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The Impact of Local Minimum Wages on Employment: Evidence from Italy in the 1950s*

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July 2014

Abstract

This paper measures the impact of wage zones – minimum wage differentials at the province level – on Italy's local labor markets during the 1950s. Using a spatial regression discontinuity design, it finds that for the industrial sectors covered under wage zones there was an increase in employment when one crossed the border from a high-wage province into a low-wage one; the effect diminished, however, as the distance from the boundary increased. The paper also illustrates that the impact on the overall (non-farm) private sector, which includes both covered and uncovered sectors, was basically zero. On balance, the scheme generated some reallocation of economic activity, albeit confined to areas close to the province border.

Keywords: minimum wages, regional economic activity, regression discontinuity

JEL classification: C14, J38, R11

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* The views expressed in the paper are those of the authors and do not necessarily correspond to those of the institutions to which they are affiliated. We wish to thank Antonio Accetturo, Sergio Destefanis, Andrea Linarello, Giovanni Mastrobuoni, Henry Overman, Vincenzo Scoppa, Paolo Sestito, Ugo Trivellato, two anonymous Referees of the Working Papers series of the Bank of Italy, participants at the 5th Italian Congress on Econometrics and Empirical Economics (Genoa, January 2013), the Bank of Italy Workshop in Regional Science (Perugia, February 2013), the 3rd European meeting of the Urban Economics Association (Palermo, August 2013), the 28th National Conference of Labour Economics (Rome, September 2013), and the FBK-IRVAPP seminars (Trento, October 2013) for their comments and suggestions. We are grateful to Gemma Steyaert and Alice Chambers for their editorial assistance.

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1. Introduction

Considering the spatial dimension of labor markets might significantly add to our knowledge of traditional labor issues (Moretti, 2011). The spatial equilibrium model (Rosen, 1979; Roback, 1982) underlines that labor market outcomes reflect the fact that workers and firms are free to move between territories, while local prices adjust to maintain the spatial equilibrium (see also, Glaeser, 2008). The regulation of local prices – such as wages and rents – has, therefore, consequences that will depend on the extent to which it is feasible to reallocate workers and firms (Glaeser and Gottlieb, 2008; Accetturo et al., 2010). In particular, fixing different minimum wages in two confining areas might trigger a shift of economic activity that will hinge on differences in remuneration and the moving costs faced by firms and workers.

This paper conducts an empirical investigation of the effect of a territorial wage regulation implemented in Italy in the aftermath of World War II. At that time, minimum wage differentials at province level (wage zones) were established with the aim of increasing overall remuneration and making wages reflect the local conditions of productivity and cost of living more accurately. We will study the consequences on local employment of different minimum wages by looking at the territories differently exposed to the regulation. As in the groundbreaking paper of Holmes (1998) we focus on what happens when one crosses province borders. This helps to isolate the effect of wage zones from that of other province characteristics, which might be related to wage regulation. At the same time, the empirical framework we employ – a spatial regression discontinuity for windows of varying width around the border – allows us to gauge the role of reallocation of firms and workers.

Our paper is linked to the literature on minimum wages. A textbook model would suggest that setting a minimum wage above the equilibrium wage raises each firm's marginal cost and reduces its demand for labor.[1] Empirically, the textbook prediction has been challenged by Card (1992) and Katz and Krueger (1992), highlighting that more

[1] Both owing to scale effects, as the price of output rises and demand for it falls, and substitution effects, as firms substitute capital for labor.

complex models of the labor market might be needed (Manning, 2003). Nowadays, the impact of minimum wages on employment remains a hot topic of discussion (see Neumark and Wascher, 2006). Part of the dispute refers to the adequacy of the territorial control groups used in the studies. For instance, in Card's (1992) study of the California minimum wage increase, control areas are taken to be Georgia, Florida, and Dallas/Ft. Worth. This choice raises a number of doubts, as places far from California are likely to be affected by many local features that are difficult to differentiate away (Deere et al., 1995). A better alternative is to have control units in close geographic proximity with the treated ones, which are located where a minimum wage regulation is binding. However, as we show in this paper, this option is not without consequences for identification. For geographically close areas moving costs are reduced: what happens in these areas is likely to reflect the reallocation of economic activity triggered by the wage policy.

Compared to the scheme analyzed in the empirical literature, it should be noted that wage zones differ from a standard minimum wage policy. They do not impose a single (absolute or indexed) wage floor; rather, they set minimum wages for each category of wage and salary workers, from very skilled white-collar workers to common labourers. Therefore, our results on overall employment are unlikely to be driven by substitution between less-skilled and more-skilled labor (see, for instance, Currie and Fallick, 1996). Moreover, wage zones were applied to the entire national territory. Thus, the econometric problems that may arise from the selection of particular spatial entities may be less severe in our case (see Combes, 2000). Like other minimum wage schemes, the industrial coverage of wage zones was partial. Therefore, our empirical strategy tries to highlight the differential impact of the scheme between covered and uncovered industries. Finally, our investigation refers to a 10-year period (1951-1961). Thus, the results we obtain are likely to reflect long-run firm reactions to the wage regulation (see Hamermesh, 1995), which include changes in the capital stock.

Our results show that for the industrial sectors covered under wage zones there was an increase in employment when one crossed the border from a high-wage province to a low-wage one; the effect diminished, however, the further one went from the boundary, supporting the idea that moving costs are relevant. Our findings suggest that (over a tenyear period) the policy resulted in a cross-border reallocation of economic activity, driven by differences in remuneration. According to our estimates, the reallocation of employees took place within 45 kilometres of the border. The paper also illustrates that the scheme had no impact on the overall (non-farm) private sector, which includes both covered and uncovered sectors (from the uncovered sectors, crossing the border from a high-wage province to a low-wage one resulted in a decrease in employment, albeit not a significant one). These findings are corroborated by a full-fledged robustness analysis.

The paper is structured as follows. The next section describes the wage scheme. Section 3 describes the data. Section 4 illustrates the identification strategy and spatial regression discontinuity. Section 5 deals with the main empirical challenges we faced, related, for instance, to the definition of the provincial borders and wage differentials. The results and the extensive robustness checks are presented in Section 6. The last section concludes.

2. Wage zones

This section briefly describes wage regulation in post-World War II Italy. We focus on the aspects most relevant to our empirical exercise. Additional details can be found, amongst others, in Cella and Treu (1989).

During World War II Italy's wages remained stacked at the (low) levels of 1940. Low wages and the high heterogeneity of remuneration across territories, also a legacy of the Fascist regime,[2] were considered a priority issue in post-war policy discussions. The introduction of minimum wages at the local level was intended to both increase overall remuneration and to have them reflect the local conditions of productivity and cost of living more accurately (see Mariani, 1962 and Ambrogi, 1955).

[2] During the 1930s and the 1940s, the Fascist regime promoted differentiated wage regulations across regions to discourage the insurgence of a nationwide workers' movement (see Zamagni, 1976).

The paper focuses on the wage zones that were introduced under the Agreements of 1949 and 1950 (*Accordi di rivalutazione salariale*), signed by representatives of private firms and the trade unions. A previous attempt to introduce wage zones in Italy in the mid 1940s proved completely ineffective, because of the hyperinflation episode of 1946-47.[3] At the end of the 1940s, a centralized wage setting scheme regulated wage differentials across both categories of workers and territories. Moreover, it provided a mechanism to compensate workers for inflation. The scheme had three main features:

– First, it identified seven categories of worker: white-collar (first, second and third level), specialized blue-collar, qualified blue-collar, specialized laborers, and common laborers. Fixed wage differentials across categories were envisaged. For instance, the wage of a specialized blue-collar was 25% higher than that of common laborers. These wage differentials across categories were binding in each wage zone.

– More interestingly for our investigation, Italy's provinces were divided into a number of wage zones (*Gabbie Salariali*) with fixed wage differentials between them. The highest remuneration was established for the province of Milan; the lowest for the province of Enna (Sicily). For instance, the wage of a specialized blue-collar worker in Enna was 30% lower than that of an equivalent worker in Milan.

