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# TITLE

# Priority to local workers and its consequences on unemployment duration

**Research paper**

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**Keywords**

Unemployment duration | Employment priority | Policy evaluation | Difference-in-differences | Labour Migration

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# Priority to local workers and its consequences on unemployment duration

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November 4, 2015

**Abstract**

The impact of immigration on national labour markets is of increasing concern for policy makers. Employment, unemployment and wages are the traditional channels examined by social scientists when bringing evidence to the decision-making table. However, this paper contributes to the more limited empirical literature on migration's effects on unemployment duration, by exploiting the deregulation of migration policies between Switzerland and the European Union. It applies a counterfactual approach and implements *difference-in-differences* methodology paired with *inverse probability weighting*. This paper concludes ultimately that the abolition of the priority granted to local workers results in a rise in unemployment duration.

*Keywords*: Unemployment duration Employment priority Policy evaluation Difference-in-differences Labour Migration

*JEL Classification Codes*: F22, J64, J68

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# 1 Introduction

Immigration plays an increasingly important role in government agendas and new policies are being implemented in order to limit its effects on the labour market. Immigrants are often blamed for the deterioration of labour market conditions and for the rising costs of social security. Trying to provide scientific evidence to policy maker, many economists and social scientists have set out to establish the consequences of immigration on local labour market outcomes (for recent literature reviews see: Longhi et al. (2008); Kerr and Kerr (2011) and Borjas (2014)). Although, the relationship between unemployment and immigration has been the focus of many studies, only a few have analysed the consequences on unemployment duration. This paper contributes to the limited empirical literature of the migration's effects on unemployment duration by exploiting the deregulation of the migration policies, part of the Agreement on the Free Movement of Persons (AFMP) between Switzerland and the European Union (EU).

The costs of long term unemployment are well documented (see for example Edin and Gustavsson (2005); Sullivan and von Wachter (2009) and Lindo (2011)) and can affect individuals at the financial, social and health level. As a consequence, longer unemployment spells also affect (directly and indirectly) the financial costs for governments. Understanding the impacts of migration on unemployment duration is crucial for policy makers wanting to reduce the costs of social security and improve the quality of the labour market conditions.

Zweimuller and Winter-Ebmer (1996) are among the few focusing on unemployment duration, they found little consequences on unemployment entry following an increase in immigration for blue-collar workers. However, there is a significant increase in unemployment duration within all subgroups analysed. More recently, Fromentin (2012) showed that migration influences short-term unemployment positively and long-term unemployment negatively in 14 OECD countries. Time spent in unemployment depends both on the number of vacancies and the extent to which the job-seeker is the best match with the job on offer (Nickell (1980), Pissarides (2000)). A liberalization of migration policies increases the number of candidates for any job and is likely to negatively affect whether or not an individual is the best candidate for a given job. On the other hand, Chassamboulli and Palivos (2014) point out that firms might anticipate lower wages for immigrants and consequentially create more jobs and opportunity for all workers. Hence, the impact of migration on unemployment duration is difficult to anticipate.

Following the evaluation framework suggested by Losa et al. (2012) we focus on the abolition of the priority granted to local workers over new cross-border workers[1] and consider it as a natural experiment, evaluating its consequence on unemployment duration. Using a *counterfactual* approach, we implement *difference-in-differences* (DID) paired with *inverse probability weighting* (IPW), to analyse whether or not the increased competition on the labour market has consequences on the permanence in unemployment for resident workers.

In addition to contribute to the scarce empirical literature studying the consequences of migration on unemployment duration, this paper is also crucial for Swiss-policy makers. The priority

[1] Cross-border workers are migrants that instead of relocating commute from their home country for work. They represent an important component of the Swiss labour market reaching up to 20% of the cantonal labour force.

granted to resident workers and its consequences on the labour market has become even more central after the referendum called "stop mass immigration" that was held in Switzerland and approved in February 2014. This referendum requires the Swiss government to fix quotas on residency and working permits and to restore the employment priority to Swiss nationals. The interest of this paper goes beyond the Swiss borders, results can be generalised and used to anticipate the consequences of the abolition of similar policies in other countries. In fact, other countries, including the UK and US, implements similar policies aimed at prioritising the local population over new immigrants.

The paper is organised as follows: section 2 introduces in details the policy analysed in this paper and the challenges for its evaluation; section 3 outlines the methodology and datasets and section 4 introduce results on the methodology's robustness. Finally, section 5 presents the results of the evaluation framework and section 6 concludes the paper.

## 2 The free movement of persons: a counterfactual approach

The policy studied in this paper is part of bilateral Agreement on the Free Movement of Persons (AFMP) between Switzerland and the European Union (EU). The deregulation of the labour market, resulting from the AFMP, has generated a long debate: supporters say it should boost the economy, generating new opportunities for national firms and (as a consequence) for workers. Meanwhile, for critics, the increased competition will deteriorate the quality of the labour market, generating more unemployment and decreasing local wages. In order to prevent sudden changes in the Swiss labour market, the government decided to gradually implement the bilateral agreement between 2002 and 2007 and maintain until 2014 the possibility of limiting migration.

Several studies tried to analyse the impact of the AFMP, some analysis were conducted with an *ex-ante* General Equilibrium Analysis and found no effects on immigration flows (Straubhaar, 1999) and small but positive effects on welfare (Müller and Grether (1999); Müller and van Nieuwkoop (1999) and Grether and Müller (2001)). Studies conducted ex-post (e.g. KOF (2008); Stadler (2010) and Gerfin and Kaiser (2010)) focuses on the effects on welfare, employment and wages. Basten and Siegenthaler (2013) found that the rise in immigration over the last decade reduced unemployment of residents by enabling them to fill jobs requiring higher skills, while it did not have negative effects on wages and employment. Losa et al. (2012) found important effects on employment and wages, specifically a reduction of employment and a rise in wages. Their results vary depending on the studied population and in particular when comparing Swiss to immigrants and skill levels.

This paper focuses on the repeal of the law prioritizing locals in allocating jobs. Before the $1^{st}$ of June 2004, firms wanting to hire a new employee had to prioritize locals over new immigrants. An employer wanting to apply for a working permit for a new migrant, had first to produce evidence that he was unable to find a suitably qualified person, in terms of education and job experience, already residing in Switzerland. After the $1^{st}$ of June 2004 this priority was abolished and firms were thereafter free to hire people from all signatory countries[2]. The

[2]At first the agreement covered the countries members of EU-15 (Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, Sweden and the United Kingdom) as well as Cyprus and Malta. The agreement was then extended to the others member of

priority to locals was monitored and implemented by sectoral commissions (composed by local government, unions and firms representatives) in a strict way. Some economic sectors, where the local labour supply was not enough to fill the need of firms, were granted working permits directly, in the other sectors the choice was made case by case by the commission.

The gradual implementation of the agreements generated some discontinuities that can be exploited in an evaluation framework. Specifically, in our paper we exploit the presence of regional limitations on the employment of cross-border workers. This limitation, implemented in the 1970s, was removed only in June 2007. Unlike firms in the border region (BR, in green in figure 1), firms in the central region of Switzerland (CR, in yellow in figure 1), were not allowed to hire cross-border workers. As a consequence, persons living in CR are not directly affected by the increased competition with this component of the labour force. If we limit the analysis to data prior to June 2007 and focus on the abolition of the priority granted to local workers over cross-border workers, CR can be considered as a natural control group for BR.

