# Immigrants and Native Workers:

# New Analysis Using Longitudinal Employer-Employee Data

Mette Foged (University of Copenhagen)

Giovanni Peri (University of California, Davis) *

March 6, 2014

**Abstract**

This paper makes progress on a long standing issue: what is the effect of unskilled immigrants on the labor market outcomes of similarly educated natives? Using the universe of individuals and firms in Denmark for the period 1991-2008 we follow natives over time tracking how their wage, employment and occupational choice responded to a large, exogenous inflow of immigrants. We focus on a largely unexplored inflow of non-European immigrants to Denmark, beginning in 1995 and driven by a sequence of international political crises in Bosnia, Somalia, Afghanistan and Iraq, and an economic crisis in Turkey. We find that an increased supply of non-EU immigrants in a Danish municipality pushed less educated native workers to pursue more complex and less manual-intensive occupations. This reallocation took place mainly through the movement of individuals across firms and resulted in higher or unchanged wages. Immigration increased the mobility of natives but did not increase their probability of unemployment.

**JEL Codes**: F22, J24, J61.

**Keywords**: Immigration, job transitions, complexity, employment, careers, wages.

*Mette Foged; Mette.Foged@econ.ku.dk; University of Copenhagen, DK-1353 Copenhagen K, Denmark. Giovanni Peri; gperi@ucdavis.edu; University of California, Davis, One Shields Avenue, Davis CA 95616. We thank the Economic Policy Research Network for funding this research project and Jakob Roland Munch for helpful suggestions and discussions. Søren Leth-Petersen, Cedric Jean-Laurent Elie Gorinas, Anna Piil Damm, Christian Dustmann, Tito Boeri, Jim Harrigan and Hillel Rappoport provided useful comments and advice.

# 1 Introduction

In this paper we use individual data on the universe of Danish workers matched to data on the establishments where they worked during the years 1991-2008 to quantify the consequences of a supply-driven inflow of less educated immigrants on the occupational choice and working careers of natives. The detail and scope of the data, and the size and nature of the immigration shock allow us to use a credible identification strategy, perform a detailed analysis of outcomes, and explore the mechanisms of adjustment in response to immigration. Do immigrants displace similarly skilled native workers and increase their jobless rates? Or do they complement natives and stimulate natives to specialize in complex tasks? Are effects concentrated within or across firms? Do the combined effects reduce or increase native wages? This paper provides answers to these questions.

The main limitations of existing studies are the ability to identify a genuine supply-shock in the inflow of immigrants and to track the full response of native workers' labor market outcomes. The immigration inflow considered in this paper is that of non-European (non-EU) immigrants, beginning with ex-Yugoslavian immigrants in 1995 following the war and ensuing crisis, and continued due to waves of refugees from Somalia, Afghanistan and Iraq. Turkey, plagued by an economic crisis in 1993-94 was another large supplier of non-EU immigrants. The data shown in Figure 1 point to a discontinuity in the growth rate of the non-EU immigrant population beginning in 1994. In the same period immigrants from the rest of European Union (EU) to Denmark did not increase at all.

For most refugees Denmark applied a Spatial Dispersal Policy across municipalities between 1986 and 1998.[1] This makes their early distribution exogenous to economic conditions as the dispersal policies aimed at spreading refugees across municipality without consideration for their economic performance . Later, when family reunification and working permits were the main causes of entry, immigrants settled, at least for a while, where their family sponsors were located.[2] Hence, the distribution across Danish municipalities of immigrants from refugees' countries as of 1994 was determined by the early dispersal policies. The distribution of Turks (the other group with a large inflow from 1995-2007), instead, was determined mainly by the presence of pre-existing ethnic communities, dating back to the sixties. Both conditions were orthogonal to economic outcomes in those municipalities

[1] The Bosnians were an exception as they were sent disproportionately to rural districts with small existing immigrant communities (Damm, 2009). We therefore exclude them when considering refugees subject to the Dispersal Policy.

[2] By law the sponsor needed "adequately sized accommodation" for the re-unified family. In practice this meant that, at least initially, new family members lived at the same address as their sponsor.

before 1994, as we will show, and this reinforces our trust in their lack of correlation with unobserved determinants of labor market outcomes after 1994.

We exploit the pre-1994 refugee dispersion in our empirical designs, and construct an imputed population of refugee-country immigrants by interacting the post-1994 push-driven flows from crisis-stricken countries with the pre-1994 distribution determined by the early dispersal policy. We also use a similar strategy extended to all non-EU immigrants using the pre-1988 distribution of non-EU communities. This strategy provides variation in refugees (or non-EU immigrants) over time, linked to the timing of crises in sending countries. Their dispersion across municipalities, instead, depends on initial dispersal policy (or to the distribution of pre-existing non-EU communities.

The fact that our data are available beginning in 1991, prior to the surge in non-EU immigration, allows us to identify a "pre-immigration" period (1991-1994) and to test the exogeneity of the instruments to pre-existing economic trends. Our instruments turn out to be relatively strong, they are not correlated with pre-existing trends in economic outcomes of municipalities, and are justified by the credible push-driven episodes in the countries of origin.

The non-EU immigrants considered were significantly less educated than native workers and largely concentrated among non-college educated. They usually spoke the Danish language with low levels of proficiency.[3] These characteristics imply that they were most likely to compete with less educated Danish workers, especially in manual-intensive occupations. The canonical model would imply, therefore, that these immigrants worsened the employment and wage prospects of less educated natives. Non-EU immigrants in other European countries have similar skill composition, thus lending external validity to our study of immigration in Denmark. However, the Danish labor market was and is very flexible relative to many other EU countries. Especially for establishments in the private sector, the hiring and firing/layoff of workers had relatively low costs, the transitions across jobs and occupations were frequent, and wage bargaining was mainly (and increasingly over time) done at the decentralized firm-level (see Dahl, le Maire, and Munch, 2013). This flexibility enhanced the possibility for native workers and firms to make adjustments that responded optimally to immigration.

Our analysis focuses on four main outcomes: the complexity of natives' occupations, their hourly wages, their yearly earnings and the length of their working year. We focus on less educated workers,

[3] Asylum seekers are not in our data and not allowed to work in Denmark. Once (if) their case has been approved they will move into an address in Denmark (assigned to them under the dispersion policy), be allowed to work and appear in the registers. Asylum seekers may attend language causes while their case is being processed.

but we also separately consider more educated natives. First, we analyze what happened to native workers within establishments when exposed to local market inflows of non-EU immigrants. By using a panel regression that includes worker-establishment fixed effects and a host of individual and firm controls, we identify the within-employment-spell variation of outcomes and relate them to non-EU immigrant shares in the local market, instrumented by their imputed values. Second, we use worker-municipality fixed effects in similar panel regressions to identify immigration-induced adjustments within local labor markets. Then we analyze the transition of native outcomes over time following cohorts of native workers during their working careers. This part of the analysis, structured as a difference-in-difference approach, exploits the differential exposure of native incumbent workers to immigrants, based on their 1994 location (before the surge in non-EU immigrants). We follow native individuals over 18 years so as to characterize the short and long-run effects of immigration. Finally, we analyze the impact of non-EU immigrants over the long-run using long-differences in the data to identify the cumulative effects on employment and on inter-establishment and inter-municipality mobility of natives.[4]

Our analysis has three main findings. First, considering native workers within municipalities, larger flows of non-EU immigrants increased their occupational mobility, measured as the probability of changing occupation. This increase was strongly associated with mobility towards complex jobs for workers who changed establishment. This suggests that natives changed their specialization in response to immigrant workers in the local labor market mainly by moving across firms. Second, less educated natives experienced positive or null wage effects. The positive effects were particularly strong for natives initially working in the "advanced service" sector. The only case in which some incumbent native workers had negative effects on their wages was for those in the public sector. Third, the cumulative effect shows that immigration increased the mobility, particularly for highly skilled, across establishments and across municipalities in response to non-EU immigration. However, natives did not experience any effect on cumulative weeks of employment. Therefore immigration increased the cross-establishment and cross-municipality mobility of natives but did not affect the length of their working year.

The rest of the paper is organized as follows. Section 2 frames the present contribution within the existing literature. Section 3 describes the immigration inflow that we consider and the salient

[4]The cumulative regressions are similar to those of Autor et al. (2013) who consider the effect of import competition.

features of the Danish labor market. Section 4 and 5 present the main data, their trends and summary statistics. Section 6 describes a simple decomposition to organize our empirical analysis and discusses the specification and the identification in our regressions. Section 7 shows and discusses the estimation results. Section 8 concludes the paper.

## 2 Literature Review

The analysis of the labor market effects of immigration has a long history. Considered as a labor supply shock, within the labor demand-labor supply "canonical" framework, a series of studies estimated the impact of immigration on wages and employment of natives in local and national economies.[5] Those studies have generally found small effects of immigration on wages and employment of competing natives.[6] This is at odds with the canonical model's that predicts, other things equal, a negative and significant impact of immigrants on wage and employment of similar native workers. More recently a new generation of studies has focused on new mechanisms and margins of adjustments that depart from the canonical model's predictions. Considering a richer environment one may account for the zero or even positive effects of immigration on native wages. The main departures from the canonical framework considered in recent studies are the following: workers have multiple differentiated skills that differ systematically between immigrants and natives[7]; immigrant labor generates the possibility of specialization and productivity effects within and across firms[8]; and investment and technology are adjusted to absorb immigrant labor in local markets.[9] These new lines of inquiry have produced new hypotheses about the possible impact of immigrants on the economy and on firms, and economists have analyzed a richer set of outcomes to validate them.[10] Our paper follows this line of analysis and presents estimates of a set of native workers' outcomes in response to immigration.

Our analysis also relates to the literature analyzing the effect of aggregate shocks on individual labor market outcomes. The only previous studies using comparable data are **?**, who produces within job-spell estimates of the effect of increased outsourcing on wages in manufacturing firms. The same

[5] Examples are Altonji and Card (1991); Card (2001); Friedberg (2001); Borjas (2003); Ottaviano and Peri (2012).

[6] See for instance the meta-analysis in Longhi, Nijkamp, and Poot (2005), or the review article by Blau and Kahn (2012). Exceptions finding significantly negative or significantly positive effects exist, but overall the estimates are centered around zero.

[7] Manacorda, Manning, and Wadsworth (2012); Ottaviano and Peri (2005, 2012); D'Amuri, Ottaviano, and Peri (2010)

[8] One paper analyzing this channel is Peri and Sparber (2009).

[9] Examples are Lewis (2011, 2013); Ottaviano, Peri, and Wright (2013).

[10] See the recent analysis of immigration and productivity in Peri (2012), Immigration and firm creation in Olney (2013) and immigration and economic growth in Ortega and Peri (2013).

Danish data are used in Malchow-Møller, Munch, and Skaksen (2012) who employ establishment-worker fixed effects to analyze the impact of immigrants on wages of native coworkers.[11] However, the joint analysis of the impact of immigration on wages, occupation and employment of natives within firms, and on inter-firm and inter-municipality mobility is original to our study. Moreover, the analysis over time, following a cohort of workers and using a difference-in-difference approach is new in this literature.[12] The ability of the difference-in-difference method to analyze in the same framework the short- and long-run responses and to test the absence of pre-event trends in outcomes makes it very appealing in this context. We are not aware of other studies of the effects of immigration using such methods.

Very few existing studies analyze the dynamic effects of immigration. Cohen-Goldner and Paserman (2011) allow for labor market effects of immigration on natives to change over time but they assume that this is due to the dynamic adjustment of capital and of immigrants, not to a potentially dynamic response of natives. Notice also that our approach follows workers wherever they move. Hence it makes our analysis, immune from the criticisms of area studies (e.g. Borjas, 2003), which posits that wage effects are not captured when limiting the analysis within a geographic area. By following individuals, our approach captures the effects of immigrants on individuals that may "spill" to other regions through mobility.

Previous studies on the effects of immigration constructed pseudo-panel data sets rather than following a genuine individual panel. By using local or national "cells" of workers they linked over time different groups and looked at their outcomes. Selection/attrition and transition of workers across cells can therefore cloud those results. Hence, we know little about wage, career and occupational effects on individuals from those studies. Similarly, with few very recent exceptions (Cattaneo, Fiorio, and Peri, 2013) career and occupation effects of immigration have only been analyzed in the aggregate by previous studies (e.g. Peri and Sparber, 2009; D'Amuri and Peri, forthcoming). Our study analyzes, for the first time, outcomes for native individuals within and across firms over time. Finally, relative to the

[11] Using similar data Malchow-Møller et al. (2013) analyze the impact of immigrant hirings on firm's job creation in the farm sector; Malchow-Møller, Munch, and Skaksen (2011) look at the Danish preferential tax scheme for foreign professionals and estimate the effect of hiring them on wages and productivity within the firm; and Parrotta, Pozzoli, and Pytlikova (2012) look at the effect of an ethnically diversified workforce on firm productivity. Contrary to these papers we consider the effect of changes in the immigrant share at the municipality - and not the firm - level, and we identify an abrupt change in the share of foreign born driven by refugee-sending countries.

[12] This methodology is somewhat reminiscent of Walker (2013) who uses such a method to analyze the effect of environmental regulation on jobs and wages. Von Wachter, Song, and Manchester (2007) use a similar approach to track the long-run effects of job separations in recession.

previous literature, the availability of the universe of individuals in the data minimizes measurement error and eliminates (or drastically reduces) the concern for attenuation bias expressed in studies such as Aydemir and Borjas (2011).

## 3 Immigration and Labor Markets in Denmark

Our analysis focuses on Denmark. Three reasons make this case interesting. First, the extraordinary scope and richness of the individual longitudinal data enables us to track several individual outcomes for a longer period than ever done before. Second, non-EU refugees and economic immigrants in Denmark after 1994 represent a little known push-driven episode, ideal to identify the impact of immigration on economic outcomes. Third, Danish labor markets were quite flexible, different from those in many other European countries but more similar to those in the US and UK. They exhibited high turnover rates, low costs of hiring and layoffs and decentralization in wage setting (Dahl, le Maire, and Munch, 2013). This is the frame in which wage and employment should best reflect marginal productivity. Moreover, as occupational and cross-firm mobility turn out to be important margins of adjustment, a flexible labor market such as the Danish one, allows this mechanism to operate most efficiently.

In this section we briefly describe the features of immigration to Denmark during the period 1991-2008 over which we have data. Immigrants were already in the country before 1995. Their presence, however, as share of employment was not large. They represented three percent of total population and were almost equally divided between EU and non-EU, as seen in Figure 1. A generous program to admit refugees and a policy to promote their dispersion across municipalities was set in place since 1986 (see Damm, 2009). This policy dealt only with a limited number of refugees in the first nine years of its existence. This changed in 1995, when a large wave of immigrants from the regions of Former Yugoslavia, and soon afterwards from Somalia, Afghanistan and Iraq entered the country as refugees, because of ruinous wars in their countries of origin. Since then the share of non-EU immigrants grew significantly until year 2007 (Figure 3). The non-EU immigration boom was fueled during the 1995-2003 period by a sequence of refugees waves driven by international crisis, namely by Bosnians and Somalis in the period 1995-2000 and by Afghani and Iraqis in the period around 2000-2003 (Figure 2). The other major non-EU group was represented by Turkish, whose inflow surged following a deep economic crisis in 1993-94. In our analysis we use either immigrants from countries subject to the

Refugee Dispersal Policy or all non-EU immigrants as explanatory variable.

Figure 1 shows EU and non-EU immigrants as a percentage of employment. The figure confirms two features anticipated above. First, we observe the discontinuity in the trend of foreign born (as a percentage of employment) beginning in 1995. Second, the exclusive role of non-EU immigrants in determining this trend is evident. The overall inflow was sizeable, when cumulated over the whole period. From beginning to end the cumulative increase of immigrants was equal to 3.1 percentage points of total employment (from 3.0% to 6.1%). During the same period the growth of foreign born in typical immigration-receiving countries was similar. In Canada it was +3.5%, in the US it was +3.8%, in the UK it was +3.9% (as percentage of the population in working age).[13] All these economies have received much more attention in the analysis of the effects of immigrants. Figure 3 shows, more specifically, that non-EU immigrants were mainly from refugee-countries and from less developed countries outside of Eastern Europe. The inflow from Eastern European Enlargement countries and from developed non-EU economies in fact account for very little of the increased inflow.[14]

Two other features make the 1995-2007 inflow interesting in terms of its potential labor market consequences on natives. First, non-EU immigrants were less educated than natives. 52% of them did not have a post-secondary education versus only 36% among natives. Second most of them did not speak Danish, and as they were coming from non-European countries, they were often culturally and even ethnically different. Hence, they were likely to be employed in low-skilled manual occupations (as we shall see below). A final, but certainly important reason to focus on the impact of non-EU immigrants is that their entry, differently from the entry of EU immigrants was and is regulated by immigration policies. If we are to learn the consequences of immigration to inform immigration policies in developed countries, this is the group of immigrants we should consider.

## 4 Data and Variables Definition

The data we use are from the Integrated Database for Labor Market Research (IDA). IDA is a collection of registers that link data on individual characteristics of the workers to data on the characteristics of establishments using unique individual and establishment identifiers. The data are recorded annually

[13] During the same period, in Germany the inflow of immigrants implied only a growth by 1.4 percentage points of the labor force and, similarly in France that percentage increase by only 1.1 points.

[14] Eastern European laborers could come to Denmark for work and stay for up to 6 months without registering (like the EU-group) since 2004. Their share of employment is small. Partly because short stays (for temporary work) are under-represented in annual records.

for each individual and establishment in Denmark. Therefore we can observe in what year a match between a worker and an establishment is formed and when it is dissolved. We can also observe detailed occupation and salary for each worker within an establishment.

We select individuals who are between 18 and 65 years old, not attending school (i.e. not eligible for student grants), and not permanently out of the labor force (i.e. not receiving disability pension). This implies that we consider the universe of individuals potentially available to work in the labor market and we refer to them as the "labor force". We eliminate from the sample observations with a missing value in foreign born status or in the municipality of residence (a very small group). We restrict our first empirical analysis (section 6.3.1) to employed individuals in order to analyze hourly wage changes and occupational upgrade within firm and municipality. When turning to the difference-in-difference approach (section 6.3.2) we consider a balanced panel of individuals who were employed in 1994 and we analyze their employment and annual earnings without imposing further restrictions.[15]

We consider three main outcome variables. They are the occupation, the wage and the employment status of Danish native individuals. Specifically, the database contains the annual earnings and employment as the fraction of year worked the labor market status (categorized as self-employed, employed, unemployed, or out of the labor force), the hourly wage rate and the occupation code (according to the ISCO-88 classification) for each individual in each year.

We correct hourly wage and the annual earnings to include mandatory payments to pension schemes. These pension contributions are administered by the employer and reported separately from the income. They are, however, part of the total labor payment and should be accounted for as part of the gross hourly wage and annual labor income[16]. All income variables have been deflated using the Danish consumer price index.

As a measure of the labor supply of an individual we use the fraction of the full-time year worked. The variable takes a value of one if the worker was a full-time employee throughout the year. If either the person was part-time employed and/or if the person was only employed part of the year (and unemployed the rest) the employment variable takes a fractional value equal to a share of the regular

[15]Natives aged 21-51 in 1994 satisfy the age criterion (18-65) throughout the panel and will be included in the panel unless they go back to study, become disabled, leave Denmark or die within the sample.

[16]These mandatory pension contributions vary substantially across industries (between 0 and 17 percent of earnings). As data on the pension payments are available only from 1995 onwards, we only consider wage and income net of pension contributions when we include pre-1995 observations. This might introduce some measurement error in the income variables. The spell analysis however, that can be implemented with net or gross earnings, proved to be robust to the choice of income measures.

working year. The employment of each individual is associated to an occupation according to the internationally standardized ISCO-88 codes.[17] In order to measure the skill content of each occupation we merge the American O*NET database (from the Bureau of Labor Statistics) to the Danish registers using the four-digit ISCO classification of occupations. Thereby, we are able to link most workers to measures of the intensity of use of different abilities on the job. We follow Ottaviano, Peri, and Wright (2013) and aggregate the index of each ability into three categories: communication, analytical and manual skills. We construct an occupational complexity index by combining them. The complexity of an occupation is defined as a composite index increasing in the intensity of communication and analytical skills and decreasing in the intensity of manual skills used.[18]

This method of calculating the skill content of an occupation assumes that such content for a given occupation is similar for Denmark and the US. For instance a "Machine Operator" would use the same intensity of manual, cognitive and communication skills in the US and in Denmark. We also directly observe occupational changes. Hence, we construct a variable that we call "occupational mobility" that equals one whenever an individual changes the (ISCO-88) occupation from period $t-1$ to $t$. To get a sense of the direction of the mobility, we also combine this variable with the hourly wage measure and define "career upgrade" as a variable that takes the value of one when a worker changes occupation and, at the same time, experiences a wage increase. A "career downgrade", instead, is a change in occupation accompanied by a decrease in wage.

