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No. 335
May 2013

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Do immigrants take or create residents' jobs? Quasi-experimental evidence from Switzerland

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This version: May 2013

Abstract

We estimate the causal effect of immigration on the labor market outcomes of resident employees in Switzerland, whose foreign labor force has increased by 32.8% in the last decade. To address endogeneity of immigration into different labor market cells, we develop new variants of the shift-share instrument, tailored for small-open economies, that exploit only that part in the variation of immigration which can be explained by migration push-factors in the source countries. We find that immigration has reduced unemployment of residents and has enabled them to fill more demanding jobs, while it had no adverse effect on wages and employment.

JEL-Classification: F22, J21, J61

Keywords: Immigration, native employment, labor shortage, shift-share instrument

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1 Introduction

Do immigrants crowd out resident employees or do they fill gaps in the resident workforce, thus raising the productivity and job chances of the latter? Both scenarios are conceivable on a theoretical basis and so only the data can tell which one is more relevant for a given country and time period. The prominent approach to estimate labor market impacts of immigration on native workers is to partition the labor market into different labor market "cells" and to correlate differences in the extent of immigration with labor market outcomes of resident employees in those cells. Different studies have used different cell definitions, including education-experience [Borjas, 2003, 2006], region-occupation [Card, 2001, Orrenius and Zavodny, 2007, Glitz, 2012] and region-education cells [Altonji and Card, 1991].[1]

Regardless of the cell definition, however, three problems arise when such cross-cell comparisons are to be interpreted as the causal effect of immigration on the resident workforce: endogeneity of immigration, outflows of resident workers out of the predefined labor market cells, and spillovers of the effects of immigration across cells [cf. Blau and Kahn, 2012]. We address the first problem with three new variants of the shift-share instrument suited for a small open economy like Switzerland, exploiting only *that* part of the variation in immigration that can be explained by push-factors in immigrants' countries of origin. We define cells by occupation and age group on the national level, rather than in terms of regions, as the existence of a national labor market cannot be rejected for small economies. The second identification problem arises because resident workers may change their broad occupational group because of immigrant inflows. We show that in our sample these "native outflows" reflect that immigrants enable natives to grow professionally, and demonstrate

[1] For recent overviews of the literature on the impact of immigration on the labor market, see Blau and Kahn [2012] and Okkerse [2008]. An alternative to exploiting quasi-experimental variation in immigration is a more structural approach as pioneered by Borjas [2003] and refined by Ottaviano and Peri [2012] and Manacorda et al. [2012]. Gerfin and Kaiser [2010] and Müller et al. [2013] have examined the Swiss case using such an approach. For a critical assessment of this literature see Card [2009].

how controlling for these outflows affects our baseline results. We also investigate how results change when we explicitly account for cross-cell spillovers, exploiting how employees in each sector are distributed across the different occupation-age cells. After accounting for endogeneity, native outflows, and cross-cell spillovers, we find that the effect of immigration on residents in Switzerland is on average one of complementarity, rather than of crowding out.

The first identification problem, endogeneity of immigration, arises because immigrants might select labor market cells with good economic prospects and thus high labor demand, giving rise to a coincidence of high immigration and high resident employment growth, which one might mistake for evidence of complementarity between immigrants and residents. Conversely, when we observe the coincidence of many immigrants into a cell and low hiring of residents, then this need not prove that immigrants are crowding out the resident workforce, since both may occur due to the limited availability of sufficiently qualified residents. The methodologically ideal response to this problem would be to have border officials throw a dice to determine which labor market cells immigrants are allowed into, but of course such a research design is not feasible in practice. A possible alternative to such an experiment are quasi-experimental methods that exploit only that part of the variation in total immigration to a country which is due to migration push factors in the countries of origin, which are unrelated to labor market shocks in the labor market segment of the destination country. Some authors have therefore examined "natural experiments" with undoubtedly push-driven migration into certain countries.[2]

While the examination of such episodes has allowed overcoming the endogeneity problem and has led to clean identification of the effect of immigration on the labor market, researchers that are interested in the labor market effect of immigration for a specific country and time period might not have a natural experiment at hand

[2]For instance, Friedberg [2001], and Cohen-Goldner and Paserman [2011] examine the fall of the Iron Curtain, Hunt [1992] the repatriation of French-Algerians following the end of the colonial rule of France, and Card [1990] the immigration of Cubans after the Mariel boat lift.

that leads to exogenous variation in immigration [Blau and Kahn, 2012]. A possible solution in this case is the instrumental variable (IV) approach suggested by Altonji and Card [1991] and refined by Card [2001]. The idea is to build labor market cells in terms of regional labor markets and to exploit the finding of Bartel [1989] that immigrants tend to seek connection to earlier immigrants from the same country of origin, and therefore move to regions where many fellow compatriots already live.

The problem of the instrument is that it requires applying the "spatial approach" which is only credible if a labor market is sufficiently regionally segmented, because with a "national labor market" immigration into one might spuriously affect another regional labor market, featuring as the control group. As the existence of a national labor market is even subject to debate for countries as large as the US [Borjas, 2003, 2006, Card and DiNardo, 2000, Card, 2001, Peri, 2011], its existence seems hard to reject for small open economies such as Switzerland [cf. also the arguments in Friedberg, 2001].

This paper therefore develops more broadly applicable shift-share instruments that are tailored to examine the labor market impact of immigration in small open economies. Focusing on a national labor market and on labor market cells defined in terms of occupation and experience, the concept of our shift-share instruments is to predict, for each year and cell, total immigration as the sum of predicted immigration into that cell from each country of origin. The country-cell element itself is given by the total immigration into Switzerland from that country, multiplied by a time-invariant distribution of how immigrants from a given source country are distributed across occupation-experience cells, i.e. the *shares* translate *shifts* in the number of immigrants from a specific country of origin in a given year into cell-specific shocks.

We propose three methods to compute the time-invariant shares, so that we end up with three related instruments. The first instrument exploits the fact that the distribution of workers from a specific source country across occupational groups is persistent across time, for example due to network effects [Patel and Vella, Forthcoming]. One can therefore use historical data on how the stock of immigrants from a specific country of origin has been distributed across the occupation-age cells to construct the shares. The second instrument uses the average distribution across all sample years. The third instrument, by contrast, relies on the distribution of the labor force within each source country of immigration.

The distinctive feature of our IV strategy is that, since we look at occupation-age cells in a nationwide labor market, the source of the variation exploited by our instrumental variables across years and cells originates in *sending country specific predictions* how immigrants might be distributed across cells, rather than in differences how specific *regions in the receiving country* might be affected by immigration [cf., e.g., Card, 2001]. We discuss possible concerns of endogeneity that may remain even after applying our IV strategy. Moreover, by demonstrating which part of the immigrating workforce is picked up by the different instruments, the paper tries to address more thoroughly than in previous studies how externally valid the results from the IV estimations are.

The second methodological issue discussed thoroughly in this paper is the problem of native outflows, i.e. the possibility that resident workers respond to the immigrant inflows by exiting their labor market cell. If such outflows occur, a researcher who correlates immigration inflows into a cell with employment or with wages of residents in the cell would underestimate the effects of immigration on natives. Previous studies argue explicitly or implicitly that native outflows are a less prevalent concern if cells are defined in terms of occupational groups as it is more difficult for resident workers to change the own skill mix in the short-run, than, for example, the regional labor market. We show, however, that one important effect of immigration in Switzerland is that it allows natives to change to more demanding jobs, implying outflows of residents into occupational groups with higher skill requirements and higher average earnings. We demonstrate how controlling for these outflows affects our estimates of the effect of immigration on employment and wages.

The third identification problem addressed in this paper is that, depending on how cells are defined, immigration into one cell may affect employment and wages also in another cell because of substitution effects or complementarity across cells, invalidating the latter as a suitable control group absent adequate corrections for such spillovers. We provide a straightforward method for accounting for these cross-cell interactions in a regression framework, and demonstrate that our results are robust to their inclusion. To our knowledge, no study applying a regression approach to estimate the effects of immigration on the labor market has shown whether cross-cell effects influence the estimated elasticities.

Our empirical application is concerned with the case of Switzerland which has recently been subject to an influx of foreign workers of extraordinary magnitude. The increase in the immigrant inflow is linked to the introduction of a free movements of persons regime with the EU/EFTA states in June 2002 that gradually led to a full liberation of immigration from these countries.[3]

The case of Switzerland is interesting for at least three reasons. First, Switzerland has experienced a substantial inflow of foreign employees in recent years. Between 2002 and 2011, Switzerland's foreign resident population grew on average by 2.4% percent *annually*. Relative to the resident population, no other OECD country had a larger immigrant inflow than Switzerland in 2010 (cf. Figure 1). Second, a substantial share of the immigrating employees have been highly skilled, which is in contrast to the situation analyzed in most previous studies of the labor market effects of immigration, mainly examining episodes of large-scale immigration of low-skilled workers. Switzerland's experience might thus exemplify the potential labor market effects of high-skilled immigration in an advanced knowledge economy. Third, the main driver of immigration to Switzerland is labor shortage.[4] We are not aware of any

[3] See Section A.1 of the Online Appendix for a short discussion of the impact of this treaty on immigration to Switzerland

[4] In a wage survey by *UBS [2010]*, 72% of the sampled Swiss firms reported that the reason for recruiting foreign personnel is a shortage of resident employees.

study that examines the impact of immigration for a country in which immigration is equally driven by the needs of firms to fill their vacancies.[5]

Our results show that on average Switzerland's native workforce benefits from immigration. In particular, our quasi-experimental estimates suggest that push-driven immigration of 100 persons reduces registered unemployment by about 3 persons and increases the chances of natives to work in higher-skilled occupations, but have no measurable effect on wages and employment of residents within cells. Our results support the claims of many state officials and business representatives that immigration of mostly high-skilled employees to Switzerland does not harm (average) wages while improving job opportunities of the resident workforce.

2 Methodology

2.1 Specification

In order to analyze the labor market effects of immigration in Switzerland, we segment the labor market by occupational groups for a national labor market, rather than by geographical regions. This approach has two important advantages. First, we cannot reject the hypothesis of a national labor market for a country as small-scale as Switzerland. Second, unobserved shocks, simultaneously affecting immigration and the labor market situation of resident workers, are not likely to have a strong differential effect on cells defined in terms of occupational groups: shocks to labor demand occur to firms or to industries, and occupational groups are relatively broadly distributed across these groups.

[5] Several studies have examined the *wage* effects of the recent immigration wave to Switzerland. Most of these studies do not find evidence that wages of low-skilled employees are affected by the inflow, while some papers find modest negative effects on wages of high-skilled employees [Gerfin and Kaiser, 2010, Favre, 2011, Cueni and Sheldon, 2011, Müller et al., 2013, Stalder, 2010]. However, the fact that most studies do not find significant effects of the immigration influx on wages of resident workers might be due to the fact that wages are fairly downward rigid in Switzerland [see Fehr and Goette, 2005, Puhani, 2003]. The main contribution of this study to the literature on the effects of immigration in Switzerland is therefore its consideration of the impact of the immigration inflow on unemployment and employment of Swiss residents.

To account for the fact that workers with differences in work experience are imperfect substitutes [Borjas, 2003], we segment each occupational group using the age of the labor force. We use 9 (in Sections 5.2 and A.5 3) age groups, and occupations are split into 9 major groups (i.e. the first digit) of the International Standard Classification of Occupations 1988 (ISCO-88), yielding a total of 81 (27) "skill groups" in the Swiss labor market.[6]

A regression of employment and unemployment on immigrant inflows may suffer from different specification errors due to scale effect, i.e. the problem that there arises a positive correlation between immigrant inflows and employment and unemployment in a cell simply because we are likely to observe more immigration into larger cells. To overcome these problems we follow the recommendations in Peri and Sparber [2011] and estimate the following regression model:

$$\Delta O_{it} = \alpha + \beta(I_{it}/LF_{it-1}) + \gamma X_{it} + \tau T_t + \epsilon_{it} \tag{1}$$

In this equation, ΔO_{it} represents the change in the outcome of interest in skill group i and year t. The outcomes that we consider are, alternatively, the change in the absolute number of unemployed or employed native workers relative to the cell-specific total labor force in the previous year (i.e. $(U_{it} - U_{it-1})/LF_{it-1}$ and $(E_{it} - E_{it-1})/LF_{it-1}$), or the change in their logarithmic wage (Δw_{it}). The regressor T_t represents time fixed effects that capture all aggregate effects affecting the growth of the outcome of interest equally across cells.[7] Since we run our estimations in first

[6]The nine age categories that we define are 15–24 years, 25–29 years, 30–34 years, 35–39 years, 40–44 years, 45–49 years, 50–54 years, 55–60 years and 60 and above. We work with 9 instead of 10 broad occupational groups because ISCO major group 0 ("Armed Forces") is quantitatively irrelevant in our application. As was pointed out by Cohen-Goldner and Paserman [2011], such a narrow definition of skill groups implies a relatively high degree of substitutability between immigrant and resident workers and it is hence likely that the short-run impacts of immigration on natives might be larger in our analysis than if we defined the skill groups, for example, in terms of broad education-age groups. We tested the robustness of our results to a broader cell definition and found qualitatively similar results. For example, coefficients of interest estimated in regressions that have just 3 instead of 9 age groups are shown in Tables 8 and A.4.

