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Migration, Trade and Unemployment

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Abstract A source of anxiety of policy makers and the public in general is the detrimental impact of trade and immigration on unemployment. The transitory restrictions for worker migration after the EU enlargements of 2004 and 2007 exemplify the supposed negative effect of immigration on labor markets. This paper aims to identify the effects of immigration alongside trade on unemployment controlling for the high correlation between immigration and goods flows in order to prevent an omitted variable bias. The authors use data from 24 OECD countries over the period from 1997 to 2007 and employ instrumental variables fixed effects and dynamic panel estimators in order to account for unobserved heterogeneity as well as the potential endogeneity of migration flows and the high persistence of unemployment. We find no significant effect of immigration on unemployment on average.

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1 Introduction

Does immigration lead to higher unemployment rates in the destination country? As immigration and trade exposure of a country are highly correlated, it is the aim of this paper to study the effect of immigration on unemployment in OECD countries explicitly taking into account trade volumes of receiving countries.

This question is of eminent political importance as its answer, or at least what policy makers perceive as its correct answer, has direct consequences for millions of potential migrants across the globe. For example, as a reaction to rising unemployment rates in the wake of the financial crisis, several countries implemented voluntary return programs (VRPs) for migrants with entitlements to domestic unemployment benefit schemes. These programs offered financial incentives like a free one way return ticket as well as lump sum payments if immigrants left the host country and did not return for at least three years. Even though few of the migrants eligible for the programs did actually participate, according to Manzano and Vaccaro (2009), the Spanish government spent €21 million in 2009 on this kind of program.[1]

At the European level, in the vein of the last two enlargements of the European Union, both treaties of accession[2] contained clauses about a transition period before workers from the new member states could be employed on equal, non-discriminatory terms in the old member states as policy makers feared negative effects on labor markets in the EU-15 countries. The old member states had the possibility to impose restrictions for worker immigration for a transitional period of two years. Afterwards, they could decide to extend it for another three years. After five years, if the country informed the European Commission of serious disruptions on its labor market the period could be extended for the last time for

[1] Besides Spain also the Czech Republic and Japan have introduced VRPs. For further details see Fix et al. (2009).

[2] The “Treaty of Accession 2003” was the agreement between the European Union and ten countries (Czech Republic, Estonia, Cyprus, Latvia, Lithuania, Hungary, Malta, Poland, Slovenia, Slovak Republic) concerning these countries’ accession into the EU that took place 2004. The “Treaty of Accession 2005” is an agreement between the European Union and Bulgaria and Romania concerning accession into the EU of the latter two countries that took place 2007.

two more years.[3] Austria and Germany were the only member states which used up the whole seven year period for shutting off their labor markets from inflows from eight of the ten accession countries from 2004 (from all but Malta and Cyprus). This seven year period ended on May 1st, 2011.

How did Austria and Germany actually argue for the serious disruption on their labor markets? Basically, two arguments where brought forward defending transitional immigration restrictions. First, Germany's State Secretary for Employment Gerd Andres defended Germany's decision to maintain restrictions by pointing out that the disruptions brought about by adjustment effects would be too high without the transitional restrictions. Second, he argued that "the geographical position is very different for Germany and Austria than it is for France or the UK".[4] EU-Employment Commissioner Vladimir Špidla accepted the application for prolongation of the restrictions from both Austria and Germany by arguing that both countries "are undergoing serious disturbance of their labour markets as a consequence of the general economic downturn."[5] In essence, the reports to the European Commission only argued for the supposedly existing disruptive consequences of what was perceived as a premature opening of labor markets. However, to the best of our knowledge, no evidence was provided which would back up the causal link between higher immigration and unemployment or any other detrimental labor market effects. The causality, it seems, was taken for granted.[6]

This example illustrates the widely held belief that on average, immigration has detrimental effects on the labor market in the destination country.[7] This is in contrast with much of the current empirical evaluations of the effects of

[3] For more details, see European Commission: Employment, Social Affairs and Inclusion. Enlargement–transitional provisions. Retrieved on 09/02/2012 from http://ec.europa.eu/social/main.jsp?catId=466&langId=en.

[4] EurActiv.com (2009). Free movement of labour in the EU 27. *EurActiv.com*. Retrieved on 09/02/2012 from http://www.euractiv.com/socialeurope/free-movement-labour-eu-27/article-129648.

[5] Slegers, M. (2009). Spidla "accepts" German and Austrian labour market move. *Europolitics*. Retrieved on 07/02/2012 from http://www.europolitics.info/spidla-accepts-german-and-austrian-labour-market-move-artr239442-25.html.

[6] See European Commission (2006).

[7] Using European Social Survey data, Dustmann and Preston (2004) show that EU citizens believe that average wages are brought down by immigrants. In addition, even though Europeans do not

migration on wages of domestic workers in the destination country. These studies can be grouped into three types. The first uses the elementary model of labor demand and carries out simulations in order to quantify the effects (see for example Borjas (1999)). The second approach uses natural experiments, i.e. supposedly exogenous inflows of migrants, like a short episode of easier Cuban immigration to Miami (Mariel boat lift study by Card (1990)) or the immigration to France in the wake of the Algerian independence (Hunt (1992)).[8] The third approach estimates parameters of a regression of (changes) in wages or employment on the number of migrants and a set of control variables to identify the causal effect of immigration (Borjas, Freeman, and Katz (1997), Borjas (1999), and Friedberg and Hunt (1995)). All three approaches usually find very modest effects of immigration on workers in the destination country.[9] Not surprisingly, the Czech government opposed the prolongation of immigration restrictions in Germany and Austria as these were against "available evidence".[10]

All these empirical studies of the effects of immigration were done by labor economists. To the contrary, analysis of the process of European integration, or more broadly globalization in general, typically falls in the domain of trade economists. While trade economists paid only scant attention to labor market frictions for a long time, the effects of globalization on unemployment featured more prominently in recent trade models. This recent literature focuses on models with heterogeneous firms and increasing returns to scale (see Egger and Kreickemeier (2009, 2011), Felbermayr, Prat, and Schmerer (2011a), Helpman and Itskhoki (2010), Helpman, Itskhoki, and Redding (2009, 2010a, 2010b)). One of

think that immigrants take away jobs from domestic workers, they do not think that immigration can relieve labor shortages.

[8] Recent studies have also used the mass inflow of German expellees into West Germany after World War II and of ethnic Germans from former socialist countries after the fall of the Iron Curtain as quasi-natural experiments to identify the causal labor market effects of immigration (see Braun and Mahmoud (2011) and Glitz (2012)). Also internal migration caused by the Great Depression in the US during the 1930s has been identified as a quasi-natural experiment to study the labor market consequences of immigration, see Boustan, Fishback, and Kantor (2010).

[9] For a very recent survey on the economic impacts of immigration, see Kerr and Kerr (2011).

[10] EurActiv.com (2009). Free movement of labour in the EU 27. *EurActiv.com*. Retrieved on 02/09/2012 from http://www.euractiv.com/socialeurope/free-movement-labour-eu-27/article-129648.

the main findings in this literature is that trade liberalization is likely to reduce unemployment rates.

This literature also spurred new empirical investigations into the trade and unemployment nexus. Dutt, Mitra, and Ranjan (2009) as well as Felbermayr, Prat, and Schmerer (2011b) investigate empirically the trade and unemployment nexus using high-quality OECD cross-section and panel data. They both find support for a negative relationship between openness and unemployment levels.

