Labor Market Institutions and The Effect of Immigration on National Employment

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1 Introduction

Integration processes in Europe resulted in intensification of migration flows. Immigrants account now for a large share of population in many European countries. For example, in 2011 immigrants account for more than 10% in Belgium, more than 12% in Spain, around 10% in Austria, almost 9% in Germany (OECD [2012]). This naturally leads to wide labor policy debates. One of the most important questions is whether immigrants affect the level of native employment. A point of view that immigrants take jobs form natives is quite widespread. The European Monitoring Centre on Racism and Xenophobia published a special analysis of the attitudes towards minorities in EU countries Eurobarometer 2000. They found that one in two EU citizens worry about competing with immigrants for the same vacancies and afraid of losing their jobs because of presence of foreign workers (Thalhammer et al. [2001]). Different measures and institutions which protect native workers have nevertheless an ambiguous effect. On the one hand labor protective institutions such as minimal wage, replacement rate or firing restrictions will protect existing workers and reduce a firing rate. On the other hand, firms will take into consideration these additional costs of firing and will be less likely to employ new workers. At the same time, it is argued that immigrants are probably less likely to be covered by these institutions since they are more likely to work in non-unionized jobs, on short-term fixed contract or even illegally (Angrist and Kugler [2003]). In addition, immigrants, being new on the labor market, may be less aware of employment protection regulations and less likely to claim their rights in court (Sa [2011]). These facts imply that protective institutions cover mostly natives and therefore make immigration labor force comparatively less costly. Labor market protection may therefore amplify a negative effect of immigrants on native employment if it exists.

Another interesting idea is that the effect of protective institutions can be not permanent but changing over time (Z. and Yashiv [2002], or Jean and Jimnez [2011]). It is quite natural to assume that immigrants enter a product market more quickly than a labor market. So immigration inflow boosts a product demand and therefore a labor demand first and only on the later stages progressively increases labor supply (Z. and Yashiv [2002]). As a result a negative effect of immigration can be delayed in time.

This paper attempts to evaluate the effect of immigration inflow on employment level of natives and reveal whether this effect changes in different institutional environment using EU-countries data. In addition to static specification it uses a dynamic specification to draw conclusions about long-term and short-term effects separately. The paper is organized as follows: Section 2 gives a brief literature review. Section 3 presents a replication of the one of the most important and cited paper in described area - paper of Angrist and Kugler [2003]. In addition, some critical questions are raised here. Section 4 evaluates the similar specification using more recent data and time-varying indicator for institutional restrictions. Section 5 then turns to a dynamic specification. Section 6 concludes.

2 Literature Review

The effect of immigration inflow on national labor market is a quite popular topic for research. However, the results are not so obvious and there is no common point of view in the literature. Pope D. and Winters G. (1993) paper, for example, was one of the first. Their approach based mostly on the theoretical framework and provided no evidence of immigration influence on native employment level. Pischke and Velling [1997] analyzed the impact of increased immigration on employment outcomes for natives in Germany using the change in immigrants share as an independent variable. They used previous labor market outcomes to control for immigrants self-selection problem. As results suggest, there is no evidence of any displacement effect. Weyerbrock [1995] computed a general equilibrium model for EU and concluded that a negative effect of immigration, like increasing unemployment or decreasing wages, is very small even with a large immigration flows. Longhi [2005] reviewed 165 different estimates form nine different studies for different OECD countries. They found that negative effect from immigrants is stronger for low-skilled than for high-skilled workers but on average is almost negligible. More recent researches use different and more comprehensive techniques. Winter-Ebmer and Zweimuller [2000] employed a probit model and a Weibull duration model. Using data from Austria they showed that there is no effect on employment probability and unemployment duration (see also Gang and Rivera-Batiz [1994] for analysis for EU). Morley [2006] turned to time-series ARDL specification and tested causality tests for Australia, Canada and the USA. He showed that causality goes form GDP to migration and not vice versa. Therefore, any independent policy which aims to control immigration processes could not be fully successful.

However, one can not conclude that there is no effect at all. Firstly, the effect can change over time and therefore there is a need to distinguish between long- and short-term prospectives. Damette and Fromentin [2000] used non-stationary panel data methodology with data from 14 OECD countries. They estimated a trivariate Vector Error Correction Model and derived causality tests to simultaneously assess the long- and short-term macroeconomic impact of newcomers. The results suggest that an increase of immigrants is likely to increase wages only in the short run and they also found an evidence of adverse effects on unemployment due to immigration for Anglo-Saxon countries in the short term.

In general, there are several basic approaches to estimate the immigration effect on local labor market as comprehensively described in Okkerse [2008]. Geographical method is a comparison of regions with different shares of immigrants (examples here areAltonji and Card [1991] and Card [2001]). There can be several problems here. First, both labor market conditions and immigration inflows can be simultaneously affected by unobservable regional shocks. Secondly, and more importantly, immigrants choose where to settle not exogenously. They often decide to move to the regions with good labor market conditions. On the other hand, immigration inflows themselves may worsen a labor market situation in the region. So, the causality can go in both directions. As Okkerse [2008] pointed out, the resulting correlation between these two variables will measure a net effect and not just one causal relationship. One way to resolve these problems is to use instrumental variables. This could be a reform or political event that affects immigration flows but is not correlated with wage or level of unemployment on the local labor market. Of course it is quite difficult to find appropriate instruments in this case. For instance, Sa [2011] provides evidence of institutional effect on immigration displacement for the EU countries based on two natural experiments (government reforms) for Spain and Italy. Other examples are Altonji and Card [1991] and Card [2001]). Although, one should note that natives can also respond to immigration entry by moving to another region (Okkerse [2008], Borjas [1999], Card [2001]). Another way of dealing with endogeneity is to control for the share of immigrants in the previous period. This method based on the idea, that people often decided to move to the area where there is already a settlement of previous immigrants so they can benefit from friends or relatives network (Pischke and Velling [1997], Schoeni [1997] and others).

Other methods to estimate displacement effect of immigration are, for example, estimation of a production function and elasticity with respect to labor or time-series approach which allows for Granger causality tests (see Layard et al. 1991, Pope and Withers [1993]).

An effect of labor market institutions is also described in a wide class of the literature. Beginning with Blanchard and Wolfers [2000], who used EU data and showed a positive effect of protective institutions on unemployment level. Namely, more protective institutions lead to a large effect of negative labor demand shocks. Jean and Jimnez [2011] asses the same effect for OECD countries and the role of economic policy to adjust for such an effect. He found no long-run effect and showed that short-run effect can be observed in a strict institutional environment with stringent product market regulation, high replacement rate or unemployment benefits. Sa [2011] provides evidence of institutional effect on immigration displacement for the EU countries. The results suggest that strict employment protection legislation gives immigrants a comparative advantage relative to natives. Stricter employment protection reduces hiring and firing rates for natives but has a much smaller effect on immigrants.