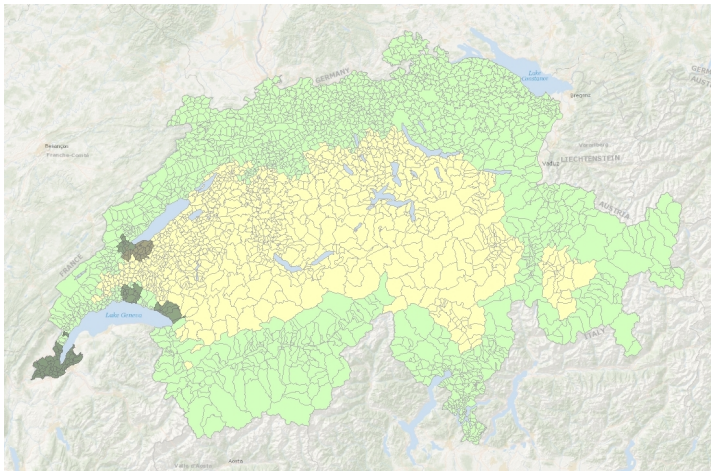


*Figure 1: Areas of Switzerland allowed (green) or not (yellow) to hire cross-border workers.*

One should note that the priority to local workers not only concerned cross-border workers but also migrants looking to relocate to Switzerland from the EU. Until June 2007, quotas on the number of working permit were in place[3]. Residency permit reached the maximum number allowed, before and after June 2004, making the abolition of the priority irrelevant. However, short-term permit (of less than 12 months) never made the quota and might be a concern when evaluating the repeal of the priority to the resident population. We implement *difference-in-differences* which has the benefit of excluding nationwide policies from the estimated effect, provided that these interact in the same way throughout the country. Under this assumption, the effect of the abolition of the priority granted to resident workers over migrants looking to relocated is entirely excluded from our analysis and the final estimate only capture the effect due to cross-border workers.

To formalise, the *treatment* evaluated in this paper is the abolition of the priority granted to the resident population over cross-border workers. The treatment only affects unemployed persons living in BR, making them the *treated*. Finally, unemployed persons in CR are not

the EU.

[3]Because we focus our analysis on data prior to this date, the removal of this quotas does not affect the estimated effected of the abolition of the priority.

affected by the policy and can thus be used as the *control group*. The variable identifying the treatment for individual $i$ is:

$$D_i = \begin{cases} 1 & \text{if } i \text{ lives in BR after the change in policy} \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

The identification of the control group is based on the assumption that unemployed persons living in CR are not affected by the presence of cross-border workers. This assumption perhaps requires some justification, especially around the internal border between CR and BR. An unemployed person living in either BR or CR might be looking for jobs in the other region. As a consequence, the policy studied here might affect the competition for jobs and ultimately, unemployment duration even in CR reducing the observed effects of the treatment under study. Some remarks can be done in order to justify this assumption. Firstly, cross-border workers are more likely to be in municipalities close to the national border; the further away the lower the incentive to commute is, reducing the consequence of the liberalisation in the areas around the internal border. Secondly, there are important geographical barriers (e.g. mountains and lakes) and cultural obstacles (e.g. regional languages), especially in the southern regions[4]. In order to reduce these concerns, we tested the evaluation process excluding municipalities around the internal border between CR and BR. Additionally, a simple regression discontinuity (RD) design using the distance between CR and BR has been implemented.

Another concern for DID is the presence of other policies that affect treated and control groups differently. The reform of Swiss national unemployment insurance (July 2003) might be a confounding policy. The main change of the reform was the reduction of the number of maximum daily allowances from 520 to 400 (i.e. from 24 to 18 consecutive months). This policy was implemented at the national level one year before the abolition of the priority to local workers and as a consequence any long term effect (which would overlap with June 2004) might be captured in our analysis. As mentioned above, DID exclude nationwide policies from the estimated effect as long as these interact in the same way throughout the country. Comparing unemployed persons that have similar characteristics and face similar labour market conditions reduces any difference in reactions between the two regions that might not be captured by DID. In the next section we introduce *Inverse Probability Weighting* (IPW) which, paired with DID, allow us to reduce the concerns just risen by improving the balance of characteristics between treated and control groups.

A last point to be made, is that some regions characterised by high unemployment rate, were entitled to 520 daily allowances, rather than 400. The estimated effect of the policy, in these regions, will capture the effects of both the abolition of the priority and the extra allowances, making the interpretation of the results biased by this policy. Lalive et al. (2013) found that similar policy changes in Austria presented some external effects in neighbouring regions. Furthermore, because the regions that are affected by this policy (darkened in figure 1, i.e. the canton of Geneva, and the regions of Lausanne, Vevey and Yverdon[5]) are all in the French speaking area of Switzerland the results shown in these paper concerns only the German and

[4] For example the canton of Valais, in the south of the country bordering with France and Italy, is completely included in BR and on its north end there are mountains that make the commute to the neighbouring CR very difficult.

[5] Other regions, as before all in the French speaking areas, were also entitled to extra allowances but never in periods influencing our analysis.

Italian speaking areas[6], which represent more than 70% of the studied population. A second motivation to support the choice of excluding entirely the French speaking area is that cultural differences can explain differences in unemployment duration (Brügger et al., 2009).

# 3 Methodological framework

## 3.1 Data and Population

The State Secretariat for Economic Affairs (SECO) is responsible for the management of statistics related to unemployment. Two registers, *PLASTA* and *SIPAC*, keep track of all the job-seekers registered in a regional office for employment by collecting information on professional and personal aspects as well as on all benefits claimed.

We identify both treated and control groups as the population residing[7] in the German and Italian speaking areas, that start a new spell of registered unemployment[8]. Individuals are considered registered unemployed if they are actively looking for a job, are available to start a new job immediately and register with a regional employment office[9]. As anticipated above we are interested in unemployment duration, which we measure as the number of consecutive days as unemployed in the 24 months following registration with a regional employment office.

To implement the difference-in-differences methodology we need observations before and after the implementation of the treatment (i.e. June 2004). The post-treatment group is composed of those that registered with an employment office between June 2004 and May 2005. The time frame as been chosen to avoid overlaps with the studied change in policy (June 2004) and the abolition of the regional limitations (June 2007). On the other hand the pre-treatment group is composed of individuals starting the new spell of unemployment during 2001[10]. Figure 2 represents the time frame for the analysis showing the maximum spells analysed in the pre- and post- treatment periods. Finally, individuals are either in the treated or control groups depending on their residency, respectively in BR or CR.

Due to their precariousness, individuals aged 55 years old or more, receive 120 extra daily allowances and should be analysed separately. Following the reasoning for the regions entitled to the extra allowances they have been excluded from the main analysis.

In appendix A we present the list of variables used in the analysis. These variables should capture both the difficulties in accessing the labour market whilst in unemployment (i.e. unemployment duration) and the differences between CR and BR across the analysed period.

