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ABSTRACT

Replacing Churches and Mason Lodges? Tax Exemptions and Rural Development*

This paper uses regression discontinuity design to provide quasi-experimental estimates of the impact of a tax credit program targeted at rural areas in France, including corporate and payroll tax exemptions. We find no impact of the program on total employment or the number of businesses, and no impact of the different program components on targeted subsets of firms. Comparison with a contemporaneous urban scheme suggests ways the incentives of the rural program could be targeted more effectively.

JEL Classification: J23, J32, H32

Keywords: tax exemptions, rural development, enterprise zones

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"Everybody knows the role of associations, in particular in rural areas, where they replace churches, mason lodges, pubs, the police, tax collectors, and Communist party sections. They are the only ones creating social links."

— a member of parliament, quoted in Daniel et al. (2009).

Governments across the world issue tax credits and subsidies to disadvantaged labor markets in an effort to reduce spatial inequalities. Examples of these schemes include so-called "EZ programs" like the state-level Enterprise Zone or the federal Empowerment Zone programs in the US. However, evidence on the effectiveness of EZs is mixed; moreover, these programs remain quite dissimilar in design, reflecting the lack of evidence on the optimal way to structure fiscal incentives in order to enhance local employment growth.

In this paper, we analyze a rural EZ program called *Zones de revitalisation rurale* or ZRR, a tax credit program targeted at areas of low population density in France. The ZRR program was launched in the mid 1990s and is still operating. In 2008, the program cost was above 400 million euros.[1] During the first phase of implementation (1996-2004), incentives were restrictively targeted toward two types of businesses: newly formed ones and small ones (with fewer than 50 employees) that increased their workforce. The new businesses were eligible for corporate and business tax cuts, and the small-but-growing firms benefited from temporary payroll tax exemptions. A turning point occurred in 2005 when, somewhat by surprise, a parliamentary amendment made the scheme more generous, granting a large, permanent payroll tax cuts for all employees of a specific group of employers, the so-called "public interest organizations" (PIOs). As made clear by the above quote, the member of parliament promoting the amendment had in mind small associations that maintain a form of social life in rural areas. An unexpected side-effect of these new exemptions was that it benefited some large employers in the health and education sectors (Daniel et al., 2009). Another 2005 change was an increase in corporate and business tax exemptions that remained restricted to new firms. As a result of this evolution, the ZRR program presents an opportunity to contrast different strategies to foster rural development through fiscal incentives.

[1]The order of magnitude is similar to what is found in several EZ programs in the US: $1.21 billion for the Federal Empowerment Zones, or $290 million in tax credits in California (2008). We discuss the generosity of the tax cuts below.

We analyze the ZRR program impacts between 1996 and 2004 (initial version of the program) and between 2005 and 2009 (second version). We take advantage of a discontinuous criterion in the rule that determined whether a given local jurisdiction was eligible for the program, and implement a fuzzy regression discontinuity design. We find no impact of the rural EZ program on local employment or the number of businesses. The impact is quite precisely estimated, and the results are robust to a variety of specification checks. Given the focus of the program on specific subsets of firms, we separately analyze employment and the number of plants in firms with fewer than 50 employees, newly created firms, and public interest organizations. Again, we do not find any significant effects and can in most cases reject large positive impacts. Overall, the program clearly failed to increase employment and net business creation in the eligible areas. To dig deeper into potential explanations for the lack of effect, we look at a contemporaneous French urban EZ program that has been extensively evaluated in the recent literature and found to yield quite large impacts. Comparing the different versions of the ZRR program to its urban counterpart reveals suggestive evidence that the ZRR program was probably ill-targeted, in that it provided insufficient incentives to new jobs and new businesses, excluded relocating businesses, and provided more generous incentives to a sector with fewer job creations and destructions (the public interest organizations).

Our paper fits into the growing literature evaluating the impact of EZ programs on employment and other outcomes (Ham et al. (2011) and Neumark and Kolko (2010) include recent reviews. See also Glaeser and Gottlieb (2008) and Greenstone and Looney (2010)). As detailed by Neumark and Kolko (2010), this literature faces difficult identification challenges. Because the designation of EZs is a highly political and endogenous process that may be related to unobserved trends in the outcomes, it is hard to find appropriate control groups for the enterprise zones. In addition, program effects may be confounded by other geographically targeted programs. Gathering the appropriate data is also a challenge: the boundaries of enterprise zones often do not follow standard geographic designations, making it hard to gather outcome data that match the treatment precisely. In this regard, our analysis takes advantage of a particularly favorable setting. The designation of the rural enterprise zones in the ZRR program followed a centralized process, using preexisting jurisdictions on which 1990 population census data were available, and applying a complex algorithm that involves a discontinuous criterion in terms of population density.

Specifically, the probability to be included in the program drops by about 60 percentage points for local jurisdictions whose 1990 population density is just above 31 inhabitants per square kilometer, as compared to those whose density is just below that mark. Such a selection process lends itself quite naturally to a fuzzy regression discontinuity design analysis. In this setting, we find robust evidence against large employment effects, in line with recent findings in other contexts by Neumark and Kolko (2010), Bondonio and Engberg (2000), and Bondonio and Greenbaum (2007).

Of course, this result only holds for a given program in a given setting. EZ programs vary a lot in terms of geographical coverage, incentives and eligibility rules. Interestingly, there are also recent evaluations which find positive impacts – in particular Busso and Kline (2008); Busso et al. (2013), Ham et al. (2011), and, in France, Givord et al. (2013) and Givord et al. (2012). As illustrated by Bondonio and Engberg (2000), a comparative perspective is needed to draw more general lessons on how to design the incentives. Our second contribution is to provide such a perspective, using comparisons within the ZRR program (between firms that benefit from different incentives) and comparisons across programs (between the ZRR program and its urban counterpart). Just like Bondonio and Engberg (2000), we find that no version of the ZRR program had a significant impact. But the comparison with the urban EZ program in France suggests that a mix of more generous incentives and different targeting techniques might lead to more positive results.

A last contribution of our analysis is to provide evidence on rural EZs, which have been evaluated much less frequently than urban ones (Wortman, 1996). Our results are consistent with previous evaluations of the French rural EZ program (in its 1996-2004 version) that use panel data and matching approaches (Lofredi and Schmitt, 2006; Cardot et al., 2012). We substantially extend the regression discontinuity design results by Lorenceau (2009) by using more reliable administrative data, considering different target groups of firms, and analyzing the altered 2005 scheme. The absence of impact is also consistent with Devereux et al. (2007) in finding that firms are less responsive to government subsidies in areas where there are fewer existing plants in their industry. It is also arguably consistent with results by Briant et al. (2012) on the impact of geographical constraints on the effectiveness of local tax breaks.

The paper proceeds as follows: in Section 1, we describe the French rural EZ program.

In Section 2 we describe our econometric approach and the data. In Section 3 we present our estimates of the program's overall impact and the impact of its components. Section 4 provides a comparison with an urban EZ program in place in France at the same time as the ZRR program, and discusses hypotheses to interpret the impact gap. The last section concludes.

1 The French rural EZ tax exemptions

The French Rural Enterprise Zone program (*Zones de revitalisation rurale* or ZRR) was created by law in 1995.[2] A prominent theme of the 1995 presidential campaign had been the issue of "social divide" (*fracture sociale*), which referred to the economic and social distress of French suburbs but also to other types of spatial and social disparities. The debate tended to legitimate "positive discrimination" in favor of distressed territories. After the new president was elected, the government quickly passed laws providing tax exemptions in favor of rural areas (the ZRR program on which we focus here) and urban areas (the *Zones franches urbaines* or ZFU, an urban enterprise zone scheme that provides a benchmark to our evaluation).

1.1 Zone definition

In France, zoning policies originated with the definition of large zones by the European Commission in the late 1980s. It was immediately followed at the national level by the designation of rural territories to be developed in priority (TRDP). The corresponding zones were not defined according to strict rules. The ZRR program differs in that respect, as the eligibility of each local jurisdiction follows from a complex yet mechanical rule.

First, a demographic criterion requires that a municipality[3] be either part of a *canton*[4]

[2]Eligible zones and the details of implementation were only defined in February 1996, so that we take 1996 as a starting point for the evaluation. Starting the evaluation in 1995 makes little difference.

[3]The municipality (*commune*) is the smallest French administrative subdivision. On 1 January 2010, there were 36,570 municipalities in Metropolitan France.

[4]A canton is a territorial subdivision of a district and in most cases a grouping of municipalities. There are about 3,600 cantons in metropolitan France. In some cases, cantons do not respect communal boundaries: the most populated municipalities span over several cantons.

with a population density that is less than 31 inhabitants per square kilometer or belongs to a district[5] with a population density under 33 inhabitants per square kilometer. Population densities are measured in the 1990 population census.

Second, if a municipality is part of a canton or a district with a population density lower than five inhabitants per square kilometer, it is automatically included in the rural EZ. Otherwise, one of the following criteria must be verified. The population or the labor force (at the canton or district level) must have diminished or the share of the population that works in the agricultural sector must be twice the national average (8.2%) or more. Finally, an institutional constraint also applies: to be included, the municipality should belong to a TRDP.

These criteria have two important consequences. First, even though the algorithm used to determine the eligibility of a given municipality is rather complex, it turns out that the criterion bearing on the population density of the canton plays a predominant role. This is illustrated by Figure 1, which displays the probability that a given canton benefits from the rural EZ program as a function of its 1990 population density. The probability is almost cut by three quarters once the threshold of 31 inhabitants per square kilometer is reached. This feature is the basis of our empirical strategy to identify the impact of the program, as detailed below.[6] Second, the rural enterprise zones turn out to cover a sizable part of the French territory (38% in terms of area) – see Figure 2. Of course, the share of the population covered is much smaller: about 8%.

From 1996 to 2004, the zone definition remained unchanged and the criteria were computed based on the 1982 and 1990 population censuses. In 2005 a new law was passed and the zoning slightly modified in two ways. First, the reference to the TRDP zoning was done away with and a municipality was now required to join an inter-communal establishment (EPCI), a group of municipalities that manage local public services jointly.[7] Second, the 1990 and 1999 censuses became the new reference for the computation of the criteria. In particular, the new criterion stated that canton population density as measured

[5]Districts (*arrondissements*) are administrative subdivisions of the 96 French *départements* and a group of cantons that respects the boundaries of municipalities.

[6]We could not get access to the data that was used in 1995 to assess the other criteria determining eligibility. Although these criteria display discontinuities that could have been useful in the analysis, they only play a secondary role.

[7]The declared objective was not to modify the zoning but rather to provide incentives to the municipalities to organize in EPCIs.

during the 1999 population census should be below 31 inhabitants per square kilometer. However, the new criteria were implemented in an asymmetric way: new cantons were included in the zoning (if they fulfilled the eligibility criteria using the updated measures) but no cantons were withdrawn (even if they no longer fulfilled the eligibility criteria). As a consequence, both population density measured in 1990 and in 1999 determine the inclusion in the program, in a discontinuous manner (see Supplementary Figures 10 and 11).

Overall, even though the rural EZ zoning expanded somewhat after 2005 (Figure 2), it remained rather stable over the period we analyze. The next section details the more significant variations in the targets and the generosity of the tax exemptions.

1.2 Main features of tax exemptions

Payroll tax exemptions

As detailed in Table 1, the program witnessed two main phases: from 1996 to 2004; and from 2005 onward.[8] Exemptions are more generous in the second phase. Consider payroll tax exemptions first (upper panel). From 1996 to 2004, the payroll tax exemptions remain concentrated on a restricted number of jobs: only firms with fewer than 50 employees that have not laid off any worker over the previous 12 months are eligible. The measure is further restricted to the manufacturing, retail, and craft industries, as well as to agriculture. Employers are fully exempted from payroll contributions for the fraction of wages below 1.5 times the minimum wage, during the first year the worker is employed at the firm. Employer contributions amount to 30.3% of the gross wage; at its maximum (for workers earning less than 1.5 times the minimum wage), the implied labor cost cut is therefore about 23% ($= 30.3/(100 + 30.3)$). There is no additional payroll tax exemption for the fraction of the wage exceeding 1.5 times the minimum wage, so that the tax cut is lower (in percentage term) for high-wage workers. The rural EZ payroll tax exemption partly substitutes for another national program of payroll tax exemption targeted at low-wage workers. As a result, the net impact of the rural EZ exemption is less than 30.3% for

[8]The last column considers a contemporaneous urban enterprise zone program and will be commented upon below.

low-wage workers. This is illustrated by Figure 3, which shows the evolution of the net labor cost cut implied by the rural EZ program over time, at different wage levels. In particular, workers paid at the minimum wage were already eligible for a 18.2% employer payroll tax exemption when the program started; for those workers, the labor cost cut is only 9.3% ($= 12.1/(100 + 30.3)$) of the gross wage. The cut is larger above the minimum wage (as preexisting payroll tax exemptions are less generous at these levels). It reaches its maximum (23%) at 1.3 to 1.5 times the minimum wage: at that level there are no national payroll tax exemptions, and the rural EZ program entails full exemption from employer payroll tax contributions. Figure 3 also illustrates the fact that the national program of payroll tax exemptions for low-wage workers becomes increasingly generous over years, so that the rural EZ program makes less difference over time.

A law passed in 2005 introduced a substantially more generous scheme for a subgroup of employers located in rural EZs, known as "public interest organizations" (PIOs).[9] The fiscal administration bestows PIO status following two criteria: the organization must provide a public service and be non-profit.[10] The category includes some NGOs, but also non-profit hospitals or clinics. As employers, public interest organizations (whatever their size) became eligible for the same payroll tax exemption as above, but for all their employees (not only new hires) and for the full length of their contract. As a consequence of the new scheme, the budgetary cost of rural EZ payroll tax exemptions increased sharply (see below). In an attempt to curb these costs, the fiscal administration amended the scheme in 2007, closing it to workers hired after November of that year.[11]

Corporate and business tax exemptions

The second main measure consists of corporate and business tax exemptions for newly created firms (see lower panels of Table 1). Until 2004, there was full exemption over the first two years, and partial exemption over the next three. The measure was substantially extended in 2005 (with a retroactive effect on firms created after 1 January 2004): full

[9]The measures for firms with less than 50 employees were maintained unchanged after 2005 if they did not qualify as public interest organizations.

[10]The label is used to grant tax incentives for charitable giving: individual donors to PIOs are eligible for income tax rebates.

[11]For new hires, PIOs may be eligible for the initial rural EZ payroll tax exemption scheme if they have fewer than 50 employees.

exemption is now granted for the first four years; the exemption then progressively fades out over the next ten.

Assessing the tax cuts' generosity

One important question is whether or not the different tax benefits constitute strong financial incentives. As mentioned in the introduction, the annual budgetary cost of the ZRR program in its second phase (above 400 million euros) is in line with EZ programs in the US. The budgetary cost before 2005 is smaller – Lorenceau (2009) provides an estimate of 100 million euros. The contemporaneous French urban EZ program (*Zones franches urbaines*) constitutes a useful point of comparison. In 2008, payroll tax exemptions amounted to 315 million euros in the urban EZ program (for about 68,000 jobs in 18,000 plants), compared to 200 million euros for PIOs in the ZRR program (for about 38,500 jobs in 3,300 plants) and 38 million euros for other firms in the ZRR program (for about 9,000 jobs in 6,000 plants). Again, the orders of magnitude are quite similar.

This first measure of the program's generosity is, however, partly endogenous, as it depends on the firms' responses to the incentives. A second measure that is not subject to this problem consists of computing the transfers to firms that the program implies based on the situation before program (in 1995), i.e. not taking into account any response by the firms. This is done in Supplementary Table 10, which computes statistics on corporate and business taxes paid by firms in the ZRR zones before the program developed. A significant share does not pay corporate taxes; the fraction that does not pay any business tax is lower. The median amounts paid may seem quite modest; this reflects the fact that many firms in the ZRR are small. Note however that these statistics are for all firms and not only new ones (which are not identified in the fiscal data). Similarly, data limitations prevent us from precisely computing the amount of payroll tax exemptions that would have been granted to firms as of 1995.

Firms' take-up behavior may also provide an indication of the program's generosity, as firms should only apply if the benefits outweigh the associated administrative costs. Unfortunately, no data is available to measure take-up at the firm level (this would require to link each firm's eligibility to the tax benefits it received). We can however use aggregate

data on the number of recipients. Excluding PIOs, an average of 11,000 jobs benefited from the ZRR payroll tax exemptions each year between 1997 and 2007. From the data detailed below, we estimate that on average 18,000 jobs were eligible for these exemptions yearly.[12] The resulting estimate of take-up is around 60%. This suggests that a minority of firms did not find it profitable to apply for benefits, perhaps because the cuts coming in addition to the national exemption schemes were limited in their case. Still, for a majority of firms, it seems that the expected benefits outweighed the costs.

A final way to assess the strength of the incentives of the ZRR program is to use estimates from other evaluations. In particular, payroll tax exemptions have been thoroughly evaluated in the French context, using a nationwide scheme that was initiated in 1993. These payroll tax exemptions only concerned low-wage workers (below 1.3 times the minimum wage) and the reduction in employer payroll tax contributions was only partial, but it was permanent. Crépon and Desplatz (2001) find large effects of the measure: a 1% reduction in labor cost is estimated to increase employment by 3.4% in the service sector and by 2.6% in manufacturing.