Our individual level controls are age, labor market experience (the cumulative employment in years, since first joining the labor force), job tenure (calculated as the period elapsed between the hiring in the current establishment and the present), education and marital status. In terms of schooling, we define individuals with tertiary education as high skilled, and other workers as low skilled. Using information on the country of origin and a variable that categorizes each individual into native and foreign born, we define as immigrants only those individuals who are born abroad and we use the country of origin to calculate immigrant populations by sending countries.

[17] Occupations are reported to Statistics Denmark by firms and there are no legal consequences of misreporting as opposed to, for example, the income of the worker that is reported for tax-purposes. We constructed an algorithm that replaces a missing or invalid ISCO-88 by the next within the match with the firm if the next is also the most frequent within the worker-firm match. We used next and not previous, since the occupation code is most often missing in the beginning of the worker-firm spell possibly due to lag in registering. This algorithm as well as lack of incentives for firms to change the occupation reported for an employee may lead to under-estimation of the true job mobility within firms.

[18] The index, is calculated as: $\ln\left((\text{Communication} + \text{Analytical})/\text{Manual}\right)$. The underlying skill intensities have been standardized to be between zero and one and each is the average of a series of indicators within the category. Hence the constructed complexity index can take values between -$\infty$ and +$\infty$.

Immigrants are separated in two groups: One consisting of individuals from countries which have had free mobility of labor agreements with Denmark since 1995. These are the EU15 countries plus Norway, Iceland and Liechtenstein (as members of the European Economic Area) and Switzerland (through a bilateral agreement). We define this group (somewhat improperly) as EU. The other group, consisting of immigrants from any other sending country, is defined as non-EU immigrants. They are the source of the variation of immigrants analyzed in this paper. The non-EU group is dominated by Turkey and Former Yugoslavia, but whereas a large number of Turks arrived before our analysis window, refugees from Former Yugoslavia and several other refugee sending countries such as Afghanistan, Iraq, Sri Lanka, Pakistan, Iran and Somalia fueled the immigration we analyze.

The geographic units that we use to approximate local labor markets are 98 municipalities that can be identified consistently in Denmark, over time, beginning in 1988 till 2007. We merge Frederiksberg and Copenhagen since those two municipalities constitute one integrated labor market. This leaves us with 97 areas where Copenhagen, Aarhus and Aalborg are the biggest, most populous ones.[19] Most municipalities are in the mainland part of Denmark. Some municipalities are islands. Bornholm, for instance, is separated by a 5.5 hours boat trip from the nearest municipality in Denmark and is thereby a rather isolated labor market. Municipalities are small geographical units. As we can follow workers across municipalities, we observe that most of the mobility of workers takes place across firms within municipality confirming that municipality are rather self-contained units. Only around 10% of the workers who move across establishments each year change municipality.

## 5 Descriptive Statistics

The top three receiving municipalities (Ishøj, Arbertslund and Brøndby) experienced an increase of foreign-born larger than 10 percentage points of total employment in the considered period. The bottom three (Læsø, Assens and Lejre) experienced an increase of 1 percentage point or less. Figure 4 provides summary evidence that a remarkable gap between high and low non-EU immigration opened rather abruptly across municipalities beginning in 1995. The figure shows the difference in the non-EU share of employment between highly exposed (above the median) and less exposed (below the median)

[19] Copenhagen (including Frederiksberg) had 603 thousand inhabitants in 2008, and Aarhus and Aalborg had, respectively, 298 and 195 thousand inhabitants. The smallest municipalities are islands with two to seven thousands inhabitants, which will count very little in our estimations. The next smallest municipalities begin at around twelve thousand. In the large cities the employment/population ratio is about 60%, while it is 40% in the more isolated, rural municipalities.

municipalities to non-EU immigrants.[20] It is clear that there is no trend in the pre-1994 difference in share of non-EU immigrants between these two types of municipalities. It is also clear that starting in 1995 a steady and continued inflow of non-EU immigrants increased the gap in the immigrant share across those two types of municipalities. Moreover, Figure 5 shows no break (and essentially no change) in the differential trend for the EU immigrants in the same two groups of municipalities. EU immigrants were free to work in any Danish municipality. Hence if the discontinuity and differential growth shown in Figure 4 was driven by differential demand and labor market conditions it should have manifested itself mainly (or also) with EU immigrants. The presence of no differential trend for EU immigrants does not suggest a local labor demand driven event in the receiving municipalities.

Among the areas with the largest immigrant inflows some are larger cities, such as Copenhagen and Aarhus. The dispersal policy in place between 1986 and 1998, however, spread the non-EU immigrants also to smaller towns. While differences in the initial characteristics of the municipalities will be controlled for, we also run tests in section 6.4 to check that our instruments are uncorrelated with the pre-existing economic trends of a municipality, and in the difference in difference approach we check that a pre-1994 trend is not present in the differences of native outcomes in the municipalities exposed and not exposed to immigration.

In some specifications we distinguish between four broad sectors: manufacturing, complex services, non-complex services and public sector. While the first two sectors tend to produce tradable and differentiated goods and services and are subject to international competition and technological change, the other two tend to produce less differentiated goods and are more protected from competition and international market forces. The largest non-EU immigrant inflow was into manufacturing. The increase in non-EU immigrant workers took place among elementary, manual intensive occupations requiring little education. These were also occupations employing low skilled natives in larger percentages.

Table 1 lists the occupations that experienced the lowest and the highest inflow of non-EU workers, measured as the change in the share of non-EU immigrants employment between 1994 and 2008. For those occupations we also show the index of intensity of use of cognitive, communication and manual tasks and the derived complexity index that combines all of them. Occupations experiencing the largest inflow of non-EU immigrants were significantly more intensive in manual skills and less intensive in

[20] The exact definition of highly and less exposed municipalities is explained in section 6.3.2.

cognitive and communication skills than those attracting a small share of immigrants.[21]

The empirical analysis is based on a 20% random sample of natives.[22] Summary statistics for the controls and for the dependent variables used in the empirical analysis are provided in Table 2. The table is based on the sample used in the spell regressions, which includes only individuals, as long as they are working, over the considered period (1995-2008).[23] We divide the sample between low skilled and high skilled, based on their education (no tertiary or tertiary education) when they first enter the sample. The group of low skilled is younger, has less labor market experience and lower job tenure, and as expected also has, on average, lower hourly wages and lower annual earnings.

# 6 Framework, Empirical Strategy and Identification

Our identification relies on the variation of non-EU immigrants over time, across Danish municipalities. In this section we first argue that the local labor market, proxied by the municipality, rather than the firm, is the right unit to measure variation in the explanatory variable and to construct a credibly supply-driven change of non-EU immigrants. We then show an easy decomposition of the effects that justifies our two main empirical approaches. Finally, we describe our empirical specifications and discuss identification and instrumental variables.

## 6.1 Local Supply Shock of Non-EU Immigrants

Previous studies using Danish data such as Malchow-Møller, Munch, and Skaksen (2012) and Parrotta, Pozzoli, and Pytlikova (2012) have considered the increase of immigrants at the *firm level* as explanatory variable. Those studies analyze the correlation between the presence of foreign born and the wages of natives within the firm. They find mainly negative effects. Our strategy, focuses on the variation of immigrants within local labor markets instead. The response of native individuals within and across firms, over time, to changes in the local supply of foreign-born constitutes our outcome of

[21] The low share of immigrants among skilled agricultural workers is somewhat surprising. The share of immigrants in agriculture increased 11 percentage points between 1994 and 2008 (Malchow-Møller et al., 2013). But they do different kinds of unskilled work categorized for instance as "Agricultural, fishery and related labores" (which scores -1.128 in the complexity index) and other elementary occupations.

[22] Immigrant shares (the explanatory variable of interest and instrument) are calculated on the full sample to avoid measurement error.

[23] The difference-in-difference analysis uses all individuals who were working in 1994 and follows them over the period 1991-2008. Their characteristics in terms of age, labor market experience, education and wages are not very different from those of the unbalanced sample of employed reported in Table 2. We define low/high skilled in the cohort sample based on the education in 1994.

interest.

Databases like ours allow the researcher to construct the share of immigrants both at the firm level and at the geographical level (local labor markets). We want to emphasize that it is a much more reasonable strategy to identify a supply-driven shock of immigrants at the geographical level, rather than at the firm level. This is because, the pre-1995 location of refugees and their families, mainly the result of previous enclaves and early dispersal policies, interacted with the post-1995 inflow, driven by international political and economic crises, is likely to be exogenous to economic trends in Danish municipalities since 1995. To the contrary, the pre-1995 hiring of immigrants across firms in a municipality was certainly affected by firm-specific factors. If they are persistent and correlated with its trend in productivity and specialization after 1995 they may be correlated with native outcomes in that period. Moreover, the high mobility of workers within a municipality implies that, even when firms have some market power and ethnic networks make new immigrants more available to some firms than others, wages for a specific occupation are determined at the municipality level. It is more reasonable to think that the supply of a certain type of workers is region-specific rather than firm-specific.

Finally, if we entertain a firm-level supply change of immigrants and construct the instrument based on the initial share of immigrants we can only use the sample of long-lived firms, as they need to exist pre-1995. Those would be very selected firms, that survived for a long time.[24] Hence firm-level data can improve our understanding of the *consequences* of immigration, when analyzing the impact of an exogenous change in immigrant supply on within firm effects and between firm mobility. The units to capture these shocks, however, are local labor markets. Recently, Dustmann and Glitz (2011) also considered immigrants in local labor markets when analyzing the adjustment mechanisms of the local firms. Schmidt and Jensen (2013) use aggregate data on regions in Denmark between 1997 and 2006 and find positive or non-negative effects of immigration on wages and employment of natives.

### 6.2 A Simple Decomposition

Consider a municipality[25] in which each native worker, that we denote with the index $i$, works in an establishment (firm) that we denote with the index $j$. Such initial match, for given initial conditions,

[24] As described in section 6.4 we use 1988-shares to impute our instrument for the total non-EU group, and 1994 for the refugee-sending countries during the Spatial Dispersal Policy.

[25] In this section we omit the municipality index, for brevity. The formulas should be considered as relative to the representative municipality.

maximizes her wage (utility). There is a set of $M$ establishments in the municipality. Each has a specific productivity when matched to worker $i$. $I_{ij}$ is an indicator that equals 1, when worker $i$ chooses to work in establishment $j$ and it is defined as

$$\begin{aligned} I_{ij} &= 1 \text{ if } w_{ij} = \max\{w_{i1,...}w_{iM}\} \\ I_{ij} &= 0 \text{ for all other values of } j \end{aligned} \tag{1}$$

where $M$ is the number (and the set) of different establishments in the municipality. The wage that each worker receives depends on specific characteristics of the worker, of the firm and on the firm-worker match. The demographic characteristics of the worker $X_i$, the productivity of the firm $A_j$, as well as local labor market conditions in the municipality affect the wage that each worker receives from a firm. We focus, in particular, on the effect of the share of foreign born in the municipality, $S$, on the wages in each establishment. Hence, explicitly capturing this dependence, we can write $w_{ij}(S)$.

There are several channels through which the supply of foreign born can affect native wages in the municipality and in each establishment. First immigrants affect the supply of some skills making the value of complementary skills higher and substitutable skills lower in the municipality e.g. Ottaviano and Peri (2012); Peri and Sparber (2009). Second, immigrants may affect the productivity of the municipality by increasing the variety of skills and intermediate goods produced and used there (Ottaviano and Peri, 2005; Ortega and Peri, 2013). They may also affect the productivity of the establishment (Ottaviano, Peri, and Wright, 2013). Such productivity effects may be stronger in establishments that employ a large share of foreigners. Hence, the share of immigrants affects the relative wages faced by individual $i$ in different establishments and therefore also the optimal matching rule can be written as $I_{ij}(S)$.

We consider the aggregate of native workers initially in a municipality in year $t$ and we denote it with $N_t$. We indicate the initial share of immigrants with $S$ and we write the aggregate native wage in the municipality as

$$W_t = \sum_{i=1...N_t} \sum_{j \in M} [I_{ij}(S) * w_{ij}(S)] \tag{2}$$

Consider now that between year $t$ and year $t+\Delta t$ the share of immigrants in the municipality increases to $S + \Delta S$. This change has an impact on the wage that each establishment pays to native workers which would equal $w_{ij}(S + \Delta S)$ after the inflow. It will also affect the decision of a worker to stay

in an establishment or to move through crowding-out, productivity or complementarity effects. The optimal decision would be $I_{ij}(S+\Delta S)$ after the inflow. Moreover, as the municipality is an open economy, native workers may also move out of it and find employment in an establishment outside of $M$. Therefore, we can decompose the effect of an increase in the immigrant share by $\Delta S$, on the average wage of workers who resided in the municipality at time $t$, into the following three terms

$$\begin{aligned}\Delta W_t \;=\; & \sum_{i=1\ldots N_t}\sum_{j\in M}\underbrace{I_{ij}(S)[w_{ij}(S+\Delta S)-w_{ij}(S)]}_{\text{Wage Change Stayers}}+ \\ & +\sum_{i=1\ldots N_t}\sum_{j\in M}\underbrace{\left[I_{ij}(S+\Delta S)w'_{ij}(S+\Delta S)-I_{ij}(S)w_{ij}(S)\right]}_{\text{Wage Change for Workers changing Firm}}+ \\ & +\sum_{i=1\ldots N_t}\sum_{j\notin M}\underbrace{I_{ij}(S+\Delta S)w'_{ij}(S+\Delta S)-I_{ij}(S)w_{ij}(S)]}_{\text{Wage Change for Workers changing Municipality}}\end{aligned} \tag{3}$$

The first term captures the wage change of people who remained in the same establishment.[26] As immigration affects the productivity of plants and municipalities this term captures simply the changes in the wages of natives who kept their job with the original employer. The second and third term, capture the change in wages of native workers who moved out of the original establishments. The important part of these terms is the fact that immigration affected both the distribution of natives across establishments and the wage of natives in the new establishments. The term $I_{ij}(S+\Delta S)$ captures the new allocation of native workers for those who changed establishment so that $I_{ij}(S+\Delta S)-I_{ij}(S)$ is a measure of the flows to different establishments. By focusing on this term we can analyze how immigration has affected inter-firm movements. The second summation term in expression (3) includes native individuals who changes establishment within the municipality $j\in M$, while the third term includes those who moved to establishments outside of the municipality $j\notin M$. Finally the term $w'_{ij}(S+\Delta S)$ captures the wage for native workers who moved establishment. The notation $w'_{ij}(S+\Delta S)$ implies that the wage for mover $i$ in the new establishment $j$ differ from the previous wage both because the new wages across establishment are affected by immigrants $w_{ij}(S+\Delta S)$ and because moving may have caused a loss of specific capital to the mover. Hence the notation $w'_{ij}(S+\Delta S)$ indicates the individual-specific wage for a mover and can be smaller or higher than $w_{ij}(S+\Delta S)$, the wage for an identical stayer in the same establishment.

Our empirical specifications analyze the effects of non-EU immigrants on native outcomes pro-

[26] The indicator $I_{ij}(S)$ denotes an allocation for these workers as it was before the change in $S$.

gressively including the different components of expression (3). We also analyze the impact on the inter-establishment flows of native workers $I_{ij}(S + \Delta S) - I_{ij}(S)$. While equation (3) considers wage as native outcomes in our empirical analysis we also look at other outcomes such as specialization in complex tasks, career advancements and labor supply.

The first empirical specification focuses on the effects on individuals within firms. Using a "employment-spell" regression, we will identify changes in outcomes for workers within a worker-establishment match. This correspond to the first term in the right hand side of expression (3). As there is limited literature analyzing the effect of immigration on workers outcomes within a firm, these results will be relatively new.[27] A similar empirical specification, using a different set of fixed effects, allows us to estimate the first two terms of (3) together. In a "municipality-spell" regression we analyze the wage effects (and other outcomes) for native workers who stay within the municipality. Finally the long-run effects on all native workers initially in a municipality, including all three terms in equation (3) are estimated with the difference-in-difference approach. Within this approach we also estimate the effect that immigration has on the flows $I_{ij}(S + \Delta S) - I_{ij}(S)$ across establishments and out of the municipality. We are also able to estimate whether the transition implies that some workers exit employment altogether (adding non-employment as another choice to the set of establishments). The empirical specifications and how we identify the response to immigration is the focus of the remaining of this section.

### 6.3 Empirical Specifications

In an economy in which workers and firms are heterogeneous and in which mobility is imperfect and costly, analyzing the effects of immigrants on workers within firms, across firms and across municipalities in the short and long run can provide a complete picture of the impact of immigration on natives. Hence, the rich set of outcomes and the variety of empirical specifications help provide a more complete picture of the margins and mechanisms of adjustment.

[27] Malchow-Møller, Munch, and Skaksen (2011, 2012); Malchow-Møller et al. (2013); Parrotta, Pozzoli, and Pytlikova (2012) produce estimates of the effect of hiring immigrant workers on firm outcomes and worker outcomes within the firms. Kerr and Lincoln (2010) exploits the H-1B visa reform to estimate the effect of high skilled immigration on the patenting activity of 77 large firms.

### 6.3.1 Effects within Establishment or Municipality: The Spell Regressions

The first specification focuses on the effect of immigration on the wages, occupational complexity, career mobility and labor supply of workers within an establishment (the first component of expression (3)) or within a municipality (the sum of the first two terms in expression (3)). It does not consider the potential effect of immigration on workers who move out of the municipality or become non-employed or self-employed. Hence, important displacement effects of immigration will be lost by this approach if immigration, for instance, increases separation rates and workers experience unemployment periods. Moreover, this approach is based on year-to-year within spell-variation and it misses the long-run cumulated effects of immigration. These shortcomings will be addressed in the next section 6.3.2.

The outcomes relative to native ($NAT$) individual $i$ in establishment $j$ in municipality $m$ at time $t$ will be indicated as the variable $y_{ijmt}^{NAT}$ in regression (4) below. The first outcome analyzed is occupational complexity. We consider three outcomes relative to career mobility: upgrade, downgrade and simply mobility. Then we analyze the logarithm of hourly wages, the logarithm of annual earnings and the log of employment, measured as a fractional value of a complete working year. The main explanatory variable is the non-EU immigrant (or Refugee) share of employment in municipality $m$ and year $t$, $S_{mt}^{nonEU}$, calculated as $F_{mt}^{nonEU}/P_{mt}$, where $F_{mt}^{nonEU}$ is the stock of employed immigrants of non-EU origin and $P_{mt}$ is the total employment in municipality $m$ and year $t$. In the 2SLS specifications we instrument $S_{mt}^{nonEU}$ with $\widehat{S}_{mt}^{nonEU}$ that we describe and discuss in section 6.4. The regression has the following structure:

$$y_{ijmt}^{NAT} = x_{it}'\alpha + \beta S_{mt}^{nonEU} + \phi_{t,IND} + \phi_{t,REG} + \gamma_{i,u} + \varepsilon_{ijmt} \tag{4}$$

The variable $x_{it}$ is a vector of time-varying individual characteristics including age, labor market experience, experience squared, job tenure, tenure squared, education, and whether the person is married. $\phi_{t,IND}$ and $\phi_{t,REG}$ are industry-by-time and region-by-time effects capturing regional and industry-specific time patterns. Regions are the five administrative regions in Denmark and industries are the eight industries of the 1-digit NACE industrial classification scheme.[28]

The key set of controls in regression (4) is indicated by $\gamma_{i,u}$. It represents fixed effects for each individual ($i$)-unit ($u$) pair. Depending on which unit we choose, the inclusion of these effects allow us

[28] The regions and industries are listed in Table 2.

to identify the impact of immigration on outcomes for different groups of native workers. In the first set of regressions we choose the unit $u$ to be an establishment, $j$. In this case the set of fixed effects $\gamma_{i,j}$ will vary for each different employee-establishment pairing.[29] This is a demanding specification and it implies that our regression controls for any unobserved heterogeneity that is specific to the worker-establishment match (job spell). The regression identifies the impact of an increased supply of non-EU immigrants in the municipal labor market on the outcome of native workers within job-spell.[30] The results of these regressions shed light on the effects for workers within firms (establishment) when an inflow of immigrants increases the availability of non-EU workers in their municipality. This corresponds to the first term of decomposition (3).

