (Hanushek and Kimko, 2000; Hanushek and Woessmann, 2011*b*).¹⁷

In Hanushek, Ruhose and Woessmann (2014), we investigate the importance of state human capital for economic development in the United States. This study uses development and growth accounting techniques to decompose the differences in GDP per capita across U.S. states into their factors of production. For this project, we compiled human capital stocks for the labor force by state in 2007. We measure the quantity of education by average years of schooling in a given state. The quality of education is measured by cognitive test scores, which we obtained from the National Assessment of Educational Progress (NAEP). However, constructing measures of cognitive skills for the current workforce is not simple: there are two major problems. First, test scores at the state level are available only from 1992 onward. Thus, we have no test scores for about three-quarters of the labor force. Second, on average, only about 60 percent of the people currently living in the

¹⁶ There are at least three possible reasons for why the human capital of the workforce is important for economic growth. First, Mankiw, Romer and Weil (1992) argue that education increases the human capital stock of the labor force and this increases productivity and, subsequently, output. Second, models of endogenous growth postulate that education fosters innovative activity, which is beneficial for economic development (Lucas, 1988; Romer, 1990; Aghion and Howitt, 1998). Third, the implementation of innovations and new technologies requires human capital as well. Thus, education is necessary for adopting and taking advantage of new knowledge (Nelson and Phelps, 1966; Benhabib and Spiegel, 2005).

¹⁷ See Hendricks (2002) and Schoellman (2012) or Jones (2014) for different approaches to this problem.

state were born in that state. Thus, a considerable fraction of workers did not go to school in the state where they are currently working. Additionally, the United States is home to large groups of international migrants, which further complicates the construction of cognitive skill measures for the workforce.

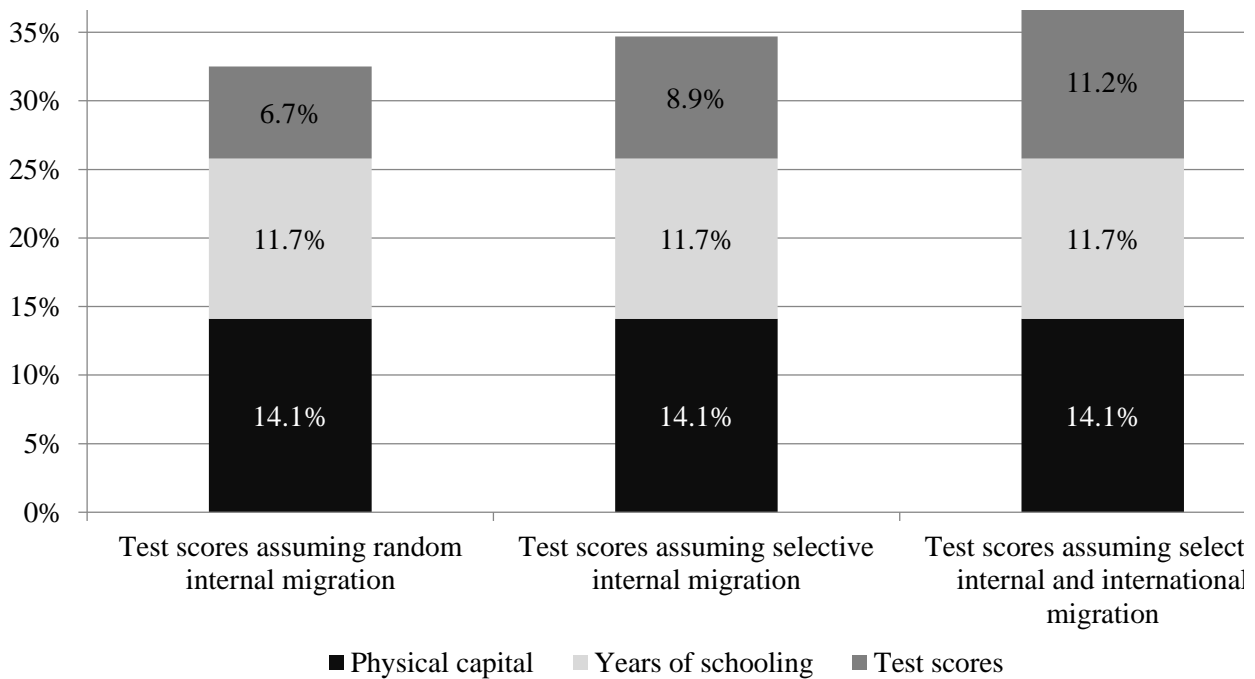
We deal with the issue of migration in three steps. First, we use the Census to estimate the composition of the workforce based on their state of birth for natives and country of birth for international migrants. Second, we assign to each birthplace the average test score obtained by students in the state or country of birth. Third, we sum all test scores within a given state, weighted by the birthplace fraction, to obtain an estimate for the cognitive skills of the workforce. We tackle the problem of missing test scores by constructing test score series based on college admission test scores (SAT test scores) and national test scores, which are available since 1972 and 1978, respectively. Here, I am only presenting results obtained from average test scores. However, the results from projected test score series are not much different.¹⁸

Having constructed measures for the quality and quantity of education and using data on physical capital from Turner, Tamura and Mulholland (2013), we use development accounting techniques to decompose differences in GDP per capita across states. Figure 1.5 displays the results for different test score specifications. The first bar uses the average test score by state, adjusted for internal migration by assigning each workforce fraction the average state of birth test score. The percentage numbers indicate how much of the differences in GDP per capita can be attributed to differences in input factors. In the first setting, we can attribute 6.7 percent to differences in test scores, 11.7 percent to differences in years of schooling, and 14.1 percent to differences in physical capital.

Assigning average test scores to those who have moved assumes that they are randomly drawn from the state or country of birth population. However, as discussed in Section 1.1, migration is not a random process, but highly selective (Borjas, 1987; Borjas, Bronars and Trejo, 1992). In the development accounting framework, we account for self-selection along educational attainment by breaking down the workforce fractions into groups of university educated and non-university educated. We assign test scores for children from parents with and without university education to the fraction of the workforce with and without university education. The second bar in Figure 1.5 shows the development accounting results by using this test score modification, which now accounts for selective internal migration. The model fit is improved and we can now attribute 8.9 percent of the differences in GDP per capita to differences in cognitive skills.

¹⁸ Hanushek, Ruhose and Woessmann (2014) provide a much more detailed discussion of both issues, including several robustness checks.

Figure 1.5
Development Accounting Results Across U.S. States, 2007



Notes: Development accounting results for different test score specifications (Hanushek, Ruhose and Woessmann, 2014). Test scores refer to eighth-grade math. Data come from the National Assessment of Educational Progress (NAEP). *Test scores assuming random internal migration* assign internal migrants the average test score of their state of birth. *Test scores assuming selective internal migration* assign internal migrants and locals with and without university education the state of birth test score of children of parents with and without university education. Locals are those who still live in their state of birth. *Test scores assuming selective internal and international migration* assign international migrants the 90th percentile of their country of birth test score distribution and internal migrants and locals with/without university education the state of birth test score of children of parents with/without university education. Calculations assume 0.2 return per standard deviation of test scores (Hanushek and Zhang, 2009) and 0.1 return per year of schooling (Card, 1999). Hanushek et al. (2014) provide current estimates on the returns to skills and schooling. Alaska, Wyoming, and Delaware are dropped because a large share of GDP in those states is not due to human capital, but from natural resources (Alaska and Wyoming) or tax haven status (Delaware). Data on years of schooling are from the U.S. Census (Ruggles et al., 2010). Data on physical capital are from Turner, Tamura and Mulholland (2013) for the year 2000.

The third specification (see right bar in Figure 1.5), which attributes 11.2 percent of differences in GDP per capita to differences in cognitive skills, deals with the contribution of international migrants to the United States. We assume that international migration to the United States is highly positively selective and therefore assign each foreign-born workforce fraction the 90th percentile of the home countries' test score distribution.¹⁹ Using this specification, we attribute nearly a quarter of the differences in GDP per capita across U.S. states in 2007 to differences in total human capital (years of schooling plus test scores). Accounting for selective internal and international migration almost doubles the contribution of test scores to GDP per capita differences (6.7 percent vs. 11.2

¹⁹ We use PISA and TIMSS test scores for this purpose, which are rescaled to NAEP test scores. See Hanushek, Peterson and Woessmann (2012) and Hanushek, Ruhose and Woessmann (2014) for details.

percent), indicating that selective migration is important for explaining development levels across U.S. states.

1.3 Causal Inferences in Migration Economics

This section discusses microeconomic estimation strategies used to identify causal effects in migration economics. Identifying the impact of migrants on regional outcomes is challenging. The most important problem, as mentioned, is that migration is not random. People choose different locations for specific reasons. Due to migrant selection, it is hard to distinguish correlation from causation when estimating the impact of immigration on regional outcomes. The problem can be illustrated by looking at the labor market effects of migrants. Suppose we observe that the share of immigrants in a region is positively correlated with the region's economic performance. There could be two reasons for this. First, immigrants might improve economic performance or, second, it could be that migrants simply self-select into booming economic regions.

Random variation in the stock or flow of migrants is key for making causal statements about an observed correlation. The *gold standard* would be to distribute migrants randomly across regions. However, freedom of movement is an important human right and restricting it would be unethical. Such a restriction would be acceptable if migrants concentrated in some areas impose large costs on local public budgets (e.g., social assistance, healthcare, housing, etc.). Take refugees, for example. Most countries assign refugees to municipalities irrespective of their own characteristics so as to spread the burden of accommodating them. Thus, in such a case, self-selection is highly restricted. One can then exploit the variation in the placement of refugees to examine the impact of immigration on, for example, labor market outcomes (Edin, Fredriksson and Åslund, 2003; Damm, 2009), school performance (Åslund et al., 2011), crime (Bell, Fasani and Machin, 2013), or the willingness to redistribute (Dahlberg, Edmark and Lundqvist, 2012). Chapter 5 exploits a dispersal policy that, after the fall of the Iron Curtain, assigned migrants from Eastern Europe (ethnic Germans) across German counties.²⁰ We use this variation in the inflow of migrants to assess the impact of immigration on crime and find that crime in the region increased greatly after the inflow of ethnic Germans.

In the absence of randomly distributed migrants, we have to rely on quasi-experimental methods to construct a counterfactual world to answer the question of how the situation would look if

²⁰ Glitz (2012) also uses this allocation of ethnic Germans to evaluate their impact on natives in the labor market.

immigration had not occurred. A frequently applied method of doing so is the *shift share approach*. This approach makes use of the fact that migrants prefer to move to regions where other migrants from the same ethnicity or the same country have already settled (Bartel, 1989). One of the first papers to use this approach to identify labor market effects of immigration is Card (2001).²¹ In this paper, Card argues that the current inflow of immigrants can be decomposed into two parts: one exogenous part that is uncorrelated with economic conditions and one endogenous part that is correlated with economic conditions that attract migrants to a particular location. The first part captures exogenous supply-push components of the migrant inflow that can be derived from historical migration patterns. The second part is driven by endogenous demand-pull components that capture all deviations from the historical pattern. This part also includes, for example, changing labor market conditions that cause migrants to settle in specific regions. The variation in the supply-push component of the migrant inflow can be used as an instrument in an instrumental variables estimation. Specifically, the instrument in this setting is constructed by predicting migrant shares based on the historical distribution of migrants by birthplace together with birthplace-specific national inflow rates. The approach gives the share of immigrants by birthplace that would be observed in the absence of region-specific demand-pull factors.

This shift-share approach is employed in the migration literature to study the labor market impacts of immigration (e.g. Card, 2001; Borjas, 2003; Ottaviano and Peri, 2006; Peri and Sparber, 2011a; Suedekum, Wolf and Blien, 2012), as well as the effects of immigration on economic growth (Ager and Brueckner, 2013), productivity (Peri and Sparber, 2009; Peri, 2012), schooling (Hunt, 2012), innovation (Kerr and Lincoln, 2010; Hunt and Gauthier-Loiselle, 2010), consumer prices (Cortes, 2008), housing prices (Saiz, 2007), and crime (Bianchi, Buonanno and Pinotti, 2012; Bell, Fasani and Machin, 2013). In Chapter 4, I use the shift share approach to identify the causal effect of immigration on the intergenerational income mobility of citizens across U.S. metropolitan areas.

Another way of discovering the effects of immigration on regional outcomes is to compare the outcome of interest for regions that experienced a large inflow of immigrants with the outcome of regions that did not experience such an inflow before and after the immigration event has taken place. The before and after comparison between a treated and an untreated group is known as a differences-in-differences estimator. For example, Card (1990) and Saiz (2003) analyze the Mariel Boatlift, which occurred in April 1980 when Fidel Castro briefly allowed Cubans to emigrate to the United States. Almost 125,000 Cubans moved to Miami, which increased that city's population by

²¹ See Altonji and Card (1991) for an earlier contribution.

7 percent. Card (1990) compares the Miami labor market to the labor market of four similar U.S. cities (Saiz (2003) used more comparison cities) before and after the inflow of Cubans to Miami. This approach has the advantage of being able to compare wage and employment growth (instead of levels) to each other, thereby controlling for time-invariant city-specific factors. The identifying assumption is that Miami's labor market would have developed similarly to that of the other four cities in the absence of the migrant inflow (common or parallel trend assumption).²²

In Chapter 3, we use a differences-in-differences approach in a completely different setting. We examine the effects of early educational tracking on the educational achievement of migrants compared to that of natives across countries. For this study, we use all available large-scale international student achievement tests that test students in primary and secondary school. Our differences-in-differences approach takes advantage of the fact that no country tracks students based on ability in primary school, whereas some countries track students in secondary school and some do not. Therefore, we compare the difference in the migrant-native achievement gap in primary versus secondary school between countries that track their students early and countries that track their students late or not all. Comparing our results to conventional cross-sectional regressions, we find a substantial bias from unobserved country traits. The bias might be due, for example, to different migrant populations selecting into early or late tracking countries. Using the differences-in-differences approach, we can keep migrant compositions (almost) fixed and compare only changes (and not levels) in the migrant-native achievement gap between the primary and secondary school level.

To this point, the focus has been on identifying causal effects from immigration on natives (or other migrants). In Chapter 2, we discuss non-pecuniary (psychic) migration costs that are incurred by moving from a culturally familiar to a culturally unfamiliar region in Germany. The identification challenge is that individuals who decide to move to an either more familiar or more unfamiliar region are expected to be different in observable and unobservable characteristics. For example, Bauernschuster et al. (2014) show that it is easier for more educated and more risk-loving people to overcome cultural barriers. It is likely that both characteristics lead to higher wages, which would bias the estimated returns of moving. To mitigate this selection problem, we condition on pre-migration wage profiles. The argument for this approach is that risk-loving or more educated people should garner higher wages, compared to risk-averse or less educated people, even *before* the move. Pre-migration wages, therefore, should capture unobserved individual characteristics that

²² Angrist and Krueger (1999) challenge this assumption by showing that a "[b]oatlift that didn't happen" a few years later also had labor market effects.

would otherwise confound the analysis. This approach is taken from the literature on the effects of training on wages (Ashenfelter, 1978; Ashenfelter and Card, 1985; LaLonde, 1986; McKenzie, Gibson and Stillman, 2010). McKenzie, Gibson and Stillman (2010) transfer this idea from the labor economics literature to the migration economics literature and compare the performance of different estimators in revealing returns to migration. They show that a differences-in-differences estimator taking wages before migration into account is among the better performing non-experimental estimators and derives results that are relatively close to the causal effect of immigration on post-migration wages. In Chapter 2, we discuss several improvements of the estimator used in McKenzie, Gibson and Stillman (2010), and also find evidence that controlling for pre-migration wage profiles substantially reduces self-selection bias.

1.4 Outline of the Thesis

This thesis consists of four self-contained empirical studies. Each chapter fills a gap in the current literature on the economics of migration. I cover several aspects of the migration process, including the characteristics of migrant flows in Chapter 2, the performance of migrants in Chapter 3, and the effect of migrants on the host country society in Chapters 4 and 5. Chapter 6 contains a summary and discussion of the results.

CHAPTER 2 covers the importance of non-pecuniary migration costs for internal migrants in Germany. Section 1.1 has already mentioned that migrants self-select into regions along cultural ties. In this chapter, we quantify the non-pecuniary (i.e. psychic) migration costs that are incurred by moving from a familiar to an unfamiliar surrounding. Quantifying migration costs is important; labor immobility is associated with substantial welfare losses as it prevents the efficient allocation of labor (Clemens, 2011). Disentangling cultural costs from other costs of migration is thus important from a policy perspective because policy can influence travel costs, for example, by providing adequate infrastructure, but it can hardly change deeply rooted cultural biases. This is one of the first studies to use a more direct measure for differences in cultural proximity to quantify non-pecuniary migration costs.

We assume that living in a culturally unfamiliar environment is comparable to a disamenity in the Roback model (Roback, 1982). Consequently, a potential internal migrant will move to a culturally unfamiliar environment only if she is compensated for this disamenity in the form of a higher wage and/or lower rent compared to her place of origin.

We use administrative social security panel data to identify internal migrants in Germany. Internal migrants are defined as job switchers for whom the job switch involves moving from one county to another. We merge the internal migrants' wage profiles over time with information on the geographic and cultural distance between their counties of origin and destination. Cultural distance is calculated from unique data on historical dialect dissimilarity between German counties (Falck et al., 2012). As the most prominent expression of social identity, almost like a genome, historical dialects stored information about past interactions between German counties over time.

Our findings imply that, conditional on geographic distance and quarterly pre-migration wage profiles, internal migrants demand a wage premium of about 1 percent for overcoming one standard deviation in historical dialect distance. If we index wages with respect to local rents, we document an indexed wage premium of 1.5 percent. The effect is driven by males and those who earn above-average occupational wages, is more pronounced for geographically short moves, and persists over time. Including high-order polynomial functions of geographic distance in the regressions provides additional confidence that the effect of dialect distance is not simply reflecting a non-linearity in the geographic distance effect. We also analyze those who have made multiple moves within a relatively short period and find that internal migrants who made a *wrong decision* in the first move correct this decision in the second move and demand a much higher wage premium.

CHAPTER 3 looks at the effect of early educational tracking on the educational performance of migrants relative to that of natives. Educational tracking is the process of sorting children into different school tracks based on their perceived ability. Studying the performance of migrants in different school systems is motivated by the fact that in many European countries, migrants are failing to assimilate (Algan et al., 2010). Therefore, increasing migrants' educational achievement and improving their labor market outcomes is a key priority for policy makers.

One institutional feature of several school systems in Europe that has been shown to generally increase educational inequality is early tracking of students into different types of secondary school based on their ability (e.g., Hanushek and Woessmann, 2006). It is therefore often recommended that educational systems be made more comprehensive and less selective so as to improve opportunities for migrant students and reduce migrant-native differences in student achievement (e.g., Network of Experts in Social Sciences of Education and Training (NESSE), 2008). However, the evidence for a causal effect of early tracking on migrant-native test score gaps is less than convincing and cross-country comparisons of differences in student outcomes may be biased by unobservable

country-level factors. It is therefore important to look at variation *within* countries to control for country-specific selective migration and different migrant compositions.

This chapter sheds new light on how ability-based early tracking of students into different types of secondary school impacts migrant-native test score gaps. To investigate this issue, we pooled data from all existing waves of the three largest international assessments of student achievement during primary and secondary school, namely, PISA, TIMSS, and PIRLS.²³ Doing so provided us with information on student achievement and background characteristics for more than 1 million students in 45 countries. Our identification strategy takes advantage of the fact that no country tracks students in primary school, thus allowing us to estimate a differences-in-differences model that exploits variation in migrant-native test score gaps between primary and secondary school as well as variation in the age of tracking between countries.

We find that early tracking does not significantly affect the evolution of migrant-native test score gaps from primary to secondary school in math and science. Nor do we find significant effects for reading, but the results here are somewhat less clear-cut. Across subjects, however, there are no substantial negative impacts of more than 10 percent of a standard deviation. This average result conceals an important heterogeneity in the degree of immigrant integration. We find evidence that less integrated second-generation immigrants who do not speak the testing country's language at home suffer from early tracking. Therefore, this study provides first evidence on an important interaction between migrant proficiency in the national language and the age of tracking students across schools by ability. While early educational tracking does not appear to be systematically related to relative achievement of all students with a migration background, our findings indicate that tracking at later stages could enhance the educational opportunities of children from less integrated families.

CHAPTER 4 investigates the relationship between immigration and the intergenerational income mobility of citizens in the United States. Intergenerational income mobility is measured by the correlation between parental and child income and is often used to measure to what extent a country provides equal opportunities. The study contributes by offering a to date neglected connection between the literature on the determinants of intergenerational income mobility of citizens and the literature on income effects of immigration on the host economy.

A recent study by Chetty et al. (2014) finds that aggregate country-level intergenerational income elasticity masks substantial variation across U.S. regions. For example, cities such as

²³ PISA: Programme for International Student Assessment, TIMSS: Trends in International Mathematics and Science Study, PIRLS: Progress in International Reading Literacy Study.

Milwaukee (WI) and Memphis (TN) have elasticities that are twice as large as the elasticities of Los Angeles (CA) and Denver (CO). Indeed, the latter two cities are comparable to Scandinavian countries in terms of intergenerational income mobility. In this chapter, I argue that immigration is responsible for some of the regional variation in the intergenerational income mobility of U.S. citizens. I find that U.S. citizens who are born in a metropolitan area with a relatively higher share of foreign-born immigrants exhibit significantly higher intergenerational income mobility.

To demonstrate the effect of immigration on the intergenerational income mobility of U.S. citizens, I use newly compiled data from the *Equality of Opportunity Project*, which provides detailed income mobility measures for the United States at the regional level. Chetty et al. (2014) use all children born in 1980 and match them to the tax filers who claimed them as dependents on their tax records. Intergenerational income mobility is then measured as the correlation between the child income rank in the national child income distribution in 2010–2011 and the parent income rank in the national parent income distribution in 1996–2000 (*rank-rank correlation*) at the metropolitan–area level. I merge this information with Census data from different years (Ruggles et al., 2010) and regress the rank-rank correlation on the share of foreign-born in 1980. Identification is established by using the hypothetical share of foreign-born in 1980 based on the distribution of migrants in 1930 together with the national inflow of migrants between 1930 and 1980 by world region as exogenous variation in an instrumental variables approach (*shift share methodology*; see, e.g., Card (2001) and Peri (2012)). I find that being born in a region with a larger migrant population is associated with more intergenerational income mobility of U.S. citizens. Specifically, the correlation between the parent income rank and the child income rank is reduced by 6.4 percent for a one standard deviation increase in the share of foreign-born (almost 4 percentage points).

My explanation for this finding is that immigration changes the occupational structure of the metropolitan area such that it pushes natives into occupations that are more associated with income mobility. The native-born accomplish this endogenously by obtaining higher educational levels that allow them to choose occupations that are more complementary to the skill level and occupational choices of immigrants. Other explanations, like school inputs, family resources, entrepreneurial activity, patenting, or crime, play only minor roles or none at all.²⁴

CHAPTER 5 investigates the effect on crime rates of a particular group of immigrants to Germany who were allocated across regions by German authorities upon arrival, meaning that self-

²⁴ In regard to the financing of education, Krieger and Ruhose (2013) discuss whether there is an intergenerational conflict between old and young. Using a panel of OECD countries, we do not find that this is the case in general. However, we demonstrate that those who are close to statutory retirement age disfavor public expenditure on families and education.

selection into regions was highly restricted. This allocation provides a unique quasi-experimental setting for studying the effects of immigration on crime. Furthermore, as the allocation policy arguably led to similar skill and age distributions of immigrants across regions, we are able to assess the importance of regional conditions.

The group of immigrants we focus on in this study are ethnic Germans who lived in Eastern Europe and the former Soviet Union before migrating to Germany after the fall of the Berlin Wall. Between 1988 and 2005, more than 3 million ethnic Germans immigrated, increasing Germany's population by about 5 percent. Ethnic German immigrants were granted German citizenship, were allowed to work, and were eligible for social assistance (like German natives) immediately upon arrival.

We combine annual county-specific inflows of ethnic German immigrants with annual county-specific crime rates. Furthermore, we add county-specific information on labor market conditions and demographics to assess whether (and, if so, which) regional conditions play a role in the impact of immigration on crime. We focus on West Germany (excluding Berlin) from 1996 until 2005, the period during which newly arriving ethnic German immigrants were allocated across regions by German authorities. The allocation was binding for three years and noncompliance with the assignment was sanctioned with withdrawal of all social benefits.

We find that ethnic German immigrants increase crime rates substantially. An immigrant inflow of one ethnic German per 1,000 inhabitants increases total crime by about 0.9 percent, which amounts to an elasticity of approximately 0.45. Effects vary by type of crime, with strong impacts on burglary, property damage, and battery, but no effect on street-related types of crime. We also find evidence that regional labor market conditions are crucial: in regions with low unemployment rates, immigration has no effect on crime, whereas crime effects are strong in regions with high unemployment. Furthermore, we find a similar pattern with respect to preexisting crime levels: the impact of immigration on crime is much stronger in regions with high preexisting crime levels.

In their economic theory of crime, Becker (1968) and Ehrlich (1973) argue that the propensity for crime decreases with legitimate earning opportunities, the probability of being convicted, and the cost of conviction. The last factor is particularly important in this study because ethnic Germans received citizenship immediately upon arrival, which is typically not the case for immigrant groups studied elsewhere. Since citizenship reduces the cost of conviction by eliminating the threat of deportation, this could explain why the crime effects we find are so much larger than those found in other studies. Furthermore, we contribute to the literature by providing the first direct assessment

of the importance of local labor market conditions for the crime impact of immigration. Finally, we estimate short-run effects of immigration on crime by exploiting annual immigration inflows, whereas other research estimates medium-run or long-run effects based on changes in immigrant stocks over time. If immigrants are more likely to commit crimes in the first years after arrival, for example, because integration into the new society takes some time, then we are more likely to find stronger crime effects.

CHAPTER 6 provides a nontechnical summary of all the studies included in this thesis. I also discuss the findings' policy implications.

Chapter 2

The Cost of Migrating to a Culturally Different Location*

Culture matters has become a general wisdom in the economics literature. Indeed, recent literature shows that norms and values, such as trust, which is an important component of culture, determines economic activity and eventually growth¹. Specifically, cultural biases hinder economic exchange across locations². Since culture only changes slowly over time³, it is also a main source of the legacy of history emphasized by economic historians (Nunn, 2009). We contribute to the economics-of-culture literature by quantifying the non-pecuniary cost of migrating to a culturally different location. The key problem in quantifying this cost is to disentangle differences in culture from other (formal) institutional differences. To solve this problem, we look inside a single country, where formal institutions do hardly differ across locations, and study internal migration across locations within a country. Thereby, we exploit cultural biases between locations coming from a lack of social and economic interactions in the past.

Quantifying migration costs is important because labor immobility is associated with substantial welfare losses as it prevents the efficient allocation of labor (Clemens, 2011). Welfare losses might also arise because the mobility of workers is a precondition for cluster formation and the

* This chapter was coauthored by Oliver Falck, University of Munich and Ifo Institute and Alfred Lameli, Research Centre Deutscher Sprachatlas.

¹ See, e.g., Algan and Cahuc (2010); Guiso, Sapienza and Zingales (2006, 2011); Tabellini (2010); Bruegger, Lalive and Zweimueller (2009).

² See, e.g., Belot and Ederveen (2011); Comin, Dmitriev and Rossi-Hansberg (2012); Felbermayr and Toubal (2010); Guiso, Sapienza and Zingales (2009); Mayda (2009); Spolaore and Wacziarg (2009).

³ Guiso, Sapienza and Zingales (2006) define culture as *those customary beliefs and values that ethnic, religious, and social groups transmit fairly unchanged from generation to generation*.

realization of agglomeration economics (Duranton, 2011). Worker mobility also leads to the rapid dissemination and cross-fertilization of ideas, eventually resulting in innovation (Saxenian, 1994). Disentangling cultural costs from other costs of migration is thereby important from a policy perspective because policy can influence travel costs, for example, by providing adequate infrastructure, but it can hardly influence deeply rooted cultural biases.

Conceptually, the decision to migrate, and where exactly to go, is determined by comparing the costs and benefits of moving to the costs and benefits of alternatives (Todaro, 1969; Harris and Todaro, 1970). Benefits and costs can be monetary or non-monetary; also including the non-monetary psychic migration costs of moving from a familiar to an unfamiliar surrounding (Sjaastad, 1962). Examples for these costs are that migrants might have to leave family and friends or have to cope with different cultural traits and habits in the new destination. However, it is hard to find direct measures of these psychic costs. For this reason, the internal migration literature typically uses simple geographic distance between the place of origin and destination as a catch-all proxy for various costs of migration (Greenwood, 1975).⁴ That is also the reason why the more current literature that tries to quantify non-pecuniary migration costs is still rather scarce and tailored to specific populations. For example, Barrett and Mosca (2013) find that Irish male return migrants are more likely to have suffered from alcohol problems than those who never moved. Another paper, dealing also with internal migrants, by Dahl and Sorenson (2010) looks at skilled technical workers in Denmark. They find that these workers are more likely to accept job offers from regions close to where they grew up. Based on conditional logit regressions, the paper finds substantial psychic migration costs through some back-of-the-envelope calculations to quantify, for example, the effect of doubling the distance to home or doubling the distance to parents.

We quantify the psychic costs of migration as the wage premium that migrants demand when moving to a culturally different location. This approach is motivated by regional general equilibrium models in the tradition of Roback (1982). We assume that living in a culturally unfamiliar environment is comparable to a disamenity in the Roback model. Consequently, a potential internal migrant will move to a culturally unfamiliar environment only if she is compensated for this disamenity in the form of a higher wage and/or lower rent compared to her place of origin. For this purpose, we use administrative social security panel data to identify internal migrants in Germany. Internal migrants are defined as job switchers for whom the job switch involves the move from one

⁴ Schwartz (1973, p. 1161) justifies the use of this particular catch-all proxy: "Psychic cost can be transformed into permanent transportation cost by figuring the needed frequency of visits to the place of origin so as to negate the agony of departure from family and friends".

county to another.⁵ We merge the internal migrants' wage profiles over time with information on the geographic and cultural distance between their counties of origin and destination. Cultural distance is calculated from unique data on historical dialect dissimilarity between German counties (Falck et al., 2012). This historical dialect information comes from a government-funded dialect survey conducted in the German Empire at the end of the 19th century. At the time, dialects were still the prevalent languages of communication, often leading to significant problems in understanding between people from different regions or even nearby towns. As the most prominent expression of social identity, almost like a genome, historical dialects stored information about past interactions across German counties over time. Our broad and evolutionary perspective of culture is thus similar to that of Guiso, Sapienza and Zingales (2006) who define culture as *those customary beliefs and values that ethnic, religious, and social groups transmit fairly unchanged from generation to generation*.⁶

Our findings imply that, conditional on geographic distance and quarterly pre-migration wage profiles, internal migrants demand a wage premium of about 1 percent for overcoming one standard deviation in historical dialect distance. If we index wages with respect to local rents, we document an indexed wage premium of 1.5 percent. The wage premium is most likely a lower-bound estimate for internal migrants since the county of immediate origin of an internal migrant is not necessarily the place where she was born and socialized. For those cases, however, we would not expect to find a systematic correlation between wage changes and dialect distance. It could also be that migrants in culturally more distant regions are discriminated and earn lower wages. However, this would also work against our finding that migrants in culturally more distant regions earn more. The effect is driven by males and those who earn above average occupational wages, more pronounced for geographically short moves, and persistent over time. Considering higher polynomial functions of geographic distance provides additional confidence that the effect of dialect distance is not only reflecting a non-linearity in the geographic distance effect. We also analyze those who have made multiple moves within a relatively short period and find that internal migrants who made a *wrong*

⁵ Thus, we do not study commuting. We are also not looking at gains to migration in general. McKenzie, Gibson and Stillman (2010) show that these gains are hard to retrieve from non-experimental data. Therefore, we focus solely on the internal margin, that is, looking only at individuals who have changed their place of work and place of residence and do not discuss outcomes from individuals who have changed their place of work without moving to another region. Refer to Lehmer and Ludsteck (2011) for a study about gains to interregional migration within Germany.

⁶ The linguistic situation changed when social and economic exchange was intensified after the founding of the German empire in 1871. At that point, the national language (*Hochdeutsch*), which, until then, had been mostly reserved for written contexts, became increasingly adopted for speech also. At the same time, and considerably more so after World War II, German dialects show signs of both convergence and linguistic transfer from the national language. Obtaining explicit cultural consolidations at a very small geographic scale is thus made easier by using historical dialect data.

decision in the first move correct this decision in the second move and demand a much higher wage premium. Our results also imply that analyses of returns to migration that do not consider these psychic costs of migration overestimate the rate of return to monetary resources invested in migration.

We interpret our findings within a model of search and matching (e.g., Mortensen and Pissarides, 1994; Mortensen, 2011) where cultural barriers to migration represent a labor market friction that prevents the efficient allocation of labor. The rationale is the following: Détang-Dessendre, Drapier and Jayet (2004), for example, argue that the search process of people looking for a job is not random and that individuals accept wage offers only when they are compensated for (pecuniary and non-pecuniary) migration costs. Because of cultural barriers, migrants do not consider the whole spectrum of wage offers available and make suboptimal choices in terms of wage income. However, individuals' welfare might not suffer from this search behavior because they are compensated by the higher cultural familiarity to the destination region.

The remainder of the chapter is organized as follows. Section 2.1 introduces the data. Section 2.2 explains our estimation strategy. Section 2.3 shows the results. Section 2.4 concludes.

2.1 Data

2.1.1 Historical Dialect Distance between German Counties

Our proxy for cultural distance is based on historical dialect data from German localities. This unique source of data is derived from the language survey conducted for the Linguistic Atlas of the German Empire (*Sprachatlas des Deutschen Reichs*; data exploration between 1879 and 1888). Under the direction of the linguist Georg Wenker, pupils in more than 45,000 German schools were asked to translate 40 German sentences (more than 300 words) into their local dialect.⁷ One of the chief results of this project was the discovery of 66 prototypical characteristics of pronunciation and grammar that Wenker and his successors isolated during an extensive evaluation process (Wrede, Mitzka and Martin, 1927). These characteristics are most relevant for structuring the German-language area. Hand-drawn maps illustrate the distribution of each of these prototypical characteristics across German counties.⁸

⁷ The results are available in the form of phonetic protocols for each school, cf. <http://www.regionalsprache.de>.

⁸ All hand-drawn maps are published online as the 'Digitaler Wenker-Atlas' (DIWA), see <http://www.diwa.info> or, more recently, <http://www.regionalsprache.de>.

Using these prototypical characteristics, Falck et al. (2012) construct a dialect similarity matrix across all German counties. Based on the hand-drawn maps, they map the 66 prototypical characteristics to the current boundaries of the 439 German counties (NUTS–3 level) and count for every pair of counties how many of the characteristics can be found in both dialect profiles. Thus, the dialect similarity is 0 if there is no overlap in the dialect and 66 if all characteristics can be matched (for more details, see Falck et al. (2012) and Lameli (2013)). The authors end-up with a dialect similarity matrix of the dimension 439×439 .

$$\text{Dialect distance}_{sd} = 1 - \text{Dialect similarity}_{sd}/66 \in [0, 1] \quad (2.1)$$

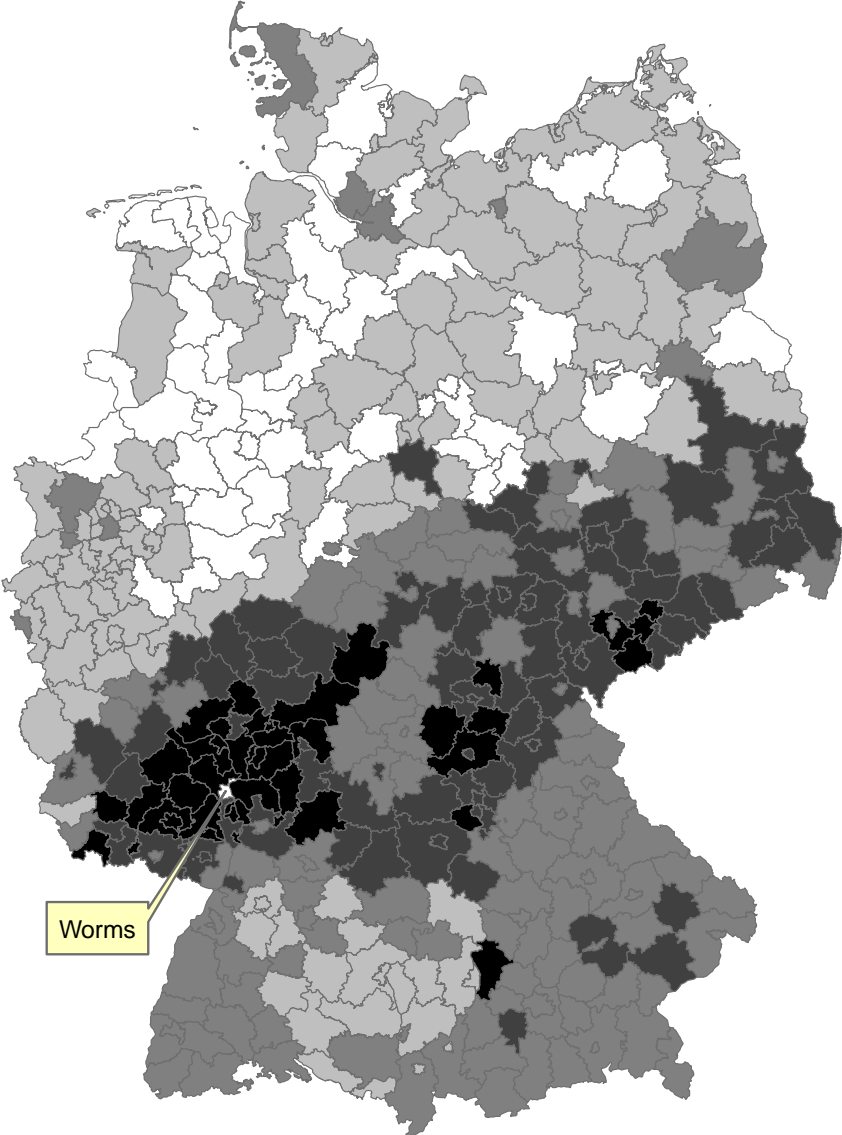
For means of comparison with geographic distance, we convert the dialect similarity measure from a similarity matrix into a distance matrix by taking one minus the dialect similarity divided through the maximum value of 66 (Equation (2.1)). The resulting dialect distance matrix across all counties has a dimension of 439×439 , with elements ranging between 0 (dialect identity) to 1 (maximum dialect distance). The subscript s indicates the sending county and d the destination county. To illustrate, Figure 2.1 shows the dialect distance of all other counties to the city of Worms (Rhineland-Palatinate). The figure reveals that dialect distance is low for counties to the east, west, and north of Worms, but high for counties to the south of Worms. The map documents that counties closer to Worms are, on average, culturally more similar to Worms. However, if we draw a concentric circle around Worms, which we do when we control for geographic distance, there are counties on that circle that are culturally more and less similar to Worms. We exploit this variation in our empirical setup later on.

2.1.2 Internal Migration in Germany

Information on internal migration in Germany stems from the IAB Employment Panel (Schmucker and Seth, 2009). Based on the quarterly employment statistics of the Federal Employment Agency, the panel is a 2 percent subsample of the universe of employees who are subject to social security in Germany. Besides gross monthly wages, the data provide information on age, gender, educational attainment, nationality, and place of work and residence.⁹ Our sample period covers the years 1998 to 2006 and thus includes about 26 million quarterly observations from around 925,000 individuals. Since information on hours worked is not accurate in the IAB Employment Panel, we restrict our

⁹ To obtain the regional identifiers for the county of work and county of residence, we use the confidential weakly anonymous version of the scientific use file (Schmucker and Seth, 2009).

Figure 2.1
Dialect Distance – The Case of Worms



Notes: The figure shows dialect distance of all districts to the reference point Worms (20 quintiles of dialect distance). Degrees of dialect distance (from highest to lowest) are indicated by: white, grey, black.

analysis to full-time employed individuals. However, there are still workers who receive zero wages even if they are full-time employed. We follow Card, Heining and Kline (2013) and drop all workers with daily wages below €10. Another problem is that the wage data are top-coded at the social security maximum. The number of workers affected by this restriction in the full sample is of the order of 10 to 12 percent of male workers and 1 to 3 percent of female workers (Card, Heining and Kline, 2013). The literature proposes imputing the missing wage information by assuming a normal wage distribution (Dustmann, Ludsteck and Schoenberg, 2009; Card, Heining and Kline, 2013). However, we restrict our sample to include only persons who have moved between two quarters. We find that only about 2 percent of the movers, either one quarter before or after the move, report top-coded wages. Thus, in total, we have only about 4 percent of top-coded observations. Therefore, instead of using imputation methods, we check the robustness of our results by excluding this group and find that the results do not change (Column (1) of Table 2.3).¹⁰ Finally, we restrict our sample to German citizens only because it is not clear how dialect distance should affect those born abroad.

We define internal migrants as individuals who have changed their county of residence and their county of work between two consecutive quarters. In some cases, the information on county of residence and work is missing. In these cases, we allow for an administrative lag of one quarter and determine whether the person has moved by comparing the work and residence status of the person in the wave before the missing entry with the wave after the missing entry.¹¹ Our final sample contains 9,090 internal migrants. The internal migration rate in our sample is roughly 3 percent, which is comparable to official aggregate internal migration statistics for Germany.¹²

Panel A of Table 2.1 shows the distribution of wages four quarters before ($t - 1, t - 2, t - 3, t - 4$) and one quarter after the move ($t + 1$). Wages are in 2010 prices, that is, we adjust wages for the national consumer price index (Federal Statistical Office, 2014). The average monthly gross wage in 2010 prices before the move is €2,867 and increases by 3.9 percent to €2,980 after the move.

$$wage_{ict}^{indexed} = \frac{wage_{ict}}{index_c} \quad (2.2)$$

¹⁰ The social security data should only report wages until the social security maximum. However, there are a few cases in which the reported wage exceeded this amount. We restricted these cases back to the social security maximum. We also performed robustness checks by omitting the bottom and top 5 percent of the wage distribution and the results are not sensitive to this omission.

¹¹ Omitting individuals with an administrative lag from the sample or controlling for them with an indicator variable does not change the results.

¹² The average overall internal migration rate for the period 1998 to 2006 was 4.6 percent (own calculations based on official migration and population data of the Federal Statistical Office (2013)). Since our sample consists only of working individuals subject to social security, the internal migration rate in our sample is slightly lower.

Table 2.1
Summary Statistics

Variable	Dialect distance					
	Total sample		Below median		Above median	
	Mean (SD)	Min Max	Mean (SD)	Min Max	Mean (SD)	Min Max
<i>Panel A: Wage data</i>						
Wage in 2010 prices _(t+1)	2,980 (1,291)	240 5,692	2,857 (1,266)	248 5,692	3,098 (1,303)	240 5,692
Wage in 2010 prices _(t-1)	2,867 (1,285)	222 5,698	2,758 (1,257)	222 5,698	2,971 (1,302)	250 5,686
Wage in 2010 prices _(t-2)	2,852 (1,301)	223 5,692	2,744 (1,270)	223 5,654	2,956 (1,323)	225 5,692
Wage in 2010 prices _(t-3)	2,825 (1,319)	221 5,716	2,719 (1,288)	225 5,686	2,926 (1,340)	221 5,716
Wage in 2010 prices _(t-4)	2,806 (1,329)	221 5,716	2,705 (1,298)	227 5,670	2,903 (1,351)	221 5,716
Indexed wage in 2010 prices _(t+1)	5,271 (2,402)	300 15,836	5,180 (2,405)	300 14,992	5,358 (2,397)	319 15,836
Indexed wage in 2010 prices _(t-1)	5,200 (2,371)	273 14,398	5,034 (2,353)	273 14,398	5,358 (2,378)	467 13,838
Indexed wage in 2010 prices _(t-2)	5,174 (2,398)	273 14,476	5,010 (2,371)	273 14,475	5,331 (2,414)	341 13,915
Indexed wage in 2010 prices _(t-3)	5,129 (2,432)	225 14,570	4,968 (2,407)	225 14,570	5,282 (2,446)	431 13,930
Indexed wage in 2010 prices _(t-4)	5,100 (2,444)	227 14,586	4,951 (2,424)	227 14,586	5,243 (2,455)	359 13,928
<i>Panel B: Distance data</i>						
Dialect distance	0.372 (0.207)	0 0.833	0.189 (0.103)	0 0.364	0.548 (0.104)	0.379 0.833
Geographic distance (km)	200 (170)	1 818	76 (72)	1 595	318 (150)	15 818
<i>Panel C: Individual characteristics</i>						
Lowest and middle academic track, without VET	0.019	0 1	0.021	0 1	0.017	0 1
Lowest and middle academic track, with VET	0.523	0 1	0.572	0 1	0.477	0 1
Highest academic track	0.144	0 1	0.135	0 1	0.152	0 1
University	0.3	0 1	0.26	0 1	0.339	0 1
Education unknown	0.014	0 1	0.012	0 1	0.015	0 1
Male	0.557	0 1	0.57	0 1	0.545	0 1
Age	32.038 (8.049)	18 63	31.965 (8.307)	18 62	32.108 (7.794)	18 63
Experience	11.687 (8.083)	0 43	11.79 (8.335)	0 43	11.587 (7.835)	0 43
Industry change	0.588	0 1	0.578	0 1	0.598	0 1
Observations	9,090		4,444		4,646	

Notes: Summary statistics are based on the baseline sample. Only the first observed move is considered for individuals who moved several times. We have full information on 567 individuals, or 6.2 percent, who moved two times during our time period. Wage data and data on individual characteristics are drawn from the IAB Employment Panel (Schmucker and Seth, 2009). Wages are denoted in 2010 Euros by using the consumer price index from the Federal Statistical Office (2014). The dialect and geographic distance data are from Falck et al. (2012). Standard deviations are not computed for dummy variables. The variable t indicates the timing of the move: $t + 1$ denotes the first observation after the move and $t - 1$ denotes the quarter before the move. Experience represents potential labor market experience and is computed by $Age - 6 - years\ of\ schooling$. Years of schooling is assumed to be equal to 10 years for lowest and middle academic track without vocational education and training (VET), 13 years for lowest and middle academic track with VET, 13 years for highest academic track without VET, 15 years for highest academic track with VET, 17 years for university, and 10 years for education unknown. We merged highest academic track without VET and highest academic track with VET into one education category.

To account for the fact that moving to a culturally unfamiliar environment might also be capitalized in rents, we also calculate an index wage based on local rents. We use rental prices averaged over the years 2004 to 2008 as reported by the Federal Institute for Research on Building, Urban Affairs and Spatial Development, as well as by the IDN ImmoDaten GmbH. The rental prices are transformed into a price index ($index_c$) expressed in terms of the most expensive place, *Munich*. Thus, the index ranges between 1 for *Munich* and 0.35 for the county *Hof* in Northern Bavaria. Equation (2.2) gives the indexed gross wage (in 2010 prices) of individual i in county c at time t . The average monthly gross wage (relative to Munich) before the move is 5,200 and increases by 1.4 percent to 5,271 after the move (see Panel A of Table 2.1).

Another observation from Panel A of Table 2.1 is that the average wage for movers to counties farther away than the median dialect distance is higher than that of movers to counties closer than the median dialect distance. This is the case not only one quarter after the move but at each point in time. This suggests that these *far* movers have higher skills than *close* movers. However, these skills are also reflected in pre-migration wages.

Panel B of Table 2.1 shows descriptive statistics for the distance data. The geographic distance correlates with dialect distance.¹³ The mean geographic distance is 318 km (197.6 miles) for individuals who moved to a county farther away than the median dialect distance whereas the destination county is only 76 km (47.2 miles) away for the closer-than-median mover. On average, an internal migrant moves 200 km (124.3 miles) and experiences 0.372 in cultural distance by doing so.

The selection of individuals into moving across cultural borders can be seen in Panel C of Table 2.1. Above-median movers are more likely to have a university degree (33.9 percent vs. 26 percent) and to have attended the highest academic track in secondary school (15.2 percent vs. 13.5 percent). There is also a gender gap. The share of male migrants is higher in both the below-median mover group and in the above-median mover group. Age and (potential) labor market

¹³ The correlation between dialect distance and geographic distance is 0.7697. Appendix Figure A2.1 provides a graphical illustration. We construct the figure by portioning the dialect distance into 20 equal sized bins (5 percentage intervals) and compute the mean geographic distance for each of the bins. The relationship between both distances shows that a higher dialect distance is associated with a higher geographic distance. One standard deviation increase in the dialect distance is associated with an increase in geographic distance by 130 km (80.8 miles). The curvature follows an s-shaped curve, with an accelerating increase in geographic distance at low levels of dialect distance and decreasing increases in geographic distance at higher levels of dialect distance. This curvature is in line with the argument that dialect distance (or cultural distance) might explain non-linearity in geographic distance. The positive correlation between geographic distance and dialect distance makes it important to control for geographic distance in the following analysis. We also control for non-linearities in geographic distance in some specifications. This can be viewed as a conservative approach since it removes some of the non-linearity that might have its origin in culture.

experience are comparable across both groups.¹⁴ The average age of the movers is 32. The question arises whether the wage earned around age 30 is a meaningful reflection of an individual's lifetime productivity or earnings. Studies by Haider and Solon (2006) who look at the relationship of current and life-time earnings, and by Chetty et al. (2014) who look at the relationship of parental and child earnings, show that measures using wages at age 30 are fairly stable predictors of life-time earnings or intergenerational mobility, respectively. Finally, slightly less than 60 percent of the internal migrants change the industry in which they work when they move.

2.2 Empirical Strategy

2.2.1 Identification

The purpose of this chapter is to show whether and how much people value cultural familiarity to a county. Intuitively, the identification problem can be illustrated in Figure 2.1 where we plot the dialect distance for each county to the city of Worms. If we think of one exemplary circle around Worms, there are counties on that circle that are culturally closer to Worms – counties to the east, west, and north of Worms – and there are counties that are culturally farer away to Worms - counties to the south of Worms. The empirical question is whether internal migrants to the south (culturally farer away) demand higher wages than internal migrants to the north (culturally closer). If we could randomly allocate migrants to the north and the south of Worms, our treatment effect would be the mean difference between the north and the south in post-migration wages. However, the location choice is not random. For example, Bauernschuster et al. (2014) show that skilled and risk-loving people are more likely to cross cultural borders. We can control for the educational background of movers, however, we cannot directly control for unobserved individual characteristics, such as risk-aversion or innate ability, which are important determinants in the location choice. Thus, comparing unconditional post-migration wages of migrants who have moved to culturally closer regions with wages of migrants to culturally more farer regions is confounded by these unobserved individual characteristics.

To deal with self-selection into different locations, we adopt an estimation strategy from the labor economics literature on the effects of training programs on wages (e.g. Ashenfelter and Card, 1985; LaLonde, 1986). The basic idea in this strand of literature is to use pre-treatment wages to control for unobserved selection into programs. Comparing individuals with similar pre-treatment

¹⁴ Potential labor market experience is computed by $age - 6 - years\ of\ education$.

wage profiles should mitigate the selection problem because unobserved individual characteristics should already show up in pre-treatment wages. McKenzie, Gibson and Stillman (2010) evaluate the transferability of this estimation strategy to the context of gains to migration. They analyze the performance of different estimators in identifying the gains to migration causally. The authors make use of a special arrangement between New Zealand and Tonga in which the New Zealand government granted randomly the right to immigrate to Tongan applicants. After extensive surveys among applicants and non-applicants, McKenzie, Gibson and Stillman (2010) find evidence that applicants are positively selected along observed and unobserved characteristics. They show that in the New Zealand–Tonga case, a differences-in-differences estimator, taking the wage before migration into account, is among the better performing non-experimental estimators. However, they also show that this estimator does not recover experimental estimates, which is also true for instrumental variable estimators. However, our data and approach allows us to improve on their setup.

First, McKenzie, Gibson and Stillman (2010) only have one observation for pre-treatment wages. Kratz and Bruederl (2013) point out that internal migrants in Germany are also selected based on wage growth instead of wage levels. Thus, the parallel trends assumption of the differences-in-differences estimator is clearly violated. In addition, there is a one year gap between the pre- and post-treatment wage observation, which are collected by a survey. Instead, we use administrative quarterly panel data and several lags of pre-treatment wages as our control variables. This more detailed wage profile should control more accurately for unobserved individual characteristics. The approach should also control for the possibility that internal migrants are selected based on wage growth instead of wage levels. Second, McKenzie, Gibson and Stillman (2010) cite the labor economics literature by arguing that non-experimental estimators perform better when the probability for receiving the treatment is more balanced in the population (common support). This should be more the case in our exercise because we look at internal migration, which has lower barriers to migration than international migration. Third, we only look at the internal margin of migration, that is, where to move, whereas McKenzie, Gibson and Stillman (2010) look at the external margin, that is, whether to move at all. Conditioning on the sample of movers, all individuals are more likely to receive the same treatment, that is, to choose a familiar or unfamiliar region.

Local amenities, such as schools, transport infrastructure, health care providers, shopping alternatives, or leisure facilities, and also disamenities, such as pollution, congestion, and the like, are also capitalized in local wages and rents. However, the local amenity level should not bias our

estimate as long as the difference between amenities in two counties are not correlated with dialect distance. We check this assumption by controlling for several pair-wise county characteristics in the robustness checks (Column 5 of Table 2.3). Among other things, we include two differences in local amenities, that is, differences in weather conditions and differences in per-capita expenditures on local amenities (expenditures on schools, theaters, baths, and public parks). None of these pair-wise controls significantly alters the coefficient on dialect distance. Specifically for expenditures on local amenities, we find that higher per-capita expenditure levels in the destination than in the source county substitute for higher post-migration (indexed) wages, however, they do not affect the coefficient on dialect distance.

There are two potential biases that work against our hypothesis of a wage premium for moving to a culturally more unfamiliar environment: First, our data does not allow us to pick up the people in the county where they have grown up or got socialized. However, if we think of a complete random distribution of people in the extreme, that is, that actually no one is attached to the county where he or she is coming from, we should not observe a significant correlation between dialect distance and (conditional) post-migration wages. Second, Grogger (2011) finds wage discrimination against African Americans in the United States based on their speech patterns. If internal migrants in Germany are also discriminated in regions that are culturally farer away from their former home county, we should observe that they earn less instead of receiving a wage premium for moving to that more unfamiliar region. Thus, discrimination would work against our hypothesis of a wage premium. In that sense, we interpret our results as lower bound estimates of the effect of cultural distance on post-migration wages. This is underlined by the fact that we can only trace out effects from people that actually move; which are only 3 percent of the working population (see above). For non-movers, it seems that migration costs are so high that they do not consider moving at all, even though there are substantial differences in wages across German regions (Buettner and Ebertz, 2009).

2.2.2 Estimation Setup

Following the identification discussion from above, we estimate the following wage regression:

$$\begin{aligned} \log wage_{idt+1}^{indexed} = & \alpha + \beta \text{dialect distance}_{sd} + \sum_{j=1}^4 \gamma_j \log wage_{ist-j}^{indexed} \\ & + \lambda \text{geographic distance}_{sd} + \phi X_{it-1} + \mu_t + \varepsilon_{idt+1} \end{aligned} \quad (2.3)$$

The log of the indexed wage (see Equation (2.2)) received by internal migrant i in destination county d in the quarter after the move, that is, at $t + 1$, is regressed on the dialect distance between the origin county s and destination county d (see Equation (2.1)). We control for the (linear) geographical distance between the two counties¹⁵ as well as gender, education (five dummies), experience (and its square), and a dummy indicating an industry change accompanying the move.¹⁶ The quarter-year fixed effects μ_t capture all time-specific shocks.¹⁷ Finally, ε_{idt-1} is an idiosyncratic error term. We use robust standard errors throughout the analysis.¹⁸

The coefficient of interest is β , which is the wage premium in percent for overcoming one unit in dialect distance. The identification assumption under which β reports the causal effect of dialect distance on the wage after the move requires that dialect distance not be correlated with unobserved individual characteristics. Guided by the discussion from above, we argue that we can control for unobserved individual characteristics to a large extent by including the last four quarterly wages before the move, $\sum_{j=1}^4 \log wage_{ist-j}^{indexed}$.¹⁹ The identifying assumption, under which β describes a causal effect is that, conditional on the pre-treatment wage profile and observed individual characteristics, the move to a familiar versus unfamiliar region is as good as random. Although there could be still omitted variables that bias the coefficient in one way or the other (see discussion above), we are confident that this strategy leads to a close to causal interpretation of the estimated treatment effect.

¹⁵ We deal with non-linear geographic distance in Section 2.3.2.

¹⁶ An alternative for using a dummy for the industry change is to use industry fixed effects. The results do not change.

¹⁷ In a robustness check, we also use source county fixed effects. The results do not change. Appendix Table A2.1 provides the results.

¹⁸ In various robustness checks, we clustered standard errors at various levels. However, clustering at the origin county \times destination county, the origin county, or the destination county yield almost the same standard errors.

¹⁹ Using four quarterly pre-migration wages as controls is somewhat arbitrary. We check this specification by changing the structure of the pre-migration wage profiles in Section 2.3.2.

2.3 Results

2.3.1 Wage Premium for Overcoming Dialect Distance

Table 2.2 sets out our baseline results. The sample is restricted to the internal migrants' first move that we observe in the data.²⁰ Column (1) shows the unconditional association between dialect distance and post-migration log indexed wages. The association is positive and highly significant, meaning that a higher dialect distance leads to higher post-migration indexed wages. Column (2) shows the unconditional association between geographical distance and post-migration log indexed wages. The coefficient is also positive but not significant. Including both variables in Column (3) more than doubles the coefficient on the dialect distance and decreases the coefficient on geographic distance, which is significantly negative now. Thus, the small wage premium implied by the positive coefficient on geographic distance in Column (2) is completely captured and reversed by dialect distance. The negative coefficient on geographic distance can be explained by the construction of the dependent variable, which has the (index of the) rental price of the destination county in the denominator. We come back to this issue at the end of this section.

In Column (4), we add the last four quarterly pre-migration log indexed wages to control for unobserved individual characteristics that could drive unobserved self-selection into regions. The coefficient on dialect distance drops by almost a factor of four and almost all pre-migration wages are highly significant predictors of post-migration wages. This indicates that self-selection is indeed a serious issue and that neglecting pre-migration wages in the regression will lead to an upwardly biased effect of dialect distance. After controlling for pre-migration wage profiles, adding further control variables in Column (5) does not lead to a statistically significant change in the coefficient on dialect distance. The coefficient decreases slightly to 0.075 and is still highly significant. Thus, a one standard deviation increase in dialect distance (about 0.2) increases the post-migration indexed wage by about 1.5 percent.