– In addition to the salary there was a "contingency allowance", a compensation for the erosion of workers' purchasing power due to inflation. The compensation mechanism was based on the national inflation index with a two-area cost-of-living indexation: the compensation for the Centre and South was lower than that envisaged for the North. Moreover, the allowances were qualification-specific. For instance, in the province of Enna the salary of a common laborer was augmented each year by a percentage of the inflation rate; this percentage was lower than that established for a common laborer residing in

[3] The problem with the *Accordi Interconfederali*, signed on 6 December 1945 for the Centre-North and 23 May 1946 for the South, was that it envisaged a uniform compensation for inflation for all categories of worker based on a province-specific indexation. Due to the hyperinflation of 1946-47 (in 1947 the inflation rate reached 62%) and the circumstance that the compensation for inflation was implemented very erratically across the provinces (contingency allowances did not reflect the true local increase in the cost of living, rather they reflected local political influences and trade union powers: see Mariani, 1962) the scheme was a dead letter by the end of the 1940s. Contingency allowances became the larger part of total wages; therefore the territorial wage ranking designed a few years previously was rendered completely ineffective. Moreover, as the compensation for inflation was equal for the various qualifications, differences between the categories of worker became very small.

Milan and also lower than the compensations received in Enna by more highly qualified workers.

The wage zones were applied in the emerging manufacturing sectors, with the notable exception of textiles and printing (which represented almost 20% and 2% of manufacturing employment, respectively).[4] Therefore, mining, construction and buildings, and private services were not covered. Overall, covered sectors accounted for 41% of total (non-farm) private sector employment in 1951. The wage zones took effect shortly after the publication of the Census (1951), ideal for our empirical investigation as the year 1951 can be used as a reasonable pre-intervention period to control for selection issues. The scheme was agreed at the nationwide level (centralized wage bargaining) and effectively implemented, with no slippages, at the local level (see Cella and Treu, 1989). The wage zones remained in place – with only minor modifications[5] – for the whole decade. In 1961 a new reform (*Accordo interconfederale* dated 2 August 1961) reduced the territorial differentials. Under heavy pressure from the trade unions, that led to the 1969 "hot autumn" of labour conflict,[6] at the end of the 1960s the *Gabbie Salariali* began to be phased out and by 1972 were definitively eliminated.[7]

3. Data

We calculate a local wage index for each of Italy's 99 provinces,[8] reflecting both the salary and compensation for inflation (see Section 2). First, we use the 1949-50

[4] As explained by Mariani (1962) these sectors were not covered by the scheme because of their specific production characteristics and a tradition of autonomy from centralized bargaining.

[5] A new agreement signed in 1954 basically confirmed the 1949-50 Agreements. In 1957 the two-zone compensation mechanism was (slightly) modified. In 1951 it was decided that differences in indexation between the North and the Centre and South amounted to 20%; in 1957 this figure was revised downward to 14%.

[6] The trade unions and leftist political parties considered the scheme to be against the interests of the workers, for whom "equal work should correspond to equal pay".

[7] On the subsequent developments in wage bargaining arrangements in Italy, see Destefanis et al., (2005).

[8] In 8 out of 99 cases, wage zones were defined at a more detailed level of stratification than an administrative province. They are listed in the Appendix. One province (Trapani, which included 22 municipalities) is missing because we were unable to collect data for worker types at the local level.

Agreements to collect 1951 wage differentials across workers and territories. Our 1951 local wage index is calculated by weighting wages by worker type at the local level (i.e. using as weights the share of that type of worker in local employment, derived from the Census conducted by the Italian National Institute of Statistics (Istat). Then, from 1951-61 we recover the annual qualification-specific two-area compensations for inflation from the *Rassegna di Statistiche del Lavoro* (various years) to take into account the increase in local wages due to the contingency allowance. Finally, the yearly indexes are averaged over the 1951-61 period. Figure 1 shows a map of Italy's 99 provinces, coloured according to the value of our local wage index (the province list of the local wage indexes is provided in the Appendix). The map also illustrates the boundaries of each province: our analysis will be based on the municipalities close to these borders. Our local wage index proxies for the actual average local wage differentials experienced during the 1950s. However, as suggested by Mariani (1962), the compensation for inflation might have only slightly impacted on the local wage differences decided in 1951. Therefore, in the result section below (Section 6) we start by considering the local wage index that we have calculated and then, as a robustness check, we use measures for the 1951 wages – rather than the 1951-61 local wage index – to estimate the impact of the policy. As it turns out, the results are very similar.

We use a number of variables taken from different sources. Data on Italy's municipalities in the 1950s (including the distance matrix at the municipality level) were taken from the Istat archive *Comuni italiani. Dall'unificazione al 2001: popolazione, aggregazioni, soppressioni*. Data on the outcome (the growth rate of employment at the municipality level) are taken from the 1951 and 1961 Istat census of industry and services (*Censimento Industria e Servizi*). These data provide sectoral breakdowns at the city level. We also make use of a number of additional observables at the municipality level, listed in Tables 1 and 2 below. These variables are taken from the archive of the National Association of Italian Municipalities (ANCI) *1861-2011: L'Italia dei Comuni: 150 anni di Unità*, with the exception of data on political turnout, which are taken from the Ministry of the Interior.

4. Identification strategy

Our goal is to evaluate whether wage zones made a difference to local employment. As explained above, the 1949-50 Agreements split the Italian provinces into a number of such zones. We exploit the borders between high- and low-wage provinces to investigate the causal impact of the policy. In principle, provinces on two sides of a wage border can vary in terms of many observed and unobserved characteristics that can be correlated with measures of local development. Crucially, in 1951 high-wage provinces were characterized by a higher degree of prosperity. Figure 2 plots the 1951 employment rate (employment over population) against local minimum wages in the same year. The positive relationship that is shown in the figure comes as no surprise, as the rationale of the policy was to keep wages down in the lagging areas, characterized by lower productivity and lower cost of living.

We use a regression discontinuity design (RDD) to differentiate out all the characteristics that may confound the identification. The main idea behind this research strategy (Angrist and Lavy, 1999; Black, 1999; van der Klaauw, 2002) is that municipalities on one side of the boundary make good comparisons with those just on the other side of it. The RDD has already been used in a spatial context, to investigate the impact of policies that vary across borders. Leading examples include Holmes (1998); Black (1999); Gibbons and Macin (2003); Bayer et al., (2007); Duranton et al., (2011); Dachis et al., (2012).

The RDD is deemed preferable to other non-experimental methods because if the units of the analysis (in our case the Italian municipalities) are unable to manipulate precisely the forcing variable (the distance from the border),[9] the variation in treatment (changes in minimum wages) around the border is randomized as though the municipalities had been randomly drawn on just one or other side of the boundary (see Lee, 2008). One implication of the local randomized result is that the empirical validity of the RDD can be tested. If the variation in the treatment near the edge is approximately randomized, it

[9] This is trivially verified, as the provincial borders long predate the 1949-50 Agreements. See, for instance, Caringella et al., (2007).

follows that all “baseline covariates” – those variables determined prior to the start of the policy – should have about the same distribution on the two sides of the border. Section 6 presents a test for the absence of discontinuity in baseline characteristics around the boundaries that substantiates the empirical strategy. Using a propensity score matching, it also deals with the potential pitfalls of the fact that the policy envisaged lower minimum wages for relatively less developed areas. To be sure: we will compare municipalities that are taken to be similar with respect to their pre-treatment levels of development. One potential risk of our empirical strategy is the possibility that aside from the discontinuity in wage zones something else could vary at the border. In that unfortunate case, our results cannot be attributed to the sole effect of the mandatory changes in the local wages documented in Section 2. This problem is tackled in Section 6, where we account for the major potential confounders: the funds received by southern territories under the *Cassa per il Mezzogiorno* (an aid scheme promoted also with US money) and the circumstance that territorial wage regulations were also defined for agriculture, the sector from which workers mainly shift to join the manufacturing and service sectors. In both instances, we find very reassuring results.

We are interested in studying the extent to which the policy triggered a spatial reallocation of workers (see Section 5.1). Therefore, we run local (Pagan and Ullah, 1999) linear regressions on windows of varying width around the border. The causal effect of the wage zones could be assessed by estimating the following equations, one for each side (low-wage *l* province and high-wage *h* province, respectively) of the border:

$$Y = \alpha_l + \beta_l (X - c) + \varepsilon \qquad \text{where} \quad c \le X \le c + w \qquad (1)$$

$$Y = \alpha_h + \beta_h (X - c) + \varepsilon \qquad \text{where} \quad c - w \le X < c \qquad (2)$$

where *Y* is the growth rate of employment, *c* represents the border, *(X-c)* is the distance of the municipality from the border, and *w* denotes a window of width *w* on both sides. In this case the impact of wage zones can be computed as the difference between the two regression intercepts, α_l and α_h, on the two sides of the boundary.