In the second set of regressions the unit $u$ is the municipality. Hence, we include a set of individual-municipality fixed effects $\gamma_{i,m}$. These specifications controls for individual-municipality specific productivity, and they estimate the impact of immigrants on the wage, occupation and labor supply of native workers who remain within the same municipality (but may change establishment). Comparing the estimated effects using these two different types of variation allow us to distinguish the effects on workers who do not change establishment and on workers who do. More specifically, we can assess how large and significant occupation, employment, and wage adjustments are for people who do not change establishment in response to immigration, and how these adjustments compare when including all workers in the municipality.

The key explanatory variable, the share of non-EU immigrants, varies at the municipality-year level. This implies that we cannot control for a municipality-year effect, as it would absorb all the identifying variation. To minimize omitted variable bias we use the instruments described below. To account for error correlation within the level of variation of the explanatory variable we cluster standard errors at the municipality level. The estimates cannot be affected by composition effects such as the changing type of firms or of workers over time because only variation within firm-worker match are used.

[29] This is similar to the fixed effects used in Hummels et al. (forthcoming) and Malchow-Møller, Munch, and Skaksen (2011).

[30] 10.7 percent of the observations (individuals × year) are in job spells where the worker changes municipality of residence at some point during the match with the employer. This includes small moves across municipality borders and moves that are due to imperfect timing of job change and change of residence. We exclude these job spells from the within worker-firm match regressions, but results are not sensitive to whether they are excluded or included as two different job spells.

### 6.3.2 Following Workers: The Difference-in-Difference Approach

To identify the short- and long-run outcomes for all native workers, including those who moved out of the municipality and hence including all terms of expression (3), we use a difference-in-difference empirical approach. The goal is to follow the trajectory of wages, employment and occupation for native workers in response to the supply-driven change in non-EU immigrants described in section 5.

Previously we showed that the immigrant share increased abruptly in some municipalities beginning in 1995, while leaving other municipalities virtually unaffected. As anticipated and as we will discuss in section (6.4) below, a good predictor of the actual non-EU immigration across municipalities is the presence of non-EU communities in 1988 interacted with non-EU aggregate flows post 1995, which we call the *imputed* immigration. Predicted immigrant shares can then be obtained from a first stage regression of the actual immigrant shares on imputed immigrant shares (as well as year and municipality fixed effects). So we define as "exposed to immigration" or the "treated group" those individuals who in year 1994 were living in areas that experienced a subsequent non-EU immigration inflow above the median as measured by the *predicted* exposure. "Non-exposed" or the "control group" are those individuals who lived in areas with less than median inflow of non-EU between 1994 and 2008 as measured by *predicted* exposure.[31] Instead of using the median as a watershed between high and low exposure, we also replicated the analysis comparing the upper and lower quartile of immigration exposure (omitting the intermediate quartiles). This analysis gave larger but less precise estimates. The preferred specification showed here uses the median value as separator.

This difference-in-difference approach has another advantage. It allows us to define a pre-treatment period as the years 1991-1994 and a post-treatment period, 1995-2008. We treat mobility and outcomes after 1995 as endogenous. Hence area, region and industry fixed effects are associated to the worker considering his/her 1994 characteristics and location. We analyze the outcomes of natives in the post-treatment period and test for pre-1995 trends in native outcomes. This will test whether the performance of workers in highly exposed and less exposed municipalities (post 1995) differed already before 1995.

We implement the difference-in-difference estimates within a regression framework, by interacting $M_i$ an indicator for exposure, corresponding to one if individual $i$ was in a treated municipality $m$ as

[31] Specifically, the population weighted distribution of the 1994-2008 difference in the *predicted* non-EU immigrant share is our measure of *predicted* exposure. This strategy, as opposed to using the *imputed* exposure directly, mirrors the 2SLS strategy of the other empirical specifications.

of 1994, with a set of year dummies, $D(year = t)$, that are one in the relevant year and zero otherwise. The coefficients $\gamma_t$ in equation (5) below capture the difference in outcomes from 1991 (year -3) to 2008 (year 14) between treated and non-treated individuals. Year 1994 is year 0 and the coefficient for that year is standardized to 0.

$$\begin{aligned} y_{imt}^{NAT} &= \tilde{x}_i'\alpha + \sum_{t=-3}^{-1} \gamma_t M_{im} D(year = t) + \sum_{t=1}^{14} \gamma_t M_{im} D(year = t) + \\ &\quad + \tilde{\phi}_{t,IND} + \tilde{\phi}_{t,REG} + \tilde{\phi}_{t,EDUC} + \tilde{\phi}_{t,OCC} + \tilde{\phi}_m + \varepsilon_{it} \end{aligned} \tag{5}$$

A tilde indicates variables that are measured in year 1994; hence, they capture individual characteristics before the non-EU immigration boom. Equation (5) is estimated using a strongly balanced panel to be able to identify the effect on individual workers (unaffected by compositional changes and non-random sorting across industries and areas). We include fixed effects for the 1994-municipality of the worker, $\tilde{\phi}_m$, and industry-by-year, $\tilde{\phi}_{t,IND}$, region-by-year , $\tilde{\phi}_{t,REG}$, education-by-year, $\tilde{\phi}_{t,EDUC}$ and occupation-by-year $\tilde{\phi}_{t,OCC}$[32] fixed effects.[33] The remaining controls $\tilde{x}_i$ are as those defined as in equation (4), but relative to the worker in year 1994.

Consistently with the model of section 6.3.1, we consider as outcome variables, $y_{imt}^{NAT}$, occupational complexity, hourly wages, annual earnings and employment as fraction of the full-time year worked. The new information in this approach is that we can follow all workers including those that endogenously decided to leave the area or to leave employment, and that this framework allows us to examine the pre-1995 trends of native outcomes.

To capture the effect of immigration on the probability of transition out of the establishment or out of the municipality (i.e. the specific impact on term $I_{ij}(S + \Delta S) - I_{ij}(S)$ in section 6.2) or out-of employment we calculate the cumulative fraction of each year spent in the initial and in new establishments and municipalities as well as in unemployment. We also calculate the cumulative effect on the present discounted value of earnings to summarize the overall impact on the exposed workers

[32] The occupations are: skilled worker, intermediate professional, higher grade professional and managerial position within the firm.

[33] As we include municipality and year effects in the model we omit year 1994 in the interactions with the "treatment" effects. Hence, 1994 is the reference year, namely year 0 in the event. We let NACE 1 in 1991 be reference for the industry-by-year effects, and leave all year effects for one region, education and occupation out. Lastly, since the municipality fixed effects are collinear with region-year fixed effects, one municipality per region is left out.

1995-2008. The regression on these cumulated variables looks as follows:

$$\Delta y^{NAT}_{i,m,1995-2008} = \alpha \tilde{x}'_{i,1994} + \beta \Delta S^{nonEU}_{m,1994-2008} + \tilde{\phi}_{IND} + \tilde{\phi}_{REG} + \tilde{\phi}_{EDUC} + \tilde{\phi}_{OCC} + \varepsilon_i \qquad (6)$$

$\Delta y^{NAT}_{i,m,1995-2008}$ is the cumulated 1995 and 2008 outcomes, and $\Delta S^{nonEU}_{m,1994-2008}$ is the actual change in the immigrant share from the pre-treatment year 1994 to 2008. To avoid correlation between the changes in non-EU immigrants and unobserved municipality-specific shocks we instrument the change with the imputed supply-push variable that we now describe. This regression is simply a cumulated version of equation (5). Standard errors are clustered at the municipality level in both equations since this is the level of variation in our variable of interest.

### 6.4 Identification and Instrumental Variable

Our explanatory variable of interest measures non-EU migrants as a share of employment in the municipality $m$ at time $t$ (or the change in those shares). We denote this as $S^{nonEU}_{mt}$. The inflow of non-EU immigrants may be correlated with unobserved demand shocks. In all specifications we control for the time invariant differences between municipalities, and for the industry- and regional-level fluctuations in demand. Nevertheless, we may be left with some municipality-specific unobserved shock affecting both native and immigrant labor demand. Therefore we build an instrument based on the distribution of non-EU population by nationality across municipalities in Denmark as of year 1988, six years before 1994-95 the acceleration in the non-EU immigration. In an alternative instrument, we use the 1994-distribution of refugees. Our hypothesis is that the geographic distribution of early non-EU communities and the distribution of early refugees produced by the Spatial Dispersal Policy (1986-1994) are both uncorrelated with the post-1995 labor demand changes across municipalities.

We then use the national inflow of non-EU immigrants, or refugees only, by nationality, driven mainly by country of origin political and economic crises, and independent of municipality-specific economic shocks. Interacting these aggregate national inflows and the municipality pre-existing shares we obtain the supply-driven increase in non-EU immigrants in each municipality. This method is not new and follows the literature since Altonji and Card (1991).[34] However, the focus on non-EU immigrants and refugees, the post-1994 increase in immigration rates associated with country of origin

[34]Schmidt and Jensen (2012) show for Denmark that initial immigrant shares and subsequent immigrant inflows are positively correlated. (The municipalities they use are before the reform in 2007 where the number of municipalities was three times higher compared to the new, larger municipality definition that we employ.)

crises, the comparison with EU immigrants and the test of orthogonality with the pre-1994 trends (that we will show below) reinforce our confidence in the fact that the instrument variation is supply-driven.

Let $F_{ct}$ denote the total population of immigrants from country $c$ residing in Denmark in year $t$, and $s_{cm1988}$ the share of that population residing in municipality $m$ as of year 1988.[35] We then construct $\widehat{F}_{cmt}$ the imputed population from country $c$ in municipality $m$ in year $t$ as follows: $\widehat{F}_{cmt} = s_{cm1988} \times F_{ct}$ and the imputed total share of immigrants with non-EU origin as: $\widehat{S}_{mt}^{nonEU} = (\sum_{c \in nonEU} \widehat{F}_{cmt})/P_{m1988}$, where $P_{m1988}$ is the total population in municipality $m$ in year 1988. The variation of $\widehat{S}_{mt}^{nonEU}$ is only driven by the changes in the imputed non-EU population (the denominator is held fixed at is 1988-value) and it is used as instrument for the actual share of non-EU immigrants in municipality $m$ at time $t$ ($S_{mt}^{nonEU}$).

The exclusion restriction requires that the imputed inflow of non-EU immigrants is uncorrelated with the unobserved determinants of municipal trends in labor demand and labor market conditions once we control for fixed effects and observed variables. Besides the evidence provided above, we perform some important falsification tests. In Table 3 we show whether the 1994-2008 change in the imputed non-EU labor share, our instrument, is correlated with trends in any of the outcome variables (occupational complexity, hourly wages, fraction of year worked and yearly earnings) between 1991 and 1994, the pre-immigration surge period. The unit of observation is the municipality. A significant correlation with trends that pre-date the non-EU immigrant surge would cast doubts on the validity of the instrument.[36]

The regressions of Table 3 include age, labor market experience, job tenure, (and each of them squared) and marital status averaged over the labor force in each municipality in 1994 as controls and weights each municipality by its labor force in 1994. In the upper part of the table we consider imputed immigrants including all non-EU countries. The first rows include estimates using outcomes for low skilled. In the next rows of the table we consider outcomes for high skilled natives, instead. The estimated coefficients on the pre-1994 changes are small and never statistically significant at any standard level. The last column, to the contrary, shows the correlation of the instrument change from 1994-2008 with the explanatory variable (the change in actual labor share of non-EU immigrants). The very significant coefficients and large $F$-statistics suggest that the instrument is strong.

[35] In the construction of the instrument, as in the analysis of the labor market and as described in section 4, the population that we consider are individuals 16 to 65 years old, not in school and not permanently disabled.

[36] In the analysis of the cohort-based transitions, in section 7.2, we will check whether there is a pre-1994 trend in the differences in outcomes between the high-immigration municipalities and the low immigration municipalities.

The lower part of the table shows the correlation with pre-1994 trends when the instrument is constructed only using countries contributing large numbers of refugees between 1986-1998 and subject to the random Dispersal Policy. Damm and Dustmann (forthcoming) exploit this policy to study the effect on criminal behavior of exposure to crime in the local neighborhood. We follow them and exclude Former Yugoslavia when considering refugees because the unusual large inflow of Bosnians in the early 1990s meant that an exemption had to be made from the random assignment to locations in order to accommodate the large number of refugees who were granted asylum (the so-called Bosnian programme, see Damm, 2009). For remaining refugee-sending, the policy guaranteed that early distributions across municipalities should genuinely be uncorrelated with economic trends. The correlation confirms this assumption, except for a significant (negative) correlation with labor supply of highly educated natives. If anything this would suggest location of refugees in municipalities with bad labor market conditions for highly skilled and would result in a downward biased estimates of the effects on highly skilled natives (which is not the main focus of our analysis).

Overall, these tests are consistent with the identifying assumption that our instrument only affects the outcomes of native workers in the municipality through its effect on the actual share of non-EU workers in the area.[37] We use the imputed non-EU share or refugee share of the labor force as an instrument in the spell regression (equation 4) and the change in the imputed non-EU share as the instrument in the difference-in-difference approach (equation 5 and 6).

Let us also emphasize that Aydemir and Borjas (2011) point out that this instrumental variable approach may not solve attenuation bias due to measurement error in the immigrant share, if a correlated measurement error is also present in the instrument. Aydemir and Borjas (2011) show that when calculations are based on one percent samples of the American census the bias can be large. The presence of fixed effects in the regression may worsen such a bias by identifying the coefficient on time differences only. Our data, however, are not subject to measurement error arising from sampling. In fact they include the *universe* of individuals and firms in Denmark. This allows us to use the full population to calculate the exact immigrant shares of each municipality limiting measurement error bias concerns.

[37] Figure A.1 and A.2 in Appendix shows the partial scatter plots of the 1991-1994 trend in outcomes on the 1994-2008 change in the instrument (thus plots corresponding to the relationships reported in Table 3) for the non-EU immigrants.

# 7 Results

## 7.1 Effects within Establishment and Municipality

Tables 4 and 5 show the 2SLS estimates of the effect of immigrants on natives within establishments and within municipalities, respectively. The corresponding OLS estimates are reported in Tables A.1 and A.2 in Appendix. The tables show only the estimates of the coefficient of interest $\beta$ from specifications (4). Each entry in the tables is an estimate from a different regression using different outcomes (listed as rows), and using the instrument based on all non-EU immigrants (Columns 1 and 3), or on refugees only (Columns 2 and 4).[38]

The first two columns identify the effects for natives without post-secondary education, that we call "low skilled". Columns three and four show the estimated effects for native workers with tertiary education ("high skilled"). We separate the analysis between the two skill groups because, as described in section 3, immigrants from non-EU countries, as a group, were more likely to compete with low-skilled Danish workers and to complement highly skilled Danish workers.

The structure of Tables 4 and 5 (and Tables A.1 and A.2 ) is the same. The first row shows the effects of an increase in non-EU immigrants by one percentage point of the labor force on the occupational complexity of native workers. The second, third and fourth rows report the estimated effects on the probability of a career upgrade, a career downgrade and a change in occupation. The fifth row reports the effects on the (logarithm of) hourly wages. The sixth row shows the effect on the (logarithm of) annual earning. The seventh row shows the effect on the fraction of the full-time year that the individual worked. The number of observations, the $F$-statistic, and the coefficient on the excluded instrument in the first stage regression appear in the last rows of the table. In parenthesis under the estimates we report the heteroskedasticity robust standard errors clustered at the municipality level to account for within municipality error correlation.

A tendency of immigrants to settle in areas with fast growing labor demand would generally produce an upward bias in the OLS estimated coefficients. However, as we consider non-EU immigrants doing manual-type of jobs that are potentially attracted by low housing costs, one may think that the correlation between the inflow of these groups and the economic conditions of a municipality can be negative, which would result in downward biased OLS estimates. In the specific case considered here,

[38]The extremely high dimensionality of the fixed effects $\gamma_{i,u}$ implies that the fixed effects estimator has to be implemented by performing a within-transformation. This is inconsequential since we are not interested in the fixed effects per se and hence we do not miss any relevant estimate.

the differences between the OLS estimates and 2SLS estimates show a downward bias of OLS which might suggest a negative correlation between the actual inflow and the contemporaneous labor market conditions.

The instrument is reasonably strong with a $F$-statistics of the first stage always above 20. Usually researchers consider a value of 10 as threshold below which one could incur in weak instrument problems (Stock and Yogo, 2005). Using non-EU immigrants or refugees to construct the imputed instrumental variable produce qualitatively similar effects. However the point estimates using the refugee instrument are usually larger. This could indicate that the refugees are a more homogenous group of workers focused in manual jobs, and thus more strongly complementary to natives. The more heterogeneous composition of workers in the non-EU group could produce an attenuated effect.

The first interesting result is that on average hourly wages, annual earnings and labor supply increase (not always by a statistically significant amount) in response to immigration for skilled and unskilled native workers, both within establishment and within municipality. Within establishment (Tables 4) both low skilled and high skilled exhibit some evidence of career upgrade, higher hourly wages and larger labor supply, especially in response to refugees. Within municipality (Table 5), including workers who change establishment, low skilled natives exhibit a large and significant shift towards occupational complexity. Among less skilled workers, those remaining within establishments seem to achieve the wage gains without specialization, while those moving between establishment show large occupational changes towards complex jobs. This could happen if natives who do not have to change establishment are those performing less manual intensive jobs that are less substitutable with immigrants. Those who are pushed to change establishments, instead, performed manual intensive jobs and moved towards more complex jobs to protect their wages. The ability to disentangle these responses allows us to identify these important differences between less skilled workers who do and do not change establishments.

High skilled workers increase their specialization towards complex occupations significantly less. Immigrants are likely to be complementary to high skilled natives and increase their wages and earnings even in the absence of occupational specialization of high skilled natives. This is reasonable as high skilled natives already perform production tasks quite different from immigrants. An interesting implication of our results is that, in general, immigration spurs occupational mobility of natives, including more career upgrade as well as more downgrade for those who move out of the establishment.

While on average this mobility rewards natives with higher wages and employment (though not always significant), it is also likely to increase the variance in performance of natives. Immigrants generate an opportunity for natives: those who take advantage of it by upgrading skills gain, while those who do not may lose.

Quantitatively the estimated effects are non trivial, but not unreasonably large. Municipalities exposed to above-average immigration experienced a growth of the non-EU share of employment 2 percentage points larger than the municipalities below-average. This translates, over the 1995-2008 period in 1.0% and 1.8% higher wages for low skilled and high skilled native workers, respectively, within an establishment (almost 4% looking at the refugee immigrants). If we consider the effect on all native workers in the municipality, including those who changed establishment, the average gain for less skilled is an insignificant 0.2% while high skilled gained 2.2% of their hourly wages (2.6% and 4.3% using the refugees). For comparison, the overall increase in average real wages in Denmark during the 1994-2008 period was 18 percentage points for less skilled workers and 19 percentage points for high skilled. One tenth of the wage gain of more educated during this period can be attributed to immigration.

Taken together these results suggest that non-EU immigrants encouraged low-skilled natives to take more complex occupations especially when they changed establishment. On average native wages increased in the local labor market, but the variance of native outcomes increased also and was driven by significant downgrade and upgrade among those who (involuntarily and voluntarily) changed establishment during the surge in the non-EU share in the municipality.

### 7.1.1 Differences Across Sectors

Pushing the analysis a step further, it is reasonable to think that the degree of complementarity and task specialization/upgrade available to natives in response to immigrants depends on their industry of employment. In industries producing differentiated goods or services and using a larger range of manual and complex abilities, the need for differentiated skills, and the complementarity across workers may be larger. In industries producing more homogenous goods and services, with limited varieties of skills, the opportunities for these gains from complementarity/diversity may be smaller. A second feature that could make workers and firms more responsive to immigration is their exposure to market pressures. Private sector workers should be able to move across occupations more easily

and firms would have stronger incentives to encourage efficient worker allocation and specialization, with stronger potential for the observed specialization/complementarity effects, especially in sectors were wages are bargained at the establishment level. Both mobility and decentralized bargaining were feature of the private sector. In the public sector, instead, workers' wage and firms' specialization may not respond actively to local complementarity as pay is centrally determined and natives simply defend their occupational status as insiders. .

To examine these differences we divide the economy in four broad sectors. The first is manufacturing, the sector producing goods, several of which can be highly differentiated and exposed to international competition. The second is non-complex services (utility, construction, wholesale, retail and hospitality services) producing non-tradable, local and manual-intensive services. The third is complex services (transport, telecommunication, finance, business and real estate) producing differentiated and more sophisticated, skill-intensive services. The last is the public sector (including mainly administration, health care, education and armed forces) whose wages and employment level may be much less responsive to the market and to productivity. Table 6 shows the effects of non-EU immigrants on native workers analyzing each of the four sectors defined above separately and only reporting occupational complexity and hourly wages as outcomes.