While both papers use a battery of labor-market related control variables in their regressions, none considers the effects of (im)migration. This is astonishing as it is well known since Mundell (1957) that "[c]ommodity movements are at least to some extent a substitute for factor movement". In a standard two goods, two factors trade model without trade costs, factor prices will equalize through goods trade. Hence, goods trade has the same effect as if factors could wander freely between countries. In other words, immigration has the same impact on factor prices as trade. When factor prices cannot fully adjust, there will be additional effects on the quantity of labor used, i.e. the unemployment rate. Hence, (factor) price differences between countries will trigger both, trade and immigration flows, implying that trade and immigration are not statistically independent and therefore correlated. While standard neoclassical trade theory predicts that price differentials can be mitigated by either migration or trade which leads to a negative correlation between trade and migration, recent evidence has suggested that immigration may actually spur trade (e.g. Gould (1994), Felbermayr, Jung, and Toubal (2009)). Theoretical predictions concerning the effects of immigration on unemployment are ambiguous and depend inter alia on factor endowments, production and market structure and differences in institutions. In the labor demand model with one sector and rigid wages, immigration leads to an increase of unemployment (see Boeri and van Ours (2008), pp. 178). In general equilibrium trade models with capital and labor as production factors, constant returns to scale and perfect competition, immigration has an ambiguous effect on aggregate unemployment (see for example Brecher and Chen (2010)). To the contrary, with increasing returns to scale and monopolistic competition, immigration leads to a fall of unemployment (see Epifani and Gancia (2005) and Südekum (2005)). There are many good surveys about international migration and trade. Gaston and Nelson (2011) is a particular useful one in the context of this paper as it surveys current theoretical and empirical research on international migration with a particular emphasis on the links between trade theory and labor empirics.

In the light of this discussion the question arises why goods trade should have a statistically significant effect on unemployment whilst (im)migration has not. And when trade decreases unemployment, should not (im)migration, too? If the answer to these questions is in the affirmative, one has to conclude that previous studies may suffer from a potential omitted variable bias. The direction of this bias is not clear a priori, as it depends on whether trade and migration are substitutes or complements.[11]

We want to contribute to both the trade and immigration literature and address this omitted variable bias by considering not only the effects of goods trade flows on unemployment, but also of migration flows. In order to do so, we have to deal with the problem that migrants do not select their destination countries randomly. Rather, it is likely that they migrate into countries with better economic conditions, including countries with lower unemployment rates. This creates an endogeneity problem. We deal with it by using dynamic panel regressions as well as a Frankel and Romer (1999) type instrument. It uses the fact that immigration flows are to a large part determined by geographic variables like the distance between sending and receiving country, i.e. factors which are arguably exogenous to the determination of the unemployment rate.[12]

Finally, note that we do not distinguish between the impact of immigrants of different skill groups on unemployment as panel data for different immigrant skill classes for a large set of countries and a sufficient time span are not available. We therefore focus on aggregate migration flows to address the concern of policy makers and the public at large which presupposes a positive impact of immigration on the level of the unemployment rate *on average*. Accordingly, the transition periods of the EU accession treaties also presuppose on average a positive impact

[11] Relatedly, Ortega and Peri (2011) also argue that previous studies of the effects of both trade and migration suffer from an omitted variable bias as both trade and migration are highly correlated. They use data on OECD countries from 1980 to 2007 to study the effects of trade and immigration on GDP per capita. However, they do not study effects on unemployment rates.

[12] Ottaviano, Peri, and Wright (2010) study the impact of both migration and offshoring in the US on employment of US workers using a theoretical trade framework as basis for their empirical analysis across manufacturing industries. However, they do not study overall unemployment.

and do not distinguish between workers of different skill levels. By this we offer an alternative empirical strategy which complements the more micro-level based empirical studies typically undertaken by labor economists.

The remainder of the paper is structured as follows. Section 2 describes the database and gives suggestive evidence. Section 3 describes the empirical specification. Section 4 provides the empirical results. The last section concludes.

2 Data and Descriptive Evidence

To examine the relationship between migration, trade and unemployment we collected a panel dataset from 1997 to 2007 for 24 OECD countries.[13] The selection of countries as well as the time period is driven by concerns of data availability. In addition, we try to follow Felbermayr, Prat, and Schmerer (2011b) and use the same control variables in order to replicate their results on the trade and openness link for our dataset. The dataset has the advantage that it allows to control for time-invariant country-specific effects and the dynamics (persistence) of unemployment rates. The variables used are summarized in Table 1 and 2. We describe each variable in turn in the following.

2.1 Unemployment Rates, Immigration and Trade Openness

The dependent variable is the yearly average harmonized unemployment rate (as percentage of the civilian labor force) from the OECD (2011d) Key Short-Term Economic Indicators, the same data as used in Felbermayr, Prat, and Schmerer (2011b). These data have the advantage that they are available for the whole time period under consideration and for all OECD member countries. In addition, the OECD has ensured that unemployment rates are comparable across countries.

The migration data are from the OECD (2011b) International Migration Database. It contains bilateral data both on flows and stocks of immigrants. Note that the data do not contain information on illegal migration. Even though data

[13] The countries included are Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Poland, Portugal, Slovak Republic, Spain, Sweden, Switzerland, United Kingdom, and United States.

Table 1: Summary statistics for migration, trade and unemployment dataset

Variable	Mean	Std. Dev.	Min.	Max.	*N*
Total unemployment rate	6.735	2.896	2.245	19.025	207
Migration data					
Net immigrant inflows (ln)	10.834	1.381	7.651	14.041	207
Total immigrant inflows (ln)	11.308	1.292	8.598	14.041	207
Stock of immigrants (foreign nationals) (ln)	13.386	1.319	9.222	15.711	150
Stock of immigrants (foreign born) (ln)	14.041	1.541	11.365	17.441	111
Net inflows (ln) (prediction)	11.566	1.269	8.949	14.044	207
Openness measures					
Total trade openness	78.883	41.244	22.884	217.786	207
Total current price openness	80.491	38.867	18.188	184.308	207
Merchandise curr. price open.	31.218	17.046	8.236	91.566	207
Merchandise openness	30.325	16.847	8.535	106.512	207
Labor market data					
Wage distortion (index)	57.170	18.418	25.187	92.17	207
EPL (index)	2.008	0.818	0.170	4.330	207
Union density (index)	32.755	20.362	7.617	81.285	207
High corporatism (index)	2.546	1.364	0	6	207
PMR (index)	2.348	0.728	0.900	4.700	207
Other control variables					
Population (ln)	16.749	1.228	15.127	19.525	207
Output gap (%)	0.487	1.562	-2.901	4.752	207
Civil liberties (index)	1.159	0.367	1	2	207
Years since perm. trade lib./1945	42.304	12.423	12	62	207

Table 2: Summary statistics for gravity dataset

Variable	Mean	Std. Dev.	Min.	Max.	*N*
Bilateral immigrant inflows	1,286	6,316	0	218,822	41,545
Bilateral geographical distance (ln)	8.570	0.885	5.081	9.880	41,545
Contiguity	0.025	0.157	0	1	41,545
Common official language	0.120	0.325	0	1	41,545
Colonial relationship after 1945	0.019	0.138	0	1	41,545

for some countries are available before 1997, broad coverage only starts then and we therefore opt to start our analysis with this year. Specifically, it contains data on the inflows and outflows of immigrants from country i to j defining a migrant as someone with a different nationality than the receiving country. From these data we construct total inflows of immigrants by collapsing the bilateral data. Also note that outflows do only include foreigners, i.e. return migrants. It does not include nationals leaving their home country. Hence net inflows are inflows of foreign nationals. Note that our regressions only include the receiving countries of immigrants. However, to construct the inflow data we use information about the immigrants from all 198 sending countries of immigrants.[14] The huge discrepancy between the high number of sending countries but low number of receiving countries stems from the fact that few countries provide accurate data on immigration. However, those countries that do report these data also have data on the nationalities of all the persons immigrating into the country. Therefore, our data set includes immigrants from all major immigrant sending countries like China, India, North-African and Latin American countries.[15] In addition, the data contain information on the total stock of immigrants, using either an immigrant definition based on the nationality of the person or its country of birth. Note that stock data are only available for a different set of countries as national governments differ in their used definitions of migrants and hence do not necessarily collect data using both definitions.[16]

[14] The complete list of sending countries can be found in Table 3.

[15] Countries with flow data used in the regressions are Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Poland, Portugal, Slovak Republic, Spain, Sweden, Switzerland, United Kingdom, and United States. Outflows are not available (for a subset of years) for Canada, France, Ireland, Italy, Korea, Poland, Slovak Republic, Spain, and Turkey. For these cases, we treat total inflows as net inflows, in effect overstating the number of migrants entering the country. Our main results are robust to this treatment.