[7]We will replace the year effects by occupation-year effects in most regressions, and in even more demanding specifications we also control for age-year effects and include the lagged dependent

differences, time-invariant factors affecting the level of the outcome variable—i.e. skill group specific effects μ_i—are automatically accounted for. I_{it}/LF_{it-1} is the central independent variable in the regressions and indicates the number of newly hired foreign employees in skill group i relative to the resident (Swiss and foreign) labor force in that skill group in the previous year. If a labor market cell experiences an influx of foreign employees in the course of year t within a given skill group, this fraction rises, reflecting the increase in the relative labor supply of immigrants. Note, firstly, that we do not first-difference immigration since it already is a flow, and secondly that we do not log this fraction because I_{it}/LF_{it-1} already approximates how immigration affects log skill ratios relative to other skill groups.[8] When it is specified in this way, a negative (positive) β indicates displacement effects of immigration when the outcome is employment (unemployment), and represents an inverse elasticity of substitution across skill groups when the outcome is wages.

Finally, X_{it} is the vector of control variables. This vector comprises the ratio of tertiary to primary educated resident employees in the cell (rescaled by 1/100), the share of female employees in the cell, the average age and age squared of the employees in the cell, and the share of state workers in the occupational group in a given year.[9]

An important concern with the above regressions is that our central independent variable does not represent *net* but gross inflows because cell-specific outflows and hence stocks of foreign employees in the cell are not observed in ZEMIS, which is the source of our immigration data. This might be problematic because certain skill groups are characterized by higher labor turnover than others, for example due to a higher importance of seasonality effects. Clearly, the inflow of employees relative to

variable (ΔO_{it-1}).

[8] Log skill ratios are the relevant skill mix measure in, for example, a nested CES production function framework. See Peri [2011] for a theoretical derivation of our specification.

[9] The motivation to include the third regressor is that state workers are, for example, significantly less subject to unemployment [for Switzerland, see Rolf Schenker and Martin Straub, 2011] and, moreover, immigration of foreign employees is likely to be negatively related to the share of state workers in the cell.

the labor force is positively correlated with higher labor turnover. Furthermore, because for instance seasonality effects are more likely for low-skilled occupations and absolute changes in the number of unemployed are also higher in low-skilled occupations, there possibly exists a positive cross-cell correlation between labor turnover and the absolute change in the number of unemployed or employed in a cell. To overcome these concerns we compute, on an annual basis and using the Swiss Labor Force Survey (SLFS), average job tenure in years in the cells, motivated by the presumption that job tenure proxies for the extent of labor turnover in the cells, and include the level of this variable and its square as a control variable in all regressions (rescaled by 1/100).[10]

2.2 Accounting for endogeneity

Endogeneity of immigration is a concern in the above regression. For example, if the labor market in certain cells is characterized by a shortage of skilled labor, then the coincidence of low hiring of resident workers together with a high influx of new foreign employees just mirrors the lack of qualified employees in the cell and not the negative or positive effects of immigrants. Note that part of such a bias will already be controlled for by estimating the regression model in first differences because first differencing removes all time-invariant cell-specific factors affecting the level of the outcome variable, among others constant differences in labor shortage across cells.

As a further step to remove the possible bias in the estimate of (I_{it}/LF_{it}), we propose three related shift-share instruments. The central idea behind these instruments is to distribute the total number of immigrants from a given country of origin (i.e. the *shifts*) across cells using *shares* that are plausibly unrelated to the unobserved cell-specific shocks (ϵ_{it}) causing the endogeneity. More specifically, let π_{ij} be a share of workers from country j in skill group i, derived from the (time-invariant)

[10] We have run a set of test regressions of our model using cell-specific *net* inflows of foreign employees computed from the SLFS, and the results are qualitatively similar to those presented in this paper. These results are available from the authors upon request.

distribution of workers from country j across cells, and let I_{ijt} be the actual number of immigrants in each skill group from country j in year t. We denote as $\bar{I_{jt}}$ the sum of the number of immigrants from a country in a given year across all skill groups, i.e. $\bar{I_{jt}} = \sum_i I_{ijt}$. Our prediction of the number of immigrants ($\hat{I}_{ijt}$) from a certain country j in skill group i and year t is then given by

$$\hat{I}_{ijt}^{basic} = \sum_i \pi_{ij} \bar{I_{jt}} \tag{2}$$

Note that skill-group specific shocks (shocks to ϵ_{it}) that lead to pull-driven immigration from different countries of origin still influence this instrument by increasing the total number of immigrants from a specific country of origin ($\bar{I_{jt}}$). To purge the influence of these shocks completely from our instrument, we refine it by subtracting the own cell's contribution to the total as was suggested by Wozniak and Murray [2012]:

Firstly, compositional shifts are only unrelated to ϵ_{it} because Switzerland as a small open economy does arguably exert negligible influence on the economic situation in immigrants' countries of origin and hence does not (differentially) influence out-migration from there.

Secondly, compositional changes in immigrants' countries of origin would create endogeneity if a shock that differentially affects the occupation-age cells leads to a shortage of a specific type of labor that is supplied in a few specific countries of origin only. As a consequence, the relative importance of immigration from a certain source country might increase. We deal with this problem by excluding the own cell's contribution to the total number of immigrants when computing our instrument (cf. Equation (2)), as an increase in cell-specific labor demand is not mirrored in the own cell's country-specific predicted number of immigrants.

$$\hat{I}_{ijt} = \sum_i \pi_{ij} (\bar{I_{jt}} - I_{ijt}) \tag{3}$$

These *country-specific* predictions are then aggregated over certain or all countries of origin of immigrants for each year, and we normalize the predictions with the lagged labor force size in the cell, similarly as we do with the immigration variable. This yields our instrumental variable for I_{it}/LF_{it-1}:

$$\frac{\hat{\hat{I}}_{it}}{LF_{it-1}} = \frac{\sum_j \hat{I}_{ijt}}{LF_{it-1}} \tag{4}$$

Our three proposed instruments differ in the way in which the country-specific shares π_{ij} are computed. The first approach to compute these shares is inspired by the approach pioneered by Altonji and Card [1991] which has become standard in the immigration literature [Card, 2001, 2009, Blau and Kahn, 2012]. Studies employing this instrumental variable strategy build labor market cells in terms of regional labor markets and exploit the finding of Bartel [1989] that immigrants tend to seek connection to earlier immigrants from the same country of origin, and therefore move to regions where many fellow compatriots already live, independent of the labor market situation in a certain regional labor market.

The study of Patel and Vella [Forthcoming] indicates that a related pattern can be exploited in an analysis based on occupation-age groups for a national labor market as it is likely that a historical distribution of the stock of foreign workers from a specific country across occupation-age groups is similar to the distribution of new immigrants from that country across the groups even many years later due to network effects.[11] To implement this strategy, we use data from the Swiss population census of 1990 and build country-specific predictions for immigrants from 80 different countries of origin based on their historical distribution across occupation-age groups in the census data.[12] The central assumption behind this instrument is that the

[11]There are other reasons besides network effects that might explain persistence in how immigrant workers are distributed across occupation-age cells. Most importantly, it is likely that immigrants in later years will be similarly distributed across cells simply due to the fact that the educational system and industrial structure of the country of origin of foreign workers—and hence the skill composition of workers from a country—do not change rapidly.

[12]We only consider countries that either belong to the current EU/EFTA states or to countries

distribution of immigrants of a specific nationality across occupation-age cells in 1990 is independent from the cell-specific labor market situation many years later, and therefore independent of shocks to ϵ_{it}.

Our second instrumental variable follows the same line of reasoning, but instead of distributing newly arriving immigrants according to a historical distribution in Switzerland, we use the distribution of the labor force across occupation-age groups *in immigrants' countries of origin* in the year 2000 to build the country-specific shares π_{ij}. Arguably, the distribution of migrants from a specific country of origin across cells is likely to be related to the distribution of its labor force across cells as in 2000, but the latter should be unrelated to occupation-age specific shocks occuring in Switzerland posterior to 2002.[13]

Our third instrumental variable builds the shares simply according to the *average* of how newly arriving immigrants are distributed across cells over the ten years in the sample. The idea here is that if cell-specific shocks are not too persistent or unevenly distributed across occupation-age cells, using the average share of immigrants into each cell instead of the actual yearly share might sufficiently reduce endogeneity arising from unobserved cell-specific shocks in a given year. Clearly, this third instrument has a lower internal validity than the first and second instrument. Our primary motivation to include this third instrumental variable is, however, one of external validity (cf. the discussion in Section 5.1).

The distinctive feature of our IV strategy is that the predictive power of our instrumental variables originates in *sending country specific predictions* how immigrants might be distributed across cells. More specifically, the variation in the IVs stems from the country- and cell-specific shares π_{ij} and from *changes in the compo-*

with at least 100 resident employees in Switzerland in 1990.

[13]This data-demanding approach is feasible because the Labor Force Survey (LFS) of Eurostat provides harmonized cell-specific employment and unemployment figures for most EU countries since the late 1990s, and the EU member countries are the major countries of origins of immigration of foreign employees to Switzerland. Qualitatively, the results do not depend on our choice of the base year.

sition of countries of origin j of immigrants across time (combine Equations (3) and (4) to see this formally).[14]

This raises the question whether changes in the share of immigrants from specific countries of origin in the total number of immigrants might lead to a systematic correlation between our predictions ($\hat{\hat{I}}_{it}$) and ϵ_{it}. We analyze this question in Figure 2, which plots the relative share of the number of immigrating foreign employees from six different source regions in the total number of immigrating foreign employees in Switzerland from 2002 to 2011. The first four of these regions were hit by negative shocks to different push factors of migration throughout the last decade, leading to substantial (lagged) increases in the number of immigrating foreign employees from these countries relative to the total number of immigrating foreign employees.[15] The relative gain of importance of immigration from the last two source regions, the EU-8 and EU-2 countries, is attributable to the introduction of a free movement regime in mid-2006 (EU-8 countries) and mid-2009 (EU-2) between Switzerland and these regions, which facilitated the influx of workers.

It thus seems reasonable to exploit compositional shifts in the countries of origin across time when estimating with our instruments, as these shifts provide a *summary measure* of changes in push factors affecting total migration to Switzerland, i.e. changes in the economic, demographic, political or cost determinant that affect out-migration in the countries of origin. However, as the discussion about immigration from the EU-8 and EU-2 countries to Switzerland has shown, changes in

[14]Indeed, once year fixed effects are accounted for, relative changes of total immigration $\bar{I_{jt}}$ from different countries j are the sole source of *within-cell* variation in our IV. Controlling for year fixed effects is necessary because they absorb aggregate movements in the total number of immigrants from all countries. ($\hat{\hat{I}}_{it}$ changes in the hypothetical situation where the country-composition of immigrants is unchanged while the total number of immigrants from all countries varies.)