In Column (6) of Table 2.2, we provide an alternative specification in which we relate the log wage to the dialect distance and insert for the log rental price in the destination and the source county on the right-hand side of the regression. We also use non-indexed quarterly pre-migration wages as controls. This specification gives us an indication whether the effect of dialect distance comes from a tradeoff in wages (nominator of the dependent variable) or in rental prices (denominator of

²⁰ We analyze multiple-time movers in Section 2.3.3.

Table 2.2
Dialect Distance and Post-Migration Wages

<i>Dependent variable:</i>	<i>log indexed wage</i> _(<i>t</i>+1)					<i>log wage</i> _(<i>t</i>+1)
	(1)	(2)	(3)	(4)	(5)	(6)
Dialect distance	0.1691*** (0.0249)		0.3872*** (0.0389)	0.0952*** (0.0302)	0.0747*** (0.0287)	0.0486** (0.0234)
Geographic distance (km)		0.000018 (0.00003)	-0.00035*** (0.00005)	-0.00012*** (0.00004)	-0.00013*** (0.00003)	-0.000002 (0.000029)
Log (indexed) wage _(<i>t</i>-1)				0.342*** (0.0272)	0.2608*** (0.0256)	0.3799*** (0.0273)
Log (indexed) wage _(<i>t</i>-2)				0.1137*** (0.0334)	0.088*** (0.0301)	0.0984*** (0.0307)
Log (indexed) wage _(<i>t</i>-3)				-0.0134 (0.0376)	-0.0098 (0.0334)	0.0178 (0.035)
Log (indexed) wage _(<i>t</i>-4)				0.2000*** (0.0293)	0.1509*** (0.0267)	0.0897*** (0.0286)
Log rental price (destination)						0.1984*** (0.0144)
Log rental price (source)						-0.0216 (0.0158)
Lowest and middle academic track, with VET					0.0869*** (0.0332)	0.0635** (0.029)
Highest academic track					0.2002*** (0.0346)	0.1685*** (0.0304)
University					0.3537*** (0.0344)	0.2939*** (0.0305)
Education unknown					0.167*** (0.0473)	0.1114*** (0.0408)
Male					0.0928*** (0.0081)	0.0765*** (0.007)
Experience					0.0124*** (0.0017)	0.0059*** (0.0015)
Experience squared × 10 ⁻⁴					-2.7144*** (0.5098)	-1.2637*** (0.4391)
Industry change					-0.0542*** (0.0073)	-0.0366*** (0.0061)
Quarter-year fixed effects	yes	yes	yes	yes	yes	yes
Observations	9,090	9,090	9,090	9,090	9,090	9,090
R ²	0.0166	0.0115	0.0225	0.4399	0.4994	0.4997

Notes: The indexed wage is the gross wage in 2010 prices divided by the index of the rental rate. Only the first observed move is considered for individuals who moved several times. Subscripts indicate x quarters before the move ($t - x$) and one quarter after the move ($t + 1$). The omitted education category is lowest and middle academic track without VET. Quarterly pre-migration wages in Column (6) are not indexed. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the dependent variable). We find that the log rental price in the destination county is a significant predictor of post-migration log wages. Thus, wages in areas with high rental rates are also relatively high. The rental price in the source county is not associated with the post-migration wage. Given rental prices in the destination and the source county, increasing dialect distance by one standard deviation increases the post-migration wage by about 1 percent.²¹ This model also reveals that geographic distance is not correlated with post-migration wages when we condition on dialect distance, rental prices, and pre-migration wage profiles.²² The result suggests that the negative coefficient on geographic distance in Columns (3) to (5) originates from a correlation between geographic distance and higher rental prices in the destination county. An explanation for this finding is that longer distance moves are associated with moves toward bigger cities. Internal migrants move on average 269 km when one of the five biggest cities in Germany (Berlin, Hamburg, Munich, Cologne, and Frankfurt) is the destination. For all other moves, we observe that people are only moving 179 km on average.

To benchmark our results, we compare the effect size found in Table 2.2 to the average increase in wages from before to after the move and to the most recent collective wage agreements. The indexed wages of internal migrants increase, on average, by about 1.4 percent from the quarter before the move to the first quarter after the move. This implies that the wage premium necessary to compensate for one standard deviation in dialect distance is about 107 percent of the average indexed wage gain in 2010 prices from internal migration. In terms of non-indexed wages, we observe an increase from the quarter before the move to the quarter after the move by 3.9 percent. Thus, in 2010 nationwide prices, the wage premium has to be 26 percent of the average wage gain in 2010 prices from internal migration. The effect size of 1 percent per standard deviation in non-indexed wages is sizable when compared to estimated gains to internal migration in Germany, which are around 3 percent for the average mover (Kratz and Bruederl, 2013; Lehmer and Ludsteck, 2011). The treatment effect is also sizable when compared to the most recent (2013) collective wage agreements in Germany. For example, in the public sector, there was an agreed upon increase of 2.65 percent in nominal wages (ver.di, 2013) and in manufacturing, an increase of 3.4 percent in nominal wages was negotiated (IG Metall, 2013).

²¹ The results are comparable when we use a regional price index instead of rental rates. However, we think that rental rates are better able to capture amenities than are price levels.

²² Appendix Table A2.2 shows that the correlation between geographic distance and non-indexed post-migration wages is positive and highly significant. Adding pre-migration wages and rental prices in the destination and source county does not change this result. However, once we include dialect distance, the coefficient on geographic distance turns negative (and insignificant), which shows that the wage premium obtained from geographic distance is entirely due to overcoming cultural barriers.

Figure 2.2 provides a graphical illustration of the relationship between dialect distance and post-migration indexed wages. The figure shows an added-variable plot where we use only the variation in the post-migration indexed wage and the dialect distance that remains after taking account of the full control set of the baseline model in Column (5) of Table 2.2.²³ The figure reveals an almost linear relationship between residual dialect distance and the conditional post-migration indexed wage once the dialect distance crosses the 10th percentile (first two bins). This ensures that the effect is not driven by outlier observations at the top and the bottom of the dialect distance distribution and that there are no substantial non-linearities.²⁴

2.3.2 Robustness Checks

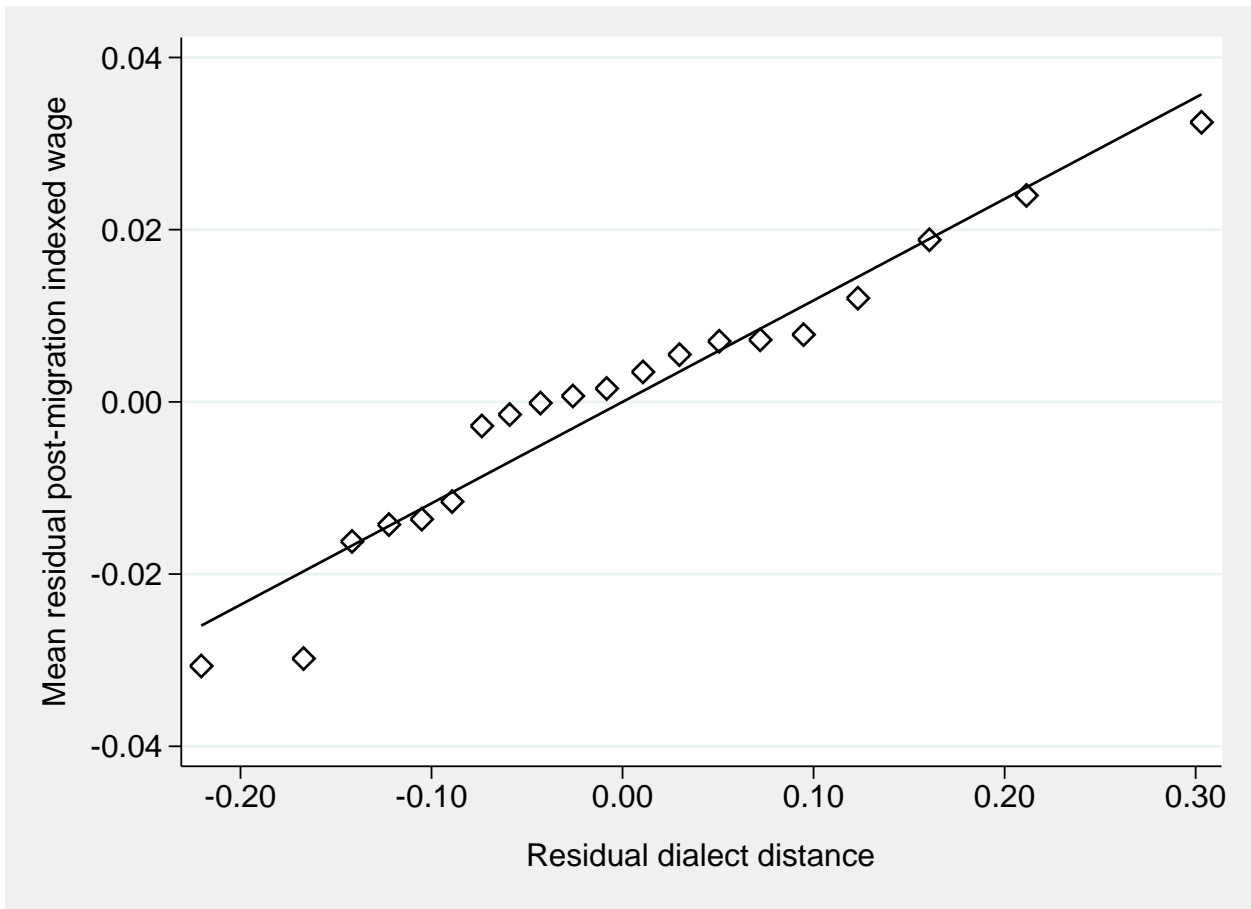
In this section, we conduct a couple of robustness checks. We first check whether our results are driven by top-coded wages. As mentioned above, the number of top-coded wages among the movers is relatively low compared to the number of top-coded wages in the overall working population. Column (1) of Table 2.3 shows the results of omitting internal migrants who report a wage that is at or above the social security maximum at either directly after the move ($t + 1$) or at some time before the move ($t - 1$ to $t - 4$). However, top-coded wage observations do not affect the coefficient on dialect distance. It could also be that the observed effect is driven by moves from and to large agglomerations, which might differ from other counties in terms of amenities. To check this, we exclude the five largest cities in Germany (Berlin, Hamburg, Munich, Cologne, and Frankfurt) as destination and source counties (Column (2) of Table 2.3). Even though these cities account for almost a quarter of the sample, the coefficient of the regression stays virtually the same. As contrast, restricting the sample of movers toward these five largest cities, dialect distance is only a weak explanatory variable (coefficient on dialect distance is 0.0364 and insignificant). Interestingly, the coefficient on geographic distance in this sample is positive and marginally significant at the 10 percent level. This indicates that moves toward the big agglomerations are more driven by economic rather than cultural motives.

In Column (3) of Table 2.3, we test the robustness of the effect by including dummies for moving from East to West Germany, West to East Germany, and moving within East Germany

²³ The figure is a binned scatterplot where the residual dialect distance is binned into 20 equal-sized bins. The mean of the conditional post-migration indexed wage within each bin is then computed and plotted against the dialect distance.

²⁴ Nevertheless, we have also estimated models considering non-linear effects in the dialect distance by adding a squared term to the model. The effects size for the median dialect distance is equal to 1.6 percent (significant at 1 percent) increase for one standard deviation increase in dialect distance and, thus, very close to the baseline effect of 1.5 percent.

Figure 2.2
 Added-Variable Plot of Dialect Distance and the Post-Migration Wage



Notes: The figure shows a binned scatterplot of residual post-migration indexed wages on residual dialect distance. The residual post-migration indexed wages are obtained from residuals from regressions on the geographic distance, quarter-year fixed effects, the last four quarterly pre-migration wages, education dummies, male, experience, experience squared, and an industry change dummy. The residual dialect distance is obtained from residuals from regressions on the same control set. The figure is constructed by binning dialect distance into 5-percentile point bins (so that there are 20 equal-sized bins) and computing the mean conditional post-migration indexed wage within each bin. The slope of the regression of post-migration wages, conditional on the full set of control variables is equal to 0.075 (0.029). The coefficient is significant at a 1 percent level.

(moving within West Germany is the baseline category). This specification controls for important cultural differences between German regions. However, the coefficient on dialect distance is not affected.

Unfortunately, we do not know where the individuals in our sample were born and socialized, raising the concern that a migrant might not be attached to the county he or she left.²⁵ In this case, however, we would not expect to see any effect of cultural distance on post-migration wages. Thus, our baseline results should indicate a lower bound of the effect of cultural distance on migration

²⁵ For example, migration flows in the aftermath of World War II (e.g., refugees, ethnic Germans, etc.) might have substantially involuntarily reshuffled the German population with respect to cultural roots.

Table 2.3
Robustness Checks

	<i>Dependent variable: log indexed wage_(t+1)</i>				
	(1)	(2)	(3)	(4)	(5)
	Top-coded	5 largest cities	East-west dummies	7 years at origin	Pair-wise controls
Dialect distance	0.0721** (0.0295)	0.0741** (0.0303)	0.0783*** (0.0291)	0.1295** (0.0653)	0.0985*** (0.0282)
Geographic distance (km)	-0.000118*** (0.000035)	-0.000149*** (0.000038)	-0.000153*** (0.000036)	-0.000158** (0.000078)	-0.00012*** (0.00004)
Log indexed wage _(t-1)	0.2569*** (0.0258)	0.286*** (0.0297)	0.2603*** (0.0256)	0.3601*** (0.0623)	0.2980*** (0.0258)
Log indexed wage _(t-2)	0.0945*** (0.03)	0.0903** (0.0357)	0.0877*** (0.0301)	-0.0103 (0.0783)	0.0806*** (0.0300)
Log indexed wage _(t-3)	-0.0143 (0.0335)	0.0304 (0.0403)	-0.0099 (0.0333)	0.0937 (0.0586)	-0.0090 (0.0329)
Log indexed wage _(t-4)	0.1467*** (0.0269)	0.1224*** (0.0317)	0.1512*** (0.0266)	0.0692* (0.0363)	0.1449*** (0.0258)
Control variables	yes	yes	yes	yes	yes
Quarter-year fixed effects	yes	yes	yes	yes	yes
East-west fixed effects	–	–	yes	–	–
Pair-wise controls	–	–	–	–	yes
Observations	8,727	6,946	9,090	1,815	9,090
R ²	0.4856	0.5395	0.5007	0.5327	0.5372

Notes: The indexed wage is the gross wage in 2010 prices divided by the index of the rental rate. Only the first observed move is considered for individuals who moved several times. Subscripts indicate x quarters before the move ($t - x$) and one quarter after the move ($t + 1$). *Control variables:* education dummies, male, experience, experience squared, industry change. Column (1) drops all movers with top-coded wages (at or above the social security maximum) at either $t + 1$ or $t - 1$ to $t - 4$. Column (2) drops the five largest cities (Berlin, Hamburg, Munich, Cologne, Frankfurt) as destination and source counties. Column (3) includes fixed effects for movers from East Germany to West Germany, from West Germany to East Germany, and moving within East Germany (moving within West Germany is the baseline category). Column (4) conditions the sample on having lived at least seven years in the county of origin. Column (5) includes several pair-wise controls: log difference in slope, historical rail distance, different religion dummy, difference in share Catholics, difference in historical industry structure, difference in the current industry structure, difference in temperature, difference in sunshine duration, difference in precipitation, and difference in per-capita expenditures on local amenities (expenditures on schools, research, theaters, concerts, sport facilities, public parks, and baths). Data on pair-wise controls come from Falck et al. (2012), from the Deutscher Wetterdienst (DWD) for the climate data, and from the Federal Statistical Office for expenditures on local amenities. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

wage gains. To get some sense of the extent to which we underestimate the true effect of cultural distance on migration wage gains, we restrict the sample to those internal migrants who have not changed their place of work or residence for a reasonable period before the move. Living in a region for a longer period could make a person more attached to that county than to the former home county (Burchardi and Hassan, 2013). Given that our panel covers nine years, we restrict our analysis to those 1,815 individuals who resided and worked in the origin county for at least seven years and then moved to a different county during the last two years of our panel. The result of this procedure is shown in Column (4) of Table 2.3. The coefficient of dialect distance almost doubles, providing more support for our argument that the baseline effect is more of a lower bound and that being attached to a certain area for a longer period increases the cost of moving.

Table 2.4
Adding More/Other Pre-Migration Wages

	<i>Dependent variable: log indexed wage_(t+1)</i>		
	(1)	(2)	(3)
Dialect distance	0.1106*** (0.038)	0.0858** (0.0372)	0.0846** (0.0386)
Log indexed wage _(t-4)	0.2710*** (0.0211)		
Log indexed wage _(t-8)	0.0560** (0.0231)		
Log indexed wage _(t-12)	0.0851*** (0.0176)		
Mean log indexed wage _{(t-1) to (t-4)}		0.4161*** (0.0279)	
Mean log indexed wage _{(t-5) to (t-8)}		-0.0281 (0.0300)	
Mean log indexed wage _{(t-9) to (t-12)}		0.1017*** (0.0204)	
Log indexed wage _(t-5)			0.0331 (0.0604)
Log indexed wage _(t-6)			-0.0466 (0.0663)
Log indexed wage _(t-7)			0.0859 (0.0665)
Log indexed wage _(t-8)			-0.0914 (0.0694)
Log indexed wage _(t-9)			0.0612 (0.0502)
Log indexed wage _(t-10)			0.0659 (0.0443)
Log indexed wage _(t-11)			-0.0123 (0.0366)
Log indexed wage _(t-12)			0.0317 (0.0311)
Control variables	yes	yes	yes
Quarter-year fixed effects	yes	yes	yes
Geographic distance	yes	yes	yes
Log indexed wages _(t-1,t-2,t-3,t-4)	-	-	yes
Observations	5,411	5,411	4,681
R ²	0.495	0.519	0.5293

Notes: The indexed wage is the gross wage in 2010 prices divided by the index of the rental rate. Only the first observed move is considered for individuals who moved several times. Subscripts indicate x quarters before the move ($t - x$) and one quarter after the move ($t + 1$). *Control variables:* education dummies, male, experience, experience squared, industry change. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The last robustness check (Column (5) of Table 2.3) introduces various pair-wise historical and contemporaneous controls between the counties of origin and destination that might be correlated with both migration flows and historical dialect distance. We include the log difference in slope, the historical rail distance, a dummy that is 1 when the dominant religion is not the same in both counties, the difference in share Catholics, the difference in the historical industry structure, the difference in the current industry structure, the difference in per-capita expenditures on local amenities, and climate controls such as the difference in temperature, difference in sunshine duration, and difference in precipitation.²⁶ None of these controls significantly changes the coefficient on dialect distance.²⁷

A crucial assumption is that the four quarterly pre-migration wages sufficiently capture the migrant's unobserved ability. Table 2.4 introduces alternative specifications regarding the structure of the pre-migration wage profile. Column (1) of Table 2.4 includes pre-migration wages one, two, and three years before the move. The coefficient on dialect distance substantially increases, which indicates that a yearly pre-migration wage profile does not capture unobserved individual characteristics better than the quarterly pre-migration wage profile from the baseline model. The next specification in Column (2) uses the average yearly pre-migration wages from the last three years as control variables. The coefficient on dialect distance is almost the same as in the baseline model. In the last column of Table 2.4, we include the quarterly pre-migration wages of the last three years prior to the move. This specification also shows a slightly larger coefficient on the dialect distance compared to the baseline model in Table 2.2.

The previous robustness check indicates that adding pre-migration wages beyond four quarters do not significantly change the results. In Table 2.5, we provide another specification check by shifting the timing of the move to two and three years prior to the move (placebo treatment). At that time, people should not know yet that they are moving two or three years later. Therefore, the dialect distance at the time of the move should not be able to predict wages in the following

²⁶ Data on log difference in slope, the historical rail distance, a dummy for a different religion, the difference in share Catholics, the difference in the historical industry structure, the difference in the current industry structure are taken from Falck et al. (2012). Climate data come from the Deutscher Wetterdienst (DWD). We use long-term averages from 1961 to 1990. We mapped all weather monitoring stations to counties and calculated averages. We use state averages for missing county observations. Data on differences in per-capita expenditures on local amenities come from a special statistical evaluation for the year 2004 from the Federal Statistical Office. These expenditures include expenditures on schools, research, theaters, concerts, sport facilities, public parks, and baths.

²⁷ We also run a regression by adding the current average bilateral migration flow (2000 – 2006) to the model in Column (5) of Table 2.3. Even though the bilateral migration flow might be a bad control, the coefficient on dialect distance decreases only slightly to 0.0778 and is still highly significant. The coefficient on the migration flow is, as expected, highly significant negative, indicating that the current migration flow decreases indexed post-migration wages substantially.

quarter. If we would find that the dialect distance is able to predict wages prior to the move, we would be worried that these people are on different wage growth trajectories long before they decide to actually migrate. But Table 2.5 shows that this is not the case. The coefficient on the dialect distance is close to 0 and insignificant. Especially the indexed wage one quarter for the placebo move is close to one, which signals high persistence in wages. This placebo treatment analysis gives more confidence that dialect distance, conditional on pre-treatment wage profiles and observable factors, is not capturing unobserved individual characteristics.

Table 2.5
Placebo Treatment

<i>Dependent variable: log indexed wage_(t-k)</i>		
	(1)	(2)
	<i>k</i> = 8	<i>k</i> = 12
Dialect distance	-0.0256 (0.0174)	0.0097 (0.0187)
Geographic distance (km)	0.000043* (0.000023)	-0.000002 (0.000025)
Log indexed wage _(t-k-1)	0.7793*** (0.0562)	0.8600*** (0.0323)
Log indexed wage _(t-k-2)	0.0651 (0.0543)	0.0244 (0.0384)
Log indexed wage _(t-k-3)	0.0469 (0.0343)	0.0517* (0.0302)
Log indexed wage _(t-k-4)	0.0282 (0.0223)	0.0278*** (0.0105)
Control variables	yes	yes
Quarter-year fixed effects	yes	yes
Observations	5,436	3,815
<i>R</i> ²	0.9268	0.9476
Coefficient in baseline	0.0933** (0.0383)	0.1099** (0.0474)

Notes: The indexed wage is the gross wage in 2010 prices divided by the index of the rental rate. Only the first observed move is considered for individuals who moved several times. Subscripts indicate *x* quarters before the move ($t - x$) and one quarter after the move ($t + 1$). *Control variables:* education dummies, male, experience, experience squared, industry change. The *coefficient in baseline* gives the coefficient on dialect distance from the baseline regression of log indexed wage_(t+1) on the full control set restricted to the respective sample. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The next concern is the possibility that dialect distance captures non-linearities in geographic distance that do not have their origin in culture. Table 2.6 sets out several specifications that include non-linear geographic distance measures. Column (1) replicates the baseline regression for means of comparison. Column (2) includes the geographic distance of power two and three. The coefficient on dialect distance increases slightly, which indicates that dialect distance does not capture strong non-linearities in geographic distance. However, it could be that geographic distance is an insufficient distance proxy for dialect distance. Therefore, in Columns (3) and (4) of Table 2.6,

we use the travel distance between counties by car in minutes as an alternative geographic distance measure.²⁸ Column (3) shows a specification in which we include the travel distance instead of the geographic distance. The coefficient of dialect distance decreases but remains significant. When we include travel distance to the power two and three in Column (4), we see that the coefficient increases slightly again compared to Column (3). However, travel time could be affected by dialect distance because it is very likely that transportation hubs and networks have developed along historical travel routes. Therefore, it is likely that some of the effect that should be attributed to dialect distance actually goes through travel distance. Nevertheless, the results of this exercise indicate that there are only minor nonlinear effects, if any, of geographic distance that are picked up by dialect distance.

Table 2.7 uses alternative measures of dialect distance to check the robustness of our results. To this point, we have used a metric measure of dialect distance. However, it could be that cultural space is dependent not only on gradual differences but on categorical ones. That is, the decision to move could be due to a difference between, for example, *Swabian* and *Bavarian* as such and not to the actual gradual difference between the counties within the Swabian and Bavarian region. To test for the impact on migration of categorical differences between smaller regions, we use a classification introduced by Lameli (2013) that captures the most prominent 13 dialect areas in Germany.²⁹ Column (1) of Table 2.7 sets out the results. The coefficient is positive and significant. A one standard deviation in the dialect distance by language area (1.04) leads to 1.11 percent higher post-migration indexed wages.

As the most important linguistic difference between German dialects is that between Low German (northern part of Germany) and High German (southern part), we further construct a dummy that substantiates the particular locality of the counties and tests for movements within the two larger areas of Low German and High German. Column (2) of Table 2.7 includes a dummy for moving from a High German county to a Low German county, a dummy for moving from a Low German county to a High German county, and a dummy for moving from a High German county to another High German county. The omitted category is moving from a Low German county to another Low German county. The results show that the effect of dialect distance remains robust when testing for the north-south distinction. We find, however, a slight north-south divide, indicating the relevance of a categorical conceptualization of cultural space.

²⁸ Data are provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development.

²⁹ The measure results from bootstrapped hierarchical cluster analysis, based on the measurement of linguistic similarity of German counties.

Table 2.6
Non-Linearities in Distance

	<i>Dependent variable: log indexed wage_(t+1)</i>			
	(1)	(2)	(3)	(4)
Dialect distance	0.0747*** (0.0287)	0.0846*** (0.0324)	0.0603** (0.0291)	0.0640** (0.0315)
Geographic distance (km)	-0.00013*** (0.00003)	0.00002 (0.00018)		
Travel distance (min)			-0.00014*** (0.00005)	0.00029 (0.00025)
Geographic distance (km) squared $\times 10^{-5}$		-0.0924 (0.064)		
Geographic distance (km) cubic $\times 10^{-5}$		0.00012* (0.00007)		
Travel distance (min) squared $\times 10^{-3}$				-0.002** (0.001)
Travel distance (min) cubic $\times 10^{-5}$				0.0003** (0.0001)
Log indexed wage _(t-1)	0.2608*** (0.0256)	0.2608*** (0.0255)	0.2614*** (0.0256)	0.2614*** (0.0255)
Log indexed wage _(t-2)	0.088*** (0.0301)	0.0874*** (0.03)	0.0882*** (0.0301)	0.0868*** (0.0301)
Log indexed wage _(t-3)	-0.0098 (0.0334)	-0.0095 (0.0334)	-0.0099 (0.0335)	-0.0091 (0.0334)
Log indexed wage _(t-4)	0.1509*** (0.0267)	0.1514*** (0.0267)	0.1512*** (0.0268)	0.1514*** (0.0267)
Control variables	yes	yes	yes	yes
Quarter-year fixed effects	yes	yes	yes	yes
Observations	9,090	9,090	9,090	9,090
R^2	0.4994	0.4997	0.4991	0.4994

Notes: The indexed wage is the gross wage in 2010 prices divided by the index of the rental rate. Only the first observed move is considered for individuals who moved several times. Subscripts indicate x quarters before the move ($t - x$) and one quarter after the move ($t + 1$). *Control variables:* education dummies, male, experience, experience squared, industry change. Travel distance is the travel time by car in minutes between counties. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2.7
Alternative Dialect Measures

	<i>Dependent variable: log indexed wage_(t+1)</i>	
	(1)	(2)
Dialect distance by language area	0.0106** (0.005)	
Dialect distance		0.0582* (0.0307)
Moving from High German to Low German		0.0847*** (0.0148)
Moving from Low German to High German		-0.0955*** (0.0144)
Moving from High German to High German		-0.0425*** (0.0095)
Geographic distance (km)	-0.000104*** (0.000031)	-0.000131*** (0.000033)
Log indexed wage _(t-1)	0.2619*** (0.0255)	0.2687*** (0.0252)
Log indexed wage _(t-2)	0.0874*** (0.0301)	0.0866*** (0.0298)
Log indexed wage _(t-3)	-0.0095 (0.0335)	-0.0077 (0.0331)
Log indexed wage _(t-4)	0.151*** (0.0267)	0.1476*** (0.0265)
Control variables	yes	yes
Quarter-year fixed effects	yes	yes
Observations	9,090	9,090
R ²	0.4992	0.5093

Notes: The indexed wage is the gross wage in 2010 prices divided by the index of the rental rate. Only the first observed move is considered for individuals who moved several times. Subscripts indicate x quarters before the move ($t - x$) and one quarter after the move ($t + 1$). *Control variables:* education dummies, male, experience, experience squared, industry change. *Moving from High German to Low German* indicates a move from a county in which mostly High German is spoken to a county in which mostly Low German is spoken. *Moving from Low German to High German* indicates a move from a county in which mostly Low German is spoken to a county in which mostly High German is spoken. *Moving from High German to High German* indicates a move from a county in which mostly High German is spoken to a county in which mostly High German is spoken. The omitted category is *Moving from Low German to Low German*. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

2.3.3 Effect Heterogeneity

The question arises as to whether there is a group of individuals that is driving the baseline results. To answer this question, we split in Table 2.8 the sample by age (Panel A), gender and education (Panel B), education \times gender (Panel C), geographic distance of the move (Panel D), whether the wage received before the move is above or below the average wage in the specific occupation (Panel E), and occupational change (Panel F).

Panel A of Table 2.8 stratifies the sample between young (below age 30) and older (above age 30) movers. In the first column, we look only at movers who are 30 or younger to discover whether age plays a crucial role in overcoming cultural distance, as argued by Schwartz (1973). The coefficient is large and significant for these younger movers. The coefficient on dialect distance for older movers in the second column is smaller and not significant. This indicates that our results are more driven by young movers than by older movers, even though the difference between both coefficients is not significant. Schwartz (1973) further argues that the interaction of geographic distance with age should indicate the importance of the psychic costs of migration. Therefore, in a different specification (not shown), we interacted geographic distance with age and find that the interaction is not significant. The effect of dialect distance remains therefore unchanged, indicating that dialect distance better captures the psychic costs of migration than does an interaction between geographic distance and age.

Panel B of Table 2.8 shows that the wages of men are more responsive to culture than those of women. Possibly this is because in most families the male adult is the household head and his place of work largely determines where the family lives. We also see that low- and medium-qualified migrants find culture more of a barrier to migration than do higher qualified migrants.³⁰ However, the difference between the groups is not large. Panel C shows that within the group of men, it is again the group of lower qualified migrants that shows a larger coefficient, but the differences are not significant. The results for women are insignificant again and the coefficient for lower qualified women is slightly higher than for higher qualified women. Panel D reveals that the effect comes mainly from shorter-distance moves, that is, moves less than 300 km from the former home county. Thus, the wage increases from moving to a culturally more distant county are not driven by long-distance moves as one might have expected.

³⁰ The group of low- and medium-qualified migrants consists of those with a degree from the lowest and middle academic track with and without vocational education and training (VET). We also include people for whom level of education is unknown. However, the picture does not change by omitting this group. The group of high-qualified people is comprised of those having a degree from the highest academic track or a university degree.

Table 2.8
Effect Heterogeneities

	< 30		≥ 30	
<i>Panel A: Age</i>	0.0930** (0.0404)		0.0494 (0.0407)	
Observations	4,384		4,706	
R^2	0.3948		0.5042	
	Gender		Education	
	Men	Women	Low, medium	High
<i>Panel B: Gender and education</i>	0.1046*** (0.0367)	0.0367 (0.0451)	0.0844** (0.0406)	0.0669 (0.041)
Observations	5,063	4,027	5,051	4,039
R^2	0.5057	0.4027	0.3615	0.4351
	Men		Women	
	Low, medium	High	Low, medium	High
<i>Panel C: Gender \times education</i>	0.1143** (0.0548)	0.0934* (0.0492)	0.0519 (0.0598)	0.0215 (0.0700)
Observations	2,727	2,336	2,324	1,703
R^2	0.3522	0.3872	0.3377	0.3457
	< 200 km	< 300 km	≥ 200 km	≥ 300 km
<i>Panel D: Geographic distance</i>	0.0984** (0.0402)	0.1179*** (0.0356)	0.0534 (0.0545)	-0.0279 (0.0702)
Observations	5,325	6,575	3,765	2,515
R^2	0.5155	0.5112	0.4862	0.4822
	Average wage _(t-1 to t-4) compared to average national-level occupational wage _(t-1 to t-4)		Average wage _(t-1 to t-4) compared to average county-level occupational wage _(t-1 to t-4)	
	Above	Below	Above	Below
<i>Panel E: Compared to occupational wage</i>	0.1141*** (0.0360)	0.0129 (0.0465)	0.0884** (0.0346)	0.0404 (0.0496)
Observations	5,225	3,865	5,416	3,674
R^2	0.4977	0.3759	0.5153	0.3817
	Occupation available	Occupational change		
		Switchers	Stayers	
<i>Panel F: Occupational change</i>	0.0806** (0.0313)	0.1154** (0.0506)	0.0423 (0.0383)	
Observations	7,337	3,479	3,858	
R^2	0.5089	0.4638	0.5757	

Notes: Each panel shows the coefficient on dialect distance from a regression on the baseline model. Dependent variable in all specifications is the *log indexed wage*_(t+1). The indexed wage is the gross wage in 2010 prices divided by the index of the rental rate. Only the first observed move is considered for individuals who moved several times. Subscripts indicate x quarters before the move ($t-x$) and one quarter after the move ($t+1$). All regressions include geographic distance, four quarterly pre-migration indexed wages, education dummies, male, experience, experience squared, industry change, and quarter-year fixed effects. In Panels B and C, low and medium education corresponds to the lowest and middle academic track with and without VET, plus unknown education. High education corresponds to the highest academic track plus university education. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel E of Table 2.8 splits the sample by those who have earned a higher or lower wage compared to the average wage in their occupation before the move. The rationale for this split is that those who have earned a wage that is lower than the comparison wage in their occupation might move more for economic reasons and do not put so much emphasis on culture. However, those who have earned a higher wage than is typically achieved in their occupation might put a higher weight on culture.³¹ To test this hypothesis, we compute average occupational wages at the national level (Columns (1) and (2)) and at the county-of-origin level (Columns (3) and (4)). Then, we compare the average wage of the four pre-migration periods (quarters) to the average occupational wage for the same period. Using either of the two occupational wages, we see that the baseline effect is mainly driven by internal migrants who earn above occupational wages before the move. This indicates that movers who can be expected to move for monetary reasons, that is, to receive a higher wage in the first instance, are less responsive to cultural differences. We also looked at the subsample of internal migrants who switch occupation when they move (Panel F). Compared to occupational stayers, switchers are compensated more for their move to a dialect-dissimilar county.

We also analyze in more detail the 567 two-time movers in our sample.³² Recall that the total time period under analysis is nine years, meaning that every second move occurs within a relatively short time window. For the second move, we use the dialect distance and geographic distance between the origin county of the first move and the destination county of the second move. This should mimic the hypothetical direct move to the destination county in the second move. All other control variables (quarterly pre-treatment wages, education, experience, age, etc.) are taken from the second move. Figure 2.3 illustrates the composition of the sample. As mentioned above, we have 567 individuals who move at least two times. Interestingly, almost 34 percent (194 migrants) of the two-time movers in our sample return in the second move to exactly the same county from which they came.³³ However, only 32 of the 194 repatriates return to the same firm.

Table 2.9 shows the results of the two-time mover analysis by first and second move and by timing of the move, that is, whether the migrant moved another time after or before eight quarters (two years). Panel A shows the results with repatriates included and Panel B shows the results

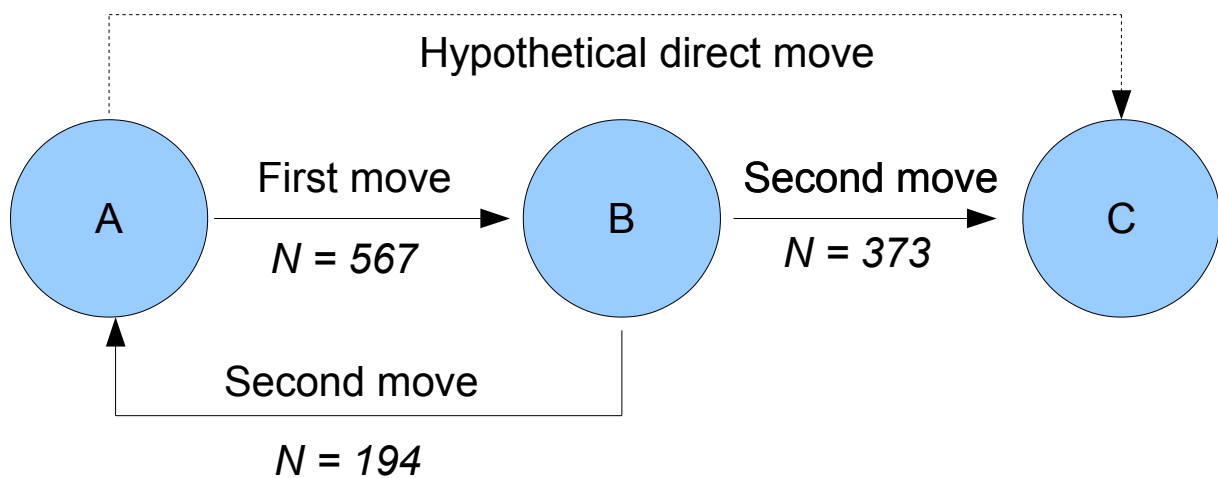
³¹ Complementary to that, it could also be that those who have earned above the average occupational wage have a better bargaining position. Thus, due to the higher outside option at home, they are able to negotiate a wage premium.

³² There are some individuals who moved more than two times, but this group is too small for an in-depth investigation.

³³ *Repatriates* are those who move to their previous county of residence and again work in their previous county of work. Thus, migrants who return to their previous home county but work in a different county than before are not repatriates.

without this group. The first move shows, independent of the timing, that the coefficients are larger than in the baseline sample. This indicates that these particular people, who we know are going to move again within the next nine years, value culture highly. The second move is more interesting. The coefficient for those who moved another time within eight quarters is almost seven times as large as the baseline coefficient. The above findings lead us to view these two-time movers as people who made the wrong decision about where to live and work for the first move and are now willing to sacrifice a lot more money in return for a more familiar environment.

Figure 2.3
Sample of Two-Time Movers



Notes: The figure illustrates the sample of two-time movers. In total, we have 567 internal migrants, who moved at least two times. 373 individuals moved to a different county of residence or county of work (county C) than their source county in the first move (county A). 194 individuals moved back to the same county of residence and county of work in the second move (from county B to county A). We call this group of people *repatriates*.

Table 2.9
Two-Time Mover Analysis

	(1)	(2)	(3)	(4)
	First move		Second move	
	≥ 8 quarters	< 8 quarters	≥ 8 quarters	< 8 quarters
<i>Panel A: With repatriates</i>				
Dialect distance	0.2907* (0.1752)	0.1208 (0.1881)	0.0363 (0.1411)	0.5027*** (0.1559)
Observations	245	322	245	322
R^2	0.5813	0.5157	0.7002	0.5616
<i>Panel B: Without repatriates</i>				
Dialect distance	0.3145 (0.2001)	0.257 (0.2693)	0.1621 (0.157)	0.6122*** (0.1932)
Observations	192	181	192	181
R^2	0.6224	0.5166	0.7074	0.6019

Notes: Each panel shows the coefficient on dialect distance from a regression on the baseline model. Dependent variable in the specifications is the log indexed wage after the first or second move, respectively. The indexed wage is the gross wage in 2010 prices divided by the index of the rental rate. In the second move, dialect and geographic distance are from the (hypothetical) direct move from county A to county C (or back to county A) as illustrated in Figure 2.3. All regressions include four quarterly pre-migration indexed wages, education dummies, male, experience, experience squared, industry change, and quarter-year fixed effects from the first or second move, respectively. *Repatriates* are those individuals who move back to their county (or counties) of origin (both county of work and county of residence) in the second move. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

2.3.4 Persistence of Wage Premium

We now turn to the question of whether the initial effect directly after the move is persistent over time. We model this question by comparing the wage growth after the move between those who have moved to a more familiar versus those who have moved to a more unfamiliar region. For the United States, Borjas, Bronars and Trejo (1992) show that internal migrants earn initially less than comparable non-migrants in the new destination, but that this penalty disappears within a few years. Thus, for our case, it could be that the initially wage penalty pays off for migrants in more familiar regions because they might feel more comfortable, integrate more quickly, and are more productive in the end. Therefore, our hypothesis is that migrants who move to culturally more familiar regions experience a steeper wage growth after the move than migrants to more unfamiliar regions. Empirically, we look at wage growth after the first move by estimating the following regression:

$$\begin{aligned} \left[\log wage_{idt+k}^{indexed} - \log wage_{idt+1}^{indexed} \right] / k = & \alpha + \beta dialect\ distance_{sd} + \gamma \log wage_{ist+1}^{indexed} \\ & + \lambda geographic\ distance_{sd} + \phi X_{it-1} + \mu_t + \varepsilon_{idt+1} \end{aligned} \quad (2.4)$$

Conditional on the logged initial wage level after the move, we regress the average yearly wage growth from period $t + 1$, that is, the first quarter after the move, to period $t + k$, on dialect distance (Equation (2.4)). Thereby, k takes a maximum value of 32 (quarters), that is, we analyze wage growth within a maximum of eight years after the move. Note that by extending the growth period of analysis year by year, the number of internal migrants remaining in the sample drops significantly, until finally, in the analysis of eight-year post-move wage growth, there are less than 700 internal migrants. All other control variables remain equivalent to the baseline model. As mentioned above, due to a catching-up process, we expect that migrants who moved to culturally more dissimilar counties will exhibit lower wage growth rates. Table 2.10 shows the results for the three- to six-year wage growth rates. The coefficient on the logged initial wage level after the move shows that internal migrants with initially higher wages after the move generally have lower wage growth in the future. However, dialect distance is not significantly associated with future wage growth. Thus, we conclude that the initial wage sacrifice is persistent over time.

Table 2.10
Persistence of Wage Premium

	Dependent variable: $[\log \text{ indexed wage}_{(t+k)} - \log \text{ indexed wage}_{(t+1)}] / k$							
	$k = 4$	$k = 8$	$k = 12$	$k = 16$	$k = 20$	$k = 24$	$k = 28$	$k = 32$
Dialect distance	0.0088* (0.0048)	0.0034 (0.0034)	0.0022 (0.003)	0.0005 (0.0026)	0.0003 (0.0024)	0.0022 (0.0024)	0.0001 (0.0029)	-0.0002 (0.0044)
Log indexed wage $_{(t+1)}$	-0.0431*** (0.0035)	-0.0338*** (0.0022)	-0.028*** (0.0017)	-0.0223*** (0.0015)	-0.0194*** (0.0013)	-0.0166*** (0.0013)	-0.0164*** (0.0014)	-0.015*** (0.0021)
Control variables	yes	yes	yes	yes	yes	yes	yes	yes
Quarter-year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Geographic distance	yes	yes	yes	yes	yes	yes	yes	yes
Observations	8,209	6,872	5,875	4,910	4,076	3,142	1,923	698
R^2	0.0922	0.1224	0.119	0.1317	0.1333	0.1271	0.1491	0.1627

Notes: The indexed wage is the gross wage in 2010 prices divided by the index of the rental rate. Only the first observed move is considered for individuals who moved several times. Subscripts indicate x quarters before the move $(t-x)$ and one quarter after the move $(t+1)$. Control variables: education dummies, male, experience, experience squared, industry change. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

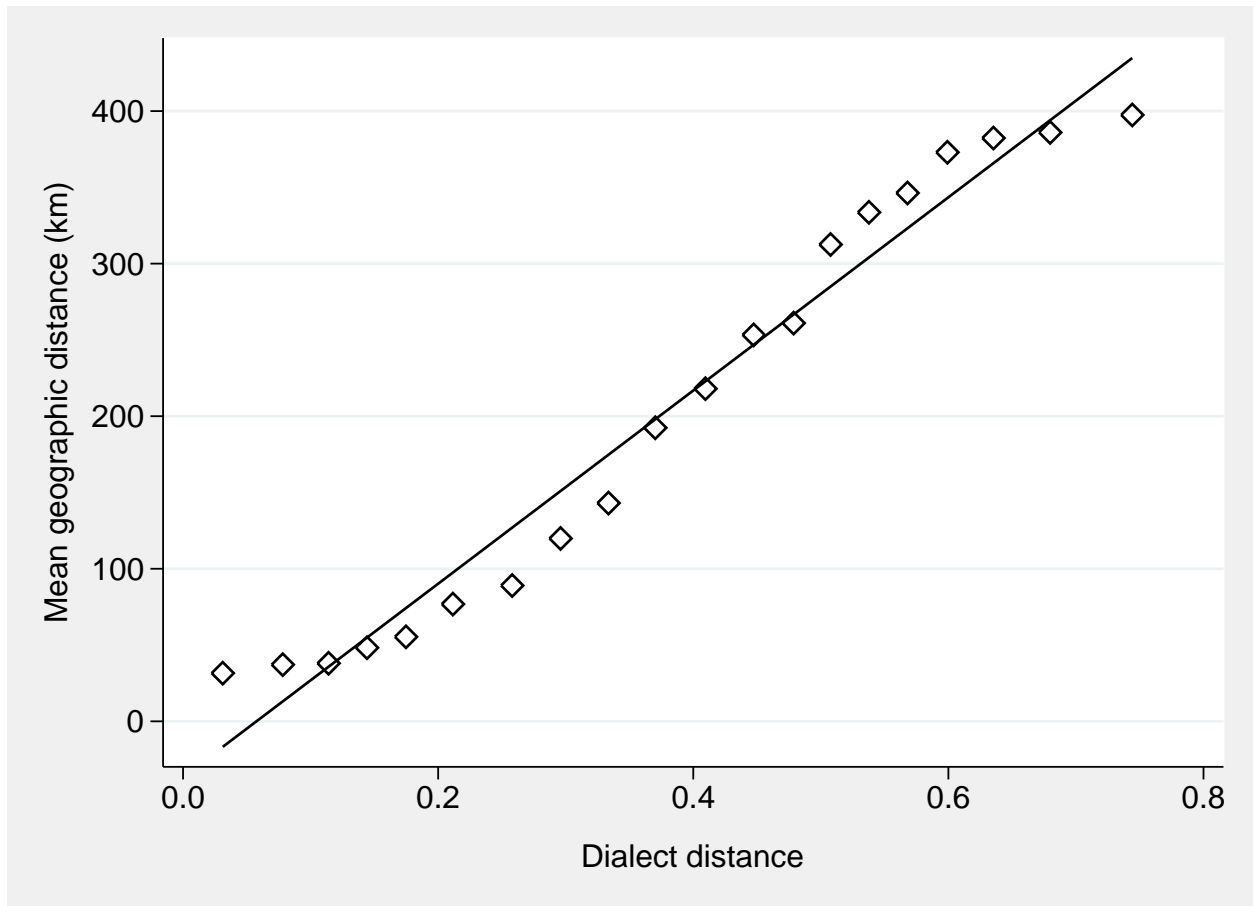
2.4 Conclusion

In this project, we quantify the psychic costs of migration by combining administrative social security panel data with a proxy for cultural differences that is based on the historical dialect distance between German counties. Internal migrants demand a wage premium of about 1 percent for a one standard deviation increase in dialect distance. Using wages that are indexed by local rents, we find an indexed wage premium of 1.5 percent. Compared to the general wage gain associated with internal migration, as well as to general wage increases negotiated in recent collective agreements, this wage premium is economically substantive and persistent over time. We argue that the effects are lower-bound estimates, because discrimination in regions that are culturally more distant and picking up people in regions where they might not have been socialized, lead to an underestimation of the true effect. Additionally, all non-migrants should have larger migration costs than those who migrate. This implies a further underestimation of the effect. Important effect heterogeneities arise: We observe that the effect is driven by males and those who earn above average occupational wages, more pronounced for geographically short moves, and persistent over time. Considering higher polynomial functions of geographic distance in the regressions provides additional confidence that the effect of dialect distance is not only reflecting a non-linearity in the geographic distance effect. We also analyze those who have made multiple moves within a relatively short period and find that internal migrants who made a *wrong decision* in the first move correct this decision in the second move and are willing to sacrifice much more of their wages to return to a more familiar region. Our results also imply that analyses of returns to migration that do not consider these psychic costs of migration overestimate the rate of return to monetary resources invested in migration.

We interpret our findings within a model of search and matching (e.g., Mortensen and Pissarides, 1994; Mortensen, 2011) where cultural barriers to migration represent a labor market friction that prevents the efficient allocation of labor. The rationale is the following: Détang-Dessendre, Drapier and Jayet (2004), for example, argue that the search process of people looking for a job is not random and that individuals accept wage offers only when they are compensated for (pecuniary and non-pecuniary) migration costs. We have demonstrated that cultural differences represent substantial migration costs. As a consequence, potential migrants might not consider wage offers from specific local labor markets because they would dislike to live there. Thus, because migrants do not consider the whole spectrum of wage offers, they make suboptimal choices. However, individuals' welfare might not suffer from this search behavior because they are compensated by the higher cultural familiarity of the destination region.

Appendix

Figure A2.1
Geographic Distance and Dialect Distance



Notes: The figure shows a binned scatterplot of geographic distance on dialect distance. The figure is constructed by binning dialect distance into 5-percentile point bins (so that there are 20 equal-sized bins) and computing the mean geographic distance within each bin. The slope of the regression on the microdata is 629.89 (5.436) and is significant at a 1 percent level.

Table A2.1
Source-County Fixed Effects

	<i>Dependent variable: log indexed wage_(t+1)</i>		
	(1)	(2)	(3)
Dialect distance	0.0936*** (0.0328)	0.0956*** (0.0323)	0.0861* (0.0483)
Geographic distance (km)	-0.000174*** (0.00004)	-0.000146*** (0.000039)	-0.000121** (0.000057)
Log indexed wage _(t-1)	0.333*** (0.0278)	0.2671*** (0.0257)	0.3698*** (0.0437)
Log indexed wage _(t-2)	0.0959*** (0.0317)	0.0903*** (0.0306)	0.0935** (0.0463)
Log indexed wage _(t-3)	-0.0193 (0.036)	-0.0092 (0.0352)	-0.0453 (0.0439)
Log indexed wage _(t-4)	0.151*** (0.0285)	0.1517*** (0.0283)	0.155*** (0.0412)
Residence county fixed effects (source)	yes	–	–
Work county fixed effects (source)	–	yes	–
Residence * work county fixed effects (source)	–	–	yes
Control variables	yes	yes	yes
Quarter-year fixed effects	yes	yes	yes
Observations	9,090	9,090	9,090
R ²	0.5503	0.5284	0.719

Notes: The indexed wage is the gross wage in 2010 prices divided by the index of the rental rate. Only the first observed move is considered for individuals who moved several times. Subscripts indicate x quarters before the move ($t - x$) and one quarter after the move ($t + 1$). *Control variables:* education dummies, male, experience, experience squared, industry change. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2.2
Geographic Distance and Non-Indexed Post-Migration Wages

	<i>Dependent variable: log wage_(t+1)</i>			
	(1)	(2)	(3)	(4)
Geographic distance (km)	0.000186*** (0.000029)	0.00009*** (0.000019)	0.000057*** (0.00002)	-0.000014 (0.00003)
Dialect distance				0.0756*** (0.0246)
Log wage _(t-1)		0.4797*** (0.0301)	0.4684*** (0.0297)	0.4657*** (0.0297)
Log wage _(t-2)		0.1223*** (0.0352)	0.1208*** (0.0348)	0.1214*** (0.0346)
Log wage _(t-3)		0.0163 (0.0398)	0.0103 (0.0395)	0.0097 (0.0393)
Log wage _(t-4)		0.1120*** (0.0316)	0.1209*** (0.0312)	0.1212*** (0.0312)
Log rental price (destination)			0.2095*** (0.0149)	0.2110*** (0.0149)
Log rental price (source)			-0.0219 (0.0166)	-0.0186 (0.0165)
Control variables	–	–	–	–
Quarter-year fixed effects	yes	yes	yes	yes
Observations	9,090	9,090	9,090	9,090
R ²	0.0169	0.5867	0.5957	0.5961

Notes: The wage is the gross wage in 2010 prices. Only the first observed move is considered for individuals who moved several times. Subscripts indicate x quarters before the move ($t-x$) and one quarter after the move ($t+1$). Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Chapter 3

Does Early Educational Tracking Increase Migrant-Native Achievement Gaps? Differences-In-Differences Evidence Across Countries*

Migrants in major European countries are failing to assimilate economically (Algan et al., 2010). Thus, their better integration has become a priority for policymakers in the European Union. However, it is less clear what public policy can do to effectively address this situation. As equal opportunity in education is key to the successful long-term integration of immigrants and the educational achievement of migrants lags behind that of native students in almost all European countries¹, education policies are being looked at with particular interest.

One institutional feature of several school systems in Europe that has been shown to generally increase educational inequality is early tracking of students into different types of secondary school based on their ability (e.g., Hanushek and Woessmann, 2006). In the context of migrant inclusion, early tracking may have detrimental effects if migrants are more likely to be inadequately matched to educational pathways.² This may be the case when migrants face difficulties to signal

* This chapter was coauthored by Guido Schwerdt, University of Konstanz.

¹ See, e.g., Ammermueller (2007); Nielsen et al. (2003); Van Ours and Veenman (2003); Bauer and Riphahn (2007); Schnepf (2007); Schneeweis (2011).

² For example, Luedemann and Schwerdt (2013) and Kiss (2013) show that second-generation immigrants in Germany receive worse grades and worse teacher recommendations for secondary school tracks conditional on student achievement.

their educational potential at an early stage, either because of a systematically different parental background³ or a lack of proficiency in the language of instruction (Akresh and Akresh, 2010).

These general insights lead many to conclude that early tracking systems might be specifically detrimental to students with a migration background (e.g., Van de Werfhorst and Mijs, 2010) and it is often explicitly recommended to policymakers that educational systems be made less selective to improve opportunities for migrant students (e.g., Network of Experts in Social Sciences of Education and Training (NESSE), 2008). Direct evidence based on cross-sectional data on the relationship between complete forms of educational tracking and migrants' relative achievement indeed seems to support these conclusions (e.g., Cobb-Clark, Sinning and Stillman, 2012). However, cross-sectional estimates are plagued with endogeneity concerns arising, for example, due to potentially selective migration into countries with early tracking systems.

This chapter studies the impact of ability-based early tracking of students into different types of secondary school on migrant-native test score gaps in a differences-in-differences framework, which implicitly controls for unobserved differences in relevant characteristics of the migrant and native student populations that remain constant over educational stages. In particular, our identification strategy makes use of the fact that no country tracks students in primary school. This allows us to exploit variation in migrant-native test score gaps between primary and secondary school as well as variation in the age of tracking between countries to identify the effect of early tracking. To benchmark our differences-in-differences estimates, we additionally present results based on commonly estimated cross-sectional models that rely entirely on selection-on-observable assumptions.

Our empirical analysis is based on a comprehensive analytical sample that we obtained by pooling data from all existing waves of the three largest international assessments of student achievement during primary and secondary school – PISA, TIMSS, and PIRLS – which test students in reading, math, and science. As a result, we have internationally comparable information on student achievement and background characteristics for more than one million students from 45 countries.

We find that early tracking does not significantly affect the evolution of overall migrant-native test score gaps from primary to secondary school in math and science. Nor do we find significant effects for reading, but results are somewhat less clear-cut. For all subjects, however, there are

³ Several studies document that early educational tracking between school types increases the effects of parental background on educational outcomes (e.g., Bauer and Riphahn, 2006; Meghir and Palme, 2005; Pekkarinen, Uusitalo and Kerr, 2009; Kerr, Pekkarinen and Uusitalo, 2013).

no substantial negative impacts of more than 10 percent of a standard deviation. The small and insignificant estimates of the overall effect conceal a detrimental effect of early tracking on the relative achievement of second-generation immigrants who do not speak the language of the testing country at home. For this subgroup of migrant students, we find a significant detrimental effect of early tracking on relative achievement in reading of about 11 percent of a standard deviation and similar, but insignificant, effect sizes in terms of math and science achievement. Thus, our findings show that tracking students early into different types of schools by ability does not substantially reduce relative achievement growth of all migrant children, but it does so for those who are presumably less integrated into the country's society.

The key empirical challenge for such an investigation is to distinguish accidental correlation from causation. When investigating effects of a system-level variable, such as early educational tracking, empirical research is basically forced to exploit cross-country variation in educational policies for identification.⁴ However, simple cross-country identification strategies based on cross-sectional data rely on strong conditional independence assumptions, basically assuming away any differences in non-ignorable unobservable country traits. It is questionable whether any policy conclusions can be drawn from such evidence.

As first noted by Hanushek and Woessmann (2006), the availability of test score data by country before and after tracking allows estimating effects of early educational tracking in a differences-in-differences framework. Employing this framework and identifying the effect of early tracking based on a comparison of the evolution of migrant-native achievement gaps from primary to secondary school reveals no significant effects of early tracking. The results from commonly estimated cross-sectional models would, however, imply a substantial positive association between early educational tracking and the size of the migrant-native test score gap. In all three domains – math, science, and reading – migrant-native achievement gaps in secondary school are found to be between 20 - 30 percent of a standard deviation larger in countries that track students before the age of 15.

However, we show that a similar relationship already exists in primary school, a period during which no country has yet tracked students according to ability. This indicates more that the association between early tracking and the migrant-native achievement gap is driven by selective migration toward early tracking countries. Among the late tracking countries, there are the

⁴ Questions regarding the school system could also be addressed by exploiting school reforms within countries. However, especially in the case of migration economics, results from a particular country with a particular migrant population are hard to generalize to other countries with other school systems and other migrant populations.

anglophone countries – United States, Australia, Canada, New Zealand, and United Kingdom – that have highly selective migration policies in place, whereas early tracking countries are mainly located in Europe – Germany, Austria, the Netherlands, Belgium, and Italy – and do not select migrants as strictly as the other countries do. Stricter immigration policies create a more selected migrant population (Grogger and Hanson, 2011), which could partly explain the large cross-sectional differences in the migrant-native achievement gap. Furthermore, there could be bilateral country factors between sending and receiving countries, such as language differences, cultural differences, networks and diasporas, and religious differences, that can shape the skill structure of the migrant population. For our study, this insight is crucial because it implies that all cross-sectional models are likely to be confounded by country-specific migrant populations and how they interact with the country-specific school system.

The remainder of the chapter is structured as follows. Section 3.1 describes the data. In Section 3.2, we present our empirical strategy. Section 3.3 presents our main results and Section 3.4 contains a discussion as to their robustness. Section 3.5 investigates whether our main results conceal more pronounced effects for specific groups of students. Section 3.6 concludes.

3.1 Data

3.1.1 International Large-Scale Student Assessments

To address our research question as comprehensively as possible, we use data from all existing waves of the three largest international large-scale student assessments: the Programme for International Student Assessment (PISA), the Trends in International Mathematics and Science Study (TIMSS), and the Progress in International Reading Literacy Study (PIRLS).⁵

Each of these large-scale student assessments has a different focus with respect to the tested population and tested domains. PISA tests students at age 15 in reading, science, and math. TIMSS evaluates performance in math and science in grades 4 and 8. PIRLS tests fourth graders in reading only. Thus, for primary school students, we have test scores for reading from PIRLS and for math and science from TIMSS; for secondary school students, we have math and science test scores from both TIMSS and PISA and reading test scores from PISA.

