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Do Toll-free Highways Foster Firm Formation and Employment Growth? Results from a Quasi-natural Experiment

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ABSTRACT

DO TOLL-FREE HIGHWAYS FOSTER FIRM FORMATION AND EMPLOYMENT GROWTH? RESULTS FROM A QUASI-NATURAL EXPERIMENT*

David B. Audretsch, Dirk Christian Dohse, and João Pereira dos Santos

The paper studies the impact of a switch from free to charged highway provision on firm numbers and private sector employment in a cross-section of Portuguese municipalities. It exploits the fact that highway tolls in Portugal were unexpectedly raised in reaction to the financial crisis to establish causality. Results from a difference-in-differences analysis indicate a significantly negative effect of highway tolls on number of firms and employment in treated municipalities vis-à-vis the control group. We also find negative effects of tolls in municipalities not directly traversed by the treated highways, with larger firms and manufacturing firms being most strongly affected.

Keywords: infrastructure provision, regional economic development, quasi-natural experiment
JEL classification: R48, L25, R12

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I. Introduction

While there is a rich literature on the macroeconomic growth and productivity effects of public infrastructure investment (Aschauer, 1989; Gramlich, 1994; Cohen and Morrison, 2004; Duranton and Turner, 2012; Melo, Graham & Brage-Ardao, 2013), far less attention has been given to the impact of infrastructure provision on private sector firm formation and the employment performance of private sector firms. This apparent neglect in the literature is surprising since there are good reasons to assume that publicly financed infrastructure is conducive to private firms' economic activities. It is a well- known stylized fact that most entrepreneurs start their firm in the region where they live and/or work (see, for instance, Michelacci and Silva; 2007 or Figueiredo, Guimarães & Woodward, 2002),[1] such that they are particularly dependent on the locational quality and accessibility of their home region. Investments in infrastructure enhance a region's or city's accessibility and bring the firms located there – in economic terms – closer to potential customers and suppliers. This is particularly important for peripheral locations which move – in economic terms – closer to the centers of economic activity. Moreover, by enhancing connectivity and linkages with other economic agents, infrastructure facilitates the recognition of entrepreneurial opportunities and the ability of entrepreneurs to put opportunities into practice (Audretsch, Heger & Veith, 2015b). Thus, an improvement of infrastructure, should – ceteris paribus – have a positive impact on the number of firms as well as on private sector employment at a given location, whereas a deterioration of infrastructure should – ceteris paribus – have the opposite effect.

The current paper makes use of a natural experiment in order to investigate whether there is a causal relationship between the cost of usage of transport infrastructure (toll-free highways) and (i) the growth in the number of firms and (ii) the growth in private sector employment in a cross-section of Portuguese municipalities.

In an effort to move the country closer to the core of Europe, Portuguese authorities have built a system of modern, toll-free highways, the so-called **SCUTs** (acronym for " **S**em **C**ustos para o **Ut**ilizador"/Without Costs for the Users) between 1999 and 2007. The SCUT system accounted for

[1] As Michelacci and Silva (2007) have shown, the share of entrepreneurs working in the region where they were born is significantly higher than the corresponding fraction for dependent workers. Figueiredo, Guimarães & Woodward (2002) find that Portuguese entrepreneurs in the manufacturing sector were willing to accept labor costs three times higher than in alternative locations to locate the new business in their current region.

nearly 1000 kilometers (more than one third of total Portuguese highways) and helped cut average travel time between Lisbon and the Spanish border (as well as between Lisbon and provincial cities) by more than 40 percent.

However, in the course of the sovereign debt crisis the toll free provision could no longer be maintained, as the Portuguese government was forced to cut down public spending and to increase public revenues. As the Financial Times put it, "To help keep Portugal's €78bn bailout on track, the government has been forced to introduce charges on more than 900km of roads where there was previous none, triggering angry protests, increasing business costs and confusing tourists." (Financial Times of August 25, 2013) The tolls on the (former toll free) SCUT highways were so high that they had a substantial negative effect on traffic.[2] This has just recently been acknowledged by the Portuguese government who decided to cut back the tolls on SCUT highways by 15% from August 1st, 2016 on.[3]

While bad news for Portuguese drivers and business people affected by the high tolls and trying to avoid them by using the old (and much slower) road network, the events in Portugal provide a unique quasi-natural experiment to study the causal effects of a drastic increase in the price of modern transport infrastructure on firm formation and employment dynamics, as the introduction of tolls was exclusively motivated by budgetary reasons and not by considerations with respect to employment or new business formation.[4] This paper finds strong evidence for a negative impact of highway tolls on firm numbers and firm employment. The negative effects of the tolls are not restricted to municipalities which are directly traversed by the SCUTs highways but affect – to a somewhat lesser extent and with some interesting modifications – more distant municipalities as well.

The paper is organized as follows: Section 2 provides a brief review of the pertinent literature and puts the new approach pursued in this paper into perspective. Section 3 presents the empirical strategy and the data. Section 4 presents and discusses the results, and Section 5 concludes.

[2] In 2011, when the tolls were introduced, the price was 9 cents per km. According to a study by the Institute for Road Infrastructures (INIR) traffic along the SCUTs highways decreased substantially between the first quarter 2011 and the first quarter 2012 (The Portugal News 2012).

[3] Note that all other highways in Portugal were subject to charges long before the sovereign debt crisis and the magnitude of the charges was not affected by the crisis.

[4] Note that municipal peculiarities played no role in the decision as this was a purely national matter. The mayors of the SCUTs regions were against the introduction of tolls (even those who belonged to the same party as the national government), and there were massive protests from the local populations too.

II. Infrastructure, firm formation and employment dynamics

The relationship between highway infrastructure and regional economic development is a complex one. Due to its network properties, transportation infrastructure has the ability to shift market areas and to affect communication and business channels (Rietveld, 1989). Transportation infrastructure may be viewed as a (local) public input into private agent's production processes and may thus affect their locational decisions (Dohse, 1998), as well as households' residential choices, by lowering commuting costs (Rephann and Isserman, 1994). Moreover, the availability of infrastructure may also affect economic agents' decision to start a new business as well as the post-entry performance of new-firm startups (Audretsch, Heger & Veith, 2015b).

The decision made by individuals to start a new firm has generally been analyzed in the economics literature using the model of entrepreneurial choice, which compares the expected returns accruing from entrepreneurship with the anticipated wages earned as an employee in an incumbent firm (Hsieh, Parker & van Praag, 2016). The magnitude of the perceived returns to entrepreneurship is influenced by the extent of the perceived opportunity and the ability of the entrepreneur to exploit or take advantage of that opportunity (Winter, 2016). Infrastructure might be expected to influence the decision to become an entrepreneur in three important ways:

The first is that by enhancing the connectivity or potential linkages and networks of nascent entrepreneurs, infrastructure also enhances the information and ideas available and therefore the set of entrepreneurial opportunities. While Saxenien (1994), identified the key role that linkages and networks play in the spillover of knowledge and new ideas that fuel entrepreneurship (Colombelli, 2016), she did not explicitly identify the potential contribution to enhancing such knowledge spillovers made by infrastructure, such as highways. To the extent that network enhancing infrastructure, such as highways, facilitates the exchange and interface of ideas, it will also serve as a conduit for knowledge spillovers, and hence would be expected to be conducive to entrepreneurship. The second impact of infrastructure investment is that, as the resource-based view of the entrepreneurial firm emphasizes (Alvarez and Busenitz, 2001) it will facilitate the ability of entrepreneurs to access the requisite resources, including labor and technological capabilities. The third impact is that better and less expensive access to infrastructure enhances the ability of entrepreneurial firms to access a broader range of geographic markets, enabling startups to achieve higher rates of post-entry growth and survival.[5]

Although there is a large literature on the macroeconomic growth and productivity effects of public infrastructure investment, the impact of infrastructure on firm-level performance and on firm numbers is not well explored as yet.

The discussion today remains influenced by the highly prominent debate triggered by Aschauer's pioneering work in the late 1980s and early 1990s (see, for instance Aschauer, 1989; Munnell 1992, Gramlich, 1994). Aschauer estimated an aggregate Cobb Douglas production function including infrastructure investment as an additional input alongside with the conventional labor and private capital inputs. Using both cross-sectional as well as time-series data for the US, Aschauer (1989) found a substantial impact of infrastructure investment on the growth rate of GDP.

Subsequent research motivated by Aschauer's seminal paper tried to provide broader evidence for other countries, sub-national entities (regions or states) and other kinds of infrastructure. One common result was that estimated benefits of infrastructure investment appear larger in aggregate analyses than in disaggregated (i.e. region or state level) studies. "As the geographic focus narrows, the estimated impact of public capital becomes smaller." (Munnell, 1992, pp. 193-194).[6] The early studies by Aschauer, Munnell and others were, however, not free of logical and econometric problems, the most important of which are discussed in Gramlich's 1994 review essay (Gramlich, 1994). It was criticized that the correlation between productivity growth and public-capital accumulation could be spurious because relevant variables were omitted, that the direction of causation was unclear or that sectoral differences were not adequately taken into account.

