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Tax exemptions and rural development: Evidence from a quasi-experiment

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Abstract

This paper provides quasi-experimental (RDD) estimates of the impact of a tax credit program targeted at less densely populated areas. The program was launched in the mid 1990s in rural France and includes corporate and payroll tax exemptions. Variations over time and across firm types allow un-bundling the overall program impact into three components: a quite restrictive, short-term (1-year) payroll tax exemption on new hires; permanent payroll tax exemptions in the non-profit sector; and corporate tax exemptions for new firms. We find no significant impact of the program on total employment or the number of plants, and no impact of the different program components on targeted subsets of firms. Large positive effects can be statistically rejected.

Keywords: Tax exemptions; rural development; enterprise zones.

JEL codes: J23, J32, H32.

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1 Introduction

Tax credits and subsidies to disadvantaged labor markets – such as the state Enterprise Zone or the federal Empowerment Zone (EZ) programs in the U.S. – are widely used by policy makers aiming at reducing spatial inequalities. While the theoretical case for EZ programs is mixed¹, existing empirical evaluations yield contrasted, yet mostly negative results (Papke (1994), Bondonio and Greenbaum (2007), Neumark and Kolko (2010), Oakley and Tsao (2006)). To yield general lessons, the empirical literature at least faces two challenges: finding valid sources of variation to identify the impact of programs whose allocation is driven by a political, highly endogenous process; un-bundling the impacts of programs that often include many heterogeneous components.

The recent literature uses a variety of approaches. A first group of evaluations uses cross-sectional comparisons with propensity score matching (O’Keefe (2004), Elvery (2009)).² The source of identification that is used in the remaining of the literature amounts to a difference-in difference (DD) approach. Two approaches are available to mitigate endogeneity issues – by which a zone is selected to participate to an EZ because of its (future) labor market dynamics for example. The first one is to control for zone-specific trends using past data. This corresponds to the random growth model estimated in Papke (1994) for the evaluation of the Indiana enterprise zone. The author finds a 19% decline in unemployment claims. The second one is to select control groups whose dynamics are likely more comparable. Bondonio and Greenbaum (2007) estimate the impact of the state enterprise zones using a difference-in-difference matching approach. They separately observe outcomes for new, existing and vanishing firms and show that despite the mean zero effect of the state enterprise zone program on employment, the effect is positive for new firms and negative for vanishing firms. Busso and Kline (2008) follow the same procedure but further restrict the sample to areas that are selected into EZs at some point of time. They conclude on a 3% effect of the federal empowerment zone program on the employment rate. Neumark and Kolko (2010) combine the different approaches. They

¹The differential treatment of neighboring areas may have strong distorting effects, diverting investments from non-benefiting areas and leading in lower average returns on investments.

²O’Keefe (2004) concludes on a 3% increase of employment growth due to the California’s enterprise zone program (which is one of the most generous ones). It raises employment growth by 3% a year during the six years following the implementation, in particular in the businesses that are targeted. However, this impact is offset by the -3% effect found for the following 6 years of the program. Similarly, Elvery (2009) finds no impact of the state enterprise zone on resident employment.

restrict the sample to areas that will be selected in future draft as control groups but also take into account geographical proximity to reweigh observations. They find a small and insignificant effect of the enterprise zone program (0.4%) on the annual growth rate. Last, Ham, Swenson, Imrohglu, and Song (2011) provide a comprehensive evaluation of different programs using combinations of difference-in-difference comparisons, as well as flexible random growth models (including quadratic trends). They conclude on a 3% decrease in the unemployment rate in California, Massachusetts and New York enterprise zones but the impact is not significant in less generous states.

This paper evaluates the medium-run impact of a tax credit program targeted at less densely populated areas in France, that was launched in the mid 1990s and is comparable to US Enterprise Zone programs (henceforth, the rural EZ program). In 2008, the program cost was above 400 million euros, which corresponds to about 80 euros per capita in the covered areas. The program has two components: corporate and payroll tax exemptions. These components vary over time and across firm types (new vs. old, small vs. large, for profit and non-profit³). These variations allow disentangling the overall program impact into three components: a quite restrictive, short-term (1-year) payroll tax exemption on new hires; permanent payroll tax exemptions in the non-profit sector; and corporate tax exemptions for new firms. Labor market impacts are measured in terms of employment and number of plants using a fuzzy regression discontinuity design (RDD): the main criterion used to allocate the program to a given area is indeed whether its population density was above or below a threshold of 31 inhabitants per square kilometer at a previous population census.

We find no impact of the rural EZ program on total employment or the number of plants. While this global result may indicate that the program is too diluted to have detectable average effects⁴, we also find no effect when analyzing program components that are more narrowly and intensively targeted at subsets of firms. Extending corporate tax exemptions from the first 5 to the first 14 years of existence of firms does not increase the number or the size of new firms. Permanent payroll tax exemptions also have no impact on the growth of the non-profit sector. Such zero-effect results may seem uncomfortable: standard models

³More precisely, a group of “public interest organizations” (*Organismes d'intérêt général*) is specifically targeted by a more generous scheme starting in 2005; see section 2.

⁴As the amount of 80 euros per capita may reflect. Note however that this amount also reflects the firms' take-up behavior, and not only the program's generosity.

imply that the tax exemptions should have led to some reallocation of economic activities into or within rural EZs. The RDD set-up makes identification issues rather unlikely. As usual, the precision of the estimates involves a trade-off between (potential) bias and variance. It seems however that large positive effects can be rejected. Rather, the results suggest that the incentives were not strong enough in the case of profit maximizing firms – the limited savings were probably outweighed by the implied administrative burden or the associated stigma. Non-profit firms did seize the opportunity of making substantial and long-lasting savings on payroll costs; however, their development is not affected by the savings, implying a large dead weight loss for the government.

These results extend the literature in several ways. First, applying the RDD framework on areas that are not geographically contiguous allows identifying impacts without facing the potential bias due to border effects when comparing adjacent areas. Second, we are able to separately evaluate the impact of different types of tax cuts (even though on different subsets of firms). Last, we add evidence on the impact of EZ programs on rural areas, that have been much less evaluated than urban areas. Our results are consistent with previous evaluations of the French rural EZ program (in its 1995 version) using panel data and matching approaches (Lofredi and Schmitt (2006) and Cardot, Musolesi, and Schmitt (2012)). They substantially extend the RDD results by Lorenceau (2009) by using more reliable administrative data, considering different target groups of firms, and analyzing the more generous 2005 scheme. The absence of impact is consistent with Devereux, Griffith, and Simpson (2007) finding that firms are less responsive to government subsidies in areas where there are fewer existing plants in their industry. It is also consistent with results by Briant, Lafourcade, and Schmutz (2012) on the impact of geographical constraints on the effectiveness of local tax breaks.

The outline of the paper is as follows: In Section 2, we describe the French rural EZ program. In Section 3 we describe our econometric approach and the data. In Section 4 we present our estimates of the overall impact and the impact of its different components. Section 5 concludes the paper.

2 The French rural EZ tax exemptions

The French Rural enterprise zone (*Zones de revitalisation rurale*) program was created by law in 1995, but the list of eligible zones and the details of implementation were only defined in February 1996. It is a complex package of tax exemptions that evolved over time. The total cost of the rural EZ tax exemptions (in the French state budget) is above 400 million euros in 2008 (equivalent to about 80 euros per inhabitant in the covered areas). The two main components of that cost are payroll tax exemptions (89%) and corporate tax exemptions (11%) (Daniel, Garcia, Roche-Bruyn, Ruiz, Verlhac, and Sardais (2009)). Only a subset of firms and jobs are eligible for the exemptions. The general principle over the period is that corporate tax exemptions are restricted to newly created firms (during 5 to 14 years), while payroll tax exemptions are granted for new hires in firms with less than 50 employees. The exact rules of eligibility, the generosity and the duration of the exemptions however evolve over time, as detailed below.⁵

The explicit goal of policy makers when creating rural EZs was to ensure equity in terms of development opportunities throughout the whole territory. The scheme was truly conceived as a “positive discrimination” measure toward the most backward areas.⁶ During the first phase of implementation (1996-2004) local job and business creations are put forward in order to eventually prevent the inhabitants from leaving the zone. From 2005, an additional dimension is highlighted: the availability of services has to be guaranteed and sustained at the local level. A parliamentary amendment modifies the scheme and more generous tax exemptions are granted to public interest organizations (PIOs). Interestingly, the member of parliament promoting the amendment did not have

⁵As they are the main source of the evaluations, it is worth noticing the features that are generally shared by most of the US states enterprise zone programs. They aim at boosting urban depressed areas by providing tax breaks and business incentives. Most of the time the declared objective is to create jobs and relocate businesses and local hiring is required. The most important determinants of area eligibility are economic factors such as unemployment, income, education level. Another particularity is the fact that quite often, local communities have to apply for becoming an EZ. However, there is considerable variation in the generosity of the schemes across states, in the way the incentives are provided as well as in the eligibility rules for the firms. For example, the yearly potential benefit for a firm ranges from \$32000 per employee living in the EZ in California to \$900 in Minnesota. The tax benefits can be in form of income tax, property tax or sales/profit tax. In some states firms are required to increase their total employment to benefit from a scheme while in others it is targeted to low-skill or young residents etc. At the end, it is important to keep in mind that most of the evaluation can not distinguish between the different channels and generally estimate the global impact of one or more enterprise zones. From a policy perspective, this is clearly a limit of the current literature.

⁶Such positive discrimination remains quite controversial in France, making the rural EZ program a quite original measure in the French legislative landscape.

in mind the large employers (in the health and education sectors) that turned out to be the main recipients of the scheme; rather, the goal was to maintain small associations.⁷

2.1 Zone definition

In France, zoning policies have originated with the definition of large zones by the European Commission in the late 1980s. It has immediately been followed at the national level by the designation of rural territories to be developed in priority (TRDP). Within these TRDP, the rural EZs aim to isolate the most depressed areas. The zoning is based on three combined criteria.

First, a demographic criterion requires that a municipality⁸ be either part of a *canton*⁹ with a population density that is less than 31 inhabitants per km^2 or belongs to a district¹⁰ with a population density under 33 inhabitants per km^2 .

If a municipality is part of a *canton* or a district with a population density lower than 5 inhabitants per km^2 , it is automatically classified in 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: the municipality should belong to a TRDP.

Between 1995 and 2005, the zone definition remains unchanged and the criteria are computed on the basis of the 1982 and 1990 population censuses. In 2005 a new law is passed, and the zoning is slightly modified in two ways. First, the reference to the TRDP zoning stops and a municipality is now required to join an inter-communal establishment (EPCI), which is a group of municipalities that manage local public services jointly.¹¹

⁷“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, Garcia, Roche-Bruyn, Ruiz, Verlhac, and Sardais (2009).

⁸The municipality (*commune*) is the smallest French administrative subdivision. At January 1st, 2010, there were 36,570 municipalities in Metropolitan France.

