Can Tax Enforcement Increase Employment?

Besart Avdiu*

Job Market Paper

November 11, 2018

Abstract

This paper shows that tax enforcement can increase employment. I consider enforcement policies aimed at increasing the issuance of verifiable sales receipts, which reduces unregistered sales and therefore tax evasion by firms. At first glance, the associated increase in the tax burden should negatively impact employment. I argue, however, that tax enforcement of this form can also help firms monitor their employees. If employees can divert some of the unregistered sales from the firm to themselves, then tax enforcement can increase employment by reducing diversion. To test this hypothesis, I use administrative data and exploit quasi-experimental variation from a tax enforcement intervention in Portugal. The results support the diversion channel and suggest that the enforcement reform did in fact increase employment.

Keywords: Tax Enforcement, Employment, Informality

JEL Classification: H25, H26, J23, 017

*Goethe University Frankfurt. Contact: avdiu@its.uni-frankfurt.de. I would like to thank Alfons Weichenrieder, Matthias Schündeln, Philipp Harms, Ester Faia, Andreas Haufler, Christian Holzner, Marko Koethenbuerger, Dominika Langenmayr, Niklas Potrafke, Dirk Schindler, as well as participants at various presentations for their comments. I am indebted to the team at the Banco de Portugal for assistance with accessing the data, especially Marta Silva, Joana Pimentel and Miguel Portela.

1 Introduction

In the efforts to raise sufficient tax revenue, curbing evasion has recently received particular attention[1]. In addition to undermining public finances, evasion and informality pose a crucial problem, because they introduce various forms of inequity. As La Porta and Shleifer (2014) note, informality can take many forms, ranging from firms not being registered at all to firms that hire formal employees and comply with regulations, but underreport sales to evade taxes.

Fighting informality, however, is not uncontroversial, because the associated increase in tax burdens can come at a large cost for the economy. Furthermore, Besley and Persson (2014) argue that the most important factor in reducing informality is not any improvement of the tax system, but rather an increase in economic growth. La Porta and Shleifer (2014) come to a similar conclusion and show that measures encouraging formality often fail. They caution that policies attempting to formalize firms may not only fail to raise revenue, but also harm informal firms, thereby increasing unemployment. In this sense, reforms aimed at curbing evasion can be misguided. Despite such concerns, however, there has not been much empirical research on the employment effects of tax enforcement[2]. Furthermore, this literature seems to suggest that policymakers in countries with high informality and unemployment may be unable to actively improve their situation[3].

This paper focuses on the employment effects of tax enforcement to reduce the underreporting of sales and shows that such policies can in fact increase employment. In this context, enforcement takes the form of an increase in the issuance of verifiable sales receipts, which helps the government detect tax evasion. At first glance, such policies should reduce employment due to the increase in the effective tax rate. I show, however, that there are also positive effects, when employees can divert sales from the firm. If employees pocket unregistered sales,

[1] Beyond a recent academic literature summarized in Slemrod (2016), this is also reflected in policy measures, such as the plan for Portugal by the European Commission, Directorate-General for Economic and Financial Affairs (2011).

[2] While much of the literature has focused on the tax compliance effects of tax enforcement, the papers studying employment effects largely analyze policies that make voluntary formal registration more attractive, e.g. de Mel et al. (2013), Bruhn (2011), Kaplan et al. (2011), Fajnzylber et al. (2011).

[3] One issue is that promoting growth, which could solve informality, is difficult without sufficient tax revenues.

receipts can reduce diversion by helping employers monitor their employees, just as it helps the government to monitor the firms. In turn, this will reduce effective wages (and potentially monitoring costs), thereby increasing labor demand. While reduced diversion may also decrease labor supply, it is still possible that equilibrium employment increases, if labor demand is more sensitive to diversion than labor supply.

As an example, consider a hairdresser, who is not forced to issue receipts. It is then very difficult for an absent owner to know the true sales, which the employee can pocket. With receipts this becomes less likely. While employee diversion has been an understudied issue, it remains an important concern, especially in developing economies, where informality and tax evasion are most widespread[4]. A recent World Bank survey by Yesegat et al. (2015) illustrates this point. They find that 20% of Ethiopian firms see reduced theft by employees as an advantage of electronic sales registration machines, which communicate business transactions directly to the tax authorities.

For the empirical analysis, I exploit quasi-random variation from a tax enforcement intervention in Portugal and use administrative data on essentially the universe of non-financial firms. For certain sectors[5], the new policy incentivized consumers to request verifiable receipts, by allowing them to deduct a portion of the VAT paid from their personal income taxes. When claiming a tax deduction, consumers reported any sales that businesses may have hidden. Such interventions introduce third-party reporting, which is a popular form of tax enforcement that has been shown to reduce tax evasion (e.g. Kleven et al. 2011; Naritomi 2015; Carrillo et al. 2017; Brockmeyer et al. forthcoming). Indeed, the European Commission report by Fooken et al. (2014) also notes that this Portuguese intervention has in fact improved tax compliance[6]. As a result, similar policies are becoming increasingly popular and many countries have established incentives for consumers to request receipts (see Bird 1992; Cowell 2004; Fooken et al. 2014; Fabbri 2015; Marchese 2009).

[4]For evidence on the link between informality and development see e.g. Besley and Persson (2014), Schneider and Enste (2002) and Gordon and Li (2009).

[5]The affected sectors were maintenance and repair of motor vehicles and motorcycles, accommodation, food services, hairdressing and beauty parlors.

[6]They indicate a 2.5%-9.5% increase in reported growth for the affected sectors and an increase in the number of companies issuing receipts of almost 40%.

The channel proposed here is particularly important for service sectors, such as those affected by the Portuguese intervention, where there is little or no inventory, which makes diversion easier. The results indeed show positive effects of the reform, with firms increasing formal[7] employment by around 3.5% according to the baseline estimates. Nevertheless, this estimate may underestimate the true employment increase, because the intervention is likely to first increase informal employment, which is cheapest, before increasing the number of formal employees. For the same reason, it is unlikely that the effects can be explained by a pure shift of informal to formal employment.