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[5] Fernhaber, Gilbert & McDougall (2008) provide compelling evidence how startups engaged in international activities achieve a stronger performance, measured in terms of growth. Internationalization is just a special case of greater market access. Thus, both the prevalence and the post-entry performance of new-firm startups should be enhanced by better access to infrastructure.

[6] This result was often attributed to geographic spillovers in productivity benefits that are not captured by disaggregated analyses (see, for instance Munnell 1992). However, a study by Holtz-Eakin and Schwartz (1993) analyzing the effects of state highway investments in the US finds no evidence of quantitatively important productivity spillovers beyond the narrow confines of each state's borders. Their result is in line with Gramlich (1990), who finds that even on major interstate highways most drivers are from within the state.

In the meantime, hundreds of papers have been published, showing a rather complex – and partly ambiguous – picture. Survey articles by Afraz et al. (2006) and Romp and de Haan (2007) suggest that the majority of studies find small but non-negligible effects of public infrastructures expenditure on production and GDP growth, although there are large differences with respect to countries, regions and sectors and although – most importantly – the causality issue is not settled. The results of a recent meta-analysis by Melo, Graham & Braga-Ardao (2013) indicate that the productivity effect of transport infrastructure varies among main industry groups and tends to be stronger for manufacturing and construction industries than for service industries. Moreover, there is evidence of higher productivity effects for roads, compared to other transport modes such as airports, railways, and ports and a higher output elasticity of transport for the US economy, compared to European countries, which is not too surprising, given that the US on the whole is more dependent on road transport than European economies (Melo, Graham & Braga-Ardao, 2013, p. 704).

Notwithstanding the progress in the recent literature, current research suggests that causality and the exact channels through which infrastructure provision impacts on the performance of private enterprises remain still unsettled today (e.g. Crescenzi and Rodríguez-Pose, 2012; Duranton and Turner, 2012; Redding and Turner, 2015; Holl 2016), such that there is an urgent need for innovative empirical approaches to address the causality issue. Making use of natural experiments to aid identification appears as an adequate way to overcome the problems caused by endogenous placement of highway infrastructure (Datta, 2012).

III. Empirical strategy and data

III.1 Identification strategy and econometric model

Our identification strategy relies on the fact that the decision to introduce tolls on the prior SCUT highways was enforced by an exogenous shock to the Portuguese political system (the sovereign debt crisis) that left no room for discretionary favouring or discrimination of municipalities. We use all 278 mainland Portuguese municipalities as unit of observation, and the observation period is 2007 – 2013.

The treatment group consists of municipalities which have a segment of the SCUT highway network.[7] Between 2007 and 2010, none of the SCUT highways had direct user costs. Between October 2010 and December 2011, tolls were introduced in each of the seven SCUT highways, which traverse 59 municipalities. Table 1 shows which municipalities were affected and Figure 1 displays their geographical distribution. Our control group consists of the remaining 219 municipalities in Portugal. The 1,482 km of highways in the municipalities which do not have SCUTs are tolled motorways. Note that these other ("non SCUT") highways were subject to charges long before the sovereign debt crisis and the magnitude of the charges was not affected by the crisis. The secondary network of national and municipal roads is not tolled.

[Insert Table 1 here]

[Insert Figure 1 here]

We run difference-in-differences (diff-in-diff) regressions to estimate the effects of exogenously increasing transportation costs by comparing the pre- and post-treatment differences in the outcome Y_{it} of a treatment ($Scut = 1$) and a control group ($Scut = 0$) as follows:

(1) $$Y_{it} = \beta_0 + \gamma_t + \delta_1 Scut \cdot dT_i + \alpha_i + \beta_1 X_{it} + e_{it}$$

where α_i are municipality fixed effects (characteristics of municipalities that do not change over time), γ_t are year fixed effects, and e_{it} is an independent and identically distributed error term. Municipalities and time are indexed by i and t, respectively. We include a vector of time-varying covariates (controls) X_{it} in order to rule out by design that omitted variables induce any considerable bias. Clustered standard errors per municipality are corrected for hetereskodestacity and autocorrelation issues (Bertrand, Duflo & Mullainathan, 2004).

The main variable of interest in equation (1) is $Scut \cdot dT_i$, the interaction of the *Scut*-dummy and the *treatment period*-dummy. The treatment period is defined as follows: for the municipalities where the introduction of tolls occurred on October 15th, 2010, the treatment period dummy dT_i equals 1 from 2011 onwards. Similarly, for the ones where the introduction of tolls happened on December 8th, 2011, dT_i equals 1 in 2012 and 2013.

[7] Note that all 59 municipalities in the treatment group are traversed by a SCUT highway and have direct access (a ramp) to that SCUT highway.

It might be argued, however, that firms in municipalities not directly crossed by SCUT highways could also use the SCUT highways for at least part of their journey and are thus also affected by the introduction of tolls, although indirectly and (presumably) to a lesser extent. We capture this possibility by constructing a second, distance-dependent treatment variable (d-dtv), defined as

$$[(1-p)/100]^{dist}.dT_i$$

where *dist* denotes the driving distance between the city center (town hall) of the municipality in which the firm is located and the nearest SCUT highway and p is a given distance decay rate. Figures 2 and 3 illustrate driving distances between municipalities (town halls) and charged (former toll-free) SCUT highways in 2011 (Figure 2) and from 2012 on (Figure 3).

[Insert Figures 2 and 3 here]

Our second treatment which may be seen as a test of effect heterogeneity (dependent on distance), may thus be written as

(2) $Y_{it} = \beta_0 + \gamma_t + \delta_1[(1-p)/100]^{dist} \cdot dT_i + \alpha_i + \beta_1 X_{it} + e_{it}$

Our empirical strategy is similar in spirit to Datta (2012) who uses a different natural experiment (the 'Golden Quadrilateral' project in India) and constructs similar treatment groups. However, the current study uses a more general distance decay function and has a clear focus on firm numbers and firm performance (in terms of employment), whereas the focus of Datta's study is on days of inventory held and change of supplier relations, i.e. rather indirect measures of firm performance. Moreover, in our data more than one pre-treatment period is available, which enables us perform placebo tests (see section IV.3) and to test the crucial assumption that firms inside and outside the treatment group have common trends during the pre-treatment period (see Sections III.2 and IV.3).

III.2 The data

Dependent variables

The number of firms and the number of employees working in these firms between 2007 and 2013 are calculated from the IES (*Informação Estatística Simplificada*) micro database, an annual survey conducted by the Portuguese Ministry of Finance. Employees are observed at their place of work. Our definition of firm encompasses all strictly private businesses with at least one paid employee in mainland Portugal (cases of self-employment are thereby excluded). Moreover, we restrict our sample to firms' headquarters that do not have branches somewhere else to make a more careful comparison between municipalities. Therefore, we dismiss from our analysis, for example, firms operating in the financial sector. In addition, non-profit organizations are omitted from the dataset.

We also provide a more refined analysis dividing our two dependent variables by sector of activity and by size. The data allow us to distinguish between primary, secondary and tertiary sector firms on the one hand, and between micro (with one or two paid employees), small (from three to ten), and medium and large firms (more than eleven workers) on the other hand.

Controls

To take care of possible confounds (interregional differences not caused by the treatment) we include municipal fixed effects and a vector of time-variant controls to rule out other possible mechanisms affecting our results.[8] For this end, we merge several data sources.

Among the standard controls considered in the literature on regional determinants of new firm formation and employment growth are measures of population density, the share of working age population in total population, regional unemployment and measures reflecting the business cycle (see, for instance, Armington and Acs, 2002 or Audretsch, Dohse & Niebuhr, 2015a). Following Bleakley and Lin (2012) we measure *population density* by the number of inhabitants per square kilometer. We consider the age structure of the regional population by the *age dependency ratio* (number of people above 65 and below 15 divided by the active population). Audretsch, Dohse &Niebuhr (2015a) have shown that not only the level but also the structure of regional unemployment matter for new firm formation. We thus consider not only the *regional unemployment rate* but also the *share of unemployed that have prior working experience* (and are thus more likely to start a new firm than unemployed without working experience). As for Portugal there is no data available for municipal GDP, we use as a proxy of municipal income and purchasing power the *Sales Index* computed by Marktest, as proposed by several papers studying local political business cycles in Portugal (see, for example, Martins and Veiga, 2014).

[8] Moreover, we check the crucial common trends assumption and run placebo regressions in later parts of the paper.