[16] Stock data based on nationality are available (for at least a subset of years) for Austria, Belgium, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Netherlands, Norway, Poland, Portugal, Slovak Republic, Spain, Sweden, Switzerland, United Kingdom; stock data based on country of birth are available (for at least a subset of years) for Australia, Austria, Belgium, Canada, Denmark, Finland, France, Hungary, Ireland, Netherlands, New Zealand, Norway, Slovak Republic, Spain, Sweden, Switzerland, United Kingdom, and United States.

Table 3: List of sending countries of immigrants to construct the inflow data

List of sending countries
Afghanistan, Albania, Algeria, Andorra, Angola, Antigua and Barbuda, Argentina, Armenia, Australia, Austria, Azerbaijan, Bahamas, Bahrain, Bangladesh, Barbados, Belarus, Belgium, Belize, Benin, Bermuda, Bhutan, Bolivia, Bosnia and Herzegovina, Botswana, Brazil, Brunei Darussalam, Bulgaria, Burkina Faso, Burundi, Cambodia, Cameroon, Canada, Cape Verde, Central African Republic, Chad, Chile, China, Chinese Taipei, Colombia, Comoros, Congo, Cook Islands, Costa Rica, Croatia, Cuba, Cyprus, Czech Republic, Côte d'Ivoire, Democratic People's Republic of Korea, Democratic Republic of the Congo, Denmark, Djibouti, Dominica, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Eritrea, Estonia, Ethiopia, Fiji, Finland, Former Yug. Rep. of Macedonia, France, Gabon, Gambia, Georgia, Germany, Ghana, Greece, Grenada, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, Hong Kong (China), Hungary, Iceland, India, Indonesia, Iran, Iraq, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Kiribati, Korea, Kuwait, Kyrgyzstan, Laos, Latvia, Lebanon, Lesotho, Liberia, Libya, Lithuania, Luxembourg, Macao, Madagascar, Malawi, Malaysia, Maldives, Mali, Malta, Marshall Islands, Mauritania, Mauritius, Mexico, Micronesia, Moldova, Mongolia, Morocco, Mozambique, Myanmar, Namibia, Nauru, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Niue, Norway, Oman, Pakistan, Palau, Palestinian administrative areas, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Puerto Rico, Qatar, Romania, Russian Federation, Rwanda, Saint Kitts and Nevis, Saint Lucia, Saint Vincent and the Grenadines, Samoa, San Marino, Sao Tome and Principe, Saudi Arabia, Senegal, Serbia and Montenegro, Seychelles, Sierra Leone, Singapore, Slovak Republic, Slovenia, Solomon Islands, Somalia, South Africa, Spain, Sri Lanka, Sudan, Suriname, Swaziland, Sweden, Switzerland, Syria, Tajikistan, Tanzania, Thailand, Timor-Leste, Togo, Tokelau, Tonga, Trinidad and Tobago, Tunisia, Turkey, Turkmenistan, Tuvalu, Uganda, Ukraine, United Arab Emirates, United Kingdom, United States, Uruguay, Uzbekistan, Vanuatu, Venezuela, Viet Nam, Yemen, Zambia, Zimbabwe.

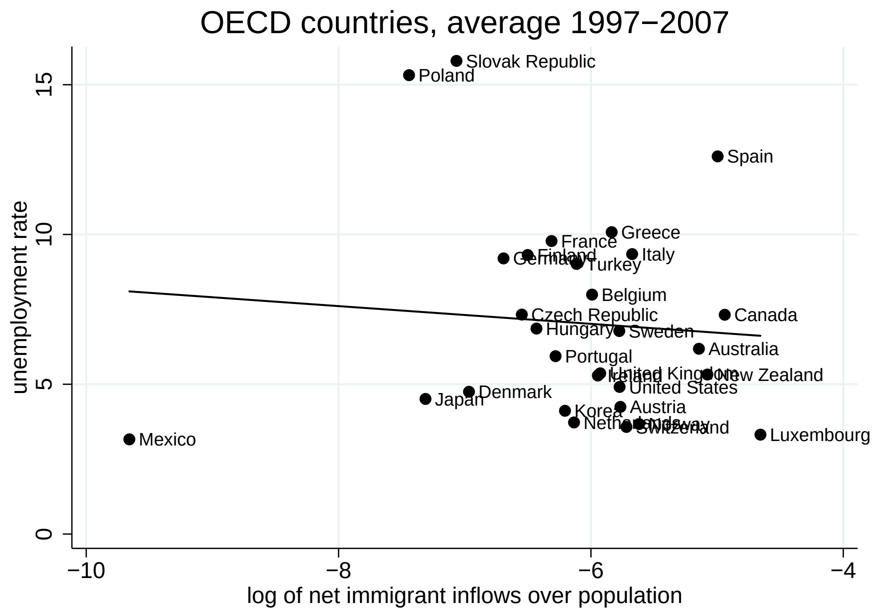

Figure 1: Average unemployment and log of net immigrant inflows over population of the receiving country

Figure 1 provides a first look on the unemployment migration nexus. It plots the average unemployment rate over the period of 1997 to 2007 against the average logged immigration net inflows over the population of the receiving country for the period of 1997 to 2007. As we see, this figure suggests a negative relationship between immigration and unemployment. Figures 2 and 3 plot average unemployment rates against the average of the logged stock of foreign nationals over the population of the receiving country and the logged stock of foreign born immigrants over population, respectively. Again, we find a negative relationship between immigration and unemployment.

This correlation between unemployment and migration may be misleading due to two main effects: i) It is an unconditional correlation, ignoring potential

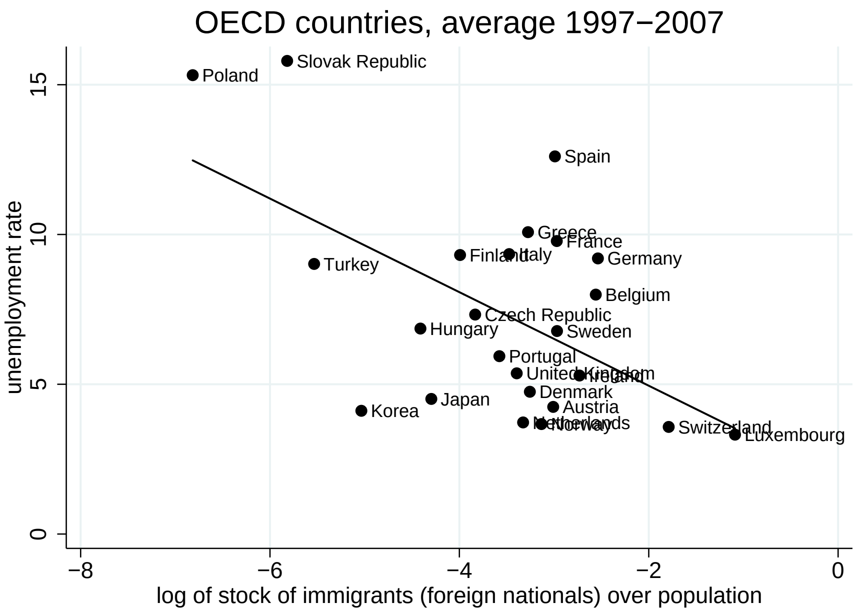

Figure 2: Average unemployment and log of stock of immigrants (foreign nationals) over population of the receiving country

heterogeneity of countries and other driving factors, and ii) the endogeneity of migration flows and unemployment.

Concerning migration from the perspective of an individual, two questions arise: The first question is whether to migrate at all, and the second question, given that one decided to migrate, where to migrate. The labor literature typically models those two decisions sequentially, where the second step depends on expected wage differences between the origin and destination country, accounting for unemployment differences (see Cahuc and Zylberberg (2004) and Boeri and van Ours (2008) for an overview). In other words, immigration will be larger all else equal into countries with lower unemployment rates. This is consistent with Figures 1, 2, and 3.