⁵ The data from all TIMSS and PIRLS surveys, along with detailed documentation of the data and the survey design, are available at <http://timss.bc.edu>. The PISA data and documentation are available at <http://www.oecd.org/pisa>.

We pool data from all cycles of these large-scale student assessments conducted between 1995 and 2012. Specifically, we use information on reading achievement in primary school from the PIRLS waves 2001 and 2006, and on math and science achievement in primary school from the TIMSS waves 1995, 2003, and 2007.⁶ Our data on achievement in secondary school come from the PISA waves 2000, 2003, 2006, 2009, and 2012 in the domains of reading, math, and science as well as from the TIMSS waves 1995, 1999, 2003, 2007, 2011 for math and science. In sum, our sample contains information on more than one million tested students in 45 countries.

3.1.2 Defining Early Educational Tracking

The 45 countries have differently structured school systems. One difference of particular importance to our work is that countries track students based on ability into different schools at different ages. Information on school systems for a large number of countries, including information on the age of first tracking, is provided by the UNESCO International Bureau of Education (2013). We checked this information against OECD data (OECD (2007, Table 5.2, p. 162), OECD (2010, Table A5.2, p. 147)) and information provided in other studies on educational tracking (Brunello and Checchi, 2007; Waldinger, 2007; Hanushek and Woessmann, 2006) to ensure that the age of first tracking information is accurate and consistent with what is commonly used in the literature.

Table 3.1 reports age and grade level of first selection in the school system by country as obtained from the UNESCO International Bureau of Education (2013). The table reveals considerable variation in the age of tracking. For example, Austria and Germany track students according to ability into different tracks of secondary school at age 10, whereas in the majority of countries students stay in comprehensive schools until age 15 or 16. We follow Hanushek and Woessmann (2006) and define early tracking as the process of being tracked for the first time into different school tracks before age 15. Table 3.1 reports which countries are early tracking countries based on this definition. There are 15 early tracking countries and 30 late tracking countries. Although this way of defining early educational tracking is common in the literature, it is still somewhat arbitrary. Therefore, we test the robustness of our main results using alternative definitions in Section 3.4.

⁶ PIRLS 2011 and TIMSS 2011 in grade 4 do not provide information on whether the child and/or the parents are born in the testing country. We therefore cannot use these surveys in our analysis.

Table 3.1
Tracking Status of Countries

Late tracking countries				Early tracking countries			
Country	Baseline	Tracking		Country	Baseline	Tracking	
	sample	Age	Grade		sample	Age	Grade
Canada	yes	16	8	Austria	yes	10	4
Denmark	yes	16	10	Belgium	yes	12	7
Greece	yes	15	9	Czech Republic	yes	11	5
Hong Kong	yes	16	11	Germany	yes	10	4
Iceland	yes	16	10	Hungary	yes	11	4
Latvia	yes	16	9	Italy	yes	14	8
Lithuania	yes	15	8	Netherlands	yes	12	6
New Zealand	yes	16	12	Singapore	yes	12	6
norway	yes	16	10	Slovak Republic	yes	11	4
Qatar	yes	15	9	Ireland	no	12	6
Russia	yes	15	9	Luxembourg	no	13	6
Slovenia	yes	15	9	Macedonia	no	14	9
Sweden	yes	16	9	Mongolia	no	11	4
Taipei	yes	15	9	Trinidad and Tobago	no	11	6
United Kingdom	yes	16	12	United Arab Emirates	no	14	9
United States	yes	16	12				
Argentina	no	15	9				
Australia	no	16	10				
Cyprus	no	15	10				
El Salvador	no	16	9				
France	no	15	9				
Georgia	no	15	9				
Israel	no	15	10				
Kazakhstan	no	15	9				
Kuwait	no	17	12				
Moldova	no	15	10				
Morocco	no	16	12				
Portugal	no	15	9				
Spain	no	16	10				
Tunisia	no	16	10				

Notes: Early tracking is defined as the age of first selection before age 15. The age and grade of tracking describes the first selection in the school system. The UNESCO International Bureau of Education (2013) provides information about a large number of school systems around the world. We inferred the tracking status of a country from these information. The data was double-checked with OECD data (OECD (2007, Table 5.2, p. 162) and OECD (2010, Table A5.2, p. 147)) and with data from Brunello and Checchi (2007) and Waldinger (2007).

3.1.3 Analytical Sample

We restrict our focus to second-generation immigrants to ensure that both native and migrant students have spent their entire school career in the testing country. We define as second-generation immigrants all students born in the testing country who have at least one parent who was born abroad. A native student is born in the testing country and has parents who are also born there. We test the robustness of our main results with respect to alternative definitions of migrant status in Section 3.4.

All students with missing information on their parents' country of birth or students born abroad are removed from the sample. Students with missing information on test scores, age, gender, and books at home are also excluded from our analysis. To avoid having our results largely driven by countries with very few migrant students, we follow common practice and drop all countries with a migrant ratio below 3 percent.⁷ We check the robustness of our main results with respect to this sample restriction in Section 3.4.

For our main analysis, we include only those countries for which we have information on all three domains (reading, math, and science) in at least one assessment during primary school and one assessment during secondary school. This restriction should ensure that comparisons of results across domains are not confounded by differences in the country composition of estimation samples. Table 3.1 lists the countries included in this baseline sample. We explore the robustness of our main results with respect to this sample restriction in Section 3.4.

To ease interpretation of regression coefficients, we standardize test scores in each domain within each survey cycle before we restrict the sample to the countries in our analysis to have an international mean of 0 and a standard deviation of 1. Thus, when using standardized test scores as regression outcomes, coefficients represent changes in standard deviations of test scores.⁸

Table 3.2 provides summary statistics separately for students tested in primary and secondary school on test scores by tracking and migrant status. Panels A and B of Table 3.2 report mean test scores by migrant status separately for late and early tracking countries. Test score averages in Panel A refer to secondary school; those reported in Panel B refer to primary school. In secondary school, natives generally outperform migrants independently of the tracking system with the exception of reading achievement in late tracking systems. However, migrant-native test score

⁷ All countries listed in Table 3.1 meet this requirement.

⁸ Alternative, we have standardized test scores only over the sample of countries that are included in our baseline regression. However, whether we standardize within educational stage or within each survey, the results do not change qualitatively. Results with different standardizations are not presented, but are available upon request.

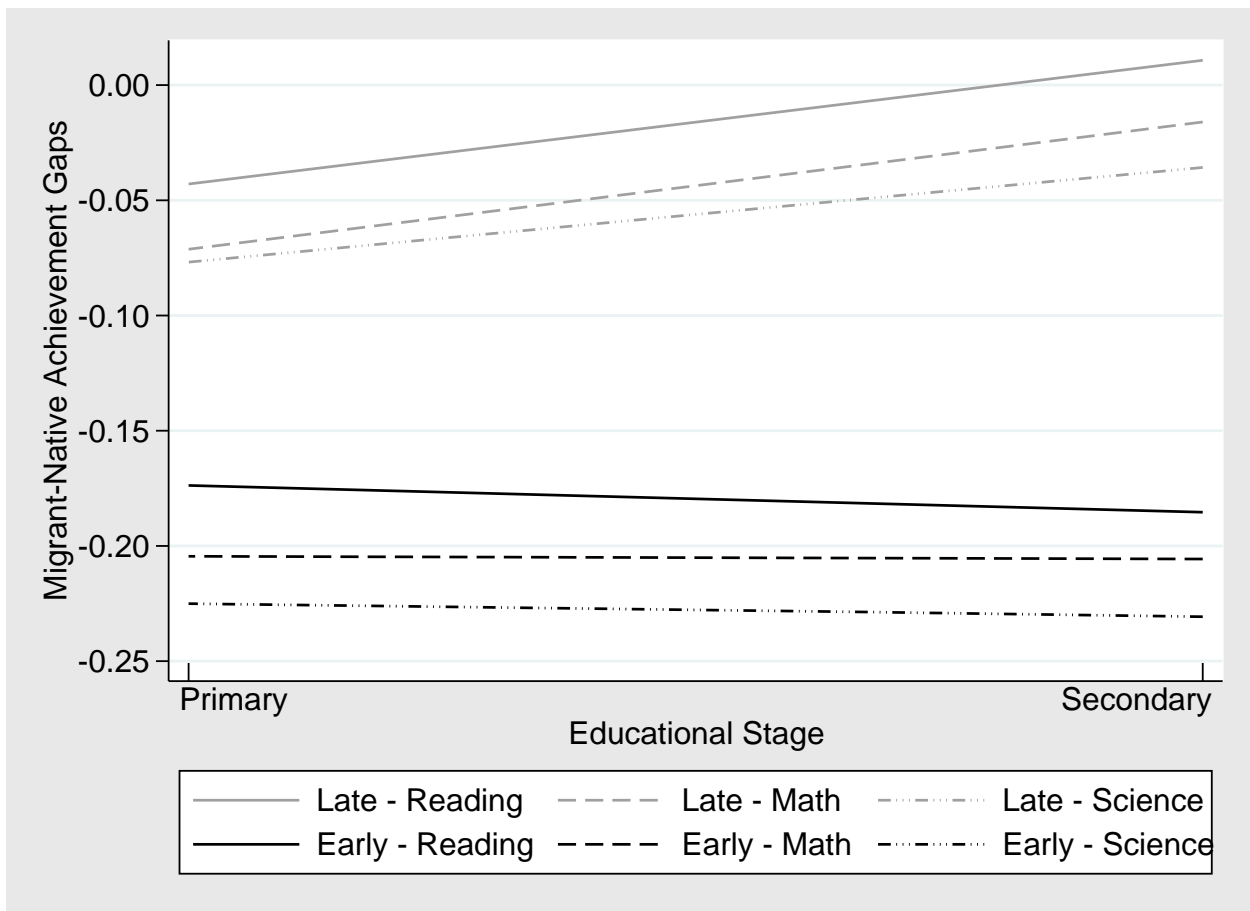
gaps are substantially larger in early tracking countries. For example, the unconditional test score gap in math is more than ten times larger in early tracking (-0.206) than in late tracking systems (-0.016).

However, this overall pattern is not very different from the general pattern in primary school, as can be seen in Panel B of Table 3.2. As no country has yet tracked students in primary school, this suggests that the observed correlation between early educational tracking and migrant-native test score gaps in secondary school may at least partly arise due to factors other than educational tracking. Nevertheless, early educational tracking may aggravate educational disadvantages that already exist during primary school.

This possibility is illustrated in Figure 3.1, which plots the average migrant-native achievement gaps in primary and secondary school reported in Table 3.2. The figure reveals noticeable differences between tracking systems in the evolution of migrant-native achievement gaps over educational stages. While differences in test scores between natives and migrants become smaller in all subjects between primary and secondary school in late tracking countries, unconditional test score gaps widen in early tracking countries. Whether these differences are in fact statistically significant and whether they can be explained by compositional differences in the student population over time and across countries is the focus of our empirical analysis in the following sections.

To investigate to what extent compositional differences in the student population matter for our analysis, we collect information on the demographic background and on the socioeconomic background, which we proxy with the commonly used books at home variable, from the three large-scale student assessments. These background variables include age, gender, books at home (five categories), a dummy indicating whether students are tested in an OECD country, and GDP per capita of the test country. Panels A and B of Table 3.3 report averages of the demographic and socioeconomic background variables in primary and secondary school by tracking and migrant status and reveal some noteworthy differences. Migrants are overrepresented in OECD countries and countries with a higher GDP per capita. Students (both natives and migrants) in late tracking countries are more likely to be living in an OECD and high-income country. Finally, as expected, the information on books at home shows that migrants are on average underrepresented in the category of having more than 200 books and somewhat overrepresented in the lower categories. This relationship is stronger in the early tracking countries than in the late tracking countries.

Figure 3.1
Average Migrant-Native Achievement Gaps in Primary and Secondary School



Notes: The figure shows average migrant-native achievement gaps by subtracting the unconditional mean average migrant test score from the unconditional mean native test score (see summary statistics in Table 3.2). Means of the variables and are compiled by using all waves (all years) of TIMSS, PISA and PIRLS separately by primary and secondary school for the set of countries in the pooled application, i.e. corresponding to the baseline results. Student weights are used for weighting.

Table 3.2
Summary Statistics – Test Scores

<i>Panel A: Test scores in secondary school</i>											
Test score	Late tracking countries				Early tracking countries				Difference	Obs	Difference
	Migrants		Natives		Migrants		Natives				
	Mean	Obs	Mean	Obs	Mean	Obs	Mean	Obs			
Reading	0.009 (0.879)	72,910	-0.002 (0.859)	327,765	-0.161 (0.951)	24,725	0.024 (0.878)	199,072	-0.185*** [0.006]		
Math	-0.013 (0.809)	116,018	0.003 (0.795)	479,924	-0.179 (0.887)	31,939	0.026 (0.831)	225,690	-0.206*** [0.005]		
Science	-0.029 (0.869)	116,036	0.006 (0.820)	479,972	-0.201 (0.940)	31,933	0.03 (0.851)	225,737	-0.231*** [0.005]		

<i>Panel B: Test scores in primary school</i>											
Test score	Late tracking countries				Early tracking countries				Difference	Obs	Difference
	Migrants		Natives		Migrants		Natives				
	Mean	Obs	Mean	Obs	Mean	Obs	Mean	Obs			
Reading	-0.034 (0.716)	25,064	0.009 (0.673)	91,123	-0.147 (0.653)	8,986	0.026 (0.612)	46,827	-0.174*** [0.007]		
Math	-0.058 (0.699)	37,860	0.014 (0.678)	132,594	-0.174 (0.693)	13,213	0.031 (0.654)	62,803	-0.204*** [0.006]		
Science	-0.062 (0.749)	37,860	0.015 (0.695)	132,594	-0.191 (0.699)	13,213	0.034 (0.629)	62,803	-0.225*** [0.006]		

Notes: Summary statistics show means of the variables and are compiled by using all waves (all years) of TIMSS, PISA and PIRLS separately by primary and secondary school for the set of countries in the pooled application, that is, corresponding to the baseline results. Test scores are de-meant and standardized to have an international mean of 0 and a standard deviation of 1. Student weights are used for weighting. Each observation is weighted such that each country carries a weight of one. Standard deviations in round and standard errors in rectangular parentheses. A migrant is defined as second generation migrant: The student is born in the testing country but at least one parent is born outside this country. Significance levels for testing whether the difference is equal to zero or not: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.3
Summary Statistics – Demographics and Socioeconomic Background

<i>Panel A: Demographics and socioeconomic background in secondary school</i>						
Variable	Late tracking countries			Early tracking countries		
	Migrants	Natives	Difference	Migrants	Natives	Difference
Age	15.062 (0.964)	15.097 (0.948)	-0.035 0.003	15.327 (0.816)	15.374 (0.784)	-0.048*** [0.004]
Male	0.492	0.497	-0.005*** [0.002]	0.497	0.503	-0.006** [0.003]
0–10 books	0.139	0.092	0.047*** [0.001]	0.156	0.091	0.065*** [0.002]
11–25 books	0.195	0.164	0.031*** [0.001]	0.194	0.157	0.037*** [0.002]
26–100 books	0.289	0.305	-0.016*** [0.001]	0.303	0.318	-0.014*** [0.003]
101–200 books	0.168	0.197	-0.029*** [0.001]	0.159	0.200	-0.042*** [0.002]
> 200 books	0.207	0.240	-0.033*** [0.001]	0.186	0.233	-0.046*** [0.002]
OECD	0.540	0.644	-0.104*** [0.002]	0.774	0.905	-0.131*** [0.002]
GDP per capita	35,817 (22,983)	30,867 (22,264)	4,950*** [70]	32,800 (12,132)	26,642 (12,575)	6,158*** [69]
Observations	126,325	543,206		37,674	276,946	

<i>Panel B: Demographics and socioeconomic background in primary school</i>						
Variable	Late tracking countries			Early tracking countries		
	Migrants	Natives	Difference	Migrants	Natives	Difference
Age	10.098 (0.672)	10.173 (0.687)	-0.075*** [0.003]	10.247 (0.561)	10.203 (0.537)	0.044*** [0.004]
Male	0.487	0.492	-0.006*** [0.002]	0.504	0.500	0.004 [0.004]
0–10 books	0.131	0.081	0.050*** [0.001]	0.138	0.074	0.064*** [0.002]
11–25 books	0.209	0.190	0.019*** [0.002]	0.256	0.216	0.040*** [0.003]
26–100 books	0.312	0.342	-0.031*** [0.002]	0.325	0.371	-0.046*** [0.004]
101–200 books	0.164	0.194	-0.030*** [0.002]	0.146	0.182	-0.036*** [0.003]
> 200 books	0.184	0.193	-0.009*** [0.002]	0.135	0.157	-0.022*** [0.003]
OECD	0.592	0.633	-0.041*** [0.002]	0.784	0.907	-0.123*** [0.002]
GDP per capita	31,579 (19,601)	27,602 (20,696)	3,977*** [92]	30,181 (10,870)	24,900 (12,374)	5,281*** [89]
Observations	62,924	223,717		22,199	109,630	

Notes: Summary statistics show means of the variables and are compiled by using all waves (all years) of TIMSS, PISA and PIRLS separately by primary and secondary school for the set of countries in the pooled application, that is, corresponding to the baseline results. The table shows the result of pooling samples by educational stage, that is, pooling PISA and TIMSS for secondary school and PIRLS and TIMSS for primary school. Numbers of observations between test score observations in Panel A of Table 3.2 do not add up compared to Panel A of this table because math and science test scores are reported both by PISA and TIMSS and PISA contains missing test scores. This summary statistic, however, reports background information for all students with at least one valid test score in one of the three subjects. In Panel B of both tables, number of observations add up because reading test scores are reported by PIRLS only and math and science test scores by TIMSS only. Neither of the surveys contains missing test scores. Student weights are used for weighting. Each observation is weighted such that each country carries a weight of one. Standard deviations in round and standard errors in rectangular parentheses. A migrant is defined as second generation migrant: The student is born in the testing country but at least one parent is born outside this country. Significance levels for testing whether the difference is equal to zero or not: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.2 Empirical Strategy

All else equal, does early educational tracking increase migrant-native achievement gaps? We begin answering this question by estimating conventional cross-sectional models using micro data and control for country fixed effects. We then investigate how results change in a differences-in-differences framework.

3.2.1 Cross-Sectional Model

To benchmark our differences-in-differences estimates, we first estimate commonly estimated cross-sectional regression models (e.g., Cobb-Clark, Sinning and Stillman, 2012). We estimate these models based on pooled data for the three domains of math, science, and reading using the following equation:

$$Y_{ic} = \alpha_0 + \alpha_1 ET_c \times MIG_i + \alpha_2 MIG_i + \mathbf{X}'\boldsymbol{\gamma} + \mu_c + \varepsilon_{ic}, \quad (3.1)$$

where Y_{ic} is the standardized tests score of individual i in country c , ET_c is a binary variable taking the value 1 if country c is an early tracking country and 0 otherwise, MIG_i is a binary variable taking the value 1 if student i is a second-generation immigrant and 0 otherwise, \mathbf{X} is a vector of student and country background variables, μ_c is a survey times country fixed effect, and ε_c is an idiosyncratic error.⁹

The key parameter of interest is α_1 . This parameter captures any additional effect of being a migrant in an early tracking country over and above the isolated effect of migration background on test scores in a late tracking country, which is captured by α_2 . Estimating Equation (3.1) by OLS produces unbiased estimates for α_1 only if the observable covariates included in the model are the sole reason $ET_c \times MIG_i$ and ε_{ic} might be correlated.

However, it seems likely that this selection-on-observables assumption is violated in this case. For example, migrant students in early as opposed to late tracking countries might be very different with respect to unobserved characteristics that affect the level of achievement at any educational stage. One example could be the Guestworker program in Germany that has attracted a lot of Turkish workers during the 1960s and 1970s (Danzer and Yaman, 2013). Later, the workers'

⁹ Note that due to the inclusion of country fixed effects, the parameter on an indicator variable for early tracking countries is not identified, which is why ET_c only enters Equation (3.1) interacted with MIG_i . Estimating versions of Equation (3.1) that include the single ET_c dummy, but exclude country fixed effects, leads to similar results.

families were allowed to move to Germany as well. As a result, Germany is now one of the main destinations for Turkish migrants (Grogger and Hanson, 2011). Cross-sectional regressions cannot account for this historical event and would show an accidental correlation between early tracking and low educational migrant performance. Another concern is that other educational features affecting migrant-native achievement differences might systematically differ between early and late tracking countries. For example, the quality of the teaching force differs widely between countries (Hanushek, Piopiunik and Wiederhold, 2014), which is not captured in a cross-sectional design.

3.2.2 Differences-In-Differences Model

Longitudinal data on the evolution of test scores after tracking for the same cohort of students in a large set of countries would allow us to eliminate any confounding impact of unobserved time-invariant characteristics on the size of migrant-native test score gaps in a straightforward differences-in-differences framework. Unfortunately, such data are not available. However, as first noted by Hanushek and Woessmann (2006), information on student achievement and background from large-scale international student assessments, such as PISA, PIRLS, and TIMSS, that test and survey cross-sections of students in different countries at different educational stages permits a differences-in-differences estimation strategy.

Our identification strategy makes use of the fact that no country tracks students in primary school. Thus, we can obtain information on migrant-native achievement differences prior to tracking from the student assessments in grade 4 provided by PIRLS (reading) and TIMSS (math/science). Information on migrant-native achievement differences in secondary school, after tracking, is provided by PISA (reading/math/science), which tests students at age 15, and the TIMSS (math/science) assessments in grade 8. The corresponding differences-in-differences model based on pooled data on students observed at two educational stages (e), where e might be primary or secondary school, is given by:

$$\begin{aligned}
 Y_{ice} = & \beta_0 + \beta_1 ET_c \times SEC_e \times MIG_i + \\
 & \beta_2 ET_c \times MIG_i + \beta_3 ET_c \times SEC_e + \beta_4 MIG_i \times SEC_e + \\
 & \beta_5 MIG_i + \beta_6 SEC_e + \mathbf{X}'\delta + v_c + \eta_{ice},
 \end{aligned} \tag{3.2}$$

which, in addition to the variables included in Equation (3.1), includes the dummy variable SEC_e that identifies observations in secondary school together with its interactions with $ET_c \times MIG_i$, ET_c , and MIG_i .

The key parameter of interest in Equation (3.2) is the parameter on the triple interaction term β_1 . This parameter measures the effect of early educational tracking on the migrant-native achievement gap. The advantage of estimating Equation (3.2) is that the inclusion of the interaction $ET_c \times MIG_i$ captures any confounding impact of unobserved educational-stage-invariant characteristics on the level of the migrant-native test score gap, which is represented by the parameter β_2 .

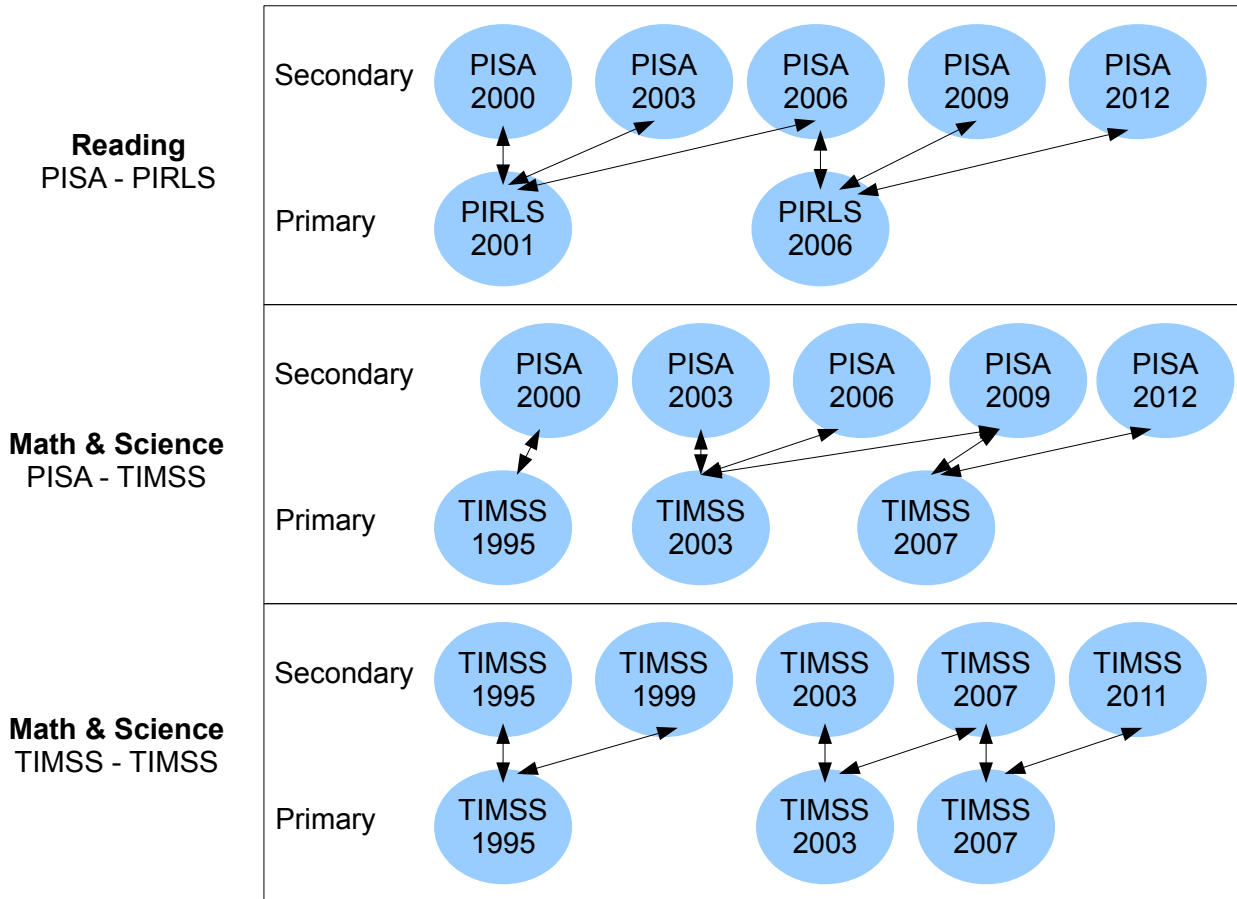
The effect β_1 is identified under the assumption that any unobserved differences between students in early and late tracking countries does not differentially affect the evolution of the migrant-native test score gap between primary and secondary school. This is the well-known "parallel trends" assumption of differences-in-differences models. We argue that this identifying assumption is more credible than the selection-on-observables assumption required to causally interpret estimates based on Equation (3.1). The parallel trends assumption would be violated if the migrant-native achievement gap in primary school is affected by early tracking. One example could be that native parents in early tracking countries anticipate the tracking event and therefore push their children at the end of primary school to perform well. We discuss this issue more in Section 3.4.4.

To estimate Equation (3.2), we have to match to each assessment in primary school (PIRLS for reading, and TIMSS for math and science) one assessment in secondary school (PISA for reading, math, and science and TIMSS for math and science). Because there are multiple waves of PISA, TIMSS, and PIRLS, we can construct multiple matches of survey waves (Hanushek and Woessmann, 2006). Figure 3.2 visualizes how we match the different survey waves. Altogether, we construct 18 different matches, containing exactly one cohort of students tested in primary and one tested in secondary school.¹⁰ For the final dataset, we append the student-level data for each survey match. Appending all survey matches lead to the problem that some countries are overrepresented. Therefore, the original sampling weights are adjusted such that each country carries a weight of 1. The survey matches can be grouped into 3 types: PISA-PIRLS, PISA-TIMSS, and TIMSS-TIMSS

¹⁰ Appendix Table A3.2 provides country information on the inclusion of the country in a specific survey match. As the table shows, the composition of countries changes from survey match to survey match because not every survey is carried out in the same countries. Section 3.4.3 provides robustness checks by changing the composition of the survey matches.

comparisons. In the baseline analysis, we merge PISA-TIMSS and TIMSS-TIMSS comparisons for math and science.¹¹

Figure 3.2
Matching of Survey Waves



Notes: The figure shows the matching of survey waves for the differences-in-differences model. Author's own depiction.

3.3 Results

3.3.1 Results from the Cross-Sectional Model

We begin the presentation of our results by showing OLS estimates of Equation (3.1). Table 3.4 sets out regression results for math, science, and reading achievement in secondary school when educational tracking has already occurred in some countries, but not in others. Our baseline sample includes 25 countries, nine of which track students before age 15. Results of estimating Equation (3.1) are presented both with and without other control variables. However, all regression

¹¹ Appendix Table A3.3 provide differences-in-differences results for each comparison separately.

models include country-by-survey-cycle fixed effects. To allow for within-country correlation of error terms, we cluster standard errors at the country level. We estimate weighted regressions using original sampling weights, which we adjusted such that each country carries a weight of 1.

Table 3.4
OLS Estimation – Secondary School

	(1)	(2)	(3)	(4)	(5)	(6)
	Reading		Math		Science	
	PISA		PISA, TIMSS		PISA, TIMSS	
Early tracking × migrant	−0.207* (0.105)	−0.128** (0.058)	−0.198* (0.103)	−0.109* (0.056)	−0.203 (0.120)	−0.097 (0.057)
Migrant	0.012 (0.054)	0.103** (0.042)	−0.018 (0.053)	0.086*** (0.030)	−0.039 (0.066)	0.107** (0.039)
<i>Further controls</i>						
GDP per capita × migrant		0.004** (0.002)		0.003** (0.001)		0.003* (0.002)
OECD × migrant		−0.180*** (0.055)		−0.201*** (0.046)		−0.272*** (0.058)
Male		−0.326*** (0.018)		0.086*** (0.015)		0.076*** (0.020)
Age		0.151*** (0.010)		0.008 (0.027)		0.042 (0.029)
11-25 books		0.284*** (0.025)		0.265*** (0.027)		0.295*** (0.026)
26-100 books		0.556*** (0.035)		0.542*** (0.037)		0.581*** (0.035)
101-200 books		0.776*** (0.043)		0.747*** (0.042)		0.799*** (0.041)
> 200 books		0.990*** (0.055)		0.936*** (0.053)		1.001*** (0.053)
Constant	0.538*** (0.029)	−2.081*** (0.173)	0.864*** (0.029)	0.289 (0.433)	0.696*** (0.035)	−0.439 (0.460)
Survey × country fixed effects	yes	yes	yes	yes	yes	yes
R^2	0.146	0.289	0.232	0.328	0.179	0.288
Individual observations	624,472	624,472	853,571	853,571	853,678	853,678
Distinct country observations	25	25	25	25	25	25
<i>Early tracking countries</i>	9	9	9	9	9	9

Notes: The samples are restricted to the set of countries used in the baseline DiD models. Dependent variables are the original test scores which are standardized within each survey and year to international mean 0 and sd 1. Robust standard errors in parentheses and clustered at the country level. Original students weights are used for weighting. Each observation is weighted such that each country carries a weight of one. *Early tracking* is 1 for countries with tracking age below 15 and 0 otherwise. The reference category for *books* is 0-10 books. *GDP per capita* is yearly de-measured to provide deviations from a country with mean GDP per capita. A migrant is defined as second generation migrant: The student is born in the testing country but at least one parent is born outside this country. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Estimations without further controls show that in all three domains, early educational tracking is related to lower achievement by migrant relative to native students. In late tracking countries, average achievement of migrant and native students is about the same. The regression coefficients on the migrant dummy, the estimates of α_2 in Equation (3.1), are small and statistically insignificant. In early tracking countries, however, migrants score on average about 20 percent of a standard

deviation lower than native students. Estimates of α_1 in Equation (3.1) are statistically significant at the 10 percent level in reading and math and insignificant in science. Overall, these findings are in line with the descriptive evidence presented above.

To investigate to what extent these differences can be explained by compositional differences in student populations, we control for demographic and socioeconomic background variables. Results are reported in Columns 2, 4, and 6 of Table 3.4. The coefficients on the additional covariates have the expected signs and are comparable with other evidence on the determinants of student achievement. Number of books at home is the strongest predictor of student achievement. For example, the difference in test scores of students with more than 200 books at home compared to the reference group of students amount to almost an entire standard deviation. Boys perform significantly worse than girls in reading, but perform slightly better in math and science. Age is positively related with achievement in reading.

To account for selective migration into more economically developed destination countries, we also include two country characteristics—GDP per capita and OECD membership—interacted with migrant status as covariates.¹² The interaction with OECD membership is highly significant and negative, indicating that migrants in non-OECD countries—relative to their native peers—score substantially better than migrants in OECD countries.

Turning to our main parameters of interest, we observe in all three domains that estimates of α_1 in Equation (3.1), the coefficients on the interaction between early tracking and migrant status, become smaller in absolute terms, suggesting that migrant-native achievement gaps are in fact partly attributable to demographic and socioeconomic background variables. Compared to the unconditional regression results, estimated effects of early educational tracking shrink in absolute terms to 13 percent of a standard deviation in reading, 11 percent of a standard deviation in math, and 10 percent of a standard deviation (insignificant) in science.¹³ Interestingly, unconditional results for the estimates of α_2 in Equation (3.1) indicate that the average migrant-native achievement gap is mainly driven by early tracking countries. In late tracking countries, migrants even slightly outperform natives (on average) in reading (but not significantly so). Conditional on all covariates, migrants in non-OECD countries with mean GDP per capita and late tracking school systems significantly outperform their native peers in all subjects (see estimates of α_2 in Equation (3.1)). The coefficients on the OECD \times migrant interactions reveal that the migrant-native achievement

¹² Note that GDP per capita is de-meant to provide the interpretation in terms of deviations from a country with mean international income.

¹³ Appendix Table A3.3 reproduces the estimates found in Table 3.4, but estimates specifications for math and science separately for PISA and TIMSS data. Results are qualitatively comparable to the results presented in Table 3.4.

gap is mainly an issue in OECD countries. There, we observe that migrants perform significantly worse than natives.

As argued above, the estimates of α_1 and α_2 presented in Table 3.4 may not have a causal interpretation if unobserved differences between early and late tracking countries systematically affect the relative performance of migrants with respect to their native peers. In this case, the differences in secondary school performance between the two groups of countries may arise even if the age of educational tracking by ability were the same in all countries. While we cannot observe this counterfactual scenario, the availability of test score data on primary school performance allows a reasonable robustness analysis. Whether a country tracks students early should have no effect on migrant-native achievement gaps before tracking has actually occurred.¹⁴ Thus, we expect to find no significant estimate of α_1 when estimating Equation (3.1) based on primary school data.

Table 3.5, however, confirms that there is already a migrant-native achievement gap in early tracking countries in primary school. Coefficients on the interaction terms between early educational tracking and migrant status are negative and statistically significantly different from 0 in all three domains. This suggests that differences between early and late tracking countries in the performance of migrants relative to their native peers already exist in primary school. Unconditionally, estimates of α_1 in primary school are around 14 percent to 15 percent of a standard deviation in all three domains—about 5 percentage points lower than in secondary school. With further controls, estimates are 8 percent of a standard deviation in reading and 9 percent of a standard deviation in math and science. Overall, these estimates are just slightly lower in absolute terms than the estimates in secondary school, with a somewhat larger difference in reading. This clearly suggests that the estimates of the impact of early educational tracking on secondary school performance of migrants relative to their native peers presented in Table 3.4 are biased upward in absolute terms. However, there may be small effects of early educational tracking on migrant-native achievement gaps that result in an increase in achievement gaps over educational stages. Comparing coefficient estimates in Tables 3.4 and 3.5 suggest that this may particularly be the case for reading achievement.

¹⁴ We discuss this hypothesis, which is related to the parallel trends assumption and important for the causal interpretation of our results, further in Section 3.4.4.

Table 3.5
OLS Estimation – Primary School

	(1)	(2)	(3)	(4)	(5)	(6)
	Reading		Math		Science	
	PIRLS		TIMSS		TIMSS	
Early tracking × migrant	−0.138*	−0.077**	−0.136**	−0.092***	−0.151*	−0.091**
	(0.074)	(0.036)	(0.056)	(0.029)	(0.076)	(0.040)
Migrant	−0.046	0.081**	−0.077**	0.013	−0.083	0.039
	(0.039)	(0.033)	(0.034)	(0.026)	(0.051)	(0.032)
<i>Further controls</i>						
GDP per capita × migrant		0.003*		0.003***		0.004**
		(0.002)		(0.001)		(0.001)
OECD × migrant		−0.191***		−0.137***		−0.205***
		(0.042)		(0.032)		(0.045)
Male		−0.128***		0.060***		0.067***
		(0.013)		(0.008)		(0.010)
Age		−0.047*		0.132***		0.129***
		(0.025)		(0.046)		(0.042)
11-25 books		0.256***		0.230***		0.229***
		(0.021)		(0.029)		(0.028)
26-100 books		0.470***		0.443***		0.432***
		(0.033)		(0.037)		(0.039)
101-200 books		0.602***		0.556***		0.553***
		(0.043)		(0.045)		(0.047)
> 200 books		0.608***		0.571***		0.603***
		(0.049)		(0.054)		(0.058)
Constant	0.361***	0.470*	0.692***	−0.964**	0.222***	−1.432***
	(0.019)	(0.243)	(0.017)	(0.451)	(0.026)	(0.413)
Survey × country fixed effects	yes	yes	yes	yes	yes	yes
R^2	0.238	0.308	0.317	0.367	0.238	0.296
Individual observations	172,000	172,000	246,470	246,470	246,470	246,470
Distinct country observations	25	25	25	25	25	25
<i>Early tracking countries</i>	9	9	9	9	9	9

Notes: The samples are restricted to the set of countries used in the baseline DiD models. Dependent variables are the original test scores which are standardized within each survey and year to international mean 0 and sd 1. Robust standard errors in parentheses and clustered at the country level. Original students weights are used for weighting. Each observation is weighted such that each country carries a weight of one. *Early tracking* is 1 for countries with tracking age below 15 and 0 otherwise. The reference category for *books* is 0-10 books. *GDP per capita* is yearly de-measured to provide deviations from a country with mean GDP per capita. A migrant is defined as second generation migrant: The student is born in the testing country but at least one parent is born outside this country. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.3.2 Results from the Differences-In-Differences Model

The previous subsection showed that migrant-native achievement gaps are present in primary and secondary school. In this section, we combine both results to investigate whether early tracking has an effect on the change in migrant-native achievement gaps over educational stages by estimating the differences-in-differences model in Equation (3.2). Table 3.6 presents the results from estimating the differences-in-differences model once with and without further controls. All regressions include country-by-survey-cycle fixed effects. As before, we estimate regression models separately for each domain.

Estimates of the impact of educational-stage-invariant factors on migrant-native test score gaps (the parameter β_2 in Equation (3.2)) are set out in the second row of Table 3.6. The coefficient estimates on the early tracking times migrant interaction basically capture any differences in migrant-native test score gaps between early and late tracking countries that already exist in primary school. Consequently, the estimated coefficients in the second row are very similar to the estimates of α_1 in Table 3.5.

Our key parameter of interest, β_1 , in the differences-in-differences model is given by the estimates in the first row of Table 3.6. Only Column 1 shows a significant coefficient, suggesting that early educational tracking increases migrant-native test score gaps in reading by 7 percent of a standard deviation. However, when we additionally control for observable demographic and socioeconomic background characteristics, the coefficient estimate is one-third lower in absolute terms and statistically insignificant. For math and science, none of the coefficients on the triple interaction is significant. Moreover, point estimates in the specifications with further controls are small at -0.04 in math and -0.02 in science. Due to the clustering of standard errors at the country level, the precision of our estimates is somewhat low. Standard errors on the triple interaction coefficients in the estimations with further controls range from 0.03 to 0.06. We therefore cannot rule out the possibility that early educational tracking might have some small effects.

Overall, based on the findings, not much of a case can be made for a substantial negative impact of early educational tracking on overall migrant-native test score gaps. At most, our findings indicate that early educational tracking may have some detrimental impact on immigrants' relative achievement in reading. Although effect sizes below 0.1 cannot be rejected, our differences-in-differences estimates give rise to concern that estimates based on cross-sectional models, such as the one presented in Table 3.4, should not be interpreted causally.

Table 3.6
Differences-In-Differences Estimation – Baseline Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Reading		Math		Science	
Secondary: Primary:	PISA PIRLS	PISA PIRLS	PISA, TIMSS TIMSS	PISA, TIMSS TIMSS	PISA, TIMSS TIMSS	PISA, TIMSS TIMSS
Early tracking × secondary × migrant	−0.072* (0.039)	−0.048 (0.032)	−0.062 (0.057)	−0.037 (0.045)	−0.050 (0.064)	−0.020 (0.052)
Early tracking × migrant	−0.139* (0.074)	−0.082** (0.037)	−0.136** (0.056)	−0.080** (0.029)	−0.153* (0.076)	−0.084** (0.040)
Migrant × secondary	0.059** (0.025)	0.037* (0.019)	0.055* (0.027)	0.038** (0.015)	0.039* (0.022)	0.019 (0.012)
Migrant	−0.046 (0.039)	0.074** (0.032)	−0.077** (0.034)	0.030 (0.025)	−0.081 (0.051)	0.062* (0.035)
<i>Further controls</i>						
GDP per capita × migrant		0.003* (0.002)		0.003*** (0.001)		0.004** (0.002)
OECD × migrant		−0.186*** (0.047)		−0.170*** (0.036)		−0.236*** (0.050)
Male		−0.227*** (0.013)		0.074*** (0.011)		0.069*** (0.013)
Age		0.024 (0.019)		0.068* (0.035)		0.082** (0.034)
Secondary × 11-25 books		0.033 (0.019)		0.038 (0.030)		0.071*** (0.021)
Secondary × 26-100 books		0.094*** (0.023)		0.109*** (0.034)		0.161*** (0.025)
Secondary × 101-200 books		0.186*** (0.025)		0.204*** (0.039)		0.260*** (0.030)
Secondary × > 200 books		0.388*** (0.030)		0.379*** (0.045)		0.417*** (0.037)
11-25 books		0.253*** (0.022)		0.226*** (0.029)		0.224*** (0.029)
26-100 books		0.467*** (0.034)		0.435*** (0.037)		0.423*** (0.039)
101-200 books		0.599*** (0.044)		0.547*** (0.044)		0.541*** (0.046)
> 200 books		0.610*** (0.049)		0.561*** (0.054)		0.588*** (0.058)
Constant	0.538*** (0.029)	−0.131 (0.295)	0.865*** (0.029)	−0.637 (0.548)	0.697*** (0.035)	−1.058* (0.534)
Survey × country fixed effects	yes	yes	yes	yes	yes	yes
R ²	0.186	0.295	0.271	0.346	0.207	0.294
Individual observations	1,209,793	1,209,793	1,832,197	1,832,197	1,832,304	1,832,304
Distinct country observations	25	25	25	25	25	25
Early tracking countries	9	9	9	9	9	9

Notes: Dependent variables are the original test scores which are standardized within each survey and year to international mean 0 and sd 1. Robust standard errors in parentheses and clustered at the country level. Original students weights are used for weighting. Each observation is weighted such that each country carries a weight of one within each educational stage. *Early tracking* is 1 for countries with tracking age below 15 and 0 otherwise. *Secondary* is equal to 1 for testing in secondary school at age 14/15 (grade 8/9) and 0 for testing in primary school at age 9/10 (grade 4). A migrant is defined as second generation migrant: The student is born in the testing country but at least one parent is born outside this country. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

However, the relatively low statistical precision of these estimates demands a closer inspection of the data before drawing firm policy conclusions. In particular, the small negative point estimates may be a result of true zero effects for some groups of migrant students and substantial negative effects for others. We explore the possibility of substantial effect heterogeneities in Section 3.5, after conducting several robustness checks with respect to the choice of our preferred empirical specification in the next section.

3.4 Robustness Checks

In this chapter, we argue that, at a minimum, our differences-in-differences approach identifies the effect of early educational tracking under more credible assumptions than those used in making cross-sectional estimates. However, unobserved factors affecting the evolution over educational stages of migrant-native test score gaps that are correlated with age at tracking may bias our main results. In this section, we conduct a wide range of sensitivity checks to assess the robustness of our main results.

3.4.1 Different Country Samples

Table 3.7 presents a set of robustness checks and is organized similarly to Table 3.6. Each cell shows estimates of our differences-in-differences model in Equation (3.2). Outcomes are reading scores in Columns 1 and 2, math scores in Columns 3 and 4, and science scores in Columns 5 and 6. For each outcome, we estimate Equation (3.2) once with and without other controls. However, Table 3.7 only reports estimates of our key parameter of interest, β_1 . Each row represents a different robustness check, the type of which is specified in the first column.

We begin by estimating Equation (3.2) based on an extended set of countries. In this analysis, we include all 45 countries listed in Table 3.1. Results are reported in the first row of Table 3.7. The overall pattern of results is virtually the same. We obtain similar results again when restricting our sample to a set of countries for which we have data for all potential survey matches, that is, matches for reading in PIRLS–PISA, math in PISA–TIMSS and TIMSS–TIMSS, and science in PISA–TIMSS and TIMSS–TIMSS (see also Table 3.1). This restriction reduces the sample to 20 countries, six of which are early tracking countries. The results from this variation can be found in the second row of Table 3.7.

Table 3.7
Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Reading		Math		Science	
Secondary: Primary:	PISA PIRLS		PISA, TIMSS TIMSS		PISA, TIMSS TIMSS	
[1] All countries	-0.072* (0.036)	-0.049 (0.032)	-0.056 (0.068)	-0.035 (0.061)	-0.037 (0.069)	-0.019 (0.059)
[2] Common set of countries	-0.080* (0.041)	-0.064* (0.033)	-0.090 (0.063)	-0.064 (0.052)	-0.088 (0.079)	-0.056 (0.068)
[3] OECD countries	-0.091** (0.034)	-0.066** (0.028)	-0.043 (0.058)	-0.038 (0.056)	-0.039 (0.060)	-0.032 (0.058)
[4] Main destination countries	-0.131*** (0.032)	-0.078*** (0.024)	-0.094*** (0.028)	-0.066** (0.023)	-0.074** (0.023)	-0.040 (0.022)
[5] Migrant ratio > 10%	-0.099** (0.043)	-0.077** (0.031)	-0.107* (0.060)	-0.081 (0.049)	-0.095 (0.071)	-0.062 (0.062)
[6] Tracking grade 6	-0.062 (0.041)	-0.037 (0.028)	-0.035 (0.056)	-0.010 (0.036)	-0.012 (0.057)	0.017 (0.035)
[7] Tracking after grade 5	-0.076 (0.051)	-0.059 (0.040)	-0.072 (0.080)	-0.051 (0.067)	-0.078 (0.105)	-0.053 (0.090)
[8] Continuous tracking age	0.015* (0.008)	0.010 (0.006)	0.012 (0.011)	0.007 (0.008)	0.009 (0.011)	0.003 (0.009)
[9] Tracking grade	-0.057 (0.040)	-0.048 (0.032)	-0.037 (0.053)	-0.029 (0.039)	-0.043 (0.054)	-0.032 (0.043)
[10] Continuous tracking grade	0.010 (0.006)	0.008 (0.005)	0.005 (0.009)	0.004 (0.006)	0.004 (0.009)	0.002 (0.006)
[11] Same cohorts	-0.078* (0.039)	-0.049 (0.029)	-0.076 (0.062)	-0.050 (0.049)	-0.058 (0.066)	-0.026 (0.054)
[12] Same time	-0.101* (0.050)	-0.069 (0.050)	-0.042 (0.059)	-0.024 (0.054)	-0.048 (0.077)	-0.024 (0.067)
Further controls	-	yes	-	yes	-	yes
Survey × country fixed effects	yes	yes	yes	yes	yes	yes

Notes: All entries show the coefficient on the triple interaction between Early Tracking × Secondary × Migrant. Dependent variables are the original test scores which are standardized within each survey and year to international mean 0 and sd 1. Robust standard errors in parentheses and clustered at the country level. Original students weights are used for weighting. Each observation is weighted such that each country carries a weight of one within each educational stage. *Further controls:* early tracking × migrant, migrant × secondary, migrant, GDP per capita × migrant, OECD × migrant, male, age, secondary × 11–25 books, secondary × 26–100 books, secondary × 101–200 books, secondary × > 200 books, dummies for books at home. *Early tracking* is 1 for countries with tracking age below 15 and 0 otherwise. *Secondary* is equal to 1 for testing in secondary school at age 14/15 (grade 8/9) and 0 for testing in primary school at age 9/10 (grade 4). For continuous tracking age, tracking grade, and continuous tracking grade, the early tracking dummy is replaced by continuous tracking age, a dummy for tracking before grade 9, or continuous tracking grade respectively. A migrant is defined as second generation migrant: The student is born in the testing country but at least one parent is born outside this country. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

A country's level of economic development may matter for our analysis because of selective migration into more or less developed countries. Following Cobb-Clark, Sinning and Stillman (2012), we limit our sample to OECD countries (see Row 3 of Table 3.7). The estimates are slightly larger in absolute terms in all domains, but only the estimates for reading remain significant once further controls are included. In Row 4, we restrict the sample to include only main destination countries as defined in Grogger and Hanson (2011).¹⁵ This restriction results in even larger effect sizes in absolute terms, which are now significant in reading and math, and almost significant in science. The results indicate that especially the three countries Austria, Germany, and the Netherlands drive the small effects in the baseline regressions, including all countries.

The estimates change in a similar but more pronounced way when we exclude countries with a migrant ratio below 10 percent (instead of 3 percent as in the baseline sample). However, as shown in Row 5 of Table 3.7, estimates in math and science remain insignificant once further controls are included.¹⁶

Restricting our sample to subsets of countries (Rows 2 to 5 of Table 3.7) revealed that the selection of countries has some impact on our estimates. While some degree of country heterogeneity can be expected, a key concern is that our overall finding is entirely driven by the evolution of the relative achievement of migrants in a specific country. To investigate this possibility, we re-estimate our preferred specification several times and exclude each time a different country. Appendix Table A3.4 presents the results from this piecewise deletion exercise. The results are very similar and show that our main result is not driven by a specific country.

3.4.2 Different Tracking Definitions

As discussed in Section 3.1, we followed Hanushek and Woessmann (2006) in defining early tracking systems as education systems that track students for the first time into different school tracks before age 15. Rows 6 to 10 of Table 3.7 show the robustness of our main results to alternative definitions of early educational tracking.

¹⁵ These countries include the early tracking countries Austria, Germany, and the Netherlands, and the late tracking countries Canada, Denmark, New Zealand, Norway, Sweden, the UK, and the United States, which accounted – together with Australia, Finland, France, Ireland, Spain, which are excluded due to data availability – for 87.6 percent of all immigrants hosted by OECD countries in 2000 (Grogger and Hanson, 2011).

¹⁶ This restriction excludes the early tracking countries Czech Republic, Hungary, Italy, and Slovak Republic, and the late tracking countries Greece, Iceland, Lithuania, and Taipei. We also exclude Norway, Russia, and the United Kingdom in some survey matches. Thus, we have almost the same set of early tracking countries as in the main destination countries sample before, that is, Austria, Germany, the Netherlands, plus Singapore and Belgium.

We first employ a more restrictive definition of early tracking (see Row 6 of Table 3.7) and include as early tracking countries only those that track their students before age 12 and before grade 6 (see also Table 3.1). In Row 7, we check whether results are driven by the very early tracking countries and exclude countries that track students before grade 6. Using a dummy for categorizing countries according to their tracking status could lead to false classifications. In Row 8, we avoid this issue and use the actual tracking age (as reported in Table 3.1) instead of the early tracking dummy variable. The findings of Waldinger (2007) indicate that results may partly depend on whether age or grade of first selection is used to define early tracking systems. Thus, in Row 9, we define as early tracking countries those that track students before grade 8. Finally, in Row 10, we use the actual tracking grade (as reported in Table 3.1) instead of the early tracking dummy variable.

The results of these robustness checks indicate that our finding of no substantial impact of early educational tracking on migrant-native test score gaps is in no way driven by how "early tracking country" is defined.

3.4.3 Different Survey Matches Definitions

Our identification strategy may be influenced by potentially confounding cohort and calendar time effects as we basically compare different cohorts tested in different years. The existence of general cohort or calendar time effects is of no concern, but cohort or calendar time effects that differentially affect the relative performance of migrant students in early and late tracking countries might bias our main estimates.

To address this concern, we limit our attention to combinations of surveys (or test pairs) that either keep the cohort or the calendar year constant (see Figure 3.2). For example, one of these test pairs pools information on all fourth and eighth graders tested in TIMSS in 2007. In this specific test pair, calendar time effects should play no role as all students are tested at the same time, whereas cohort effects remain a worry. In another test pair, we pool information on all fourth graders tested in TIMSS 2003 and all eighth graders in 2007. In this case, cohort effects might be less of a concern as both samples should be drawn from the same cohort of students. In Row 11 of Table 3.7, we use only data from combinations of surveys that sample from the same cohort; in Row 12 we use only data from combinations of surveys that are conducted in the same calendar year. Once again the results are very similar to our baseline results.

In sum, the robustness checks generally support the validity of our main findings. In particular, none of the robustness checks that alter the definition of early tracking or that try to control for cohort or calendar time effects show significant negative effects. Only the restriction to main destination countries reveals significant effects in reading and math. However, this is more likely to reflect some true effect heterogeneity rather than being an indication for a bias in our results.

3.4.4 Anticipation Effects

Finally, a potential threat to our identification strategy would be if the migrant-native test score gap in primary school is affected by educational tracking itself. This could be the case, for example, when native parents put more effort than migrant parents in supporting their children in anticipation of the upcoming educational tracking. The possibility of such anticipation effects is a common and known threat to all studies with this identification strategy (e.g., Hanushek and Woessmann, 2006; Waldinger, 2007).

While we have no direct way of testing for anticipation effects, a widening of the migrant-native test score gap during primary school as students get closer to the tracking decision may be some indication for the relevance of anticipation effects. The fact that TIMSS has administered the test in primary school in 1995 within the same country to children in two different grades in the early tracking countries Singapore, Austria, Czech Republic, and the Netherlands allows us to document the evolution of migrant-native achievement gaps in these countries primary school grades. We do so by estimating Equation (3.1) separately for different grades in primary school.

We find that migrant-native achievement gaps increase only very little between the earlier and the later grade in the four countries (Appendix Table A3.1).¹⁷ Thus, this admittedly crude test reveals no indication for anticipation effects.¹⁸ Ultimately, we have to assume that anticipation effects do not confound our identification strategy, but this assumption is further supported by evidence presented in Waldinger (2007), who also concludes that anticipation effects are probably not very important.

¹⁷ For Singapore, Austria, and Czech Republic *earlier grade* refers to grade 3 and *later grade* refers to grade 4. For the Netherlands, *earlier grade* refers to grade 5 and *later grade* refers to grade 6.

¹⁸ Naturally, anticipation effects may still be relevant even in the absence of changes in migrant-native achievement gaps in later grades if native parents support the learning efforts of their children more than migrant parents in anticipation of the tracking decision already in first grade (or kindergarten).

3.5 Effect Heterogeneity

Overall, we find no strong evidence for a significant universal effect of early educational tracking on the relative achievement of migrant students, but results are also not very clear-cut and small effect sizes cannot be excluded. This may conceal a substantial detrimental effect of early tracking for specific groups of migrant students, while other migrant students indeed do not suffer from an earlier tracking age. Thus, it is less clear why early tracking should have any detrimental effect on relative achievement on children of migrants who are highly integrated into the host country's society. However, migrant students from less integrated families may have difficulties to signal their educational potential at an early stage of compulsory schooling and thus suffer more from a system that aims to match students to different educational pathways at an early age.

Proficiency in the national language is an obvious requirement for full participation in a country's society and consequently a good proxy for the degree of integration of migrants. Language proficiency itself is also essential to children's educational success. Previous studies already showed that second-generation migrants perform substantially better when they are more proficient in the national language (e.g., Dustmann, Frattini and Lanza, 2012; Schneeweis, 2011). Moreover, Akresh and Akresh (2010) provide experimental evidence that foreign-born students have better test results when taking a test in their native language instead of the language of the host country.

Thus, proficiency in the test language may be a key source of effect heterogeneity. We can create a rough binary measure of test language proficiency by using self-reported information on whether students frequently speak the test language at home. We then extend the regression model in Equation (3.2) by adding interaction terms between the migrant indicator and the dummy variable that identifies migrant students who either only sometimes or never speak the test language at home. Table 3.8 presents the results of estimating this extended model.

Overall, the results confirm previous evidence that migrant students who either sometimes or never speak the test language at home perform worse in all three domains than migrant students who almost always speak the test language at home. More importantly, however, estimates in Rows 1 and 2 of Table 3.8 suggest that the effect of early educational tracking in fact depends crucially on the proficiency in the language of the test. For the baseline category of migrant students who speak the test language at home, the coefficients on the triple interaction are now insignificant, close to zero and even have positive signs. For migrants who do not speak the test language at home, we instead find that early tracking reduces their relative achievement by 11 percent of a standard deviation in

reading. For math and science we find similar effects, which are, however, not significant because of large standard errors.

Table 3.9 tests for other effect heterogeneities. In this table, we estimate Equation (3.2) separately for subgroups of the student population. The type of subgroup analysis is indicated in the first column of Table 3.9. In Rows 1 and 2, we replicate the previous subgroup analysis along the language spoken at home divide in this more flexible way to provide a better benchmark. Other subgroups include boys, girls, and students within specific books-at-home categories. Additionally, we assess the robustness of our main results with respect to an alternative definition of migrant students. Specifically, we limit our attention to second-generation immigrants with both parents born abroad, excluding all other second-generation immigrants from the analysis.

In contrast to the effect heterogeneity that we observe with respect to the language spoken at home, we find very similar results for boys and girls as well as for students from different family backgrounds. The latter finding is particularly important as it shows that the detrimental effect of early tracking for migrant students who do not speak the test language at home is not a mere byproduct of a correlation between family background and language proficiency. Overall, these results underline the importance of national language proficiency and integration as the key source of effect heterogeneity.