Moreover, our rich municipal level data set allows us to control for institutional and political differences among municipalities that might affect firm formation (and closure) as well as employment growth in the respective municipalities. These include the *business tax rate* (set by the municipal assembly), *mayor tenure* (i.e. the number of consecutive years that the mayor of a given municipality is in power) and a dummy for *same political party*, indicating whether the mayor of a given municipality belongs to the same political party as the prime minister.[9] Finally, we consider a *highways* dummy which takes the value one if there is at least one highway (SCUT or a normal one) crossing a given municipality and zero otherwise.[10]

Table 2 describes the control variables in more detail as well as their specific sources.

[Insert Table 2 here]

Table 3 displays the descriptive statistics for all the variables used in our analysis.

[Insert Table 3 here]

Internal validity considerations

Internal validity of a diff-in-diff framework relies on the parallel trends assumption, i.e. the trend in each of the dependent variables must be the same for all municipalities in the absence of treatment. One common technique to test this requirement is to compare the evolution of the different outcome variables in treated and control units during the pre-treatment period, in our case, between 2007 and 2010. (Angrist and Pischke, 2009, p. 231). Figure 4 shows the pre-treatment evolution for all our dependent variables.

[Insert Figure 4 here]

As can be seen from Figure 4, our graphical inspection does not provide substantive evidence of distinct trends between treatment and control regions capable of undermining the empirical strategy.

[9] The same political party dummy is expected to have a positive sign, as mayors from the same political party as the prime minister are likely to attract more federal funding to their municipality. *Mayor tenure*, by contrast, may have diametral effects: On the one hand, more experience in managing the town hall and a better understanding of the necessities of the region may be beneficial for firm performance in the region. On the other hand, a long mayor tenure and less political competition may be linked with structural conservatism and more corruption, such that the expected overall effect is ambiguous.

[10] As will be shown later, skipping the highways dummy does not change the main results.

Therefore, in light of this analysis, diff-in-diff coefficient estimates can be assumed to depict causal treatment effects.[11]

IV. Results

IV.1 The baseline model

All results discussed in this section include municipal and year fixed effects and have robust standard errors clustered at the municipal level. All dependent variables considered are divided by 100 inhabitants.

The correlation matrix (Table 4) shows that correlation between most RHS variables is rather low. An exception is the relatively high correlation between sales index and population density. We thus ran robustness checks excluding the sales index from the list of control variables, finding that this has no impact on the main results.

[Insert Table 4 here]

Tables 5 and 6 present our baseline results for the diff-in-diff estimations. Table 5 displays the impact of the introduction of tolls on the number of firms per municipality, whereas Table 6 shows the results for the number of employees. Both tables use a binary variable to capture the treatment status for the full sample of mainland Portuguese municipalities. Even columns contain a series of covariates (as discussed in section III) to control for differences in observables between the treatment and control groups.

In all specifications, and in all cuts of the data, our main variable of interest is the causal effect of the introduction of tolls on the SCUTs highways, as portrayed by the interaction term ($Scut \cdot dT_i$).

[Insert Table 5 here]

[Insert Table 6 here]

[11] A more rigorous test of the common trends assumption is provided in Section IV.3.

As can be seen from Tables 5 and 6, the introduction of highway tolls had a significantly negative impact on both, the total number of private firms and on the total number of employees working in these firms in the municipalities traversed by the SCUT highways. This finding is robust with respect to different model specifications (with and without controls) and applies to the majority of sectors and firm size classes that were investigated.[12]

There are, however, some interesting sectoral and size-related differences:

Firms in the agricultural sector, and even more importantly, industrial and manufacturing firms seem to have been hit most severely by the tolls, whereas service sector firms appear to be less affected. We consider this a plausible result, since most service sector firms serve a local market, whereas manufacturing firms, in particular, serve more distant markets or export their products, such that they are more dependent on affordable highway usage than service sector firms. Moreover, firm size seems to play a role as well: While micro firms seem to be not much affected, the implementation of tolls had a significantly negative impact on relatively larger firms. Again, this appears to be a plausible result, given that larger firms are more likely to export their outputs, not only to other countries but also to other municipalities within Portugal.[13, 14]

IV.2 Effect heterogeneity

In the baseline model the treatment group consists of municipalities which are directly crossed by the SCUT highways. As argued in Section III, it might be, however, that firms in municipalities further away from the SCUTs are also affected by the tolls, although (presumably) to a lesser extent.[15] We capture this possibility by constructing a second, distance-dependent treatment variable that takes values

[12] The controls in Tables 5 and 6 include a dummy for highways, capturing both SCUTs and non-SCUTS highways. Note that the results without highway dummy are very similar as can be seen in Appendix A1.

[13] As Caves and Porter (1977) and Porter (1979) argue and provide compelling empirical evidence, the smallest firms tend to occupy what they term as "strategic niches" with limited market opportunities. By contrast, larger firms have the opportunity to access larger markets but are also challenged by a higher cost structure and, in particular, by changes of transport costs.

[14] Moreover, larger firms are more flexible to adjust. With few employees, it might just be infeasible to fire people without having to shut down the firm.

[15] Quite obviously, they depend less on the SCUTs highways as only part of their journey is affected and they are more likely to circumvent them than firms directly located at the SCUT's highways.

between 1 and 0. It is close to 1 for municipalities very close to the SCUT highways and becomes smaller, the further a municipality is away from the next SCUT highway.[16]

As it is not a priori clear, which distance decay rate p is most adequate – and as the pertinent literature gives little guidance in this respect – we present results for a wide range of plausible values of p ($0.05 \leq p \leq 0.15$). As can be seen from Table 7, the results for different values of p are rather similar: If we consider all (mainland) Portuguese municipalities weighted with inverse distance from the SCUTs, the negative significant impact of the road tolls on total employment in private firms is confirmed, whereas the effect on total firm numbers is negative (as expected) but not significant. Hence, it seems that the negative effect of highway tolls on total firm numbers is restricted to the municipalities directly traversed by (or very close to) the SCUTs, whereas the negative employment effect also holds for regions in greater distance to the SCUT highways.

[Insert Table 7 here]

The negative effect on firm numbers is significant, however, for firms in the primary and secondary (manufacturing) sector and – in particular – for larger firms (regardless of the sector). A similar picture emerges for the employment effect: The negative and significant effect of highway tolls on private firms' employment is particularly strong in the manufacturing sector and for larger firms (all sectors).[17]

To sum up, the negative effect of highway tolls on private firm numbers and private firm employment is strongest in the municipalities directly crossed by the SCUT highways, but firm numbers and firm employment in more distant municipalities are affected as well, which is evident in particular for manufacturing firms and for larger firms with more than 10 employees.

IV. 3 Further robustness checks: Event study and placebo regressions

IV.3.1 Event study

[16] See Section III.1 for details.

[17] We performed the same exercise with other functional forms of the distance decay function (simple linear and simple quadratic distance decay) which yielded similar results.

In the baseline model and all its modifications analyzed so far we have identified the average treatment effect over several years in which Portuguese firms were faced with tolls on SCUT highways. We now turn to a parametric event study which allows us (i) to assess whether the strength of the treatment effect varies with the duration of the treatment, and (ii) to assess the validity of the common trends assumption underlying the DID approach more rigorously than before.

The estimation equation for the event study follows the pertinent literature[18] and extends the baseline regression in the following way:

$$(3)\quad Y_{it} = \beta_0 + \gamma_t + \sum_{t=2007}^{2009} \delta_t \cdot Scut \cdot year_t + \sum_{t=2011}^{2013} \delta_t \cdot Scut \cdot year_t + \alpha_1 Scut + \beta_1 X_{it} + e_{it}$$

In equation 3 we consider interaction terms for each single pre-treatment year (except 2010) and for each single year of the treatment period. All coefficients are estimated relative to the year 2010, i.e. relative to the last year before the treatment set in.

Table 8 displays the coefficients that were estimated from equation (3) for both, total number of firms and total employment, whereas Figure 5 illustrates the results along with the 90 %-confidence intervals.

[Insert Table 8 here]

[Insert Figure 5 here]

The estimated interaction terms for all pre-treatment years are small and not significantly different from zero. Hence, the results of the event study provide strong support for the common trends assumption underlying our DID approach.

Only from 2011 on (i.e. after the treatment has set in) the interaction terms become significantly negative. The results imply that already in the first year after the tolls on SCUT highways have been introduced there is a statistically significant difference between SCUT-regions and Non-SCUT regions, both in terms of firm numbers and in terms of employment. There is, however, an interesting difference in detail between our dependent variables. While the negative impact of the introduction of

[18] See Falck, Gold & Heblich, 2016, and Pierce and Schott, 2016 for similar approaches.

highway tolls on firm numbers is strongest in the first year and tends to become smaller over time, the negative impact on employment appears to increase over time.[19]

In sum, the results of the event study do not only support the common trends assumption, but also confirm the results of the baseline model discussed in Section IV.1.