As recommended in Lee and Lemieux (2010), we use the pooled version of equations (1) and (2). Therefore, by letting $\tau = \alpha_l - \alpha_h$, and using D to indicate the treatment variable, which takes on the value of one for municipalities located on the low-wage side of the border, we get our estimating equation:

$$Y = \alpha_h + \tau \cdot D + \beta_h (X - c) + (\beta_l - \beta_h) \cdot D \cdot (X - c) + \varepsilon \quad \text{where} \quad c - w \leq X \leq c + w \qquad (3)$$

Note that equation (3) allows the regression function to differ on both sides of the border by including interaction terms between D and X. The parameter of interest is τ – that is, the average treatment effect of having a low-wage zone (compared to a high-wage one) and can be interpreted as the jump between the two regression lines at the border. Operationally, we run local linear regressions and estimate a rectangular kernel (Hahn et al., 2001).

5. Empirical issues

This section describes three main empirical challenges that are dealt with in the paper.

5.1 *Action at the border.* As discussed by Holmes (1998), simple theoretical reasoning suggests that when adjacent provinces obey different minimum wage policies the impact at the border might reflect relevant reallocation effects. Firms have an incentive to move where wages are lower, while workers will have an incentive to do the opposite. In a world where firms and households are footloose – a reasonable assumption for municipalities that are only a few kilometres apart – the final equilibrium configuration will depend on the wage zone differences, the moving costs faced by firms and workers and their respective degree of market power. As Holmes puts it, "finding a big effect at the border by no means implies that a policy has a big effect far from the border" (Holmes, 1998, p. 676). Reallocations across the border implies that welfare analysis is a tricky business, as an increase in local development triggered by a more favourable minimum wage might come entirely at the expense of the adjacent province. We deal extensively with this issue in the empirical section (Section 6). Basically, we run our estimating equation (3) for samples of increasing widths *(w)* around the border. The idea is that reallocation effects should show up with smaller bandwidths (while vanishing out with larger ones).

5.2 *Relevant boundaries.* Unlike in other countries with smoother terrain and a more recent history of province formation, Italy's provincial borders are very jagged. This implies that in some cases a municipality belonging to one province might have two or more provincial borders close to it. This is illustrated in Figure 3. The municipality of Varese Ligure (in the province of La Spezia) clearly shows our point. It is bordered by Albareto in the Province of Parma but also by Castiglione Chiavarese (Province of Genoa) and Zeri (the Province of Massa Carrara). Note also that all these contiguous provinces have different wage zones. This multiplicity of potentially relevant borders might jeopardize our research design – which is based on the idea of comparing municipalities across a single boundary. We tackle this issue by taking a very prudent stance, eliminating from our sample all the municipalities, like Varese Ligure in Figure 3, for which a problem of multiple relevant borders can arise. Basically, we adopt a safety band (b) and consider only municipalities for which no boundary beyond the one we study (with a wage zone other than the one selected), is found on that piece of land (operationally, for each (w) we impose the requirement of no other border for a distance of (w + b)).[10] The width of the safety band is first set arbitrarily (10 Km) and is then allowed to vary to probe robustness (see Section 6).

5.3 *Wage differences.* As illustrated in Figure 1, in our RDD exercise the spatial structure of our data is extremely rich. We have many provincial borders and a high number of cross-border differences in minimum wages. Figure 4 shows the distribution of wage zones across Italian municipalities. Note that the distribution is skewed to the right, as many boundaries divide provinces that differ only slightly in minimum wages. This is somewhat unfortunate, because the impact of wage zones is more easily identified when the jump is large. Here, we take a cross-province perspective and pool minimum wage

[10] Imposing the safety band has a cost in terms of observations. For instance, by introducing the 10 km band we are left with 4,100 observations (from the 7,800 originally available).

differences into two groups: Low Differentials and High Differentials. This also allows us to obtain a sizeable data set, which is useful for checking the sensitivity of our results to different bandwidth samples and to carry out a number of data demanding robustness experiments (Section 6). Note that the estimates for Low Differentials can also be seen as placebos. Since the variation induced by the policy variable is negligible, the estimated jump should be negligible as well (or, if an effect is found, it should be less evident than in the estimation that makes use of the High-Differential group). We initially use ad-hoc definitions for Low (from 0% to 3%) and High Differentials (from 4% to 22%), and then probe our results by varying the grouping.

6. Results

This section describes our baseline results and then turns to robustness.

6.1 *Baseline results*. Our results are derived from two different samples. The *Raw Sample* includes all the municipalities located at the two sides (for windows of various widths, see Section 5.1) of a relevant border (uniquely defined by imposing the safety band, see Section 5.2), grouped according to the degree of minimum wage differences at the border (Low and High Differentials, see Section 5.3). We start with almost 7,800 municipalities and, after implementing the above steps, obtain the number of observations documented in Tables 1 and 2 below.[11] The propensity score sample (*PS-Sample*) first matches treated and control municipalities through a PS routine and then allocates them to width intervals and the two wage differential groups as in the Raw Sample (again, it excludes municipalities close to more than one boundary). The PS matching makes justice of all observable pre-treatment characteristics which might determine selection into treatment.[12] In particular, it deals with the possible confounding factors stemming from the fact that the wage zones envisaged lower wages for the less-developed territories. Figure 5 shows the wage differential distribution of municipalities in the Raw Sample and the PS-

[11] The sample is trimmed at the 5^{th} and 95^{th} percentiles.

[12] As suggested by Austin (2011), our caliper is taken to be 0.2 of the standard deviation of the (logit) estimated PS.

Sample when a width of 20 km is considered. Note that these figures mirror Figure 4, where the universe of Italy's municipalities is instead considered.

To substantiate the idea that the assignment of the treatment near the border is approximately randomized, we examine whether observed baseline covariates are locally balanced on either side of the boundary. The regression discontinuity framework provides a natural framework to check whether some confounding factor is driving some spurious correlation. It suffices to run RDD regressions (of the type in equation (3) above) using as dependent variables those factors that the researcher suspects could be driving the results. If no effect is detected then that variable can be considered as controlled for in the RDD exercise. We focus on a large number of characteristics that should capture most of the municipality heterogeneity. Some of them depict the physical characteristics of territories. For instance: kilometres squared, elevation, steepness of the municipal territory (difference in elevation within a municipality), dummies for macro-areas (North, Centre and South), and being an administrative centre (a province capital). We also include population. Other covariates (plants and employees) refer to the strength of local economic development at the beginning of the 1950s in different sectors (covered sectors and total non-farm private sector). As recent literature has shown that during the post-World War II period social capital was a powerful driver of prosperity (Albanese and de Blasio, 2014), we also control for the local endowments of civic virtues.

Balancing results are shown in Table 1 and Table 2, for the Low- and High-Differentials group respectively. For the first group we find that basically no jump occurs at the boundary for the overwhelming majority of the covariates, even in the Raw Sample. One exception is related to elevation, which indicates that the larger bandwidths treated are less likely to be on a mountain.[13] As expected, the PS routine levels out all differences in observables.

[13] As explained by Lee and Lemieux (2010), however, some of the differences in cross-border covariates might be statistically significant by random chance. To check for this possibility, we combine the multiple tests into a single test statistic (a stacked test) that measures whether data are broadly consistent with the random treatment hypothesis around the border. The last line of Table 1 and Table 2 presents a χ^2 test for discontinuity gaps in all the equations equal to zero.

The results from the High-Differentials group are very different. For the Raw Sample we find that several important variables are not randomized around the border. The treated municipalities are characterized by a smaller population, a smaller area, and a lower degree of economic development as measured by plants and employment. As argued above, given the design of the policy, this is clearly an expected outcome. Again, by using the PS matching all the differences in observables disappear (and the stacked test is highly supportive).[14]

We start by presenting in Table 3 the estimates obtained with the Raw Sample. The focus is on the covered sectors. Panel A presents the results from the Low-Differentials group, which uses cities close to boundaries for which the maximum or minimum wage differential is 3%. In each column a different bandwidth is used starting from the values of 20 km and gradually increasing the distance from the border.[15] Panel B displays the results from the High-Differentials group, which includes municipalities around boundaries with wage variations from 4% to 22%. For the Raw Sample we fail to find any effect of the wage zones on employment growth for either group. However, as shown in Table 2, the results for the High- Differentials experiment should be taken *cum grano salis*, as this sample is featured by significant city heterogeneity across the provincial border.