To test the employee diversion channel, I also analyze the effect on wages. The model predicts that the intervention will cause an increase, because less diversion results in increased labor demand and possibly a decline in labor supply. This effect is also confirmed by the data, with wages increasing by around 2%.

Furthermore, social trust may also play a role in this context. It is likely that regions with stronger trust have trustworthier employees, who engage less in sales theft. Therefore, firms in such districts benefit less from the increased monitoring induced by tax enforcement and should exhibit smaller employment effects. By using voter turnout in the European Parliamentary Election of 2009 as a proxy for trust, similarly to Guiso et al. (2004), I show that firms in districts with lower trust indeed experienced stronger employment and wage increases[8].

The results imply that such policies can simultaneously fight unemployment and tax evasion. While the net effect on employment of such policies may not always be positive, the evidence suggests that enforcement can have a positive labor demand effect, which mitigates the negative effects associated with increased tax burdens. Therefore, policymakers facing informality may be able to raise tax revenues at a lower economic cost than previously thought.

The two most related papers are Naritomi (2015) and Pavia (2017). Pavia (2017) analyzes the same Portuguese reform, but focuses only on the effects on corporate tax compliance and

[7] Informal employees cannot be reasonably captured in administrative data.

[8] Lower trust areas may also have higher initial tax evasion rates, in which case we would expect the opposite effects, as these firms experience larger tax burden increases.

does not consider employment. She shows that firms bunch at zero profits, which is a sign of tax evasion and that the degree of bunching is reduced by the intervention. Naritomi (2015) analyzes the tax compliance effects of a similar reform in São Paulo, Brazil. As a robustness check, she also examines the effects on employment, but finds none, which is attributed to an insufficient increase in the tax burden that only affects evasion rents. I argue, however, that enforcement can put upward pressure on labor demand, which is consistent with her non-negative effects. An explanation for the differences in our results could be that the size of the positive labor demand effect varies across countries. Furthermore, the presence of informal employees might bias the intervention's effects toward zero[9]. If employees in Brazil are less likely to be formally registered[10] than in Portugal, this could explain why I find positive effects, while Naritomi (2015) finds none.

Another closely related paper is Desai et al. (2007), who analyze situations where insider shareholders can divert funds from outsiders and show that the tax system affects the amount of private benefits extracted. The paper is also related to the literature on the economic effects of social trust (e.g. Knack and Keefer 1997; Guiso et al. 2004; Nannicini et al. 2013). The main contribution, however, is to the literature on the nexus of information and enforcement for business taxation (e.g. Almunia and Lopez-Rodriguez 2018; Best et al. 2015; Carrillo et al. 2017; Slemrod et al. 2017; Pomeranz 2015; Wan 2010), which has largely focused on the compliance effects of tax enforcement.

The rest of this paper is organized as follows. Section two presents the theoretical framework. Section three introduces the institutional details, data set and identification strategy. Section four describes the empirical results, while section five concludes.

[9]Informal employment is not included in administrative data and is likely to grow before formal employment.

[10]Monteiro and Assunção (2012) note that labor taxes and fees make up over 50% of the wage costs in Brazil so that incentives to underreport employment there are strong.

2 Theoretical Framework

This section presents a theoretical analysis of policies such as the Portuguese intervention, which is meant to guide the empirical work. I consider a model in which employees can divert some of the unregistered sales from the firm. Tax enforcement is shown to reduce diversion, thereby differing from a simple increase in effective tax rates. The reduced diversion decreases effective wages, thereby putting upward pressure on labor demand, which can mitigate the usual negative employment effects of tax enforcement. If a large amount of sales revenues are being diverted, then the policy can have a positive net effect on labor demand, despite the increasing tax burden. The intuition is that employees effectively impose a 100% tax on sales that are diverted. Tax enforcement removes this "employee tax" and imposes a lower government tax. Labor supply, however, may fall as a result of decreased diversion. Equilibrium employment can nevertheless increase, if labor demand is more sensitive to diversion than labor supply. Furthermore, we can expect an increase in wages, if diversion is reduced.

As part of the enforcement program in Portugal, consumers as well as tax auditors can cross-check whether sales with receipts were actually reported to the government by the firm. It is then reasonable to suppose that the detection probability of hidden sales which have a receipt is very high. Hence, for simplicity, I assume that the government detects this evasion with probability one, whereas the detection probability on evasion for sales without receipts is zero. Such a structure simplifies the analysis without changing the basic results.

Let the fraction of sales with a receipt, i.e. the fraction the firm reports to the tax authorities, be denoted α. Further, a fraction β of sales x that are not subject to receipts is diverted by the employees[11]. The total diverted amount is therefore $\beta(1-\alpha)x$, where the consumer price is normalized to one. The diversion of the employee is detected by the firm with a probability $(1-\gamma)$, which is increasing in the diverted share β, i.e. $\gamma'(\beta) < 0$. If the employee is detected diverting, then an exogenous[12] penalty θ is incurred. Therefore, the expected diversion costs are

[11] Employees do not divert sales with receipts, because this is discovered by the government (and therefore the firm) with certainty.

[12] Having the penalty depend positively on the diverted amount does not affect the basic results.

$\theta(1-\gamma)$. To reflect the difficulty of diverting all sales, these costs are convex, which follows from assuming a convex detection probability, i.e. $\gamma''(\beta) < 0$. Furthermore, the employees are small relative to the market and do not consider their diversion to have an effect on labor demand. The diversion decision solves:

$$\max_{\beta} \gamma\beta(1-\alpha)x - (1-\gamma)\theta \tag{1}$$

The interior solution is given by:

$$\beta^* = -\frac{\gamma}{\gamma'} - \frac{\theta}{(1-\alpha)x} \Rightarrow \frac{d\beta^*}{d\alpha} < 0 \tag{2}$$

Taking into account corner solutions, we have:

$$\beta = \begin{cases} \beta^* & \textit{if}\, \beta^* \in [0,1], \\ 0 & \textit{if}\, \beta^* < 0, \\ 1 & \textit{if}\, \beta^* > 1 \end{cases} \tag{3}$$

The policy intervention increases the fraction of sales with receipts α. As long as there are sales without receipts and diversion is positive, the diverted fraction of total sales $\delta = \beta(1-\alpha)$ is decreasing in α. The enforcement reform therefore reduces diversion by employees. Furthermore, labor supply is given by:

$$L^S = L^S[w, \delta] \tag{4}$$

where w denotes the wage rate and L^S increasing in both terms.

