Essays on International Worker Mobility

by

Bartosz Woda

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Doctoral Committee:

Professor John Bound, Chair
Professor Hoyt Bleakley
Professor Adam Chilton
Professor Emerita Sherrie A. Kossoudji

Bartosz Woda

woda@umich.edu

ORCID iD: 0000-0001-9073-3475

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ABSTRACT

In this dissertation, I investigate various aspects of international labor migration in different parts of the world.

In the first chapter, I examine how migrants in the United Kingdom adjust their labor supply in response to exchange rate shocks of their home countries' currencies. I hypothesize that migrants from countries which are members of the European Union and the European Free Trade Agreement would respond differently from those whose countries are outside those organizations. The former group incurred much lower costs of moving into and out of the UK. I find that both groups reduce their labor supply in response to a depreciation of their home currencies (a favorable shock to the migrant), consistent with an income effect dominating a substitution effect, or income-targeting behavior. However, the EU/EFTA migrants respond by altering the timing of their return home while the non-EU/EFTA ones adjust their labor supply inside the UK. This result has implications for long-term consequences of the Brexit vote and immigration reform in the UK.

The second chapter, coauthored with Adam Chilton, examines the impact of Bilateral Labor Agreements (BLAs) on migration and remittances. Research on the effects of those agreements is scarce due to substantial data limitations. In this chapter, we overcome those constraints by focusing on the Philippines, a particularly prolific signer of BLAs. We identify 68 different BLAs that the Philippines has signed with countries in Asia, Europe, the Middle East, and North America. We use administrative data on new work contracts and remittance flows and we do not find any concrete evidence that signing a new BLA has an impact on either worker deployment or remittances. This is consistent with the fact that BLAs often have vague and unenforcable provisions, likely to have little effect on the behavior of

governments, firms, or workers.

In the third chapter, I investigate how the heterogeneity of communication skills among less educated foreign-born individuals in the United States affects U.S.-born workers' occupational mobility response to immigration. I build on previous theoretical and empirical work and develop six different measures of foreign-born communication type, based on language ability, years since arrival in the U.S., and country of origin. I also construct an instrument which I argue is correlated with both foreign-born shares and communication types. I confirm the result from previous literature that immigration pushes native-born workers to specialize in more communication-intensive occupations, but I also find that this effect is weaker if the foreign-born are more productive in communication tasks. The latter effect is especially precise when communication type is measured with years since arrival in the U.S., which I interpret as suggesting that communication ability is a function of more than just English proficiency.

CHAPTER I

Exchange Rate Shocks and Foreign Migrants in the UK

1.1 Introduction

Migrants[1], both temporary and permanent, often maintain links to their home countries for many years after initial migration. They may send remittances to their family members, save a portion of their earnings to consume or invest in their home country upon returning, and consume goods imported from their home country. Therefore, macroeconomic shocks in their home countries should affect their consumption and leisure patterns in the host (destination) country, as well the decision to return home and later remigrate. In this paper, I explore avenues through which migrants respond to one particular type of macroeconomic shock: fluctuations of the exchange rates of their home countries' currencies against the currency of the host country. A depreciation of the migrant's home currency against the destination country's currency is a favorable shock to the migrant since it raises the value of remittances and savings repatriated upon returning. The migrant can obtain more consumption in the home country (through higher remittances and savings) per unit of the destination country's currency. Two main channels of immigrants' response to exchange rate shocks have been identified and empirically studied in the literature: 1) altering the timing of return home; 2) adjusting labor supply in the home country, e.g. by changing the

[1]I define as a migrant anyone who was born in a different country from the one they currently reside in. Therefore, I use the terms *migrant*, *immigrant*, and *foreign-born* interchangably.

number of hours worked. However, to my knowledge, no paper has attempted to examine how those different ways of adjustment may be available to, or preferred by, different groups of migrants. In particular, migrants who are on work visas or who are in the country illegally may find it much more costly to change the timing of their return, either due to a possibility of apprehension or because visa terms constrain them to a particular duration of stay. I attempt to bridge this gap in the literature by looking at responses to exchange rate shocks of foreign-born individuals in the United Kingdom (UK) along two margins: earnings and hours worked within the UK, and returning to the home country. I compare how those responses vary between migrants from EU/EFTA[2] countries and those from non-EU/EFTA countries. EU/EFTA migrants in the UK face much lower costs of migration due to geographic proximity and free movement of labor within the EU (by definition, they cannot be illegal and they do not need visas to access the UK labor market). Therefore, they should find it much less costly to respond to shocks in their home countries by adjusting the timing of their return. On the other hand, non-EU/EFTA migrants are more likely to respond by adjusting their work effort while remaining in the UK. I examine this hypothesis empirically by looking at earnings and survey attrition (which proxies for returns home) of the foreign-born from the UK Labour Force Survey 1997-2017, as well as aggregate estimates of migrant stocks produced by the UK Office of National Statistics from 2000 to 2017.

The paper contributes to the existing literature in three ways: 1) it explores the relationship between exchange rates and labor supply in the UK, an important immigration destination; 2) it studies the impact of exchange rate shocks on migrant returns from the UK; 3) it analyzes how migrant heterogeneity affects their responses to economic shocks. The paper is organized as follows: section 1.2 presents a literature review and background on the UK immigration system; section 1.3 outlines a theoretical model of migration with different international mobility costs; section 1.4 describes the data and summary statistics; section

[2]EU stands for the European Union and includes 27 member countries. EFTA is the European Free Trade Agreement and includes 4 additional countries. See appendix Table A.1 for a full list of both organizations' member states.

1.5 examines the relationship between exchange rates and earnings of the foreign-born; section 1.6 analyzes exchange rates and migrant attrition and stocks; section 1.7 presents some robustness checks of earnings and returns results; section 1.8 concludes.

1.2 Literature review and background

1.2.1 Migrant remittances and savings

There are several reasons why the exchange rate can affect foreign-born individuals' labor supply and return decisions in the host country. The ones that have received the most attention in the literature are remittances and savings. There is abundant evidence that remittances from overseas migrants are an important source of income to households in many countries. Remittances may raise consumption and/or investment in Mexico (Massey and Parrado, 1998; Durand et al., 1996), Pakistan (Adams, 1998), the Philippines (Yang, 2008), Tonga (Brown, 1994; Brown and Ahlburg, 1999), and Samoa (Brown and Ahlburg, 1999). Remittances may also affect other important outcomes, e.g., Edwards and Ureta (2003) find that remittances substantially reduce the hazard of leaving school in El Salvador, whereas Amuedo-Dorantes and Pozo (2011) show that they raise health expenditures among Mexican households. In the UK context, the results of a survey conducted among Polish migrants who returned home, reported by Iglicka (2008), suggest that most of them sent remittances in order to support consumption of families back home. While data limitations make it difficult to determine how much of their income migrants remit, Albert and Monras (2019) estimate that, conditional on remitting at all, immigrants in the United States remit 10 – 15% of their income. The corresponding figures for migrants in Germany, reported by Sinning (2011), are 11.8 – 18.9%, with 15.6 – 34% of migrants remitting. Dustmann and Mestres (2010) report an even higher share of migrant households in Germany that remit: 46.2%. Importantly, even though international remittances have been primarily studied in the context of developing countries, Sinning (2011) shows that migrants from all countries

send substantial transfers to their home countries. This is important because many migrants in the UK are from relatively wealthy European countries.

The other main channel through which exchange rate shocks affect migrants is by changing the value of their savings accumulated in the destination country and repatriated to the home country upon returning. For example, Hill (1987), Djajić and Milbourne (1988), and Dustmann (2003) develop theoretical models where individuals migrate to the destination country temporarily in order to accumulate savings for consumption in the origin country after returning. Stark et al. (1997) develop a model of return migration explicitly focused on the difference in the purchasing power of migration-generated savings between the home and foreign countries. Foreign-born individuals may also save in the destination country in order to fund investment projects back home, especially when credit markets in their home countries are imperfect. Mesnard (2004) demonstrates such a saving motive among Tunisian migrants, while McCormick and Wahba (2001) show the same for Egyptian ones. Polish nationals in the UK reported purchasing an apartment in Poland as the most common reason to save (Iglicka, 2008). Immigrants often exhibit sizable saving rates, ranging from 17.4 – 22.4% among migrants in Germany (Sinning, 2011) to 40% among Egyptian ones (McCormick and Wahba, 2001), which makes them quite exposed to exchange rate fluctuations when those savings are repatriated upon return.

1.2.2 Exchange rates and migrant labor supply

The literature on the effect of exchange rate shocks on migrants' labor supply on the intensive margin (e.g., hours worked or effort) in the destination country is quite limited. The earliest attempt to explore this topic was made by Fox and Stark (1987). They use data from a survey of Mexican migrants in the United States to show that favorable exchange rate shocks (i.e., depreciation of the peso) are correlated with more hours worked by the migrants. More recent work by Nekoei (2013) uses data from the U.S. Current Population Survey 1994 – 2011 to examine the relationship between exchange rate fluctuations and

labor market outcomes of the foreign-born. He finds that depreciation of migrants' home currencies is associated with a *decrease* in USD-denominated earnings, with an elasticity of annual earnings with respect to the real exchange rate (home currency divided by USD) equal to approximately -0.092. About 60% of that effect is explained by a decrease in annual hours worked, of which two-thirds stems from a decline in annual weeks worked and the rest from a decline in hours per week. Nguyen and Duncan (2017) obtain roughly similar results from analyzing immigrants in Australia, although their elasticities of earnings and hours worked w.r.t. to the real exchange rate are attenuated somewhat when they control for individual fixed effects. They also show a significant gender gap in migrants' response to exchange rate shocks, with males generally reducing their earnings more than females for a given change in the exchange rate. Finally, Bello (2020) focuses on cross-border commuters from Italy to Switzerland and finds that appreciation of the Swiss franc is correlated with higher commuter traffic between the two countries, as well as a spike in Google searches for job offers in Switzerland originating from a border region in Italy. In addition, she uses survey data to show that a stronger Swiss franc is associated with more hours worked by cross-border workers.

Several authors have used microdata to examine the impact of exchange rate shocks on the decision by migrants to return home. Yang (2006) exploits the variation in exchange rate shocks experienced by Filipino migrants in different destination countries during the 1997 Asian financial crisis and estimates that a 10% depreciation of the Philippine peso leads to a decline in the return rate by 1.4 percentage points. This result implies that migrants remained in the destination countries longer in order to take advantage of favorable exchange rate fluctuations. However, migrants from the middle of the earnings distribution were least affected, suggesting income-targeting behavior in that subsample. Abarcar (2017) also examines the Asian financial crisis and finds that a 10% depreciation of the migrant's home country currency leads to a 10% lower probability of returning home within two years among the foreign-born in Australia. Finally, using data on immigrants from four countries who

reside in Germany, Kirdar (2009) shows that a depreciation of the home country's currency reduces return rates among young migrants but increases it among older migrants who are closer to retirement.

The relationship between exchange rates and migration can also be studied with aggregate data. Both Hanson and Spilimbergo (1999) and Davila (1983) observe that apprehensions of Mexican migrants illegally crossing the border into the U.S. spike after devaluations of the Mexican peso. Mishra and Spilimbergo (2011) look at migration flows from 66 countries into various OECD destinations and note that depreciation of origin country currencies is associated with higher emigration rates from those countries. Keita (2016) uses a larger set of 165 origin countries to show that a 10% depreciation of the migrant's home country currency is associated with an increase in migrant inflows of 18.2 – 19.4%.

Most evidence on the effect of exchange rates on migrants specifically in the UK is speculative or anecdotal. For example, Pollard et al. (2008) predicted that as the pound sterling lost value against currencies of the new EU member states which had joined in 2004, migrants from those countries would be less likely to migrate into the UK and more likely to return home. Similar opinions were expressed in the following article from 2008[3]:

> "A survey by Britain's largest Polish-speaking radio station at the end of last year reported that almost 40 per cent of migrant Polish workers would seriously consider returning home if the exchange rate fell to four zlotys to the pound."

Additional evidence that migrants are concerned about exchange rates comes from Google searches. In appendix Figure A.1, I show the volume of searches from inside the UK for the Polish phrase "kurs walut", meaning "exchange rates," between 2004 and 2017. There is a spike from mid-2007 till early 2009, corresponding to a period during which the UK pound severely depreciated against the Polish zloty. There is a second peak in mid-2016, following the unexpected success of the Brexit campaign, which led to depreciation of the

[3]Taylor, Andrew. "Weak pound has Poles eyeing homeland." *Financial Times*, May 25, 2008. Accessed November 1st, 2020. https://www.ft.com/content/593a9752-2a97-11dd-b40b-000077b07658 . Found through Mishra and Spilimbergo (2011).

pound, too. However, while this data is suggestive that the foreign-born pay close attention to movements in exchange rates, it does not tell us anything about how those movements affect their labor supply and return plans.

1.2.3 International mobility costs

Various costs borne by international migrants have been recognized since the early days of economic analysis of migration (Sjaastad, 1962; Harris and Todaro, 1970). However, relatively little attention has been devoted to the costs that the migrant might incur once he or she is already *in* the country of destination. To the extent that such costs have been studied, it has been primarily in the context of illegal migration. For example, Massey et al. (2016) argue that the rapidly rising U.S.-Mexico border enforcement has had a perverse effect of increasing the undocumented population living inside the United States, as migrants fear that they will not be able re-enter the country if they leave.

1.2.4 Background on free movement of labor within the UK

Most foreign-born individuals who wish to work in the United Kingdom legally must apply for a visa.[4] The application process varies depending on the type of work the migrant performs. Foreign-born individuals in "skilled" occupations require a licensed sponsor, must pay fees potentially exceeding 1000 GBP, and may not remain in the visa status for more than 6 years. There are also short-term visas for farm and domestic workers which are valid for up to 6 months. Some visa categories may allow migrants to apply for permanent status, but only after they have fulfilled certain requirements, such as continuous residency in the UK and satisfactory knowledge of English.

In contrast, those legal constraints did not apply to migrants from countries which are members of the European Union (EU) or the European Free Trade Agreement (EFTA). Free

[4]Following its departure from the European Union in 2020, the UK introduced a new points-based immigration system. However, the system outlined in this section applied throughout the period of my analysis. See https://www.gov.uk/guidance/new-immigration-system-what-you-need-to-know and https://www.gov.uk/browse/visas-immigration/work-visas, accessed October 31st, 2020.

movement of labor is one of the foundational principles of the EU and it is extended to the EFTA. As a member of the EU until its departure in January 2020, the UK had to abide by that principle and allow labor market access to EU/EFTA migrants under the same terms as UK nationals, although some caveats applied to migrants from new EU members in Eastern Europe, which joined in 2004 or later. When 10 new countries joined the EU in May 2004, almost all other EU members imposed temporary restrictions on movement of labor from those countries. However, the UK was one of a handful of old EU members that allowed complete and unrestricted labor market access to migrants from the new countries. The only requirement was that workers from those countries registered with the Worker Registration Scheme when they first began employment in the UK (Pollard et al., 2008). The registration fee was 50 GBP, later raised to 90 GBP. Workers were no longer required to register once they had completed 12 months of continuous employment in the UK. Many migrants did not register at all, as they did not need proof of registration to begin employment and there were no serious penalties for failing to register. The scheme was discontinued in 2011.

More stringent restrictions were imposed until 2014 on migrants from Romania and Bulgaria when those countries joined the EU in 2007. Individuals from those countries were initially subject to similar rules as non-EU/EFTA migrants. However, starting in 2008, participation in low-skilled migration schemes were limited to immigrants from Romania and Bulgaria, giving them a large advantage over non-EU/EFTA migrants (Pollard et al., 2008). Also importantly, workers from those countries gained unrestricted access to UK labor markets after 12 months of legal employment. Finally, Romanians and Bulgarians who were classified as self-employed were not subject to any restrictions. For all those reasons, I count Romanian and Bulgarian foreign-born in the EU/EFTA category beginning in 2007, i.e. the year their countries joined the EU. I do the same with Croatian migrants, who were subject to similar restrictions and who became EU members in 2013. Nonetheless, as a robustness check, I perform all my analysis with an alternative classification of immigrants from those three countries, described in section 1.4.5.

1.2.5 Migration flows in the UK

The UK has been an important destination country for international migrants for the past 25 years. Net migration in the UK was close to zero in 1993 but it rapidly rose to 163,000 in 1999 and then above 200,000 after the 2004 EU expansion, reaching a peak of 331,000 in 2015 (Sumption and Vargas-Silva, 2020). The composition of international migrant inflows has also changed over this time period. Until 2004, fewer than 20% of immigrants came from other EU countries but their share increased to about 35% by 2008 (Vargas-Silva, 2013). Many migrants, both from the EU and outside of it, are temporary. For example, based on visa data, between 2016 and 2017, 42,000 non-EU migrants came to the UK to work while 30,000 work visa holders left (Office for National Statistics, 2018). It is difficult to produce equivalent figures for EU migrants due to their lack of visa requirements. However, in 2019, an estimated 195,000 EU migrants moved to the UK for any reason and 137,000 moved out (Office for National Statistics, 2020). Pollard et al. (2008) estimate that between 2004 and 2007, 1 million migrants from the new EU countries came to the UK but half of them left permanently by the end of 2007. EU migrants tend to be much more flexible about their intended duration of stay in the UK than non-EU ones. Nearly half of immigrants from the EU do not know how long they will remain in the UK – the corresponding number for non-EU ones is only 11% (Office for National Statistics, 2019).

In addition, it has been increasingly recognized that many immigrants are "circular", i.e., they move back and forth between the UK and their countries of origin. For example, among those non-EU migrants who have a work, study, or family visa valid for 4 years or more, the average number of trips to the UK is 8.7 and the average trip duration is 115.6 days (Office for National Statistics, 2018). Pollard et al. (2008) argue that circular migration is especially likely for EU migrants:

> "(...) in contrast to previous migrants, it is financially and logistically possible for migrants from the new EU member states to come to the UK on a temporary or seasonal basis, and to regularly visit home while living in Britain. One in ten

of those returned Poles in the survey had been in the UK for three months or longer on more than one occasion in recent years."

According to a survey of Polish immigrants conducted by Pollard et al. (2008), 18% said they only came to work seasonally (another 16% said they always intended to return once they saved a certain amount of money). A separate 2006 University of Surrey survey of 500 Poles in the UK found that 22% of them identified as seasonal migrants (Pollard et al., 2008). In an ethnographic study of Polish migrants in London, Eade (2007) finds that 20% of interviewees moved back and forth between the UK and Poland, e.g. a man who comes to London for 3-5 months each year and spends the remaining months working on his farm back home. In my own sample of foreign-born workers in the Labour Force Survey, described in section 1.4.1, 7.8% of EU migrants and 6.9% of non-EU ones have not lived in the UK continuously. For those individuals, the mean difference between their first and most recent arrivals in the UK is 11.2 years, suggesting that circular migration may persist for a long time.

1.3 Theory

In the previous section, I argued that exchange rates are related to labor supply and return decisions of the foreign-born through their impact on remittances and savings. The exact nature of this relationship could be illustrated with a life-cycle model of consumption and leisure, where the migrant consumes a portion of her foreign income in the home country (Nekoei, 2013). In such a model, a depreciation of the migrant's home currency triggers a substitution effect away from leisure as its price rises in terms of the home currency, and an income effect towards leisure, since the migrant's earnings are worth more in terms the home currency. The net impact on hours and work effort depends on the relative magnitudes of those effects. In addition, if the migrant prefers consumption in her home country to that in the destination country, as is generally assumed in models of return migration (Hill,

1987; Stark et al., 1997; Dustmann, 2003), the income effect pushes the migrant to return home sooner while the substitution effect pulls her in the opposite direction. Such life-cycle models can be modified to explain income-targeting behavior, for example, by assuming that migrants need a certain amount of investment capital for a project back home but are credit-constrained (Mesnard, 2004; Yang, 2006).

The main contribution of this paper is to show that different types of migrants may have access to different channels of adjustment to exchange rate shocks, depending on the international mobility costs that they face. In order to guide intuition, I outline a simple dynamic model of repeat, or circular, migration (i.e. where the foreign-born individual may choose to migrate and return home more than once during her lifetime), based on Dustmann and Görlach (2016). Consider a migrant from country i with characteristics X_t residing in the UK in the current period t. X_t should be thought of as a vector of variables which affect the migrant's labor supply outcomes, such as age, education, language ability, etc. In each period, the migrant decides whether to continue living in the UK for at least one more period or return to her home country i. Given X_t, she obtains value $V_{UK}(X_t)$ if she chooses to stay in the UK and $V_i(X_t)$ if she chooses to return. Thus, the migrant obtains

$$V(X_t) = max\{V_{UK}(X_t), V_i(X_t)\}$$

$$V_{UK}(X_t) = \max_{Y_{UK,t}} u(\pi_{UK}, Y_{UK,t}) + \beta V(X_{t+1}) = u(\pi_{UK}, Y^*_{UK,t}) + \beta V(X_{t+1})$$

$$V_i(X_t) = \max_{Y_{i,t}} u(\pi_i, Y_{i,t}) + \beta V(X_{t+1}) = u(\pi_i, Y^*_{i,t}) + \beta V(X_{t+1})$$

where $Y^*_{L,t}$ is an optimally chosen vector of variables that affect the migrant's utility, $L \in \{UK, i\}$, and β is a discount factor. The variables in $Y^*_{L,t}$ include the migrant's own consumption of commodities and leisure but may also include the consumption obtained by her family residing in the home country. π_L is a preference parameter such as that $\frac{\partial u}{\partial \pi_L} > 0$ and $\pi_i > \pi_{UK}$, i.e. the migrant prefers consumption in her own country to that in the UK.

Consider an exchange rate shock experienced by the migrant. Specifically, suppose that

the UK pound appreciates against the migrant's country's currency (conversely, the home currency depreciates). Also, suppose that the migrant's pre-shock optimal choice was to remain in the UK and consume $Y^*_{UK,t}$. She can respond to the shock either by altering her location decision or by remaining in the UK while changing her choice from $Y^*_{UK,t}$ to $\tilde{Y}^*_{UK,t}$. In particular, if the migrant is a target earner or the income effect dominates the substitution effect, she may either choose to return to her home country sooner or remain in the UK but work less. She will choose the former if $\tilde{V}_{UK}(X_t) < \tilde{V}_i(X_t)$ and the latter if $\tilde{V}_{UK}(X_t) > \tilde{V}_i(X_t)$ (the tilde symbol refers to the post-shock value).

Now suppose that the migrant faces costs to adjusting the return decision. Specifically, a foreign-born individual must incur cost $C(i)$ in order to enter the UK, so that

$$V(X_t) = max\{V_{UK}(X_t) - C(i), V_i(X_t)\}$$

This cost may entail having to apply for a visa or, in the case of illegal migrants, having to pay smuggling fees and risking detection and deportation. In addition, as discussed above, certain visa categories may have a continuous residency requirement if the migrant wishes to obtain permanent status. A forward-looking migrant who expects to return to the UK s periods in the future would change her return decision relative to what it would be in the absence of $C(i)$, if $V_i(X_t) > V_{UK}(X_t) > V_i(X_t) - \beta^s C(i)$. In other words, the migrant would like to return home in response to the exchange rate shock, but because she knows that returning in the future is costly, she instead chooses to remain in the UK and respond by adjusting her consumption and labor supply.

Another type of international mobility cost stems from overstaying a visa that allows a foreign-born individual to work in the UK. Suppose that in each period following the expiration of her visa, the migrant incurs a cost $K(i)$ if she continues to stay in the UK, so that

$$V(X_t) = max\{V_{UK}(X_t) - K(i), V_i(X_t)\}$$

This cost arises because the migrant has to expend effort to avoid detection, she may be prevented from obtaining certain social benefits, or she may be unable to enter the UK legally in the future if detected. In this case, a migrant who would like to remain in the UK longer in response to an exchange rate shock may nonetheless return home if staying for additional periods would cause her to overstay her visa. Mathematically, $V_{UK}(X_t) > V_i(X_t) > V_{UK}(X_t) - K(i)$.

Thus, international mobility costs lead migrants to stay in the UK longer or shorter than would be optimal when faced with an exchange rate shock. Instead of responding by adjusting their duration of stay in the UK, foreign-born who have to bear such costs will respond by altering their consumption and labor supply while remaining in the UK. Importantly, both types of costs depend on the migrant's home country i. Due to various international agreements, immigrants from some countries face much lower costs of entering the UK or staying in the country longer than originally planned. Specifically, migrants from countries which are members of the European Union or the European Free Trade Agreement enjoyed complete and unconstrained access to the UK labor market until 31 January 2020[5], so that $C(i) \approx K(i) \approx 0$. Thus, relative to non-EU/EFTA migrants, we would expect EU/EFTA ones to be more likely to alter the timing of their departure from the UK and less likely to change their labor supply within the UK in response to exchange rate shocks.

1.4 Data and summary statistics

1.4.1 Labour Force Survey

My primary source of data is the UK Quarterly Labour Force Survey (LFS), from the first quarter of 1997 to the second quarter of 2017. The LFS is a survey of a random sample of households residing in the United Kingdom, conducted since 1973. Under the current design, which was implemented in 1992, each household is interviewed five times, at exactly

[5]Following its Brexit vote in 2016, the UK left the EU on 31 January 2020. However, free labor movement for EU/EFTA migrants applied throughout my sample period.

3-month intervals, so that the fifth interview (wave 5) takes place exactly 1 year after the first one (wave 1). After wave 5, the household is rotated out of the survey. In 2016, slightly over 70,000 households were in the sample. However, the non-response rate is quite high and has increased over time, from about 25% in wave 1 in the first quarter of 2003 to 45% in wave 1 in the first quarter of 2017 (Northern Ireland Statistics and Research Agency, Central Survey Unit, Office for National Statistics, Social Survey Division, 2020). Moreover, there is non-trivial attrition within any LFS cohort. For example, out of the 8,335 responding households in wave 1 in the first quarter of 2016, only 5,372 responded in wave 5 a year later, implying an attrition rate of 35.5% (Northern Ireland Statistics and Research Agency, Central Survey Unit, Office for National Statistics, Social Survey Division, 2020).

The LFS contains questions about each household member's labor force status and characteristics, such as hours worked, occupation, industry, etc. Most questions refer to the *reference week*, which is the full week (Monday to Sunday) preceding the date of the interview. Not all questions are asked in each wave, e.g. earnings, which is my main outcome of interest, are only asked about in waves 1 and 5. In addition to labor force variables, basic demographic characteristics of each household member are recorded. Household members who were born outside the UK are asked their country of birth and the year they first came to the UK. Importantly, the survey can be conducted in a number of foreign languages if the initial interviewer determines that the respondent does not have sufficient English proficiency.

There are some important limitations of the LFS when it comes to studying migrants. It is thought to undercount immigrants who have recently arrived or who stay in the UK for a relatively short time (Clark and Drinkwater, 2008). In addition, migrants have a relatively high propensity to live in group quarters, which are not covered by the LFS (Pollard et al., 2008). Nevertheless, the survey is the most comprehensive source of microdata on the foreign-born in the UK and has been used to study return migration (Dustmann and Weiss, 2007), labor market performance of recent migrants (Clark and Drinkwater, 2008), and the fiscal consequences of immigration from new EU member states (Dustmann et al., 2010).

1.4.2 Aggregate migrant stock data

In addition to the LFS microdata, I use annual estimates of aggregate migrant stocks in the UK, by country of birth, published by the UK Office for National Statistics. Those estimates are obtained from the Annual Population Survey (APS) which is composed of the LFS and a number of boost samples. Note that these estimates include all foreign-born individuals, regardless of age and labor force status. In addition, values for countries with relatively small populations living in the UK are estimated with sizable standard errors. I use those estimates for years 2000 – 2017 (data for years prior to 2000 are not available).

1.4.3 Exchange rate data

I obtain data on nominal exchange rates and consumer price indices at monthly and annual frequencies from the International Financial Statistics (IFS) database maintained by the IMF. A unique challenge is presented by countries which adopt the euro as their national currency during the sample period.[6] In order to deal with this problem, I follow the approach of Feenstra et al. (2015) and convert those countries' pre-euro national currencies to the euro at the fixed conversion rate that prevailed at the time they joined the eurozone.[7]

1.4.4 Other country-level data

Finally, I use data on a number of time-varying characteristics of migrants' home countries:

1. real GDP per capita, obtained from the World Bank's World Development Indicators database.

[6]These include, in 2001 – Austria, Belgium, Finland, France, Germany, Ireland, Italy, Luxembourg, The Netherlands, Portugal, Spain; in 2001 – Greece; in 2007 – Slovenia; in 2008 – Cyprus, Malta; in 2009 – Slovakia; in 2011 – Estonia; in 2014 – Latvia; in 2015 – Lithuania. See https://www.ecb.europa.eu/euro/intro/html/index.en.html (accessed November 2nd, 2020).

[7]See https://www.ecb.europa.eu/euro/intro/html/index.en.html (accessed November 2nd, 2020) for a list of the conversion rates.

2. total trade flows from the IMF's Direction of Trade Statistics database, supplemented with estimates from the Correlates of War Trade Data Set, Version 4.0 (Barbieri et al., 2009; Barbieri and Keshk, 2016).

3. polity2 scores produced by the Center for Systemic Peace. Polity2 scores measure how democratic a country is on a scale from -10 to 10, with higher values indicating a more democratic system and lower values indicating a more autocratic one.

4. numbers of people killed and otherwise affected by natural and technological disasters from the International Disaster Database (EM-DAT).

5. numbers of deaths in organized violence, from the Uppsala Conflict Data Program (Pettersson and Öberg, 2020; Sundberg and Melander, 2013). Organized violence is defined as wars between sovereign states, non-state conflicts, and one-sided violence towards particular groups (e.g., based on ethnicity).

All these variables are measured at an annual level.

1.4.5 Summary statistics

In my primary analysis, I use data from the LFS starting in quarter 1 of 1997 and ending in quarter 2 of 2017. I only look at data from wave 1 households in each quarter in order to avoid double counting. I include individuals born outside the United Kingdom who are employees (the LFS does not report the earnings of the self-employed), are not full-time students, and who report positive gross weekly earnings. In addition, I only include individuals who worked at all in the reference week, as those who are employed but did not work in the reference week may be temporarily detached from the labor force (e.g. because they are on sick leave). Table 1.1 presents summary statistics from this sample, separately for migrants from EU/EFTA and non-EU/EFTA countries. A person is only classified as an EU/EFTA migrant after his or her country joins one of those organizations. For instance, a person born in Latvia is only an EU/EFTA migrant if he or she is interviewed in May 2004

or later, i.e. after Latvia becomes and EU member. I take the same approach with migrants from Bulgaria and Romania, which joined the EU in 2007, and Croatia which joined in 2013. However, for reasons described in section 1.2.4, I perform a robustness check where I reclassify Romanians and Bulgarians as EU/EFTA immigrants starting only in 2014, and Croatians as non-EU/EFTA in my entire sample period.

The sample includes 48,417 individuals, out of whom 39.5% are from EU/EFTA countries. On average, EU/EFTA migrants are slightly younger and have been in the UK for a shorter time than non-EU/EFTA ones. They are significantly less likely to be married[8] and slightly more likely to be less educated.[9] The distribution of employment across major sectors is similar for the two groups, although a larger share of EU/EFTA migrants are employed in manufacturing and a smaller share in health and social work.[10] Panel C of Table 1.1 shows that average gross weekly earnings of EU/EFTA migrants are slightly lower than those of non-EU/EFTA ones, even though the former tend to work more hours, both regular and overtime. Finally, Panel B demonstrates that EU/EFTA countries are, on average, more prosperous, democratic, and peaceful than non-EU/EFTA countries, at least among those whose nationals work in the United Kingdom. In appendix table A.2, I list the countries with the most migrants in the UK in my sample. Nearly 11% of the sample consists of individuals born in India, 8.9% in Poland, 7.3% in Ireland, and 5.9% in Germany. No other country contributes more than 5% of the sample. Overall, migrants from 156 different countries are included.

In order to construct my measure of real exchange rates, I multiply monthly nominal exchange rates (units of the home country currency per pound) by the ratio of monthly

[8] I classify individuals who are separated as married.

[9] Prior to 2011, most educational qualifications obtained outside the UK were not coded in the LFS. Because of this, foreign-born individuals' education would usually be classified as "other," regardless of their actual degrees obtained. Therefore, I use the question on age at which the person completed full-time education and classify as less educated those who left full-time schooling at 18 or younger.

[10] The "Other" category includes: agriculture, hunting, and forestry; fishing; mining; electricity, gas, and water supply; construction; financial intermediation; public administration and defense; other community, social, and personal; private households with employed persons; extra-territorial organizations. In all my regressions, I control for each of those categories separately.

price levels (measured by the CPI). That is, the real exchange rate that a migrant from country i faces in month t is: $realEX_{i,t} = nomEX_{i,t} \times \frac{CPI_{UK,t}}{CPI_{i,t}}$. Therefore, an increase in the real exchange rate, i.e. a depreciation of the migrant's home currency (an appreciation of the pound) should be considered a favorable shock to the migrant, as it allows her to afford more consumption or investment in the home country for any given earnings in the UK. In the bottom panel of Figure 1.1, I plot the real exchange of currencies of several countries with some of the largest migrant populations in the UK as of 2017. For this figure, I convert real exchange rates to an index whose value is equal to 100 in January 1997 for all countries. In addition, the top panel shows the real effective exchange rate of the pound, i.e. an exchange rate index of the pound against all other countries' currencies, weighted by the volumes of trade between the UK and each country. Figure 1.1 demonstrates two facts: 1) there is substantial variation in exchange rates, both across countries and within countries over time; 2) there was a large decline in the value of the pound between July 2007 and January 2009 but it was not uniform across the different countries.

1.5 Exchange rate shocks and earnings

In this section, I analyze the effect of exchange rate fluctuations on earnings of the foreign-born. I begin in section 1.5.1 by looking at the full sample, in order to establish a baseline result and compare my results to other authors'. Then, in section 1.5.2, I compare the responses of EU/EFTA and non-EU/EFTA migrants.

1.5.1 All migrants

Using my sample of foreign-born employees, described in section 1.4.5, I test whether migrants adjust their earnings in response to exchange rate shocks. Specifically, I run the following regression:

$$ln(Y_{j,i,t}) = \beta_0 + \beta_{EX} ln(realEX_{i,t}) + \beta_X X_{j,i,t} + \beta_Z Z_{i,t} + \gamma_i + \delta_t + \epsilon_{j,i,t} \tag{1.1}$$

where $Y_{j,i,t}$ are the gross weekly earnings of migrant j from country i in month t; $realEX_{i,t}$ is the real exchange rate that the migrant faces, as defined in section 1.4.5; $X_{j,i,t}$ are migrant-specific characteristics which may be time-varying; $Z_{i,t}$ is a vector of time-varying characteristics of the migrant's home country; γ_i is a home country fixed effect; and δ_t is a month $\times$ year fixed effect (e.g. March 2001, September 2014, etc.). Thus, the coefficient β_{EX} identifies the elasticity of the migrant's earnings with respect to the real exchange rate. I cluster the standard errors at the country, month $\times$ year, and industry of employment level, following the approach developed by Correia (2016). The results of this regression are presented in table 1.2[11]. Column (1) presents a model only with the exchange rate, and country and month $\times$ year dummies and shows a negative exchange rate elasticity of earnings which is significant at the 10% level. In column (2), I add the migrant's demographic characteristics[12]. In order to control for aspects of labor demand that may not be fully captured by month $\times$ dummies, in model (3), I also introduce dummies for the individual's major sector of employment. It is important to control for the migrant's industry as certain sectors may be more exposed to exchange rate fluctuations than others (Campa and Goldberg, 2001). It is also possible that exchange rate shocks are correlated with other events in the foreign-born's home countries which also affect their earnings through remittance commitments or savings targets. Hence, in column (4), I add various time-varying country-level controls. The coefficient on exchange rates remains negative and statistically significant in models (2) – (4), although the sample size is much lower in model (4) as a result of missing observations. This coefficient implies that a 10% depreciation of the migrant's home currency (a favorable shock, as discussed previously) leads her to reduce her earnings by 0.79 – 0.85%. Migrants in my sample have mean

[11] In these regressions, as well as all the other regressions reported in the main paper, I choose not to use person weights from the LFS. Those weights do not take into account the individual's country of birth and the foreign-born in the UK are quite different demographically from the UK natives. However, including weights does not change the results substantively. Weighted regressions are reported in the appendix. The version of table 1.2 with LFS weights is Table A.3.

[12] Also of note is the positive and statistically significant coefficient on years since arrival, suggesting that migrants become better integrated with the UK labor market as they spend time in it, with a 0.7 – 0.8% increase in earnings per year spent in the UK. There is a large literature on whether migrants' earnings catch up with natives' over time, e.g. Borjas (1985) and Adsera and Chiswick (2007).

weekly earnings of 464 pounds (in 2010 GBP) with a standard deviation of 453. Thus, a 10% depreciation would lead them to reduce their earnings by about 4 pounds, on average. The negative coefficient on the exchange rate suggests either that the income effect dominates the substitution effect in the context of a neoclassical model of labor supply, or that migrants have income targets, for example due to remittance commitments. It is also worth noting that my estimate is very close to the elasticities reported in Nekoei (2013) and Nguyen and Duncan (2017), who run similar regressions using U.S. and Australian data, respectively.

1.5.2 EU/EFTA vs. non-EU/EFTA migrants

In this section, I test whether EU/EFTA and non-EU/EFTA migrants respond differently to exchange rate shocks. I begin by plotting log weekly earnings against log exchange rates (after controlling for country and month × year dummies and removing the mean), separately for those two types of migrants, in Figure 1.2[13]. The right-hand panel, which includes non-EU/EFTA foreign-born, shows a negative relationship, similar to the combined sample in section 1.5.1. However, the opposite is true for EU/EFTA migrants, suggesting that they may actually *increase* their earnings in response to a depreciation of their home currencies.

I present those results more formally in Table 1.3, which shows the results of the same regression as in Equation 1.1 and Table 1.2, estimated separately for the two groups of migrants[14]. The row labeled "Ln(real exrate) difference" contains the difference between the coefficients on the exchange rate and its standard error. The table shows that the exchange rate elasticity of earnings is estimated more precisely for the non-EU/EFTA migrants than the EU/EFTA ones and it is larger in absolute value, although the difference is not statis-

[13]The figure is created by residualizing log weekly earnings and log exchange rates on country and month × year dummies, dividing the resulting exchange rate residuals into 20 equally sized bins, and calculating mean log exchange rates and log weekly earnings in each bin.