Angrist and Kugler [2003] paper took a fresh look on the immigration consequences in Western Europe using a quasi-experiment design and constructing instruments based on the Balkan Wars. The authors tried to find out whether the high and persistent level of unemployment in Europe is caused by specific labor protection institutions. The paper therefore provided a new insight into both immigration displacement and institutional effects at the same time and based on both classes of the described literature. In addition, Angrist and Kugler found a significant and negative effect of immigration inflow for some model specifications, which makes it interesting to compare the paper with previous studies. Next section presents the paper in more details and replicates the main tables.

3 Replication

Angrist and Kugler "Protective or counter-productive? Labor market institutions and the effect of immigration on EU natives?" paper (2003) addresses the immigration effect on native employment along with the role of institutions in determining this effect. The authors use a panel data set from European Commission statistical agency-Eurostat for European Economic Area countries for 1983-1999. They begin with simple evaluation of immigration effect for all countries, using the following specification.

$$\ln(y_{ijt}) = \mu_i + \delta_t + \beta_j + \alpha_i \ln(s_{jt}) + \epsilon_{ijt} \tag{1}$$

where $\ln(y_{ijt})$ is the log of the employment-to-population ratio for natives and $\ln(s_{jt})$ is the log of the immigrant (non-national) proportion in labor force for demographic group i, (e.g. men or women) country j, year t. The coefficients α_i for younger (under 40) and older (over 40) men and women can be observed in Table 1 which are replication of Table 3 in Angrist and Kugler[1]. First three columns present result for specification as in equation (1), while the columns (4)-(6) are extended with country specific trends $\beta_{0j} + \beta_{1j}t$ instead of dummy β_j. This extension address the concern that in the long time-series data migration could be correlated with country specific trend. The original specification will give biased results in this case. Alternative specification removes a trend or near-trend component in immigration.

In the original specification the effect is negligible overall and significant for young native men only. When country specific trends are added, the coefficients for men becomes insignificant and coefficient for women becomes negative and significant.

Table 1: Basic Model

	(1)	(2)	(3)	(4)	(5)	(6)
				With trends	With trends	With trends
	Pooled	Under 40	Over 40	Pooled	Under 40	Over 40
Men	-0.0096	-0.021***	0.0023	-0.0094	-0.011	-0.0074
	(0.0069)	(0.0072)	(0.0045)	(0.013)	(0.013)	(0.0060)
Observations	422	211	211	420	211	211
Women	0.00017	0.0018	-0.0014	-0.0125	-0.0221*	-0.0029
	(0.028)	(0.013)	(0.022)	(0.0342)	(0.0132)	(0.012)
Observations	422	211	211	420	211	211

Robust standard errors in parentheses

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

[1]We used the same data set and some codes provided on Angrist Data Archive http://economics.mit.edu/faculty/angrist/data1/data/angkug03

We can conclude that the effect for men are mostly driven by common trend in immigration and employment. The effect for women are vise versa permanent and does not depend on time-varying component. Although these results are very preliminary.

As was described in the previous section a geographical approach to estimation of migration effect suffers from unobservable local productivity and labor demand shocks. Angrist and Kugler use the log of share of immigrants with EU nationality $ln(u_{jt})$ to control (partially) for local demand factors that may increase the overall immigration. In addition, it will be interesting to see whether the internal migration acts to offset negative effect of external migration. The fact that $ln(u_{jt})$ is potentially endogenous should not bias the following IV estimation if instruments are uncorrelated with migration from other EU countries(Angrist and Kugler [2003]).

Table 2 presents the results for specification with EU-share. For models without country specific trends the coefficient for younger men becomes large and overall effect becomes significant from zero. An increase of immigration by 10% costs about 0.21% of native jobs. In addition, the effect of internal immigration is positive and significant which means that migration within EU indeed reduces negative effect of external immigration. In other words, estimation considering immigrants all together leads to the insignificant coefficients. But this is resulted from the fact that external and internal immigration affect employment in different directions and one need to distinguish between them. For women none of the results are significant in specification without trends.

Table 2: Specification with EU-share

		(1)	(2)	(3)	(4)	(5)	(6)
					With trends	With trends	With trends
		Pooled	Under 40	Over 40	Pooled	Under 40	Over 40
Men	non-EU	-0.021**	-0.037***	-0.0039	-0.011	-0.012	-0.010
		(0.0080)	(0.0076)	(0.0054)	(0.015)	(0.012)	(0.0071)
	EU	0.036**	0.053***	0.018*	0.022	0.028***	0.016**
		(0.016)	(0.014)	(0.0095)	(0.019)	(0.0093)	(0.0063)
Observations		402	201	201	402	201	201
Women	non-EU	-0.026	-0.026	-0.026	-0.012	-0.023*	-0.0018
		(0.032)	(0.016)	(0.026)	(0.048)	(0.012)	(0.015)
	EU	0.086*	0.092***	0.081**	0.0083	0.018*	-0.0016
		(0.047)	(0.026)	(0.031)	(0.049)	(0.011)	(0.013)
Observations		402	201	201	402	201	201

Robust standard errors in parentheses

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

In specifications with country trends the effect for men is zero as before. For women the coefficient is significant for younger women only and approximately the same as for men -0.23%. Internal immigration of younger women has a positive effect as for men but lower in magnitude.

As Angrist and Kugler pointed out, inclusion of country specific trends and EU-shares does not, of course, eliminate the problem of endogenous immigration decisions. To deal with an endogeneity problem, an IV strategy is used. The authors used two Balkan Wars as natural quasi-experiments and constructed instruments based on the distance from the wars' main centers. This motivated by the fact, that "the flow from former Yugoslavia became an important part of the European migration picture after 1990 with the number of former Yugoslavian asylum-seekers peaked in 1992 (Bosnia War) and in 1999 when NATO launched air strikes in the Kosovo War. Yugoslavs accounted for more than 30% of asylum-seekers in the war years" (Angrist and Kugler [2003]). As a result the distance from Bosnia and Kosovo intersected with the war years may be potentially good instruments for the intensity of the immigration inflows. To examine this proposal the first-stage equation (2) is estimated.

$$\ln(s_{jt}) = \tau_t + \psi_j + b_{jt}\pi_b + n_{jt}\pi_n + k_{jt}\pi_k + \epsilon_{ijt} \tag{2}$$

where j represents country as before, i - a demographic group, τ and ψ - are year and country dummies,
b_{jt} - the distance from Sarajevo $\times$ dummy for 1991-95 (Bosnia War)
n_{jt} - the distance from Sarajevo $\times$ dummy for 1996-97 (inter war years)
k_{jt} - the distance from Pristina $\times$ dummy for 1998-98 (Kosovo War)
are excluded instruments.