⁹A *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 belong to several cantons.

¹⁰Districts (*arrondissements*) are administrative subdivisions of the 96 French *départements* and a group of cantons that respects the boundaries of municipalities.

¹¹The declared objective was not to modify the zoning but rather to provide incentives to the municipalities to organize themselves in EPCI.

Second, the 1990 and 1999 censuses become the new reference for the computation of the criteria. However, there are no immediate changes in the zoning.¹² Overall, the rural EZ zoning is extremely stable. Taking into accounts the new statuses in the post-2005 analysis makes little empirical difference.

2.2 Main features of tax exemptions

Payroll tax exemptions

As detailed in tables 1 and 2, the program witnessed two main phases: from 1996 to 2004; and from 2005 onward. Exemptions are more generous in the second phase. Consider payroll tax exemptions first. From 1996 to 2004, the payroll tax exemptions remain concentrated on a restricted number of jobs: only firms with less 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 contribution 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 low wage workers. 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 pre-existing payroll tax exemptions are less generous at these levels). Figure 1 shows the evolution of the net labor cost cut implied by the

¹²Different statuses are granted. Municipalities filling the three criteria are given a ‘permanent’ rural EZ status. Municipalities that are not part of an EPCI yet but verify other criteria are categorized as ‘conditional’. They have to comply by 2007 or they will be excluded of the zoning. Finally, municipalities for which the demographic criteria are not respected anymore when the reference to the new census is made are planned to be progressively phased out. They are given a ‘temporary’ status and still benefit from the tax exemptions until 2007 at which date they will be excluded.

rural EZ program over time, at different wage levels. The national program of payroll tax exemptions for low wage workers became increasingly generous over the period, implying that the rural EZ program makes less difference over time.

A law passed in 2005 introduces a substantially more generous scheme for a subgroup of employers located in rural EZs, known as “public interest organizations” (PIOs).¹³ Public interest organizations receive this label from the fiscal administration following two criteria: providing a public service and being non profit.¹⁴ The category includes some NGOs, but also non-profit hospitals or clinics. As employers, public interest organizations (whatever their size) become eligible for the same payroll tax exemption as above, but for all their employees (not only new hires) and the full length of their contract. As a consequence of the new scheme, the budgetary cost of rural EZ payroll tax exemptions increases sharply from 38 millions euros in 2005 to 418 million euros in 2008 (Daniel, Garcia, Roche-Bruyn, Ruiz, Verlhac, and Sardais (2009)). In an attempt to curb down these costs, access to the new scheme is closed for workers hired after November 2007.¹⁵ The exemption is however maintained for the stock of employees hired by public interest organizations before that date.

Corporate tax exemptions

The second main measure is the corporate tax exemption (see table 2). Its main component is the corporate tax exemption for newly created firms. Until 2004, there is full exemption over the first two years, and partial exemption over the next three years. The measure is substantially extended in 2005 (with a retroactive effect on firms created after January 1, 2004): full exemption is granted for the first four years; the exemption then progressively fades out over the next ten years.

¹³The measures for firms with less than 50 employees are maintained unchanged after 2005, if these firms do not qualify as public interest organizations.

¹⁴The label is used to grant tax incentives for charitable giving: individual donors to public interest organizations are eligible for income tax rebates.

¹⁵For new hires, public interest organizations may be eligible for the initial rural EZ payroll tax exemption scheme if they have less than 50 employees.

3 Econometric approach and data

3.1 Data

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

First, the French business register (SIRENE) follows all French firms and plants. The information for each plant covers: the location (at least the ZIP 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.¹⁶

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. Employment can be measured in various ways: full-time equivalents over a year, number of employees on the payrolls at one point of the year or the other, number of employees as of January 1st. 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 (on January 1st of each year), 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 sub-populations of firms. This allows looking at the impact of the different components of the program, which are targeted to 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. This allows to define subsamples of jobs/plants in new firms (less than 3 years of existence) and in small firms (less than 50 employees). As described in section 2, the definition of PIOs is not immediate and this status is granted by the fiscal administration; no register following all PIOs is available. We use the administrative forms filled by

¹⁶The register also keeps track of plants and firms relocation (the number of new plants created from January 1st to December 31st 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 registration number changes and the file corresponding to the flow of plants relates the new and the old registration numbers.

PIOs when recruiting new workers to create a proxy for PIO status combining the firm’s legal status (at the 4-digit level) and the industry (at the 4-digit level), that can be used with our main data sources.

3.2 Econometric model

We take advantage of the discontinuous criterion introduced by the 1995 law (and still used in the 2005 law), which targets areas (*cantons*) with a population density below 31, in order to assess the impact of the rural EZ program. Figure 2 displays the probability that a given area benefits from the rural EZ program as a function of the forcing variable, the 1990 population density (henceforth, *dens*). The probability drops sharply at the 31 threshold. Under the assumption that $E[y_0|dens]$ (the potential outcome regression function in the absence of program) is a continuous function of *dens*, this fuzzy regression-discontinuity design identifies a local average treatment effect (LATE) of the program (see Hahn, Todd, and Van der Klaauw (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 pre-determined: population density was 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 an innocuous 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 figures 12-15 in the appendix, 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 3 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 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 larger window (population density between 11 and 51 inhabitants per square kilometers), we estimate

$$g = a + bEZ + c\mathbf{1}_{dens \leq 31} \times dens + d\mathbf{1}_{dens > 31} \times dens + x\beta + \epsilon, \quad (1)$$

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. x denote control variables that can be used to increase precision. This regression allows for a continuous linear effect of the forcing variable on each side of the threshold.

Alternatively, we consider a smaller window (population density between 21 and 41) and estimate

$$g = a' + b'EZ + x\beta' + \epsilon', \quad (2)$$

by 2SLS, instrumenting EZ (the fact to be an enterprise zone) with $\mathbf{1}_{dens \leq 31}$. On this smaller window, the underlying assumption is that of difference-in-difference approaches: employment are 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. More generally, we show standard RDD graphs that allow assessing the sensitivity of the estimation to choices of the estimation window and of functional forms.

4 Impact of the rural EZ program and its components

4.1 Graphical evidence

Figures 4-11 display graphical evidence on the reduced form of models (1) and (2) for different outcome variables and different subpopulations. In each graph, average growth rates are plotted against the forcing variable (*dens*) over ranges of 2 inhabitants per square kilometers ($]11, 13]$, $]13, 15]$, ...). Following Lee (2008), we fit a polynomial of second order on each side of the threshold, represented by a full line.

Overall impact

Figures 4 and 5 consider global impacts of the rural EZ program on total employment and the number of plants, respectively. We consider three time horizons: 1996-98, 1996-2001, 1996-2004. Over each of these periods, we compute average yearly growth rates.¹⁷ As a specification check (or “placebo test”), the bottom right figure considers changes between 1995-96, i.e. before the law of effective. None of the four graphs in figure 4 displays a discontinuous increase in employment around the 31 threshold. There is no discontinuity before the policy is implemented (at least, no more discontinuity at 31 than at 23, say), which is consistent with the RDD identifying assumption. The top left graph may suggest a short-run positive impact, with higher growth rates at the left of the threshold. However, growth rates seem rather noisy (varying somewhat randomly with the forcing variable), which may simply reflect sampling variations. This interpretation is supported by the next two graphs over the medium and long run: employment growth is not correlated with initial population density anymore, and the regression function seems continuous at the threshold.

The four graphs in figure 5 deliver a similar message. To sum up, the average employment growth and the net creation of plants seem unaffected by the rural EZ program. Nonetheless, these average effects may combine a zero or negative effect on non-targeted

¹⁷To reduce the noise associated with a particular endpoint, 1998, 2001 and 2004 correspond to average employment (resp. number of plants) over 3-year windows: 1997-99, 2000-02 and 2003-05.

firms¹⁸ and a positive impact on targeted firms. In order to statistically detect the impact on targeted firms, one may need to analyze them separately. We now turn to this more disaggregated analysis.¹⁹

Impact of temporary tax exemptions on small firms

Figures 6-9 consider only wage employment and distinguish jobs located in firms with more or less than 50 employees. Only firms with less than 50 employees are eligible for the temporary payroll tax exemptions in place between 1996 and 2004. This is therefore a way to focus on the impact of this particular component of the program. Figure 6 focuses on employment in firms with less than 50 employees.²⁰ The picture is however quite similar to the picture for total employment (of which small firms actually make a large share in less densely populated areas. There is no clear-cut evidence that employment in small firms grew faster below the 31 threshold than above. As shown on figure 7, there is also no indication (except perhaps in the short run) that the corresponding number of plants increased.

Evolutions documented for larger firms in figures 8 and 9 do not display clear patterns. Note that an effect of the rural EZ program (if any) would be a rather indirect crowding-out effect.²¹ Given the sampling variations implied by the relatively small number of large firms in these areas, it is not surprising that we do not detect any effect.

Effects of permanent payroll tax exemptions and more durable corporate tax exemptions

Figures 10 and 11 focus on the period after 2005. At that date, the program became more generous for PIOs on the one hand (as the payroll tax exemptions became permanent) and for new firms on the other hand (as the corporate tax exemption became more generous and more durable). Figure 10 contrasts PIOs and non-PIOs in terms of employment

¹⁸A negative effect would be due to displacement (or crowding out) effect if non-targeted firms are in competition with targeted ones on the market for inputs or for goods.

¹⁹To complete the analysis, we looked in more details at plant creations and destructions; we found no significant results and do not report this part. Significant changes in plant creations and destructions would anyway be a second-order result if there is no impact on net creations.

²⁰With a restriction to the industries covered by the payroll tax exemption.

²¹These firms are not eligible for the payroll tax exemptions and very few of them are new firms eligible for the corporate tax exemptions.

growth and growth of the number of plants over the three years after the new scheme is in place.²² PIOs benefited from permanent payroll tax exemptions over that period. If anything, the graphs suggest faster growth for non-PIOs below the 31 threshold, but no difference for PIOs. This is hard to reconcile with the fact that the scheme was made more generous for PIOs and not for other firms. Sampling variations may be the reason, unless a positive effect for non-PIOs is explained by the more generous corporate tax exemptions for new firms among them. There is however no evidence of this on figure 11.²³ Firms younger than 3 years in 2008 have benefited from a more generous treatment and are expecting a more durable corporate tax exemption scheme than firms younger than 3 years in 2005: we would expect that the number of jobs located in these firms has increased. Again, if anything, a discontinuity may seem to appear only for firms that are not targeted – here, this is suggested by increased employment in older firms at the left of the threshold.

To sum up, the graphical analysis does not show any positive impact of the rural EZ program on the number of jobs or of plants, on average or on subsets of firms more specifically targeted. The only exceptions that go in the expected direction may be a short-run impact on total employment and the number of plants belonging to small firms after 1995. However, these tentative effects do not last and come with less expected tentative effects on non-targeted groups of firms. A more likely explanation is that these presumed effects are due to statistical variations, which the statistical analysis will now explore.