[14]Specifically, the regression is estimated by fully interacting all covariates with a dummy for EU/EFTA. Mechanically, this produces identical point estimates of coefficients to running the regression for the two groups separately. However, standard errors may vary slightly between these two approaches, due to different overall sample sizes and degrees of freedom. The results of running the regressions separately for the two groups are not reported, but they are virtually indistinguishable from Table 1.3 and are available from the author upon request.

tically significant, mainly due to the wide confidence interval of the EU/EFTA estimate. These results suggest that non-EU/EFTA migrants are more likely to alter their earnings in response to exchange rate shocks than EU/EFTA ones and that the magnitude of that response may be larger (although note that the 95% confidence interval of the coefficient for the EU/EFTA group contains relatively large responses in both directions)[15].

The regressions in Table 1.3 do not tell us anything about *how* migrants adjust their earnings. I attempt to shed some light on this question by running the same regression as in Table 1.3 but with different labor market outcomes, presented in Table 1.4. For each dependent variable, I report the coefficient on the exchange rate for the EU/EFTA migrants, non-EU/EFTA migrants, and the difference between the two. For the sake of parsimony, I only include models (3) and (4) from Table 1.3. However, the results of models (1) and (2) are not substantively different and are available upon request. Table 1.4 shows that in response to a depreciation of their home currencies, non-EU/EFTA migrants respond by reducing hours worked, both regular and overtime. Meanwhile, EU/EFTA migrants may actually increase their hours, although the coefficients are not statistically significant. Both groups might also be less likely to look for a new or additional job. Other potential margins of adjustment, which I do not investigate, include working different shifts, changing firms within the same industry (de Matos, 2017a), or altering the amount of effort[16].

1.6 Exchange rate shocks and migrant returns

In this section, I examine whether exchange rate shocks have differential effects on migrants' decisions to return to their home countries, depending on their country's membership in the EU/EFTA. Based on the theory outlined in section 1.3, I hypothesize that EU/EFTA foreign-born are more likely to respond to exchange rate shocks by adjusting the timing of

[15]Estimating the regressions with LFS person weights (appendix Table A.4) or with an alternative classification of Bulgaria, Romania, and Croatia (appendix Table A.7) generates very similar results.

[16]See appendix Table A.5 and Table A.8 for specifications with weights and alternative definitions of EU membership, respectively.

their return home because their costs of doing so are much lower. I measure migrant returns in three ways: 1) individual attrition from the LFS between waves 1 and 5; 2) aggregate attrition from the LFS, by cohort; 3) changes in the aggregate stock of migrants in the UK.

1.6.1 Migrant returns - individual attrition

I measure migrant returns with attrition from the LFS between the migrant's first and final waves. In order to evaluate the validity of this approach, I conduct two tests. First, I compare survey attrition among the foreign-born to that of natives: we would expect the former to be meaningfully higher than the latter if part of migrants' attrition is due to emigration. For each LFS quarter, I count the number of all foreign-born who meet my sample criterita (employees, not students, worked in the reference week) and are in their first LFS wave. I then count how many such individuals are in their fifth wave one year later. I perform the same analysis for UK-born natives. The implied attrition rates for the two groups are plotted in Figure 1.3. Attrition is higher for immigrants than natives in most quarters, sometimes substantially so, consistent with the idea that at least some migrants drop out of the LFS because they return to their home countries. Throughout the sample period, the mean attrition rate for the foreign-born is 17.2%, compared to 13.5% for natives. Assuming that the only difference between natives' and immigrants' attrition rates stems from migrants' returning home, this would imply an annual return rate of 3.7%, broadly in line with migrant return rates estimated in the literature (Dustmann, 2003; Yang, 2006).

Clearly, LFS attrition is a noisy measure of emigration. One concern is that a portion of the attrition rate that is not due to migrant returns is correlated with exchange rates for some reason. Therefore, I also check whether survey attrition among *natives* is correlated with effective exchange rates of the pound. In Figure 1.4, I plot both variables over time, with attrition on the left y-axis and effective exchange rates on the right y-axis. At first glance, it may seem that those two series are negatively correlated. However, upon closer inspection, it is clear that while attrition rises over time, the effective exchange rate is relatively stable until

2007 when it suddenly collapses to a much lower level and remains stable before picking up somewhat in 2014. The exchange rate collapse in 2007 is not accompanied by a meaningful increase in survey attrition among natives. Conversely, the increase in attrition between 2001 and 2004 does not seem to be matched by changes in the exchange rates. Likewise, the decline in attrition between 2010 and 2012 appears uncorrelated with any changes in the exchange rates. Therefore, I am reasonably confident that the portion of survey attrition due to factors other than emigration of the foreign-born is uncorrelated with exchange rates.

Unfortunately, the version of the LFS accessible to researchers outside the UK is limited in that it is more difficult to follow individuals across their 5 survey waves. Therefore, I match individuals between waves 1 and 5 based on a number of survey variables and individual characteristics. Prior to the second quarter of 2001, the data contained system variables which made it possible to follow households over time. However, it was not possible to definitively follow individuals within those households because person identifiers within the household could change if its composition changed. Therefore, I first link households between waves 1 and 5 using the system variables and then link individuals within the households using their age, sex, country of birth, and year of first arrival in the UK, i.e., variables which should remain constant over time.

Starting in the third quarter of 2001, some of the system variables used to follow households over time were removed and it is no longer possible to use them to match households across waves with certainty. Therefore, I link individuals between waves 1 and 5 using the remaining system variables and the four demographic characteristics mentioned above. In cases where I cannot create unambiguous matches, I then use age at which the person completed full-time education and person number within the household. Using this method, I am able to match 99.5% of my sample[17]. As a test of the robustness of my matching algorithm, I apply my post-2001 method to the pre-2001 data and compare the resulting matches to those created with the (more reliable) pre-2001 method. The two methods generate identical

[17]Note that I also cannot match anyone after quarter 2 of 2016, as my sample ends in quarter 2 of 2017

matches except in one case. Thus, I am resonably confident that my matches are accurate. Nevertheless, it is possible that there is some measurement error, i.e. I classify some migrants as leaving before wave 5 and vice versa.

Using the method outline above, I create a variable equal to 0 if I can match a migrant between waves 1 and 5 and 1 if I only observe them in wave 1 but not wave 5. I then regress this survey attrition variable on exchange rates and the same set of covariates as in Table 1.3, separately for EU and non-EU migrants. My exchange rate measure, however, is different from the contemporaneous real exchange rate used in section 1.5. I only know whether the person left the survey within 12 months after their wave 1 interview but I cannot pinpoint the exact month of their departure. Therefore, I use an average real exchange rate over the 12 months following the month of the interview. The results, in Table 1.5, demonstrate that for EU/EFTA migrants, the probability of leaving the survey increases as a result of their home currency's depreciation. For example, the coefficient in model (4) implies that a 10% depreciation raises the probability of survey attrition by 4.7 percentage points. This is a sizable effect, equal to over 10% of the mean attrition rate in the EU/EFTA subsample of 45.1%[18]. While there may be reasons for survey attrition other than migrant returns, the fact that the coefficient for non-EU/EFTA migrants is a fairly precisely estimated zero suggests that most of the effect does indeed stem from migrants' going home in response to favorable exchange rate shocks and extending their stay in the UK in response to unfavorable ones. Note also that the unconditional means of individual attrition rates, reported at the bottom of Table 1.5, are very similar across the two groups. Taken together with the results from section 1.5.2, the results in Table 1.5 suggest both EU/EFTA and non-EU/EFTA migrants respond to favorable exchange rate shocks by reducing their labor supply in the UK, implying either that the income effect dominates the substitution effect or that they have income targets. However, the non-EU/EFTA foreign-born respond on the intensive margin – by altering their labor supply within the UK, while EU/EFTA ones respond on

[18]The results are robust to the inclusion of individual weights (appendix Table A.6) and an alternative classification of Bulgarian, Romanian, and Croatian migrants (appendix Table A.9).

the extensive margin – by adjusting the timing of their return home.

1.6.2 Migrant returns - stocks by cohort from the LFS

As a further robustness check of my individual attrition results, I check if exchange rates affect aggregate migrant attrition from the LFS. Here, I no longer rely on my matching algorithm and instead create aggregate stocks of migrants in the LFS, by country of origin, year, and arrival cohort. That is, I count the number of people from each country and arrival cohort (starting with the 1996 cohort) in my sample still present in the data in the years that follow. Each observation in the resulting dataset is at the arrival cohort by country by year level and it is the number of people from that cohort and country still remaining in the dataset in that year. I continue to only look at individuals in their first interview wave, but conduct my analysis at an annual, instead of monthly level, in order to obtain enough variation and non-zero observations. In addition, I add people who are self-employed or who have ever been employed in a paid job and are younger than 65. I regress the numer of migrants on annual real exchange rates; country of birth, year, and arrival cohort fixed effects; and time-varying country-level characteristics. Again, all coefficients are estimated separately for EU/EFTA and non-EU/EFTA foreign-born. In order to deal with the large number of zeros in the dependent variable, I estimate the model with a Poisson regression. The results are presented in Table 1.6 and they provide additional evidence that migrants return home sooner (later) in response to a depreciation (appreciation) of their home currency. The coefficient on the exchange rate implies that a 10% depreciation of EU/EFTA immigrants' home currency reduces their stock in the UK by about 16% but has no effect on non-EU/EFTA migrants. That is quite a large effect but it also estimated with a relatively wide confidence interval.

1.6.3 Migrant returns - stocks from the APS

Finally, in this section, I conduct another test of the effect of exchange rates on aggregate stocks of migrants, this time using data on overall numbers of foreign-born resident in the UK, published by the UK Office of National Statistics and derived from the Annual Population Survey. The data are at a country of origin and year level and include *all* migrants, including minors and retirees. I run the same regression as in Table 1.6, minus arrival cohort fixed effects. The results are in Table 1.7 and are broadly similar to those in Table 1.6.

1.6.4 EU/EFTA migrant returns - treatment vs. selection

My results are consistent with a hypothesis that non-EU/EFTA migrants, who face high international mobility costs when traveling in and out of the UK, react to exchange rate shocks by adjusting their labor supply within the UK. On the other hand, EU/EFTA foreign-born respond by altering the timing of their return to the home country. An interesting question is why exactly the latter occurs. One possibility is that there is an EU/EFTA "treatment effect" – once a country joins the EU, its citizens face lower costs of migration and are more likely to respond to exchange rate shocks by moving in or out of the UK. Another explanation is a selection effect – once a country joins the EU, the types of migrants it sends to the UK are different from the earlier migrants in ways that make them more likely to respond to exchange rate fluctuations in that manner (perhaps they are "opportunistic," short-term migrants but note that this selection could occur precisely because the costs of migration are lower). In order to distinguish between these two competing hypotheses, I use the fact that there is a group of countries in the sample which joined the EU during the sample period (see appendix Table A.1). I restrict the sample only to those 13 new EU member countries and only to the period when they are EU members. I then compare the cohorts that arrived in the UK before the country's membership in the EU to those that arrived after. Under the "treatment effect" hypothesis, there should be no difference in their responses to exchange rate shocks, while under the "selection" effect, the cohorts that

arrived later should be more responsive. I then regress the individual attrition variable (see section 1.6.1) on my exchange rate measure, separately for the two groups. The results are presented in Table 1.8 where the first columns, labeled as "EU cohorts," refer to migrantrs who arrived after their countries joined the EU and the "non-EU cohorts" to those who arrived earlier. The coefficients on exchanges rates are positive in all four model for the EU cohort migrants, whereas the ones for non-EU migrants are negative in models (2) – (4), lending some support to the selection hypothesis. However, they are generally not statistically significant at conventional levels and neither are the differences between them, reported in the row of Table 1.8 labeled "Ln(exrate) difference."

1.7 Robustness checks

I argue that the primary difference between EU/EFTA and non-EU/EFTA migrants which would affect their response to exchange rate shocks is the cost of migrating into and out of the UK. However, those two groups likely vary along other dimensions, some of which may also impact their response to exchange rate fluctuations. Therefore, in this section, I propose several robustness checks of my results.

1.7.1 European and high-income countries

It is possible that migrants' origin countries affect their response to exchange rates in ways that are not fully captured by the country of birth dummies and time-varying country-level controls. For example, one might expect that migrants from less developed and more developed countries face different opportunity costs of migration. Therefore, in this section I present the results of earnings and attrition regressions where the sample is limited only to countries in Europe, along with several high-income countries with meaningfully large populations in the UK: the United States, Canada, Australia, New Zealand, Japan, Hong Kong, Singapore, and South Korea. Those non-EU European and other high-income countries are likely a better comparison group to the EU/EFTA. The results for earnings are in Table 1.9

and for attrition in Table 1.10. The attrition results are quite similar to the main results in Table 1.5. For earnings, non-EU migrants seem more responsive to exchange rate shocks than in the baseline results, with an elasticity of -0.387 in model (1). However, the point estimate gets closer to zero and loses statistical significance as more controls are added. This may indicate that migrants from EU/EFTA countries are, in fact, more similar to those from other relatively wealthy countries.

1.7.2 New EU members

Another way to control for the migrants' origin countries is to limit the analysis only to the set of EU/EFTA members which joined those organizations during the sample period. In Table 1.11 and Table 1.12, I only look at the 13 new EU members which joined in 2004, 2007, and 2013. Thus, the non-EU countries are the same as the EU ones but in the years before they joined the organization. The estimated elasticity of earnings with respect to the exchange rate is very large in model (4) for non-EU migrants and significant at the 5% level. However, the sample size is very small, consisting of only 514 observations. The other exchange rate coefficients are not statistically significant. For attrition, the coefficients on exchange rates are not significant for EU/EFTA countries but are significant (at 10% in models (1) and (3) and 5% in model (2)) for non-EU/EFTA ones, except in model (4). However, due to small sample sizes, the results should be interpreted with caution.

1.7.3 Long-term migrants

There is substantial evidence in the literature that migrants are most likely to return to their home countries within the first few years after the initial migration (Constant and Massey, 2003). It is possible that migrants who remain in the UK for a longer time are more similar to one another, regardless of their countries of origin. Therefore, in Table 1.13 and Table 1.14, I present the results of regressions of earnings and attrition, respectively, where the sample is limited to migrants who have been in the UK for more than 5 years. The

results are very similar to the baseline ones: exchange rate depreciations are correlated with lower earnings for non-EU/EFTA migrants and higher attrition for EU/EFTA ones.

1.8 Conclusion

In this paper, I investigated the responses of international migrants in the UK to changes in the exchange rate of their home currencies against the UK pound. I hypothesized that migrants from EU/EFTA countries, who face lower international mobility costs vis-à-vis the UK, would respond differently to exchange rate shocks from non-EU/EFTA migrants, who generally face greater obstacles to migrating into the UK. My results show that both types of migrants respond to favorable exchange rate shocks by reducing their labor supply in the UK, but the margins of response are different for the two groups. EU/EFTA foreign-born adjust the timing of their return home, whereas non-EU/EFTA ones remain in the UK but alter their labor supply on the intensive margin. There is weak evidence that EU migrants who came to the UK after their country joined the EU are more responsive to exchange rate shocks than those who came before but more research is needed on the selection vs. treatment question.

Table 1.1: Summary statistics of LFS and country-level variables.

	EU/EFTA migrants				**Non-EU/EFTA migrants**			
	Mean	SD	10th pct	90th pct	Mean	SD	10th pct	90th pct
Panel A: Individual-level controls								
Years since arrival	16.7	15.8	2	42	19.4	14.6	3	41
Age	37.9	11.6	25	55	40.8	10.8	27	56
Female	0.52				0.47			
Married	0.52				0.74			
Less educated	0.47				0.44			
Industry of employment								
Manufacturing	0.18				0.12			
Wholesale and retail trade	0.13				0.13			
Hotels and restaurants	0.091				0.079			
Transport, storage, communication	0.074				0.07			
Real estate and renting activities	0.13				0.14			
Education	0.085				0.087			
Health and social work	0.13				0.2			
Other	0.19				0.18			
Panel B: Country-level controls								
Real GDP per capita	33.3	10.2	21	45.1	12.5	15.2	2.22	39.6
Trade with the UK	24.9	23.5	1.85	64.6	6.39	13.2	0.125	9.76
Polity2 score	9.8	0.43	9	10	5.7	4.9	-3	10
Deaths in conflicts	0.0012	0.011	0	0	0.77	1.8	0	2.2
Deaths in disasters	0.24	1.6	0	0.2	1.7	6.3	0	3
People affected by disasters	19	196	0	5.8	12379	48447	0	23215
Panel C: Labor market outcomes								
Gross weekly earnings (2010 GBP)	443	434	136	822	477	465	121	912
Actual hours in both jobs, incl. OT	37	14.2	16	52	35.9	14.5	16	51
Actual hours in main job, incl. OT	36.6	14.1	16	50	35.5	14.4	16	50
Actual hours in main job, excl. OT	34.1	12.5	16	45	33.3	12.8	16	45
Worked OT	0.14				0.11			
Has a second job	0.037				0.038			
Is looking for a new job	0.092				0.1			
Is looking for an additional job	0.083				0.09			
Would like to work more	0.16				0.18			
N	19103				29314			

Notes. OT - overtime. EU - European Union, EFTA - European Free Trade Agreement. EU/EFTA status is determined by country of birth and its membership in EU or EFTA at the time of the migrant's interview. *Would like to work more* is a union of two questions: the migrant is looking for an additional job, the migrant would like to work more hours at the current wage rate.

Figure 1.1: Real exchange rate against the pound.

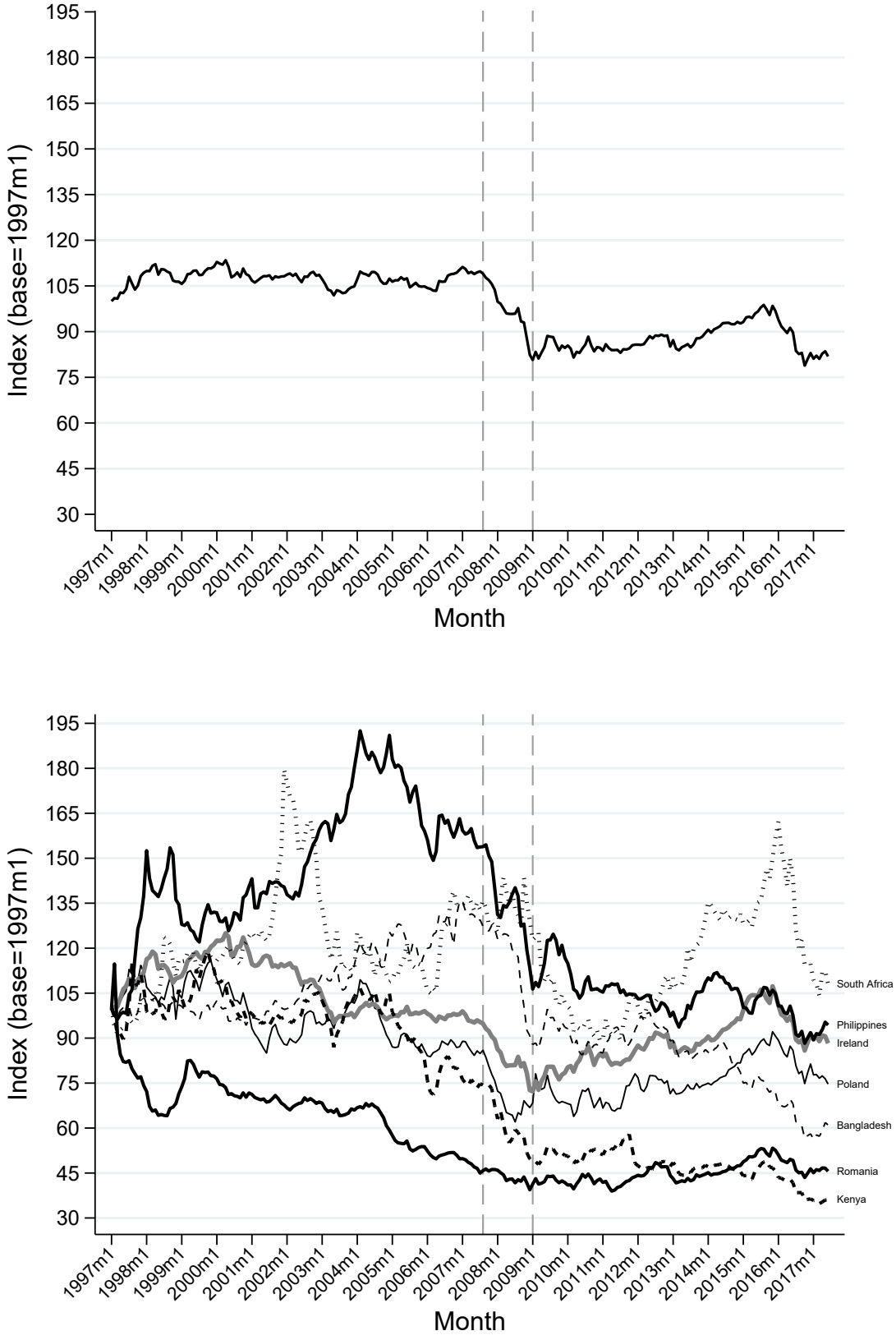


Table 1.2: Total earnings from employment and real exchange rates.

	(1)	(2)	(3)	(4)
Ln(real exrate)	-0.068*	-0.083**	-0.079***	-0.085***
	(0.037)	(0.031)	(0.025)	(0.021)
Years since arrival		0.008**	0.008**	0.007**
		(0.003)	(0.003)	(0.003)
(Years since arrival)^2		-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)
Age		0.078***	0.071***	0.074***
		(0.008)	(0.008)	(0.009)
Age^2		-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)
Female		-0.434***	-0.421***	-0.421***
		(0.038)	(0.029)	(0.029)
Married		-0.037*	-0.039**	-0.045**
		(0.019)	(0.017)	(0.018)
Less educated		-0.427***	-0.378***	-0.381***
		(0.044)	(0.046)	(0.045)
Ln(real GDP per capita)				-0.100
				(0.058)
Ln(trade with the UK)				0.017
				(0.020)
Polity 2 score				0.000
				(0.003)
Ln(deaths in conflicts)				0.001
				(0.005)
Ln(deaths in disasters)				-0.001
				(0.004)
Ln(people affected by disasters)				0.000
				(0.001)
Country of birth FE	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes
N	48417	47701	47701	38742
R^2	0.110	0.276	0.344	0.339

Notes. The dependent variable is log gross weekly earnings from one or two (if applicable) jobs. All regressions estimated with OLS, with standard errors clustered at the country of birth, month × year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Figure 1.2: Exchange rate and earnings deviations.

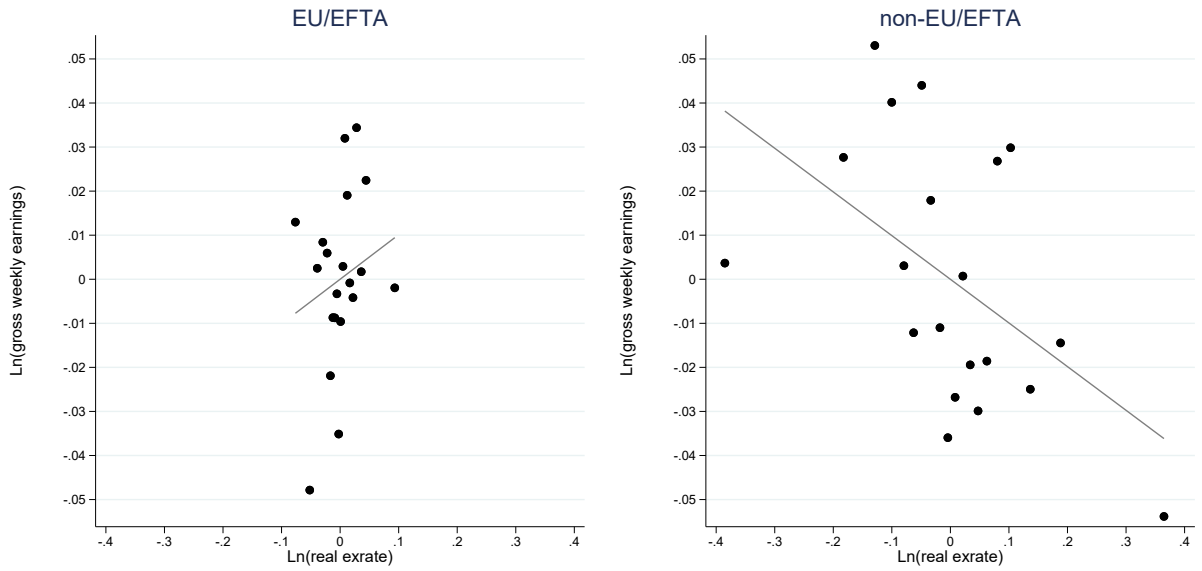


Table 1.3: Total earnings from employment and real exchange rates.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate)	0.108	0.036	-0.007	-0.041	-0.098**	-0.095***	-0.091***	-0.083***
	(0.168)	(0.128)	(0.130)	(0.177)	(0.036)	(0.030)	(0.027)	(0.027)
Years since arrival		0.001	0.001	-0.001		0.009*	0.009**	0.010**
		(0.005)	(0.004)	(0.005)		(0.004)	(0.004)	(0.004)
(Years since arrival)^2		-0.000	-0.000	-0.000		-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		0.089***	0.081***	0.087***		0.071***	0.064***	0.064***
		(0.014)	(0.013)	(0.013)		(0.007)	(0.007)	(0.008)
Age^2		-0.001***	-0.001***	-0.001***		-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.444***	-0.398***	-0.407***		-0.422***	-0.434***	-0.430***
		(0.050)	(0.047)	(0.052)		(0.046)	(0.029)	(0.029)
Married		-0.007	-0.013	-0.022		-0.042**	-0.039**	-0.040**
		(0.023)	(0.023)	(0.028)		(0.016)	(0.014)	(0.018)
Less educated		-0.361***	-0.337***	-0.343***		-0.459***	-0.388***	-0.385***
		(0.060)	(0.057)	(0.062)		(0.038)	(0.042)	(0.055)
Ln(real GDP per capita)				0.031				0.018
				(0.103)				(0.077)
Ln(trade with the UK)				-0.007				0.014
				(0.057)				(0.024)
Polity 2 score				0.035				0.002
				(0.054)				(0.004)
Ln(deaths in conflicts)				0.015				0.001
				(0.019)				(0.005)
Ln(deaths in disasters)				-0.000				-0.002
				(0.006)				(0.006)
Ln(people affected by disasters)				0.000				0.000
				(0.002)				(0.002)
Ln(real exrate) difference					0.205	0.131	0.084	0.042
					(0.182)	(0.139)	(0.140)	(0.205)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
N (sub-sample)	19103	18851	18851	15123	29314	28850	28850	23619
N	48417	47701	47701	38742	48417	47701	47701	38742
R^2	0.116	0.286	0.356	0.353	0.116	0.286	0.356	0.353

Notes. The dependent variable is log gross weekly earnings from one or two (if applicable) jobs. All regressions estimated with OLS, with standard errors clustered at the country of birth, month × year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table 1.4: Channels of earnings adjustment and real exchange rates.

	EU/EFTA countries		**Non-EU/EFTA countries**		**Difference**	
	(3)	(4)	(3)	(4)	(3)	(4)
Ln(actual hours in both jobs, incl. OT)	0.082	0.093	-0.017	-0.026	0.099	0.120
	(0.098)	(0.107)	(0.010)	(0.017)	(0.102)	(0.124)
Ln(actual hours in main job, incl. OT)	0.091	0.098	-0.019*	-0.032*	0.110	0.130
	(0.108)	(0.110)	(0.010)	(0.017)	(0.111)	(0.120)
Ln(actual hours in main job, excl. OT)	0.110	0.116	-0.017*	-0.029	0.127	0.145
	(0.099)	(0.105)	(0.010)	(0.017)	(0.103)	(0.112)
Worked OT	-0.053	-0.110	0.006	0.003	-0.059	-0.112
	(0.072)	(0.068)	(0.015)	(0.016)	(0.072)	(0.069)
Has a second job	0.006	0.007	0.008*	0.013	-0.002	-0.006
	(0.020)	(0.025)	(0.005)	(0.008)	(0.025)	(0.031)
Is looking for a new job	-0.025***	-0.033	-0.012	-0.026*	-0.014	-0.007
	(0.005)	(0.022)	(0.007)	(0.013)	(0.031)	(0.033)
Is looking for an additional job	-0.008	0.012	-0.017**	-0.032**	0.009	0.044
	(0.018)	(0.034)	(0.008)	(0.014)	(0.032)	(0.038)
Would like to work more	0.001	0.001	-0.007	-0.029	0.009	0.030
	(0.009)	(0.034)	(0.014)	(0.018)	(0.014)	(0.051)

Notes. The dependent variable is indicated in the first column. *Would like to work more* is a union of two questions: the migrant is looking for an additional job, the migrant would like to work more hours at the current wage rate. Only regression models (3) and (4) from Table 1.3 are presented. The last two columns show the difference in coefficients between EU and non-EU migrants. All regressions estimated with OLS, with standard errors clustered at the country of birth, month × year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Figure 1.3: Survey attrition among the foreign-born and UK-born.

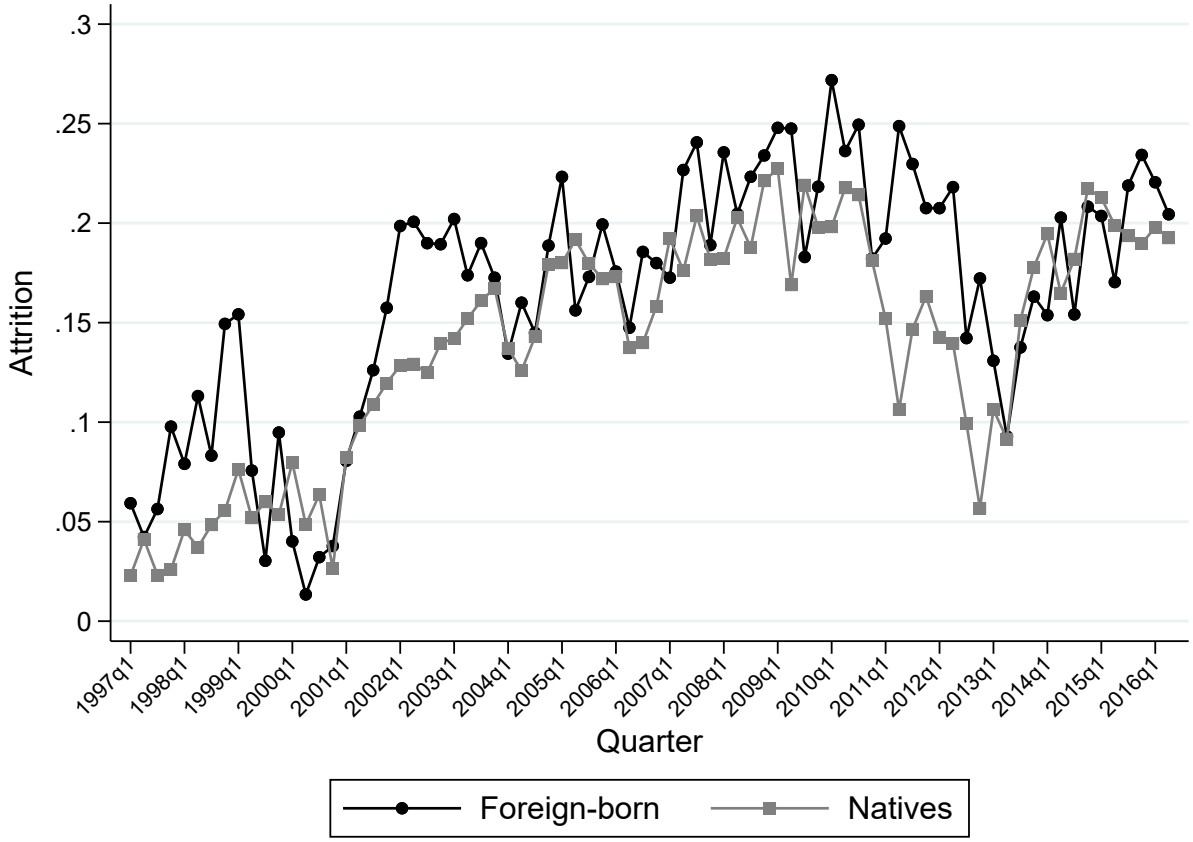


Figure 1.4: Survey attrition among UK-born natives and effective exchange rates.

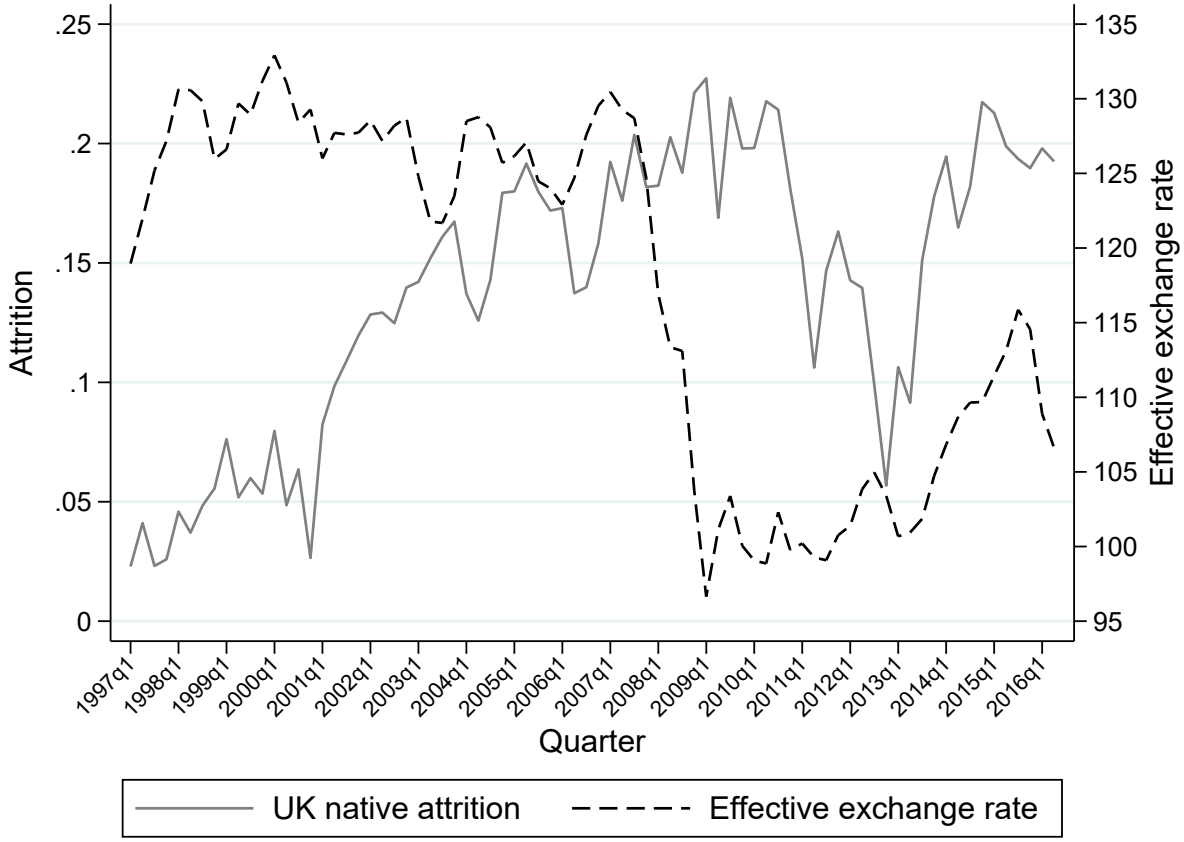


Table 1.5: Attrition from the survey and real exchange rates.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate over next 12 mos.)	0.365*	0.433***	0.435***	0.467***	-0.011	-0.005	-0.005	-0.008
	(0.188)	(0.137)	(0.134)	(0.154)	(0.020)	(0.016)	(0.015)	(0.015)
Years since arrival		-0.013***	-0.012***	-0.013***		-0.005***	-0.005***	-0.005***
		(0.002)	(0.002)	(0.002)		(0.001)	(0.001)	(0.001)
(Years since arrival)^2		0.000***	0.000***	0.000***		0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		-0.023***	-0.022***	-0.022***		-0.022***	-0.021***	-0.022***
		(0.004)	(0.003)	(0.004)		(0.002)	(0.002)	(0.002)
Age^2		0.000***	0.000***	0.000***		0.000***	0.000***	0.000***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.036***	-0.029**	-0.035***		-0.025***	-0.022**	-0.027**
		(0.010)	(0.010)	(0.012)		(0.008)	(0.008)	(0.009)
Married		-0.048***	-0.048***	-0.049**		-0.047***	-0.045***	-0.049***
		(0.014)	(0.014)	(0.019)		(0.011)	(0.012)	(0.015)
Less educated		0.025**	0.020	0.014		0.024***	0.019**	0.017*
		(0.011)	(0.013)	(0.015)		(0.008)	(0.007)	(0.009)
Ln(real GDP per capita)				-0.061				0.027
				(0.128)				(0.068)
Ln(trade with the UK)				-0.046				-0.001
				(0.061)				(0.027)
Polity 2 score				0.042***				0.002
				(0.014)				(0.002)
Ln(deaths in conflicts)				-0.002				0.003
				(0.009)				(0.003)
Ln(deaths in disasters)				-0.004				-0.004
				(0.005)				(0.003)
Ln(people affected by disasters)				0.005*				-0.001
				(0.002)				(0.001)
Ln(exrate) difference					0.376*	0.438***	0.441***	0.475***
					(0.190)	(0.140)	(0.137)	(0.156)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
Attrition in sub-sample	0.456	0.455	0.455	0.451	0.447	0.446	0.446	0.439
N (sub-sample)	17492	17358	17358	15114	27461	27135	27135	23590
N	44953	44493	44493	38704	44953	44493	44493	38704
R^2	0.055	0.094	0.096	0.099	0.055	0.094	0.096	0.099

Notes. The dependent variable is individual-level attrition from the LFS. It is equal to 0 if the migrant is observed in both waves 1 and 5 of the LFS and 1 if they are observed only in wave 1. All regressions estimated with OLS, with standard errors clustered at the country of birth, month × year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table 1.6: Aggregate migrant stocks from the LFS, by arrival cohort.

	EU/EFTA countries			**Non-EU/EFTA countries**		
	(1)	(2)	(3)	(1)	(2)	(3)
Log(real exrate)	-1.741*	-1.599*	-1.568*	-0.172	-0.041	-0.048
	(0.890)	(0.910)	(0.887)	(0.128)	(0.097)	(0.096)
Log(population)		-4.005***	-4.086***		2.474***	2.527***
		(1.148)	(1.145)		(0.849)	(0.819)
Log(real GDP per capita)		0.700**	0.649***		1.255***	1.214***
		(0.285)	(0.187)		(0.423)	(0.429)
Ln(trade flow)		0.042	0.019		-0.013	-0.010
		(0.216)	(0.208)		(0.053)	(0.053)
Polity2 score		0.064	0.057		0.027***	0.028**
		(0.051)	(0.056)		(0.009)	(0.013)
Log(deaths in organized violence)			0.007			-0.012
			(0.020)			(0.019)
Log(deaths from disasters)			0.020			-0.019
			(0.028)			(0.012)
Log(people affected by disasters)			-0.006			0.003
			(0.012)			(0.012)
Ln(exrate) difference				-1.569*	-1.558*	-1.520*
				(0.884)	(0.915)	(0.898)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Arrival cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
N (sub-sample)	5943	4079	4079	20652	13830	13830
N	26595	17909	17909	26595	17909	17909
R^2						

Notes. The dependent variable is the number of migrants in a particular country of birth × year × year of arrival in the UK cell. All regressions estimated with a Poisson model, with standard errors clustered at the country of birth, year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table 1.7: Aggregate foreign-born stock estimates, PPML.