The four approaches complement each other, suggesting that in its first phase the program was probably somewhat less generous than urban EZ programs in France and elsewhere, but in its second phase it was roughly as generous. However, the assessment of the incentives in the first phase depends on the firms' responsiveness to subsidies for low-wage workers. Existing estimates for the period considered suggest that this responsiveness was quite high. One may therefore expect quite large effects, as policymakers did when they introduced the program.

[12]More precisely, we compute the sum of positive job flows in firms with fewer that 50 employees, in eligible industries and located in program areas. We further restrict the sample to firms that had positive or zero job flows in the previous year, since firms that have laid off workers in the previous year are not eligible. Not imposing this last restriction reduces the estimated take-up rate by about 10 percentage points.

2 Econometric approach and data

2.1 Data

We use two administrative datasets to obtain rich information on firm demography (number of plants) as well as employment.

The French business register (SIRENE) follows all French firms and plants. The information for each plant covers: the location (at least the postal code which can be linked to the canton code), the firm's legal status, the sector of activity and the firm's year of creation.[13]

The second data source (DADS) is an exhaustive administrative employee database with information on wages and hours of work. It provides yearly employment data for each plant in the private sector. Employment in this database is measured in various ways: full-time equivalents over a year, number of employees on the payrolls at one point or another over the year, number of employees as of 1 January. We use the latter measure which is available and consistent across years; robustness checks with other measures show no significant differences.

SIRENE and DADS data are available from 1995 to 2009, that is more than a year before the introduction of the tax exemptions, and twelve years after, which allows long run evaluation.

For this evaluation, the data have been aggregated at the canton level, on different subpopulations of firms. This allows us to look at the impact of the different components of the program, which are targeted at different groups of firms. Note that the tax exemptions apply to jobs located in plants that are in eligible areas, if the firm owning the plant satisfies certain conditions. Plant-level data is therefore matched with the French business register of firms (SIRENE). This allows us to define subsamples of jobs/plants in new firms (in existence less than three years) and in small firms (fewer than 50 employees). "Total

[13]The register also keeps track of plants and firms relocation (the number of new plants created from 1 January to 31 December of each year). It also determines whether a new plant location is an actual creation or relocation. Indeed, each plant is identified by a registration number. In case of relocation of the plant, this number changes but the file corresponding to the flow of plants links the new and old registration numbers.

employment" is computed by adding wage employment (from DADS) to the number of self-employed (from SIRENE).

As described in Section 1, the definition of PIOs is not immediate and this status is granted by the fiscal administration; no register of PIOs is available. We use the administrative forms filled out by PIOs when recruiting new workers to create a proxy for PIO status combining the firm's legal status (at the four-digit level) and the industry (at the four-digit level), that can be used with our main data sources. With our proxy, 98% of hires for which the employer fills out the administrative forms are classified as PIOs: we do not miss many PIOs. However, our approach may lead us to classify as PIOs some firms that are not recognized as such by the fiscal administration, if they make few hirings but share the same legal status and industry as other organizations that appear as PIOs through the forms. Supplementary Table 11 shows the distribution of wage employment across sectors of activity in ZRR cantons, as of 2005 (column 1) and, for each sector of activity, the share of wage earners working for PIOs. Most PIOs contributing to local employment are in the health and education sectors. This is consistent with statistics provided by Daniel et al. (2009), using a different source.

Descriptive statistics on the different samples used in the analysis are displayed in Table 2. The four panels respectively consider all firms (including the self-employed), firms with 1 to 50 employees, new firms (less than three years old), and public interest organizations. Firms with fewer than 50 workers constitute about 40% of local employment, while public interest organizations account for about 15 to 20% of jobs in the sample. As discussed above, this is likely to slightly overestimate the share of PIOs in employment.[14]

2.2 Econometric model

We take advantage of the discontinuous criterion introduced by the 1995 law, which targets areas (cantons) with a 1990 population density below 31 per square kilometer, in order to assess the impact of the rural EZ program.[15] As seen above, the probability that a

[14]The order of magnitude is consistent with existing estimates of the share of the "social and solidarity-based economy" (*économie sociale et solidaire*), a different but somewhat related concept gathering associations, cooperatives, mutual insurance companies and foundations, estimated to make up about 10% of employment in France.

[15]To simplify notations, the discussion is restricted to the program as of 1996 to 2004, when only population density measured in 1990 entered the eligibility rule. The approach can be directly extended

given canton benefits from the rural EZ drops sharply at the 31 threshold. This suggests adopting a fuzzy regression discontinuity design approach (fuzzy RDD). Denote by *dens* the canton's population density in 1990, and y_0 the potential outcome in the absence of program (for instance, employment growth if the canton was not to be eligible for the program). Under the assumption that $E[y_0|dens]$ is a continuous function of *dens*, the fuzzy RDD identifies a local average treatment effect (LATE) of the program (Hahn et al., 2001).

A well-known threat to identification is the risk that the discontinuous eligibility rule has been manipulated. In our setting, the forcing variable is predetermined: population density is measured at the 1990 population census, i.e. long before the rule is known. This implies that areas could not modify their value of the forcing variable. However, another type of possible manipulation lies in the choice of the threshold itself. 31 is not a random number: it could have been chosen to include some areas or to exclude others, potentially implying that areas on both sides of the threshold are not comparable. The interviews we conducted with civil servants in charge of defining the eligibility thresholds confirm that the process was iterative, and that different thresholds were tried. However, they report that the goal was to satisfy the government's budget constraint and not to select some areas rather than others. This is plausible given the centralized administrative process at play, in which local politicians could not easily interfere, while the ministry of finance was the real decision maker. If this description is correct, the 31 threshold can be thought of as arbitrary with regard to outcomes of interest such as local employment growth. The empirical evidence is consistent with that view. We do not find any discontinuity at the 31 threshold in outcome variables measured before 1995 – see Supplementary Figures 12-15, which show no discontinuity at the threshold in terms of the share of older people in 1990, income in 1990, employment growth between 1982 and 1990, or unemployment in 1990. Moreover, Figure 4 shows no discontinuity in the density of the forcing variable at the threshold. This is consistent with the fact that areas had no way to manipulate their value of the forcing variable (McCrary, 2008).

Following the literature, we estimate the LATE of the rural EZ program by two-stage least squares (2SLS) instrumenting program eligibility by $\mathbf{1}_{dens \leq 31}$, an indicator variable

to the program after 2005 by considering two forcing variables: the 1990 population density (denoted *dens*) and the 1999 population density (denoted *dens*99). See Section 3.1.

equal to 1 if the area satisfies the population density criterion. We face the standard trade-off between bias and variance (Imbens and Lemieux, 2008), depending on the size of the window around the threshold and the flexibility when controlling for continuous effects of the forcing variable. We present results for two specifications. Considering a narrow window for the forcing variable (population density between 21 and 41 per square kilometer), we estimate

$$g = a + bEZ + x\beta + \epsilon, \tag{1}$$

by 2SLS, instrumenting EZ (the fact to be an enterprise zone) with $\mathbf{1}_{dens \leq 31}$. g stands for alternative outcome measures: the growth rate in total employment, or in the number of plants, over alternative periods.[16] x denotes control variables that can be used to increase precision.

Alternatively, considering a larger estimation window (population density between 11 and 51 per square kilometer), we estimate

$$g = a' + b'EZ + c'\mathbf{1}_{dens \leq 31} \times (dens - 31) + d'\mathbf{1}_{dens > 31} \times (dens - 31) + x\beta' + \epsilon', \tag{2}$$

by 2SLS, instrumenting EZ with $\mathbf{1}_{dens \leq 31}$. This regression allows for a continuous linear effect of the forcing variable on each side of the threshold.

On the smaller window, the identifying assumption amounts to that of difference-in-difference approaches: employment is assumed to follow parallel trends for areas in the $]21; 31]$ and $]31; 41]$ brackets. The next section provides graphical evidence that this assumption is not unreasonable. We show standard RDD graphs that allow assessing the sensitivity of the results to choices to specification choices.

[16] In all the regressions, data are aggregated at the canton level (over various subsets of employers) so units of observation are always cantons.

3 Impact of the rural EZ program and its components

3.1 First stage

Table 3 displays the estimated impact for a canton of being below the population density threshold (our instrument) on the probability to be included in the rural EZ program between 1996 and 2004 (Panel A) and between 2005 and 2009 (Panel B). We consider the first-stage specifications corresponding to models (1) and (2). In the first two columns (first stage of model (1)), we consider a narrow window around the threshold and include only cantons with a density between 21 and 41 inhabitants per square kilometer. In column 1, the instrument is the only explanatory variable. Column 2 includes geographical controls (county (*département*) dummies and controls for the initial industry mix[17]). In the next two columns (corresponding to model (2)), we control for linear effects of the population density on each side of the threshold and include all cantons with a density between 11 and 51 inhabitants per square kilometer.

Inclusion in the program between 1996 and 2004 depends on population density measured by the 1990 population census. As expected from Figure 1, the probability jumps significantly around the 31 threshold (by more than 60 percentage points, highly significant). This remains true when controlling for linear effects of population density on both sides of the threshold. The instrument (having a population density below 31) is not weak, as evidenced by the large F-statistics.

Inclusion in the program after 2005 depends both on the population density measured by the 1999 census, and on previous ZRR status (as no canton exited the program), hence, on 1990 population density (see Supplementary Figures 10 and 11). We therefore expand the first-stage equation explaining treatment status after 2005 to include two instruments ($\mathbf{1}_{dens\leq 31}$ and $\mathbf{1}_{dens99\leq 31}$), indicating that the canton's density was below 31 at the 1990 and 1999 population censuses, respectively. Accordingly, in the first-stage equation associated with model (2), we include linear effects (on each side of the threshold) of the two forcing

[17]Specifically, we measure the share of 36 different industries in each canton in 1995 and introduce the corresponding variables as controls.

variables, *dens* and *dens*99. Even though *dens* and *dens*99 are of course correlated, results in Panel B show that the two instruments separately contribute to explaining treatment status. Importantly, their effects are jointly strongly significant (F-statistics above 100 in all specifications).

3.2 Overall impact on employment and number of businesses

We now turn to 2SLS estimates of the impact of being part of a rural EZ on local employment and the number of businesses. Table 4 provides estimates corresponding to models (1) and (2). The table is made of groups of four columns corresponding to the four columns of Table 3. We consider impacts on average annual growth rates in the short run (1996-98) and in the longer run (1996-2004).[18]

The upper panel shows results on the annual growth rates of local employment. Whatever the specification or the time horizon, the impact of the rural EZ program on local employment is never statistically significant. Point estimates are small (they tend to be positive in the short run, and negative in the long run). These estimates allow to reject large positive impacts on employment. Depending on whether we prefer model (1) or (2), we can reject the null hypothesis that the program increased local employment growth by more than 0.4 (respectively, 0.8) percentage point per year between 1996 and 2004. Similarly, estimated effects on the number of businesses are small and statistically non-significant. As discussed below, the literature finds much larger positive effects in some EZ programs (and no effect in other cases). Rejecting effects above one percentage point is therefore quite informative.

We perform a variety of robustness and specification tests. First, a usual concern in RDD settings is that results may be driven by the choice of the estimation window around the threshold or the functional form used to control for continuous effects of the forcing variable. Figure 5 plots the annual growth of local employment between 1996-2004 against the forcing variable over ranges of two inhabitants per square kilometer $(]11, 13],]13, 15], ...)$. Following Lee (2008), we fit a polynomial of second order on each side of the threshold, represented by a solid line. There is no indication of a discontinuous

[18]We also provide a year-by-year analysis in Figures 7 and 8, and analyze the impact of the second version of the ZRR program, after 2005, in the next two subsections.

jump in employment around the 31 threshold, and this does not seem to be driven by the window of observation. Figure 6 similarly gives no sign of discontinuity for the number of businesses.

Second, we check that the results are not sensitive to the time horizon considered. Our yearly data allows us to measure impacts year after year until 2004. Figures 7 and 8 display cumulative effects of the program estimated with the same specification as in column 6 of Table 4. Impacts are small and statistically non-significant, irrespective of the endpoint. Also, note that there is no statistically significant effect on employment growth and job creation between January 1995 and January 1996, *before* the program was implemented. This can be viewed as a placebo test, where the absence of effect is consistent with our identification strategy.

Third, we check that effects are not biased due to spatial externalities. Indeed, untreated areas could be negatively affected by the rural EZ program if there are displacement effects toward treated areas. They could also be positively affected in the longer run if employment growth in neighboring rural EZs generate positive externalities due to agglomeration economies. The econometric analysis can be easily extended to control for such externalities, by estimating:

$$g = a + bEZ + cN + x\beta + \epsilon, \tag{3}$$

where EZ denotes the treatment status of a canton and N measures the treatment status of its neighbors. We consider two definitions for N: the share of adjacent cantons that are included in the ZRR program, and an indicator variable for having at least one neighboring canton treated. Just like EZ, N is likely to be endogenous. However, the RDD approach can be extended: we instrument N by an indicator variable equal to 1 if at least one neighboring canton has a population density below 31, controlling for the share of adjacent cantons in the 21-41 range (respectively, for the fact that at least one neighboring canton has a population density between 21 and 41). Results displayed in Supplementary Table 12 still show no direct effect, and no spillover effects.

Next, Supplementary Table 13 displays IV quantile treatment effects (QTEs), following Abadie et al. (2002). This serves a double purpose: it allows us to check the robustness

of the 2SLS estimates to outliers and to explore whether program impacts are larger in faster-growing areas.[19] Irrespective of the quantile (Q25, Q50 or Q75), the estimated treatment effects are close to the equivalent 2SLS estimates in Table 4 (columns 2 and 6). This shows that outliers are not driving the initial results, and brings no evidence of treatment effect heterogeneity based on the areas' growth.

Last, we consider results by industry. Program impacts remain small and statistically non-significant across the six sectors that we consider (results available upon request).

In sum, the ZRR program failed to increase total employment and the number of businesses in eligible areas.[20] Yet, this lack of effect may hide positive impacts on some specific segments. We might not be able to detect these effects using aggregate data, due to the lack of statistical power. Furthermore, if there are displacement effects, positive impacts on firms that were targeted by the program may be at the expense of other firms, so that the lack of aggregate impact on employment may hide opposite effects on different segments of the local economy. As discussed above, the ZRR program has changed targets over its lifetime. Initially designed to support business creation as well as job creation in small firms, it shifted its support to existing firms and associations of a specific type, the "public interest organizations". In the next two subsections, we turn to the analysis of program impacts on these different targets.

3.3 Impact of support to business creation and job creation in small firms

As discussed above, small and new firms were the initial target of the ZRR program: before 2005, only firms with fewer than 50 employees were eligible for the temporary payroll tax exemptions and only new firms were eligible for the corporate tax exemptions. We therefore test whether the program had positive impacts on employment and business creation in these specific segments of the economy.

[19] Note that the latter interpretations of the IV QTEs assumes that the program preserves the ranks of the different cantons in terms of outcomes.

[20] As noted above, new cantons were added in 2005 and the tax exemptions were modified. However, we do not find any impact if we continue the analysis from 1996 to 2005 and later years, using eligibility to the program as of 1995 as the variable of interest.

Table 5 replicates the same analysis as Table 4, restricting the sample to firms with fewer than 50 employees, in industries eligible for the payroll tax exemptions (agriculture, manufacturing, retail and craft industry). Similar to Table 4, we do not find that the program increased creation of employment or business. In the long run (1996-2004), our preferred estimate of the employment impact is precisely estimated at 0 (columns 6 and 8).

Our data does not allow us to consider new firms separately in the mid 1990s, so as not to analyze the impact of the first version of the ZRR program (around 1996) on new firms. We can however identify new firms around 2005, making it possible to analyze the impact of the increase in corporate and business tax exemptions that occurred at that time. Results are displayed in Table 6. Compared to previous tables, the treatment variable is updated to take into account the fact that new cantons were added in 2005 (while cantons eligible in 1996 were maintained in the program).[21] Outcomes are measured in terms of annual growth rates between January 2005 (just before the corporate tax exemptions were made more generous) and January 2007 or 2009. The sample is restricted to new firms (less than three years old). The table shows no effect of the more generous tax exemptions on new firms. Employment did not grow faster in such firms in the program zones, and the number of new businesses did not increase faster. Note however that the estimates are not very precise, which reflects the fact that employment in new firms is volatile (the standard deviation of the annual growth rate of employment in these firms is more than .3[22]). Correspondingly, we cannot reject that the program had positive effects on new firms in the short run.[23] However, we can reject that this translated into a large positive impact on total employment in ZRR cantons, as new firms only account for a small part of local employment.[24]

Overall, Tables 5 and 6 reinforce the diagnostic based on Table 4: temporary payroll tax and corporate tax exemptions did not boost employment or net business creation in

[21]The set of instruments and controls is expanded as explained in section 3.1.

[22]Recall that outcomes are measured at the canton level. Here, growth rates are computed on the number of jobs in new firms, by cantons.

[23]The large standard deviation of the outcome suggests that some outliers may be driving the results. In order to check this, we ran quantile regressions in the same way as in Supplementary Table 13. Results in Supplementary Table 14 are stable across quantiles, and not very far from the 2SLS results.