I now turn to the labor demand side. Consider a representative firm in perfect competition with no fixed costs, which produces the consumption good x with the production function $x = F(L)$, where L denotes labor. The production function is increasing and exhibits a decreasing

marginal productivity, i.e. $F'(L) > 0$ and $F''(L) < 0$. The firm pays a value-added tax (VAT) τ on reported sales and corporate income tax (CIT) ψ on reported profits. For simplicity, I assume the firm truthfully reports all costs. Underreporting costs increases the tax burden and is therefore not sensible[13]. Conversely, allowing firms to overreport costs is equivalent to assuming a lower (effective) CIT rate. For example, if firms always overreport costs in order to report zero profits, then one could simply set $\psi = 0$ here. The consumption good x has a fully elastic demand and the consumer price is normalized to one. Furthermore, due to the aforementioned detection probabilities, the firm reports all sales with a receipt and none of the sales without a receipt to the tax authorities. This further implies that the firm never pays any penalties from tax evasion in equilibrium. Let subscripts $j \in \{0, 1\}$ denote the period before and after the reform, respectively. Profits are:

$$\Pi_j = (1-\psi)\Big[\alpha_j \frac{F(L_j)}{1+\tau} - wL_j\Big] + (1-\alpha_j)(1-\beta_j)F(L_j) \tag{5}$$

The labor demand functions for given wages are determined by:

$$F'(L_j) = \frac{(1-\psi)w}{1 - \alpha_j \frac{\tau+\psi}{1+\tau} - \delta_j} \tag{6}$$

Note that labor demand is decreasing in diversion. Furthermore, if the reform only increases tax compliance (i.e. α), then labor demand would certainly fall. Nevertheless, the reform also reduces the diverted fraction β. Hence, we have $\alpha_1 > \alpha_0$ and $\beta_1 < \beta_0$. By comparing equation (6) before and after the reform, we see that labor demand will increase if and only if:

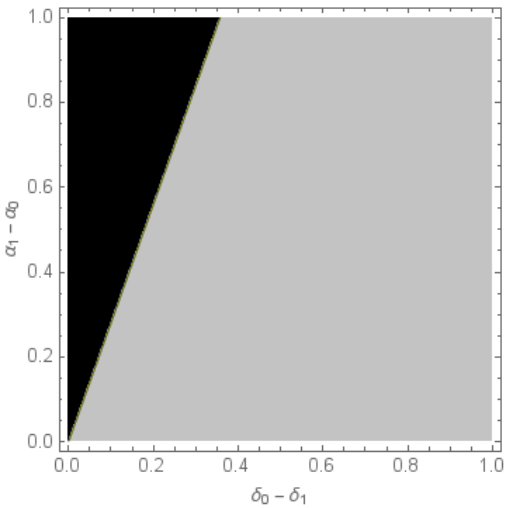
$$\delta_0 - \delta_1 > \frac{\tau+\psi}{1+\tau}[\alpha_1 - \alpha_0] \tag{7}$$

Hence, for a sufficiently large reduction in the diverted amount, the reform can increase labor demand, even if the reform increases the tax burden on firms. As an example, suppose that

[13] In a richer framework, where the reported costs provide information on true sales, the firm may underreport true costs. Nevertheless, such a structure would not affect the basic results.

there is total non-compliance before the reform and that there is full compliance afterward, i.e. $\alpha_0 = 0$ and $\alpha_1 = 1$. Furthermore, suppose that initially all sales without receipts are diverted by employees and that the reform completely eliminates diversion, i.e. $\beta_0 = 1$ and $\beta_1 = 0$. Condition (7) then implies that labor demand increases as a result of the reform, assuming $\tau, \psi < 1$. The intuition is that employees effectively impose a 100% tax on diverted sales. Receipts reduce this "employee tax" and subject registered sales to a tax by the government, at a lower rate than full taxation. Figure 1 plots the possible values[14] for the changes in diversion on the x-axis and for receipts on the y-axis. The gray area corresponds to parameters for which equation (7) implies an increase in labor demand from the tax enforcement policy, while the black area corresponds to a decrease. As can be seen, a labor demand increase is possible for most parameter combinations. Furthermore, if there are no changes in diversion, the policy always implies a decrease in labor demand, if the number of receipts increases.

Figure 1: Parameters for an Increase in Labor Demand



Note: The figure plots values of $(\delta_0 - \delta_1)$ on the x-axis and the values of $(\alpha_1 - \alpha_0)$ on the y-axis. The gray area corresponds to parameters for which equation (7) implies an increase in labor demand from the tax enforcement policy in Portugal.

Having established that the reform can increase labor demand, I now turn to analyzing equilibrium employment. By reducing diversion, the policy also reduces labor supply. The equilibrium effects on employment are therefore unclear. Nevertheless, they will be positive, if labor demand is more sensitive to the reform than labor supply.

[14] I assume that enforcement does not reduce receipts or increase diversion. Furthermore, I take the tax rates of $\tau = 0.23$ and $\psi = 0.21$ for Portugal from PricewaterhouseCoopers (2018).

From equation (6) it follows that the labor demand function can be written as:

$$L^D = L^D[w, \delta] \tag{8}$$

where L^D is decreasing in both terms. Suppose labor demand is increasing as a result of the reform, as discussed above. The qualitative effects of the reform on employment are then the same as the effects of a reduction in δ. Equilibrium employment is determined by setting the inverse labor demand equal to the inverse labor supply:

$$L^{D^{-1}}[L, \delta] = L^{S^{-1}}[L, \delta] \tag{9}$$

Therefore, employment increases when diversion is reduced if:

$$\frac{dL}{d\delta} = -\frac{\frac{\partial L^{D^{-1}}}{\partial \delta} - \frac{\partial L^{S^{-1}}}{\partial \delta}}{\frac{\partial L^{D^{-1}}}{\partial L} - \frac{\partial L^{S^{-1}}}{\partial L}} < 0 \tag{10}$$

The denominator of equation (10) is negative, whereas the sign of the numerator is a difference of two negative numbers and therefore unclear. Employment will increase if:

$$\left| \frac{\partial L^{D^{-1}}}{\partial \delta} \right| > \left| \frac{L^{S^{-1}}}{\partial \delta} \right| \tag{11}$$

Hence, it is possible that the reform can increase equilibrium employment, if labor demand reacts more strongly to diversion than labor supply, which would be ensured by a low elasticity of labor supply with respect to earnings.