4.2 Regression results

The absence of impact of the program on employment overall, as well as its more intensive components on subsets of firms, is confirmed by tables 3-7, which provide estimates corresponding to models (1) and (2). The tables are made of groups of four columns corresponding to four different specifications: the first column corresponds to equation 2 without controls; the second one adds geographical controls (county – *département* –

²²Year-by-year analysis does not yield different results. As above, results reported for 2008 correspond to an average over 2007-09 to limit noise due to year-to-year fluctuations.

²³The dependent variable is the growth of employment located in firms less than three years old (alternatively, of the number of plants located in these firms).

dummies and controls for the initial industry mix²⁴); the third and fourth columns display the results of equation 1 with and without controls, respectively.

The striking feature of tables 3-7 is the small number of significant coefficients for the rural EZ variable: 11 coefficients out of 92 are significant at the 10% level, which is close to what would be expected with randomly generated data. The overall picture is that one cannot reject the absence of impact. An important question is whether this simply reflects a lack of statistical precision, or whether one can reject large positive impacts of the rural EZ program. We argue for the latter. Let us focus on estimates in the long run, which are certainly of interest and are more precisely estimated. Considering firms with less than 50 employees in targeted sectors (on which the effect of temporary payroll tax exemptions should be concentrated), the last column of table 4 (a -1.7 percentage point estimate with a 1.4 pp standard error) implies that one can reject that the program increased yearly employment growth by more than 0.6 percentage point.²⁵ Under the more restrictive difference-in-difference assumption in equation 2²⁶, the 0 estimate in column 10 (with a 0.3 standard error) implies that one can even reject effects larger than 0.5 percentage point. If there are positive effects, they are likely to be small compared to the strong variations in employment growth from one *canton* to the other: as shown in table 4, the standard deviation of employment growth located in small firms is 2.7 percentage points; a 0.5 percentage point effects is therefore equal to 18% of a standard deviation (or: a 18% standardized effect). Such effects are usually considered as reasonably small when doing power calculations.²⁷ Estimates for the 2005 version of the program are somewhat less precise. There, one can reject that employment grew by more than an additional 2.4 percentage points per year in PIOs (a 28% standardized effect) and by more than 5.8 percentage points per year among new firms (a 22% standardized effect). Again, these are reasonably small effects by usual standards: lack of power does not seem to be driving the 0-effect result.²⁸

²⁴Specifically, we measure the share of 36 different industries in each *canton* in 1995 (or 2003) and introduce the corresponding variables as controls.

²⁵At the usual 95% confidence level, and with a one-sided test, one can reject effects above the point estimate plus 1.64 times the standard error.

²⁶This assumption seems to hold according to the above graphical analysis.

²⁷See for instance Duflo, Glennerster, and Kremer (2008).

²⁸Note that the RDD estimates based on model 1 are quite imprecise, but that model 2 substantially improves precision.

5 Conclusion

Using quasi-experimental variations, this paper finds no significant effects of a tax exemption program aimed at promoting employment growth in less densely populated areas in rural France. This lack of impact is not due to a lack of statistical power. In particular, one can reject even small positive effects of the program basic component – a temporary payroll tax exemption for small firms and a 5-year corporate tax exemption on new hires – on total employment and net creation of plants, in the short run as well as the long run. There is also no indication of a positive effect of the more generous corporate tax exemptions after 2005 or of permanent payroll tax exemptions targeted to a subset of non-profit firms defined as Public interest organizations (PIOs); large positive standardized effects can also be rejected.

Taken at face value, these 0-impact results are somewhat puzzling. A possible explanation is that the program's incentives are weak. This is however not obvious from tables 1 and 2, or from figure 1. It may be useful to compare the rural EZ scheme to other contemporaneous tax exemption programs and their estimated effects. As mentioned above, a national payroll tax exemption on low wage labor was in place (starting in 1993). The implied labor cost cuts are on average lower (there is no rebate for workers earning 1.3 times the minimum wage and above), yet the payroll tax exemption is permanent. Existing ex post evaluation find substantial positive impacts of this national scheme (Crepon and Desplatz (2001)). This points toward the idea that being temporary reduced the attractiveness of payroll tax exemption in the rural EZ program. Another contemporaneous program to which the rural EZ program can be compared is the French urban EZ program (the so-called *Zones franches urbaines*, or ZFU). Incentives in ZFUs lie somewhere in-between the initial version (1995) of the rural EZ program and the more generous scheme for PIOs, with corporate tax exemptions for six years and payroll tax exemptions (along the same schedule as the rural EZ program) for 5 years, plus partial exemption over the next 5 years. The estimated effects on total employment growth are however massive: growth rates increase by 14 to 30 percentage points per year after the program is in place (Givord, Quantin, and Trevien (2012)). These impacts are statistically highly significant. The ZFU experience suggests that one could have expected large impacts on plant creations or on employment growth, at least among PIOs. Two hypotheses may ex-

plain why these effects did not materialize. The first one is that PIOs are less sensitive to financial incentives as they are not maximizing profit. They benefit from a windfall effect without changing their employment decisions. The second one is that the rural context itself reduces the opportunities or increases the constraints faced by firms in order to take advantage of the tax cuts: lack of infrastructures, of human resources or of the favorable environment of plants in the same sector. This interpretation is consistent with findings in Devereux, Griffith, and Simpson (2007) and Briant, Laffourcade, and Schmutz (2012). It is an important hypothesis for further research.

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Figure 1: Labor cost cut implied by the rural EZ tax exemptions

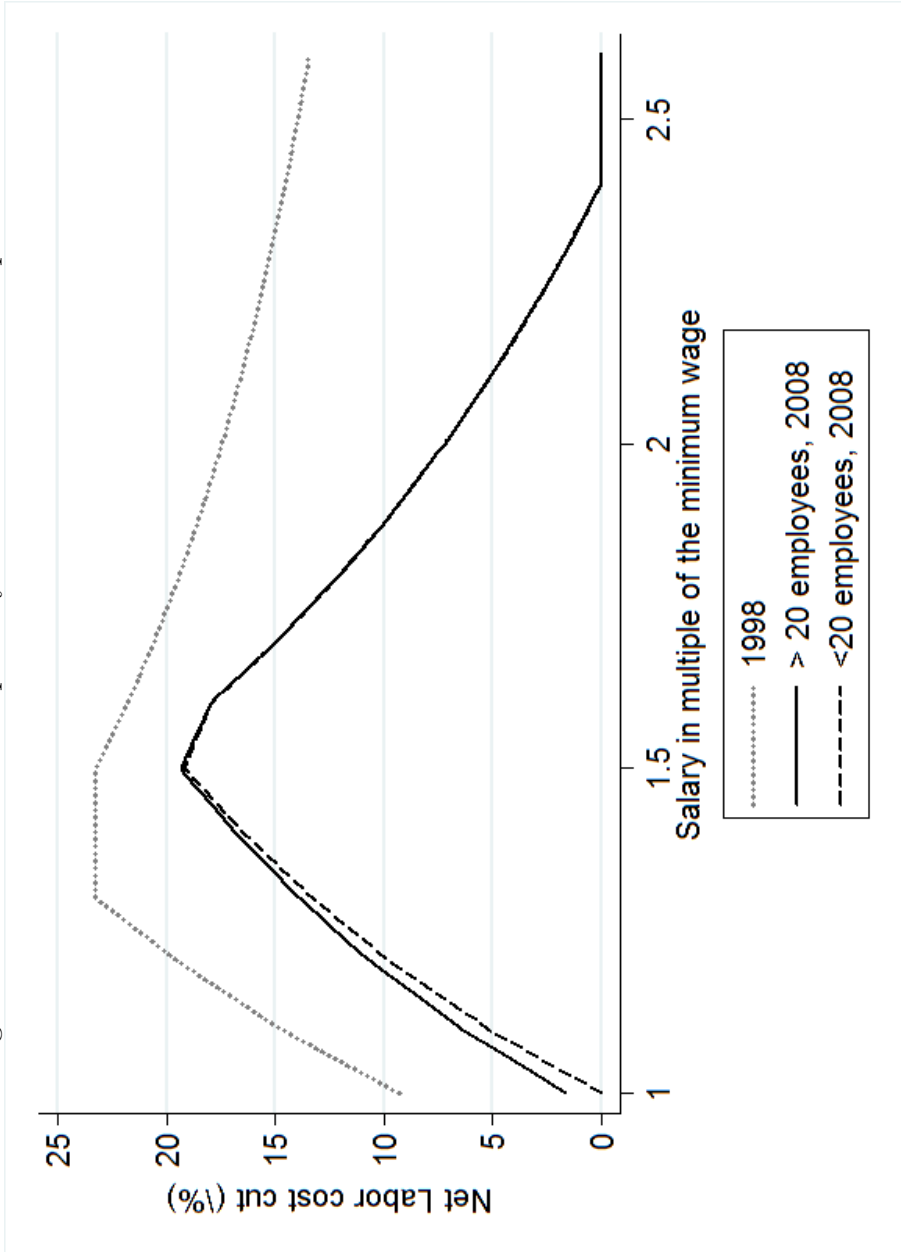


Figure 2: Share of rural EZ cantons as a function of the 1990 population density (1st stage)