Table 4 presents the results obtained with the PS-Sample. These results are not biased by differences in pre-treatment observables. For the Low-Differentials group the results obtained are similar to those of the Raw Sample, as their relative similarity in observables, even before using the PS routine, would suggest. As for the-High Differentials group, our results indicate that wage zones have a significant effect on employment growth. For the bandwidth of 20 km the estimated RDD impact at the border is positive and statistically significant: a 1% decrease in the minimum wage brings about a 1.71% 10-year cumulative increase in employment growth. The effect diminishes when more distant cities are included in the sample. For the 35 km bandwidth, the elasticity amounts to 1.02. For bandwidths equal to or larger than 45 km we fail to find any impact.

[14] Note that in the PS procedure including population and elevation is enough to censure the balancing across all the remaining variables.
[15] For bandwidths of less than 20 km the PS-routine fails to find, in some specifications, an appropriate number of matches.

These findings suggest that the wage zone policy triggered a reallocation of economic activity from territories with higher minimum wages to areas benefiting from a wage regulation that was more favourable to firms. Our findings also highlight that moving costs were an important factor. The fact that the reallocation was limited to the areas close to the boundaries suggests that workers might have had the opportunity to change their place of work without moving residence.[16] As matter of fact, RDD estimates of the effect of wage zones on the municipal resident population point to a zero impact.[17] Figure 6 provides the usual RDD graph for the two specifications of Table 4 based on the 20 km bandwidths.

Table 5, Panel A provides the results for the uncovered sectors (which include textiles and printing, mining, construction and buildings, and private services: see Section 2). To save space, from now on we will present only the results of the PS-Sample for the High-Differential groups, which is the one that more reasonably documents all the actions that takes place for the covered industries because of the wage zones. For the sectors not covered under the scheme, the estimated jump at the border is now negative (though never significant). Crossing the border from a high-wage province to a low-wage one seemed to have had no impact (or a negative one) for these sectors. Panel B reproduces the same exercise for total (non-farm) private sector employment: the sum of covered and uncovered sectors. We find that the estimated jump at the boundary is now positive; it is, however, never significant. Overall, the findings presented in Table 5 point to some reallocation of economic activity, from the sectors that are wage zone exempt to those that have to obey to the rule.

[16] In a previous version of this paper we also used the growth rate of plants as an additional outcome variable. The results mirrored those obtained by using employment growth as an outcome.

[17] These results are available from the authors.

6.2 *Sensitivity checks*. Table 6 presents a selection of the robustness analyses we have performed.[18] In the table we only focus on the covered sectors and the overall (non-farm) private sector and show the results of the PS-Sample for the High-Differential Groups. However, robustness checks have also been conducted for all the remaining sector categories and sample groupings, with the results consistently in line with those presented above.

Panel A augments the specifications with a number of covariates (we include the variables depicted in Tables 1 and 2 above).[19] As discussed by Lee and Lemieux (2010), because of its local randomized experimental nature it is not necessary to include additional controls in an RDD setting to obtain consistent estimates. However, doing so might reduce the sample variability in the estimator. As a matter of fact, our results show that the inclusion of the additional controls slightly reduces the standard errors, thus validating the identification strategy. Point estimates mirror those obtained without covariates.

Panel B presents the results obtained by limiting our exercises to municipalities located in the Centre and North of the country. This experiment is intended to tackle the issue of a potential confounder (i.e., an omitted variable that varies across provincial borders: see Section 4). During the decade 1951-61 a substantial inflow of public money went to southern territories under the patronage of the *Cassa per il Mezzogiorno*, a public development agency set up in 1950 to promote economic development. Note that a geographic breakdown of the funds by province of destination of the financing is not available. However, it is not unreasonable to conclude that more generous financing was provided to the relatively more underdeveloped places in the South. In theory this might

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[18] We have also replicated all the experiments presented in the text by including a set of fixed effects for the municipalities sharing the same border. This implies (see Duranton et al., 2011) that the estimates reflect only the variability within the group of observations that share the same discontinuity (variability between groups is thus differentiated away). As a matter of fact, these results (not reported but available upon request) are almost indistinguishable from those of the baseline. We also compared our results with the ones derived from parametric specifications. This assured us that our findings are not driven by non-parametric specification bias (see Imbens and Lemieux, 2008). For instance, the results from a degree-four polynomial specification (the ones suggested by the Akaike criterion), indicate that – for the samples of Table 4 – the jump at the border for the covered sector is estimated to be 12.186 (s.e. = 5.264).

[19] However, as for the measures of employment and plants we only include those referring to total private sector, which are very much correlated with those calculated for the covered sectors.

(upwardly) bias our results for the southern sample of municipalities in the Raw Sample, as the provinces with less economic fortunes benefited, in addition to lower wages, from public aid. Regarding the PS-Sample, however, the bias should be moderate, as municipalities are made to be comparable also with respect to their pre-treatment economic development. The results presented in Panel B show that this is actually the case. When our sample is taken to be that of the Centre and North of Italy, where there was no *Cassa per il Mezzogiorno*, the results are very similar to those obtained with the sample that fully covers the national territory, with point estimates only slightly higher. The only difference refers to the distance for which the impact vanishes: in this example 50 km.

Next, we check for the role of another potential confounder: the existence of local minimum wages in agriculture. In 1951, this sector represented 44% of the national workforce. By 1961 the share had fallen to 33%. The reallocation of workers from agriculture to the (highly productive) sectors of industry and services is known to be a key feature of Italy's economic performance during the 1950s. As wages in agriculture were in any event lower than wages elsewhere (Broadberry et al., 2012) we do not expect territorial minimum wages in this sector to have any significant impact on our estimates. In any case, in Panel C we present the results derived from a sample of municipalities where (cross-border) differences in agricultural wages were very low (<=3%), on the basis of some historical documentation (*Accordo del 24 settembre 1952 per la scala mobile nei salari agricoli*). The results are again similar to the baseline ones, with point estimates somewhat larger. In this experiment, however, the bandwidth for which the impact is found to be nil is now shorter (35 km).

Beyond the potential effects of the *Cassa per il Mezzogiorno* and minimum wages in agriculture, very little scope remains for potential confounders at the local level. During the 1950s, Italy had a very centralized structure for the provision of local public services. Therefore, provinces were basically local jurisdictions that did not perform any significant role for the local economy (the province borders were designed for political reasons by Mussolini in 1927: see *Regio Decreto Legge 2 gennaio 1927, no. 1*). On the other hand, the regions, which would actually run important local public services, not established until 1970.

The large number of very small cities in our sample might lead to high heterogeneity along some unobserved dimensions (for instance, the local endowment of infrastructures for which we have no data). This implies that our estimates might be biased by the presence of several outliers, which we struggle to identify ex ante. For this reason we initially drop all the municipalities with a population of less than 1,000 inhabitants (roughly 1,300). Panel D describes the results for this sample. They are very similar to those obtained with cities of any size. Panel E takes a more prudent stance on the safety band (see Section 5.2) by augmenting its width by 50%. Again, the results remain undisputed, with the exception of the distance over which the reallocation takes place (now estimated to be 50 km). Next, we check the sensitivity of the results to the wage differential grouping (remember, Low-Wage Differentials go from 0% to 3%; High-Wage Differentials from 4% to 22%). In Panel F we consider a larger treatment group by including municipalities with a 3% wage difference at the border. Once more, the results are in line with those documented above. However, the point estimates are now slightly lower (as they should be, given that we are adding to the treatment group places with reduced wage differentials).[20]

Finally, we tackle the issue of potential mismeasurements for our local wage index. Readers will recall (Section 3) that the index is derived from a reconstruction based on historical sources. In Panel G, we replicate our estimates by using the 1951 local wage index, instead of the one used so far: this index, averaged over the 1950s, allows for inflation compensations. We find that when using this measure the results differ very little. This finding is consistent with Mariani (1962), according to whom the 1951 wage structure was only modestly changed over the decade by the qualification-specific compensation for inflation. Finally, Panel H reports the results from an experiment where the index for local wages is derived by using as weights the national breakdown of employment across qualifications, instead of the local one. Again, the estimates are very similar.