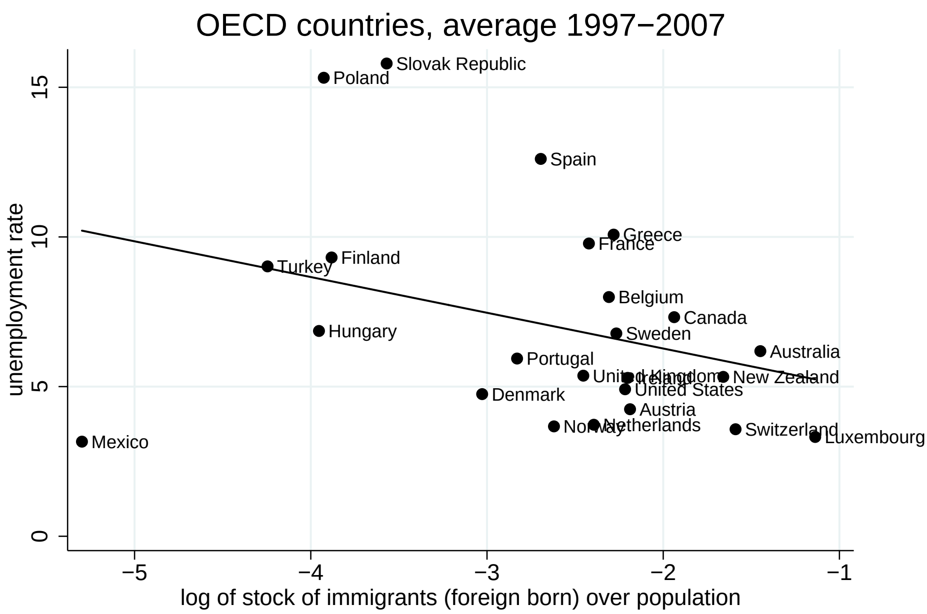

Figure 3: Average unemployment and log of stock of immigrants (foreign born) over population of the receiving country

However, we are interested in the causal effect of immigration on unemployment. Hence, we have to control for the reversed causality. In order to get rid of the endogeneity problem due to reversed causality, we are pursuing two strategies. First, we control for time-invariant and country-specific effects. This wipes out all level effects between countries. Hence, these regressions only use the change in unemployment levels and immigration inflows to identify the coefficients.

Figure 4 plots the change of unemployment against the change of immigration inflows over the population of the receiving country. This transformation removes the unobserved time-invariant country-specific heterogeneity. Again, the figure suggests a negative relationship between immigration and unemployment.

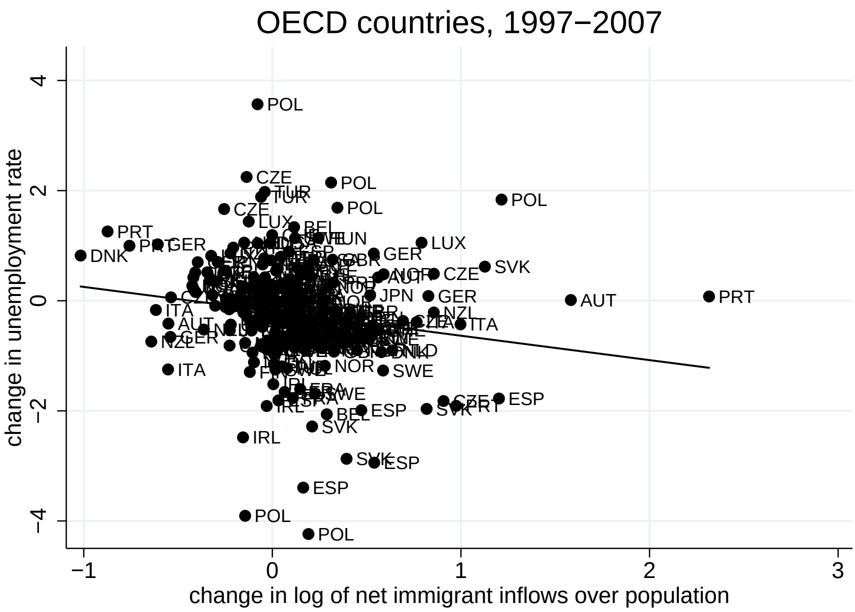

Figure 4: Change in unemployment and change in log of net immigrant inflows over population of the receiving country

Our second approach to control for the endogeneity due to reversed causality is to instrument the migration flows. Besides using external instruments, we will follow the established methodology used in Dutt, Mitra, and Ranjan (2009) and Felbermayr, Prat, and Schmerer (2011b) and rely on dynamic panel estimators which use internal instruments, i.e. suitable lags of regressors in both differences and levels, in order to control for both unobserved time-invariant heterogeneity as well as endogeneity of the migration variable. In addition, these estimators allow to control for the possible endogeneity of other control variables like e.g. the openness measure capturing trade linkages of the migration receiving country.

Let us next describe our second main explanatory variable, trade openness. We follow Alcalá and Ciccone (2004) and Felbermayr, Prat, and Schmerer (2011b) and construct a real openness measure, labeled *total trade openness*. It is defined as the sum of total imports and exports in exchange rate US-$ over GDP in purchasing power parity US-$. We construct it by multiplying the current price openness measure (*total current price openness*) times the GDP price level from the Penn World Tables, edition 7.0 from Heston, Summers, and Aten (2011).[17]

In addition, we construct openness measures for merchandise trade only using data from the OECD (2011c) International Trade by Commodity Statistics database. Again, we calculate a real and current price openness measure (*merchandise openness* and *merchandise current price openness*, respectively). We will describe the used control variables in the following section.

2.2 Controls

We closely follow Felbermayr, Prat, and Schmerer (2011b) in our choice of control variables.

Wage distortion is the sum of the tax wedge and the average replacement rate. The tax wedge is the average tax wedge on labor as a percentage of total labor compensation and is computed for a couple with two children and averages across different situations regarding the wage of the second earner. Tax wedge data are from the OECD. Specifically, we use the tax wedge data from Bassanini and Duval (2009) until 2003 and the publicly available data for 2004 to 2007 from the OECD (2011f) Taxing Wages database. Note that for the overlapping years, data from both sources do not perfectly match for some countries. In general, however, data are nearly or even exactly the same. We therefore merge the data to fill up the variable for the whole sample period. The average replacement rates are from the Benefits and Wages study from the OECD (2007). They are defined as the average of the gross unemployment benefit replacement rates for two earnings levels, three family situations and three durations of unemployment and is comparable across countries. As data are available only for odd years, we follow Bassanini and Duval (2009) and interpolate the data for even years. For a detailed description of the OECD replacement rate measures, see Martin (1996).

[17] Felbermayr, Prat, and Schmerer (2011b) use the Penn World Tables, edition 6.2.

EPL is an employment protection legislation index which is comparable across countries and is from the Going for Growth database from the OECD (2010). It measures protection for regular employment and ranges from 0 to 6 from weakest to strongest protection.

Union density corresponds to the ratio of wage and salary earners that are trade union members to the total number of wage and salary earners and is from the OECD (2011e) Labour Force Statistics.

High corporatism is an index variable from the Database on Institutional Characteristics of Trade Unions, Wage Setting, State Intervention and Social Pacts which is compiled at the Amsterdam Institute for Advanced Labour Studies (AIAS) at the University of Amsterdam by Visser (2011). It measures the degree of coordination of wage bargaining in the respective country where 1 indicates firm-level wage bargaining and 5 equals economy-wide bargaining.[18]

PMR is a measure of product market regulation on a scale from 0 to 6 indicating increasing regulatory restrictions to competition from Conway et al. (2006). We again follow Felbermayr, Prat, and Schmerer (2011b) and use the OECD data on regulation in seven sectors—telecoms, electricity, gas, post, rail, air passenger transport, and road freight—to measure overall product market regulation. As manufacturing sectors are less regulated and open to foreign competition, and most anti-competitive legislation is concentrated in the considered sectors, the measures do reflect an important part of the overall degree of product market regulation in a country, see Conway et al. (2006). The measures are based on regulation-related policies in the respective countries and are specifically constructed to allow cross-country comparisons. Further details on these measures can be found in Conway and Nicoletti (2006).[19]

Population is the population of the receiving country from the OECD (2011e) Labour Force Statistics.