Table 3.8
The Role of Language Spoken at Home

	(1)	(2)	(3)
	Reading	Math	Science
	Secondary: Primary: PISA PIRLS	PISA, TIMSS TIMSS	PISA, TIMSS TIMSS
Early tracking × secondary × migrant	0.003 (0.036)	0.017 (0.040)	0.047 (0.049)
Early tracking × secondary × migrant × foreign language	-0.109** (0.048)	-0.109 (0.082)	-0.142 (0.085)
Early tracking × migrant	-0.028 (0.045)	-0.048 (0.039)	-0.049 (0.048)
Migrant × secondary	0.027 (0.020)	0.027** (0.011)	0.006 (0.016)
Migrant	0.131*** (0.032)	0.065** (0.030)	0.128*** (0.045)
Early tracking × migrant × foreign language	-0.047 (0.049)	-0.045 (0.035)	0.011 (0.061)
Migrant × secondary × foreign language	-0.037 (0.042)	0.102 (0.060)	0.101 (0.061)
Migrant × foreign language	-0.124*** (0.034)	-0.165*** (0.026)	-0.308*** (0.061)
Constant	-0.149 (0.298)	-0.608 (0.555)	-1.049* (0.542)
Further controls	yes	yes	yes
Survey × country fixed effects	yes	yes	yes
R^2	0.296	0.347	0.297
Individual observations	1,198,521	1,809,465	1,809,568
Distinct country observations	25	25	25
<i>Early tracking countries</i>	9	9	9

Notes: Dependent variables are the original test scores which are standardized within each survey and year to international mean 0 and sd 1. Robust standard errors in parentheses and clustered at the country level. Original students weights are used for weighting. Each observation is weighted such that each country carries a weight of one within each educational stage. *Early tracking* is 1 for countries with tracking age below 15 and 0 otherwise. *Secondary* is equal to 1 for testing in secondary school at age 14/15 (grade 8/9) and 0 for testing in primary school at age 9/10 (grade 4). A migrant is defined as second generation migrant: The student is born in the testing country but at least one parent is born outside this country. *Foreign language* is 1 when the student does not speak (or speaks only sometimes) the language of the test country at home. We recode the foreign language variable to zero for natives who report that they do not speak the language of the test country at home. *Further controls:* GDP per capita × migrant, OECD × migrant, male, age, secondary × 11-25 books, secondary × 26-100 books, secondary × 101-200 books, secondary × > 200 books, 11-25 books, 26-100 books, 101-200 books, and > 200 books. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.9
Subgroup Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Reading		Math		Science	
Secondary: Primary:	PISA PIRLS		PISA, TIMSS TIMSS		PISA, TIMSS TIMSS	
[1] Language sometimes or never spoken	-0.217* (0.110)	-0.174*** (0.057)	-0.186 (0.158)	-0.100 (0.104)	-0.203 (0.170)	-0.126 (0.128)
[2] Language (almost) always spoken	-0.045 (0.039)	-0.020 (0.029)	-0.020 (0.045)	-0.008 (0.036)	-0.005 (0.052)	0.011 (0.043)
[3] Books at home: 0–25 books	-0.045 (0.044)	-0.042 (0.042)	-0.030 (0.053)	-0.022 (0.050)	-0.017 (0.062)	-0.007 (0.058)
[4] Books at home: 26–100 books	-0.055 (0.036)	-0.051 (0.033)	-0.043 (0.060)	-0.036 (0.055)	-0.024 (0.068)	-0.015 (0.063)
[5] Books at home: 101–200 books	-0.044 (0.049)	-0.047 (0.044)	-0.067 (0.048)	-0.057 (0.044)	-0.053 (0.052)	-0.042 (0.048)
[6] Books at home: > 200 books	-0.013 (0.045)	-0.012 (0.045)	-0.040 (0.048)	-0.029 (0.047)	-0.012 (0.051)	-0.000 (0.049)
[7] Male	-0.084* (0.045)	-0.058 (0.040)	-0.073 (0.065)	-0.055 (0.051)	-0.056 (0.066)	-0.031 (0.053)
[8] Female	-0.062 (0.043)	-0.040 (0.038)	-0.050 (0.057)	-0.021 (0.049)	-0.043 (0.067)	-0.010 (0.057)
[9] Both parents born abroad	-0.136** (0.061)	-0.070 (0.044)	-0.149* (0.086)	-0.073 (0.068)	-0.149 (0.091)	-0.058 (0.078)
Further controls	–	yes	–	yes	–	yes
Survey × country fixed effects	yes	yes	yes	yes	yes	yes

Notes: All entries show the coefficient on the triple interaction between Early Tracking × Secondary × Migrant. Dependent variables are the original test scores which are standardized within each survey and year to international mean 0 and sd 1. Robust standard errors in parentheses and clustered at the country level. Original students weights are used for weighting. Each observation is weighted such that each country carries a weight of one within each educational stage. *Further controls:* early tracking × migrant, migrant × secondary, migrant, GDP per capita × migrant, OECD × migrant, male, age, secondary × 11–25 books, secondary × 26–100 books, secondary × 101–200 books, secondary × > 200 books, dummies for books at home. *Early tracking* is 1 for countries with tracking age below 15 and 0 otherwise. *Secondary* is equal to 1 for testing in secondary school at age 14/15 (grade 8/9) and 0 for testing in primary school at age 9/10 (grade 4). Except in Row 9, a migrant is defined as second generation migrant: The student is born in the testing country but at least one parent is born outside this country. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

3.6 Conclusion

In this chapter, we investigate whether early ability-based tracking of students into different types of secondary school systematically increases migrant-native test score gaps. Based on individual test score data from 12 large-scale international tests, we show that estimates from cross-sectional regression models suggest large positive effects on migrant-native test score gaps of tracking students before the age of 15. However, estimates from differences-in-differences models that exploit variation in migrant-native test score gaps between primary and secondary school as well as variation in the age at tracking between countries reveal no significant effects. Although these

estimates lack precision and we cannot rule out small negative effects of early tracking, overall effect sizes suggested by cross-sectional estimates are overstated.

A large set of additional robustness checks confirms our main findings. Our main results are not driven by a particular subset of countries or by our definition of early educational tracking. Yet, in terms of reading achievement, there is some weak indication for a detrimental effect from early tracking.

A closer inspection of the data, however, reveals an important effect heterogeneity with respect to how frequently migrant students speak the language of the test at home. For migrant students who almost never speak the test language at home, we find a significant detrimental effect of early tracking on relative achievement in reading of about 11 percent of a standard deviation and similar, but insignificant, effect sizes in terms of math and science achievement.

In the context of migrant inclusion, proficiency in the national language has been shown to be a key determinant of educational success for children of immigrants (e.g., Dustmann, Frattini and Lanzar, 2012; Schneeweis, 2011; Akresh and Akresh, 2010). This study adds to the literature by providing first evidence on an important interaction between migrant's proficiency in the national language and the age of first tracking across school by ability. While early educational tracking does not appear to be systematically related to relative achievement of all students with a migration background, our findings indicate that tracking at later stages could enhance the educational opportunities of children from less integrated families. Thus, calls for a more comprehensive school system to improve opportunities for migrant students appear to be justified in particular in countries in which proficiency in the national language constitutes a barrier to integration for the majority of migrant families.

Appendix

Table A3.1
Migrant Achievement Gaps in Primary School in TIMSS 1995

	(1)	(2)	(3)	(4)	(5)	(6)
	Math			Science		
	Both grades	Earlier grade	Later grade	Both grades	Earlier grade	Later grade
Migrant	-0.120*** (0.016)	-0.099*** (0.021)	-0.116*** (0.022)	-0.131*** (0.016)	-0.103*** (0.022)	-0.138*** (0.022)
Male	0.042*** (0.012)	0.069*** (0.015)	0.067*** (0.016)	0.132*** (0.011)	0.127*** (0.016)	0.176*** (0.015)
Age	0.383*** (0.010)	-0.089*** (0.020)	-0.150*** (0.020)	0.315*** (0.009)	-0.046** (0.020)	-0.067*** (0.019)
11-25 books	0.324*** (0.029)	0.309*** (0.034)	0.280*** (0.040)	0.340*** (0.028)	0.323*** (0.037)	0.312*** (0.037)
26-100 books	0.692*** (0.027)	0.606*** (0.032)	0.617*** (0.037)	0.672*** (0.026)	0.630*** (0.034)	0.599*** (0.034)
101-200 books	0.871*** (0.028)	0.760*** (0.033)	0.813*** (0.039)	0.868*** (0.027)	0.798*** (0.036)	0.817*** (0.036)
> 200 books	0.908*** (0.028)	0.795*** (0.033)	0.888*** (0.040)	0.949*** (0.027)	0.866*** (0.035)	0.937*** (0.036)
Constant	-4.149*** (0.100)	0.183 (0.189)	1.589*** (0.208)	-3.371*** (0.096)	-0.044 (0.191)	0.726*** (0.201)
Country fixed effects	yes	yes	yes	yes	yes	yes
R^2	0.208	0.167	0.164	0.168	0.114	0.127
Individual observations	26,629	13,131	13,498	26,629	13,131	13,498
Distinct country observations	4	4	4	4	4	4

Notes: The sample is restricted to early tracking countries in TIMSS 1995 in primary schooling: Netherlands, Singapore, Austria, and Czech Republic. For Singapore, Austria, and Czech Republic *earlier grade* refers to grade 3 and *later grade* refers to grade 4. For the Netherlands, *earlier grade* refers to grade 5 and *later grade* refers to grade 6. Dependent variables are the original test scores which are standardized within each survey and year to international mean 0 and sd 1. Robust standard errors in parentheses. Original students weights are used for weighting. Each observation is weighted such that each country carries a weight of one. The reference category for *books* is 0-10 books. A migrant is defined as second generation migrant: The student is born in the testing country but at least one parent is born outside this country. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A3.2
Country List by Matched Survey Waves

Country	Baseline sample	PISA-PIRLS						TIMSS-TIMSS						PISA-TIMSS						
		S2000 P2001	S2006 P2001	S2006 P2006	S2009 P2006	S2012 P2006	S1995 P1995	S2003 P2003	S2007 P2007	S1999 P1995	S2007 P2003	S2011 P2007	S2000 P1995	S2003 P2003	S2006 P2003	S2009 P2003	S2009 P2007	S2012 P2007		
<i>Panel A: Early tracking countries</i>																				
Austria	yes		yes	yes	yes	yes	yes	yes										yes		
Belgium	yes		yes	yes	yes	yes	yes	yes										yes		
Czech Republic	yes	yes	yes	yes	yes	yes	yes	yes										yes		
Germany	yes	yes	yes	yes	yes	yes	yes	yes										yes		
Hungary	yes		yes	yes	yes	yes	yes	yes										yes		
Italy	yes	yes	yes	yes	yes	yes	yes	yes										yes		
Netherlands	yes	yes	yes	yes	yes	yes	yes	yes										yes		
Singapore	yes	yes	yes	yes	yes	yes	yes	yes										yes		
Slovak Republic	yes	yes	yes	yes	yes	yes	yes	yes										yes		
Ireland	no																	yes		
Luxembourg	no		yes	yes	yes													yes		
Macedonia	no																	yes		
Mongolia	no							yes										yes		
Trinidad and Tobago	no				yes													yes		
United Arab Emirates	no																	yes		
<i>Panel B: Late tracking countries</i>																				
Canada	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Denmark	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Greece	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Hong Kong	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Iceland	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Latvia	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Lithuania	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
New Zealand	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
norway	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Qatar	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Russia	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Slovenia	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Sweden	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Taipei	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
United Kingdom	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
United States	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Argentina	no																	yes		
Australia	no																	yes		
Cyprus	no																	yes		
El Salvador	no																	yes		
France	no	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Georgia	no																	yes		
Israel	no		yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes		
Kazakhstan	no																	yes		
Kuwait	no																	yes		
Moldova	no																	yes		
Morocco	no																	yes		
Portugal	no																	yes		
Spain	no																	yes		
Tunisia	no																	yes		
45		17	19	20	20	23	27	27	27	27	17	20	12	13	18	15	14	16	24	25

Notes: Early tracking is defined as the age of first selection before age 15. The letter in front of the survey year indicates testing in primary school (P) or secondary school (S).

Table A3.3
OLS Estimation – Secondary School by Survey

	Reading			Math			Science			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		PISA	PISA	PISA	TIMSS	TIMSS	PISA	PISA	TIMSS	TIMSS
Early tracking × migrant	-0.207* (0.105)	-0.128** (0.058)	-0.193* (0.110)	-0.100 (0.062)	-0.161 (0.108)	-0.098* (0.053)	-0.205* (0.120)	-0.101 (0.061)	-0.159 (0.136)	-0.088 (0.056)
Migrant	0.012 (0.054)	0.103** (0.042)	-0.029 (0.056)	0.080** (0.037)	0.013 (0.050)	0.087*** (0.021)	-0.043 (0.063)	0.095** (0.044)	-0.016 (0.072)	0.114*** (0.033)
<i>Further controls</i>										
GDP per capita × migrant		0.004** (0.002)		0.003** (0.002)		0.004*** (0.001)		0.003* (0.002)		0.004** (0.002)
OECD × migrant		-0.180*** (0.055)		-0.202*** (0.053)		-0.186*** (0.037)		-0.247*** (0.059)		-0.287*** (0.054)
Male		-0.326*** (0.018)		0.114*** (0.015)		0.041** (0.015)		0.037* (0.018)		0.122*** (0.030)
Age		0.151*** (0.010)		0.149*** (0.014)		-0.066* (0.036)		0.154*** (0.008)		-0.022 (0.036)
11-25 books		0.284*** (0.025)		0.264*** (0.025)		0.235*** (0.033)		0.294*** (0.025)		0.262*** (0.027)
26-100 books		0.556*** (0.035)		0.548*** (0.035)		0.485*** (0.043)		0.579*** (0.034)		0.525*** (0.040)
101-200 books		0.776*** (0.043)		0.767*** (0.041)		0.662*** (0.050)		0.805*** (0.041)		0.719*** (0.049)
> 200 books		0.990*** (0.055)		1.003*** (0.054)		0.775*** (0.056)		1.041*** (0.055)		0.863*** (0.056)
Constant	0.538*** (0.029)	-2.081*** (0.173)	0.870*** (0.031)	-1.939*** (0.233)	0.725*** (0.027)	1.174*** (0.493)	0.697*** (0.034)	-2.185*** (0.138)	0.108** (0.039)	-0.149 (0.509)
Country × Survey fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.146	0.289	0.178	0.294	0.322	0.391	0.140	0.262	0.245	0.330
Individual observations	624,472	624,472	521,593	521,593	331,978	331,978	521,700	521,700	331,978	331,978
Distinct country observations	25	25	25	25	21	21	25	25	21	21
<i>Early tracking countries</i>	9	9	9	9	6	6	9	9	6	6

Notes: The samples are restricted to the set of countries used in the baseline DiD models. Dependent variables are the original test scores which are standardized within each survey and year to international mean 0 and sd 1. Robust standard errors in parentheses and clustered at the country level. Original students weights are used for weighting. Each observation is weighted such that each country carries a weight of one. *Early tracking* is 1 for countries with tracking age below 15 and 0 otherwise. The reference category for *books* is 0-10 books. *GDP per capita* is yearly demeaned to provide deviations from a country with mean GDP per capita. A migrant is defined as second generation migrant. The student is born in the testing country but at least one parent is born outside this country. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A3.4
Piecewise Deletion of Countries

	(1)		(2)		(3)	
	Reading		Math		Science	
	Secondary: Primary:	PISA PIRLS	PISA, TIMSS TIMSS	PISA, TIMSS TIMSS	PISA, TIMSS TIMSS	PISA, TIMSS TIMSS
<i>Excluded country</i>	<i>Coefficient</i>	<i>Std. error</i>	<i>Coefficient</i>	<i>Std. error</i>	<i>Coefficient</i>	<i>Std. error</i>
Baseline	-0.048	(0.032)	-0.037	(0.045)	-0.020	(0.052)
<i>Panel A: Early tracking countries</i>						
Austria	-0.043	(0.034)	-0.033	(0.051)	-0.019	(0.060)
Belgium	-0.028	(0.030)	-0.000	(0.036)	0.027	(0.034)
Czech Republic	-0.051	(0.033)	-0.037	(0.048)	-0.022	(0.055)
Germany	-0.040	(0.034)	-0.027	(0.052)	-0.016	(0.063)
Hungary	-0.056*	(0.031)	-0.046	(0.044)	-0.027	(0.053)
Italy	-0.056*	(0.031)	-0.048	(0.046)	-0.031	(0.054)
Netherlands	-0.044	(0.035)	-0.035	(0.051)	-0.011	(0.059)
Singapore	-0.057*	(0.033)	-0.055	(0.048)	-0.045	(0.055)
Slovak Republic	-0.053	(0.032)	-0.051	(0.043)	-0.033	(0.052)
<i>Panel B: Late tracking countries</i>						
Canada	-0.044	(0.033)	-0.037	(0.046)	-0.024	(0.052)
Denmark	-0.054*	(0.031)	-0.043	(0.045)	-0.027	(0.052)
Greece	-0.047	(0.032)	-0.035	(0.045)	-0.019	(0.052)
Hong Kong	-0.056*	(0.032)	-0.042	(0.046)	-0.020	(0.053)
Iceland	-0.045	(0.032)	-0.038	(0.045)	-0.020	(0.052)
Latvia	-0.054	(0.032)	-0.040	(0.045)	-0.022	(0.052)
Lithuania	-0.054*	(0.031)	-0.037	(0.045)	-0.022	(0.052)
New Zealand	-0.051	(0.032)	-0.041	(0.045)	-0.022	(0.052)
Norway	-0.052	(0.032)	-0.040	(0.045)	-0.024	(0.052)
Qatar	-0.029	(0.031)	-0.021	(0.044)	-0.011	(0.054)
Russia	-0.045	(0.032)	-0.035	(0.045)	-0.017	(0.052)
Slovenia	-0.045	(0.032)	-0.040	(0.045)	-0.022	(0.052)
Sweden	-0.048	(0.033)	-0.043	(0.045)	-0.023	(0.052)
Taipei	-0.043	(0.032)	-0.036	(0.046)	-0.015	(0.052)
United Kingdom	-0.045	(0.032)	-0.033	(0.045)	-0.016	(0.052)
United States	-0.040	(0.032)	-0.035	(0.045)	-0.016	(0.052)

Notes: All entries show the coefficient on the triple interaction between Early Tracking \times Secondary \times Migrant. Dependent variables are the original test scores which are standardized within each survey and year to international mean 0 and sd 1. Robust standard errors in parentheses and clustered at the country level. Original students weights are used for weighting. Each observation is weighted such that each country carries a weight of one within each educational stage. All regressions include survey \times country fixed effects and all further controls. *Further controls:* early tracking \times migrant, migrant \times secondary, migrant, GDP per capita \times migrant, OECD \times migrant, male, age, secondary \times 11–25 books, secondary \times 26–100 books, secondary \times 101–200 books, secondary \times > 200 books, dummies for books at home. *Early tracking* is 1 for countries with tracking age below 15 and 0 otherwise. *Secondary* is equal to 1 for testing in secondary school at age 14/15 (grade 8/9) and 0 for testing in primary school at age 9/10 (grade 4). For continuous tracking age, tracking grade, and continuous tracking grade, the early tracking dummy is replaced by continuous tracking age, a dummy for tracking before grade 9, or continuous tracking grade respectively. A migrant is defined as second generation migrant: The student is born in the testing country but at least one parent is born outside this country. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Chapter 4

Immigration and Intergenerational Income Mobility: Evidence from U.S. Metropolitan Areas

Equal opportunity is generally viewed as a desirable feature of society. The correlation between (log) parental income and (log) child income is commonly used to characterize the extent of equal opportunity (Solon, 1999; Black and Devereux, 2011). A higher correlation signals lower income mobility because the success of the child depends more strongly on the success of the parents. Comparing intergenerational income mobility elasticities of father-son earnings across countries, the Scandinavian countries (Denmark 0.071, Norway 0.155, Finland 0.173, Sweden 0.258) show especially high income mobility whereas the United Kingdom (0.306) and the United States (0.517) exhibit relatively low income mobility (Jaentti et al., 2006). The high elasticity in the United States indicates that climbing the social ladder is not so much a matter of own will and diligence but depends strongly on parental background. However, a recent study by Chetty et al. (2014) finds that the aggregate country-level elasticity masks substantial regional variation in the intergenerational income mobility of U.S. citizens. For example, cities such as Milwaukee (WI) and Memphis (TN) have elasticities that are twice as large as the elasticities of Los Angeles (CA) and Denver (CO). Indeed, the latter two cities are comparable to Scandinavian countries in terms of intergenerational income mobility.

In this chapter, I argue that immigration is responsible for some of the regional variation in intergenerational income mobility of U.S. citizens. Starting with the National Immigration Act of

1965, which mainly allowed the immigration of family members of former migrants, and continuing to the present day, the U.S. has experienced a massive inflow of immigrants – raising the share of people born abroad from 6.8 percent in 1980 to 14.3 percent in 2010 (Grieco et al., 2012). Recent immigrants are on average less skilled than natives (Smith, 2014) and are especially likely to have immigrated from Latin American countries (Antecol, Cobb-Clark and Trejo, 2003).

To show the effect of immigration on the intergenerational income mobility of U.S. citizens, I use newly compiled data from the *Equality of Opportunity Project*, which provides detailed income mobility measures for the United States at the regional level (Chetty et al., 2014). I find that U.S. citizens who are born in a metropolitan area with a relatively higher share of foreign-born immigrants exhibit significantly higher intergenerational income mobility. Causation is established by using predicted shares of foreign-born immigrants, constructed from historical distributions of migrants across metropolitan areas and observed national inflow rates of immigrants over time by world region, to instrument more current shares of foreign-born immigrants. My explanation for this finding is that immigration changes the occupational structure of a metropolitan area such that it pushes natives into occupations that are more associated with higher income mobility. The results suggest that the native born accomplish this endogenously by specializing in occupations that are more complementary to, rather than competitive with, the occupational choices of immigrants.

Based on extant literature on the economic effects of immigration, it is unclear whether and how immigration-induced efficiency gains are distributed in the population and whether they contribute to increase income mobility. The traditional literature on how immigration influences labor market outcomes for natives, starting with the work of Card (1990) on the Mariel Boatlift from Cuba, argues that more immigrants should depress labor market opportunities by lowering wages and increasing unemployment (Borjas, 2003; Aydemir and Borjas, 2011). However, recent papers show non-negative or even positive wage effects due to the inflow of immigrants (Peri, 2011; Glitz, 2012; Ottaviano and Peri, 2012; Manacorda, Manning and Wadsworth, 2012; Dustmann, Frattini and Preston, 2013). In attempting to explain this result, Peri and Sparber (2009) show that due to the inflow of low-skilled immigrants, low-skilled natives are pushed out of manual-physical task-intensive occupations and more into communication-language task-intensive occupations. Peri (2012) indicates that this shift in occupational structure is indeed productivity enhancing at the U.S.–state level. Ottaviano and Peri (2006) find that more immigrants increase wage levels and rental prices in U.S.–metropolitan areas. Thus, the change in occupational structure paired with wage increases for low-skilled natives predict higher income mobility.

However, choosing better occupations or higher educational levels might be difficult, especially for children from low-income families. Restuccia and Urrutia (2004) argue that credit constraints are one of the main forces keeping low-income families from investing in their children's education. Thus, it could well be that children want to change their occupation and increase their education in response to immigration, but that parents are not able to afford the additional expenditures. In addition, several studies find that a heterogeneous population could decrease the provision of public goods (Alesina, Baqir and Easterly, 1999; Desmet, Ortuño Ortín and Weber, 2009; Dahlberg, Edmark and Lundqvist, 2012). In this context, Behrman, Gaviria and Szekely (2001) suggest that more public educational spending increases intergenerational income mobility because credit constraints of low-income families become less important. Thus, if high-immigration cities are less willing to support public education, we might see that immigration leads to less income mobility. Thus, theoretically, the effect of immigration on intergenerational income mobility of the host society population is ambiguous.

This study tries to clarify whether being born into an environment with a high share of foreign-born immigrants is beneficial or detrimental for intergenerational income mobility of U.S. citizens. The analysis starts by using newly compiled data from Chetty et al. (2014) to show the effect of the share of foreign-born immigrants on intergenerational income mobility. Chetty et al. (2014) use all children born in 1980-2 and match them to the tax filers who claimed them as dependents on their tax records. Intergenerational income mobility is then measured by the correlation between the national child income rank in 2010-2011 and the national parent income rank in 1996-2000 (*rank-rank correlation*) at the metropolitan area level. I merge this information with Census data from different years (Ruggles et al., 2010) and regress the rank-rank correlation on the share of foreign-born in 1980. Identification is established by using the hypothetical share of foreign-born in 1980 based on the distribution of migrants in 1930 together with national inflow rates between 1930 and 1980 by world region as exogenous variation in an instrumental variables approach (*shift-share methodology*; see, e.g., Card (2001), Peri (2012)). My baseline finding is that being born into a society with a larger migrant population is associated with more intergenerational income mobility as the correlation between the parent income rank and the child income rank is reduced by 6.4 percent for one standard deviation increase in the share of foreign-born (almost 4 percentage points).

The connection between the share of foreign-born and intergenerational income mobility is of a reduced form nature. I find evidence that immigration-induced change in occupational structure, together with the educational choices of children born in 1980, can explain the effect of the share

of foreign-born on intergenerational income mobility. Other explanations, such as school inputs, family resources, entrepreneurial activity, patenting, or crime, play only a minor role or none at all. More specifically, I find that immigration has very little impact on school inputs (measured by school expenditures per pupil, pupil-teacher ratio, and teacher salaries) and family resources (measured by family income at the age of 10), which explain why I do not find a strong connection between these two factors and intergenerational income mobility.

The remainder of the chapter is organized as follows. Section 4.1 describes the data, sets out summary statistics, and illustrates the regional variation of intergenerational income mobility and the share of foreign-born. Section 4.2 explains the econometric setup, provides conditional correlation results, and presents the identification strategy together with the main results of our analysis. In Section 4.3, I provide evidence for the channels through which the share of foreign-born affect intergenerational income mobility of U.S. citizens. Section 4.4 concludes.

4.1 Immigration and Income Mobility of U.S. Citizens across the United States

Our main outcome of interest is intergenerational income mobility within U.S.–citizen families. The Harvard-based *Equality of Opportunity Project* compiled different income mobility measures at the level of U.S. metropolitan areas (Chetty et al., 2014). The authors used federal tax records of U.S. citizens from the Internal Revenue Service (IRS). Their sample consists of children born between 1980–1982 (almost 10 million children).¹ They were able to match 95 percent of children to tax filers who claimed them as dependents on their tax records. Child income and parental income are then measured as pre-tax total family income. For the children, the period under investigation is the years 2010 and 2011 and the parental income is a five-year average from 1996 to 2000.

Chetty et al. (2014) rank all children and parents separately according to their income rank in the national income distribution. The intergenerational mobility measures are based on the correlation between the parents' income rank and the child income rank.² When assessing regional

¹ Tax records do not allow the identification of the country of birth. Thus, it could be that first- or second-generation migrants who possess the U.S. citizenship are included in the data. Unfortunately, there is not much that we can do about it, however, we discuss this issue further in Section 4.2.5 and in the mechanism section of this chapter, where we use Census data and can restrict the sample to U.S. born natives.

² This method is closely related to that employed by Dahl and DeLeire (2008). Using rank-rank correlations instead of the classical approach to regress log child wages on log parent wages (Solon, 1992) has two main advantages: first, it is not necessary to drop zero wages and, second, the nonlinear relationship between log child income and log parent income makes cross-sectional comparisons inappropriate, whereas the rank-rank correlation is almost linear.

intergenerational income mobility, children are permanently assigned to the parents' region, thus providing some indication of where the children grew up.³ Chetty et al. (2014) find that 38 percent of children do not live in the same region today that they did when they were 15. However, Chetty et al. (2014) also show that the migration of children might not be an issue when regions are characterized according to their income mobility. Using only children who stayed in their region of birth, they find very similar statistics that correlate almost perfectly with the baseline intergenerational mobility measures.

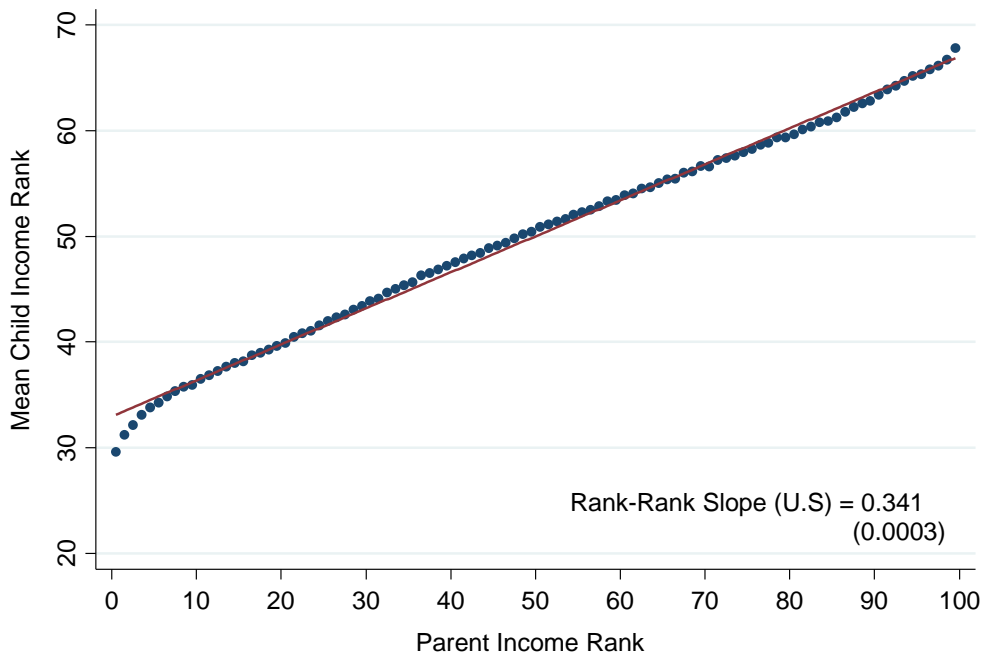
Figure 4.1 is a binned scatter plot of the children's income rank versus the parents' income rank and reveals an almost linear relationship. Interpreting the coefficient of the underlying OLS regression, an increase of parental income rank by 1 percentage point (rank) is associated with an increase of 0.341 percentage points (ranks) in the child income rank. This is far from a perfect determination of income ranks by parental income rank; for a perfect determination, the resulting correlation would be near to one. However, the parents income rank is a substantial factor in the child's success.⁴

Chetty et al. (2014) define two main intergenerational income mobility measures: absolute and relative income mobility. These are derived from the observation that the rank–rank correlation is almost linear at the national level and also within regions. Therefore, the intercept of the OLS regression of child rank on parents rank, as well as the slope of this regression, are sufficient to characterize income mobility within a region. Relative mobility is represented by the slope of the regression line. This parameter can be used for discovering the relative outcome between children from high-income versus children from low-income families. Contrary to relative mobility,

³ Specifically, Chetty et al. (2014) assign children to the ZIP code from which their parents filed taxes in the first year the child was claimed as a dependent. Because the data start in 1996, the location is measured in the same year for almost all children. At that time, most children are age 15 and thus not very likely to work full-time. In addition, using more recent cohorts, Chetty et al. (2014) show that 83.5 percent of children live in the same region at age 16 as they did at age 5.

⁴ Chetty et al. (2014) discuss two issues with regard to the reliability of the intergenerational income mobility measures. One potential issue is lifecycle bias in the intergenerational mobility estimates because lifetime earnings are difficult to predict based on earnings in younger ages (Haider and Solon, 2006). Chetty et al. (2014) show, however, that the rank–rank correlation stabilizes after measuring child income after the age of 30. Additionally, the rank–rank correlation is not much affected in regard to what years of income are used to determine parental income rank. Another issue is attenuation bias due to transitory income shocks and measurement error. The present rank–rank correlations do not suffer much from attenuation bias. By varying the years used to compute parental income, Chetty et al. (2014) show that the bias is around 6.5 percent which is much less than the 33 percent frequently found in survey–based measures (Solon, 1992). Chetty et al. (2014) argue that their rank–rank correlation is more stable because income tax data are measured with less error, the concept of family income compared to individual income is less volatile and less frequently subject to transitory shocks, and the rank–rank specifications are less affected by fluctuations at the bottom of the distribution than are log–log specifications. Chetty et al. (2014) also check whether attenuation bias in child income could be a problem, and find that the years taken to compile average child income do not matter much for the rank–rank correlation.

Figure 4.1
Mean Child Income Rank by Parent Income Rank



Notes: The panel divides parents by percentiles (on the x-axis) based on their income. Children are ranked from 0 to 1 by earnings as adults. The figure then depicts mean income rank for children by percentile of parent's income. The figure is taken from Chetty et al. (2014).

the intercept, together with the slope at a given percentile, defines absolute mobility and reveals the expected income rank of children when the parents are at a specific percentile. Chetty et al. (2014) focus on the 25th percentile of the parents rank ($E[\text{child rank} | \text{parent} = p25]$). With some simple calculations, we can also construct expected child income ranks for other parental ranks. To check whether immigration has an effect on children whose parents are at the top of the income distribution, we use the expected rank of the child given that the parents are at the 75th percentile, $E[\text{child rank} | \text{parent} = p75]$.

Relative and absolute mobility have different features. A higher relative mobility, that is, a flatter slope, in region A compared to region B does not necessarily mean that children from low-income families exhibit more upward mobility in region A compared to children from low-income families in region B. It could well be that the smaller slope is caused by children from high-income parents in region A who do not do as well as children from high-income families in region B. It is up to the social planner to decide whether absolute or relative mobility is more important. In the analysis, we use both measures, but rely more on relative income mobility as this measure characterizes the income mobility conditions within a region without having to choose a specific percentile for

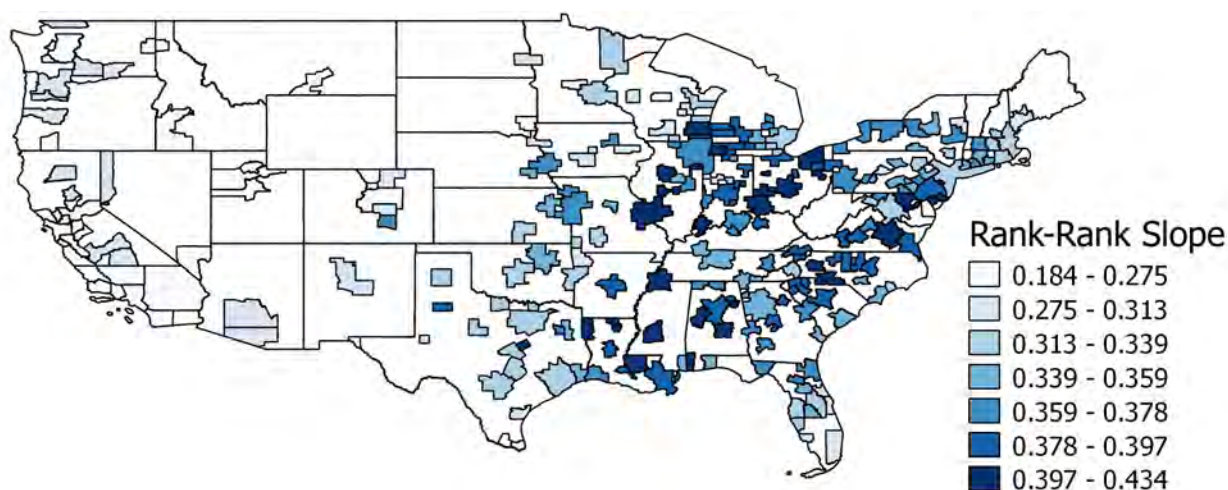
the absolute income mobility. Moreover, in the literature on intergenerational mobility, it is more common to use relative mobility.

Table 4.1 sets out summary statistics for two different samples of metropolitan areas. The full sample shows statistics for the 232 areas for which we have data in 1980. The restricted sample shows statistics for the 117 areas for which we have data in 1980 and 1930. We use this restricted sample in the instrumental variables estimation, which is introduced and discussed later in the chapter.

Summary statistics for relative income mobility (*rank-rank slope*) and for absolute income mobility ($E[\text{child rank}|\text{parent} = p25]$ and $E[\text{child rank}|\text{parent} = p75]$) is shown in Panel A of Table 4.1. The mean of the rank-rank slope in the full sample (0.340) is almost identical to the national average (0.341). The regional distribution across U.S. metropolitan areas of the rank-rank slope is shown in Figure 4.2. There is considerable regional variation. Metropolitan areas in the West Coast region show especially high income mobility (low rank–rank correlations) whereas, for example, metropolitan areas in the South can be characterized by low income mobility.

Figure 4.2

Intergenerational Rank-Rank Slope Across U.S. Metropolitan Areas



Notes: The figure shows the correlation between the income rank of the child and the income rank of the parents in the national income distribution. Darker areas indicate a higher correlation between the ranks. Source: own depiction based on Chetty et al. (2014).

Other data used in this study include several iterations of the U.S. Census and the American Community Survey (ACS). Both are provided by the Integrated Public Use Microdata Series (IPUMS) of the Minnesota Population Center of the University of Minnesota (Ruggles et al., 2010). The data are used to construct our main explanatory variable of interest –the share of foreign-born population at the metropolitan area level in the year the child is born (1980)– as well as most of our control

Table 4.1
Summary Statistics

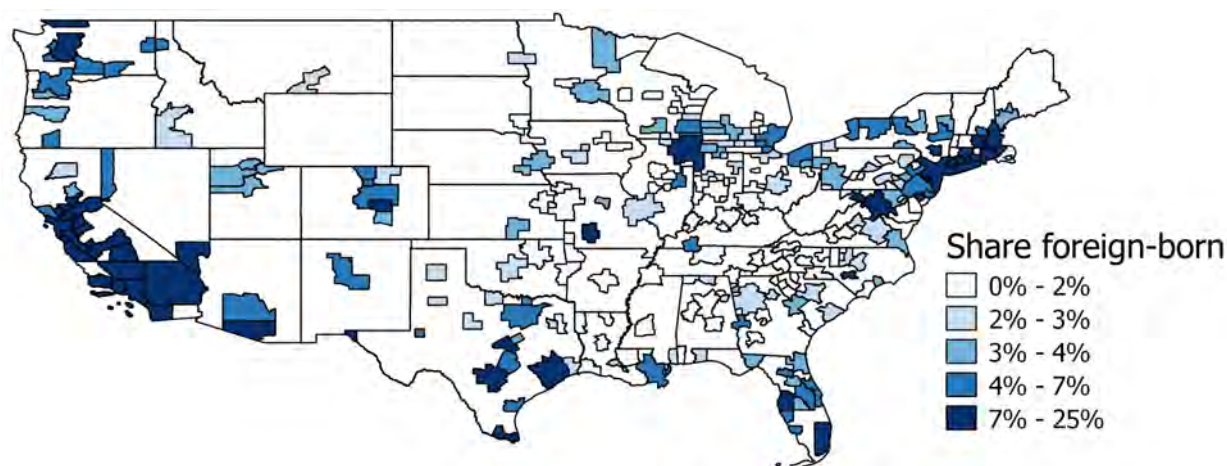
Variable	Full Sample		Restricted Sample		Mean	
	Mean	SD	Mean	SD	Difference	
<i>Panel A: Income mobility</i>						
Rank-rank slope	0.340	0.053	0.359	0.044	-0.019	***
$E[\text{child rank} \text{parent} = p25]$	41.111	3.237	40.755	3.147	0.356	
$E[\text{child rank} \text{parent} = p75]$	58.119	2.665	58.710	2.457	-0.591	**
<i>Panel B: Share of foreign-born</i>						
Share foreign-born ₁₉₈₀	0.053	0.048	0.047	0.038	0.005	
Share foreign-born without children ₁₉₈₀	0.033	0.027	0.029	0.022	0.004	
Share low-skilled foreign-born ₁₉₈₀	0.044	0.048	0.040	0.036	0.005	
Share high-skilled foreign-born ₁₉₈₀	0.009	0.008	0.009	0.008	0.000	
Share foreign-born ₁₉₃₀	0.111	0.084	0.108	0.082	0.003	
Share foreign-born ₁₉₈₀ , predicted	0.066	0.104	0.048	0.044	0.018	*
<i>Panel C: Controls</i>						
Median income ₁₉₈₀	9,466	1,357	9,876	1,135	-410	***
75/25 income ratio ₁₉₈₀	6.370	3.115	6.033	1.564	0.337	
Population ₁₉₈₀	684,554	1,385,185	1,076,861	1,847,797	-392,306	**
Population density ₁₉₈₀	235	227	312	253	-77	***
Share unemployed ₁₉₈₀	0.050	0.021	0.048	0.016	0.002	
Share college educated ₁₉₈₀	0.217	0.064	0.218	0.054	-0.001	
Share male ₁₉₈₀	0.486	0.015	0.482	0.010	0.004	***
Median age ₁₉₈₀	28.871	2.966	29.227	1.723	-0.356	
Share blacks ₁₉₈₀	0.101	0.095	0.117	0.099	-0.016	
High/low-skilled migrant ratio ₁₉₈₀	0.336	0.127	0.342	0.101	-0.006	
Share out-of-state natives ₁₉₈₀	0.367	0.199	0.329	0.180	0.039	*
Share low-income families ₁₉₈₀	0.014	0.008	0.012	0.004	0.002	**
<i>Panel D: Further controls</i>						
Racial segregation ₂₀₀₀	0.207	0.090	0.240	0.090	-0.034	***
Income segregation ₂₀₀₀	0.079	0.026	0.088	0.024	-0.009	***
Capital exp. per worker ₂₀₀₇	12,074	8,268	12,293	8,551	-219	
Avg. number of establishments _{1991–2005}	29,991	50,610	45,275	64,707	-15,283	**
Average export share _{2003–2011}	0.118	0.051	0.122	0.044	-0.004	
Migration outflow rate _{2004–2005}	0.020	0.008	0.018	0.006	0.002	**
Migration inflow rate _{2004–2005}	0.022	0.011	0.018	0.007	0.004	***
Bank deposits per capita ₂₀₀₉	24,792	39,336	25,621	28,882	-829	
Cost of living index ₂₀₁₁	1.016	0.146	1.015	0.149	0.000	
Cost of housing index ₂₀₁₁	1.039	0.357	1.029	0.363	0.009	
Average population growth _{1930–1980}			0.017	0.010		
Average wage growth _{1970–1980}			0.092	0.015		

Notes: The rank-rank slope is the gradient of the OLS regression of child income rank on parent income rank. $E[\text{child rank}|\text{parent} = p25]$ represents the expected child income rank of parents who are at the 25th percentile of their income distribution (absolute income mobility). Similarly, $E[\text{child rank}|\text{parent} = p75]$ represents the expected child income rank of parents who are at the 75th percentile of their income distribution. The full sample has 232 observations and the restricted sample has 117 observations. See Table A4.1 and Table A4.2 for a description of variable construction and data sources. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

variables. The share of the foreign-born population is defined as people who are born outside the United States as a percentage of the total population. The baseline regressions use the share of foreign-born at the year of birth of the child cohort in 1980.

The summary statistics in Panel B of Table 4.1 show that the population of the average metropolitan area consists of almost 5 percent foreign-born. However, the standard deviation of 5 percentage points means that there is a great deal of regional variation, which is illustrated by Figure 4.3. Generally speaking, the coastal areas host the most foreign-born as a percentage of their population, but some more interior cities, for example, Chicago, have large foreign-born populations too.

Figure 4.3
Share of Foreign-Born Across U.S. Metropolitan Areas



Notes: The figure shows the share of foreign-born immigrants as a percentage of the total population. Darker areas indicate a higher share of foreign-born. Source: own computations based on Ruggles et al. (2010).

Summary statistics for other control variables, which we discuss in the next section, are set out in Panels C and D of Table 4.1. Appendix Tables A4.1 and A4.2 describe the detailed construction of and data sources for all the main variables.

4.2 Causal Effect of Immigration on Income Mobility of U.S. Citizens

4.2.1 Regression Setup and Baseline Control Variables

Our econometric specification begins with the formulation of an OLS model. We regress the rank–rank slope, that is, the correlation between the parent national income rank in 1996–2000 and the child national income rank in 2010–2011, in metropolitan area i , which is in region r , on the share of foreign-born in 1980, control variables, which are summarized in \mathbf{X}_{ir} , and region-fixed effects, $region_r$. ε_{ir} is an idiosyncratic error term.

$$Rank - Rank Slope_{ir} = \alpha_0 + \alpha_1 ForeignBorn_{ir}^{1980} + \mathbf{X}_{ir}'\gamma + region_r + \varepsilon_{ir} \quad (4.1)$$

The region–fixed effects contain a dummy for each of the four Census regions: West, Midwest, Northeast, and South. This specification ensures that we only compare similar metropolitan areas in the same Census region.⁵ The vector \mathbf{X}_{ir} includes several control variables for the year 1980. We control for the income distribution of a metropolitan area by including the *log median income* and the *75/25 income ratio*. The first controls for general shifts in the income distribution; the latter controls for the overall income inequality of the metropolitan area. We use the *total population* and *population density* to capture agglomeration effects. The same reasoning applies to the *share of college educated*. The *share of unemployed* should capture employment opportunities. The *ratio of high-skilled to low-skilled migrants* should capture the selection of migrants with different skills into different metropolitan areas.⁶ The *share out-of-state natives* measures the share of natives of a metropolitan area who were not born in the same state. We use this variable as an indicator for the general attractiveness of a metropolitan area. Next, we include the *share of low-income families*. The reason for doing so is that a higher proportion of low-income families could have different effects on intergenerational mobility. On the one hand, it might be easier for children from

⁵ Results are not statistically significant different to the baseline result when we include fixed effects for the nine Census divisions: Pacific, Mountain, West North Central, East North Central, West South Central, East South Central, South Atlantic, New England, and Middle Atlantic. Therefore, to save degrees of freedom, we include Census region fixed effects only.

⁶ This control variable might be problematic as it could also be a treatment variable. For example, receiving relatively more high-skilled migrants might be more beneficial for natives than receiving a lower skill mix. But that might also be a threat to our identification strategy, which we will discuss later. The problem here is that cities that attract a better skill mix might have done so several decades before and that is what is driving the results. Therefore, we find it more important to use this variable as a control variable rather than as a treatment variable. However, we do not observe that the ratio of high-skilled to low-skilled migrants in 1980 or 2010 affect our results.

those regions to experience high mobility because the current level of living is low. On the other hand, it could be more difficult for them because they receive less support than other children from metropolitan areas home to richer parents. Other important variables are detailed *employment shares by occupation*. There are 22 employment shares that are constructed by following the Standard Occupational Classification (SOC) system (see Table A4.4). Even though they drive down the degrees of freedom of model, they are important when we discuss the mechanism in Section 4.3. We also include in our baseline model the *median age*, the *share males*, and the *share of blacks*. For the construction of variables at the metropolitan area level, we use the 1980 Census and apply survey sampling weights (Ruggles et al., 2010). Summary statistics of all variables are set out in Panels C and D of Table 4.1. Appendix Table A4.1 provides an overview of how the variables are constructed. Other controls are introduced along with the discussion of the results.

4.2.2 Conditional Correlation Results

Table 4.2 shows the baseline OLS results from the model in Equation (4.1). Column (1) displays the simple correlation between the share of foreign-born in 1980 and the rank-rank slope. The coefficient is negative and highly significant, indicating that a higher share of foreign-born is associated with a lower rank–rank correlation, that is, with higher intergenerational income mobility of U.S. citizens.⁷

Column (2) of Table 4.2 adds control variables, which are generated from the 1980 Census (Ruggles et al., 2010). The coefficient on the share of foreign-born decreases in absolute size but continues to be highly significant. The strongest predictor of income mobility among the control variables is the share of blacks. This is in line with the findings of Chetty et al. (2014). Especially the South, with its history of slavery, shows high shares of blacks and low income mobility (see Figure 4.1). There are a few variables that are also significantly correlated with income mobility (population density, share college educated, share male, high/low-skilled migrant ratio, and share out-of-state natives); however, the significance as well as the economic importance of these variables disappears when we control for occupational structure in 1980 (Column (3)) and add region–fixed effects (Column (4)). As mentioned before, following the Standard Occupational

⁷ Chetty et al. (2014) use the contemporaneous share of foreign-born in 2000 as one of the covariates. Using their dataset and all commuting zones, we find that the coefficient of the share of foreign-born is equal to -0.318 and significant at the 1 percent level. Restricting to commuting zones that intersect with a metropolitan area, the coefficient increases in absolute size to -0.416 . Using the 232 metropolitan areas in our sample and regressing the rank–rank correlation on our own constructed share of foreign-born in 2000, we obtain a coefficient on the share of foreign-born of -0.418 , which is almost identical to the estimate obtained from the Chetty et al. (2014) data.

Table 4.2
Immigration and Intergenerational Income Mobility

	(1)	(2)	(3)	(4)	(5)
Share foreign-born ₁₉₈₀	-0.596*** (0.119)	-0.435*** (0.125)	-0.385*** (0.128)	-0.342*** (0.112)	-0.312*** (0.115)
Controls:					
Log median income ₁₉₈₀		0.011 (0.021)	0.019 (0.023)	0.004 (0.025)	-0.004 (0.026)
75/25 income ratio ₁₉₈₀		-0.001 (0.001)	0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)
Log population ₁₉₈₀		-0.003 (0.003)	-0.001 (0.003)	-0.000 (0.003)	-0.007 (0.006)
Population density ₁₉₈₀ × 10 ⁻³		0.049*** (0.016)	0.022 (0.015)	0.016 (0.013)	0.019 (0.013)
Share unemployed ₁₉₈₀		-0.110 (0.106)	-0.172* (0.098)	-0.031 (0.123)	0.040 (0.132)
Share college educated ₁₉₈₀		-0.218*** (0.057)	-0.187 (0.114)	-0.028 (0.104)	-0.019 (0.108)
Share male ₁₉₈₀		-0.565*** (0.202)	-0.269 (0.197)	-0.236 (0.183)	-0.092 (0.180)
Median age ₁₉₈₀ × 10 ⁻²		-0.063 (0.091)	0.162 (0.102)	0.091 (0.101)	0.053 (0.113)
Share blacks ₁₉₈₀		0.234*** (0.031)	0.249*** (0.036)	0.244*** (0.040)	0.212*** (0.043)
High/low-skilled migrant ratio ₁₉₈₀		0.051** (0.020)	0.019 (0.021)	-0.001 (0.021)	-0.007 (0.020)
Share out-of-state natives ₁₉₈₀		-0.033*** (0.012)	-0.003 (0.011)	-0.002 (0.010)	0.000 (0.012)
Share low-income families ₁₉₈₀		-0.389 (0.422)	-0.264 (0.423)	-0.104 (0.403)	-0.372 (0.396)
Further controls:					
Racial segregation ₂₀₀₀					0.051 (0.045)
Income segregation ₂₀₀₀					0.109 (0.133)
Log capital expenditure per worker ₂₀₀₇					-0.004 (0.004)
Log avg. establishments ₁₉₉₁₋₂₀₀₅ × 10 ⁻²					0.373 (0.624)
Average export share ₂₀₀₃₋₂₀₁₁					0.011 (0.052)
Migration outflow rate ₂₀₀₄₋₂₀₀₅					-0.590 (0.496)
Migration inflow rate ₂₀₀₄₋₂₀₀₅					-0.234 (0.321)
Log bank deposits per capita ₂₀₀₉ × 10 ⁻²					0.186 (0.540)
Cost-of-living index ₂₀₁₁					-0.033 (0.021)
Constant	0.372*** (0.006)	0.617*** (0.212)	0.135 (0.329)	0.385 (0.324)	0.417 (0.316)
Occupational employment shares ₁₉₈₀	–	–	yes	yes	yes
Region fixed effects	–	–	–	yes	yes
R ²	0.284	0.639	0.703	0.750	0.760
Observations	232	232	232	232	232

Notes: Dependent variable is the *rank-rank slope*, i.e. the correlation between the parent (1996-2000) and child income (2010-11) ranks in the national income distribution. The intergenerational income data is from Chetty et al. (2014). See Table A4.1 and Table A4.2 for a description of variable construction and data sources. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Classification (SOC) system, we use 22 occupational employment shares (see Table A4.4) to model the occupational structure of a metropolitan area. Region–fixed effects ensure that we only compare metropolitan areas within the four Census regions (West, Midwest, Northeast, and South). Including all these control variables leads to a reduction in the size of the coefficient from -0.596 in the raw correlation (Column (1)) to -0.342 (Column (4)).

Even though region–fixed effects have been included, there could be some concern that the observed correlation is entirely driven by the main immigration hubs in the coastal areas: California, New York, and Florida (see Ottaviano and Peri, 2006). By excluding metropolitan areas in these regions, we find that the coefficient is slightly more negative and significant.⁸

In Column (5) of Table 4.2, we include several other control variables. Racial and income segregation might be affected by a larger inflow of foreign-born migrants and Chetty et al. (2014) show that these variables matter for income mobility. We add log capital expenditure per worker in 2007 because booming areas might be able to invest more per worker, which would make them more productive. Higher productivity could lead to higher income mobility. We use the log average number of establishments between 1991 and 2005 to see whether areas with more establishments drive the relationship between immigration and income mobility. The next variable is average exports as a share of GDP over the period 2003 to 2011. This controls, on the one side, for economic activity, and on the other side for the general openness of the metropolitan area, which could be a factor in attracting migrants. For the same reason, we include the average migration in- and outflow of the metropolitan area in 2004–2005. Another concern is that areas with a high share of foreign-born have higher cash deposits, which might lead to higher income mobility due to interest payments or from greater inherited wealth. It could also be that areas with higher cash deposits are less credit constrained and therefore can invest more in child education. Therefore, we include log bank deposits per capita from 2009. Chetty et al. (2014) examine cost of living differences between metropolitan areas to check whether their estimates are flawed by different purchasing powers, and find that this not to be the case. Nevertheless, we also check our results by using a cost-of-living indicator.

All these variables might be bad controls because they are measured after the share of foreign-born in 1980 is realized (Angrist and Pischke, 2009). We were not able to collect data for earlier years. However, adding these variables does not affect the coefficient on the share of foreign-born

⁸ The correlation also survives the introduction of state–fixed effects. Including all control variables as in Column (3) and state–fixed effects, we find a coefficient of -0.254 (0.116) (significant at the 5 percent level). However, as a substantial number of metropolitan areas overlap state boundaries and most states have only a few metropolitan areas, we do not use state–fixed effects in our baseline model.

significantly. We can also include the variables separately. There is no model that shows a significant change in the coefficient on the share of foreign-born compared to the baseline model in Column (4). Thus, we rule out these variables as the driving force behind the relationship between the share of foreign-born and income mobility.

4.2.3 Identification Strategy

The main barrier to identifying a causal parameter for the share of foreign-born in the previous OLS regression is that migrants may be attracted to (booming) cities that provide higher income mobility for U.S. citizens. If this would be the case, the coefficients from the OLS regression would be too negative, that is, it is biased upward in absolute terms. Moreover, the proposed OLS regression is unable to capture all possible determinants of intergenerational mobility at the metropolitan area level. Thus, the estimates are very likely prone to omitted variables (over and above that of booming cities). Lastly, because there is some measurement error in the share of foreign-born, in that these figures are from survey data, the estimates are biased toward zero (Aydemir and Borjas, 2011). A more severe measurement error is the problem of child migration. Chetty et al. (2014) permanently assign children to the area in which they reside at age 15. However, as they report that 38 percent move away from the region in which they grew up⁹, we potentially assign irrelevant shares of foreign-born to some children. By doing so, we add measurement error to the model when, for example, the share of foreign-born at the time of entering the labor market matters most for intergenerational mobility. This should again lead to estimates that are biased toward zero. Thus, the direction of the bias is unknown a priori.

To provide exogenous variation in the share of foreign-born so as to cope with omitted variable bias and measurement error in an instrumental variables approach, we rely on the commonly used *shift-share methodology* (Card, 2001; Peri, 2012). Card (2001) argues that the current inflow of immigrants can be decomposed into two parts: first, an exogenous supply-push component, which is derived by the fraction of migrants by birthplace who live in a region and the national inflow of migrants into the country. Second, an endogenous demand-pull component that captures all deviations from the historical pattern. The instrument is constructed by predicting migrant shares by birthplace region based on the historical distribution of migrants together with national birthplace region-specific inflow rates. This approach gives the share of immigrants by birthplace that would be observed in the absence of region-specific demand-pull factors.

⁹ Note that we concentrate on the metropolitan areas only and that the number shown is valid for all regions of the U.S. The number of movers from metropolitan areas only is not reported by Chetty et al. (2014).

Table 4.3
National Birthplace Shares 1930 – 2010

World region	Year			National growth rates	
	1930	1980	2010	1930-1980	1980-2010
India, Pakistan, Asia others	0.009%	0.182%	0.979%	1,878%	438%
Cuba	0.015%	0.277%	0.366%	1,738%	32%
Central America	0.011%	0.172%	1.008%	1,474%	486%
Africa	0.008%	0.096%	0.530%	1,156%	450%
South America	0.026%	0.269%	0.919%	932%	242%
US oversea	0.049%	0.472%	0.552%	857%	17%
Southeast Asia	0.079%	0.429%	1.276%	445%	197%
China, Singapore	0.044%	0.212%	0.730%	388%	244%
Caribbean	0.068%	0.296%	0.879%	335%	197%
Japan, Korea	0.100%	0.278%	0.537%	178%	94%
Mexico	0.524%	0.999%	3.868%	91%	287%
Middle East	0.048%	0.069%	0.175%	44%	154%
Turkey	0.039%	0.026%	0.037%	-34%	45%
Southern Europe	1.766%	0.631%	0.329%	-64%	-48%
Australia, NZ, Canada, UK	2.827%	0.888%	0.697%	-69%	-22%
Northwestern Europe	3.019%	0.855%	0.634%	-72%	-26%
Russia, Central Europe	3.102%	0.630%	0.740%	-80%	17%
U.S. Total	11.734%	6.781%	14.256%	-42%	110%

Notes: The table shows national birthplace shares as a percentage of the total U.S. population. The world regions are ranked according to their growth rates between 1930 and 1980. Source: own computations based on Ruggles et al. (2010).

The data allow us to go back in time until 1930. Going back in time leads to sample size problems because many of the metropolitan areas did not exist in the past. The instrument for the share of foreign-born in 1980 is constructed as follows (see Ottaviano and Peri (2006)): assume that BPL_{ci}^t is the birthplace share of migrants from world region c among the population of metropolitan area i at time t . Thus, the share of foreign-born at time t of metropolitan area i is the sum of all birthplace shares, $ForeignBorn_i^t = \sum_c BPL_{ci}^t$. Let BPL_c^t be the national share of migrants from world region c as a percentage of the total U.S. population.¹⁰

$$g_c^{1930-80} = \frac{BPL_c^{1980} - BPL_c^{1930}}{BPL_c^{1930}} \quad (4.2)$$

The national growth rate of the birthplace share is given by Equation (4.2). The national birthplace shares and their growth rates are set out in Table 4.3.

$$\widetilde{BPL}_{ci}^{1980} = BPL_{ci}^{1930} * (1 + g_c^{1930-80}) \quad (4.3)$$

¹⁰ Note that $BPL_c^t \neq \sum_i BPL_{ci}^t$ because the sum only includes the metropolitan areas.