[6] The results for the total population, excluding only the regions directly affected by extra allowances, have been computed and are available with the author

[7] This includes Swiss, and foreigners with a residency permit (type B and C).

[8] Individuals that enter unemployment for a reasons different than losing their jobs (the alternative reasons being: invalidity claims, divorce and education) have different entitlements because of their special circumstances. Precisely because of these circumstances and entitlements, they have been excluded from the analysis.

[9] Individuals without a job that decide not to register with an unemployment office are not observed in the dataset and other public statistics, however they represent a minority. The majority of individuals losing their job in Switzerland are entitled to unemployment allowances and in order to claim them or to benefits from support by the government, one need to register with a regional employment office.

[10] Data for the year 2000 are not used due to poor quality and the lack of information on the education level of unemployed persons. The choice of 2001 is therefore imposed by the data itself.

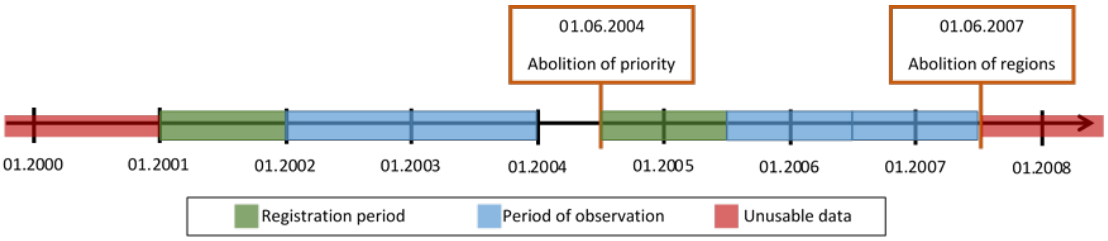


*Figure 2: Time frame for the difference-in-differences evaluation*

Variables are divided into *personal* and *regional characteristics* explaining the difference in the local labour market and the external factors affecting unemployment duration and finally *personal employment history* explaining the personal performance in the labour market.

### 3.2 The evaluation process

The aim of policy evaluation is to attribute, in a *causal* sense, part of the observed change in the recipients' conditions to the change in policy. To estimate the true effect of the policy the evaluation process needs to be implemented in a *counterfactual* framework, which defines the *treatment effect* as the difference between what is observed while subject to the treatment and what would have been observed in its absence. This paper applies the well-established *difference-in-differences* (DID) methodology (see Lechner (2010) for an exhaustive coverage of DID). One of the advantages of using this methodology is that by including CR as a control in the DID model, any bias caused by variables common to BR and CR is implicitly controlled for, even when these variables are unobserved.

The simple idea behind DID is that in the absence of the policy, treated and untreated are subject to the same time trends, so the effect of the change in policy can be estimated as the difference between the two observed time trends. Unless the policy has been implemented in a randomized controlled experiment, the common trend assumption is hard to support and applying the *unconfoundedness* assumption is necessary in order to ensure meaningful results. In the DID case, this assumption requires that, conditional to a set of covariates $X$, unemployment duration in CR follows the same trend that BR would have followed in the absence of the policy change. If *unconfoundedness* is respected then the difference between the two observed trends is the sole effect of the abolition of the priority to local workers.

We are interested in the *Average Treatment Effect on the Treated (ATET)*, which is given by:

$$\tau = \{E_X[Y_1|D=1] - E_X[Y_0|D=1]\} - \{E_X[Y_1|D=0] - E_X[Y_0|D=0]\} \tag{2}$$

where $E_X$ is the expectation conditional to X, $Y_t$ is the observed outcome at time $T = t$ ($T \sim t \in 0, 1$) and D is the treatment variable introduced in equation (1). This identification is well known and its proof is available in the literature (see Lechner (2010) for a recent formulation).

When using repeated cross-sectional data, an intuitive way to evaluate the $ATET$ is derived

from equation (2) by adding and subtracting $E_X[Y_1|D=1]$, obtaining:

$$\begin{aligned}\tau = & \{E_X[Y_1|D=1] - E_X[Y_0|D=1]\} + \{E_X[Y_1|D=1] - E_X[Y_1|D=0]\} \\ & - \{E_X[Y_1|D=1] - E_X[Y_0|D=0]\} \\ = & \tau_1 + \tau_2 - \tau_3\end{aligned} \tag{3}$$

where each $\tau_k$ can be estimated separately by applying any of the methodologies for the cross-sectional case. Intuitively this is the same as adjusting for differences in observed variables, removing biases from comparisons between the treated and the three control groups. In $\tau_1$ the treated are compared to the same group in the pre-treatment period, in our case unemployed in the border region prior to the treatment (*BR-pre*). In $\tau_2$ they are compared to the non-treated in the post-treatment period (*CR-post*) and in $\tau_3$ to the non-treated in the pre-treatment period (*CR-pre*). This approach has been commonly applied in matching procedures (Blundell and Costa Dias (2000), page 451), where each group is matched to the treated in the post-treatment period and the ATET is obtained as simple DID of averages on the matched groups.

Any cross-sectional procedure can be implemented in the estimation of the single components of $\tau$. We decided to focus on methods based on the probability of being treated, i.e. the *propensity score* (Rosenbaum and Rubin, 1983), specifically we implement *Inverse Probability Weighting* (IPW). Unlike matching, weighting has the advantage of including all the data (unless weights are set to zero) and fewer distributional assumptions about the underlying data are required. Another advantage of IPW is that once the scores forming the weights are estimated no additional functional form assumption is needed. Each of the three components, $\tau_k$ is estimated through IPW, and then aggregated according to equation (3) to obtain $\hat{\tau}_{IPW}$,

$$\hat{\tau}_{IPW} = \hat{\tau}_{1,IPW} + \hat{\tau}_{2,IPW} - \hat{\tau}_{3,IPW} \tag{4}$$

We implement a simple weighting scheme, exploiting propensity score, which is fundamentally due to Horvitz and Thompson (1952), where the $\tau_{k,IPW}$ is defined as:

$$\tau_{k,IPW} = E\left[D \cdot Y_1 - \frac{(1-D)\cdot Y_t}{1-p(X)}\right]$$

where the appropriate population has been selected for the relative $k$, if $k=1$ the treated in $t=1$ are compared to the treated[11] in $t=0$, if $k=2$ the treated in $t=1$ are compared to the control group in $t=1$ and if $k=3$ the treated in $t=1$ are compared to the control group in $t=0$.

Hirano et al. (2003) establish conditions under which replacing $p(X)$ with a non-parametric estimator, rather than the rarely available true propensity score, leads to an efficient estimate of the average treatment effect. Non-parametric estimations require a high number of data, especially when the number of covariates is high, this is known as the *curse of dimensionality*. Using a parametric approach allows us to avoid this issue at the cost of the asymptotic result. The results on consistency, unlike the asymptotic efficiency, obtained by Hirano et al. (2003) remain valid even when $p(x)$ is estimated through a logistic regression and the model

[11] Often the term treated is used inappropriately for this group even if they are not subject to the treatment iet.

is correctly specified. Once the estimated probability of being treated, $\hat{p}(X)$, is obtained the average treatment effect on the treated can be easily calculated as:

$$\hat{\tau}_{k,IPW} = \frac{1}{N} \cdot \sum_{i=1}^{N_k} \left( D_i \cdot Y_i - \frac{(1 - D_i) \cdot Y_i}{1 - \hat{p}_i(X)} \right)$$

and then introduced in equation (4) to obtain $\hat{\tau}_{IPW}$.