[18] Note that Felbermayr, Prat, and Schmerer (2011b) only use a dummy variable from Bassanini and Duval (2009) to indicate high wage coordination. These data, however, are only available until 2003 and do not vary across our sample period and hence would be dropped from the regression. We therefore use the index measure which contains more information.

[19] Note that the OECD also compiles data on economy-wide measures of product market regulation. These, however, are only collected irregularly, prohibiting their use in a panel study. The used measure is highly correlated with the economy-wide measure for the years where it is available.

Output gap is the output gap in percent as reported in the OECD (2011a) Economic Outlook No. 89 data.

In additional regressions, we include control variables from Dutt, Mitra, and Ranjan (2009). *Civil liberties* is an index computed by Freedom House (2011) which gives the amount of civil liberties in a country. It runs from 1 to 7 where 1 indicates a maximum of liberties. Dutt, Mitra, and Ranjan (2009) include a dummy which is 1 in the years after a country has permanently liberalized trade. In our sample, all countries have free trade according to this index, hence we cannot include this dummy as it does not have variation. Therefore, we construct the variable *years since liberalization* which measures the years since a country has permanently liberalized its trade. It is based on data collected by Wacziarg and Welch (2008).[20]

To generate the instrumental variable, the predicted bilateral migration flows from a gravity-type migration regression, we use indicators for contiguity, common official language, and common colonial relationship after 1945 as well as the weighted bilateral distance between economic centers of the receiving and sending countries. All variables are from CEPII, see Head, Mayer, and Ries (2010). Summary statistics for the gravity dataset can be found in Table 2.

3 Empirical Specification

We follow Nickell, Nunziata, and Ochel (2005) and Felbermayr, Prat, and Schmerer (2011b) and estimate variants of the following dynamic model:

$$\begin{aligned} u_{it} &= \rho u_{i,t-1} + \alpha NETINFLOW_{it} + \gamma OPENNESS_{it} \\ &\quad + \delta \boldsymbol{CONTROLS}_{it} + \nu_i + \nu_t + \varepsilon_{it}, \end{aligned} \tag{1}$$

where u_{it} is the unemployment rate in country i at time t, $NETINFLOW_{it}$ is the net inflow of immigrants into country i at time t, $OPENNESS_{it}$ is a standard openness measure (the sum of imports and exports over GDP), $\boldsymbol{CONTROLS}_{it}$ is a vector of

[20] We assume the year 1945 for all countries where Wacziarg and Welch (2008) report “always” instead of a specific year as the permanent liberalization year. In our sample, these countries are Norway, Portugal, Switzerland, United Kingdom, and United States.

control variables, and v_i, v_t, ε_{it} are country and period effects and an error term, respectively. In contrast to Felbermayr, Prat, and Schmerer (2011b) we do not use five-year averages for our regressions as we would lose a lot of observations given the short time-series of the migration data. Additionally, we also want to capture the short-term transitional effects of migration on unemployment in our dynamic specifications which precludes us from taking averages over years.

The standard estimator for dynamic panel models with unobserved time-invariant heterogeneity is the difference GMM estimator as proposed by Arellano and Bond (1991). However, this estimator suffers from potentially huge small sample bias when the number of time periods is small and the dependent variable shows a high degree of persistence, see Alonso-Borrego and Arellano (1999). As unemployment numbers are very persistent, we follow Arellano and Bover (1995) and Blundell and Bond (1998) and also present estimates of the model using system GMM which circumvents the finite sample bias if one is willing to assume a mild stationarity assumption on the initial conditions of the underlying data generating process.[21] This estimator uses moment conditions for the model both in differences and in levels to reap significant efficiency gains. However, efficiency gains do not come without a cost: The number of instruments tends to increase exponentially with the number of time periods. This proliferation of instruments leads to an overfitting of endogenous variables and increases the likelihood of false positive results and suspiciously high pass rates of specification tests like Hansen's J-test, a routinely used statistic to check the validity of the dynamic panel model, see Roodman (2009a). We therefore follow his advice and present results with a collapsed instrument matrix for both the difference and system GMM estimators.[22] We also use the Windmeijer (2005) finite sample correction for standard errors.

As described above, $NETINFLOW_{it}$ is likely to be endogenous. Hence, we instrument this variable by suitable lags. In addition, we use an external instrument. To find an external valid instrument, we have to look for other determinants of migration besides destination country unemployment. A natural candidate are predicted migrant inflows, a method inspired by Frankel and Romer (1999) who

[21] Specifically, the deviations from the long-run mean of the dependent variable have to be uncorrelated with the stationary individual-specific long-run mean itself, see Blundell and Bond (1998).
[22] All GMM estimations are carried out using the `xtabond2` package in Stata, see Roodman (2009b).

use predicted trade flows as an instrument for trade flows.[23] The predictions of migrant flows are obtained by estimating a gravity equation. The gravity equation has a long history in the literatures on bilateral aggregate trade and migration flows. In fact, the earliest uses of the gravity equation were to model migration flows, cf. Ravenstein (1885, 1889). Since then, the gravity equation has been used extensively to model migration flows, cf. Zipf (1946), Stewart (1948), Isard (1975), Sen and Smith (1995). The gravity model was first adopted for studying international trade flows in Tinbergen (1962) and Linnemann (1966), and is well established in the trade literature.

More precisely, bilateral international migration $INFLOW_{ijt}$ is specified as a function of geographic variables, GDPs and so called "multilateral resistance" terms (see Anderson and van Wincoop (2003)):

$$INFLOW_{ijt} = \frac{Y_{it}Y_{jt}}{Y_{wt}} \frac{DIST_{ij}}{P_{it}P_{jt}}, \tag{2}$$

where Y_{it} and Y_{jt} are the GDPs of the origin and destination, Y_{wt} is world income, $DIST_{ij}$ is a (potentially multidimensional) time-invariant distance measure between country i and j, and P_{it} and P_{jt} are the measures for origin and destination market potential, or "multilateral resistance" terms.

Typically, Y_{it}/P_{it} and Y_{jt}/P_{jt} are replaced by origin$\times$year and destination$\times$year fixed effects (which also take account of Y_{wt}) and one takes logs of Equation (2) in order to get an empirical specification linear in the parameters, allowing to estimate the parameters via ordinary least squares. However, as migration data are likely to be heteroskedastic and contain zero migration flows, taking logs is no longer feasible.[24] Fortunately, there are a couple of recent contributions concerning gravity equation estimation taking into account heteroskedasticity and zero trade flows. Helpman, Melitz, and Rubinstein (2008) propose a sample selection model to account for zero trade flows and show that omitting zero trade flows leads to biased estimates.

[23] See Felbermayr, Hiller and Sala (2010) who also use a Frankel and Romer (1999) instrument for immigration to investigate the effect of immigration on per capita income.

[24] Some authors replace zero values by a unit value for the migration flow. In general, this leads to inconsistent estimates.

Santos Silva and Tenreyro (2006, 2008) suggest to estimate the gravity model in multiplicative form employing a Poisson pseudo maximum likelihood estimator in order to account for the "log of gravity". The "log of gravity" says that taking logs of the right and left hand side of the gravity equation may lead to inconsistent and biased estimates because of Jensen's inequality, i.e., $E(\ln INFLOW_{ijt}) \neq \ln E(INFLOW_{ijt})$. This is for example the case in the presence of heteroskedasticity, which is very likely the case with migration and trade data.