[24]Between 2005 and 2007, if we replicate the estimations of column 4 in Table 6 adding employment in firms older than three years, the estimated impact of the ZRR program on local employment growth is 0.016 (with 0.011 standard errors); the corresponding estimate for number of businesses is -0.003 (with 0.005 standard errors).

firms and sectors that were specifically targeted. This lack of impact may justify the introduction of an alternative approach, providing permanent payroll tax exemptions to a specific sector. We now turn to this second aspect of the ZRR program.

3.4 Impact of support to public interest organizations

As detailed above, public interest organizations located in eligible areas became eligible in 2005 for permanent payroll tax exemptions for all their employees, including those hired prior to that date. The take-up of the measure was high: many organizations, associations and firms that were not registered as public interest organizations applied to the fiscal administration in order to benefit from the measure. As a result, the cost of the ZRR program increased sharply. However, as exemptions were not conditioned by net job creations, this apparent success may be due to a windfall effect, benefiting firms and jobs that would have existed anyway.

Table 7 displays the estimated impacts of the permanent payroll tax exemptions on the employment in and the number of PIOs, measured as explained in Section 2. The treatment variables, the outcome variables and the model specifications are the same as in Table 6. We do not find any evidence that the new measure increased the number of public interest organizations or employment in them. This strongly suggests that while eligible associations applied for the tax exemption, the measure induced no change in job creations and destructions in the sector.

To sum up, we do not find any significant impact of the ZRR program on local employment and the number of businesses, despite the variety of incentives that were provided across types of firms and over time. Results are robust to various specification checks and sufficiently precise to rule out sizable positive effects.

4 Comparing the ZRR program impacts to its urban counterpart

In this section, we discuss possible explanations for the lack of effects of the ZRR program. To do so, we build on a comparison with a contemporaneous urban EZ program called "*Zones franches urbaines*" (ZFU, henceforth "urban EZ program") that shares the same goals and uses the same type of fiscal incentives. As explained above, the rural and urban programs were started in the mid 1990s.[25] As shown in Table 1, the urban program sets very generous incentives for all businesses (businesses already operating in an eligible zone, newly created businesses, and businesses relocating to the zone). In that sense, it is similar to the second, more generous version of the ZRR programs. However, a key difference is that it is not restricted to a specific industry or group of employers.

The upper panels of Table 8 compare the estimated effects of the two programs on local employment and the number of businesses. Estimates for the ZRR program are taken from estimates of model (1) in Table 4. Estimates for the urban EZ program are based on Givord et al. (2012). The urban EZ program has large effects on local employment and the number of businesses (note that the effects on annual growth rates cumulate over eight years, which implies that total employment more than doubles as a result of the program). This illustrates the fact that the lack of impact of the ZRR program is not simply due to insufficient statistical power: if the effects of the ZRR program had been comparable to those of the urban EZ program, they would have been easily detected in our estimations. In what follows, we build on the comparison between the urban EZ program and the ZRR program to understand why the latter was not more effective. We consider the two versions of the ZRR program (1996 and 2005) in turn.[26]

[25] A second, lighter urban EZ program called "*Zones de rénovation urbaine*" (ZRU) started at the same time. We put it aside in this comparison. Its estimated effects are small and lie in-between the ZRR and ZFU estimated impacts. But their estimation rests on stronger identifying assumptions and the estimates are not very precise (Givord et al., 2012).

[26] A second wave of the urban EZ program started in 2004. Its estimated effects on employment and the number of businesses are also positive and statistically significant, yet smaller than the estimated effects of the first urban EZ program that we use as a benchmark (Givord et al., 2013). The reasons for the differential impact of the two urban EZ programs are not clear. We prefer to perform a comparison of the ZRR program with the first urban EZ program because they were in place at the same time. Moreover, despite their different magnitudes, the two urban EZ programs have *qualitatively* similar impacts: in particular, as detailed below, impacts are concentrated in new and relocating businesses, and are associated with large displacement effects.

4.1 Why did the initial ZRR program fail to increase employment?

We first consider the ZRR program as effective between 1996 and 2004. Both the ZRR and the urban EZ programs have incentives that aim at increasing the number of local businesses, but the urban EZ program succeeds while the rural EZ program fails. Why?

A first hypothesis is that this impact gap may be due to the fact that business relocations are eligible for tax cuts under the urban EZ program, but not under the rural one. This explanation is directly testable, as the data allows us to separate business creations from business relocations. Panel D in Table 8 using estimates from Givord et al. (2012) shows a large, significant impact of the urban EZ program on the number of business relocations. By contrast, the impact of the rural EZ program on such relocations is small and statistically non-significant. To assess how much of the impact gap would be closed if business relocations were not eligible for tax cuts in the urban EZ program, one needs to take into account the fact that there are on average eight business relocations for 34 business creations in a given urban enterprise zone. This suggests that about a quarter ($= 8 \times .405/(8 \times .405 + 34 \times .25)$) of the impact on the stock of businesses in the urban program is due to business relocations.[27] Consequently, the exclusion of business relocations only partly accounts for the lack of impact of the ZRR program.

A second hypothesis is that the impact gap may be due to a difference in the size of the zones: urban EZs are distant from each other and cover small geographical areas, and are therefore in a better position to attract activities from neighboring, untreated areas. Indeed, existing evaluations of the urban EZ program (Givord et al., 2012, 2013) show that it had sizable displacement effects. These are more likely to occur in a setting where a small treated area is surrounded by non-treated areas, because the small treated area then benefits from a large "caption area". In that regard, the urban EZ program stands at a strong advantage. Between 1997 and 2003, there were 45 urban EZs that constitute small islands scattered all over France. By contrast, as shown by Figure 2, the rural EZ program covers more than a third of French territory in broad contiguous areas. The possibility for

[27] The computation assumes that destruction rates are the same in newly created and in relocating businesses.

a given canton to act as a magnet attracting activities from neighboring non-treated areas is therefore limited. Cantons at the border of a zone, and isolated ZRR cantons, may be in a different position, though. We therefore test whether the impact of the ZRR program depends on whether or not neighboring cantons are treated. Specifically, we estimate the following extension of model (1):

$$g = a + bEZ + cN + dN \times EZ + x\beta + \epsilon, \tag{4}$$

where EZ denotes the canton's own treatment status (ZRR or not) while N characterizes the treatment status of neighboring cantons. We consider two definitions of N: the share of adjacent cantons that are in the ZRR program ($Share$); and an indicator variable equal to 1 if no adjacent canton is in the ZRR program ($Isolated$). Of the 784 cantons in the 21 to 41 density range, 506 (64%) have less than half of their adjacent cantons in the ZRR program. 131 cantons (17%) have no treated neighbor, and among those 131, 19 (2.4% in 784, or 5% in 409 ZRR cantons) are themselves treated. $Share$ and $Isolated$ are endogenous; they are therefore instrumented by variables capturing whether neighboring cantons are below the threshold of 31 inhabitants per square kilometer, controlling for the fact that they are in the [21; 41] density bracket.[28]

Results are given in Table 9. The upper panel suggests that in the long run (1996-2004), the program has a positive differential impact on the growth of local employment on isolated ZRR cantons (the growth rate increases by 4.1 percentage points, significant at the 5% level). As before, there is no impact on other ZRR cantons, and no spillover effects. However, the results should not be overinterpreted. First, if we consider $Share$ rather than $Isolated$ as an interaction variable, the estimates become non-significant. Second, we do not find any positive impact on the number of businesses, which is at odds with the interpretation according to which isolated ZRR cantons are able to act like magnets attracting firms that would have been created in neighboring areas otherwise.[29]

[28]For instance, when $N = Isolated$, the set of instruments is $\mathbf{1}_{dens\leq 31}$, $\mathbf{1}_{dens\_neigh\leq 31}$, $\mathbf{1}_{dens\leq 31} \times \mathbf{1}_{dens\_neigh\leq 31}$, and the controls are $\mathbf{1}_{21\leq dens\_neigh\leq 41}$ and the usual controls, where $dens\_neigh \leq 31$ indicates that one the of neighboring cantons has density below 31. Note that controlling for the canton's own density being in the [21; 41] bracket with $\mathbf{1}_{21\leq dens\leq 41}$ is not necessary, as the estimation sample only consists of cantons within that bracket.

[29]In addition, note that the interaction coefficient d is estimated on only 19 observations (cantons that are included in the ZRR program but have no neighbor in the program). This limits the precision of the estimate. It is also possible that these cantons differ from other ZRR cantons in other dimensions

A third possible explanation lies in the lower generosity of tax cuts in the ZRR program. As shown in Table 1, incentives in the ZRR program are lower than in the urban EZ program, during the 1996-2004 period considered here. However, this would explain why the ZRR program had a smaller effect, but not why it had *no* effect. The absence of effect can arise if firms incur some fixed costs in order to benefit from the tax cuts (the cost of gathering the information, for example, or the administrative burden to apply for the benefits). We do not have quantitative evidence on these costs, and lack sources of variation in the generosity of the benefits. There is however abundant anecdotal evidence that many entrepreneurs did not understand the program well, and were uncertain whether their firm would be eligible, or were discouraged by the administrative burden. As one of them put it, "To benefit from the rural EZ tax cuts, you need to have a good accountant." (Daniel et al., 2009) This suggests that the limited tax cuts associated with administrative fixed costs contributed to the absence of effect of the program, at least for some firms. However, the 60% take-up we estimated in Section 1 implies that a majority of firms still found it profitable to apply for the tax cuts once they had created eligible jobs.

Of course, a last possible explanation is simply that the environments of the two programs differ: one is urban, the other rural, and the differences in terms of, for instance, infrastructure and locally available workforce shape the costs of business and employment creation, impacting the response of firms to the incentives. This explanation is consistent with findings by Devereux et al. (2007) that firms are less responsive to government subsidies in areas where there are fewer existing plants in their industry, or that rural firms tend to rely particularly strongly on their internal labor market when adjusting to technological shocks, presumably because the external labor market is less deep in such areas (Behaghel et al., 2012). As an attempt to test whether ZRR effects would be larger in less-rural areas, we re-analyzed impacts restricting the sample to more densely populated municipalities (*communes* with more than 2,000 inhabitants). We do not find any significant impact. But these areas remain arguably quite rural.[30]

To sum up, the exclusion of firm relocations from the ZRR scheme can explain part of the impact gap between the urban and rural EZ programs. There is also some evidence

than being isolated, even though they are scattered throughout France and descriptive statistics show no obvious difference with other ZRR cantons (see Supplementary Figure 16 and Table 15.)

[30]Detailed results are available upon request.

that smaller ZRR zones tend to have positive impacts. Differential generosity of tax cuts also presumably plays a role. Taken together, this suggests that the ZRR program may be more effective if it were to refocus on smaller areas and increase the tax cuts so as to act as a magnet attracting newly created and relocating businesses.

4.2 Why did the 2005 program fail to boost public interest organizations?

The 2005 reform took a somewhat different tack, expanding the zoning and making the program more generous to a very specific group of employers, the public interest organizations. As shown above, that attempt was no more successful. One important question is whether this is due to the specificities of PIOs, that may make them less responsive to tax cuts. This would imply a trade-off for policymakers: non-profit employers may have positive social externalities, but they may also be less responsive to tax incentives than for-profit firms, making it unclear which sector should be targeted.

As a first piece of evidence, we reproduce the analysis by Givord et al. (2012) on a subsample in their data, so as to isolate the impact of the urban EZ program on a sector that matches public interest organizations as closely as possible.[31] Impacts are cut by two thirds compared to the impacts on total employment displayed in Table 8.[32] This suggests that employers in public interest organizations may be less responsive to fiscal incentives and payroll tax exemptions, compared to employers in other firms. One interpretation is that the public interest sector is characterized by less creation and destruction of jobs and businesses, so the margin for attracting new jobs and plants is smaller. As a second piece of evidence, we compare the share of jobs that are located in newly created establishments in the public interest sector versus the rest of the economy. Specifically, we compute the share of total employment located in establishments that were created after 2000 (Figure 9). By construction, this share is 0 in 2000; in 2008 it reaches 50% outside of the public interest sector, but it is only 30% among PIOs. This supports the hypothesis that it is

[31] Unfortunately, the data they use only contains the PIOs providing "marketable services" (or goods), i.e. PIOs that sell (totally or partially) services at economically significant prices.

[32] The estimated impacts on employment growth rates are equal to 0.07 and 0.06 over 1996-98 and 1996-2004 respectively, significant at the 10% and 1% levels.

harder to substantially alter employment levels in PIOs by influencing job and business creation rates.

Of course, other differences may explain why the urban EZ program performed better than the second version of the ZRR program. The periods of implementation differ: the second part of the 1990s for the urban EZ program versus the late 2000s for the ZRR one. It may be the case that the latter period was less favorable.[33] Last, the two explanations discussed for the 1996-2004 period may also apply for the second wave of the ZRR program: rural enterprise zones may be handicapped by their size or by the rural environment. One explanation that does not apply, though, is the difference in the generosity of the tax cuts: in both cases, the payroll tax exemptions concern current and hired employees, and employers are fully exempted from payroll contribution for a fraction of wages (below 1.4 times the minimum wage and during five years for the urban EZ program, below 1.5 times the minimum wage and throughout the duration of the contract for the rural EZ program).

To summarize: public interest organizations received substantial financial subsidies through payroll tax exemptions, but this money was not used to create new jobs, perhaps because turnover is lower in that sector or because job creation decisions are predominantly driven by non-financial factors. All this leaves an intriguing question: where did the money received by PIOs go? One possibility is that PIOs used the subsidy to increase wages. The DADS dataset provides wage data along with employment measures. However, using the same specification as in Table 7, we do not find such effect.[34] Another possibility is that the money was used to restore the balance sheet of the PIOs. To document such effects, we need accounting data of PIOs over the second half of the 2000s. We obtained fiscal data that contains information on the establishments' balance sheet and on subsidies received (*subventions d'exploitation*). Unfortunately, only a fraction of PIOs fill the standard fiscal forms, so that this data only covers 8% of the PIOs that are present in our main sample, and underrepresents larger establishments, which are the main beneficiaries of the ZRR program according to partial evidence reported in Daniel et al. (2009). With this caveat in mind, we do not find any impact of the ZRR program on the balance sheet or on other

[33] As noted above, while the first wave of the rural EZ program evaluated by Givord et al. (2012) had very large employment effects, impacts in later waves were positive and significant, yet more modest (Givord et al., 2013).

[34] See Supplementary Table 16.

subsidies received by establishments in this subsample.[35] This finding is reminiscent of Givord et al. (2013) who do not find any impact of the second wave of the urban EZ program on accounting variables of establishments already set up at the beginning of the urban EZ program, although they benefited from substantial tax cuts.[36]

5 Conclusion

This paper has evaluated the impact of a program of tax exemptions targeted at rural areas in France, over a 12-year window. The striking conclusion is that the program failed to have any sizable impact on employment and business creation. This is not simply due to the fact that the program was not generous. Even if tax cuts amounted to less than 100 million euros before 2005, they culminated in above 400 million in 2008, which is comparable to a much more successful, contemporaneous urban EZ program in France, and similar to amounts reported in the literature on EZ programs in the US.

The absence of effect is at odds with the longevity of the program (almost 20 years), which suggests either that policymakers overrate such programs, or that there are pressures that prevent them from scaling them down.[37] But it is consistent with some (if not all) results found in the economic literature on EZ programs.

We use the contemporaneous French urban EZ program as a benchmark to understand why firms seemed not to respond to the financial incentives in the ZRR program. The difficulty of the exercise is that the two programs differ in several dimensions, and we cannot fully assess the impact of all these differences. However, several elements in the comparison with the urban program suggest that the targeting of the ZRR program was not effective: too broad in terms of geographic coverage, too narrow in its exclusion of business relocations and its restriction of the more generous incentives to sectors in which

[35] Detailed results are available upon request.

[36] Specifically, Table 6 in Givord et al. (2013) shows no impact of the program on existing firms' income, sales, wages, cash flow, sales or investments. (As noted above, there is also no employment impact on these firms.)

[37] In July 2013, the government had to go back on its decision to withdraw a number of municipalities from the ZRR program after local elected politicians denounced it as a "villainous decision" (*"décret scélérat"*). See e.g. http://www.ariegenews.com/ariege/actualites_economie/2013/64893/denoncant-un-decret-scelerat-jean-pierre-bel-clarifie-la-situation-et-.html.

business and job creations are quantitatively less important (and which are potentially less responsive to financial incentives).

The literature on place-based policies has accumulated a significant body of evidence, which can be mobilized to question the relevance of these programs or to improve their design if they are to be maintained. In the case of the ZRR program, our suggestion would be to provide for-profit firms with strong incentives to locate in the program zones, and to limit the cost by restricting the program's geographic coverage. This in turn could induce efficiency costs for which a welfare analysis *à la* Busso et al. (2013) would be indispensable.