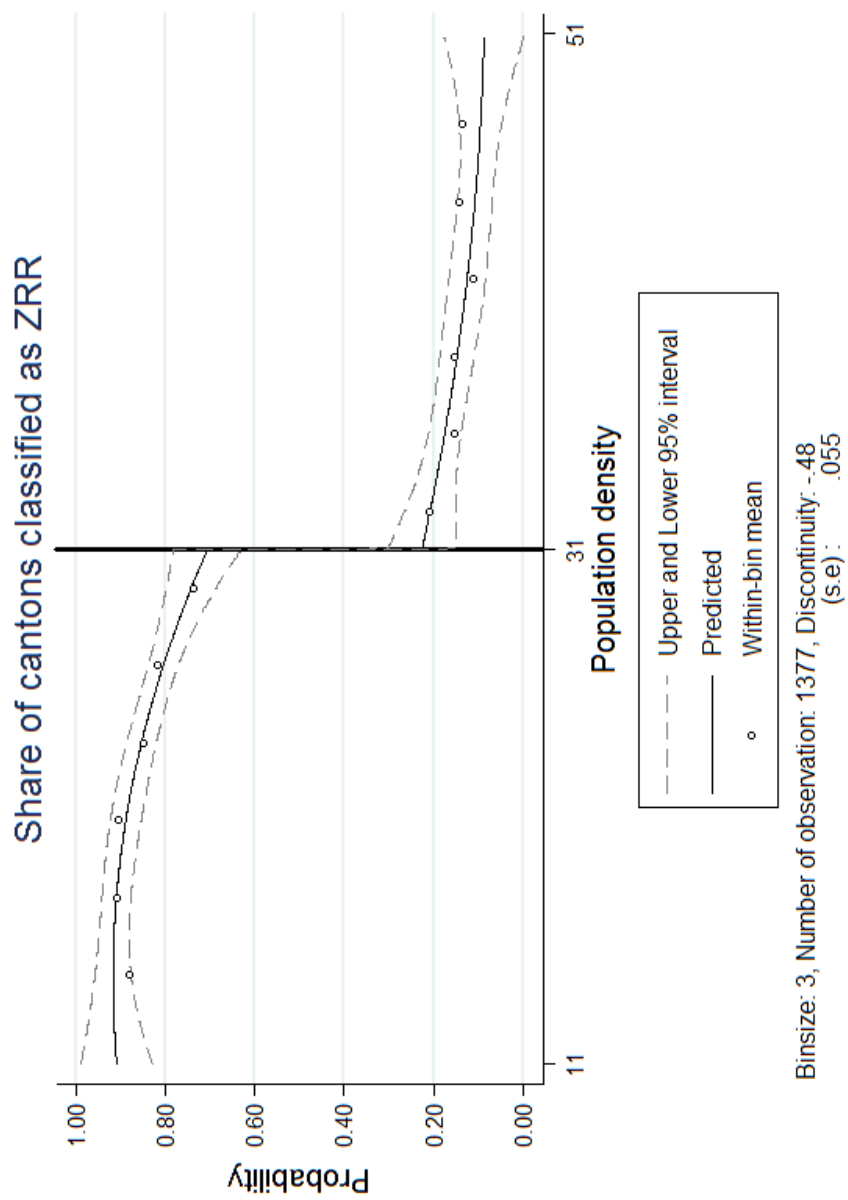


Figure 3: Density of the 1990 population density variable

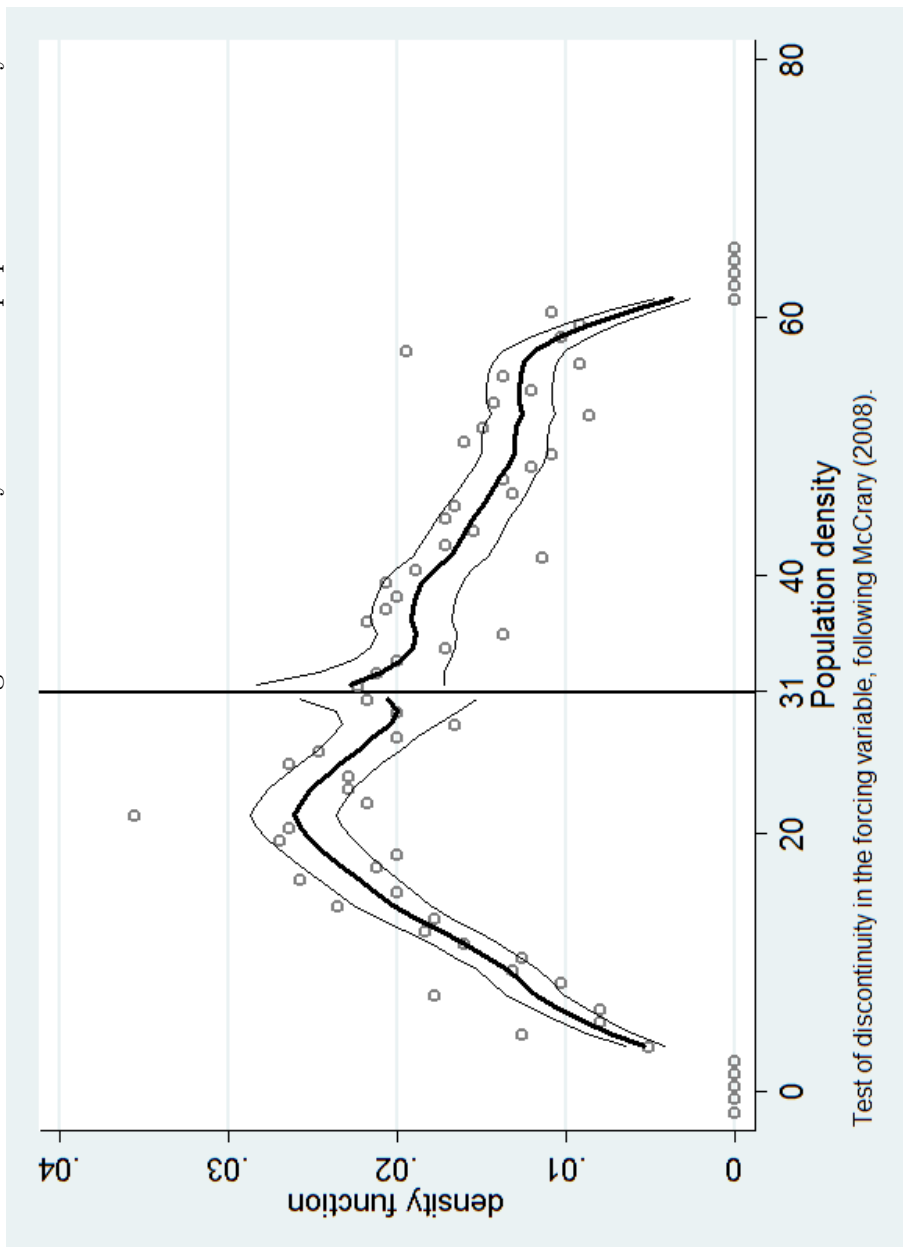


Figure 4: Yearly total employment growth, by 1990 population density

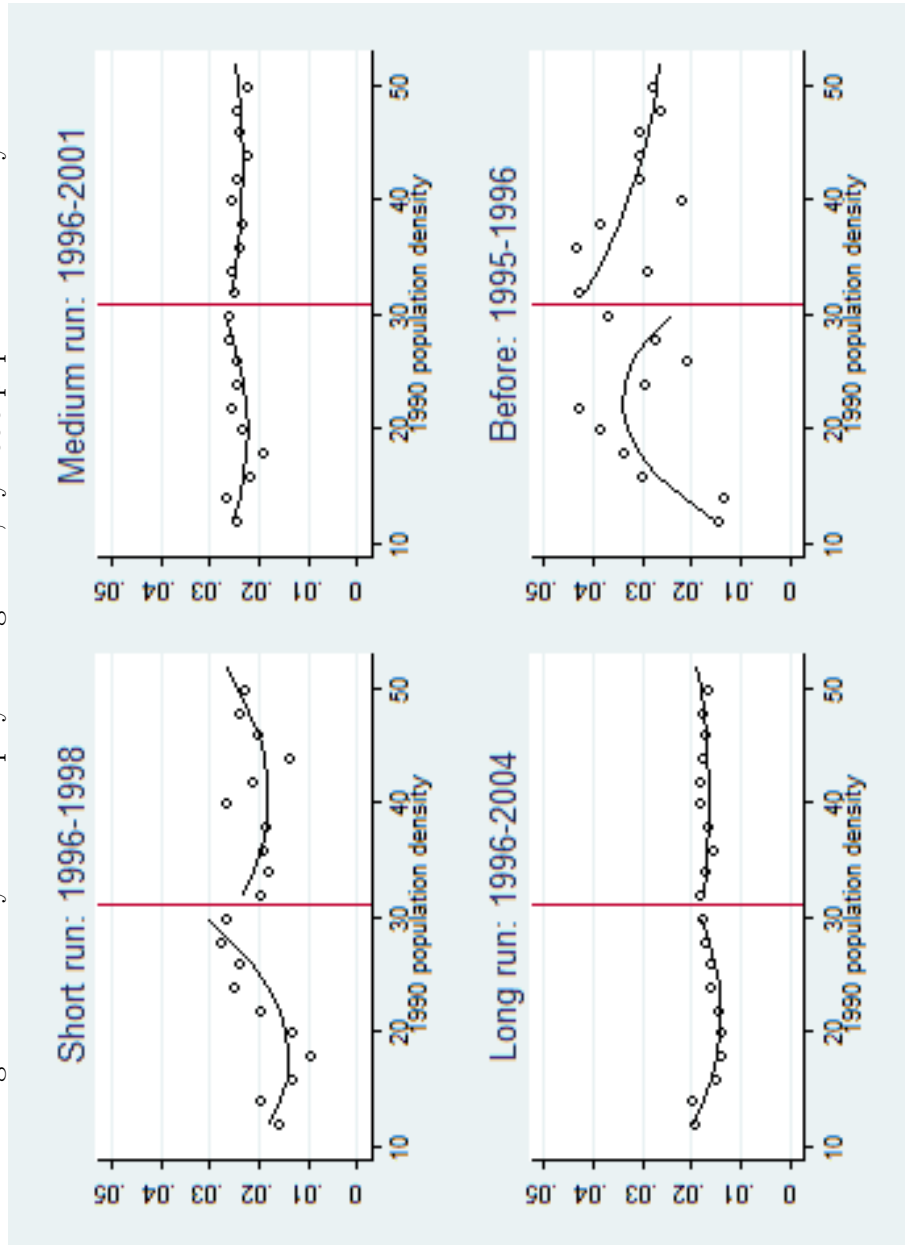


Figure 5: Yearly growth rate in total number of plants, by 1990 population density