In order to account for the heterogeneity and zeros in the bilateral migration flow data, we follow the approach of Santos Silva and Tenreyro (2006, 2008). Our empirical specification for the first step gravity model of international bilateral migration flows is therefore:

$$INFLOW_{ijt} = \exp\left(DIST_{ij} + \nu_{it} + \nu_{jt}\right)\varepsilon_{ijt}, \tag{3}$$

where ν_{it} and ν_{jt} are origin$\times$year and destination$\times$year fixed effects, and ε_{ijt} is a multiplicative remainder error term. Note that the fixed effects also control for origin and destination variables commonly used in Frankel and Romer (1999) type regressions like the land area covered by the respective country as well as its population.[25]

We specify $DIST_{ij}$ to consist of bilateral geographical distance ($GDIST_{ij}$)[26], a contiguity dummy between countries ($CONTIG_{ij}$), a dummy for a common official primary language ($COMLANG\_OFF_{ij}$), and a dummy indicating whether the two countries had a colonial relationship after 1945 ($COL45_{ij}$), i.e.

$$\begin{aligned} DIST_{ij} \quad = \quad & \rho_1 \ln\left(\mathrm{GDIST}_{ij}\right) + \rho_2 CONTIG_{ij} + \rho_3 COMLANG\_OFF_{ij} \\ & + \rho_4 COL45_{ij}. \end{aligned} \tag{4}$$

As our migration data are bilateral but our second stage regression for explaining the unemployment rate has only country-time but no bilateral variation, we sum up our predictions of migration flows $\widehat{INFLOW}_{ijt}$ over all origin countries, i.e.,

[25] The gravity equation explains bilateral total flows of migrants. Hence, we use bilateral total inflows as dependent variable in specification (3).

[26] We use the simple weighted bilateral distance measure as proposed by Head and Mayer (2000) which is provided by CEPII and which is defined as distance between the regions in the respective countries weighted by the economic size of the regions.

$\widehat{INFLOW}_{jt} = \sum_{i=1}^{N} \widehat{INFLOW}_{ijt}$ where N is 198, the number of sending countries of immigrants.[27]

4 Regression Results

In this section, we present our results. In the first subsection we present regression results from our benchmark specification using different estimators. The second subsection discusses several robustness checks concerning different measures of migration, trade openness as well as using additional control variables and sample definitions.

[27] Note that we even do not need to estimate the parameters of the migration equation consistently to use $\widehat{INFLOW}_{jt}$ as a valid instrument. The only assumption we need is that $\widehat{INFLOW}_{jt}$ is a constructed exogenous measure of migration stocks or flows. For a similar argument, see Felbermayr, Prat, and Schmerer (2011b).

Table 4: Benchmark regressions

Dependent variable: unemployment rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FE	FE	FE	FE-IV	Diff-GMM	Diff-GMM	Sys-GMM	Sys-GMM
Lag dep. var.					2.067	1.424	0.953***	0.951***
					(8.917)	(2.867)	(0.083)	(0.082)
Net inflow (ln)			-0.099	-0.562		-2.832		-0.888**
			(0.287)	(0.393)		(23.82)		(0.390)
Total trade openness		0.028*	0.027*	0.025***	0.376	0.183	0.005	0.003
		(0.013)	(0.014)	(0.010)	(3.444)	(1.415)	(0.008)	(0.007)
Wage distortion (index)	0.019	0.028	0.029	0.035**	0.991	1.260	-0.001	-0.011
	(0.024)	(0.021)	(0.020)	(0.017)	(9.842)	(10.319)	(0.040)	(0.029)
EPL (index)	-1.259	-1.069	-1.125	-1.387*	50.209	26.510	1.205	0.948
	(1.023)	(0.951)	(0.968)	(0.753)	(486.914)	(226.431)	(0.996)	(0.641)
Union density (index)	0.144	0.170	0.173	0.191***	1.012	-0.017	-0.002	-0.003
	(0.105)	(0.105)	(0.106)	(0.060)	(9.368)	(1.388)	(0.027)	(0.021)
High corporatism (index)	-0.023	-0.013	-0.011	-0.002	5.859	-0.415	-0.152	0.345
	(0.079)	(0.083)	(0.081)	(0.115)	(58.163)	(7.215)	(0.473)	(0.473)
PMR (index)	0.487	0.741	0.719	0.614**	0.068	1.852	-0.100	-0.250
	(0.543)	(0.592)	(0.594)	(0.272)	(4.919)	(13.485)	(0.438)	(0.299)
Population (ln)	-20.241*	-18.786*	-18.880*	-19.319***	66.471	48.194	0.240	1.136**
	(11.354)	(10.110)	(9.938)	(4.935)	(459.307)	(222.346)	(0.282)	(0.443)
Output gap	-0.461***	-0.468***	-0.464***	-0.446***	0.190	-0.334	-0.319***	-0.205**
	(0.103)	(0.102)	(0.106)	(0.070)	(4.483)	(1.498)	(0.121)	(0.085)
Observations	224	224	224	224	181	181	207	207
Countries	24	24	24	24	24	24	24	24
Instruments				1	18	19	25	27
R^2 (within)	0.529	0.548	0.549					
R^2 (between)	0.017	0.018	0.018					
R^2 (overall)	0.016	0.015	0.015					
Hansen test (OID)							0.702	0.971
AR(1)					0.924	0.923	0.815	0.587
AR(2)					0.997	0.883	0.337	0.934

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; all models control for unobserved country and period effects. H_0 for AR(1) and AR(2) is no autocorrelation. Openness, output gap, wage distortion, and net inflow treated as endogenous in GMM regressions. Maximum number of lags used is 1. Instrument matrix was collapsed as proposed by Roodman (2009b). Constant estimated but not reported.

4.1 Benchmark Results

Table 4 presents eight different specifications which all use as dependent variable the unemployment rate and some or all of the following explanatory variables: the net inflows of migrants into the country (in logs), a measure of total trade openness, an index of wage distortion, a measure of employment protection legislation, a measure of union density, an index of the centralization of the wage bargaining process, a measure of product market regulation, a country's size as measured by its population (in log), as well as a measure of the output gap to control for business cycle effects. For the dynamic panel estimators, this list of regressors is augmented by the lagged dependent variable.

Column (1) reproduces column (1) in Table 1 of Felbermayr, Prat, and Schmerer (2011b) for our sample using a fixed effects estimator (FE). Qualitatively, results are exactly the same as in Felbermayr, Prat, and Schmerer (2011b). However, in our case only *population* and the *output gap* are significant.

Column (2) adds real total openness as defined in Alcalá and Ciccone (2004). Contrary to Dutt, Mitra, and Ranjan (2009) and Felbermayr, Prat, and Schmerer (2011b), we find a significant positive effect of international trade on unemployment. However, this does not imply that our results are necessarily at odds with empirical findings in the literature. Both Dutt, Mitra, and Ranjan (2009) and Felbermayr, Prat, and Schmerer (2011b) use data for a different time period (1985–2004 and 1980–2003, respectively) and also for a larger set of countries with vastly differing levels of development. In addition we do not use five-year averages of the data. Our sample only focuses on a subset of OECD countries for a recent 10-year period due to data availability of the migration data. Differences in the results may therefore simply be due to the specifics of the sample under study. Also remember that we treat all variables as exogenous in specifications (1) and (2), so our results could simply be a result of the endogeneity of our regressors.

In column (3), we add the net inflow of migrants to the specification given in column (2), again using fixed effects. It turns out that the sign of the coefficient of the immigration flow is negative but statistically insignificant. The sign is in line with predictions from new trade theory models with international migration but seems to be in contradiction with predictions based on the labor demand model with wage rigidities. Hence, immigration seems at least not to increase unemployment in the destination country. However, our specification given in column (3) may suffer from an endogeneity bias. As stated in the introduction, migrants might select into countries with lower unemployment rates.

Hence, in column (4) we take as instrument the predicted migration flows based on the Frankel and Romer (1999) instrument described in Section 3. We use an instrumental variables panel estimator with fixed effects (FE-IV). Instrumenting migration flows preserves the negative sign but still does not lead to a precise estimate. The coefficient implies that a 1 percent increase in migration inflows leads to a decrease in the unemployment rate of 0.006 percentage points. Openness still has a significant positive effect on unemployment.

Specification (4) still ignores both the persistence of unemployment rates as well as the potential endogeneity of other control variables like trade openness and wage distortion. In addition, the exogeneity of the instrument could be debated as it is inter alia a proxy for the remoteness of a country. It is well known that general remoteness to foreign markets is a determinant of many aggregate variables and therefore could influence unemployment directly. We therefore investigate the effect of migration on unemployment presenting difference and system GMM estimates in columns (5) to (8).