An interesting example of such a case could be the presence of minimum wages, which may be relevant for some of the sectors targeted by the Portuguese intervention. In that case, it is even likelier that the reform increases employment. If a minimum wage causes labor supply to exceed labor demand at all diversion levels, then the impact of the reform on employment is entirely determined by the effect on labor demand. The reform then always increases employment

if it increases labor demand, i.e. if equation (7) holds.

In the absence of binding minimum wages, the model also implies that the intervention puts upward pressure on wages, if it reduces diversion. This is because a decrease in diversion increases labor demand and decreases labor supply. To see this, take the total differential of the equilibrium condition $L^S = L^D$ which yields:

$$\frac{dw}{d\delta} = \frac{\frac{\partial L^D}{\partial \delta} - \frac{\partial L^S}{\partial \delta}}{\frac{\partial L^S}{\partial w} - \frac{\partial L^D}{\partial w}} < 0 \tag{12}$$

To summarize, the model implies that it is possible for the reform to increase employment by reducing diversion. In that case, we can also expect an increase in wages, if minimum wages are not binding for all employees.

3 Institutional Background and Data

In January 2013, a tax enforcement reform[15] came into effect in Portugal which incentivized consumers to request receipts that were associated with their taxpayer identification number. In conjunction with this, the e-invoice system was introduced, which required all businesses to issue receipts and report them monthly to the tax authority. Consumers could deduct 15% of the VAT paid on these transactions from their personal income tax, with a maximum of €250 per household. In doing so, they could enter invoice elements that were not reported by businesses. The reform targeted the following four sectors, which were chosen due to perceived issues with tax compliance, but not due to any employment concerns:

- Maintenance and repair of motor vehicles
- Maintenance and repair of motorcycles and related parts and accessories
- Accommodation and food services[16]

[15] The relevant laws are Decree-law 197/2012 and 198/2012, which were passed in August of 2012.

[16] I use the Portuguese classification of economic activities revision 3. According to this classification, accommodation and food services include 33 different industries, such as hotels, restaurants, bars and cafés.

- Hairdressing and beauty parlors

This intervention acts as a natural experiment providing a plausibly exogenous increase in tax enforcement. I exploit this variation using a difference-in-difference estimator as well as the synthetic control method, with these sectors serving as the treated units. The rest of the Portuguese economy is then used to construct the control group.

In a second step, a VAT lottery was introduced in 2014 for all sectors in Portugal. Receipts then doubled as lottery tickets, with the possibility to win a car, thereby providing incentives to request receipts for all sectors. The lottery will only affect the estimates of the 2013 reform if the treatment intensity of this second enforcement intervention differs across the aforementioned treatment and control groups. If the lottery leads to more enforcement among the four treated groups compared to others, there will be two layers of increased tax enforcement. The estimates would then capture the effects of the broader 2013 and 2014 tax enforcement reforms. This is the likeliest case, as the targeted sectors in 2013 were chosen due to low tax compliance. Furthermore, the aforementioned treatment group consists of retailers, while the control group also includes wholesale firms. The latter are less affected by enforcement via consumer receipts, which is the variation used in Naritomi (2015). Lastly, there is a positive treatment effect on sales, which is a proxy for compliance, as discussed in Appendix D. This is consistent with stronger overall tax enforcement effects for my treatment group as opposed to the control group. If, however, the lottery leads to less enforcement in the treatment group compared to the control, then my estimates would underestimate of the effects of the 2013 enforcement reform.

I analyze the effects of increased enforcement on employment using administrative firm level data from 2010-2015[17]. The data come from the Central Balance Sheet Database, provided by the Portuguese central bank, *Banco de Portugal*. This database contains anonymized annual data on the population of all non-financial firms in Portugal. All Portuguese firms are obliged to file the balance sheet, which is done through the electronic database *Informação Empresarial Simplificada* (*IES*). The data set is mostly based on information reported through

[17]The reporting standards for the data in Portugal changed starting January 1st 2010, which is why I begin with this year. The most recent available data are from 2015.

the *IES*, but also contains further information calculated by the central bank. Due to the filing requirements for all firms in Portugal, the data set provides a good coverage of smaller firms as well, which are often missing in other data sets. The sample contains data on 796[18] sectors of economic activity, with 37 of them serving as the treated sectors.

In this context, social capital and trust may also play a role. If the negative real effects of tax enforcement on employment can be mitigated through increased monitoring of employees, then we might expect the effects to vary by the level of social trust. Firms in regions where there is more social capital and trust and may have trustworthier employees and therefore benefit less from the increased monitoring induced by tax enforcement. In order to test this mechanism, I use voter turnout in the 2009[19] European Parliamentary Election at the district level as a proxy for social capital and trust. Voter turnout has been used as a proxy in a large literature, starting with Guiso et al. (2004), who look at referenda. They note that any such measure must involve a social activity for which there are no (major) legal or economic incentives. In this sense, voters provide a public good without strong personal gain, thereby behaving according to social norms. I use the abstention rate (defined as one minus the turnout rate) in the European Parliamentary Election, as such an election has much lower stakes than others and therefore serves as a good proxy[20]. The data set contains 22 districts, which is the smallest geographical level available and is provided by the National Elections Commission of Portugal.

Table 1 shows the summary statistics for the main sample. Here, all sectors affected by the 2013 intervention comprise the treatment group, with all other sectors in the control group. Alternative samples are explored in Appendix C.

The total employees variable refers to the number of paid and unpaid[21] employees. I use the number of paid employees as the main dependent variable, because a firm is possibly unconstrained in hiring unpaid employees and the most policy relevant effect is on the number

[18]The data are missing for a few of these sectors in certain years.