A particular concern in IPW is that particularly unbalanced covariates between treated and controls can produce estimates of the propensity score close to zero or one. This will generate weights that can be large, in the case of the ATET if $\hat{p}(X)$ is close to one then the associated weight will be high, giving too much importance to the associated observation. Trimming of high propensity scores (Lee et al., 2011) is implemented to reduce overweighting of extreme cases resulting in weights defined as:

$$W_i = \begin{cases} 1 & \text{if } i \text{ is a treated} \\ \frac{1}{1-\hat{p}_i(X)} & \text{if } i \text{ is a control and } \hat{p}(X) <= \hat{p}_{90\%} \\ \frac{1}{1-\hat{p}_{90\%}} & \text{if } i \text{ is a control and } \hat{p}(X) > \hat{p}_{90\%} \end{cases} \tag{5}$$

where $\hat{p}_{90\%}$ is the $90^{th}$ percentile of $\hat{p}$.

Finally, standard errors for $\hat{\tau}_{IPW}$ are necessary for drawing inference. Analytical forms of the distribution for the ATET obtained through IPW are available and could be used to obtain standard errors for the three $\hat{\tau}_{k,IPW}$. However standard errors for the aggregated ATET, $\hat{\tau}_{IPW}$,cannot be directly obtained because the three component are correlated, instead bootstrapping is implemented. The procedure to estimate $\tau_{IPW}$, is repeated 999 times[12] on populations individually re-sampled for the four groups (i.e. pre and post treatment for both BR and CR) and the standard errors are drawn from the sample distribution of the 999 estimated $\hat{\tau}_{IPW}$. Note that the possible issues of serial correlation of the outputs raised by Bertrand et al. (2004) are not a concern in this paper because we use only two periods of time.

# 4 Robustness of the evaluation framework

The control and treated groups are not defined in a strict *laboratory* experiment, hence making sure that the two are comparable is key for the interpretation of the results. As presented in the previous section, covariates presented in appendix A are regressed, using a logistic model, separately for the three subgroups ($k$), these results are shown in appendix B and used to estimate the propensity scores. The weights are than computed according to equation (5), and are used to obtain a *reweighted* population that is balanced in the covariates.

IPW conditions on propensity score and not on all covariates, in order to ensure the comparability between control and treated groups we introduce the *standardised bias* (Rosenbaum and Rubin, 1985). This statistic is used in several evaluation analyses (e.g. Lechner (1999); Sianesi (2004); Caliendo et al. (2005) and Losa et al. (2012)) and measures the distance between the

[12]Considering the size of the population studied here and the speed of convergence observed for the standard errors, 999 repetitions are considered satisfying for the estimation of the standard error. An odd number of repetition is preferred (see Davidson and MacKinnon (2000))

*Table 1: Balancing covariates with IPW*

| | No weights | | | IPW | | |
|---|---|---|---|---|---|---|
| | k=1 | k=2 | k=3 | k=1 | k=2 | k=3 |
| Woman | 2.6 | 2.0 | 1.0 | 0.6 | 0.2 | 0.2 |
| Age | 10.3 | 3.7 | 12.3 | 3.8 | 1.1 | 5.4 |
| Marital status | | | | | | |
| *Single* | 16.3 | 9.8 | 10.1 | 3.2 | 0.1 | 4.1 |
| *Married* | 13.6 | 7.7 | 8.0 | 2.4 | 0.0 | 3.2 |
| Local mother language | 12.6 | 9.5 | 14.0 | 2.0 | 0.2 | 3.1 |
| Supported persons | 16.4 | 5.9 | 12.1 | 3.0 | 0.1 | 4.9 |
| Nationality | | | | | | |
| *Swiss* | 11.6 | 15.1 | 2.7 | 1.4 | 0.7 | 0.1 |
| *EU15 /AELS* | 1.5 | 7.7 | 8.3 | 0.2 | 0.6 | 1.9 |
| Permit C | 6.9 | 14.4 | 6.4 | 1.7 | 1.0 | 0.1 |
| Education | | | | | | |
| *Secondary* | 18.1 | 3.5 | 19.3 | 1.9 | 1.0 | 3.7 |
| *Tertiary* | 5.0 | 8.8 | 9.8 | 0.6 | 0.8 | 0.5 |
| *Vocational* | 22.4 | 0.2 | 24.1 | 1.8 | 1.3 | 3.9 |
| Job pre FT | 6.3 | 1.2 | 5.3 | 0.0 | 0.7 | 0.5 |
| Manager | 2.9 | 2.4 | 3.7 | 0.8 | 1.6 | 1.6 |
| Insured salary | 0.9 | 4.5 | 4.0 | 1.1 | 1.2 | 2.2 |
| Economic sector | | | | | | |
| *Agricolture* | 0.5 | 1.5 | 1.0 | 0.1 | 0.2 | 0.2 |
| *Construction* | 3.8 | 3.3 | 2.3 | 0.5 | 1.5 | 2.6 |
| *Retail, etc.* | 1.0 | 5.6 | 8.5 | 0.2 | 1.9 | 4.1 |
| *IT and com.* | 4.6 | 0.6 | 0.2 | 0.2 | 0.3 | 0.4 |
| *Financial serv.* | 2.3 | 4.0 | 5.6 | 0.1 | 0.1 | 0.7 |
| *Admin. serv.* | 5.5 | 3.5 | 2.1 | 0.4 | 1.1 | 0.9 |
| *Public serv.* | 5.9 | 3.1 | 0.1 | 0.0 | 0.6 | 0.6 |
| *Other serv.* | 2.4 | 3.7 | 1.6 | 0.2 | 1.3 | 1.5 |
| *Missing* | 4.8 | 1.7 | 13.9 | 1.1 | 1.4 | 3.0 |
| U personal | 24.6 | 1.6 | 24.6 | 4.7 | 0.4 | 7.3 |
| U regional | 11.3 | 39.3 | 35.3 | 1.6 | 18.7 | 23.7 |
| U evolution | 0.1 | 16.7 | 13.1 | 0.5 | 16.5 | 15.9 |
| Rural | 5.5 | 24.7 | 17.4 | 0.8 | 5.2 | 6.4 |
| Quarter | | | | | | |
| *1st* | 7.2 | 0.3 | 6.8 | 1.2 | 0.5 | 2.5 |
| *2nd* | 6.4 | 1.2 | 7.2 | 0.4 | 0.3 | 0.0 |
| *3rd* | 11.1 | 1.6 | 12.7 | 1.2 | 0.3 | 1.1 |
| Distance BR/CR | 0.9 | 12.8 | 14.4 | 0.3 | 4.1 | 3.1 |
| Average | 7.7 | 6.9 | 9.6 | 1.2 | 2.0 | 3.4 |
| Pseudo R2 | 0.041 | 0.074 | 0.093 | 0.001 | 0.011 | 0.017 |

note 1: k=1 - BR-pre, k=2 - CR-Post and K=3 - CR-Pre

note 2: For a full description of the covariates see appendix A.

marginal distributions, of treated and control, for each variable included in the logistic regression. The smaller the bias the better balanced the covariates are between the two groups. In

the empirical literature, average values between 3 and 5 are considered sufficient to ensure a good quality in balancing the covariates.