[20] We also considered a smaller treatment group by excluding municipalities with less than 5% differentials, with no changes in our results.

7. Concluding remarks

Our findings suggest that local minimum wage regulation resulted in a cross-border reallocation of economic activity, which took place in the areas close to the province borders featured by high differentials in minimum wages. The reallocation concerned the manufacturing sectors covered under the policy. The impact on overall employment (including both covered and uncovered sectors) was zero, even in the borderline areas.

The study highlights that to evaluate the effects of minimum wage policies the fact that workers and firms are free to move across territories should be carefully taken into account. In particular, a minimum wage policy might have considerable consequences in terms of moving people and jobs around, without improving overall employment or welfare. It would be useful, therefore, to include geography more accurately in evaluating labor market regulations. Our results also show that the areas close to those where a minimum wage policy is in place are the obvious candidates for the inflows and outflows of economic activity that are triggered by the policy. Therefore, their use as counterfactuals should not be taken for granted, even though these areas display the greatest similarity with the treated areas.

A final remark from our empirical investigation refers to Italy's economic development. The role of the wage zones for the extraordinary growth rates recorded in the aftermath of World War II has remained unexplored to date. Casual empiricism could have suggested that the scheme was a good thing, as its implementation went hand in hand with unrecorded growth (and strong regional convergence). Our results suggest that this was unlikely to be the case.

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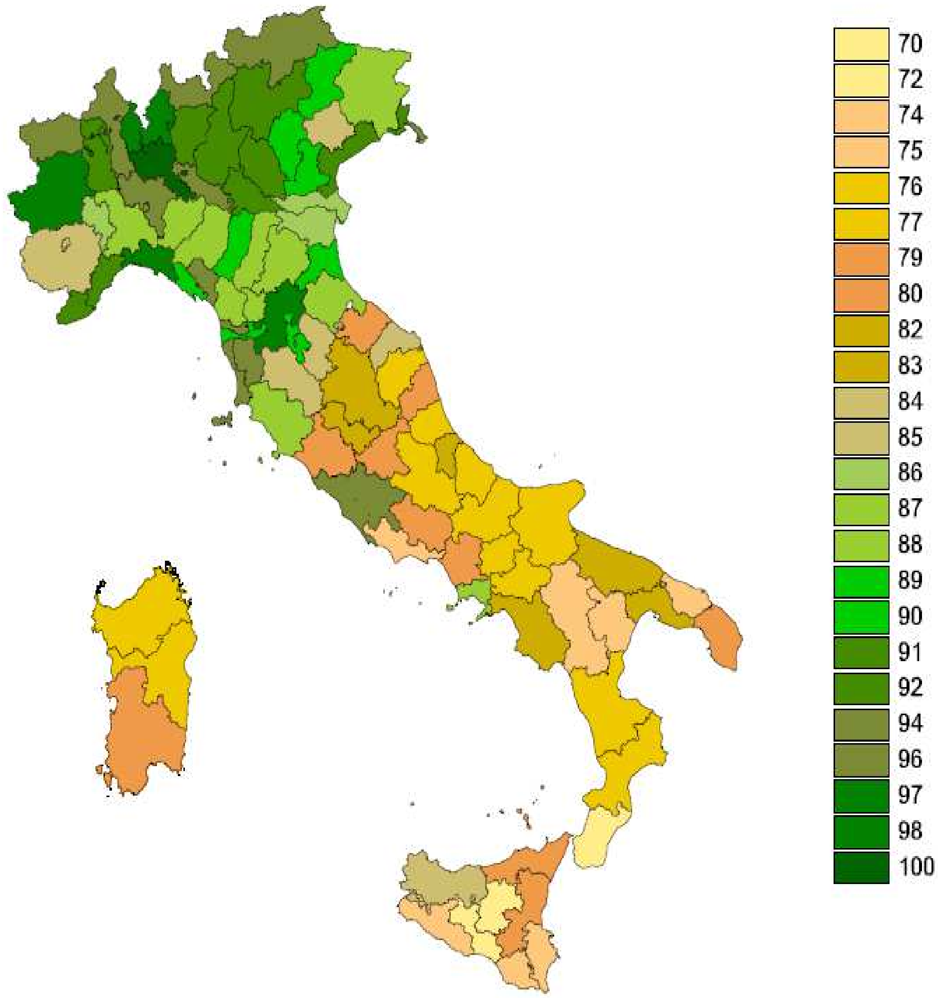
Figure 1. Italy's wage zones during the 1950s

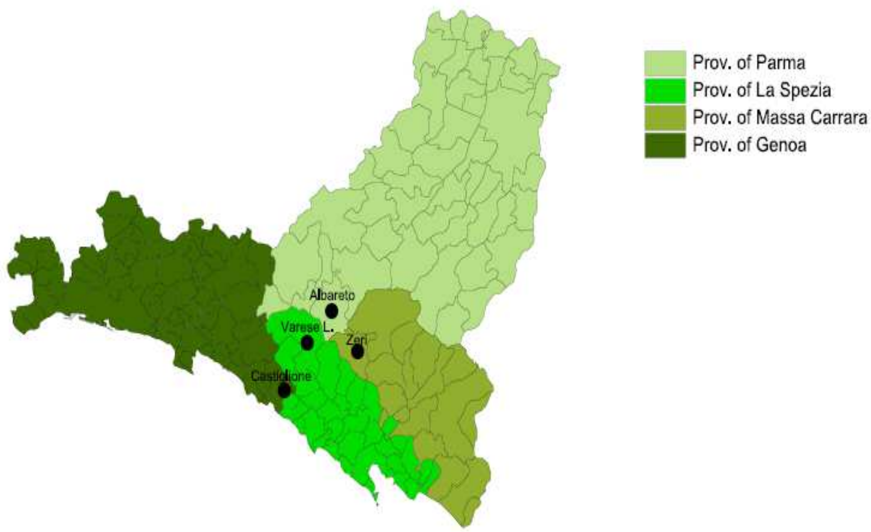


Note: the map is based on our local wage index.

Figure 2. Wage zones and the employment rate in 1951

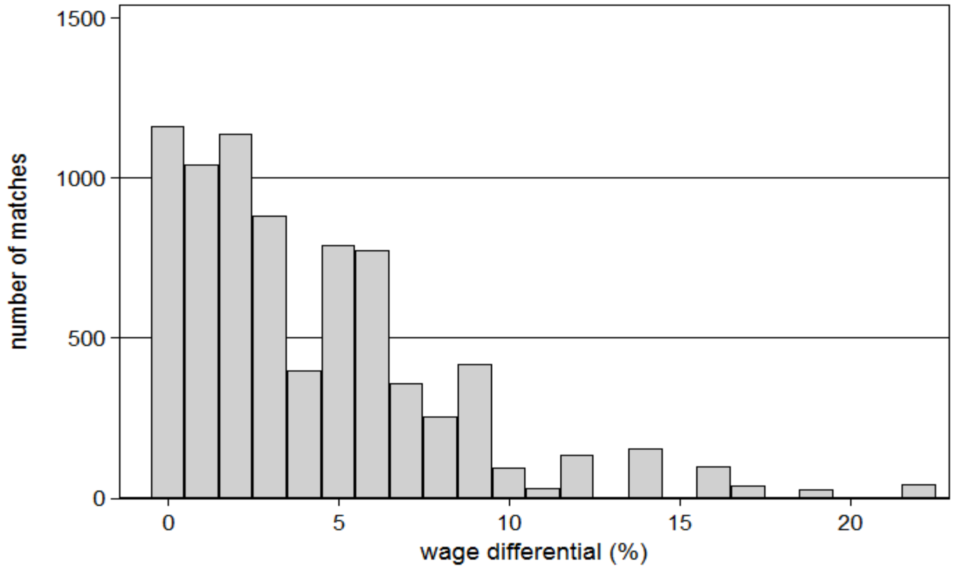
Notes: the wage zones are based on our local wage index. Data for employment and population are taken from the 1951 Census.