[19]District level data was not available for the 2014 election. Nevertheless, trust changes slowly and the 2009 data are likely to capture the relevant societal trust for the years 2010-2015. Furthermore, 2014 falls within the treatment period and this turnout could be affected by the treatment, thereby making it a bad control.

[20]While many proxies exist in the literature, it was impossible to find appropriate data for others. Surveys measuring trust, for example, typically do not have information at the district level, but rather at the country level.

[21]Unpaid employees here are typically partners, family workers and unpaid managers.

of paid employees. Nevertheless, the results are robust to using the total number of employees as well. The yearly wages per employee (the wage rate) are reported in Euro, while the yearly hours worked by paid employees represents the sum of all hours worked by such employees at the firm. The abstention rate captures the proportion of eligible voters that did not vote in the European Parliamentary Election of 2009. Furthermore, the exit rate variable shows the number of firm exits in year t as a proportion of firms in year $t-1$. This is meant to capture the extensive margin of the firm's employment decision, since firms may drop out of the market and not hire anyone, if tax burdens increase excessively.

The treatment and control groups in the baseline sample are in general quite similar with respect to exits and abstention rates. While there are differences in the other variables in absolute terms, the data does support the parallel trends assumption, as discussed in the next section.

Table 1: Summary Statistics

Variable	Obs	Mean	Std. Dev.
Treatment Group			
Nr. of paid employees	273 829	5.2	34.66
Total employees	274 943	5.2	34.59
Yearly wages per employee	186 625	5 326	3 600
Yearly hours worked by paid employees	266 596	9 126	57 137
Abstention Rate in the EU Parliamentary Election	275 911	62.04%	3.33
Exit Rate	275 911	.085	.187
Control Group			
Nr. of paid employees	1 979 329	7.2	91.998
Total employees	1 988 772	7.3	91.87
Yearly wages per employee	1 151 566	7 825	8 287.77
Yearly hours worked by paid employees	1 900 422	13 048	161 190
Abstention Rate in the EU Parliamentary Election	1 996 453	61.86%	3.15
Exit Rate	1 996 453	.079	.199

4 Empirical Analysis

I analyze the effects of the tax enforcement intervention in Portugal, as well as the mechanisms driving the results, primarily using a difference-in-difference (DD) estimator as well as the synthetic control method. The intervention began on January 1st 2013, which implies three pre-treatment and three treatment periods in the sample. An advantage here is that only certain sectors of the economy were targeted by the 2013 reform, thereby allowing for a reasonable comparison between treated and non-treated firms. In the baseline regressions, firms affected

by the 2013 reform comprise the treatment group, while the rest of the economy serves as the control group. The choice of control group is then subsequently refined. I begin with the analysis of the intensive margin responses at the firm level and then examine firm exit rates and other effects at an aggregated level. The results show an increase in employment and wages, with small and insignificant effects on exit rates. Furthermore, firms in districts with lower trust exhibit stronger effects on employment and wages, which is in line with the employee diversion hypothesis.

4.1 Firm Level Regressions

4.1.1 Baseline Regressions

This section analyzes the effects at the firm level, with firms from all untreated sectors in the control group as a baseline, before the control group is refined in the next subsection using propensity score matching. Figure 2a shows the evolution of the average log employment across firms in the treatment and control groups. Before treatment, the two groups are very similar, but strongly diverge afterward. Furthermore, the raw data seem to suggest a parallel trend in the absence of treatment. Although a small divergence appears in the last pre-treatment period, this difference is not statistically significant, as shown by a more formal test of the parallel trends assumption in Figure 2b. This figure plots the difference-in-difference coefficients[22] and confidence intervals using leads and lags of treatment, with the last pre-treatment period, 2012, as the baseline. As can be seen, the coefficients before the intervention are statistically insignificant and relatively constant. After the treatment, however, the differences start to grow significantly. The leads and lags for this regression and others can also be found in the tables.

Figures 2c and 2d plot the coefficients for the leads and lags using the number of hours worked and the wage rate as dependent variables, respectively. In line with the parallel trends assumption, the point estimates before the intervention are close to zero and statistically in-

[22]The regressions for Figures 2b - 2d are given by equation (14). The only control variable is a dummy indicating whether a firm existed before the intervention.

significant, with positive and significant coefficients after the intervention.

Figure 2: Pre-Trends for Baseline Regressions

(a) Log Employment Over Time

(b) Leads and Lags for Log Employment

(c) Leads and Lags for Log Hours Worked

(d) Leads and Lags for the Log Wage Rate

Note: Figure (a) plots the evolution of the average log number of paid employees across firms in the treatment (blue line) and control groups (red line) over time. The control group here consists of firms from all sectors not targeted by the intervention. Figures (b), (c) and (d) plot coefficients on leads and lags for the log of paid employees, hours worked by these employees and wages per employee respectively, as described in equation (14). The only control variable is a dummy indicating whether a firm existed before the intervention. Standard errors are clustered at the sector level. There are 793 clusters for panels (a) and (b) and 792 for panel (d). Source: Own calculations based on the data described in section 3.

The main specification estimates an equation of the form:

$$lnE_{ist} = \gamma_s + \lambda_t + \beta D_{st} + \delta X_{ist} + \epsilon_{ist} \tag{13}$$

where E_{ist} denotes employment of firm i in sector s at time t, γ_s denotes sector fixed effects, λ_t denotes year fixed effects and β is the DD estimator. D_{st} is defined as $D_{st} = Treat_s * Post_t$, where $Treat_s$ is a dummy equal to 1 if a sector is affected by the 2013 intervention and $Post_t$ is a dummy for observations in the treatment period (2013-2015). X_{ist} is a vector of control

variables. As controls I choose various fixed effects and for all regressions in this subsection I include a dummy indicating whether a firm existed before the intervention. The standard errors are clustered at the sector level, which is the level at which the intervention takes place.

The results from estimating equation (13) are shown in Table 2. Column (1) shows the average effect of the treatment on employment over the whole treatment period, where DD refers to the difference-in-difference coefficient. I find an increase of employment due to the intervention of about 3.6%. Column (2) shows a similar result when including district fixed effects as well.