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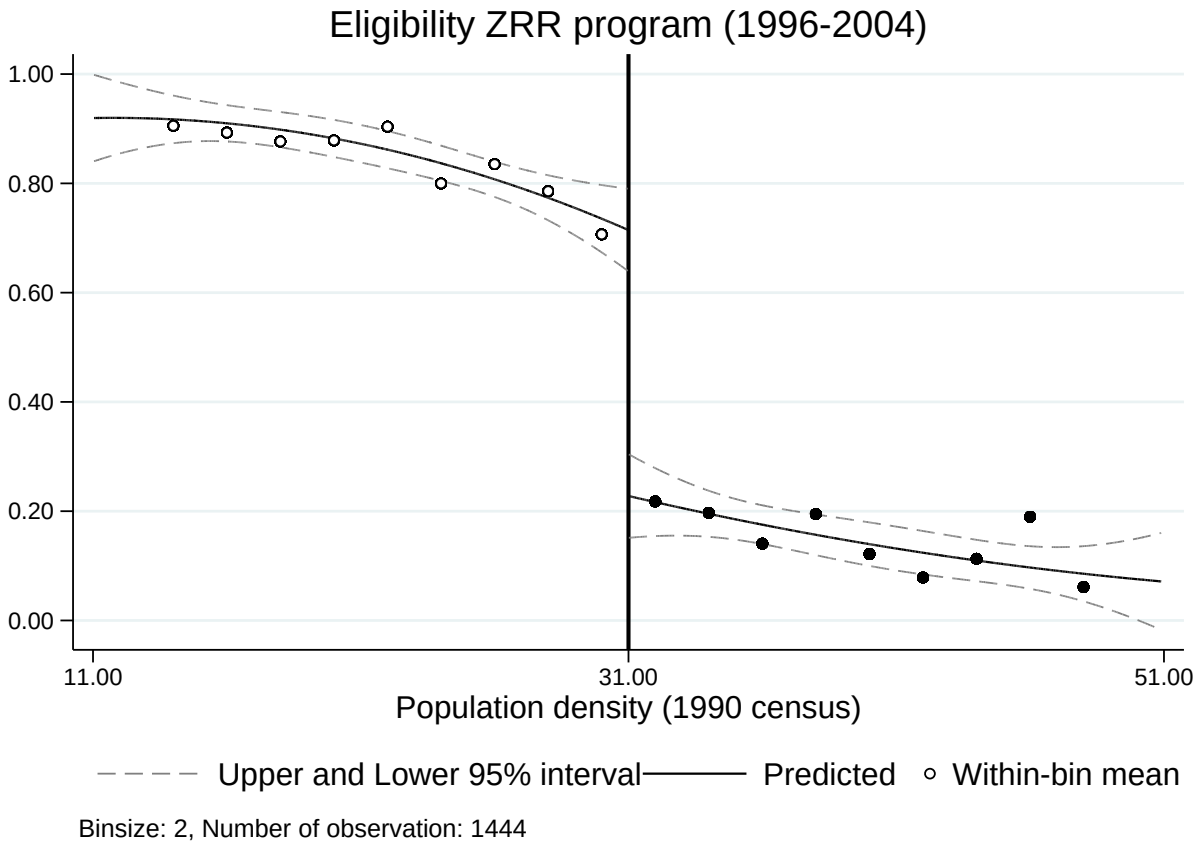
Figure 1: Share of rural EZ cantons (1996-2004) as a function of the 1990 population density (1st stage)



Sources: Population census, Insee. Notes: The graph displays the probability the share of cantons included in the rural EZ (ZRR) scheme as function of their 1990 population density. Dots correspond to means over bins of 2 inhabitants per km^2. The solid line corresponds to a quadratic fit, with 95% intervals in dashed lines.

Figure 2: Zones covered by the ZRR program

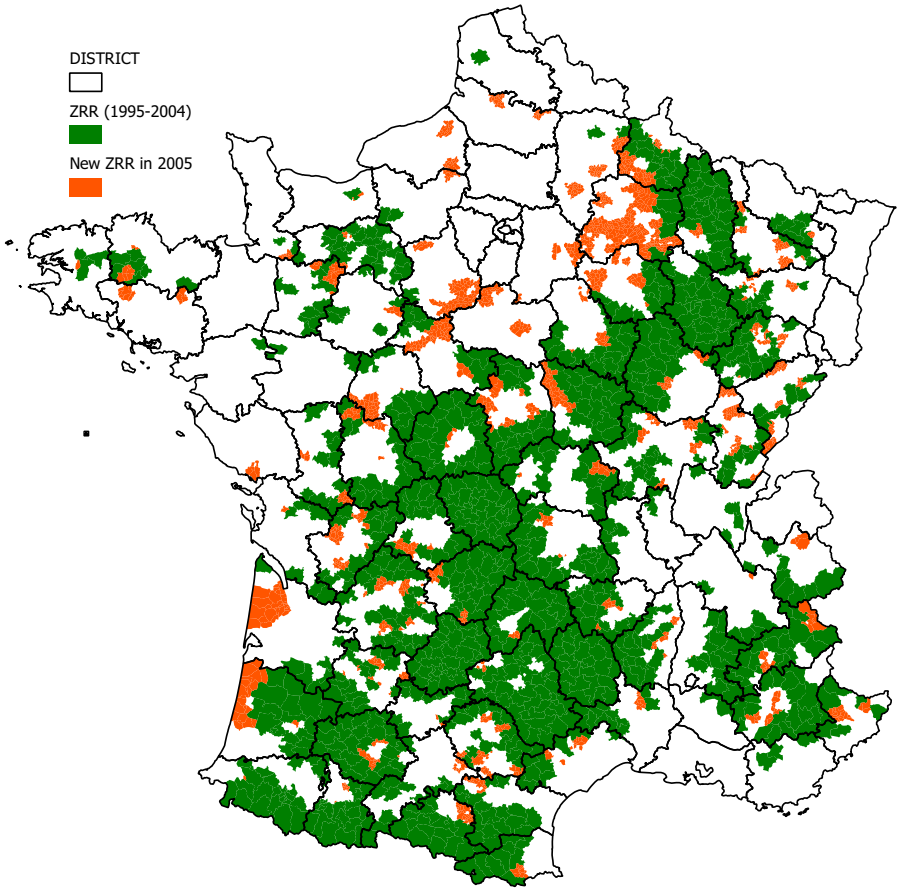
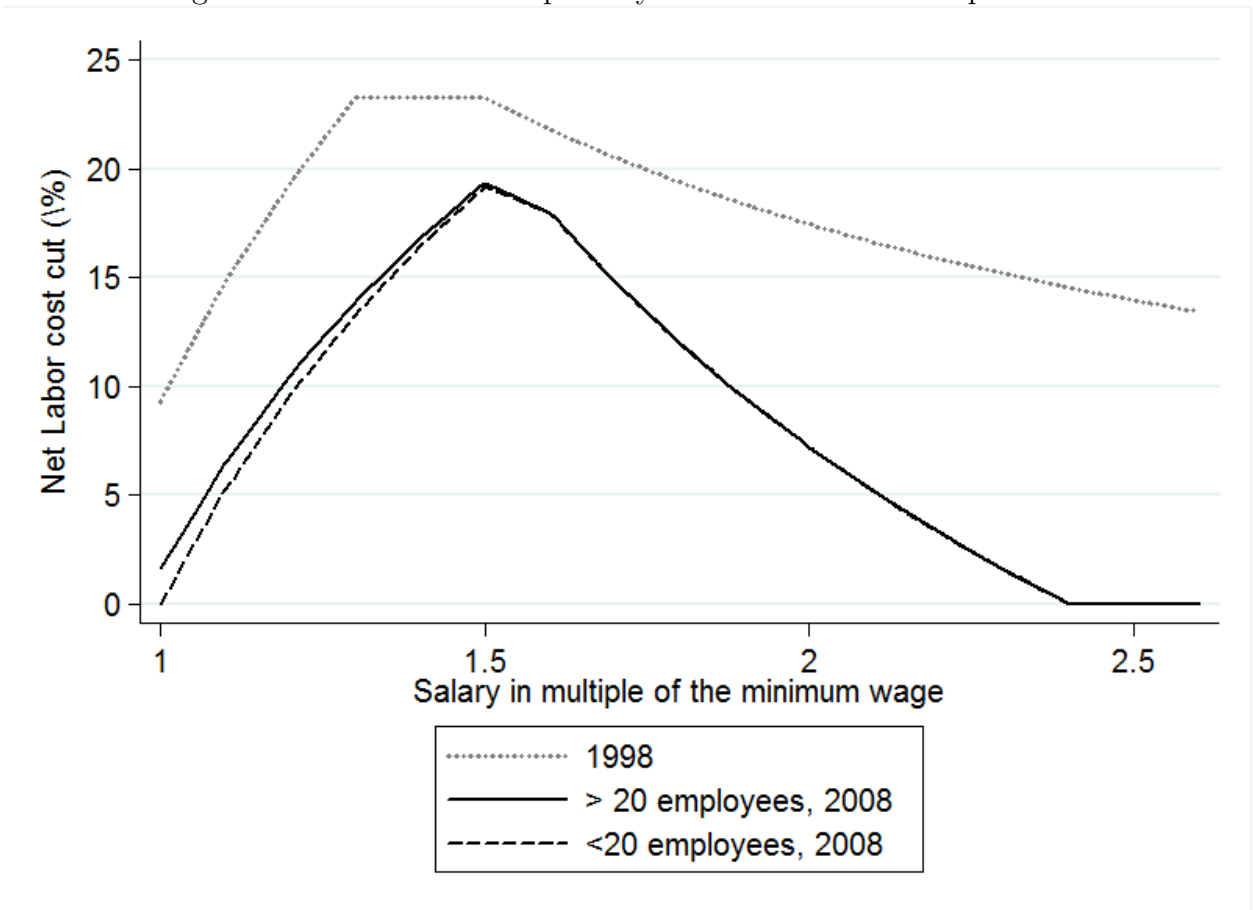
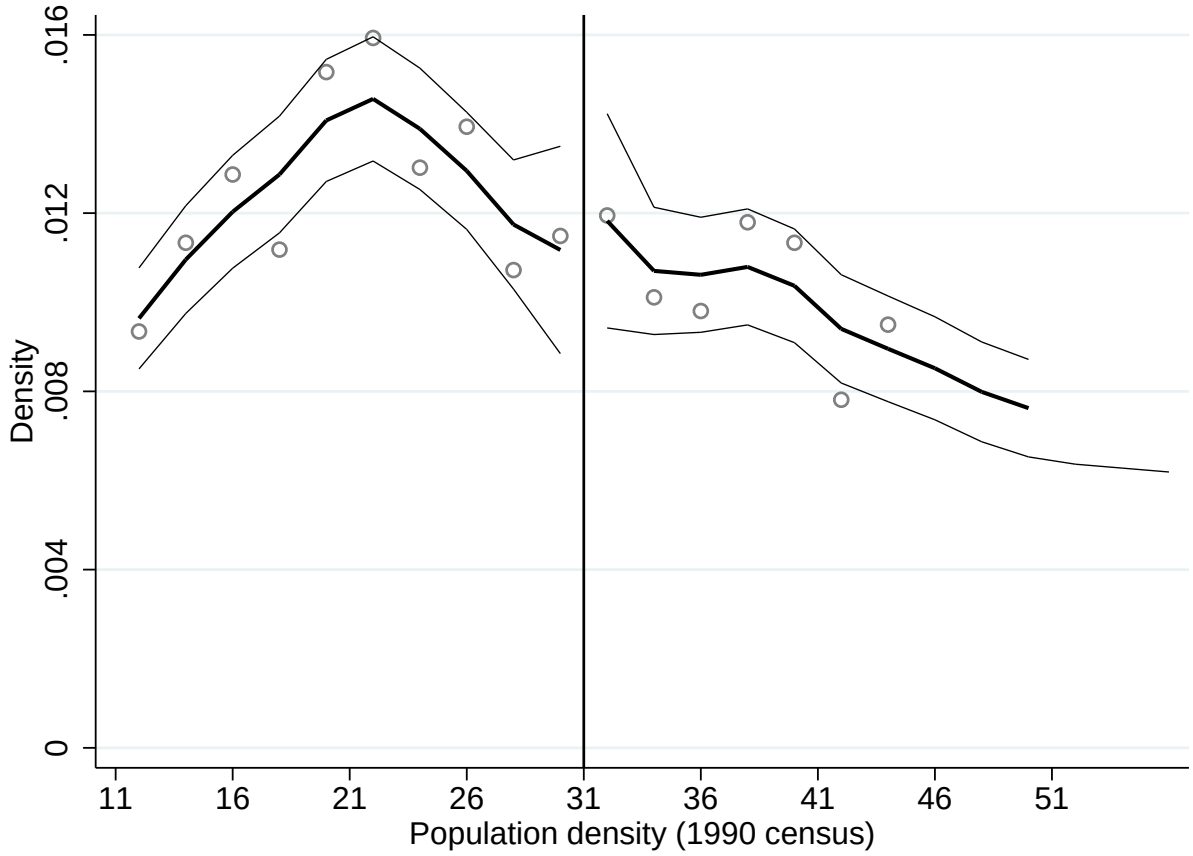


Figure 3: Labor cost cut implied by the rural EZ tax exemptions



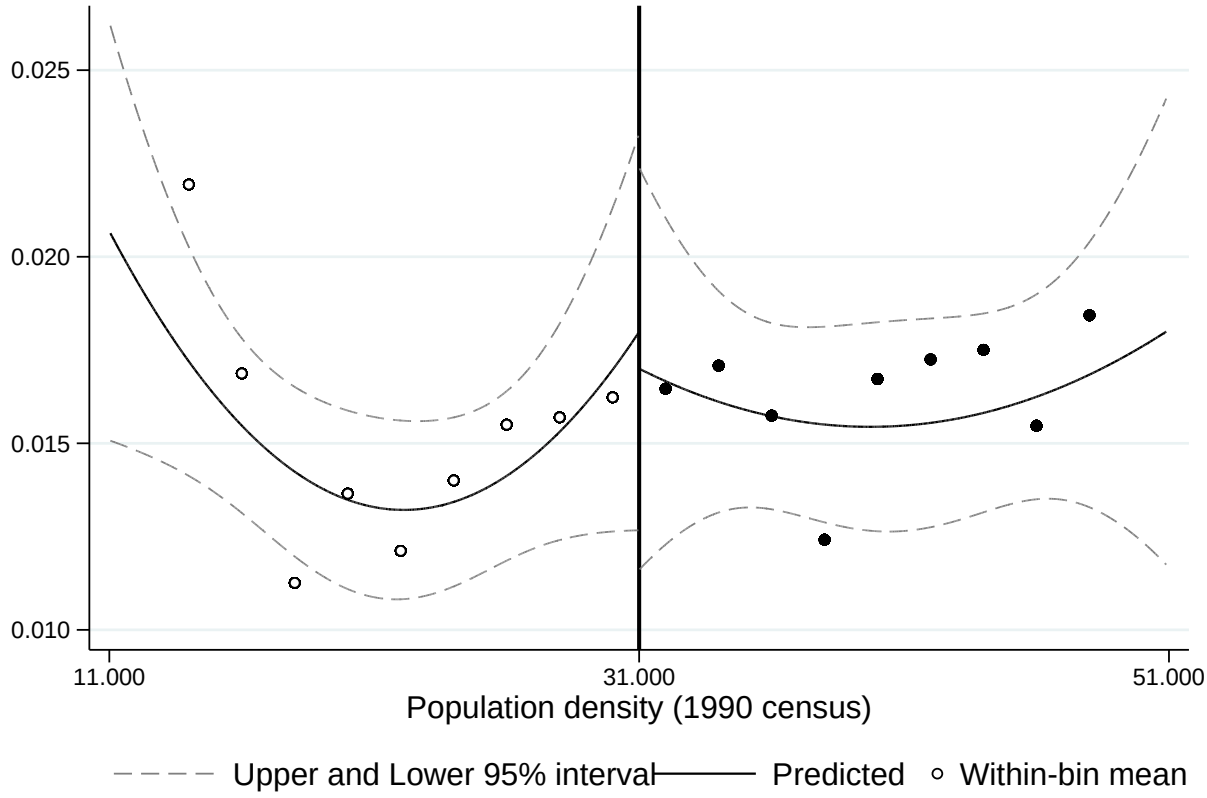
Source: Authors' computations based on legislative texts. Notes: The three lines display the labor cost cut associated with the ZRR program, as a function of the employee's wage. The cut is net of other labor cost cuts due to national payroll tax exemption schemes. In 2008, the national schemes were slightly more generous for firms with fewer than 20 employees.