As before the specifications with and without country trends are considered. Moreover, the distance could be measured as a distance from the capital of from the nearest big (in terms of population) city. All specifications are presented in Table 3.

As we can see, all the instruments are significant in all cases. Larger distance from former Yugoslavia is associated with a lower immigrants share during war years. For example, in countries 500 miles away from War centers (like for example Graz, Austria) a share of immigrants is lower by on average 30%. The inter-war period dummy is also significant and has negative coefficient witch could be explained by a prolonged effect of Bosnia war. The pre-war dummy is not significant for most specifications wich is encouraging since it indicates no long-run trend associated with distance form Sarajevo. As an additional check Angrist and Kugler tried the same instruments for EU-nationals and found no influence proving that results in Table 3 indeed reflect the effect of former Yugoslav immigrants.

For the second stage, Table 4, the authors used distance from big city and estimate once again specifications with and without country trends.

Results for men are significant in pooled version without trends (-0.05) and even large for younger men (-0.08). The coefficients now become larger than with OLS estimation namely 10% increase in immigrants share leads to a 0,5% decrease of employment for native men overall. Supposing that the IV estimates are more precise, we can say that these results quite high, especially in comparison with the similar studies for US.

Table 3: First Stage of IV

	(1) Big city No trends	(2) Big city No trends	(3) Big city With trends	(4) Big city With trends	(5) Capital No trends	(6) Capital No trends	(7) Capital With trends	(8) Capital With trends
Bosnia War	-0.51***	-0.53***	-0.61***	-0.50***	-0.40***	-0.54***	-0.55***	-0.61***
	(0.075)	(0.10)	(0.079)	(0.10)	(0.090)	(0.13)	(0.092)	(0.12)
Inter War	-0.40***	-0.42***	-0.74***	-0.61***	-0.21*	-0.34**	-0.63***	-0.71***
	(0.098)	(0.12)	(0.11)	(0.13)	(0.11)	(0.15)	(0.13)	(0.16)
Kosovo War	-0.64***	-0.66***	-1.10***	-0.97***	-0.50***	-0.62***	-1.06***	-1.14***
	(0.13)	(0.14)	(0.11)	(0.14)	(0.16)	(0.18)	(0.14)	(0.17)
Pre-War		-0.049		0.099		-0.29**		-0.057
		(0.11)		(0.069)		(0.15)		(0.097)
Observations	844	844	844	844	844	844	844	844

Robust standard errors in parentheses

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

Coefficients are scaled for 1000 miles

Table 4: IV Estimates

	(1) Polled No trends	(2) Under 40 No trends	(3) Over 40 No trends	(4) Polled With trends	(5) Under 40 With trends	(6) Over 40 With trends
Men	-0.050**	-0.082***	-0.018	0.019	0.020	0.017
	(0.022)	(0.027)	(0.015)	(0.025)	(0.022)	(0.014)
Observations	422	211	211	422	211	211
Women	-0.24**	-0.19***	-0.30***	-0.019	0.0038	-0.042
	(0.11)	(0.063)	(0.093)	(0.13)	(0.024)	(0.034)
Observations	422	211	211	422	211	211

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

As Angrist and Kugler noted, this could be explained, perhaps, by more restrictive protective institutions in Europe. Inclusion of trends make all the results insignificant, showing that trend component plays an important role in determining the displacement effect of immigration for men.

Results for women now become significantly negative but too large in magnitude. On the other hand, when country specific trends are added non of the results are significantly distinguishable from zero. This all suggest that results for women are probably driven by factors other then migration and these other factor are correlated with distance from Sarajevo and Bosnia and changing over time (Distance from Pristina is indeed has highly significant explanatory power for women native employment, as we additional checked). One possible explanation, suggested by Angrist and Kugler is a labor force participation. Women labor force participation increased a lot during the period under review and this growth happened to be larger in countries father from former Yugoslavia (Angrist and Kugler [2003]). Therefore, it is not fully appropriate to use provided instrument for identification of the effect for women.

Adding EU-share as a control has little effect on estimates (Angrist and Kugler [2003]) which seems reasonable since we already controlled for endogenity by using IV. These result are therefore not provided.

Finally, Angrist and Kugler proceed with estimation the effect of institutions. Estimation is conducted for men only since for women the instrument used are correlated with employment, with and without country trends. The estimated model is very similar but now the institutional indicator x_j is added.

$$\ln(y_{ijt}) = \mu_i + \delta_t + \beta_j + (\alpha_{oi} + \alpha_{1i} x_j) \ln(s_{jt}) + \epsilon_{ijt} \tag{3}$$

Here μ_i and δ_t are dummies for demographic group and year, country dummy β_j replaced by country trend in some specifications.

To describe an institutional environment Angrist and Kugler used three indicators:

- Labor standards including the employment protection, restriction on working hours and employment contracts, administrative or unions control, minimum wages.
- Replacement rate (average level)
- Entry costs which are an index of barriers to entrepreneurship

First two indicators are taken from Nickell and Jackman [1991]. Labor standard expressed as an index ranging from 0 to 7 (7 for the most restrictive institutions), replacement rate ranges from 20 to 90%. Entry barriers are taken from Nicoletti et al. [2000] and expressed as an index ranging from 0.5 to 2.75.

Because all three institutional indicators are measured in different ways they are standardized for comparability purpose. The coefficient α_{oi} is therefore represents the effect for country with average institutions and α_{1i} shows how the effect changes with one standard deviation change in institutional indicator x. The result are provided in Table 5[2].

First three columns present an OLS estimation. The main effect of immigration is significant and negative in all the cases. 10 percentage higher immigration inflow results in 0.23-0.27 percentage lower native employment. Interaction with institutions prove the authors' hypothesis about negative effect of protective institutions. All three institutional indicators have negative coefficients when considered individually. For example, stricter (by one standard deviation) labor standards will increase immigration displacement effect on native employment by 0.011% for older men, for 0.02% for younger men and for 0.015% overall. Estimation of all three types of institutional indicators together gives a less clear results, main effect becomes larger for younger men and therefore on average and interaction term is significant for replacement rate only. SLS estimation provides larger negative effect of labor standards and of entry barriers (for older men and overall). Results for replacement rate are not significant. Main effect is significant and quite large in specification with entry barriers and with all the institutions together.