	EU countries			**Non-EU countries**		
	(1)	(2)	(3)	(1)	(2)	(3)
Log(real exrate)	-2.430**	-1.917***	-1.874***	-0.075	-0.060	-0.060
	(1.060)	(0.733)	(0.721)	(0.055)	(0.047)	(0.047)
Log(population)		-3.621***	-3.710***		-0.231	-0.249
		(1.205)	(1.224)		(0.512)	(0.499)
Log(real GDP per capita)		0.702	0.730		0.298**	0.318**
		(0.509)	(0.473)		(0.140)	(0.145)
Ln(trade flow)		0.233	0.215		0.033	0.026
		(0.177)	(0.173)		(0.069)	(0.068)
Polity2 score		-0.018	-0.021		0.015**	0.013**
		(0.040)	(0.035)		(0.006)	(0.006)
Log(deaths in organized violence)			-0.058			0.008
			(0.037)			(0.006)
Log(deaths from disasters)			0.005			-0.003
			(0.012)			(0.006)
Log(people affected by disasters)			-0.002			0.001
			(0.004)			(0.003)
Ln(exrate) difference				-2.356**	-1.857**	-1.814**
				(1.062)	(0.733)	(0.722)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N (sub-sample)	470	354	354	1719	1210	1210
N	2189	1564	1564	2189	1564	1564

Notes. The dependent variable is the number of migrants in a particular country of birth × year × cell, estimated by the ONS. All regressions estimated with a Poisson model, with standard errors clustered at the country of birth, year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table 1.8: Attrition from the survey and real exchange rates, EU countries only, by cohort.

	EU cohorts				**Non-EU cohorts**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate over next 12 mos.)	0.243	0.354	0.346	0.668*	0.076	-0.198	-0.159	-0.079
	(0.235)	(0.238)	(0.238)	(0.354)	(0.237)	(0.384)	(0.395)	(0.514)
Years since arrival		-0.052***	-0.052***	-0.055**		-0.009	-0.008	-0.007
		(0.014)	(0.014)	(0.022)		(0.006)	(0.006)	(0.008)
(Years since arrival)^2		0.003**	0.003**	0.003		0.000	0.000	-0.000
		(0.001)	(0.001)	(0.002)		(0.000)	(0.000)	(0.000)
Age		-0.027***	-0.026***	-0.030***		-0.026***	-0.024***	-0.027**
		(0.005)	(0.005)	(0.007)		(0.007)	(0.006)	(0.011)
Age^2		0.000***	0.000***	0.000***		0.000***	0.000***	0.000**
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.011	-0.008	-0.023		-0.102***	-0.090***	-0.103**
		(0.024)	(0.026)	(0.027)		(0.023)	(0.021)	(0.041)
Married		-0.074***	-0.075***	-0.085**		-0.033	-0.039	-0.048
		(0.018)	(0.017)	(0.029)		(0.040)	(0.044)	(0.057)
Less educated		0.014	0.011	-0.007		0.033	0.028	0.044
		(0.017)	(0.018)	(0.030)		(0.029)	(0.034)	(0.052)
Ln(real GDP per capita)				-0.347				0.259
				(0.329)				(0.715)
Ln(trade with the UK)				0.118				-0.028
				(0.072)				(0.260)
Polity 2 score				0.082				0.118
				(0.104)				(0.115)
Ln(deaths in conflicts)				0.000				0.000
				(0.000)				(0.000)
Ln(deaths in disasters)				0.004				-0.003
				(0.013)				(0.022)
Ln(people affected by disasters)				0.006				0.014
				(0.006)				(0.010)
Ln(exrate) difference					0.168	0.552	0.505	0.747
					(0.182)	(0.371)	(0.382)	(0.541)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
Attrition in sub-sample	0.612	0.611	0.611	0.622	0.444	0.444	0.444	0.474
N (sub-sample)	5130	5071	5071	4024	1358	1347	1347	1037
N	7159	7082	7082	5584	6488	6418	6418	5061
R^2	0.132	0.167	0.172	0.187	0.113	0.149	0.153	0.167

Notes. The dependent variable is individual-level attrition from the LFS. It is equal to 0 if the migrant is observed in both waves 1 and 5 of the LFS and 1 if they are observed only in wave 1. Only migrants EU/EFTA countries at the time of the interview are included. EU cohorts refers to migrants who arrived in the UK when their country of birth was already a member of the EU/EFTA. All regressions estimated with OLS, with standard errors clustered at the country of birth, month × year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table 1.9: Total earnings from employment and real exchange rates. Only European and high-income countries included.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate)	0.104	0.034	-0.012	-0.052	-0.387***	-0.275**	-0.225*	-0.046
	(0.168)	(0.131)	(0.134)	(0.192)	(0.100)	(0.112)	(0.121)	(0.211)
Years since arrival		0.002	0.001	-0.000		-0.004	-0.002	-0.002
		(0.005)	(0.005)	(0.005)		(0.006)	(0.004)	(0.004)
(Years since arrival)^2		-0.000	-0.000	-0.000		0.000	0.000	-0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		0.089***	0.082***	0.087***		0.090***	0.078***	0.076***
		(0.014)	(0.013)	(0.014)		(0.010)	(0.010)	(0.016)
Age^2		-0.001***	-0.001***	-0.001***		-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.444***	-0.399***	-0.408***		-0.543***	-0.464***	-0.483***
		(0.051)	(0.048)	(0.061)		(0.040)	(0.040)	(0.049)
Married		-0.008	-0.014	-0.023		-0.054	-0.046	-0.035
		(0.025)	(0.023)	(0.032)		(0.031)	(0.030)	(0.035)
Less educated		-0.360***	-0.335***	-0.341***		-0.492***	-0.433***	-0.429***
		(0.062)	(0.058)	(0.058)		(0.064)	(0.044)	(0.051)
Ln(real GDP per capita)				0.005				0.719***
				(0.104)				(0.152)
Ln(trade with the UK)				-0.002				-0.021
				(0.060)				(0.058)
Polity 2 score				0.035				-0.008
				(0.059)				(0.023)
Ln(deaths in conflicts)				0.015				-0.035*
				(0.018)				(0.017)
Ln(deaths in disasters)				-0.000				-0.004
				(0.007)				(0.017)
Ln(people affected by disasters)				0.000				0.000
				(0.003)				(0.006)
Ln(real exrate) difference					0.492**	0.309**	0.212	-0.006
					(0.179)	(0.117)	(0.125)	(0.282)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
N (sub-sample)	18771	18523	18523	14853	4624	4570	4570	3259
N	23395	23093	23093	18112	23395	23093	23093	18112
R^2	0.136	0.321	0.378	0.382	0.136	0.321	0.378	0.382

Notes. The dependent variable is log gross weekly earnings from one or two (if applicable) jobs. Only includes countries in Europe, the United States, Canada, Australia, New Zealand, Hong Kong, Japan, South Korea, Singapore. All regressions estimated with OLS, with standard errors clustered at the country of birth, month × year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table 1.10: Attrition from the survey and real exchange rates. European and high-income countries only.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate over next 12 mos.)	0.369*	0.438***	0.441***	0.466***	0.065	0.063	0.068	0.026
	(0.189)	(0.137)	(0.135)	(0.157)	(0.095)	(0.092)	(0.089)	(0.086)
Years since arrival		-0.013***	-0.013***	-0.013***		-0.010***	-0.010***	-0.009***
		(0.002)	(0.002)	(0.002)		(0.001)	(0.002)	(0.002)
(Years since arrival)^2		0.000***	0.000***	0.000***		0.000***	0.000**	0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		-0.023***	-0.022***	-0.021***		-0.023***	-0.022***	-0.028***
		(0.003)	(0.003)	(0.004)		(0.004)	(0.005)	(0.005)
Age^2		0.000***	0.000***	0.000***		0.000***	0.000***	0.000***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.033***	-0.027**	-0.033**		-0.029**	-0.028*	-0.032
		(0.010)	(0.011)	(0.011)		(0.013)	(0.015)	(0.022)
Married		-0.049***	-0.049***	-0.050**		-0.061***	-0.057***	-0.073***
		(0.015)	(0.014)	(0.019)		(0.018)	(0.018)	(0.024)
Less educated		0.025**	0.020	0.014		0.029	0.025	-0.000
		(0.010)	(0.012)	(0.015)		(0.021)	(0.020)	(0.028)
Ln(real GDP per capita)				-0.073				0.270**
				(0.129)				(0.114)
Ln(trade with the UK)				-0.051				-0.037
				(0.061)				(0.034)
Polity 2 score				0.042**				-0.021
				(0.015)				(0.016)
Ln(deaths in conflicts)				-0.002				0.002
				(0.013)				(0.013)
Ln(deaths in disasters)				-0.004				-0.010
				(0.006)				(0.010)
Ln(people affected by disasters)				0.005*				0.004
				(0.003)				(0.007)
Ln(exrate) difference					0.304	0.375**	0.372**	0.440***
					(0.211)	(0.166)	(0.162)	(0.136)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
Attrition in sub-sample	0.457	0.456	0.456	0.453	0.402	0.400	0.400	0.398
N (sub-sample)	17183	17049	17049	14844	4366	4335	4335	3258
N	21549	21384	21384	18102	21549	21384	21384	18102
R^2	0.080	0.130	0.133	0.143	0.080	0.130	0.133	0.143

Notes. The dependent variable is individual-level attrition from the LFS. It is equal to 0 if the migrant is observed in both waves 1 and 5 of the LFS and 1 if they are observed only in wave 1. Only includes migrants from countries in Europe, the United States, Canada, Australia, New Zealand, Japan, Hong Kong, Singapore, South Korea. All regressions estimated with OLS, with standard errors clustered at the country of birth, month × year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table 1.11: Total earnings from employment and real exchange rates. Only new EU/EFTA members included.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate)	-0.190	-0.147	-0.175	-0.221	-0.594	-0.540	-0.490	-1.823**
	(0.155)	(0.105)	(0.104)	(0.135)	(0.634)	(0.466)	(0.608)	(0.715)
Years since arrival		0.023***	0.021***	0.025***		0.039***	0.027***	0.023**
		(0.005)	(0.005)	(0.006)		(0.009)	(0.007)	(0.009)
(Years since arrival)^2		-0.000**	-0.000*	-0.000**		-0.000**	-0.000**	-0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		0.039***	0.034***	0.036**		0.095***	0.089***	0.103***
		(0.010)	(0.010)	(0.013)		(0.020)	(0.017)	(0.023)
Age^2		-0.000***	-0.000***	-0.000**		-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.333***	-0.299***	-0.293***		-0.756***	-0.644***	-0.655***
		(0.044)	(0.045)	(0.054)		(0.074)	(0.084)	(0.099)
Married		-0.002	-0.008	-0.021		0.027	0.045	0.001
		(0.021)	(0.023)	(0.035)		(0.086)	(0.078)	(0.079)
Less educated		-0.207***	-0.201***	-0.179***		-0.453***	-0.434***	-0.369***
		(0.063)	(0.065)	(0.047)		(0.067)	(0.059)	(0.100)
Ln(real GDP per capita)				-0.038				-0.296
				(0.201)				(1.668)
Ln(trade with the UK)				0.007				-0.407**
				(0.063)				(0.134)
Polity 2 score				0.323				0.078**
				(0.205)				(0.031)
Ln(deaths in conflicts)				0.000				0.000
				(0.000)				(0.000)
Ln(deaths in disasters)				-0.009				-0.028
				(0.011)				(0.032)
Ln(people affected by disasters)				0.000				-0.007
				(0.008)				(0.013)
Ln(real exrate) difference					0.405	0.393	0.314	1.602*
					(0.671)	(0.545)	(0.691)	(0.774)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
N (sub-sample)	7519	7425	7425	5150	668	654	654	514
N	8187	8079	8079	5664	8187	8079	8079	5664
R^2	0.097	0.258	0.312	0.325	0.097	0.258	0.312	0.325

Notes. The dependent variable is log gross weekly earnings from one or two (if applicable) jobs. Only includes new EU and EFTA members, i.e. those which joined either organization during the sample period. All regressions estimated with OLS, with standard errors clustered at the country of birth, month × year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table 1.12: Attrition from the survey and real exchange rates. Only new EU/EFTA members are included.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate over next 12 mos.)	0.118	0.151	0.140	0.213	0.819*	0.778**	0.703*	0.215
	(0.225)	(0.203)	(0.194)	(0.355)	(0.449)	(0.339)	(0.371)	(0.489)
Years since arrival		-0.017***	-0.016***	-0.016***		-0.008*	-0.007	-0.008
		(0.002)	(0.003)	(0.003)		(0.004)	(0.005)	(0.008)
(Years since arrival)^2		0.000***	0.000***	0.000**		0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		-0.030***	-0.030***	-0.034***		-0.019	-0.016	-0.013
		(0.002)	(0.002)	(0.005)		(0.013)	(0.012)	(0.011)
Age^2		0.000***	0.000***	0.000***		0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.029	-0.024	-0.034		-0.059	-0.049	-0.077
		(0.021)	(0.026)	(0.026)		(0.037)	(0.039)	(0.061)
Married		-0.070***	-0.070***	-0.080***		-0.075*	-0.079**	-0.109
		(0.015)	(0.016)	(0.026)		(0.039)	(0.034)	(0.072)
Less educated		0.022*	0.019	0.010		0.006	-0.011	-0.038
		(0.012)	(0.015)	(0.019)		(0.055)	(0.060)	(0.070)
Ln(real GDP per capita)				-0.019				-0.356
				(0.267)				(1.086)
Ln(trade with the UK)				0.015				0.137
				(0.099)				(0.220)
Polity 2 score				0.054				-0.037
				(0.118)				(0.030)
Ln(deaths in conflicts)				0.000				0.000
				(0.000)				(0.000)
Ln(deaths in disasters)				-0.001				-0.014
				(0.010)				(0.025)
Ln(people affected by disasters)				0.007				0.012
				(0.005)				(0.009)
Ln(exrate) difference					-0.701	-0.627	-0.563	-0.002
					(0.550)	(0.414)	(0.417)	(0.574)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
Attrition in sub-sample	0.574	0.572	0.572	0.588	0.351	0.349	0.349	0.372
N (sub-sample)	6581	6509	6509	5143	661	654	654	514
N	7242	7163	7163	5657	7242	7163	7163	5657
R^2	0.102	0.146	0.151	0.163	0.102	0.146	0.151	0.163

Notes. The dependent variable is individual-level attrition from the LFS. It is equal to 0 if the migrant is observed in both waves 1 and 5 of the LFS and 1 if they are observed only in wave 1. Only includes EU/EFTA countries which joined either organization during the sample period. All regressions estimated with OLS, with standard errors clustered at the country of birth, month × year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table 1.13: Total earnings from employment and real exchange rates. Migrants with more than 5 years since arrival.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate)	0.166	0.067	0.042	0.077	-0.099***	-0.095***	-0.083***	-0.080**
	(0.162)	(0.107)	(0.092)	(0.150)	(0.029)	(0.026)	(0.024)	(0.030)
Years since arrival		-0.005	-0.004	-0.007		0.012**	0.010**	0.010**
		(0.004)	(0.003)	(0.004)		(0.004)	(0.004)	(0.004)
(Years since arrival)^2		0.000	0.000	0.000		-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		0.101***	0.095***	0.102***		0.067***	0.063***	0.063***
		(0.010)	(0.010)	(0.009)		(0.009)	(0.008)	(0.009)
Age^2		-0.001***	-0.001***	-0.001***		-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.512***	-0.468***	-0.482***		-0.435***	-0.449***	-0.448***
		(0.046)	(0.044)	(0.049)		(0.047)	(0.031)	(0.036)
Married		-0.024	-0.031	-0.047		-0.059***	-0.050**	-0.057**
		(0.021)	(0.022)	(0.036)		(0.020)	(0.018)	(0.021)
Less educated		-0.398***	-0.377***	-0.385***		-0.469***	-0.402***	-0.402***
		(0.051)	(0.057)	(0.055)		(0.038)	(0.043)	(0.050)
Ln(real GDP per capita)				-0.015				0.000
				(0.210)				(0.074)
Ln(trade with the UK)				0.020				0.004
				(0.082)				(0.022)
Polity 2 score				-0.009				0.001
				(0.039)				(0.004)
Ln(deaths in conflicts)				0.019*				0.001
				(0.010)				(0.007)
Ln(deaths in disasters)				0.004				-0.002
				(0.010)				(0.006)
Ln(people affected by disasters)				0.001				0.001
				(0.004)				(0.003)
Ln(real exrate) difference					0.265	0.161	0.124	0.156
					(0.181)	(0.137)	(0.124)	(0.200)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
N (sub-sample)	12960	12776	12776	10126	23489	23107	23107	18502
N	36449	35883	35883	28628	36449	35883	35883	28628
R^2	0.110	0.289	0.356	0.353	0.110	0.289	0.356	0.353

Notes. The dependent variable is log gross weekly earnings from one or two (if applicable) jobs. Only migrants with 5 or more years since arrival are included. All regressions estimated with OLS, with standard errors clustered at the country of birth, month × year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table 1.14: Attrition from the survey and real exchange rates. Migrants with 5 or more years since arrival.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate over next 12 mos.)	0.367***	0.427***	0.426**	0.647***	-0.009	-0.009	-0.009	-0.019
	(0.110)	(0.143)	(0.148)	(0.190)	(0.013)	(0.013)	(0.013)	(0.018)
Years since arrival		-0.003**	-0.003*	-0.003		0.002	0.002	0.002
		(0.001)	(0.001)	(0.002)		(0.001)	(0.001)	(0.002)
(Years since arrival)^2		0.000	0.000	0.000		-0.000***	-0.000***	-0.000**
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		-0.022***	-0.021***	-0.021***		-0.021***	-0.020***	-0.021***
		(0.003)	(0.003)	(0.003)		(0.003)	(0.003)	(0.003)
Age^2		0.000***	0.000***	0.000***		0.000***	0.000***	0.000***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.029**	-0.020	-0.022		-0.023**	-0.018*	-0.024
		(0.011)	(0.012)	(0.017)		(0.009)	(0.010)	(0.016)
Married		-0.033**	-0.033*	-0.036		-0.027**	-0.027*	-0.028*
		(0.015)	(0.016)	(0.028)		(0.011)	(0.013)	(0.014)
Less educated		0.031*	0.026	0.018		0.031***	0.025**	0.022
		(0.017)	(0.019)	(0.034)		(0.009)	(0.009)	(0.014)
Ln(real GDP per capita)				0.166*				0.023
				(0.091)				(0.063)
Ln(trade with the UK)				0.003				-0.018
				(0.041)				(0.022)
Polity 2 score				0.054***				0.001
				(0.015)				(0.002)
Ln(deaths in conflicts)				-0.012				0.002
				(0.015)				(0.004)
Ln(deaths in disasters)				-0.005				-0.004
				(0.010)				(0.004)
Ln(people affected by disasters)				0.007*				-0.000
				(0.004)				(0.002)
Ln(exrate) difference					0.377***	0.436***	0.435**	0.666***
					(0.113)	(0.146)	(0.150)	(0.195)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
Attrition in sub-sample	0.366	0.366	0.366	0.355	0.413	0.412	0.412	0.402
N (sub-sample)	11818	11748	11748	10121	21869	21623	21623	18477
N	33687	33371	33371	28598	33687	33371	33371	28598
R^2	0.053	0.071	0.073	0.071	0.053	0.071	0.073	0.071

Notes. The dependent variable is individual-level attrition from the LFS. It is equal to 0 if the migrant is observed in both waves 1 and 5 of the LFS and 1 if they are observed only in wave 1. Only migrants with 5 or more years since arrival are included. All regressions estimated with OLS, with standard errors clustered at the country of birth, month × year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

CHAPTER II

The Effect of Bilateral Labor Agreements on Migration and Remittances: Evidence from the Philippines

2.1 Introduction

Roughly 10 percent of the world's population—an estimated 736 million people—live on less than US $ 1.90 per day.[1] A large body of social science research has tried to address this problem by testing interventions designed to increase household income. Although many of these interventions have been shown to produce substantial gains (e.g., Blattman et al., 2016; Banerjee et al., 2015), the magnitude of the best existing interventions are about 40 times smaller than the potential gains from allowing a poor person to work in a rich country (Pritchett, 2018). For instance, Clemens et al. (2019) estimated real wage gains for migrants from 42 countries moving to the United States, and they found that the workers in the United States made 4.5 times as much as observably equivalent workers in the median origin country.[2] These benefits are typically unrealized, however, because developed countries erect substantial barriers to migration. In fact, the annual global costs of existing restrictions on

[1] These numbers are from the most recent World Bank estimates as of Fall 2019, but they are based on 2015 data. *See* World Bank, Decline of Global Extreme Poverty Continues but Has Slowed, *Press Release, September 18, 2019*, available at https://www.worldbank.org/en/news/press-release/2018/09/19/decline-of-global-extreme-poverty-continues-but-has-slowed-world-bank (accesed December 18th, 2020).

[2] Clemens et al. (2019) estimate real wage gaps for workers with a range of demographic characteristics, but these estimates are based on their primary focus: "low-skill males educated abroad (35-39 years old, nine to twelve years of education acquired in the home country)."

migration are estimated to be trillions of dollars a year (di Giovanni et al., 2015; Kennan, 2013; Benhabib and Jovanovic, 2012; Clemens, 2011). Finding ways to facilitate legal labor migration under clearly defined terms is thus perhaps the most promising way to promote development.

One way developing countries have tried to facilitate labor migration is by signing Bilateral Labor Agreements (BLAs). BLAs are international treaties that regulate flows of workers. Although they have received little academic attention, hundreds of BLAs have been signed during the post-war period. The exact provisions and purposes of these treaties vary, but they typically are signed by relatively poor source countries with excess labor capacity and relatively wealthier host countries with excess labor demand. The standard explanation for why treaties are used to regulate these relationships is that both countries can benefit from BLAs: the source countries want workers to gain employment in foreign countries and guarantees that their workers will be treated better while they are abroad, and the host countries want help screening and repatriating migrants.

However, little is known about whether BLAs actually affect the flow or treatment of workers. This is for two reasons. First, until recently, there was simply not data on which countries had signed BLAs. Unlike other kinds of international political economy treaties—e.g. Preferential Trade Agreements and Bilateral Investment Treaties—BLAs are not consistently tracked by any international organization. It is only in the last few years that academics have built datasets that document which countries have signed these agreements (Chilton and Posner, 2018; Peters, 2019). Second, there are limits to existing datasets on bilateral flows of workers and remittances. Migration flows are not tracked annually for all pairs of countries, and remittance data has not been widely collected until recently. This makes it difficult to use cross-country data to make strong claims about the benefits of BLAs for the countries that sign these agreements.

The lack of academic attention paid to BLAs is notable, especially given the large literature that has examined the effects of other forms of international treaties. For instance, a

large number of papers have studied the effect of human rights treaties (e.g., Lupu, 2015; Simmons, 2009), international trade treaties (e.g., Eicher and Henn, 2011; Rose, 2004), Bilateral Taxation Treatments (BTTs) (e.g., Azémara and Dharmapala, 2019; Blonigen et al., 2014; Blonigen and Davies, 2004), and Bilateral Investment Treaties (BITs) (e.g., Neumayer and Spess, 2005; Peinhardt. and Allee, 2012). The literature that is perhaps most relevant to BLAs is the body of research on BITs. BITs are similar to BLAs in that the agreements are signed by pairs of countries where the expectation is typically that flows (capital in the case of BITs and migrants in the case of BLAs) will go in one direction. Although there has been some evidence that BITs increase investment, the most recent and sophisticated empirical evidence suggests that BITs have no effect on investment flows (Jones, 2019; Poulsen, 2010). One explanation for this finding is that BITs do not offer enough concrete changes to alter private parties' investment decisions, but that, even despite knowing that, countries still sign the agreements because there are modest political benefits to doing so (Chilton, 2016; Poulsen and Aisbett, 2016). The evidence from other treaties generally, and BITs specifically, thus provide good reason to be at least initially skeptical about whether BLAs would result in changes to behavior.

In this paper, we overcome previous data limitations that have made it impossible to systematically study the effect of BLAs by focusing on a single country: the Philippines. The Philippines is unique among large countries because it has made exporting labor a central part of its development strategy. As part of that strategy, in 1982 the government created the Philippine Overseas Employment Administration (POEA) to promote and protect Filipinos working abroad. The POEA both has signed a large number of BLAs – 68 by 2019 – and collected a range of data on Overseas Filipino Workers (OFWs). By combining new data on BLAs and administrative data from the Philippine government, we are thus able to conduct what we believe is the most comprehensive examination of the effect of BLAs.

Specifically, we study the Philippine BLA program in two ways. First, we examine whether the Philippines having a BLA with another country is associated with larger de-

ployment of Filipino workers to that country. Using annual data on new hires of OFWs from the POEA, we find that signing a BLA is at best weakly associated with an increase in new hires overall, but the results are far from statistically significant. Second, we investigate whether the Philippines having a BLA with another country is associated with larger remittances to the Philippines from that country. Using monthly remittance data from the Philippine Central Bank, we find that having signed a BLA is not meaningfully associated with remittances. Taken together, this evidence is consistent with the theory that the benefits of BLAs are likely to be modest at best.

This paper proceeds as follows. Part 2.2 provides background on BLAs and the Philippine system of regulating labor migration. Part 2.3 explains our data on the Philippine BLAs, labor deployment, and remittances. Part 2.4 describes our research design. We then turn to presenting our results. Part 2.5.1 reports our results assessing the relationship between signing BLAs and deployment of workers, Part 2.5.2 reports our results on the relationship between signing BLAs and remittances to the Philippines, and Parts 2.5.3 and 2.5.4 explore the robustness of our results to a range of alternative specifications. Finally, Part 2.6 concludes by discussing the limitations of our results and the need for future research.

2.2 Background

2.2.1 The Rise and Content of Bilateral Labor Agreements

Bilateral Labor Agreements are legal instruments used to regulate labor migration between two countries. The pair of countries that negotiate a BLA typically explicitly includes one "home" country where the migrants are likely to leave and one "host" country where the migrants go to work. Although the first BLA may have been signed as early as 1893, this form of treaty only became common after 1945.[3] Since WWII, the ratification of these treaties has fallen into roughly three periods (Wickramasekara, 2015).

[3] For a review of the history of BLAs, see Wickramasekara (2015); Plotnikova (2011); Trachtman (2009).

To illustrate, Panel A of Figure 2.1 plots the total number of BLAs that have been signed from 1945 to 2018 using data from the Bilateral Labor Agreements Dataset (Chilton et al., 2017). The first period of BLA formation lasted from roughly 1945 to 1973 and was primarily defined by rich European countries entering into BLAs with poorer countries on the periphery of Europe. The second period of BLA signing lasted from roughly 1974 to 1989 and was characterized by limited use of BLAs as stagnation and slow growth in developed economies reduced the demand for foreign workers. The third period began in roughly 1990 and is notable for the rise of treaties between rich countries in Europe and the Middle East and labor exporting countries in Eastern Europe and Asia. Although the exact number of BLAs that were signed during these three periods is unknown, data from the Bilateral Labor Agreements database suggests that nearly 600 BLAs were signed between 1945 and 2015.[4]

These approximately 600 BLAs have been signed by a diverse array of countries. Panel B of Figure 2.1 presents the number of BLAs signed by each country. As the figure shows, countries in all regions of the world have signed BLAs. The most prolific signers of BLAs are primarily major destinations for migrant laborers, including Qatar (22), Canada (34), Germany (41), Italy (58), and France (98). But many labor sending countries have also signed multiple BLAs, e.g. Indonesia (15), Tunisia (19), and the Philippines (68). In total, at least 125 different countries have signed at least one BLA (of these, 30 countries signed exactly 1 BLA).

In addition to having been signed by a diverse group of countries, BLAs vary substantially by content. There are BLAs that are long, detailed, and legally binding treaties; there are BLAs that are short, vague, and non-binding memoranda of understanding; and there are a large number of BLAs that fall somewhere between these poles. Despite this considerable diversity, there are four elements that are common to most BLAs (Chilton and Posner,

[4] As previously noted, the exact number of BLAs is unknown because there is not a centralized international organization that systematically tracks them, and individual countries do not even keep good records of their own BLAs. This has resulted in different estimates of the number of BLAs. For instance, Chilton and Posner (2018) identified 582 BLAs and Peters (2019) identified 779 BLAs. It is unclear, however, whether these differences are due to definitional issues, as many BLAs are amendments, protocols, or simple renewals of previously signed agreements.

2018; Trachtman, 2009). First, they typically establish criteria that potential migrants must meet when seeking employment in the host country (for instance, criteria related to health, criminal records, or qualifications). Second, BLAs typically require the source state to take steps to screen workers before they depart and to facilitate the repatriation of workers when their contracts expire or are violated. Third, they usually place some regulations on the behavior of both migrant workers and employers. Fourth, BLAs establish institutional mechanisms for administering the agreement, like regular meetings between the home and host states. While these four elements are not present in all BLAs, they are core features of this kind of international agreement.

2.2.2 Prior Research

There have been a handful of papers that have discussed the development of BLAs generally (e.g., Wickramasekara, 2015), explored specific treaties (e.g., Plotnikova, 2011), and theorized about their potential effects (e.g., Sykes, 2013). But to our knowledge, only two papers have empirically examined the use or effect of BLAs. Chilton and Posner (2018) developed the Bilateral Labor Agreements database, and then used it to empirically test what had been a conventional wisdom that BLAs were signed by host states that are rich and have bad human rights records with home states that are poor and concerned with the rights of their citizens. They found that this conventional account of BLAs fits the agreements signed with Middle Eastern host countries, but on average BLAs are signed by pairs of countries with relatively similar levels of wealth and democratization. Chilton and Posner (2018) also conducted some exploratory analysis of the effect of BLAs on migration, and found that signing BLAs is associated with increased migration. However, their panel data on migration between pairs of countries was substantially limited and the trends they observed appeared to start before BLAs were signed, which led Chilton and Posner (2018) to conclude that they could not make strong statements about the effect of BLAs.

Peters (2019) developed a separate database on BLAs and examined when countries are likely to sign them. Peters (2019) found that, among other things, host states are likely to sign BLAs when they have small pools of reserve labor (e.g. there is demand for labor) and when they are remote (e.g. they have difficulty attracting migrants). Peters (2019) also explored changes in migration for 39 BLAs where OECD data on migration flows was available. Of those 39 BLAs, Peters (2019) found that 13 resulted in large increases in migration (defined as increases in migration of 200 percent or more), 10 were associated with sizable increases in migration (defined as increases in migration of 20 percent to 180 percent), and 16 were associated with either small increases or decreases in migration (this ranged from a 95 percent decrease to a 7 percent increase). However, Peters (2019) does not make any claims about which BLAs are likely to produce these changes in migration.

Although Chilton and Posner (2018) and Peters (2019) provide the most comprehensive assessment of the effects of BLAs, there are several notable limitations of their results. For one, they are both only able to look at the effect of BLAs on migration generally, and they do not examine the effect of BLAs on labor migration specifically or on other outcomes like remittances. Additionally, while they use different data on migration, both datasets include missing observations for many countries and years. Finally, both papers are primarily focused on examining why countries sign BLAs, and they only test the effect of BLAs as extensions of their primary analysis. As a result, there still is not good, comprehensive evidence on the likely effect of BLAs.

2.2.3 The Philippines and Labor Migration

While it would admittedly be ideal to study the effect of all BLAs that have been signed around the world, current data limitations make this simply not possible. As a result, we instead focus on investigating the BLA program of one country: the Philippines. We focus on the Philippines for three reasons. First, exporting labor has been a principle part of the Philippines' development strategy. In response to a surging population and a stagnant

economy in the early 1970s, the government began to explicitly promote labor migration. To do so, it extolled the benefits of working abroad to Filipinos at home, actively marketed the strengths of Filipino workers to overseas employers, and even created a new government agency to help Filipino workers land labor contracts. Due to these efforts, over the last forty years, labor migration has become a central part of the social and economic life of the Philippines. DeParle (2019) recently summarized the importance of labor migration to the Philippines as follows:

> "No country does more to promote migration than the Philippines, where the government trains and markets overseas workers, whom presidents celebrate as 'Heroes.' More than two million Filipinos depart each year, enough to fill a dozen or more 747s a day. About one Filipino worker in seven works abroad, and the $32 billion that Filipinos send home accounts for 10 percent of the gross domestic product."

In total, the Philippine government estimates that 2.3 million Filipino workers deployed to work overseas in 2018. Of these workers, roughly 56 percent were women; 51 percent worked in four countries in the Middle East (Saudi Arabia (24.3 percent), the United Arab Emirates (15.7 percent), Kuwait (5.7 percent), and Qatar (5.2 percent)); and 96 percent had a legal employment contract.[5]

Second, the Philippines has arguably been more invested in negotiating and signing BLAs than any other labor exporting country.[6] Panel A of Figure 2.2 graphs the number of BLAs the Philippines has signed over time. The Philippines signed its first BLA with the United States in 1968. The growth of new BLAs was then slow until after the Philippines passed the "Migrant Workers and Overseas Filipinos Act of 1995 (Republic Act No. 8042)" (RA-8042).[7]

[5] *See* Philippine Statistical Authority, Results from the 2018 Survey on Overseas Filipinos, *Press Release, April 30, 2019*, available at https://psa.gov.ph/statistics/survey/labor-and-employment/survey-overseas-filipinos, accessed January 20th, 2020.

[6] For more information on the Philippines' BLA program, see Blank (2011) .

[7] *See* http://www.poea.gov.ph/laws&rules/filesMigrant%20Workers%20Act%20of%201995%20(RA%208042).html, accessed January 20th, 2020.

This new law included restrictions on countries that Filipino workers could be "deployed" to work in based on whether those countries had laws in place that would protect the rights of migrant workers. Relevantly, Section 4 of RA-8042 provides that:

> "The State shall deploy overseas Filipino workers only in countries where the rights of Filipino migrant workers are protected. The government recognizes any of the following as guarantee on the part of the receiving country for the protection and the rights of overseas Filipino workers:
>
> (a) It has existing labor and social laws protecting the rights of migrant workers;
>
> (b) It is a signatory to multilateral conventions, declaration or resolutions relating to the protection of migrant workers;
>
> (c) It has concluded a bilateral agreement or arrangement with the government protecting the rights of overseas Filipino workers; and
>
> (d) It is taking positive, concrete measures to protect the rights of migrant workers."

RA-8042 was amended in 2010 by Republic Act No. 10022 (RA-10022)[8], which, among other changes, eliminated Section 4(d) as a sufficient condition for deployment. Although these may sound like strict requirements, it is worth noting that the Philippines continues to allow the deployment of Filipino workers in countries notorious for labor violations even after the passage of RA-10022. It is thus unclear the extent to which this requirement actually increases the protection of deployed workers. Nevertheless, the number of BLAs the Philippines signed started to increase dramatically beginning in the late 1990s. By 2019, the Philippines had signed 68 total BLAs. As Panel B of Figure 2.2 shows, this included BLAs with countries throughout Southeast Asia, Europe, the Middle East, and North America.

Third, the Philippines collects a great deal of data on the Filipino workers that deploy to work overseas. Notably, Chilton and Posner (2018) and Peters (2019) were only able to

[8] *See* https://www.ilo.org/dyn/natlex/docs/ELECTRONIC/98914/117850/F586226360/PHL98914.pdf, accessed January 20th, 2020

examine the effect of signing BLAs on total migration between countries, but not whether BLAs directly increased migrant worker deployment to the new country. As we explain below in Parts 2.3.2 and 2.3.3, the administrative data the Philippine government collects makes it possible to look specifically at labor migration (including labor migration by sector) and also at remittances returned to the Philippines.

2.3 Data

2.3.1 BLA Data

Our data on the BLAs the Philippines has signed is from Mangulabnan and Daquio (2019). The government of the Philippines does not have a single, authoritative list of the BLAs that it has concluded or that are currently valid. Incomplete lists can be found in several places, such as the Philippine Overseas Employment Administration's (POEA) website where roughly 30 BLAs are mentioned.[9] Mangulabnan and Daquio, who are researchers within the Philippine Department of Labor and Employment (DOLE), collaborated with three sub-agencies in 2019—the Institute of Labor Studies (ILS), the POEA, and the International Labor Affairs Bureau (ILAB)—to document all BLAs the Philippines has previously signed. During our own meetings with these agencies and the Philippine Department of Foreign Affairs, officials said that Mangulabnan and Daquio's research produced what they believed to be a comprehensive list—but it is possible that an agreement or two may have been lost over time.

Table 2.1 lists the 68 BLAs that Mangulabnan and Daquio identified through their research. Of these 68 agreements, 43 were still valid by the summer of 2019. Those 43 valid agreements take several different legal forms: 25 are non-binding Memorandums of Understanding (MOUs) or protocols to MOUs, 15 are legally binding Memorandums of Agreement (MOAs) or protocols to MOAs, 1 is a Memorandum of Cooperation, 1 is a Joint Commu-

[9] See http://www.poea.gov.ph/laborinfo/bLB.html, accessed January 20th, 2020.

nique, and 1 is an "Arrangement."[10] The agreements also lasted for different lengths of time: 12 had no fixed duration and the remaining 31 lasted from between one year to five years (however, all are renewed automatically unless terminated). These 68 agreements were signed with 28 different countries. There are several reasons multiple BLAs were signed with the same partner countries. Sometimes it was because the BLAs covered different categories of workers, in other cases it was because agreements had expired. In six cases, it was because the government signed an agreement and an accompanying protocol at the same time.

2.3.2 Deployment Data

The Philippine Overseas Employment Administration (POEA) is a government agency which oversees deployment of Filipino workers overseas. Among its tasks are licensing private recruitment agencies, negotiating bilateral labor agreements with foreign governments, and monitoring and advertising employment opportunities abroad. In addition, it publishes certain aggregated statistics on overseas Filipino workers. We use three datasets published by the POEA:

1. Total Newly Hired Filipino workers by country and occupation annually for 222 countries for 1992-2016.

2. Combined Newly Hired and *Rehired* Filipino workers by country annually for 221 countries for 1998-2010.

3. Estimates of total stocks of overseas Filipinos by country and status (permanent, temporary, and irregular), annually, for 1997-2013. These data are only available for approximately 37 countries between 1997 and 2003, and about 210 countries for the remaining years.