Figure 4: Density of the 1990 population density variable



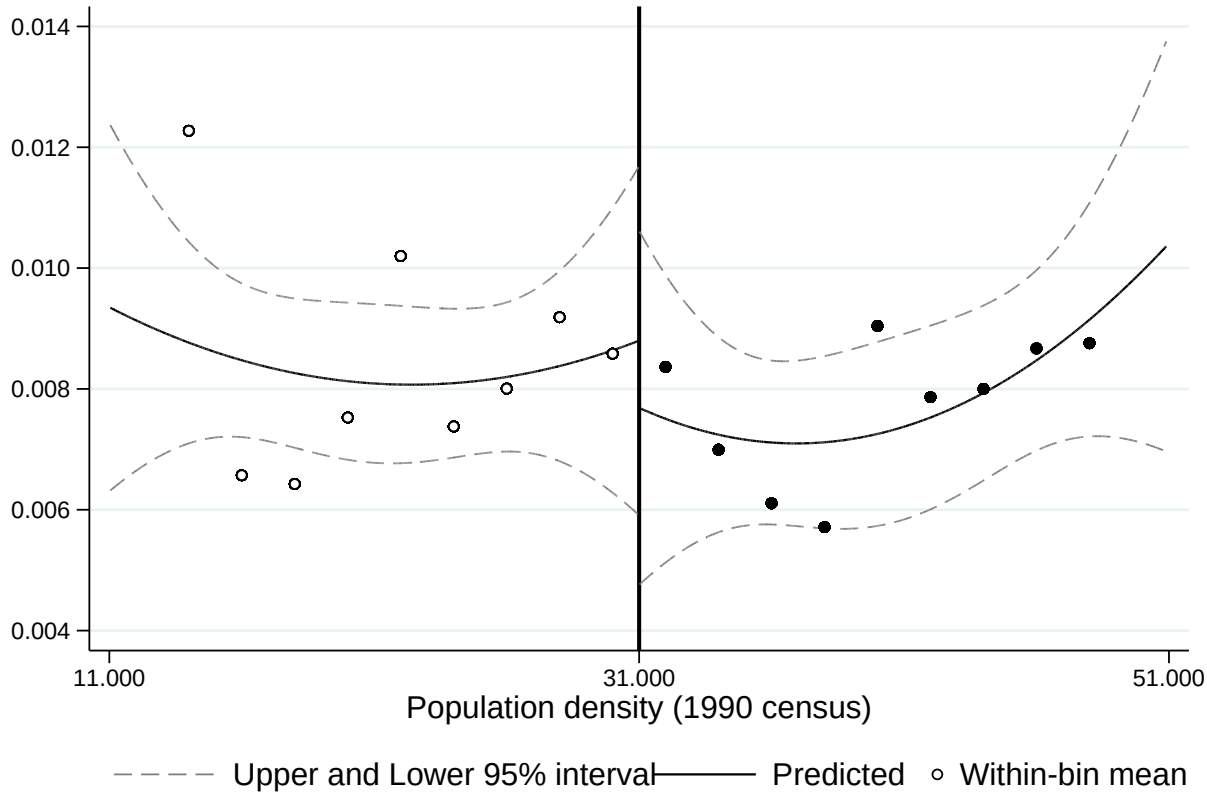
Source: 1990 Population census, Insee. Notes: The graph displays non-parametric estimates of the density of the population density variable, following McCrary (2008).

Figure 5: Local employment (annual growth rate, 1996-2004) by population density



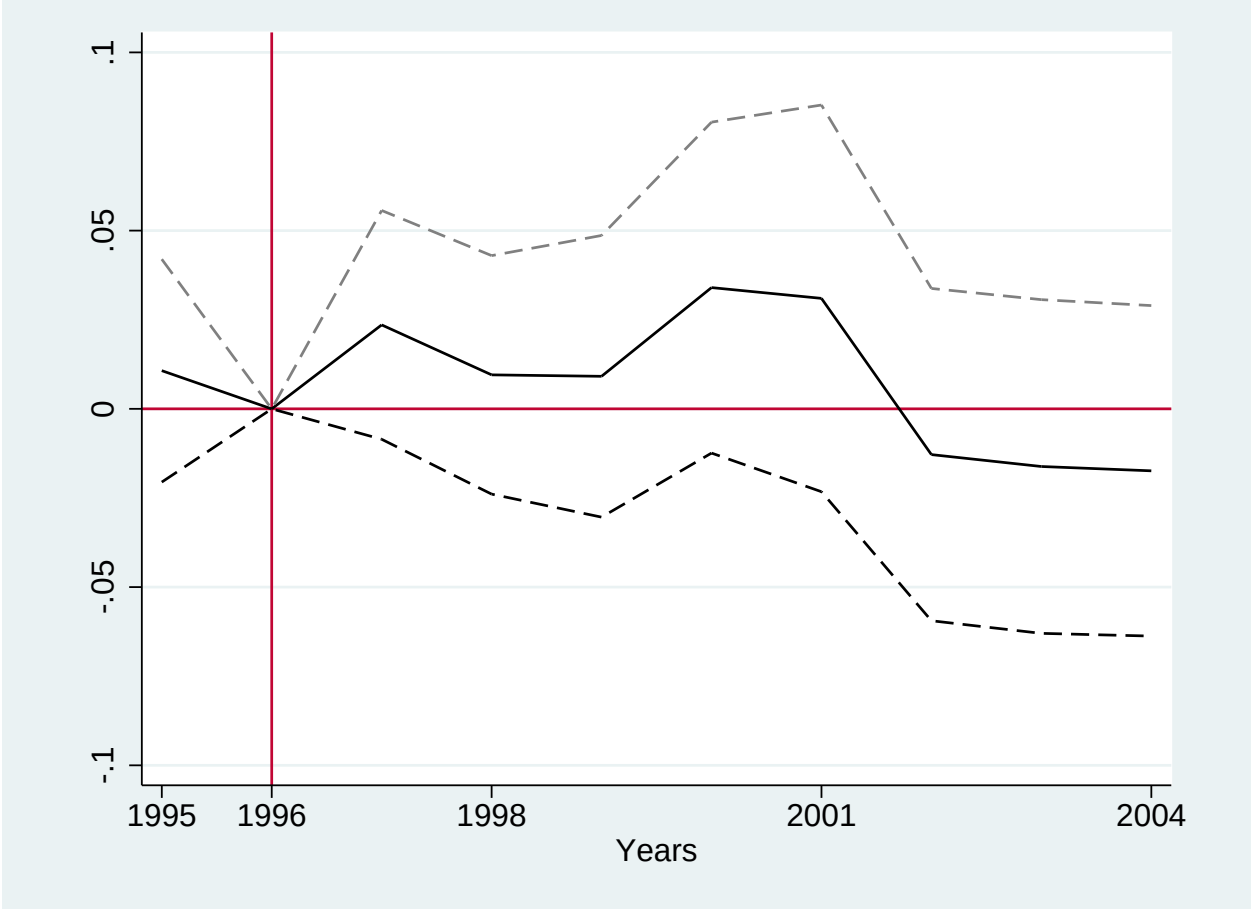
Sources: DADS, 1990 Population census, Insee. Notes: The graph displays the annual growth rate of local employment between 1996-2004, according to the 1990 population density of the cantons. Dots correspond to means over bins of 2 inhabitants per km^2. The solid line corresponds to a quadratic fit, with 95% intervals in dashed lines.

Figure 6: Number of businesses (annual growth rate, 1996-2004) by population density



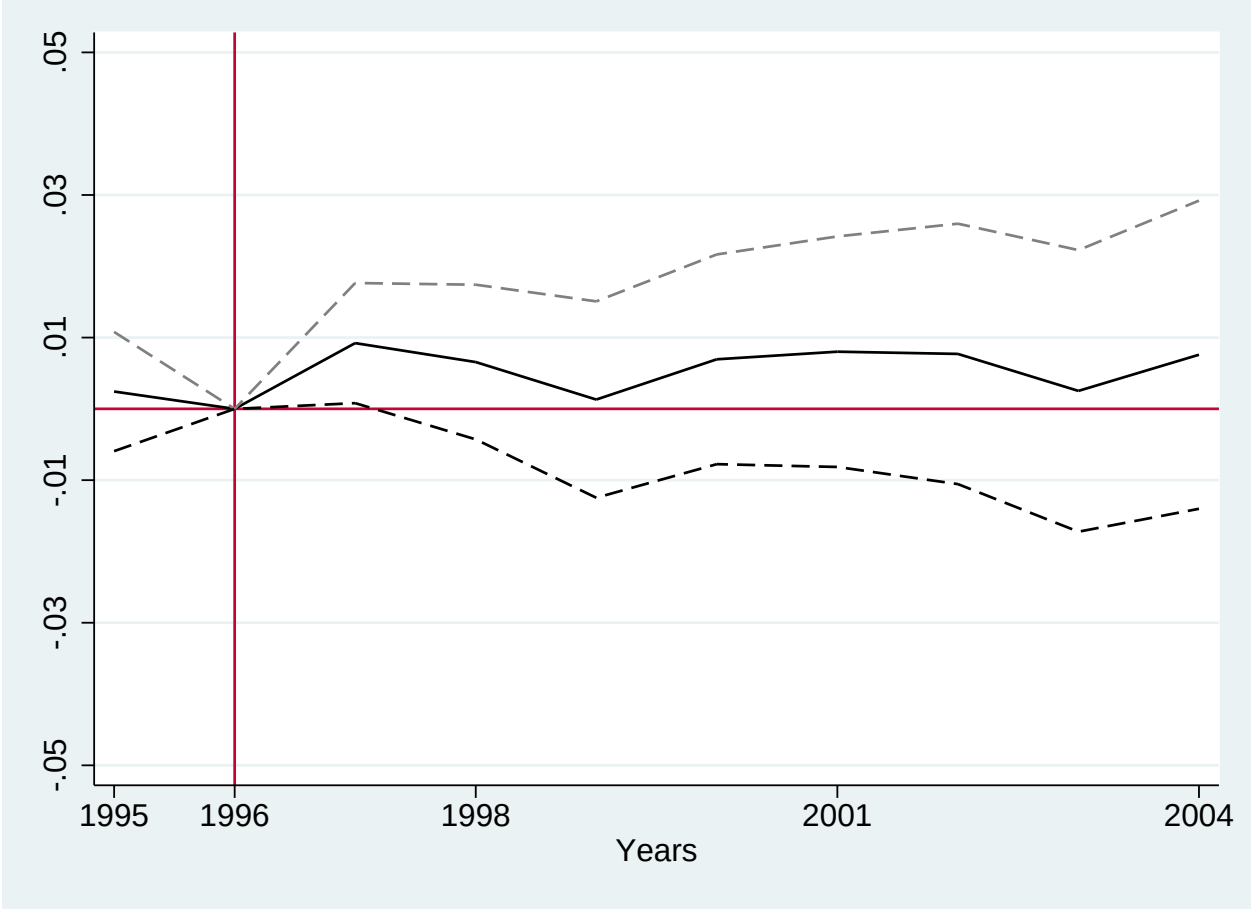
Sources: DADS, Population census. Notes: The graph displays the annual growth rate of the number of businesses between 1996-2004, according to the 1990 population density of the cantons. Dots correspond to means over bins of 2 inhabitants per km^2. The solid line corresponds to a quadratic fit, with 95% intervals in dashed lines.

Figure 7: Cumulated impact of rural EZ program on local employment



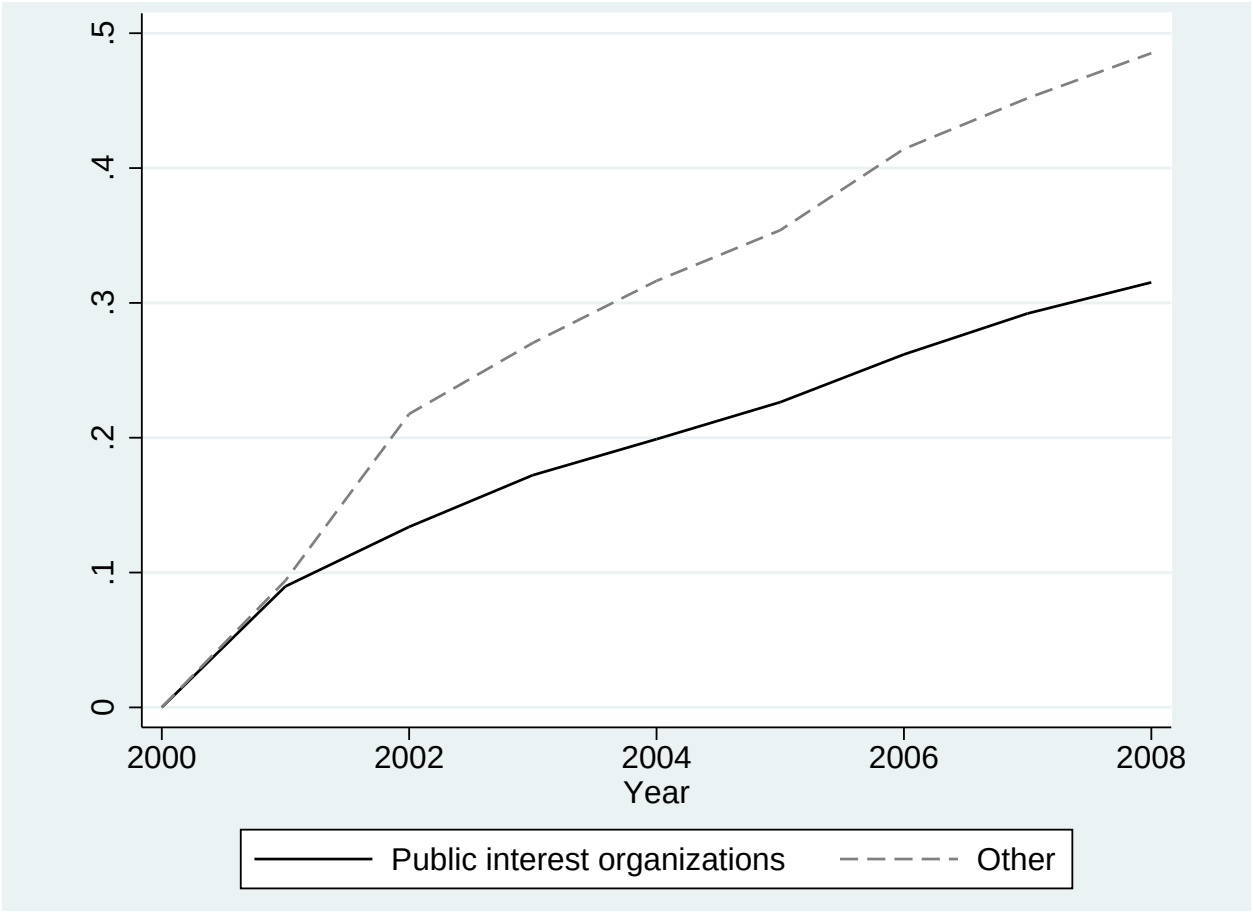
Sources: DADS, Population census. Notes: The graph displays the cumulated impact of the rural EZ program over time (log change in employment, with 1996 as a starting point), estimated with the same specification as in Table 4, columns 2 and 6. Dashed lines give 95% confidence intervals.

Figure 8: Cumulated impact of rural EZ program on number of businesses



Sources: DADS, Population census. Notes: The graph displays the cumulated impact of the rural EZ program over time, estimated with the same specification as in Table 4, columns 2 and 6. Dashed lines give 95% confidence intervals.

Figure 9: Share of employment in firms and organizations created after 2000 (2000-2008)



Sources: DADS. Notes: The graph displays the share of employment that is located in firms and organizations created after 2000. For instance, 30% of the employment in public interest organizations in 2008 is located in firms or organizations that were created after 2000.

Table 1: French Enterprise Zone Tax Cuts

	ZRR (1996-2004)	ZRR (after 2005)	ZFU
Payroll tax exemptions			
Firm eligibility	firms $\leq$ 50 employees	Public interest organizations	firms $\leq$ 50 employees
Employee eligibility	new hires	all employees	
	open-ended contracts fixed-term employment contract of more 12 months		
Exemption	fraction of salary $\leq$ 1.5 times the minimum wage		
Duration	1 year	unlimited	5 years
Corporate tax exemptions			
Eligibility	Newly created firms (only in manufacturing, retail or craft industry)		Newly created or relocating firms
Exemption	100 % the first 2 years, and 75 %, 50 % and 25 % the next 3 years	100 % the first 5 years, and decreasing the next 9 years	100 % the first 5 years and decreasing during the next 3 years ($\leq$ 5 employees) or the next 9 years (others)
Local business tax exemptions			
Eligibility	newly created firms	newly and recovered firms	all firms (created, existing, relocating)
	$\leq$ 150 employees		$\leq$ 50 employees
Exemption	100 % during 2 years	100 % during 5 years	100 % during 5 years

Source: Legislative texts (*Journal officiel*, 1995 and 2005).

Table 2: Descriptive statistics in rural EZ and non rural EZ areas

	Rural EZ		Non rural EZ	
	Mean	s.d.	Mean	s.d.
	(1)		(2)	
	A. All firms			
Employment (1995)	556.71	471.69	962.85	722.48
Employment (annual growth rate 1995-2004)	0.02	0.02	0.02	0.02
Number of businesses (1995)	197.95	122.57	277.25	195.80
Number of businesses (annual growth rate 1995-2004)	0.01	0.01	0.01	0.01
	B. Firms with 1 to 50 salaried workers			
Share of total employment	0.44	.	0.41	.
Employment (1995)	246.59	191.01	395.47	293.88
Employment (annual growth rate 1995-2004)	0.02	0.03	0.02	0.02
Number of businesses (1995)	60.67	41.52	88.89	60.96
Number of businesses (annual growth rate 1995-2004)	0.01	0.02	0.01	0.02
	C. New firms (<3 years old)			
Share of total employment	0.05	.	0.05	.
Employment (2005)	32.07	55.12	56.28	68.30
Employment (annual growth rate 2005-2008)	0.06	0.27	0.06	0.25
Number of businesses (2005)	8.70	6.77	13.71	9.96
Number of businesses (annual growth rate 2005-2008)	0.05	0.16	0.06	0.13
	D. Public interest organizations			
Share of total employment	0.20	.	0.14	.
Employment (2005)	129.78	159.71	167.83	190.76
Employment (annual growth rate 2005-2008)	0.04	0.12	0.04	0.10
Number of businesses (2005)	7.58	5.92	10.35	7.44
Number of businesses (annual growth rate 2005-2008)	0.00	0.09	0.01	0.08
Number of cantons	787		657	

Notes: Comparisons of mean employment and business count (in 1995) and annual growth rates (1995-2004) between rural EZ (column 1) and non rural EZ (column 2) areas. The different panels correspond to sub-sample of firms that were particularly targeted by the exemptions of payroll tax (Panels B and D) or corporate and business taxes (Panel C). The sample is restricted to cantons with with a population density between 11 and 51 per km^2.
Source: DADS, Insee.