The question appears here is why to use constant institutional indicators in quite long time-series. We saw already that time component plays an important role in determining the effect and it would be also interesting to take into account any trends in institutional environment itself. Last panel of the Table5 presents the results with composite institutional indicator taken from OECD [2003] database [3]. This coefficient is time-varying and ranges from 0 (least stringent) to 6 (most stringent). Coefficients provided by OLS estimation are quite the same as before. Main effect coefficient is significant in pooled regression and indicates 0.29% decrease of native employment when

[2] The results in Table 5 are slightly different from those in the original paper. It may be explained by a slightly different data which could be traced by a number of observations. Another possible explanation lays in calculations. For example, we used mean and sd functions to create standardized variables while authors wrote directly the numbers. One should note also that original code provided by authors was written in SAS and our calculations are done in STATA. In total, the differences are note crucial and do not change the main conclusions.

[3] `http://www.oecd.org/els/emp/oecdindicatorsofemploymentprotection.htm#data`

Table 5: Immigration effect: Interaction with Institutions

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	SLS	SLS	SLS
	Pooled	Under 40	Over 40	Pooled	Under 40	Over 40
Labor standards						
Main effect	-0.023*	-0.039***	-0.0064	-0.010	-0.044	0.023
	(0.013)	(0.013)	(0.0087)	(0.024)	(0.034)	(0.020)
Labor standards	-0.015**	-0.020**	-0.011**	-0.073***	-0.094***	-0.052***
	(0.0078)	(0.0083)	(0.0051)	(0.027)	(0.035)	(0.018)
Observations	334	167	167	334	167	167
Replacement rate						
Main effect	-0.024*	-0.041***	-0.0074	0.050	0.11	-0.0089
	(0.013)	(0.013)	(0.0087)	(0.049)	(0.076)	(0.036)
Replacement rate	-0.016*	-0.019*	-0.014**	0.0072	0.00010	0.014
	(0.0088)	(0.011)	(0.0063)	(0.015)	(0.021)	(0.013)
Observations	334	167	167	334	167	167
Entry barriers						
Main effect	-0.027***	-0.044***	-0.010*	-0.049***	-0.091***	-0.0061
	(0.0098)	(0.0092)	(0.0062)	(0.013)	(0.027)	(0.011)
Entry Barriers	-0.020**	-0.024**	-0.015**	-0.034*	-0.0088	-0.060***
	(0.0096)	(0.011)	(0.0067)	(0.019)	(0.030)	(0.017)
Observations	368	184	184	368	184	184
Labor standards and replacement rate						
Immigrants share	-0.022*	-0.038***	-0.0061	-0.012	-0.047	0.022
	(0.013)	(0.013)	(0.0086)	(0.026)	(0.043)	(0.014)
Labor standards	-0.012	-0.017*	-0.0082	-0.058***	-0.094***	-0.021**
	(0.0077)	(0.0084)	(0.0053)	(0.022)	(0.033)	(0.011)
Replacement rate	-0.013	-0.014	-0.011*	-0.015	-0.028	-0.0021
	(0.0085)	(0.0100)	(0.0063)	(0.015)	(0.023)	(0.010)
Observations	334	167	167	334	167	167
All three institutions						
Immigrants share	-0.031**	-0.048***	-0.015	-0.069**	-0.12***	-0.012
	(0.015)	(0.016)	(0.012)	(0.031)	(0.044)	(0.018)
Labor standards	-0.0021	-0.0057	0.0015	0.031	0.041	0.021
	(0.013)	(0.016)	(0.0097)	(0.019)	(0.025)	(0.014)
Replacement rate	-0.015**	-0.017	-0.014**	0.0079	0.0092	0.0066
	(0.0077)	(0.011)	(0.0061)	(0.011)	(0.016)	(0.0092)
Entry Barriers	-0.018	-0.019	-0.017	-0.090***	-0.13***	-0.048**
	(0.017)	(0.020)	(0.013)	(0.031)	(0.047)	(0.020)
Observations	334	167	167	334	167	167
Labor protection (OECD indicator)						
Immigrants share	-0.029***	-0.012	0.0078	-0.031	0.059	0.034**
	(0.0088)	(0.011)	(0.0074)	(0.020)	(0.041)	(0.016)
Labor protection	0.0026**	-0.0088***	-0.0048**	-0.0033	-0.030***	-0.013***
	(0.0013)	(0.0027)	(0.0019)	(0.0041)	(0.0091)	(0.0036)
Observations	343	165	178	343	165	178

Instruments used are as in Table 3 plus interaction with institutional measures. The EU-share is included and treated as exogenous. *** p<0.01, ** p<0.05, * p<0.1

immigrants share increased by 10%. The effect of institutions is now significant but an order of magnitude smaller and hardly distinguishable from zero. For example, in a country with a one point higher labor protection the 10% higher immigrants share will lead to a 0.08% lower native employment for younger men. The effect is even smaller for older men and very small for men overall. SLS results are more difficult to interpret. On the one hand, the coefficients for interaction terms are now higher and more similar to previous specifications. On the other hand, the main immigration effect for old men is now positive and significant which makes no sense.

When country trends are added the results (not presented) become less tractable and shows sometimes significant interaction terms (Angrist and Kugler [2003]). We would rather say that almost no results are significant. We suppose that in the specifications ignoring the time-trend (like constants institutional indicators) one find a correlation which presents a common trend in migration and employment. When country trends are added almost no effect of immigration is found. Restrictive institutions, however, could worsen any negative influence on employment. Our institutional indicator which varies over time suggests that in a very restrictive institutional environment with high labor standards immigration would decrease a native employment. Alternatively, it could be the case that immigration indeed increase native employment in first period and we found this increase. As it was described in first two sections the effect may switch from positive to negative over time. We will address this question in the last section.

In total, the results for men are significant and large in comparison with other studies. This could be explained by strict institutions in Europe. However, these large influence could be simply a result of common trend in immigration and employment for men. As we saw no results for men are significant when country trends are included or a time-varying indicator for institutional environment is used. We will try to review these findings with more recent data and longer time-series in the next section.

4 New findings

We now turn to the similar model but using more recent data from Labor Force Survey (LFS) provided by Eurostat [2012] (see [4]). We have panel data for 19 European countries from 1983 to 2011. We also use the same static institutional indicators as Angrist J., Kugler A. (2003) as well as general time-varying OECD indicator. Without explaining the model specifications in details since they are the same as in the previous section we will proceed to the discussion of the results.