Multiplying the share in 1930 by one plus the growth rate of each birthplace share between 1930 and 1980 predicts the birthplace shares in 1980 (Equation (4.3)).

$$\widetilde{ForeignBorn}_i^{1980} = \sum_c \widetilde{BPL}_{ci}^{1980} \quad (4.4)$$

Finally, adding up all predicted birthplace shares over all world regions c for each metropolitan area, we arrive at the predicted share of foreign-born in 1980 (Equation (4.4)). We use these predicted shares as the instrument in the first-stage relationship (Equation (4.5)). We regress the share of foreign-born in 1980 on the predicted share of foreign-born in 1980 with the same set of controls and region-fixed effects as in the OLS regression.

$$ForeignBorn_{ir}^{1980} = \beta_0 + \beta_1 \widetilde{ForeignBorn}_{ir}^{1980} + \mathbf{X}'_{ir} \boldsymbol{\theta} + region_r + v_{ir} \quad (4.5)$$

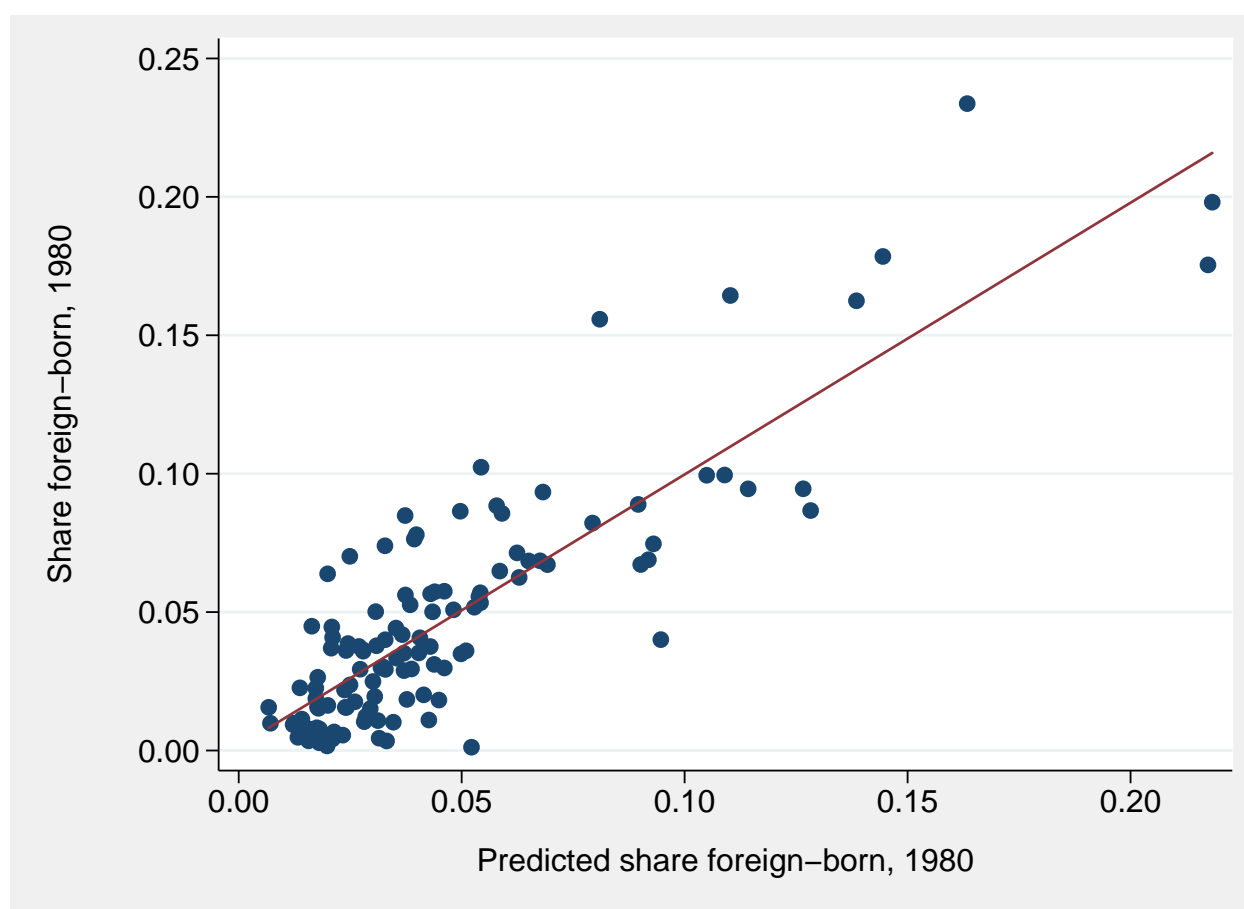
Figure 4.4 shows the unconditional correlation between the instrument and the endogenous variable. We document there a strong positive correlation.¹¹

$$Rank - rank\ slope_{ir} = \alpha_0 + \alpha_1 \widehat{ForeignBorn}_{ir}^{1980} + \mathbf{X}'_{ir} \boldsymbol{\gamma} + region_r + \varepsilon_{ir} \quad (4.6)$$

The second stage of the model regresses the rank-rank slope on the fitted values of the share of foreign-born, $\widehat{ForeignBorn}_{ir}^{1980}$, which are obtained from the first stage (Equation (4.6)). Thus, for identification, we are using only the variation in the share of foreign-born in 1980 that is due to the distribution of migrants in 1930 together with the national inflow over the period from 1930 to 1980. The identifying assumption in this specific application requires that the distribution *and* the share of migrants in 1930 is not otherwise correlated with the correlation between child income rank in 2010-2011 and parental income rank in 1996-2000.

¹¹ We drop Miami, FL because it has an exceptional position as an immigration gateway, resulting in this city having by far the largest migrant enclave in the U.S. The long time period over which we predict birthplace shares leads to four outliers: Sacramento, CA, Tampa, FL, San Antonio, TX, and El Paso, TX. We drop these cities because we predict migrant shares for them that are above the observed maximum share of foreign-born in 1980 (see Figure A4.1 in the Appendix).

Figure 4.4
Predicted and Observed Share of Foreign-Born in 1980



Notes: The figure plots the first stage relationship between the predicted share of foreign-born in 1980 and the observed share of foreign-born in 1980. The predicted share of foreign-born in 1980 is based on the distribution of foreign-born immigrants in 1930 and the national growth rate of immigrants by world region. Source: own computations based on Ruggles et al. (2010).

A threat to this identification strategy is that there are factors that are persistent over time and drive both the share of foreign-born and intergenerational income mobility. The construction of the instrument makes it possible to control for the observed share of foreign-born in 1930. Thus, our baseline model is given by:

$$\begin{aligned} Rank - rank\ slope_{ir} = & \alpha_0 + \alpha_1 \widehat{ForeignBorn}_{ir}^{1980} + \alpha_2 ForeignBorn_{ir}^{1930} + \\ & \mathbf{X}'_{ir} \gamma + region_r + \varepsilon_{ir} \end{aligned} \quad (4.7)$$

Conditioning on the share of foreign-born in 1930, the identifying assumption relaxes to the requirement that the distribution (and not the share) of migrants in 1930 is not otherwise correlated with intergenerational income mobility in the 2000s.

Remaining threats to the identification strategy are still that persistence in economic outcomes generate a correlation in the distribution of migrants in 1930 and intergenerational income mobility in the 2000s. Including the share of immigrants in 1930 captures a lot of persistent effects, such as, for example, that a changing industry structure attracts more migrants, that we might worry about. To mitigate further the threats of persistent effects, we include in the robustness section of this chapter the population growth rate and the wage growth rate as control variables to the model.¹² In addition, we include several variables (share out-of-state natives, migration in- and outflow, average export share) and a robustness check using attitudes toward immigration and measures of personality traits (Appendix Table A4.5) that should capture the general attractiveness of a region. Thus, we are confident that the resulting coefficient on the share of foreign-born reveals a close to causal effect.

4.2.4 Instrumental Variables Results

Table 4.4 displays the baseline results from the instrumental variables estimation. Because our instrument conditions on the distribution of immigrants in 1930, the sample for the IV estimation is reduced. Summary statistics in Panel A of Table 4.1 show that this restricted sample comprises areas with a significant larger slope, indicating lower income mobility. The expected income rank of the child for parents at the 25th percentile is not significantly different between the two samples, whereas the expected income rank of children from parents at the 75th percentile is significantly

¹² In fact, the share of foreign-born in 1980 cannot explain wage growth between 1970 and 1980. However, the share of foreign-born in 1980 is positively correlated with wage growth from 1980 to 2010. Nor can the share of foreign-born in 1980 explain population growth from 1980 to 2010, however, it is positively correlated with population growth between 1930 and 1980. Results are available from the author.

higher in the restricted sample. However, the absolute differences are small in economic terms. Total population and population density indicate that the restricted sample contains larger metropolitan areas. Because these areas are larger, they host more establishments and exhibit more in- and outflow of people. Moreover, these areas are also more segregated by race and income, which can explain the lower income mobility.

Column (1) of Table 4.4 replicates the OLS model from Column (4) of Table 4.2 for the reduced sample of metropolitan areas available in the IV approach. Comparing the coefficient of the OLS regression in Column (1) of Table 4.4 (-0.433) to the corresponding coefficient of the OLS regression in Column (4) of Table 4.2 (-0.342), estimated on the full sample, reveals that the estimate on the restricted sample is more negative. Testing the equality of both coefficients, however, we cannot reject that both coefficients are equal.

Column (2) of Table 4.4 describes the first-stage regression (see Equation (4.5)). The predicted share of foreign-born is a highly significant predictor for the share of foreign-born in 1980. The corresponding Kleibergen-Paap F statistic is 54.461 and the partial R^2 is 0.479.¹³ The F statistic indicates whether the IV suffers from a weak instrumental variables problem. The empirical F statistic is well above the critical values tabulated by Stock and Yogo (2005). Thus, a bias from weak instruments should not be an issue even though the critical values are only valid under i.i.d. errors.¹⁴

The reduced form is presented in Column (3) of Table 4.4 and reports a negative and significant coefficient on the predicted share of foreign-born. This is reassuring that there is a causal effect of the predicted share of foreign-born in 1980 through the observed share of foreign-born in 1980. Column (4) displays the second stage by using the predicted shares of foreign-born from the first stage instead of the share of foreign-born in 1980 directly (see Equation (4.6)).¹⁵ The coefficient on the share of foreign-born in the second stage is highly significant and almost identical to the coefficient in the OLS regression.

¹³ In the event of non-i.i.d. errors, the Cragg-Donald F statistic is no longer valid. Instead, we have to rely on the Kleibergen-Paap F statistic (Kleibergen and Paap, 2006).

¹⁴ To date, there are no critical values for the case of non-i.i.d. errors available. Baum, Schaffer and Stillman (2007) suggest using the critical values with caution. By using these critical values, we are more conservative than if we used the rule-of-thumb of a critical value of 10 from Staiger and Stock (1997).

¹⁵ The model is implemented by using `ivreg2`, which estimates the model by 2SLS. This accounts for using predicted shares of foreign-born in the second stage instead of the observed values when computing standard errors in the second stage.

Table 4.4
Baseline IV Results

Dependent variable:	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	Slope		Foreign-born		RF		Slope		Slope		Rank _{p=25}		Rank _{p=75}	
	OLS	FS	FS	RF	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
Share foreign-born ₁₉₈₀	-0.433*** (0.123)				-0.447*** (0.137)									
Share foreign-born ₁₉₈₀ , predicted		0.699*** (0.095)		-0.313** (0.127)										
Share foreign-born ₁₉₃₀										0.075 (0.074)				
Control variables ₁₉₈₀	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Occupational employment shares ₁₉₈₀	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Region fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.687	0.820	0.820	0.664	0.687	0.687	0.687	0.687	0.683	0.683	0.740	0.740	0.701	0.701
Observations	117	117	117	117	117	117	117	117	117	117	117	117	117	117
Kleibergen-Paap F statistic					54.461	54.461	54.461	54.461	29.711	29.711	29.711	29.711	29.711	29.711
Partial R^2					0.479	0.479	0.479	0.479	0.345	0.345	0.345	0.345	0.345	0.345

Notes: *Slope* is the correlation between the parent (1996-2000) and child income (2010-11) ranks in the national income distribution. *Foreign-born* is the share of foreign-born in 1980. *Rank_{p=25}* (*Rank_{p=75}*) is the expected income rank of the child given that the parent is at the 25th (75th) rank of the national income distribution. The intergenerational income data is from Chetty et al. (2014). *Control variables*: log median income, 75/25 income ratio, log population, population density, share unemployed, share college educated, share male, median age, share blacks, high/low-skilled migrant ratio, share out-of-state natives, and share low-income families. See Table A4.1 for a description of variable construction and data sources. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Including the share of foreign-born in 1930 relaxes the assumption of our exclusion restriction. Therefore, Column (5) of Table 4.4 adds the share of foreign-born in 1930 to obtain the baseline model (Equation (4.7)). Compared to the model in Column (4), the coefficient on the share of foreign-born in 1980 increases in absolute terms. The F statistic on the excluded instrument is lower, as expected, but still sufficiently high.¹⁶ The observed share of foreign-born in 1930 is not associated with intergenerational income mobility. As mentioned above, persistent factors that drive both the share of foreign-born in 1980 and intergenerational income mobility should show up in the share of foreign-born in 1930.

Regarding effect sizes, the coefficient on the share of foreign-born in Column (5) of Table 4.4 indicates that a one standard deviation increase (4 percentage points) in the share of foreign-born decreases the child income rank for each parental income rank by 0.023 ranks. Evaluated at the mean, this implies that a one standard deviation increase in the share of foreign-born decreases the correlation between child and parental income ranks by 6.4 percent.

The result in Column (5) of Table 4.4 could be due to either children from low-income families doing better or children from high-income families doing worse—or a combination of both. To disentangle these effects, we run regressions in Columns (6) and (7) by using the measures of absolute intergenerational income mobility as the dependent variable. In Column (6), the dependent variable is the expected rank of the child given that the parents are at the 25th percentile. The coefficient is positive and significant, indicating higher expected income ranks for children from low-income families who were born in metropolitan areas with a higher share of foreign-born migrants. For a one standard deviation increase in the share of foreign-born, the expected income rank of the child increases by almost one rank (0.992). Evaluated again for the average metropolitan area, we observe an increase in the rank of 2.43 percent. For children from parents at the 75th percentile, the share of foreign-born has no significant effect. This is in line with Chetty et al. (2014) who find that there is more regional variation at the bottom of the income distribution than at the top.

These results indicate that there is a causal effect of the share of foreign-born on intergenerational income mobility. Economically, the effect is sizable but not very large. However, the current setup does not allow discovering whether the share of foreign-born has real effects on native incomes.

¹⁶ Using a Herfindahl index as a measure of ethnic diversity leads to the conclusion that more birthplace diversity leads to higher intergenerational income mobility (results available upon request). This is not surprising because the share of foreign-born is closely related to ethnic diversity (Ottaviano and Peri, 2006). However, the diversity dimension is hard to identify because the Herfindahl index of ethnic diversity and the share of foreign-born correlates at 99 percent. Even the modified Herfindahl index (Audretsch, Dohse and Niebuhr, 2009), which should account for the dominant population group of natives, correlates at 90 percent.

Increases in the rank of children in high-immigration areas can occur because their incomes remain stable while the incomes of children in low-immigration areas decrease. Even though this would also provide support for a treatment effect from immigration, the policy conclusions might be different. We discuss this issue in more detail in Section 4.3.1. First, however, we address the robustness of the relationship in the baseline model.

4.2.5 Robustness Checks

Table 4.5 includes further controls. Column (1) replicates the baseline IV results from Column (7) of Table 4.4. All other control variables used in the OLS model are included step-by-step. None of the variables significantly changes the coefficient on the share of foreign-born.

In Column (2) of Table 4.5, we present another robustness check of our identification strategy. The causal estimate hinges critically on the assumption that the result is not driven by booming metropolitan areas that attract more migrants. We already introduced the share of foreign-born in 1930, which should alleviate much of this concern. However, in Column (2), we add the average yearly population growth rate from 1930 to 1980 and the average yearly wage growth rate from 1970 to 1980. These variables should control for booming areas that provide more intergenerational income mobility and attract more migrants. The coefficient actually increases in absolute terms once these variables are added to the model, providing additional evidence that the effect of the share of foreign-born on income mobility is not driven by booming metropolitan areas. The remaining columns of the table add the control variables that we have already used and discussed in the OLS regression in Column (5) of Table 4.2. None of these variables affect the coefficient on the share of foreign-born significantly.

Table 4.5
Robustness: Further Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share foreign-born ₁₉₈₀	-0.575*** (0.173)	-0.630*** (0.166)	-0.567*** (0.180)	-0.573*** (0.168)	-0.542*** (0.200)	-0.573*** (0.165)	-0.583*** (0.209)	-0.591*** (0.231)
<i>Further controls:</i>								
Average population growth _{1930–1980}		0.836* (0.491)						1.692*** (0.460)
Average wage growth _{1970–1980}		-0.284 (0.216)						-0.503*** (0.190)
Racial segregation ₂₀₀₀			0.011 (0.045)					-0.073 (0.047)
Income segregation ₂₀₀₀			0.253* (0.151)					0.412*** (0.135)
Log capital expenditure per worker ₂₀₀₇				-0.001 (0.005)				-0.003 (0.005)
Log avg. establishments _{1991–2005} × 10 ⁻²				0.821* (0.487)				0.748 (0.591)
Average export share _{2003–2011}					0.047 (0.061)			0.146** (0.063)
Migration outflow rate _{2004–2005}					-0.273 (0.944)			-0.318 (0.894)
Migration inflow rate _{2004–2005}					-1.167* (0.646)			-1.992*** (0.749)
Log bank deposits per capita ₂₀₀₉ × 10 ⁻²						0.563 (0.411)		0.617 (0.459)
Cost-of-living index ₂₀₁₁							0.002 (0.032)	-0.012 (0.029)
Control variables ₁₉₈₀	yes	yes	yes	yes	yes	yes	yes	yes
Occupational employment shares ₁₉₈₀	yes	yes	yes	yes	yes	yes	yes	yes
Share of foreign-born ₁₉₃₀	yes	yes	yes	yes	yes	yes	yes	yes
Region fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
R ²	0.683	0.680	0.684	0.679	0.699	0.684	0.678	0.722
Observations	117	117	117	117	117	117	117	117
Kleibergen-Paap F statistic	29.711	26.526	31.134	38.095	21.604	28.967	14.346	13.881
Partial R ²	0.345	0.327	0.348	0.361	0.283	0.345	0.232	0.228

Notes: Dependent variable is the *rank-rank slope*, i.e. the correlation between the parent (1996-2000) and child income (2010-11) ranks in the national income distribution. The intergenerational income data is from Chetty et al. (2014). Each specification is an IV estimation where the share of foreign-born in 1980 is instrumented by the predicted share of foreign-born in 1980 based on the distribution of migrants in 1930. *Control variables:* log median income, 75/25 income ratio, log population, population density, share unemployed, share college educated, share male, median age, share blacks, high/low-skilled migrant ratio, share out-of-state natives, and share low-income families. See Table A.4.2 for a description of variable construction and data sources. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 4.6 shows the results from different specification of the share of foreign-born. We do not change the first stage of the model. In Column (1), we provide the baseline result for comparison. In Column (2), we use the share of foreign-born without children, in Column (3), we use the share of low-skilled foreign-born and in Column (4), we use the share of high-skilled foreign-born.¹⁷ All shares are rescaled such that the standard deviation is the same as for the share of foreign-born in the baseline regression, thus making the size of the coefficients directly comparable.

Table 4.6
Robustness: Different Specifications

	(1)	(2)	(3)	(4)
Share foreign-born ₁₉₈₀	-0.575*** (0.173)			
Share foreign-born without children ₁₉₈₀		-0.585*** (0.175)		
Share low-skilled foreign-born ₁₉₈₀			-0.706*** (0.217)	
Share high-skilled foreign-born ₁₉₈₀				-0.568*** (0.189)
Control variables ₁₉₈₀	yes	yes	yes	yes
Occupational employment shares ₁₉₈₀	yes	yes	yes	yes
Share of foreign-born ₁₉₃₀	yes	yes	yes	yes
Region fixed effects	yes	yes	yes	yes
R^2	0.683	0.695	0.648	0.695
Observations	117	117	117	117
Kleibergen-Paap F statistic	29.711	29.222	16.615	27.283
Partial R^2	0.345	0.337	0.255	0.390

Notes: Dependent variable is the *rank-rank slope*, i.e. the correlation between the parent (1996-2000) and child income (2010-11) ranks in the national income distribution. The intergenerational income data is from Chetty et al. (2014). Each specification is an IV estimation where the share of foreign-born in 1980 is instrumented by the predicted share of foreign-born in 1980 based on the distribution of migrants in 1930. The standard deviation of the share of foreign-born for those without children, high- and low-skilled foreign-born are adjusted such that they all have the same standard deviation as the total share of foreign-born in 1980. *Control variables:* log median income, 75/25 income ratio, log population, population density, share unemployed, share college educated, share male, median age, share blacks, high/low-skilled migrant ratio, share out-of-state natives, and share low-income families. See Table A4.1 for a description of variable construction and data sources. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We test the share of foreign-born without children because the data by Chetty et al. (2014) mandate only that parents and children are U.S. citizens. Thus, it is quite possible that parents and children who were born outside the U.S. and later achieved U.S. citizenship are included in those data. To avoid the complication that children from second- (or first-) generation migrants show up in the share of foreign-born, we restrict the share of foreign-born to parents who do not report any children in the household. The coefficient is virtually the same as in the baseline model.¹⁸

In Columns (3) and (4) of Table 4.6 we use the share of low- and high-skilled foreign-born. The share of low-skilled foreign-born has a slightly stronger effect on intergenerational income mobility than the share of high-skilled foreign-born. This finding indicates that the effect is more driven

¹⁷ The share of high-skilled foreign-born is the share of foreign-born with at least a college degree in the total population in 1980. The share of low-skilled foreign-born is the share of foreign-born with less than a high school diploma in the total population in 1980.

¹⁸ We also used the share of foreign-born of those between 25 and 65 years. The results do not change.

by changes at the lower end of the income distribution and not so much by those at the upper end. However, the difference is not significant. A problem with this analysis is that we cannot distinguish high- and low-skilled immigration completely as both are strongly correlated with each other.

Migrants could be attracted to regions that are more open to migration. Because of their different attitudes, these regions might also be more successful in providing equal opportunities. Such a situation threatens our identification assumption, even though we control for persistent differences with the inclusion of the share of foreign-born in 1930, control for previous population and wage growth, and include variables for internal migration as proxies for the attractiveness of metropolitan areas. In a robustness check, we add proxies for regional personality profiles and attitudes toward migration to the baseline model (Appendix Table A4.5). Rentfrow, Gosling and Potter (2008) collected data on personality traits—the so-called *Big-5*—at the U.S.–state level and we use these measures to control for the general personality structure at the state level and average state values for metropolitan areas that are part of more than one state. Data on attitudes toward migration come from the American National Election Studies (ANES, 2014). This database provides opinion data for selected counties. We map counties to metropolitan areas and impute state averages for missing counties. The measure for a metropolitan area is then a county population weighted average. However, neither the personality variables nor the attitude variable changes the coefficient on the share of foreign-born in 1980.

4.3 Why Does Immigration Increase Income Mobility of U.S. Citizens?

Our main explanation for the effect of immigration on income mobility is that immigration changes the native occupational structure of the metropolitan area and that natives who were born in 1980 adjusted their occupational choice such that the foreign-born become complements rather than substitutes.

The argument is closely connected to the most recent literature on the effects of immigration on the structure of the labor market. Peri (2012) looks at U.S. states and shows that immigration has significantly increased productivity. He argues that this increase is mainly due to task specialization of low-skilled natives and migrants. Peri and Sparber (2009) specifically demonstrate that low-skilled migrants have a comparative advantage in occupations that need manual-physical labor skills compared to occupations that are communication-language intensive. These shifts in the relative

task supply of natives toward tasks that are better rewarded increase average wages, especially for low-skilled natives.¹⁹ Complementarities also arise via changes in the use of technology. Lewis (2011) shows that low-skilled immigration has led to a slower implementation of automation (complementary to middle-skilled labor) in the United States. Compared to low-immigration regions, where automation was introduced more quickly, low-skilled natives have not experienced such strong erosion in their relative wages. Dustmann and Glitz (2014) document for Germany that changes in the local labor supply are absorbed by changes in the production technology. Another example that low-skilled immigration can change a countries' industry structure is given by Braun and Kvasnicka (2014). They show that the large inflow of low-skilled expellees after World War II accelerated sectoral change from low- to high-productivity sectors in Germany.

Before discussing potential channels through which immigration affects intergenerational income mobility of U.S. citizens, we look at the effects of the share of foreign-born on income and education levels of different age cohorts. The results of this exercise shows that income gains from immigration are mainly generated at the top of the income distribution. However, income levels at the lower end of the income distribution are also increasing for those born in 1980, whereas older cohorts do not participate that much at the lower end of the distribution. This indicates that those born 1980 can adjust to the inflow of migrants better than those born later. We discuss and show that endogenous occupational and educational adjustments of those born 1980 are key factors in the relationship between immigration and intergenerational income mobility.

This exercise contributes to findings from above in three ways: first, by using Census data, we can condition the sample on those born in the United States (compared to having a citizenship only), second, we can show that immigration has real positive effects on average income levels (compared to show results for rank changes only), and, third, that these effects are stronger for younger cohorts (compared to having data for those born in 1980 only).

4.3.1 Personal Income and Immigration

Even though there is both high- and low-skilled immigration, low-skilled immigration is the prevalent flow into the United States. Card (2009), for example, shows that the average skill level is lower in cities with a higher share of immigrants, a situation mainly driven by the inflow of

¹⁹ Specialization also takes place among high-skilled. Peri and Sparber (2011*b*) demonstrate that high-skilled natives react to the inflow of high-skilled immigrants by switching to occupations that require more interactive and communication skills and that high-skilled immigrants specialize more in occupations that require quantitative and analytical skills.

low-skilled migrants and not by the existence of previous low-skilled migrants or the presence of low-skilled natives. Moreover, various papers show that the compositional effect is not driven by outflows of natives (Card, 2001; Peri and Sparber, 2011a), meaning that migrants do not push other migrants or natives out of the region, which could attenuate each labor market effect.

Since Card (1990), the literature on how immigration influences labor market outcomes has grown tremendously. A major motivation for this work is the concern that immigrants compete with natives on the labor market for the same jobs and thus depress natives' wages and/or increase natives' unemployment (Borjas, 2003; Borjas and Katz, 2007; Aydemir and Borjas, 2007; Glitz, 2012). Along these lines, Card (2009) shows that relative wages for low-skilled natives are depressed across metropolitan areas. However, Ottaviano and Peri (2012) stress the importance of looking at the total wage effect of immigration and not only at the competition effect. The total wage effect originates from the fact that migrants and natives are not perfect substitutes in production, even within the same education-experience group (Manacorda, Manning and Wadsworth, 2012; Ottaviano and Peri, 2012; Peri, 2011; Dustmann, Frattini and Preston, 2013). Migrants possess skills and work in occupations that are complementary to those skills and occupations of natives. The current literature suggests that the positive complementarity effect is able to compensate for the negative competition effect on natives. For example, Ottaviano and Peri (2006) show that the inflow of immigrants increases average wages. Card (2009) confirms that the average wage level is higher in high immigration areas. Despite showing that the relative wages for low-skilled natives are depressed, he finds that the absolute wage level of low-skilled natives is higher in high-immigration areas than the absolute wage level of low-skilled natives in low-immigration areas.²⁰

Panel A of Table 4.7 shows the effect of the share of foreign-born on personal income in 2010 of different age cohorts.²¹ We look at the working-age population (25–65 years), the cohort that is around 30 years (1980 cohort), and those between 45 to 65 years old. The 1980 cohort should approximately capture the same cohort that is analyzed with the data from Chetty et al. (2014). The older cohort contains, as a subgroup, the parents of the children born in 1980.²²

²⁰ Using a structural approach in which the degree of substitutability between natives and migrants is determined by the data, Ottaviano and Peri (2012) find that the total (real) wage change is positive for low-skilled natives in the United States. Specifically, they predict that the real wage change between 1990 and 2006 is around +1.6 percent of average wages for natives with less than high school, +0.7 percent of average wages for native high school graduates, around +1 percent of average wages for natives with some college, and between 0 and +0.6 percent for native college graduates. Thus, it seems that the average total wage effect is mainly driven by low-skilled natives.

²¹ Panel A of Appendix Table A4.3 contains summary statistics on the income variables.

²² Elsner (2013) confirms that migration, in his case emigration, can have different effects on wages of young versus old workers.

Table 4.7
Income, Education, and Immigration

	(1)	(2)	(3)	(4)
<i>Panel A: Nominal income of natives in 2010</i>				
Population	Median	P75	P25	P75-P25
Working-age population (25–65 years) in 2010	1.346*** (0.506)	2.172*** (0.491)	1.495* (0.874)	0.677 (0.731)
+Controlling for cost-of-housing index	–0.002 (0.846)	1.096 (0.785)	1.115 (1.536)	–0.019 (1.045)
Around 30 years (1980-cohort) in 2010	1.832*** (0.554)	2.204*** (0.483)	2.551** (1.198)	–0.346 (0.995)
+Controlling for cost-of-housing index	0.954 (0.864)	1.743** (0.685)	2.475 (2.140)	–0.732 (1.774)
Between 45–65 years in 2010	1.860*** (0.587)	2.579*** (0.504)	0.800 (0.691)	1.778*** (0.504)
+Controlling for cost-of-housing index	1.238 (0.901)	2.096*** (0.734)	0.119 (1.077)	1.978** (0.774)
<i>Panel B: Years of schooling of natives in 2010</i>				
Population	Mean	P75	P25	P75-P25
Working-age population (25–65 years) in 2010	2.109 (1.511)	–5.428 (4.510)	3.342 (2.189)	–8.770* (5.035)
Around 30 years (1980-cohort) in 2010	3.731** (1.774)	–8.510* (5.129)	7.043*** (2.600)	–15.553*** (5.755)
Between 45–65 years in 2010	2.715* (1.486)	–2.044 (5.535)	1.132 (0.820)	–3.176 (5.491)
Control variables ₁₉₈₀	yes	yes	yes	yes
Occupational employment shares ₁₉₈₀	yes	yes	yes	yes
Share of foreign-born ₁₉₃₀	yes	yes	yes	yes
Region fixed effects	yes	yes	yes	yes

Notes: Each cell shows the coefficient on the share of foreign-born from an IV regression where the dependent variable is one moment of either the native nominal personal income distribution (Panel A) or the native years of schooling distribution (Panel B) in 2010. The IV estimation instruments the share of foreign-born in 1980 with the predicted share of foreign-born in 1980 based on the distribution of migrants in 1930 and national growth rates of migrant shares between 1930 and 1980 by world region. We include a full set of controls in each regression: log median income, 75/25 income ratio, log population, population density, share unemployed, share college educated, share male, median age, share blacks, high/low-skilled migrant ratio, share out-of-state natives, and share low-income families. Moments are computed from Census data (Ruggles et al., 2010) for the different populations by age. For the working-age population (25–65 years), we use the 2010 American Community Survey (ACS). For the population around 30 years (1980-cohort), we pool data from the ACS from 2008 to 2011 and allow for an interval of ± 4 years around the age of 30 in 2010 in each survey. For the population between 45–65 years in 2010, we also pool data from the ACS from 2008 to 2011 and keep only individuals who are between 45 and 65 in 2010. In all settings, we only keep U.S.-born non-hispanic white and black U.S. citizens who are not in school and have not left their state of birth. We adjust personal income to 2011 prices by using the urban consumer price index from the Bureau of Labor Statistics. We assign years of schooling for degrees achieved according to Jaeger (1997). All moments of the income distribution are computed by using survey sampling weights. We dropped negative and missing incomes. In *P75-P25*, we either subtract the log personal income or years of schooling at the 25th percentile from the log personal income or years of schooling at the 75th percentile. See Appendix Table A4.3 for summary statistics. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The data in Panel A of Table 4.7 are from the 2010 American Community Survey (ACS) (Ruggles et al., 2010) and restrict the sample to the working-age population (25–65 years). For the other two samples (those around 30 years and those between 45–65 years), the sample is obtained by pooling data from the 2008–2011 ACS (Ruggles et al., 2010). For the 1980 cohort, we allow for an interval of ± 4 years around the 1980 birth year in each survey. For the cohort of 45–65 years old, we retain in each survey the people who would be 45–65 years in 2010. We adjust personal income to 2011 prices by using the urban consumer price index from the Bureau of Labor Statistics. In all settings, we only include U.S.-born non-Hispanic white and black U.S. citizens who are not in school and have not left their state of birth. We impose the race restriction because we do not know the citizenship status or birthplace of the father or the mother, which complicates the analysis because of second-generation migrants. Restricting the sample based on race means ignoring that there are second generation Hispanic migrants or migrants from Asia.²³ Ideally, we would like to assign people to their metropolitan area of birth. However, the ACS only provides the information about the state of birth. Although not ideal, this means that we can at least restrict the sample to people who still live in their state of birth.²⁴

Panel A of Table 4.7 provides two specifications for each population. The dependent variable in *median* is the log median personal income, the log personal income at the 75th percentile in *P75*, and the log personal income at the 25th percentile in *P25*. In *P75–P25*, we subtract the log personal income at the 75th percentile from the log personal income at the 25th percentile. All moments of the income distribution are computed by using survey sampling weights. We dropped negative and missing incomes. Each specification is an IV estimation where the share of foreign-born in 1980 is instrumented by the predicted share of foreign-born in 1980 based on the distribution of migrants in 1930 and the national growth rate of the share of foreign-born from 1930 to 1980 by world region. We include the full set of controls as in the baseline regression. In the first row of each population, we use nominal income as the dependent variable. In the second row of each population, we additionally control for a cost of housing index (U.S. Census Bureau, 2012).

The results of the income regressions in Table 4.7 confirm the literature on the income effects of immigration. Nominal median income is increasing in the share of foreign-born for the working-age population (Ottaviano and Peri, 2006). A one standard deviation increase in the share of foreign-born (0.04 percentage points) significantly increases median personal income by 5.38 percent. The 75th percentile increases more than the 25th percentile (even though the difference is not significant),

²³ There is no qualitative change in the results when we include all races.

²⁴ Chetty et al. (2014) show that the intergenerational income mobility measures are not very different between children who stayed in their region of birth and those who moved.

which leads to depressed relative incomes for low-skilled individuals (Card, 2009). However, the absolute income level for the low-skilled (25th percentile) increases in absolute terms (Card, 2009; Ottaviano and Peri, 2012). Controlling for the cost of housing index, we see no increase in median income for the working-age population, even though the coefficient on the share of foreign-born is positive (but not significant) for the 25th income percentile and the 75th income percentile; the effect is almost the same for both percentiles. Card (2009) summarizes the literature on the effect of immigration on housing prices by saying that the productivity and income gains from immigration are most likely to be spent on higher housing prices because of increased demand (Saiz, 2007).

For the personal income of the 1980 cohort, we find that the effect is much stronger on the income at the 25th percentile than it is for the total working-age population. This is true for nominal wages and when controlling for housing price levels. Compared to the effect for the older cohorts (45–65 years), which also includes the parents of the children studied here, we see that effect on income at the 75th percentile is almost the same. However, the effect on the income at the 25th percentile is much smaller for the older cohorts than for the 1980 cohort. These results imply that the competition effect of more (low-skilled) immigrants is stronger among the older cohort and that complementarities with low-skilled immigrants arise mainly in the 1980 cohort.

One explanation for this income effect of immigration might be that the 1980 cohort was able to adapt to the share of foreign-born in 1980 by adjusting their educational levels. For example, Hunt (2012) finds that the share of immigrants increases educational attainment among natives in the United States. She argues that natives recognize that an inflow of immigrants could cause disruptions in the labor market. For the current setting, this would mean that natives born in 1980 adapt to the labor market situation and try to accumulate more human capital, which enables them to switch to other, better paid, occupations and avoid competition with immigrants. For those who are already at the end of their school career (around end of high school at age 16) or older in 1980, it is not that easy to change their educational decisions based on the share of foreign-born in 1980.

To test this hypothesis, Panel B of Table 4.7 looks at the years of schooling obtained by different age cohorts.²⁵ For the working-age population, we find that mean years of schooling slightly increase. We also observe that the educational distribution is more compressed, that is, the 75th percentile is lower and the 25th percentile is higher, in metropolitan areas with more immigrants.²⁶

²⁵ Panel B of Appendix Table A4.3 contains summary statistics on the years of schooling variables.

²⁶ However, these results must be treated with caution because the percentiles of the years of schooling distribution do not vary much in the cross section.

In the 1980 cohort, we see that high-immigration metropolitan areas have an especially higher educational level at the 25th percentile in years of schooling distribution in 2010. The estimates imply that a one standard deviation increase in the share of foreign-born (4 pp) increases the 25th percentile by 0.28 years. No such effect is found for the older age group, thus confirming that immigration encourages natives to invest more in education at the lower end of the educational distribution. As before, the table also documents that the 75th percentile is lower in high-immigration areas, resulting in an more compressed educational distribution within the native population. One reason could be that educational attainment does not pay off so much in areas where low-skilled labor is used more heavily. The slower introduction of machinery, see for example Lewis (2011), also decreases the demand for higher skilled labor and thus decreases the incentives to invest in education.²⁷ An alternative explanation is motivated by recent studies from Amuedo-Dorantes and Almudena (2014) and Furtado and Hock (2010). Specifically, Amuedo-Dorantes and Almudena (2014) show that low-skilled immigration has led to a reduction in the time that native mothers with more than a high school degree spend on childcare. The authors argue that the reason for this finding is that low-skilled immigration decreases the price for childcare because low-skilled immigrants substantially depress wages in childcare employment.

4.3.2 Mechanism

In Table 4.8, we test whether immigration has an effect on total and native occupational-specific employment shares, as implied by the literature discussion from above (Peri and Sparber, 2009; Peri, 2012). The left column in Table 4.8 shows the results for total employment shares, that is, including immigrants. The right column restricts the sample to the native born population that we already used in the income and education analysis above, that is, U.S.-born non-hispanic white and black U.S. citizens who are between 25 and 65 years old, not in school, and have not left their state of birth. Each row in Table 4.8 presents the coefficient on the share of foreign-born from IV regressions that are set out in Equation (4.7) with the only difference that the dependent variable is now not the rank–rank slope but different occupational employment shares in 2010. Occupations are ordered according to the effect size that the share of immigrants has on the occupational employment share.²⁸ Because the regressions condition on the same occupational employment shares in 1980,

²⁷ Income gains documented in Panel A of Table 4.7 are then more a product of complementarities between high- and low-skilled labor and not so much a product of high demand for high-skilled labor.

²⁸ Appendix Table A4.4 shows how occupational employment shares have changed in general between 1980 and 2010.

the effect gives an indication about the change in the occupational employment share due to the share of foreign-born in 1980.

Results in Table 4.8 indicate that the share of foreign-born increases the total occupational employment shares (left column) for occupations such as Food Preparation and Serving Related, Building and Grounds Cleaning and Maintenance, and Construction and Extraction the most. All these occupational categories are arguably more prone to use low-skilled labor. The right column for native occupational employment shares suggests that immigration has shifted native employment shares away from occupations such as production or office and administrative support, which is the largest occupation in 1980 and 2010 (Appendix TableA4.4), toward occupations in management, food preparation and serving related, arts, design, entertainment, and education and training. Especially the move away from office and administrative support to management occupations suggests some sort of upskilling of natives such that natives who would have become hotel and billing clerks, production coordinators, or stock and inventory clerks, now become purchasing managers, construction inspectors, or postmasters. This is compatible with Hunt (2012) who argues that immigration can push natives to higher skill levels in general. Table 4.8 also reveals that natives are driven out of production occupations where we can find more manual-intensive occupations in general. This is compatible with Peri and Sparber (2009) who find shifts in native occupational structure away from manual-intensive tasks.

The analysis above demonstrates that immigration has shifted the occupational structure of natives between 1980 and 2010. The question is now whether this shift can explain the effect of immigration on intergenerational income mobility. To give an answer to this question, we now include occupational employment shares in 2010 in the main model and show how the coefficient on the share of foreign-born changes. If the share of foreign-born in 1980 has changed educational levels (as indicated above) and the occupational structure in 2010 and both are main drivers of intergenerational income mobility, then we should observe that the coefficient on the share of foreign-born decreases in absolute size. Table 4.9 shows the empirical test of our hypothesis.

In Column (1) of Table 4.9, we replicate our baseline model for comparison. In Columns (2) to (4), we test the impact of the educational choice of the 1980 cohort on the effect of immigration on income mobility. From Table 4.7, Panel B, we know that immigration has a negative effect on the difference in the 75th to 25th percentiles of the years of schooling distribution of the 1980-cohort. Therefore, we add this variable to the baseline model in Column (2). The coefficient on the P75 to P25 difference in years of schooling is positive and significant. This indicates that a higher

Table 4.8
Immigration and Occupational Employment Shares

<i>Dependent variable: Employment share in the specific occupation₂₀₁₀</i>			
Total employment shares ₂₀₁₀		Native employment shares ₂₀₁₀	
Food Preparation and Serving Related	0.230*** (0.068)	Management	0.263*** (0.097)
Building, Grounds Cleaning,...	0.211*** (0.062)	Food Preparation and Serving Related	0.144** (0.069)
Construction and Extraction	0.149** (0.06)	Arts, Design, Entertainment,...	0.135*** (0.048)
Computer and Mathematical	0.095 (0.059)	Education, Training, and Library	0.111** (0.053)
Architecture and Engineering	0.076** (0.037)	Business and Financial Operations	0.082 (0.074)
Farming, Fishing, and Forestry	0.054 (0.037)	Life, Physical, and Social Science	0.066** (0.029)
Life, Physical, and Social Science	0.043 (0.029)	Legal Occupations	0.061*** (0.022)
Arts, Design, Entertainment,...	0.028 (0.036)	Computer and Mathematical	0.049 (0.059)
Management	0.02 (0.074)	Architecture and Engineering	0.025 (0.034)
Personal Care and Service	0.011 (0.044)	Construction and Extraction	0.024 (0.062)
Legal Occupations	0.009 (0.018)	Building, Grounds Cleaning,...	0.017 (0.048)
Business and Financial Operations	0.003 (0.062)	Farming, Fishing, and Forestry	0.009 (0.009)
Education, Training, and Library	-0.070 (0.063)	Installation, Maintenance, and Repair	-0.026 (0.043)
Sales and Related	-0.089 (0.065)	Personal Care and Service	-0.032 (0.044)
Installation, Maintenance, and Repair	-0.010 (0.039)	Community and Social Service	-0.036 (0.028)
Healthcare Support	-0.020 (0.036)	Protective Service	-0.041 (0.037)
Transportation and Material Moving	-0.049 (0.052)	Sales and Related	-0.056 (0.073)
Community and Social Service	-0.058** (0.027)	Healthcare Support	-0.061 (0.044)
Protective Service	-0.080** (0.033)	Transportation and Material Moving	-0.080 (0.06)
Production	-0.107 (0.115)	Healthcare Practitioners and Technical	-0.098* (0.051)
Healthcare Practitioners and Technical	-0.124** (0.06)	Production	-0.221* (0.119)
Office and Administrative Support	-0.321*** (0.084)	Office and Administrative Support	-0.335*** (0.098)
Control variables ₁₉₈₀	yes	Control variables ₁₉₈₀	yes
Occupational employment shares ₁₉₈₀	yes	Occupational employment shares ₁₉₈₀	yes
Share of foreign-born ₁₉₃₀	yes	Share of foreign-born ₁₉₃₀	yes
Region fixed effects	yes	Region fixed effects	yes

Notes: Dependent variable is the employment share in the specific occupation in 2010 (Ruggles et al., 2010). For each occupation, we regress the employment share on the share of foreign-born in 1980, including the full set of control variables and fixed effects. Each specification is an IV estimation where the share of foreign-born is instrumented by the predicted share of foreign-born based on the distribution of migrants in 1930. *Control variables:* log median income, 75/25 income ratio, log population, population density, share unemployed, share college education, share male, median age, share blacks, high/low-skilled migrant ratio, share out-of-state natives, and share low income families. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

difference is associated with less income mobility. The coefficient on the share of foreign-born drops by 15 percent to -0.491 .

Natives can adjust their educational trajectory in several ways when they are facing more immigrants. For example, Betts and Fairlie (2003) show that for every four new immigrants in a public high school, one native student switches to a private school. Hence, because it is not clear how natives in different metropolitan areas react to the inflow of immigrants, we estimate in Column (3) of Table 4.9 a more flexible model by adding squared and cubic terms of the P75 to P25 difference in years of schooling. All interactions are significant and can explain 43 percent of the baseline effect. Adding further average test scores between 2004 and 2007 or high school dropout rates in 2000 does not add much explanatory power.²⁹ The coefficient on the share of foreign-born remains unaffected when we include the P75-P25 years of schooling difference of the 45 to 65 years old (not shown).³⁰

However, endogenous educational choice is only one part of natives' response to immigration. The literature suggests that natives also choose other occupations in response to immigration (see discussion above). Table 4.8 has revealed that, on the one side, the three occupations with the largest increase in the employment share due to immigration are management, food preparation and serving related, and arts, design entertainment, sports, and media. On the other side, the three occupations with the largest decrease in the employment share due to immigration are office and administrative support, production, and healthcare practitioners and technical. We test whether the immigration-induced change in occupational structure can explain the effect of immigration on intergenerational income mobility by adding these six occupational employment shares from 2010 to the baseline model in Column (5) of Table 4.9. By doing so, the coefficient on the share of foreign-born drops by 60 percent from -0.575 (significant at 1 percent) in the baseline model to -0.228 (insignificant) in the extended model.³¹ In Column (6) of Table 4.9, we add all other native occupational employment shares in 2010 to the model, which reduces the coefficient on the share of foreign-born even further to almost zero. Column (7) shows a very small positive and insignificant

²⁹ Test scores are family income adjusted test scores percentile ranks of grade 3-8 for math and reading, averaged over the years 2004, 2005, and 2007. The data comes the Global Report Card and is taken from Chetty et al. (2014). High school dropout rates are obtained for the 2000-2001 school year. The dropout rates are also adjusted by family income and are taken from Chetty et al. (2014).

³⁰ Results are available from the author. We can also change the sample to 50 to 65 years or to 55 to 65 years. The coefficient on the share of foreign-born is not reduced in both cases.

³¹ Including only the occupations with the largest increase in the employment share due to immigration, management, and with the largest decrease in the employment share due to immigration, office and administrative support, already decreases the coefficient on the share of foreign-born down to -0.284 (insignificant).

coefficient on the share of foreign-born when we additionally include the educational variables from Column (4).

The main driving force seems to be that employment in office and administrative support is heavily reduced by immigration because this occupation is also associated with higher rank-rank correlations, that is, lower intergenerational income mobility. The same is true for production. However, for production, the mechanism is not as pronounced as for office and administrative support. On the other side, the occupations into which natives flow do not seem to be associated with reduced intergenerational income mobility as much. Thus, we conclude from this analysis that immigration leads to a shift in occupational structure and that natives are pushed out of occupations that are associated with lower income mobility into occupations that promise (relatively) higher income mobility.

Table 4.9
Occupational and Educational Adjustments as Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share foreign-born ₁₉₈₀	-0.575*** (0.173)	-0.491*** (0.173)	-0.326* (0.192)	-0.321 (0.205)	-0.228 (0.230)	-0.109 (0.293)	0.074 (0.350)
<i>Mechanisms:</i>							
P75-P25 years of schooling difference _{1980 cohort} ₂₀₁₀		0.005** (0.003)	0.334*** (0.094)	0.337*** (0.094)			0.303*** (0.103)
P75-P25 years of schooling difference _{1980 cohort} ₂₀₁₀ , squared			-0.148*** (0.044)	-0.149*** (0.043)			-0.121*** (0.046)
P75-P25 years of schooling difference _{1980 cohort} ₂₀₁₀ , cubed			0.020*** (0.006)	0.020*** (0.006)			0.015** (0.006)
Test scores ₂₀₀₄₋₂₀₀₇ × 10 ⁻²				-0.001 (0.049)			0.017 (0.059)
High school dropout rate ₂₀₀₀				-0.184 (0.221)			0.060 (0.241)
<i>Occupational employment shares in...</i>							
Management ₂₀₁₀					-0.123 (0.258)	0.621 (0.547)	1.224** (0.562)
Food Preparation and Serving Related ₂₀₁₀					0.311 (0.301)	0.891 (0.551)	1.799*** (0.587)
Arts, Design, Entertainment, Sports, and Media ₂₀₁₀					-0.170 (0.463)	-0.095 (0.656)	0.460 (0.684)
Office and Administrative Support ₂₀₁₀					0.740*** (0.204)	1.621*** (0.502)	2.071*** (0.529)
Production ₂₀₁₀					0.217 (0.201)	0.871* (0.495)	1.453*** (0.514)
Healthcare Practitioners and Technical ₂₀₁₀					0.426 (0.290)	0.730 (0.474)	1.470*** (0.515)
All native occupational employment shares ₂₀₁₀	-	-	-	-	-	yes	yes
Control variables ₁₉₈₀	yes	yes	yes	yes	yes	yes	yes
Occupational employment shares ₁₉₈₀	yes	yes	yes	yes	yes	yes	yes
Share of foreign-born ₁₉₃₀	yes	yes	yes	yes	yes	yes	yes
Region fixed effects	yes	yes	yes	yes	yes	yes	yes
R ²	0.683	0.690	0.717	0.711	0.709	0.726	0.722
Observations	117	117	117	117	117	117	117
Kleibergen-Paap F statistic	29.711	26.835	23.834	18.814	16.374	11.749	8.327
Partial R ²	0.345	0.322	0.295	0.279	0.251	0.246	0.214

Notes: Dependent variable is the *rank-rank slope*, i.e. the correlation between the parent (1996-2000) and child income (2010-11) ranks in the national income distribution. The intergenerational income data is from Chetty et al. (2014). Test scores are family income adjusted test scores percentile ranks of grade 3-8 for math and reading, averaged over the years 2004, 2005, and 2007. The data comes the Global Report Card and is taken from Chetty et al. (2014). High school dropout rates are obtained for the 2000-01 school year. The dropout rates are also adjusted by family income and are taken from Chetty et al. (2014). The *occupational employment share* categories (22) in 2010 follow Standard Occupational Classification (SOC) system and match those in 1980. Each specification is an IV estimation where the share of foreign-born in 1980 is instrumented by the predicted share of foreign-born in 1980 based on the distribution of migrants in 1930. *Control variables*: log median income, 75/25 income ratio, log population, population density, share unemployed, share college educated, share male, median age, share blacks, high/low-skilled migrant ratio, share out-of-state natives, and share low-income families. The omitted category in Column (6) and (7) is Architecture and Engineering. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

4.3.3 Other Mechanisms

The channels of endogenous educational and occupational choices seem to explain the mechanism very well. Nevertheless, we have looked at alternative explanations (results can be found in Appendix Table A4.6). Looking at Latin America, Behrman, Gaviria and Szekely (2001) find evidence that public expenditures on education increases intergenerational income mobility. This could be because public expenditure on education decreases the importance of family resources for the educational attainment of children from low-income families. Thus, we look at school inputs in 1990, when the child is 10 years old, such as school expenditures per pupil, teacher salaries, and student-teacher ratios. It could also be that immigration in 1980 is already beneficial for parental income in 1990, which could alleviate credit constraints and permit more private educational spending. Thus, we look at family income and 75/25 family income ratios. After including these variables in the main model, the coefficient on the share of foreign-born is reduced only slightly (in absolute size), indicating only a minor role for these channels.³²

Audretsch, Dohse and Niebuhr (2009) find that entrepreneurial activity is affected by immigration. Thus, we test whether net new business formation (newly founded businesses minus the death of businesses divided by the number of establishments) can work as a channel. We find no evidence that entrepreneurial activity explains the effect of immigration on income mobility. There is a growing literature that shows that high-skilled immigration affects productivity and innovative activity (Hunt and Gauthier-Loiselle, 2010; Kerr and Lincoln, 2010; Moser, Voena and Waldinger, 2014; Hornung, 2014).³³ We test this channel by including the number of patents per employee in 2010 in our model; the coefficient on the share of foreign-born is not affected. This is in line with Peri, Shih and Sparber (2014) who shows that the inflow of high-skilled immigrants raises wages of other high-skilled natives but does not affect wages or employment of low-skilled natives. Lastly, there is the (public) fear that immigrants increase crime. Therefore, we include crime rates

³² In additional analysis, we look at the impact of immigration on family income in 1990 and school resources in 1990, that is, when the child is ten years old. Family income is constructed from the 1990 Census where we can match parents to families and restrict the analysis to those U.S.-born children and parents. School resources come mainly from the Common Core of Data and comprise school revenues per pupil from different funding sources, school expenditure per pupil on different spending categories, pupil-teacher ratio, and teacher wages. Appendix Tables A4.7 and A4.8 show that the share of foreign-born does not significantly affect family income or school resources, thus explaining why we find no role for these variables in the other regressions. In regard to the financing of education, Krieger and Ruhose (2013) discuss whether there is an intergenerational conflict between old and young. Using a panel of OECD countries, we do not find that this is the case in general. However, we demonstrate that those who are close to statutory retirement age disfavor public expenditure on families and education.

³³ We also find that immigration in 1980 affects positively patenting activity at the metropolitan area level in 2010. Results are available upon request.

for different crime categories in the model and find that doing so does not reduce the coefficient of interest.³⁴

4.4 Conclusion

This chapter sheds light on the role that the foreign-born immigrant population plays in affecting intergenerational income mobility of U.S. citizens with children born in 1980. I use the novel dataset of Chetty et al. (2014), which provides regional measures of intergenerational income mobility within U.S.–citizen families. I complement the data with survey data from the Census to show that the share of foreign-born migrants has a positive effect on intergenerational income mobility among natives. To identify a causal effect, I rely on the shift-share methodology and use the distribution of immigrants by birthplace in 1930 together with national inflow rates from 1930 to 1980 by world region as the basis for constructing predicted shares of foreign-born. I find that a one standard deviation increase (almost 4 percentage points) in the share of foreign-born migrants in 1980 decreases the correlation between child and parent income by 0.023 child income ranks for each additional parent income rank. Evaluated at the average metropolitan area, this implies a reduction in the dependency of the child's income on the parent income by 6.4 percent. Using Census data, I show that this result implies real income effects for people at the bottom of the income distribution.

As the main mechanism for this result, I argue that it is especially the low-skilled immigration that occurred after the Immigration and Nationality Act of 1965 that led to a shift in the occupational structure of natives. Indeed, our empirical analysis can show that the change in the occupational structure of natives between 1980 and 2010 can explain the positive result of immigration on intergenerational income mobility of U.S. citizens. In line with current literature, I argue that children born in 1980 adjusted to the inflow by choosing educational levels and occupations such that they become complements to immigrants rather than substitutes for them. Increasing productivity and income for children in high-immigration areas are the results. Older workers, including the parents of children born in 1980, did not have the same opportunities to switch occupations and education based on the share of foreign-born immigrants in 1980. Therefore, this older cohort has to compete more fiercely with migrants on the labor market, which leads to a lesser (insignificant) increase in their income levels.

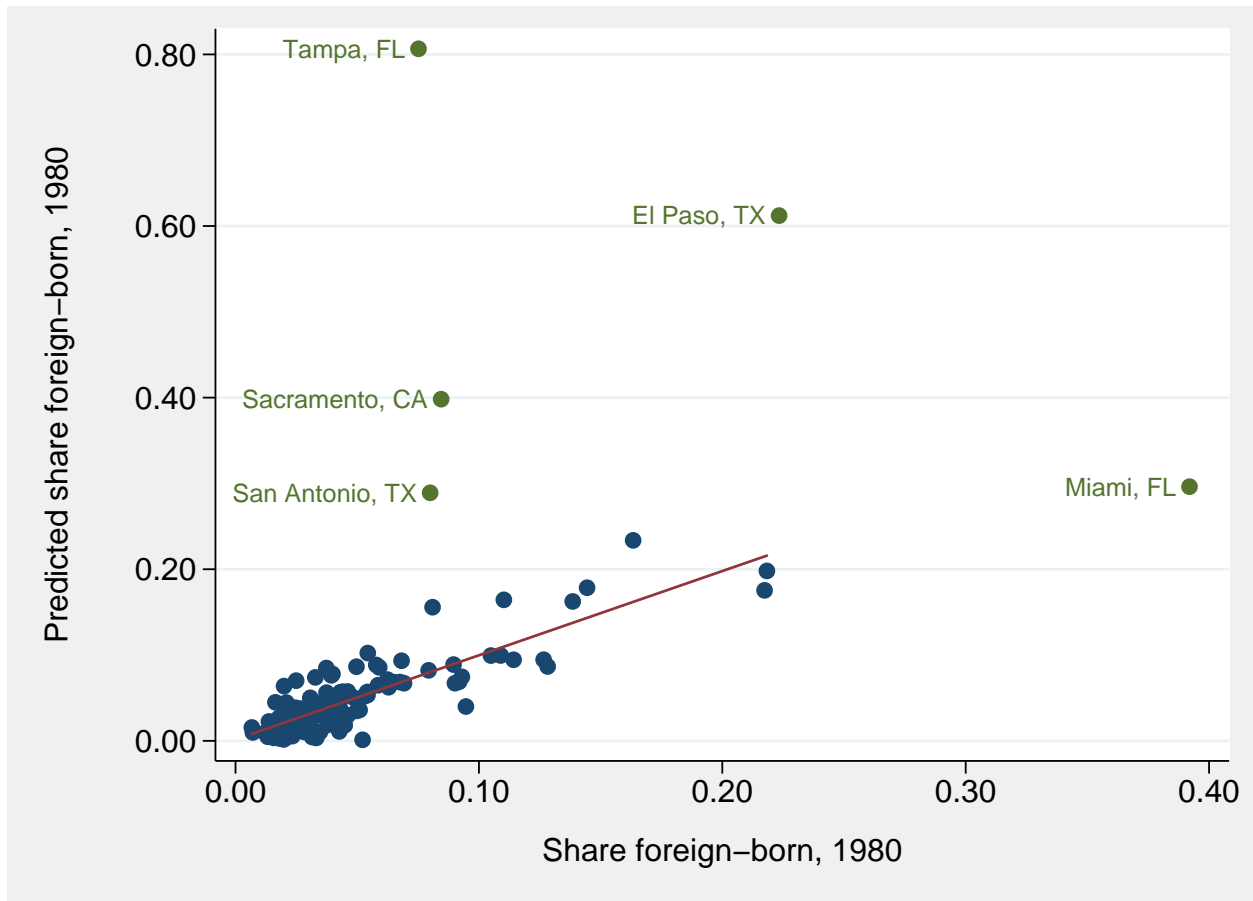
³⁴ Broadly in line with the literature on the effects of immigration on crime, we find no strong relationship between immigration in 1980 and crime in 2010. Results are available upon request.