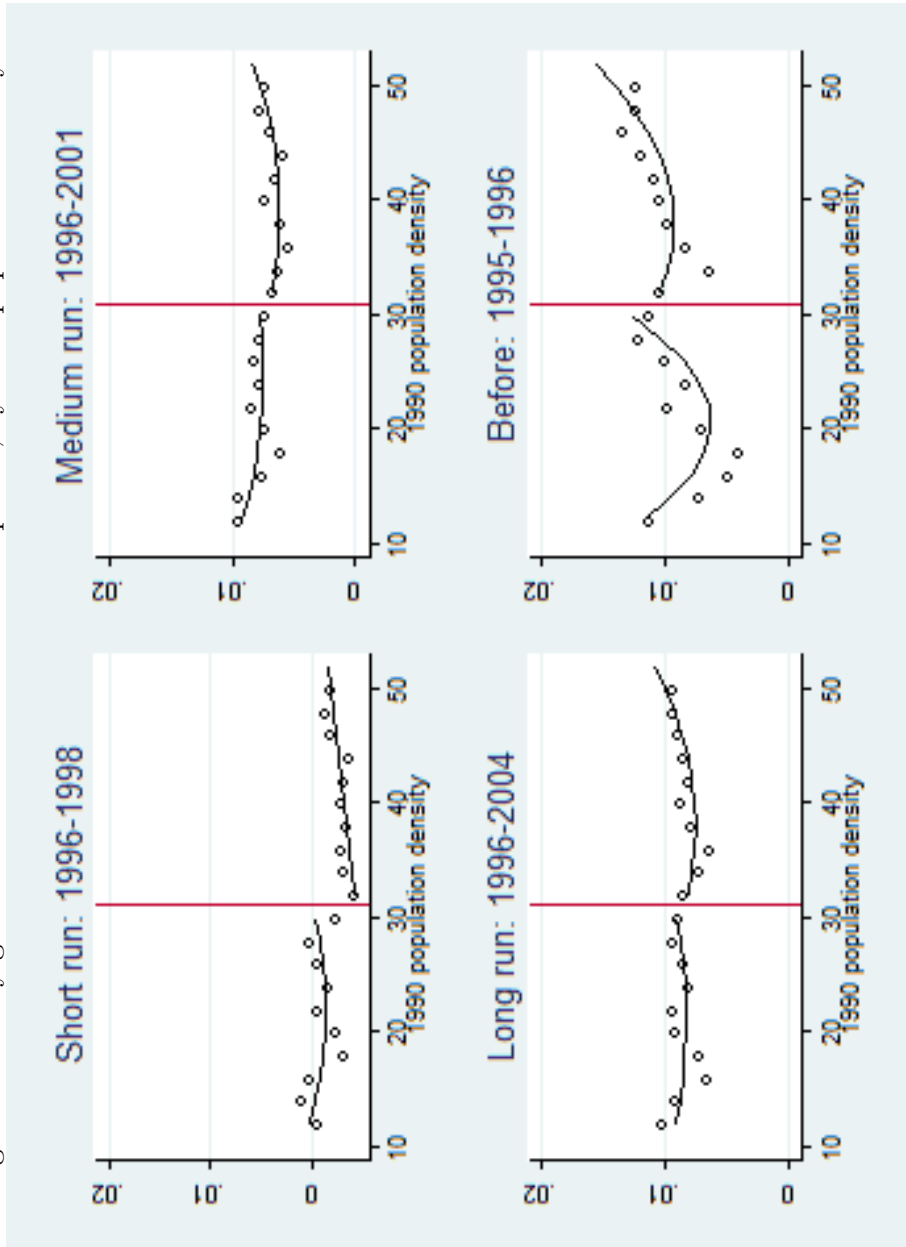


Figure 6: Yearly employment growth, firm size ≤ 50 , by 1990 population density

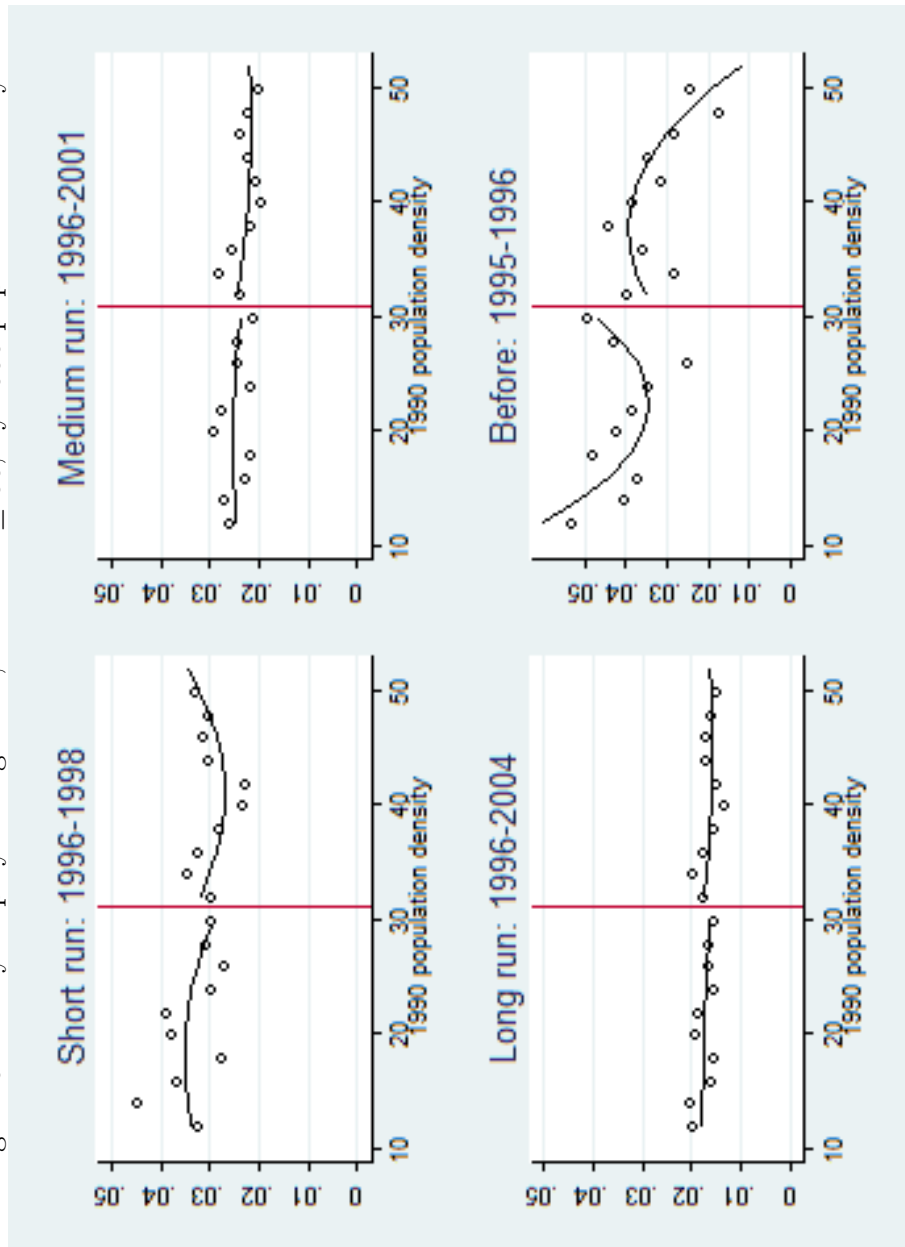


Figure 7: Yearly growth rate in number of plants, firm size ≤ 50 , by 1990 population density