We begin with IV estimation. The most difficult issue here is, of course, an appropriate instruments. It is quite difficult to find any event, affecting immigration all over the Europe since immigration policy was very different in different countries. All macroeconomic or political shocks affecting population and migration processes are likely to affect labor markets as well. It turns out, however, that we can still use the instruments proposed by Angrist and Kugler [2003]. It is not surprising when taking into account the fact that 60% of our data sample consist of the same time span. New instrument could be, of course, a way to improve the results but as Table 6, columns (1) and (2), suggests the distance from Bosnia War center is still a good instrument for non-EU nationals immigration, although the correlation is now lower. The coefficients scaled for a 1000 miles so in countries 500 miles away from War center the share of immigrants is lower by on average 18%. For EU-nationals we also observe a large negative correlation. This may reflect a new trend in immigration inflows (other than Balkan wars shock) in countries that are further away from former Yougoslavia. In any case the distance could not be influenced by the employment or migration so we can use it to deal with an endogeneity problem. The inter-war dummy is not significant in any specification and we therefore do not use it as an instrument in further analysis. To sum up, we have negative correlation between distance from Sarajevo and external immigration as well as between both distances and internal immigration.

When country trends are added-columns (3) and (4) - the coefficients for EU-nationals become insignificant so we can suppose we indeed capture the external war effect by these instruments as Angrist and Kugler. However, in this specification there is a strong positive correlation with distance from Pristina for non-EU nationals and this positive correlation appears before the war years as indicated by a pre-war dummy (although it becomes much stronger during war years). We could say that in specifications with trends we capture a new positive tendency in external immigration inflow in countries that are further away from Prisitna.

Table 7 shows the results for the second stage of SLS estimation separately for men and women, with and without country trends. In specification without trends an immigrants share has no effect for both men and women. Internal immigration has a positive effect for young men only with 10% increase in internal immigrant share increasing native employment by approximately 1% witch is in accordance with Angrist and Kugler [2003]. When country trends are added immigration effect becomes positive. It is significant

[4] http://epp.eurostat.ec.europa.eu/portal/page/portal/employment_unemployment_lfs/data/database

Table 6: IV: First Stage

	(1) No trends	(2) No trends	(3) With trends	(4) With trends
Non-nationals				
Distance from Sarjevo, Bosnia War	-0.368***	-0.360***	-0.0848*	-0.109**
	(0.0858)	(0.0867)	(0.0484)	(0.0547)
Inter-war dummy	-0.232	-0.232	0.0702	0.0694
	(0.186)	(0.186)	(0.132)	(0.132)
Distance from Pristina, Kosovo War	0.0560	0.0559	0.491***	0.491***
	(0.138)	(0.138)	(0.155)	(0.155)
Pre-War dummy		-0.0316		0.103*
		(0.213)		(0.0622)
Observations	655	655	655	655
EU-nationals				
Distance from Sarjevo,	-0.459***	-0.456***	-0.0347	-0.0440
	(0.0865)	(0.0936)	(0.0341)	(0.0393)
Inter-war dummy	0.384	0.384	0.0678	0.0678
	(0.267)	(0.267)	(0.129)	(0.129)
Distance from Pristina,	-0.421**	-0.421**	-0.0476	-0.0481
	(0.187)	(0.187)	(0.154)	(0.154)
Pre-War dummy		-0.0156		0.0413
		(0.181)		(0.0415)
Observations	573	573	573	573

Distance measured from the nearest big city

for younger and older men and is also positive and significant for older women and for women overall. Immigration inflow increase by 10% will now mean larger women native employment by 0.75%. Could it be realistic? We suppose this positive effect could be a result of increasing demand for goods and services from the side of immigrants. This new demand increases demand for labor force and therefore employment. The EU-share is also positively significant in this specification for younger and older men and for younger women.

So, in contrast to theAngrist and Kugler [2003] we found positive effect of immigration but only when the trend component of immigration share is extracted. This somehow support our ideas about Angrist and Kuger results being driven by (negative) common trends. In the longer time series these trends become less strong and the actual positive effect becomes more obvious.

We now interact the immigration effect with institutions and use OLS estimation as well as SLS. The results for men could be found in Table 8. OLS estimation gives the negative immigration effect but for older men only. When we use time-varying

Table 7: IV: Second Stage

	(1) Pooled no trends	(2) Under 40 no trends	(3) Over 40 no trends	(4) Pooled with trends	(5) Under 40 with trends	(6) Over 40 with trends
Men	0.71	-0.10	0.0097	0.013	0.050***	0.066**
	(1.07)	(0.083)	(0.034)	(0.43)	(0.010)	(0.026)
Observations	215	100	115	215	100	115
Women	0.089	0.15	-0.11	0.075***	0.012	0.051**
	(0.13)	(0.11)	(0.098)	(0.027)	(0.040)	(0.026)
Observations	235	116	119	235	116	119
Men with EU-share						
Immigrants	-0.72	-0.14	0.081	-0.15	0.0016	0.0062
	(1.12)	(0.10)	(0.13)	(0.29)	(0.016)	(0.016)
EU-share	0.38	0.095***	-0.034	0.13	0.060***	0.024**
	(0.54)	(0.035)	(0.066)	(0.18)	(0.015)	(0.011)
Observations	172	78	94	172	78	94
Women with EU-share						
Immigrants	0.36	-0.32	-0.17	-0.00074	0.0070	0.027
	(0.56)	(0.65)	(0.17)	(0.12)	(0.026)	(0.018)
EU-share	-0.16	0.21	0.12	0.031	0.046**	-0.0032
	(0.31)	(0.36)	(0.096)	(0.075)	(0.018)	(0.011)
Observations	192	96	96	192	96	96

indicator this negative result appears for younger men as well. What is more interesting institutional tightness make this negative effect smaller for younger men (labor standards also for older men). On average, while there is almost no effect of immigration on native employment, in countries with very protective labor market institutions this effect may become positive. For example, higher by one standard deviation labor standards will protect existing workers and increasing demand from new-comers will stimulate new job creation. As a result native employment will increase.

In case of SLS estimation the results for main effect are pretty the same and positive for younger men. Positive institutional effect, however, becomes too large to be explained by institutions solely which may be a result of bad instruments. The effect of replacement rate is now negative but also too large when considered separately. In specification with the time-varying indicator no effects are significant.