[15]These six regions of origin are Southern Europe (Greece, Italy, Portugal, and Spain), Iraq, Haiti, the Horn of Africa (Somalia, Ethiopia and Kenya), eight European countries joining the EU in 2004 (EU-8 countries: Poland, Hungary, Czech Republic, Slovenia, Slovakia, Estonia, Lithuania, Latvia) and the two newest EU countries (EU-2 countries: Romania and Bulgaria). The push-shocks illustrated are a severe economic crisis in the case of Greece, Portugal, Spain, and Italy, a war in the case of Iraq, a severe earthquake in Haiti in January 2010, and two devastating famines in the Horn of Africa in 2006 and 2008.

the law that facilitate labor mobility between Switzerland and certain countries of origin alter these countries' relative share in total immigration to Switzerland. Such changes might also have a differential impact on the cell-specific economic situation in Switzerland.[16] To circumvent this problem, we build separate predictions ($\hat{\hat{I}}_{it}$) for three groups of countries that have not been *differentially* affected by changes in the migration law in the period under examination and include the predictions as three separate instruments in the regressions below. Thanks to the year fixed effects, our instruments exploit only variation that arises from shifts in the country-composition *within* the sets of countries.[17] This approach has the additional advantage of yielding overidentifying restrictions, which allow us testing for endogeneity of a subset of our instruments.

3 Data

Table 1 provides summary statistics of the most important variables used and indicates the data sources.

Data on immigration into Switzerland are from the central migration information system (*Zentrales Migrationsinformationssystem*, ZEMIS). ZEMIS is in fact a continuous census of the foreign resident and nonresident population in Switzerland. In particular, ZEMIS provides a complete count of the number of *immigrating foreign employees* into our skill groups for any given year since 2002, i.e. it indicates personal characteristics of immigrating employees such as their nationality, sex, age, residency permit, and their occupation.[18] In the regressions below, we analyze the

[16]Compositional shifts are also unrelated to ϵ_{it} because Switzerland as a small open economy does arguably exert negligible influence on the economic situation in immigrants' countries of origin and hence does not (differentially) influence out-migration from there.

[17]These groups of countries are the "EU-15" countries (Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden, and United Kingdom), the EU-8 countries (Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Slovakia, and Slovenia), and the non-EU27 countries (rest of the World).

[18]Two comments on the ZEMIS data are in order. Firstly, ZEMIS does not provide the net increase in the number of foreign employees for our cells. The problem is that foreigners that change occupational group, or that leave the workforce or the country are not obligated to report

joint effect of two different kinds of inflow of foreigners into the Swiss labor market: the inflow of foreign employees taking permanent residence I_{it}^{r} (i.e. workers with a residency permit for at least one year) and the number of new cross-border permit holders I_{it}^{CB} who live in neigbouring countries and commute into Switzerland at least once a week to work. Cross-border workers are a quantitatively important phenomenon in Switzerland. In 2011, they made up 5.2% of the total labor force, increasing from 3.8% in 2002.

Since ZEMIS provides a complete count of immigrants to Switzerland, the cell-specific numbers of immigrating foreign employees are not subject to measurement error as is a major concern for many other studies that use data from household surveys to estimate the number of foreign workers in the labor market cells.[19] Another advantage of our data is that we can correctly assign immigrants to labor market cells upon arrival because the assignment is determined by the actual job they found in Switzerland. Thus, our estimates do not suffer from the problem that immigrants may change the labor market cell and compete with natives in another labor market than the one to which they had been assigned based on observed qualifications [cf. Dustmann et al., 2012].

Skill group specific employment of native workers is constructed using the Swiss Labor Force Survey (SLFS). The SLFS is a representative household survey conducted by the Federal Statistical Office (FSO) of Switzerland since 1991, covering, depending on the year, between 0.5 and 1.2% of Switzerland's labor force. It is a rotating panel in which households are surveyed for 5 consecutive years. The sample size of the SLFS has substantially increased over time. After elimination of the non-

this change. Secondly, ZEMIS employs the Swiss Standard Classification of Occupations 1990. We therefore have to recode the 5-digit occupation codes of the Swiss classification into the 4-digit ISCO-88 classification. The key to do this is, however, not one to one, which introduces some imprecision. This is not a great concern because we use only the first digit of the ISCO-88 classification, and the imprecision in the reclassification is small on this level.

[19] A recent paper by Aydemir and Borjas [2011] has demonstrated that even if the researcher has more than 100 observations per cell to estimate average immigrant inflows into cells, the coefficient of interest can have a severe attenuation bias.

employed, retirees, persons aged below 15, and persons belonging to the nonresident population, we are left with between 18,900 and 35,600 employees per year that we use to compute total cell-specific employment and all control variables mentioned above, applying the sampling weights from the SLFS when building the cell averages and counts. We define as "native" (or "resident") employee resident Swiss and foreign employees that declare having lived in Switzerland for at least 3 years. Hence, immigrants arriving in Switzerland in year t do neither affect E_{it} nor the control variables (X_{it}) in the same year. However, the labor force size (LF_{it}) used to control for the cell size in the denominator of the dependent and the central independent variable in Equation (1) contains all resident foreigners, i.e. it includes the newly arrived immigrants. This way of specifying our cell counts follows the suggestions made by Peri and Sparber [2011].

Data on cell-specific earnings of residents are not computed using the SLFS because the self-reported wage data of the SLFS suffer from substantial measurement error [Fehr and Goette, 2005]. Instead, we derive average cell-specific monthly log full-time equivalent earnings from the Social Protection and Labor Market Survey (SESAM).[20] This data set is composed of a linkage of data from the SLFS with information gathered from different social insurance registers. In particular, SESAM provides monthly earnings in the main job according registers form the old age insurance (AHV). Since contributing to the old age insurance scheme is mandatory for all employees and all wage components, our wage measure is a very broad measure of remuneration of employees.

Cell-specific unemployment of resident employees was compiled by the State Secretariat for Economic Affairs (SECO) from its electronic database on unemployed persons in Switzerland (AMSTAT). The database is a complete count of all unemployed persons registered at regional unemployment agencies in Switzerland in

[20]Following Ottaviano and Peri [2012], we exclude earnings of self-employed persons since self-employed earnings are subject to substantial reporting errors.

any month since 2004. The superior quality, sample size and reliability of this data compared to the data on employment is the reason why we study the effects of immigration on employment in Switzerland mainly using unemployment as the outcome variable, eventough employment would be the conceptually preferable measure.

The data for our instruments are derived from two other sources. Firstly, country-specific historical distributions of foreign resident employees across skill groups are derived from data of the population census in 1990. Secondly, age- and occupation-specific unemployment and employment in the countries of origin of the immigrants—required to construct the cell-specific labor force size in 2000—are from Eurostat's Labor Force Survey (LFS).[21]

4 Results

Figure 3 presents a simple scatter plot that conveys two basic results of this paper. It illustrates the reduced-form relationship between our instrumental variable and two of the outcomes considered in this paper. In particular, the subplot on the left shows the change in the number of unemployed persons in Switzerland from January in year t to January in $t+1$ relative to the labor force in $t-1$, on the vertical axis, versus the *predicted* inflow of resident immigrants from January 1 to December 31 relative to the labor force in the previous year (I_{it}^{R}/LF_{it-1}). The sample covered is January 2004 to January 2012, and predicted immigration is constructed using the census 1990 shares. The plot indicates a slightly *negative* correlation between the predicted immigration rate and the change in the number of unemployed resident workers.

[21] In the case of employment, Eurostat allows partitioning ISCO-specific employment into certain age groups. These age categories are 15–24 years, 25–39 years, 40–49 years, 50–59 years and 60+ years. However, our grid for the age groups is finer than the one of the LFS. We build cell-specific employment in the remaining categories by multiplying the number of persons in employment in an occupational group from above with the share of employees in the respective age group across all occupational groups (for which the finer age grid is available). We apply a similar procedure for the number of unemployed persons.

The subplot on the right side of Figure 3 plots changes in the number of employed resident employees between 2002 and 2011 against the predicted immigration rate. The figure demonstrates that the correlation between the two variables is virtually zero. Thus, simple correlations provide no evidence of a systematic displacement of natives by the immigration of foreign workers.

In Table 2, we examine the relationship between changes in native unemployment and immigration more systematically. The table provides WLS estimates of Equation (1). All estimations include a set of year dummy variables that control for effects affecting all cells equally. Moreover, all regressions weight observations according to their cell size[22], and standard errors are robust to clustering on the level of the skill group i.

The point estimate of the coefficient of interest (i.e. $\hat{\beta}$) in the first column is statistically significant and suggests that native unemployment has been reduced by immigrating foreign employees. Column 2 shows that this conclusion does not depend on whether we weight observations according to their cell size or not. Column 3 extends the model from the first column and includes a full set of occupation-year effects that capture, for example, occupation-specific trends in the growth of unemployment such as skill-biased technological change and occupation-specific shocks to productivity. The fourth column controls additionally for age-year specific effects, i.e. any year-specific effect that might have differentially affected the 9 experience groups. Finally, Column 5 also includes the lagged dependent variable. This variable accounts not only for persistence in the cell-specific shocks ϵ_{it} [cf. Peri, 2011], but also helps controlling for any remaining, possibly pre-existing cell-specific trends shared by the outcome variable and the immigration variable, for example due to growth of the underlying population.

Similar to the coefficient in the first column, the coefficients in Columns 3 to

[22] We use the total average labor force size from 2002 to 2011 as weight of the respective cell. All estimations are qualitatively similar if we did not weight observations at all.

5 are significant and negative. Since the dependent and the central independent variable have the same denominator, $\hat{\beta}$ can be interpreted in terms of changes in the number of unemployed persons. Since the number of newly hired foreign employees per year amounted to 100,000 on average, the point estimate in the third column implies that immigration of foreign workers has kept 3,600 resident workers from getting unemployed. Note that this estimate of the impact of immigration on lowering unemployment is not without economic relevance, as the average number of registered unemployed in the sample is only 147,000 persons.

However, the WLS estimates displayed in the table might be biased if there are unobserved cell-specific shocks causing a spurious correlation between cell-specific immigration and the outcome that are not accounted for by the vector of controls and the set of fixed effects included in the regressions. Table 3 adresses this concern and presents results of a set of two-stage least squares (2SLS) estimations in which immigration is instrumented with the three shift-share instruments proposed. A characteristic set of corresponding first stage regressions used in our paper is provided and discussed in Section A.2 in the Online Appendix.

The 2SLS estimates in the table confirm the results from the WLS estimations. In the first three columns, the shift-share instrument is constructed using the shares (π_{ij}) from the 1990 population census. The estimate of the effect of immigration using this instrument is similar in size to the corresponding WLS estimates. In our preferred specification that controls for occupation-year effects (Column 2), the point estimate suggests that immigration of 100 foreign employees preserves 3.4 resident employees in Switzerland from getting unemployed.

The estimation in Column 4 instruments immigration with predicted immigration constructed using shares that represent how the labor force in immigrants' countries of origin was distributed across the cells in 2000. Since we can build these shares only for countries included in Eurostat's LFS, the instrumented variable in this case covers only immigrants from 25 European union member states (all current EU member states except Romania and Bulgaria). Although the point estimate in Column 4 is also negative, it is not statistically different from zero.

Finally, the fifth column applies the sample distribution of immigrants across cells to build the shift-share instrument. Since there are only few year-to-year changes in the distribution of the total number of immigrants across cells, these shares predict actual immigration into the cells very accurately. The result is a very strong instrument that yields point estimates that are close to those from corresponding WLS estimations.

Three comments on these results are in order. First, our instrumental variables generally work well. The first stages are sufficiently strong—as is indicated by the Kleibergen and Paap [2006] *rk* F-statistics of tests of joint significance of all instruments in the first-stage regressions—and the p-values of the Hansen J statistic suggest that the overidentifying restrictions are valid.[23] Moreover, note that the regressions in Columns 2, 3, and 5 do not contain a constant. This is because the occupation- and age-year effects as well as the constant are "partialled out" prior to the regression in order to ensure that the estimated covariance matrix of moment conditions is of full rank.[24]

Second, the similarity of the point estimates both, across OLS and 2SLS estimation and across the different shift-share instruments, suggests that endogeneity of immigration might not be a very prevalent issue in this application. This may mirror

[23]However, instrument validity could not always be confirmed for the whole subsets of instruments. If instrument exogeneity was put into question by the J test, the suspicious instrument was excluded from the regression. We do not report an overidentification test when employing the shift-share instrument built using the labor force shares (Column 4). The reason is that the *rk* F-statistics of tests of joint significance of all instruments fell slightly below the critical value of 10 when predicted immigration is separately introduced for certain subregions. We therefore favored a stronger instrument over the possibility of a Hansen test.