Table 3: Inclusion in rural EZ program (1st stage)

	A. Included in rural EZ program (1996-2004)			
$\mathbf{1}_{dens\leq 31}$	0.639***	0.614***	0.535***	0.516***
	(0.028)	(0.027)	(0.037)	(0.034)
$\mathbf{1}_{dens\leq 31} \times (dens - 31)$			-0.010***	-0.010***
			(0.002)	(0.002)
$\mathbf{1}_{dens>31} \times (dens - 31)$			-0.008***	-0.006**
			(0.002)	(0.002)
1995 industry mix	no	yes	no	yes
County dummies	no	yes	no	yes
Observations	784	784	1,444	1,444
F-stat	533.9	526.5	212.6	230.0
	B. Included in rural EZ program (2005-2008)			
$\mathbf{1}_{dens\leq 31}$	0.234***	0.229***	0.260***	0.231***
	(0.050)	(0.051)	(0.048)	(0.046)
$\mathbf{1}_{dens99\leq 31}$	0.428***	0.396***	0.298***	0.306***
	(0.050)	(0.051)	(0.046)	(0.045)
$\mathbf{1}_{dens\leq 31} \times (dens - 31)$			0.038***	0.032***
			(0.009)	(0.010)
$\mathbf{1}_{dens>31} \times (dens - 31)$			0.024***	0.022***
			(0.006)	(0.006)
$\mathbf{1}_{dens99\leq 31} \times (dens99 - 31)$			-0.039***	-0.030***
			(0.008)	(0.009)
$\mathbf{1}_{dens99>31} \times (dens99 - 31)$			-0.037***	-0.034***
			(0.005)	(0.005)
2003 industry mix	no	yes	no	yes
County dummies	no	yes	no	yes
Observations	784	784	1,444	1,444
F-stat	312.0	263.2	129.1	130.4

Notes: First-stage (OLS) estimations of the impact of the instrument(s) ($\mathbf{1}_{dens\leq 31}$ or $\mathbf{1}_{dens99\leq 31}$) on the treatment variable EZ for the first (A) and second (B) phase of the program. In the second phase of the program, we add $\mathbf{1}_{dens99\leq 31}$ as an instrument as new cantons are treated based on the 1999 population census. Columns 1 and 2 restrict the analysis to the cantons with a population density above 21 or below 41 per km^2 and do not include linear controls in the density (either in 1990 or in 1999). Columns 3 and 4 include cantons with a density above 11 and below 51 and include linear controls of the density (in 1990 and 1999) separately on either side of the threshold. For consistency, the sample selection is based on the 1990 population density in both panels. F-statistics are for the test that instruments have no effect. Standard errors in parentheses. ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.
Sources: DADS and Population Census 1990 and 1999, Insee.

Table 4: Overall impact of the 1996 rural EZ program

	A. Local employment (annual growth rate)							
	1996-98				1996-2004			
EZ	0.003	0.005	0.010	0.009	-0.002	-0.002	-0.004	-0.002
	(0.008)	(0.009)	(0.012)	(0.013)	(0.003)	(0.003)	(0.005)	(0.005)
$\mathbf{1}_{dens\leq 31} \times (dens - 31)$			0.001**	0.001*			-0.000	0.000
			(0.000)	(0.001)			(0.000)	(0.000)
$\mathbf{1}_{dens>31} \times (dens - 31)$			-0.000	-0.000			0.000	0.000
			(0.001)	(0.001)			(0.000)	(0.000)
1995 industry mix	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes
Observations	784	784	1,444	1,444	784	784	1,444	1,444
Average of outcome	0.021	0.021	0.019	0.019	0.015	0.015	0.016	0.016
S.d. of outcome	0.068	0.068	0.063	0.063	0.025	0.025	0.025	0.025
	B. Number of businesses (annual growth rate)							
	1996-98				1996-2004			
EZ	0.005*	0.003	0.007	0.004	0.002	0.001	0.003	0.002
	(0.003)	(0.003)	(0.005)	(0.005)	(0.001)	(0.001)	(0.003)	(0.002)
$\mathbf{1}_{dens\leq 31} \times (dens - 31)$			0.000	0.000			0.000	0.000**
			(0.000)	(0.000)			(0.000)	(0.000)
$\mathbf{1}_{dens>31} \times (dens - 31)$			0.000	0.000			0.000	0.000
			(0.000)	(0.000)			(0.000)	(0.000)
1995 industry mix	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes
Observations	784	784	1,444	1,444	784	784	1,444	1,444
Average of outcome	-0.003	-0.003	-0.003	-0.003	0.008	0.008	0.008	0.008
S.d. of outcome	0.025	0.025	0.025	0.025	0.013	0.013	0.013	0.013

Notes: 2SLS estimations of the impact of EZ on the annual growth rate of local employment (A) and the number of businesses (B) in the short (96-98) and long (96-04) run. For each specification, EZ is instrumented by $\mathbf{1}_{dens\leq 31}$ which equates 1 if the canton has a population density below or equal to 31 inhabitants per km^2. In columns 1, 2, 5 and 6 we restrict the sample to the cantons with a density above 21 or below 41 and do not include linear control in the density. In columns 3, 4, 7 and 8 the sample includes cantons with a density above 11 and below 51 and we include linear controls of the density separately on either side of the threshold. Standard errors in parentheses. ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.
Sources: DADS and Population Census 1990, Insee.

Table 5: Impact of the 1996 rural EZ program: firms with fewer than 50 employees, targeted sectors

	A. Employment in small businesses (<50 workers; annual growth rate)							
	1996-98				1996-2004			
EZ	0.006	0.008	0.003	0.011	0.001	-0.000	-0.001	0.001
	(0.007)	(0.008)	(0.012)	(0.012)	(0.003)	(0.003)	(0.005)	(0.005)
$\mathbf{1}_{dens\leq 31} \times (dens - 31)$			-0.000	0.000			-0.000	0.000
			(0.000)	(0.000)			(0.000)	(0.000)
$\mathbf{1}_{dens>31} \times (dens - 31)$			-0.000	0.000			-0.000	-0.000
			(0.001)	(0.000)			(0.000)	(0.000)
1995 industry mix	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes
Observations	783	783	1,443	1,443	783	783	1,443	1,443
Average of outcome	0.032	0.032	0.032	0.032	0.017	0.017	0.017	0.017
S.d. of outcome	0.061	0.061	0.062	0.062	0.027	0.027	0.027	0.027
	B. Number of small businesses (annual growth rate)							
	1996-98				1996-2004			
EZ	0.008*	0.006	0.010	0.010	0.003	0.001	0.005	0.005
	(0.005)	(0.005)	(0.009)	(0.009)	(0.002)	(0.002)	(0.004)	(0.003)
$\mathbf{1}_{dens\leq 31} \times (dens - 31)$			-0.000	0.000			0.000	0.000**
			(0.000)	(0.000)			(0.000)	(0.000)
$\mathbf{1}_{dens>31} \times (dens - 31)$			0.000	0.000			0.000	0.000
			(0.000)	(0.000)			(0.000)	(0.000)
1995 industry mix	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes
Observations	783	783	1,443	1,443	783	783	1,443	1,443
Average of outcome	0.027	0.027	0.027	0.027	0.012	0.012	0.012	0.012
S.d. of outcome	0.044	0.044	0.047	0.047	0.017	0.017	0.019	0.019

Notes: 2SLS estimations of the impact of EZ on the annual growth rate of local employment (A) and the number of businesses (B) in the short (96-98) and long (96-04) run, in firms with less than 50 employees only. For each specification, EZ is instrumented by $\mathbf{1}_{dens\leq 31}$ which equates 1 if the canton has a population density below or equal to 31 inhabitants per km^2. In columns 1, 2, 5 and 6 we restrict the sample to the cantons with a density above 21 or below 41 and do not include linear controls in the density. In columns 3, 4, 7 and 8 the sample includes cantons with a density above 11 and below 51 and we include linear controls of the density separately on either side of the threshold. Standard errors in parentheses. ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.
Sources: DADS and Population Census 1990, Insee.

Table 6: Impact of 2005 rural EZ program: new firms

	A. Employment in new firms (<3 years old; annual growth rate)							
	2005-2007				2005-2009			
EZ	-0.026	0.032	0.036	0.047	-0.006	-0.020	0.029	0.029
	(0.044)	(0.058)	(0.079)	(0.085)	(0.025)	(0.031)	(0.047)	(0.050)
$\mathbf{1}_{dens\leq 31} \times (dens - 31)$			0.007	-0.005			-0.000	-0.003
			(0.011)	(0.013)			(0.006)	(0.008)
$\mathbf{1}_{dens>31} \times (dens - 31)$			-0.005	-0.013			-0.002	-0.005
			(0.007)	(0.008)			(0.004)	(0.005)
$\mathbf{1}_{dens99\leq 31} \times (dens99 - 31)$			-0.005	0.006			0.003	0.006
			(0.011)	(0.013)			(0.006)	(0.008)
$\mathbf{1}_{dens99>31} \times (dens99 - 31)$			0.007	0.016*			0.002	0.005
			(0.007)	(0.008)			(0.004)	(0.005)
2005 industry mix	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes
Observations	771	771	1,415	1,415	772	772	1,417	1,417
Average of outcome	0.0651	0.0651	0.0637	0.0637	0.0328	0.0328	0.0270	0.0270
S.d. of outcome	0.341	0.341	0.392	0.392	0.220	0.220	0.234	0.234
	B. Number of new businesses (annual growth rate)							
	2005-2007				2005-2009			
EZ	-0.004	0.041	0.037	0.030	-0.002	0.000	0.023	0.023
	(0.026)	(0.033)	(0.047)	(0.050)	(0.014)	(0.018)	(0.027)	(0.028)
$\mathbf{1}_{dens\leq 31} \times (dens - 31)$			0.005	0.002			-0.002	-0.003
			(0.006)	(0.008)			(0.004)	(0.004)
$\mathbf{1}_{dens>31} \times (dens - 31)$			-0.004	-0.006			-0.003	-0.004
			(0.004)	(0.005)			(0.002)	(0.003)
$\mathbf{1}_{dens99\leq 31} \times (dens99 - 31)$			-0.003	-0.001			0.004	0.005
			(0.006)	(0.008)			(0.004)	(0.004)
$\mathbf{1}_{dens99>31} \times (dens99 - 31)$			0.004	0.007			0.003	0.005*
			(0.004)	(0.005)			(0.002)	(0.003)
2005 industry mix	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes
Observations	771	771	1,415	1,415	772	772	1,417	1,417
Average of outcome	0.0579	0.0579	0.0655	0.0655	0.0434	0.0434	0.0376	0.0376
S.d. of outcome	0.203	0.203	0.231	0.231	0.125	0.125	0.131	0.131

Notes: 2SLS estimations of the impact of EZ on the annual growth rate of local employment (A) and the number of businesses (B) in the short (2005-07) and medium (2005-09), in firms less than 3 years old. For each specification, EZ is instrumented by $\mathbf{1}_{dens\leq 31}$ and $\mathbf{1}_{dens99\leq 31}$ which equate 1 if the canton has a population density below of equal to 31 inhabitants per km^2 at the 1990 and 1999 censuses, respectively. In columns 1, 2, 5 and 6 we restrict the sample to the cantons with a density above 21 or below 41 and do not include linear controls in the density. In columns 3, 4, 7 and 8 the sample includes cantons with a density above 11 and below 51 and we include linear controls of the density separately on either side of the threshold. Standard errors in parentheses. ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.
Sources: DADS and Population Census 1990, Insee.

Table 7: Impact of 2005 rural EZ program: Public interest organizations

	A. Employment in public interest organizations (annual growth rate)							
	2005-2007				2005-2009			
EZ	-0.001	0.003	0.007	0.010	-0.006	-0.006	-0.001	0.012
	(0.012)	(0.015)	(0.021)	(0.022)	(0.010)	(0.012)	(0.017)	(0.018)
$\mathbf{1}_{dens\leq 31} \times (dens - 31)$			-0.005*	-0.002			-0.003	-0.002
			(0.003)	(0.004)			(0.003)	(0.003)
$\mathbf{1}_{dens>31} \times (dens - 31)$			0.000	0.003			-0.002	-0.000
			(0.002)	(0.002)			(0.002)	(0.002)
$\mathbf{1}_{dens99\leq 31} \times (dens99 - 31)$			0.005*	0.003			0.003	0.003
			(0.003)	(0.004)			(0.003)	(0.003)
$\mathbf{1}_{dens99>31} \times (dens99 - 31)$			-0.000	-0.003			0.002	0.000
			(0.002)	(0.002)			(0.002)	(0.002)
2005 industry mix	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes
Observations	763	763	1,410	1,410	765	765	1,414	1,414
Average of outcome	0.0374	0.0374	0.0447	0.0447	0.0348	0.0348	0.0349	0.0349
S.d. of outcome	0.119	0.119	0.110	0.110	0.0896	0.0896	0.0913	0.0913
	B. Number of public interest organizations (annual growth rate)							
	2005-2007				2005-2009			
EZ	-0.005	-0.001	0.006	0.016	-0.001	0.004	0.007	0.015
	(0.010)	(0.012)	(0.017)	(0.019)	(0.007)	(0.009)	(0.013)	(0.013)
$\mathbf{1}_{dens\leq 31} \times (dens - 31)$			0.001	0.004			-0.003*	-0.001
			(0.003)	(0.003)			(0.002)	(0.002)
$\mathbf{1}_{dens>31} \times (dens - 31)$			-0.002	-0.000			-0.004***	-0.001
			(0.002)	(0.002)			(0.001)	(0.001)
$\mathbf{1}_{dens99\leq 31} \times (dens99 - 31)$			-0.000	-0.002			0.004**	0.002
			(0.003)	(0.003)			(0.002)	(0.002)
$\mathbf{1}_{dens99>31} \times (dens99 - 31)$			0.001	-0.000			0.004***	0.002
			(0.002)	(0.002)			(0.001)	(0.001)
2005 industry mix	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes
Observations	763	763	1,410	1,410	765	765	1,414	1,414
Average of outcome	0.00687	0.00687	0.0136	0.0136	0.00316	0.00316	0.000980	0.000980
S.d. of outcome	0.0861	0.0861	0.0917	0.0917	0.0669	0.0669	0.0689	0.0689

Notes: 2SLS estimations of the impact of EZ on the annual growth rate of local employment (A) and the number of businesses (B) in the short (2005-07) and medium (2005-09), in public interest organizations only. For each specification, EZ is instrumented by $\mathbf{1}_{dens\leq 31}$ and $\mathbf{1}_{dens99\leq 31}$ which equate 1 if the canton has a population density below of equal to 31 inhabitants per km^2 at the 1990 and 1999 censuses, respectively. In columns 1, 2, 5 and 6 we restrict the sample to the cantons with a density above 21 or below 41 and do not include linear controls in the density. In columns 3, 4, 7 and 8 the sample includes cantons with a density above 11 and below 51 and we include linear controls of the density separately on either side of the threshold. Standard errors in parentheses. ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.
Sources: DADS and Population Census 1990, Insee.

Table 8: Impact of urban and rural EZ program on local employment and number of businesses

	A. Local employment		B. Number of businesses	
	Rural EZ	Urban EZ	Rural EZ	Urban EZ
1996-1998	0.005	0.201***	0.003	0.119***
	(0.009)	[0.092;0.309]	(0.003)	[0.092;0.147]
1996-2004	-0.002	0.154***	0.001	0.071***
	(0.003)	[0.107;0.200]	(0.001)	[0.056;0.087]
	C. Newly created businesses		D. Relocating businesses	
	Rural EZ	Urban EZ	Rural EZ	Urban EZ
1996-1998	-0.050**	0.249***	-0.005	0,405***
	(0.025)	[0.163;0.335]	(0.043)	[0,170;0,637]
1996-2004	-0.005	0.062***	0.003	0,098***
	(0.006)	[0.028;0.097]	(0.011)	[0,026;0,170]

Notes: Comparisons of the impact of rural EZ and urban EZ on the annual growth rate of employment (A), number of businesses (B), creations of businesses (C), and relocations of businesses (D). Estimates of the urban program impacts are based on Givord et al. (2012). Standard errors in parentheses. 95% confidence intervals in brackets. ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.
Source: DADS and SIRENE, Insee.