Although we remember that our instruments are potentially correlated with employment for women we try to estimate institutional effect for women as well. As shown in Table 9 the SLS results for women are quite the same as for men. When main effect is significant it is positive and interaction with institutions is positive and probably too large. OLS estimation also gives a positive main effect for younger women, although lower than in case of SLS estimation. Labor standards and high replacement rate make this positive effect even higher for younger women. Interesting are the results for time-varying indicator (last panel of the table) because only in this specification we have negative immigration effect which is however once again unrealistically high. The effect

Table 8: Interaction with Institutions: Men

	(1) OLS Pooled	(2) OLS Under 40	(3) OLS Over 40	(4) SLS Pooled	(5) SLS Under 40	(6) SLS Over 40
Labor standards						
Main effect	-0.019	0.00041	-0.025***	-0.019	-0.090	0.076
	(0.067)	(0.012)	(0.0080)	(0.67)	(0.085)	(0.086)
Labor standards	0.018	0.038***	0.0098*	-0.083	0.11**	0.018
	(0.057)	(0.012)	(0.0057)	(0.24)	(0.053)	(0.043)
Observations	280	138	142	172	78	94
Replacement rate						
Main effect	-0.0092	0.023	-0.020**	0.36	0.069**	0.058
	(0.066)	(0.016)	(0.0082)	(0.51)	(0.034)	(0.069)
Replacement rate	0.010	0.026**	0.0065	-0.79	-0.13***	-0.20**
	(0.046)	(0.011)	(0.0048)	(0.68)	(0.043)	(0.096)
Observations	280	138	142	172	78	94
Entry Barriers						
Main effect	-0.014	0.014	-0.024***	0.26	0.089*	0.088
	(0.061)	(0.017)	(0.0079)	(0.71)	(0.053)	(0.13)
Entry barriers	0.0025	0.012	0.010	0.43	0.30**	0.057
	(0.054)	(0.020)	(0.0063)	(1.42)	(0.15)	(0.11)
Observations	290	143	147	172	78	94
All three institutions						
Main effect	-0.026	-0.0031	-0.022**	0.072	0.074***	0.018
	(0.084)	(0.018)	(0.011)	(0.33)	(0.019)	(0.019)
Labor standards	0.028	0.042**	0.0051	-0.051	0.0082	0.0038
	(0.090)	(0.019)	(0.0091)	(0.19)	(0.017)	(0.012)
Replacement rate	0.0039	0.017	-0.0018	-0.18	-0.076***	-0.071***
	(0.069)	(0.018)	(0.0071)	(0.31)	(0.029)	(0.018)
Entry barriers	-0.020	-0.032*	0.0091	0.021	0.13***	0.084**
	(0.068)	(0.016)	(0.0075)	(0.63)	(0.047)	(0.035)
Observations	280	138	142	172	78	94
Labor protection (OECD)						
Main effect	-0.045	-0.058***	-0.036***	0.78	-0.25	0.073
	(0.12)	(0.019)	(0.012)	(1.60)	(0.21)	(0.13)
Labor protection	0.012	0.031***	0.0031	-0.20	0.10	-0.0099
	(0.053)	(0.0094)	(0.0057)	(0.46)	(0.065)	(0.043)
Observations	318	157	161	168	76	92

EU-share is included and treated as exogenous

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 9: Interaction with Institutions: Women

	(1) OLS Pooled	(2) OLS Under 40	(3) OLS Over 40	(4) SLS Pooled	(5) SLS Under 40	(6) SLS Over 40
Labor standards						
Main effect	0.0149	0.0163	0.0135	-0.0188	-0.0899	0.0764
	(0.0365)	(0.0100)	(0.0174)	(0.667)	(0.0851)	(0.0862)
Labor standards	0.0340	0.0492***	0.0188	-0.0827	0.114**	0.0177
	(0.0358)	(0.0107)	(0.0158)	(0.239)	(0.0535)	(0.0430)
Observations	308	154	154	172	78	94
Replacement rate						
Main effect	0.0305	0.0454***	0.0157	0.0403	0.0980**	0.0863
	(0.0328)	(0.0141)	(0.0205)	(0.261)	(0.0399)	(0.164)
Replacement rate	0.0151	0.0334***	-0.00311	-0.108	-0.102	-0.358*
	(0.0295)	(0.00933)	(0.0122)	(0.505)	(0.0925)	(0.203)
Observations	308	154	154	192	96	96
Entry barriers						
Main effect	0.0198	0.0309**	0.00870	0.391	0.0291	-0.144
	(0.0294)	(0.0148)	(0.0176)	(0.660)	(0.147)	(0.256)
Entry barriers	0.00283	0.0134	-0.00773	-0.0623	0.0748	-0.186
	(0.0308)	(0.0187)	(0.0112)	(0.550)	(0.134)	(0.432)
Observations	318	159	159	192	96	96
All three institutions						
Main effect	-0.00564	0.0106	-0.0219	0.0700	0.109	-0.0376
	(0.0323)	(0.0147)	(0.0219)	(0.166)	(0.0665)	(0.0424)
Labor standards	0.0627*	0.0594***	0.0659***	0.0960	0.0960**	0.0728**
	(0.0344)	(0.0181)	(0.0215)	(0.0871)	(0.0419)	(0.0348)
Replacement rate	-0.00296	0.0223	-0.0282**	0.0292	0.101	-0.0791
	(0.0260)	(0.0163)	(0.0139)	(0.188)	(0.101)	(0.0600)
Entry barriers	-0.0363	-0.0453***	-0.0273	0.0702	0.137	-0.117
	(0.0262)	(0.0167)	(0.0180)	(0.253)	(0.135)	(0.0897)
Observations	308	154	154	192	96	96
Labor protection (OECD)						
Main effect	-0.044	-0.070***	-0.019	0.25	-0.11	-0.25**
	(0.081)	(0.014)	(0.031)	(0.57)	(0.16)	(0.10)
Labor protection	0.037	0.047***	0.027**	-0.0040	0.086	0.13***
	(0.035)	(0.0077)	(0.014)	(0.16)	(0.052)	(0.042)
Observations	346	173	173	184	92	92

EU-share is included and treated as exogenous

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

of protective institutions is still positiver.

To check whether some results are come from common trend we repeat all the estimation adding country specific trends. Without presenting the tables we will describe the significant coefficients only[5]. OLS estimation for men gives positive main effect coefficient which equals on average 0.04. The only significant institutional effect is labor standards (approximately 0.03). With SLS estimation none of the results are significant. In OLS estimation the main effect is significant and positive for younger women and shows approximately 0.4 percentage response for 10% increase in immigrant share. Labor standards make this effect larger. SLS estimation provides positive main effect for older women only in specification with all three institutions and no other significant results. One should remember, however, that female employment in Europe is, as a rule, lower than the male one. Therefore small changes in levels will give quite big changes in percentage points.

To conclude, we have found that for men the main effect of immigration inflow on the native employment is negative and in accordance with Angrist and Kugker (2003) results. But this results are sensitive to the inclusion of country trends and, probably, reflect only this trend correlation. A strict institutional environment protects existing workers and makes this negative effect smaller or even positive. For women, in contrast, the main effect is positive and strict institutions make it larger as well. The results provided by IV estimation for women seem to be unreliable to draw any conclusions. So the problem of finding better instruments is a potential issue for further analysis.