In table 1 we present the balancing scores of the covariates used in the analysis before (i.e. the first three columns) and after weighting (i.e. the last three columns). In terms of single variables we can see most of the variables improved their balance after applying IPW, the few that deteriorate did so in order to improve the overall balance of the population. In average bias drops from values over 6.9 when controls are excluded and no weighting scheme is implemented, to a values below 3.4. When comparing the treated in the post-treatment period with the control in the pre-treatment period (k=3) we obtain the worse balancing, intuitively because both the time and region are different from the treatment group.

Running the logistic regression on the weighted population, as suggested by Sianesi (2004), allow to see that when weighting, the logit model struggles to explain the difference between treated and control. In our analysis the pseudo R2 drop significantly (i.e. from an average of 0.069 prior to IPW to 0.010 for the weighted population), meaning that IPW make it impossible to explain the differences between the two regions using the considered variables.

Generally results in term of balance between treated and controls are satisfactory and we consider that the BR and CR are comparable.

*Verifying the robustness of the model*

Alternatives approaches were tested to verify that the results obtained are robust and do not strongly depend on the chosen model. A first issue, mentioned above, is that extreme values of $\hat{p}(x)$ can lead to extreme weights. Traditionally, trimming is implemented to overcome this problem, however to verify this choice two alternative specifications of the weights, as well as excluding theme, have been tested. Re-scaling, as suggested in Pohlmeier et al. (2012) and dropping of all individuals with a propensity to score higher then 0.9 (Crump et al. (2009)) have also been tested.

Another concern is that individuals living in CR, just across the border from BR, might be influenced by the presence of cross-border workers in the neighbouring area, which violates the assumption of independence of the outcome variable in the control group to the treatment. Municipalities along the CR/BR border have been excluded to verify the possible bias caused by unemployed persons looking for jobs in the other region.

Finally the model was implemented excluding each variable individually to verify their influence. It is important to remark that, although many variables have been included in the final model or tested in the process, some others might have been neglected or are simply not observable, an unavoidable burden common to all methods based on the identification assumption. The alternative approaches (outlined above) were introduced to verify the robustness of the results notwithstanding this shortcoming. Alternative methods, not based on the identification assumption, are based on other choices, for example non-parametric estimation requires the choice of parameters, and are subject to other critiques.

Table 6 in appendix C shows results for these robustness controls. Generally, only minor differences are observed compared to the model introduced in the previous section (first line of the table). The test of alternative weighting schemes shows minor differences. Considering the potential changes that could result using different weights or excluding a part of the population, these test are considered satisfactory.

When excluding municipalities on the internal border between BR and CR, a minor increase in the magnitude, but not the direction, of the ATET is observed. The same is observed when excluding the municipalities further away from the internal border (D>70km). We decided to include all the municipalities in the analysis for this paper to increase the size of the population and have a more representative sample. As mentioned in the previous section we also implemented a simple RD design using the distance between the two regions. In figure 3 (appendix D) we show results from a simple regression discontinuity design. This show a clear jump in the average unemployment duration near the internal border, results that support our findings. However the benefits of DID introduced above, such as the ability to exclude nation-wide policies, make RD vulnerable to critics and those results will not be discussed further.

In the last part of the table, each group of variables is excluded separately resulting in very similar ATET. Alternative specifications of the model and definitions for several variables were also tested obtaining similar results.

*Verifying the common trend assumption*

One last important concern that needs to be tackled is the common trend assumption. Running the DID model prior to the abolition of the priority should results in ATET that are statistically not different from zero, bringing evidences to support the common trend assumption. Because of the problems with data prior to 2001 and the fact that we study the 24 months following registration with a regional employment office we need to run the pre-policy test on sub-population of the pre-treatment group. We consider individuals registering in the first six months of 2001 against those registering in the second half, the second are considered treated of a *placebo* policy.

*Table 2: Average Treatment Effect on the Treated*

| | Pre | Post | $\Delta_{Pre-Post}$ |
|---|---|---|---|
| CR | 117.5 | 146.2 | 28.7 |
| BR | 127.0 | 157.6 | 30.6 |
| $\Delta_{BR-CR}$ | 9.4 | 11.4 | 2.0 |
| | | | (3.229) |

Note: Bootstrapped standard error of the ATET in brackets.

The results from the *placebo* evaluation are presented in table 2. Both BR and CR suffer a rise in the average permanence in unemployment; however, the DID analysis results in a rise of 1.5 days in unemployment. Furthermore, the bootstrapped standard errors (in brackets) show that the evolutions in the two regions are statistically indistinguishable. In conclusion, prior to the abolition of the priority and when controlling for a set of variables, the results in table 2 support the common trend assumption.

The same test has been done for every sub-population analysed and is presented in table 4, this are all statically not different from zero.

# 5 The effects of the policy

Now that we have identified the model, verified the benefits of using IPW and the robustness of the final model, we can proceed with the analysis of the results in terms of the effects of the policy on the treated population.

The weighted average of unemployment durations, for the treated and control groups, using the weights from equation (5) are reported in table 3. Unemployed persons living in the border regions suffer an increase in unemployment duration of 44.7 days, this is caused on one side by the normal trend of the local economy and on the other by the abolition of the priority granted to local workers. During the same period of time the control group (i.e. CR) increased their permanence in unemployment by 40.1 days. Under the common trend assumption, CR mimics the evolution that would have occurred in BR in the absence of the change in policy. Formally, the difference of 4.6 days is the sole consequence of the abolition of priority granted to the local workers. Firms can freely choose any worker, whether resident in Switzerland or not, which causes a delayed access to the labour market whilst in unemployment. Jobs might be allocated to a cross-border worker who is either better suited for the job or ready to accept a lower wage.

*Table 3: Average Treatment Effect on the Treated*

| | Pre | Post | $\Delta_{Pre-Post}$ |
|---|---|---|---|
| CR | 130.4 | 170.5 | 40.1 |
| BR | 139.9 | 184.6 | 44.7 |
| $\Delta_{BR-CR}$ | 9.5 | 14.1 | 4.6** |
| | | | (2.170) |

Note: Bootstrapped standard error of the ATET in brackets.

The standard errors obtained through re-sampling methods (i.e. bootstrap) are reported in brackets in table 3. This is used to compute the significance of the ATET, which in this case is significant at the 95% level and indicated by **.

Policy makers trying to reduce the cost of social security should consider that, although, an increase in unemployment of less than 5 days might not seem much from a individual perspective, we should consider that in relative terms, this correspond to an increase of 3.3% of the permanence in unemployment[13]. Furthermore, this translate into a similar relative increase of the costs of unemployment.