IV.3.2 Placebo regressions

The special structure of our data set with four pre-treatment years (years 2007 – 2010) allows us to perform an additional validity check: We split the pre-treatment period into two (2007-2008 and 2009-2010) and regress pre-treatment outcomes (years 2009-2010) on treatment.

The results of the placebo regressions are displayed in Table 9.

[Insert Table 9 here]

The findings from the placebo test suggest that the previous results for agricultural (sector 1) firms have to be seen with a grain of salt, as the interaction term ($Scut \cdot dT_i$) has a significant negative impact on firm numbers in agriculture in the pre-treatment period (2009-2010) already. We thus cannot interpret the significant effect found in the treatment period as caused by the introduction of highway tolls in the SCUT regions.

The only other significant interaction term in the placebo regressions is found in the regression for firm numbers (size class medium and large). The effect in the pre-treatment period is, however, only weakly significant and much smaller than in the treatment period (see Table 5 for comparison). It does, therefore, not necessarily contradict our previous interpretation.

All other interaction term parameters in the placebo regressions are insignificant. This is, in particular, true for all models in which the number of employees is the dependent variable.

We consider this strong evidence that the significantly negative coefficients of the interaction terms in the treatment period (as found in Tables 5 and 6) are indeed caused by the unexpected introduction of highway tolls, rather than reflecting unobserved SCUT-specific influences.

[19] A possible interpretation is that firms that were most strongly affected (e.g. small firms that could not adapt to the shock by reducing employment) reacted instantaneously by leaving or not entering the market, whereas others (presumably larger firms) reacted by continuously reducing employment.

V. Concluding Remarks

The paper exploits a unique quasi-natural experiment to better understand how an unexpected switch from free to charged highway provision impacts on private sector performance. The sovereign debt crisis that hit the Portuguese political system as a shock induced the Portuguese government to introduce massive tolls on formerly uncharged (SCUT) highways. The introduction of tolls was a purely national matter and only due to budgetary reasons, and it hit economic agents unexpectedly and at short notice.

Our findings indicate a significantly negative effect of highway tolls on the number of firms and on employment in treated municipalities vis-à-vis the control group. Comprehensive robustness checks and studies of effect heterogeneity show that negative effects of highway tolls are also effective in municipalities not directly traversed by the treated highways, and that larger firms and firms from the manufacturing sector are most strongly affected.

There are several key implications for policy from this paper. The first is that imposing fees for access to key infrastructure, such as highways, seems to be detrimental to the high priority policy goals of generating entrepreneurship and employment. In the context of Portugal, the highway tolls have had a clearly negative impact on firm numbers and employment expansion. Hence, the introduction of tolls on formerly uncharged highways – which may have been inevitable for budgetary reasons in the short run – appears to impose substantial cost on the Portuguese economy in the longer run. Given the positive network effects exhibited by infrastructure, a better policy would be to incorporate the positive network externalities by providing free or minimal cost access to key infrastructure, such as highways. The 15 % reduction of tolls announced by the Portuguese government in August 2016 indicates that policymakers have begun to realize that the true social costs of highway tolls might be higher than expected.

The relevance of the results might, however, go well beyond the Portuguese case. [20] In view of public austerity and the rapid deterioration of transport infrastructure in many leading OECD countries such

[20] Note that Portugal's highway network density is among the highest in the OECD, comparable with Germany and clearly higher than in the US (OECD 2013).

as the US or Germany (see, for instance, ASCE, 2016; Financial Times, 2016 or IMF, 2016),[21] the question arises whether investment in transport infrastructure should be a policy priority in the years to come. Although this question is, of course, too complex to give definite answers based on a single paper, the results of the current paper at least suggest that the public provision of transport infrastructure at zero (or reasonably low) user charges might pay off in terms of higher firm numbers and employment in the private business sector.

[21] Although properly designed and well-built originally they have worn out from use or have become obsolete as a result of changing needs.

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Table 1. SCUT highways and affected municipalities

Highway	Affected Municipalities
Tolls introduced on October 15th, 2010	
SCUT Grande Porto – 79 Km	
A4: AE Transmontana	Matosinhos, Maia
A41: CREP - Circular Regional Exterior do Porto	Matosinhos, Valongo, Santa Maria da Feira, Espinho
A42: AE Douro Litoral	Valongo, Paços de Ferreira, Paredes, Lousada
SCUT Litoral Norte -113 Km A28	Matosinhos, Vila do Conde, Póvoa de Varzim, Esposende, Viana do Castelo, Caminha
SCUT Costa da Prata – 110 Km A29	Estarreja, Ovar, Espinho, Vila Nova de Gaia
Tolls introduced on December 8th, 2011	
SCUT Algarve – 133 Km A22	Lagos, Monchique, Portimão, Lagoa, Silves, Albufeira, Loulé, Faro, Olhão, Tavira, Castro Marim, Vila Real de Sto. António
SCUT Beira Interior – 217 Km A23	Torres Novas, Entroncamento, Constância, Abrantes, Mação, Gavião, Vila Velha de Rodão, Vila Nova da Barquinha, Castelo Branco, Fundão, Belmonte, Covilhã, Guarda
SCUT Interior Norte – 162 Km A24	Viseu, Castro Daire, Lamego, Peso da Régua, Vila Real, Vila Pouca de Aguiar, Chaves
SCUT Beiras Litoral e Alta – 173 Km A25	Ílhavo, Aveiro, Albergaria-a-Velha, Sever do Vouga, Oliveira de Frades, Vouzela, Viseu, Mangualde, Fornos de Algodres, Celorico da Beira , Guarda, Pinhel, Almeida

Figure 1. Geographical distribution of municipalities with and without SCUT Highways

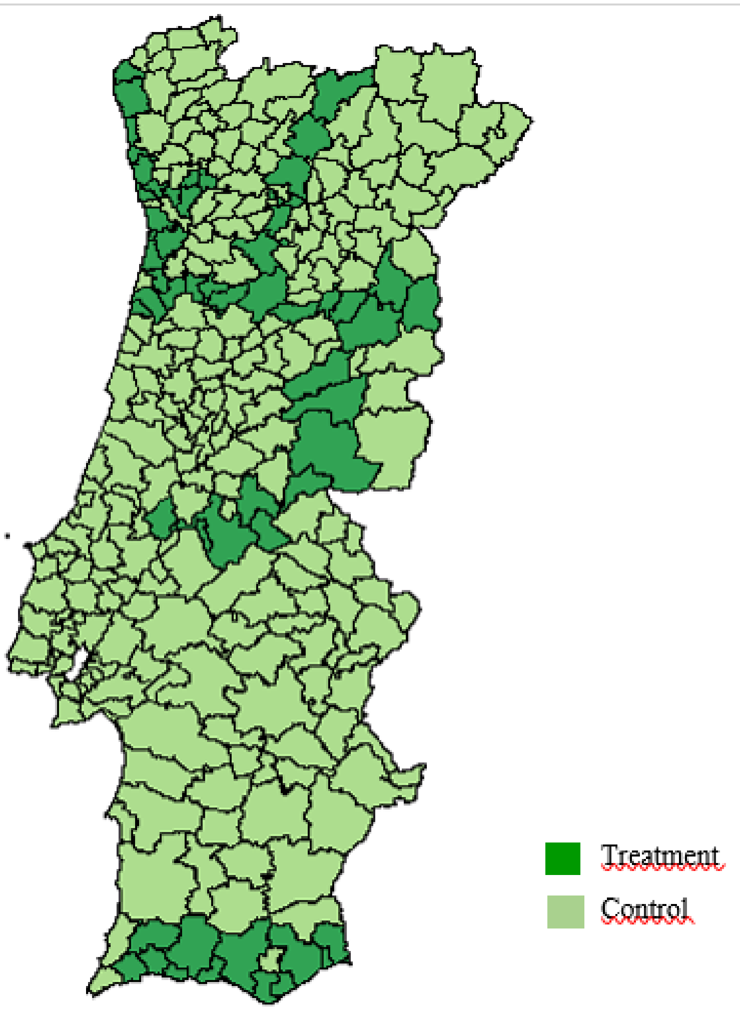


Figure 2: Driving distances from Portuguese municipalities (town halls) to charged (formerly free) SCUT highways in 2011