Figure 3. The jagged border problem: an illustration



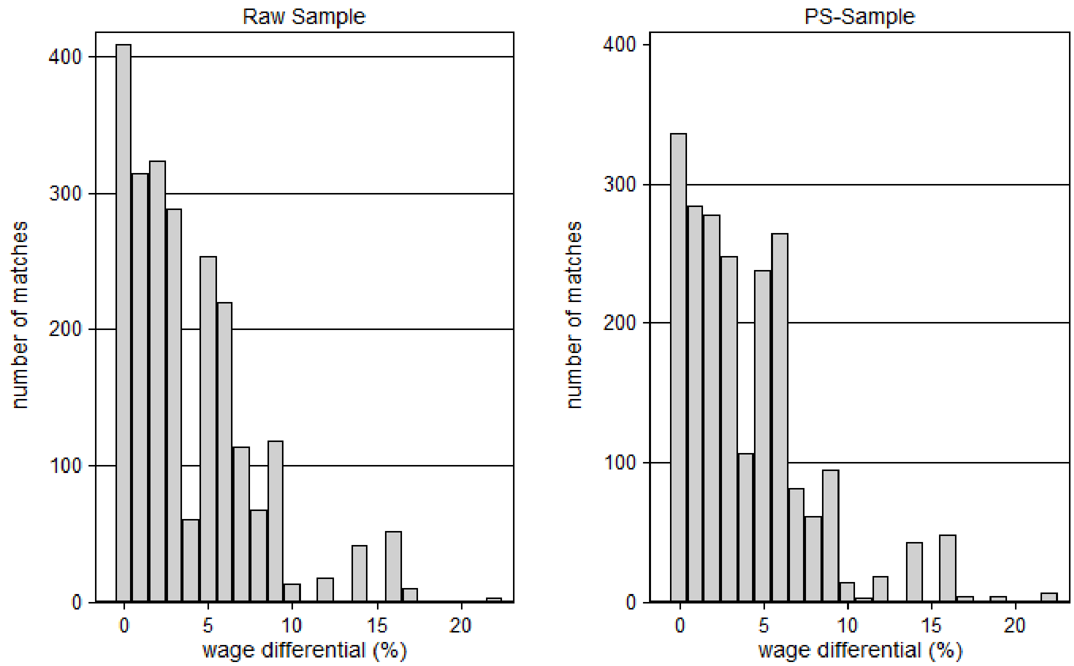
Notes: the map illustrates the contiguous provinces of Parma (in the region of Emilia Romagna), La Spezia (Liguria), Genoa (Liguria), and Massa Carrara (Tuscany). The wage zones are based on our local wage index.

Figure 4. The distribution of wage zone differentials across municipalities: universe



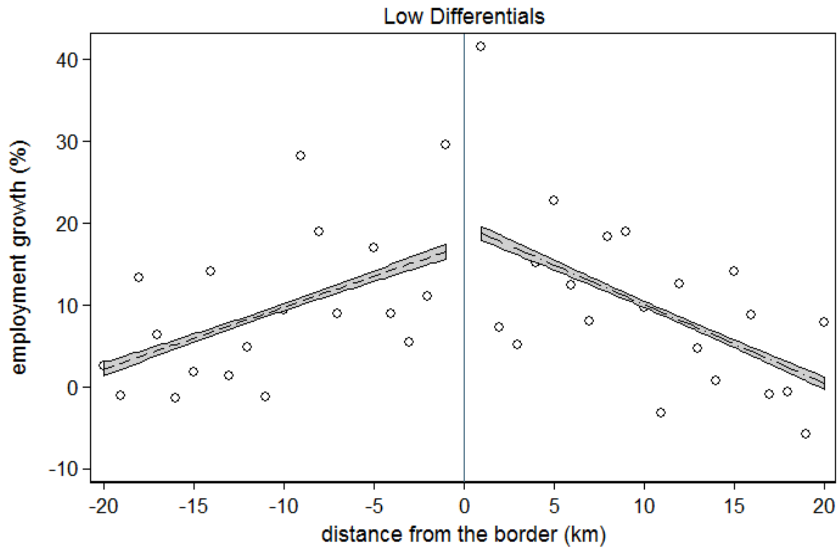
Note: the wage differentials are calculated on the basis of our local wage index.

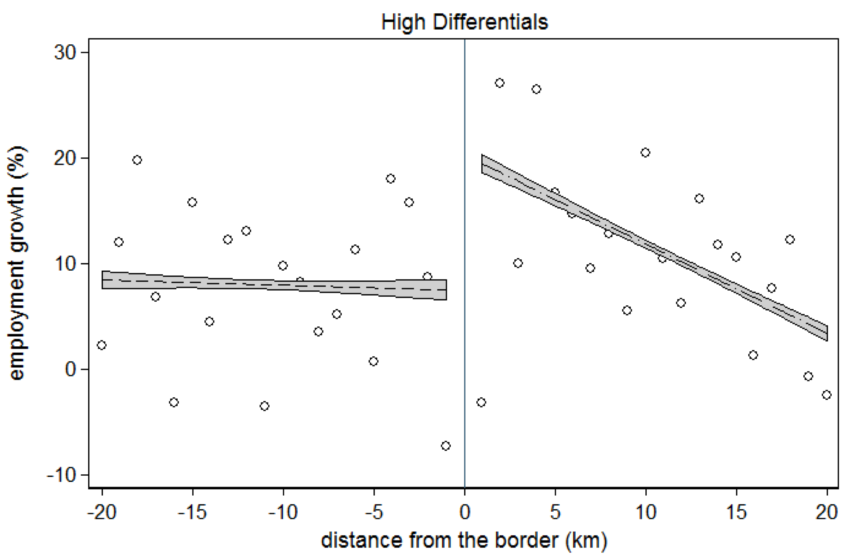
Figure 5. The distribution of wage zone differentials across municipalities: estimation samples



Note: the wage differentials are calculated on the basis of our local wage index.

Figure 6. The impact of wage zones on the covered sectors





Note: each point represents the average employment growth by bins of 1 km.

Table 1. Balancing properties for baseline covariates: Low-Differentials group

	Raw Sample					PS-Sample				
BANDWIDTH	20 km	25 km	30 km	35 km	45 km	20 km	25 km	30 km	35 km	45 km
(log) Population	-0.031	-0.006	-0.011	-0.004	0.045	-0.122	-0.092	-0.074	-0.093	-0.080
	(0.080)	(0.072)	(0.067)	(0.063)	(0.057)	(0.088)	(0.079)	(0.075)	(0.071)	(0.066)
(log) Area	-0.128*	-0.093	-0.062	-0.073	-0.044	-0.027	-0.062	-0.061	-0.073	-0.078
	(0.077)	(0.068)	(0.063)	(0.060)	(0.055)	(0.082)	(0.073)	(0.067)	(0.063)	(0.058)
(log) Elevation	-0.068	-0.112	-0.145*	-0.186**	-0.231***	-0.019	-0.004	-0.011	-0.007	-0.020
	(0.103)	(0.091)	(0.086)	(0.081)	(0.073)	(0.109)	(0.093)	(0.085)	(0.084)	(0.078)
(log) Diff. Elevation	-0.034	-0.067	-0.048	-0.072	-0.053	-0.005	-0.031	-0.033	-0.074	-0.065
	(0.124)	(0.105)	(0.096)	(0.090)	(0.080)	(0.149)	(0.127)	(0.115)	(0.108)	(0.096)
Macro-area	0.027	0.034	0.039	0.041	0.042	0.005	0.003	0.014	0.024	0.032
	(0.082)	(0.072)	(0.066)	(0.062)	(0.056)	(0.088)	(0.078)	(0.071)	(0.067)	(0.060)
Provincial Capital	0.004	0.001	-0.002	-0.000	0.000	0.003	0.007	0.004	0.005	0.006
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.007)	(0.007)	(0.006)	(0.006)
Electoral turnout	0.247	0.207	0.143	0.096	-0.015	0.026	0.043	0.227	0.226	0.187
	(0.338)	(0.290)	(0.266)	(0.250)	(0.224)	(0.366)	(0.328)	(0.303)	(0.283)	(0.254)
(log) Plants, CS	0.009	0.010	-0.007	0.009	0.057	0.028	0.028	0.012	0.002	-0.016
	(0.090)	(0.079)	(0.074)	(0.069)	(0.064)	(0.095)	(0.086)	(0.080)	(0.075)	(0.068)
(log) Empl., CS	0.049	0.076	0.069	0.079	0.162**	0.094	0.113	0.083	0.055	0.048
	(0.115)	(0.101)	(0.094)	(0.088)	(0.080)	(0.127)	(0.113)	(0.105)	(0.098)	(0.089)
(log) Plants, PS	-0.014	0.012	0.006	0.030	0.071	-0.009	-0.001	-0.006	-0.015	-0.037
	(0.083)	(0.074)	(0.069)	(0.065)	(0.059)	(0.089)	(0.081)	(0.076)	(0.071)	(0.065)
(log) Empl., PS	-0.071	-0.016	-0.010	0.013	0.089	-0.079	-0.066	-0.046	-0.040	-0.030
	(0.104)	(0.091)	(0.084)	(0.080)	(0.072)	(0.115)	(0.103)	(0.096)	(0.090)	(0.082)
Obs.	2668	2976	3154	3266	3432	2292	2712	2968	3146	3386
Joint Test	0.69	0.93	0.86	0.95	0.41	0.15	0.23	0.31	0.18	0.22