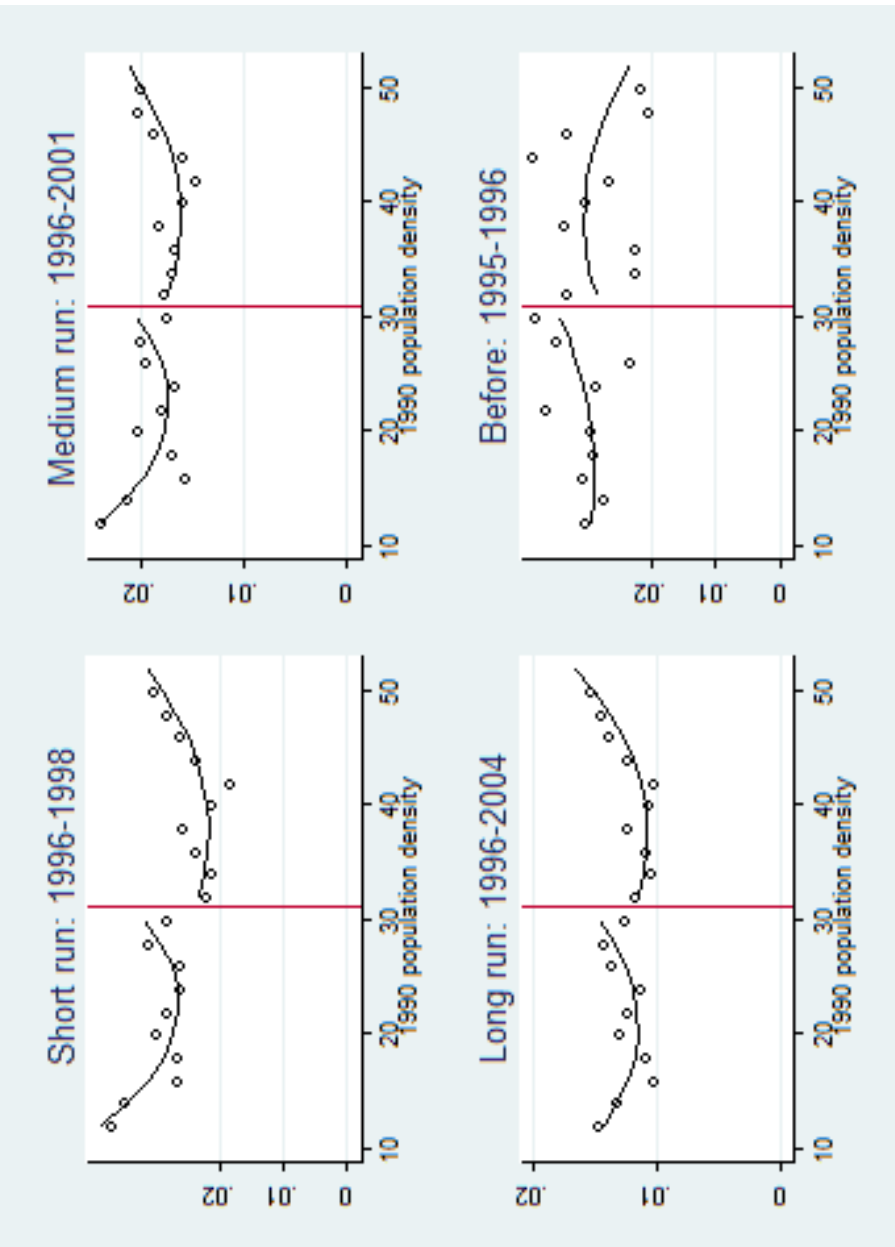


Figure 8: Yearly employment growth, firm size ≥ 50 , by 1990 population density

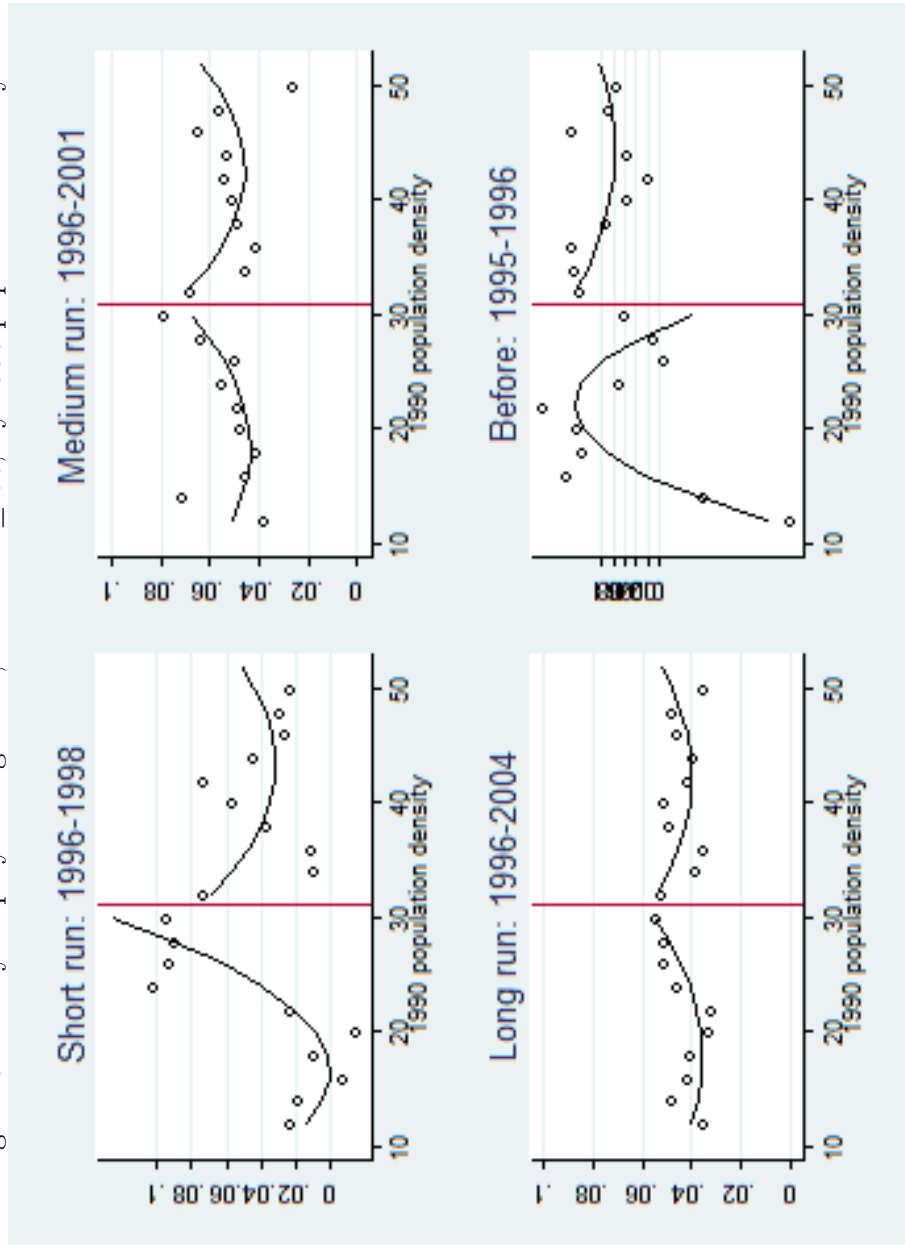


Figure 9: Yearly growth rate in number of plants, firm size ≥ 50 , by 1990 population density

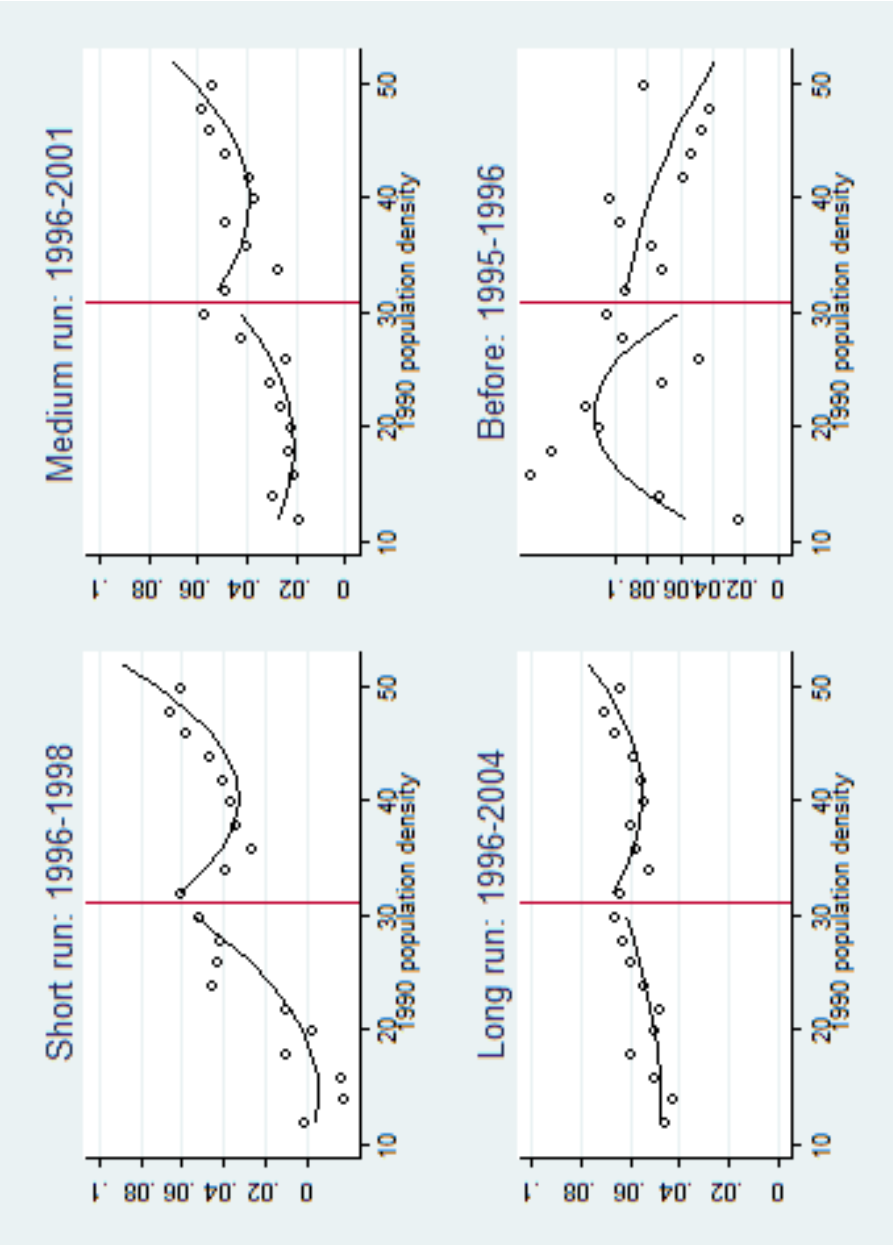


Figure 10: Yearly growth rates (2005-08) by 1990 population density: public interest organization vs. others

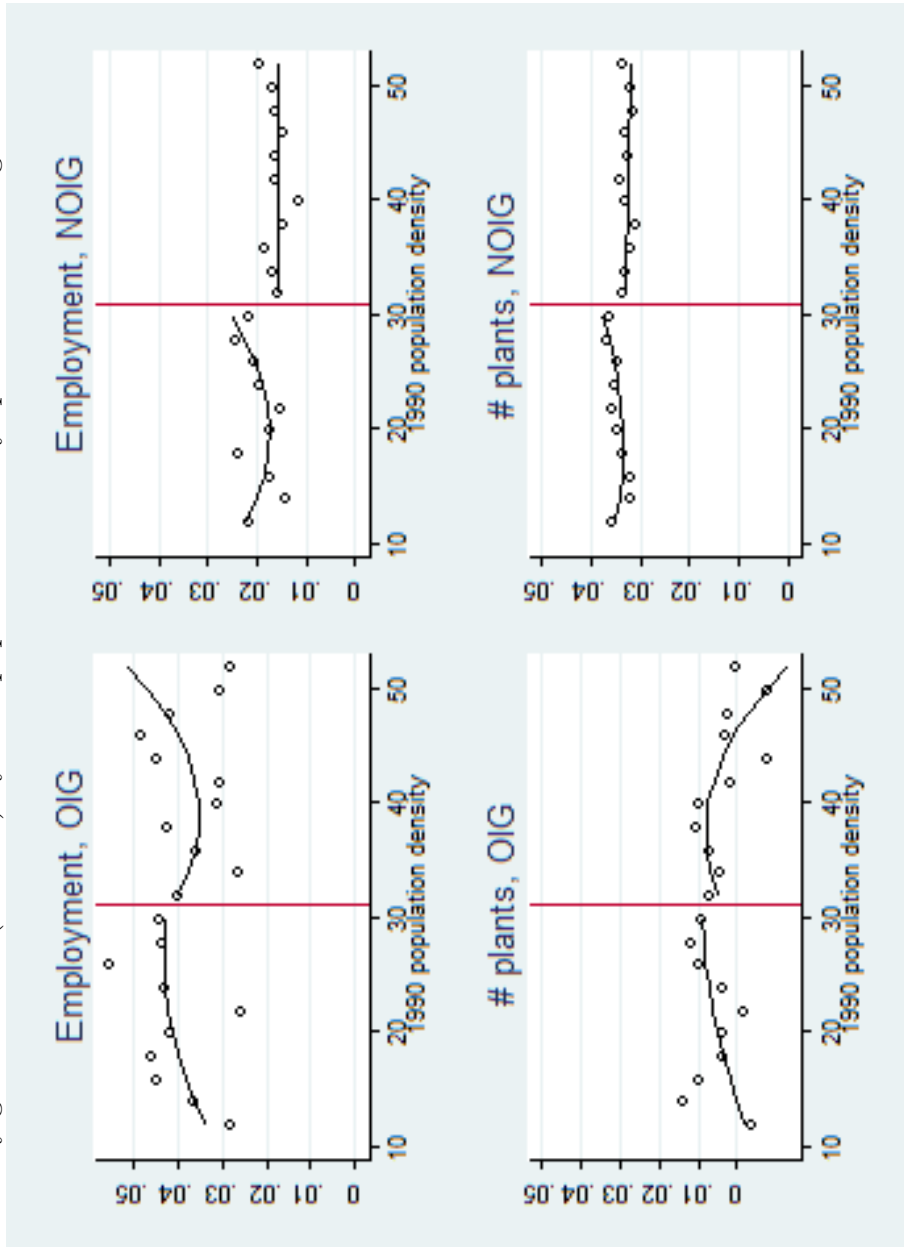


Figure 11: Yearly growth rates (2005-08) by 1990 population density: new firms vs. older firms