To test the parallel trends assumption and the evolution of the treatment effects over time, I also employ a regression with leads and lags similar to Autor (2003) of the form:

$$lnE_{ist} = \gamma_s + \lambda_t + \sum_{j=-m}^{q} \beta_j D_{st+j} + \delta X_{ist} + \epsilon_{ist} \tag{14}$$

where q represents the number of leads and m the number of lags. I exclude the last period before the treatment, i.e. the year 2012, which then serves as a baseline and otherwise include all possible leads and lags. Columns (3) and (4) of Table 2 show the effects for such a regression. Here, the variables Treatment_{2010} and Treatment_{2011} refer to the leads, while the $\text{Treatment}_{2013}-\text{Treatment}_{2015}$ variables refer to the lags. Reassuringly, the leads are all insignificant, which provides support for the parallel trends assumption. Furthermore, the treatment effect increases strongly over time, with the strongest effect two periods after the intervention began. Column (4) shows the results from a similar regression as column (3), but includes district fixed effects and displays similar results.

As discussed in section 2, without diversion, tax enforcement is expected to reduce labor demand, due to the increased tax burden, thereby depressing wages. Conversely, in the presence of employee diversion, the intervention is expected to increase wages, if it leads to increased labor demand. I test this hypothesis by using the logarithm of wages per employee as the outcome variable and running regressions of the form described by equations (13) and (14). The results are shown in Table 3. Columns (3) and (4) show the results for the leads and

Table 2: Firm Level Employment Effects

	(1) Log Employment	(2) Log Employment	(3) Log Employment	(4) Log Employment
DD (Post*Treatment)	0.0358***	0.0359***		
	(0.0109)	(0.0109)		
Treatment_{2010}			-0.0070	-0.0072
			(0.0092)	(0.0093)
Treatment_{2011}			-0.0031	-0.0039
			(0.0065)	(0.0065)
Treatment_{2013}			0.0169***	0.0169***
			(0.0054)	(0.0053)
Treatment_{2014}			0.0294***	0.0294***
			(0.0091)	(0.0089)
Treatment_{2015}			0.0509***	0.0509***
			(0.0166)	(0.0164)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
District FE	NO	YES	NO	YES
Observations	1650745	1650745	1650745	1650745
Adjusted R^2	0.018	0.021	0.018	0.021

Note: Standard errors in parentheses, clustered by sector (793 clusters). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using firm level data and firms from all untreated sectors as the control group. The dependent variable is the log number of paid employees by the firm. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The Treatment_t variables are indicators for treatment in year t. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

lags. The leads are insignificant and the effects seem to be increasing over time for the first three years of the intervention. Columns (1) and (2) show the results without and with district fixed effects, respectively. As expected, I find a positive effect of the tax enforcement reform on wages, with an increase of about 2%. Therefore, we can rule out that the increased employment could have been a result of a coincidental increase in labor supply for the treated sectors, which would have reduced wages.

This evidence suggests that enforcement through increased receipts can mitigate the negative effects of the increased tax burden and even increase employment in certain cases. To further test whether the diversion channel is indeed the underlying mechanism, I now turn to analyzing how the results vary with social trust across districts.

Firms in areas with high levels of trust may have trustworthier[23] employees. If monitoring employees is the relevant channel through which enforcement can increase employment, then

[23]Trust might foster collusion between employees, which could increase diversion. Nevertheless, collusion is unlikely to vary strongly by district in this context, as incentives to collude are already very high.

Table 3: Firm Level Wage Effects

	(1) Log Wages	(2) Log Wages	(3) Log Wages	(4) Log Wages
DD (Post*Treatment)	0.0218***	0.0212***		
	(0.006299)	(0.00625)		
Treatment_{2010}			-0.0079	-0.0069
			(0.008096)	(0.008037)
Treatment_{2011}			0.0057	0.0068
			(0.006834)	(0.006689)
Treatment_{2013}			0.0064	0.0064
			(0.006741)	(0.006705)
Treatment_{2014}			0.0189**	0.0191**
			(0.008194)	(0.007943)
Treatment_{2015}			0.0376***	0.0377***
			(0.01187)	(0.01159)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
District FE	NO	YES	NO	YES
Observations	1295413	1295413	1295413	1295413
Adjusted R^2	0.019	0.027	0.019	0.027

Note: Standard errors in parentheses, clustered by sector (792 clusters). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using firm level data and firms from all untreated sectors as the control group. The dependent variable is the log of the average wage per employee. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The Treatment_t variables are indicators for treatment in year t. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

we might expect larger effects for firms in districts with lower levels of trust. I look at voter turnout to measure trust, which has become a standard proxy in the literature[24]. For employment, I test this mechanism in column (1) of Table 4, while in column (2) the same is done for wages. Here, the variable "High abstention rate" is a dummy for districts with an abstention rate in the 2009 European Parliamentary Election that was above the median. The variable "Low abstention rate" is a dummy for districts with below median abstention. Both variables are interacted with the treatment assignment DD[25]. As expected, firms in areas with low social trust, i.e. with a high abstention rate, experience an increase in employment and wages by much more than those in areas with high trust. Firms in high trust districts increase their employment by about 2.6%, while those in low trust districts increase employment by approximately 4.7%. Wages in low trust districts increase by about 5.9%, with a small and negative effect of -1.3%, which is only significant at the 10% level for high trust districts. A concern could be that areas

[24]See e.g. Guiso et al. (2004) or Nannicini et al. (2013).

[25]Due to the inclusion of district fixed effects, the abstention rate itself does not feature as a control variable.

with low social trust may exhibit higher initial rates of tax evasion. Nevertheless, this would imply the opposite employment reaction, as firms in such districts would experience the larger tax burden increases as a result of the increased tax enforcement.

Table 4: Firm Level Abstention Effects

	(1) Log Employment	(2) Log Wages
DD * High abstention rate	0.0466***	0.0590***
	(0.0148)	(0.01178)
DD * Low abstention rate	0.0263**	-0.0134*
	(0.01325)	(0.007171)
Sector FE	YES	YES
Year FE	YES	YES
District FE	YES	YES
Observations	1650697	1295365
Adjusted R^2	0.021	0.027

Note: Standard errors in parentheses, clustered by sector. There are 793 clusters for column (1) and 792 for column (2). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays regressions using firm level data and the full sample. The dependent variables for columns (1) and (2) are the log number of paid employees and the log of the average wage per employee, respectively. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015. The variables "high abstention rate" and "low abstention rate" are dummies for districts with an abstention rate in the 2009 European Parliamentary Election that was above or below the median, respectively.