[10] Mangulabnan and Daquio (2019) do not code the 25 agreements that were not valid by the time of their research. We have acquired the text of all 68 BLAs, and we hope to complete our own coding of the status of those expired agreements.

Figure 2.3 shows the evolution of Filipino worker hires across the world. Panel A graphs the total number of new hires over time, and shows that the number remained steady at about 200,000 per year until the early 2000s. The number of new hires increased throughout the 2000s and 2010s, reaching a peak of nearly 600,000 newly deployed overseas workers per year in 2016. Panel (a) also shows that the number of rehires increased even faster, as existing overseas workers renewed their contracts. Panel (b) shows the timeline of new hires in four sectors between 1992 - 2014: (1) production workers, vehicle operators, laborers (2) professionals and technicians, (3) service workers, and (4) domestic workers.[11] After initially following similar trends, in the mid 2000s the numbers of new hires in these sectors began to diverge, as new hires of professionals and technicians declined, while new hires in the other three categories increased.

2.3.3 Remittance Data

We obtained country-level data on incoming remittances from the Philippine Central Bank, the Bangko Sentral ng Pilipinas (BSP). We specifically obtained data from the BSP at a monthly frequency from 1989 to September 2019.[12] Figure 2.4 uses this data to plot monthly level land-based[13] remittances to the Philippines in current USD. In the past twenty years, incoming remittances have become an important source of revenue for Philippine households. With the exception of a downturn following the Asian financial crisis, total remittances have increased every year from negligible levels in 1989 to about $29 billion dollars—equal to nearly 9% of Philippines' GDP—in 2018.

[11] While we have data for 2015 and 2016, POEA changed the way it classified occupations into sectors for those years, rendering data for prior years not directly comparable.

[12] The definition of remittances used by the BSP is "the sum of net compensation of employees (i.e. gross earnings of overseas Filipino (OF) workers with work contracts of less than one year, including all sea-based workers, less taxes, social contributions, and transportation and travel expenditures in their host countries), personal transfers (i.e. all current transfers in cash or in kind by OF workers with work contracts of one year or more as well as other household-to-household transfers between Filipinos who have migrated abroad and their families in the Philippines) and capital transfers between household (i.e. the provision of resources for capital purposes, such as construction of residential houses between resident and non-resident households without anything of economic value being supplied in return)."

[13] The data allow us to distinguish between remittances from land-based and sea-based workers. We use land-based remittances in our analysis, as our BLAs apply to land-based workers.

It is important to note that, although this is the most comprehensive data on remittances to the Philippines that is currently available, there are several limitations. First, the BSP data only captures flows of remittances through traditional financial institutions like banks or wire services like Western Union. The data notably does not capture flows of remittances through informal channels like Hawala networks or certain forms of new Fintech that bypass traditional financial networks. Second, the BSP data only counts cash remittances and does not include estimates of non-cash remittances. For instance, if an Overseas Filipino Worker from the United States ships a new appliance home, the appliance would not be counted in the remittance data. Third, the BSP data attributes remittances to the country from which they are directly transferred into the Philippines. However, some banks that do not have direct correspondence relationships with banks in the Philippines may route their transactions through a third country in order to send it to the Philippines. In such cases, the country recorded as the source of remittances will be the location of the third country, rather than the true origin. For instance, because many banks without direct correspondence relationships with the Philippines may first send the remittances to major banks in the United States, the dataset likely overestimates remittances originating from America.

These limitations may result in over or under estimation of remittance flows by countries. That said, these issues are unlikely to bias our results. This is because measurement error is only likely to cause bias if the error varies based on whether the country has signed a BLA with the Philippines. The only source of measurement error that may vary at the country level is the share of banks that route financial transactions through third countries like the United States. Our discussions with BSP officials suggest that this is primarily a concern for banks based in the MENA region, but as we discuss in Part 2.5.3, our results are substantially the same when excluding countries from this region from our analysis.[14]

[14] Another reason these factors may not be a major concern is that officials at the BSP believe that informal financial networks and Fintech comprised a relatively small share of total remittances as of 2019, but they played almost no role before 2016. This is significant because onlt 3 of the 19 three year event windows we examine include years after 2016.

2.3.4 Summary Statistics

We combine the data sources described above into a country-year dataset for deployment analysis and a country-month dataset for our remittance analysis.[15] Table 2.2 reports summary statistics from those datasets. Both deployment of Filipino workers and remittances are very right-skewed, with most countries hiring zero Filipino workers and sending zero remittances in any given year. In addition to collecting data on BLAs, deployment, and remittances, we also collected data on factors that are likely to influence the probability that the Philippines would sign a BLA with a country and that are also likely to influence deployment and remittances to use as control variables. These include data on basic features of the potential host countries (their GDP per capita, population, and Polity 2 score), data on the relative political and economic conditions in the Philippines and the host countries (GDP per capita ratio and Higher Polity Score), and data on the economic and political relationship between the Philippines and the host countries (total trade, BIT in the past, and their ideological distance based on UN voting).

2.4 Research Design

We use an event study research design to assess changes in deployment and remittances after a BLA goes into effect.[16] An event study is a widely used statistical method that relies on establishing a control group that would have likely developed similarly over time to the treatment group if it were not for a given change in policy (MacKinlay, 1997; Chetty et al., 2014; Rozema and Schanzenbach, 2019). The method relies on measuring changes in the

[15] However, the only variables available at a monthly level are remittances, BLAs, and BITs. All other variables are annual.

[16] The literature empirically evaluating international treaties has been repeatedly criticized for failing to use methods that can plausibly identify the effect of ratifying international agreements (Chaudoin et al., 2018; Chilton and Tingley, 2013; Lupu, 2013; von Stein, 2005). Although there are a number of reasons for this criticism, the primary objection to prior research is failure to adequately account for the endogenous nature of treaty ratification. We are unable to change the fact that treaties are not randomly assigned, but we attempt to provide more credible estimates in two ways: (1) clearly graphing and presenting data instead of reporting black-box regression results and (2) by using a research design that utilizes transparent treatment and control groups.

outcomes of interest for the treatment group relative to the control group during the period before the policy change and then the period after the policy change.

We believe an event study is the most appropriate research design to study the effect of BLAs for two reasons. First, using standard panel regression methods may obscure the counterfactual that is implicitly being used to assess the effect of signing BLAs. This is, in part, because BLAs are adopted at different times and they are in effect for different time periods. Some BLAs are replaced after their initial term, others are allowed to expire. It is thus difficult to understand what is being estimated by a panel regression where the treatment units change over time. An event study approach allows us to focus in directly on changes in policy and establishes a specific control group for those changes, providing a clear counterfactual against which to assess our results. Second, there may be long term changes in the migration patterns of Filipino workers to a given country. Changes that occur over a long period in this way may be difficult to account for in a panel regression framework. An event study instead restricts the sample to the years around the adoption of a BLA, which increases our confidence that any changes we observe are due to the adoption of a BLA and not long term trends that are picked up by a panel regression.

To create the samples for our event studies, we define an event as a new BLA's being signed by the Philippines and another country. We define event windows as the three year period before and the three year period after a BLA is signed[17]. Therefore, for dependent variables measured at an annual frequency we use seven years of data, with year zero being the year the treaty signed. For monthly variables, we use seventy-three months of data. For each event, we match a "treated" country which signed a BLA with the Philippines to "control" countries where there was no BLA with the Philippines adopted during the seven-year window. After developing a set of control countries for each treatment event, we stack all events around event time, and compare the treated countries to the control countries.

With the stacked event study datasets, we test for the significance of changes after adopt-

[17]We experiment with alternative durations of the event window and report the results in section 2.5.3

ing a BLA by estimating Equation 1.

$$y_{ijt} = \alpha + \beta_{treat,ij} + \beta_{post,jt} + \beta_{treat,ij} \times \beta_{post,jt} + \gamma_{it} + \zeta_{it}p + \eta_i + \phi_j + \psi_t + \epsilon_{ijt} \tag{2.1}$$

for host country i, event j, and year t. $\beta_{treat,ij}$ is an indicator for the treated country in the treatment event, $\beta_{post,jt}$ is an indicator for the post-treatment period within the event window. The interaction $\beta_{treat,ij} \times \beta_{post,jt}$ is the coefficient of interest, which indicates the changes in either deployment or remittances attributable to the adoption of BLAs. γ_{it} is a vector of control variables that measure time varying aspects of host country i, and ζ_{it} is a vector of control variables that measure time varying aspects of the relationship between the host country and the Philippines. The specification also includes host country fixed effects η_i, event fixed effects ϕ_j (which turn on for both the treatment and control countries in the event), and year fixed effects ψ_t.[18]

This stacked event study approach results in several dimensions of correlation in the error term ϵ_{ijt}.[19] First, a given country-year observation can appear multiple times in our sample. This is because stacking treatment events allows a given observation to appear for all events where it would be appropriate. For instance, the observation for Australia in the year 2000 may be used as a control observation for the event window around the 2002 Philippines — United Kingdom BLA and for the event window around the 2001 Philippines — Norway BLA. This creates correlation between the error terms for observations that appear multiple times in the sample. This is accommodated by clustering standard errors by country. Second, there may be common shocks in migration patterns that appear to all host countries in a given time. For instance, an economic downturn in the Philippines may lead to an increase in migrant workers seeking overseas employment. This creates correlation between the error terms for observations for a given year. We thus additionally cluster standard errors by

[18] Our deployment data is measured annually, so we are only able to include year fixed effects. Our remittance data, however, is measured monthly. For the remittance regressions, we thus include month-by-year (e.g. April 2001, January 2015) fixed effects.

[19] This approach builds on Rees-Jones and Rozema (2019).

year.[20] Finally, our selection of control countries at the event level may lead to correlation across all observations within a given event. We account for this by also clustering standard errors by event. We use the approach developed by (Correia, 2016) to perform multi-way clustering for our regressions.

It is important to note that this approach to clustering significantly reduces our statistical power. That said, although our regressions include a large number of observations because of the large number of control observations for each treatment, they are better understood as the number of treatment events where we have data with a three year window before and after ratification (this is 14 treatment events for labor deployment and 19 treatment events for remittances). This conservative approach to clustering thus accounts for the true underlying statistical uncertainty in our research design.

2.5 Results

We now turn to estimating the effect of the Philippine BLAs. Part 2.5.1 presents our results estimating the effect of signing BLAs on deployment of Filipino workers, and Part 2.5.2 presents our results estimating the effect of signing BLAs on remittances back to the Philippines. We then turn to exploring the robustness of these results. Part 2.5.3 discusses a range of alternative specifications we estimate to assess the robustness of our results, and Part 2.5.4 shows the sensitivity of our primary results to these alternative specifications.

2.5.1 Deployment

To begin by examining the raw data, Figure 2.5 graphs the number of new hires (in logs) that are deployed from the Philippines in all sectors in the three years before and after the signing of BLAs. Although the Philippines has signed 68 BLAs in total, Figure 2.5 only reports 14 plots. This is because we restrict our sample in several ways: (1) we treat multiple

[20] For the remittance regressions, we instead cluster standard errors at the month-by-year level. This can account for any common shocks that may occur in a given month.

BLAs signed on the same day as a single BLA (e.g. when countries signed an MOU and a MOU protocol on the same day); (2) we exclude countries that signed more than one BLA with the Philippines in the six year event window (unless, as just noted, they were signed on the same day); (3) we drop countries for which we do not have deployment data for the entire six year event window; (4) we only use countries for which we have non-missing observations of all control variables[21]; and (5) we drop countries which have zero deployment of Filipino workers in any year in the event window. The raw data in Figure 2.5 shows that Filipino labor deployment to host countries that have signed BLAs with the Philippines follows a range of patterns. For instance, deployment appears to have increased in the years after the Philippines signed a BLA with Saudi Arabia in 2005 and 2013, but decreased after the Philippines signed a BLA with Papua New Guinea in 2013.

Figure 2.6 collapses the data from these 14 countries into a single graph. The "treated" group line reports the average number of new deployments in the three years before and after a country signed a BLA with the Philippines. The "control" group line reports the average number of new deployments for a sample of control countries that were identified for each event using the stacked event study method described in Part 2.4. There are two trends in Figure 2.6 worth noting. First, unsurprisingly, the average number of new hires is dramatically higher for the treated countries where a BLA is signed than the control countries where a BLA was not signed. Second, both lines slope gently upward. Although the treated line may have a very slight change in slope in the years after BLAs were signed, it is at best a very modest change.

Table 2.3 estimates Equation (1) using the sample graphed in Figure 2.6, with the natural logarithm of new hires as the dependent variable. In Table 2.3, and all subsequent analysis, we estimate five regression specifications: Column (1) does not include any control variables; Column (2) includes controls for three features of the host countries (GDP per capita, population, and Polity 2 score); Column (3) includes controls for two features of the

[21] We relax this assumption in our robustness check in the Appendix section B.7

relationship between host and sending countries that Chilton and Posner (2018) identified as influencing the probability that countries would sign BLAs (their relative GDP per capita and if the host has a higher Polity 2 score); Column (4) includes controls for three aspects of the relationship between the Philippines and the host countries (the total trade between the countries, if they have previously signed a Bilateral Investment Treaty, and whether they vote similarly at the United Nations); Column (5) includes all eight of these control variables simultaneously. All five specifications include 14 treatment events—the same that are shown in Figure 2.5.

The point estimates for having signed a BLA in Table 2.3 are all very close to zero, except in model (3) which suggests a BLA increases deployment by 14.8%. However, none of the estimated effects are statistically significant at conventional significance levels. In fact, the confidence intervals are quite large and admit a range of both sizable positive and negative effects.

Figure 2.7 and Table 2.4 further explore the relationship between signing a BLA and deployment of Filipino workers by examining if there are heterogenous effects for different employment sectors. Like the overall results in Figure 2.6, the plots in Figure 2.7 suggest that, even when the data is broken out by sector, there is at best a very modest increase for the treated countries relative to the control countries after signing a BLA. These results are further confirmed by the regressions in Table 2.4, where we estimate the same five regression specifications for these alternative dependent variables (we omit the inclusion of control variables). Like with the overall results in Table 2.3, the estimates are mostly positive (except in the case of professional workers) but never statistically significant. This suggests that having signed a BLA does not result in major increases in the deployment of Filipino workers in any of these major employment categories.

2.5.2 Remittances

We now turn to studying the association between BLAs signed by the Philippines and remittances to the Philippines. Figure 2.8 begins by graphically exploring the trends in remittances in the years before and after the Philippines signed BLAs with the corresponding country. Like with deployment, BLAs appear to have varying effects. For some countries, such as Kuwait, remittances increase dramatically immediately following the BLA. But for other countries, like Spain, the BLAs appears to have little or no effect. There is thus not a uniform pattern of remittance flows after a BLA is signed.

Figure 2.9 collapses these countries into a single graph and adds a control group using the method explained in Part 2.4 to generate the sample. Like the results for deployment shown in Figure 2.6, Figure 2.9 reveals that the average remittances are higher and noisier for treated countries than control countries, and that there may be a slight increase in remittances over time for both groups of countries. But the two trends remain roughly parallel in both the pre and post-treatment periods.

Table 2.5 estimates Equation (1) using the sample graphed in Figure 2.9 while using log(monthly remittances) as the dependent variable. The Columns in Table 2.5 use the same pattern of adding control variables as the specifications in Part 2.5.1. The coefficients for signing a BLA are again close to zero, except in column (3), and none achieve statistical significance.

2.5.3 Robustness Checks and Heterogeneity

Our primary results suggest that the BLAs the Philippines has signed have not had a statistically significant effect on either the deployment of Filipino workers or the return of remittances to the Philippines. However, we ran a range of robustness checks to test whether our results are dependent on the choices we made for our primary specifications. In addition, we wish to explore if there is heterogeneity in the impact of BLAs by type of treaty or geography. The results of all these tests are reported in the Appendix and summarized

in Figure 2.10 and Figure 2.11 (which we discuss below in Part 2.5.4).

Subsetting Based on Legal Status. As described in Part 2.3.1, the BLAs that the Philippines has signed may have varied in their domestic and international legal status. We conducted a series of robustness checks to assess whether BLAs have heterogenous effects depending on these differences. First, the Philippines has negotiated both legally binding MOAs and non-binding MOUs. One possibility is that only the legally binding MOAs have had an effect. We thus limited our treatment observations to MOAs[22]. Second, many BLAs are simply revisions or renewals of previously signed treaties. It is possible that such follow-up BLAs will lead to few legal changes in either country. It is also possible that subsequent BLAs with have more modest effects because the employment relationship will already have formed after the first BLA (which would bias our results towards zero). We tested for this by restricting our analysis to only the first treaty per country and excluding countries from a potential set of control countries after they have signed at least one BLA. Third, in 1995 the Philippines passed a law called "RA-8042" that majorly reformed its labor law. It regulated deployment of workers and introduced the BLA requirement (which could be waived if other conditions were met). We account for this by ignoring any treaty signed before 1995. Fourth, in 2010 the Philippines passed a law called "RA-10022" that further strengthened the rules for deploying workers overseas. We thus look at only treaties signed after RA-10022, ignoring all preceding treaties. Finally, for comparison with the previous regression, we look only at treaties signed before RA-10022. Table B.1 and Table B.2 report the results when adjusting the sample to account for these differences in the legal statuses of agreements.

Subsetting Based on Geography. Our baseline regressions were estimated using up to 222 countries to generate our event study samples. There are good reasons to think, however, that the estimates may be heterogenous across regions. First, because remittances

[22]We used a 1-year event window in this specification because none of the countries which signed MOAs during 3-year event windows are control countries in any other 3-tear event window. This results in the treatment coefficient's being collinear with the fixed effects for those countries.

may show up as originating from countries other than the true source if they are rerouted before being transmitted to the Philippines, we drop offshore financial centers (OFCs) where many financial institutions are located (indeed, many of those places show up as substantial sources of remittances, even though there is zero or almost zero deployment of workers to them).[23] Second, we were told by the Philippine Central Bank that remittance data tend to be more reliable for Asian countries, and that most Filipino migrants end up working in other Asian countries. We thus estimated our regressions when only using Asian countries as both treated and control units. Third, during our field research, we were repeatedly told that the Philippines is particularly concerned about welfare of their workers deployed to the Middle-East and North Africa (MENA) region.[24] We thus estimated our results when only using only MENA countries as both treatment and control units (and when excluding MENA countries). Finally, in our primary specifications, our starting dataset is the list of countries used in the hiring and remittance datasets. But these datasets include more countries than the 194 widely recognized countries. We thus restricted our sample to this standard list of countries.[25] Table B.3 and Table B.4 report the results focusing on these sets of countries.

Subsetting Based on Type of Country. We also restricted our sample by excluding certain classes of countries where we believed that BLAs were unlikely to have an effect. Specifically, as Table 2.2 revealed, no Filipino migrants deploy to the median country in our dataset. Moreover, BLAs are also less likely to have an effect in countries where there is already high respect for workers' rights. Table B.5 and Table B.6 estimate regressions excluding countries with no deployment and countries with Polity 2 scores higher than 7 from the sample (as a corollary, we also estimate regressions using a sample of countries with only 7 or higher *included* in the sample).

[23] The list of OFCs was obtained from Zorome (2007).

[24] Countries included are: Algeria, Bahrain, Egypt, Iran, Iraq, Israel, Jordan, Kuwait, Lebanon, Libya, Morocco, Oman, Qatar, Saudi Arabia, Syria, Tunisia, Turkey, UAE, and Yemen.

[25] We specifically based on sample on countries listed in the "Correlates of War" template of countries by year, but also added Hong Kong (due to its large number of Filipino migrants) and Northern Mariana Islands (as it has two BLAs with the Philippines).

Alternative Treatment of the United States. Our primary specifications treated the United States as a normal partner country. But the United States is a particularly important labor market for the Philippines, and there are two reasons to think that treating the United States as a standard country may bias the results. For one, as we described in Part 2.3.3, a large amount of remittances are routed through the United States. We thus tested whether our results are robust when excluding the United States from the analysis. Additionally, the United States signed a BLA with the Philippines in 1968, but it actually only applies to American military bases in the Pacific. We thus recoded that BLA to apply only to Guam, American Samoa, and American Virgin Islands instead of all of the U.S.[26] The results when excluding the United States and when re-coding the United States are reported in Table B.7 and Table B.8.

Alternative Event Windows. We tested whether our results were robust to using alternative event windows. For the event studies we used for our primary analysis, we defined an event window as the 36 months before and after a BLA is signed. But it is possible that BLAs may only have an effect for a shorter time period, or that their effect may take longer to emerge. We thus re-estimated our results while redefining the event windows to be: 12 months, 24 months, 48 months, and 60 months. The results using these alternative windows are reported in Table B.9 and Table B.10.

Annual Remittance Data. Although most of our data was measured annually, we created a month-level dataset for our remittance regressions. However, we also have annual estimates of remittances from the BSP. We estimated our results using annual remittances as a dependent variable. We conducted this robustness test while using 12 month, 24 month, 36 month, 48 month, and 60 month event windows. The results when doing so are reported in Table B.11.

[26] Because the BLA with the United States was signed in 1968—decades before we have data on deployment or remittances—we were unable to use the United States as a treatment event in our primary analyses. It was possible, however, for the United States to be part of the control group for our primary analyses.

Relaxing the constant sample restriction. In all our specifications, we only include treated and control countries which have non-missing observations for all the covariates used in our model (5). Due to missing data, this approach causes us to lose a lot of observations in less demanding specifications (models (1)-(4)) or when data is missing for only part of the event window. Therefore, we also estimated regressions where we use all available data for each country which has non-missing observations of the requisite variables. The results are in Table B.12 and Table B.13.

Panel Regressions. We used the event study research design explained in Part 2.4 for all the results in Part 2.5.1, Part 2.5.2, and all the robustness checks. Although this approach has several benefits, it does limit the sample to events when we have complete data. We thus conducted two robustness checks with a standard panel dataset. For one robustness check, we estimated a standard difference-in-difference regression with panel data while using OLS regressions. For another robustness check, we followed an approach from the trade and investment literature and estimated a Poisson-Pseudo- Maximum-Likelihood (PPML) model while using our panel data (e.g., Silva and Tenreyro, 2006; Jones, 2019). The OLS results are reported in Table B.14 and Table B.16, whereas the PPML ones are in Table B.15 and Table B.17.

2.5.4 Sensitivity

The results of the robustness tests described in Part 2.5.3 are largely consistent with our primary results. To illustrate, Figure 2.10 graphs the point estimates for all the regressions we estimated examining the relationship between signing a BLA and labor deployment, and Figure 2.11 graphs the point estimates for all the regressions we estimated examining the relationship between signing a BLA and remittances.[27] The point estimates in these figures are ordered from the most negative to the most positive. The dots in the first gray box

[27] Figures 2.10 and 2.11 do not include the robustness checks that used panel data to estimate OLS and PPML regressions. This is because their estimated coefficients have a different meaning from that estimated using the event study method.

below each line indicate which of the five specifications from Tables 2.3 and Table 2.5 was used to produce the line, and the dots in the second gray box below each line indicate which robustness check was used. Our point estimates from our primary results—reported in Tables 2.3 and Table 2.5—are included in the figures in blue.

Both figures suggest that our estimates are fairly consistent across different regression specifications. Figure 2.10 shows that the point estimates for 59 of the 100 regression specifications we estimated are positive. Of these estimates, 34 of the point estimates are larger than 5%. Moreover, none of the estimates are statistically significant at the 95 percent level. Taken together, these estimates suggest that the BLAs the Philippines has signed have had, at best, a modest positive effect on labor deployment. Additionally, Figure 2.11 shows that the point estimates for 77 of the 120 regression specifications we estimated are negative. Of these estimates, 45 of the point estimates are lower than -5% and 37 are greater than 5%. But again none of the coefficients are statistically significant at the 95 percent level. Taken together, these estimates suggest that the BLAs the Philippines have signed have had no effect on remittances sent by Filipinos.

2.5.5 Heterogeneity - case study analysis

Figures 2.10 and 2.11 suggest that the associations between hires and BLAs, as well as remittances and BLAs, do not vary systematically with the legal status of the treaties or the types of host countries. However, given the substantial diversity in their language and provisions, BLAs are perhaps best understood as idiosyncratic events whose properties are not easily summarized by their observable characteristics. Therefore, in this section we treat each BLA in our sample as a case study and analyze its impact individually, only for the host country. We estimate that effect non-parametrically, using a synthetic control approach developed by Abadie and Gardeazabal (2003); Abadie et al. (2010, 2015). Instead of using all non-treated countries controls for the treated one, this method relies on selecting a combination of control countries which most resembles the treated country. This combination

is constructed by minimizing the predicted error of the dependent variable, based on a set of observable covariates in the pre-treatment period. In our application, we use the full set of covariates from model (5) in Tables 2.3 and 2.5 to construct the synthetic control.

The results for deployment are presented in Figure 2.12. In each graph, the solid black line shows the evolution of log new hires in the country which signed the BLA, while the dashed blue line shows the same for the synthetic control. The treatment event is indicated by the vertical red line. Thus, the portion of the blue line to the right of the red line can be interpreted as predicted evolution of new hires in the absence of a BLA. Figure 2.12 suggests that BLAs may have had an impact on deployment in some cases, but not others. For example, the Germany 2013 BLA seems to have raised deployment by 1-2 log points relative to the counterfactual. The same is true for Japan 2009, although the effect does not kick in until 2 years after the treaty is signed. Interestingly, while the raw data shown in Figure 2.5 suggests that the Bahrain 2003 BLA raised deployment, Figure 2.12 shows that synthetic control experienced a similar increase over the same time period. It is also notable that for some countries, such as Saudi Arabia, it is impossible to construct a synthetic control with similar levels of deployment, exposing the limitation of the approach when analyzing cases with uniquely large numbers of Filipino workers.

Figure 2.12 shows the results of constructing the synthetic control for remittances. Here, BLAs seem to have raised remittances from Bahrain (especially the 2007 BLA), New Zealand, and Qatar. In addition, the Japan 2009 BLA seems to have raised remittances but with a delayed effect, similar to deployment. This may reflect a delay in the implementation of the agreement. Also note that BLAs appear to have *lowered* remittances from Papua New Guinea, consistent with its effect on hires.

While these results suggest that certain BLAs have an effect on deployment and remittances (both positive and negative), more work is needed to understand why that occurs in some cases and not others. It is possible that there are treaty specific features, as well as institutional host country characterstics, which determine how effective a BLA is in regulating

deployment and remittances.

2.6 Conclusion

We have explored whether the Philippine Bilateral Labor Agreement program has increased the number of Filipinos who work abroad in countries that are treaty partners and the amount of remittances to the Philippines from countries that are treaty partners. Our results suggest that the BLAs that the Philippines has signed has increased neither deployment, nor remittances. These results thus suggest that signing more BLAs may not be an easy solution for countries wishing to increase their labor migration, and the financial benefits of that labor migration.

Before concluding, it is worth noting four limitations to our analysis. First, we cannot identify the causal effect of signing BLAs on remittances and workers. Simply put, the BLAs the Philippines government has signed are certainly not random. Instead, the Philippines has signed BLAs with host countries that were willing to sign treaties committing them to improving the working conditions of foreigners. These host countries may have begun to accept more workers, under better conditions, regardless of the treaties.

Second, although we focused on studying the Philippines in large part because we believe it has the best available data on labor deployment and remittances of any country that is a major exporter of migrant labor, the data is still not perfect. The POEA estimates may miscount the number of Filipinos who work abroad in a given country when there is irregular migration, and it is possible that the rate of irregular migration is correlated with the existence of a BLA. Similarly, the BPS data on remittances does not capture money that does not flow through the financial system, and the rate at which people transfer funds through informal cash networks may also be correlated with the existence of a BLA. These limitations of the data thus may bias our results or increase our standard errors.

Third, we cannot test all of the effects of the BLAs. These agreements not only try to ensure that countries will take more workers, but they also try to improve the conditions

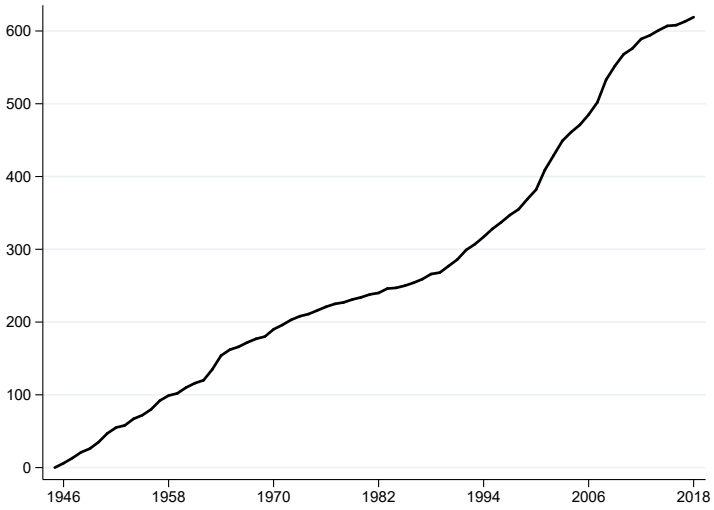
of the workers while they are abroad. For instance, a hope of BLAs is that they will help ensure that foreign laborers will be provided with safer working conditions. Although we would ideally like to test these effects as well, we are unaware of cross-country datasets on the conditions of foreign workers over time. As a result, we are only able to examine whether BLAs are associated with increases in remittances and hires of foreign workers, and not whether BLAs improve the welfare of Filipino workers more generally.

Finally, the Philippine BLA program is likely not representative of other countries' programs. As previously noted, the Philippines has made facilitating their workers' traveling abroad a central part of its development strategy, and it has taken extensive steps to promote that effort. Other countries that simply sign BLAs without making the other investments made by the Philippines in promoting and monitoring labor migration thus may not have had the same experience with BLAs. As a result, our results may not be true of BLAs more generally. Instead, our results may be best understood as evidence that a Philippines style BLA program may produce dividends.

Although these limitations prevent us from drawing strong conclusions about the effects of BLAs, we believe that our results provide the most extensive look at the effect of a form of international treaties that previously only gained limited attention. And, although future research is needed with other data and in other settings, the consistency of our results across a broad range of regression specifications does suggest that the benefits of the Philippines' BLA program have been modest at best. This in turn suggests that governments seeking to promote labor migration and improve the experience of those migrants may have to do more than simply find countries willing to sign bilateral treaties.

Figure 2.1: Diffusion of Bilateral Labor Agreements

(a) Cumulative Bilateral Labor Agreements Over Time



(b) Number of Bilateral Labor Agreements Signed by Country

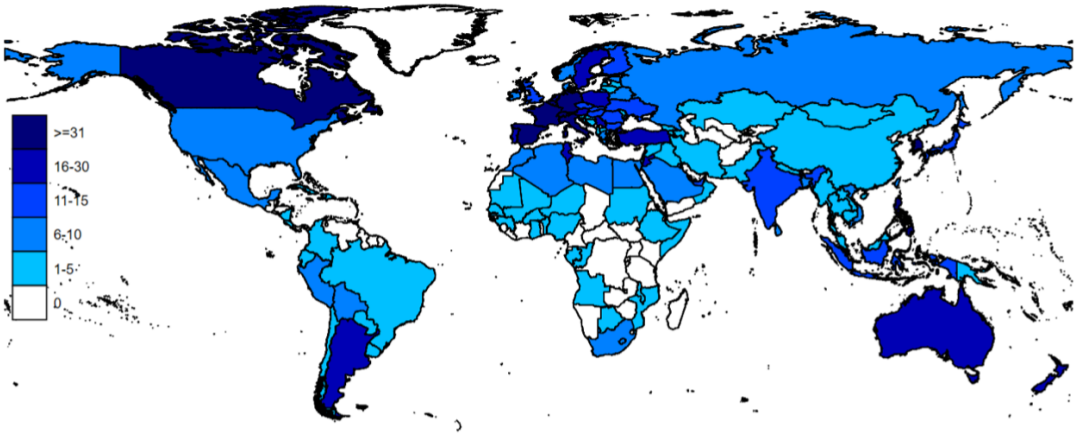
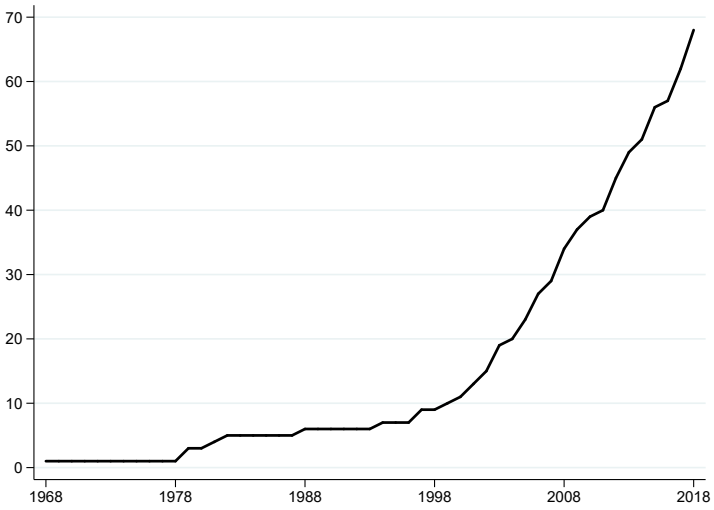


Figure 2.2: The Philippines Bilateral Labor Agreements

(a) Cumulative Bilateral Labor Agreements Over Time



(b) Philippines Bilateral Labor Agreements Partner Countries

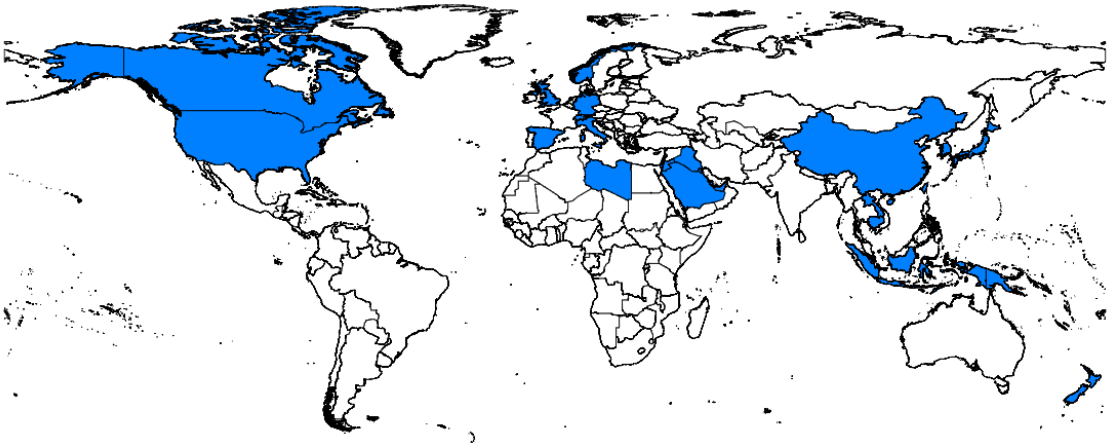


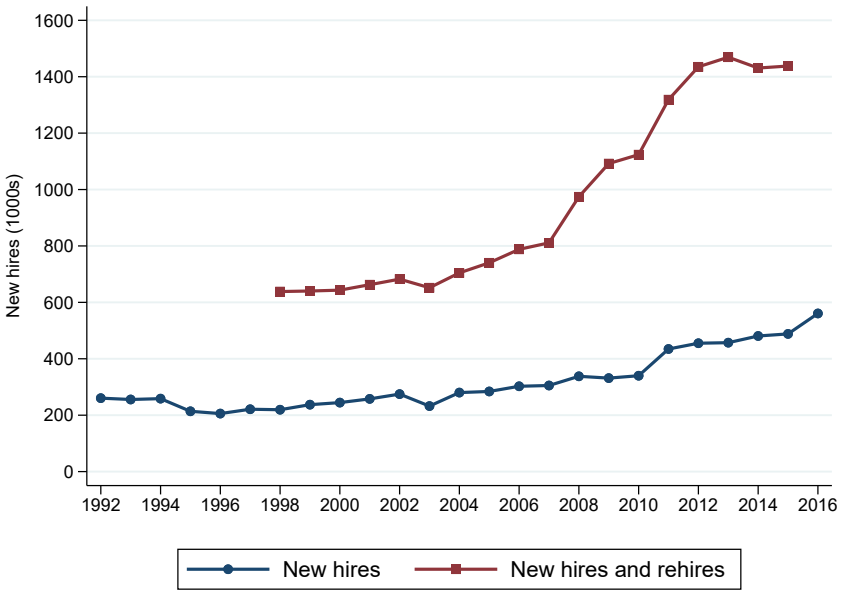
Table 2.1: List of the Philippines Bilateral Labor Agreements

Country	Date Signed	Country	Date Signed
Bahrain*	12/15/2003	Libya	10/18/1979
Bahrain*	4/4/2007	Libya*	7/17/2006
Cambodia*	12/14/2016	New Zealand*	11/4/2008
Canada	12/18/2006	New Zealand*	9/19/2015
Canada	1/29/2008	Northern Mariana Islands	9/14/1994
Canada	2/8/2008	Northern Mariana Islands*	12/18/2000
Canada	10/1/2008	Norway	6/26/2001
Canada*	9/21/2010	Papua New Guinea	3/14/1979
Canada*	5/19/2012	Papua New Guinea*	11/26/2013
Canada*	10/7/2013	Qatar*	5/10/1997
Canada*	5/8/2015	Qatar*	10/18/2008
Canada*	5/9/2015	Saudi Arabia*	10/21/2005
China*	4/10/2018	Saudi Arabia*	5/19/2013
Germany*	3/19/2013	Saudi Arabia*	4/11/2017
Indonesia*	1/18/2003	South Korea	4/23/2004
Iraq*	11/25/1982	South Korea	12/15/2005
Israel*	9/3/2018	South Korea	10/20/2006
Israel*	9/3/2018	South Korea	5/30/2009
Italy*	12/9/2015	South Korea	5/30/2009
Japan*	1/12/2009	South Korea	4/8/2014
Japan*	11/21/2017	Spain*	6/25/2006
Jordan	11/5/1981	Switzerland*	7/9/2002
Jordan	11/3/1988	Switzerland*	11/14/2014
Jordan	5/27/2010	Taiwan	9/3/1999
Jordan	1/29/2012	Taiwan	1/12/2001
Jordan*	9/6/2018	Taiwan	3/20/2003
Jordan*	9/6/2018	Taiwan*	7/26/2011
Korea*	2017	Taiwan	8/3/2015
Kuwait	9/14/1997	United Arab Emirates	4/9/2007
Kuwait*	3/23/2012	United Arab Emirates*	9/12/2017
Kuwait*	5/11/2018	United Arab Emirates*	9/12/2017
Laos*	7/27/2005	United Kingdom	1/8/2002
Lebanon*	2/1/2012	United Kingdom*	7/30/2003
Lebanon*	2/1/2012	United States of America*	12/28/1968

* BLA is still valid.