I also look at other mechanisms that could affect intergenerational income mobility. However, potential channels related to school inputs (public expenditures per pupil, teacher salary, or pupil-teacher ratio) and family resources (measured as family income when the child is 10 years old), both of which could alleviate credit constraints for low-income families, only explain a smaller fraction of the effect of the share of foreign-born on intergenerational income mobility. Other channels, such as entrepreneurial activity, patenting, or crime, cannot explain the relationship at all.

From a policy perspective, it is unclear whether there are any welfare effects for individuals because children and families choose education and occupations endogenously. We can think of a maximization problem in which children receive benefits from education and their occupational choice. However, investing in education or switching occupations is costly. Children and their families compare the costs and benefits to invest in education and to choose specific occupations. Low-skilled immigration reduce (expected) wages for natives in occupations that receive larger fractions of immigrants. Thus, whereas children in low-immigration areas still find it worthwhile to enter low-education occupations, children in high-immigration areas find it worthwhile to invest more in education, so as to avoid competition in occupations mainly occupied by immigrants. Therefore, the individual utility level of children in high- versus low-immigration areas must not be different.

Appendix

Figure A4.1
Predicted and Observed Share of Foreign-Born in 1980, including Outliers



Notes: The figure plots the first stage relationship between the predicted share of foreign-born in 1980 and the observed share of foreign-born in 1980 including outlier metropolitan areas. The metropolitan areas marked in blue represent the IV baseline sample. The metropolitan areas marked in green are excluded from the analysis. Source: own computations based on Ruggles et al. (2010). The figure reveals that there are five metropolitan areas for which the prediction procedure does not work well. El Paso had the largest Mexican enclave in 1930 with a fraction of 31.1 percent. San Antonio has the second largest fraction of Mexicans in 1930 at 13.7 percent. Following the massive inflow of Mexicans, we would predict that both cities have much larger immigrant shares. However, official statistics do not confirm this. Another example is Miami for which the prediction does not produce sensible values either. In 1930, almost no Cubans were living in Miami (0.2 percent of the Miami population). In 1980, 21 percent of the population in Miami was born in Cuba. The large inflow of Cubans was simply not counted as belonging to Miami, but instead as belonging to Tampa, which hosted the largest Cuban enclave in 1930 with 4 percent of its population comprised of Cubans. However, in 1980, most of the Cubans disappeared from Tampa and they made up only a minor fraction of the city's population (0.8 percent). According to our rule of dropping all metropolitan areas with a predicted migrant share of above 22.3 percent, we would also drop San Francisco, which has 23.4 percent. But because it only slightly violates the rule, we decided to keep San Francisco in the sample. Dropping it does not change the results. If we run an IV regression on the full sample, that is, including all outliers, we find that the coefficient on the share of foreign-born is larger in absolute terms, however, the first stage regression is not strong with a F statistic well below conventional significance levels.

Table A4.1
Data Sources of Baseline Analysis

Variable	Description	Source
<i>Panel A: Income mobility</i>		
Rank-rank slope	Rank-rank slope (relative mobility) is the slope from an OLS regression of child income rank on parent income rank within each MSA.	Chetty et al. (2014)
Expected child rank	$E[\text{child rank} \text{parent} = X]$ (absolute mobility) is the expected income rank of children whose parents are at the $X = \{25^{\text{th}}, 75^{\text{th}}\}$ percentile of the national income distribution.	Chetty et al. (2014)
<i>Panel B: Share of foreign-born</i>		
Share foreign-born _t	Share of foreign-born based on total population in $t = \{1930, 1980\}$.	IPUMS
Share foreign-born without children ₁₉₈₀	Share of foreign-born without children in the total population in 1980.	IPUMS
Share low-skilled foreign-born ₁₉₈₀	Share of foreign-born with less than a high school diploma in the total population in 1980.	IPUMS
Share high-skilled foreign-born ₁₉₈₀	Share of foreign-born with at least a college degree in the total population in 1980.	IPUMS
Share foreign-born ₁₉₈₀ , predicted	Predicted share of foreign-born based on total population in 1980. The prediction uses the distribution of the shares of foreign-born by birthplace region in 1930. National growth rates between 1930 and 1980 for each birthplace region predict the shares of foreign-born by birthplace region in 1980. The predicted share of foreign-born is then the sum of each predicted birthplace share.	IPUMS
<i>Panel C: Controls</i>		
Median income ₁₉₈₀	Median personal income of working age population (20-65 years) in 1980. Negative or missing incomes are dropped.	IPUMS
75/25 income ratio ₁₉₈₀	75 percentile to 25 percentile income ratio of working age population (20-65 years) in 1980. Negative or missing incomes are dropped.	IPUMS
Population ₁₉₈₀	Population in 1980.	IPUMS
Population density ₁₉₈₀	Population in 1980 divided by the land area in square miles from 2000.	IPUMS, U.S. Census Bureau (2009)
Share unemployed ₁₉₈₀	Share unemployed is the number of unemployed as a percent of the civilian labor force (employed + unemployed) in the working age population (20-65 years) in 1980.	IPUMS
Share college educated ₁₉₈₀	Share college education is the number of persons with at least some college as a percent of the working age population (20-65 years) in 1980.	IPUMS
Share male ₁₉₈₀	Share male is the number of males as a percent of the total population in 1980.	IPUMS
Median age ₁₉₈₀	Median age of the total population in 1980.	IPUMS
Share black ₁₉₈₀	Share of non-hispanic Blacks as a percent of the total population in 1980.	IPUMS
High/low-skilled migrant ratio ₁₉₈₀	High/low-skilled migrant ratio is the number of foreign-born migrants with a college degree or more (high-skilled) over the number of foreign-born migrants without a high school diploma (low-skilled)	IPUMS
Share out-of-state natives ₁₉₈₀	The share out-of-state natives is the fraction of the native population for whom the current state of living is not equal to the state of birth.	IPUMS
Share low income families ₁₉₈₀	The share of low income families is the fraction of persons with a family income which is below the 25th percentile of the national family income distribution.	IPUMS
Occupational employment shares ₁₉₈₀	The shares measure the fraction of the total working age population (25–65 years) working in one of the occupations in 1980. Occupations are classified according to the Standard Occupational Classification (SOC) system. See Appendix Table A4.4.	IPUMS

Notes: Integrated Public Use Microdata Series (IPUMS) data is provided by Minnesota Population Center, University of Minnesota (Ruggles et al., 2010). Constructing metropolitan statistics, we always use person sampling weights. The commuting zone data from Chetty et al. (2014) is assigned to metropolitan areas by using crosswalks provided by David Dorn (<http://www.cemfi.es/~dorn/data.htm>). This is only necessary for covariates. The data on intergenerational income mobility is provided at the metropolitan area level by Chetty et al. (2014) directly.

Table A4.2
Data Sources of Further Controls

Variable	Description	Source
Racial segregation ₂₀₀₀	Racial segregation is measured for Whites, Blacks, Hispanics, and others by using a Theil index.	Chetty et al. (2014)
Income segregation ₂₀₀₀	Using a two-group Theil index, this measure shows the degree to which individuals below the p^{th} percentile of the local household income distribution are segregated from individuals above the p^{th} percentile in each commuting zone.	Chetty et al. (2014)
Capital exp. per worker ₂₀₀₇	Total capital expenditures (new and used) in 2007 divided by total number of employees in 2007. Measure is missing for 16(6) metropolitan areas in the OLS(IV) sample. We impute state averages.	U.S. Census Bureau (2007)
Avg. number of establishments _{1991–2005}	Average number of establishments. Averages are computed from the years 1991 to 2005.	U.S. Small Business Administration, Office of Advocacy (2005)
Average export share _{2003–2011}	The export share is exports over GDP. The average is taken from 2003 to 2011. Measure is missing for 2(1) metropolitan areas in the OLS(IV) sample. We impute state averages.	Brookings (2012)
Migration outflow rate _{2004–2005}	Outflow migration is the number of people moving out of a commuting zone to other commuting zones divided by the total commuting zone population.	Chetty et al. (2014)
Migration inflow rate _{2004–2005}	Inflow migration is the number of people moving into a commuting zone from counties in other commuting zones divided by the total commuting zone population.	Chetty et al. (2014)
Bank deposits per capita ₂₀₀₉	Banking deposits divided by total population in 2009.	IPUMS, U.S. Census Bureau (2009)
Cost-of-living index ₂₀₁₁	The cost of living index measures relative price levels for consumer goods and services. The average equals 100, and each index is read as a percentage of the average for all places. The Index reflects cost differentials for the standard of living present in professional and/or managerial households. Measure is missing for 64(25) metropolitan areas in the OLS(IV) sample. We impute state averages.	U.S. Census Bureau (2012)
Cost-of-housing index ₂₀₁₁	The cost of housing index is one part of the cost of living index (29 percent) and measures relative price levels for housing. Measure is missing for 64(25) metropolitan areas in the OLS(IV) sample. We impute state averages.	U.S. Census Bureau (2012)
Average population growth _{1930–1980}	Average annual population growth rate, $g = [\ln(X_t) - \ln(X_{t-j})]/j$.	IPUMS
Average wage growth _{1970–1980}	Average annual growth rate in wages per employee, $g = [\ln(X_t) - \ln(X_{t-j})]/j$.	Bureau of Economic Analysis (2012)

Notes: Integrated Public Use Microdata Series (IPUMS) data is provided by Minnesota Population Center, University of Minnesota (Ruggles et al., 2010). Constructing metropolitan statistics, we always use person sampling weights provided. The commuting zone data from Chetty et al. (2014) is assigned to metropolitan areas by using crosswalks provided by David Dorn (<http://www.cemfi.es/~dorn/data.htm>).

Table A4.3
Further Summary Statistics

Variable	Mean	SD	Min	Max	Obs
<i>Panel A: Income</i>					
<i>Panel A1: Income of natives in working age population (25-65 years) in 2010</i>					
Median income ₂₀₁₀	30,198	5,406	15,200	50,000	117
75th percentile income ₂₀₁₀	51,528	8,332	29,360	89,000	117
25th percentile income ₂₀₁₀	13,044	3,211	5,000	22,000	117
75th - 25th log income difference ₂₀₁₀	1.39	0.18	0.86	1.86	117
<i>Panel A2: Income of natives who are around 30 years (1980-cohort) in 2010</i>					
Median income ₂₀₁₀	27,632	4,744	11,200	39,318	117
75th percentile income ₂₀₁₀	43,838	6,214	28,208	64,775	117
25th percentile income ₂₀₁₀	12,041	3,648	4,126	23,726	117
75th - 25th log income difference ₂₀₁₀	1.33	0.23	0.71	1.92	117
<i>Panel A3: Income of natives who are between 45-65 years in 2010</i>					
Median income ₂₀₁₀	33,246	5,981	20,631	52,424	117
75th percentile income ₂₀₁₀	57,429	9,446	41,263	99,000	117
25th percentile income ₂₀₁₀	14,379	3,179	7,968	24,029	117
75th - 25th log income difference ₂₀₁₀	1.40	0.16	0.99	1.75	117
<i>Panel B: Years of schooling</i>					
<i>Panel B1: Years of schooling of natives in working age population (25-65 years) in 2010</i>					
Mean years of schooling ₂₀₁₀	13.30	0.40	12.50	14.35	117
75th percentile years of schooling ₂₀₁₀	15.10	1.07	13.00	16.04	117
25th percentile years of schooling ₂₀₁₀	12.01	0.16	11.00	13.00	117
75th - 25th years of schooling difference ₂₀₁₀	3.09	1.05	1.00	4.04	117
<i>Panel B2: Years of schooling of natives who are around 30 years (1980-cohort) in 2010</i>					
Mean years of schooling ₂₀₁₀	13.37	0.43	12.29	14.45	117
75th percentile years of schooling ₂₀₁₀	15.47	0.98	13.00	16.04	117
25th percentile years of schooling ₂₀₁₀	12.03	0.26	11.00	13.00	117
75th - 25th years of schooling difference ₂₀₁₀	3.43	0.95	1.00	4.04	117
<i>Panel B3: Years of schooling of natives who are between 45-65 years in 2010</i>					
Mean years of schooling ₂₀₁₀	13.22	0.40	12.38	14.32	117
75th percentile years of schooling ₂₀₁₀	14.85	1.10	13.00	16.04	117
25th percentile years of schooling ₂₀₁₀	12.01	0.09	12.00	13.00	117
75th - 25th years of schooling difference ₂₀₁₀	2.84	1.09	1.00	4.04	117

Notes: Averages and different moments are computed from Census data (Ruggles et al., 2010). For the sample in Panels A1 and B1, we use the 2010 American Community Survey (ACS). For the sample in Panels A2 and B2, we pool data from the ACS from 2008 to 2011 and allow for an interval of ± 4 years around the age of 30 in 2010 in each survey. For the sample in Panels A3 and B3, we also pool data from the ACS from 2008 to 2011 and keep only individuals who are between 45 and 65 in 2010. In all settings, we only keep U.S.-born non-hispanic white and black U.S. citizens who are not in school and have not left their state of birth. We adjust personal income to 2011 prices by using the urban consumer price index from the Bureau of Labor Statistics. We assign years of schooling for degrees achieved according to Jaeger (1997). All moments of the income distribution are computed by using survey sampling weights. We dropped negative and missing incomes. In *P75-P25*, we either subtract the log personal income or years of schooling at the 75th percentile from the log personal income or years of schooling at the 25th percentile.

Table A4.4
Occupational Employment Shares

Occupation	Total employment shares			Native employment shares		
	1980	2010	Growth	1980	2010	Growth
Office and Administrative Support	0.177	0.143	-19%	0.180	0.150	-17%
Production	0.153	0.074	-52%	0.147	0.065	-56%
Sales and Related	0.096	0.100	4%	0.098	0.104	6%
Transportation and Material Moving	0.093	0.061	-34%	0.093	0.060	-36%
Management	0.081	0.096	18%	0.083	0.103	24%
Education, Training, and Library	0.054	0.065	20%	0.055	0.070	26%
Construction and Extraction	0.044	0.055	25%	0.044	0.052	17%
Healthcare Practitioners and Technical	0.039	0.059	49%	0.039	0.059	52%
Installation, Maintenance, and Repair	0.038	0.034	-10%	0.038	0.035	-9%
Food Preparation and Serving Related	0.035	0.044	27%	0.033	0.038	15%
Building and Grounds Cleaning and Maintenance	0.034	0.040	17%	0.033	0.033	1%
Business and Financial Operations	0.028	0.050	79%	0.028	0.053	88%
Architecture and Engineering	0.027	0.020	-27%	0.026	0.019	-26%
Healthcare Support	0.018	0.025	41%	0.018	0.024	36%
Personal Care and Service	0.017	0.032	87%	0.017	0.030	77%
Protective Service	0.015	0.019	30%	0.015	0.021	37%
Farming, Fishing, and Forestry	0.014	0.004	-73%	0.014	0.002	-82%
Arts, Design, Entertainment, Sports, and Media	0.012	0.017	41%	0.012	0.018	49%
Community and Social Service	0.009	0.018	95%	0.009	0.019	103%
Life, Physical, and Social Science	0.007	0.008	14%	0.007	0.008	10%
Legal Occupations	0.005	0.011	107%	0.006	0.012	121%
Computer and Mathematical	0.004	0.025	612%	0.004	0.024	555%

Notes: The table shows occupational employment shares based on the Standard Occupational Classification (SOC) system, which are obtained from the IPUMS (Ruggles et al., 2010). *Total employment shares* give the share of employment in the specific occupation compared to the total working age population (25–65 years), i.e. including foreign-born. *Native employment shares* show the share of employment within the native population, that is, white and black working age population that is born in the United States. *Growth* is the percentage change in the employment share from 1980 to 2010.

Table A4.5
Robustness: Testing Personality Traits and Attitudes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share foreign-born ₁₉₈₀	-0.606*** (0.175)	-0.596*** (0.168)	-0.603*** (0.169)	-0.741*** (0.164)	-0.655*** (0.186)	-0.624*** (0.160)	-0.569*** (0.172)	-0.773*** (0.179)
<i>Personality Traits:</i>								
Openness _{1999–2005}	0.011** (0.005)							0.004 (0.007)
Extraversion _{1999–2005}		-0.006* (0.003)						0.003 (0.004)
Conscientiousness _{1999–2005}			-0.006 (0.005)					0.001 (0.005)
Neuroticism _{1999–2005}				0.018*** (0.006)				0.017** (0.007)
Agreeableness _{1999–2005}					-0.012** (0.006)			-0.007 (0.007)
Entrepreneurship-prone personality						-0.009* (0.005)		
<i>Attitudes:</i>								
Opinion: U.S. immigration should decrease _{1992–1998}							-0.006 (0.012)	0.0004 (0.012)
Control variables ₁₉₈₀	yes	yes	yes	yes	yes	yes	yes	yes
Occupational employment shares ₁₉₈₀	yes	yes	yes	yes	yes	yes	yes	yes
Share of foreign-born ₁₉₃₀	yes	yes	yes	yes	yes	yes	yes	yes
Region fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.692	0.686	0.683	0.702	0.687	0.687	0.679	0.686
Observations	117	117	117	117	117	117	117	117
Kleibergen-Paap F statistic	28.610	29.698	30.183	27.520	28.638	31.689	28.193	21.370
Partial R^2	0.343	0.349	0.347	0.327	0.325	0.363	0.352	0.313

Notes: Dependent variable is the *rank-rank slope*, that is, the correlation between the parent (1996–2000) and child income (2010–11) ranks in the national income distribution. The intergenerational income data is from Chetty et al. (2014). Each specification is an IV estimation where the share of foreign-born in 1980 is instrumented by the predicted share of foreign-born in 1980 based on the distribution of migrants in 1930. *Control variables*: log median income, 75/25 income ratio, log population, population density, share college education, share unemployed, share male, median age, share blacks, high/low-skilled migrant ratio, share out-of-state natives, and share low income families. Data on the Big-5 personality traits at state level is from Rentfrow, Gosling and Potter (2008) and data on personality-prone personality is from Obschonka et al. (2013). Data on opinions toward U.S. immigration comes from ANES (2014) and are collected at the county level (Nippelwetter, 1984). For missing counties, we impute average values from the state level. We have no data for six metropolitan areas. We impute values by state averages. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A4.6
Potential Other Mechanisms

	(1)	(2)	(3)	(4)	(5)
Share foreign-born ₁₉₈₀	-0.502*** (0.133)	-0.497** (0.194)	-0.690*** (0.175)	-0.543*** (0.160)	-0.882*** (0.165)
<i>Mechanisms:</i>					
Student-teacher ratio ₁₉₉₀	0.003 (0.002)				
Log real teacher salary ₁₉₉₀	0.045* (0.024)				
Log educational expenditure per student ₁₉₉₀	-0.042* (0.023)				
Log median family income ₁₉₉₀		-0.057 (0.045)			
75-25 family income ratio ₁₉₉₀		0.020 (0.021)			
Net new business formation ₂₀₀₅			-0.009*** (0.003)		
Patents per employee ₂₀₁₀				-1.493 (2.155)	
Log property crime rate ₂₀₁₀					0.040* (0.023)
Log violent crime rate ₂₀₁₀					-0.001 (0.010)
Log burglary crime rate ₂₀₁₀					-0.034* (0.018)
Log robbery crime rate ₂₀₁₀					0.049*** (0.012)
Log murder crime rate ₂₀₁₀					-0.003 (0.006)
Control variables ₁₉₈₀	yes	yes	yes	yes	yes
Occupational employment shares ₁₉₈₀	yes	yes	yes	yes	yes
Share of foreign-born ₁₉₃₀	yes	yes	yes	yes	yes
Region fixed effects	yes	yes	yes	yes	yes
R^2	0.772	0.693	0.709	0.683	0.723
Observations	107	117	117	116	104
Kleibergen-Paap F statistic	32.619	27.113	30.074	26.934	21.049
Partial R^2	0.400	0.314	0.350	0.350	0.354

Notes: Dependent variable is the *rank-rank slope*, i.e. the correlation between the parent (1996-2000) and child income (2010-11) ranks in the national income distribution. The intergenerational income data is from Chetty et al. (2014). *Pupil-teacher ratio* is total enrollment divided by the number of teachers and from the Common Core of Data (1990b). *Teacher salary* is a constructed measure: *Teacher wage per pupil / (1/Pupil-teacher ratio)*. All statistics are weighted (by enrollment) averages over all public primary and secondary schools in the metropolitan area. *Real teacher salaries* are obtained by dividing teacher salaries through the mean salary of similar occupations in 1990. Similar occupations are all occupations others than those of teachers in the category *Managerial and Professional Specialty Occupations*. The means of these occupations are computed from the 1990 Census (Ruggles et al., 2010) by using survey weights. Data on *expenditure per pupil* is from the Common Core of Data (1990a) and for the year 1990. It is again a weighted (by enrollment) averages over all public primary and secondary schools in the metropolitan area. *Median family income* is the family income of the parental cohort of those born in 1980. The measure is constructed by using the 1990 Census (Ruggles et al., 2010) and keep all children who are between the age of 8 to 12. We further restrict the sample to children and parents who are born in the U.S. We drop all children who moved outside their state of birth. The *75-25 family income ratio* is the ratio of the 75th family percentile income to the 25th family percentile income. Family income is missing for two metropolitan areas. We predict missing values by regressing family income or percentiles thereof on log median income, 75/25 income ratio, log population, population density, share male, median age, share blacks, and 22 employment shares in 1990. *Net new business formation* is the birth of new establishments minus the death of establishments divided by the total number of establishments in 2005. Data comes from U.S. Small Business Administration, Office of Advocacy (2005). *Patents per employee* is the number of utility patent grants divided by the number of employees. Data for the number of patents comes from U.S. Patent and Trademark Office (2011) and data for the number of employees comes from Bureau of Economic Analysis (2012). Data on crime is from the Federal Bureau of Investigation (2010). Crime is reported in crime rates per 100,000 inhabitants and for the year 2010. Each specification is an IV estimation where the share of foreign-born in 1980 is instrumented by the predicted share of foreign-born in 1980 based on the distribution of migrants in 1930. *Control variables:* Share of foreign-born in 1930, occupational employment shares, log median income, 75/25 income ratio, log population, population density, share unemployed, share college education, share male, median age, share blacks, high/low-skilled migrant ratio, share out-of-state natives, and share low income families. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A4.7
Other Outcomes: Family Income

	(1)	(2)	(3)	(4)
	<i>Median</i>	<i>P75</i>	<i>P25</i>	<i>P75-P25</i>
<i>Panel A: Nominal income in 1990</i>				
Share foreign-born ₁₉₈₀	1.297** (0.535)	0.960* (0.502)	1.127 (0.722)	-0.167 (0.460)
R^2	0.748	0.789	0.693	0.455
Observations	117	117	117	117
Kleibergen-Paap F statistic	29.711	29.711	29.711	29.711
Partial R^2	0.345	0.345	0.345	0.345
<i>Panel B: Nominal income in 1990 controlling for cost of housing index</i>				
Share foreign-born ₁₉₈₀	0.494 (0.729)	0.190 (0.694)	-0.406 (1.019)	0.596 (0.762)
R^2	0.755	0.785	0.701	0.477
Observations	117	117	117	117
Kleibergen-Paap F statistic	12.198	12.198	12.198	12.198
Partial R^2	0.201	0.201	0.201	0.201

Notes: The dependent variables in Panel A and B are based on the family income of those born in 1980. The measures are constructed by using the 1990 Census (Ruggles et al., 2010) and keep all children who are between the age of 8 to 12. We further restrict the sample to children and parents who are born in the U.S. We drop all children who moved outside their state of birth. The dependent variable in *median* is the log median family income, the log family income at the 75th percentile in *P75*, and the log family income at the 25th percentile in *P25*. In *P75-P25*, we subtract the log family income at the 75th percentile from the log family income at the 25th percentile. All moments of the income distribution are computed by using survey sampling weights. Family income is missing for two metropolitan areas. We predict missing values by regressing family income or percentiles thereof on log median income, 75/25 income ratio, log population, population density, share male, median age, share blacks, and 22 employment shares in 1990. Each specification is an IV estimation where the share of foreign-born in 1980 is instrumented by the predicted share of foreign-born in 1980 based on the distribution of migrants in 1930. We include a full set of controls: share of foreign-born in 1930, occupational employment shares, log median income, 75/25 income ratio, log population, population density, share unemployed, share college education, share male, median age, share blacks, high/low-skilled migrant ratio, share out-of-state natives, share low income families, and region fixed effects. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A4.8
Other Outcomes: School Resources

<i>Panel A: Log school revenues per pupil₁₉₉₀</i>					
	Total	Federal	State	Local	
Share foreign-born ₁₉₈₀	0.871 (1.168)	0.989 (1.192)	-0.476 (3.524)	1.047 (3.188)	
R^2	0.730	0.685	0.149	0.679	
Observations	117	117	117	117	
Kleibergen-Paap F statistic	14.346	14.346	14.346	14.346	
Partial R^2	0.232	0.232	0.232	0.232	
<i>Panel B: Log school expenditure per pupil₁₉₉₀</i>					
	Total	Instruction	Support	Other Programs	Nonele. or sec.
Share foreign-born ₁₉₈₀	0.061 (1.172)	-0.673 (1.025)	1.686 (1.701)	-4.359 (4.585)	-15.136* (8.926)
R^2	0.715	0.759	0.691	-0.014	0.370
Observations	117	117	117	117	116
Kleibergen-Paap F statistic	14.346	14.346	14.346	14.346	13.979
Partial R^2	0.232	0.232	0.232	0.232	0.232
<i>Panel C: Teacher ratio and teacher salaries₁₉₉₀</i>					
	Pupil-teacher ratio	Log teacher wage	Log teacher wage per pupil		
Share foreign-born ₁₉₈₀	7.547 (8.880)	-0.518 (0.731)	-0.814 (1.010)		
R^2	0.768	0.511	0.564		
Observations	107	107	117		
Kleibergen-Paap F statistic	30.064	30.064	29.711		
Partial R^2	0.381	0.381	0.345		

Notes: Revenues per pupil are available for funding for all sources: federal, state, and local sources. Expenditure per student is available in total, as spending on instruction, as spending on support services (i.e. student and instructional staff support, general and school administration, student transport, etc.), as spending on other elementary-secondary programs (i.e. food services, enterprise operations, etc.) or as spending on non-elementary/non-secondary programs (non-ele./non-sec.) (i.e. community services, adult education, etc.). All data on school finances is from the Common Core of Data (1990a) and for the year 1990. Teacher wage per pupil is also from the Common Core of Data (1990a). All statistics are weighted (by enrollment) averages over all public primary and secondary schools in the metropolitan area. Pupil-teacher ratio is total enrollment divided by the number of teachers and from the Common Core of Data (1990b). It is again an enrollment-weighted averages over all schools in the metropolitan area. Teacher wage is then a constructed measure: Teacher wage per pupil / (1/Pupil-teacher ratio). Teacher wages and teacher wages per pupil are divided by the mean salary of similar occupations in 1990. Means are computed from the 1990 Census (Ruggles et al., 2010) by using survey weights. Similar occupations are all occupations others than those of teachers in the category *Managerial and Professional Specialty Occupations*. Each specification is an IV estimation where the share of foreign-born in 1980 is instrumented by the predicted share of foreign-born in 1980 based on the distribution of migrants in 1930. We include a full set of controls: share of foreign-born in 1930, occupational employment shares, log median income, 75/25 income ratio, log population, population density, share unemployed, share college education, share male, median age, share blacks, high/low-skilled migrant ratio, share out-of-state natives, share low income families, and region fixed effects. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Chapter 5

Immigration, Regional Conditions, and Crime: Evidence from an Allocation Policy in Germany*

Criminal behavior of immigrants is a huge concern in many countries. In Europe, for example, the majority of native residents are worried about immigrants increasing crime, whereas only a minority is worried about immigrants taking jobs away (Fitzgerald, Curtis and Corliss, 2012). The widespread concern about crime might therefore play a greater role in immigration restrictions than do labor market concerns (Card, Dustmann and Preston, 2012). While the literature on the labor market effects of immigration is huge¹, the crime effects of immigration has been explored much less.

We investigate the crime impact of a particular group of low-educated immigrants to Germany who were allocated across regions by German authorities upon arrival. We find that these immigrants increased crime substantially. The crime effects are much stronger in regions with adverse conditions such as high unemployment and high crime levels.

Existing research on the crime impact of immigration tends to find zero or small effects. Bianchi, Buonanno and Pinotti (2012) exploit the increase in the immigrant population in Italy in the 1990s that was mainly driven by the collapse of the Soviet Union and the Balkan Wars. They instrument increases in region-specific immigrant stocks by combining source country-specific immigration

* This chapter was coauthored by Marc Piopiunik, Ifo Institute.

¹ See Borjas (1994, 1999); Friedberg and Hunt (1995); Dustmann, Glitz and Frattini (2008); Card (2009) for overviews.

flows toward the rest of Europe with regional variation in source country-specific distributions of resident immigrants. The authors find strong effects on robberies, but a negligible effect on total crime. Bell, Fasani and Machin (2013) study the impact on crime of two different immigration waves to the United Kingdom: migrant laborers from eight European countries that joined the EU in 2004 and asylum seekers. Using a supply-push instrument for the first group and exploiting a dispersal policy for the second group, the authors find only an effect for asylum seekers, and only on property crime. Using U.S. state panel data, Butcher and Piehl (1998, 2007) find no significant relationship between immigration and crime. Using U.S. metropolitan area data, Chalfin (2013) finds no evidence that Mexican immigrants increase crime. Spenkuch (2013) uses U.S. county data and finds effects on property crime, but not on violent crime.

The group of immigrants we focus on in this study are ethnic Germans who lived in Eastern Europe and the former Soviet Union before migrating to Germany after the fall of the Berlin Wall. Between 1988 and 2005, more than 3 million ethnic Germans immigrated, increasing Germany's population by about 5 percent. Upon arrival, ethnic German immigrants were granted German citizenship, were allowed to work, and were eligible to social security assistance (like German natives). Importantly, ethnic German immigrants have considerably lower education levels, worse labor market outcomes, and lower incomes than native Germans (see Section 5.1). Based on these characteristics, ethnic German immigrants are, however, similar to other immigrants in Germany.

We exploit a dispersal policy to identify the causal effect of ethnic German immigrants on crime.² Upon arrival, ethnic Germans are first allocated across German states and subsequently allocated across counties within states. The number of migrants allocated to a particular county is set by German policymakers, which largely depends on the number of residents, economic conditions, and other factors such as housing capacities. The specific county of residence where a newly arriving ethnic German immigrant is allocated to, depends mostly on family ties. While German authorities determine the *number* of ethnic German immigrants allocated to a particular county, each immigrant can express preferences to live in the county where her relatives are already living. If the county quota is not already exhausted, authorities try to meet these family preferences.³ Noncompliance

² Other studies exploiting dispersal policies (typically applied only to refugees) include, for example, Edin, Fredriksson and Åslund (2003); Damm (2009); Glitz (2012); Damm and Dustmann (2014).

³ Note that although the majority of individual residence choices depended on family ties, the variation in the *size* of immigrant flows across regions is not based on networks as in the shift-share approach; this approach combines supply-push factors – events in origin countries that increase the propensity to emigrate – with the preexisting distribution of immigrants by origin country to instrument for the immigrant inflows across regions. In the shift share approach, the regional distribution of resident immigrants is a good predictor of current location decisions since new immigrants tend to settle in areas where immigrants from their home country have settled (Card, 2001).

with the residence allocation, which is binding for three years, is severely sanctioned with the withdrawal of all social benefits, meaning that self-selection into regions is highly restricted. Since the residence of family members is the main allocation criterion and labor market skills do not play any role (Glitz, 2012, p. 180), the allocation is likely exogenous with respect to regional crime and labor market conditions.⁴ This allocation, therefore, provides a unique quasi-experimental setting for studying the effects of immigration on crime.⁵ Furthermore, as the allocation policy arguably led to similar skill and age distributions of ethnic German immigrants across regions, we are able to assess the importance of regional conditions for the crime impact.

We combine annual county-specific inflows of ethnic German immigrants with annual county-specific crime rates. We add county-specific information on labor market conditions and demographics to assess whether (and, if so, which) regional conditions play a role in the impact of immigration on crime. We focus on West Germany (excluding Berlin) from 1996 until 2005, the period during which newly arriving ethnic German immigrants were allocated across regions by German authorities. The allocation was binding for three years and noncompliance with the assignment was sanctioned with withdrawal of all social benefits.

Results indicate that ethnic German immigrants increase crime rates substantially. An immigrant inflow of one ethnic German per 1,000 inhabitants increases total crime by about 0.9 percent, which amounts to an elasticity of approximately 0.45. Effects vary by type of crime, with strong impacts on burglary, property damage, and battery, but no effect on street-related types of crime. We also find evidence that regional labor market conditions are crucial: While immigration has no effect on crime in regions with low unemployment rates, crime effects are strong in regions with high unemployment. Furthermore, we find a similar pattern with respect to preexisting crime levels. The impact of immigration on crime is much stronger in regions with high preexisting crime levels, which might be due to social interactions (Glaeser, Sacerdote and Scheinkman, 1996).

In contrast, in our setting, the regional distribution of resident immigrants cannot be used as an instrument for the allocation of new immigrants across regions.

⁴ One potential issue is that children of criminal fathers are much more likely of having a criminal conviction than those with noncriminal fathers, with parents' background accounting for the vast majority of this intergenerational crime relationship (Hjalmarsson and Lindquist, 2012). Therefore, a threat to our identification strategy is that family members or other relatives could have self-selected into low- or high-crime counties. However, if the selection is due to permanent characteristics of the county, we control for this by including county fixed effects. And if relatives selected into counties based on temporary features of the county (such as good labor market conditions), this only leads to biased estimates if the historical local temporary shock is correlated with the current local temporary shock. But since relatives have chosen their county of residence long before ethnic Germans immigrated (due to the closed borders during the Cold War), a correlation of temporary shocks is unlikely. We discuss this in more detail in the empirical model section.

⁵ Glitz (2012) exploits the same allocation policy to estimate the impact of immigrants on skill-specific employment rates and wages of natives, finding a displacement but no wage effect.

In their economic theory of crime, Becker (1968) and Ehrlich (1973) argue that the propensity for crime decreases with legitimate earning opportunities,⁶ the probability of being convicted, and the cost of conviction. The last factor is particularly important in this study because ethnic Germans received citizenship immediately upon arrival, which is typically not the case for immigrant groups studied elsewhere. Since citizenship reduces the cost of conviction by eliminating the threat of deportation (Spenkuch, 2013), this could explain why the crime effects we find are so much larger than those found in other studies.⁷

Furthermore, we contribute to the literature by providing the first direct assessment of the importance of local labor market conditions for the crime impact of immigration.⁸ This analysis is possible since the allocation policy arguably led to similar skill and age distributions of newly arriving ethnic German immigrants across counties, whereas the immigrant groups studied for example in Bianchi, Buonanno and Pinotti (2012) and Bell, Fasani and Machin (2013) could freely choose their region of residence (with the exception of refugees in the United Kingdom). Finally, we estimate short-run effects of immigration on crime by exploiting annual immigration inflows, whereas other research estimates medium-run or long-run effects based on changes in immigrant stocks over time. If immigrants are more likely to commit crimes in the first years after arrival, for example, because integration into the new society takes some time, then we are more likely to find stronger crime effects.

The remainder of the chapter proceeds as follows. Section 5.1 describes ethnic German immigrants and their allocation across regions. Section 5.2 presents the data and Section 5.3 the empirical model. Section 5.4 reports the main results on the effects of immigration on crime, robustness checks, and effect heterogeneities with respect to regional conditions. Section 5.5 concludes.

⁶ This implies that immigrants should have higher crime propensities than natives if they face worse labor market opportunities than natives.

⁷ In the United States, for example, both legal and illegal immigrants who commit serious crimes are being expelled from the country and can therefore not commit any further crimes. For example, “From 1998 to 2007, 816,000 criminal aliens were removed from the United States because of a criminal charge or conviction. This is equal to about one-fifth of the nation’s total jail and prison population. These figures do not include those removed for the lesser offense of living or working in the country illegally” (Camarota and Vaughan, 2009, p. 2). Additionally, immigrants to the U.S. might be “positively selected” because aliens who committed serious crimes prior to entry in the U.S. are ineligible to receive visas and ineligible to be admitted to the United States (see <http://www.law.cornell.edu/uscode/text/8/1182>).

⁸ In contrast, other studies either investigate the impact of local labor market conditions on crime in general (without focusing on immigrants) or study the impact of immigrants on crime without considering the potentially important role of local labor market conditions.

5.1 Ethnic German Immigrants and the Allocation Policy

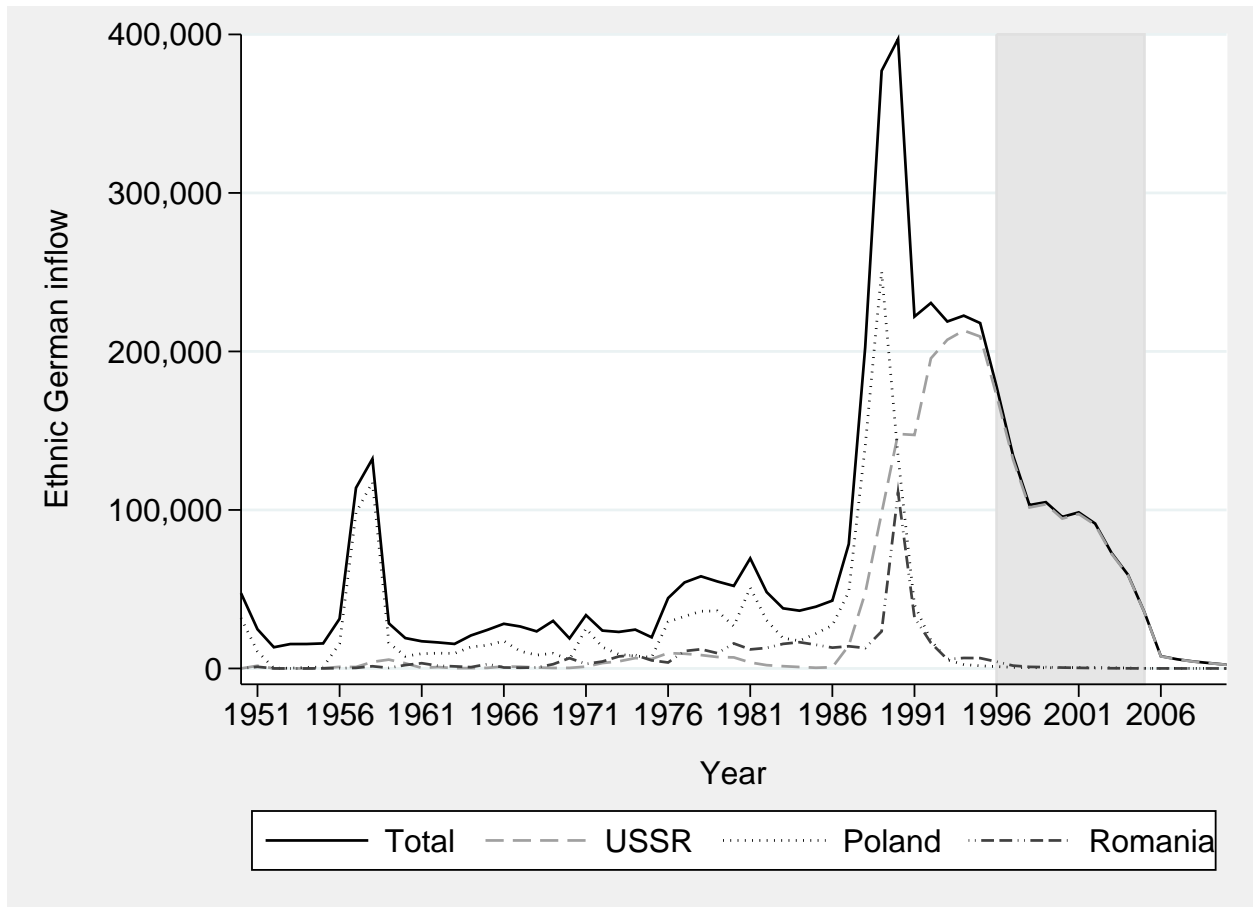
Ethnic German immigrants are descendants of German colonists who had migrated to Russia and other East European countries in the 18th and 19th century (Bade, 1990). After Nazi attacks during World War II, many ethnic Germans—considered potential collaborators with the Nazi regime—were forced to leave their original settlements and to move eastward to Siberia, Kazakhstan, and several regions in Middle Asia where they were kept in special settlements.

During the Cold War, due to political restrictions, only few ethnic Germans were allowed to emigrate (see Figure 5.1). Toward the end of the communist regime, emigration restrictions were alleviated, leading to a massive emigration wave driven by bad economic conditions and unstable political conditions in the Soviet Union (Bade and Oltmer, 2003). Since the end of the 1980s, more than 3 million ethnic German immigrants have arrived in Germany. There was a heavy inflow of ethnic German immigrants in the late 1980s and early 1990s, with a peak of 397,000 individuals in 1990. Due to these large immigration waves, a yearly quota of about 225,000 individuals was introduced in 1993, which was reduced to about 100,000 individuals per year in 2000. Due to these quotas, and because the stock of German descendants in the former Soviet Union was becoming smaller, the number of immigrating ethnic Germans decreased over time.⁹ The collapse of the Soviet Union also led to a significant change in the source countries of ethnic German immigrants. Between 1950 and 1987, only about 7 percent of ethnic German immigrants came from countries of the former Soviet Union. In contrast, since the mid-1990s, almost all ethnic German immigrants have come from former Soviet Union countries, especially Kazakhstan, Kirgistan, Uzbekistan, Ukraine, and the Russian Federation (see Figure 5.1).

The immigration of ethnic Germans to Germany was administered centrally by the Federal Office of Administration (*Bundesverwaltungsamt*). First, all ethnic German immigrants had to apply for a visa at the German embassy in their country of residence. To be recognized as an ethnic German, an applicant had to fulfill all requirements of the Federal Refugees Act. Legally, ethnic Germans are members of German minorities who meet three requirements (Peters, 2003): (1) must be descendants of at least one parent or grandparent with German nationality; (2) must have declared “confession to German ethnicity” before leaving the settlement area (typically by

⁹ Ethnic German immigrants do not include East Germans who moved to West Germany after the fall of the Berlin Wall.

Figure 5.1
Annual Inflow of Ethnic German Immigrants to Germany



Notes: The figure shows the annual inflow of ethnic Germans to Germany by source country/region. The gray area between 1996 and 2005 highlights the period used in the analysis. Source: Federal Office of Administration (*Bundesverwaltungsamt*).

possessing the German nationality, sending their children to a German school, or through being a member in a German association); and (3) be able to conduct a simple conversation in German. Due to assimilation pressures in their settlement areas, ethnic German immigrants had rather low German language proficiency, however. Therefore, language requirements were kept at a relatively low level. To maintain the unity of the family, ethnic Germans were allowed to immigrate together with their spouses and offsprings, given that the marriage had existed for at least three years. Ethnic Germans and their relatives migrating with them were granted German nationality immediately upon arrival.¹⁰

All arriving ethnic German immigrants had to pass through a central admission center. If ethnic German immigrants did not have a job or other source of income, which was the case for the

¹⁰ In the remainder of the chapter, *ethnic German immigrants* includes both persons of German descent as well as their spouses and children.

vast majority (see below), they were then allocated to one of the 16 German states according to predefined quotas (*Königsteiner Schlüssel*). These quotas, which are adjusted every year, are based on the tax revenues (weight of two thirds) and the population size (one third) of each state.

Based on the Place of Residence Allocation Act (*Wohnortzuweisungsgesetz*), which has been established in 1989 due to the large inflows, most states subsequently allocated ethnic German immigrants across counties. The goal of this allocation was a socially acceptable integration of these immigrants. Because ethnic German immigrants preferred to live close to their relatives and noncompliance with the assignment did not have any consequences, many ethnic Germans left their assigned county and moved to other counties. This led to a strong inflow of ethnic German immigrants into some regions (Swiaczny and Mamme, 2001). Because municipalities were responsible for the housing of ethnic German immigrants and for the payment of social assistance, this implied great financial burdens for municipalities with many ethnic German immigrants. Furthermore, high concentrations of ethnic German immigrants increased the necessity of integration measures and lowered the acceptance among native Germans (Haug and Sauer, 2006). Seven counties, which were particularly hit by massive inflows of ethnic German immigrants, signed the so-called “Gifhorn Declaration for the Integration of Ethnic German immigrants” in March 1995, demanding a more solidary distribution of the burdens between states and municipalities (Niedersächsische Landeszentrale für Politische Bildung, 2002, p. 10).¹¹ This declaration led to an important amendment to the Place of Residence Allocation Act that mandated that ethnic German immigrants were bound to their assigned county during the first three years after immigration. Especially, the amendment introduced severe sanctions: Ethnic German immigrants would lose all social benefits and any type of public assistance in case of noncompliance with the allocation decision.¹² Only immigrants who could prove both a job and housing in their preferred county were exempt from this rule. However, survey evidence (presented below) indicates that only a minority of newly arriving ethnic German immigrants were able to freely choose their county of residence. Most West German states implemented the new law in March 1996, whereas Hesse adopted the law in January 2002, and two states (Bavaria and Rhineland-Palatine) did not adopt it at all.¹³

¹¹ These counties were Wolfsburg, Salzgitter, Gifhorn, Nienburg/Weser, Cloppenburg, Emsland, and Osnabrück. Dropping these counties from the analysis does not affect the results.

¹² Ethnic German immigrants were supported more than other immigrants by the German government. They received allowances (*Eingliederungshilfe*) and were offered German language courses.

¹³ The amendment to the Place of Residence Allocation Act was indeed effective. For example, the counties of the Gifhorn Declaration which experienced a massive inflow until 1995 received only few ethnic German immigrants after the law was adapted in March 1996 (Wenzel, 1999). Glitz (2012) furthermore notes that the perception at the Ministry of the Interior and the Association of German Cities and Towns was that the sanctions were effective and ensured high compliance with the initial allocation decision.

The allocation of ethnic German immigrants within states across counties was not handled uniformly by the states. Typically, the number of ethnic German immigrants allocated to counties was based on state-specific quotas that partly depended on the size of the resident population. The allocation also depended on other factors, such as intake capacities or size of the county area in some states (e.g. North Rhine-Westphalia). Some states adjusted their quotas to changes in the counties' population size, whereas others did not. Appendix Table A5.1 indicates that ethnic Germans were allocated according to these criteria: counties with larger populations and counties with higher GDP per capita received more ethnic German immigrants (although the latter coefficient is statistically insignificant). Because previous inflows capture time-persistent county characteristics that are not captured by the other covariates, the compound inflow in the previous three years is a predictor of the current inflow.¹⁴

The county of residence of newly arriving ethnic German immigrants was largely determined by family ties. While German authorities determined the *number* of ethnic German immigrants allocated to a particular county, each new immigrant could express her preferences to live in the county where her relatives were already living. If the quota was not already exhausted, the authorities tried to meet the preferences. The Ministry of the Interior estimates that the presence of family members was the decisive factor for the allocation decision in about 90 percent of all cases. The presence of health and care facilities and the infrastructure for single parents were additional factors. Most importantly, however, the skill level or previous crime history of ethnic German immigrants did not play any role in the allocation process (Glitz, 2012, p. 180).

Even though official data are lacking¹⁵, there is strong evidence that the vast majority of ethnic German immigrants stayed in their assigned county of residence during the period of retention (first three years after immigrating). First, ethnic German immigrants were much more likely to depend on social welfare than native Germans. Therefore, leaving the assigned county would be very costly. In a survey among 1,554 ethnic German immigrants, only 11 percent reported that they did *not* receive any type of social benefit or state assistance during the first three years after

¹⁴ One potential issue, frequently discussed in the migration literature (Borjas, 1994), might be that residents move to other counties in response to immigrant inflows. In our case, potential outflows of residents would be problematic if people with particularly high (low) crime propensities would move, thus leading to an underestimation (overestimation) of the true effect. In line with findings that Germany has a rather inflexible labor market (Pischke and Velling 1997), Glitz (2012) has shown that there are no systematic outflows of residents in response to the inflows of ethnic German immigrants.

¹⁵ Because ethnic German immigrants cannot be identified in the registration offices of the municipalities due to their German citizenship, the number of ethnic German immigrants residing in a county is unknown. Therefore, there are no official statistics on how many ethnic German immigrants were able to choose their place of residence because they had both job and housing, nor how many of them changed their assigned county within the three-year period of retention.

arrival (Haug and Sauer, 2007, p. 101). And among those who immigrated after 1996, only 36 percent worked at *some point* during the first three years after arrival (Haug and Sauer, 2007, p. 120). Second, ethnic German immigrants were likely to comply with the assignment because they depended on state-financed housing. After arriving in Germany, about 80 percent of ethnic German immigrants lived in temporary residential homes or admission centers (Seifert, 1996). Furthermore, they stayed on average about two years in these temporary residential homes (Mammey, 1999). Adolescent ethnic German immigrants are also very likely (about 51 percent) to live in residential establishments or in social housing, compared to 10 percent of native Germans (Dietz, 1999, Table 12).

The socioeconomic characteristics of ethnic German immigrants are worse than those of native Germans. One reason might be that the share of ethnic German immigrants of own German descent was only 47.7 percent in 1996 and declined further to 21.5 percent in 2005 (Haug and Sauer, 2006, p. 417). The proficiency in the German language of ethnic German immigrants is surprisingly low. One reason is certainly that ethnic Germans had to assimilate in their home countries, thereby losing German language skills. Even among ethnic German immigrants who immigrated in the early 1990s, when the majority was of own German descent, self-assessed German language proficiency at the end of the 1990s was not better than the language proficiency of other immigrants who immigrated in the same years, and is worse than the language proficiency of second-generation immigrants in Germany (Haug, 2005).¹⁶

Apart from language deficiencies, young ethnic German immigrants also tend to have low education. Administrative school data from the largest German state, North Rhine-Westphalia, for the school year 1996/1997 show that ethnic German children are highly overrepresented in the least academic secondary school track (*Hauptschule*) (Dietz, 1999, Table 9). The occupation composition of newly arriving ethnic German immigrants also indicates a rather low-educated immigrant group. Ethnic German immigrants are highly overrepresented in lower skill occupations such as farmers, laborers, transport workers, operatives, and craft workers (see Glitz 2012, p. 188f.). In sum, although ethnic German immigrants are of German descent, this group has much poorer socioeconomic characteristics and employment outcomes than native Germans. Employment outcomes of ethnic German immigrants and other immigrant groups in Germany, however, are rather similar.

¹⁶ The low German language proficiency might partly be due to the fact that only few ethnic Germans had native Germans as friends (Dietz, 1999). One reason for the lack of friendships might be the negative attitude of native Germans toward ethnic Germans, which was sometimes more negative than toward foreigners (Fertig and Schmidt, 2001).

5.2 Data

We have collected administrative data on reported crime at the county level (NUTS-3) from the State Offices of Criminal Investigation (*Landeskriminalämter*) for the years 1997–2002 and from the Federal Criminal Police Office (*Bundeskriminalamt*) for the years 2003–2006. We were able to collect data on total crime and important subcategories: battery, drug offense, property damage, burglary, and street crime.¹⁷

Table A5.2 reports the West German states used in the analysis, along with county-year-level observation numbers for the different crime categories. We include all West German states that allocated ethnic German immigrants across counties.¹⁸ The first year in our sample is 1996 because the allocation was not binding before, and the last year is 2005 as ethnic German inflows into Germany were rather small afterwards (see Figure 5.1).¹⁹

$$Crime\ rate_t = \frac{reported\ crimes_t \times 100,000}{population_{t-1}} \quad (5.1)$$

Table 5.1 reports summary statistics of the county-year-level data. Panel A reports the official crime rates for total crime and the five subcategories. Crime rates are defined as the number of crimes reported to the police per 100,000 inhabitants (Equation 5.1). The average total crime rate in our sample is 6,883 crimes per 100,000 inhabitants. Compared to the mean, the standard deviation of 2,661 crimes is large, which is particularly due to large differences between counties. After taking out permanent differences between counties and yearly fluctuations within states by regressing crime rates on county and year*state fixed effects, the variations in crime rates decrease (Adj. SD).

¹⁷ *Total crime* contains any type of crime reported to the police. *Battery* includes, among others, bodily harm with fatal consequences, grievous and serious bodily harm, and bodily injury caused by negligence. *Drug offense* contains all crimes associated with general violations of the narcotics law, drugs trafficking, and illegal importation of drugs. *Property damage* covers all crimes that involve destroying, damaging or making useless another person's property. *Burglary* contains all crimes that include housebreaking for committing theft. *Street crime* is a mixed category that includes various street-related crimes, for example, theft of, in, and out of cars and robberies. Crime categories can be reviewed in the List of Crimes (*Straftatenkatalog*) from the Federal Criminal Police Office.

¹⁸ Bavaria and Rhineland-Palatinate did not allocate ethnic German immigrants across counties. We focus on West Germany (excluding East Germany and Berlin) because crime data at the county level are not available for East German states for the years 1997–2002.

¹⁹ Hamburg starts only in 1999 due to missing information on ethnic German immigrant inflows, and Hesse starts only in 2003 because the allocation of ethnic German immigrants to counties started only in 2002. Observation numbers vary by crime category due to missing data. Furthermore, crime data for North Rhine-Westphalia were only provided at the level of police administration areas. While in most cases police administration areas are identical with the borders of counties, we had to aggregate 14 counties (8.3 percent) into 6 police administration areas.

Table 5.1
Summary Statistics

Variable	Mean	Min	Max	SD	Adj. SD	Obs
<i>Panel A: Crime rates</i>						
Total crime	6,883	620	18,569	2,661	516	1,687
Battery	493	58	1,727	222	46	1,358
Drug	291	35	1,730	176	65	1,687
Property damage	760	254	2,375	282	79	1,358
Burglary	133	15	593	83	26	1,365
Street crime	1,862	466	5,404	967	159	1,358
<i>Panel B: Ethnic German immigrants</i>						
Ethnic German inflow	293	0	2,393	270	93	1,687
Ethnic German inflow rate	0.0011	0	0.0049	0.0007	0.0004	1,687
<i>Panel C: County characteristics</i>						
Population	262,615	50,878	1,754,182	203,034	3,274	1,687
Foreign population share	8.94	2.67	26.28	4.16	0.29	1,687
Young male population share	16.76	13.09	21.41	1.33	0.17	1,687
Unemployment rate	10.1	4	25.2	3.12	0.58	1,687
GDP per capita	25,376	12,634	77,318	8,724	1,076	1,687
<i>Panel D: Clear-up rates</i>						
Total crime	53.4	34.8	75.9	6.9	2.4	1,687
Battery	90.8	80.4	99.7	2.9	1.3	1,358
Drug	96	50.9	104.2	3.3	2.0	1,687
Property damage	26.2	12.5	70.5	6	4.1	1,358
Burglary	28.2	2	91.3	15.2	7.5	1,365
Street crime	16.8	6.6	43.8	5	2.8	1,358

Notes: *Crime rates* (Panel A) refer to years 1997–2006. Variables in Panels B–D refer to years 1996–2005. *Crime rates* is defined as the number of crimes reported to the police in year t per 100,000 inhabitants in the previous year $t - 1$. *Ethnic German inflow rate* is defined as number of allocated ethnic German immigrants in year t divided by the total population in the previous year $t - 1$. Summary statistics in Panels B–D are computed for counties with data on total crime (see Table A5.2). The *young male population share* is the share of 15 to 39 year old adults in the total population. *Foreign population share* and *unemployment rate* are in percent. The adjusted standard deviation (*Adj. SD*) is the standard deviation of the residuals coming from a regression of the indicated variable on county and year \times state fixed effects. Data sources: see text.

The relative importance of each crime category can be expressed by the number of crimes in the respective category as a percentage of total crime. The largest crime category is street crime covering 27.1 percent of all reported crimes. The second biggest category is property damage with 11 percent, followed by battery (7.2 percent), drug offense (4.2 percent), and burglary (1.9 percent). Taken together, these five crime categories account for 51.4 percent of all reported crimes.

Annual data on the inflows of ethnic German immigrants into counties come from two sources. Inflow data from 1996 to 2001 is taken from Glitz (2012), which come from the admission centers in each state. Inflow data from 2002 to 2006 come from the *Bundesarbeitsgemeinschaft Evangelische Jugendsozialarbeit e.V., Jugendmigrationsdienste*, which is a church initiative that takes care of underprivileged youths. This initiative collected the data directly from the state Ministries of the Interior. We checked the comparability of the two different data sources by comparing the data for the overlapping years (1998–2001), finding a very high degree of consistency.

Summary statistics of ethnic German immigrant inflows across counties are reported in Panel B. The average yearly inflow between 1996 and 2005 was 293 ethnic German immigrants per county, with a minimum of 0 and a maximum of 2,393 immigrants. There is substantial variation across counties, with a standard deviation of 270 ethnic German immigrants. Taking out time-invariant county differences and yearly within-state fluctuations, the adjusted standard deviation (Adj. SD) is 93 ethnic German immigrants.

$$\text{Ethnic German inflow rate}_t = \frac{\text{number of allocated ethnic German immigrants}_t}{\text{population}_{t-1}} \quad (5.2)$$

The main explanatory variable of interest is the *ethnic German inflow rate*. The inflow rate is defined as the number of ethnic German immigrants assigned to a county in year t divided by the population of that county at the end of year $t - 1$ (Equation 5.2).²⁰ The average inflow rate was about one ethnic German immigrant per 1,000 inhabitants in each year. Over the entire sample period (1996–2005), the average inflow rate was 12 ethnic German immigrants per 1,000 inhabitants, that is, more than one percent of Germany's population. This inflow rate implies that the average county received 3,151 ethnic German immigrants between 1996 and 2005. The inflow rate over our sample period varies between 2 ethnic German immigrants per 1,000 inhabitants and 22 ethnic German immigrants per 1,000 inhabitants.

²⁰ Alternatively defining the inflow rate as the number of assigned ethnic Germans divided by the county's population plus the number of assigned ethnic Germans yields very similar results.

Because quotas were not only based on population size, inflow rates vary across counties. Nevertheless, the allocation policy lowers the variation in the German inflow rates across counties that can be exploited. Thus, the effect of the ethnic German inflow on crime will not be estimated very precisely (although most coefficients are statistically significant at the 5 percent level).

Summary statistics of additional county characteristics are reported in Panel C, which include demographic information such as population, share of foreigners, and share of young male adults aged 15 to 39. The unemployment rate and GDP per capita reflect labor market conditions.²¹ Finally, we use the crime-specific clear-up rates as proxies for the probability of being arrested (Panel D). The clear-up rate is defined as the number of crimes that are cleared times 100 divided by the number of reported crimes.