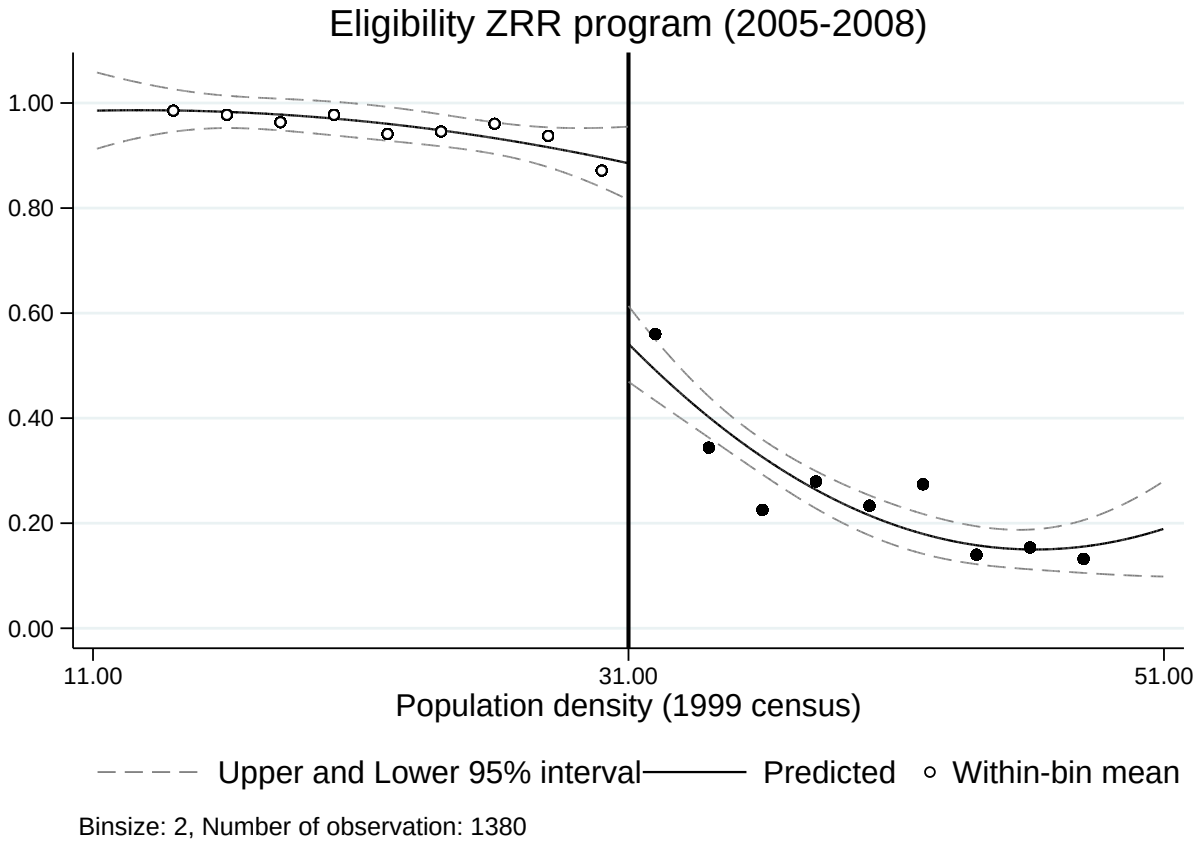
Table 9: Impact of the 1996 rural EZ program by geographical context: total employment and number of plants

	A. Total employment			
	1996-98		1996-2004	
EZ	0.005	0.002	0.002	-0.004
	(0.029)	(0.009)	(0.010)	(0.003)
Share of neighbors in rural EZ program ($N1$)	0.041		0.006	
	(0.050)		(0.017)	
$EZ \times N1$	-0.089		-0.019	
	(0.067)		(0.023)	
No neighbor in rural EZ program ($N2$)		-0.010		-0.008
		(0.025)		(0.009)
$EZ \times N2$		0.089*		0.041**
		(0.054)		(0.019)
Observations	784	784	784	784
Average of outcome	0.021	0.021	0.015	0.015
S.d. of outcome	0.068	0.068	0.025	0.025
	B. Number of plants			
	1996-98		1996-2004	
EZ	0.002	0.003	0.001	0.001
	(0.009)	(0.003)	(0.004)	(0.001)
Share of neighbors in rural EZ program ($N1$)	0.005		-0.003	
	(0.016)		(0.008)	
$EZ \times N1$	-0.010		-0.001	
	(0.021)		(0.010)	
No neighbor in rural EZ program ($N2$)		-0.006		0.003
		(0.008)		(0.004)
$EZ \times N2$		0.000		0.009
		(0.017)		(0.009)
Observations	784	784	784	784
Average of outcome	-0.003	-0.003	0.008	0.008
S.d. of outcome	0.025	0.025	0.013	0.013

Notes: 2SLS estimations of the impact of EZ on the annual growth rate of local employment (A) and the number of businesses (B) in the short (96-98) and long (96-04) run taking into account the geographical context (isolation of the zone). For each specification, EZ is instrumented by $\mathbf{1}_{dens \leq 31}$, $N1$ by the share of neighbors with a population density below 31 per km^2 and $N2$ by a dummy that equates 1 if at least one neighbor has a density below 31. The interaction terms ($EZ \times N1$ and $EZ \times N1$) are instrumented by the interaction of the instruments. We restrict the sample to cantons with a density above 21 or below 41 and do not include linear controls in the density. The neighboring cantons refer to the cantons that have a border in common with the canton of reference. Standard errors in parentheses. ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.
Sources: DADS and Population Census 1990, Insee.

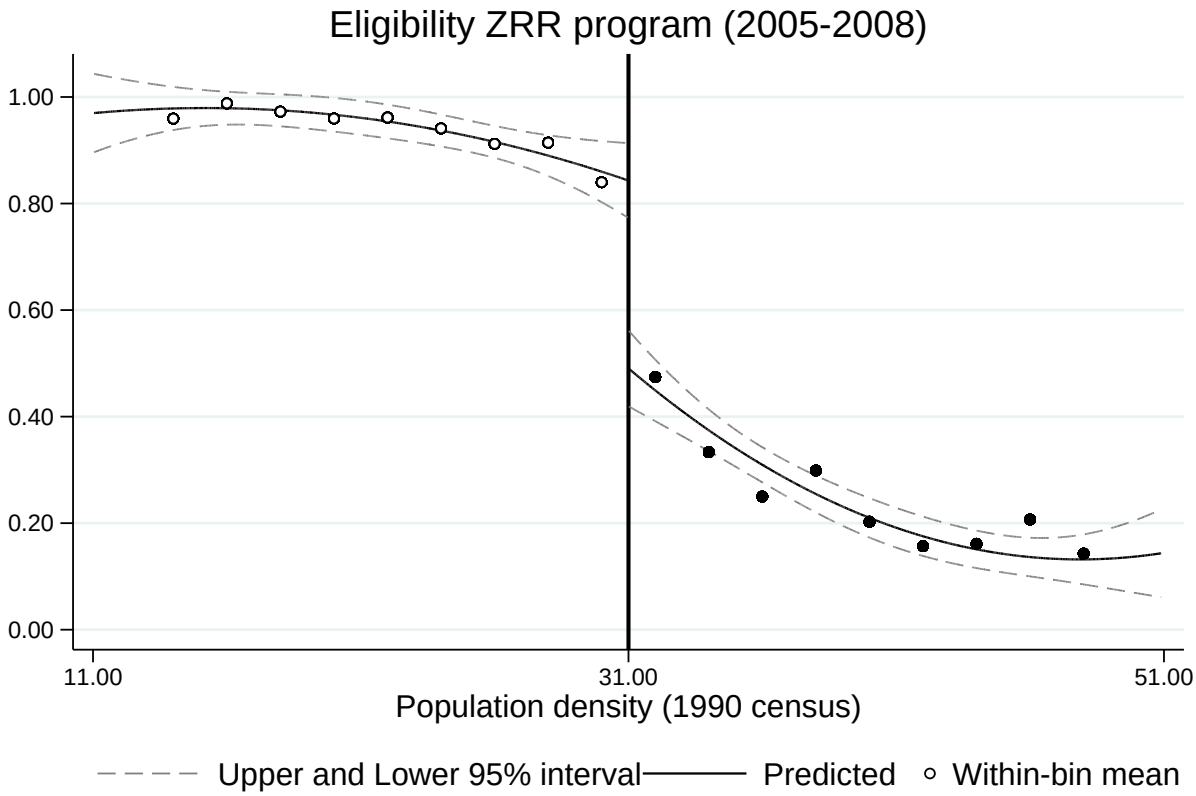
Supplementary figures and tables

Figure 10: Share of rural EZ cantons (2005-08) as a function of the 1999 population density (1st stage)



Notes: see Figure 1.

Figure 11: Share of rural EZ cantons (2005-08) as a function of the 1990 population density (1st stage)



Binsize: 2, Number of observation: 1444

Figure 12: Share of population above age 60 in 1990

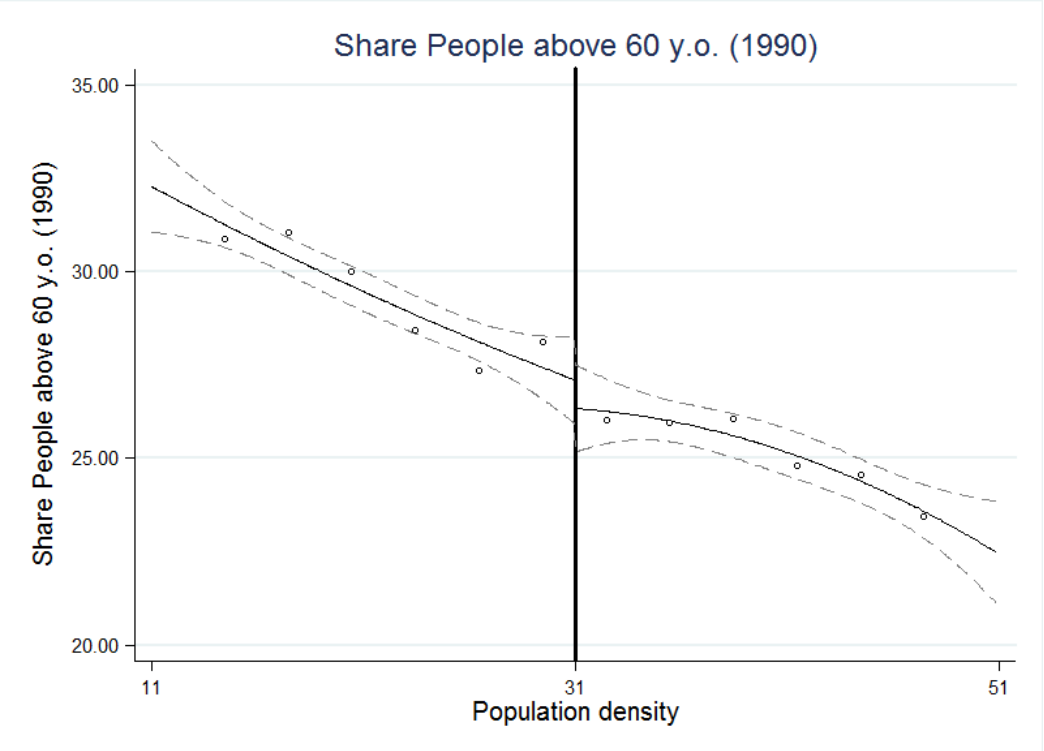


Figure 13: Income in 1990

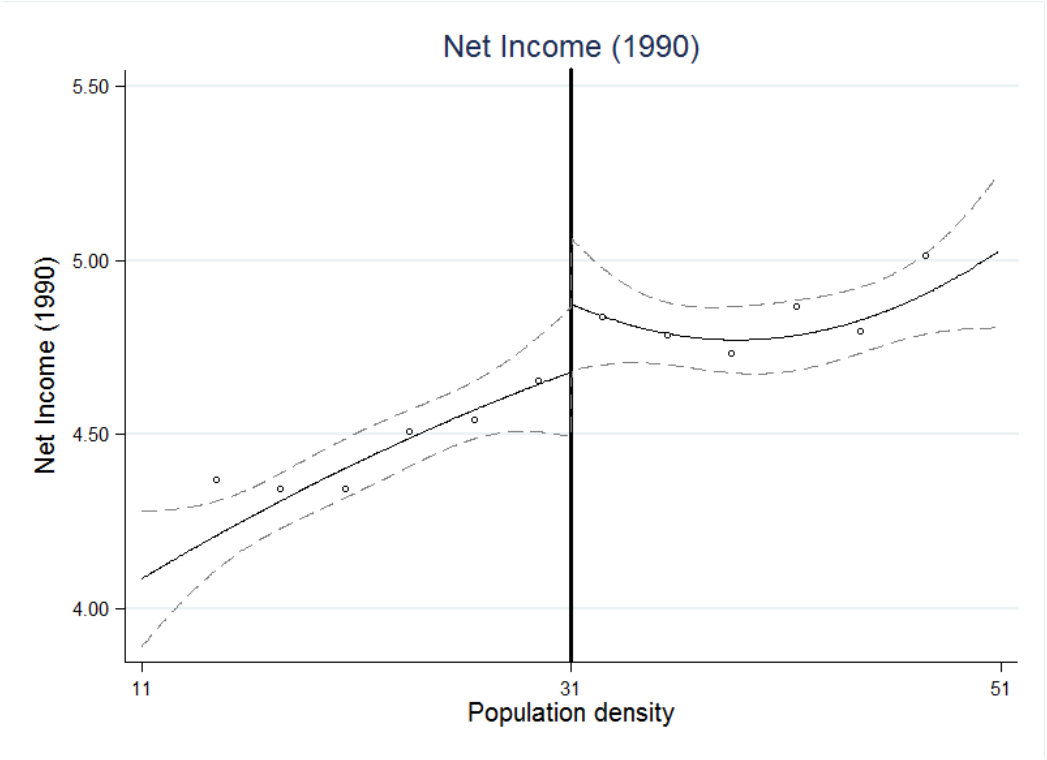


Figure 14: Employment growth (1982-90)

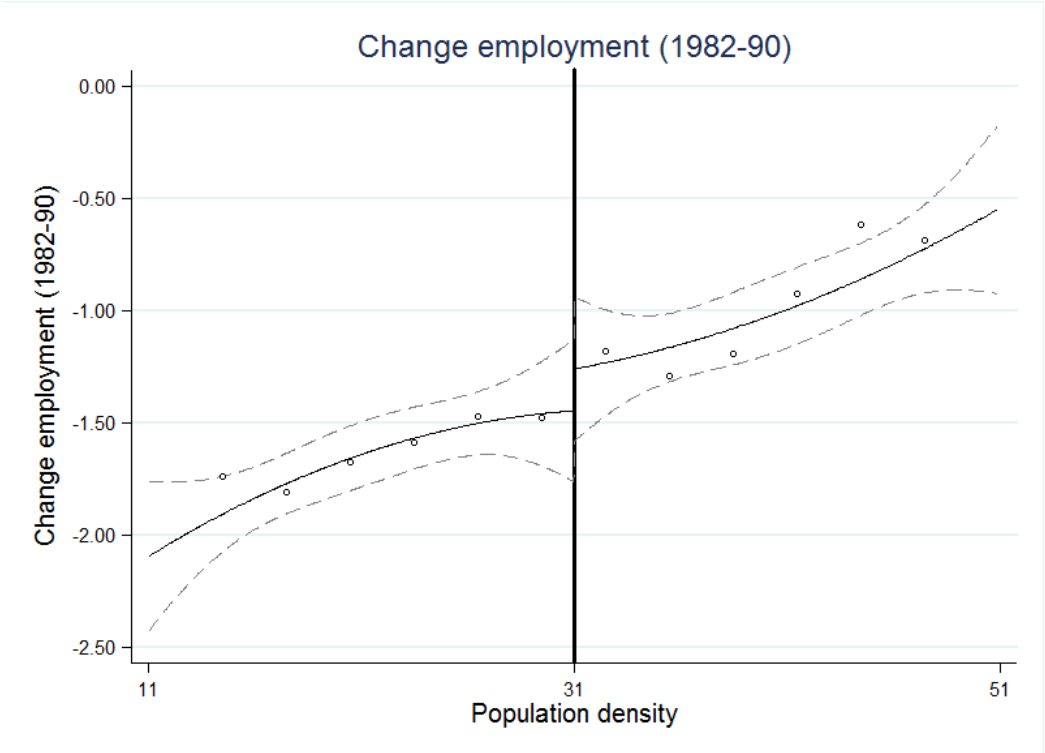


Figure 15: Unemployment rates (1990)