*Heterogeneity of the treatment effect*

Some categories of population might react differently, depending on their characteristics and skills, compared to the new competition on the labour market. In table 4, results for some selected sub-population are presented[14]. These results are obtained by reproducing the procedure, from the estimation of $p(X)$ to the computation of the ATET, independently for each

[13] The percentage ATET is computed with reference with the starting point of the treated BR in the pre-treatment period, in this case 4.6 compared to the starting point of 139.9.

[14] Results, including including the French speaking regions, not directly affected by the extra daily allowances (darkened in figure 1), are very similar to those presented in table 4 for all the subcategories (e.g. the ATET over the all population is +3.8 instead of +4.6). Including them might generate a bias (Lalive et al., 2013) from the extra unemployment allowances given to some regions.

group. The first column also shows the starting point of the outcome variable, i.e. unemployment duration in 2001 in BR, which is used for the computation of the relative average treatment effect on the treated (ATET(%)) and provide a better comparison between groups.

*Table 4: Average Treatment Effect on the Treated (ATET) for selected demographic groups*

| | | | Placebo | Final model | | |
|---|---|---|---|---|---|---|
| | Y(BR01) | Std.Bias | ATET | ATET | | ATET(%) |
| Tot | 139.9 | 2.3 | 2.0 | 4.6 | ** | 3.3 |
| | | | (3.229) | (2.170) | | |
| Gender | | | | | | |
| *Men* | 137.5 | 2.4 | 3.8 | 3.2 | | 2.3 |
| | | | (4.352) | (3.012) | | |
| *Women* | 141.0 | 2.4 | 0.9 | 7.1 | ** | 5.0 |
| | | | (4.883) | (3.048) | | |
| Nationality | | | | | | |
| *Swiss* | 128.1 | 2.5 | 3.1 | 1.5 | | 1.1 |
| | | | (3.911) | (2.443) | | |
| *EU15* | 142.1 | 2.6 | -5.4 | 13.5 | * | 9.5 |
| | | | (9.994) | (6.954) | | |
| *Others* | 169.9 | 2.3 | 3.4 | 8.2 | | 4.8 |
| | | | (7.332) | (5.072) | | |
| Local Language | | | | | | |
| *Yes* | 125.9 | 2.4 | 2.7 | 2.2 | | 1.8 |
| | | | (4.113) | (2.684) | | |
| *No* | 160.6 | 2.4 | 1.8 | 9.6 | *** | 6.0 |
| | | | (5.905) | (3.653) | | |
| Industry sector | | | | | | |
| *Secondary* | 133.8 | 3.0 | 7.1 | 2.7 | | 2.0 |
| | | | (6.822) | (4.606) | | |
| *Tertiary* | 140.0 | 2.4 | 2.6 | 6.9 | *** | 4.9 |
| | | | (4.061) | (2.551) | | |

note 1: Standard error are presented in brackets

note 2: *,**,*** respectively represent significant level of 90%, 95% and 99%.

First of all, it is important to note that all the placebo test are statistically not-different from zero, this mean that the comparison of treated and controls is meaningful and the common trend assumption is satisfied.

We can see that when studying the effect by gender only women have a significant effect. Despite facing an already higher unemployment duration prior to the change in policy (i.e. the first column in table 4, labelled Y(BR01)), women see their time spent in unemployment increase even further. In average they spend 7.1 days in unemployment more than they would have done if the priority to the residents was still in place. Women are traditionally considered more vulnerable in the labour market and see their opportunities to access employment further reduced due to the increased pressure imposed by cross-border workers.

When considering nationality, Swiss persons do not seem to be affected by the pressure of cross-border migrants. They suffer an increase of 1.1% in unemployment duration, which is statistically not different from zero. Individuals from one of the signatory countries of the studied policy (EU15/EFTA) suffer the most: as a consequence of the abolition of the policy they spend 13.5 extra days in unemployment, this corresponds to a relative rise of 9.5%. Finally, those from other countries see their unemployment duration rise by 4.8%, however due to their high variability this results is not statistically different from zero. Individuals already living in Switzerland and with nationality from signatory countries are bound to be the most similar to the new cross-border migrants and consequentially, to be in direct competition for the same jobs.

Cross-Border migrants reside in the neighbouring region and, for the majority, their mother tongue is the same language spoken in the area of Switzerland where they work. They might be seen as better match for a job than individuals living in Switzerland and with a mother tongue that is not the local one. This is going to negatively affect unemployment duration (Pissarides, 2000) and can explain the results in table 4. Individuals not speaking the local language see their unemployment duration rise by 4.2 percentage points more than those that speak the local language. Their permanence in unemployment increased by almost 9.6 days compared to 2.2 (which is also statistically not different from zero).

The effect by the economic sector, of the firms employing the individual before the studied unemployment spell, is of no change for individuals in the secondary sector and a rise of 4.9% for those coming from the tertiary sector. Cross-Border migrant traditionally supply the manufacturing and construction sector[15], filling the need of industries. Hence, proving that there was a need for a new migrant might have been easy enough. The abolition of the priority might be not have an impact in this sector because it was already not relevant prior to the policy.

# 6 Conclusion

This paper sets out to evaluate the effect of the abolition of the priority granted to residents of Switzerland over cross-border workers, in terms of unemployment duration. The change in policy is part of the bilateral agreements between Switzerland and the EU, the impact of which have given rise to national concern.

The abolition of the priority granted to local workers results in an increase of available labour supply with a lower reservation wage. The liberalization of migration policies increases the time spent in unemployment by altering the two determinants of unemployment duration, i.e. the number of vacancies and the probability of being the best match for a given job (Nickell, 1980; Pissarides, 2000). Our results, also show that women and foreigners (especially those that do not have the local language as their mother tongue) have stronger effects than the average ATET. These individuals traditionally considered more vulnerable (i.e. with more difficulties in accessing the labour market), compete with cross-border workers for similar jobs and spend more time than others in unemployment following the change in policy.

[15] In 2003, cross border workers represented 8% of the secondary sector and only 3% of the tertiary sector. In the same year, 45% of cross border workers were in the secondary sector compared to 23% of the resident population.

The introduction of quotas and priority to Swiss nationals, approved in the national referendum "stop mass immigration" in 2014, might reduce the consequences on unemployment duration, however firms will have less choice in the labour market and might consider moving abroad, decreasing the number of jobs on offer and resulting in a negative effect on unemployment duration. In addition if the abolition of the priority brings positive effects on other components of the labour market, such as wages and employment (Losa et al., 2012), this might be reduced or even reverted by the results of the referendum. In order to reduce the effect on unemployment duration for this vulnerable population and the rising costs for social insurances[16], a better understating of the needs of local firms and training schemes aimed at increasing the attractiveness of local unemployed persons could lead to a better competitiveness of local workers over cross-border migrants on the labour market.

In conclusion, the abolition of the priority granted to local workers resulting in an increase of migrant job-seekers with a lower reservation wage, diminishes the likelihood of finding a job and ultimately increases unemployment duration.