Figure 2.3: Deployment of Filipino Workers Overseas

(a) Number of New Hires of Filipino Workers, 1992 to 2016



(b) Number of New Hires of Filipino Workers by Sector, 1992 to 2014

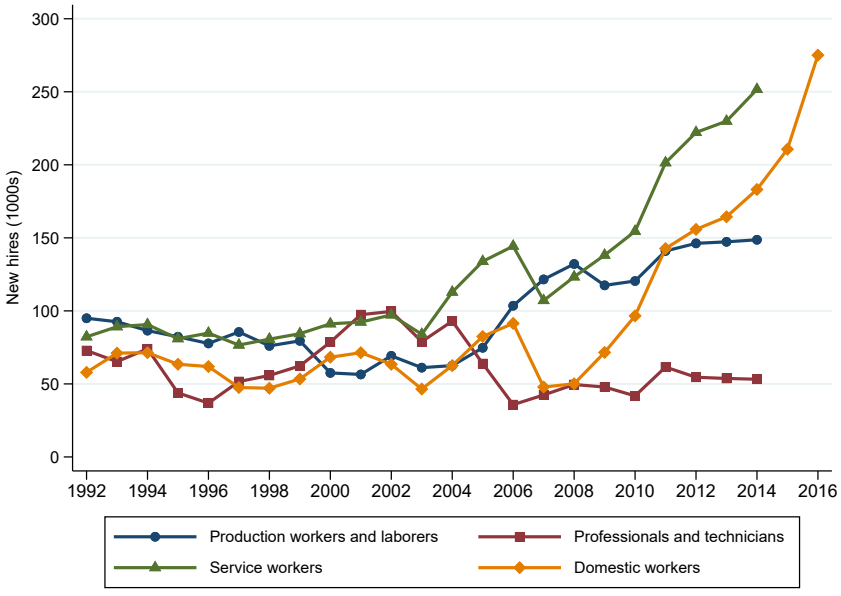


Figure 2.4: Total Monthly Remittances to the Philippines, 1989 to 2019

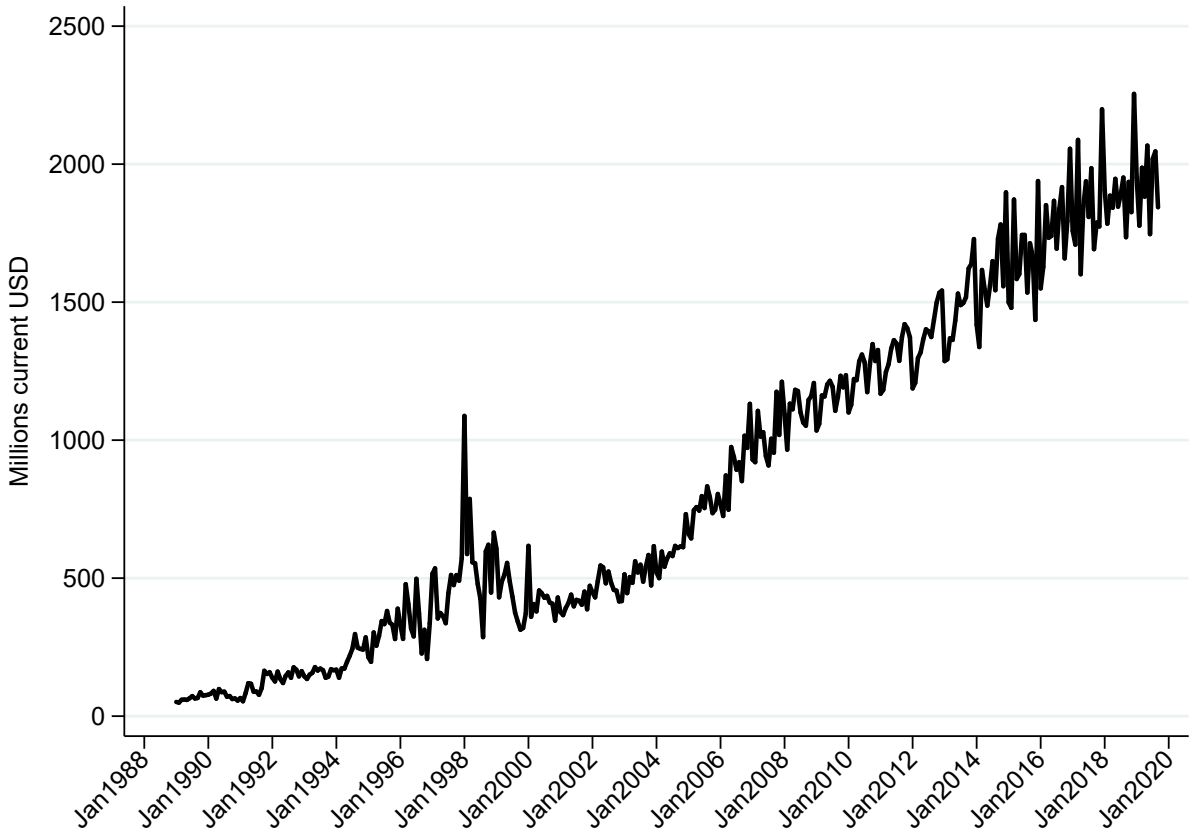


Table 2.2: Summary Statistics of Outcome and Control Variables

	N	Mean	SD	Median	75th pct	99th pct
Deployment Data (1000s per year)						
New hires and rehires	2861	3.48	19.1	.027	.317	87.8
New hires - all sectors	5504	1.44	9.22	.004	.059	42.4
New hires - production workers	5060	.442	3.55	0	.013	15.6
New hires - professional workers	5060	.28	2.96	0	.008	3.99
New hires - service workers	5060	.564	3.52	0	.006	19.5
New hires - domestic workers	5504	.428	3.29	0	.001	13.6
Remittance Data (USD mil.)						
Monthly land-based remittances	74985	4.07	31.5	0	.033	91.7
Monthly total remittances	74984	5.05	40.1	0	.042	113
Annual land-based remittances	6097	47.2	363	.003	.457	1078
Annual total remittances	6097	58.5	465	.004	.576	1320
Control variables						
GDP per capita of host	5301	16497	19337	9156	23742	92349
Population of host (mil.)	6220	30.2	122	5.41	18.5	323
Polity2 of host	4787	3.05	6.66	6	9	10
GDP per capita ratio (host to PHL)	5301	3.3	3.92	1.82	4.51	19.3
Higher Polity Score in PHL	4787	.619	.	1	1	1
Total trade (USD mil.)	3795	724	2790	11.1	185	15996
BIT in the past	6878	.13	.	0	0	1
Ideological difference	5412	.713	.672	.457	1.21	2.84

Note: summary statistics cover the following periods: (1) Monthly remittances: Jan. 1989 - Sept. 2019; (2) Annual remittances and control variables: 1989-2018; (3) New hires in all sectors and new hires of domestic workers: 1992-2016; (4) New hires in production, professional, and service sectors: 1992-2014; (5) New hires and rehires: 1998-2010.

Figure 2.5: Deployment Results: Three Years Before and After BLA by Country

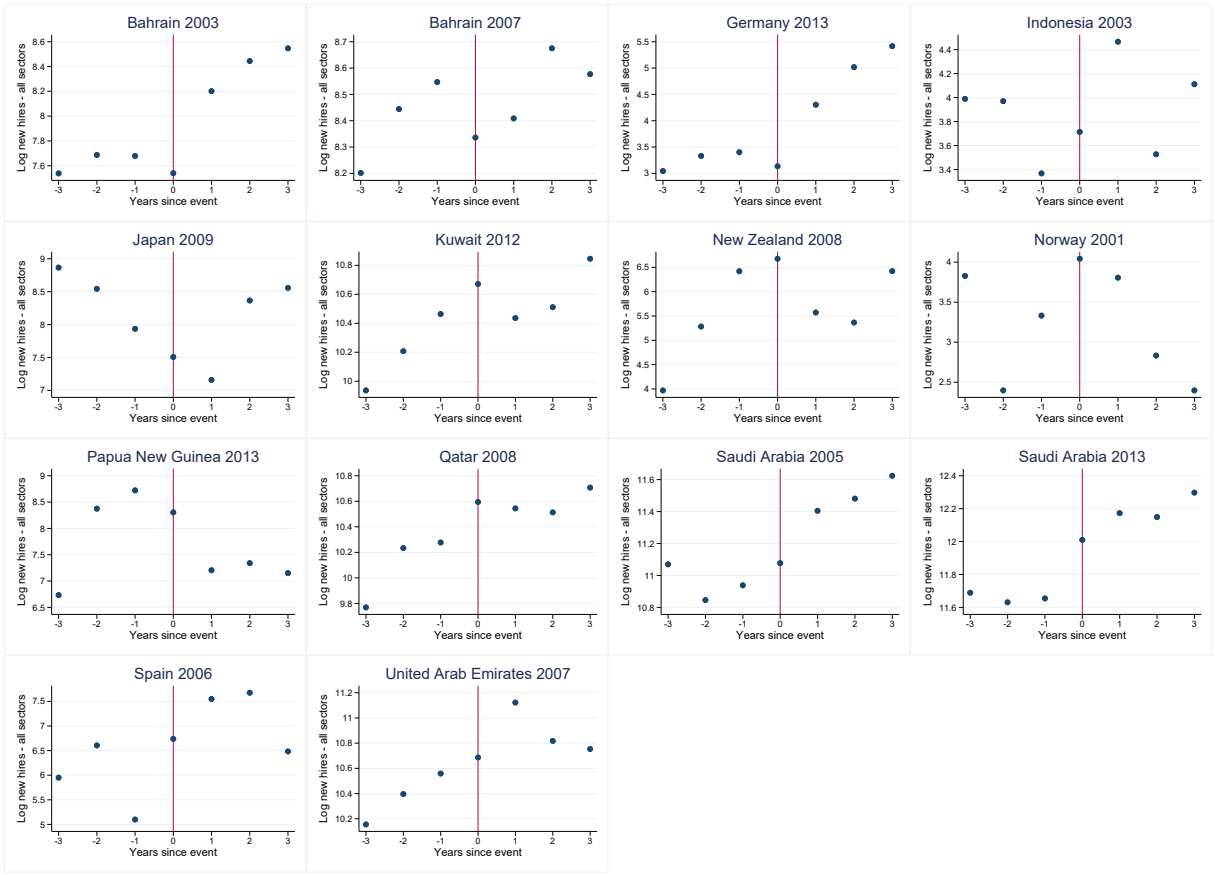


Figure 2.6: Deployment Results: Three Years Before and After BLA Event Study

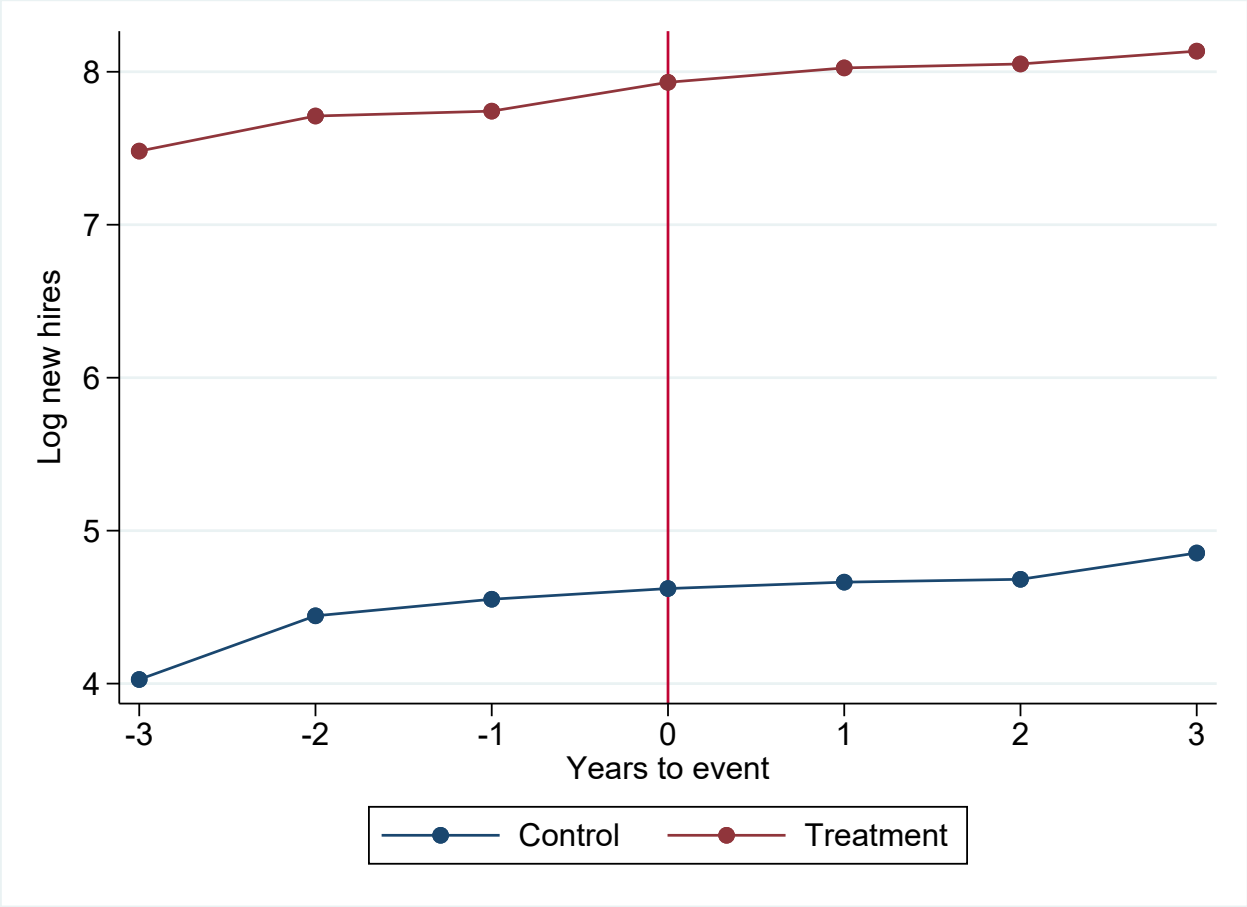


Table 2.3: Total new hires: all workers. Stacked event model - 3 years around event.

	(1)	(2)	(3)	(4)	(5)
Treated unit	0.203	0.198	0.143	0.247	0.248
	(0.259)	(0.220)	(0.206)	(0.267)	(0.223)
Post-treatment period	0.044	0.034	0.035	0.047	0.034
	(0.046)	(0.038)	(0.042)	(0.041)	(0.039)
Treated × post-treatment	0.015	0.031	0.148	-0.009	0.013
	(0.184)	(0.172)	(0.185)	(0.183)	(0.171)
Ln(GDP per capita of host)		2.714***			2.231***
		(0.572)			(0.605)
Ln(population of host)		1.802**			2.073*
		(0.676)			(0.974)
Polity2 of host		0.022			0.012
		(0.020)			(0.024)
GDP per capita ratio (host to PHL)			0.161		0.072
			(0.091)		(0.089)
Higher Polity Score in PHL			-0.361		-0.251
			(0.258)		(0.307)
Ln(total trade)				0.176	0.054
				(0.113)	(0.116)
BIT in the past				-0.281	-0.115
				(0.360)	(0.309)
Ideological difference				-0.229	-0.245
				(0.212)	(0.189)
Year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes
Unique events	14	14	14	14	14
N	5215	5215	5215	5215	5215
R^2	0.842	0.853	0.845	0.844	0.854

Note: Only countries with all non-missing controls and dependent variables are included. In addition, countries with the dependent variable equal to zero in at least one period in the event window are dropped. All models estimated with OLS. Standard errors, clustered at the event, host country, year and month level, are in parentheses. Significance levels: * 10% ** 5% *** 1%.

Figure 2.7: Deployment Results: Three Years Before and After BLA Event Study by Sector

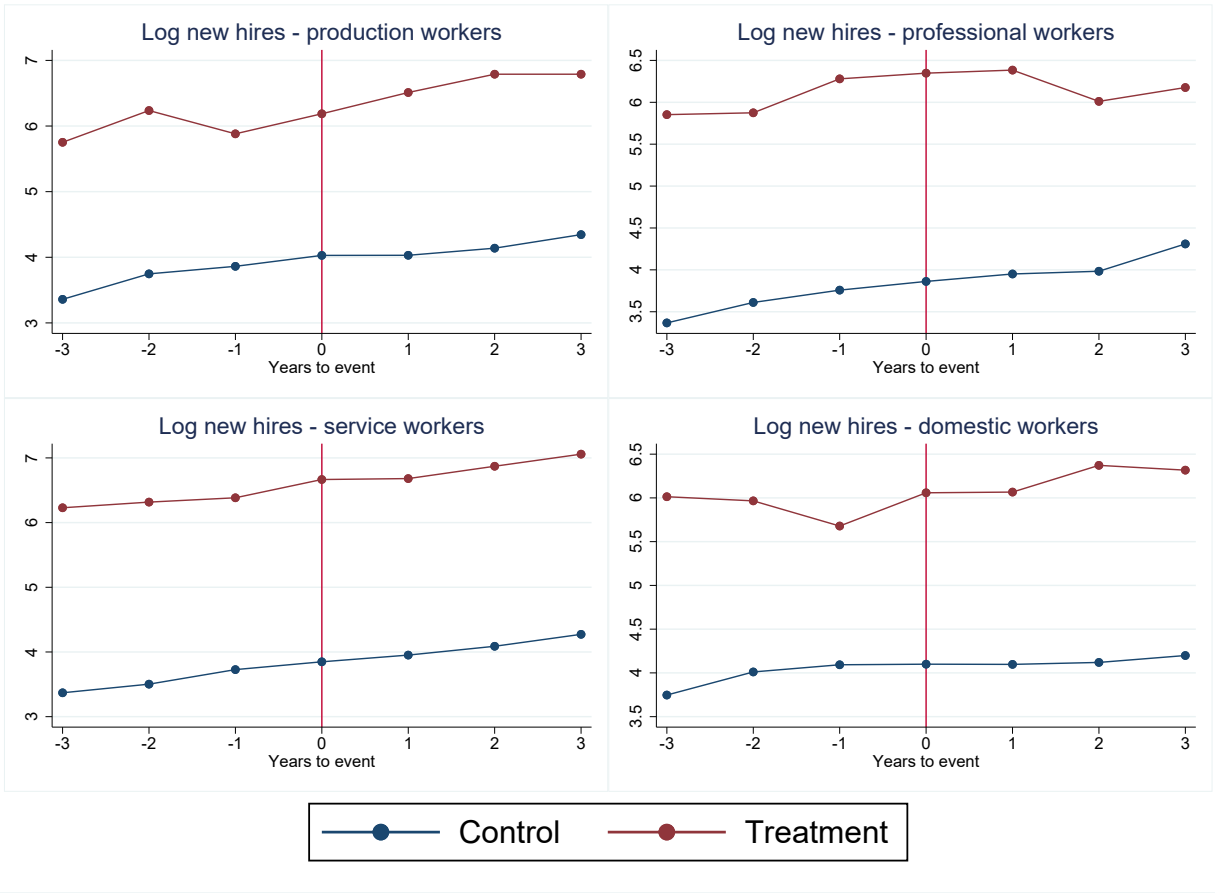


Table 2.4: Deployment results: alterative outcome variables.

	(1)	(2)	(3)	(4)	(5)
A. *Production workers and laborers*					
Treated × post-treatment	0.115	0.077	0.090	0.113	0.076
	(0.198)	(0.267)	(0.374)	(0.208)	(0.295)
N	1596	1596	1596	1596	1596
B. *Professional workers*					
Treated × post-treatment	-0.204	-0.444	-0.085	-0.230	-0.403
	(0.215)	(0.283)	(0.236)	(0.226)	(0.263)
N	1764	1764	1764	1764	1764
C. *Service workers*					
Treated × post-treatment	0.008	0.138	0.124	-0.002	0.120
	(0.126)	(0.189)	(0.144)	(0.138)	(0.211)
N	1967	1967	1967	1967	1967
D. *Domestic workers*					
Treated × post-treatment	0.117	-0.022	0.142	0.087	-0.049
	(0.184)	(0.189)	(0.357)	(0.155)	(0.202)
N	1974	1974	1974	1974	1974
E. *All hires and rehires*					
Treated × post-treatment	-0.008	0.068	0.174	0.003	0.124
	(0.052)	(0.138)	(0.132)	(0.069)	(0.158)
N	3220	3220	3220	3220	3220
Year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, and year levels, are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

Figure 2.8: Remittances Results: Three Years Before and After BLA by Country

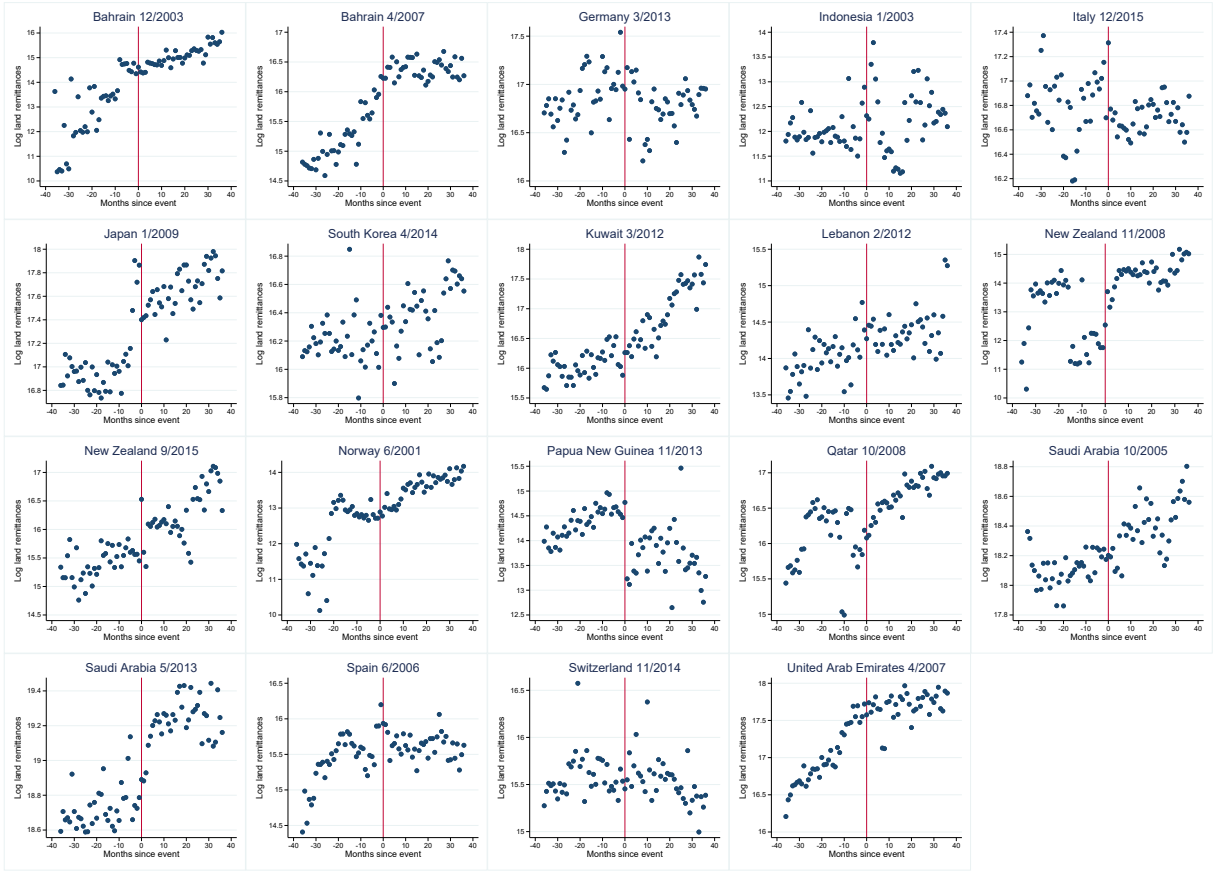


Figure 2.9: Remittance Results: Three Years Before and After BLA Event Study

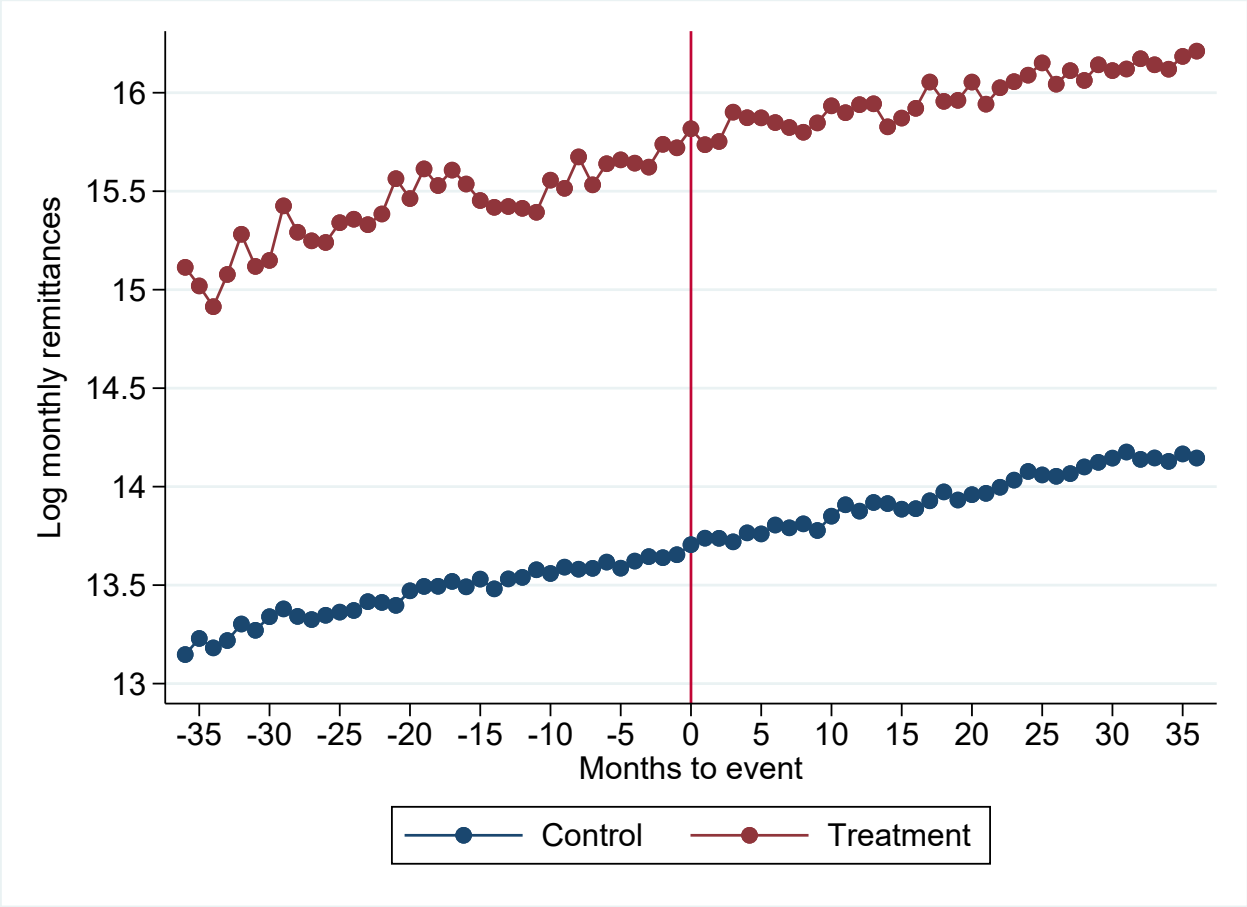


Table 2.5: Remittances from landbased workers. Stacked event model - 36 months around event. OLS.

	(1)	(2)	(3)	(4)	(5)
Treated unit	-0.119	-0.011	-0.180	-0.044	0.005
	(0.160)	(0.144)	(0.147)	(0.162)	(0.145)
Post-treatment period	-0.019***	-0.015***	-0.021***	-0.016***	-0.015***
	(0.003)	(0.004)	(0.003)	(0.005)	(0.005)
Treated × post-treatment	0.038	-0.001	0.101	-0.004	-0.015
	(0.165)	(0.171)	(0.159)	(0.176)	(0.174)
Ln(GDP per capita of host)		2.401***			2.170**
		(0.561)			(0.840)
Ln(population of host)		2.678***			2.715***
		(0.404)			(0.555)
Polity2 of host		-0.049*			-0.035
		(0.027)			(0.030)
GDP per capita ratio (host to PHL)			0.064		0.024
			(0.092)		(0.080)
Higher Polity Score in PHL			0.585**		0.383
			(0.227)		(0.238)
Ln(total trade)				0.212**	0.045
				(0.087)	(0.063)
BIT in the past				0.363*	0.605*
				(0.183)	(0.302)
Ideological difference				-0.369*	-0.182
				(0.181)	(0.146)
Month × year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes
Unique events	19	19	19	19	19
N	55115	55115	55115	55115	55115
R^2	0.915	0.926	0.916	0.917	0.926

Note: Only countries with all non-missing controls and dependent variables are included. In addition, countries with the dependent variable equal to zero in at least one period in the event window are dropped. All models estimated with OLS. Standard errors, clustered at the event, host country, month × year level, are in parentheses. Significance levels: * 10% ** 5% *** 1%.

Figure 2.10: Sensitivity Analysis: Point Estimates of 95 Regressions Estimating Effect of BLA on Deployment

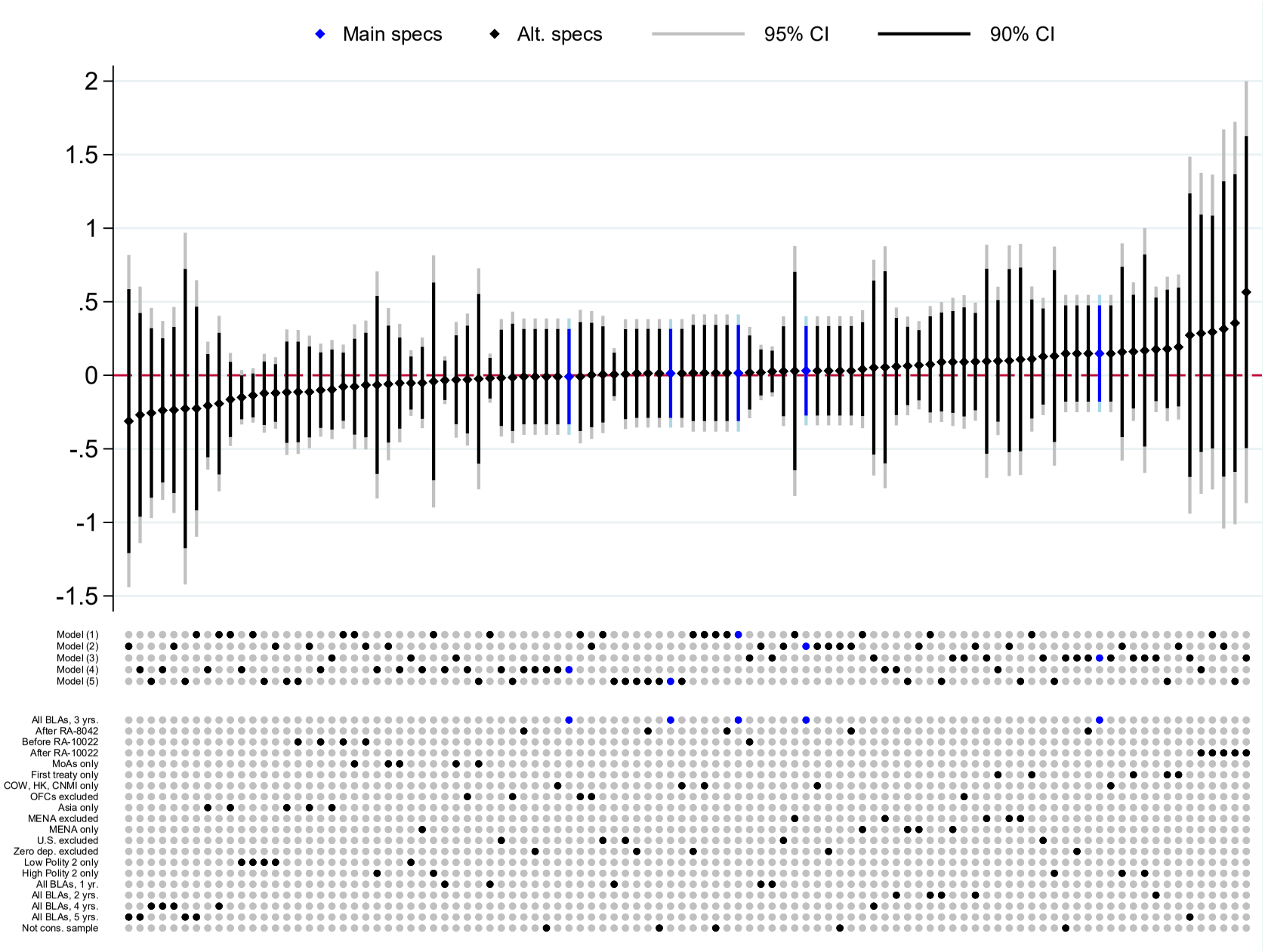


Figure 2.11: Sensitivity Analysis: Point Estimates of 120 Regressions Estimating Effect of BLA on Remittances

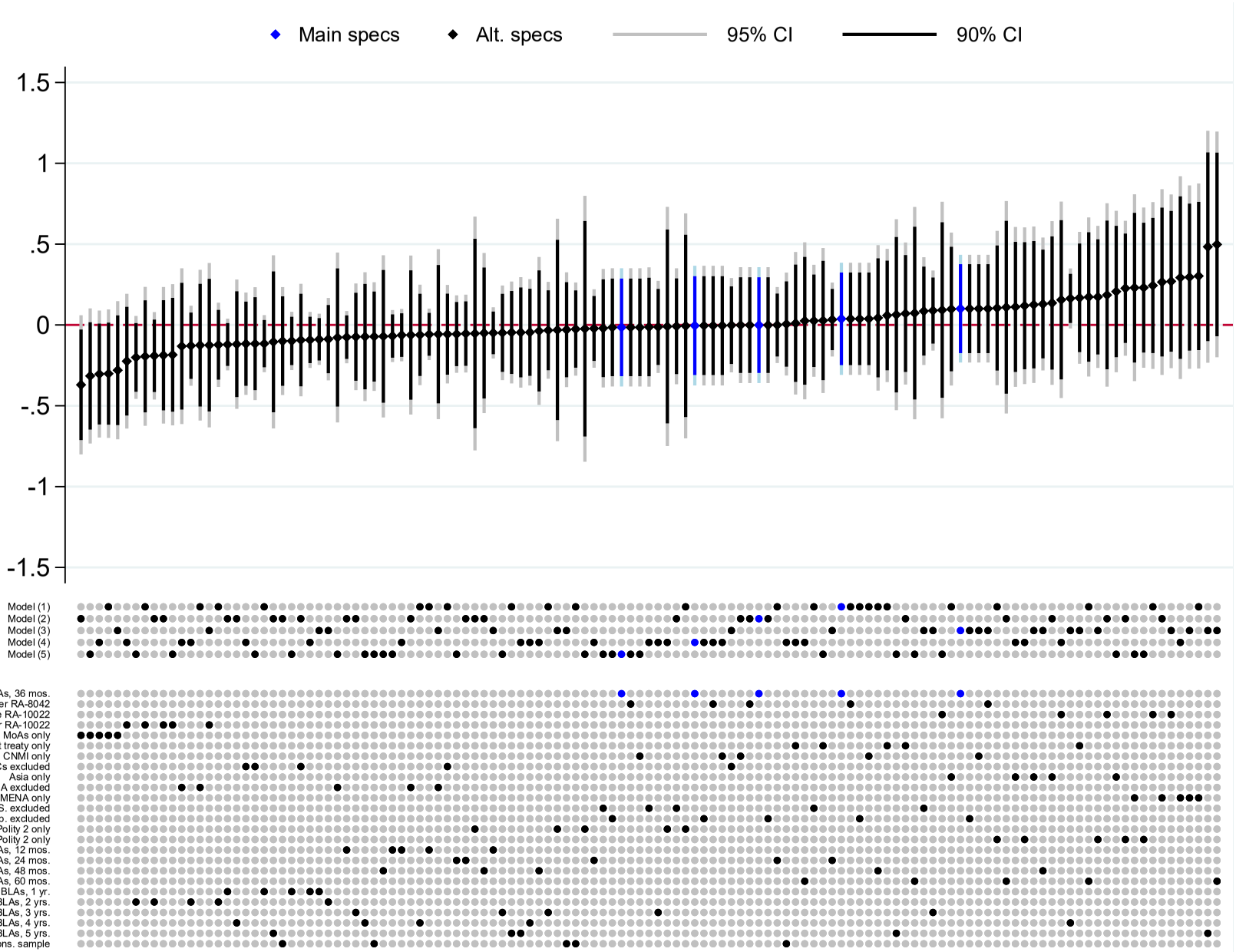


Figure 2.12: Synthetic Control Analysis: Deployment.