Positive effect of immigration for women could be explained by high demand of newcomers on some special services. Woman are more likely to work on part-time positions and overrepresented in service sector (seeEurostat [2011], Melkas and R. [2001] and L. Dijk [2002]). Increasing demand for goods and services raise a labor demand and therefore employment in this spheres. Protective labor market institutions save existing workers from being replaced and the overall employment is therefore increased. However, as it was discussed in the Introduction, active participation in goods market and not so active in labor market is more likely on the initial stages after immigration. This raise a question about the dynamics of the immigration effect. The specification used by Angrist and Kugler implies that the effect is immediate and permanent. They therefore study a long-term effect of immigration and institutions in equilibrium. As it was discussed in previous sections some papers focus on dynamic dimension of this influence and we will as well in the next section.

[5] All the results discussed it the paper could be found in our STATA-code

5 Dynamic Specification

As we discussed already it is quite natural to suppose that the effect of immigration is not permanent but changing over time. Immigrants enter a product market more quickly than labor market. So immigration inflow boosts a product demand and therefore a labor demand first and only on the later stages progressively increases labor supply (Z. and Yashiv [2002]).

Moreover, the main assumption about protective institutions covering natives rather than new-comers seems to be more plausible in short-run. When an immigrant had came to the country, for instance, 5 years ago, it is unlikely that he is still unaware of his rights on labor market and existing protective institutions or that he is still working illegally. To sum up, the effect could be different in short and long-run.

Along with Jean and Jimnez [2011] paper we will consider the following specification

$$y_{jt} = \alpha + \mu_j + \delta_t + \beta_{1j}t + (\lambda_0 + \lambda_1 x_{jt})y_{j(t-1)} + \sum_{l=0}^{L}(\alpha_{0l} + \alpha_{1l}x_{jt})\Delta s_{j(t-l)} + \\ + (\alpha_{LR0} + \alpha_{LR1}x_{jt})s_{j(t-L-1)} + \epsilon_{jt} \tag{4}$$

Since we use lags it is not necessary to employ a logarithmic specification. y_{jt} is a native employment as before, μ_j is a country dummy, δ_t - year dummy, $\beta_{1j}t$ - country specific trend, x_{jt} is an institution indicator, we use both a time-varying indicator and constant indicators from Angrist and Kugler [2003], expressed in standard deviations. $\Delta s_{j(t-l)}$ represents lagged changes in immigrant share and $s_{j(t-L-1)}$ is the lagged level, L is a maximum number of lags. The reason to include the first lag of dependent variable $y_{j(t-1)}$ is, as before, an endogenity issue. This term controls for previous labor market outcomes (shocks). In contrast to Jean and Jimnez [2011] we do not include any measure of macroeconomic shocks except this control[6].

The model focuses on a time profile of immigration shocks and represents an "impulse-response" idea (Jean and Jimnez [2011]). Parameters of interest are αs. α_{0l} indicates a temporary impact of change in immigrant share on employment level l periods ahead and α_{1l} shows how this impact is affected by a labor policy, i.e. institutions. α_{LR0} and α_{LR1} capture the effect of lagged level which remains after all the temporary influence died out. This could be interpreted as a long-term effect.

The first question for estimation is how many lags to include. Jean, Jimnez found no significant short-run effect after three years and therefore include 5 lags. We will use the same approach. For more precise identification one could use additional statistical tests. Next step is to choose an estimation strategy. Jean, Jimnez used fixed-effect feasible GLS accounting for heteroscedasticity across panel. However, a presence of fixed effects in a lagged-dependent-variable model makes the y_{t-1} endogenous by construction. We can also face a problem with autocorrelation in y when the time-series become longer. Moreover, we are not sure that we completely eliminate endogenity by introducing a

[6] As Jean, Jimnez pointed out themselves, inclusion of this macro shocks is aguable (see Blanchard and Wolfers [2000]

control for previous market shocks and finding good instruments could be a tricky task as it was discussed above. Assuming that the error-term ϵ_{jt} is not serially correlated, we used an estimator suggested by Arellano and Bond [1991] with generalized method of moments (gmm) estimation. The main idea is to take first differences to eliminate an individual effect and use past lags of variables (or lags of variables changes) as instruments[7]. We begin with estimation of immigration effect without institutions. Results of both methods could be found in Table10.

Table 10: Dynamic Effect of Immigration

	(1)	(2)	(3)
VARIABLES	FGLS	Arelano-Bond GMM	Arelano-Bond with EU-share
L.e_p	0.97***	0.95***	0.95***
	(0.0031)	(0.029)	(0.036)
D.Imm	-0.0086	-0.018**	-0.015
	(0.0092)	(0.0085)	(0.018)
LD.Imm	-0.015	-0.034***	-0.032
	(0.011)	(0.013)	(0.033)
L2D.Imm	-0.018	-0.047**	-0.045
	(0.015)	(0.019)	(0.050)
L3D.Imm	-0.018	-0.058**	-0.054
	(0.019)	(0.025)	(0.067)
L4D.Imm	-0.016	-0.064**	-0.062
	(0.023)	(0.030)	(0.078)
L5D.Imm	-0.045	-0.15**	-0.17*
	(0.047)	(0.062)	(0.095)
L6D.Imm	0.0025	-0.11*	-0.12
	(0.042)	(0.064)	(0.10)
L7D.Imm	-0.032	-0.18***	-0.20*
	(0.031)	(0.065)	(0.11)
L8.Imm	-0.039	-0.20***	-0.18
	(0.039)	(0.074)	(0.12)
L8.eu_lf1			0.014*
			(0.0072)
Observations	585	515	442
R-squared	0.998		
Number of id		62	62

Robust standard errors in parentheses

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

[7]We also implicitly assume that lagged differences which used as instruments for level-variables, s_{t-L-1} in our case, are uncorrelated with unobservable country specific effects.

In the table L's denote lags and D's differences. For, example, L2D.Imm is here the second lag of difference in immigrant share, D.Imm is the first difference (current change), L.Employment is the first lag of dependent variable.

In both specifications the first lag of employment is strongly significant meaning a high degree of persistence in employment shocks. Jean, Jimnez also get a significant coefficient 0.68 for the first lag of employment, and approximately 0.4 for the second and the third lags of difference in immigration share. This results become smaller or insignificant when controls for macroeconomic shocks are added. They found no significant coefficient for lagged level of immigrants share in any specification meaning no long-term effect. Our results from FGLS are pretty the same and show no long-run effect of immigration on employment. In contrast to FGLS, an Arelano-Bond estimator shows significant and negative effect of change in immigration inflow during the first 7 years and then also a long-term effect -0.2. These effects disappear except ffor 5th and 7th lags when EU-share control is added. Unfortunately, we can not check the autocorrelation assumption by estat abond test because of unbalanced panel.