[16]The increase of unemployment duration of 3.3% observed on the overall population translates in a direct increase in the costs for unemployment insurance as well as a indirect rise due to the consequences of longer spells of unemployment.

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# A Variables used in the analysis:

A number of covariates have been used to balance the groups in the two regions and periods. The variables are divided into three categories: *Personal* characteristics; *Regional* characteristics capturing the local labour market status and *Personal employment history*. The selected variables should capture the factors explaining the difficulties in accessing the labour market (i.e. the outcome variable) as well as those explaining differences across regions and time (i.e. the treatment variable).

*Personal traits*

**Gender:** Dummy variable that equals 1 if the individual is a woman 0 otherwise.

**Age:** Age in years of the individual. A categorisation of age as been tested providing little change in the results.

**Marital status:** Set of dummy variables for the categories: Divorced or Widowed (*ref.*[17]); Single and Married.

**Local language:** Dummy variable that equals 1 if the mother tongue of the concerned individual is the same as the local languages, 0 otherwise.

**Supported persons:** Number of persons that the individuals support in his household.

**Nationality:** A set dummy variables for residents with Swiss nationality, EU15/EFTA nationality (EU15: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden and United Kingdom and EFTA: Iceland, Norway and Principality of Liechtenstein) and one for the others nationalities *ref.*. An ecxtra dummy variable for having a long term residency permit (permit C) was also included.

**Educational level:** The maximum level of education has been included as a set of dummy variables for the categories: compulsory/missing (*ref.*); Secondary and those with a Tertiary degree.

**Vocational Education:** Dummy variable that equals 1 if the individual has a vocational training and 0 otherwise.

*Regional variables*

**Rurality:** Dummy variable that is 1 if the individual lives in a rural area, 0 otherwise.

**District unemployment evolution:** Relative evolution of the number of unemployed persons between 1999 and 2000 by district. Alternative periods and definitions have been considered, including definition that incorporate regional unemployment in the neighbouring countries.

**District unemployment:** Unemployment rate by district in 2001

**Distance between BR and CR:** Driving distance in Km between the municipality of residence and the closest municipality in the other region (i.e. CR or BR)

[17] Ref. in brackets indicate the category used as a reference in the estimation of the propensity score

*Personal employment history*

**Full Time in previous job:** Dummy variable that equals 1 if the individual worked full time in the previous job 0 otherwise.

**Previously Manager of Self-employed:** Dummy variable that equals 1 if the individual was a manager or a self employed in the previous occupation 0 otherwise.

**Previous economic sector:** Set of dummy variables representing the economic sector of the firm of the last job. The aggregation chosen[18] here is as follow: (1) Agriculture and Forestry; (2) Industry (*ref.*); (3) Construction; (4) Retail, Transport and Accommodation; (5) IT and Communication; (6) Financial and real estate services; (7) Admin and specialized activities; (8) Public, Education and Social activities; (9) Other Services and (10) Missing. Alternatives categorisation have been tested and provide little variations to the final result.

**Number of days in U:** Number of days in the year previous to the registration in unemployment that each individual has spent in unemployment. A dummy that equals 1 if the individual has previously been unemployed and 0 otherwise has been tested.

**Insured salary:** Amount of salary earned before unemployment and used to compute the daily allowances.

*Others*

**Period of registration:** Set of dummy variables for the quarter in which each individual registered in unemployment ($4^{th}$ quarter as reference).

[18] The numbers in brackets refer to the numbers used in the various tables to save space.

# B Results from logistic regressions:

*Table 5: Results from logit model*

| | k=1 | | | k=2 | | | k=3 | | |
|---|---|---|---|---|---|---|---|---|---|
| Woman | -0.030 | * | (0.016) | 0.010 | | (0.018) | -0.015 | | (0.023) |
| Age | -0.004 | *** | (0.001) | -0.002 | | (0.001) | -0.011 | *** | (0.001) |
| Marital status | | | | | | | | | |
| *Single* | 0.149 | *** | (0.028) | -0.326 | *** | (0.035) | -0.125 | *** | (0.041) |
| *Married* | 0.101 | *** | (0.027) | -0.052 | | (0.034) | 0.066 | * | (0.039) |
| Local language | -0.062 | *** | (0.021) | 0.664 | *** | (0.022) | 0.357 | *** | (0.028) |
| Supported persons | -0.066 | *** | (0.008) | -0.006 | | (0.010) | -0.071 | *** | (0.011) |
| Nationality | | | | | | | | | |
| *Swiss* | 0.044 | | (0.029) | -0.577 | *** | (0.034) | -0.379 | *** | (0.040) |
| *EU15 /AELS* | 0.089 | *** | (0.025) | -0.213 | *** | (0.031) | 0.007 | | (0.038) |
| Permit C | 0.004 | | (0.024) | 0.211 | *** | (0.031) | 0.193 | *** | (0.036) |
| Education | | | | | | | | | |
| *Secondary* | 0.203 | *** | (0.024) | 0.090 | *** | (0.029) | 0.359 | *** | (0.035) |
| *Tertiary* | 0.330 | *** | (0.032) | 0.410 | *** | (0.038) | 0.590 | *** | (0.047) |
| *Vocational* | 0.240 | *** | (0.022) | 0.035 | | (0.027) | 0.262 | *** | (0.033) |
| Insured Salary | 0.000 | ** | (0.000) | 0.000 | *** | (0.000) | 0.000 | ** | (0.000) |
| Job pre FT | -0.224 | *** | (0.018) | 0.093 | *** | (0.021) | -0.179 | *** | (0.026) |
| Manager | -0.204 | *** | (0.030) | -0.387 | *** | (0.035) | -0.450 | *** | (0.042) |
| Economic sector | | | | | | | | | |
| *Agricolture* | 0.079 | | (0.157) | -0.096 | | (0.175) | 0.135 | | (0.223) |
| *Construction* | 0.316 | *** | (0.032) | 0.032 | | (0.038) | 0.130 | *** | (0.046) |
| *Retail, etc.* | 0.149 | *** | (0.022) | -0.127 | *** | (0.026) | -0.036 | | (0.031) |
| *IT and com.* | -0.241 | *** | (0.048) | -0.058 | | (0.061) | -0.064 | | (0.074) |
| *Financial serv.* | 0.161 | *** | (0.039) | 0.101 | ** | (0.048) | 0.235 | *** | (0.061) |
| *Admin. serv.* | 0.021 | | (0.027) | 0.001 | | (0.033) | 0.097 | ** | (0.039) |
| *Public serv.* | 0.196 | *** | (0.029) | -0.156 | *** | (0.033) | -0.025 | | (0.040) |
| *Other serv.* | 0.136 | *** | (0.038) | 0.126 | *** | (0.047) | -0.060 | | (0.053) |
| *Missing* | 0.440 | *** | (0.030) | -0.126 | *** | (0.034) | 0.629 | *** | (0.047) |
| Rural | 0.142 | *** | (0.020) | -0.724 | *** | (0.020) | -0.624 | *** | (0.024) |
| U regional | -26.681 | *** | (1.554) | -35.417 | *** | (0.756) | -38.839 | *** | (0.993) |
| U personal | -0.010 | *** | (0.000) | 0.001 | | (0.000) | -0.008 | *** | (0.000) |
| Quarter | | | | | | | | | |
| *1st* | 0.526 | *** | (0.019) | 0.051 | ** | (0.023) | 0.549 | *** | (0.027) |
| *2nd* | 0.504 | *** | (0.020) | 0.054 | ** | (0.024) | 0.544 | *** | (0.029) |
| *3rd* | 0.509 | *** | (0.019) | 0.082 | *** | (0.023) | 0.586 | *** | (0.027) |
| U evolution | -0.065 | | (0.123) | -1.236 | *** | (0.124) | -1.379 | *** | (0.159) |
| Distance BR/CR | -0.001 | *** | (0.000) | 0.008 | *** | (0.000) | 0.009 | *** | (0.000) |
| Observations | 91486 | | | 79683 | | | 69988 | | |
| Pseudo R2 | 0.039 | | | 0.077 | | | 0.093 | | |
| LR $\chi 2$ | 4770 | | | 7497 | | | 6507 | | |