Consider first the effects on low skilled workers. The largest positive and significant effects on occupational complexity and hourly wages are experienced by native workers in the complex service sector. The magnitude of the effect is quite large: a one point increase in the non-EU immigrant percentage of the labor force produces an increase in native hourly wages between 1.7% and 2.1%, depending on whether we consider only within establishment or within municipality. Their mobility towards occupational complexity is similarly strong implying a growth by 2.6 to 4.3 percentage points in the complexity index for each increase of non-EU immigrants by one percentage of the labor force. Highly skilled natives in the complex service sector were also positively affected in their wages, without any effect on the complexity of their occupation, as expected.

Low-skilled natives in the non-complex service sector and manufacturing sector were much less affected; experiencing a significant effect only on hourly wages for workers in manufacturing when we considered within-municipality effects. High skilled natives experienced positive effects on wages in non-complex services, indicative of complementarity at work, and positive effects on the complexity of their occupations. Interestingly, low skilled native workers in the public sector were the only group

experiencing negative effects on their hourly wages. Probably because of the lack of mobility and job turnover in that sector. They did not respond to immigrants with any move towards more complex occupations. In the public sector immigration had a negative effect on complexity of tasks performed by skilled natives, the effects on hourly wages, however, was positive.

These results confirm the idea that the gains from complementarity and specialization are larger in complex, diversified sectors that respond to private incentives. In those sectors natives increased the complexity of their occupations in response to immigration, and the high skilled gained directly from complementarity with immigrants. Hence, both low and high skilled natives in complex industries are able to increase their marginal productivity in response to immigration. In sectors with less scope and no private incentives for differentiation (the public sector) natives do not move towards complex jobs and high skilled even decrease their progression towards more complex jobs. High skilled in the public sector still increase their marginal productivity due to their complementarity with immigrants, but the wages of low skilled in the public sector decrease due to competition effects from immigrants without adjustment.

The results of this section add several new findings to the literature. While it was known from Peri and Sparber (2009) that immigration can cause specialization and positive productivity effects for natives, we learn using individual data that occupational mobility of unskilled natives towards more complex jobs in response to immigrants takes place mainly across firms. It also increases wage dispersion such that some workers may experience significant downgrade while other experience upgrade with resulting zero or positive effects on the wage of an average unskilled worker. Specialization is strongest when movers across firms are considered. The gains from specialization offset the loss of firm-specific human capital. We also learn that the positive effects are stronger in sectors producing complex differentiated goods and services and follow market incentives. As in D'Amuri and Peri (forthcoming) and Angrist and Kugler (2003) this seems to support the idea that mobility and flexibility are important characteristics for the firms and workers to earn productivity dividends from immigration.

### 7.2 Transitions in the Difference-in-Difference Approach

The whole trajectory of the difference in outcomes between three years before and fourteen years after the surge in the immigrant share (1994) based on the specification in equation (5) are shown in Figure

6.[39] As usual we separate the effects on more and less educated natives and show the trajectory of four different outcomes: occupation complexity, hourly wage, annual earning and fraction of year worked. The figure show three important results. First, except for hourly wages of highly educated, which show a slight upward trend before 1994, there is no sign of a pre-event trend in the other differences in outcomes between treated and control municipalities. This is reassuring and it confirms that after controlling for individual characteristics, constant and time-varying fixed effects there was no systematic difference in the trend of wage, employment and occupational complexity of natives before 1994 between high- and low-immigration municipalities.

Second, confirming the within-spell regressions, we find clear evidence that both more and less educated native workers moved, slowly but steadily, towards more complex occupations in response to high non-EU immigration. Fourteen years after 1994 (1994 is denoted as year 0 in the graph) natives in high immigration municipalities had moved to more complex jobs resulting in a significant effect equal to 3 points of the complexity index (see Table A.3 and A.4 in Appendix). This corresponds to a small but significant change of the complexity of an occupation, equal to 4 percent of a standard deviation in the complexity index in the Danish population.

Third, in part as a consequence of this occupational move there is also evidence of a positive effect on hourly wages of less educated in the medium run (3 to 9 years after the beginning of the event), while in the long run the effect is less clear. No significant effect on employment, measured as fraction of year worked, of either group is found in the short and medium run. Towards the end of the response period (after 10 years) a small, barely significant effect on labor supply (positive for high skilled and negative for low skilled) appears to arise. However, as we will see below, these effects are mainly due to older workers who 11 to 14 years after the event might go on early retirement.[40]

These results confirm some findings of the spell regression and at the same time are the first results in this literature, to the best of our knowledge, obtained by following over time (and across municipalities) a cohort of individuals working in municipalities with high or low exposure to immigrants. Hence, this is the first time that we can track the actual workers exposed to an exogenous change in competition from immigrants and measure the impact on their wages, specialization and employment over time. These estimates cannot be driven by changes in composition or selection out of the municipality as the composition of the group is kept constant. They confirm a clear result revealed in

[39] Table A.3 and A.4 in Appendix report the estimates for selected years before and after the surge in immigration.

[40] Effect of immigrants on early retirement can be an additional outcome to analyze. We leave it for future research.

the spell regression, that natives moved to complex occupations in response to immigrants and that wages increased or remained unchanged, and add to this finding that natives were not displaced out of employment.

The magnitudes of the positive effects estimated for the medium to long run in Table A.3 (5 to 9 years) are larger than those estimated in the spell regression for the low skilled but similar for the high skilled. After nine years from 1994 the difference in share of non-EU immigrants between treated and non-treated municipalities was about 1.25% of the labor force. The effect on the wage of less skilled was a positive 1.1 percentage points and for the high skilled was a (non-significant) 0.7 percentage points. This implies and elasticity of 0.9 and 0.6 (respectively for low and high skilled), while the within municipality estimates of those elasticities in Table 5 were 0.1 and 1.1. This suggests that those who changed municipality were differently selected among the low and high skilled, and that the contemporaneous effects estimated in the spell regressions can be different from the long-run effect on all workers. The wage effects for high skilled seem more modest in the long run, when considering also those workers who changed municipality.[41] For the low skilled, instead, their hourly wages slowly increased in response to immigration when considering all workers, revealing positive medium and long run effects.

Analyzing the full transition for less educated workers (Figure 6) we see how the long-run effects accrue over time. In particular we can observe a progressive increase in the occupational complexity, faster in the first five years after the shock. Hourly wages also climb in the first five years and then stabilizes to a permanently higher level. At the same time, we do not observe any significant change in labor supply in the first 9 years after the event. Only towards the very end a slight decline (barely significant) may be due to early retirement behavior (as we will discuss below the effect is driven by older people). Patterns are similar for highly educated, with positive and occasionally significant effects on hourly wages and employment and a progressive and significant increase in the occupational complexity. Towards the end of the period there is an increase in labor supply for highly skilled in treated municipalities, and again it may have to do with their retirement behavior. Overall, there is no evidence of negative effects from displacement, wage competition and loss of specific capital, when we consider all workers exposed to immigrant competition. Low skilled slowly move towards more complex tasks thereby raising their productivity and wage. This margin of response, considering all

[41] We should be cautious, however, as the difference for high skilled is not significant if we consider the standard errors. Moreover we found a pre-1994 trend for high skilled wages which can pollute the estimates.

workers seems to show a more beneficial effect in the long run.

### 7.2.1 Transitions for Different Groups

To complete the picture of the native labor market transitions following the non-EU immigration surge we consider two further partitions of the native labor force (besides the usual split into more and less educated). First, we consider young and old workers, namely those who were 21 to 36 years old in 1994 and those 37 to 51. All those workers were still below the statutory retirement age (65) as of 2008. The older workers (aged 46-51 in 1994) turn 60 within the last years of the transitions and thereby become eligible for early retirement pension ("efterløn"). The second dimension we consider is the tenure of workers in the establishment as of 1994. We call "low tenure" those workers with less than average tenure (4.35 years) and "high tenure" those with more than 4.35 years of tenure at the establishment, at the time of the beginning of the immigration boom. In both cases we can expect the group of young, low-tenure workers to have lower costs and more opportunities to upgrade and change their occupation. If the opportunity of wage gains from immigration is in part linked to the ability of upgrading and increasing one's occupational complexity, then low tenure, young workers should be better positioned to take advantage of it.[42]

Figures 7 and 8 show the transitions of the usual four outcomes (occupational complexity, hourly wage, annual earnings and fraction of year worked) separately for old and young workers (still separating high and low skilled). Figures 9 and 10 show the split between outcomes of high and low tenure native workers. The results are as expected. For less skilled natives Figures (7 and 9) show that the low-tenure workers are those who respond to immigration with stronger move towards higher occupational complexity in treated municipalities. This implies larger hourly wage gains for them. Young low skilled workers have also larger hourly wage gains, relative to old low skilled workers in treated municipalities. The labor supply of young low skilled workers does not respond significantly in treated municipalities, nor does the labor supply of old low skilled workers, except in the last 3-4 years when a decline in the treated municipalities may be due to early retirement behavior.

The reallocation towards more complex jobs is less noisily estimated for high skilled workers (shown in Figures 8 and 10). Young and low-tenure high skilled workers experience more significant mobility towards occupational complexity in treated versus untreated regions compared to similar low skilled.

[42] We also examined the transitions by sector of employment in 1994 (Figures A.3 and A.4 in Appendix). These results confirmed our findings in the spell regressions.

Whether this translates into higher wages as for the low skilled is harder to establish though, since wages for young and low-tenure high skilled workers exhibit a bit of a pre-trend. Older and high-tenured ones, have a smaller increase in occupational complexity and no significant effect on hourly wages and earnings.[43]

Separating between groups also shows that the decline observed in labor supply after 9 years from the event is mainly due to older worker and hence possibly driven by early retirement behavior. It is possible that the only long-run displacement effect of immigrants on less educated natives is to push some of them into early retirement. Overall, the largest benefit from immigration accrue to young, less experienced workers who can direct their careers towards more complex occupations, complementary to immigrant skills. Their upgrade may imply some further training, but it does not need to come at the expenses of labor supply.[44]

### 7.3 Cumulated Effects in the Difference-in-Difference Approach

Table 7 (low skilled) and Table 8 (high skilled) report the estimated effects of an increase in non-EU immigrants by one percentage point of the labor force on cumulated variables (over the 14 years). Those estimates are based on equation (6). The first line reports the impact on employment including all sectors (column 1), and then in turn considering natives initially in manufacturing, non-complex services, complex services and the public sector (column 2-5). The following rows produce estimates of the increased (decreased) length of employment in the same (and new) establishment, in the same (and new) sector and in the same (and new) municipality in response to non-EU immigration increases by one percentage point of the labor force. Then we show the effects on the length of cumulated unemployment and self-employment.

The estimated coefficient in the first column and row of Table 7 implies that less educated native workers in municipalities receiving an increase in non-EU immigrants equal to one percentage point of the labor force experienced a non-significant decline in cumulated employment (over fourteen years) by five percent of one work-year, namely two working weeks. A high skilled native also experienced an

[43] In the Figure A.5 and A.6 in Appendix we show the split in transition between men and women. The strongest positive effects on complexity and wages are for men.

[44] In a further analysis (shown in Figure A.7 of the Appendix), we find that the probability of low skilled obtaining a higher degree increases by 2 percentage points given a one percentage point increase in the non-EU immigration share of the municipality, and that the effect accrued mainly in the early years of the immigration boom. The effect was driven by vocational education which is often organized as training programmes that allow workers to obtain formal competencies on the job. No significant effects on educational upgrading are found for high skilled or older workers.

insignificant change to their cumulative employment over the fourteen year period (Table 8).[45] Hence, non-EU immigration did not have any significant effect overall on cumulative employment of native individuals. Similarly, immigration did not affect the cumulative time spent as unemployed either for low or for high skilled natives. In Table 8 we see that immigration actually decreased the probability of high skilled to become self-employed, while it did not have any significant effect on probability of self-employment of less skilled.

Even more interesting is to consider the effect of immigration on cumulative employment in the same establishment, in the same sector and in the same municipality. For highly educated natives immigration increased the time spent in a new establishment and municipality and decreased the time spent in the original one. For low skilled this effect is significant only for the municipality. On average highly educated natives spent six weeks less in the same establishment over the following 14 years, for each increase of non-EU immigrants by one percent point of the workforce. Similarly, they spent 15 working weeks less in the original municipality and 15 weeks more in a new one during the 14 years, if the original municipality experienced an increase in non-EU immigrants by one percentage point of the labor force. The effects were smaller, but very significant in terms of municipality switching, also for less educated natives. Hence cross-municipality mobility of natives was positively affected by non-EU immigration. Cross-sector mobility was not much affected by immigration, except for workers in the manufacturing sector who moved out of municipalities with high immigration earlier, while workers in the complex service sector remained longer in their original sector when experiencing higher immigration rates. Immigration, therefore, was associated with a movement of the native labor force away from manufacturing and into complex services. This is consistent with the findings of section 7.1 and 7.2 that natives move towards more complex tasks in response to immigration. These moves are likely to be associated with wage and earnings gains that may offset and reverse the cost of moving across establishments and sectors due to loss of specific human capital.

Overall immigration seems to increase the churning of jobs and generate a tendency of moving towards more complex jobs, a higher tendency to moving out of the establishment and out of the municipality and out of the manufacturing sector into more complex and differentiated industries.[46] Most of these changes are associated to upgrades and better opportunity, rather than to displacement

[45] We are using 46 weeks as the usual full-time work-year for a Danish worker.

[46] This effect is also consistent with a potential job-creating effect of immigrants that increases the job finding rates for natives, as illustrated by Chassamboulli and Palivos (2014).

and loss of skills, as they may generate increases in wages and yearly income. The probability of being employed or unemployed was not affected.

# 8 Discussion and Conclusions

In this paper we have used a unique source of individual and firm data during a period that contains a sustained and supply-driven boom of non-EU immigrants to Denmark. We estimate the short- and long-run effects of this boom on native occupations, wages, and employment. The fact that our data allows us to follow every single worker in Denmark and the high quality of the register information imply high reliability. It also implies that we can analyze immigration's effects on workers who remained within the original establishment as well as those who left establishment and municipality. We can also estimate the effects of immigration on mobility of workers across establishments, municipalities, and in and out of employment. Lastly, we exploit a quasi-experiment where we observe a pre-period in which Danish municipalities essentially saw no change in their non-EU immigrant share, followed by a period of large inflows of non-EU refugees to Denmark that were driven by political and economic crises in sending countries. Importantly, the Danish municipalities where such refugees ended up in were exogenously determined by randomized government dispersal policies, and by immigrant preferences to locate in areas with pre-existing immigrant enclaves.

We find robust evidence that native workers, especially less skilled, within and across municipalities responded to immigration increasing significantly their mobility towards more complex occupations. Immigration also increased mobility of natives across firms and out of the municipality. We do not observe an increased probability of unemployment, nor a decrease in employment. Hourly wages of less educated natives were on average positively affected by immigration, the effect increases as the low skilled gradually moved towards more complex occupations.

We think that this analysis is much richer and detailed than ever done before in that it analyzes individual responses of natives to immigrants within and across firm and local labor markets. We produces a much more detailed picture of the impacts of immigration by tracking occupations, careers, wages and employment of natives in response to immigrants. We also show the importance of looking at the dynamic adjustment mechanisms for native workers and looking at individuals in a municipality as well as to include those who (endogenously) may leave over time. We hope that the future analysis of the impact of immigration in several other countries may follow the detail and the approach adopted

in this paper.

## References


Altonji, Joseph G. and David Card. 1991. "The Effects of Immigration on the Labor Market Outcomes of Less-skilled Natives. Chapther 7 in M. J. Abowd and R. B. Freeman (Eds.)." *Immigration, Trade and the Labor Market* (Chicago: University of Chicago Press):201–234.

Angrist, Joshua D. and Adriana D. Kugler. 2003. "Protective or counter-productive? labour market institutions and the effect of immigration on EU natives." *The Economic Journal* 113 (488):302–331.

Autor, David H., David Dorn, Gordon H. Hanson, and Jae Song. 2013. "Trade Adjustment: Worker Level Evidence." *NBER Working Paper* No. 19226:Cambridge, MA.

Aydemir, Abdurrahman and George J. Borjas. 2011. "Attenuation Bias in Measuring the Wage Impact of Immigration." *Journal of Labor Economics* 29 (1):69–112.

Blau, Francine and Lawrence Kahn. 2012. "Immigration and the Distribution of Incomes." *NBER Working Paper* No. 18515:Cambridge, MA.

Borjas, George J. 2003. "The Labor Demand Curve Is Downward Sloping: Reexamining the Impact of Immigration on the Labor Market." *Quarterly Journal of Economics* 118 (4):1359–1374.

Card, David. 2001. "Immigrant Inflows, Native Outflows and the Local Labor Market Impacts of Higher Immigration." *Journal of Labor Economics* 19 (1):22–64.

Cattaneo, Cristina, Carlo V. Fiorio, and Giovanni Peri. 2013. "What Happens to the Careers of European Workers When Immigrants "Take Their Jobs"?" *IZA Discussion Paper* (No. 7282).

Chassamboulli, Andri and Theodore Palivos. 2014. "A Search-Equilibrium Approach to the Effects of Immigration on Labor Market Outcomes." *International Economic Review* 55 (1):111–129.

Cohen-Goldner, Sarit and M. Daniele Paserman. 2011. "The dynamic impact of immigration on natives' labor market outcomes: Evidence from Israel." *European Economic Review* 55 (8):1027–1045.

Dahl, Christian M., Daniel le Maire, and Jakob R. Munch. 2013. "Wage Dispersion and Decentralization of Wage Bargaining." *Journal of Labor Economics* 31 (3):501–533.

Damm, Anna P. 2009. "Determinants of recent immigrants' location choices: quasi-experimental evidence." *Journal of Population Economics* 22 (1):145–174.

Damm, Anna P. and Christian Dustmann. forthcoming. "Does Growing Up in a High Crime Neighborhood Affect Youth Criminal Behavior?" *American Economic Review* .

D'Amuri, Francesco, Gianmarco I.P. Ottaviano, and Giovanni Peri. 2010. "The Labor Market Impact of Immigration in Western Germany in the 1990's." *European Economic Review* 55 (4):550–570.

D'Amuri, Francesco and Giovanni Peri. forthcoming. "Immigration, Jobs and Labor Market Institutions: Evidence from Europe." *Journal of the European Economic Association* .

Dustmann, Christian and Albrecht Glitz. 2011. "How Do Industries and Firms Repond to Changes in Local Labor Supply." *CReAM Discussion Paper* No. 18/11.

Friedberg, Rachel M. 2001. "The Impact of Mass Migration on the Israeli Labor Market." *Quarterly Journal of Economics* 116 (4):1373–1408.

Hummels, David, Rasmus Jørgensen, Jakob Roland Munch, and Chong Xiang. forthcoming. "The Wage Effects of Offshoring: Evidence from Danish Matched Worker-Firm Data." *American Economic Review* .

Kerr, William R. and William F. Lincoln. 2010. "The Supply Side of Innovation: H-1B Visa Reforms and US Ethnic Invention." *Journal of Labor Economics* 28 (3):473–508.

Lewis, Ethan. 2011. "Immigration, Skill Mix, and Capital-Skill Complementarity." *Quarterly Journal of Economics* 126 (2):1029–1069.

———. 2013. "Immigration and Production Technology." *Annual Review of Economics* 5 (6):165–191.

Longhi, Simonetta, Peter Nijkamp, and Jacques Poot. 2005. "A Meta-Analytic Assessment of the Effect of Immigration on Wages." *Journal of Economic Surveys, Wiley Blackwell* 19 (3):451–477.

Malchow-Møller, Nikolaj, Jakob Roland Munch, Claus Aastrup Seidelin, and Jan Rose Skaksen. 2013. "Immigrant Workers and Farm Performance: Evidence from Matched Employer-Employee Data." *American Journal of Agricultural Economics* 95 (4):819–841.

Malchow-Møller, Nikolaj, Jakob Roland Munch, and Jan Rose Skaksen. 2011. "Do Foreign Experts Increase the Productivity of Domestic Firms?" *IZA Discussion Paper* (No. 6001):1–44.

———. 2012. "Do Immigrants Affect Firm-Specific Wages?" *The Scandinavian Journal of Economics* 114 (4):1267–1295.

Manacorda, Marco, Alan Manning, and Jonathan Wadsworth. 2012. "The Impact of Immigration on the Structure of Wages: Theory and Evidence from Britain." *Journal of the European Economic Association* 10 (1):120–151.

Olney, William W. 2013. "Immigration and Firm Expansion." *Journal of Regional Science* 53 (1):142–157.

Ortega, Francesc and Giovanni Peri. 2013. "Migration, Trade and Income." *IZA Discussion Paper* No. 7325:1–42.