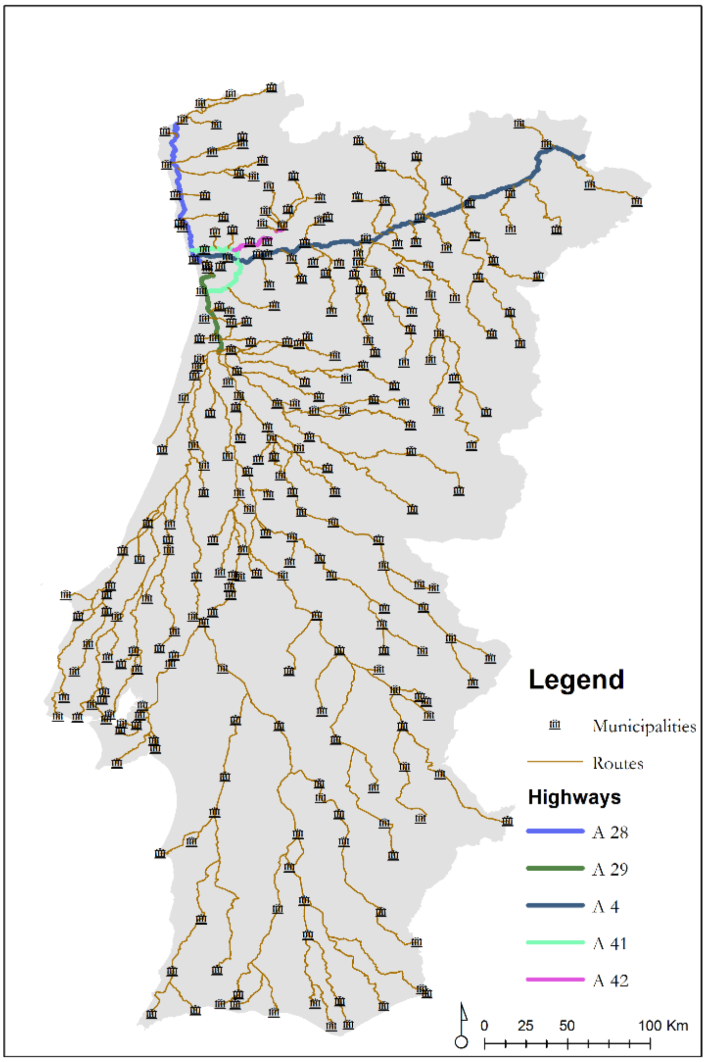


Figure 3: Driving distances from Portuguese municipalities (town halls) to charged (formerly free) SCUT highways from 2012 on

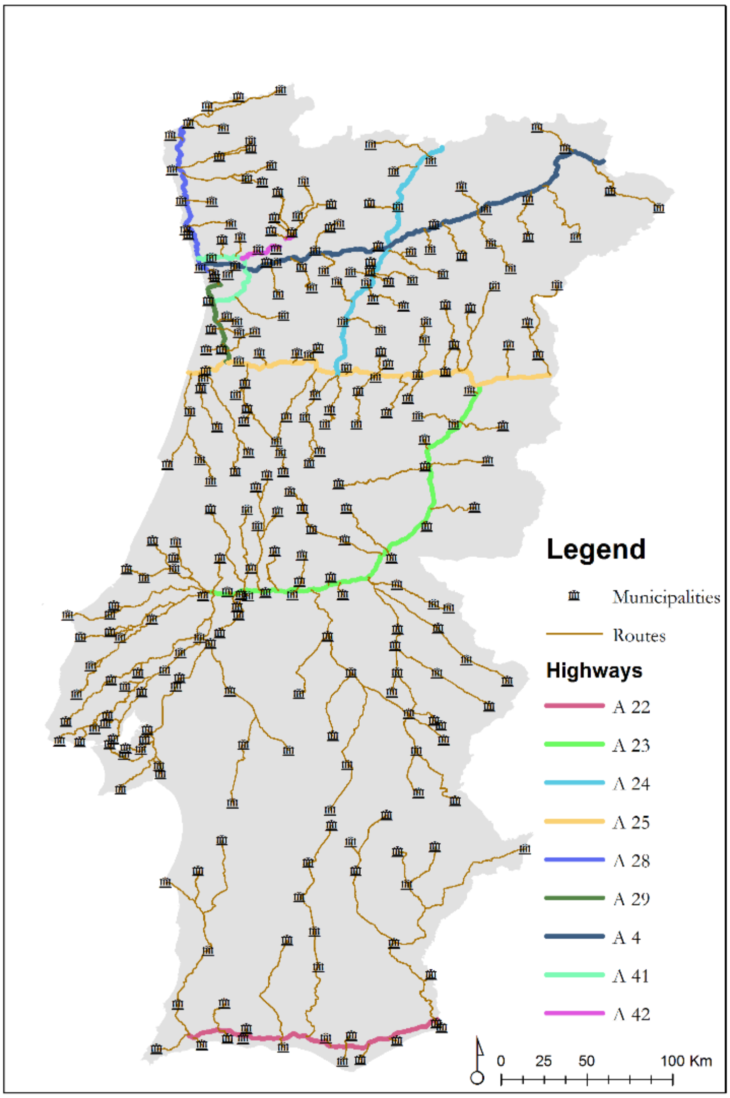


Table 2. Control variables description

Variable	Operational Description	Data source
Population density	Total number of citizens inhabiting a given municipality divided by the respective total area	*INE*
Age Dependency Ratio	Ratio of individuals typically not in the labor force (aged 0-14 and 65+) and active population (aged 15-64)	*INE*
Unemployment Rate	Ratio of registered unemployed per 100 active aged individuals in the municipality (Percentage)	*IEFP*
UnempShare_Not First	Share of the unemployed that were already employed, at least once, in the past per municipality (Percentage)	*IEFP*
Sales Index	This index takes into account both population and wealth in each municipality. It is computed according to the following equation: $$Sales\ Index\ m = 0.2\ Pop\ m + 0.8\ (\sum_{j=1}^{5} W\ mj)/5$$ Where $Pop\ m$ is the share of municipality m in the national population, and $W\ mj$ is the weight of the municipality m in the country total regarding each of the five variables j (fiscal burden, electricity consumption, number of cars sold, number of bank agencies, and number of retail commercial establishments). $Sales\ Index$ is normalized so that a value of 100 corresponds to the country average.	*Marktest*
Mayor Tenure	Number of consecutive years that the mayor of a given municipality remains is in power	*DGAL*
Business Tax Rate	Tax rate set by the Municipal Assembly that is paid by the firms in each municipality	*DGAL*
Same Political Party dummy	Binary variable that takes the value one if the prime minister and the mayor of a given municipality belong to the same national party	*CNE*
Highways dummy	Binary variable that takes the value one if there is at least one highway crossing a given municipality	*ANSR*

INE (Statistics Portugal); IEFP (Instituto de Emprego e Formação Profissional) - National Employment Agency; Marktest, a private company that builds indicators for Portuguese municipalities; DGAL (*Direção Geral das Autarquias Locais*) - government body for local institutions; CNE (*Comissão Nacional de Eleições*) - government body for elections; ANSR (*Autoridade Nacional de Segurança Rodoviária*) - government body for road security.

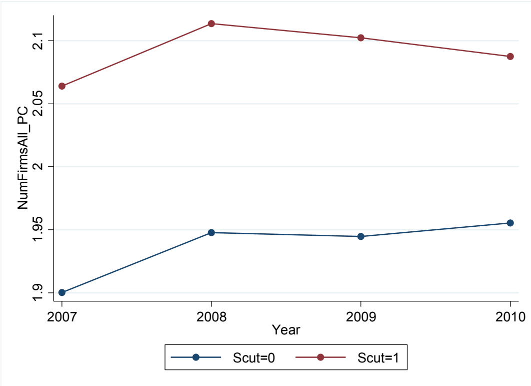
Table 3. Descriptive statistics.

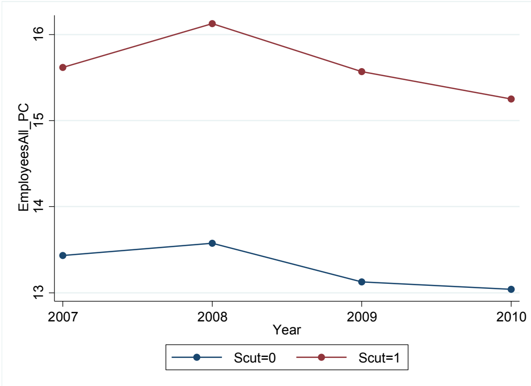
Variable	Obs	Mean	Std. Dev.	Min	Max
Firms					
Total_PC	1946	1.956	0.587	0.798	5.103
Sector 1_PC	1946	0.147	0.146	0	0.880
Sector 2_PC	1946	0.549	0.250	0.077	1.530
Sector 3_PC	1946	1.260	0.498	0.308	4.621
Size Micro_PC	1946	0.874	0.284	0.282	2.859
Size Small_PC	1946	0.844	0.261	0.270	1.918
Size Medium_PC	1946	0.238	0.126	0	0.716
Employees					
Total_PC	1946	13.277	6.732	2.572	46.650
Sector 2_PC	1946	6.375	5.016	0.231	33.905
Sector 3_PC	1946	5.979	3.852	0.809	40.828
Size Micro_PC	1946	1.266	0.403	0.382	3.909
Size Small_PC	1946	4.249	1.378	1.416	9.781
Size Medium_PC	1946	7.762	5.575	0	38.106
Vector of controls					
Population Density	1946	0.310	0.844	0.005	7.384
Age Dependency Ratio	1946	0.591	0.119	0.376	1.088
Unemployment Rate	1946	0.090	0.083	0.004	0.194
Unemp Share_Not First	1946	0.894	0.047	0.693	0.990
Sales Index	1946	3.597	7.882	0.190	113.230
Mayor Tenure	1946	10.520	7.445	1.000	37.000
Business Tax Rate	1946	0.009	0.007	0	0.015
Same Political Party dummy	1946	0.405	0.491	0	1
Highways dummy	1946	0.562	0.496	0	1

Note: PC stands for 100 inhabitants.