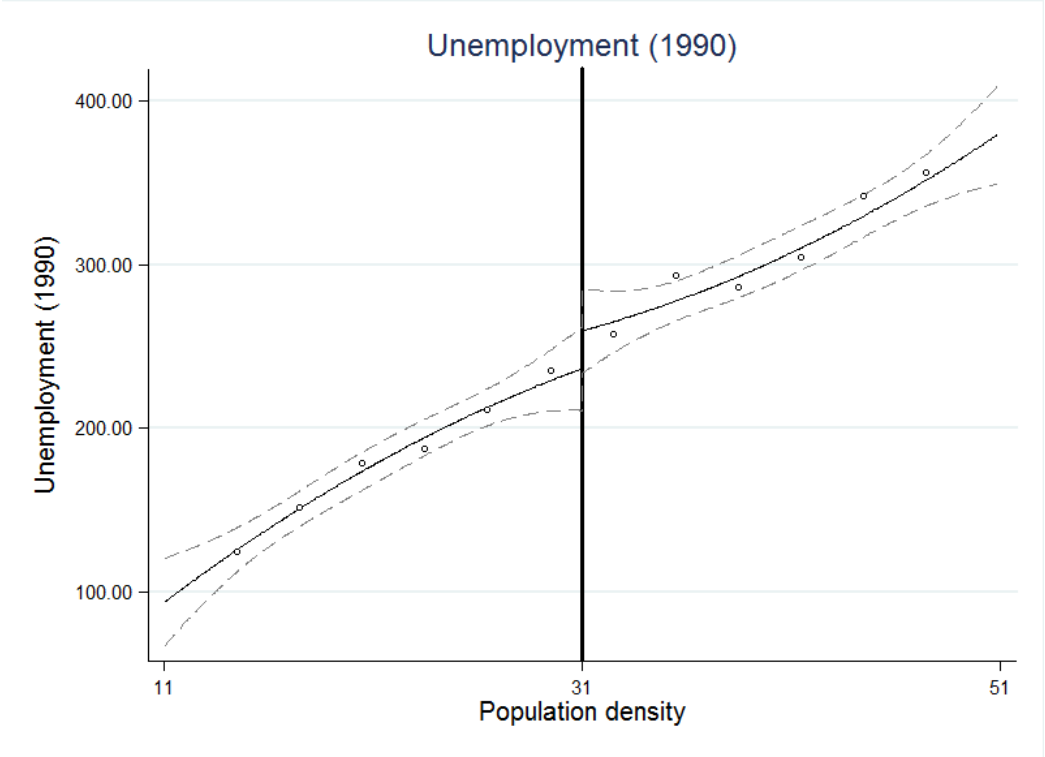


Figure 16: Isolated cantons in the ZRR program

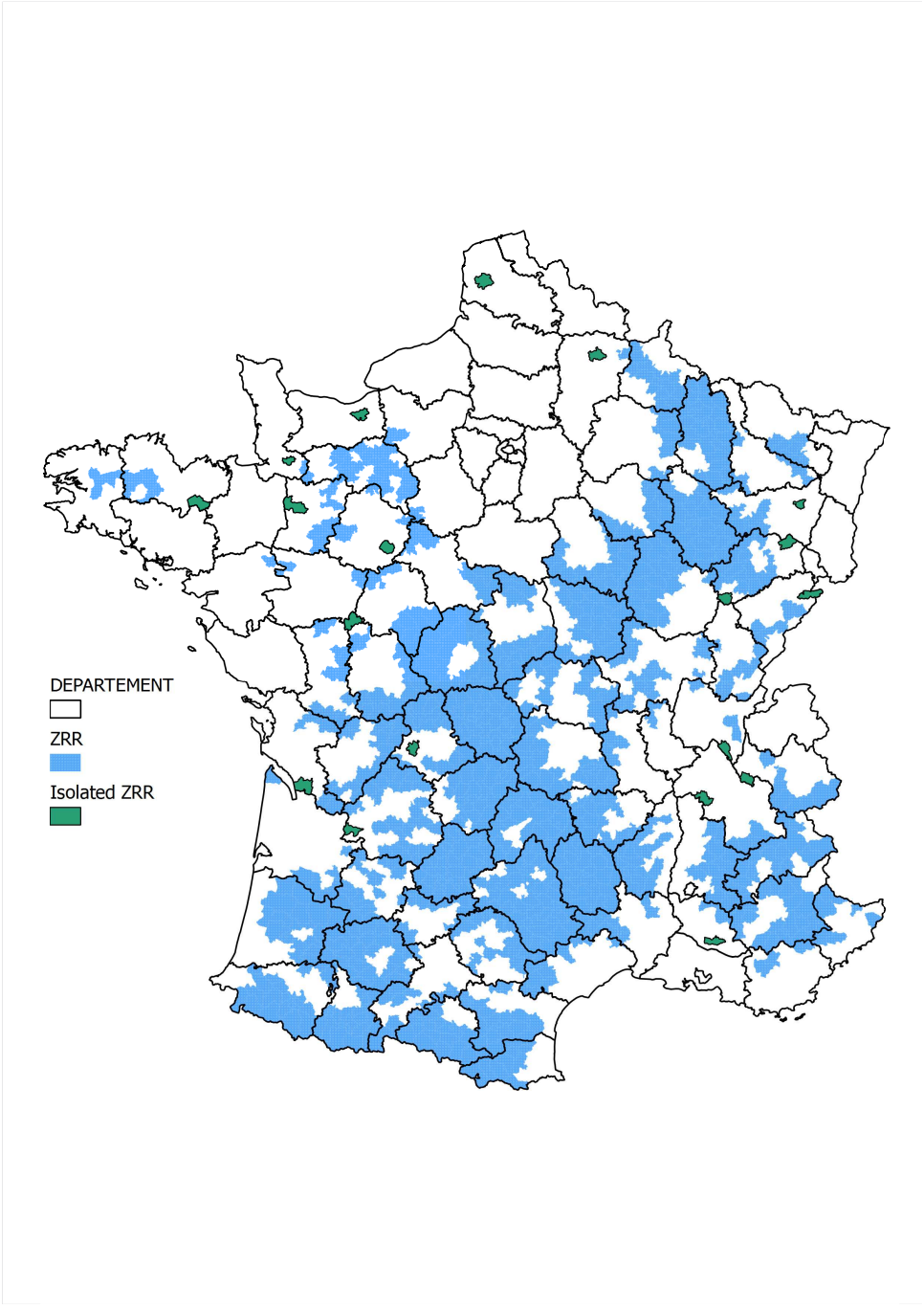


Table 10: Summary statistics on corporate and business tax in future ZRR (1995)

	Corporate tax		Business tax	
	% of non-taxed firms	Median tax paid*	% of non-taxed firms	Median tax paid*
Total	0,23	0,6	0,84	3,2
Agriculture	0,37	0,3	0,88	2,7
Food industry	0,13	1,1	0,86	4,6
Final good ind.	0,26	0,9	0,77	3,7
Car industry	0,14	3,7	0,62	5,4
Capital good ind.	0,17	1,4	0,72	5,9
Intermediate good ind.	0,18	3,5	0,63	7,8
Energy	0,17	1,5	0,66	6,1
Building	0,21	0,5	0,88	2,6
Retail	0,18	0,9	0,83	3,7
Transportation	0,19	2,0	0,75	4,0
Finance	0,41	0,5	0,50	6,4
Real estate	0,37	0,2	0,90	1,2
Business services	0,37	0,6	0,72	2,6
Household serv.	0,23	0,5	0,89	1,4
Health, educ.	0,13	4,4	0,62	3,7

***:** for taxed firms - in thousand of euros

Reading note: for all French companies present in 1995 in future ZRR, we estimate the proportion that did not pay any business tax and estimate the median business tax for those having a strict positive business tax.

Source: Fiscal database (BRN-RSI), Insee.

Table 11: Distribution of total wage employment and share of public interest organizations, by sector of activity (ZRR zoning, 2005)

	% of employees working in	Share of employment in PIOs
Agriculture	1,7	0,0
Industry		
Food ind.	6,2	0,0
Car ind.	0,6	0,0
Final good ind.	4,5	0,0
Capital good ind.	3,9	0,0
Intermediate good ind.	12,9	0,0
Energy	0,7	0,0
Services		
Building	11,2	0,0
Retail	15,6	0,0
Transportation	5,7	0,0
Finance	1,5	0,0
Real estate	0,8	0,0
Firms serv.	6,6	2,6
Household serv.	6,6	8,3
Health and education	19,6	86,5
Administration	1,8	52,9
Total	100	18,7

Notes: The first column provides the distribution of wage earners across sector of activity in the rural EZ areas in 2005. For each sector of activity, the second column gives the share of wage-earners working in public interest organizations (PIOs).
Source: DADS, Insee.

Table 12: Overall impact of the 1996 rural EZ program, with spillover effects

	A. Local employment (annual growth rate)							
	1996-98				1996-2004			
EZ	0.003	0.005	0.005	0.005	-0.002	-0.002	-0.000	-0.002
	(0.009)	(0.009)	(0.008)	(0.009)	(0.003)	(0.003)	(0.003)	(0.003)
Share of neighbors in rural EZ	0.001	-0.002			0.003	-0.003		
	(0.020)	(0.034)			(0.007)	(0.012)		
At least one neighbor in rural EZ			-0.017	-0.017			-0.009	-0.004
			(0.017)	(0.023)			(0.006)	(0.008)
1995 industry mix	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes
Observations	784	784	784	784	784	784	784	784
Average of outcome	0.021	0.021	0.021	0.021	0.015	0.015	0.015	0.015
S.d. of outcome	0.068	0.068	0.068	0.068	0.025	0.025	0.025	0.025
	B. Number of businesses (annual growth rate)							
	1996-98				1996-2004			
EZ	0.005*	0.003	0.005*	0.003	0.002	0.001	0.003**	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.001)	(0.002)	(0.001)
Share of neighbors in rural EZ	-0.003	0.001			0.001	-0.004		
	(0.007)	(0.011)			(0.004)	(0.005)		
At least one neighbor in rural EZ			-0.002	0.006			-0.007**	-0.006
			(0.006)	(0.007)			(0.003)	(0.004)
1995 industry mix	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes
Observations	784	784	784	784	784	784	784	784
Average of outcome	-0.003	-0.003	-0.003	-0.003	0.008	0.008	0.008	0.008
S.d. of outcome	0.025	0.025	0.025	0.025	0.013	0.013	0.013	0.013

Notes: 2SLS estimations of the impact of EZ on the annual growth rate of local employment (A) and the number of businesses (B) in the short (96-98) and long (96-04) run taking into consideration potential spillover effects. For each specification, EZ is instrumented by $\mathbf{1}_{dens\leq 31}$, the share of neighbors in EZ by the share of neighbors with a population density below 31 per km^2 and whether at least one neighbor is in EZ by a dummy that equates 1 if at least one neighbor has a density below 31. In columns 1, 2, 5 and 6 we restrict the sample to the cantons with a density above 21 or below 41 and do not include linear control in the density. In columns 3, 4, 7 and 8 the sample includes cantons with a density above 11 and below 51 and we include linear controls of the density separately on either side of the threshold. The neighboring cantons refer to the cantons that have a border in common with the canton of reference. Standard errors in parentheses. ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.
Sources: DADS and Population Census 1990, Insee.

Table 13: Quantile treatment effects of the 1996 rural EZ program

	A. Local employment (annual growth rate)					
	1996-1998			1996-2004		
	q25	q50	q75	q25	q50	q75
EZ	-0.002	0.004	0.005	-0.002	-0.001	-0.002
	(0.006)	(0.006)	(0.007)	(0.003)	(0.003)	(0.003)
Observations	784	784	784	784	784	784
Quantile value	-0.009	0.016	0.042	0.002	0.015	0.028
S.d. of outcome	0.068	0.068	0.068	0.025	0.025	0.025

	B. Number of businesses (annual growth rate)					
	1996-1998			1996-2004		
	q25	q50	q75	q25	q50	q75
EZ	0.001	0.004	0.003	0.000	-0.001	0.000
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)
Observations	784	784	784	784	784	784
Quantile value	-0.017	-0.004	0.010	-0.001	0.006	0.015
S.d. of outcome	0.025	0.025	0.025	0.013	0.013	0.013

Notes: Quantiles treatment effects of EZ on the annual growth rate of employment (A) and the number of business (B) based on Abadie et al. (2002). EZ is instrumented by $\mathbf{1}_{dens \leq 31}$ as in the 2SLS estimations. The results are provided for the 25^{th} quantile (q25), the median (q50) and the 75^{th} quantile of the outcome variable. The sample is restricted to the cantons with a population density between 21 and 41 per km^2. Controls for the 2003 industry mix and region dummies are included. Bootstrapped standard errors (1000 replications). ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.
Source: DADS, Insee.

Table 14: Quantile treatment effects of the 2005 rural EZ program: new businesses

	A. Employment in new businesses (<3 years; annual growth rate)					
	2005-2007			1996-2004		
	q25	q50	q75	q25	q50	q75
EZ	0.047	0.033	0.042	-0.020	-0.012	0.036
	(0.057)	(0.055)	(0.054)	(0.030)	(0.033)	(0.034)
Observations	771	771	771	772	772	772
Quantile value	-0.144	0.059	0.265	-0.084	0.025	0.164
S.d. of outcome	0.398	0.398	0.398	0.220	0.220	0.220

	B. Number of new businesses (annual growth rate)					
	1996-1998			1996-2004		
	q25	q50	q75	q25	q50	q75
EZ	0.008	0.025	0.006	-0.009	-0.004	-0.003
	(0.029)	(0.032)	(0.030)	(0.019)	(0.015)	(0.022)
Observations	771	771	771	772	772	772
Quantile value	-0.077	0.067	0.203	-0.033	0.039	0.113
S.d. of outcome	0.230	0.230	0.230	0.125	0.125	0.125

Notes: Quantiles treatment effects of EZ on the annual growth rate of employment in new businesses (A) and the number of new business (B) based on Abadie et al. (2002). EZ is instrumented by $\mathbf{1}_{dens\leq 31}$ as in the 2SLS estimations. The results are provided for the 25^{th} quantile (q25), the median (q50) and the 75^{th} quantile of the outcome variable. The sample is restricted to the cantons with a population density between 21 and 41 per km^2. Controls for the 2003 industry mix and region dummies are included. Bootstrapped standard errors. ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.
Source: DADS, Insee.

Table 15: Descriptive statistics in rural EZ and isolated rural EZ areas

	Rural EZ		Isolated rural EZ	
	Mean	s.d.	Mean	s.d.
	(1)		(2)	
			A. All firms	
Employment (1995)	556.71	471.69	488.74	224.30
Employment (annual growth rate 1995-2004)	0.02	0.02	0.01	0.02
Number of businesses (1995)	197.95	122.57	168.89	63.11
Number of businesses (annual growth rate 1995-2004)	0.01	0.01	0.01	0.02
			B. Firms with 1 to 50 salaried workers	
Share of total employment	0.44	.	0.41	.
Employment (1995)	246.59	191.01	200.89	78.71
Employment (annual growth rate 1995-2004)	0.02	0.03	0.02	0.03
Number of businesses (1995)	60.67	41.52	48.63	18.23
Number of businesses (annual growth rate 1995-2004)	0.01	0.02	0.02	0.02
			C. New firms (<3 years old)	
Share of total employment	0.05	.	0.04	.
Employment (2005)	32.07	55.12	24.58	15.38
Employment (annual growth rate 2005-2008)	0.06	0.27	0.11	0.26
Number of businesses (2005)	8.70	6.77	9.05	4.90
Number of businesses (annual growth rate 2005-2008)	0.05	0.16	0.06	0.14
			D. Public interest organizations	
Share of total employment	0.20	.	0.13	.
Employment (2005)	129.78	159.71	71.95	94.36
Employment (annual growth rate 2005-2008)	0.04	0.12	0.02	0.07
Number of businesses (2005)	7.58	5.92	5.47	4.13
Number of businesses (annual growth rate 2005-2008)	0.00	0.09	-0.02	0.09
Number of cantons	787		19	

Notes: Comparisons of mean employment and business count (in 1995) and annual growth rates (1995-2004) between rural EZ (column 1) and isolated rural EZ (column 2) areas. The different panels correspond to sub-sample of firms that were particularly targeted by the exemptions of payroll tax (Panels B and D) or corporate and business taxes (Panel C). The sample is restricted to cantons with with a population density between 11 and 51 per km^2 for rural EZs (as in Table 2), and between 21 and 41 for isolated rural EZs (as in Table 9).
Source: DADS, Insee.

Table 16: Impact of 2005 rural EZ program on wages in public interest organizations

	A. Wages (annual growth rate)							
	2005-2007				2005-2009			
EZ	0.012	0.014	0.067	0.049	-0.011	-0.019	-0.007	-0.019
	(0.024)	(0.030)	(0.045)	(0.048)	(0.009)	(0.012)	(0.016)	(0.017)
$\mathbf{1}_{dens\leq 31} \times (dens - 31)$			0.001	0.003			0.001	0.000
			(0.006)	(0.008)			(0.002)	(0.003)
$\mathbf{1}_{dens>31} \times (dens - 31)$			0.004	0.006			0.003**	0.005***
			(0.004)	(0.005)			(0.002)	(0.002)
$\mathbf{1}_{dens99\leq 31} \times (dens99 - 31)$			0.003	-0.000			-0.001	-0.000
			(0.007)	(0.008)			(0.002)	(0.003)
$\mathbf{1}_{dens99>31} \times (dens99 - 31)$			-0.002	-0.004			-0.003**	-0.005***
			(0.004)	(0.005)			(0.002)	(0.002)
2005 industry mix	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes
Observations	774	774	1,431	1,431	772	772	1,428	1,428
Average of outcome	0.0353	0.0353	0.0298	0.0298	0.0294	0.0294	0.0295	0.0295
S.d. of outcome	0.221	0.221	0.234	0.234	0.0865	0.0865	0.0848	0.0848

Notes: 2SLS estimations of the impact of EZ on the annual growth rate of wages in the short (2005-07) and long (2005-09) run and for public interest organizations only. For each specification, EZ is instrumented by $\mathbf{1}_{dens\leq 31}$ and $\mathbf{1}_{dens99\leq 31}$ which equate 1 if the canton has a population density below or equal to 31 inhabitants per km^2 at the 1990 and 1999 censuses, respectively. In columns 1, 2, 5 and 6 we restrict the sample to the cantons with a density above 21 or below 41 and do not include linear controls in the density. In columns 3, 4, 7 and 8 the sample includes cantons with a density above 11 and below 51 and we include linear controls of the density separately on either side of the threshold. Standard errors in parentheses. ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively. Sources: DADS and Population Census 1990, Insee.