[24]The issue arises because the cluster-robust covariance matrix of moment conditions converges in the number of clusters, and when including a full set of age-year and occupation-year effects, the number of regressors exceeds the number of clusters. Stata's partial option to the ivreg2 command provides a solution to this problem by partialling out the fixed effects and the constant from the regression prior to estimation without effect on the estimated coefficients. This is admissible because of the Frisch-Waugh-Lovell (FWL) Theorem.

the fact that shocks to ϵ_{it} that differentially affect occupation-experience groups in a nationwide labor market are rather unlikely, such that concerns of endogeneity of immigration are reduced by the way we define cells.

Third, most estimated models control for occupation-year effects. In these estimations, the coefficients are effectively identified from variation in changes in the number of unemployed from one year to another across age groups within the 9 occupational group. This naturally carries over to the first stage: in these regressions, we only exploit variation (i) arising from differences in the cell-specific shares π_{ij} within occupational group, and (ii) variation in predicted immigration that arises because changes in the country-composition of immigration to Switzerland lead to shifts in the way we predict immigration to vary across the 9 age groups within occupational groups. Thus, the instruments basically exploit demographic differences in immigrants' countries of origin.

Tables 4 and 5 contain the results when the outcome in Equation (1) is the change in the number of resident employees relative to the labor force (i.e. $(E_{it} - E_{it-1})/LF_{it-1})$ and mean log monthly nominal full-time equivalent earnings in the cell, respectively. In contrast to the unemployment data which are a complete count from administrative registers, the cell-specific number of employees and their wage are just sample estimates calculated from household surveys (SLFS and SESAM, respectively). Moreover, in both cases we regress the change in the outcome from the second quarter in year $t-1$ to the second quarter in year t on the inflow of foreign employees during year $t-1$ (both variables relative to the labor force in $t-2$). The implied time lag between dependent and independent variable is not optimal but necessary because the SLFS and SESAM are conducted in the second quarter of the year. Both facts certainly introduce imprecisions. The sample covers, on the other hand, now also the years 2002 and 2003.

The WLS estimates in Table 4 suggest that inflows of foreign employees had no impact on employment of natives. The table thus confirms the visual evidence from the scatter plot above. The standard errors of these estimates are, however, relatively large, and two of five point estimates presented are negative. As negative coefficients would imply crowding out of natives, we need to test whether our results concerning unemployment are driven by resident workers leaving the labor force due to immigration.

This is done in Section A.3 of the Online Appendix. As we cannot construct labor force participation or employment rates given the way we defined the cells[25], we examine this issue by counting the number of resident workers leaving the labor force from one year to another and by subsequently regressing this count on the immigrant inflow rate. These regressions indicate that resident outflows out of the labor force do not drive our previous results. This is not very surprising considering that labor force participation among residents has been constant during the period under consideration.

The results of WLS and 2SLS pertaining to wages are shown in Table 5. The regressions indicate that the inverse elasticity of substitution between resident and foreign workers is close to 0. Overall, there is thus no evidence of a negative impact of inflows of foreign workers on the average labor market outcomes of natives.

The Online Appendix extends the findings of this section in two important ways. First, in Section A.4, we differentially analyze the impact of immigration on unemployment and wages for certain subgroups of the population. Most importantly, we find that the outcome-improving effect of immigration on unemployment of natives is driven by (young) low-skilled resident workers which seem to benefit most from the immigrant inflow. This result is in line with previous research on the wage effects of immigration in Switzerland. Several papers have established that low-skilled immigrants are imperfect while high-skilled immigrants are perfect substitutes of resident workers in Switzerland [cf. in particular Müller et al., 2013].

Second, Section A.5 analyzes whether the "constant", or static effects established

[25]The problem arises because we do not know the occupation of all non-employed in the SLFS.

above might hide that the impact of immigration on labor market outcomes changes over time [Wozniak and Murray, 2012, Cohen-Goldner and Paserman, 2011]. The important message from this analysis is that estimations which constrain β to be constant across time hide differences in the short- and medium-run impact of immigration. In particular, the short-run impact of immigration on all outcome variables is clearly outcome-improving, but the positive impact is lowered the longer the immigrants are in Switzerland's labor market. This is consistent with the fact that resident workers and immigrants initially have relatively complementary skills, but progressively become closer substitutes as immigrants acquire local human capital, resulting in an outcome-deteriorating medium-run effect of immigration on the labor market outcomes of natives [Cohen-Goldner and Paserman, 2011].

5 Extensions

5.1 External and internal validity of the IV estimates

A limitation of previous papers that apply shift-share instruments to examine the effects of immigration on native workers is that it id hard for the reader to gauge to what extent estimates are not only internally, but also externally valid. However, the shift-share instruments applied may not lead to a valid estimation of an average treatment effect (ATE), but, depending on the specific shares used, only yield an internally valid estimate of the local treatment effect (LATE), showing the effect of those immigrants on native workers whose immigration decision is manipulable by the instrument [the so-called "compliers", cf. Angrist, 2004]. For example, recent studies have shown that network effects, which underlie a substantial fraction of the predictive power of the shift-share instruments applied, are particularly important for low-skilled workers [Beine et al., 2011]. Or, low- and high-skilled immigrants may, depending on the episode examined, differentially respond to push-factors of migration. As a consequence, differences between OLS and IV estimates might not arise due to endogeneity of immigration but due to the fact that there are heterogeneous treatment effects—for example, because low-skilled immigrants might be closer substitutes to resident workers than high-skilled workers. Clearly, this would have important implications for the external validity of the IV estimates.

A simple way to gain better insight into the characteristics of the subgroup of compliers is to run separate first-stage regressions for interesting subsamples of the population [cf. Angrist, 2004]. Table 6 thus shows the estimate and the strength of the first-stage relationship between actual and predicted immigration ($\hat{\alpha}$) for each of our three instruments for four subgroups of immigrants: young and old low- and high-skilled workers, respectively.

The table confirms that the three shift-share instruments exploit different variation to identify the parameter of interest. Pertaining to the instrument based on the historical shares of 1990, the differences in the t-statistics of the four first-stage estimates reveal that the predictive power of the instrument mainly stems from accurate predictions of how young and low-skilled immigrants are distributed across cells. The instrument does, on the other hand, only weakly exploit immigration of young high-skilled workers and it does not use variation in immigration of older workers. The reason why the instrument mainly picks up young and low-skilled immigrants is that workers immigrating to Switzerland have been mostly low-skilled prior to 1990. The shares built from the stock of foreigners in Switzerland in 1990 do hence mainly predict the distribution of low-skilled immigrants across cells.[26]Similarly, the second instrument, applying shares derived from the distribution of the labor force across in immigrants' countries of origin, identifies the effect of interest using variation in how young low-skilled and young high-skilled migrants are distributed across cells.

By contrast, the third instrument, based on the distribution of immigrants across

[26]By comparing the size of the first-stage coefficients, it can also be seen that the census shares underpredict the number of young compared to the number of old immigrants. This is because actual immigrants from a certain country of origin are younger than the stock of immigrants from that country in Switzerland in 1990. This is a smaller limitation to the external validity of the IV estimates, as most immigrants are young.

cells over all years in the sample, yields by construction an accurate prediction of how immigrants in the sample period are distributed across occupation-age groups. This property is the main reason why we include this instrument in our study. Clearly, however, the gain in terms of external validity when employing the instrument comes at the cost of a lower internal validity, as persistent shocks, especially if they are unevenly distributed across cells, might influence the averages computed from ten years of data. In this case, the instrument might not overcome possible problems of endogeneity of immigration in the regressions.

The discussion shows that our instruments identify a LATE for different subpopulations of immigrants. However, since these LATE (i.e. point estimates of β) are of similar size and do not strongly deviate from OLS estimates, the combination of the regressions presented in the last section yield in our view a consistent picture about the average effect of immigration on natives.

5.2 Native professional advancements and the effects of immigration

An important concern in all studies applying the area approach to estimate the effects of immigration on resident workers are native outflows, i.e. the possibility that resident workers might respond to immigrant inflows by changing the labor market region. If such employee movements across the preassigned labor market cells occur, the impact of immigration into a cell would spread to other labor market regions and thus influence the outcome in the control group, biasing the estimates of the effect of immigration downwards.

Previous studies using occupation-experience cells [such as Friedberg, 2001] argue explicitly or implicitly that native outflows are a less prevalent concern when using occupation cells, as it is harder to change occupation than the own regional labor market as a response to large immigrant inflows. In fact, to our knowledge no study defining cells in terms of occupational groups has analyzed the importance of native outflows.

A recent study by Peri and Sparber [2009], however, provides evidence that this might be wrong. They show that residents change to more communication-intensive tasks as a response to immigration, leaving manual work to immigrants. More generally, resident employees might change to another occupational group not only because of displacement effects, but also in the case of complementarity between immigrant and native employees, as in this case immigrants might enable resident workers to grow professionally.

We therefore test the importance of native outflows given our definition of cells and exploit the fact that we observe if an employee changes to another occupational group from one year to another in the SLFS. Using the observation weights from the SLFS we approximate the number of native workers changing from one occupational group to another between two consecutive second quarters in years t and $t+1$ ($Outflows_{it}$). Since on average only 2.1% of all employees change to another ISCO major group annually, measurement error in the count of native outflows is an important problem. We therefore reduce the number of age categories by pooling age groups 1 and 2 (15–29 years), 3 to 5 (30–44 years), and 6 to 9 (45 and more). This approach has the additional advantage that it allows testing whether our results depend on the narrow definition of age categories used so far.

We then regress the count of outflows on the number of immigrating foreign workers entering the cell in year t, both relative to the past labor force (LF_{it-1}). The result of this regression is shown in Column 1 of Table 7. The positive estimate of β implies that immigration does indeed cause resident workers to change to another occupational group.

To gain more insights into the driving force behind the causal impact of immigration on outflows, we differentiate between "involuntary" and "voluntary" changes of the ISCO major group. We consider job changes that occur to an ISCO major group with an equal or lower required skill level as involuntary. Conversely, a voluntary change of the ISCO major group occurs if the employee moves into a higher ISCO skill level. Our data show that changing to next highest ISCO skill group is on average associated with a wage increase of 23.1%.[27]

The relationships between the actual immigration rate and voluntary and involuntary outflows, respectively, are visualized in Figure 4. The figure suggests no or even a negative relationship between involuntary job changes of natives and immigration, and a clear positive relationship between immigration and voluntary outflows of resident workers. The 2SLS regressions in Table 7 confirm that these correlations are indeed causal. More specifically, Columns 2 and 3 demonstrate that immigration negatively affects involuntary native outflows. On the other hand, the regressions in Columns 4 and 5 show that immigration has a positive causal impact on voluntary job changes of resident workers. This evidence is very robust and does not require instrumenting the immigration variable. Since we instrument immigration using the shift-share instruments, we can also rule out reverse causality, i.e. that professional advancements of natives cause immigrant inflows. Hence, the results suggest that resident employees can fill more demanding jobs due to immigration of foreign workers into their occupational group.

The presence of native outflows in our setting poses the question how much our previous results are influenced by native outflows. This question is analyzed in Table 8, in which we present two different estimates of β: one if we estimate the model as above but with just 3 instead of 9 age groups, and another if we include the share of native outflows relative to the labor force occuring throughout year t as an additional control variable. The difference between the "basic" and the "full" specifications in

[27]The ISCO major groups are grouped into four skill levels. ISCO major group 2 (professionals) requires skill level 4, major group 3 (technicians and associate professionals) skill level 3, major groups 4 to 8 require skill level 2, and occupations in major group 9 (elementary occupations) only require elementary skills (level 1). We add major group 1 (legislators, senior officials, and managers) to the highest skill category. In our sample, mean log FTE monthly earnings are 8.32 on average in skill group 1, 8.49 in skill group 2, 8.8 in skill group 3, and 9.01 in skill group 4.

the table is that the latter control for a full set of occupation- and age-year effects.[28]

The first part of the table shows that the point estimate of β is uniformly less negative across the four estimated models if native outflows are held constant. This is because native outflows reduce unemployment in the cells, as is shown by the estimated coefficient on native outflows itself (also shown in the table). Since native outflows are increased due to immigration, part of the outcome-improving effect of immigration on unemployment estimated above is due to the fact that resident workers grow professionally due to immigration, therefore improving job opportunities of natives staying in the cell.