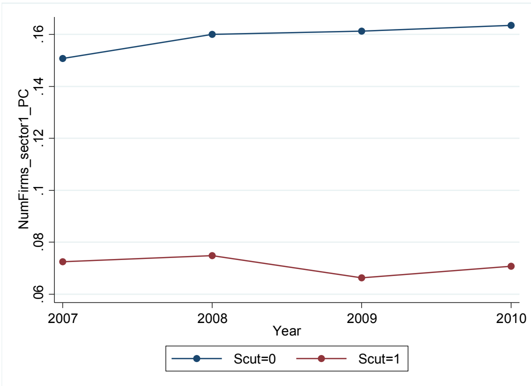
Figure 4. Parallel trends assumption

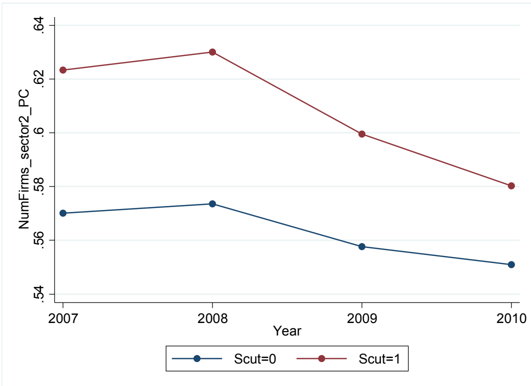
1. Number of Firms and Number of Employees (total, per 100 inhabitants)



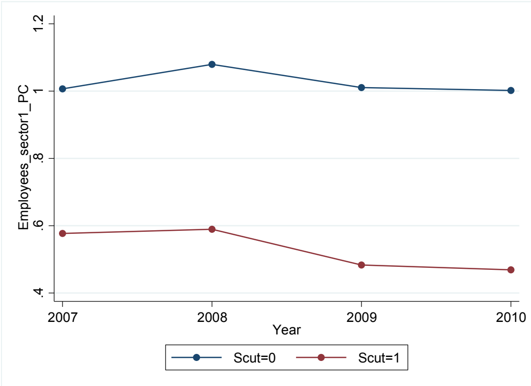


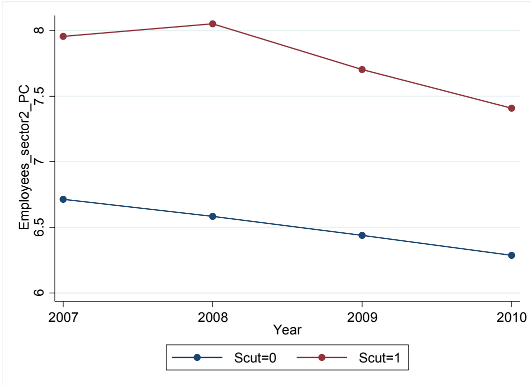
2. Number of Firms and Number of Employees per sector
   (a) Number of Firms

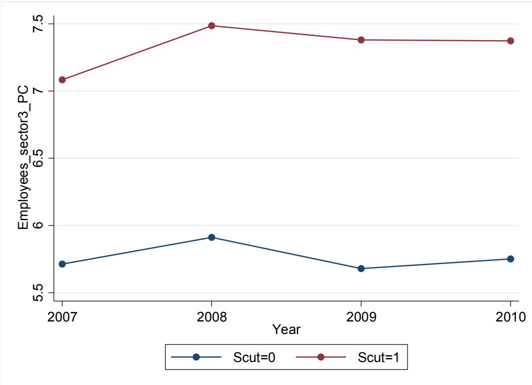




(b) Employees

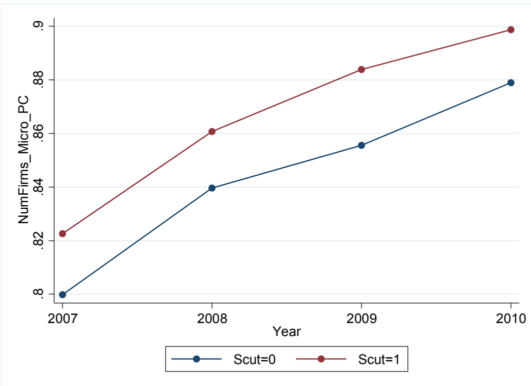


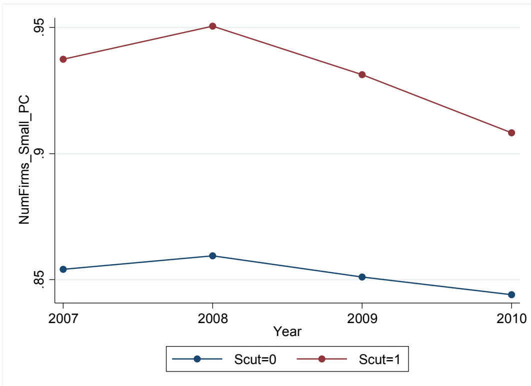


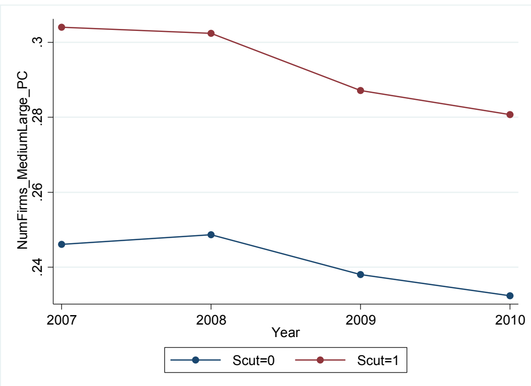


3. Number of Firms and Number of Employees per size

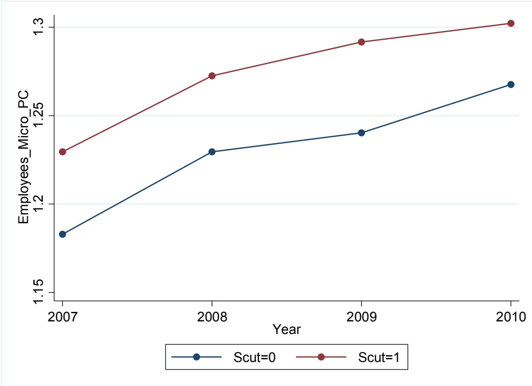
(a) Number of Firms

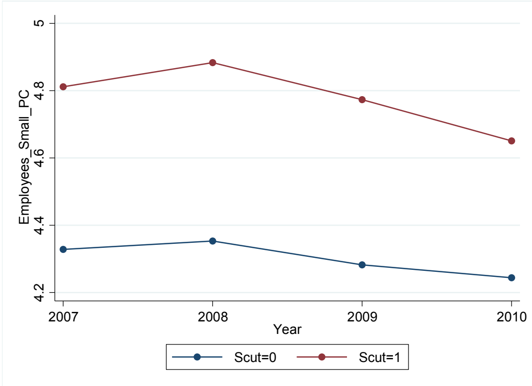






(b) Employees


Employees_Micro_PC
1.15
1.2
1.25
1.3
2007
2008
2009
2010
Year
Scut=0
Scut=1


Employees_Small_PC
4.2
4.4
4.6
4.8
5
2007
2008
2009
2010
Year
Scut=0
Scut=1

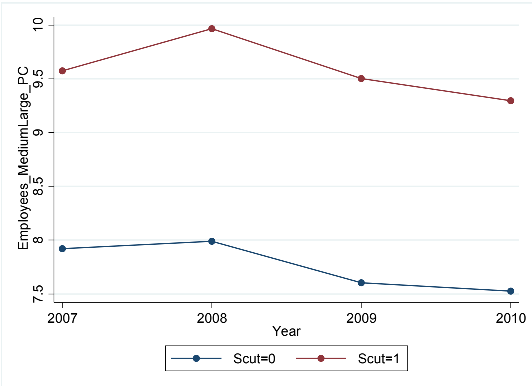

Employees_MediumLarge_PC
7.5
8
8.5
9
9.5
10
2007
2008
2009
2010
Year
Scut=0
Scut=1

Table 4. Correlation Matrix.