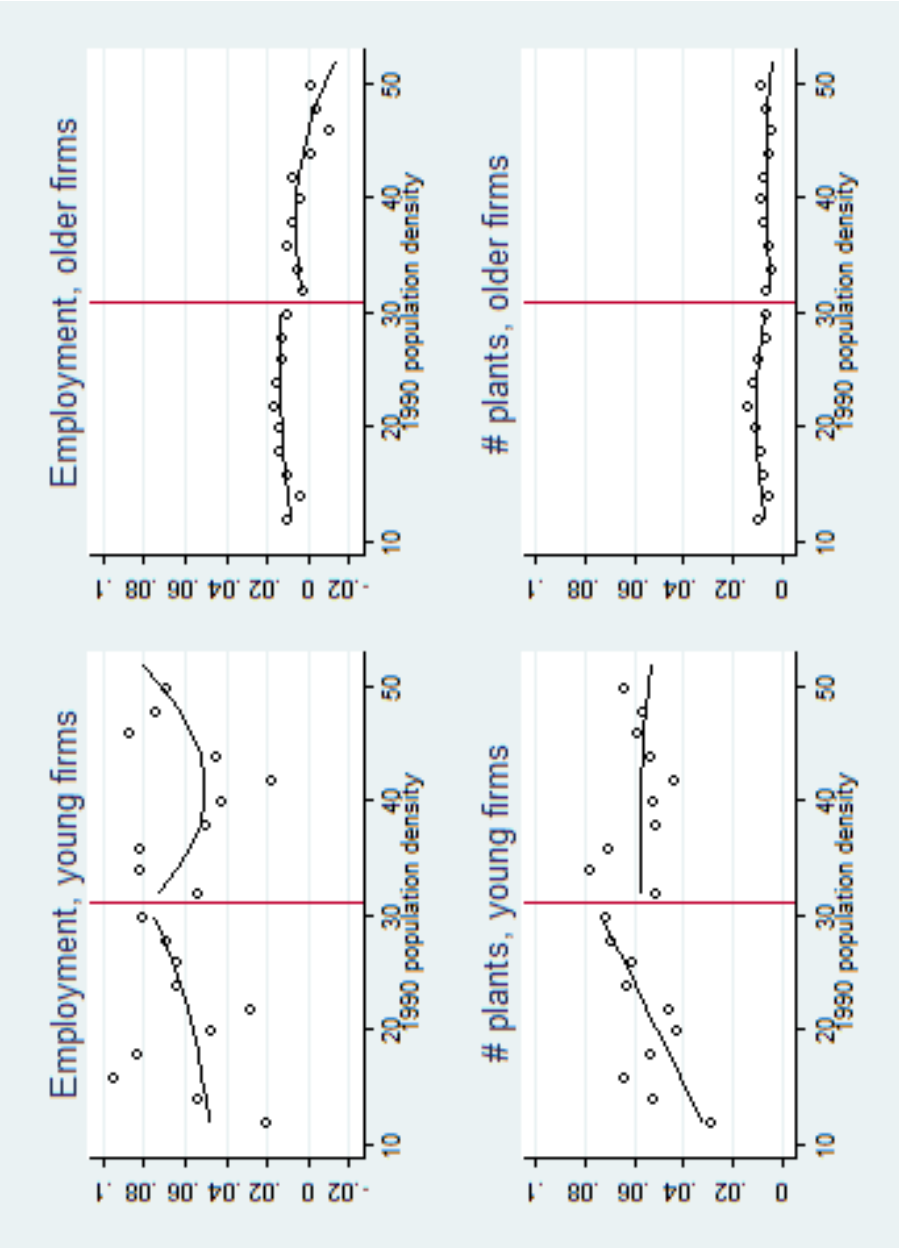


Table 1: Rural EZ payroll tax exemptions

Eligibility	Exemption	Duration
Since 1996		
Firms ≤ 50 employees in manufacturing, retail, agriculture or craft industry New hires with contract ≥ 12 months No layoff for 12 months	Full exemption of employer payroll contribution (30.3% of gross wage) on the fraction of wages $\leq 1.5 \times$ the minimum wage	1st year of job
Since 2005		
“Public interest organizations” All employees New hires excluded after Nov. 2007	Full exemption of employer payroll contribution (30.3% of gross wage) on the fraction of wages $\leq 1.5 \times$ minimum wage	Full duration of job

Notes:

“Public interest organizations” are defined based on the activity (providing a public service) and legal status (non profit).

In 2008, the exemption in the two schemes is reduced to 0 at 2.4 times the minimum wage (exemption reduced linearly between 1.5 and 2.4 times the minimum wage)

Table 2: Corporate tax exemptions

Eligibility	Exemption	Duration
	1996 - 2004	
Newly created firms in manufacturing, retail, or craft industry	Corporate tax on profits Local business taxes	Year 1-2: 100% exemption Year 3: 75% Year 4: 50% Year 5: 25%
	2005 -	
Firms created after Jan. 1, 2004, in manufacturing, retail, or craft industry	Corporate tax on profits Local business taxes	Year 1-5: 100% exemption Year 6-10: 60% Year 11-12: 40% Year 13-14: 20%

Table 3: Impact of the 1996 rural EZ program: total employment and number of plants

	1996-98		1996-2001		1996-2004						
	A. Total employment										
Rural EZ program	0.004 (0.007)	0.006 (0.008)	-0.028 (0.030)	0.002 (0.004)	0.004 (0.005)	-0.009 (0.018)	-0.014 (0.017)	-0.002 (0.003)	-0.002 (0.003)	0.002 (0.013)	-0.008 (0.012)
dens_left			0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
dens_right			-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-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	no	yes	no
County dummies	no	yes	no	yes	no	yes	no	yes	no	yes	no
Observations	784	784	1,444	784	1,444	1,444	784	784	784	1,444	1,444
Average of outcome	0.021	0.021	0.019	0.025	0.024	0.024	0.016	0.016	0.016	0.016	0.016
S.d. of outcome	0.062	0.062	0.057	0.037	0.034	0.034	0.024	0.024	0.024	0.024	0.024
	B. Number of plants										
Rural EZ program	0.005* (0.003)	0.003 (0.002)	0.013 (0.012)	0.003* (0.002)	0.002 (0.002)	0.008 (0.008)	-0.001 (0.007)	0.002 (0.001)	0.001 (0.001)	0.007 (0.007)	-0.004 (0.005)
dens_left			0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000** (0.000)
dens_right			0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	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	no	yes	no
County dummies	no	yes	no	yes	no	yes	no	yes	no	yes	no
Observations	784	784	1,444	784	1,444	1,444	784	784	784	1,444	1,444
Average of outcome	-0.002	-0.002	-0.002	0.007	0.007	0.007	0.008	0.008	0.008	0.008	0.008
S.d. of outcome	0.023	0.023	0.022	0.014	0.014	0.015	0.015	0.013	0.013	0.013	0.013

Table 4: Impact of the 1996 rural EZ program: firms with less than 50 employees, targeted sectors

	1996-98		1996-2001		1996-2004						
A. Total employment											
Rural EZ program	0.006 (0.007)	0.008 (0.008)	0.015 (0.032)	0.002 (0.031)	0.004 (0.004)	0.003 (0.004)	-0.021 (0.018)	0.001 (0.003)	-0.000 (0.003)	-0.001 (0.014)	-0.017 (0.014)
dens_left			-0.000 (0.000)	0.000 (0.000)			-0.000 (0.000)			-0.000 (0.000)	0.000 (0.000)
dens_right			0.000 (0.001)	0.000 (0.001)			-0.000 (0.000)			-0.000 (0.000)	-0.000 (0.000)
1995 industry mix	no	yes	no	yes	no	yes	no	yes	no	yes	no
County dummies	no	yes	no	yes	no	yes	no	yes	no	yes	no
Observations	783	783	1,443	1,443	783	783	1,443	1,443	783	783	1,443
Average of outcome	0.032	0.032	0.032	0.032	0.025	0.025	0.024	0.024	0.017	0.017	0.017
S.d. of outcome	0.061	0.061	0.062	0.062	0.036	0.036	0.036	0.036	0.027	0.027	0.027
B. Number of plants											
Rural EZ program	0.008* (0.005)	0.006 (0.005)	0.041 (0.026)	0.020 (0.022)	0.003 (0.003)	0.001 (0.003)	0.017 (0.014)	0.003 (0.002)	0.001 (0.002)	0.013 (0.010)	-0.003 (0.008)
dens_left			0.000 (0.000)	0.000 (0.000)			0.000 (0.000)			0.000 (0.000)	0.000** (0.000)
dens_right			0.001 (0.001)	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	no	yes	no
County dummies	no	yes	no	yes	no	yes	no	yes	no	yes	no
Observations	783	783	1,443	1,443	783	783	1,443	1,443	783	783	1,443
Average of outcome	0.027	0.027	0.027	0.027	0.018	0.018	0.018	0.018	0.012	0.012	0.012
S.d. of outcome	0.044	0.044	0.047	0.047	0.023	0.023	0.025	0.025	0.017	0.017	0.019

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

	1996-98		1996-2001		1996-2004							
A. Total employment												
Rural EZ program	0.037 (0.044)	0.024 (0.052)	-0.428* (0.252)	-0.406 (0.266)	-0.002 (0.024)	0.005 (0.028)	-0.127 (0.133)	-0.135 (0.138)	-0.007 (0.017)	-0.001 (0.019)	-0.085 (0.094)	-0.103 (0.095)
dens_left			0.003 (0.003)	0.002 (0.003)			0.000 (0.001)	0.001 (0.001)			0.000 (0.001)	0.001 (0.001)
dens_right			-0.006 (0.005)	-0.006 (0.005)			-0.002 (0.003)	-0.002 (0.002)			-0.001 (0.002)	-0.001 (0.002)
1995 industry mix	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
Observations	652	652	1,153	1,153	646	646	1,144	1,144	647	647	1,150	1,150
Average of outcome	0.052	0.052	0.041	0.041	0.054	0.054	0.052	0.052	0.045	0.045	0.043	0.043
S.d. of outcome	0.339	0.339	0.333	0.333	0.182	0.182	0.181	0.181	0.130	0.130	0.133	0.133
B. Number of plants												
Rural EZ program	-0.023 (0.022)	-0.046* (0.026)	-0.176 (0.132)	-0.204 (0.138)	-0.024* (0.013)	-0.034** (0.014)	-0.052 (0.070)	-0.051 (0.070)	-0.009 (0.009)	-0.014 (0.010)	-0.043 (0.048)	-0.067 (0.049)
dens_left			0.002 (0.001)	0.002 (0.001)			0.001 (0.001)	0.001 (0.001)			0.000 (0.001)	0.001 (0.001)
dens_right			-0.001 (0.003)	-0.002 (0.002)			-0.000 (0.001)	0.000 (0.001)			-0.000 (0.001)	-0.000 (0.001)
1995 industry mix	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
County dummies	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
Observations	652	652	1,153	1,153	646	646	1,144	1,144	647	647	1,150	1,150
Average of outcome	0.035	0.035	0.030	0.030	0.037	0.037	0.037	0.037	0.057	0.057	0.057	0.057
S.d. of outcome	0.173	0.173	0.180	0.180	0.096	0.096	0.098	0.098	0.067	0.067	0.068	0.068

Table 7: Impact of 2005 rural EZ program: new vs. older firms

	New firms		Older firms	
	A. Total employment			
Rural EZ program	-0.010 (0.029)	-0.000 (0.034)	-0.065 (0.140)	0.015* (0.007)
dens_left			0.003 (0.144)	-0.028 (0.032)
dens_right			0.002 (0.002)	-0.000 (0.000)
2003 industry mix	no	yes	no	yes
County dummies	no	yes	no	yes
Observations	773	773	1,423	783
Average of outcome	0.0591	0.0591	0.0594	0.0105
S.d. of outcome	0.257	0.257	0.263	0.0585
				1,443
				0.00833
				0.0615
				0.007
				(0.020)
				0.000
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6 Appendix figures