5.3 Empirical Model

To investigate the effect of immigration on crime, we estimate the following model:

$$\ln \text{crime rate}_{ist} = \alpha + \beta \text{ethnic German inflow rate}_{ist-1} + \mathbf{X}'_{ist-1}\gamma + \mu_i + (\mu_t \times \mu_s) + \varepsilon_{ist} \quad (5.3)$$

where the logarithm of the crime rate in county i in state s in year t is regressed on the ethnic German inflow rate of the previous year.²² We control for county fixed effects (μ_i) to eliminate any time-invariant determinants of crime across counties. Furthermore, we include year*state fixed effects ($\mu_t \times \mu_s$) to net out crime trends that are common to all counties within a state.²³ \mathbf{X}_{ist-1} includes lagged time-varying county characteristics, especially demographics and economic conditions.

²¹ Population data and GDP per capita come from the Federal Statistical Office (*Regionalstatistik*) and unemployment rates from the Federal Employment Agency (*Bundesagentur für Arbeit*).

²² Using the *absolute* ethnic German inflow, that is, the number of assigned ethnic Germans, leads to very similar results (not reported).

²³ This model is more restrictive, but also more realistic, than a model that includes only year fixed effects. There are three reasons for including year*state fixed effects. First, each state decides how to allocate ethnic German immigrants across its counties. Therefore, changes in the allocation policy would affect all counties within the same state, but would not affect counties in other states. Second, states, and not the federal level, have the authority over police issues. Each state decides, for example, about hiring new police officers and how to allocate police officers across counties. Third, states differ in the proximity to neighboring countries, which might lead to different crime developments over time. Appendix Figure A5.1 presents the crime trends for six states in our sample, showing that state-specific crime rates develop differently over time. These differing crime trends are taken into account by including year*state fixed effects.

It is a well-known problem in the crime literature that reported crimes underestimate the true (but unobserved) number of committed crimes (e.g., Bianchi, Buonanno and Pinotti, 2012). This may lead to biased estimates if the extent of underreporting is correlated with the determinants of crime. For this reason, crime studies use the logarithm of the crime rate and include region and year fixed effects (Levitt, 1996; Gould, Weinberg and Mustard, 2002; Oester and Agell, 2007; Fougère, Kramarz and Pouget, 2009; Bianchi, Buonanno and Pinotti, 2012). Using the logarithm of the crime rate and including fixed effects eliminates measurement error that is constant within regions over time and measurement error that is constant within time periods across regions. This arguably eliminates various important sources of underreporting, such as cultural differences across regions or changing crime-awareness over time (e.g. transported via national media).

We cluster standard errors at the levels of the included fixed effects, that is, we assume the following error structure: $\varepsilon_{ist} = \theta_i + (\theta_t \times \theta_s) + \eta_{ist}$. We allow the error term to be correlated within counties over time (θ_i) and within year*state cells across counties ($\theta_t \times \theta_s$). Clustering standard errors at the year*state level is motivated by the fact that states are responsible for allocating both ethnic German immigrants and police forces across counties. We use two-way clustered standard errors to account for the two dimensions of correlated standard errors.²⁴

We also control for several lagged county characteristics, \mathbf{X}_{ist-1} , that might affect crime rates.²⁵ Demographic characteristics include the log population²⁶, the share of foreigners, and the share of young male adults (aged 15-39) since this population group is particularly likely to commit crimes (Freeman, 1999). We also control for log GDP per capita and unemployment rate to capture labor market opportunities. Finally, we include the clear-up rate for the respective crime category as a proxy for the expected costs of crime (see Ehrlich, 1996). Note that all county characteristics are also lagged by one year since the inflow of ethnic German immigrants might have affected these characteristics, thus rendering the county characteristics in year t bad controls (Angrist and Pischke, 2009).²⁷

Given that the allocation of ethnic German immigrants was binding for three years, we are able to identify (only) the short-run impact of immigration on crime. Because we exploit annual

²⁴ See Cameron, Gelbach and Miller (2011) and Thompson (2011) for theoretical derivations of the two-way clustering method and Acemoglu and Pischke (2003) for an application. We use the Stata command IVREG2 for computing two-way clustered standard errors.

²⁵ See Freeman (1999) for a review on crime determinants.

²⁶ Since the model includes county fixed effects, population size also captures population density, which is one determinant of criminal behavior (Glaeser and Sacerdote, 1999).

²⁷ Results do not change if we use contemporaneous control variables instead.

inflow data – and since data on the stock of ethnic German immigrants are not available – we cannot identify medium- to long-run effects.

The empirical model is set up to reflect this short-run perspective: the crime rate in year t is related to the migrant inflow in the previous year ($t - 1$). Using the one-year lag of the inflow ensures that we do not erroneously attribute crimes committed at the beginning of year t to ethnic German immigrants who arrived at the end of year t . Depending on the exact date of arrival in year $t - 1$, the model captures any crime committed during the first or first two years after arrival. If immigrants have particularly poor labor market outcomes in the first years after arrival, for example due to occupational downgrading (Dustmann and Preston, 2012), crime impacts might be stronger in the short run than in the medium or long run. In that sense, our results provide an upper bound on the medium- to long-run crime impacts of immigration.

The identification of a causal effect on crime depends on the effectiveness of the allocation procedure and in particular on the exogeneity of the allocation with respect to transitory regional crime (and labor market) conditions. Glitz (2012, p. 193) points out that the exogeneity requirement is likely satisfied because the main allocation criterion was the proximity of family members and because labor market skills did not play a role in the allocation process. Glitz provides evidence that the allocation across states was exogenous with respect to individual characteristics “as suggested by the overwhelming importance of family ties for the allocation decision.” He finds that the age distributions of ethnic German immigrants across the West German states are very similar (with standard deviations much smaller than the corresponding standard deviations of the overall population), which should be the case if immigrants were exogenously allocated with respect to individual characteristics. Glitz argues that this finding also indicates that the allocation within states to different counties was also exogenous since the allocation across counties followed “similar administrative processes and decision criteria” as the previous allocation to states. In sum, because skills and age—two potentially important determinants of crime—were no factors for allocating ethnic German immigrants to counties, the skill and age distributions of newly arriving ethnic German immigrants should be similar across counties.²⁸

One potential issue is that children of criminal fathers are much more likely of having a criminal conviction than those with noncriminal fathers, with parents’ background accounting for the vast majority of this intergenerational crime relationship (Hjalmarsson and Lindquist, 2012). For the

²⁸ Glitz (2012) notes that this assumption may not hold if the skills between immigrants and their relatives are correlated. However, because family members were typically separated a long time ago and attended different education systems, the correlation in skills is likely small.

intergenerational correlation in crime to invalidate our identification strategy, the family members already living in Germany who are less criminal must have self-selected into regions with low unemployment or low crime rates. To the extent that county-specific labor market conditions and crime levels are persistent over time, we solve this issue by including county fixed effects in the analysis. The only remaining worry is that individuals with low crime propensities moved to regions with good conditions *during* our 9-year-sample period before their family members immigrated. In addition, in our setting, the intergenerational correlation in crime between newly arriving ethnic German immigrants and their family members is likely to be zero or small since these families were typically split up a long time ago (Glitz, 2012, p. 183), and therefore raised in very different environments (unlike the fathers and offsprings in Hjalmarsson and Lindquist, 2012).

5.4 Results

We first present results on the impact of immigration on total crime and on various crime categories (Section 5.4.1). We then provide evidence that the allocation of ethnic Germans was not affected by regional crime rates as well as robustness checks of the baseline model (Section 5.4.2). Finally, we assess whether crime effects depend on regional labor market conditions, preexisting crime levels, or on the share of foreigners (Section 5.4.3).

5.4.1 Main Results

Table 5.2 presents the effects of immigration on total crime. Controlling for county and year*state fixed effects, we find that higher inflows of immigrants in a given year raises total crime the following year (Column 1). Adding population size and GDP per capita, the two crucial factors determining the allocating of ethnic German immigrants across counties, does not change the coefficient (Column 2). Given that county fixed effects are included, it is not surprising that the coefficients on population size and GDP per capita are close to zero. Adding unemployment rate, share of foreigners, share of young men, and clear-up rate decreases the coefficient of interest only marginally (Column 3). The coefficients on these control variables are all close to zero.

The specifications assume that the ethnic German inflow rates are exogenous to local crime conditions, conditional on all covariates. However, the effect of immigration on crime is not estimated consistently if there are factors that affect both the inflow rates and crime rates (e.g. labor market conditions that are not perfectly captured by the included controls). The coefficient would

Table 5.2
Effect of Ethnic German Immigrants on Total Crime

	(1)	(2)	(3)	(4)
<i>Dependent variable: log total crime rate_t</i>				
Ethnic German inflow rate _{t-1}	9.303** (4.591)	9.455** (4.599)	8.756** (4.196)	7.324 (6.422)
Log population _{t-1}		0.045 (0.282)	-0.031 (0.306)	-0.086 (0.433)
Log GDP per capita _{t-1}		0.016 (0.061)	-0.010 (0.061)	0.102 (0.086)
Unemployment rate _{t-1}			-0.006 (0.005)	-0.013 (0.010)
Foreign population share _{t-1}			0.003 (0.004)	0.002 (0.003)
Young male population share _{t-1}			-0.021 (0.020)	-0.045 (0.052)
Clear-up rate _{t-1}			-0.002 (0.001)	-0.002* (0.001)
County fixed effects	yes	yes	yes	yes
Year*state fixed effects	yes	yes	yes	yes
County-specific linear time trends	-	-	-	yes
R^2	0.916	0.916	0.916	0.928
Observations	1,687	1,687	1,687	1,687

Notes: Ethnic German inflow rate_{t-1} equals ethnic German inflow in year $t - 1$ over population in year $t - 2$. Robust standard errors in parentheses are two-way clustered at the county and year \times state level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

be underestimated, for example, if states allocated more ethnic German immigrants to counties with falling crime rates.²⁹ Therefore, we add county-specific linear time trends to estimate the effect of immigration on crime that results from deviations of linear time trends. Adding linear time trends reduces the coefficient of interest only slightly, but inflates the standard error by 40 percent (Column 4). However, controlling for region-specific time trends is problematic because time trends likely pick up also treatment effects, and not just preexisting trends (Wolfers, 2006). This is especially problematic in this setting because our sample period does not include any pre-treatment periods, that is, years without any inflow of ethnic German immigrants. For this reason, we consider the specification in Column (3) as the baseline model.

How large is the effect of immigration on crime? For expositional purposes, we compute the effect size for a doubling of the average ethnic German inflow rate, that is, an increase in the

²⁹ This seems, however, not to be the case as inflow rates are completely unrelated to the lagged crime rate (see Table 5.4).

inflow of immigrants of one immigrant per 1,000 inhabitants (or 293 immigrants per county).³⁰ Multiplying the increase in the inflow rate (0.001) with the coefficient of interest yields an increase in total crime by 0.8756 percent, or about 0.9 percent.

To compare this semi-elasticity with the crime elasticities reported in other studies, we need a proxy for the stock of ethnic German immigrants in Germany. The Census 2008 is the first large-scale administrative dataset that allows identifying ethnic German immigrants (and their offsprings) unambiguously. At that time, the stock of ethnic Germans living in Germany was around 3.2 million (Worbs et al., 2013). In the eight states used in the analysis, the share of ethnic German immigrants (including offsprings) in the total population is 5 percent, or 50 ethnic German immigrants per 1,000 residents.³¹ Therefore, an increase in the inflow of one ethnic German immigrant per 1,000 inhabitants (which raises crime by 0.9 percent) increases the stock of ethnic German immigrants by about 2 percent ($= 1/50$). This implies an elasticity for total crime of 0.45 ($= 0.9/2$). Importantly, this elasticity is an upper bound of the true elasticity since the stock of ethnic German immigrants was somewhat lower during the analysis period (1996–2005). Assuming, for example, that the stock of ethnic German immigrants was one percentage point lower (that is, 4 percent) among the population, which is consistent with the average ethnic German inflow rate of 1 percent during our analysis period, the crime elasticity would be 0.36.

Our crime elasticity is considerably larger than those reported in previous studies. Bianchi, Buonanno and Pinotti (2012) estimate a very low elasticity for total crime of about 0.03 for Italy, finding a statistically significant effect only for robberies (of elasticity 1). Bell, Fasani and Machin (2013) find an elasticity for total crime of about 0.16 for asylum seekers in England.

There are two potential explanations why we find much stronger effects. The main explanation concerns the crucial difference between the immigrant groups: Ethnic German immigrants are granted German citizenship immediately upon arrival, whereas the immigrants studied elsewhere typically remain foreigners. This is a crucial difference because immigrants who possess the citizenship of the host country cannot be expelled from the country in case of conviction. This lowers the expected conviction costs which, in turn, increases the crime probability (Becker, 1968; Spenkuch, 2013). The immigrant groups studied in other papers, in contrast, face the potential risk of being deported in case of conviction.

³⁰ Note that this increase is somewhat larger than the standard deviation of the inflow rate (0.0007).

³¹ The number comes from own computations based on population data from the Federal Statistical Office and the stock of ethnic Germans in the eight states documented in Worbs et al. (2013).

A second explanation for the stronger crime impact in our study relates to the period when crime effects are measured. We identify short-run effects on crime in the first two years after immigration. In contrast, other studies estimate crime impacts in the medium to long run. Recent studies, however, show that newly arriving immigrants suffer from occupational downgrading upon arrival, with labor market outcomes gradually improving over time (e.g. Eckstein and Weiss (2004), Dustmann and Preston (2012), and Dustmann, Frattini and Preston (2013)). Because opportunity costs of committing crimes increases with time spent in the destination country, one would expect that immigrants are more criminal in the first years after arrival.

Immigration might increase crime rates for two distinct reasons. First, immigrants commit crimes themselves (direct effect). Second, immigration increases crime because residents respond to the inflow of immigrants (indirect effect). The indirect effect could arise, for example, if labor market outcomes of previous residents worsens due to the inflow of immigrants. Borjas et al. (2010), for example, show that U.S. natives, especially black males, committed more crimes in response to labor market competition with immigrants. This might also be one possible mechanism in our case as Glitz (2012) has shown that ethnic German immigrants displace native Germans in the labor market, with 3.1 resident workers becoming unemployed for every 10 migrants who find a job.

Because ethnic Germans are typically not identifiable in official crime statistics due to their German nationality, we cannot directly assess whether (and how much) the increase in crime is due to a direct or indirect effect. However, a few small-scale evaluations—where the background of suspects or prison inmates has been recorded—indicate that ethnic Germans are partly responsible for the increase in crime themselves. Among inmates in 19 juvenile prisons in West Germany in 1998, for example, ethnic Germans are overrepresented by 100 percent relative to their population share (Pfeiffer and Dworschak, 1999). Official crime statistics of youth (under age 21) for one large state show that 18.6 percent of German suspects of violent crime have ethnic German background (Landeskriminalamt Baden-Württemberg, 2007). For total crime, the fraction of ethnic German suspects is 11.6 percent. Finally, during the 1990s, when large waves of ethnic Germans immigrated, the share of ethnic Germans in juvenile prisons in Baden-Württemberg increased substantially: from 0.5 percent in 1993 to 19.1 percent in 2001 (Walter, 2002). In sum, the small-scale surveys indicate that ethnic Germans are overrepresented among suspects and prison inmates, suggesting that the increase in crime is at least partly due to a direct effect.³²

³² If the entire increase in crime was due to a direct effect, the average newly-immigrating ethnic German would raise total crime by 0.6 crimes (evaluated at the mean of 6,883 crimes per 100,000 inhabitants).

Table 5.3
Effect of Ethnic German Immigrants on Crime Subcategories

	(1)	(2)	(3)
<i>Panel A: log total crime rate_t</i>			
Ethnic German inflow rate _{t-1}	9.303** (4.591)	9.455** (4.599)	8.756** (4.196)
<i>R</i> ²	0.916	0.916	0.916
Observations	1,687	1,687	1,687
<i>Panel B: log burglary crime rate_t</i>			
Ethnic German inflow rate _{t-1}	42.146* (23.259)	53.779*** (20.477)	55.852*** (20.635)
<i>R</i> ²	0.895	0.898	0.898
Observations	1,365	1,365	1,365
<i>Panel C: log property damage crime rate_t</i>			
Ethnic German inflow rate _{t-1}	23.648** (9.935)	27.553*** (9.755)	27.968*** (9.217)
<i>R</i> ²	0.926	0.926	0.927
Observations	1,358	1,358	1,358
<i>Panel D: log battery crime rate_t</i>			
Ethnic German inflow rate _{t-1}	20.228 (13.036)	24.656* (12.715)	26.786** (11.766)
<i>R</i> ²	0.945	0.946	0.946
Observations	1,358	1,358	1,358
<i>Panel E: log drug offense crime rate_t</i>			
Ethnic German inflow rate _{t-1}	9.355 (20.311)	16.683 (20.109)	19.807 (20.215)
<i>R</i> ²	0.831	0.832	0.833
Observations	1,687	1,687	1,687
<i>Panel F: log street crime rate_t</i>			
Ethnic German inflow rate _{t-1}	5.213 (7.950)	5.362 (7.990)	3.303 (7.934)
<i>R</i> ²	0.978	0.978	0.979
Observations	1,358	1,358	1,358
Control variables – Set 1	–	yes	yes
Control variables – Set 2	–	–	yes
County fixed effects	yes	yes	yes
Year*state fixed effects	yes	yes	yes

Notes: Ethnic German inflow rate_{t-1} equals the ethnic German inflow in year $t - 1$ over population in year $t - 2$. *Control variables – Set 1:* log population_{t-1} and log GDP per capita_{t-1}. *Control variables – Set 2:* unemployment rate_{t-1}, foreign population share_{t-1}, young male population share_{t-1}, and crime-specific clear-up rates_{t-1}. Robust standard errors in parentheses are two-way clustered at the county and year \times state level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5.3 reports crime effects for several subcategories. Panel A reproduces the effect on total crime for comparison purposes. Overall, immigration raises crime rates in several categories. Furthermore, almost all effects are larger than the effect on total crime. This is possible because the five subcategories cover only half of all crimes. Based on the most restrictive specification in Column (3), immigration has the largest impact on burglary (Panel B). An increase in the inflow of one immigrant per 1,000 inhabitants, increases the crime rate by 5.59 percent (almost six times larger than for total crime). The coefficient for property damage (Panel C) and battery (Panel D) are three times larger than the effect on total crime. The coefficient on drug offense (Panel E) is also large, but statistically insignificant. For street crime (Panel F), we find a very small (0.3 percent) and statistically insignificant effect.

5.4.2 Robustness Checks

A necessary condition for our identification strategy to be valid is that the allocation of ethnic German immigrants does not depend on regional crime rates. If regions with low crime rates, for example, would systematically receive more ethnic Germans, we would underestimate the true impact of immigrants on crime. To test whether this is an issue, we regress the ethnic German inflow rate in year t on the crime rate in the previous year $t - 1$. Following the baseline model, we condition on county and year*state fixed effects. Throughout various specifications with different sets of control variables, the ethnic German inflow rate in the current year is completely unrelated to the crime level in the previous year (Table 5.4). This finding indicates that ethnic Germans have not been allocated on the basis of the crime level in a region.

The coefficient on population size is negative and statistically significant. This could arise mechanically since the population size in year $t - 1$ also enters the denominator of the dependent variable. Similarly, the negative coefficient on GDP per capita might arise because population in $t - 1$ enters the denominators of both the explanatory and dependent variable. While the unemployment rate is statistically significant, its magnitude is negligible (an increase of 1 percentage point is associated with an increase in the inflow rate of only 1 ethnic German immigrant per 1,000,000 inhabitants, or 0.14 percent of a SD). Finally, note that the compound inflow rate over the previous three years has no predictive power for the current inflow rate when conditioning on county fixed effects.

Next, we show that the baseline results are robust to alternative specifications (Table 5.5). The baseline model is repeated in Column (1) for comparison. As a first robustness check, we weight

Table 5.4
Testing for the Effect of Lagged Crime Rates on Current Ethnic German Inflow Rates

	(1)	(2)	(3)	(4)
<i>Dependent variable: ethnic German inflow rate_t</i>				
Log total crime rate _{t-1}	0.00005 (0.00005)	0.00006 (0.00005)	0.00005 (0.00005)	0.00006 (0.00005)
Log population _{t-1}		-0.0048*** (0.0017)	-0.0045*** (0.0017)	-0.0041*** (0.0015)
Log GDP per capita _{t-1}		0.0012*** (0.0004)	0.0007** (0.0003)	0.0008*** (0.0003)
Unemployment rate _{t-1}			-0.0001*** (0.00003)	-0.0001*** (0.00003)
Foreign population share _{t-1} × 10 ⁻³			0.0004 (0.0108)	-0.0005 (0.0099)
Ethnic German inflow rate of previous 3 years _{t-1}				0.0177 (0.0265)
County fixed effects	yes	yes	yes	yes
Year*state fixed effects	yes	yes	yes	yes
R ²	0.708	0.721	0.727	0.729
Observations	1,660	1,660	1,660	1,660

Notes: Ethnic German inflow rate of previous 3 years_{t-1} = ethnic German inflow rate_{t-1} + ethnic German inflow rate_{t-2} + ethnic German inflow rate_{t-3}. Robust standard errors in parentheses are two-way clustered at the county and year × state level. Sample period: 1996–2005. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

each county-year observation with the current population, thus giving more weight to counties with more inhabitants (Column 2). Using population weights does not change the coefficient of interest.

Crime rates within counties are persistent because of unobserved county-specific factors that change slowly over time (e.g., criminal gangs living there). If these unobserved factors also influence the allocation of ethnic German immigrants, this would lead to biased estimates. To control for this potential confound, we include lagged crime rates to the baseline model. Column (3) shows that controlling for the lagged crime rate does not change the results.

Instead of controlling for the lagged crime rate and including county fixed effects, we can also exploit within-county variation in the crime rate by using a first-differenced model:

$$\begin{aligned} \Delta \ln \text{crime rate}_{ist} = & \alpha + \beta \text{ethnic German inflow rate}_{ist-1} \\ & + \Delta \mathbf{X}'_{ist} \boldsymbol{\gamma} + (\boldsymbol{\mu}_t \times \boldsymbol{\mu}_s) + \boldsymbol{\varepsilon}_{ist} \end{aligned} \quad (5.4)$$

The dependent variable is the change in crime rates across two consecutive years. Control variables are also in first differences. This model is more restrictive than the model in Column (3) because the coefficient on the lagged crime rate is restricted to be equal to one. A further disadvantage of the first-differenced model is that the lagged ethnic German inflow has, by construction, an effect on current population, current GDP per capita, and all other control variables as the covariates use either the current or the lagged population in the denominator. Therefore, the differenced control variables are to some extent bad controls since they are measured *after* the ethnic German inflow. With these caveats in mind, the first-differenced model yields a coefficient very similar to the baseline model, though with larger standard errors (Column 4).

Finally, we assess whether results are robust to aggregating inflows of ethnic Germans, rather than exploiting potentially more volatile yearly inflows. First, we aggregate the inflow rates of the previous three years, that is, over the period the assignment to a particular county was binding.³³ This specification mitigates idiosyncratic shocks in the lagged inflow rate. It turns out that the coefficient on the three-year ethnic German inflow rate is very similar to the baseline estimate (Column 5).

Furthermore, we aggregate inflows by using two-year averages of all variables. Specifically, we compute mean crime rates for 1997/1998, 1999/2000, 2001/2002, 2003/2004, and 2005/2006. Similarly, all control variables are averaged in the prior years (with the first observation starting in 1996/1997). This procedure should reduce measurement error in the relationship between the ethnic German inflow and the crime rate because it purges out idiosyncratic shocks to either of the two variables. Column (6) shows that the coefficient of interest increases slightly, suggesting that measurement error might be an issue. Because the number of observations is reduced by half, standard errors increase substantially, rendering the coefficient statistically insignificant.

One worry is that counties with conservative political majorities are strongly opposed to receiving ethnic German immigrants. Accordingly, conservative counties might lobby the state government for reducing the number of ethnic Germans assigned to them. Since counties ultimately decide how police resources are actually used, which might affect crime detection probabilities, the political majority in a county is a potential confounding factor. As we include county fixed effects in the analyses, only changes in political majorities over time are potentially problematic. We address this issue by adding several distinct measures of political majorities in our main model,

³³ The variable is constructed by summing the total ethnic German inflow of the previous three years $((t-1) + (t-2) + (t-3))$ and dividing the sum by population in year $t-4$. Note that using the inflows in the previous three years means that observations of the first two sample years (1997 and 1998) have to be dropped.

including a dummy whether the same party has the majority in a county and the state. The results in Appendix Table A5.3 indicate that the party majority in a county does not affect our baseline estimates at all.

One further issue might be that immigrants commit crimes in other counties than their county of residence, potentially biasing our estimates. We use spatial lags to control for these regional spillovers (see Bianchi, Buonanno and Pinotti, 2012; Gibbons, 2004; Zenou, 2003). Spatial lags are distance-weighted averages of crime rates in neighboring regions, thus allowing for the dependence of crime rates between counties. Specifically, the spatial lag is the log of the sum of the total crime rate in all other counties, weighted by the inverse of the Euclidean geographic distance between county centroids in kilometers (*distance km*) and the travel time distance between county capitals in car minutes (*travel distance min*), respectively. Distance data come from Falck et al. (2012). Each regression controls for the full set of control variables of the baseline specification. Including spatial lags does not change the results (see Table A5.4). The coefficients on both versions of spatial lags are small and statistically insignificant.³⁴ This confirms official statistics on criminal youth in one large German state (Baden-Württemberg), which suggest that ethnic German immigrants are more likely to commit crime in their county of residence than native Germans or foreigners (Landeskriminalamt Baden-Württemberg, 2007, p. 28).³⁵

³⁴ We cannot use crime rates of neighboring counties that did not provide crime rates at the county level. Thus, for computing spatial lags, we are not using counties in Bavaria, Rhineland-Palatinate, Berlin or East Germany. For these regions, we have to impose the assumption that there are no spillover effects. However, we observe county-level crime rates of neighboring counties for the vast majority of counties.

³⁵ In general, crimes in Germany are committed to a large degree in the county of residence. For example, the Federal Criminal Police Office (*Bundeskriminalamt*) reports that about 75 percent of all crimes reported in 2006 were committed in the county of the residence of the suspects (Bundeskriminalamt, 2006, Table 21).

Table 5.5
Alternative Model Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
	baseline	weighted	lag	first-diff	3-yr sum	2-yr avg
<i>Dependent variables:</i>		$\log \text{ total crime rate}_t$		$\Delta \log \text{ total crime rate}_t$	$\log \text{ total crime rate}_t$	$\text{Avg } \log \text{ total crime rate}_t$
Ethnic German inflow rate _{t-1}	8.756** (4.196)	8.518** (4.080)	8.543** (4.347) -0.020 (0.093)	8.202 (5.343)		
Log total crime rate _{t-1}					8.638* (4.740)	
Three-year ethnic German inflow rate _{t-1}						
Two-year avg. ethnic German inflow rate _{t-1}						9.395 (6.454)
Control variables	yes	yes	yes	yes [‡]	yes	yes
County fixed effects	yes	yes	yes	no	yes	yes
Year*state fixed effects	yes	yes	yes	yes	yes	yes
R ²	0.916	0.922	0.916	0.103	0.907	0.958
Observations	1,687	1,687	1,687	1,687	1,369	844

Notes: The dependent variable in Columns (1)–(3) and (5) is the log total crime rate_t. In Column (2), observations are weighted with current population. In Column (4), the dependent variable is in first differences: $\Delta \log \text{ total crime rate}_t = \log \text{ total crime rate}_t - \log \text{ total crime rate}_{t-1}$. [‡]All control variables are in first differences. In Column (6), we average crime rates over two consecutive years (1997/1998, 1999/2000, 2001/2002, 2003/2004, and 2005/2006). Similarly, all control variables are averaged in the prior years (with the first observation starting in 1996/1997), reducing the number of observations by half. Ethnic German inflow rate_{t-1} equals ethnic German inflow in year $t - 1$ over population in year $t - 2$. Three-year ethnic German inflow rate_{t-1} equals ethnic German inflow in year $(t - 1) + (t - 2) + (t - 3)$ over population in year $t - 4$. Two-year avg. ethnic German inflow rate_{t-1} equals the simple average of the ethnic German inflow rate of two years. *Control variables:* log population_{t-1}, log GDP per capita_{t-1}, unemployment rate_{t-1}, foreign population share_{t-1}, young male population share_{t-1}, and clear-up rate_{t-1}. Robust standard errors in parentheses are two-way clustered at the county and year \times state level. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1.

5.4.3 Effect Heterogeneity

So far, we have looked at the *average* impact of immigration on crime. Given that the allocation of ethnic German immigrants across counties was likely homogenous with respect to individual characteristics (see Section 5.1), heterogeneous crime effects across counties can therefore be attributed to differences between counties rather than to differences between ethnic German immigrants. We exploit the uniform distribution of immigrants across counties to assess whether the impact of immigrants on crime depends on the characteristics of the region.

We first investigate the importance of local labor market conditions. According to the economic model of crime, one would expect that the effect of immigrants on crime is smaller in regions with better labor market conditions because better legitimate employment opportunities increase the opportunity costs of crime.

First, we assess whether the effect of immigration on crime differs between regions with very low unemployment and other regions. For this analysis, we characterize counties by quartiles in the county-level unemployment rate distribution separately for each year. Counties with an unemployment rate below the 25th percentile in year $t - 1$, for example, are indicated by $I(\text{unemployment rate}_{t-1} \leq 25^{\text{th}} \text{ percentile})$.

Panel A in Table 5.6 indicates that counties with low levels of unemployment (below 25th percentile) experience no crime increase after immigrant inflows (Column 1), with the coefficient being close to zero. In contrast, the crime impact of immigrants for counties with high unemployment (above 75th percentile) is 1.5 percent (sum of the coefficients 6.1 + 9.3). This effect is almost twice as large as the average effect on crime (0.9 percent). These results indicate that immigration does not increase crime in regions with low unemployment levels, but increases crime considerably in regions with high unemployment.³⁶

The crime-enhancing effect of bad labor market conditions is consistent with the predictions of the economic model of crime (see Becker, 1968; Ehrlich, 1973): crime impacts are stronger in regions where legitimate earnings opportunities are scarce. Furthermore, the results are also in line with empirical studies showing that improvements in regional labor market conditions reduce crime rates. For example, Gould, Weinberg and Mustard (2002) and Raphael and Winter-Ebmer (2001)

³⁶ In further analysis (available upon request), we find no evidence that crime effects differ across counties with different levels of GDP per capita. One interpretation is that GDP per capita is a poor proxy for employment opportunities. Consistent with this interpretation, the cross-county correlation between GDP per capita and unemployment is very low in our sample, likely because unemployment is much more volatile than GDP per capita.

provide evidence for the U.S. that lower unemployment rates in local labor markets decrease the incidence of crimes, especially among less educated men.³⁷

To assess whether the crime impact is stronger in regions with preexisting high crime levels, we interact the immigration inflow with an indicator for whether the region has low or high crime levels. Column (1) in Panel B suggests that the crime impact of immigration is small in counties with low crime levels. Similarly, we find strong crime effects in regions with very high crime levels (Column 3).

The stronger crime impacts in regions with higher crime levels is consistent with social interactions. Existing research indicates that social interactions is an important explanation for criminal behavior. Damm and Dustmann (2014), for example, find that early exposure to neighborhood crime, as measured by the share of young people convicted for crimes, increases convictions of male adolescents. Similarly, juvenile offenders who serve time in the same correctional facility affect each other's criminal activities upon release (Bayer, Hjalmarsson and Pozen, 2009). Glaeser, Sacerdote and Scheinkman (1996) provide evidence that the large cross-city variance in crime rates in the U.S. can likely not be explained by differences in costs and benefits of crime, but that social interactions are considerable, especially for petty and moderate crimes. Furthermore, Zenou (2003) shows that individuals are more likely to commit crimes if their peers are criminal. Given this existing evidence, interactions between newly arriving ethnic Germans and criminal residents might explain why crime effects are stronger in regions with higher preexisting crime levels.

Finally, we investigate whether crime effects differ across regions with different shares of foreigners. This analysis is motivated by the observation that foreigners are more likely to commit crimes than natives (Bundeskriminalamt, 2006).³⁸ The pattern is similar to that of preexisting crime levels (Panel C): crime effects are much larger in regions with larger shares of foreigners. Given that foreigners are on average more criminal than natives, social interactions might also explain this pattern.

³⁷ Furthermore, Oester and Agell (2007) find for Sweden that a decrease of municipality-level unemployment decreases crime rates. Fougère, Kramarz and Pouget (2009) provide region-level evidence for France that increases in youth unemployment induce increases in crime.

³⁸ Although crime rates of natives are not directly comparable to those of foreigners (Bundeskriminalamt, 2006, p. 76), foreigners are highly overrepresented among suspects.

Table 5.6
Effect Heterogeneity

	(1)	(2)	(3)
<i>Dependent variable: log total crime rate_t</i>			
<i>Panel A: Interaction with unemployment rate_{t-1}</i>			
Ethnic German inflow rate _{t-1}	11.369** (4.479)	3.529 (5.838)	6.110 (4.911)
Inflow rate × I(unemployment rate _{t-1} ≤ 25 th percentile)	-12.393* (6.370)		
Inflow rate × I(unemployment rate _{t-1} ≥ 50 th percentile)		9.243* (5.119)	
Inflow rate × I(unemployment rate _{t-1} ≥ 75 th percentile)			9.303 (7.056)
Effect for county groups	-1.024 (7.109)	12.772*** (4.573)	15.413** (6.515)
<i>Panel B: Interaction with log total crime rate_{t-1}</i>			
Ethnic German inflow rate _{t-1}	9.868** (4.346)	7.238 (5.003)	6.513 (4.432)
Inflow rate × I(log total crime rate _{t-1} ≤ 25 th percentile)	-4.637 (8.896)		
Inflow rate × I(log total crime rate _{t-1} ≥ 50 th percentile)		3.392 (4.824)	
Inflow rate × I(log total crime rate _{t-1} ≥ 75 th percentile)			16.410** (8.076)
Effect for county groups	5.231 (8.604)	10.629** (4.742)	22.922*** (8.136)
<i>Panel C: Interaction with foreign population share_{t-1}</i>			
Ethnic German inflow rate _{t-1}	10.854** (4.491)	9.502* (4.914)	7.504* (4.227)
Inflow rate × I(foreigner share _{t-1} ≤ 25 th percentile)	-7.256 (10.286)		
Inflow rate × I(foreigner share _{t-1} ≥ 50 th percentile)		-2.491 (8.998)	
Inflow rate × I(foreigner share _{t-1} ≥ 75 th percentile)			14.033 (12.152)
Effect for county groups	3.598 (9.236)	7.010 (7.692)	21.537* (12.163)
Control variables	yes	yes	yes
County fixed effects	yes	yes	yes
Year*state fixed effects	yes	yes	yes

Notes: Ethnic German inflow rate_{t-1} equals the ethnic German inflow in year $t - 1$ over population in year $t - 2$. *Inflow rate* denotes ethnic German inflow rate_{t-1}. *I*(·) denotes an indicator variable. *Control variables:* log GDP per capita_{t-1}, unemployment rate_{t-1}, log population_{t-1}, foreign population share_{t-1}, young male population share_{t-1}, and crime-specific clear-up rates_{t-1}. R^2 (0.916) and number of observations (1,687) are the same in each model. Robust standard errors in parentheses are two-way clustered at the county and year × state level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

5.5 Conclusion

The criminal behavior of immigrants is a huge concern among residents in many industrialized countries. Despite this fact, little is known about the impact of immigration on crime. This chapter aims at contributing to this literature, with a focus on the importance of regional labor market conditions.

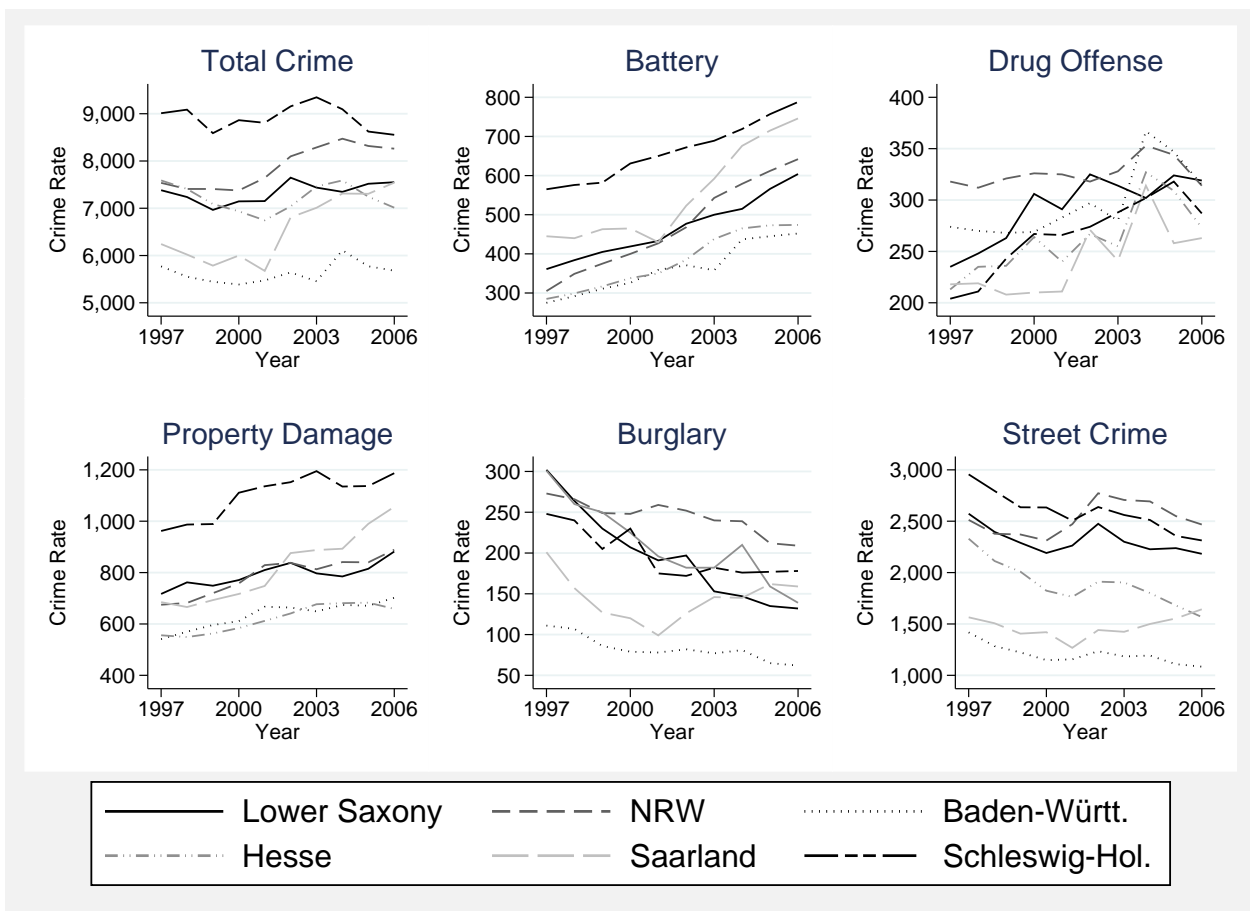
We investigate the crime effect of ethnic German immigrants who were allocated across regions by German authorities upon arrival. Because this allocation was based on the presence of family members, and noncompliance was strongly sanctioned, the possibility of self-selection into regions was severely limited. Therefore, inflows into regions were likely not driven by local crime or labor market conditions, thus providing a unique quasi-experimental setting for studying the effects of immigration on crime.

We find that the inflow of ethnic German immigrants strongly increased crime rates. Importantly, we find that the crime impact depends heavily on the labor market condition in a region: in regions with low unemployment, immigration does not increase crime. In contrast, in regions with high unemployment, immigration raises crime rates substantially. The crime-enhancing effect of bad labor market conditions is consistent both with the economic theory of crime and with empirical studies finding that improvements in local labor market conditions reduce crime. Furthermore, the impact of immigration on crime seems particularly strong in regions with high preexisting crime levels and in regions with large foreigner shares – findings that point toward social interactions.

Existing studies tend to find zero or small effects of immigration on crime. In contrast, this study finds large effects, suggesting that characteristics of immigrant groups are crucial for crime effects. In particular, immigrants in this study were not threatened by deportation in case of conviction. Furthermore, the findings suggest that poorly educated migrants with deficiencies in the host country language are particularly vulnerable in economically disadvantaged regions. In sum, our study suggests that crime effects across different countries should be compared carefully since both labor market conditions and immigrant characteristics (for example, the possibility of being expelled from the country) might differ substantially. Furthermore, a successful integration of immigrants immediately after arrival into the labor market seems crucial for crime prevention.

Appendix

Figure A5.1
Crime Trends by State



Notes: The figure shows crime rates per 100,000 residents by crime category and state. *Battery* includes only grievous and serious bodily harm and aggravated battery. The city states Hamburg and Bremen are excluded from the figure for expositional reasons because both states have much higher crime levels. Source: Federal Criminal Office, Police Criminal Statistics (*Polizeiliche Kriminalstatistik*), 1997–2006.

Table A5.1
Determinants of Ethnic German Immigrant Inflows into Counties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Dependent variable: ethnic German inflow_t</i>							
Log population _{t-1}	306.33*** (32.54)				329.12*** (35.29)		112.61*** (28.95)
Log GDP per capita _{t-1}		102.81 (69.47)			57.99 (43.47)		11.38 (18.06)
Unemployment rate _{t-1}			-6.28 (6.51)		0.64 (3.42)		0.36 (1.60)
Foreign population share _{t-1}				13.03** (5.18)	-9.47*** (3.31)		-1.57 (1.33)
Ethnic German inflow of previous 3 years _{t-1}						0.22*** (0.01)	0.17*** (0.02)
Year fixed effects	yes	yes	yes	yes	yes	yes	yes
R ²	0.618	0.147	0.140	0.177	0.630	0.767	0.795
Observations	1,660	1,660	1,660	1,660	1,660	1,660	1,660

Notes: *Ethnic German inflow* is the number of ethnic German immigrants allocated to a county. Robust standard errors in parentheses are two-way clustered at the county and state \times year level. Sample period: 1996–2005. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5.2
Analysis Sample

Federal state	Counties	Years	County–year observations by crime category					
			(1)	(2)	(3)	(4)	(5)	(6)
Baden-Württemberg	44	1997-2006	440	440	440	440	440	440
Bremen ^a	1	1997-2006	10	3	10	3	10	3
Hamburg ^b	1	1999-2006	7	7	7	7	7	7
Hesse ^c	26	2003-2006	104	104	104	104	104	104
Lower Saxony ^d	46	1997-2006	456	456	456	456	456	456
North Rhine-Westphalia ^e	46	1997-2006	460	138	460	138	138	138
Saarland	6	1997-2006	60	60	60	60	60	60
Schleswig-Holstein	15	1997-2006	150	150	150	150	150	150
Total	185		1,687	1,358	1,687	1,358	1,365	1,358

Notes: Crime categories: (1) total crime, (2) battery, (3) drug, (4) property damage, (5) burglary, and (6) street crime. ^aBremen consists of two counties, but crime data are available only at the state level. Crime rates for (2), (4), and (6) are available only for years 2003–2006. ^bCrime data are available only for years 1999–2006. ^cAllocation of ethnic Germans started only in 2002. ^dEthnic German inflow data not available for city of Hannover for 2003–2006. ^eCrime rates for (2), (4), (5), and (6) are available only for years 2003–2006.

Table A5.3
Effect of Ethnic German Immigrants on Total Crime
with County-Specific Party Majorities

	(1)	(2)	(3)	(4)
<i>Dependent variable: log total crime rate_t</i>				
Relative ethnic German inflow _{t-1}	8.756** (4.196)	8.850** (4.269)	8.661** (4.281)	8.969** (4.177)
Social Democrats simple majority _{t-1}		-0.014 (0.012)	-0.015 (0.012)	
Different simple majority in state and county _{t-1}			0.008 (0.010)	
Conservatives/Liberals absolute majority _{t-1}				0.004 (0.013)
Social Democrats/Greens absolute majority _{t-1}				-0.016 (0.020)
Control variables	yes	yes	yes	yes
County fixed effects	yes	yes	yes	yes
Year*state fixed effects	yes	yes	yes	yes
R ²	0.916	0.916	0.916	0.916
Observations	1,687	1,687	1,687	1,687

Notes: Ethnic German inflow rate_{t-1} equals ethnic German inflow in year $t - 1$ over population in year $t - 2$. *Control variables:* log population_{t-1}, log GDP per capita_{t-1}, unemployment rate_{t-1}, foreign population share_{t-1}, young male population share_{t-1}, and clear-up rate_{t-1}. *Social Democrats simple majority_{t-1}* equals 1 if the Social Democratic Party (SPD) has the largest share of votes; 0 otherwise. *Conservatives/Liberals absolute majority_{t-1}* equals 1 if the conservative party, the Christian Democratic Union (CDU), and the liberal party, the Free Democratic Party (FDP), together have the majority of votes; 0 otherwise. *Social Democrats/Greens absolute majority_{t-1}* equals 1 if the Social Democratic Party (SPD) and the Green party together have the majority of votes; 0 otherwise. *Different majority in state and county_{t-1}* equals 1 if the party with the largest share of votes in the county is different than the party with the largest share of votes in the state; 0 otherwise. Robust standard errors in parentheses are two-way clustered at the county and year \times state level. Data on party votes come from the Federal Statistical Office and the State Statistical Offices (*Regionalstatistik*). Shares of votes refer to second votes in the regional elections and in the state elections, respectively. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5.4
Including Spatially Lagged Crime Rates

	(1)	(2)	(3)
<i>Dependent variable: log total crime rate_t</i>			
Ethnic German inflow rate _{t-1}	8.756** (4.196)	9.269** (4.709)	10.584** (5.266)
Spatial lag (distance in kilometers)		-0.381 (0.813)	
Spatial lag (travel distance in minutes)			-2.417 (1.665)
Control variables	yes	yes	yes
County fixed effects	yes	yes	yes
Year*state fixed effects	yes	yes	yes
<i>R</i> ²	0.916	0.916	0.917
Observations	1,687	1,687	1,687

Notes: The spatial lag is the log of the sum of the total crime rate in all other counties, weighted by the inverse of the Euclidean geographic distance between county centroids in kilometers or the travel time distance between county capitals in car minutes. Data come from Falck et al. (2012). *Control variables:* log population_{t-1}, log GDP per capita_{t-1}, unemployment rate_{t-1}, foreign population share_{t-1}, young male population share_{t-1}, and clear-up rate for total crime_{t-1}. Ethnic German inflow rate_{t-1} equals the ethnic German inflow in year $t - 1$ over population in year $t - 2$. Robust standard errors in parentheses are two-way clustered at the county and state \times year level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Chapter 6

Summary and Discussion

Given a large increase in the migrant population of many developed countries, it is important to know both how migrants integrate into the host countries and how immigration affects natives. This thesis contains several studies that contribute to our understanding of the performance of migrants in the host society and the impact of migrants on the citizens of these host societies. Because every study included in this thesis finds that selective migration has consequences for the impacts of migration, the thesis also touches upon mechanisms that explain and evaluate the importance of migrant selectivity.

Chapter 1 covers three studies, the joint work of Eric A. Hanushek, Tim Krieger, Nicolai Netz, Matthias Parey, Laura Renner, Fabian Waldinger, and Ludger Woessmann and myself, that deal with explanations for migrant selection and the importance of migrant selection to labor force quality. The chapter also includes a discussion of empirical methods in migration economics and provides an outline of the thesis. Chapter 2, coauthored by Oliver Falck and Alfred Lameli, studies the importance of non-monetary migration costs by estimating the wage premium an internal migrant in Germany demands when moving from a familiar to an unfamiliar environment. Chapter 3, written with Guido Schwerdt, focuses on whether early ability sorting of children into different school types, that is, early educational tracking, affects the educational achievement of migrants more than that of natives. Chapters 4 and 5 deal with the consequences of migration for the host society. In Chapter 4, I examine the effect of immigration on the intergenerational income mobility of U.S. citizens and Chapter 5, which is joint work with Marc Piopiunik, looks at the effect of immigration on crime in Germany. This chapter provides a nontechnical summary of the findings of the studies included in the thesis and discusses their implications for public policy.

Given the findings from the projects in this thesis, the skill content of the migrant flows and stocks is important in regard to migrants' effects on natives and their integration into the host society. This thesis started by arguing that migration is not random. People have specific motives for moving to specific regions. Understanding the process of selective migration is key to understanding and interpreting the effects of migration on host societies. In economics, the migration decision process is modeled as a cost-benefit analysis in which people migrate if and only if the benefits are larger than the costs. Assuming the existence of abstract individual utility functions, we can capture pecuniary as well as non-pecuniary factors in the model, both of which affect the migration decision.

In Chapter 2, we investigate whether psychic migration costs, for example, leaving friends and family or coping with different cultural traits and habits in the destination, exist and whether they can be expressed in monetary units. Quantifying migration costs is important because labor immobility is associated with the welfare loss of non-efficient allocation of labor. Welfare losses might also occur given that worker mobility is a precondition for cluster formation and the realization of agglomeration economics. Worker mobility also leads to the rapid dissemination and cross-fertilization of ideas, eventually resulting in innovation. Disentangling cultural costs from other costs of migration is thereby important from a policy perspective because policy can influence travel costs, for example, by providing adequate infrastructure, but it can hardly influence deeply rooted cultural biases.

In the chapter, we measure cultural differences between German counties by using historical dialect data from the 19th century. Falck and coauthors already demonstrated that differences in the dialect distance can predict current migration flows between counties. In this study, we merge the dialect distance matrix with administrative social security panel data to calculate the wage premium that a migrant is willing to sacrifice in order to move to a culturally more familiar region. We mitigate the bias from selective migration by conditioning on quarterly pre-migration wage profiles. We find that a migrant is willing to forgo about 1 percent of the migration-induced increase in gross wages to move to a county that is one standard deviation closer to the former home county, a economically sizeable effect. The effect is long-lasting and more pronounced for men. Theoretically, we explain our findings within a framework of search and matching labor market models. Cultural barriers to migration can be interpreted as labor market frictions and individuals only search for jobs within area that are culturally similar to their own. The reduced choice set is responsible for a less than optimal labor market outcome. However, individuals' welfare might not suffer from this search behavior because they are compensated by the higher cultural familiarity

of the destination region. Our results also imply that analyses of returns to migration that do not consider these psychic costs of migration overestimate the rate of return to monetary resources invested in migration.

In many countries, migrants are failing to economically integrate and migrant children lag behind native children in terms of educational achievement. Therefore, Chapter 3 focuses on the school system as a tool for migrant integration. The question is whether early educational tracking affects migrants more than it does natives. We do not find that early tracking per se has different effects on migrants and natives. Contrary to conventional cross-country analyses, our approach enables us to exclude time-invariant country-specific explanations, which, for example, could include the dominance of different migrant populations in different destination countries. However, by digging deeper into the data, we find that the least integrated families, that is, families that do not speak the host country's language at home, are negatively affected by early tracking. The lesson to be learned from this study is that tracking at later stages could enhance the educational opportunities of children from less integrated families, but that making school systems more comprehensive does not appear to be an effective means of integration.

In Chapter 4, I look at the importance of immigration for intergenerational income mobility of citizens across U.S. cities. This study contributes to the determinants of intergenerational income mobility, and it also contributes to the literature on the labor market effects of migrants. In that sense, it is the first study that causally connects both strands of the literature. I find that immigration has a positive effect on the income mobility of U.S. citizens and that this is mainly driven by children from low-income families performing better. I further document that the parental income is only marginally affected and that the income of children is substantially increased when the children were born in a region with relatively many migrants.

To understand why this is the case, we need to study the literature on the effects of immigration on local employment structure. Whether migrants are beneficial or detrimental for the labor market outcomes of natives depends crucially on the substitutability of migrants versus natives. If we assume that migrants and natives are perfect substitutes, that is, migrants and natives with the same education, age, and experience can do the very same job equally well, then theory and empirics imply worse labor market outcomes for natives. However, the recent literature strongly suggests that migrants and natives are imperfect substitutes because migrants bring skills to the country that are not present in the native population. Moreover, recent studies show that low-skilled natives in the United States react to low-skilled immigration by, for example, specializing

in language-intensive occupations. Migrants themselves specialize based on their comparative advantage in manual-intensive occupations. This labor market adjustment process increases overall productivity and raises wages for everyone. In this chapter, I show that the positive effect of immigration on intergenerational income mobility is fully explained by endogenous educational and occupational adjustment of native children. These children and their families recognize that the children are going to have to compete with migrants on the labor market and thus invest more in education so as to give themselves the opportunity to choose occupations that are more complementary to the educational and occupational structure of the migrant population.

Whether higher intergenerational income mobility is socially desirable is open to debate and depends on its determinants. There is a role for public policy when certain factors outside their control prevent children from moving up in life. Those factors include, for example, the school system, race, region of birth, and parental background (Corak, 2013). Public policy can play less of a role if the barriers to upward social mobility are inherited and distortionary redistributive costs are high (Piketty, 2000). The findings of my study indicate that immigration leads endogenously to higher intergenerational income mobility. Therefore, it also implies that the educational and occupational choices of natives in low immigration areas might very well be optimal. Given less immigration-induced competitive pressure on wages and employment in certain occupations, natives might find it optimal to not invest in more education and/or to switch to other occupations. Thus, from a policy perspective, it is unclear whether there are any welfare effects for individuals. Therefore, if the policy goal is to increase intergenerational income mobility, the literature suggests that increasing the quality of public education, decreasing income and racial segregation, and increasing the provision of local public goods can be effective strategies for achieving this objective. Nevertheless, the study should make us reassess the way we think about immigration, especially as low-skilled immigration is widely viewed as something that takes jobs away from low-skilled natives. This might be true in the short run; however, my study suggests that the children from low-income families actually gain in the medium and long run by adjusting educational and occupational trajectories.

Chapter 5 deals with the crime effects of migrants. This is another important topic in light of the fact that opinion surveys report that people are more worried about immigrants causing crime than about them taking jobs away. The current conventional wisdom in the literature is that the average migrant does not increase crime substantially. But the literature is rather scarce and the different country settings it deals with make the results hardly transferable to other migrant populations and countries. Therefore, Chapter 5 looks at the effects of immigration on crime in the largest

European economy: Germany. We take advantage of the unprecedented inflow of ethnic Germans after the fall of the Iron Curtain. These migrants were the descendants of German colonists who had migrated to Russia and other East European countries in the 18th and 19th centuries. Between 1988 and 2005, more than 3 million ethnic Germans immigrated, increasing Germany's population by about 5 percent. Ethnic German immigrants were granted German citizenship, were allowed to work, and were eligible for social assistance (like German natives) immediately upon arrival. We combine annual county-specific inflows of ethnic German immigrants with annual county-specific crime rates. Furthermore, we add county-specific information on labor market conditions and demographics to assess whether (and, if so, which) regional conditions play a role in the impact of immigration on crime. We focus on West Germany (excluding Berlin) from 1996 until 2005, the period during which newly arriving ethnic German immigrants were allocated across regions by German authorities and sanctioned with withdrawal of all social benefits in case of noncompliance. Importantly, ethnic German immigrants have considerably lower education levels, worse labor market outcomes, and lower incomes than native Germans. We find that this group of immigrants caused substantially higher crime rates in German counties. The effects are mainly driven by ethnic Germans who are allocated to counties with poor preexisting labor market conditions and higher preexisting crime rates. However, because we only have the flow of ethnic Germans, we only identify short-run effects. Assuming that the integration of migrants improves over time, we may overestimate long-run effects. Nevertheless, we learn from this study that migrants can indeed have detrimental effects on crime, which are mediated through regional economic and socioeconomic conditions. The results again call for policies to integrate low-skilled migrants into the economy early on.

All the studies included in this thesis find that the skill composition of migrants has an important influence on the effects of migration not only on the migrants themselves but also on the host societies. Thus, understanding selective migration can contribute to our understanding of the consequences of migration and, moreover, to the discussion on the tightness of immigration laws. But, what drives migrant selectivity? Differences in labor market returns is one factor that has received a great deal of attention in the literature. Pioneering work by George J. Borjas predicts that better skilled migrants will move to countries that have a more unequal earnings distribution compared to that of the sending country. The reason is that the migrant's skills are more highly valued in the other country, which is driving the inequality in the earnings distribution. Less skilled migrants are expected to move to countries that have a more equal earnings distribution than the sending country. The reason here is that the compressed earnings distribution acts as insurance

against low wages. However, due to certain model assumptions, the results from the literature testing this theory are far from conclusive. First, the skill distribution between sending and receiving country has to be comparable. Second, the skill distribution needs to be correlated with the earnings distribution. Third, legal migration restrictions that favor high skilled over low skilled migrants should not play a major role. All of these assumptions are difficult to justify in light of real-world migration from developing to developed countries.

In the paper I wrote with Matthias Parey, Fabian Waldinger, and Nicolai Netz, we use a detailed survey of German university graduates who were surveyed one and five years after their graduation (Parey et al., 2014). The unique feature of the survey is that it tracks graduates even if they go abroad. We document that, if they migrate, the more able graduates move to countries that have a more unequal earnings distribution than Germany and the less able graduates move to countries that have a more equal earnings distribution than Germany. Thus, the findings strongly support the Borjas theory within this sample of high-skilled individuals. On the one hand, this is surprising because of our selected sample. On the other hand, it is not surprising because the assumptions of the Borjas model tend to actually apply in our study: (i) migration flows that we observe are only to other developed countries; (ii) most destinations are within the European Union where no legal migration and work restrictions exist; and (iii) high-skilled individuals should have easier access to labor markets in countries with a selective immigration policy.

The Borjas model above explains migrant selection by different monetary benefits to high- and low-skilled migrants. But monetary benefits are not the only factor that affects migration decisions. To discover whether non-monetary migration costs have any impact on international selective migration, Tim Krieger, Laura Renner, and I conducted a study in which we examined the effect of cultural differences between countries on the skill mix of current migrant populations in major destination countries (Krieger, Renner and Ruhose, 2014). There is a whole body of literature showing that culture and genes develop in the same direction, known as the dual inheritance theory, which justifies using genetic differences between two populations as a proxy for cultural differences. Our findings demonstrate that the migrant skill mix is dominated by low-skilled migrants for country pairs with rather low levels of genetic distance and dominated by high-skilled migrants for country pairs with high levels of genetic distances. These results are in accordance with the prediction that cultural differences are more of a barrier to migration for low-skilled individuals than they are for high-skilled people.

Given that the migrant skill mix varies across countries (and also across regions within countries), it might be asked whether migrant selectivity has any impact on workforce quality and, if so, whether that has any consequences for macroeconomic outcomes. The paper that I co-wrote with Eric A. Hanushek and Ludger Woessmann employs development accounting and growth accounting techniques to find evidence of a connection between human capital and economic development, measured by GDP per capita, within the United States (Hanushek, Ruhose and Woessmann, 2014). For this purpose, we constructed new measures of human capital for each of the U.S. states. Our measures of human capital are composed of a quantity element of human capital (years of completed schooling from Census data) and a quality element of human capital (test scores from student achievement tests). We weight both components based on their labor market returns to obtain an index of the human capital stock for each state. We use this measure to decompose differences in GDP per capita across states according to differences in the stock of human capital. We find that we can attribute almost a third of the differences in GDP per capita to differences in human capital. Importantly, assumptions of migrant selectivity are crucial to this analysis. Abandoning models with random migration for those that allow for selective migration improve the model fit substantially. Thus, selective migration is also important in explaining macroeconomic phenomena.