	Firms: Total_PC	Employees: Total_PC	Population Density	Age Dependency Ratio	Unemployment Rate	Unemp Share_Not First	Sales Index	Mayor Tenure	Business Tax Rate	Same Political Party dummy	Highways dummy
Firms: Total_PC	1										
Employees: Total_PC	0.792	1									
Population Density	0.391	0.318	1								
Age Dependency Ratio	-0.436	-0.541	-0.277	1							
Unemployment Rate	-0.067	-0.044	-0.029	-0.049	1						
Unemp Share_Not First	0.364	0.311	0.263	-0.297	0.081	1					
Sales Index	0.510	0.412	0.693	-0.254	-0.057	0.241	1				
Mayor Tenure	0.018	-0.015	-0.004	-0.040	-0.020	0.024	-0.010	1			
Business Tax Rate	0.298	0.327	0.245	-0.388	0.021	0.265	0.259	0.034	1		
Same Political Party dummy	-0.019	-0.037	-0.017	0.031	0.010	-0.015	-0.037	0.007	-0.006	1	
Highways dummy	0.341	0.397	0.195	-0.402	0.001	0.297	0.288	-0.022	0.343	0.013	1

Table 5. Number of Firms

	Total_PC	Total_PC	Sector 1_PC	Sector 1_PC	Sector 2_PC	Sector 2_PC	Sector 3_PC	Sector 3_PC	Size Micro_PC	Size Micro_PC	Size Small_PC	Size Small_PC	Size Medium and Large_PC	Size Medium and Large_PC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
$Scut \cdot dT_i$	-0.036**	-0.031*	-0.009**	-0.008**	-0.026**	-0.023**	-0.001	-0.000	0.006	0.004	-0.029**	-0.022**	-0.014***	-0.014***
	(0.018)	(0.017)	(0.004)	(0.004)	(0.010)	(0.010)	(0.011)	(0.012)	(0.009)	(0.009)	(0.011)	(0.011)	(0.005)	(0.005)
Population Density		0.069		0.008		-0.001		0.062		0.067*		0.002		0.000
		(0.065)		(0.008)		(0.021)		(0.045)		(0.040)		(0.034)		(0.010)
Age Dependency Ratio		-1.404***		-0.256**		-0.622***		-0.525**		-0.199		-1.136***		-0.068
		(0.425)		(0.104)		(0.221)		(0.242)		(0.275)		(0.206)		(0.077)
Unemployment Rate		-0.094		-0.069		-0.043		0.018		0.033		-0.068		-0.060
		(0.199)		(0.052)		(0.103)		(0.100)		(0.107)		(0.145)		(0.049)
UnempShare_NotFirst		-0.236		-0.112**		-0.035		-0.088		-0.210**		0.006		-0.032
		(0.193)		(0.049)		(0.096)		(0.120)		(0.104)		(0.117)		(0.041)
Sales Index		0.002		-0.001		0.005		-0.003		-0.001		0.003		0.001
		(0.005)		(0.001)		(0.003)		(0.006)		(0.006)		(0.003)		(0.002)
Mayor Tenure		-0.000		0.000		-0.000		-0.000		0.001*		-0.001		-0.000
		(0.001)		(0.000)		(0.001)		(0.001)		(0.001)		(0.001)		(0.000)
Business Tax Rate		-0.765		-0.485*		0.215		-0.496		-0.267		-0.587		0.089
		(1.502)		(0.293)		(0.875)		(0.880)		(0.733)		(0.879)		(0.359)
Same Political Party dummy		0.006		-0.000		-0.002		0.008		0.003		0.002		0.001
		(0.010)		(0.002)		(0.005)		(0.006)		(0.005)		(0.006)		(0.002)
Highways dummy		0.010		0.003		0.003		0.004		-0.034		0.038		0.005
		(0.049)		(0.010)		(0.020)		(0.030)		(0.030)		(0.026)		(0.012)
Municipal Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946
Adjusted R2	0.057	0.094	0.098	0.120	0.259	0.280	0.072	0.085	0.263	0.271	0.213	0.266	0.367	0.371

Note: Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***). PC stands for 100 inhabitants.

Table 6. Number of Employees

	Total_PC	Total_PC	Sector 1_PC	Sector 1_PC	Sector 2_PC	Sector 2_PC	Sector 3_PC	Sector 3_PC	Size Micro_PC	Size Micro_PC	Size Small_PC	Size Small_PC	Size Medium and Large_PC	Size Medium and Large_PC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
$Scut \cdot dT_i$	-0.648***	-0.529**	-0.079**	-0.070**	-0.442**	-0.365*	-0.126	-0.094	0.005	0.003	-0.166***	-0.133**	-0.487**	-0.399*
	(0.232)	(0.221)	(0.033)	(0.030)	(0.198)	(0.187)	(0.103)	(0.104)	(0.015)	(0.014)	(0.062)	(0.059)	(0.213)	(0.203)
Population Density		-0.059		0.038		-0.095		-0.002		0.077		0.037		-0.172
		(0.815)		(0.046)		(0.248)		(0.651)		(0.049)		(0.168)		(0.710)
Age Dependency Ratio		-9.991***		-0.175		-5.916**		-3.900***		-0.511		-5.385***		-4.094
		(3.278)		(0.862)		(2.578)		(1.487)		(0.393)		(1.172)		(2.741)
Unemployment Rate		-2.770		0.605		-2.327		-1.048		0.028		-0.247		-2.552
		(2.574)		(0.617)		(2.121)		(1.544)		(0.164)		(0.791)		(2.363)
UnempShare_NotFirst		-0.810		-0.648		1.042		-1.204		-0.319**		0.129		-0.621
		(1.779)		(0.570)		(1.415)		(0.834)		(0.158)		(0.603)		(1.497)
Sales Index		-0.064		0.006		-0.020		-0.050		-0.002		0.010		-0.073*
		(0.044)		(0.005)		(0.040)		(0.059)		(0.007)		(0.013)		(0.041)
Mayor Tenure		-0.024		-0.005		-0.012		-0.007		0.002		-0.005		-0.020
		(0.018)		(0.007)		(0.010)		(0.009)		(0.001)		(0.005)		(0.016)
Business Tax Rate		-6.856		-4.330		0.368		-2.894		0.659		-2.206		-5.309
		(15.032)		(2.772)		(11.846)		(8.023)		(1.164)		(4.455)		(12.793)
Same Political Party dummy		-0.176*		0.006		-0.155*		-0.027		-0.001		0.003		-0.177*
		(0.106)		(0.023)		(0.084)		(0.049)		(0.008)		(0.031)		(0.091)
Highways dummy		0.303		0.046		0.194		0.062		-0.052		0.132		0.222
		(0.477)		(0.082)		(0.327)		(0.182)		(0.046)		(0.117)		(0.371)
Municipal Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946
Adjusted R2	0.267	0.285	0.007	0.010	0.254	0.265	0.076	0.085	0.146	0.158	0.264	0.301	0.206	0.216

Note: Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***). PC stands for 100 inhabitants.

Table 7. Distance Decay Functions Robustness Checks.

	No of Firms					No of Employees				
	Total_PC	Sector 1_PC	Sector 2_PC	Size Small_PC	Size Medium and Large_PC	Total_PC	Sector 1_PC	Sector 2_PC	Size Small_PC	Size Medium and Large_PC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
p=0,05	0.004	-0.011*	-0.018	-0.005	-0.013**	-0.495*	-0.095	-0.467**	-0.045	-0.489*
	(0.025)	(0.006)	(0.012)	(0.015)	(0.006)	(0.286)	(0.058)	(0.223)	(0.077)	(0.254)
p=0,075	-0.013	-0.011*	-0.026**	-0.015	-0.017**	-0.647**	-0.099*	-0.546**	-0.096	-0.585**
	(0.026)	(0.006)	(0.013)	(0.016)	(0.007)	(0.307)	(0.056)	(0.243)	(0.082)	(0.273)
p=0,1	-0.022	-0.012*	-0.031**	-0.02	-0.020***	-0.739**	-0.102*	-0.592**	-0.123	-0.645**
	(0.027)	(0.006)	(0.014)	(0.017)	(0.007)	(0.329)	(0.056)	(0.261)	(0.088)	(0.293)
p=0,125	-0.028	-0.012*	-0.033**	-0.023	-0.022***	-0.801**	-0.106*	-0.622**	-0.139	-0.690**
	(0.029)	(0.006)	(0.016)	(0.018)	(0.008)	(0.35)	(0.057)	(0.278)	(0.095)	(0.313)
p=0,15	-0.032	-0.013*	-0.035**	-0.025	-0.024***	-0.850**	-0.110*	-0.644**	-0.148	-0.726**
	(0.030)	(0.007)	(0.017)	(0.019)	(0.009)	(0.372)	(0.059)	(0.294)	(0.101)	(0.334)
Municipal Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946
Adjusted R2										
p=0,05	0.053	0.098	0.254	0.208	0.364	0.261	0.007	0.252	0.258	0,204
p=0,075	0.053	0.098	0.256	0.209	0.366	0.263	0.006	0.253	0.258	0,205
p=0,1	0.053	0.098	0.256	0.209	0.367	0.263	0.006	0.253	0.259	0,205
p=0,125	0.054	0.097	0.256	0.209	0.367	0.263	0.006	0.252	0.259	0,205
p=0,15	0.054	0.097	0.256	0.209	0.367	0.263	0.006	0.252	0.259	0,204

Note: All regressions include municipal and year fixed effects. Results with the set of controls considered in the previous exercise are very similar. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***). PC stands for 100 inhabitants.