Table 11 then shows the results for interaction with institutions. In all the models the first lag of employment has significant and strong influence on today's employment. In case of replacement rate (RR) the first lag of difference in immigration share also matters (when we do not control for EU-share) and this influence is positive. Moreover, this positive influence becomes larger if replacement rate increases. This could mean that in the first year immigrants participate mostly in product market increasing product demand and therefore a demand for labor force. This positive shock could be amplified by internal demand if replacement rates (and therefore income of retired people) are higher. Entry barriers (EB) do not influence displacement effect in native employment and change in immigration share is also insignificant in this specification. The same situation is observed in case of Labor standards (LS) for short-term effect. The long-run effect of immigration is positive in this case (when do not control on EU-share).

Unfortunately, the estimates for OECD time-varying indicator (not presented) is very sensitive to the number of lags included and coefficient have mostly sign and magnitude which is difficult to interpret. FGLS estimation (not presented), in contrast, provides no significant result for any number of lags in this case. Either there is indeed no influence of labor protection institutions,that are included in the OECD indicator, on immigration displacement effect or it could be also the case that the trend in labor protection development is simply correlated with immigration. Namely, countries with larger immigration inflow create more protective institutions for natives.

We should also note that EU-share included as a control has a positive coefficient which confirms the ideas about internal immigration offsetting the negative effect of external immigration.

To sum up, the immigration inflow has on average no effect on native employment during the first years and could have negative effect after 5-7 periods while in the long-run no effect is observed. When protective labor market institutions are considered, the short-run effect of immigration is found to be positive. Institutions, namely first difference in replacement rate, have, as in the static specification, positive effect on employment level in the next period. Overall we confirm our ideas about positive effect of new-comers on native employment level and protective institutions saving jobs of existing workers. We now have an evidence that such an effect is temporary and that there is no long run effects observed.

6 Conclusion

This paper reviews some results about immigration displacement effect. A question of special interest was a policy implication issue. Could the labor policy protect existing workers or will it only increase costs of hiring native workers and result in natives loosing their jobs. This topic is of great interest and importance since an immigration policy becomes more important in Europe nowadays. Many countries try to develop new labor market regulations in order to stimulate employment and at the same time provide immigrants a possibility for integration.

After replication of the Angrist and Kugler [2003] results, this paper used the same specification and instruments to estimate the same effect on more recent data. The effect of immigrant share for men is found to be negative. However, this result is sensitive to the inclusion of country specific trends and may simply reflect a negative correlation between immigration inflows and instruments. When country trends are added there is no effect in almost all specification. The effect for women is positive in contrast to the original paper. When the share of immigrants increases by 10% a native employment of women increased by on average 0.4%. This could be explained by the fact that the new-comers increase a country demand for goods and services which in turn raises a demand for labor force. Women could be more sensitive to this shock because of some specific features of women employment in Europe such as occupation in services or high rate of part-time employment.

The paper shows also that protective labor market institutions fulfill their function of protecting existing workers. More stringent labor standards or replacement rate mitigate negative immigration effect or amplify positive effect. The dynamic specification suggests, however, that both immigration and institutional effects are quite temporary and disappear after one year. This specification provides also no evidence of any long-term effect of immigration.

Although the paper lefts an open question about more appropriate instruments, it shows no support to the idea of negative effect of protective institutions or amplification of immigration displacement effect.

Table 11: Dynamic Effect, Iteration with Institutions

	(1) RR	(2) RR with EU	(3) EB	(4) EB with EU	(5) LS	(6) LS with EU
L.employment	0.97***	0.98***	0.96***	0.96***	0.97***	0.97***
	(0.024)	(0.037)	(0.026)	(0.041)	(0.024)	(0.037)
D.	0.019	0.040	0.012	-0.0042	0.044	0.046
	(0.025)	(0.034)	(0.032)	(0.058)	(0.030)	(0.039)
LD.	0.051**	0.090**	0.028	-0.018	0.017	0.018
	(0.026)	(0.043)	(0.037)	(0.096)	(0.031)	(0.055)
L2D.	0.018	0.082	0.033	-0.029	0.00069	-0.0017
	(0.028)	(0.056)	(0.043)	(0.14)	(0.036)	(0.078)
L3D.	-0.034	0.049	0.0081	-0.090	-0.026	-0.036
	(0.034)	(0.073)	(0.052)	(0.18)	(0.046)	(0.11)
L4D.	0.00076	0.10	0.010	-0.090	0.0080	-0.028
	(0.038)	(0.090)	(0.060)	(0.21)	(0.051)	(0.13)
L5D.	0.025	0.12	0.012	-0.15	0.035	-0.031
	(0.043)	(0.082)	(0.071)	(0.25)	(0.057)	(0.15)
D.Imm	0.025	0.021	0.013	0.036	0.014	0.037
	(0.033)	(0.038)	(0.031)	(0.040)	(0.011)	(0.032)
LD.Imm	0.066*	0.057	0.027	0.057	0.0023	0.044
	(0.035)	(0.045)	(0.036)	(0.056)	(0.013)	(0.056)
L2D.Imm	0.024	0.014	0.039	0.084	-0.00023	0.056
	(0.038)	(0.051)	(0.041)	(0.071)	(0.014)	(0.082)
L3D.Imm	-0.044	-0.056	0.020	0.073	-0.0065	0.075
	(0.046)	(0.066)	(0.050)	(0.096)	(0.018)	(0.11)
L4D.Imm	0.0076	-0.0020	0.030	0.11	0.012	0.096
	(0.050)	(0.065)	(0.056)	(0.10)	(0.020)	(0.13)
L5D.Imm	0.043	0.012	0.058	0.11	0.027	0.11
	(0.050)	(0.068)	(0.060)	(0.11)	(0.030)	(0.14)
L6.neu_rr1	0.013	0.079	0.0099	-0.36	-0.016	-0.20
	(0.047)	(0.086)	(0.077)	(0.27)	(0.031)	(0.15)
L6.Imm	0.057	0.028	0.076	0.13	0.050*	0.20
	(0.057)	(0.075)	(0.031)	(0.15)		
L6.		0.24***		0.36***		0.29***
		(0.083)		(0.091)		(0.080)
Observations	485	382	497	394	485	382
Number of id	54	54	58	58	54	54

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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