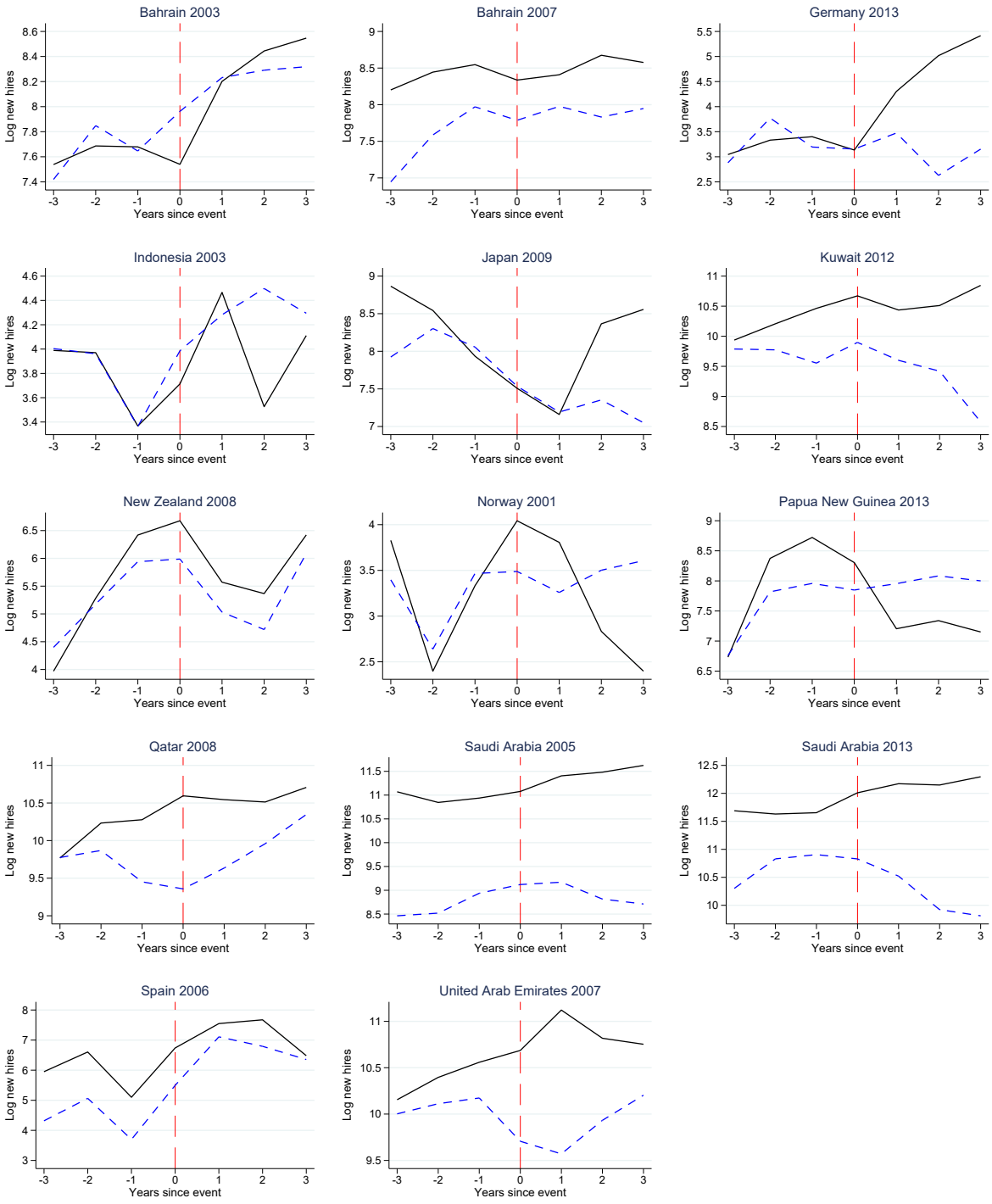
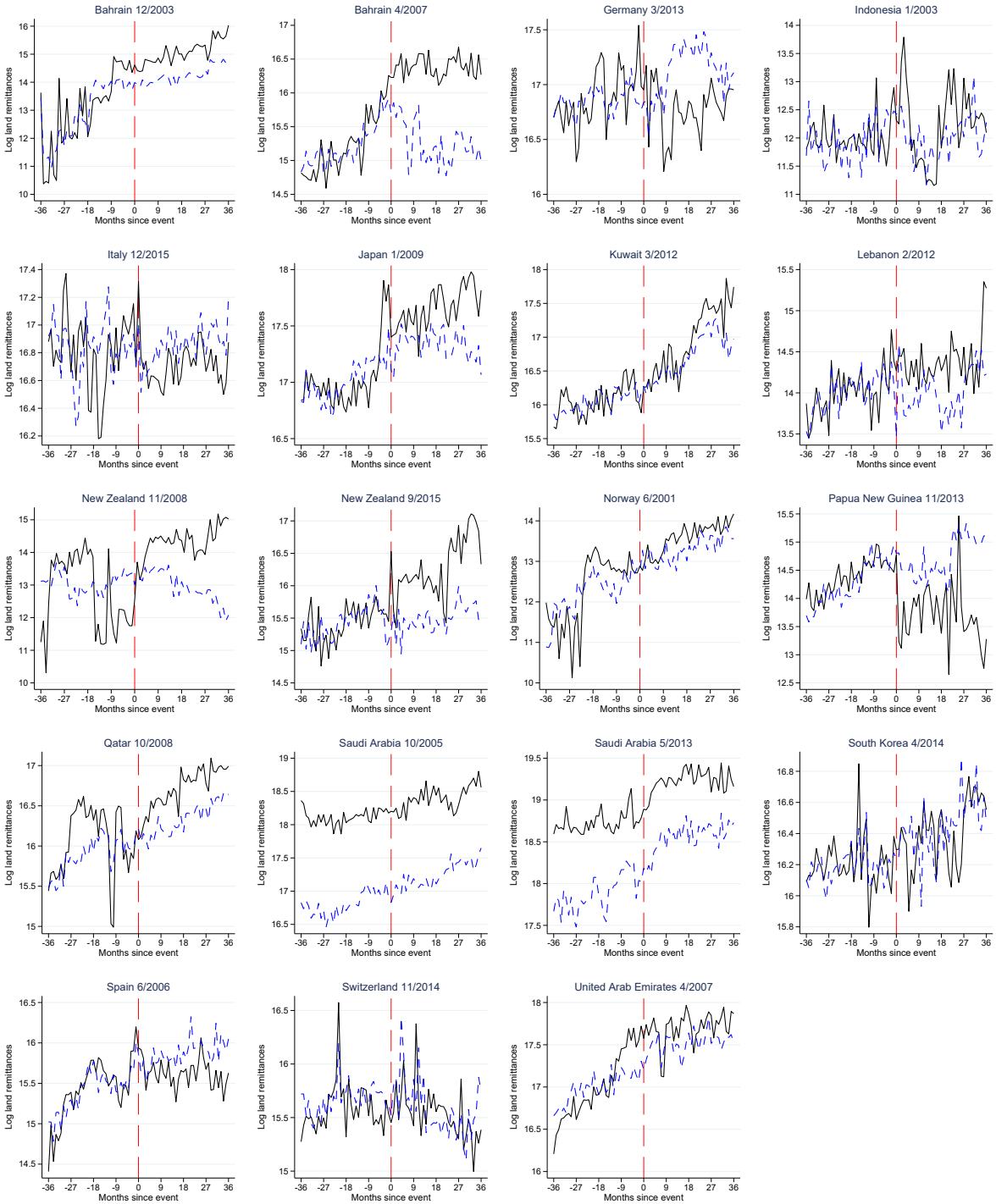


Figure 2.13: Synthetic Control Analysis: Remittances



CHAPTER III

Impact of Immigrants with Heterogenous Communication Skills on U.S. Workers' Labor Supply

3.1 Introduction

Since the early 1980s, the United States has experienced a substantial influx of immigrants. In 2010, the share of foreign-born individuals reached 14% of the total U.S. population, as high as during the Age of Mass Migration in the late 19th and early 20th centuries (Abramitzky and Boustan, 2017). A large portion of those immigrants are relatively less educated, with a high-school degree or less. As a result, concerns have arisen about the impact of those migration flows on the labor market prospects of less educated native (U.S.-born) workers. Economists have explored wage and employment consequences of immigration, as well as the avenues through which native workers respond to it. One response channel which has received attention in the literature is occupational sorting. Peri and Sparber (2009) argue that native U.S.-born workers have a comparative advantage over migrants in tasks requiring verbal and written communication, due to their English language skills and possession of other forms of U.S.-specific human capital. Conversely, immigrants have a comparative advantage in manual tasks which require physical strength and dexterity. Therefore, in response to an influx of foreign-born workers, less educated natives should sort into occupations which are relatively more intense in communication tasks. Using Census

data for the period 1960 – 2000, they show that in states with higher foreign-born shares, U.S.-born workers supply relatively more communication skills and fewer manual ones.

In this paper, I build on the theoretical and empirical framework developed by Peri and Sparber (2009) and examine how heterogeneity of immigrants' communication abilities affects the native response to immigration in the U.S. I argue that in a labor market where the foreign-born are relatively productive in communication tasks, natives would not sort into communication-intensive occupations to the same degree as in a location where immigrants are less efficient in those tasks. That is, higher communication productivity among the foreign-born should suppress natives' occupation switching response. In order to capture migrants' communication productivity, I experiment with six different measures of foreign-born communication types[1], informed by previous literature and the data. My measures are based on immigrants' language skills, years since arrival in the United States, and countries of origin. I follow Peri and Sparber (2009) in estimating the correlation between natives' relative supply of tasks and the foreign-born share but I also interact that share with my measures of foreign-born type, one at a time. I verify my results with an instrumental variables approach, using an instrument that I argue is correlated with both state-level foreign-born shares and my type measures. My findings suggest that natives do switch to more communication-intensive occupations in response to immigration but only if the migrants are of relatively low communication types.

The rest of the paper is organized as follows: section 3.2 briefly summarizes the literature on the impact of immigration on less educated natives, section 3.3 explains the theoretical model, section 3.4 summarizes the data and empirical approach, section 3.5 presents the results, and section 3.6 concludes.

[1]I use the term *communication type* to refer to immigrants' productivity in communication tasks. Thus, a *low* communication type immigrant is one who is not able to effectively perform communication tasks in the U.S. labor market. I do not make any statements about the communication ability of such a migrant in other countries' labor markets.

3.2 Previous literature

The literature on the impact of less educated immigrants on natives' employment and wages can be divided into two broad strands. One estimates the effect by comparing foreign-born shares across some well-defined labor markets, such as states or cities, and correlating them with natives' outcomes in those markets. Papers that adopt this approach, e.g. Card (2007) and Cortes (2008), generally find small effects of immigration on natives. A related methodology is to study a natural experiment where a plausibly exogenous immigration shock occurs in a particular labor market. A famous study of that type is Card (1990) who finds that the Mariel boatlift, which substantially and rapidly increased the labor force in Miami, had little impact on natives' employment and wages. Dustmann et al. (2017), who study Czech workers in a German border region, find stronger effects of migration on natives but also show that those effects vary with labor force characteristics of the native workers.

A potential shortcoming of comparing different labor markets within the U.S. to one another is that workers may relocate in response to immigration, hence attenuating any observed effects on wages and employment. Therefore, the second strand of literature emphasizes the importance of examining effects of migration using national data. Proponents of this approach usually divide native and foreign-born workers into groups based on observable labor market characteristics, such as education and experience. This method generally results in larger estimated effects of immigration on less educated natives. Some prominent examples in that literature include Borjas (2003) and Borjas and Katz (2007).

More recently, authors have focused on imperfect substitutability between foreign-born and native-born workers by studying how those two groups differ in their task productivities. If immigrants and natives bring different productive skills to the labor market, immigration should have limited impact on natives' wages and employment. Papers that have emphasized this point include Ottaviano and Peri (2012) and Dustmann et al. (2013). Related to the notion of different distributions of skills is the idea that natives can respond to immigration by altering the mix of productive tasks they supply to the market, i.e. switching occupa-

tions. Peri and Sparber (2009) find that less educated U.S.-born workers shift towards more communication-intensive jobs in response to the arrival of foreign-born who have a comparative advantage in manual tasks. As a consequence, their simulated effects of immigration on natives' average wages are negative but close to zero at the national level, although there is some variation across states. D'Amuri and Peri (2014) conduct a similar analysis for a group of Western European countries and conclude that less educated migrants in those countries in the late 90s and 2000s also pushed natives to move away from manual jobs and into more communication-intensive ones. This resulted in a slight increase in natives' average wages. Bisello (2014) replicates the Peri and Sparber (2009) approach in the UK context and finds an even larger degree of occupational mobility among natives in response to immigration. Finally, Amuedo-Dorantes and de la Rica (2011) apply the Peri and Sparber (2009) framework to Spain and find that Spanish-born natives also switch into communication-intensive occupations as a consequence of immigration. They observe that about half of recent migrants into Spain are from Spanish-speaking countries which suggests that they should be relatively good substitutes for natives in communication tasks. They hypothesize that this might mute natives' occupation switching response. However, their estimated effects are twice as large as those in Peri and Sparber (2009). One possible reason is that they only look at migrants who have been in Spain for 5 years or less, a point to which I return later.

3.3 Theory

In this section, I briefly summarize the theoretical model developed by Peri and Sparber (2009)[2] and explain how its predictions motivate my own analysis. A final consumption good Y is produced according to the following production function:

$$Y = \left[\beta Y_L^{\frac{\sigma-1}{\sigma}} + (1-\beta)\, Y_H^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}},$$

[2]The model is exactly the same as in Peri and Sparber (2009), except with slightly changed notation.

where Y_H and Y_L are intermediate goods produced by high and low-education workers, respectively. In turn, Y_L is produced according to the production function:

$$Y_L = \left[\beta_L M^{\frac{\theta_L - 1}{\theta_L}} + (1 - \beta_L) C^{\frac{\theta_L - 1}{\theta_L}}\right]^{\frac{\theta_L}{\theta_L - 1}}.$$

The inputs C and M are communication and manual tasks, respectively, and are supplied by less educated workers. The less educated labor force consists of foreign-born workers (indexed by F) and native ones (indexed by N). Further, each worker i supplies $c_{ij} = (1 - l_{ij})^{\delta}\zeta_j$ communication tasks and $m_{ij} = l_{ij}^{\delta}\mu_j$, where $j = F, N$; $l_{ij} \in (0, 1)$ is the share of time the worker spends on manual tasks; ζ_{ij} and μ_{ij} are the worker's communication and manual productivities, respectively; and $\delta \in (0, 1)$, so that all workers supply positive amounts of both tasks. Crucially, Peri and Sparber (2009) assume that $\frac{\zeta_N}{\mu_N} > \frac{\zeta_F}{\mu_F}$, that is, native workers have a comparative advantage over foreign-born ones in communication tasks. The equilibrium aggregate relative supply of communication to manual tasks by native workers as a function of the foreign-born share f is given by:

$$\frac{C_N^*}{M_N^*} = \alpha \left[\varphi(f) \left(\frac{\zeta_F}{\mu_F}\right)^{\frac{1}{1-\delta}} + (1 - \varphi(f)) \left(\frac{\zeta_N}{\mu_N}\right)^{\frac{1}{1-\delta}}\right]^{\frac{-1}{(1-\delta)\theta_L + \delta}\frac{\delta}{1-\delta}} \tag{3.1}$$

$$\alpha = \left(\frac{1 - \beta_L}{\beta_L}\right)^{\frac{\delta\theta_L}{(1-\delta)\theta_L + \delta}} \left(\frac{\zeta_N}{\mu_N}\right)^{\frac{1}{1-\delta}} > 0$$

$$\varphi(f) = \frac{M_F}{M_F + M_D}$$

Because $\varphi'(f) > 0$ and it is assumed that $\frac{\zeta_N}{\mu_N} > \frac{\zeta_F}{\mu_F}$, equation (3.1) implies that natives' relative task supply depends positively on the share of foreign-born in the labor force f. However, it also depends *negatively* on the relative communication to manual productivity of the foreign-born, $\frac{\zeta_F}{\mu_F}$, or what I refer to as the foreign-born "type." Moreover, because the foreign-born type muliplies the foreign-born share in Equation (3.1), a higher foreign-born type reduces the effect of a given foreign-born share. In the extreme case that $\frac{\zeta_N}{\mu_N} = \frac{\zeta_F}{\mu_F}$, i.e.

foreign-born workers are equally productive in communication tasks to native workers, the foreign-born share no longer affects natives' relative supply choice[3]. In this paper, I focus on this interaction of the foreign-born share with the foreign-born communication productivity.

3.4 Data and empirical specification

The predictions of the Peri and Sparber (2009) model outlined in section 3.3 suggest the following regression specification:

$$ln\left(\frac{C_N}{M_N}\right)_{st} = \beta_0 + \beta_f f_{st} + \beta_p p_{st} + \beta_{fp} f_{st} \times p_{st} + \beta_s + \beta_t + \varepsilon_{st}, \tag{3.2}$$

where $ln(C_N/M_N)_{st}$ is the aggregate relative supply of communication to manual tasks by natives in state s and year t, f_{st} is the foreign-born share of the less educated labor force, p_{st} is the communication type of the foreign-born, $f_{st} \times p_{st}$ is the interaction of the share and the type, β_s is a state fixed effect, β_t is a year fixed effect, and ε_{st} is the error term. This is similar to the model estimated by Peri and Sparber (2009) except that they did not control for the foreign-born type and its interaction with the share. I focus my analysis on the foreign-born share f_{st} and the interaction term $f_{st} \times p_{st}$. Given the theoretical model, I expect that $\beta_f > 0$ and $\beta_{fp} < 0$, i.e. a higher foreign-born share drives less educated natives to increase their relative supply of communication to manual tasks but this effect diminishes if the foreign-born are of a higher communication type. In addition, I follow Peri and Sparber (2009) in looking at the aggregate supplies of communication and manual tasks separately. Therefore, I also estimate:

$$ln\left(C_N\right)_{st} = \beta_{C0} + \beta_{Cf} f_{st} + \beta_{Cp} p_{st} + \beta_{Cfp} f_{st} \times p_{st} + \beta_{Cs} + \beta_{Ct} + \varepsilon_{Cst} \tag{3.3}$$

[3]It is also possible that foreign-born workers have *higher* communication to manual productivity than natives, in which case the model predictions would be reversed. I consider that case unlikely and do not elaborate on it further.

$$ln\left(M_N\right)_{st} = \beta_{M0} + \beta_{Mf} f_{st} + \beta_{Mp} p_{st} + \beta_{Mfp} f_{st} \times p_{st} + \beta_{Ms} + \beta_{Mt} + \varepsilon_{Mst}. \quad (3.4)$$

I estimate equations (3.2), (3.3), and (3.4) by ordinary least squares. However, both foreign-born share and type (and their interaction) may be endogenous with respect to natives' aggregate supply of tasks. One concern is that unobserved state-specific demand shocks may drive both the mix of tasks supplied by less educated natives in the state, as well as foreign-born immigration into the state (or emigration out of it). Furthermore, the same demand shocks may affect the type of foreign-born who choose to locate in the state. Therefore, I also estimate my regressions with an instrumental variables approach. I use data from the 1980, 1990, and 2000 Census, as well as the American Communities Survey (ACS) 2001-2019, included in the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al., 2020). In the remainder of this section, I explain how I construct the aggregate task supplies, the foreign-born shares, and the foreign-born type measures, as well as my instrument for the potentially endogenous independent variables.

3.4.1 Aggregate task supply measures

My aggregate task measures are constructed using data on occupational characteristics obtained from the Department of Labor's O*NET database[4], following the method and definitions described in Peri and Sparber (2009). The O*NET dataset ranks the importance of 52 different abilities, such as deductive reasoning and arm-hand steadiness, to each occupation identified in the Standard Occupational Classification (SOC). Every occupation is assigned a value between 1 (not important at all) and 5 (very important) of each ability, indicating how crucial the ability is in performing the occupation. I link those rankings to the occupations in the 2000 Census and convert them to percentiles, based on the number of workers in each occupation. Therefore, a worker with a score of 0.65 in "written expres-

[4]The O*NET datasets can be downloaded from https://www.onetcenter.org/db_releases.html (accessed November 29th, 2020). I use version 11.0 in this paper.

sion" supplies that skill more intensely than 65% of the less educated labor force in 2000. I then construct each worker's individual supply of communication and manual tasks as an average of 4 and 19 different abilities, respectively, following the *basic* definitions in Peri and Sparber (2009)[5]. I merge those definitions with the remaining Census and ACS years using the variable *occ1990*, which is a time-consistent classification of occupations provided in the IPUMS datasets. Finally, I construct aggregate state-by-year supply levels by adding all the individual communication and manual tasks supplied by native workers, weighted by their hours worked in the previous year. I include individuals between the ages of 18 and 65 who worked a positive number of weeks in the preceding 12 months, were born in the United States, had at most a high-school degree, and who did not live in group quarters. In addition, I exclude individuals who reported attending school[6]. I follow Peri and Sparber (2009) in cleaning task supplies of demographic characteristics before aggregating them to the state level. In order to do this, I regress individual communication and manual supplies on dummies for work experience (derived from age), gender, high-school degree, and race, separately for each year, and weighted by hours worked. I then aggregate the resulting residuals.

3.4.2 Foreign-born shares and types

Foreign-born shares are defined simply as the number of hours worked by less educated workers born outside the U.S. divided by the number of hours worked by *all* less educated workers in a given state. In order to capture the variation in foreign-born communication types, I propose six different measures.

[5]Communication task supply is the average of oral comprehension, oral expression, written comprehension, and written expression. Manual task supply is the average of arm-hand steadiness, manual dexterity, finger dexterity, control precision, multilimb coordination, response orientation, rate control, reaction time, wrist-finger speed, speed of limb movement, extent flexibility, dynamic flexibility, gross body coordination, gross body equilibrium, static strength, explosive strength, dynamic strength, trunk strength, and stamina.

[6]Peri and Sparber (2009) do not exclude those individuals. However, most of those who report school attendance in the data are college or graduate students and should not be considered part of the less educated labor force.

3.4.2.1 Foreign-born types measured by language skills

Since 1980, the Census and the ACS have included a question about each household member's ability to speak English. I use that question to propose the following measures of foreign-born communication types: *share of the foreign-born who speak English well/v.well/only* and *ordinal English ability of the foreign-born*. The former is simply the share of foreign-born individuals in the state who report speaking english well, very well, or English only. I obtain the latter by recoding the language proficiency question into numerical values according to the following scheme: 0 - does not speak English, 1 - speaks English but not well, 2 - speaks English well, 3 - speaks English very well, 4 - speaks only English. I then average those values over the immigrant workers in each state. Table 3.1 reports individual-level summary statistics on the foreign-born's relative supply of communication to manual tasks (column labeled "C/M ratio"), as well as communication and manual tasks, individually (columns labeled "Communication" and "Manual"). The data are from the 1980, 1990 and 2000 Census, and single-year ACS from 2001 to 2019. Panel B shows that the self-assessed English ability question correlates highly with the foreign-born's supply of those tasks. Those who speak English very well or only English work in more communication-intensive occupations than the foreign-born overall (reported in the second row of Panel A). In fact, the foreign-born who speak only English work in occupations with a C/M ratio nearly as high as natives (the first row of Panel A). The differences among the language ability groups are driven both by higher communication and lower manual supply of immigrants with higher English proficiency.

There is also evidence in the literature that English proficiency affects immigrants' communication productivity in the U.S. labor market. For example, Kossoudji (1988) finds that poor English skills impede migrants' occupational mobility, suggesting that they may be unable to move into more communication-intensive jobs. Bleakley and Chin (2010), show that English ability reported in the Census predicts a variety of social outcomes of immigrants and that those with better English skills tend to be more like U.S. natives in terms of divorce

rates, fertility, and spouse choices. This is particularly important if communication type is determined not just by language proficiency but by some deeper level of integration into American society and understanding of American culture.

3.4.2.2 Foreign-born types measured by years since arrival

My second set of foreign-born communication type variables relies on years since arrival in the United States and includes the following two measures: *share of foreign-born who arrived in the U.S. 21 or more years ago* and *average years since arrival in the U.S. among the foreign-born*. Panel C of Table 3.1 shows that immigrants who have been in the U.S. longer tend to perform occupations with higher communication intensities. This is consistent with the idea of assimilation: as migrants spend time in the United States, they learn both the language and customs of the native-born population. In economics, the question of immigrants' assimilation has been explored in the context of earnings and employment (Chiswick, 1978; Borjas, 1989; Antecol et al., 2006). The literature generally finds that assimilation does occur along this dimension, albeit quite slowly.

3.4.2.3 Foreign-born types measured by country of birth

My final two communication type measures are determined by the immigrants' countries of birth: *share of foreign-born from countries with English as an official language* and *country-specific communication abilities*. In order to calculate the share of immigrants born in countries with English as an official language, I use data on country languages from Melitz and Toubal (2012, 2014). The *country-specific communication abilities* measure is constructed by first regressing each foreign-born's individual communication task supply on gender, high-school degree, and dummies for experience, years since arrival, race, year, state, and country of birth. I interpret the coefficients on dummies for country of birth as measures of communication productivity associated with migrants from those countries. I then rescale those coefficients to percentiles and calculate their averages for each state and year. Panel D

of Table 3.1 shows that an immigrant's country of birth is strongly correlated with her supply of communication tasks. Those born in countries with English as an official language hold occupations with nearly the same communication intensity as U.S.-born natives. It is important to note that the official language status may proxy for more than just the migrant's language skills, such as shared culture, history, or beliefs, all of which would raise the migrant's communication productivity. In Table 3.1, I also list countries whose migrants have occupations with the highest and lowest communication intensities in the U.S. labor market (among those with non-zero numbers of foreign-born in the U.S. in each year of my data). Not surprisingly, migrants with the highest communication task supplies are those from the UK, Australia, and two Western European countries with a high prevalence of English as a second language. Interestingly, those individuals hold occupations with much greater communication intensities than natives, suggesting some kind of selection in migration or hiring decisions.

Literature which suggests using country of birth as a measure of communication type includes Bacolod and Rangel (2017), who find that migrants from countries with languages more similar to English sort into more communication intensive occupations, and de Matos (2017b), who shows that relative to Eastern European immigrants in Portugal, Brazilian immigrants (who speak Portugese) perform jobs with more emphasis on language skills.

Note that all my type variables are constructed in such a way that higher values imply a higher communication type of the foreign-born. Therefore, I expect to find a negative coefficient on the interaction of the foreign-born share and type. Table 3.2 presents the summary statistics of all my foreign-born type measures at the state by year level (as used in the regressions), along with foreign-born shares and the outcome variables, weighted by the size of the less educated labor force. The mean foreign-born share is 19% but there is substantial variation across time and states, from 0.51% in 1990 West Virginia to 49.9% in 2012 California. Likewise, all my type measures vary quite a bit: state-level average years since arrival are equal to 13.4 at the 10th percentile and 20.8 at the 90th, shares from

countries with English as an official language are 0.04 at the 10th percentile and 0.22 at the 90th , and the corresponding values for the share who speak English well, very well, or only English are 0.5 and 0.73.

3.4.3 Instrument for foreign-born shares and types

A concern about estimating equations (3.2), (3.3), and (3.4) with OLS is that there may be time-varying state-specific demand shocks which simultaneously affect natives' supply of tasks, as well as the share and type of the foreign-born locating in that state. Consider a technological improvement that reduces the demand for manual tasks in a particular state (e.g. self-driving buses which are only approved in California). Such a change might raise the relative supply of natives' communication tasks, while also reducing the number of migrants choosing to locate in California. It may also affect the *type* of immigrants who choose to locate in the state.

In addition, some of my type measures are subject to measurement error. For example, it has been argued that the self-assessed English-speaking ability reported in the Census and the ACS is measured with considerable error (Bleakley and Chin, 2004; Dustmann and Fabbri, 2003). Also, English-speaking ability is potentially a choice variable which may respond to the relative supply of tasks by natives. Further, the reliability of some measures may vary over time and with the country of origin, e.g. both Philippines and Canada have English as an official language but we would expect migrants from those two countries to be of different communication types in the U.S. labor market.

Therefore, I propose a set of instruments based on historical migration and national growth rates in the number of immigrants from different countries. First, for each country of birth represented in the 1980 Census, I calculate the number of migrants from that country in each state in 1980, keeping only those countries which have a positive number of migrants in all my sample years (1980, 1990, 2000-2019). I then calculate the changes over time in the population of migrants from those countries at the *national* level. Based on the initial

1980 allocation and the national growth rates, I predict the number of immigrants from each country residing in each state in all the other years in my sample. Finally, I convert them to shares of the overall less educated labor force in each state. Thus, I obtain a set of K instruments[7]:

$$\tilde{f}_{kst} = \frac{mig_{ks,1980} \times (1 + g_{k,t-1980})}{nat_{st} + \sum_{k=1}^{K}(mig_{ks,1980} \times (1 + g_{k,t-1980}))}, \tag{3.5}$$

where $\tilde{f}_{kst}$ is the predicted share of foreign-born from country k in state s and year t; $mig_{ks,1980}$ is the number of migrants from country k in state s in the 1980 Census; $g_{k,t-1980}$ is the national growth rate in the number of migrants from country k between years 1980 and t; nat_{st} is the number of natives in state s and year t; and $k \in \{1, ..., K\}$. This instrument exploits the fact the immigration is persistent over time, i.e. migrants from a particular country tend to settle in the same locations as previous migrants from the same country, as there are strong network effects associated with international migration (Munshi, 2003). It is valid under the assumption that historical location decisions are not correlated with current demand shocks. Instruments based on past migration decisions have been used commonly in immigration literature, going back at least to Altonji and Card (1991). My version is closest to the ones used by D'Amuri and Peri (2014) and Bisello (2014).

I argue that the set of instruments described in equation (3.5) are correlated both with the foreign-born shares and all my communication type measures. Due to migrants' from the same country concentrating in the same locations over time, predicted shares are highly correlated with my measures based on country of origin. Similarly, because English-speaking ability is correlated with the country of origin, historical immigrant mix will be associated with my language ability-based measures. Finally, the instrument is correlated with measures based on years since arrival if the mix of migrants coming to the United States changes over time. For example, in 1980 the United States had a lot of migrants from Italy who arrived

[7]There are 112 countries in my sample which meet the criterion of non-zero immigrants in all the sample years. However, in order to avoid problems associated with weak instruments, I only use shares from countries with the 30 largest populations in the United States in the 2000 Census.

many years prior and at the same time new migration from Italy was very low, so a state with a large share of Italian-born immigrants would have a relatively large average years since arrival value. Conversely, there were relatively few Guatemalan-born individuals in the U.S. at the time but immigration from Guatemala was rapidly increasing, implying that a location with a sizable Guatemalan-born population would have fairly low average years since arrival. Figure 3.1 shows that there have indeed been substantial changes in the mix of migrants' countries of origin over my sample period, with the share from countries such as Canada and Italy declining and the share from some South American and Asian ones on the rise[8].

3.5 Results

Here, I present the results of estimating equations (3.2), (3.3), and (3.4), using each of my foreign-born communication type measures, one at a time. Section 3.5.1 includes my OLS estimates, whereas section 3.5.2 shows estimates using the instrument in equation (3.5). All models use total less educated employment in the state and year as weights.

3.5.1 OLS results

Table 3.3 shows the results of OLS estimation. In Panel A, the dependent variable is log aggregate relative task supply by natives, $ln(C_N/M_N)$; in Panel B, log aggregate communication task supply by natives, $ln(C_N)$; in Panel C, log aggregate manual task supply by natives, $ln(M_N)$. The first column contains a regression without any foreign-born type measurement, similar to the model estimated by Peri and Sparber (2009). The estimated coefficient of 0.21 in Panel A implies that a 10 percentage point increase in the foreign-born share (which would be quite a large change given that the sample mean is 19% with a standard deviation of 14%) is associated with an increase in natives' relative communication

[8]The figure excludes Mexico because the share of Mexican-born migrants in the less educated labor force dwarves all other countries and makes the figure less readable. The Mexican-born share reached approximately 10% in the 2000s

to manual task supply of 2.1% and this effect is statistically significant at the 5% level. Panels B and C show that most of that occurs through an increase in the supply of communication tasks (1.7%, significant at the 5% level), while a smaller part is due to a reduction in the supply of manual tasks (-0.4%, not significant at conventional levels). Those effects are smaller and less precisely estimated than the corresponding coefficients in Peri and Sparber (2009): 3.4% for relative supply, 3.1% for communication supply, and -0.3% for manual supply. The discrepancy may stem from different samples, as they use the 1960 and 1970 Census but not the ACS, and they do not exclude individuals currently in high school, college, or graduate school.

The remaining columns show the results of interacting the foreign-born share with one of my foreign-born communication type measures. The exact measure used in each regression is identified at the top of each column. The results show that the impact of immigration on natives' supply of tasks does depend on the communication type of immigrants. In all models, the interaction term has the expected, negative sign in regressions of $ln(C_N/M_N)$ and $ln(C_N)$, and positive sign in regressions of $ln(M_N)$. In models (4) and (5), with communication type measures based on years since arrival, the coefficients on both the foreign-born share and the interaction are significant at the 1% level. In addition, the interaction is significant at the 10% level in model (6). In each Panel, I also report the results of an F-test of joint significance of the coefficient on the foreign-born share and its interaction with the foreign-born type. The test rejects the null of no joint significance with high probability in all models, except (2) and (3), where the type measures are based on self-reported English ability, a variable which may suffer from high measurement error, as discussed in section 3.4.3.

In order to aid in the interpretation of the results in Table 3.3, I illustrate how the impact of immigration on natives' relative task supply varies with immigrant types in Figure 3.2. In each graph, the coefficient on the foreign-born share in a regression of $ln(C_N/M_N)$ (the y-axis) is plotted against different values of the foreign-born type used in that regression (the x-axis). The solid black line shows the point estimate, while the solid gray lines mark the 95%

confidence interval. The dashed red line indicates the sample mean of the communication type, while the dashed blue lines denote the range of the type found in the sample. The numbers included in x-axis labels correspond to model numbers in Table 3.3. Table 3.3 and Figure 3.2 show that in model (2) a 10 percentage point increase in the foreign-born share is correlated with a 3.6% increase in natives' relative communication to manual task supply if *all* the foreign-born do not speak English well or at all. The effect declines as a larger share of immigrants speak English well, very well, or only English, reaching nearly 0 at the maximum value of this communication type. Note, however, that the effect is generally not statistically significant over the distribution of the communication type. Model (3) implies that a 10 percentage point increase in the foreign-born share is correlated with a 4% increase in natives' relative task supply if the foreign-born's average English ability is 0, i.e. they do not speak English at all. The effect declines to 2.8% if the foreign-born's English ability rises by 1, equal to about 3 times the standard deviation in the sample. Again, zero is in the 95% confidence over almost the entire distribution of this type measure.

Models (4) and (5) suggest that migrants' tenure in the United States (i.e. average years since arrival) is important in determining their impact on natives' relative task supply. The coefficients from equation (4) imply that natives raise their relative task supply by 5.3% in response to a 10 percentage point increase in the foreign-born share if the foreign-born are brand new arrivals, i.e. their years since arrival are zero. The effect declines by 0.2% for each additional year since arrival, reaching zero at about 27 years, as illustrated in Figure 3.2. Taken literally, this would suggest that immigrants have the same communication to manual productivity as natives after spending 27 years in the United States. The importance of migrants' U.S. tenure in their communication productivity is confirmed by model (5) which suggests that a larger share of long-term immigrants (those who have lived in the U.S. for 21 years or longer) dampens natives' relative task supply response to immigration. This response starts at a 3.6% increase in relative supply due to a 10 percentage point rise in the foreign-born share if none of the migrants have lived in the U.S. for more than 21 years, to 0

once three-quarters are long-term migrants. The effect of years since arrival on the impact of immigration, which is estimated more precisely than the effect of language abilities, suggests that language skills are not the only ones necessary for high communication productivity in the less educated U.S. labor market. Rather, migrants acquire knowledge of the U.S. culture, customs, and ways of interacting, as they spend time in the country. In addition, my result might explain why Amuedo-Dorantes and de la Rica (2011) find a much larger effect of immigration on natives' relative task supply in Spain than Peri and Sparber (2009) in the U.S., even though about half the immigrants in their sample are from Spanish-speaking countries. Amuedo-Dorantes and de la Rica (2011) limit their analysis to immigrants who have been in Spain for five years or fewer, who, according to my estimates, would have the largest impact on natives' communication and manual task supply.

Finally, models (6) and (7) show that the foreign-born's countries of origin may also affect how natives adjust their relative task supply as a consequence of immigration. The coefficient in model (6) implies that if all immigrants are from countries which supply the lowest communication productivity in the U.S. labor market, natives increase their communication to manual task ratio by 9.4% in response to a 10 percentage point increase in the foreign-born share. That effect declines to 3.2% if all the migrants are from a median communication type country and may even become negative with high communication productivity countries (although the confidence interval is very wide, due to a small number observations in that range). Finally, in model (7), natives raise their relative supply by 2.7% due to a 10 percentage point rise in foreign-born share if no migrants are from countries with English as an official language and the effect declines with more immigration from English-speaking countries. However, note that the effect over most of the type domain is very imprecisely estimated.

Taken together, these results suggest that the type of immigrants who locate in a particular labor market is important in determining the natives' relative task supply response, with a higher foreign-born communication type dampening that response. In appendix Figure C.1

and Figure C.2, I present similar plots to those in Figure 3.2, separately for communication and manual tasks. The results are similar: lower foreign-born communication type raises natives' communication task supply response to immigration and lowers their manual task supply response.

3.5.2 IV results

In this section, I present results from an instrumental variables estimation, using my set of instruments explained in section 3.4.3. Appendix Table C.1 presents the results of tests for weak instruments in the first stage. In Panel A, I present F-test statistics adjusted for multiple testing, based on Angrist and Pischke (2009). In Panel B, I report the Kleibergen-Paap F-statistics (Kleibergen and Paap, 2006), which are a version of the Cragg-Donald F-statistics (Cragg and Donald, 1993) for non-i.i.d. error terms (Christopher F. Baum and Mark E. Schaffer and Steven Stillman, 2015; Frank Kleibergen and Mark E. Schaffer and Frank Windmeijer, 2007). Based on the critical values for 3 endogenous variables and 30 instruments, reported in Stock and Yogo (2002) (see their Table 1), my instruments are not weak in any regression.

Table 3.4 and Figure 3.3 show the results of the IV estimation in the same layout as Table 3.3 and Figure 3.2. There are only minor differences from the OLS findings. The coefficient in model (1), without a type measure, is no longer statistically significant in any of the three regressions. The coefficients on foreign-born shares in Panel A and Panel B are slightly larger in models (2), (3), (6), (7), and slightly smaller in models (4), (5). The reverse is true for Panel C. Figure 3.3 shows the same patterns of interactions with measures of communication types, although the confidence intervals are generally wider for all measures. Thus, the IV results confirm my analysis from section 3.5.1. For completeness, appendix Figure C.3 and Figure C.4 present separate graphs of natives' communication and manual task supplies over ranges of communication types.

3.6 Conclusion

In this paper, I have built on the theoretical model and empirical approach developed by Peri and Sparber (2009) in order to estimate how the impact of immigration on natives' supply of communication and manual tasks varies with the immigrants' communication productivity. I have proposed six different variables which I argue are plausible measures to capture the foreign-born's communication type, derived from their language skills, years of residence in the U.S., and countries of origin. Using regressions at the state and year level, I have shown that natives' adjustment of relative task supply is muted as the foreign-born's communication productivity increases. My results are confirmed by instrumental variables estimation, using an instrument based on historical settlement patterns by immigrants from different countries. In particular, years since arrival in the United States appear to be particularly important in determining migrants' communication productivity, more so than language ability-based measures. This finding suggests that communication productivity is a multidimensional skill developed over time, rather than a simple function of English proficiency. It is worth exploring further how migrants' acquisition of communication skills evolves over their stay in the U.S. and how it interacts with occupational sorting by U.S.-born natives.

Table 3.1: Summary statistics of communication and manual tasks supplied by natives and different types of foreign-born.