4.1.2 Matching

To refine the control group, I consider the difference-in-difference estimator using propensity score matching[26] on firms, as developed by Heckman et al. (1997, 1998). Furthermore, I exclude from the control group the six sectors[27] with employment levels larger than any sector in the treatment group for each year. Nevertheless, similar results are also obtained from propensity score matching on the full sample, which is not reported for parsimony. Excluding the six largest sectors should, however, provide a more comparable control group. Results using this sample without matching are discussed in Appendix C.

[26]The analysis is partially based on code provided by Villa (2016).

[27]These sectors are the construction of residential and non-residential buildings, hospital activities, retail sale in supermarkets and hypermarkets, temporary employment agency activities, freight transport by road and the manufacture of other ready-to-wear outerwear.

I run the same specifications as in the baseline regressions and match firms on the pre-treatment number of hours worked and earnings before interest and taxes (EBIT), using the Epanechnikov kernel. These variables are likely to identify appropriate firms with respect to the counterfactual evolution of the main outcome variables in the absence of treatment. A type of matching at the sector level is also done in section 4.2.2. Tables 5 and 6 show the results for employment and wages, respectively. Reassuringly, all the leads are insignificant, as reported in columns (3) and (4). There is still a positive and statistically significant coefficient for employment, which is now somewhat smaller. The results indicate a 2.3% increase in employment, with larger effects in low trust districts (a 2.96% increase) compared to high trust districts (a 1.63% increase). The results for wages are similar to the baseline, with a treatment effect of around 2%. Again, we see stronger effects for low trust districts, which experienced wage increases of approximately 4.5%, with smal,l insignificant effects for high trust districts.

Table 5: Firm Level Employment Effects with Matching

	(1)	(2)	(3)	(4)	(5)
	Log Employment	Log Employment	Log Employment	Log Employment	Log Employment
DD (Post*Treatment)	0.0226**	0.0225**			
	(0.00910)	(0.00893)			
Treatment_{2010}			-0.00172	-0.00195	
			(0.00689)	(0.00683)	
Treatment_{2011}			0.000466	0.000535	
			(0.00494)	(0.00490)	
Treatment_{2013}			0.00582	0.00565	
			(0.00500)	(0.00493)	
Treatment_{2014}			0.0183**	0.0183**	
			(0.00813)	(0.00792)	
Treatment_{2015}			0.0421***	0.0421***	
			(0.0150)	(0.0147)	
DD * High abstention rate					0.0296**
					(0.0117)
DD * Low abstention rate					0.0163*
					(0.00991)
Sector FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
District FE	NO	YES	NO	YES	YES
Observations	1331557	1331557	1331557	1331557	1331512
Adjusted R^2	0.182	0.184	0.182	0.184	0.184

Note: Standard errors in parentheses, clustered by sector (782 clusters). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions with propensity score matching on pre-treatment hours worked and EBIT using firm level data and all firms from untreated sectors except the six largest sectors (in terms of employment) as the control group. The dependent variable is the log number of paid employees by the firm. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The Treatment_t variables are indicators for treatment in year t. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015. The variables "high abstention rate" and "low abstention rate" are dummies for districts with an abstention rate in the 2009 European Parliamentary Election that was above or below the median, respectively. In the last column both variables are interacted with the treatment assignment DD.

Table 6: Firm Level Wage Effects with Matching

	(1) Log Wages	(2) Log Wages	(3) Log Wages	(4) Log Wages	(5) Log Wages
DD (Post*Treatment)	0.0200***	0.0194***			
	(0.00569)	(0.00573)			
Treatment_{2010}			-0.00578	-0.0053	
			(0.00776)	(0.00766)	
Treatment_{2011}			0.00725	0.0080	
			(0.00677)	(0.0066)	
Treatment_{2013}			0.00327	0.0031	
			(0.00674)	(0.0068)	
Treatment_{2014}			0.0183***	0.018***	
			(0.00690)	(0.0067)	
Treatment_{2015}			0.0397***	0.0395***	
			(0.00929)	(0.009)	
DD * High abstention rate					0.045***
					(0.00954)
DD * Low abstention rate					-0.0037
					(0.0063)
Sector FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
District FE	NO	YES	NO	YES	YES
Observations	1161715	1161715	1161715	1161715	1161667
Adjusted R^2	0.127	0.133	0.127	0.133	0.133

Note: Standard errors in parentheses, clustered by sector (775 clusters). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions with propensity score matching on pre-treatment hours worked and EBIT using firm level data and all firms from untreated sectors except the six largest sectors (in terms of employment) as the control group. The dependent variable is the log of the average wage per employee. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The Treatment_t variables are indicators for treatment in year t. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015. The variables "high abstention rate" and "low abstention rate" are dummies for districts with an abstention rate in the 2009 European Parliamentary Election that was above or below the median, respectively. In the last column both variables are interacted with the treatment assignment DD.

4.2 Sector Level Regressions

The previous section focuses on regressions at the firm level. This analysis is important in order to understand how the employment decision of the firm is affected by the intervention. While these results capture intensive margin responses, they cannot, however, account for the extensive margin. Therefore, if there are exits (or entries) as a result of the intervention, the firm level regressions may not be indicative of what happens to employment overall. Nevertheless, I do not find any effect on exits. I begin with the baseline regressions, which include all untreated sectors in the control group, before refining the latter using synthetic control.

4.2.1 Baseline Regressions

This section uses a sample with the data aggregated across firms. I will continue to examine the heterogeneous effects by the abstention rate, which varies at the district level. In creating this aggregate sample, I therefore sum the number of employees and exits as well as wages paid across firms by sector, district and - in order to prevent a large loss in variation - by the registered legal form. The next subsection presents synthetic control results for observations simply summed across firms at the sector level, without any variation by district or legal form.