Ottaviano, Gianmarco I. P. and Giovanni Peri. 2005. "Cities and Cultures." *Journal of Urban Economics* 58 (2):304–307.

———. 2012. "Rethinking the Effect of Immigration on Wages." *Journal of the European Economic Association* 10 (1):152–197.

Ottaviano, Gianmarco I. P., Giovanni Peri, and Greg C. Wright. 2013. "Immigration, Offshoring and American Jobs." *American Economic Review* 103 (5):1925–1959.

Parrotta, Pierpaolo, Dario Pozzoli, and Mariola Pytlikova. 2012. "Does labor diversity affect firm productivity?" *IZA Discussion Paper* No. 6973:1–31.

Peri, Giovanni. 2012. "The Effect of Immigration on Productivity: Evidence from U.S. States." *Review of Economics and Statistics* 94 (1):348–358.

Peri, Giovanni and Chad Sparber. 2009. "Task Specialization, Immigration and Wages." *American Economic Journal: Applied Economics* 1 (3):135–169.

Schmidt, Torben Dall and Peter Sandholt Jensen. 2012. "Social networks and regional recruitment of foreign labour: Firm recruitment methods and spatial sorting in Denmark*." *Papers in Regional Science* 91 (4):795–821.

———. 2013. "Foreign labor and regional labor markets: aggregate and disaggregate impact on growth and wages in Danish regions." *The Annals of Regional Science* 50 (3):809–840.

Stock, James H. and Motohiro Yogo. 2005. "Testing for weak instruments in linear IV regression. Chapter 5 in D. W. K. Andrews and J. H. Stock (Eds.)." *Identification and Inference in Econometric Models: Essays in Honor of Thomas J. Rothenberg* (Cambridge: Cambridge University Press).

Von Wachter, Till, Jae Song, and Joyce Manchester. 2007. "Long-Term Earnings Losses due to Job Separation During the 1982 Recession: An Analysis Using Longitudinal Administrative Data from 1974 to 2004." *Columbia University, Department of Economics, Discussion Paper* No. 0708-16.

Walker, Reed. 2013. "The Transitional Costs of Sectoral Reallocation: Evidence from the Clean Air Act and the Workforce." *Quarterly Journal of Economics* 128 (4).

Figure 1: Foreign born share in Denmark, 1991-2008

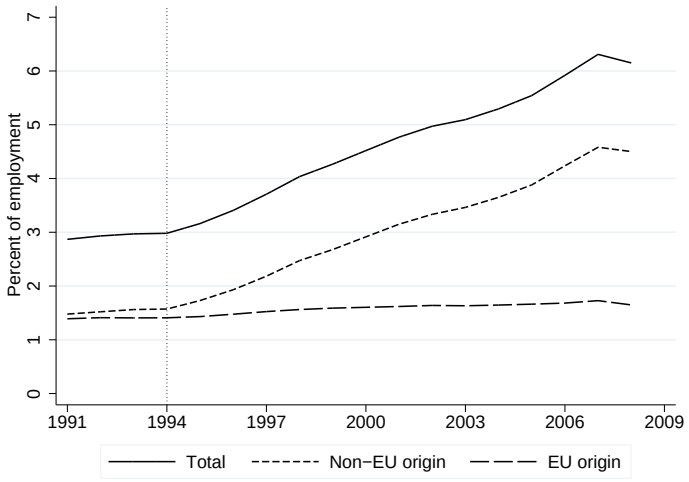
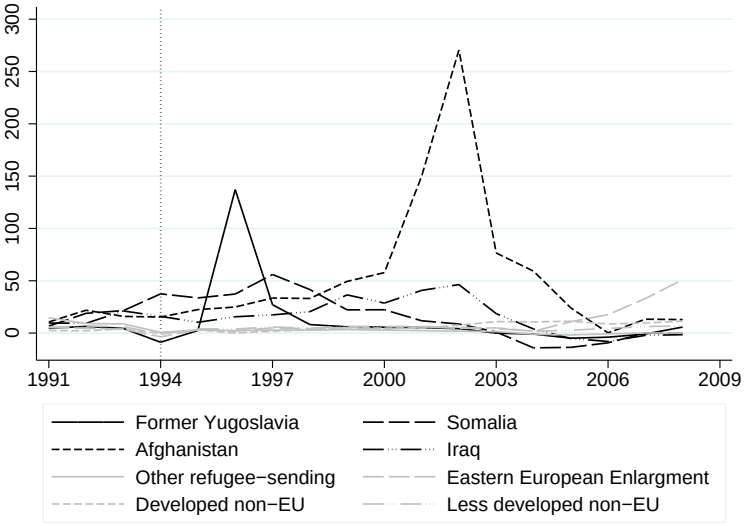



Figure 2: Drivers of non-EU immigration growth, 1991-2008




Notes: Annual inflows in percent of populations in 1994.

Figure 3: Decomposed non-EU share in Denmark, 1991-2008

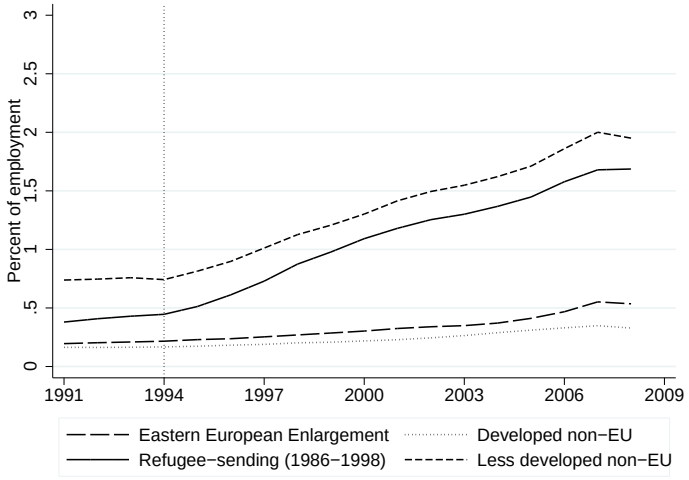
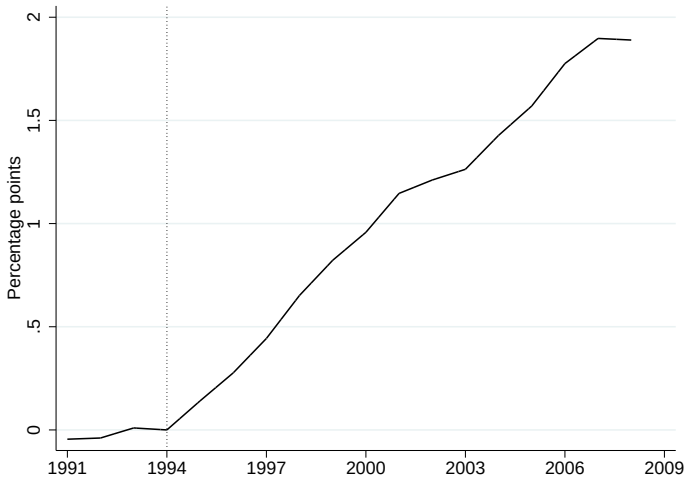
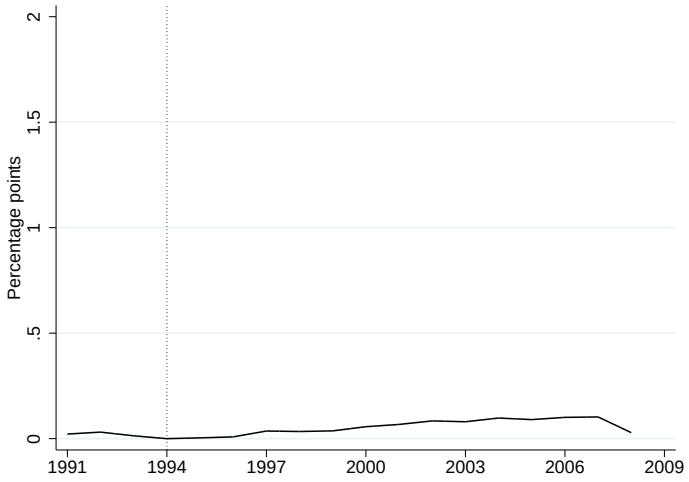


Figure 4: Differential trend in non-EU share of employment




Notes: Difference in actual non-EU share of employment for employed natives above versus below the median of the 1994-2008 difference in predicted non-EU share, normalized to zero in 1994.

Figure 5: Differential trend in EU share of employment




Notes: Difference in actual EU share of employment for employed natives above versus below the median of the 1994-2008 difference in predicted non-EU share, normalized to zero in 1994.

Table 1: Skill content of occupations and their non-EU inflow between 1994-2008

| | Non-EU share | Skill content of occupation | | | |
|---|---|---|---|---|---|
| | 1994-2008 dif. | Cognitive | Communication | Manual | Complexity |
| *Lowest inflow* | | | | | |
| Managers of small enterprises | -0.018 | 0.666 | 0.677 | 0.432 | 1.136 |
| Legislators and senior officials | 0.002 | 0.897 | 0.989 | 0.303 | 1.828 |
| Corporate managers | 0.003 | 0.796 | 0.796 | 0.367 | 1.488 |
| Armed forces | 0.003 | 0.441 | 0.390 | 0.633 | 0.225 |
| Skilled agricultural and fishery workers | 0.007 | 0.362 | 0.248 | 0.736 | -0.328 |
| *Highest inflow* | | | | | |
| Drivers and mobile plant operators | 0.039 | 0.352 | 0.265 | 0.810 | -0.322 |
| Laborers in mining, construction, mfr. and transport | 0.045 | 0.215 | 0.156 | 0.769 | -0.783 |
| Machine operators and assemblers | 0.057 | 0.276 | 0.146 | 0.790 | -0.655 |
| Other elementary occupations | 0.087 | 0.260 | 0.205 | 0.742 | -0.633 |
| Sales and services elementary occupations | 0.148 | 0.126 | 0.103 | 0.695 | -1.234 |

Notes: The skill content of each occupational grouping (2-digit ISCO) is the population weighted average of the underlying occupations (4-digit ISCO).

Table 2: Summary statistics for spell-sample

| | Low skilled | | | | High skilled | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | S.d. | Min | Max | Mean | S.d. | Min | Max |
| Age | 38.17 | 12.26 | 18.00 | 65.00 | 43.28 | 9.95 | 18.00 | 65.00 |
| Labor market experience | 15.03 | 10.13 | 0.00 | 45.00 | 19.39 | 9.31 | 0.00 | 45.00 |
| Job tenure | 4.29 | 5.52 | 0.00 | 28.00 | 5.63 | 6.23 | 0.00 | 28.00 |
| Married | 0.48 | 0.50 | 0.00 | 1.00 | 0.63 | 0.48 | 0.00 | 1.00 |
| Education, primary | 0.64 | 0.48 | 0.00 | 1.00 | 0.00 | 0.00 | 0.00 | 1.00 |
| secondary | 0.15 | 0.36 | 0.00 | 1.00 | 0.00 | 0.01 | 0.00 | 1.00 |
| vocational | 0.16 | 0.37 | 0.00 | 1.00 | 0.57 | 0.50 | 0.00 | 1.00 |
| higher | 0.04 | 0.21 | 0.00 | 1.00 | 0.43 | 0.50 | 0.00 | 1.00 |
| Region, Northern Jytland | 0.11 | 0.32 | 0.00 | 1.00 | 0.10 | 0.30 | 0.00 | 1.00 |
| Central Jytland | 0.23 | 0.42 | 0.00 | 1.00 | 0.23 | 0.42 | 0.00 | 1.00 |
| Southern Denmark | 0.23 | 0.42 | 0.00 | 1.00 | 0.21 | 0.41 | 0.00 | 1.00 |
| Greater Copenhagen Area | 0.27 | 0.45 | 0.00 | 1.00 | 0.31 | 0.46 | 0.00 | 1.00 |
| Zealand | 0.15 | 0.36 | 0.00 | 1.00 | 0.15 | 0.36 | 0.00 | 1.00 |
| Agriculture, fishing and quarrying | 0.03 | 0.16 | 0.00 | 1.00 | 0.01 | 0.10 | 0.00 | 1.00 |
| Manufacturing | 0.23 | 0.42 | 0.00 | 1.00 | 0.17 | 0.38 | 0.00 | 1.00 |
| Electricity, gas and water supply | 0.01 | 0.07 | 0.00 | 1.00 | 0.01 | 0.09 | 0.00 | 1.00 |
| Construction | 0.09 | 0.28 | 0.00 | 1.00 | 0.06 | 0.24 | 0.00 | 1.00 |
| Wholesale and retail sale, hotels and rest. | 0.18 | 0.38 | 0.00 | 1.00 | 0.14 | 0.34 | 0.00 | 1.00 |
| Transport, post and telecommunications | 0.10 | 0.30 | 0.00 | 1.00 | 0.05 | 0.23 | 0.00 | 1.00 |
| Finance and business activities | 0.09 | 0.29 | 0.00 | 1.00 | 0.14 | 0.34 | 0.00 | 1.00 |
| Public and personal services | 0.28 | 0.45 | 0.00 | 1.00 | 0.42 | 0.49 | 0.00 | 1.00 |
| Occupational complexity | 0.13 | 0.90 | -2.69 | 2.11 | 0.66 | 0.81 | -2.69 | 2.11 |
| ln(Hourly wagerate) | 5.03 | 0.38 | 0.13 | 9.17 | 5.24 | 0.35 | -0.17 | 10.01 |
| ln(Annual earnings) | 12.33 | 0.50 | 7.05 | 16.97 | 12.60 | 0.44 | 4.20 | 17.96 |
| Fraction of year worked | 0.92 | 0.17 | 0.00 | 1.00 | 0.95 | 0.13 | 0.00 | 1.00 |
| Observations | 1787910 | | | | 3154753 | | | |

Notes: Employed natives 1995-2008. High/low skilled is defined as the individual enters the panel. Some low skilled upgrade their education level while at the labor market (16% that start out with no post-secondary education obtain a vocational education and 5% obtain a higher education). Native-municipality combinations that are singletons are dropped, since they would not contribute to any of the spell-regressions because all spells are nested within municipalities.

Table 3: Instrument power and correlation with pre-trends in native outcomes

| | 1991-1994 difference in average | | | | 1994-2008 dif. |
|---|---|---|---|---|---|
| | Occupational complexity | Hourly wage | Annual earnings | Fraction of year worked | in actual share |
| | | | *Non-EU* | | |
| *Low skilled* | | | | | |
| 1994-2008 dif. in instrument | 0.277 | -0.091 | 0.325 | 0.029 | 0.519*** |
| | (0.269) | (0.155) | (0.401) | (0.130) | (0.122) |
| $F$-statistic instrument | 1.06 | 0.34 | 0.66 | 0.05 | 17.98 |
| Observations | 97 | 97 | 97 | 97 | 97 |
| R-squared | 0.37 | 0.64 | 0.44 | 0.81 | 0.71 |
| *High skilled* | | | | | |
| 1994-2008 dif. in instrument | 0.107 | 0.127 | 0.074 | -0.158 | 0.574*** |
| | (0.123) | (0.075) | (0.176) | (0.083) | (0.116) |
| $F$-statistic instrument | 0.76 | 2.85 | 0.18 | 3.67 | 24.42 |
| Observations | 97 | 97 | 97 | 97 | 97 |
| R-squared | 0.69 | 0.74 | 0.47 | 0.81 | 0.72 |
| | *Refugees subject to Dispersion Policy 1986-1998.* | | | | |
| *Low skilled* | | | | | |
| 1994-2008 dif. in imputed share | -0.344 | 0.273 | 1.361 | 0.160 | 0.409*** |
| | (0.622) | (0.357) | (0.914) | (0.299) | (0.053) |
| $F$-statistic instrument | 0.31 | 0.59 | 2.22 | 0.29 | 59.47 |
| Observations | 97 | 97 | 97 | 97 | 97 |
| R-squared | 0.37 | 0.64 | 0.45 | 0.81 | 0.87 |
| *High skilled* | | | | | |
| 1994-2008 dif. in imputed share | 0.148 | 0.157 | -0.228 | -0.593** | 0.483*** |
| | (0.277) | (0.170) | (0.393) | (0.176) | (0.056) |
| $F$-statistic instrument | 0.29 | 0.85 | 0.34 | 11.31 | 73.61 |
| Observations | 97 | 97 | 97 | 97 | 97 |
| R-squared | 0.69 | 0.73 | 0.47 | 0.83 | 0.84 |

Notes: Each regressions is at the municipality level and weighted by the size of the labor force in the municipality. The table shows correlation of instrument with pre-trends in native outcomes and with actual change in foreign born share. Controls not shown are those listed in Table 2 averaged for each municipality in 1994. Refugees from the Former Yugoslavia are excluded from the refugee-group since they constitute an exemption from the random spatial dispersion.

Table 4: Within worker-establishment spell regressions (2SLS)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Low skilled | | High skilled | |
| | Non-EU | Refugee | Non-EU | Refugee |
| Occupational complexity | 0.544 | 1.039 | 0.105 | 0.618 |
| | (0.302) | (0.688) | (0.167) | (0.475) |
| Career upgrade | 0.468** | 0.893 | 0.478** | 1.272*** |
| | (0.175) | (0.563) | (0.185) | (0.341) |
| Career downgrade | 0.106 | 0.245 | 0.088 | 0.441* |
| | (0.091) | (0.396) | (0.065) | (0.198) |
| Occupational mobility | 0.574* | 1.138 | 0.565** | 1.712*** |
| | (0.227) | (0.844) | (0.208) | (0.439) |
| Hourly wage | 0.508* | 1.816*** | 0.911** | 2.049*** |
| | (0.222) | (0.442) | (0.282) | (0.550) |
| Annual earnings | 0.603* | 1.960*** | 0.964** | 1.459* |
| | (0.271) | (0.512) | (0.318) | (0.587) |
| Fraction of year worked | 0.314*** | 0.862*** | 0.126 | 0.136 |
| | (0.093) | (0.219) | (0.085) | (0.146) |
| Observations | 1,541,654 | 1,541,654 | 2,883,266 | 2,883,266 |
| First stage $F$-statistic | 26.12 | 55.38 | 30.96 | 61.72 |
| First stage coefficient | 0.401*** | 0.362*** | 0.416*** | 0.382*** |
| | (0.078) | (0.049) | (0.075) | (0.049) |

Notes: *** p< 0.001, ** p< 0.01, * p< 0.05. Each entry of the table is the coefficient on the explanatory variable of interest in equation (4) using a sample of employed natives between 1995 and 2008. The dependent variables (left column) have the same first stage except for occupational complexity that has fewer observations (some missings). Control variables not shown are: age, experience, tenure, (each of those squared), marital status, education, region by year and industry by year dummies (listed in Table 2). Standard errors in parentheses and $F$-statistic for significance of excluded instrument are clustered by municipality.