References

- Acemoglu, Daron, and Joern-Steffen Pischke.** 2003. "Minimum Wages and On-the-job Training." *Research in Labor Economics*, 22: 159–202.
- Ager, Philipp, and Markus Brueckner.** 2013. "Cultural Diversity and Economic Growth: Evidence from the US during the Age of Mass Migration." *European Economic Review*, 64: 76–97.
- Aghion, Philippe, and Peter Howitt.** 1998. *Endogenous Growth Theory*. Cambridge, MA:MIT Press.
- Akresh, Richard, and Ilana Redstone Akresh.** 2010. "Using Achievement Tests to Measure Language Assimilation and Language Bias among the Children of Immigrants." *Journal of Human Resources*, 46(3): 647–667.
- Alesina, Alberto, Reza Baqir, and William Easterly.** 1999. "Public Goods and Ethnic Divisions." *Quarterly Journal of Economics*, 114(4): 1243–1284.
- Algan, Yann, and Pierre Cahuc.** 2010. "Inherited Trust and Growth." *American Economic Review*, 100(5): 2060–2092.
- Algan, Yann, Christian Dustmann, Albrecht Glitz, and Alan Manning.** 2010. "The Economic Situation of First- and Second-Generation Immigrants in France, Germany, and the UK." *Economic Journal*, 120(542): F4–F30.
- Altonji, Joseph G., and David Card.** 1991. "The Effects of Immigration on the Labor Market Outcomes of Less-Skilled Natives." In *Immigration, Trade and the Labor Market*. Edited by John M. Abowd and Richard B. Freeman, Chapter 7, 201–234. Chicago:University of Chicago Press.
- Ammermueller, Andreas.** 2007. "Poor Background or Low Returns? Why Immigrant Students in Germany Perform so Poorly in PISA." *Education Economics*, 15(2): 215–230.
- Amuedo-Dorantes, Catalina, and Sevilla Almudena.** 2014. "Low-Skilled Immigration and Parenting Investments of College-Educated Mothers in the United States: Evidence from Time-Use Data." *Journal of Human Resources*, 49(3): 509–539.
- ANES.** 2014. "American National Election Studies (www.electionstudies.org). The ANES Guide to Public Opinion and Electoral Behavior." Ann Arbor, MI: University of Michigan, Center for Political Studies [producer and distributor].
- Angrist, Joshua D., and Alan B. Krueger.** 1999. "Empirical Strategies in Labor Economics." In *Handbook of Labor Economics*. Vol. 3A, edited by Orley C. Ashenfelter and David Card, Chapter 23, 1277–1366. Amsterdam:Elsevier, North-Holland.
- Angrist, Joshua D., and Joern-Steffen Pischke.** 2009. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton, NJ:Princeton University Press.

- Antecol, Heather, Deborah A. Cobb-Clark, and Stephen J. Trejo.** 2003. "Immigration Policy and the Skills of Immigrants to Australia, Canada, and the United States." *Journal of Human Resources*, 38(1): 192–218.
- Ashenfelter, Orley C.** 1978. "Estimating the Effect of Training Programs on Earnings." *Review of Economics and Statistics*, 60(1): 47–57.
- Ashenfelter, Orley C., and David Card.** 1985. "Using Longitudinal Structure of Earnings to Estimate the Effect of Training Programs." *Review of Economics and Statistics*, 67(4): 648–660.
- Åslund, Olof, Per-Anders Edin, Peter Fredriksson, and Hans Grönqvist.** 2011. "Peers, Neighborhoods, and Immigrant Student Achievement: Evidence from a Placement Policy." *American Economic Journal: Applied Economics*, 3(2): 67–95.
- Audretsch, David, Dirk Dohse, and Annekatrien Niebuhr.** 2009. "Cultural Diversity and Entrepreneurship: A Regional Analysis for Germany." *Annals of Regional Science*, 45(1): 55–85.
- Aydemir, Abdurrahman, and George J. Borjas.** 2007. "Cross-Country Variation in the Impact of International Migration: Canada, Mexico, and the United States." *Journal of the European Economic Association*, 5(4): 663–708.
- Aydemir, Abdurrahman, and George J. Borjas.** 2011. "Attenuation Bias in Measuring the Wage Impact of Immigration." *Journal of Labor Economics*, 29(1): 69–112.
- Bade, Klaus J.** 1990. "Aussiedler - Rückwanderer über Generationen hinweg." In *Neue Heimat im Westen. Vertriebene. Flüchtlinge. Aussiedler*. 128–149. Münster: Westfälischer Heimatbund.
- Bade, Klaus J., and Jochen Oltmer.** 2003. "Aussiedler: deutsche Einwanderer aus Osteuropa." In *Einführung: Aussiedlerzuwanderung und Aussiedlerintegration. Historische Entwicklung und aktuelle Probleme*. 9–51. Göttingen: IMIS-Schriften 8.
- Barrett, Alan, and Irene Mosca.** 2013. "The Psychic Costs of Migration: Evidence from Irish Return Migrants." *Journal of Population Economics*, 26(2): 483–506.
- Bartel, Ann P.** 1989. "Where Do the New U.S. Immigrants Live?" *Journal of Labor Economics*, 7(4): 371–391.
- Batista, Catia, Aitor Lacuesta, and Pedro C. Vicente.** 2012. "Testing the 'Brain Gain' Hypothesis: Micro Evidence from Cape Verde." *Journal of Development Economics*, 97(1): 32–45.
- Bauernschuster, Stefan, Oliver Falck, Stephan Heblich, and Jens Suedekum.** 2014. "Why are Educated and Risk-Loving Persons More Mobile across Regions?" *Journal of Economic Behavior & Organization*, 98: 56–69.
- Bauer, Philipp, and Regina T. Riphahn.** 2006. "Timing of School Tracking as a Determinant of Intergenerational Transmission of Education." *Economics Letters*, 91(1): 90–97.
- Bauer, Philipp, and Regina T. Riphahn.** 2007. "Heterogeneity in the Intergenerational Transmission of Educational Attainment: Evidence from Switzerland on Natives and Second-Generation Immigrants." *Journal of Population Economics*, 20(1): 121–148.
- Baum, Christopher F., Mark E. Schaffer, and Steven Stillman.** 2007. "Enhanced Routines for Instrumental Variables/Generalized Method of Moments Estimation and Testing." *Stata Journal*, 7(4): 465–506.

- Bayer, Patrick, Randi Hjalmarsson, and David Pozen.** 2009. "Building Criminal Capital behind Bars: Peer Effects in Juvenile Corrections." *Quarterly Journal of Economics*, 124(1): 105–147.
- Becker, Gary.** 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy*, 76(2): 169–217.
- Behrman, Jere R., Alejandro Gaviria, and Miguel Szekely.** 2001. "Intergenerational Mobility in Latin America." *Economía*, 2(1): 1–31.
- Beine, Michel, Frédéric Docquier, and Hillel Rapoport.** 2001. "Brain Drain and Economic Growth: Theory and Evidence." *Journal of Development Economics*, 64(1): 275–289.
- Beine, Michel, Frédéric Docquier, and Hillel Rapoport.** 2008. "Brain Drain and Human Capital Formation in Developing Countries: Winners and Losers." *Economic Journal*, 118(528): 631–652.
- Bell, Brian, Francesco Fasani, and Stephen Machin.** 2013. "Crime and Immigration: Evidence from Large Immigrant Waves." *Review of Economics and Statistics*, 95(4): 1278–1290.
- Belot, Michèle, and Sjef Ederveen.** 2011. "Cultural Barriers in Migration between OECD Countries." *Journal of Population Economics*, 25(3): 1077–1105.
- Benhabib, Jess, and Mark M. Spiegel.** 2005. "Human Capital and Technology Diffusion." In *Handbook of Economic Growth*. Vol. 1A, edited by Philippe Aghion and Steven N. Durlauf, Chapter 13, 935–966. Amsterdam:Elsevier, North-Holland.
- Bertrand, Marianne, and Sendhil Mullainathan.** 2004. "Are Emily and Greg More Employable Than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination." *American Economic Review*, 94(4): 991–1013.
- Betts, Julian R., and Robert W. Fairlie.** 2003. "Does Immigration Induce 'Native Flight' from Public Schools into Private Schools?" *Journal of Public Economics*, 87(5-6): 987–1012.
- Bhagwati, Jagdish, and Koichi Hamada.** 1974. "The Brain Drain, International Integration of Markets for Professionals and Unemployment: A Theoretical Analysis." *Journal of Development Economics*, 1(1): 19–42.
- Bianchi, Milo, Paolo Buonanno, and Paolo Pinotti.** 2012. "Do Immigrants Cause Crime?" *Journal of the European Economic Association*, 10(6): 1318–1347.
- Black, Sandra E., and Paul J. Devereux.** 2011. "Recent Developments in Intergenerational Mobility." In *Handbook of Labor Economics*. Vol. 4B, edited by Orley C. Ashenfelter and David Card, Chapter 16, 1487–1541. Amsterdam:Elsevier, North-Holland.
- Bleakley, Hoyt, and Aimee Chin.** 2004. "Language Skills and Earnings: Evidence from Childhood Immigrants." *Review of Economics and Statistics*, 86(2): 481–496.
- Bleakley, Hoyt, and Aimee Chin.** 2010. "Age at Arrival, English Proficiency, and Social Assimilation Among US Immigrants." *American Economic Journal: Applied Economics*, 2(1): 165–192.
- Borjas, George J.** 1985. "Assimilation, Changes in Cohort Quality, and the Earnings of Immigrants." *Journal of Labor Economics*, 3(4): 463–489.
- Borjas, George J.** 1987. "Self-Selection and the Earnings of Immigrants." *American Economic Review*, 77(4): 531–553.

- Borjas, George J.** 1993. "The Intergenerational Mobility of Immigrants." *Journal of Labor Economics*, 11(1): 113–135.
- Borjas, George J.** 1994. "The Economics of Immigration." *Journal of Economic Literature*, 32(4): 1667–1717.
- Borjas, George J.** 1999. "The Economic Analysis of Immigration." In *Handbook of Labor Economics*. Vol. 3A, edited by Orley C. Ashenfelter and David Card, Chapter 28, 1697–1760. Amsterdam:Elsevier, North-Holland.
- Borjas, George J.** 2003. "The Labor Demand Curve Is Downward Sloping: Reexamining the Impact of Immigration on the Labor Market." *Quarterly Journal of Economics*, 118(4): 1335–1374.
- Borjas, George J.** 2013. *Labor Economics*. 6 ed., New York, NY: McGraw-Hill/Irwin.
- Borjas, George J., and Lawrence F. Katz.** 2007. "The Evolution of the Mexican-Born Workforce in the United States." In *Mexican Immigration to the United States*. Edited by George J. Borjas, Chapter 1, 13–55. Chicago:University of Chicago Press.
- Borjas, George J., and Stephen G. Bronars.** 1991. "Immigration and the Family." *Journal of Labor Economics*, 9(2): 123–148.
- Borjas, George J., Stephen G. Bronars, and Stephen J. Trejo.** 1992. "Self-Selection and Internal Migration in the United States." *Journal of Urban Economics*, 32(2): 159–185.
- Braun, Sebastian, and Michael Kvasnicka.** 2014. "Immigration and Structural Change: Evidence from Post-War Germany." *Journal of International Economics*, 93(2): 253–269.
- Brookings.** 2012. "Export Nation 2012: How U.S. Metropolitan Areas Are Driving National Growth." http://www.brookings.edu/about/projects/state-metro-innovation/~link.aspx?_id=D72F7602A7C741D3B028295A344F8FA5&_z=z.
- Bruegger, Beatrix, Rafael Lalive, and Josef Zweimueller.** 2009. "Does Culture Affect Unemployment? Evidence from the *Röstigraben*." *CESifo Working Paper No. 2714*.
- Brunello, Giorgio, and Daniele Checchi.** 2007. "Does School Tracking Affect Equality of Opportunity? New International Evidence." *Economic Policy*, 22(52): 781–861.
- Buettner, Thiess, and Alexander Ebertz.** 2009. "Spatial Implications of Minimum Wages." *Jahrbücher für Nationalökonomie und Statistik*, 229(2-3): 292–312.
- Bundeskriminalamt.** 2006. "Polizeiliche Kriminalstatistik 2006."
- Burchardi, Konrad B., and Tarek A. Hassan.** 2013. "The Economic Impact of Social Ties: Evidence from German Reunification." *Quarterly Journal of Economics*, 128(3): 1219–1271.
- Bureau of Economic Analysis.** 2012. "Regional Economic Accounts." <http://www.bea.gov/regional/>.
- Butcher, Kristin, and Anne Morrison Piehl.** 1998. "Recent Immigrants: Unexpected Implications for Crime and Incarceration." *Industrial and Labor Relations Review*, 51(4): 654–679.
- Butcher, Kristin, and Anne Morrison Piehl.** 2007. "Why Are Immigrants' Incarceration Rates so Low? Evidence on Selective Immigration, Deterrence, and Deportation." *NBER Working Paper No. 13229*.

- Cahuc, Pierre.** 2014. *Labor Economics*. 2 ed., Cambridge, MA: MIT Press.
- Camarota, Steven, and Jessica Vaughan.** 2009. "Immigration and Crime: Assessing a Conflicted Issue." *Washington, DC: Center for Immigration Studies*.
- Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller.** 2011. "Robust Inference with Multiway Clustering." *Journal of Business & Economic Statistics*, 29(2): 238–249.
- Card, David.** 1990. "The Impact of the Mariel Boatlift on the Miami Labor Market." *Industrial and Labor Relations Review*, 43(2): 245–257.
- Card, David.** 1999. "The Causal Effect of Education on Earnings." In *Handbook of Labor Economics*. Vol. 3A, edited by Orley C. Ashenfelter and David Card, Chapter 30, 1801–1863. Amsterdam:Elsevier, North-Holland.
- Card, David.** 2001. "Immigrant Inflows, Native Outflows, and the Local Labor Market Impacts of Higher Immigration." *Journal of Labor Economics*, 19(1): 22–64.
- Card, David.** 2005. "Is the New Immigration Really so Bad?" *Economic Journal*, 115(1): F300–F323.
- Card, David.** 2009. "How Immigration Affects U.S. Cities." In *Making Cities Work: Prospects and Policies for Urban America*. Edited by Robert P. Inman, Chapter 6, 158–200. Princeton:Princeton University Press.
- Card, David, Christian Dustmann, and Ian Preston.** 2012. "Immigration, Wages, and Compositional Amenities." *Journal of the European Economic Association*, 10(1): 78–119.
- Card, David, Joerg Heining, and Patrick Kline.** 2013. "Workplace Heterogeneity and the Rise of West German Wage Inequality." *Quarterly Journal of Economics*, 128(3): 967–1015.
- Carlsson, Magnus, and Dan-Olof Rooth.** 2007. "Evidence of Ethnic Discrimination in the Swedish Labor Market using Experimental Data." *Labour Economics*, 14(4): 716–729.
- Cascio, Elizabeth U., and Ethan G. Lewis.** 2012. "Cracks in the Melting Pot: Immigration, School Choice, and Segregation." *American Economic Journal: Economic Policy*, 4(3): 91–117.
- Chalfin, Aaron.** 2013. "What is the Contribution of Mexican Immigration to U.S. Crime Rates? Evidence from Rainfall Shocks in Mexico." *American Law and Economics Review*, 16(1): 220–268.
- Chetty, Raj, Nathaniel Hendren, Patrick Kline, and Emmanuel Saez.** 2014. "Where is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States." *Quarterly Journal of Economics*, 129(4).
- Chiquiar, Daniel, and Gordon H. Hanson.** 2005. "International Migration, Self-Selection, and the Distribution of Wages: Evidence from Mexico and the United States." *Journal of Political Economy*, 113(2): 239–281.
- Chiswick, Barry R.** 1978. "The Effect of Americanization on the Earnings of Foreign-Born Men." *Journal of Political Economy*, 86(5): 897–921.
- Chiswick, Barry R.** 1999. "Are Immigrants Favorably Self-Selected?" *American Economic Review*, 89(2): 181–185.
- Chiswick, Barry R., and Paul W. Miller.** 1995. "The Endogeneity between Language and Earnings: International Analyses." *Journal of Labor Economics*, 13(2): 246–288.

- Clemens, Michael A.** 2011. "Economics and Emigration: Trillion-Dollar Bills on the Sidewalk?" *Journal of Economic Perspectives*, 25(3): 83–106.
- Cobb-Clark, Deborah A., Mathias Sinning, and Steven Stillman.** 2012. "Migrant Youths' Educational Achievement: The Role of Institutions." *ANNALS of the American Academy of Political and Social Science*, 643(1): 18–45.
- Comin, Diego A., Mikhail Dmitriev, and Esteban Rossi-Hansberg.** 2012. "The Spatial Diffusion of Technology." *NBER Working Paper No. 18534*.
- Commander, Simon, Mari Kangasniemi, and L. Alan Winters.** 2004. "The Brain Drain: Curse or Boon? A Survey of the Literature." In *Challenges to Globalization*. Edited by Robert E. Baldwin and L. Alan Winters, Chapter 7, 235–278. Chicago:University of Chicago Press.
- Common Core of Data.** 1990a. "Local Education Agency (School District) Finance Survey (F-33) Data." <https://nces.ed.gov/ccd/f33agency.asp>.
- Common Core of Data.** 1990b. "Local Education Agency (School District) Universe Survey Data." <https://nces.ed.gov/ccd/pubagency.asp>.
- Corak, Miles.** 2013. "Income Inequality, Equality of Opportunity, and Intergenerational Mobility." *Journal of Economic Perspectives*, 27(3): 79–102.
- Cortes, Patricia.** 2008. "The Effect of Low-Skilled Immigration on US Prices: Evidence from CPI Data." *Journal of Political Economy*, 116(3): 381–422.
- Dahlberg, Matz, Karin Edmark, and Helene Lundqvist.** 2012. "Ethnic Diversity and Preferences for Redistribution." *Journal of Political Economy*, 120(1): 41–76.
- Dahl, Gordon B.** 2002. "Mobility and the Return to Education: Testing a Roy Model with Multiple Markets." *Econometrica*, 70(6): 2367–2420.
- Dahl, Michael S., and Olav Sorenson.** 2010. "The Migration of Technical Workers." *Journal of Urban Economics*, 67(1): 33–45.
- Dahl, Molly W., and Thomas DeLeire.** 2008. "The Association between Children's Earnings and Fathers' Lifetime Earnings: Estimates Using Administrative Data." University of Wisconsin-Madison, Institute for Research on Poverty.
- Damm, Anna Piil.** 2009. "Ethnic Enclaves and Immigrant Labor Market Outcomes: Quasi-Experimental Evidence." *Journal of Labor Economics*, 27(2): 281–314.
- Damm, Anna Piil, and Christian Dustmann.** 2014. "Does Growing Up in a High Crime Neighborhood Affect Youth Criminal Behavior?" *American Economic Review*, 104(6): 1806–1832.
- D'Amuri, Francesco, Gianmarco I. P. Ottaviano, and Giovanni Peri.** 2010. "The Labor Market Impact of Immigration in Western Germany in the 1990s." *European Economic Review*, 54(4): 550–570.
- Danzer, Alexander M., and Firat Yaman.** 2013. "Do Ethnic Enclaves Impede Immigrants' Integration? Evidence from a Quasi-Experimental Social-Interaction Approach." *Review of International Economics*, 21(2): 311–325.
- Dee, Thomas S.** 2005. "A Teacher Like Me: Does Race, Ethnicity, or Gender Matter?" *American Economic Review*, 95(2): 158–165.

- Desmet, Klaus, Ignacio Ortuño Ortín, and Shlomo Weber.** 2009. "Linguistic Diversity and Redistribution." *Journal of the European Economic Association*, 7(6): 1291–1318.
- Détang-Dessendre, Cécile, Carine Drapier, and Hubert Jayet.** 2004. "The Impact of Migration on Wages: Empirical Evidence from French Youth." *Journal of Regional Science*, 44(4): 661–691.
- Dietz, Barbara.** 1999. "Jugendliche Aussiedler in Deutschland: Risiken und Chancen der Integration." In *Aussiedler: Deutsche Einwanderer aus Osteuropa*. Edited by Klaus J. Bade and Jochen Oltmer, 153–176. Göttingen:IMIS-Schriften 8.
- Docquier, Frédéric, B. Lindsay Lowell, and Abdeslam Marfouk.** 2007. "A Gendered Assessment of the Brain Drain." <http://perso.uclouvain.be/frederic.docquier/oxlight.htm>.
- Duranton, Gilles.** 2011. "California Dreamin': The Feeble Case for Cluster Policies." *Review of Economic Analysis*, 3(1): 3–45.
- Dustmann, Christian, Albrecht Glitz, and Tommaso Frattini.** 2008. "The Labour Market Impact of Immigration." *Oxford Review of Economic Policy*, 24(3): 477–494.
- Dustmann, Christian, and Albrecht Glitz.** 2011. "Migration and Education." In *Handbook of the Economics of Education*. Vol. 4, edited by Eric A. Hanushek and Stephen Machin and Ludger Woessmann, Chapter 4, 327–439. Amsterdam:Elsevier, North-Holland.
- Dustmann, Christian, and Albrecht Glitz.** 2014. "How Do Industries and Firms Respond to Changes in Local Labor Supply?" *Journal of Labor Economics*, forthcoming.
- Dustmann, Christian, and Ian Preston.** 2012. "Comment: Estimating the Effect of Immigration on Wages." *Journal of the European Economic Association*, 10(1): 216–223.
- Dustmann, Christian, Johannes Ludsteck, and Uta Schoenberg.** 2009. "Revisiting the German Wage Structure." *Quarterly Journal of Economics*, 124(2): 843–881.
- Dustmann, Christian, Stephen Machin, and Uta Schoenberg.** 2010. "Ethnicity and Educational Achievement in Compulsory Schooling." *Economic Journal*, 120(546): F272–F297.
- Dustmann, Christian, Tommaso Frattini, and Gianandrea Lanzar.** 2012. "Educational Achievement of Second Generation Immigrants: An International Comparison." *Economic Policy*, 27(69): 143–185.
- Dustmann, Christian, Tommaso Frattini, and Ian Preston.** 2013. "The Effect of Immigration along the Distribution of Wages." *Review of Economic Studies*, 80(1): 145–173.
- Eckstein, Zvi, and Yoram Weiss.** 2004. "On the Wage Growth of Immigrants: Israel, 1990-2000." *Journal of the European Economic Association*, 2(4): 665–695.
- Edin, Per-Anders, Peter Fredriksson, and Olof Åslund.** 2003. "Ethnic Enclaves and the Economic Success of Immigrants - Evidence from a Natural Experiment." *Quarterly Journal of Economics*, 118(1): 329–357.
- Ehrlich, Isaac.** 1973. "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation." *Journal of Political Economy*, 81(3): 521–565.
- Ehrlich, Isaac.** 1996. "Crime, Punishment, and the Market for Offenses." *Journal of Economic Perspectives*, 10(1): 43–67.
- Elsner, Benjamin.** 2013. "Emigration and Wages: The EU Enlargement Experiment." *Journal of International Economics*, 91(1): 154–163.

- Falck, Oliver, Stephan Heblich, Alfred Lameli, and Jens Suedekum.** 2012. "Dialects, Cultural Identity, and Economic Exchange." *Journal of Urban Economics*, 72(2-3): 225–239.
- Federal Bureau of Investigation.** 2010. "Crime in the United States by Metropolitan Statistical Area." <http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/tables/table-6>.
- Federal Statistical Office.** 2013. "Bevölkerung und Erwerbstätigkeit, Fachserie 1, Reihen 1.2 und 1.3."
- Federal Statistical Office.** 2014. "Consumer Price Indices." <https://www.destatis.de/EN/FactsFigures/Indicators/ShortTermIndicators/Prices/pre110.html>.
- Felbermayr, Gabriel J., and Farid Toubal.** 2010. "Cultural Proximity and Trade." *European Economic Review*, 54(2): 279–293.
- Fernández-Huertas Moraga, Jesús.** 2011. "New Evidence on Emigrant Selection." *Review of Economics and Statistics*, 93(1): 72–96.
- Fertig, Michael, and Christoph M. Schmidt.** 2001. "First- and Second-Generation Migrants in Germany - What Do We Know and What Do People Think." *IZA Discussion Paper No. 286*.
- Fitzgerald, Jennifer, K. Amber Curtis, and Catherine L. Corliss.** 2012. "Anxious Publics: Worries About Crime and Immigration." *Comparative Political Studies*, 45(4): 477–506.
- Fougère, Denis, Francis Kramarz, and Julien Pouget.** 2009. "Youth Unemployment and Crime in France." *Journal of the European Economic Association*, 7(5): 909–938.
- Freeman, Richard.** 1999. "The Economics of Crime." In *Handbook of Labor Economics*. Vol. 3C, edited by Orley C. Ashenfelter and David Card, Chapter 52, 3529–3571. Amsterdam:Elsevier, North-Holland.
- Friedberg, Rachel, and Jennifer Hunt.** 1995. "The Impact of Immigrants on Host Country Wages, Employment, and Growth." *Journal of Economic Perspectives*, 9(2): 23–44.
- Friedberg, Rachel M.** 2001. "The Impact of Mass Migration on the Israeli Labor Market." *Quarterly Journal of Economics*, 116(4): 1373–1408.
- Furtado, Delia, and Heinrich Hock.** 2010. "Low Skilled Immigration and Work-Fertility Tradeoffs among High Skilled US Natives." *American Economic Review*, 100(2): 224–228.
- Gennaioli, Nicola, Rafael La Porta, Florencio Lopez-de Silanes, and Andrei Shleifer.** 2013. "Human Capital and Regional Development." *Quarterly Journal of Economics*, 128(1): 105–164.
- Gibbons, Steve.** 2004. "The Costs of Urban Property Crime." *Economic Journal*, 114(499): F441–F463.
- Glaeser, Edward L., and Bruce Sacerdote.** 1999. "Why Is There More Crime in Cities?" *Journal of Political Economy*, 107(S6): S225–S258.
- Glaeser, Edward L., Bruce Sacerdote, and Jose Scheinkman.** 1996. "Crime and Social Interactions." *Quarterly Journal of Economics*, 111(2): 507–548.
- Glitz, Albrecht.** 2012. "The Labor Market Impact of Immigration: A Quasi-Experiment Exploiting Immigrant Location Rules in Germany." *Journal of Labor Economics*, 30(1): 175–213.

- Gould, Eric D., Bruce A. Weinberg, and David B. Mustard.** 2002. "Crime Rates and Local Labor Market Opportunities in the United States: 1979-1997." *Review of Economics and Statistics*, 84(1): 45-61.
- Gould, Eric D., Victor Lavy, and M. Daniele Paserman.** 2009. "Does Immigration Affect the Long-Term Educational Outcomes of Natives? Quasi-Experimental Evidence." *Economic Journal*, 119(540): 1243-1269.
- Greenwood, Michael J.** 1975. "Research on Internal Migration in the United States: A Survey." *Journal of Economic Literature*, 13(2): 397-433.
- Grieco, Elizabeth M., Edward Trevelyan, Luke Larsen, Yesenia D. Acosta, Christine Gambino, Patricia de la Cruz, Tom Gryn, and Nathan Walters.** 2012. "The Size, Place of Birth, and Geographic Distribution of the Foreign-Born Population in the United States: 1960 to 2010." *U.S. Census Bureau Population Division Working Paper No. 96*.
- Grogger, Jeffrey.** 2011. "Speech Patterns and Racial Wage Inequality." *Journal of Human Resources*, 46(1): 1-25.
- Grogger, Jeffrey, and Gordon H. Hanson.** 2011. "Income Maximization and the Selection and Sorting of International Migrants." *Journal of Development Economics*, 95(1): 42-57.
- Grubel, Herbert B., and Anthony D. Scott.** 1966. "The International Flow of Human Capital." *American Economic Review*, 56(1/2): 268-274.
- Guiso, Luigi, Paola Sapienza, and Luigi Zingales.** 2006. "Does Culture Affect Economic Outcomes?" *Journal of Economic Perspectives*, 20(2): 23-48.
- Guiso, Luigi, Paola Sapienza, and Luigi Zingales.** 2009. "Cultural Biases in Economic Exchange." *Quarterly Journal of Economics*, 124(3): 1095-1131.
- Guiso, Luigi, Paola Sapienza, and Luigi Zingales.** 2011. "Civic Capital as the Missing Link." In *Handbook of Social Economics*. Vol. 1A, edited by Jess Benhabib, Alberto Bisin, and Matthew O. Jackson, Chapter 10, 417-480. Amsterdam:Elsevier, North-Holland.
- Haider, Steven, and Gary Solon.** 2006. "Life-Cycle Variation in the Association between Current and Lifetime Earnings." *American Economic Review*, 96(4): 1308-1320.
- Hanushek, Eric A., and Dennis D. Kimko.** 2000. "Schooling, Labor-Force Quality, and the Growth of Nations." *American Economic Review*, 90(5): 1184-1208.
- Hanushek, Eric A., and Lei Zhang.** 2009. "Quality-Consistent Estimates of International Schooling and Skill Gradients." *Journal of Human Capital*, 3(2): 107-143.
- Hanushek, Eric A., and Ludger Woessmann.** 2006. "Does Educational Tracking Affect Performance and Inequality? Differences-In-Differences Evidence across Countries." *Economic Journal*, 116(510): 63-76.
- Hanushek, Eric A., and Ludger Woessmann.** 2008. "The Role of Cognitive Skills in Economic Development." *Journal of Economic Literature*, 46(3): 607-668.
- Hanushek, Eric A., and Ludger Woessmann.** 2011a. "How Much Do Educational Outcomes Matter in OECD Countries?" *Economic Policy*, 26(67): 427-491.

- Hanushek, Eric A., and Ludger Woessmann.** 2011b. "The Economics of International Differences in Educational Achievement." In *Handbook of the Economics of Education*. Vol. 3, edited by Eric A. Hanushek and Stephen Machin and Ludger Woessmann, Chapter 2, 89–200. Amsterdam:Elsevier, North-Holland.
- Hanushek, Eric A., and Ludger Woessmann.** 2012a. "Do Better Schools Lead to More Growth? Cognitive Skills, Economic Outcomes, and Causation." *Journal of Economic Growth*, 17(4): 497–512.
- Hanushek, Eric A., and Ludger Woessmann.** 2012b. "Schooling, Educational Achievement, and the Latin American Growth Puzzle." *Journal of Development Economics*, 99(2): 497–512.
- Hanushek, Eric A., Guido Schwerdt, Simon Wiederhold, and Ludger Woessmann.** 2014. "Returns to Skills around the World: Evidence from PIAAC." *European Economic Review*, forthcoming.
- Hanushek, Eric A., Jens Ruhose, and Ludger Woessmann.** 2014. "Human Capital and Income Differences across States: Development Accounting for the U.S." *Unpublished Manuscript*.
- Hanushek, Eric A., Marc Piopiunik, and Simon Wiederhold.** 2014. "The Value of Smarter Teachers: International Evidence on Teacher Cognitive Skills and Student Performance." *Munich Discussion Papers No. 2014-51*.
- Hanushek, Eric A., Paul E. Peterson, and Ludger Woessmann.** 2012. "Achievement Growth: International and U.S. State Trends in Student Performance." *Education Next*, 12(4): 24–33.
- Hao, Lingxin, and Suet-Ling Pong.** 2008. "The Role of School in the Upward Mobility of Disadvantaged Immigrants' Children." *Annals of the American Academy of Political and Social Science*, 620(1): 62–89.
- Haque, Nadeem U., and Se-Jik Kim.** 1995. "'Human Capital Flight': Impact of Migration on Income and Growth." *IMF Staff Papers*, 42(3): 577–607.
- Harris, John R., and Michael P. Todaro.** 1970. "Migration, Unemployment and Development: A Two-Sector Analysis." *American Economic Review*, 60(1): 126–142.
- Haug, Sonja.** 2005. "Zum Verlauf des Zweitspracherwerbs im Migrationskontext." *Zeitschrift für Erziehungswissenschaft*, 8(2): 263–284.
- Haug, Sonja, and Lenore Sauer.** 2006. "Zuwanderung und räumliche Verteilung von Aussiedlern und Spätaussiedlern in Deutschland." *Zeitschrift für Bevölkerungswissenschaft*, 31(3-4): 413–442.
- Haug, Sonja, and Lenore Sauer.** 2007. "Zuwanderung und Integration von (Spät-) Aussiedlern - Ermittlung und Bewertung der Auswirkungen des Wohnortzuweisungsgesetzes." Bundesamt für Migration und Flüchtlinge Forschungsbericht 3, Nürnberg.
- Heath, Anthony F., Catherine Rethon, and Elina Kilpi.** 2008. "The Second Generation in Western Europe: Education, Unemployment, and Occupational Attainment." *Annual Review of Sociology*, 34: 211–235.
- Heckman, James J.** 1979. "Sample Selection Bias as a Specification Error." *Econometrica*, 47(1): pp. 153–161.
- Hendricks, Lutz.** 2002. "How Important Is Human Capital for Development? Evidence from Immigrant Earnings." *American Economic Review*, 92(1): 198–219.

- Hjalmarsson, Randi, and Matthew J. Lindquist.** 2012. "Like Godfather, Like Son: Exploring the Intergenerational Nature of Crime." *Journal of Human Resources*, 47(2): 550–582.
- Hornung, Erik.** 2014. "Immigration and the Diffusion of Technology: The Huguenot Diaspora in Prussia." *American Economic Review*, 104(1): 84–122.
- Hunt, Jennifer.** 2012. "The Impact of Immigration on the Educational Attainment of Natives." *NBER Working Paper No. 18047*.
- Hunt, Jennifer, and Marjolaine Gauthier-Loiselle.** 2010. "How Much Does Immigration Boost Innovation?" *American Economic Journal: Macroeconomics*, 2(2): 31–56.
- IG Metall.** 2013. "Collective Wage Agreement in the Manufacturing Sector 2013." <http://www.igmetall.de/SID-49F5A1E2-969B8B98/metall-tarifrunde-2013-bayerisches-ergebnis-in-allen-11891.htm>.
- Jaeger, David A.** 1997. "Reconciling the Old and New Census Bureau Education Questions: Recommendations for Researchers." *Journal of Business & Economic Statistics*, 15(3): 300–309.
- Jaentti, Markus, Knut Røed, Robin Naylor, Anders Björklund, Bernt Bratsberg, Oddbjørn Raaum, Eva Oesterbacka, and Tor Eriksson.** 2006. "American Exceptionalism in a New Light: A Comparison of Intergenerational Earnings Mobility in the Nordic Countries, the United Kingdom and the United States." *IZA Discussion Paper No. 1938*.
- Jaffe, Adam B., Manuel Trajtenberg, and Rebecca Henderson.** 1993. "Geographic Localization of Knowledge Spillovers as Evidenced by Patent Citations." *Quarterly Journal of Economics*, 108(3): 577–598.
- Jones, Benjamin F.** 2014. "The Human Capital Stock: A Generalized Approach." *American Economic Review*, 104(11): 3752–3777.
- Kaas, Leo, and Christian Manger.** 2011. "Ethnic Discrimination in Germany's Labour Market: A Field Experiment." *German Economic Review*, 13(1): 1–20.
- Kaestner, Robert, and Ofer Malamud.** 2014. "Self-Selection and International Migration: New Evidence from Mexico." *Review of Economics and Statistics*, 96(1): 78–91.
- Kerr, Sari Pekkala, Tuomas Pekkari, and Roope Uusitalo.** 2013. "School Tracking and Development of Cognitive Skills." *Journal of Labor Economics*, 31(3): 577–602.
- Kerr, William R., and William F. Lincoln.** 2010. "The Supply Side of Innovation: H-1B Visa Reforms and U.S. Ethnic Invention." *Journal of Labor Economics*, 28(3): 473–508.
- Kim, Seik.** 2013. "Wage Mobility of Foreign-Born Workers in the United States." *Journal of Human Resources*, 48(3): 628–658.
- Kiss, David.** 2013. "Are Immigrants and Girls Graded Worse? Results of a Matching Approach." *Education Economics*, 21(5): 447–463.
- Kleibergen, Frank, and Richard Paap.** 2006. "Generalized Reduced Rank Tests Using the Singular Value Decomposition." *Journal of Econometrics*, 133(1): 97–126.
- Kratz, Fabian, and Josef Bruederl.** 2013. "Returns to Regional Migration: Causal Effect or Selection on Wage Growth?" *Journal of Applied Social Science Studies*, 133(2): 227–238.
- Krieger, Tim, and Jens Ruhose.** 2013. "Honey, I Shrunk the Kids' Benefits-Revisiting Intergenerational Conflict in OECD Countries." *Public Choice*, 157(1-2): 115–143.

- Krieger, Tim, Laura Renner, and Jens Ruhose.** 2014. "Culture, Selection, and International Migration." *Unpublished Manuscript*.
- Kristen, Cornelia, and Nadia Granato.** 2007. "The Educational Attainment of the Second Generation in Germany: Social Origins and Ethnic Inequality." *Ethnicities*, 7(3): 343–366.
- LaLonde, Robert J.** 1986. "Evaluating the Econometric Evaluations of Training Programs with Experimental Data." *American Economic Review*, 76(4): 604–620.
- LaLonde, Robert J., and Robert H. Topel.** 1991. "Immigrants in the American Labor Market: Quality, Assimilation, and Distributional Effects." *American Economic Review*, 81(2): 297–302.
- LaLonde, Robert J., and Robert H. Topel.** 1997. "Economic Impact of International Migration and the Economic Performance of Migrants." In *Handbook of Population and Family Economics*. Vol. 1B, edited by Oded Stark and Mark R. Rosenzweig, Chapter 14, 799–850. Amsterdam:Elsevier, North-Holland.
- Lameli, Alfred.** 2013. *Strukturen im Sprachraum. Analysen zur arealtypologischen Komplexität der Dialekte in Deutschland*. Berlin, Boston:De Gruyter.
- Landeskriminalamt Baden-Württemberg.** 2007. "Jugendkriminalität und Jugendgefährdung in Baden Württemberg." Landeskriminalamt Baden-Württemberg Jahresbericht 2006, Stuttgart.
- Lehmer, Florian, and Johannes Ludsteck.** 2011. "The Returns to Job Mobility and Inter-Regional Migration: Evidence from Germany." *Papers in Regional Science*, 90(3): 549–571.
- Levels, Mark, and Jaap Dronkers.** 2008. "Educational Performance of Native and Immigrant Children from Various Countries of Origin." *Ethnic and Racial Studies*, 31(8): 1404–1425.
- Levitt, Steven.** 1996. "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation." *Quarterly Journal of Economics*, 111(2): 319–351.
- Lewis, Ethan.** 2011. "Immigration, Skill Mix, and Capital Skill Complementarity." *Quarterly Journal of Economics*, 126(2): 1029–1069.
- Lubotsky, Darren.** 2007. "Chutes or Ladders? A Longitudinal Analysis of Immigrant Earnings." *Journal of Political Economy*, 115(5): 820–867.
- Lucas, Robert E.** 1988. "On the Mechanics of Economic Development." *Journal of Monetary Economics*, 22(1): 3–42.
- Luedemann, Elke, and Guido Schwerdt.** 2013. "Migration Background and Educational Tracking: Is There a Double Disadvantage for Second-Generation Immigrants?" *Journal of Population Economics*, 26(2): 455–481.
- Mammey, Ulrich.** 1999. "Segregation, regionale Mobilität und soziale Integration von Aussiedlern." In *Aussiedler: deutsche Einwanderer aus Osteuropa*. Edited by Klaus J. Bade and Jochen Oltmer, 107–126. Göttingen:IMIS-Schriften 8.
- Manacorda, Marco, Alan Manning, and Jonathan Wadsworth.** 2012. "The Impact of Immigration on the Structure of Wages: Theory and Evidence From Britain." *Journal of the European Economic Association*, 10(1): 152–197.
- Mankiw, N. Gregory, David Romer, and David N. Weil.** 1992. "A Contribution to the Empirics of Economic Growth." *Quarterly Journal of Economics*, 107(2): 407–437.

- Manuelli, Rodolfo E., and Ananth Seshadri.** 2014. "Human Capital and the Wealth of Nations." *American Economic Review*, 104(9): 2736–2762.
- Mayda, Anna Maria.** 2009. "International Migration: A Panel Data Analysis of the Determinants of Bilateral Flows." *Journal of Population Economics*, 23(4): 1249–1274.
- McKenzie, David, John Gibson, and Steven Stillman.** 2010. "How Important Is Selection? Experimental vs. Non-Experimental Measures of the Income Gains from Migration." *Journal of the European Economic Association*, 8(4): 913–945.
- Meghir, Costas, and Marten Palme.** 2005. "Educational Reform, Ability, and Family Background." *American Economic Review*, 95(1): 414–424.
- Mincer, Jacob A.** 1974. "Schooling, Experience, and Earnings." In *Schooling and Earnings*. Edited by Jacob A. Mincer, Chapter 3, 41–63. New York, NY: Columbia University Press.
- Miyagiwa, Kaz.** 1991. "Scale Economies in Education and the Brain Drain Problem." *International Economic Review*, 32(3): 743–759.
- Mortensen, Dale T.** 2011. "Markets with Search Friction and the DMP Model." *American Economic Review*, 101(4): 1073–1091.
- Mortensen, Dale T., and Christopher A. Pissarides.** 1994. "Job Creation and Job Destruction in the Theory of Unemployment." *Review of Economic Studies*, 61(3): 397–415.
- Moser, Petra, Alessandra Voena, and Fabian Waldinger.** 2014. "German Jewish Émigrés and U.S. Invention." *American Economic Review*, 104(10): 3222–3255.
- Mountford, Andrew.** 1997. "Can a Brain Drain be Good for Growth in the Source Economy?" *Journal of Development Economics*, 53(2): 287–303.
- Nelson, Richard R., and Edmund Phelps.** 1966. "Investment in Humans, Technology Diffusion, and Economic Growth." *American Economic Review*, 56(2): 69–75.
- Network of Experts in Social Sciences of Education and Training (NESSE).** 2008. "Education and Migration: Strategies for Integrating Migrant Children in European Schools and Societies - A Synthesis of Research Findings for Policy-makers." <http://www.nesse.fr/nesse/activities/reports/activities/reports/education-and-migration-pdf>.
- Niedersächsische Landeszentrale für Politische Bildung.** 2002. "Hier geblieben. Zuwanderung und Integration in Niedersachsen 1945 bis heute." <http://www.politische-bildung.de/niedersachsen/hiergeblieben-zuwanderung.pdf>.
- Nielsen, Helena S., Michael Rosholm, Nina Smith, and Leif Husted.** 2003. "The School-to-Work Transition of 2nd Generation Immigrants in Denmark." *Journal of Population Economics*, 16(4): 755–786.
- Nunn, Nathan.** 2009. "The Importance of History for Economic Development." *Annual Review of Economics*, 1(1): 65–92.
- Obschonka, Martin, Eva Schmitt-Rodermund, Rainer K. Silbereisen, Samuel D. Gosling, and Jeff Potter.** 2013. "The Regional Distribution and Correlates of an Entrepreneurship-Prone Personality Profile in the United States, Germany, and the United Kingdom: A Socioecological Perspective." *Journal of Personality and Social Psychology*, 105(1): 104–122.
- OECD.** 2007. *PISA 2006 - Volume 2: Data / Données*. OECD Publishing.

- OECD.** 2010. *PISA 2009 Results: What Makes a School Successful? Resources, Policies and Practices - Volume IV*. OECD Publishing.
- Oester, Anna, and Jonas Agell.** 2007. "Crime and Unemployment in Turbulent Times." *Journal of the European Economic Association*, 5(4): 752–775.
- Ohinata, Asako, and Jan C. van Ours.** 2013. "How Immigrant Children Affect the Academic Achievement of Native Dutch Children." *Economic Journal*, 123(570): F308–F331.
- Orcutt, Harriet, and Daniel J. Dowhan.** 2002. "Insights from Longitudinal Data on the Earnings Growth of U.S. Foreign-Born Men." *Demography*, 39(3): 485–506.
- Ottaviano, Gianmarco I. P., and Giovanni Peri.** 2006. "The Economic Value of Cultural Diversity: Evidence from US Cities." *Journal of Economic Geography*, 6(1): 9–44.
- Ottaviano, Gianmarco I. P., and Giovanni Peri.** 2012. "Rethinking the Effect of Immigration on Wages." *Journal of the European Economic Association*, 10(1): 152–197.
- Parey, Matthias, and Fabian Waldinger.** 2011. "Studying Abroad and the Effect on International Labor Market Mobility." *Economic Journal*, 121(551): 194–222.
- Parey, Matthias, Jens Ruhose, Fabian Waldinger, and Nicolai Netz.** 2014. "The Selection of High-Skilled Migrants." *Unpublished Manuscript*.
- Pekkarinen, Tuomas, Roope Uusitalo, and Sari Pekkala Kerr.** 2009. "School Tracking and Intergenerational Income Mobility: Evidence from the Finnish Comprehensive School Reform." *Journal of Public Economics*, 93(7-8): 965–973.
- Peri, Giovanni.** 2011. "Rethinking the Area Approach: Immigrants and the Labor Market in California." *Journal of International Economics*, 84(1): 1–14.
- Peri, Giovanni.** 2012. "The Effect of Immigration on Productivity: Evidence from U.S. States." *Review of Economics and Statistics*, 94(1): 348–358.
- Peri, Giovanni, and Chad Sparber.** 2009. "Task Specialization, Immigration, and Wages." *American Economic Journal: Applied Economics*, 1(3): 135–169.
- Peri, Giovanni, and Chad Sparber.** 2011a. "Assessing Inherent Model Bias: An Application to Native Displacement in Response to Immigration." *Journal of Urban Economics*, 69(1): 82–91.
- Peri, Giovanni, and Chad Sparber.** 2011b. "Highly Educated Immigrants and Native Occupational Choice." *Industrial Relations: A Journal of Economy and Society*, 50(3): 385–411.
- Peri, Giovanni, Kevin Shih, and Chad Sparber.** 2014. "STEM workers, H1B Visas and Productivity in US Cities." *Journal of Labor Economics*, forthcoming.
- Peters, Wilfried.** 2003. "Aussiedlerzuzug: Entwicklung und Perspektiven." *Zeitschrift für Ausländerrecht und Ausländerpolitik*, 23(5/6): 193–197.
- Pfeiffer, Christian, and Birke Dworschak.** 1999. "Die ethnische Vielfalt in den Jugendstrafvollzugsanstalten. Ergebnisse einer Umfrage aus dem Sommer 1998." *DVJJ- Journal*, 164(2): 184–187.
- Piketty, Thomas.** 2000. "Theories of Persistent Inequality and Intergenerational Mobility." In *Handbook of Income Distribution*. Vol. 1, edited by Anthony B. Atkinson and François Bourguignon, Chapter 8, 429–476. Amsterdam:Elsevier, North-Holland.

- Pischke, Joern-Steffen, and Johannes Velling.** 1997. "Employment Effects of Immigration to Germany: An Analysis Based on Local Labor Markets." *Review of Economics and Statistics*, 79(4): 594–604.
- Raphael, Steven, and Rudolf Winter-Ebmer.** 2001. "Identifying the Effect of Unemployment on Crime." *Journal of Law and Economics*, 44(1): 259–283.
- Rentfrow, Peter J., Samuel D. Gosling, and Jeff Potter.** 2008. "A Theory of the Emergence, Persistence, and Expression of Geographic Variation in Psychological Characteristics." *Perspectives on Psychological Science*, 3(5): 339–369.
- Restuccia, Diego, and Carlos Urrutia.** 2004. "Intergenerational Persistence of Earnings: The Role of Early and College Education." *American Economic Review*, 94(5): 1354–1378.
- Roback, Jennifer.** 1982. "Wages, Rents, and the Quality of Life." *Journal of Political Economy*, 90(6): 1257–1278.
- Romer, Paul M.** 1990. "Endogenous Technological Change." *Journal of Political Economy*, 98(5): S71–S102.
- Roy, A. D.** 1951. "Some Thoughts on the Distribution of Earnings." *Oxford Economic Papers*, 3(2): 135–146.
- Ruggles, Steven, J. Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder, and Matthew Sobek.** 2010. "Integrated Public Use Microdata Series: Version 5.0 [Machine-readable database]." Minneapolis, MN: Minnesota Population Center [producer and distributor].
- Ruhose, Jens.** 2013a. "Bildungsleistungen von Migranten und deren Determinanten - Teil I: Überblick und Vorschulbereich." *ifo Schnelldienst*, 66(9): 41–47.
- Ruhose, Jens.** 2013b. "Bildungsleistungen von Migranten und deren Determinanten - Teil II: Primar-, Sekundar- und Tertiärbereich." *ifo Schnelldienst*, 66(10): 25–38.
- Saiz, Albert.** 2003. "Room in the Kitchen for the Melting Pot: Immigration and Rental Prices." *Review of Economics and Statistics*, 85(3): 502–521.
- Saiz, Albert.** 2007. "Immigration and Housing Rents in American Cities." *Journal of Urban Economics*, 61(2): 345–371.
- Saxenian, AnnaLee.** 1994. *Regional Advantage: Culture and Competition in Silicon Valley and Route 128*. Cambridge, MA:Harvard University Press.
- Scheve, Kenneth F., and Matthew J. Slaughter.** 2001. "Labor Market Competition and Individual Preferences over Immigration Policy." *Review of Economics and Statistics*, 83(1): 133–145.
- Schmucker, Alexandra, and Stefan Seth.** 2009. "BA-Beschäftigtenpanel 1998–2007 Codebuch, FDZ Datenreport 01/2009." *Bundesagentur für Arbeit Nürnberg*.
- Schneeweis, Nicole.** 2011. "Educational Institutions and the Integration of Migrants." *Journal of Population Economics*, 24(4): 1281–1308.
- Schnepf, Sylke V.** 2007. "Immigrants' Educational Disadvantage: An Examination across Ten Countries and Three Surveys." *Journal of Population Economics*, 20(3): 527–545.
- Schoellman, Todd.** 2012. "Education Quality and Development Accounting." *Review of Economic Studies*, 79(1): 388–417.

- Schwartz, Aba.** 1973. "Interpreting the Effect of Distance on Migration." *Journal of Political Economy*, 81(5): 1153–1169.
- Seifert, Wolfgang.** 1996. "Neue Zuwanderergruppen auf dem westdeutschen Arbeitsmarkt: eine Analyse der Arbeitsmarktchancen von Aussiedlern, ausländischen Zuwanderern und ostdeutschen Übersiedlern." *Soziale Welt*, 2: 180–201.
- Sjaastad, Larry A.** 1962. "The Costs and Returns of Human Migration." *Journal of Political Economy*, 70(5): 80–93.
- Smith, James P.** 2014. "The Human Capital (Schooling) of Immigrants in America." In *Handbook of the Economics of International Migration*. Vol. 1A, edited by Barry R. Chiswick and Paul W. Miller, Chapter 4. Amsterdam:Elsevier, North-Holland.
- Solon, Gary.** 1992. "Intergenerational Income Mobility in the United States." *American Economic Review*, 82(3): 393–408.
- Solon, Gary.** 1999. "Intergenerational Mobility in the Labor Market." In *Handbook of Labor Economics*. Vol. 3A, edited by Orley C. Ashenfelter and David Card, Chapter 29, 1761–1800. Amsterdam:Elsevier, North-Holland.
- Spenkuch, Joerg L.** 2013. "Understanding the Impact of Immigration on Crime." *American Law and Economics Review*, 16(1): 177–219.
- Spolaore, Enrico, and Romain Wacziarg.** 2009. "The Diffusion of Development." *Quarterly Journal of Economics*, 124(2): 469–529.
- Sprietsma, Maresa.** 2013. "Discrimination in Grading: Experimental Evidence from Primary School Teachers." *Empirical Economics*, 45(1): 523–538.
- Staiger, Douglas, and James H. Stock.** 1997. "Instrumental Variables Regression with Weak Instruments." *Econometrica*, 65(3): 557–586.
- Stark, Oded, Christian Helmenstein, and Alexia Prskawetz.** 1997. "A Brain Gain with a Brain Drain." *Economics Letters*, 55(2): 227–234.
- Stock, James H., and Motohiro Yogo.** 2005. "Testing for Weak Instruments in Linear IV Regression." In *Identification and Inference for Econometric Models: Essays in Honor of Thomas J. Rothenberg*. Edited by James H. Stock and Donald W.K. Andrews, Chapter 5, 80–108. Cambridge:Cambridge University Press.
- Suedekum, Jens, Katja Wolf, and Uwe Blien.** 2012. "Cultural Diversity and Local Labour Markets." *Regional Studies*, 48(1): 173–191.
- Swiaczny, Frank, and Ulrich Mammey.** 2001. "Aussiedler." In *Nationalatlas Bundesrepublik Deutschland, Band 4: Bevölkerung*. Edited by Paul Gans and Franz-Josef Kemper, 132–135. Leipzig/Heidelberg:Spektrum.
- Tabellini, Guido.** 2010. "Culture and Institutions: Economic Development in the Regions of Europe." *Journal of the European Economic Association*, 8(4): 677–716.
- Thompson, Samuel B.** 2011. "Simple Formulas for Standard Errors that Cluster by Both Firm and Time." *Journal of Financial Economics*, 99(1): 1–10.
- Todaro, Michael P.** 1969. "A Model of Labor Migration and Urban Unemployment in Less Developed Countries." *American Economic Review*, 59(1): 138–148.

- Turner, Chad, Robert Tamura, and Sean E. Mulholland.** 2013. "How Important are Human Capital, Physical Capital and Total Factor Productivity for Determining State Economic Growth in the United States, 1840–2000?" *Journal of Economic Growth*, 18(4): 319–371.
- UNESCO International Bureau of Education.** 2013. "Country Dossiers." <http://www.ibe.unesco.org/en/worldwide.html>.
- United Nations, Department of Economic and Social Affairs, Population Division.** 2013. "Trends in International Migrant Stock: The 2013 Revision (United Nations Database, POP/DB/MIG/Stock/Rev.2013)."
- U.S. Census Bureau.** 2007. "Economic Census." <http://factfinder2.census.gov/faces/nav/jsf/pages/searchresults.xhtml?refresh=t>.
- U.S. Census Bureau.** 2009. "State and Metropolitan Area Data Book." <http://www.census.gov/compendia/smadb/SMADBmetro.html>.
- U.S. Census Bureau.** 2012. "Cost of Living Index–Selected Urban Areas." https://www.census.gov/compendia/statab/cats/prices/consumer_price_indexes_cost_of_living_index.html.
- U.S. Patent and Trademark Office.** 2011. "Utility Patent Grants." http://www.uspto.gov/web/offices/ac/ido/oeip/taf/cls_cbsa/allcbsa_gd.htm.
- U.S. Small Business Administration, Office of Advocacy.** 2005. "Statistics of U.S. Businesses." <http://archive.sba.gov/advo/research/data.html>.
- Van de Werfhorst, Herman G., and Jonathan J.B. Mijs.** 2010. "Achievement Inequality and the Institutional Structure of Educational Systems: A Comparative Perspective." *Annual Review of Sociology*, 36(1): 407–428.
- Van Ewijk, Reyn.** 2011. "Same Work, Lower Grade? Student Ethnicity and Teachers' Subjective Assessments." *Economics of Education Review*, 30(5): 1045–1058.
- Van Ours, Jan C., and Justus Veenman.** 2003. "The Educational Attainment of Second-Generation Immigrants in the Netherlands." *Journal of Population Economics*, 16(4): 739–753.
- ver.di.** 2013. "Collective Wage Agreement in the Public Sector 2013." <http://www.verdi.de/themen/geld-tarif/tarifunde-oeffentlicher-dienst-der-laender-2013/++co++a5c366fa-784c-11e2-8d18-52540059119e>.
- Vidal, Jean-Pierre.** 1998. "The Effect of Emigration on Human Capital Formation." *Journal of Population Economics*, 11(4): 589–600.
- Waldinger, Fabian.** 2007. "Does Ability Tracking Exacerbate the Role of Family Background for Students' Test Scores?" *Working Paper, University of Warwick*.
- Walter, Joachim.** 2002. "Jugendvollzug in der Krise?" *DVJJ- Journal*, 5: 127–143.
- Wenzel, Hans-Joachim.** 1999. "Aussiedlerzuwanderung als Strukturproblem in ländlichen Räumen." In *Aussiedler: deutsche Einwanderer aus Osteuropa*. Edited by Klaus J. Bade and Jochen Oltmer, 265–281. Göttingen:IMIS-Schriften Band 8.
- Wolfers, Justin.** 2006. "Did Unilateral Divorce Raise Divorce Rates? A Reconciliation and New Results." *American Economic Review*, 96(5): 1802–1820.

- Worbs, Susanne, Eva Bund, Martin Kohls, and Christian Babka von Gostomski.** 2013. "(Spät-)Aussiedler in Deutschland: Eine Analyse aktueller Daten und Forschungsergebnisse." Forschungsbericht 20, Bundesamt für Migration und Flüchtlinge.
- Wrede, Ferdinand, Walther Mitzka, and Bernhard Martin.** 1927. *Deutscher Sprachatlas. Auf Grund des von Georg Wenker begründeten Sprachatlas des Deutschen Reichs.* Marburg:Elwert.
- Zenou, Yves.** 2003. "The Spatial Aspects of Crime." *Journal of the European Economic Association*, 1(2-3): 459–467.

CURRICULUM VITAE

JENS RUHOSE

BORN 10.02.1984 IN LIPPSTADT

- MAY 2011 – MAY 2014 PHD STUDENT AND JUNIOR RESEARCHER AT THE IFO INSTITUTE - LEIBNIZ-INSTITUTE FOR ECONOMIC RESEARCH AT THE UNIVERSITY OF MUNICH
- JAN. 2014 – MAY 2014 VISITING FELLOW AT THE HOOVER INSTITUTION, STANFORD UNIVERSITY
- OCT. 2009 – SEPT. 2010 STUDIES OF ECONOMICS AT THE UNIVERSITY OF WARWICK (DEGREE: M.SC. ECONOMICS)
- OCT. 2008 – APRIL 2011 STUDIES OF INTERNATIONAL ECONOMICS AT THE UNIVERSITY OF PADERBORN (DEGREE: M.SC. INTERNATIONAL ECONOMICS)
- OCT. 2005 – SEPT. 2008 STUDIES OF MANAGEMENT AND ECONOMICS AT THE UNIVERSITY OF PADERBORN (DEGREE: B.SC. MANAGEMENT AND ECONOMICS)
- SEPT. 2004 – AUG. 2005 FRIEDRICH-LIST-VOCATIONAL COLLEGE HAMM