Concerning the effects of immigration on employment and wages, the estimates are more positive if native outflows are held constant. Pertaining to employment, this happens because native outflows reduce the number of employed persons in a cell one to one, and a regression that does not account for the number of persons leaving the cell due to immigration understates the true within-cell beneficial impacts of immigration on employment of residents. A similar reasoning can be applied when wages are the outcome considered: since those natives that leave the cell are likely to figure among those employees within the cell that earn relatively well, a regression that does not hold outflows fixed overstates the negative impact of immigration on wages.

5.3 Robustness to cross-cell effects

In two influential contributions, Ottaviano and Peri [2012] criticize previous empirical work on the effects of immigration on labor market outcomes of natives because it does not account for cross-skill group effects. Their critique is based on general equilibrium considerations: if different types of labor are imperfectly substitutable and the relative supply of a specific type of worker changes due to immigration, the

[28] Note that including these dummy variables reduces the impact of controlling for native outflows on the estimated β's, as particularly the occupation-year effects absorb large part of the variation in the share of outflows across cells.

demand for other types of labor and relative prices of goods will shift as well, leading to cross-cell effects of immigration on the outcome of interest. Another possible scenario for cross-cell effects in our empirical application is that immigration of high-skilled workers to Switzerland increases job opportunities of low-skilled workers because firms are prevented from outsourcing jobs or relocate to Switzerland because of the availability of qualified personnel.

The problem of such interactions between cells is that immigration into cell A may influence employment and wages in cell B. In this case, the use of cell B as a counterfactual for cell A without accounting for spillovers implies that we might under- or overestimate the counterfactual employment or wage change.[29] The "partial" elasticities estimated above (i.e., the effect of immigration on the outcome in the own cell) might be misleading.[30] In the jargon of the treatment effects literature, the bias occurs because cross-cell effects constitute a violation of the Stable Unit Value Treatment Assumption (SUTVA): the control group is treated as well, invalidating it as a proper control group absent any controls for the cross-cell effects in the regression.

Accounting for cross-cell effects, however, poses the problem that there are 81 times 80 possible spillover effects per year in our case with 81 skill groups. Given the available data, it is hence impossible to estimate the cross-cell effects without imposing *a priori* (homogeneity and exclusion) restrictions on the elasticities.[31] In the regression framework outlined above, controlling for cross-cell effects can be achieved by including weighted averages of the inflow of immigrants into other cells

[29]Suppose, for instance, that the employment prospects of construction workers are increased by the inflow of foreign engineers. As a result, both the number of resident and immigrant construction workers increases. Then our estimated elasticity of employment to immigration in the own cell would be biased upwards, as it is not the inflow of foreign construction workers that leads to increased employment of resident construction workers, but the inflow of engineers.

[30]Ottaviano and Peri [2012] distinguish the partial from the "total effect" of immigration, which takes into account all possible cross-cell interactions occurring after an inflow of foreign labor.

[31]Ottaviano and Peri [2012] and Manacorda et al. [2012] impose this structure by assuming a nested constant elasticity of substitution (CES) production function for which they estimate different elasticities of substitution.

as additional regressors, similar to what is commonly done in spatial econometrics. Intuitively, we estimate a reduced form regression that examines whether the labor market outcome of residents in cell i correlates with the weighted number of immigrants in other cells $j \neq i$.

We restrict ourselves to the estimation of two additional elasticities between natives and immigrants of differing cells, closely following the convention in the recent literature [cf. Card, 2009, Ottaviano and Peri, 2012, Manacorda et al., 2012]: an elasticity of the outcome to immigrants with different age in the same occupation (ϕ_{age}), and an elasticity of the outcome to immigrants of the same age but different occupational group (ϕ_{occ}). Formally, we estimate the following augmented model:

$$\begin{aligned} \Delta O_{it} = &\alpha + \beta(I_{it}/LF_{it-1}) + \phi_{age}\mathbf{AI_t} + \phi_{occ}\mathbf{OI_t} \\ &+ \gamma X_{it} + \tau T_t + \epsilon_{it} \end{aligned} \tag{5}$$

The new terms in Equation (5) compared to Equation (1) are $\phi_{age}\mathbf{AI_t}$ and $\phi_{occ}\mathbf{OI_t}$, where $\mathbf{I_t}$ is a 81×1 column vector in which the number of immigrants relative to native labor force into cells $i = 1, ..., 81$ (I_{it}/LF_{it-1}) are stacked up for any year t, and $\mathbf{A}$ and $\mathbf{O}$ are time-invariant weighting matrices of dimension 81×81 used to weight the number of immigrants in other cells with zero elements on the diagonal.

The coefficient ϕ_{age} captures wage and employment spillovers between immigrants and natives across age groups within an occupational group. The weights employed in the corresponding weighting matrix, $\mathbf{A}$, are just 1, and the matrix is constructed in such a way that $\phi_{age}\mathbf{AI_t}$ is the sum of all immigrant shocks (I_{it}/LF_{it-1}) in all other age groups within an occupational group. This choice mirrors our assumption that the substitutability between workers of different age groups within the same occupation is relatively homogenous.

The second type of cross-cell effects that we account for are spillovers across occupational groups (ϕ_{occ}). These might arise, on the one hand, because of labor shortage in specific occupations. For instance, since Switzerland is characterized by shortages of doctors and engineers, the increased availability of doctors and engineers might increase the number of jobs for nurses and construction workers, respectively. On the other hand, firms may substitute between different types of occupations as a reaction to changes in labor supply. In both cases, interactions between occupations are more likely if employees from different occupational groups are work colleagues. It is, conversely, very unlikely that for instance the employment prospects of construction workers are influenced by increased immigration of doctors. Following this line of reasoning, we can gauge the relative importance of spillovers between any pair of occupations by analyzing the relative frequencies with which members of the different occupational groups are workmates in different industries. This is the main idea underlying the construction of the weighting matrix $\mathbf{O}$. A detailed description of how we constructed this matrix is deferred to the Online Appendix.

Table 9 contains WLS and 2SLS estimation results for the augmented regression model according to Equation (5) for unemployment, employment, and wages. All regressions account for the standard set of control variables and year fixed effects.

The most important message from the table is that all partial elasticities of unemployment, employment and wages to immigration of foreign employees established above are robust to the inclusion of cross-cell effects. None of the point estimates changes significantly once spillover effects across cells are controlled for. These results suggests that possible failures of the SUTVA are not driving our previous estimates of β.

Furthermore, Columns 1 and 2 provide certain evidence of positive cross-cell effects on unemployment. In particular, immigration into the different occupational groups within the same age group, and also immigration into the same occupational but different age group slightly increases the number of unemployed natives.[32]

The reason why we do not control for occupation- and age-year effects in the regressions presented in the table is that, given the way we defined the weighting matrices **A** and **O**, these dummies are essentially multicollinear to our spillover coefficients, i.e. would soak up all identifying variation from our spillover regressors. Put differently, if one is willing to accept our homogeneity and exclusion restrictions about the spillover effects made above, the problem of cross-cell effects can essentially be controlled for by including occupation- and age-year effects in the regressions of interest.

6 Conclusions

The paper has developed a methodology which allows identifying the causal effects of immigration on the labor market outcomes of residents in a small open economy—a methodology which is broadly applicable beyond specific political events in the country of origin or policy changes in the destination country. Applying the proposed shift-share instruments, we have examined the effects of the recent large-scale inflow of foreign workers on the labor market situation of resident workers in Switzerland. We find that in the average labor market cell unemployment of residents has been reduced as a result of immigration of foreign employees, while employment, wages and labor force participation have not been negatively affected. We show that these findings are robust to the inclusion of cross-cell effects, and demonstrate that the labor market effects of immigration on resident workers are even more beneficial for the resident workforce once we account for the fact that resident workers can change to jobs that require higher skills due to immigration. By employing three instruments that exploit different parts of the variation in immigration, we seek to

[32]The magnitude of these spillovers can be compared with our estimates of β. Such a comparison suggests that the positive cross-cell effects are more than compensated by negative within-cell effects.

ensure that our results are externally valid, i.e. yield the average effect of all kinds of immigrants on resident workers and not just the local effect of a particular subgroup of immigrants represented by the instrument.

It has to be born in mind that our analysis is not a comprehensive cost-benefit analysis of the effects of immigration in Switzerland. It concentrates on the labor market effects of immigration and disregards many potential other impacts of immigration in Switzerland, for example on housing prices, the usage of public infrastructure, or the social security system. Even pertaining to the labor market effects of recent immigration, the study ignores two channels through which immigrant inflows might have influenced the labor market situation of resident employees. First, immigration increases demand which in turn fuels growth in GDP. Through demand-induced growth, immigration has stabilized the Swiss economy in recent years and limited the extent of job losses in Switzerland in the course of the financial crisis of 2008/2009 and was thus beneficial for the resident labor force on an aggregate level that is precluded from the above analysis due to the inclusion of year fixed effects. Second, immigration of high-skilled workers might have increased labor productivity in the economy, for example by increasing innovation activities within firms or by strengthening their competitiveness [Kerr et al., 2012, Peri, 2012]. The positive externalities on native employment arising from such productivity effects of immigration are only considered in this study to the extent to which they become manifest in wages or employment within a year.

If it is not enhanced productivity, what explains the beneficial impact of immigration on the resident workforce that we establish in the regressions above? In our view, the most important explanation is the reduction of skill shortages to which immigration led: only with the inflow of qualified immigrants have firms in Switzerland been able to keep and increase their global market share, thus securing the jobs of residents by, for instance, preventing firms from outsourcing jobs. Moreover, if immigration occurs due to labor shortage, we would not only expect that immigrating foreign workers are imperfect substitutes to the resident workforce, but might also expect that firms face reduced unit labor costs ceteris paribus (e.g., through decreased recruitment costs), causing an upward shift of their labor demand curve. While skill shortages are not equally drastic in all OECD economies, most economies are likely to suffer from skill shortages in some sectors and for those our findings underline the potentially beneficial effects of immigration.

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7 Appendix

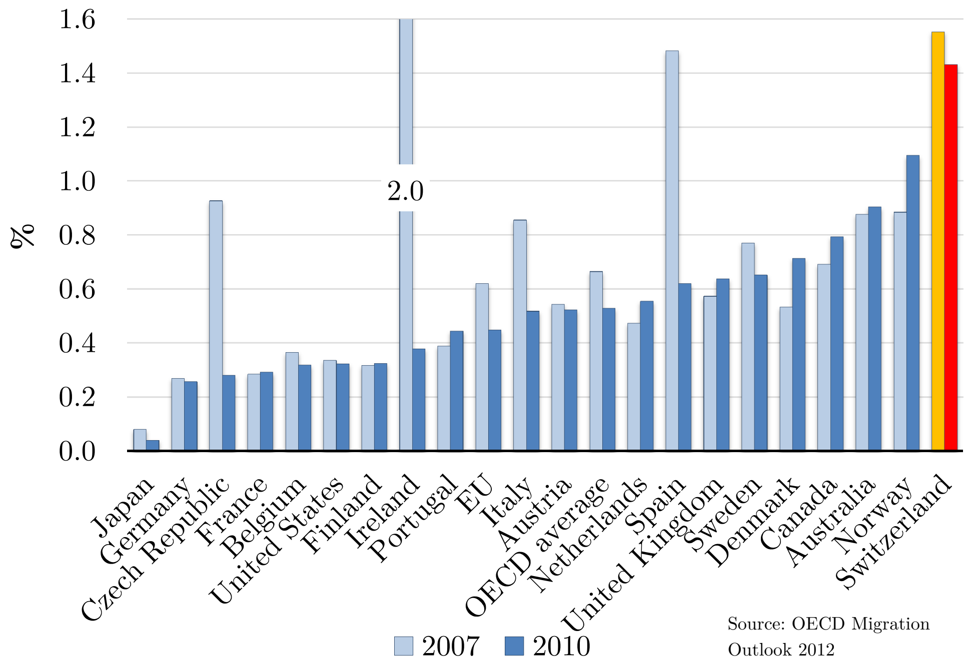

Figure 1: Permanent inflows into selected OECD and non-OECD countries 2007 and 2010 (as percentage of total resident population)