Table 5: Within worker-muncipality spell regressions (2SLS)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Low skilled | | High skilled | |
| | Non-EU | Refugee | Non-EU | Refugee |
| Occupational complexity | 2.556** | 4.455** | 0.448*** | 1.661*** |
| | (0.873) | (1.623) | (0.133) | (0.413) |
| Career upgrade | 0.520** | 1.085* | 0.481** | 1.091*** |
| | (0.173) | (0.468) | (0.150) | (0.249) |
| Career downgrade | 0.538*** | 1.139** | 0.461*** | 1.217*** |
| | (0.130) | (0.431) | (0.118) | (0.349) |
| Occupational mobility | 1.058*** | 2.223** | 0.942*** | 2.308*** |
| | (0.285) | (0.843) | (0.263) | (0.543) |
| Hourly wage | 0.078 | 1.313* | 1.095** | 2.173** |
| | (0.309) | (0.523) | (0.389) | (0.721) |
| Annual earnings | 0.513 | 1.829*** | 0.999* | 1.430* |
| | (0.277) | (0.539) | (0.392) | (0.671) |
| Fraction of year worked | 0.479*** | 1.055*** | 0.079 | 0.165 |
| | (0.114) | (0.191) | (0.082) | (0.117) |
| Observations | 1,787,910 | 1,787,910 | 3,154,751 | 3,154,751 |
| First stage $F$-statistic | 24.32 | 51.05 | 28.66 | 56.98 |
| First stage coefficient | 0.414*** | 0.397*** | 0.429*** | 0.408*** |
| | (0.084) | (0.056) | (0.080) | (0.054) |

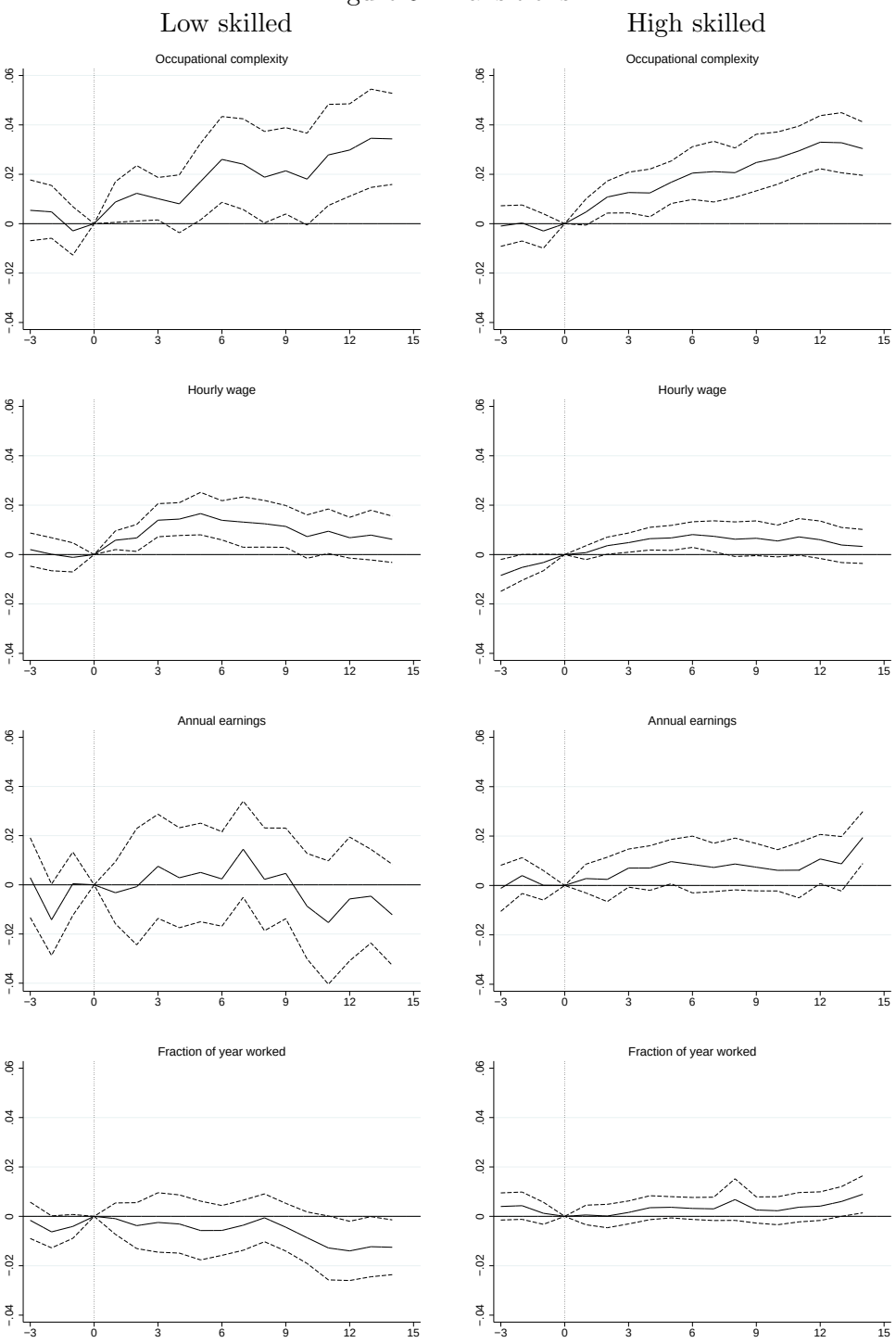
Notes: *** p< 0.001, ** p< 0.01, * p< 0.05. Each entry of the table is the coefficient on the explanatory variable of interest in equation (4) using a sample of employed natives between 1995 and 2008. The dependent variables (left column) have the same first stage except for occupational complexity that has fewer observations (some missings). Control variables not shown are: age, experience, tenure, (each of those squared), marital status, education, region by year and industry by year dummies (listed in Table 2). Standard errors in parentheses and $F$-statistic for significance of excluded instrument are clustered by municipality.

Table 6: Spell regressions (2SLS) by sector

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Within worker-establishment | | Within worker-municipality | |
| | Low | High | Low | High |
| | | *Manufacturing* | | |
| Occupational complexity | 0.489 | 1.234* | 1.194 | 1.538** |
| | (0.442) | (0.578) | (0.736) | (0.580) |
| Hourly wage | 0.864 | 0.771 | 1.364* | 0.528 |
| | (0.579) | (0.395) | (0.608) | (0.360) |
| Observations | 408,153 | 536,893 | 443,500 | 568,319 |
| First stage $F$-statistic | 38.75 | 37.65 | 41.93 | 40.45 |
| | | *Non-complex services* | | |
| Occupational complexity | 0.602 | 0.941 | 2.096 | 1.560*** |
| | (0.502) | (0.535) | (1.114) | (0.430) |
| Hourly wage | 0.417 | 0.958* | -0.561 | 0.896* |
| | (0.419) | (0.420) | (0.556) | (0.386) |
| Observations | 399,130 | 582,887 | 460,766 | 636,455 |
| First stage $F$-statistic | 20.89 | 26.37 | 20.69 | 23.60 |
| | | *Complex services* | | |
| Occupational complexity | 2.551*** | -0.124 | 4.332*** | 0.159 |
| | (0.700) | (0.389) | (1.117) | (0.326) |
| Hourly wage | 1.675*** | 1.960*** | 2.143*** | 2.494** |
| | (0.400) | (0.529) | (0.479) | (0.803) |
| Observations | 295,876 | 533,656 | 332,110 | 578,683 |
| First stage $F$-statistic | 21.21 | 29.77 | 18.39 | 25.60 |
| | | *Public* | | |
| Occupational complexity | -0.478 | -0.599** | 0.603 | -0.467* |
| | (0.554) | (0.215) | (0.640) | (0.213) |
| Hourly wage | -0.376 | 0.482*** | -0.714* | 0.506*** |
| | (0.266) | (0.141) | (0.352) | (0.130) |
| Observations | 432,847 | 1,219,449 | 485,852 | 1,309,961 |
| First stage $F$-statistic | 23.98 | 30.99 | 24.50 | 29.85 |

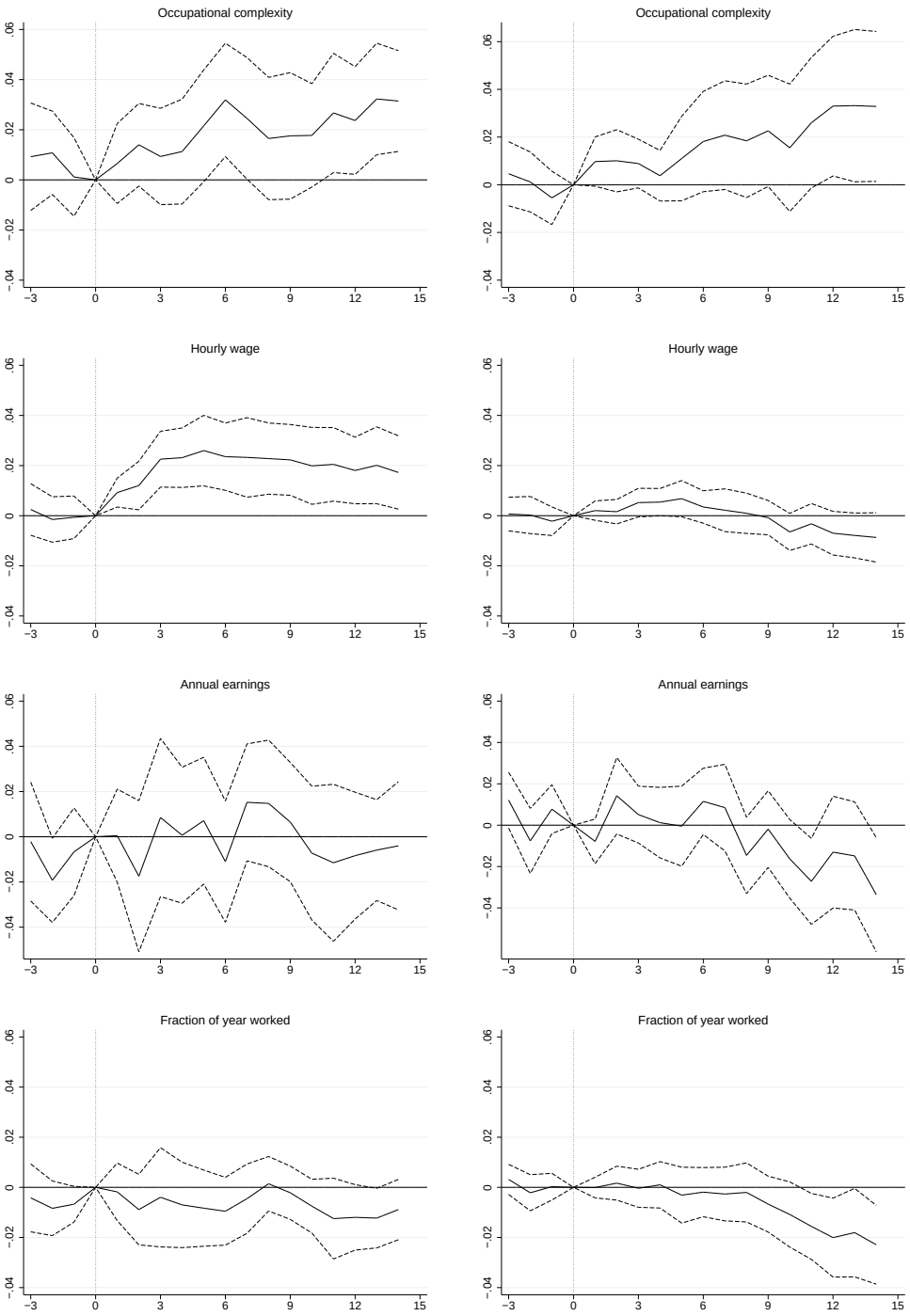
Notes: *** p< 0.001, ** p< 0.01, * p< 0.05. Each entry of the table is the coefficient on the explanatory variable of interest in equation (4) using a sample of employed natives between 1995 and 2008. The dependent variables (left column) have the same first stage except for occupational complexity that has fewer observations (some missings). Control variables not shown are: age, experience, tenure, (each of those squared), marital status, education, region by year and industry by year dummies (listed in Table 2). Standard errors in parentheses and $F$-statistic for significance of excluded instrument are clustered by municipality.

Figure 6: Transitions



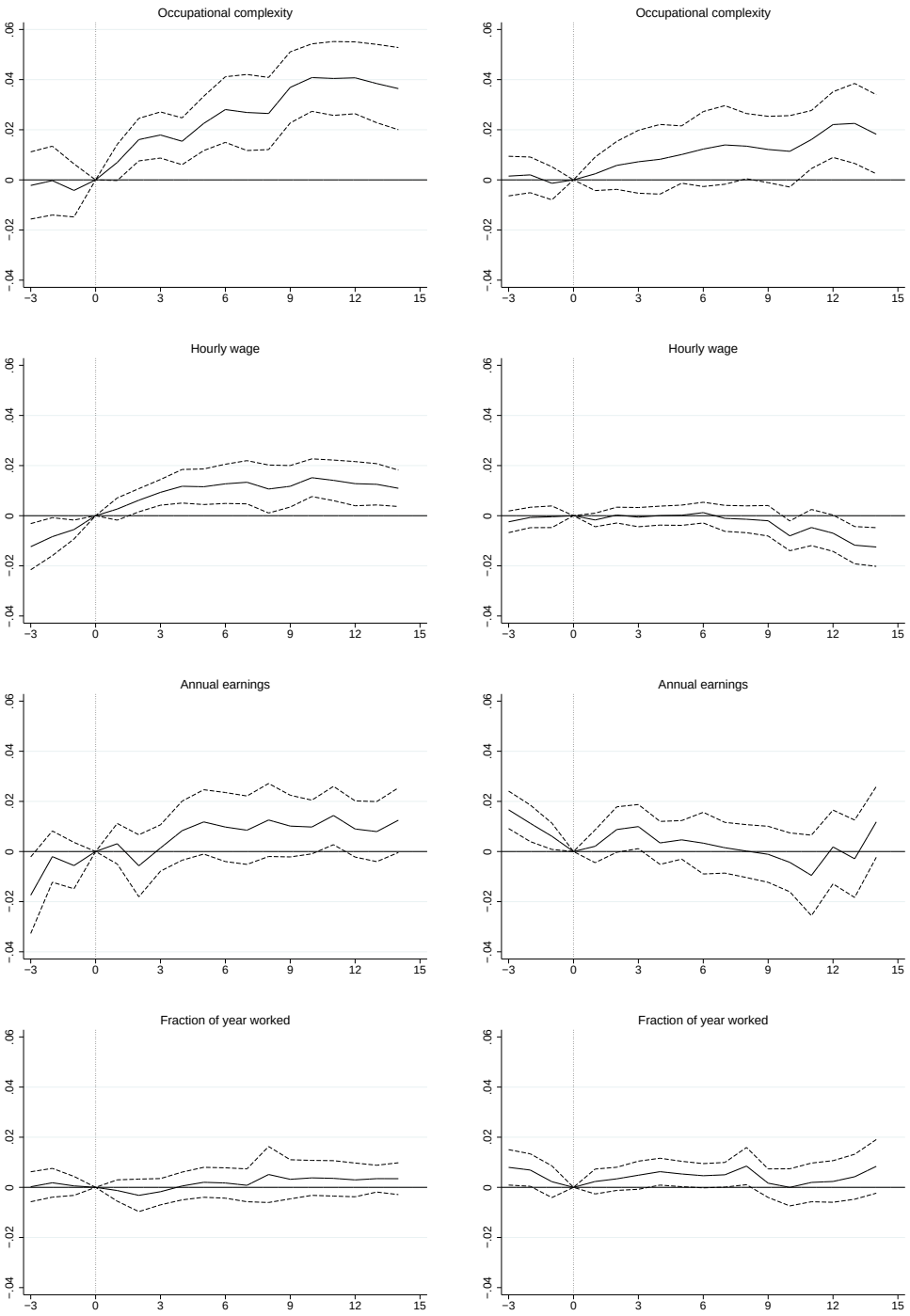

Notes: Parameter estimates (——) and 95% confidence limits (- - -) on the interaction terms of immigration exposure and year dummies in equation (5) using a *strongly balanced* panel of natives employed in 1994. Standard errors are clustered at the 1994-municipality.

Figure 7: Transitions by age in 1994 for low skilled



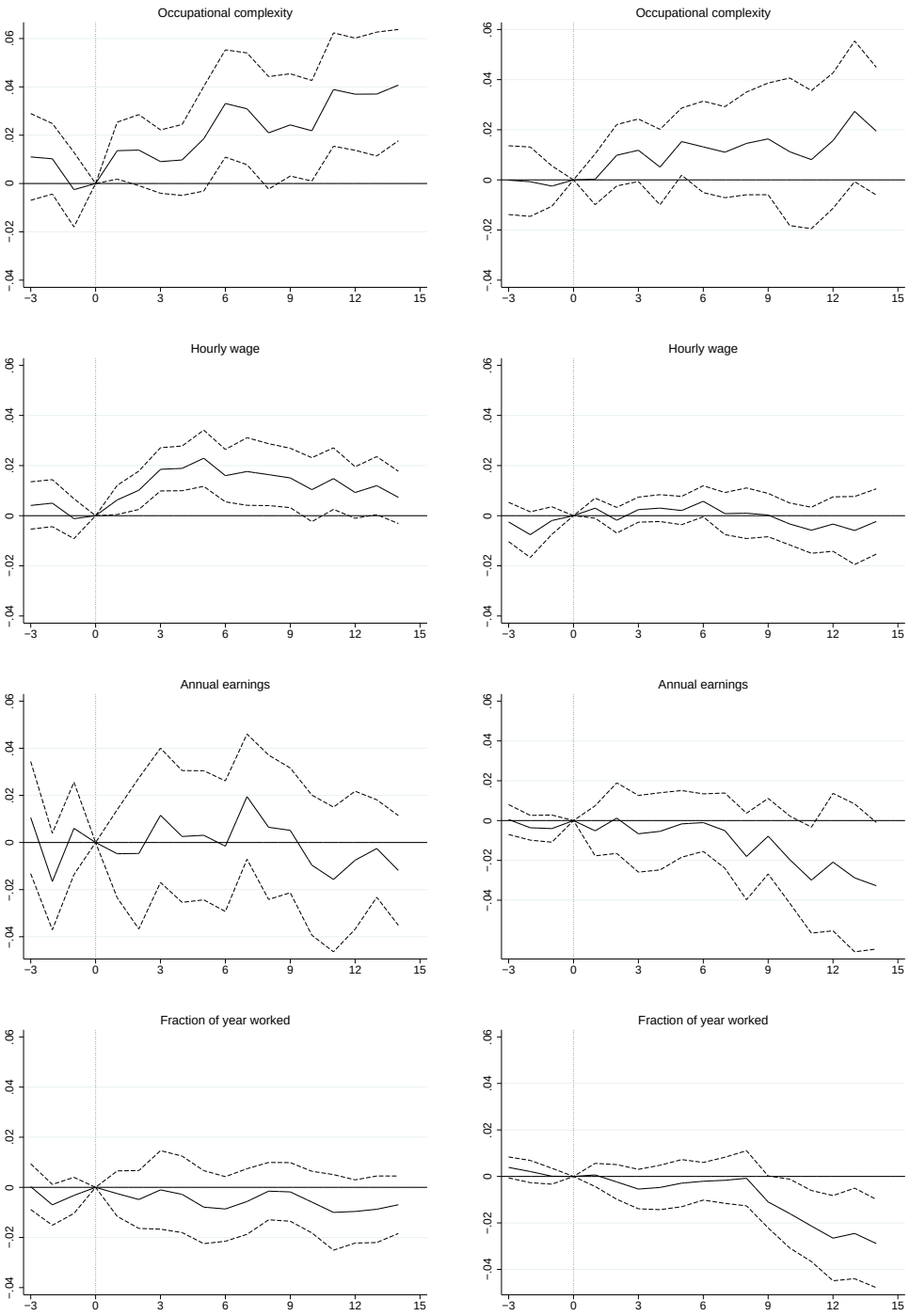

Notes: Parameter estimates (——) and 95% confidence limits (- - -) on the interaction terms of immigration exposure and year dummies in equation (5) using a *strongly balanced* panel of natives employed in 1994. Standard errors are clustered at the 1994-municipality. Young (old) are those aged 21-36 (37-51) in 1994.

Figure 8: Transitions by age in 1994 for high skilled



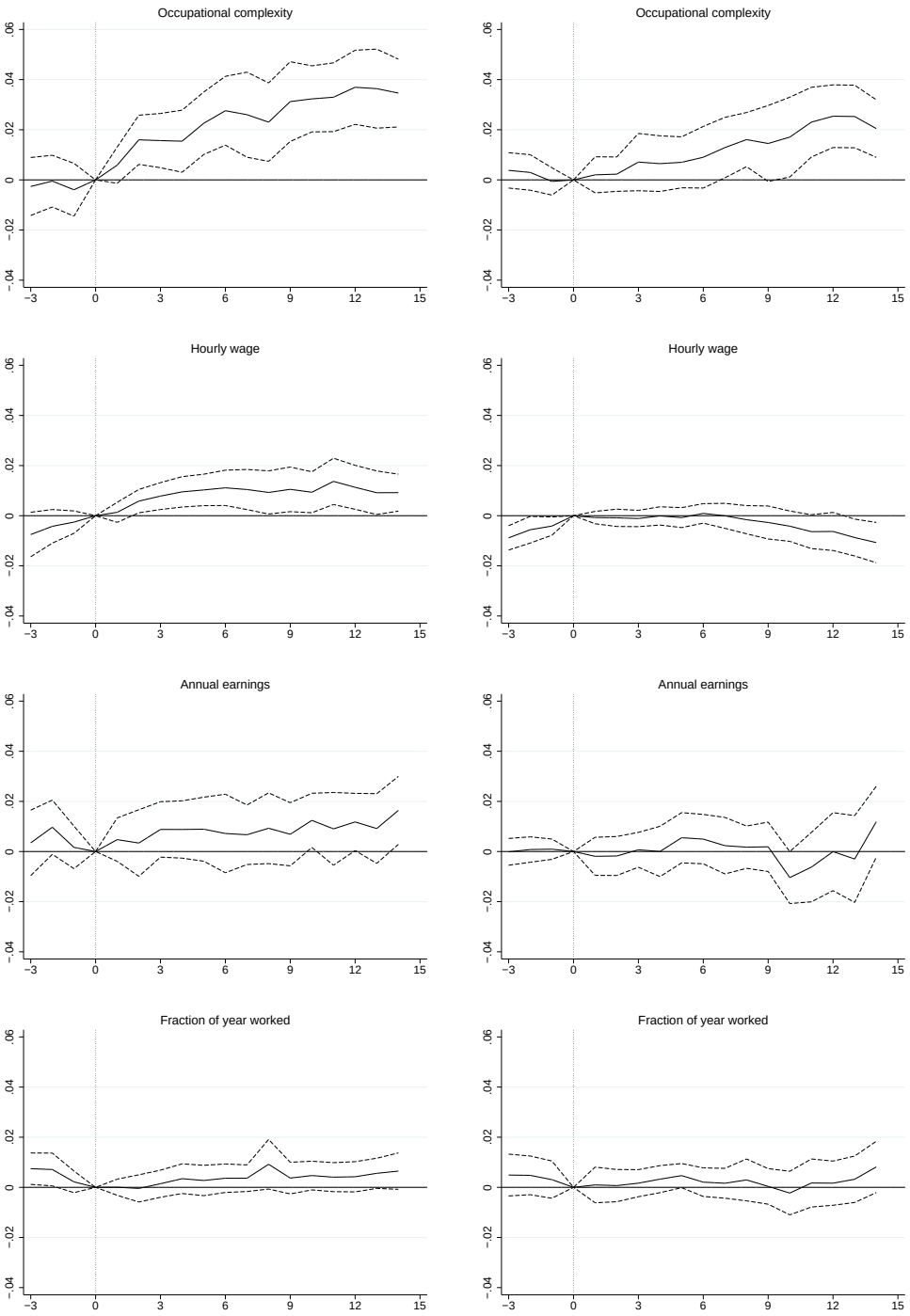

Notes: Parameter estimates (——) and 95% confidence limits (- - -) on the interaction terms of immigration exposure and year dummies in equation (5) using a *strongly balanced* panel of natives employed in 1994. Standard errors are clustered at the 1994-municipality. Young (old) are those aged 21-36 (37-51) in 1994.