note 1: k=1 - BR-pre, k=2 - CR-Post and K=3 - CR-Pre

note 2: Standard error are presented in brackets

note 3: For a full description of the covariates see appendix A.

# C Results when individually excluding each variable:

Summary of results on covariates balance when excluding each group of variables individually. To try and limit the table size and without loosing to much informations, statistics in the table are presented as the average of the three weighting procedures.

*Table 6: Alternative models*

| | Pseudo R2 | Std.Bias | ATET | | Std.err |
|---|---|---|---|---|---|
| IPW | 0.010 | 2.3 | 4.6 | ** | (2.170) |
| DID no X | | 8.1 | 4.3 | ** | (2.035) |
| DiD (X as covariates) | | 8.1 | 2.8 | | (1.969) |
| Alternative W | | | | | |
| *Untrimmed* | 0.011 | 2.1 | 4.0 | * | (2.203) |
| *Rescaled* | 0.011 | 2.1 | 4.0 | * | (2.200) |
| *Drop* | 0.018 | 4.3 | 4.4 | * | (2.276) |
| Drop by distance (D) | | | | | |
| *Adjacent* | 0.018 | 2.9 | 7.7 | *** | (2.483) |
| *D<5km* | 0.010 | 2.2 | 4.8 | ** | (2.155) |
| *D<10km* | 0.012 | 2.6 | 5.4 | ** | (2.373) |
| *D<20km* | 0.012 | 2.7 | 3.7 | | (2.919) |
| *D<25km* | 0.015 | 3.0 | 6.1 | | (3.801) |
| *D>70km* | 0.012 | 2.4 | 7.0 | *** | (2.295) |
| Excluding X 1 by 1 | | | | | |
| *Gender* | 0.010 | 2.3 | 4.6 | ** | (2.168) |
| *Age* | 0.010 | 2.2 | 5.1 | ** | (2.194) |
| *Marital status* | 0.010 | 2.3 | 4.6 | ** | (2.170) |
| *Supported persons* | 0.010 | 2.2 | 4.5 | ** | (2.174) |
| *Nationality* | 0.010 | 2.3 | 5.0 | ** | (2.151) |
| *Permit* | 0.010 | 2.3 | 4.7 | ** | (2.167) |
| *Local Lang* | 0.009 | 2.1 | 4.5 | ** | (2.171) |
| *Education* | 0.010 | 2.3 | 4.5 | ** | (2.137) |
| *Insured Salary* | 0.010 | 2.3 | 4.8 | ** | (2.167) |
| *Job Pre FT* | 0.010 | 2.3 | 4.2 | * | (2.161) |
| *Manager* | 0.011 | 2.4 | 4.4 | ** | (2.171) |
| *Industry sector* | 0.010 | 2.9 | 5.1 | ** | (2.167) |
| *Rural* | 0.011 | 2.1 | 4.7 | ** | (2.105) |
| *Pre U - personal* | 0.010 | 2.3 | 4.7 | ** | (2.166) |
| *Pre U - evolution* | 0.008 | 1.9 | 3.9 | * | (2.140) |
| *Pre U - regional* | 0.004 | 1.7 | 4.7 | ** | (2.170) |
| *Quarter* | 0.007 | 1.9 | 4.1 | * | (2.153) |
| *Distance* | 0.007 | 1.9 | 4.6 | ** | (2.167) |

note 1: Standard error are presented in brackets

# D Regression discontinuity design:

Figure resulting from a simple regression discontinuity design on the distance between the two regions (i.e. CR and BR). The first figure show an histogram-style graph of the mean conditional to the distance from the other region excluding municipalities in the top 10% of the distribution of distance, while the second show the local linear regression estimation.

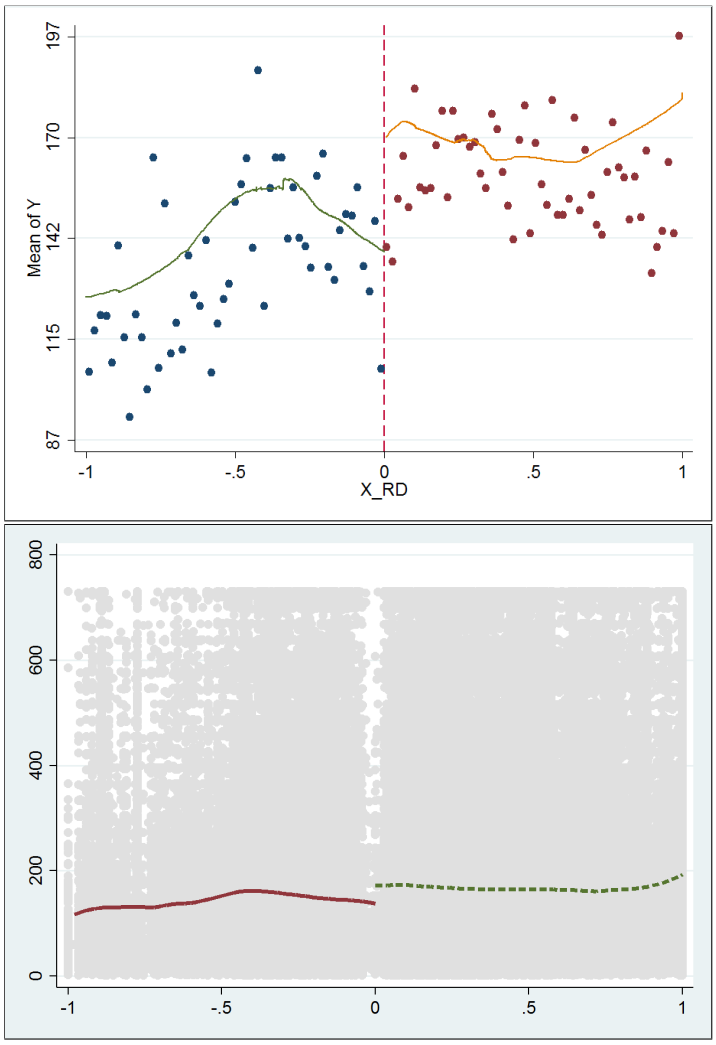


*Figure 3: RD results*