		C/M ratio		**Communication**		**Manual**	
	N	Mean	SD	Mean	SD	Mean	SD
Panel A: Natives vs. foreign-born							
Natives	18256463	1.66	1.69	0.52	0.27	0.51	0.23
Foreign-born	2461470	1.01	1.27	0.40	0.25	0.58	0.20
Panel B: English ability							
Does not speak English	307647	0.57	0.78	0.28	0.21	0.65	0.17
Speaks English, but not well	686997	0.71	0.90	0.34	0.22	0.62	0.18
Speaks English well	632918	1.01	1.20	0.41	0.25	0.58	0.20
Speaks English very well	545409	1.44	1.55	0.49	0.26	0.53	0.22
Speaks only English	317210	1.57	1.64	0.52	0.26	0.52	0.22
Panel C: Years since arrival							
0-5 years	362647	0.79	1.06	0.35	0.23	0.61	0.19
6-10 years	399545	0.84	1.07	0.37	0.24	0.60	0.19
11-15 years	405404	0.92	1.16	0.38	0.25	0.59	0.19
16-20 years	382305	1.01	1.25	0.40	0.25	0.58	0.20
21+ years	940280	1.22	1.44	0.44	0.26	0.56	0.21
Panel D: Country of origin							
English is official language							
No	2125094	0.94	1.21	0.38	0.25	0.59	0.20
Yes	303795	1.58	1.58	0.54	0.26	0.51	0.21
Top 5 C/M ratios							
UK, ns	7158	2.57	1.98	0.64	0.25	0.39	0.22
Australia	2779	2.42	1.88	0.64	0.25	0.41	0.22
Belgium	1252	2.34	1.91	0.61	0.25	0.41	0.21
Sweden	1502	2.32	1.86	0.62	0.24	0.41	0.21
England	19988	2.31	1.89	0.62	0.25	0.42	0.23
Bottom 5 C/M ratios							
Myanmar	4174	0.90	1.07	0.39	0.23	0.58	0.17
El Salvador	109006	0.84	1.10	0.36	0.24	0.60	0.19
Honduras	36128	0.76	1.07	0.33	0.23	0.61	0.19
Mexico	1065231	0.76	1.03	0.34	0.24	0.63	0.19
Guatemala	66810	0.72	1.00	0.33	0.23	0.62	0.18

Individual-level IPUMS data from Census 1980, 1990, 2000, and ACS 2001-2019. Official language from (Melitz and Toubal, 2012, 2014).

Table 3.2: Summary statistics of state-level task supplies, foreign-born shares, and foreign-born types.

	Mean	SD	10th pct	90th pct
Panel A: Natives' cleaned task supplies				
C/M ratio	0.42	0.05	0.36	0.48
Communication	0.29	0.03	0.26	0.33
Manual	0.69	0.02	0.67	0.72
Ln(C/M ratio)	-0.87	0.12	-1.03	-0.72
Ln(communication)	-1.23	0.09	-1.36	-1.12
Ln(manual)	-0.36	0.03	-0.40	-0.33
Panel B: Foreign-born share and foreign-born types.				
Foreign-born share	0.19	0.14	0.04	0.45
Share who speak English well/v.well/only	0.61	0.10	0.50	0.73
Ordinal English ability	2.00	0.32	1.65	2.34
Years since arrival in the U.S.	17.29	2.93	13.38	20.79
Share who arrived in the U.S. 21+ years ago	0.35	0.10	0.21	0.48
Country-specific communication abilities	0.63	0.09	0.52	0.75
Share from countries with English as official language	0.12	0.09	0.04	0.22

Notes.

Figure 3.1: Change in foreign-born share among workers with a high school degree or less.

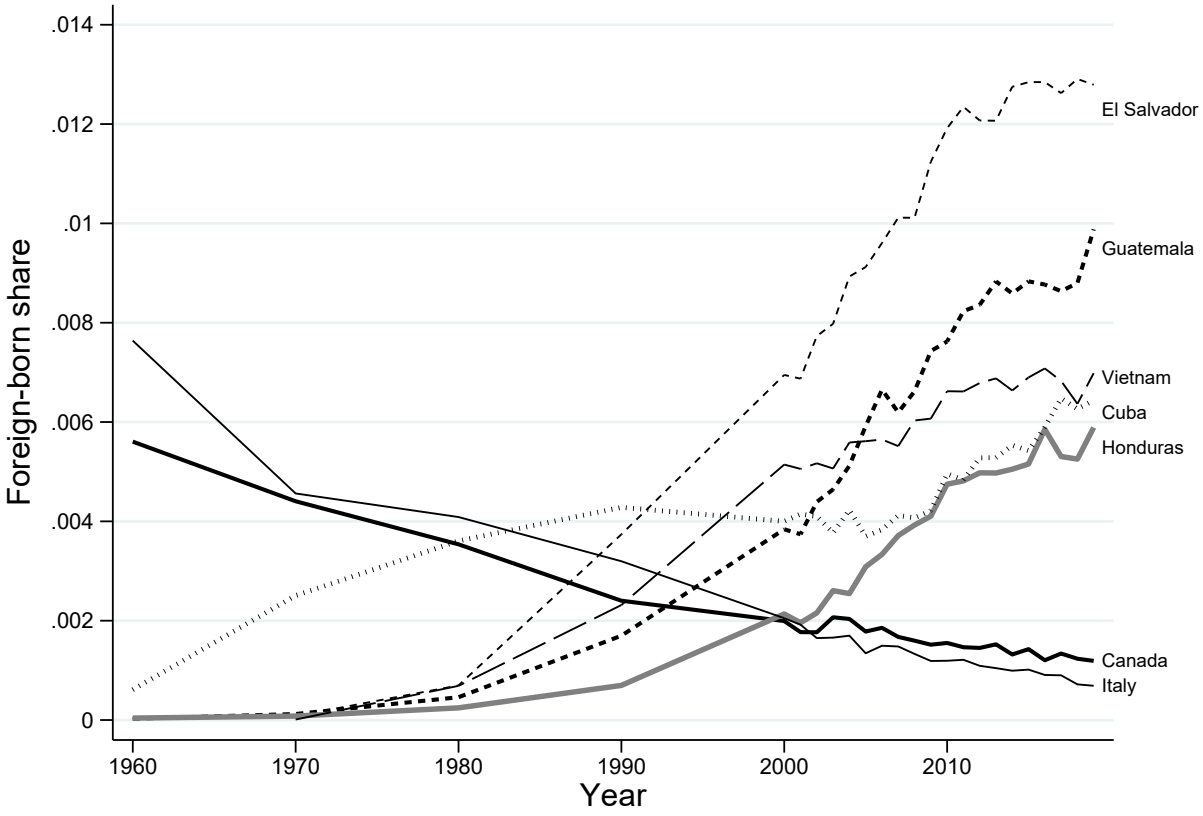


Table 3.3: Natives' communication and manual task supply, foreign-born shares and types. OLS estimates.

	Measure of foreign-born type						
	No type measure	Share who speak English well/ v.well/ only	Average ordinal English ability	Average years since arrival in the U.S.	Share who arrived in the U.S. 21+ years ago	Country-specific comm. abilities	Share from countries with English as official language
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Dependent variable - $ln(C_N/M_N)$							
Foreign-born share	0.21**	0.36	0.40*	0.53***	0.36***	0.94***	0.27***
	(0.10)	(0.23)	(0.23)	(0.09)	(0.10)	(0.33)	(0.07)
Foreign-born type		0.01	-0.00	0.00***	0.05**	-0.13**	-0.05
		(0.03)	(0.01)	(0.00)	(0.02)	(0.06)	(0.05)
Foreign-born share×type		-0.34	-0.12	-0.02***	-0.48***	-1.24*	-0.76
		(0.38)	(0.12)	(0.00)	(0.07)	(0.68)	(0.60)
Joint sig. of share + share×type:							
F		2.39	2.67	51.80	32.91	14.60	10.78
p-value		0.102	0.079	<0.001	<0.001	<0.001	<0.001
N	1122	1122	1122	1122	1122	1122	1122
Panel B: Dependent variable - $ln(C_N)$							
Foreign-born share	0.17**	0.31*	0.32*	0.42***	0.29***	0.74***	0.21***
	(0.07)	(0.17)	(0.17)	(0.06)	(0.07)	(0.24)	(0.06)
Foreign-born type		0.01	-0.00	0.00***	0.03*	-0.10**	-0.04
		(0.02)	(0.01)	(0.00)	(0.02)	(0.04)	(0.04)
Foreign-born share×type		-0.30	-0.10	-0.02***	-0.37***	-0.97*	-0.51
		(0.28)	(0.09)	(0.00)	(0.07)	(0.48)	(0.42)
Joint sig. of share + share×type:							
F		2.79	3.11	47.15	31.09	14.66	10.17
p-value		0.071	0.054	<0.001	<0.001	<0.001	<0.001
N	1122	1122	1122	1122	1122	1122	1122
Panel C: Dependent variable - $ln(M_N)$							
Foreign-born share	-0.04	-0.05	-0.07	-0.11***	-0.07**	-0.20*	-0.05***
	(0.02)	(0.06)	(0.06)	(0.04)	(0.03)	(0.10)	(0.02)
Foreign-born type		-0.00	-0.00	-0.00***	-0.01**	0.03	0.01
		(0.01)	(0.00)	(0.00)	(0.01)	(0.02)	(0.01)
Foreign-born share×type		0.04	0.02	0.00***	0.12***	0.27	0.25
		(0.11)	(0.04)	(0.00)	(0.03)	(0.21)	(0.19)
Joint sig. of share + share×type:							
F		1.13	1.24	12.60	11.10	9.24	7.59
p-value		0.331	0.299	<0.001	<0.001	<0.001	0.001
N	1122	1122	1122	1122	1122	1122	1122

Notes. Notes All regressions estimated with OLS and weighted by total employment of less educated workers in the state and year. Standard errors clustered at the state level. All models include state and year fixed effects. Significance levels: * 10% ** 5% *** 1%.

Figure 3.2: Effect of the foreign-born share on natives' aggregate relative supply of communication to manual tasks across the distribution of foreign-born types. OLS estimates.

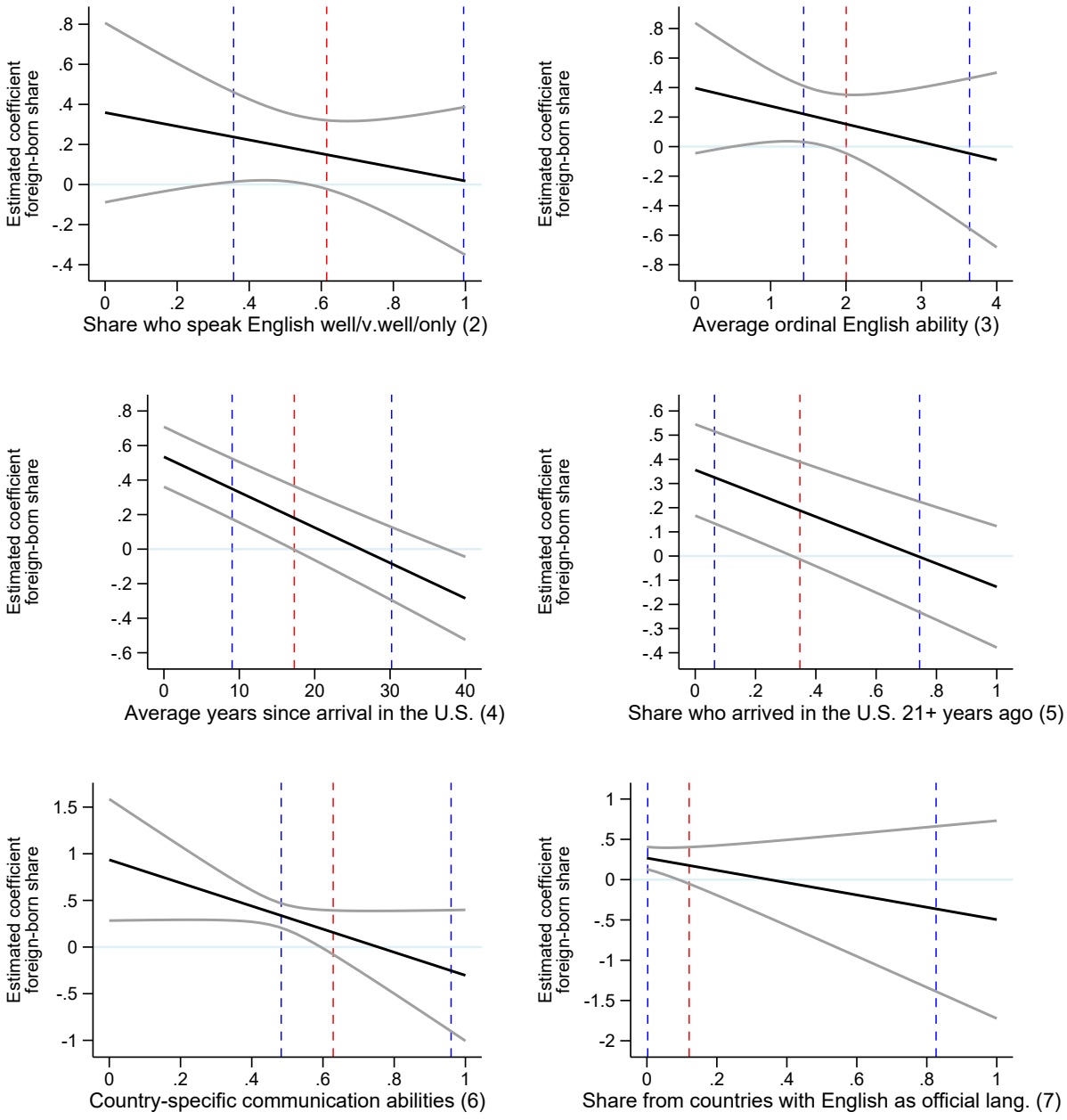
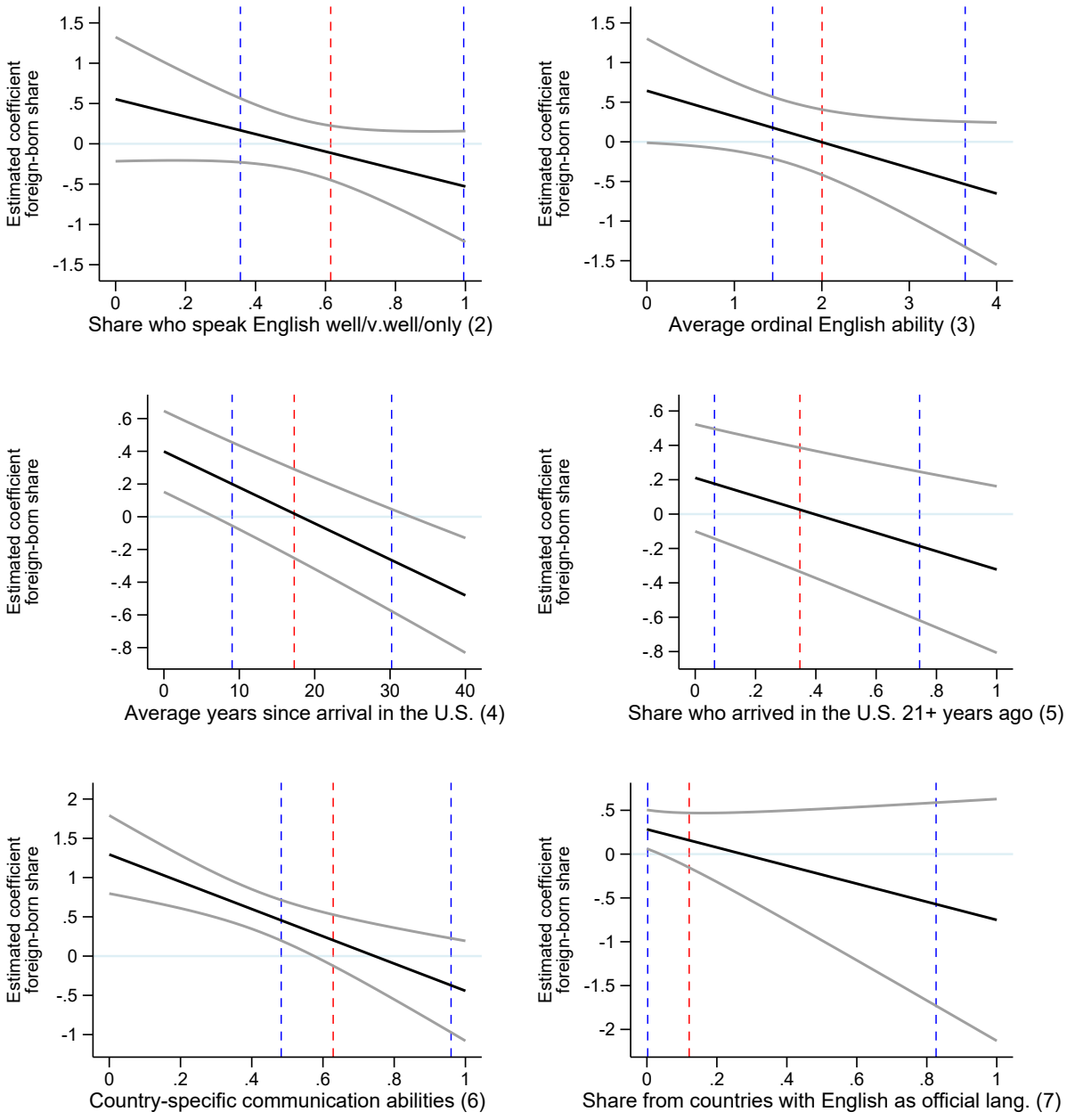


Table 3.4: Natives' communication and manual task supply, foreign-born shares and types. IV estimates.

	Measure of foreign-born type						
	No type measure	Share who speak English well/ v.well/ only	Average ordinal English ability	Average years since arrival in the U.S.	Share who arrived in the U.S. 21+ years ago	Country-specific comm. abilities	Share from countries with English as official language
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Dependent variable - $ln(C_N/M_N)$							
Foreign-born share	0.13	0.55	0.64*	0.40***	0.21	1.29***	0.28**
	(0.14)	(0.39)	(0.33)	(0.13)	(0.16)	(0.25)	(0.11)
Foreign-born type		0.09	-0.01	0.01**	0.12	-0.37**	-0.18
		(0.09)	(0.03)	(0.00)	(0.07)	(0.15)	(0.13)
Foreign-born share×type		-1.08	-0.32*	-0.02***	-0.53***	-1.74***	-1.03
		(0.67)	(0.17)	(0.00)	(0.13)	(0.52)	(0.66)
Joint sig. of share + share×type:							
Chi^2		2.66	4.04	62.63	30.92	30.26	13.22
p-value		0.265	0.133	<0.001	<0.001	<0.001	0.001
N	1122	1122	1122	1122	1122	1122	1122
Panel B: Dependent variable - $ln(C_N)$							
Foreign-born share	0.11	0.41	0.47*	0.29***	0.14	0.97***	0.20**
	(0.10)	(0.30)	(0.26)	(0.09)	(0.12)	(0.17)	(0.09)
Foreign-born type		0.10	0.00	0.01***	0.11*	-0.23*	-0.11
		(0.08)	(0.03)	(0.00)	(0.06)	(0.12)	(0.10)
Foreign-born share×type		-0.85*	-0.25*	-0.02***	-0.42***	-1.35***	-0.77
		(0.51)	(0.13)	(0.00)	(0.10)	(0.34)	(0.48)
Joint sig. of share + share×type:							
Chi^2		2.93	3.82	59.11	33.22	34.13	10.59
p-value		0.231	0.148	<0.001	<0.001	<0.001	0.005
N	1122	1122	1122	1122	1122	1122	1122
Panel C: Dependent variable - $ln(M_N)$							
Foreign-born share	-0.03	-0.14	-0.18**	-0.11***	-0.07*	-0.32***	-0.08***
	(0.03)	(0.10)	(0.08)	(0.04)	(0.04)	(0.10)	(0.02)
Foreign-born type		0.00	0.01	-0.00	-0.01	0.14***	0.08**
		(0.02)	(0.01)	(0.00)	(0.02)	(0.04)	(0.03)
Foreign-born share×type		0.24	0.08	0.00***	0.11***	0.39**	0.26
		(0.19)	(0.05)	(0.00)	(0.04)	(0.20)	(0.19)
Joint sig. of share + share×type:							
Chi^2		2.05	4.86	26.36	14.92	20.97	17.80
p-value		0.358	0.088	<0.001	<0.001	<0.001	<0.001
N	1122	1122	1122	1122	1122	1122	1122

Notes. All regressions estimated with 2SLS and weighted by total employment of less educated workers in the state and year. Standard errors clustered at the state level. All models include state and year fixed effects. Significance levels: * 10% ** 5% *** 1%.

Figure 3.3: Effect of the foreign-born share on natives' aggregate relative supply of communication to manual tasks across the distribution of foreign-born types. IV estimates.



APPENDICES

APPENDIX A

Appendix: Chapter 1

Table A.1: List of current members of the EU and EFTA.

Country	Year joined	Country	Year joined
European Union			
Belgium	1958	Cyprus	2004
France	1958	Czechia	2004
Germany	1958	Estonia	2004
Italy	1958	Hungary	2004
Luxembourg	1958	Latvia	2004
Netherlands	1958	Lithuania	2004
Denmark	1973	Malta	2004
Ireland	1973	Poland	2004
Greece	1981	Slovakia	2004
Portugal	1986	Slovenia	2004
Spain	1986	Bulgaria	2007
Austria	1995	Romania	2007
Finland	1995	Croatia	2013
Sweden	1995		
European Free Trade Association			
Norway	1960	Iceland	1970
Switzerland	1960	Liechtenstein	1991

Notes. Data obtained from https://europa.eu/european-union/about-eu/countries_en#tab-0-1 and https://www.efta.int/About-EFTA/EFTA-through-years-747, accessed November 13th, 2020.

Figure A.1: Google searches for the Polish phrase "kurs walut" ("exchange rates").

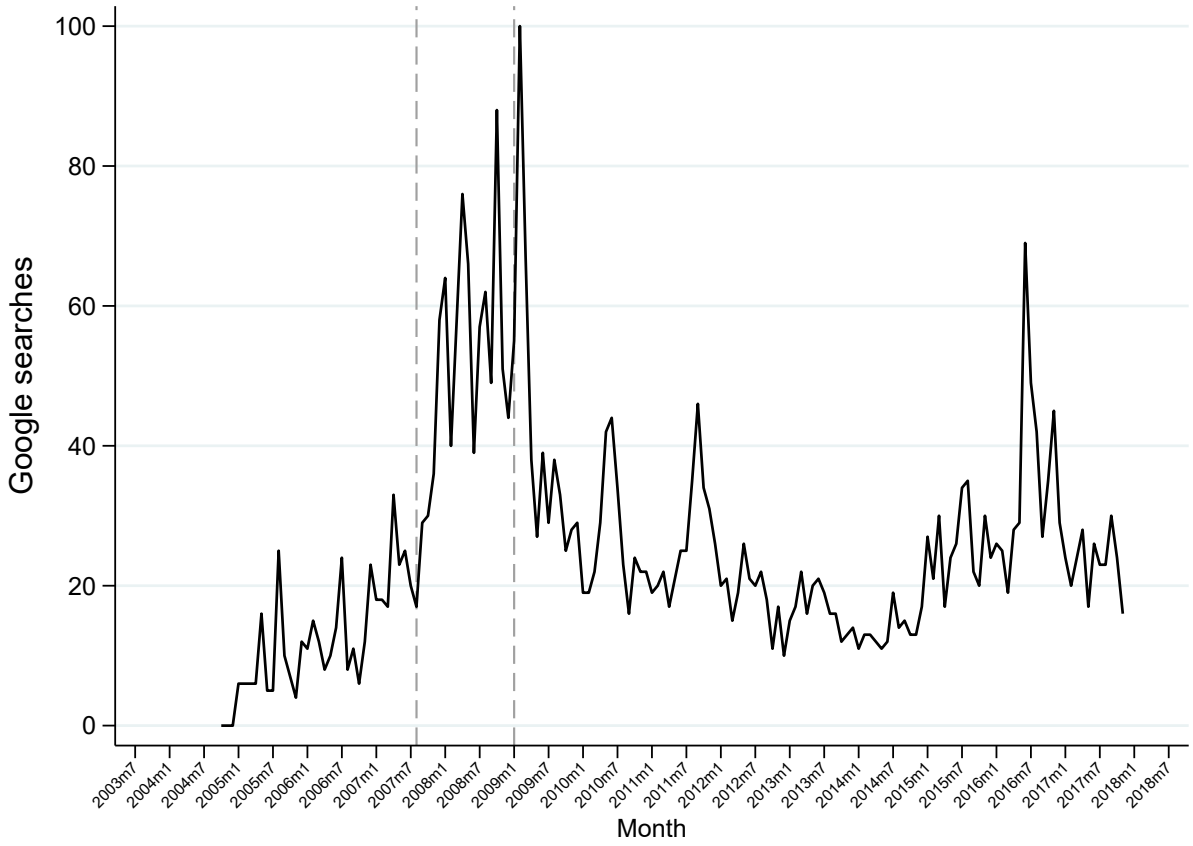


Table A.2: List of main countries of origin of migrants in the LFS.

Country	Total in sample	Percent of sample	Country	Total in sample	Percent of sample
India	5292	10.93%	Romania	533	1.10%
Poland	4308	8.90%	China	481	0.99%
Ireland	3539	7.31%	Slovakia	419	0.87%
Germany	2870	5.93%	Netherlands	415	0.86%
South Africa	2131	4.40%	Mauritius	381	0.79%
Pakistan	1879	3.88%	Turkey	375	0.77%
Kenya	1410	2.91%	Iran	366	0.76%
United States	1300	2.69%	Hungary	364	0.75%
Philippines	1253	2.59%	Tanzania	357	0.74%
Nigeria	1161	2.40%	Latvia	356	0.74%
France	1114	2.30%	Zambia	331	0.68%
Bangladesh	1001	2.07%	Malta	325	0.67%
Sri Lanka	975	2.01%	Brazil	291	0.60%
Jamaica	957	1.98%	Nepal	276	0.57%
Italy	911	1.88%	Greece	261	0.54%
Portugal	800	1.65%	Trinidad and Tobago	230	0.48%
Canada	797	1.65%	Bulgaria	224	0.46%
Ghana	793	1.64%	Japan	224	0.46%
Spain	704	1.45%	Russia	221	0.46%
Lithuania	677	1.40%	Egypt	213	0.44%
Hong Kong	670	1.38%	Sweden	210	0.43%
Cyprus	553	1.14%	Czechia	210	0.43%
Uganda	551	1.14%	Belgium	208	0.43%
Singapore	538	1.11%	Thailand	192	0.40%
Malaysia	536	1.11%	Other	4055	8.38%

A.1 Weighted regressions

In this section, I present versions of my main results with LFS person weights.

Table A.3: Total earnings from employment and real exchange rates, weighted.

	(1)	(2)	(3)	(4)
Ln(real exrate)	-0.070*	-0.086**	-0.080***	-0.087***
	(0.034)	(0.029)	(0.024)	(0.021)
Years since arrival		0.008**	0.007**	0.007**
		(0.003)	(0.003)	(0.003)
(Years since arrival)^2		-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)
Age		0.074***	0.066***	0.068***
		(0.007)	(0.007)	(0.008)
Age^2		-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)
Female		-0.413***	-0.400***	-0.397***
		(0.037)	(0.029)	(0.029)
Married		-0.025	-0.029	-0.032
		(0.018)	(0.017)	(0.019)
Less educated		-0.424***	-0.370***	-0.376***
		(0.048)	(0.048)	(0.047)
Ln(real GDP per capita)				-0.093
				(0.063)
Ln(trade with the UK)				0.014
				(0.020)
Polity 2 score				0.000
				(0.003)
Ln(deaths in conflicts)				0.002
				(0.004)
Ln(deaths in disasters)				-0.001
				(0.003)
Ln(people affected by disasters)				0.001
				(0.002)
Country of birth FE	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes
N	48417	47701	47701	38742
R^2	0.118	0.278	0.350	0.344

Notes. The dependent variable is log gross weekly earnings from one or two (if applicable) jobs. All regressions estimated with OLS, with standard errors clustered at the country of birth, year, and industry of employment level. All models weighted by LFS person weights. Significance levels: * 10% ** 5% *** 1%.

Table A.4: Total earnings from employment and real exchange rates, weighted.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate)	0.076	0.013	-0.030	-0.056	-0.098***	-0.098***	-0.094***	-0.086***
	(0.162)	(0.116)	(0.124)	(0.180)	(0.030)	(0.026)	(0.024)	(0.028)
Years since arrival		0.002	0.002	-0.001		0.009*	0.009**	0.009*
		(0.005)	(0.005)	(0.005)		(0.005)	(0.004)	(0.004)
(Years since arrival)^2		-0.000	-0.000	-0.000		-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		0.085***	0.077***	0.081***		0.066***	0.060***	0.059***
		(0.013)	(0.013)	(0.013)		(0.006)	(0.006)	(0.008)
Age^2		-0.001***	-0.001***	-0.001***		-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.420***	-0.375***	-0.382***		-0.405***	-0.415***	-0.406***
		(0.048)	(0.043)	(0.051)		(0.047)	(0.031)	(0.033)
Married		0.006	-0.000	-0.007		-0.034	-0.034*	-0.030
		(0.022)	(0.022)	(0.029)		(0.020)	(0.017)	(0.021)
Less educated		-0.354***	-0.326***	-0.333***		-0.459***	-0.383***	-0.384***
		(0.065)	(0.060)	(0.063)		(0.039)	(0.045)	(0.054)
Ln(real GDP per capita)				0.072				0.018
				(0.099)				(0.078)
Ln(trade with the UK)				-0.006				0.009
				(0.055)				(0.021)
Polity 2 score				0.039				0.002
				(0.059)				(0.003)
Ln(deaths in conflicts)				0.010				0.002
				(0.018)				(0.005)
Ln(deaths in disasters)				-0.001				-0.002
				(0.008)				(0.006)
Ln(people affected by disasters)				0.002				0.001
				(0.003)				(0.002)
Ln(real exrate) difference					0.175	0.110	0.064	0.029
					(0.175)	(0.128)	(0.133)	(0.203)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
N (sub-sample)	19103	18851	18851	15123	29314	28850	28850	23619
N	48417	47701	47701	38742	48417	47701	47701	38742
R^2	0.124	0.287	0.363	0.358	0.124	0.287	0.363	0.358

Notes. The dependent variable is log gross weekly earnings from one or two (if applicable) jobs. All regressions estimated with OLS, with standard errors clustered at the country of birth, year, and industry of employment level. All models weighted with LFS person weights. Significance levels: * 10% ** 5% *** 1%.

Table A.5: Channels of earnings adjustment and real exchange rates, weighted.

	EU/EFTA countries		**Non-EU/EFTA countries**		**Difference**	
	(3)	(4)	(3)	(4)	(3)	(4)
Ln(actual hours in both jobs, incl. OT)	0.040	0.074	-0.022*	-0.031	0.062	0.106
	(0.105)	(0.111)	(0.012)	(0.019)	(0.109)	(0.126)
Ln(actual hours in main job, incl. OT)	0.047	0.072	-0.024**	-0.038**	0.071	0.110
	(0.114)	(0.117)	(0.011)	(0.017)	(0.118)	(0.125)
Ln(actual hours in main job, excl. OT)	0.074	0.101	-0.022**	-0.034*	0.096	0.135
	(0.105)	(0.113)	(0.010)	(0.019)	(0.109)	(0.122)
Worked OT	-0.067	-0.093	0.004	0.004	-0.071	-0.097
	(0.060)	(0.062)	(0.014)	(0.015)	(0.062)	(0.065)
Has a second job	-0.000	0.015	0.008	0.013	-0.009	0.001
	(0.017)	(0.024)	(0.005)	(0.009)	(0.021)	(0.031)
Is looking for a new job	-0.025***	-0.022	-0.011***	-0.022	-0.013	-0.000
	(0.003)	(0.020)	(0.003)	(0.013)	(0.027)	(0.031)
Is looking for an additional job	-0.003	0.025	-0.019***	-0.032**	0.016	0.057
	(0.007)	(0.037)	(0.004)	(0.013)	(0.026)	(0.044)
Would like to work more	0.009	-0.002	-0.007	-0.027	0.016**	0.025
	(0.010)	(0.036)	(0.012)	(0.016)	(0.007)	(0.050)

Notes. The dependent variable is indicated in the first column. *Would like to work more* is a union of two questions: the migrant is looking for an additional job, the migrant would like to work more hours at the current wage rate. Only regression models (3) and (4) from table 1.3 are presented. The last two columns show the difference in coefficients between EU and non-EU migrants. All regressions estimated with OLS, with standard errors clustered at the country of birth, year, and industry of employment level. All models weighted with LFS person weights. Significance levels: * 10% ** 5% *** 1%.

Table A.6: Attrition from the survey and real exchange rates, weighted.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate over next 12 mos.)	0.309	0.382**	0.384**	0.421**	-0.009	-0.001	-0.001	-0.002
	(0.205)	(0.138)	(0.134)	(0.150)	(0.022)	(0.015)	(0.015)	(0.015)
Years since arrival		-0.013***	-0.013***	-0.013***		-0.005***	-0.005***	-0.005***
		(0.002)	(0.002)	(0.002)		(0.001)	(0.001)	(0.001)
(Years since arrival)^2		0.000***	0.000***	0.000***		0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		-0.028***	-0.027***	-0.026***		-0.020***	-0.020***	-0.021***
		(0.004)	(0.004)	(0.004)		(0.002)	(0.002)	(0.002)
Age^2		0.000***	0.000***	0.000***		0.000***	0.000***	0.000***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.033***	-0.028**	-0.037***		-0.026***	-0.022**	-0.028**
		(0.009)	(0.011)	(0.012)		(0.008)	(0.009)	(0.012)
Married		-0.051***	-0.050***	-0.053**		-0.048***	-0.046***	-0.052***
		(0.014)	(0.014)	(0.020)		(0.011)	(0.013)	(0.015)
Less educated		0.024**	0.020	0.013		0.019**	0.016*	0.014
		(0.011)	(0.012)	(0.014)		(0.009)	(0.008)	(0.012)
Ln(real GDP per capita)				-0.055				0.031
				(0.132)				(0.067)
Ln(trade with the UK)				-0.045				-0.001
				(0.060)				(0.024)
Polity 2 score				0.051***				0.001
				(0.015)				(0.002)
Ln(deaths in conflicts)				0.002				0.003
				(0.009)				(0.003)
Ln(deaths in disasters)				-0.005				-0.003
				(0.006)				(0.004)
Ln(people affected by disasters)				0.006**				-0.001
				(0.003)				(0.001)
Ln(exrate) difference					0.318	0.383**	0.385**	0.423**
					(0.207)	(0.140)	(0.137)	(0.152)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
Attrition in sub-sample	0.456	0.455	0.455	0.451	0.447	0.446	0.446	0.439
N (sub-sample)	17492	17358	17358	15114	27461	27135	27135	23590
N	44953	44493	44493	38704	44953	44493	44493	38704
R^2	0.054	0.095	0.097	0.101	0.054	0.095	0.097	0.101

Notes. The dependent variable is individual-level attrition from the LFS. It is equal to 0 if the migrant is observed in both waves 1 and 5 of the LFS and 1 if they are observed only in wave 1. All regressions estimated with OLS, with standard errors clustered at the country of birth, year, and industry of employment level. All models weighted by LFS person weights. Significance levels: * 10% ** 5% *** 1%.

A.2 Alternative classifiction of Bulgaria, Romania, and Croatia

In this section, I show versions of my main results where I classifiy Bulgaria and Romania as EU countries in 2014 instead of 2007, and Croatia as a non-EU country.

Table A.7: Total earnings from employment and real exchange rates. Alternative EU classification of Bulgaria, Croatia, and Romania.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate)	0.103	0.034	-0.012	-0.041	-0.099**	-0.095***	-0.091***	-0.083***
	(0.173)	(0.132)	(0.133)	(0.182)	(0.036)	(0.031)	(0.028)	(0.027)
Years since arrival		0.001	0.001	-0.001		0.009**	0.009**	0.010**
		(0.005)	(0.004)	(0.005)		(0.004)	(0.004)	(0.004)
(Years since arrival)^2		-0.000	-0.000	0.000		-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		0.089***	0.082***	0.087***		0.071***	0.064***	0.064***
		(0.014)	(0.013)	(0.014)		(0.007)	(0.007)	(0.008)
Age^2		-0.001***	-0.001***	-0.001***		-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.445***	-0.399***	-0.408***		-0.422***	-0.433***	-0.429***
		(0.051)	(0.047)	(0.053)		(0.045)	(0.029)	(0.029)
Married		-0.007	-0.013	-0.021		-0.042**	-0.039**	-0.040**
		(0.023)	(0.023)	(0.027)		(0.016)	(0.014)	(0.017)
Less educated		-0.361***	-0.337***	-0.343***		-0.457***	-0.388***	-0.384***
		(0.061)	(0.058)	(0.061)		(0.038)	(0.041)	(0.054)
Ln(real GDP per capita)				0.035				0.017
				(0.112)				(0.075)
Ln(trade with the UK)				-0.007				0.014
				(0.058)				(0.024)
Polity 2 score				0.035				0.002
				(0.054)				(0.004)
Ln(deaths in conflicts)				0.016				0.001
				(0.019)				(0.005)
Ln(deaths in disasters)				-0.000				-0.002
				(0.006)				(0.006)
Ln(people affected by disasters)				-0.000				0.001
				(0.002)				(0.002)
Ln(real exrate) difference					0.202	0.130	0.079	0.042
					(0.188)	(0.145)	(0.145)	(0.207)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
N (sub-sample)	18864	18614	18614	14893	29553	29087	29087	23849
N	48417	47701	47701	38742	48417	47701	47701	38742
R^2	0.116	0.286	0.356	0.353	0.116	0.286	0.356	0.353

Notes. The dependent variable is log gross weekly earnings from one or two (if applicable) jobs. All regressions estimated with OLS, with standard errors clustered at the country of birth, year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table A.8: Channels of earnings adjustment and real exchange rates. Alternative EU classification of Bulgaria, Croatia, and Romania.