Table 8: Results of Event Study

	No of Firms	No of Employees
	Total_PC (1)	Total_PC (2)
Scut · Year_2007	0.024 (0.023)	-0.12 (0.298)
Scut · Year_2008	0.018 (0.019)	0.194 (0.249)
Scut · Year_2009	0.013 (0.014)	0.089 (0.186)
Scut · Year_2011	**-0.049***** **(0.018)**	**-0.446*** **(0.270)**
Scut · Year_2012	**-0.039**** **(0.018)**	**-0.561**** **(0.267)**
Scut · Year_2013	-0.036 (0.023)	**-0.725**** **(0.320)**
Scut Dummy	Yes	Yes
Year Fixed Effects	Yes	Yes
Number of observations	1 946	1 946
Adjusted R2	0.388	0.393

Note: Standard errors are clustered at the municipal level and robust to heteroscedasticity.
Stars indicate significance levels of 10% (*), 5% (**), and 1% (***).
PC stands for 100 inhabitants.

Figure 5. Event Study Results with 90% Confidence Interval

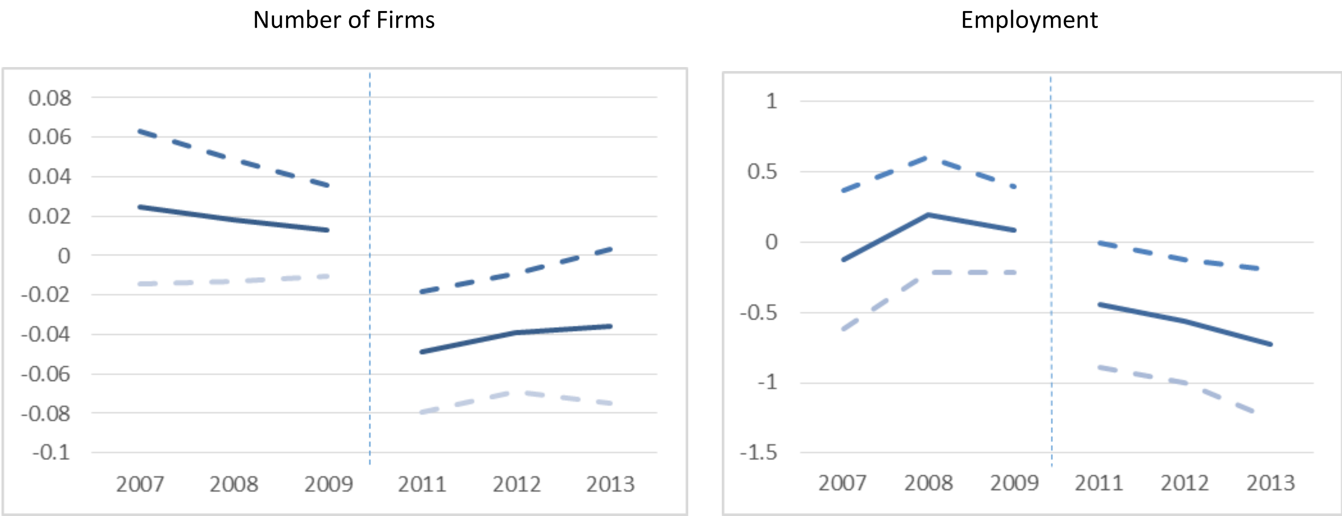


Table 9: Results of Placebo Regressions (using the years 2009 and 2010 as "treatment period")

	No of Firms					No of Employees				
	Total_PC	Sector 1_PC	Sector 2_PC	Size Small_PC	Size Medium and Large_PC	Total_PC	Sector 1_PC	Sector 2_PC	Size Small_PC	Size Medium and Large_PC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Scut . dTi	-0.02	-0.012***	-0.019	-0.015	-0.007*	-0.041	-0.07	-0.162	-0.058	0.019
	(0.015)	(0.004)	(0.012)	(0.01)	(0.004)	(0.199)	(0.055)	(0.211)	(0.051)	(0.183)
Municipal Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946
Adjusted R2	0.084	0.033	0.078	0.029	0.123	0.072	0.009	0.059	0.04	0.062

Note: Results with the set of controls considered are are very similar. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***). PC stands for 100 inhabitants.

APPENDIX

Table A1: Results of Baseline Model without Highway Dummy

	No of Firms							No of Employees						
	Total_PC	Sector 1_PC	Sector 2_PC	Sector 3_PC	Size Micro_PC	Size Small_PC	Size Medium and Large_PC	Total_PC	Sector 1_PC	Sector 2_PC	Sector 3_PC	Size Micro_PC	Size Small_PC	Size Medium and Large_PC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	-0.032*	-0.008**	-0.023**	-0.000	0.005	-0.023**	-0.014***	-0.541**	-0.072**	-0.372**	-0.096	0.005	-0.138**	-0.408**
	(0.017)	(0.004)	(0.010)	(0.012)	(0.009)	(0.011)	(0.005)	(0.221)	(0.030)	(0.186)	(0.103)	(0.014)	(0.058)	(0.203)
Population Density	0.070	0.008	-0.001	0.063	0.064	0.005	0.001	-0.031	0.042	-0.077	0.004	0.072	0.049	-0.151
	(0.065)	(0.008)	(0.020)	(0.045)	(0.040)	(0.032)	(0.010)	(0.817)	(0.045)	(0.243)	(0.653)	(0.050)	(0.163)	(0.713)
Age Dependency Ratio	-1.402***	-0.256**	-0.622***	-0.525**	-0.205	-1.130***	-0.067	-9.941***	-0.168	-5.884**	-3.889***	-0.519	-5.364***	-4.058
	(0.425)	(0.104)	(0.221)	(0.242)	(0.275)	(0.206)	(0.077)	(3.279)	(0.862)	(2.578)	(1.486)	(0.395)	(1.172)	(2.742)
Unemployment Rate	-0.093	-0.069	-0.043	0.019	0.030	-0.064	-0.059	-2.744	0.609	-2.310	-1.042	0.024	-0.235	-2.532
	(0.198)	(0.053)	(0.103)	(0.100)	(0.109)	(0.145)	(0.049)	(2.573)	(0.616)	(2.120)	(1.543)	(0.167)	(0.792)	(2.362)
UnempShare_NotFirst	-0.232	-0.111**	-0.034	-0.087	-0.222**	0.020	-0.030	-0.701	-0.632	1.113	-1.182	-0.337**	0.177	-0.540
	(0.190)	(0.049)	(0.095)	(0.119)	(0.105)	(0.117)	(0.041)	(1.762)	(0.570)	(1.408)	(0.827)	(0.159)	(0.597)	(1.488)
Sales Index	0.002	-0.001	0.005	-0.003	-0.001	0.002	0.001	-0.065	0.006	-0.021	-0.051	-0.001	0.010	-0.074*
	(0.005)	(0.001)	(0.003)	(0.006)	(0.006)	(0.003)	(0.002)	(0.043)	(0.005)	(0.041)	(0.058)	(0.007)	(0.014)	(0.040)
Mayor Tenure	-0.000	0.000	-0.000	-0.000	0.001	-0.001	-0.000	-0.023	-0.005	-0.011	-0.007	0.002	-0.005	-0.020
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.019)	(0.007)	(0.010)	(0.009)	(0.001)	(0.005)	(0.016)
Business Tax Rate	-0.772	-0.487*	0.214	-0.499	-0.242	-0.615	0.085	-7.075	-4.363	0.228	-2.939	0.696	-2.302	-5.469
	(1.502)	(0.293)	(0.876)	(0.880)	(0.735)	(0.880)	(0.359)	(15.024)	(2.771)	(11.838)	(8.021)	(1.167)	(4.454)	(12.785)
Same Political Party dummy	0.006	-0.000	-0.002	0.008	0.003	0.002	0.001	-0.176*	0.006	-0.155*	-0.027	-0.001	0.002	-0.178*
	(0.010)	(0.002)	(0.005)	(0.006)	(0.005)	(0.006)	(0.002)	(0.106)	(0.023)	(0.085)	(0.049)	(0.008)	(0.031)	(0.092)
Municipal Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946	1 946
Adjusted R2	0.094	0.121	0.280	0.086	0.270	0.264	0.371	0.285	0.010	0.265	0.086	0.156	0.300	0.217

Note: Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***). PC stands for 100 inhabitants.