Figure 12: Share of population above age 60 in 1990

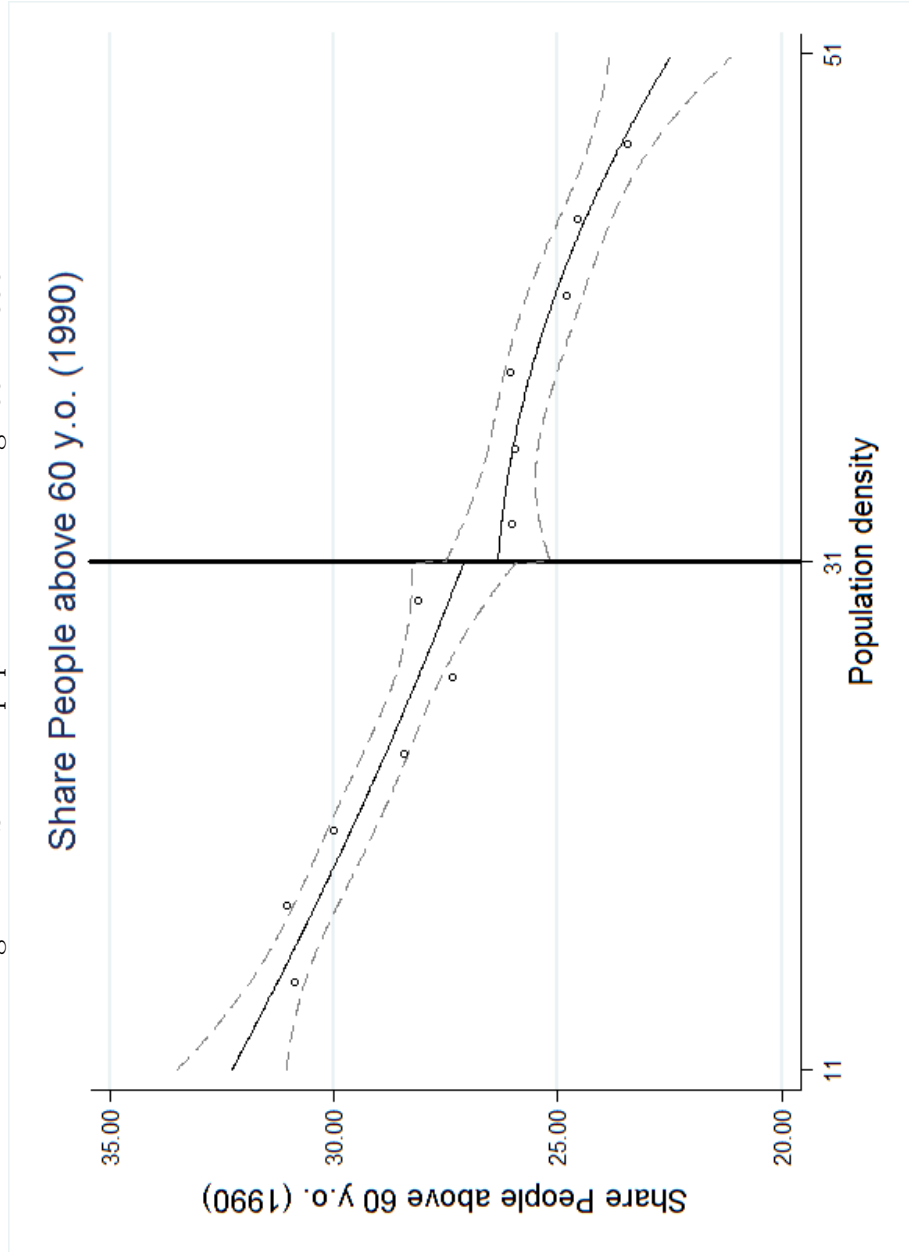


Figure 13: Income in 1990

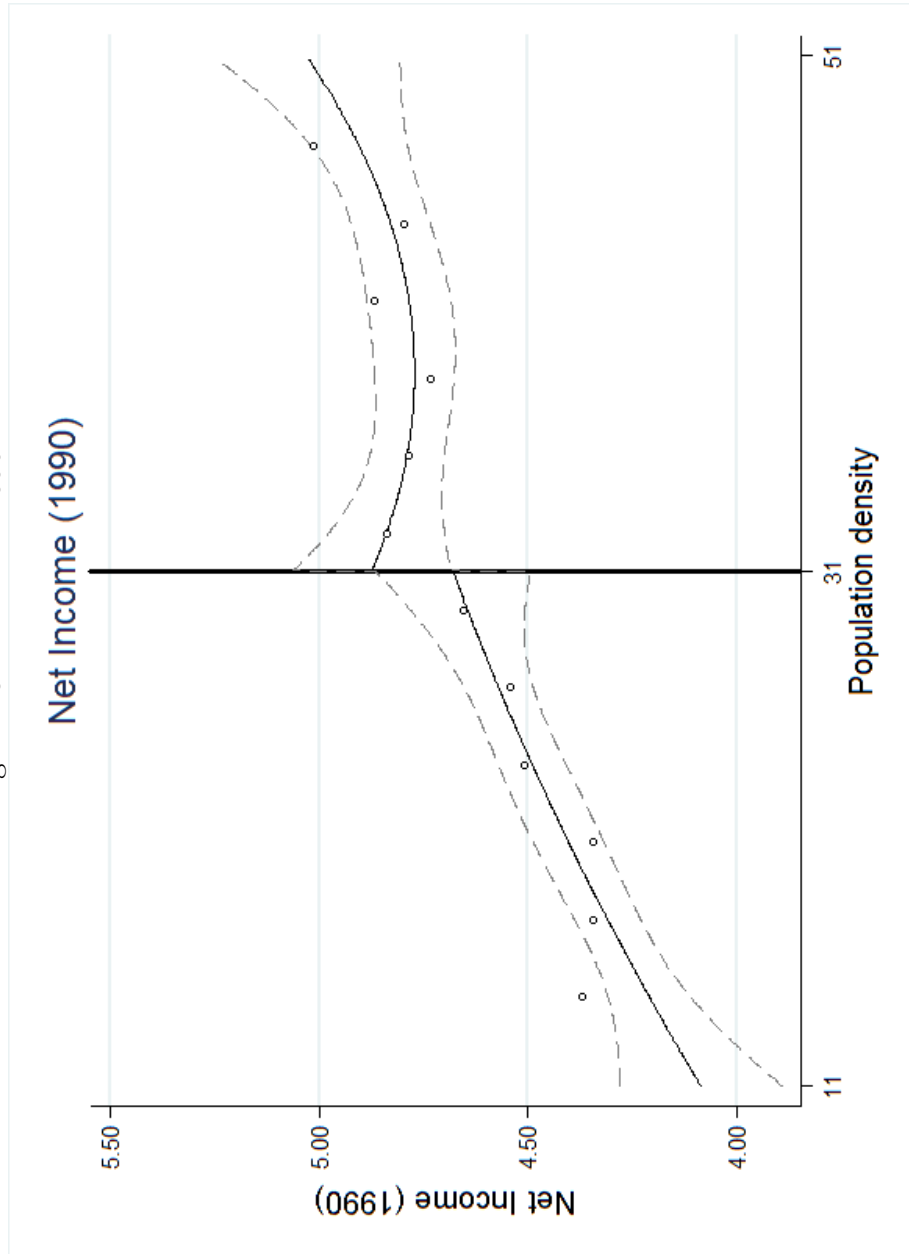


Figure 14: Employment growth (1982-90)

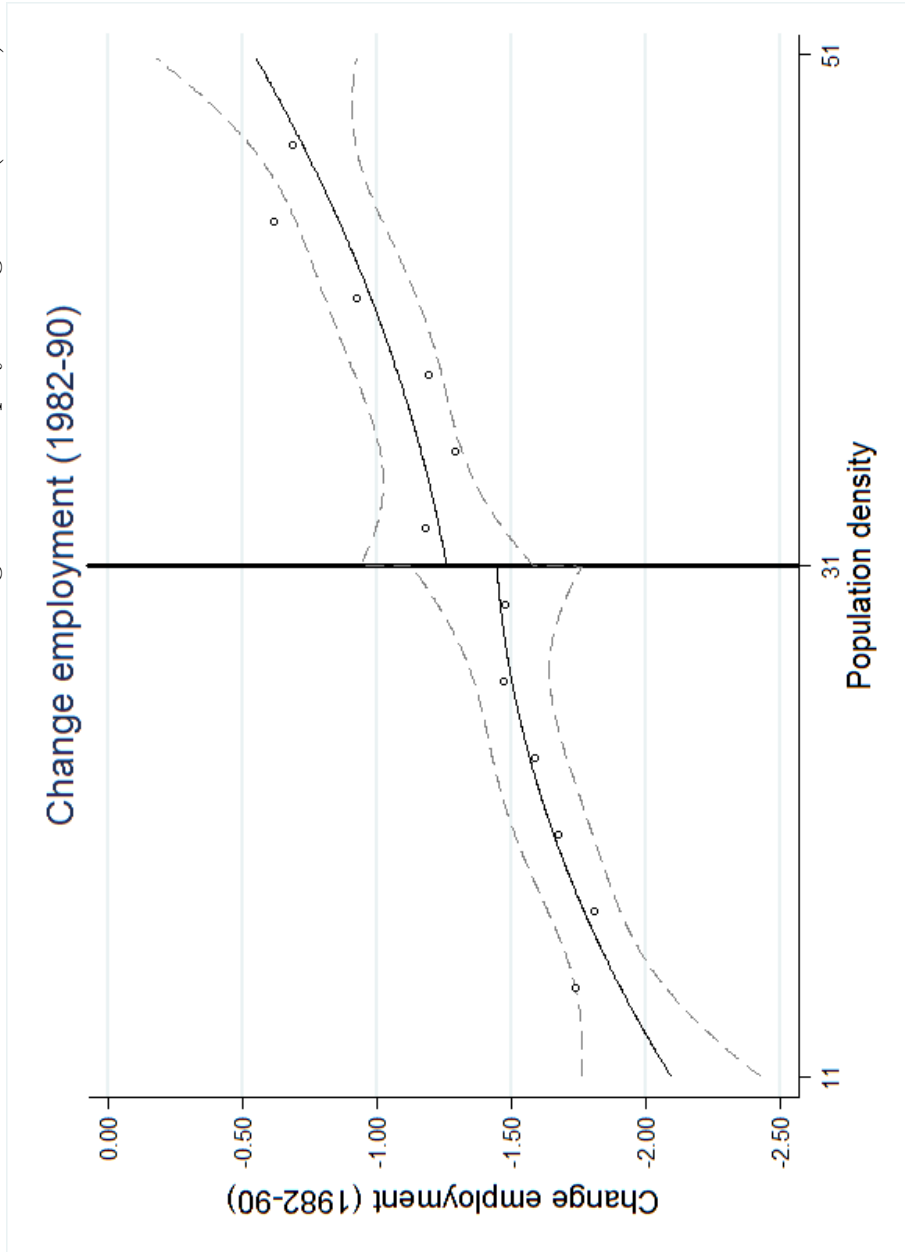


Figure 15: Unemployment rates (1990)