Figure 9: Transitions by tenure in 1994 for low skilled




Notes: Parameter estimates (——) and 95% confidence limits (- - -) on the interaction terms of immigration exposure and year dummies in equation (5) using a *strongly balanced* panel of natives employed in 1994. Standard errors are clustered at the 1994-municipality. Low (high) tenure are those with less than (at least) 4.35 years in the firm in 1994.

Figure 10: Transitions by tenure in 1994 for high skilled




Notes: Parameter estimates (——) and 95% confidence limits (- - -) on the interaction terms of immigration exposure and year dummies in equation (5) using a *strongly balanced* panel of natives employed in 1994. Standard errors are clustered at the 1994-municipality. Low (high) tenure are those with less than (at least) 4.35 years in the firm in 1994.

Table 7: The cumulative effect on employment and mobility of *low* skilled, 1995-2008

| Dependent variable | (1) All | (2) Mfr. | (3) Non-Complex | (4) Complex | (5) Public |
|---|---|---|---|---|---|
| Cumulative employment | -4.731 | -2.839 | -8.303 | 5.022 | -10.263 |
| | (4.868) | (5.697) | (6.023) | (4.562) | (5.410) |
| - same establishment | -6.137 | -8.052 | -13.947 | 2.031 | -6.309 |
| | (3.183) | (7.631) | (8.596) | (3.423) | (4.096) |
| - new establishment | 1.406 | 5.213 | 5.644 | 2.991 | -3.954 |
| | (3.715) | (8.989) | (5.474) | (5.485) | (3.876) |
| - same sector | -2.396 | -33.182** | -4.605 | 23.632*** | -2.100 |
| | (3.940) | (10.824) | (6.155) | (6.934) | (3.830) |
| - new sector | -2.335 | 30.344*** | -3.698 | -18.610** | -8.163* |
| | (1.868) | (6.909) | (3.545) | (5.805) | (4.104) |
| - same municipality | -23.049*** | -35.128** | -34.148*** | -13.696* | -14.673** |
| | (6.666) | (12.704) | (9.412) | (5.702) | (5.410) |
| - new municipality | 18.318*** | 32.289** | 25.845*** | 18.718*** | 4.410 |
| | (5.210) | (11.634) | (6.441) | (5.111) | (5.574) |
| Cumulative unemployment | 2.211 | 0.302 | 3.515 | -0.109 | 3.618* |
| | (2.397) | (3.697) | (2.469) | (2.440) | (1.790) |
| Cumulative self-employment | -0.053 | 0.612 | -1.120 | -0.368 | -0.029 |
| | (1.167) | (1.414) | (1.302) | (2.301) | (1.135) |
| PDV of annual earnings | 0.030 | 1.343 | -0.379 | 0.621 | -0.747 |
| | (0.898) | (1.019) | (1.062) | (1.287) | (0.790) |
| Observations | 71,028 | 22,274 | 14,534 | 14,572 | 19,648 |
| First stage $F$-statistic | 15.07 | 21.49 | 11.47 | 12.70 | 16.70 |

Notes: *** p< 0.001, ** p< 0.01, * p< 0.05. Each entry of the table is the coefficient on the explanatory variable of interest (immigration exposure) in equation (6) using a *strongly balanced* panel of natives employed in 1994. Additional controls not shown in the table are the list of 1994-characteristics in table A.3. Standard errors in parentheses and $F$-statistic for significance of excluded instrument are clustered at the 1994-municipality. The final row is the discounted sum of the 1995-2008 earnings stream using a four percent annual discount rate.

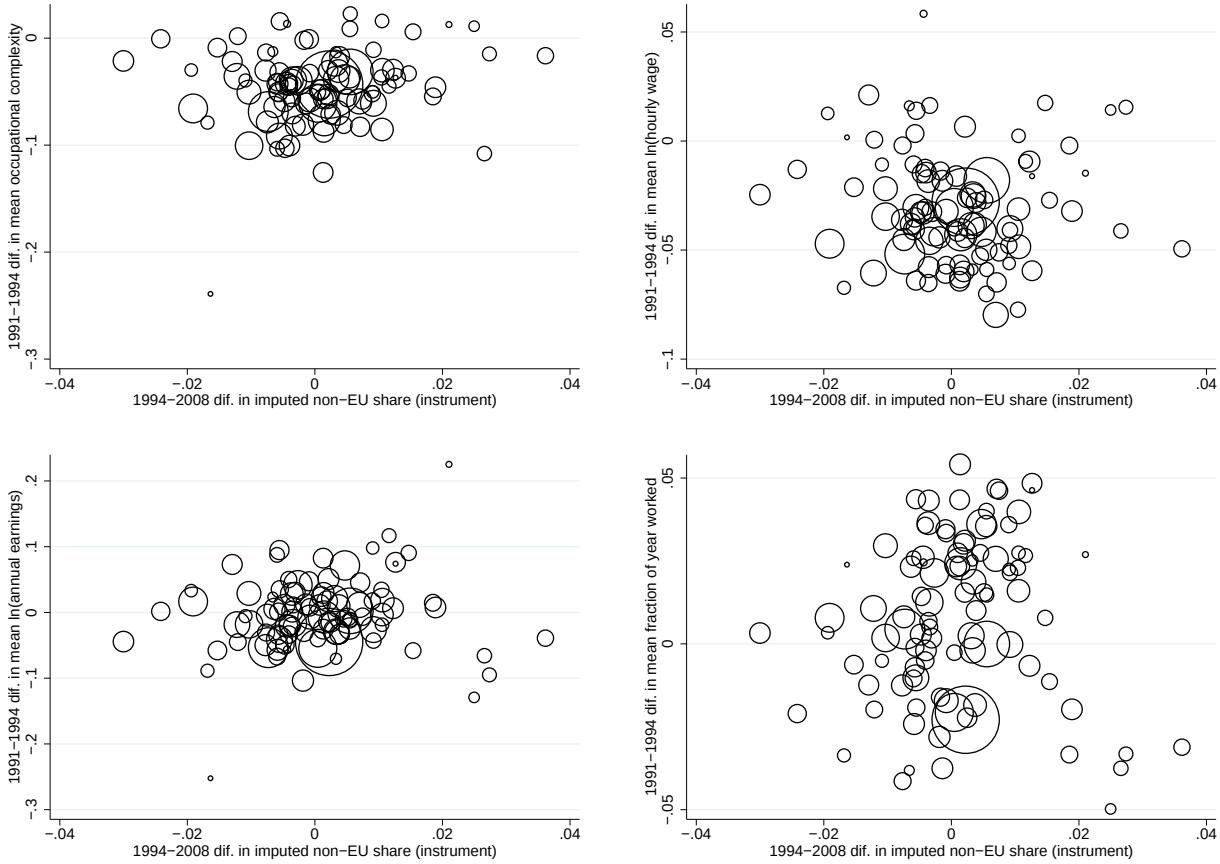
Table 8: The cumulative effect on employment and mobility of *high* skilled, 1995-2008

| Dependent variable | (1) All | (2) Mfr. | (3) Non-Complex | (4) Complex | (5) Public |
|---|---|---|---|---|---|
| Cumulative employment | -0.114 | -2.630 | -1.226 | -0.748 | 1.987 |
| | (2.597) | (3.355) | (4.052) | (3.388) | (1.891) |
| - same establishment | -12.433** | -13.914** | -13.672** | -15.608** | -8.258* |
| | (3.912) | (5.293) | (5.037) | (5.459) | (4.189) |
| - new establishment | 12.319*** | 11.285* | 12.446** | 14.860* | 10.245** |
| | (3.628) | (5.683) | (4.080) | (6.962) | (3.671) |
| - same sector | -1.055 | -26.343*** | -2.113 | 7.247** | 4.963** |
| | (1.767) | (7.139) | (4.111) | (2.266) | (1.665) |
| - new sector | 0.941 | 23.713*** | 0.887 | -7.995* | -2.976 |
| | (1.631) | (5.055) | (2.717) | (3.260) | (1.752) |
| - same municipality | -32.268** | -48.351*** | -44.684** | -38.251** | -16.312* |
| | (11.195) | (14.572) | (16.363) | (14.387) | (6.572) |
| - new municipality | 32.154*** | 45.721*** | 43.458** | 37.502** | 18.300** |
| | (9.486) | (12.583) | (13.221) | (11.727) | (6.164) |
| Cumulative unemployment | 2.198 | 3.980* | 2.649 | 1.055 | 1.696* |
| | (1.168) | (1.643) | (1.899) | (0.978) | (0.822) |
| Cumulative self-employment | -3.211*** | -3.938** | -4.558*** | -2.661* | -2.894*** |
| | (0.693) | (1.225) | (1.268) | (1.313) | (0.654) |
| PDV of annual earnings | 1.066 | 1.691* | 1.118 | 0.279 | 1.166** |
| | (0.614) | (0.747) | (1.029) | (0.828) | (0.356) |
| Observations | 164,025 | 33,833 | 37,908 | 29,229 | 63,055 |
| First stage $F$-statistic | 18.16 | 21.48 | 14.67 | 17.31 | 19.64 |

Notes: *** p< 0.001, ** p< 0.01, * p< 0.05. Each entry of the table is the coefficient on the explanatory variable of interest (immigration exposure) in equation (6) using a *strongly balanced* panel of natives employed in 1994. Additional controls not shown in the table are the list of 1994-characteristics in table A.3. Standard errors in parentheses and $F$-statistic for significance of excluded instrument are clustered at the 1994-municipality. The final row is the discounted sum of the 1995-2008 earnings stream using a four percent annual discount rate.

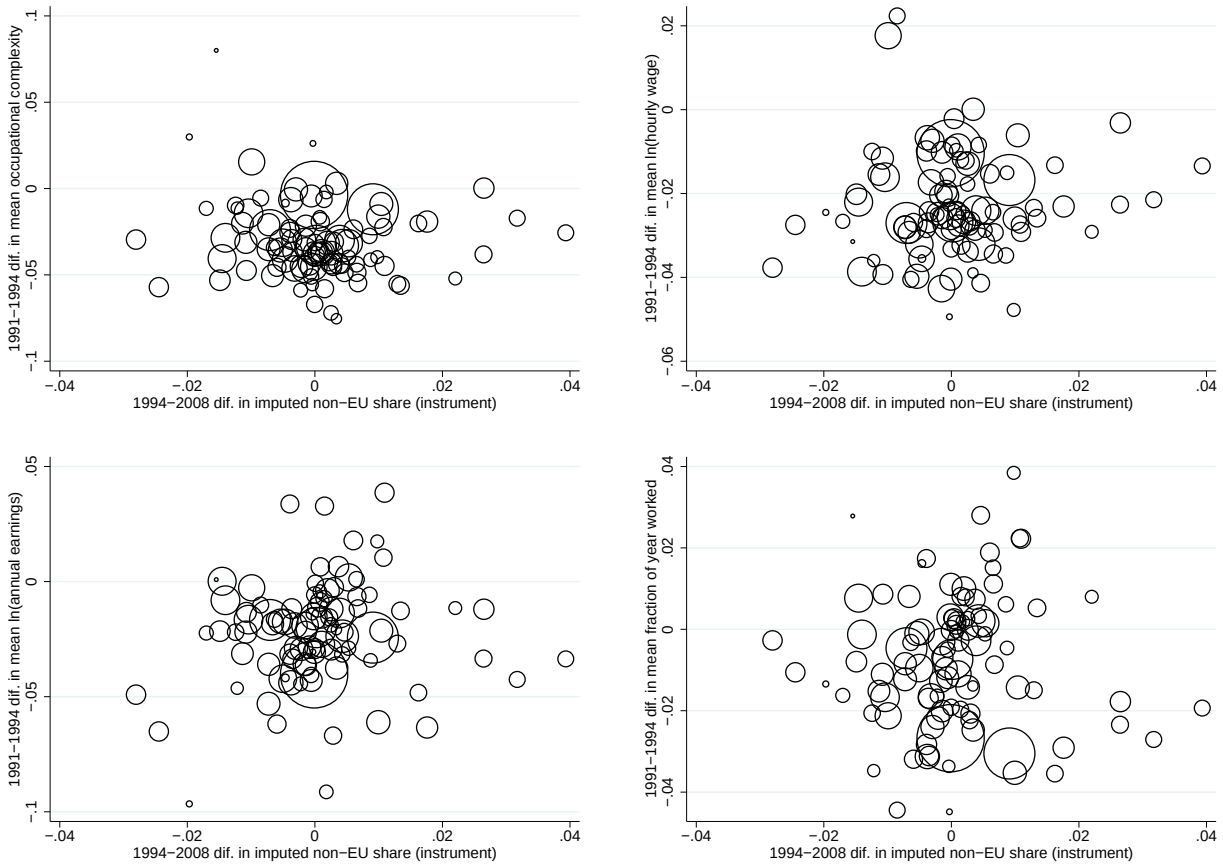
# Appendix

Figure A.1: Partial plots of pre-trend in native outcomes and in-sample trend in instrument, *low* skilled




Notes: Each circle represents a municipality, and it's size reflects the average size of the *low* skilled native labor force in the municipality in 1994. The vertical axis shows the pre-event trend in outcome variables averaged for the *low* skilled native labor force in the municipality, and the horizontal axis shows the post-event difference in the instrument (additional controls are those listed in Table 2 averaged for each municipality in 1994).

Figure A.2: Partial plots of pre-trend in native outcomes and in-sample trend in instrument, *high* skilled




Notes: Each circle represents a municipality, and it's size reflects the average size of the *high* skilled native labor force in the municipality in 1994. The vertical axis shows the pre-event trend in outcome variables averaged for the *high* skilled native labor force in the municipality, and the horizontal axis shows the post-event difference in the instrument (additional controls are those listed in Table 2 averaged for each municipality in 1994).

Table A.1: Within worker-establishment spell regressions (OLS)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Low skilled | | High skilled | |
| | Non-EU | Refugee | Non-EU | Refugee |
| Occupational complexity | 0.205 | 0.845 | 0.138 | 0.228 |
| | (0.139) | (0.479) | (0.096) | (0.263) |
| Career upgrade | 0.029 | 0.574* | 0.061 | 0.385 |
| | (0.120) | (0.292) | (0.064) | (0.200) |
| Career downgrade | 0.030 | 0.232* | 0.075 | 0.239 |
| | (0.051) | (0.110) | (0.045) | (0.159) |
| Occupational mobility | 0.059 | 0.806* | 0.136 | 0.625* |
| | (0.143) | (0.329) | (0.091) | (0.315) |
| Hourly wage | 0.222* | 1.070*** | 0.234 | 1.053*** |
| | (0.110) | (0.274) | (0.129) | (0.205) |
| Annual earnings | 0.154 | 0.877* | 0.081 | 0.791** |
| | (0.122) | (0.410) | (0.144) | (0.249) |
| Fraction of year worked | 0.044 | 0.085 | -0.038 | -0.192 |
| | (0.051) | (0.180) | (0.031) | (0.099) |
| Observations | 1,541,654 | 1,541,654 | 2,883,266 | 2,883,266 |

Notes: *** p< 0.001, ** p< 0.01, * p< 0.05. Each entry of the table is the coefficient on the explanatory variable of interest (non-EU share) in equation (4) using a sample of employed natives between 1995 and 2008. The dependent variables (left column) have the same first stage except for occupational complexity that has fewer observations (some missings). Control variables not shown are: age, experience, tenure, (each of those squared), marital status, education, region by year and industry by year dummies (listed in table 2). Standard errors in parentheses and $F$-statistic for significance of excluded instrument are clustered by municipality.

Table A.2: Within worker-muncipality spell regressions (OLS)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Low skilled | | High skilled | |
| | Non-EU | Refugee | Non-EU | Refugee |
| Occupational complexity | 0.626* | 2.778*** | 0.357*** | 0.860*** |
| | (0.285) | (0.837) | (0.098) | (0.250) |
| Career upgrade | 0.173 | 0.730* | 0.105 | 0.468** |
| | (0.093) | (0.316) | (0.055) | (0.150) |
| Career downgrade | 0.173* | 0.757*** | 0.175** | 0.619*** |
| | (0.084) | (0.167) | (0.064) | (0.157) |
| Occupational mobility | 0.347* | 1.487*** | 0.280** | 1.086*** |
| | (0.161) | (0.437) | (0.108) | (0.276) |
| Hourly wage | 0.059 | 0.695* | 0.240 | 1.268*** |
| | (0.110) | (0.326) | (0.148) | (0.265) |
| Annual earnings | 0.164 | 0.773* | 0.110 | 0.779** |
| | (0.120) | (0.379) | (0.146) | (0.286) |
| Fraction of year worked | 0.119* | 0.306* | -0.011 | -0.219* |
| | (0.055) | (0.146) | (0.034) | (0.092) |
| Observations | 1,787,910 | 1,787,910 | 3,154,751 | 3,154,751 |

Notes: *** p< 0.001, ** p< 0.01, * p< 0.05. Each entry of the table is the coefficient on the explanatory variable of interest (non-EU share) in equation (4) using a sample of employed natives between 1995 and 2008. The dependent variables (left column) have the same first stage except for occupational complexity that has fewer observations (some missings). Control variables not shown are: age, experience, tenure, (each of those squared), marital status, education, region by year and industry by year dummies (listed in table 2). Standard errors in parentheses and $F$-statistic for significance of excluded instrument are clustered by municipality.