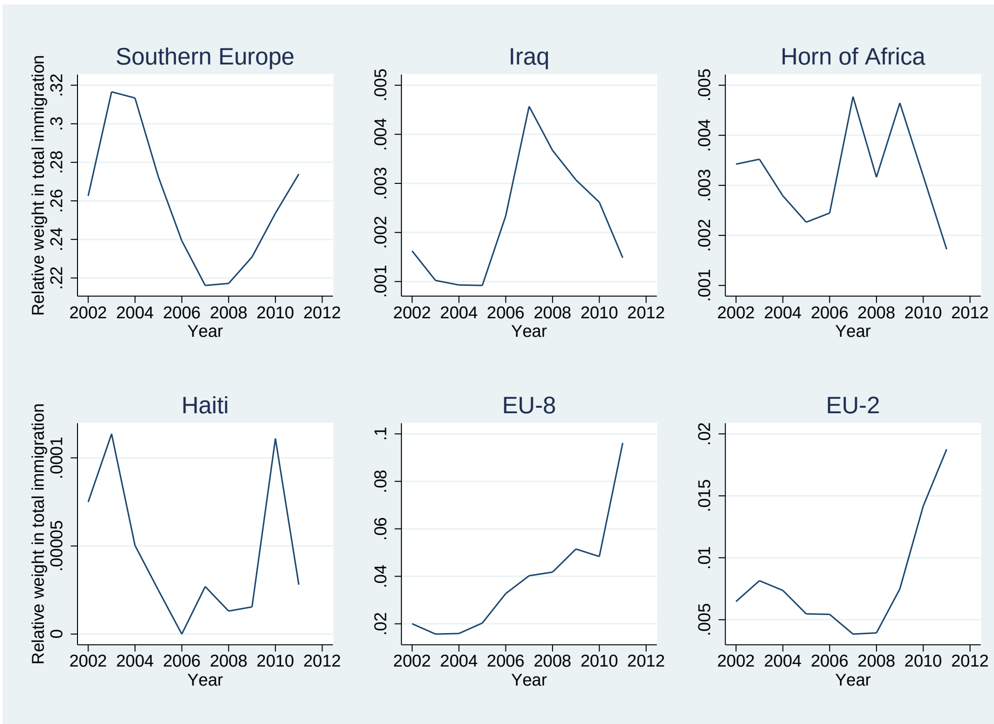

Figure 2: Share of immigrating foreign employees from certain regions of origin in total immigration of foreign employees to Switzerland

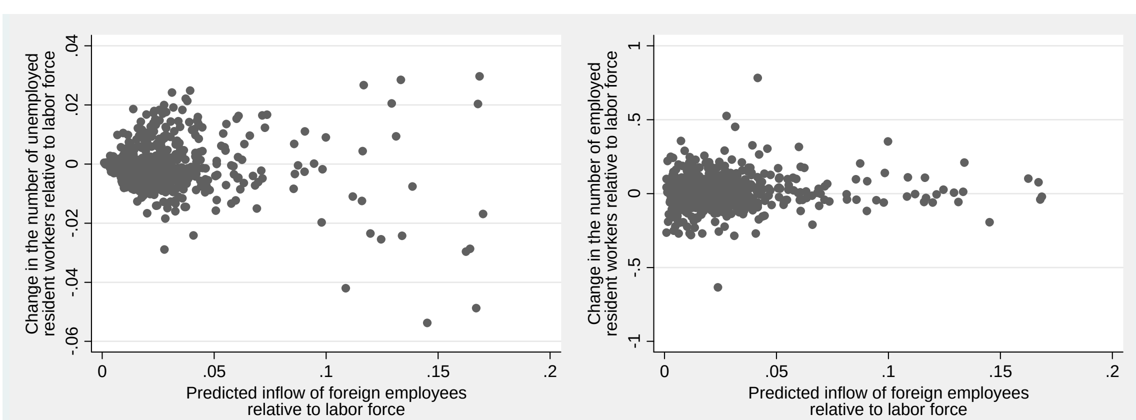

Figure 3: Relationship between predicted immigration rate and change in unemployment (left) and employment (right) of resident employees

Figure 4: The relation between actual immigration and involuntary (left) and voluntary (right) changes of ISCO major group by resident workers

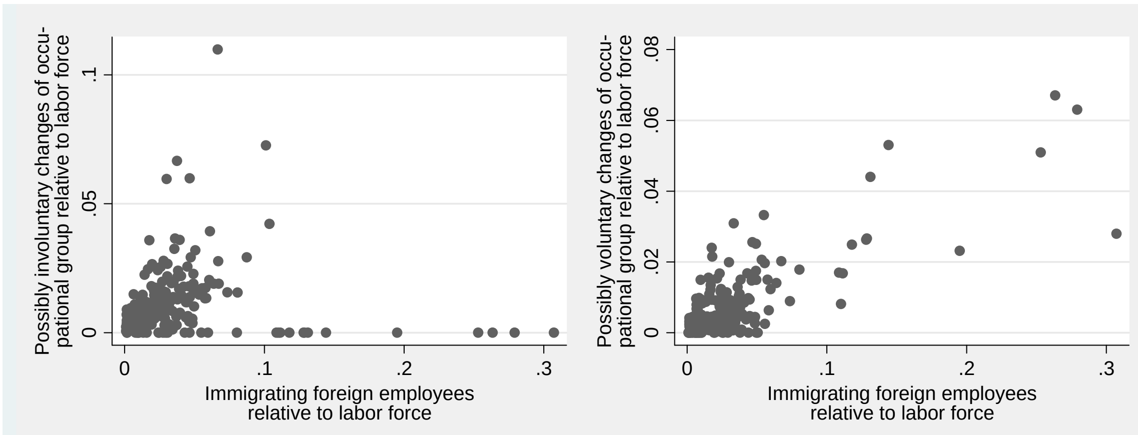

Table 1: Summary statistics

Variable	Mean	Std. Dev.	N
Number of unemployed	1,813	1,350	648
Number of employed	48,811	30,285	810
Average log monthly FTE wage	8.62	0.33	729
Immigrating employees (RP)	0.02	0.02	810
Immigrating employees (NRP)	0.03	0.05	810
New cross-border workers	0.01	0.02	810
Fraction of state employees	0.2	0.14	810
Fraction of female employees	0.43	0.21	810
Education	0.08	0.16	810
Cell-specific tenure (in years)	10.14	6.55	810

Table 2: Unemployment: Weighted least squares (WLS) estimations

VARIABLES	(1)	(2)	(3)	(4)	(5)
Immigrating employees	-0.027**	-0.027**	-0.036**	-0.048***	-0.047***
	(0.013)	(0.012)	(0.014)	(0.016)	(0.016)
Δ Fraction of state workers	0.054**	0.099***			
	(0.021)	(0.033)			
Δ Fraction of female employees	0.004	0.011	0.003	-0.002	-0.006
	(0.011)	(0.010)	(0.008)	(0.007)	(0.007)
Δ Education	-0.005**	-0.005**	0.000	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Job tenure	0.016*	0.018**	0.014*	-0.031	-0.049
	(0.009)	(0.008)	(0.007)	(0.023)	(0.031)
Job tenure squared	-0.047*	-0.053**	-0.048**	0.043	0.080
	(0.027)	(0.025)	(0.022)	(0.050)	(0.064)
Δ Age	-0.205	-0.124	-0.419	-0.420*	-0.280
	(0.330)	(0.299)	(0.283)	(0.223)	(0.216)
Δ Age squared	0.044	-0.047	0.256	0.425*	0.304
	(0.288)	(0.262)	(0.260)	(0.214)	(0.205)
Δ Unemployment $(t-1)$					0.043
					(0.063)
Constant	-0.003***	-0.002***	0.003**	0.001	0.012***
	(0.001)	(0.001)	(0.001)	(0.003)	(0.004)
Observations	648	648	648	648	567
R^2	0.620	0.578	0.804	0.894	0.898
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Occupation-year effects	No	No	Yes	Yes	Yes
Age-year effects	No	No	No	Yes	Yes
Weights	Yes	No	Yes	Yes	Yes

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Cell fixed effects are accounted for since the dependent variable is in first differences

Table 3: Unemployment: Two-stage least squares (2SLS) estimations

VARIABLES	(1) Total	(2) Total	(3) Total	(4) EU-25	(5) Total
Immigrating employees	-0.021	-0.034**	-0.046***		-0.036**
	(0.014)	(0.015)	(0.015)		(0.015)
Immigrating employees EU-25				-0.017	
				(0.012)	
Δ Fraction of state workers	0.056***				
	(0.020)				
Δ Fraction of female employees	0.004	0.003	-0.006	0.003	0.003
	(0.010)	(0.007)	(0.006)	(0.007)	(0.007)
Δ Education	-0.005***	0.000	0.000	0.000	0.000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Job tenure	0.019**	0.014**	-0.050*	0.024***	0.014**
	(0.009)	(0.007)	(0.027)	(0.006)	(0.007)
Job tenure squared	-0.052**	-0.049**	0.081	-0.070***	-0.048**
	(0.026)	(0.021)	(0.056)	(0.023)	(0.021)
Δ Age	-0.217	-0.420	-0.280	-0.435	-0.419
	(0.325)	(0.263)	(0.190)	(0.274)	(0.263)
Δ Age squared	0.056	0.257	0.304*	0.271	0.256
	(0.284)	(0.243)	(0.180)	(0.252)	(0.242)
Δ Unemployment $(t-1)$			0.044		
			(0.056)		
Constant	-0.003***			-0.006***	
	(0.001)			(0.001)	
Observations	648	648	567	648	648
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Occupation-year effects	No	Yes	Yes	Yes	Yes
Age-year effects	No	No	Yes	No	No
Weights	Yes	Yes	Yes	Yes	Yes
RMSE	0.00436	0.00313	0.00238	0.00316	0.00313
F statistic first stage	69.05	53.02	61.66	17.50	6755
p-value of Hansen J statistic	0.318	0.374	0.595	.	0.171

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Instruments: Predicted share of immigrating foreign employees
Columns 1–3: Census 1990
Column 4: Occupation-age distribution of labor force in country of origin (only EU-25)
Column 5: Average of immigration data 2002–2011
Cell fixed effects are accounted for since the dependent variable is in first differences

Table 4: Effect of immigration on employment of residents

VARIABLES	(1) WLS Total	(2) WLS Total	(3) 2SLS Total	(4) 2SLS EU-25	(5) 2SLS Total
Immigrating employees $(t-1)$	0.024	0.142	-0.040		0.028
	(0.089)	(0.152)	(0.115)		(0.136)
Immigrating employees EU-25 $(t-1)$				-0.085	
				(0.350)	
Δ Fraction of state workers	-0.611		-0.636	0.533	1.591
	(0.539)		(0.534)	(4.307)	(4.533)
Δ Fraction of female employees	0.146	-0.027	0.149	0.012	-0.026
	(0.157)	(0.178)	(0.154)	(0.156)	(0.156)
Δ Education	0.030	0.015	0.031	0.001	0.014
	(0.056)	(0.039)	(0.055)	(0.036)	(0.034)
Job tenure	0.217	-0.734	0.190	0.096	-0.604
	(0.157)	(0.573)	(0.158)	(0.195)	(0.498)
Job tenure squared	-0.278	0.270	-0.224	0.445	0.091
	(0.643)	(1.104)	(0.631)	(0.693)	(0.963)
Δ Age	-3.319	-2.880	-3.142	-3.090	-2.717
	(6.232)	(7.001)	(6.111)	(5.449)	(6.120)
Δ Age squared	4.494	6.767	4.338	5.845	6.657
	(6.625)	(6.592)	(6.500)	(5.642)	(5.765)
Δ Employment $(t-1)$		-0.261***		-0.199***	-0.260***
		(0.047)		(0.041)	(0.041)
Constant	-0.024**	0.203**	0.008		
	(0.010)	(0.085)	(0.016)		
Observations	729	729	729	729	729
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Occupation-year effects	No	Yes	No	Yes	Yes
Age-year effects	No	Yes	No	No	Yes
Weights	Yes	Yes	Yes	Yes	Yes
RMSE	0.0777	0.0738	0.0768	0.0702	0.0652
F statistic first stage	.	.	114.8	17.37	1404
p-value of Hansen J statistic	.	.	0.689	.	0.507