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. Standard errors are in parentheses. Results are from non-parametric estimation (rectangular kernel). The Joint Test reports the results for the stacked discontinuity test (Lee and Lemieux, 2010).

Table 2. Balancing properties for baseline covariates: High-Differentials group

	Raw Sample					PS-Sample				
BANDWIDTH	20 km	25 km	30 km	35 km	45 km	20 km	25 km	30 km	35 km	45 km
(log) Population	-0.321***	-0.303***	-0.295***	-0.242***	-0.203***	-0.018	0.026	-0.040	-0.037	-0.056
	(0.107)	(0.094)	(0.087)	(0.079)	(0.072)	(0.103)	(0.092)	(0.085)	(0.079)	(0.069)
(log) Area	-0.178**	-0.176**	-0.151**	-0.128*	-0.115*	0.091	0.057	0.061	0.077	0.059
	(0.086)	(0.077)	(0.072)	(0.066)	(0.060)	(0.094)	(0.082)	(0.074)	(0.069)	(0.061)
(log) Elevation	-0.154	-0.158	-0.174	-0.120	-0.063	-0.001	-0.023	-0.007	0.002	0.018
	(0.130)	(0.117)	(0.108)	(0.102)	(0.093)	(0.135)	(0.121)	(0.108)	(0.100)	(0.090)
(log) Diff. Elevation	-0.183	-0.165	-0.122	-0.081	-0.086	0.020	-0.004	0.041	0.049	0.066
	(0.170)	(0.150)	(0.138)	(0.127)	(0.114)	(0.199)	(0.170)	(0.149)	(0.136)	(0.118)
Macro-area	0.007	0.012	0.013	0.014	0.014	0.036	0.021	0.017	0.012	0.017
	(0.086)	(0.077)	(0.071)	(0.066)	(0.060)	(0.091)	(0.081)	(0.073)	(0.068)	(0.061)
Provincial Capital	0.009	0.006	0.010	0.009	0.005	0.001	0.000	0.007	0.006	0.003
	(0.011)	(0.009)	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)
Electoral turnout	-0.257	-0.313	-0.172	-0.244	-0.182	-0.019	0.152	0.051	0.150	0.059
	(0.401)	(0.360)	(0.330)	(0.304)	(0.279)	(0.443)	(0.386)	(0.359)	(0.334)	(0.296)
(log) Plants, CS	-0.349***	-0.335***	-0.300***	-0.272***	-0.215***	0.086	0.088	0.065	0.063	0.019
	(0.113)	(0.099)	(0.091)	(0.083)	(0.076)	(0.115)	(0.100)	(0.091)	(0.083)	(0.074)
(log) Empl., CS	-0.386***	-0.388***	-0.371***	-0.340***	-0.285***	0.074	0.108	0.047	0.060	-0.029
	(0.145)	(0.127)	(0.116)	(0.106)	(0.097)	(0.152)	(0.131)	(0.118)	(0.109)	(0.097)
(log) Plants, PS	-0.332***	-0.337***	-0.314***	-0.286***	-0.245***	0.099	0.092	0.066	0.053	-0.008
	(0.109)	(0.096)	(0.087)	(0.080)	(0.073)	(0.107)	(0.094)	(0.085)	(0.078)	(0.069)
(log) Empl., PS	-0.367***	-0.378***	-0.394***	-0.385***	-0.341***	0.091	0.086	0.082	0.072	-0.004
	(0.135)	(0.118)	(0.108)	(0.099)	(0.090)	(0.136)	(0.118)	(0.106)	(0.099)	(0.087)
Obs.	1942	2108	2220	2340	2476	1968	2290	2540	2722	3038
Joint Test	0.00	0.00	0.00	0.00	0.00	0.85	0.77	0.63	0.62	0.39

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. Standard errors are in parentheses. Results are from non-parametric estimation (rectangular kernel). The Joint Test reports the results for the stacked discontinuity test (Lee and Lemieux, 2010).

Table 3. The impact of wage zones on the covered sectors: Raw Sample

BANDWIDTH	20 km	25 km	30 km	35 km	45 km
			Panel A. Low-Differentials group		
Employment growth	2.075	0.838	0.345	-0.085	-0.606
	(3.848)	(3.362)	(3.078)	(2.896)	(2.605)
Elasticity	1.52	0.61	0.25	-0.06	-0.45
Obs.	2668	2976	3154	3266	3432
R-squared	0.02	0.01	0.01	0.01	0.01
			Panel B. High-Differentials group		
Employment growth	7.165*	3.788	2.081	0.836	1.152
	(4.173)	(3.701)	(3.394)	(3.107)	(2.819)
Elasticity	0.96	0.51	0.28	0.11	0.16
Obs.	1942	2108	2220	2340	2476
R-squared	0.01	0.01	0.01	0.02	0.02

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. Standard errors are in parentheses. Results are from non-parametric estimation (rectangular kernel). Elasticity is calculated as the percentage jump of the outcome at the border for a 1% reduction in wages.

Table 4. The impact of wage zones on the covered sectors: PS-Sample

BANDWIDTH	20 km	25 km	30 km	35 km	45 km
			Panel A. Low-Differentials group		
Employment growth	2.564	4.097	2.779	3.245	1.657
	(5.077)	(4.457)	(4.052)	(3.741)	(3.316)
Elasticity	1.85	3.00	2.04	2.42	1.24
Obs.	2292	2712	2968	3146	3386
R-squared	0.01	0.01	0.00	0.01	0.00
			Panel B. High-Differentials group		
Employment growth	12.421**	10.669**	9.315**	7.457*	3.146
	(5.599)	(4.806)	(4.291)	(3.936)	(3.431)
Elasticity	1.71	1.47	1.27	1.02	0.43
Obs.	1968	2290	2540	2722	3038
R-squared	0.00	0.01	0.01	0.01	0.00

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. Standard errors are in parentheses. Results are from non-parametric estimation (rectangular kernel). Elasticity is calculated as the percentage jump of the outcome at the border for a 1% reduction in wages.

Table 5. The impact of wage zones on uncovered sectors and overall private sector

	High-Differentials group, PS-Sample				
BANDWIDTH	20 km	25 km	30 km	35 km	45 km
			Panel A. Uncovered		
Employment growth	-3.071	-2.229	-2.473	-1.938	-3.542
	(4.527)	(3.992)	(3.612)	(3.349)	(2.949)
Elasticity	-0.42	-0.30	-0.33	-0.26	-0.48
Obs.	2094	2410	2646	2824	3138
R-squared	0.00	0.00	0.01	0.01	0.01
			Panel B. Private Sector		
Employment growth	1.320	1.679	3.628	3.649	1.580
	(3.728)	(3.300)	(2.978)	(2.757)	(2.442)
Elasticity	0.18	0.23	0.50	0.50	0.21
Obs.	2122	2430	2668	2858	3176
R-squared	0.00	0.00	0.00	0.00	0.00

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. Standard errors are in parentheses. Results are from non-parametric estimation (rectangular kernel). Elasticity is calculated as the percentage jump of the outcome at the border for a 1% reduction in wages.