Column (5) presents the specification in column (2) augmented by the lagged dependent variable where we treat openness, wage distortion, *EPL*, as well as the high corporatism measure as endogenous variables using the Arrellano and Bond (1991) difference GMM estimator (Diff-GMM). In this specification we do not find a significant effect of the lagged unemployment rate. Additionally, the estimated coefficient on the lag implies a non-stationary behavior and openness is again not significant.

Column (6) adds migration inflows to specification (5) which we also treat as endogenous. It turns out to be non-significant again but still negative. However, one concern in this specification is the high coefficient of the lagged dependent variable. As soon as the dependent variable is highly persistent (our estimates would even imply an explosive behavior of the unemployment rate), the difference GMM estimator has poor small sample properties, see Alonso-Borrego and Arellano (1999). This is reflected in the high standard errors of the estimates.

A suggestion for highly persistent dependent variables is the system GMM estimator due to Arellano and Bover (1995) and Blundell and Bond (1998) which exploits more information conveyed by additional moment conditions. Column (7) repeats specification (5) estimated with system GMM (Sys-GMM). Here, the output gap is significantly negative. In addition, the lagged dependent variable becomes highly significant. It also implies a very high degree of persistence in unemployment rates as expected.

In column (8) we add migration flows to specification (7). Now, migration flows are again negative and also significant on the 5% level. Openness still has a positive impact on unemployment rates but not significantly so. Additionally, the coefficient of the lagged dependent variable has the same magnitude as in previous studies. This is our preferred specification as it allows for the endogeneity of various regressors and can handle the persistence of our dependent variable. It implies that a one percent increase in migration inflows leads to a 0.009 percentage point decrease in the unemployment rate in the short-run. In the long-run, a one percent increase of the total inflow of migrants would amount to a 0.18 percentage point decrease in the unemployment rate.[28]

Note that we report p-values of Hansen's overidentifying restrictions test as well as tests on autocorrelation in the first and second differences of the residuals for both the difference and system GMM estimates. The null hypothesis of valid overidentifying restrictions is not rejected, indicating a well specified model. We do not find autocorrelation in neither the first nor the second differences. Even though one would expect to detect autocorrelation in the first differences when specifying a dynamic panel model, this is not necessary to apply dynamic panel estimators. Autocorrelation in the second differences would be more problematic as it would render some instruments invalid. In any case, it is well known that both the Hansen test as well as the autocorrelation tests suffer from potentially large losses in power for small sample sizes, see Roodman (2009a). He explicitly states that for sample sizes as the ones used in our study with only few time periods, reliance on asymptotic distributions of the test statistics is "worrisome". As there exists ample evidence on the persistence of unemployment rates, we nevertheless are confident that the system GMM estimator for the dynamic panel model is appropriate.

[28] The long-run effects are found by dividing the coefficient by one minus the coefficient on the lagged dependent variable.

To sum up, we find no robust statistically significant effect of migration inflows on unemployment rates. Hence, empirical evidence based on a cross-section of aggregate migration flows does not support the widely held belief that immigration is detrimental to employment prospects of workers in the destination country on average.

4.2 Robustness Checks

In this section we describe two tables with robustness checks. While Table 5 presents regressions using different migration measures than used in Table 4, Table 6 gives results for different trade openness measures, additional control variables and varying sample definitions.

Migration Measures

In Table 4 we used net inflows of migrants as migration measure where a migrant was defined as a person which does not have the citizenship of the receiving country. Column (1) in Table 5 reproduces our preferred specification (8) from Table 4 for convenience of comparison. By subtracting return migrants from total immigrants, we assume that it is only the net number of migrants which influences the unemployment rate. From a theoretical point of view, it is not entirely clear whether net or total migration flows should be used. If labor markets are characterized by search frictions, total inflows may be the appropriate measure especially for quantifying the short-run impact as every new migrant has to search for a job. However, in the medium- to long-run or when labor markets are very flexible, net inflows may be more appropriate.

Hence, in column (2) we use total inflow of migrants instead of net inflows. Now, immigration flows are no longer significant but still negative. Interestingly, openness now has a negative but still non-significant impact.

So far we used migration flows, implying that we identify our parameters by exploiting the variation in the change of migration flows over time in the difference GMM and system GMM specifications. As a robustness check we also investigate how the stock of migrants affects the unemployment rate, exploiting the variation in the change of migrant stocks, that is, migration flows. In columns (3) and (4) of

Table 5: Robustness checks: Different migration measures

Dependent variable: unemployment rate	(1)	(2)	(3)	(4)	(5)
	Sys-GMM	Sys-GMM	Sys-GMM	Sys-GMM	Sys-GMM
Lag dep. var.	0.951***	0.948***	1.039***	1.013***	0.927***
	(0.082)	(0.139)	(0.108)	(0.262)	(0.127)
Net inflow (ln)	-0.888**				0.083
	(0.390)				(0.389)
Total inflows (ln)		-0.059			
		(0.654)			
Total stock (nationality) (ln)			0.007		
			(0.008)		
Total stock (c. of birth) (ln)				3.085	
				(1.956)	
Total trade openness	0.003	-0.006	0.019**	0.051	0.020
	(0.007)	(0.012)	(0.008)	(0.041)	(0.017)
Wage distortion (index)	-0.011	0.059	0.074	0.067	-0.017
	(0.029)	(0.161)	(0.111)	(0.051)	(0.048)
EPL (index)	0.948	3.361	1.520	-1.040	1.308
	(0.641)	(1.936)	(1.232)	(0.966)	(0.752)
Union density (index)	-0.003	0.027	0.001	-0.011	0.014
	(0.021)	(0.033)	(0.066)	(0.027)	(0.039)
High corporatism (index)	0.345	0.315	-0.521	-1.069	-0.210
	(0.473)	(0.751)	(1.238)	(1.340)	(0.446)
PMR (index)	-0.250	-0.364	0.872	1.164	-0.141
	(0.299)	(0.371)	(0.543)	(0.855)	(0.405)
Population (ln)	1.136**	0.442	0.389	-2.975	0.641
	(0.443)	(0.501)	(1.524)	(1.914)	(0.726)
Output gap	-0.205**	-0.358***	-0.126	-0.869*	-0.466**
	(0.085)	(0.128)	(0.098)	(0.481)	(0.226)
Observations	207	207	155	111	207
Countries	24	24	21	18	24
Instruments	27	27	27	27	28
Hansen test (OID)	0.971	0.597	0.996	1.000	0.618
AR(1)	0.587	0.937	0.077		0.753
AR(2)	0.934	0.977	0.009	0.678	0.286

Notes: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; all models control for unobserved country and period effects. H_0 for AR(1) and AR(2) is no autocorrelation. Openness, output gap, wage distortion, and net inflow treated as endogenous in GMM regressions. Maximum number of lags used is 1. Instrument matrix was collapsed as proposed by Roodman (2009b). Constant estimated but not reported. Total stock (nationality) (ln) is multiplied by 10 for numerical stability.

Table 5 we therefore replace the migration flows by the stock of foreign citizens and the stock of foreign-born persons, respectively. It turns out that migrant stocks have a positive effect on the unemployment rate, but not significantly so. However, in these regressions the coefficient on the lagged unemployment rate is larger than one and highly significant, implying an explosive behavior of the unemployment rate. In addition the test statistic on autocorrelation in the second difference of residuals implies rejection of no autocorrelation in column (3), hinting at a violation of one of the system GMM assumptions. This may well be due to the limited availability of stock data which reduces our sample considerably. Overall, the regressions with migration flows seem to fit our dynamic specification better as they do not imply a counter factual explosive behavior for the unemployment rate.

Our employed dynamic GMM estimator does account for the endogeneity of migration flows by relying on internal instruments based on suitable lags of the respective variables. However, it is also possible to additionally include external instruments such as our predicted migration flows. Specification (5) in Table 5 shows the estimates from the specification given in column (1) augmented by the additional exogenous variable. The results change as now net inflows are no longer significant and have a positive impact on the unemployment rate.