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Instruments: Predicted share of immigrating foreign employees
Column 3: Census 1990
Column 4: Occupation-age distribution of labor force in country of origin (only EU-25)
Column 5: Average of immigration data 2002–2011
Cell fixed effects are accounted for since the dependent variable is in first differences

Table 5: Effect of immigration on wages of residents

VARIABLES	(1) WLS Total	(2) WLS Total	(3) 2SLS Total	(4) 2SLS EU25	(5) 2SLS Total
Immigrating employees $(t-1)$	-0.021	-0.004	-0.005		-0.021
	(0.065)	(0.163)	(0.078)		(0.146)
Immigrating employees EU-25 $(t-1)$				-0.315	
				(0.228)	
Δ Fraction of state workers	0.165		0.169	1.012	0.521
	(0.364)		(0.357)	(1.389)	(1.608)
Δ Fraction of female employees	-0.298**	-0.317*	-0.299**	-0.286*	-0.316**
	(0.136)	(0.166)	(0.133)	(0.148)	(0.146)
Δ Education	-0.022	-0.001	-0.023	-0.006	-0.001
	(0.022)	(0.036)	(0.021)	(0.026)	(0.031)
Job tenure	0.137	0.545	0.144	0.069	0.565
	(0.112)	(0.469)	(0.107)	(0.154)	(0.415)
Job tenure squared	-0.397	-1.470	-0.411	-0.320	-1.497*
	(0.428)	(0.967)	(0.415)	(0.514)	(0.851)
Δ Age	5.614	3.777	5.579	2.277	3.778
	(7.709)	(5.760)	(7.544)	(5.237)	(5.052)
Δ Age squared	-2.717	-1.729	-2.685	-0.919	-1.729
	(6.722)	(4.770)	(6.579)	(4.366)	(4.184)
Δ Wage $(t-1)$		-0.463***		-0.450***	-0.463***
		(0.045)		(0.042)	(0.040)
Constant	-0.019	0.025	-0.020		
	(0.014)	(0.074)	(0.013)		
Observations	648	648	648	648	648
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Occupation-year effects	No	Yes	No	Yes	Yes
Age-year effects	No	Yes	No	No	Yes
Weights	Yes	Yes	Yes	Yes	Yes
RMSE	0.0759	0.0696	0.0749	0.0643	0.0614
F statistic first stage	.	.	91.15	16.54	2286
p-value of Hansen J statistic	.	.	0.778	.	0.304

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Cell fixed effects are accounted for since the dependent variable is in first differences
Instruments: Predicted share of immigrating foreign employees
Column 3: Census 1990
Column 4: Occupation-age distribution of labor force in country of origin (only EU-25)
Column 5: Average of immigration data 2002–2011

Table 6: Subgroup-specific first-stage estimates for the three different shift-share instruments

Instrument		*High-skilled (ISCO 1–3)*		*Low-skilled (ISCO 4–9)*	
		Aged 15–39	*Aged 40–65*	*Aged 15–39*	*Aged 40–65*
Census 1990	$\hat{\alpha}$	2.081	0.233	1.898	0.265
	t-value	2.91	1.22	12.37	1.82
Labor force in country of origin	$\hat{\alpha}$	1.272	-0.191	1.583	0.205
	t-value	3.58	-1.03	2.11	1.16
Average of immigration data	$\hat{\alpha}$	1.1	1.041	1.101	1.063
	t-value	50.68	54.49	126	36.79

Table 7: Effect of immigration on native outflows

VARIABLES	(1) 2SLS All Outflows	(2) 2SLS Inv. Outflows	(3) 2SLS Inv. Outflows	(4) 2SLS Vol. Outflows	(5) 2SLS Vol. Outflows
Immigrating employees	0.089***	-0.098***	-0.055**	0.187***	0.168***
	(0.015)	(0.021)	(0.022)	(0.022)	(0.021)
Observations	243	243	243	243	243
Occupation-year effects	Yes	Yes	Yes	Yes	Yes
Age-year effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes
RMSE	0.00475	0.00392	0.00390	0.00329	0.00327
F statistic first stage	55.83	55.83	154.3	55.83	10347
p-value of Hansen J statistic	0.922	0.862	0.129	0.799	0.259

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Instruments: Predicted share of immigrating foreign employees

Columns 1+2+4: Census 1990

Columns 3+5: Average of immigration data 2002–2011

Controls and constant not shown

Table 8: The influence of native outflows on the estimated parameters of interest

	WLS Basic (1)	WLS Full (2)	2SLS Basic (3)	2SLS Full (4)
Unemployment				
β: Standard	-0.031**	-0.055***	-0.029**	-0.061***
	(0.012)	(0.014)	(0.013)	(0.01)
β: Accounting for native outflows	-0.011	-0.049***	-0.011	-0.053***
	(0.01)	(0.013)	(0.01)	(0.009)
Coefficient on outflows	-0.17***	-0.046	-0.171***	-0.042**
	(0.037)	(0.028)	(0.035)	(0.021)
Employment				
β: Standard	0.006	-0.139	0.121	-0.095
	(0.079)	(0.083)	(0.108)	(0.059)
β: Accounting for native outflows	0.096	-0.12	0.209	-0.072
	(0.101)	(0.098)	(0.132)	(0.07)
Coefficient on outflows	-0.875**	-0.216	-0.991**	-0.246
	(0.46)	(0.36)	(0.455)	(0.264)
Wages				
β: Standard	0.011	0.167	0.04	0.231**
	(0.057)	(0.115)	(0.062)	(0.096)
β: Accounting for native outflows	0.082	0.252*	0.108	0.304***
	(0.091)	(0.138)	(0.102)	(0.111)
Coefficient on outflows	-0.71	-1.125*	-0.735	-1.153**
	(0.548)	(0.601)	(0.55)	(0.448)

Table 9: Controlling for spillover effects

VARIABLES	(1) WLS Δ U	(2) 2SLS Δ U	(3) WLS Δ E	(4) 2SLS Δ E	(5) WLS Δ w	(6) 2SLS Δ w
Immigrating employees	-0.041***	-0.036**				
	(0.014)	(0.015)				
$\hat{\phi}_{age}$ total inflow	0.004***	0.003***				
	(0.001)	(0.001)				
$\hat{\phi}_{occ}$ total inflow	0.006***	0.006***				
	(0.002)	(0.002)				
Immigrating employees $(t-1)$			-0.074	-0.212	-0.082	-0.045
			(0.121)	(0.176)	(0.084)	(0.112)
$\hat{\phi}_{age}$ total inflow $(t-1)$			0.036**	0.050***	0.014	0.011
			(0.016)	(0.017)	(0.011)	(0.013)
$\hat{\phi}_{occ}$ total inflow $(t-1)$			-0.035	-0.023	0.034	0.031
			(0.032)	(0.030)	(0.023)	(0.024)
Observations	648	648	729	729	648	648
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
RMSE	0.00438	0.00432	0.0775	0.0765	0.0760	0.0749
F statistic first stage	.	84.49	.	67.89	.	44.90
p-value of Hansen J statistic	.	0.139	.	0.116	.	0.619

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Instruments: Change in predicted share of immigrating foreign employees (Census 1990)

Additional control variables and constant not shown

A Online Appendix

A.1 The "new immigration wave" to Switzerland

As mentioned in the main paper, immigration to Switzerland has been substantial in recent years. From 2002 to 2011, the foreign labor force grew by 351,737 persons (+32.8%) in Switzerland, which is a substantial influx in relation to the total of 4.27 million employees as of January 2002. The "new immigration wave" to Switzerland is linked to the enactment of a Free Movement of Persons Treaty (FMP) with the EU/EFTA states in June 2002 that gradually led to a full liberation of immigration from these countries. Apart from increasing the number of immigrants to Switzerland, the FMP is also likely to have had two qualitative effects on immigration Switzerland.

On the one hand, the FMP shifted the region of origin of immigration: while most net migration to Switzerland in the 1990s could be attributed to non-EU European, especially ex Yugoslavian countries, 75% of the net increase in the foreign resident population between 2000 and 2010 is due to immigration from EU member countries.

On the other hand, the FMP is also likely to have contributed to the marked increase in importance of high-skilled immigration to Switzerland [SECO et al., 2012]. Since 2002, the average formal qualifications of the immigrants exceed those of the resident workforce: while only 20% of the permanent immigrants to Switzerland from 1986 to 1995 had a tertiary education, this share was 51% [SECO et al., 2012] from 2002 to 2011. In 2010, 52% of all German citizens between 25 and 64 years living in Switzerland had a tertiary education compared to 29.6% within Switzerland's population of this age according to the Swiss Labor Force Survey. However, low-skilled immigrants remain a sizable fraction of all immigrants, and in absolute terms their number also increased compared to the nineties.

A.2 First stage estimations

This section presents a table of characteristic first-stage regressions to the 2SLS estimations presented in the paper. The first-stage regressions shown here are similar to those underlying Table 3, but do not completely correspond to them, as we do not include the instruments (i.e. the predicted number of immigrants according to Equation (4)) separately for the EU-15, EU-8 and "the rest of the World" countries, but include just the sum of them as instrument. We do this for the sake of brevity of the table.

Table A.1: First stage estimations of unemployment

VARIABLES	(1) Total	(2) EU-25	(3) Total	(4) Total	(5) Total
Pred. immigrating employees (*in-sample*)	1.106***				1.159***
	(0.016)				(0.012)
Pred. immigrating employees (Labor force)		1.279***			
		(0.306)			
Pred. immigrating employees (*in-sample*)			1.755***	1.714***	
			(0.202)	(0.221)	
Δ Fraction of female employees	-0.011	-0.015	0.016	0.013	-0.012
	(0.007)	(0.023)	(0.020)	(0.018)	(0.008)
Δ Education	-0.000	0.001	0.004	0.002	0.000
	(0.001)	(0.004)	(0.005)	(0.005)	(0.001)
Job tenure	0.014*	-0.524***	-0.388***	0.189	-0.081***
	(0.008)	(0.107)	(0.060)	(0.212)	(0.030)
Job tenure squared	-0.016	1.582***	1.047***	-0.180	0.129**
	(0.021)	(0.480)	(0.214)	(0.391)	(0.055)
Δ Age	-0.004	1.205*	-0.383	-0.341	0.213
	(0.231)	(0.715)	(0.456)	(0.447)	(0.383)
Δ Age squared	0.043	-0.770	0.564	0.356	-0.160
	(0.184)	(0.622)	(0.436)	(0.386)	(0.311)
Δ Unemployment $(t-1)$				-0.485	0.150***
				(0.301)	(0.027)
Constant	0.002	0.073***	-0.022	-0.069**	0.010**
	(0.002)	(0.021)	(0.015)	(0.028)	(0.005)
Observations	648	648	648	567	567
R^2	0.992	0.672	0.866	0.918	0.995
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Occupation-year effects	Yes	Yes	Yes	Yes	Yes
Age-year effects	No	No	No	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes
F statistic	4669	17.50	75.23	60.30	8855
Partial R^2	0.982	0.240	0.695	0.666	0.978

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
In italics: method to distribute country-specific number of immigrants across cells
NRP = Non-resident population

A.3 Outflows of the labor force

This section examines the question whether our results—especially that immigration reduces unemployment of resident workers—are driven by unemployed or employed workers leaving the labor force. In order to do this, we compute using the Swiss Labor Force Survey the cell-specific number of persons that leave the labor force from year t to the following year in our cells, expressed in relation to the one-year lagged labor force of the cell ($OutflowsLF_{it}/LF_{it-1}$). The results of regressions of Equation (1) of the paper using this variable on the left-hand side are shown in Table A.2. They suggest that immigration of foreign workers does not discourage workers from participating in the labor market. The 2SLS estimations even provide weak evidence that outflows would be larger absent immigration.