Table 6. The impact of wage zones. Robustness

	Covered Sectors					Private Sector				
	High-Differentials group, PS-Sample					High-Differentials group, PS-Sample				
BANDWIDTH	20 km	25 km	30 km	35 km	45 km	20 km	25 km	30 km	35 km	45 km
					Panel A. Including covariates					
Employment growth	13.741***	10.498**	9.388**	7.665**	4.005	3.105	2.540	4.318	4.374	2.258
	(5.177)	(4.454)	(3.971)	(3.639)	(3.160)	(3.636)	(3.207)	(2.892)	(2.675)	(2.363)
Elasticity	1.90	1.44	1.28	1.05	0.55	0.43	0.35	0.59	0.60	0.31
Obs.	1968	2290	2540	2722	3038	2122	2430	2668	2858	3176
R-squared	0.16	0.15	0.16	0.16	0.16	0.06	0.06	0.06	0.06	0.07
					Panel B. Centre-North municipalities					
Employment growth	14.782**	15.508***	11.984**	10.518**	7.365*	6.256	5.136	4.918	5.511*	3.651
	(6.421)	(5.672)	(5.136)	(4.744)	(4.168)	(4.190)	(3.783)	(3.466)	(3.211)	(2.845)
Elasticity	2.00	2.09	1.59	1.40	0.97	0.85	0.69	0.66	0.74	0.48
Obs.	1490	1666	1804	1902	2064	1638	1812	1936	2040	2214
R-squared	0.00	0.01	0.00	0.00	0.01	0.00	0.00	0.00	0.01	0.01
					Panel C. Agriculture					
Employment growth	18.935*	16.297*	15.073*	10.577	3.757	11.701	11.004*	7.817	5.446	4.405
	(10.511)	(8.812)	(8.005)	(7.119)	(6.187)	(7.677)	(6.668)	(6.114)	(5.412)	(4.772)
Elasticity	2.98	2.54	2.35	1.64	0.58	1.85	1.73	1.23	0.85	0.68
Obs.	642	776	850	938	1044	612	718	772	848	946
R-squared	0.01	0.02	0.01	0.01	0.00	0.01	0.01	0.00	0.00	0.01

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. Standard errors are in parentheses. Results are from non-parametric estimation (rectangular kernel). Elasticity is calculated as the percentage jump of the outcome at the border for a 1% reduction in wages.

Table 6 (continued). The impact of wage zones. Robustness

	Covered Sectors					Private Sector				
	High-Differentials group, PS-Sample					High-Differentials group, PS-Sample				
BANDWIDTH	20 km	25 km	30 km	35 km	45 km	20 km	25 km	30 km	35 km	45 km
						Panel D. Largest cities (>1,000 inhabitants)				
Employment growth	11.482*	11.330**	10.973**	8.981**	3.509	0.363	0.594	2.602	2.116	0.151
	(6.271)	(5.325)	(4.752)	(4.335)	(3.783)	(4.156)	(3.654)	(3.289)	(3.049)	(2.721)
Elasticity	1.62	1.58	1.53	1.25	0.49	0.05	0.08	0.36	0.29	0.02
Obs.	1602	1886	2106	2276	2542	1644	1912	2130	2296	2548
R-squared	0.01	0.01	0.01	0.01	0.01	0.00	0.00	0.00	0.00	0.00
						Panel E. Larger safety band				
Employment growth	14.885**	13.255**	12.038**	11.535***	7.692**	2.348	1.638	3.173	3.404	1.455
	(6.164)	(5.296)	(4.699)	(4.316)	(3.777)	(4.140)	(3.671)	(3.308)	(3.068)	(2.734)
Elasticity	2.07	1.84	1.65	1.59	1.05	0.33	0.23	0.44	0.47	0.20
Obs.	1516	1764	1964	2108	2354	1624	1854	2038	2192	2430
R-squared	0.01	0.01	0.01	0.01	0.00	0.00	0.00	0.00	0.00	0.00
						Panel F. Larger treatment group				
Employment growth	10.626**	9.139**	6.880*	5.701	2.620	-0.079	0.265	1.775	1.767	0.869
	(4.865)	(4.235)	(3.801)	(3.507)	(3.077)	(3.235)	(2.887)	(2.618)	(2.431)	(2.164)
Elasticity	1.66	1.42	1.06	0.88	0.40	-0.01	0.04	0.27	0.27	0.13
Obs.	2464	2862	3166	3378	3736	2628	3016	3306	3530	3906
R-squared	0.00	0.01	0.01	0.01	0.00	0.00	0.00	0.00	0.00	0.00

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. Standard errors are in parentheses. Results are from non-parametric estimation (rectangular kernel). Elasticity is calculated as the percentage jump of the outcome at the border for a 1% reduction in wages.

Table 6 (continued). The impact of wage zones. Robustness

	Covered Sectors					Private Sector				
	High-Differentials group, PS-Sample					High-Differentials group, PS-Sample				
BANDWIDTH	20 km	25 km	30 km	35 km	45 km	20 km	25 km	30 km	35 km	45 km
	Panel G. 1951 local wage index									
Employment growth	12.247**	12.714***	9.964**	8.071**	4.681	2.563	3.278	4.792*	4.902*	3.426
	(5.078)	(4.405)	(3.953)	(3.654)	(3.196)	(3.398)	(3.011)	(2.711)	(2.527)	(2.241)
Elasticity	1.69	1.75	1.36	1.10	0.63	0.35	0.45	0.66	0.67	0.46
Obs.	2264	2656	2968	3170	3542	2430	2796	3094	3302	3664
R-squared	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	Panel H. Local wage index based on nationwide weighting for worker qualifications									
Employment growth	9.125*	9.077*	8.504**	6.910*	2.679	0.658	1.101	3.180	3.598	1.838
	(5.504)	(4.730)	(4.225)	(3.886)	(3.388)	(3.656)	(3.229)	(2.918)	(2.709)	(2.399)
Elasticity	1.30	1.29	1.20	0.98	0.38	0.09	0.16	0.45	0.51	0.26
Obs.	2034	2384	2648	2832	3152	2192	2526	2778	2970	3292
R-squared	0.00	0.01	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. Standard errors are in parentheses. Results are from non-parametric estimation (rectangular kernel). Elasticity is calculated as the percentage jump of the outcome at the border for a 1% reduction in wages.

Appendix. Local wage index by province

Agrigento	74	Forlì	87	Potenza	74
Alessandria	88	Frosinone	79	Ragusa	74
Ancona	85	Genoa	97	Ravenna	90
Aosta	94	Gorizia	92	Reggio Calabria	70
Arezzo	84	Grosseto	87	Reggio Emilia	89
Ascoli	80	Imperia	92	Rieti	80
Asti	86	L'Aquila	77	Rome	96
Avellino	77	La Spezia	90	Rovigo	86
Bari	83	Latina	74	Salerno	83
Belluno	89	Lecce	80	Sassari	77
Benevento	77	*Lecco*	97	*Savigliano*	85
Bergamo	91	Livorno	94	Savona	92
Biella	98	Lucca	87	Siena	85
Bologna	88	Macerata	77	Siracusa	75
Bolzano	94	Mantua	91	Sondrio	96
Brescia	92	Massa Carrara	94	Taranto	83
Brindisi	75	Matera	74	Teramo	77
Cagliari	79	Messina	79	Terni	83
Caltanissetta	72	Milan	100	Turin	98
Campobasso	77	Modena	88	Trento	91
Caserta	79	Naples	88	Treviso	85
Catania	79	Novara	94	Trieste	94
Catanzaro	76	Nuoro	77	Udine	87
Chieti	77	Padua	90	*Valdarno*	89
Como	97	Palermo	84	*Valsesia*	91
Cosenza	77	Parma	88	Varese	98
Crema	100	Pavia	94	Venice	92
Cremona	94	Perugia	82	*Verbania*	96
Cuneo	84	Pesaro	80	*Vercelli*	91
Enna	70	Pescara	82	*Verona*	91
Ferrara	86	Piacenza	88	*Vicenza*	90
Florence	97	Pisa	94	*Viterbo*	80
Foggia	77	Pistoia	87	*Voghera*	94

Notes: localities in italics are defined at a more detailed level of stratification than an administrative province. Wage zones are measured as percentage of that of Milan (the zone with the highest wage).