Trade Openness Measures and Additional Controls

All regressions until now employed a real openness measure as proposed by Alcalá and Ciccone (2004). It is defined as the sum of imports and exports in exchange rate US-$ over GDP in purchasing power parity US-$. Traditionally, openness measures are constructed by dividing by GDP in current US-$. In order to provide comparable results, we therefore use the latter openness measure in column (1) in Table 6. Interestingly, we now can corroborate the findings of Felbermayr, Prat, and Schmerer (2011b) that openness reduces the unemployment rate. Note though that these authors argue against using these openness measures and use total trade openness instead as we do in our benchmark regressions. Still, immigration remains to have a reducing effect on unemployment. Both variables are significant at the 5% level.

As services are very hard to measure and therefore not very well comparable across countries, see e.g. Francois and Hoekman (2010), using total trade flows including services may render openness a noisy measure for actual trade openness. Therefore we re-run our preferred specification using an openness measure based on merchandise trade only. In column (2) we present the Alcalá and Ciccone (2004) real openness measure using only merchandise trade. While trade openness again turns out to have a negative influence on unemployment, it is no longer significant. Immigration has a negative impact on unemployment but is not significant. Immigration becomes negatively significant again in column (3), where we use the standard trade openness based on merchandise trade measured in current US-$ GDP. Here, openness remains negative and not significant.

In columns (4) to (6) in Table 6 we introduce additional control variables following Dutt, Mitra, and Ranjan (2009). As openness measure, we return to the total trade openness measure from our preferred specification. We add an index of civil liberties and an additional measure of trade liberalization. Specifically, we add the years since permanent trade liberalization of the country as a control. To allow for a non-linear impact of trade liberalization on unemployment we include the variable both in levels and squared. The inclusion of the civil liberty index renders net immigration non-significant. So does the inclusion of the liberalization variable. Both variables are not significant, though. Openness again turns to have a positive impact but is again not significant. If we include both variables simultaneously, the effect of immigration becomes positive again and we estimate an autoregressive parameter which again implies an explosive behavior of the unemployment rate.

In unreported regressions, we use a different output gap measure. The output gap can also be calculated as the difference between log GDP and log trend GDP. We calculate GDP by multiplying real GDP per capita (chain) by the population from the Penn World Table, edition 7.0. The trend series is calculated by Hodrick-Prescott filtering. We use 6.25 as the smoothing factor for annual data as recommended by Ravn and Uhlig (2002). [29]

Furthermore (again not reported), we re-run our preferred specification from column (8) in Table 4 only for EU receiving countries. The coefficient for net inflows (ln) is 1.425 with a standard error of 1.328. Splitting the sample in the years before and after the eastern EU enlargement of 2004 leads to a net inflow

[29] Felbermayr, Prat, and Schmerer (2011b) use it for some regressions as well. However, they use 400 as smoothing factor.

Table 6: Robustness checks: Different control variables

Dependent variable: unemployment rate	(1)	(2)	(3)	(4)	(5)	(6)
	Sys-GMM	Sys-GMM	Sys-GMM	Sys-GMM	Sys-GMM	Sys-GMM
Lag dep. var.	0.918***	0.954***	0.939***	0.946***	0.959***	1.094**
	(0.105)	(0.081)	(0.093)	(0.159)	(0.145)	(0.547)
Net inflow (ln)	-0.388*	-0.628	-0.429*	-1.312	-0.722	0.537
	(0.217)	(0.384)	(0.259)	(0.805)	(0.681)	(7.046)
Total curr. price open.	-0.013*					
	(0.007)					
Merchandise open.		-0.012				
		(0.028)				
Merch. curr. price open.			-0.013			
			(0.019)			
Total trade openness				0.004	0.009	-0.008
				(0.018)	(0.019)	(0.025)
Wage distortion (index)	0.022	0.016	0.015	0.026	-0.020	-0.013
	(0.034)	(0.028)	(0.024)	(0.068)	(0.077)	(0.239)
EPL (index)	0.249	1.306	0.784	0.105	0.900	3.787
	(0.878)	(1.243)	(0.858)	(1.610)	(1.620)	(15.165)
Union density (index)	-0.007	-0.010	-0.005	-0.027	0.015	0.017
	(0.015)	(0.027)	(0.027)	(0.033)	(0.056)	(0.189)
High corporatism (index)	-0.330	-0.192	-0.295	0.270	0.194	-1.415
	(0.226)	(0.391)	(0.476)	(1.118)	(0.707)	(5.686)
PMR (index)	0.248	-0.267	0.096	-0.248	0.017	-0.130
	(0.384)	(0.771)	(0.694)	(0.508)	(0.618)	(0.779)
Population (ln)	0.305	0.547	0.424	1.475**	1.275*	-0.651
	(0.365)	(0.504)	(0.636)	(0.633)	(0.758)	(6.850)
Output gap	-0.275***	-0.276**	-0.242	-0.222	-0.235***	-0.992
	(0.095)	(0.119)	(0.169)	(0.188)	(0.088)	(2.951)
Civil liberties				-1.420		-0.251
				(1.188)		(2.803)
Yrs. since lib.					-0.090	0.127
					(0.172)	(0.800)
(Yrs. since lib.)^2					0.001	-0.001
					(0.002)	(0.007)
Observations	207	207	207	207	207	207
Countries	24	24	24	24	24	24
Instruments	27	27	27	28	29	30
Hansen test (OID)	0.919	0.930	0.944	0.929	0.986	0.970
AR(1)	0.456	0.818	0.576	0.716	0.773	0.814
AR(2)	0.182	0.246	0.154	0.553	0.853	0.818

Notes: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; all models control for unobserved country and period effects. H_0 for AR(1) and AR(2) is no autocorrelation. Openness, output gap, wage distortion, and net inflow treated as endogenous in GMM regressions. Maximum number of lags used is 1. Instrument matrix was collapsed as proposed by Roodman (2009b). Constant estimated but not reported.

immigration coefficient of 0.160 (standard error 0.228) and -0.361 (standard error 0.933) for the pre- and post-accession period, respectively. Finally, we augment our preferred specification by an interaction term between net inflows (ln) and total trade openness. The value for the interaction term is 0.003 (standard error 0.022), while the net inflow coefficient is -1.281 (standard error 2.304). Hence, we do not find a significant interaction between trade openness and immigration.

To again summarize our results, we find no statistically significant impact of immigration on the unemployment rate across a range of specifications and using different definitions of the control variables.

5 Conclusions

How do international trade and immigration affect unemployment in the destination country? While there is ample evidence that trade openness reduces unemployment, to the best of our knowledge the literature has so far not investigated the effect of immigration on unemployment explicitly taking into account a country's exposure to trade. This is astonishing as it is well known since at least Mundell (1957) that goods trade implies implicit factor movements. Hence, when one is interested in the effect of trade on unemployment it seems important to control for additional movement of workers.

In this paper we present the first evidence of the effects of trade and migrant inflows on unemployment in the destination country taking into account that immigration and trade exposure of a country are highly correlated and therefore not statistically independent. In our sample, we find no significant aggregate effect of immigrant inflows on unemployment rates in destination countries on average.

This finding seems to be at odds with the widely held belief of a detrimental effect of immigration on unemployment amongst politicians and the public at large. More importantly, our findings leave us puzzled about how easy European decision makers willingly accepted to erect barriers to the freedom of movement: One of the corner stones of the European Common Market Policy is that workers be employed on equal, non-discriminatory terms in all member states of the European Union. Even though restrictions to this right could only be sustained for a seven year transitional period if the country informed the European Commission about serious disruptions on its labor market, two countries (Austria and Germany) actually achieved shielding their labor markets from inflows for the full seven year period. Given our results, the feared detrimental effect of immigration on domestic labor markets seems dubious at best, at least on average. In the worst case it may have hindered welfare gains for the respective countries due to more efficient allocation of labor across countries. Taking our results even a step further, on average it may have even forced additional workers in Austria and Germany into unemployment, contrary to the well-meant original intention.

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