Table A.2: The impact of immigration on the share of residents leaving the labor force

VARIABLES	(1) WLS Total	(2) WLS Total	(3) 2SLS Total	(4) 2SLS EU-25	(5) 2SLS Total
Immigrating employees	0.085	0.005	-0.308		-0.170
	(0.058)	(0.081)	(0.226)		(0.159)
Immigrating employees EU-25				-0.482*	
				(0.275)	
Δ Fraction of state workers	0.210		-2.555**	0.012	-1.711*
	(0.173)		(1.202)	(0.199)	(0.930)
Δ Fraction of female employees	0.018	-0.015	0.068	0.028	0.067
	(0.065)	(0.027)	(0.057)	(0.065)	(0.056)
Δ Education	0.014	0.003	0.012	0.018	0.012
	(0.014)	(0.005)	(0.013)	(0.015)	(0.013)
Job tenure	-0.668***	-0.059	-0.934***	-0.886***	-0.870***
	(0.170)	(0.350)	(0.159)	(0.152)	(0.149)
Job tenure squared	4.278***	0.459	5.232***	4.689***	5.091***
	(0.773)	(0.836)	(0.746)	(0.736)	(0.731)
Δ Age	-1.763	-1.014	-1.955*	-0.108	-2.206*
	(1.331)	(1.062)	(1.184)	(1.508)	(1.155)
Δ Age squared	1.649	0.792	1.843	0.203	2.048*
	(1.432)	(1.127)	(1.272)	(1.576)	(1.234)
Constant	0.044***	0.194***		0.040***	
	(0.007)	(0.042)		(0.012)	
Observations	810	810	810	810	810
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Occupation-year effects	No	Yes	Yes	No	Yes
Age-year effects	No	Yes	No	No	No
Weights	Yes	Yes	Yes	Yes	Yes
RMSE	0.0360	0.0159	0.0327	0.0379	0.0325
F statistic first stage	.	.	93.68	13.24	2631
p-value of Hansen J statistic	.	.	0.494	.	0.701

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Instruments: Predicted share of immigrating foreign employees
Column 3: Census 1990
Column 4: Occupation-age distribution of labor force in country of origin (only EU-25)
Column 5: Average of immigration data 2002–2011
Cell fixed effects are accounted for since the dependent variable is in first differences

A.4 Heterogeneous effects

In Table A.3, we examine the question whether there are heterogeneous effects of the inflow of permanent foreign employees on unemployment and wages, respectively, for certain subgroups of the resident population. All coefficients are derived from separate 2SLS estimations for each of the sub-populations. In light of the discussion in Section 5.1 of the paper, we apply the shift-share instrument that uses the immigration data itself to compute the shares.

First, the table shows that we find no statistically significant effects of immigration on wages for each of the sub-populations, except a statistically significant negative effect on wages of male resident employees.

Pertaining to unemployment, the table demonstrates that not all resident employees have equally gained from the immigrant inflow—although there were apparently no losers. Similar to other studies that analyze recent immigration to Switzerland (Henneberger and Ziegler [2011], SECO et al. [2012]), we find evidence that the labor market situation of foreign resident employees in Switzerland is less positively affected by the influx of foreigners, suggesting that foreign workers are closer substitutes to immigrating employees than Swiss citizens. Pertaining to skill groups, high-skilled workers do not seem to gain from immigration, while low-skilled workers profit in terms of unemployment. A similar pattern arises when estimating separate regressions along the age dimension, i.e. young workers seem to benefit more from the immigrant inflow than older workers.

Moreover, we also analyze the question whether the effect of immigration has been stable over time. In particular, the free movement regime with the EU-15 countries became only fully effective in mid-2007. Prior to this date, filling a vacancy with an employee from an EU-15 country was still partially restricted, particularly because employers had to provide evidence not to find a resident employee for the job (until 2004) and because of binding quotas on the total number of immigrants. These regulations expired after June 2007. Since then, EU-15 and Swiss workers are on equal legal footing on the labor market. Particularly in the case of unemployment, the change in the migration law is indeed mirrored in the estimated elasticities: the beneficial impact of inflows of employees from EU-15 countries on native unemployment is smaller after compared to before 2007. This result suggests that studies that examine the effects of the introduction of the FMP on Switzerland's labor market should separately evaluate the immigration impact on the resident labor force before and after 2007. Most existing studies on the effects of the FMP on the labor market only consider June 2002 as the important threshold date.

Table A.3: Estimated impact of total inflow of foreign workers on unemployment and wages for different subgroups of the resident population (2SLS estimations, instrument set used: average of immigration data 2002–2011)

	$\hat{\beta}$	S.E		$\hat{\beta}$	S.E
Unemployment					
Swiss citizens	-0.023	(0.006)	Foreigners	-0.015	(0.009)
High-skilled workers (ISCO 1–3)	-0.007	(0.009)	Low-skilled workers (ISCO 4–9)	-0.036	(0.015)
Young workers (age 15–40)	-0.034	(0.018)	Old workers (age 40–65)	-0.007	(0.015)
Immigration from EU-15 (2004–2007)	-0.078	(0.02)	Immigration from EU-15 (2008–2011)	-0.025	(0.016)
Permanent immigrants	-0.035	(0.015)	Short-term immigrants*	-0.047	(0.014)
Wages					
Swiss citizens	-0.056	(0.212)	Foreigners	0.036	(0.091)
High-skilled workers (ISCO 1–3)	0.174	(0.285)	Low-skilled workers (ISCO 4–9)	-0.024	(0.132)
Young workers (age 15–40)	-0.076	(0.174)	Old workers (age 40–65)	-0.33	(0.314)
Male workers	-0.298	(0.091)	Female workers	0.219	(0.195)
Immigration from EU-15 (2002–2007)	-0.198	(0.186)	Immigration from EU-15 (2008–2011)	0.108	(0.172)
Permanent immigrants	-0.045	(0.109)	Short-term immigrants*	-0.039	(0.1)

Notes: Short-term immigrants are immigrants with a residency permit of less than one year

A.5 Static versus dynamic effects of immigration

Two recent papers have highlighted that the “constant”, or static effects established in the paper might hide that the impact of immigration on labor market outcomes substantially changes over time [Wozniak and Murray, 2012, Cohen-Goldner and Paserman, 2011]. To examine this issue, we closely adapt a specification proposed by Cohen-Goldner and Paserman [2011] (their Equation (5)) and modify our model in the following way[33]:

$$\Delta O_{it} = \alpha + \beta_1(I_{it}/LF_{i2002}) + \beta_2(\sum_{s=1}^{5} I_{it-s}/LF_{i2002}) + \gamma X_{it} + \tau T_t + \epsilon_{it} \qquad (6)$$

The modification compared to the specification used in the paper is that the new model estimates two separate coefficients for the effect of immigration within the current year (β_1) and for the effect of immigration that occurred one to five years before the outcome is measured (β_2). We interpret β_1 as the short-run, and β_2 as the medium-run impact of immigration on the outcomes.[34]

An important difference between the approach in Cohen-Goldner and Paserman [2011] and our approach is that we assign immigrants to labor market cells at the time they came to Switzerland (i.e. in $t-s$). They, on the contrary, allocate immigrants to labor market cells at the time they measure the outcome (i.e. at t) and use information from the labor force survey in order to reconstruct the time of immigration ($t-s$). The advantage of their approach is that they correctly assign immigrants to their actual labor market cells if they should change their skill group after they immigrated. The disadvantage of their approach is the type of

[33]We have also tried other specifications proposed in Cohen-Goldner and Paserman [2011] with qualitatively similar results.

[34]Note also that the model now uses the labor force in 2002 as a common base year to normalize both, the central independent variable and our dependent variables (i.e. $\Delta O_{it} = (U_{it} - U_{it-1})/LF_{i2002}$ and $\Delta O_{it} = (E_{it} - E_{it-1})/LF_{i2002}$). In order to avoid loosing five cross-sections, the sum $\sum_{s=1}^{5} I_{it-s}/LF_{i2002}$ is set to 0 if $t = 2002$, to I_{i2002}/LF_{i2002} if $t = 2003$, to $(I_{i2002} + I_{i2003})/LF_{i2002}$ if $t = 2004$, and so on.

self-selection problem discussed in their paper, which is not shared by our approach. The disadvantage of our approach, however, is that our medium-run estimates are likely to be imprecisely estimated because immigrants may have left the country or may have changed broad occupational group.[35]

Table A.4 summarizes the results from the regressions of Equation (6) on the three outcome variables. The important message from the table is that the static effects which constrain β to be constant across time hide differences in the short- and medium-run impact of immigration. In particular, the short-run impact of immigration on all outcome variables is clearly outcome-improving, but the positive impact is lowered the longer the immigrants are in Switzerland's labor market. This is consistent with the fact that resident workers and immigrants initially have relatively complementary skills, but progressively become closer substitutes as immigrants acquire local human capital, resulting in an outcome-deteriorating medium-run effect of immigration on the labor market outcomes of natives [Cohen-Goldner and Paserman, 2011].

[35]The latter is, of course, mechanically true along the age dimension of our skill groups, i.e. immigrants "change" to a higher experience group after certain years of staying in Switzerland. Since we do not know the exact age of the immigrants but just to which five-year age category they belong, our data does not allow correctly assigning immigrants to different age groups after the year of their immigration. There are several ad hoc ways to reduce this problem, but we do it by simply expanding our age groups bins as in Chapter 5.2.

Table A.4: Static and dynamic effects of immigration on labor market outcomes

	Static effect	**Dynamic effects**					
	WLS (1)	WLS (2)		2SLS (3)		2SLS (4)	
	β	Short-run effect (β_1)	Medium-run effect (β_2)	Short-run effect (β_1)	Medium-run effect (β_2)	Short-run effect (β_1)	Medium-run effect (β_2)
				Unemployment			
Coefficient	-0.058***	0.008	-0.019***	-0.128	0.018	-0.098	0.013
S.E.	(0.006)	(0.017)	(0.004)	(0.101)	(0.028)	(0.08)	(0.024)
				Employment			
Coefficient	-0.087	0.17	-0.11*	0.428	-0.182**	0.411	-0.169*
S.E.	(0.093)	(0.205)	(0.054)	(0.312)	(0.089)	(0.31)	(0.087)
				Wages			
Coefficient	0.314***	1.099**	-0.35*	1.464***	-0.462***	1.346***	-0.38**
S.E.	(0.085)	(0.519)	(0.183)	(0.412)	(0.144)	(0.487)	(0.165)

Notes: Dependent variables are the change in number of unemployed or employed resident workers relative to the labor force in 2002, and the change in log wages, respectively. Estimations include the standard set of control variables and age- and occupation-year effects. Estimations in Columns (1) and (4) control for native outflows. Instruments in Columns (1), (3) and (4) use census 1990 shares

A.6 Weighting matrix O

This section documents the construction of the weighting matrix **O** used to weight cross-occupational effects of immigration. As mentioned in the main text, we argue that the extent to which immigration into one occupational group may affect outcomes in another occupational group is related to the extent to which employees from the two cells are colleagues in the same industry. We estimate these relationships from the data. In particular, for any industry on the 2-digit level (NACE reform 1.1), we count the number of employees with occupational group i working together with employees of occupational group $j \neq i$ in the same industry. The relevant data for this exercise comes from the waves 2002 through 2011 of the Swiss Labor Force Survey. By aggregating over all industries, we then compute the economy-wide frequencies $f_{occ=i,occ=j}$, indicating, for any occupational group i, the relative importance of workplace ties with occupation j. The resulting weighting matrix applied to each of the age groups is shown in Table A.5. As can be seen in the table, the probability that a manager works together with a professional worker is 18%. The reverse probability is only 9%. The asymmetry arises because the size of the own group matters: the probability that a professional worker works with a manager is smaller than the reverse because managers are a smaller group.

Table A.5: Importance of workplace ties across occupational groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Managers (ISCO 1)	0	.18	.21	.15	.18	.02	.15	.06	.07
Professionals (2)	.09	0	.37	.15	.13	.01	.11	.04	.094
Technicians (3)	.09	.30	0	.17	.19	.01	.11	.04	.08
Clerks (4)	.09	.20	.25	0	.15	.02	.15	.07	.08
Service workers (5)	.13	.16	.27	.15	0	.02	.13	.04	.10
Skilled agricultural workers (6)	.06	.12	.16	.12	.13	0	.12	.05	.25
Craft workers(7)	.10	.15	.20	.17	.11	.02	0	.14	.10
Plant operators (8)	.08	.14	.16	.17	.10	.01	.26	0	.08
Elementary occupations (9)	.07	.20	.21	.13	.16	.04	.13	.06	0