Table A.3: The long-run effect on *low* skilled

| | (1) Occupational complexity | (2) Hourly wage | (3) Annual earnings | (4) Fraction of year worked |
|---|---|---|---|---|
| t = -3 | 0.005 | 0.002 | 0.003 | -0.002 |
| | (0.006) | (0.003) | (0.008) | (0.004) |
| t = 0 | . | . | . | . |
| t = 1 | 0.009* | 0.006** | -0.003 | -0.001 |
| | (0.004) | (0.002) | (0.006) | (0.003) |
| t = 5 | 0.017* | 0.017*** | 0.005 | -0.006 |
| | (0.008) | (0.004) | (0.010) | (0.006) |
| t = 9 | 0.021* | 0.011** | 0.005 | -0.004 |
| | (0.009) | (0.004) | (0.009) | (0.005) |
| t = 14 | 0.034*** | 0.006 | -0.012 | -0.013* |
| | (0.009) | (0.005) | (0.010) | (0.006) |
| Observations | 1,072,035 | 1,071,244 | 1,206,145 | 1,280,376 |
| R-squared | 0.44 | 0.22 | 0.16 | 0.13 |

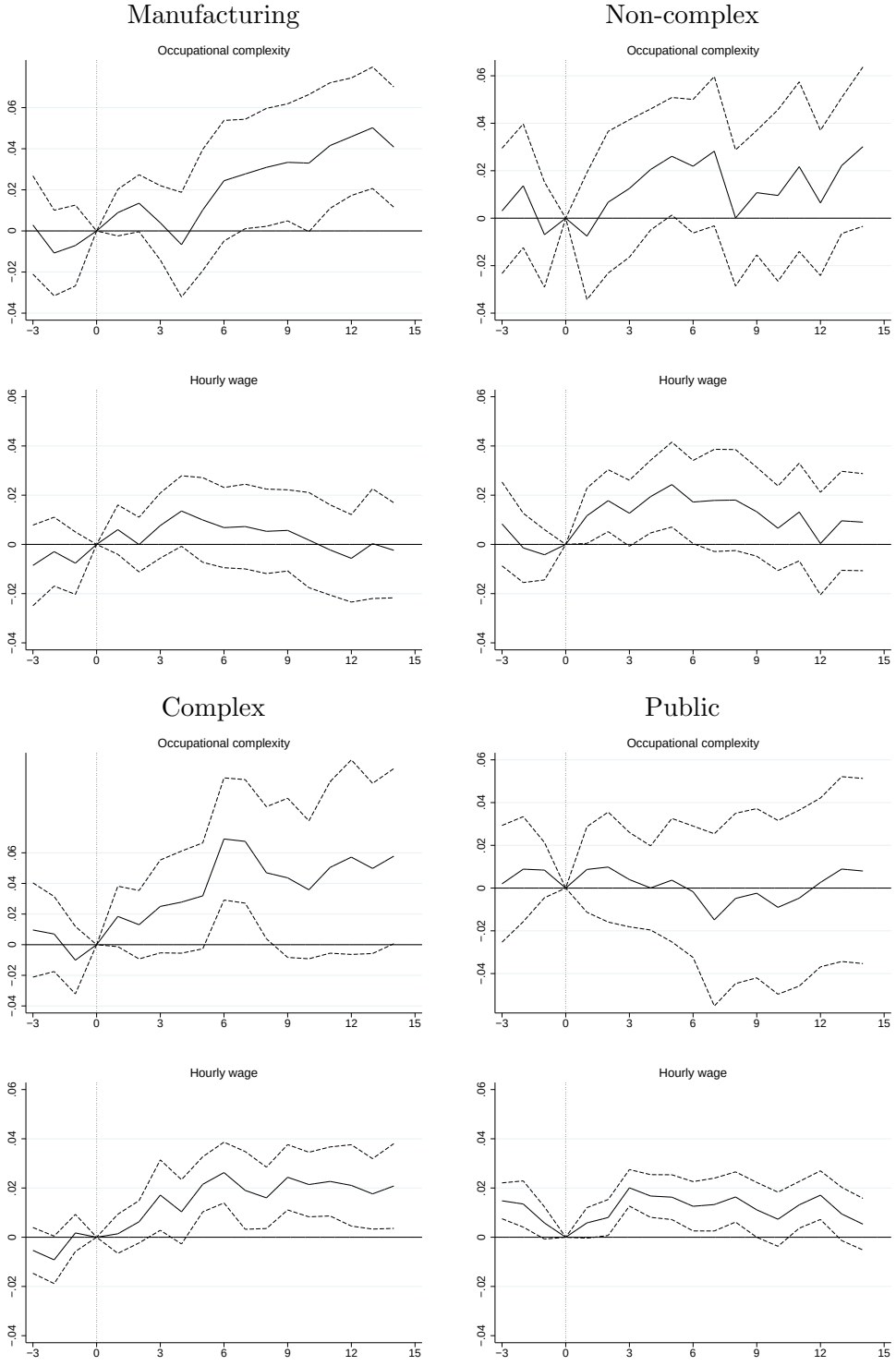
Notes: *** p< 0.01, ** p< 0.05, * p< 0.10. This table reports selected regression coefficients on the interaction terms of immigration exposure and year dummies in equation (5) using a *strongly balanced* panel of natives employed in 1994. Standard errors in parentheses are clustered at the 1994-municipality.

Table A.4: The long-run effect on *high* skilled

| | (1) Occupational complexity | (2) Hourly wage | (3) Annual earnings | (4) Fraction of year worked |
|---|---|---|---|---|
| t = -3 | -0.001 | -0.008* | -0.001 | 0.004 |
| | (0.004) | (0.003) | (0.005) | (0.003) |
| t = 0 | . | . | . | . |
| t = 1 | 0.005 | 0.001 | 0.003 | 0.001 |
| | (0.003) | (0.001) | (0.003) | (0.002) |
| t = 5 | 0.017*** | 0.007** | 0.010* | 0.004 |
| | (0.004) | (0.003) | (0.005) | (0.002) |
| t = 9 | 0.025*** | 0.007 | 0.007 | 0.003 |
| | (0.006) | (0.004) | (0.005) | (0.003) |
| t = 14 | 0.030*** | 0.003 | 0.019*** | 0.009* |
| | (0.005) | (0.003) | (0.005) | (0.004) |
| Observations | 2699752 | 2617994 | 2838069 | 2955330 |
| R-squared | 0.47 | 0.31 | 0.17 | 0.08 |

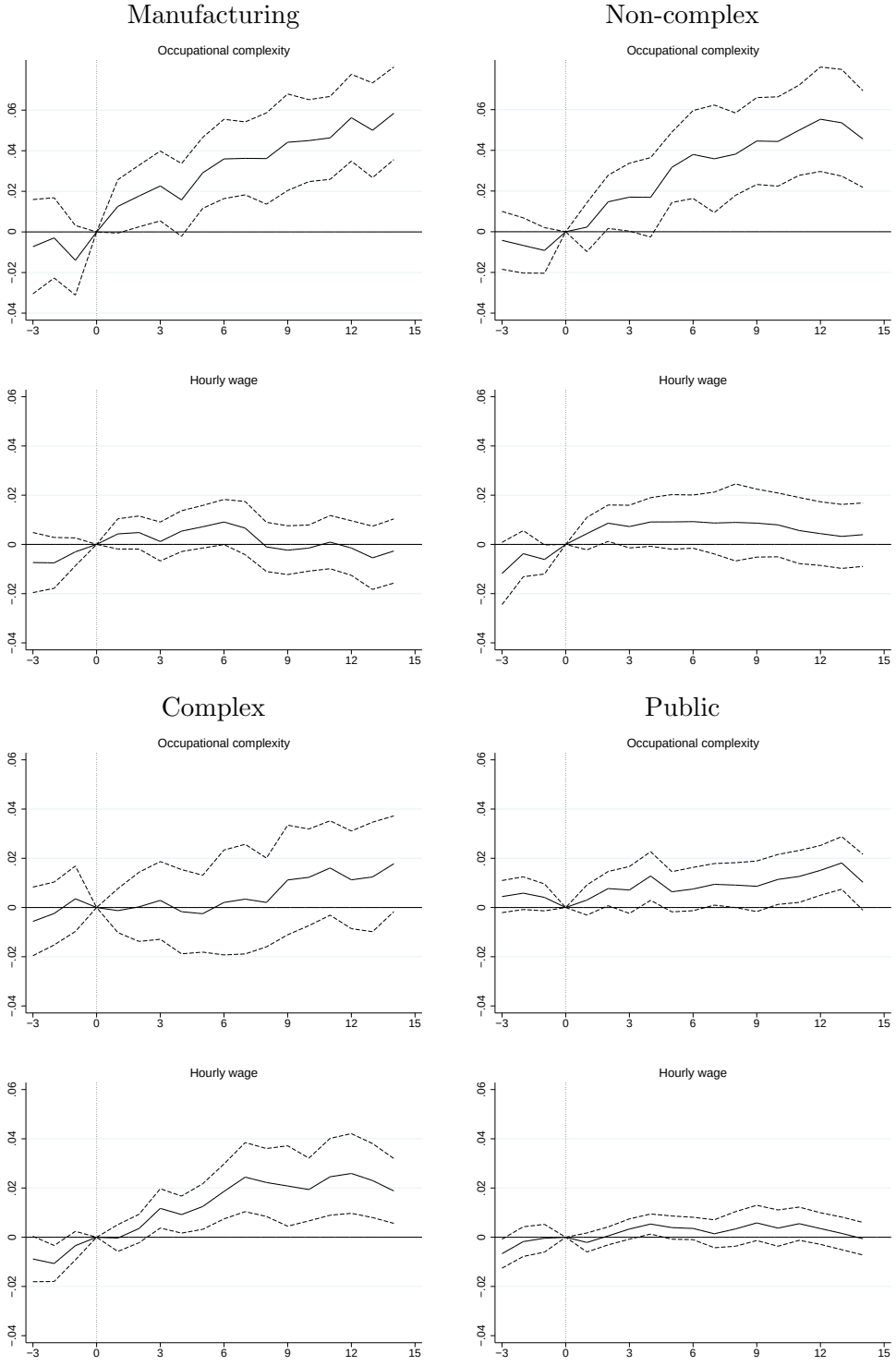
Notes: *** p< 0.01, ** p< 0.05, * p< 0.10. This table reports selected regression coefficients on the interaction terms of immigration exposure and year dummies in equation (5) using a *strongly balanced* panel of natives employed in 1994. Standard errors in parentheses are clustered at the 1994-municipality.

Figure A.3: Transitions by sector for low skilled



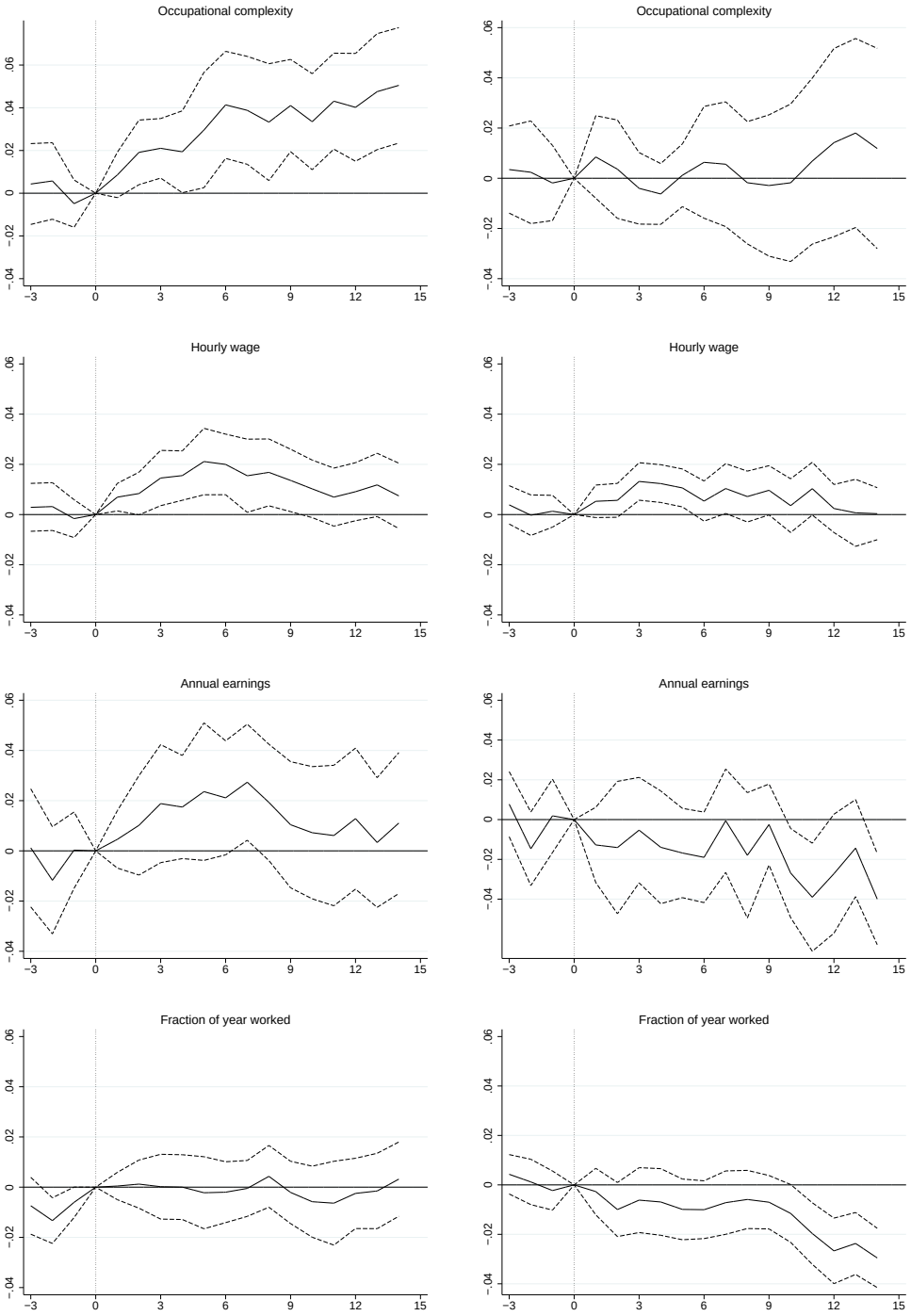

Notes: Parameter estimates (——) and 95% confidence limits (- - -) on the interaction terms of immigration exposure and year dummies in equation (5) using a *strongly balanced* panel of natives employed in 1994. Standard errors are clustered at the 1994-municipality.

Figure A.4: Transitions by sector for high skilled



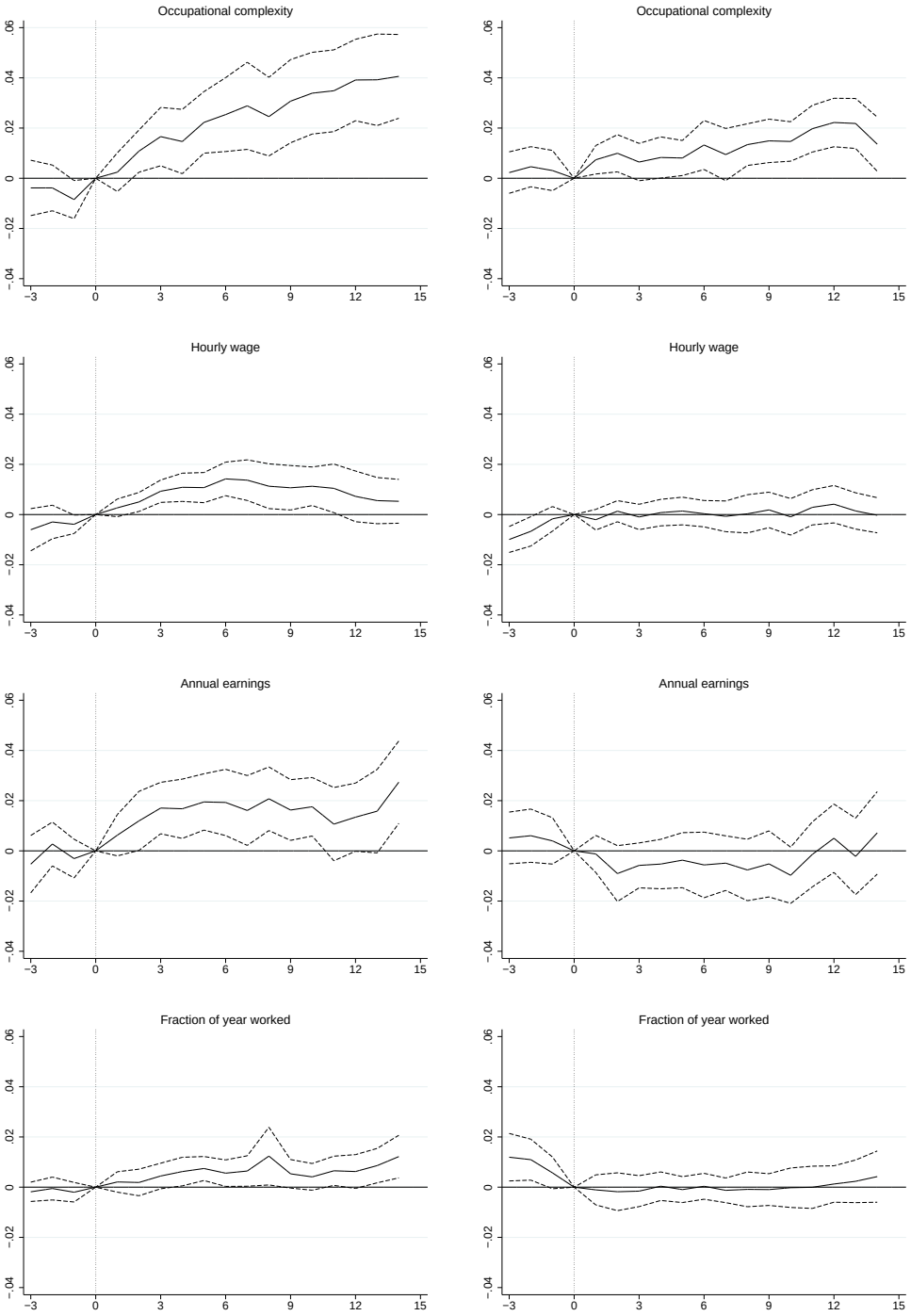

Notes: Parameter estimates (——) and 95% confidence limits (- - -) on the interaction terms of immigration exposure and year dummies in equation (5) using a *strongly balanced* panel of natives employed in 1994. Standard errors are clustered at the 1994-municipality.

Figure A.5: Transitions by gender for low skilled



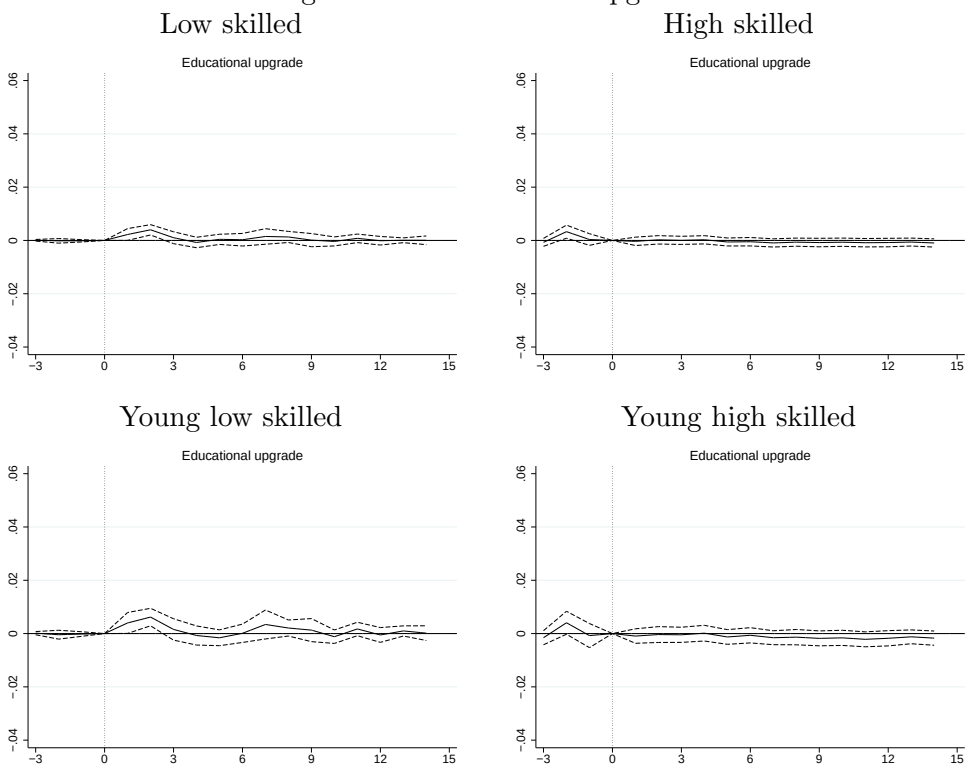

Notes: Parameter estimates (——) and 95% confidence limits (- - -) on the interaction terms of immigration exposure and year dummies in equation (5) using a *strongly balanced* panel of natives employed in 1994. Standard errors are clustered at the 1994-municipality. Low (high) tenure are those with less than (at least) 4.35 years in the firm in 1994.

Figure A.6: Transitions by gender for high skilled




Notes: Parameter estimates (——) and 95% confidence limits (- - -) on the interaction terms of immigration exposure and year dummies in equation (5) using a *strongly balanced* panel of natives employed in 1994. Standard errors are clustered at the 1994-municipality. Low (high) tenure are those with less than (at least) 4.35 years in the firm in 1994.

Figure A.7: Educational upgrade




Notes: Parameter estimates (——) and 95% confidence limits (- - -) on the interaction terms of immigration exposure and year dummies in equation 5 using a *strongly balanced* panel of natives employed in 1994. Standard errors are clustered at the 1994-municipality. Educational upgrade is a dummy variable that equals one if the individual upgrades his education between $t$ and $t-1$.