	EU/EFTA countries		**Non-EU/EFTA countries**		**Difference**	
	(3)	(4)	(3)	(4)	(3)	(4)
Ln(actual hours in both jobs, incl. OT)	0.089	0.106	-0.018*	-0.028	0.107	0.134
	(0.099)	(0.109)	(0.010)	(0.017)	(0.104)	(0.124)
Ln(actual hours in main job, incl. OT)	0.098	0.110	-0.020*	-0.035*	0.119	0.145
	(0.109)	(0.113)	(0.010)	(0.017)	(0.113)	(0.122)
Ln(actual hours in main job, excl. OT)	0.115	0.126	-0.018*	-0.031*	0.133	0.157
	(0.101)	(0.109)	(0.009)	(0.017)	(0.105)	(0.118)
Worked OT	-0.048	-0.108	0.006	0.003	-0.054	-0.111
	(0.076)	(0.073)	(0.015)	(0.016)	(0.076)	(0.074)
Has a second job	0.008	0.009	0.009*	0.013*	-0.001	-0.005
	(0.022)	(0.027)	(0.005)	(0.008)	(0.027)	(0.034)
Is looking for a new job	-0.023***	-0.037	-0.012	-0.026*	-0.011	-0.011
	(0.005)	(0.023)	(0.007)	(0.013)	(0.032)	(0.033)
Is looking for an additional job	-0.006	0.008	-0.018**	-0.032**	0.012	0.040
	(0.016)	(0.034)	(0.008)	(0.014)	(0.032)	(0.037)
Would like to work more	-0.002	-0.004	-0.007	-0.029	0.006	0.025
	(0.010)	(0.036)	(0.014)	(0.018)	(0.019)	(0.053)

Notes. The dependent variable is indicated in the first column. *Would like to work more* is a union of two questions: the migrant is looking for an additional job, the migrant would like to work more hours at the current wage rate. Only regression models (3) and (4) from table 1.3 are presented. The last two columns show the difference in coefficients between EU and non-EU migrants. All regressions estimated with OLS, with standard errors clustered at the country of birth, year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

Table A.9: Attrition from the survey and real exchange rates. Alternative EU classification of Bulgaria, Croatia, and Romania.

	EU/EFTA countries				**Non-EU/EFTA countries**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ln(real exrate over next 12 mos.)	0.403**	0.462***	0.464***	0.495***	-0.012	-0.006	-0.006	-0.008
	(0.181)	(0.135)	(0.133)	(0.158)	(0.020)	(0.016)	(0.015)	(0.015)
Years since arrival		-0.013***	-0.012***	-0.013***		-0.005***	-0.005***	-0.005***
		(0.002)	(0.002)	(0.002)		(0.001)	(0.001)	(0.001)
(Years since arrival)^2		0.000***	0.000***	0.000***		0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age		-0.023***	-0.022***	-0.022***		-0.022***	-0.021***	-0.022***
		(0.004)	(0.003)	(0.004)		(0.002)	(0.002)	(0.002)
Age^2		0.000***	0.000***	0.000***		0.000***	0.000***	0.000***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Female		-0.035***	-0.029**	-0.036**		-0.026***	-0.022**	-0.027***
		(0.010)	(0.011)	(0.012)		(0.007)	(0.008)	(0.009)
Married		-0.047***	-0.046***	-0.047**		-0.048***	-0.046***	-0.050***
		(0.014)	(0.014)	(0.020)		(0.011)	(0.012)	(0.015)
Less educated		0.024**	0.018	0.012		0.024***	0.020**	0.018*
		(0.010)	(0.012)	(0.014)		(0.008)	(0.007)	(0.009)
Ln(real GDP per capita)				-0.063				0.028
				(0.132)				(0.067)
Ln(trade with the UK)				-0.047				-0.001
				(0.061)				(0.027)
Polity 2 score				0.043**				0.002
				(0.015)				(0.002)
Ln(deaths in conflicts)				-0.002				0.003
				(0.009)				(0.003)
Ln(deaths in disasters)				-0.004				-0.004
				(0.005)				(0.003)
Ln(people affected by disasters)				0.005*				-0.000
				(0.003)				(0.001)
Ln(exrate) difference					0.414**	0.467***	0.470***	0.503***
					(0.184)	(0.139)	(0.136)	(0.160)
Country of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry of work FE	No	No	Yes	Yes	No	No	Yes	Yes
Attrition in sub-sample	0.454	0.453	0.453	0.449	0.448	0.447	0.447	0.441
N (sub-sample)	17255	17123	17123	14884	27698	27370	27370	23820
N	44953	44493	44493	38704	44953	44493	44493	38704
R^2	0.056	0.094	0.096	0.099	0.056	0.094	0.096	0.099

Notes. The dependent variable is individual-level attrition from the LFS. It is equal to 0 if the migrant is observed in both waves 1 and 5 of the LFS and 1 if they are observed only in wave 1. All regressions estimated with OLS, with standard errors clustered at the country of birth, year, and industry of employment level. Significance levels: * 10% ** 5% *** 1%.

APPENDIX B

Appendix: Chapter 2

B.1 Robustness: Subsetting Based on Legal Status

Table B.1: Annual new hires: subsetting based on legal status.

	(1)	(2)	(3)	(4)	(5)
A. MoAs only (12-month event window)					
Treated × post-treatment	-0.078	-0.060	-0.031	-0.053	-0.024
	(0.153)	(0.186)	(0.142)	(0.145)	(0.271)
N	1461	1461	1461	1461	1461
B. First treaty only					
Treated × post-treatment	0.111	0.192	0.161	0.098	0.179
	(0.228)	(0.228)	(0.218)	(0.233)	(0.227)
N	5215	5215	5215	5215	5215
C. After RA-8042 only (6/22/1995)					
Treated × post-treatment	0.015	0.031	0.148	-0.009	0.013
	(0.184)	(0.172)	(0.185)	(0.183)	(0.171)
N	5215	5215	5215	5215	5215
D. Before RA-10022 only (3/25/2010)					
Treated × post-treatment	-0.078	-0.066	0.019	-0.101	-0.113
	(0.127)	(0.194)	(0.137)	(0.140)	(0.186)
N	3388	3388	3388	3388	3388
E. After RA-10022 only (3/25/2010)					
Treated × post-treatment	0.294	0.315	0.565	0.285	0.355
	(0.336)	(0.426)	(0.451)	(0.343)	(0.430)
N	1876	1876	1876	1876	1876
Year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, and year levels, are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

Table B.2: Monthly remittances: differences in legal status.

	(1)	(2)	(3)	(4)	(5)
A. MoAs only (12-month event window)					
Treated × post-treatment	-0.301	-0.370*	-0.280	-0.304	-0.315
	(0.162)	(0.176)	(0.175)	(0.161)	(0.171)
N	12925	12925	12925	12925	12925
B. First treaty only					
Treated × post-treatment	0.059	0.071	0.168	0.010	0.027
	(0.193)	(0.206)	(0.191)	(0.207)	(0.210)
N	35843	35843	35843	35843	35843
C. After RA-8042 only (6/22/1995)					
Treated × post-treatment	0.038	-0.001	0.101	-0.004	-0.015
	(0.165)	(0.171)	(0.159)	(0.176)	(0.174)
N	55115	55115	55115	55115	55115
D. Before RA-10022 only (3/25/2010)					
Treated × post-treatment	0.244	0.185	0.270	0.155	0.092
	(0.228)	(0.251)	(0.237)	(0.269)	(0.296)
N	23871	23871	23871	23871	23871
E. After RA-10022 only (3/25/2010)					
Treated × post-treatment	-0.194	-0.188	-0.125	-0.224	-0.185
	(0.186)	(0.183)	(0.221)	(0.181)	(0.189)
N	31463	31463	31463	31463	31463
Month × year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, year, and month levels, are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

B.2 Robustness: Subsetting Based on Geography

Table B.**3:** Annual new hires: subsetting based on geography.

	(1)	(2)	(3)	(4)	(5)
A. OFCs excluded					
Treated × post-treatment	-0.009	0.002	0.091	-0.029	-0.015
	(0.206)	(0.198)	(0.206)	(0.204)	(0.203)
N	4102	4102	4102	4102	4102
B. Asia only					
Treated × post-treatment	-0.164	-0.113	-0.097	-0.206	-0.115
	(0.137)	(0.166)	(0.146)	(0.189)	(0.185)
N	1295	1295	1295	1295	1295
C. MENA excluded					
Treated × post-treatment	0.029	0.099	0.095	0.055	0.108
	(0.347)	(0.320)	(0.324)	(0.336)	(0.321)
N	2226	2226	2226	2226	2226
D. MENA only					
Treated × post-treatment	0.042	0.068	0.091	-0.051	0.064
	(0.164)	(0.123)	(0.178)	(0.126)	(0.137)
N	455	455	455	455	455
E. COW, HK, CNMI only					
Treated × post-treatment	0.015	0.031	0.148	-0.009	0.013
	(0.184)	(0.172)	(0.185)	(0.183)	(0.171)
N	5215	5215	5215	5215	5215
Year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, and year levels, are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

Table B.4: Monthly remittances: subsetting based on geography.

	(1)	(2)	(3)	(4)	(5)
A. OFCs excluded					
Treated × post-treatment	-0.057	-0.093	-0.002	-0.116	-0.116
	(0.143)	(0.163)	(0.137)	(0.148)	(0.164)
N	38617	38617	38617	38617	38617
B. Asia only					
Treated × post-treatment	0.098	0.136	0.125	0.111	0.207
	(0.212)	(0.227)	(0.217)	(0.222)	(0.224)
N	13505	13505	13505	13505	13505
C. MENA excluded					
Treated × post-treatment	-0.125	-0.062	-0.057	-0.131	-0.078
	(0.209)	(0.221)	(0.236)	(0.216)	(0.236)
N	28178	28178	28178	28178	28178
D. MENA only					
Treated × post-treatment	0.303	0.265	0.296	0.293	0.230
	(0.242)	(0.243)	(0.240)	(0.265)	(0.244)
N	3796	3796	3796	3796	3796
E. COW, HK, CNMI only					
Treated × post-treatment	0.038	-0.001	0.101	-0.004	-0.015
	(0.165)	(0.171)	(0.159)	(0.176)	(0.174)
N	55115	55115	55115	55115	55115
Month × year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, year, and month levels, are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

B.3 Robustness: Subsetting Based on Type of Country

Table B.5: Annual new hires: subsetting based on type of country.

	(1)	(2)	(3)	(4)	(5)
A. Zero deployment countries excluded					
Treated × post-treatment	0.015	0.031	0.148	-0.009	0.013
	(0.184)	(0.172)	(0.185)	(0.183)	(0.171)
N	5215	5215	5215	5215	5215
B. Low Polity 2 (<7) only					
Treated × post-treatment	-0.137	-0.120	-0.053	-0.150*	-0.122
	(0.080)	(0.105)	(0.097)	(0.080)	(0.116)
N	1589	1589	1589	1589	1589
C. High Polity 2 (>=7) only					
Treated × post-treatment	-0.042	0.158	0.168	-0.066	0.131
	(0.333)	(0.287)	(0.324)	(0.300)	(0.290)
N	1206	1206	1206	1206	1206
Year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, and year levels, are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

Table B.**6:** Monthly remittances: subsetting based on type of country.

	(1)	(2)	(3)	(4)	(5)
A. Zero deployment countries excluded					
Treated × post-treatment	0.038	-0.001	0.101	-0.004	-0.015
	(0.165)	(0.171)	(0.159)	(0.176)	(0.174)
N	55115	55115	55115	55115	55115
B. Low Polity 2 (<7) only					
Treated × post-treatment	-0.006	-0.053	-0.031	-0.009	-0.024
	(0.308)	(0.320)	(0.304)	(0.327)	(0.364)
N	11282	11282	11282	11282	11282
C. High Polity 2 (>=7) only					
Treated × post-treatment	0.105	0.227	0.174	0.119	0.231
	(0.211)	(0.184)	(0.194)	(0.214)	(0.219)
N	18036	18036	18036	18036	18036
Month × year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, year, and month levels, are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

B.4 Robustness: Alternative Treatment of the United States

Table B.7: Annual new hires: alternative treatment of the United States.

	(1)	(2)	(3)	(4)	(5)
A. U.S. excluded (event study)					
Treated × post-treatment	0.006	0.027	0.128	-0.017	0.008
	(0.185)	(0.173)	(0.184)	(0.185)	(0.173)
N	5117	5117	5117	5117	5117
Year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes
B. U.S. recoded (OLS panel)					
Signed BLA	0.277	0.330	0.435	0.277	0.341
	(0.389)	(0.371)	(0.407)	(0.403)	(0.398)
N	2694	2694	2694	2694	2694
Year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. For the event study, standard errors were clustered at the event, host country, and year levels; for the OLS panel standard errors were clustered at the host country, and year levels. Standard errors are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

Table B.8: Monthly remittances: alternative treatment of the United States.

	(1)	(2)	(3)	(4)	(5)
A. U.S. excluded (event study)					
Treated × post-treatment	0.026	-0.008	0.086	-0.013	-0.020
	(0.165)	(0.171)	(0.159)	(0.176)	(0.174)
N	53728	53728	53728	53728	53728
Month × year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes
B. U.S. recoded (OLS panel)					
Signed BLA	-0.309	-0.175	0.015	-0.329	-0.172
	(0.548)	(0.544)	(0.531)	(0.569)	(0.551)
N	36972	36972	36972	36972	36972
Month × year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	

All models estimated with OLS. For the event study, standard errors were clustered at the event, host country, year, and month levels; for the OLS panel standard errors were clustered at the host country, year, and month levels. Standard errors are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

B.5 Robustness: Alternative Event Window

Table B.9: Annual new hires: alternative event windows.

	(1)	(2)	(3)	(4)	(5)
A. *Event Window – 1 year*					
Treated × post-treatment	-0.020	0.019	0.026	-0.034	0.006
	(0.080)	(0.090)	(0.082)	(0.078)	(0.085)
N	5976	5976	5976	5976	5976
B. *Event Window – 2 years*					
Treated × post-treatment	0.074	0.093	0.176	0.060	0.090
	(0.188)	(0.190)	(0.202)	(0.190)	(0.193)
N	5560	5560	5560	5560	5560
C. *Event Window – 4 years*					
Treated × post-treatment	-0.193	-0.236	0.052	-0.239	-0.256
	(0.259)	(0.304)	(0.318)	(0.264)	(0.310)
N	3609	3609	3609	3609	3609
D. *Event Window – 4 years*					
Treated × post-treatment	-0.226	-0.312	0.272	-0.269	-0.226
	(0.356)	(0.462)	(0.496)	(0.356)	(0.489)
N	2981	2981	2981	2981	2981
Year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, and year levels, are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

Table B.10: Monthly remittances: alternative event windows.

	(1)	(2)	(3)	(4)	(5)
A. *Event Window – 12 months*					
Treated × post-treatment	-0.058	-0.075	-0.049	-0.063	-0.069
	(0.078)	(0.080)	(0.078)	(0.080)	(0.079)
N	50575	50575	50575	50575	50575
B. *Event Window – 24 months*					
Treated × post-treatment	-0.001	-0.054	0.033	-0.022	-0.056
	(0.113)	(0.116)	(0.110)	(0.117)	(0.115)
N	47775	47775	47775	47775	47775
C. *Event Window – 48 months*					
Treated × post-treatment	0.044	-0.050	0.130	-0.037	-0.071
	(0.207)	(0.227)	(0.189)	(0.210)	(0.230)
N	39673	39673	39673	39673	39673
D. *Event Window – 60 months*					
Treated × post-treatment	0.173	0.109	0.498	0.025	0.073
	(0.221)	(0.295)	(0.313)	(0.218)	(0.295)
N	35695	35695	35695	35695	35695
Month × year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, year, and month levels, are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

B.6 Robustness: Annual Remittance Data

Table B.11: Remmitances: annual data.

	(1)	(2)	(3)	(4)	(5)
A. *Event Window – 1 year*					
Treated × post-treatment	-0.116	-0.121	-0.089	-0.093	-0.099
	(0.085)	(0.077)	(0.076)	(0.083)	(0.087)
N	7908	7908	7908	7908	7908
B. *Event Window – 2 years*					
Treated × post-treatment	-0.123	-0.191	-0.088	-0.129	-0.201
	(0.125)	(0.130)	(0.123)	(0.119)	(0.123)
N	7685	7685	7685	7685	7685
C. *Event Window – 3 years*					
Treated × post-treatment	-0.033	-0.073	0.089	-0.012	-0.049
	(0.142)	(0.156)	(0.117)	(0.135)	(0.139)
N	8099	8099	8099	8099	8099
D. *Event Window – 4 years*					
Treated × post-treatment	-0.061	-0.119	0.164*	-0.046	-0.072
	(0.143)	(0.184)	(0.085)	(0.151)	(0.183)
N	6453	6453	6453	6453	6453
E. *Event Window – 5 years*					
Treated × post-treatment	-0.047	-0.105	0.484	-0.046	0.063
	(0.141)	(0.240)	(0.322)	(0.153)	(0.265)
N	6083	6083	6083	6083	6083
Year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, and year levels, are in parentheses. Sample size is reported in the third row of each alternative specification. Significance levels: * 10% ** 5% *** 1%.

B.7 Robustness: Relaxing the Constant Sample Restriction

Table B.12: Annual new hires: relaxing the constant sample restriction.

	(1)	(2)	(3)	(4)	(5)
Treated × post-treatment	-0.025	-0.026	0.074	0.071	0.056
	(0.175)	(0.165)	(0.178)	(0.174)	(0.177)
N	12334	9438	9438	9177	8284
Year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, year and month level, are in parentheses. Significance levels: * 10% ** 5% *** 1%.

Table B.13: Monthly remittances: relaxing the constant sample restriction.

	(1)	(2)	(3)	(4)	(5)
Treated × post-treatment	-0.026	-0.100	-0.030	0.006	-0.072
	(0.139)	(0.161)	(0.172)	(0.154)	(0.162)
N	84461	68725	68725	73824	67930
Month × year FE	Yes	Yes	Yes	Yes	Yes
Host country FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes

All models estimated with OLS. Standard errors, clustered at the event, host country, year and month level, are in parentheses. Significance levels: * 10% ** 5% *** 1%.

B.8 Robustness: Panel Regressions

Table B.14: Log new hires - all sectors. Panel data model - OLS Regression.

	(1)	(2)	(3)	(4)	(5)
Signed BLA in the past	0.277	0.330	0.435	0.277	0.341
	(0.389)	(0.371)	(0.407)	(0.403)	(0.398)
Ln(GDP per capita of host)		2.889***			1.750*
		(0.842)			(0.904)
Ln(population of host)		3.817*			5.256*
		(1.973)			(2.553)
Polity2 of host		-0.001			-0.016
		(0.027)			(0.024)
GDP per capita ratio (host to PHL)			0.387**		0.284
			(0.168)		(0.185)
Higher Polity Score in PHL			-0.373		-0.430*
			(0.261)		(0.236)
Ln(total trade)				0.049	0.029
				(0.041)	(0.042)
BIT in the past				-0.130	-0.233
				(0.371)	(0.395)
Ideological difference				-0.254	-0.196
				(0.271)	(0.265)
Host country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Country-specific time trends	Yes	Yes	Yes	Yes	Yes
N	2694	2694	2694	2694	2694
R^2	0.889	0.893	0.892	0.889	0.894

Note: All models estimated with OLS. Standard errors, clustered at the host country and year level, are in parentheses. Significance levels: * 10% ** 5% *** 1%.

Table B.15: Total new hires: all workers. Panel data model - PPML Regression.

	(1)	(2)	(3)	(4)	(5)
Signed BLA in the past	-0.018	0.104	-0.041	-0.023	0.088
	(0.242)	(0.321)	(0.266)	(0.229)	(0.353)
Ln(GDP per capita of host)		2.596*			3.376***
		(1.351)			(1.236)
Ln(population of host)		5.254***			4.612***
		(1.977)			(1.404)
Polity2 of host		0.004			0.006
		(0.082)			(0.082)
GDP per capita ratio (host to PHL)			-0.018		-0.105
			(0.097)		(0.098)
Higher Polity Score in PHL			-0.364		0.349
			(0.579)		(0.779)
Ln(total trade)				0.021	-0.049
				(0.107)	(0.131)
BIT in the past				-0.602	-0.302
				(0.489)	(0.424)
Ideological difference				-0.693*	-0.565**
				(0.377)	(0.279)
Host country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Country-specific time trends	Yes	Yes	Yes	Yes	Yes
N	2677	2677	2677	2677	2677

Note: All models estimated with PPML. Standard errors, clustered at the host country and year levels, are in parentheses. Significance levels: * 10% ** 5% *** 1%.

Table B.16: Log monthly remittances. Panel data model - OLS regression.

	(1)	(2)	(3)	(4)	(5)
Signed BLA in the past	-0.309	-0.175	0.015	-0.329	-0.172
	(0.548)	(0.544)	(0.531)	(0.569)	(0.551)
Ln(GDP per capita of host)		3.411***			-0.333
		(1.248)			(1.535)
Ln(population of host)		2.185			6.675
		(3.238)			(4.712)
Polity2 of host		-0.041			-0.054
		(0.060)			(0.062)
GDP per capita ratio (host to PHL)			0.813***		0.955***
			(0.236)		(0.316)
Higher Polity Score in PHL			-0.410		-0.591
			(0.723)		(0.773)
Ln(total trade)				0.135	0.100
				(0.089)	(0.091)
BIT in the past				1.775**	1.568**
				(0.754)	(0.694)
Ideological difference				0.467	0.624
				(0.399)	(0.388)
Host country FE	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes
Country-specific time trends	Yes	Yes	Yes	Yes	Yes
N	36972	36972	36972	36972	36972
R^2	0.837	0.838	0.840	0.838	0.841

Note: All models estimated with OLS. Standard errors, clustered at the host country and year × month level, are in parentheses. Significance levels: * 10% ** 5% *** 1%.

Table B.**17:** Monthly remittances in levels. Panel data model - PPML Regression.

	(1)	(2)	(3)	(4)	(5)
Signed BLA in the past	0.218	0.191	0.199	0.142	0.114
	(0.310)	(0.313)	(0.327)	(0.323)	(0.312)
Ln(GDP per capita of host)		0.337			4.417*
		(0.794)			(2.321)
Ln(population of host)		0.101			-0.518
		(0.728)			(0.802)
Polity2 of host		-0.169**			-0.161**
		(0.078)			(0.073)
GDP per capita ratio (host to PHL)			-0.028		-0.339**
			(0.042)		(0.154)
Higher Polity Score in PHL			-1.566***		-2.417***
			(0.598)		(0.702)
Ln(total trade)				-0.087	-0.140
				(0.121)	(0.144)
BIT in the past				-0.488	-0.666
				(0.510)	(0.503)
Ideological difference				0.407***	0.365***
				(0.154)	(0.131)
Host country FE	Yes	Yes	Yes	Yes	Yes
Month × year FE	Yes	Yes	Yes	Yes	Yes
Country-specific time trends	Yes	Yes	Yes	Yes	Yes
N	36912	36912	36912	36912	36912

Note: All models estimated with PPML. Standard errors, clustered at the host country and year × month level, are in parentheses. Significance levels: * 10% ** 5% *** 1%.

APPENDIX C

Appendix: Chapter 3

Figure C.1: Effect of the foreign-born share on natives' aggregate supply of communication tasks across the distribution of foreign-born types. OLS estimates.

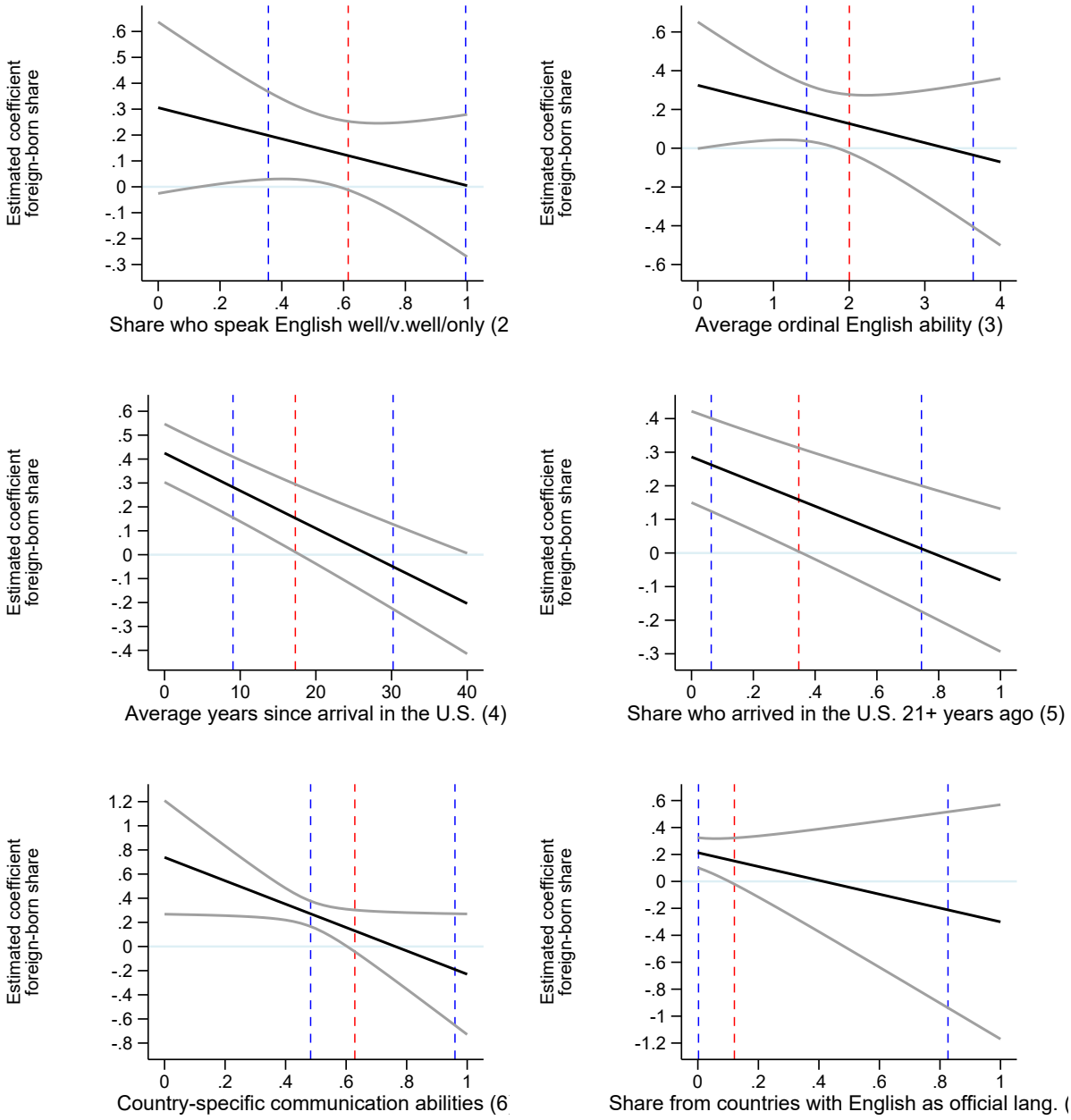


Figure C.2: Effect of the foreign-born share on natives' aggregate supply of manual tasks across the distribution of foreign-born types. OLS estimates.

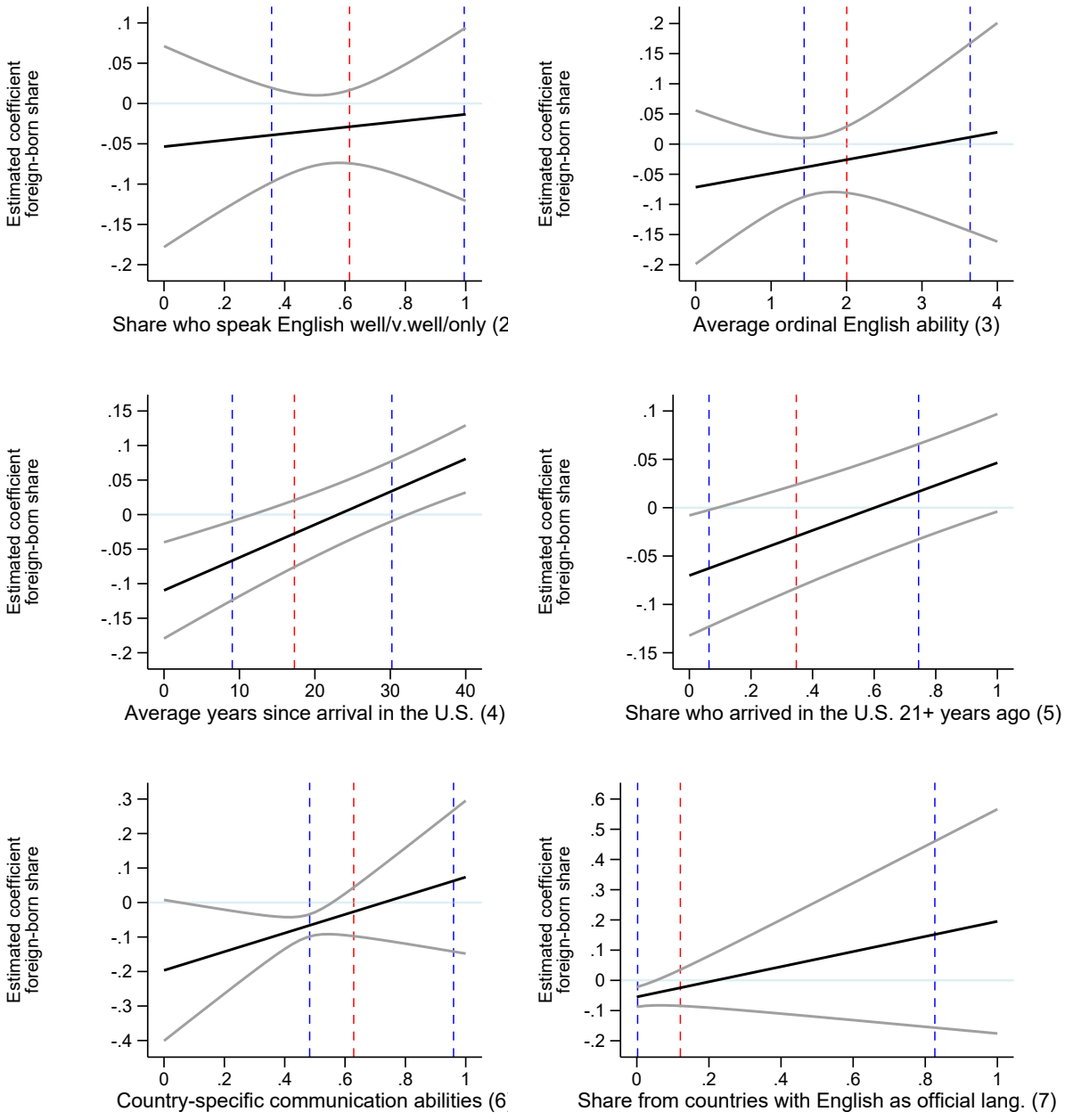


Table C.1: Statistics from first stage regressions: IV80.

	Measure of foreign-born type						
	No type measure	Share who speak English well/ v.well/ only	Average ordinal English ability	Average years since arrival in the U.S.	Share who arrived in the U.S. 21+ years ago	Country-specific comm. abilities	Share from countries with English as official language
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: F-tests of excluded instruments (Angrist and Pischke, 2009)							
Foreign-born share	415.34	47.97	52.63	26.16	67.45	24.70	794.38
Foreign-born type		28.79	24.07	42.78	42.51	20.36	79.11
Foreign-born share×type		60.24	75.88	22.78	119.59	43.20	1187.76
Panel B: Kleibergen-Paap test statistic							
	415.34	33.42	26.19	51.56	49.10	44.04	93.40

Notes. First-stage regressions in table 3.4. The instrumented endogenous variables are indicated in the first column. The foreign-born type is indicated in the top row in models (2)-(7).

Figure C.3: Effect of the foreign-born share on natives' aggregate supply of communication tasks across the distribution of foreign-born types. IV estimates.

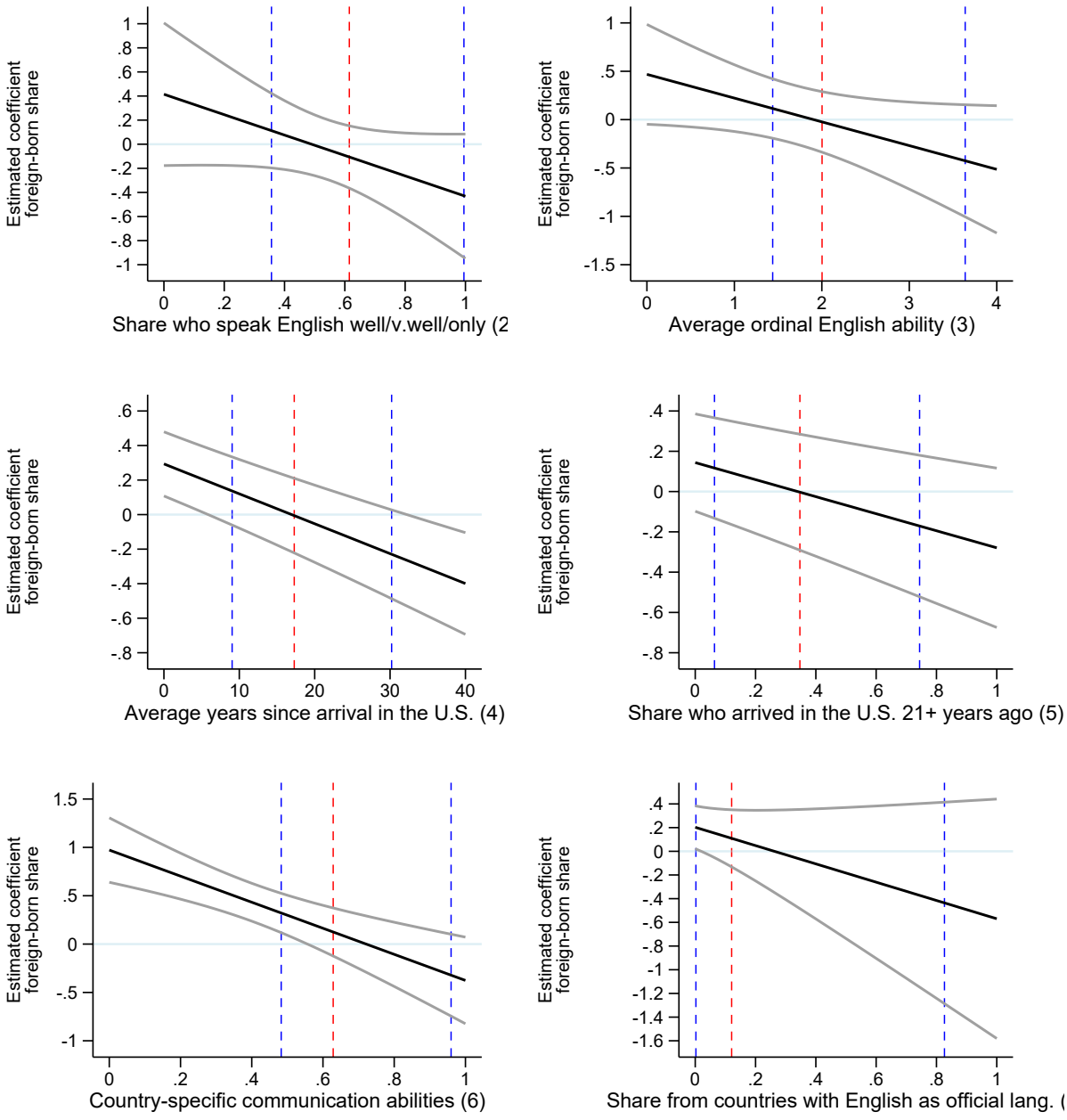
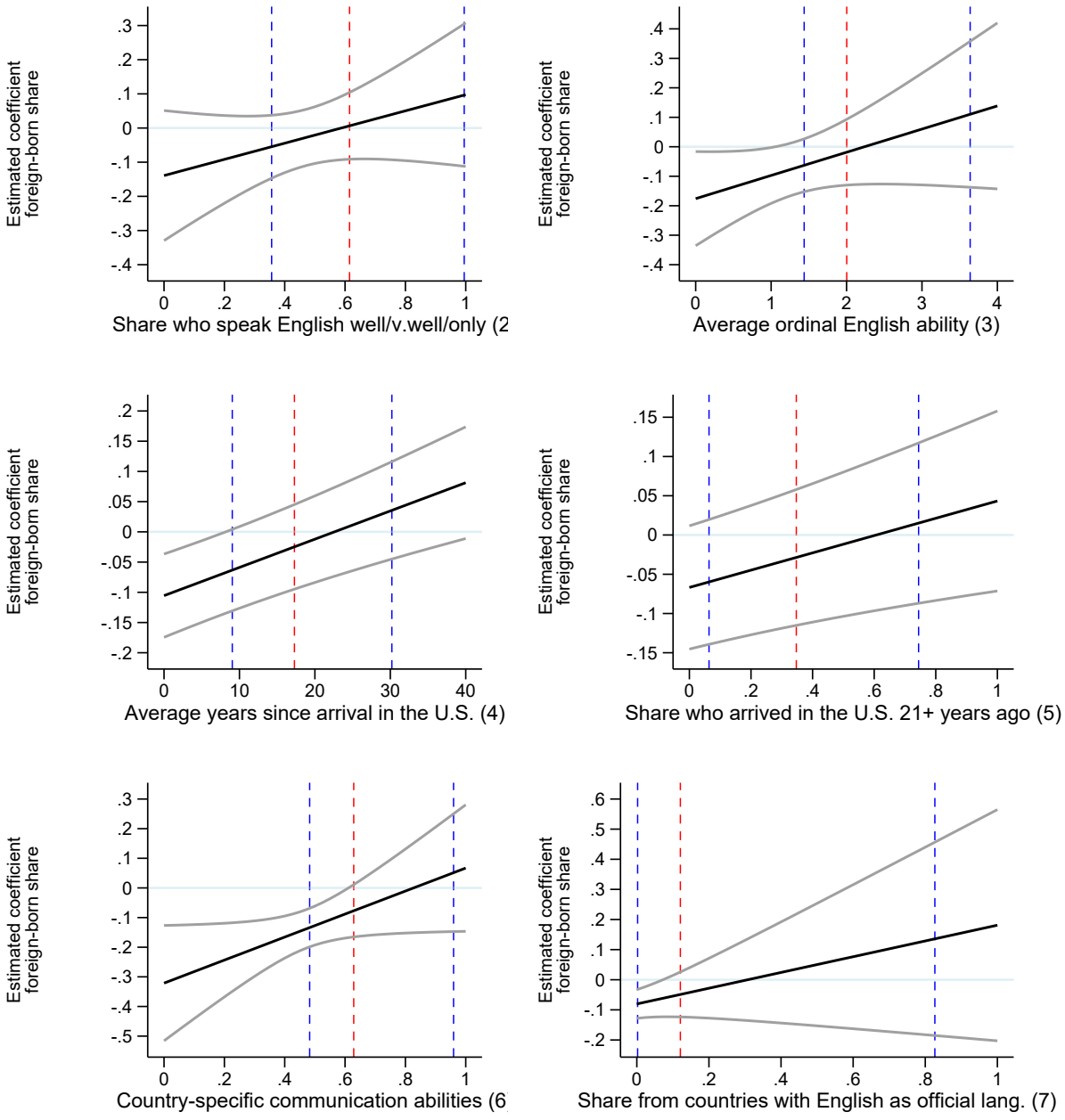


Figure C.4: Effect of the foreign-born share on natives' aggregate supply of manual tasks across the distribution of foreign-born types. IV estimates.



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