Table 7 shows the effects of the intervention on employment at the aggregated level. Column (1) presents results from a basic difference-in-difference regression with no controls. The effects indicate an average increase in employment across the whole post-treatment period of approximately 9%. Columns (2) and (3) show results from a similar regression as that of column (1), by adding legal form and district fixed effects respectively. The results are robust and similar to those of column (1). Column (4) tests the parallel trends assumption using leads and lags. The leads are again insignificant, thereby providing support for the assumption. Similarly to before, the lags are positive and increase over time.

The effects on wages are shown in Table 8. Again, as predicted by the theory, we observe a positive treatment effect. Column (1) shows the result with no additional controls, while columns (2) and (3) add legal form and district fixed effects respectively. Although the coefficient in column (1) is not statistically significant, this changes as the efficiency improves with the addition of further fixed effects. Column (4) examines the leads and lags structure. Reassuringly, the leads are all insignificant. Overall, the results indicate an increase in wages of about $2\% - 2.5\%$ as a result of treatment, which is surprising given that wages are often considered to be sticky.

Table 9 shows how the effects on employment and wages vary by the level of social trust. As before, I find that districts with lower trust experience stronger positive effects. The employment increase in low trust districts is around $13\% - 15\%$. Conversely, the increase in districts with high trust is much lower and statistically insignificant. A similar pattern can be

Table 7: Sector Level Employment Effects

	(1) Log Employment	(2) Log Employment	(3) Log Employment	(4) Log Employment
DD (Post*Treatment)	0.0928**	0.0946**	0.0915**	
	(0.0385)	(0.0414)	(0.0421)	
Treatment_{2010}				-0.0527
				(0.0344)
Treatment_{2011}				-0.0009
				(0.0294)
Treatment_{2013}				0.0362
				(0.0288)
Treatment_{2014}				0.0694**
				(0.0313)
Treatment_{2015}				0.121***
				(0.0439)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Legal Form FE	NO	YES	YES	NO
District FE	NO	NO	YES	NO
Observations	102862	102862	102862	102862
Adjusted R^2	0.162	0.376	0.513	0.162

Note: Standard errors in parentheses, clustered by sector (793 clusters). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using data aggregated across firms by sector, district and legal form with all untreated sectors as the control group. The dependent variable is the log number of paid employees. The Treatment_t variables are indicators for treatment in year t. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

observed for wages with increases of $7 - 8\%$ for low trust district and negative or statistically insignificant effects for high trust districts.

The effects of the intervention on exit rates are explored in Table 10. A common concern when increasing tax enforcement is that it may cause certain businesses to shut down completely, thereby resulting in no increased tax revenue from these firms. Column (4) tests the parallel trends assumption, as in the other regressions, and finds insignificant effects of the leads. Column (1) shows results from a difference-in-difference regression without controls, while columns (2) and (3) add legal form and district fixed effects respectively. The effects on exit rates are slightly negative, but very close to zero and insignificant. This is consistent with the evidence in Naritomi (2015), who also finds no increase in exit rates from a similar tax enforcement intervention in Brazil.

Table 8: Sector Level Wage Effects

	(1) Log Wages	(2) Log Wages	(3) Log Wages	(4) Log Wages
DD (Post*Treatment)	0.0216	0.0246*	0.0254**	
	(0.0147)	(0.0127)	(0.0129)	
Treatment_{2010}				-0.0176
				(0.0298)
Treatment_{2011}				0.00736
				(0.0239)
Treatment_{2013}				0.000175
				(0.0205)
Treatment_{2014}				0.0240
				(0.0210)
Treatment_{2015}				0.0310
				(0.0227)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Legal Form FE	NO	YES	YES	NO
District FE	NO	NO	YES	NO
Observations	95553	95553	95553	95553
Adjusted R^2	0.185	0.370	0.388	0.185

Note: Standard errors in parentheses, clustered by sector (792 clusters). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using data aggregated across firms by sector, district and legal form with all untreated sectors as the control group. The dependent variable is the log of the average wage per employee. The Treatment_t variables are indicators for treatment in year t. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

Table 9: Sector Level Abstention Effects

	(1) Log Employment	(2) Log Employment	(3) Log Wages	(4) Log Wages
DD * High abstention rate	0.149**	0.129**	0.0880***	0.0800***
	(0.0650)	(0.0640)	(0.0195)	(0.0205)
DD * Low abstention rate	0.0405	0.0552	-0.0394**	-0.0277
	(0.05463)	(0.05696)	(0.01906)	(0.01819)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Legal Form FE	NO	YES	NO	YES
District FE	YES	YES	YES	YES
Observations	102826	102826	95517	95517
Adjusted R^2	0.239	0.513	0.217	0.388

Note: Standard errors in parentheses, clustered by sector. There are 793 clusters for columns (1) and (2) and 792 for columns (3) and (4). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays regressions using data aggregated across firms by sector, district and legal form with all untreated sectors as the control group. The dependent variable for columns (1) and (2) is the log number of paid employees. The dependent variable for columns (3) and (4) is the log of the average wage per employee. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015. The variables "high abstention rate" and "low abstention rate" are dummies for districts with an abstention rate in the 2009 European Parliamentary Election that was above or below the median, respectively.

Table 10: Exit Rates

	(1) Exit Rate	(2) Exit Rate	(3) Exit Rate	(4) Exit Rate
DD (Post*Treatment)	-0.0004	-0.0003	-0.0003	
	(0.0049)	(0.0049)	(0.0049)	
Treatment_{2010}				-0.0028
				(0.0070)
Treatment_{2011}				-0.0084
				(0.0064)
Treatment_{2013}				-0.0054
				(0.0073)
Treatment_{2014}				-0.0029
				(0.0078)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Legal Form FE	NO	YES	YES	NO
District FE	NO	NO	YES	NO
Observations	99206	99206	99206	99206
Adjusted R^2	0.016	0.030	0.032	0.016

Note: Standard errors in parentheses, clustered by sector (796 clusters). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using data aggregated across firms by sector, district and legal form with all untreated sectors as the control group. The dependent variable is the exit rate, defined as the number of exits in year t as a proportion of firms in year $t-1$. The Treatment_t variables are indicators for treatment in year t. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

4.2.2 Synthetic Control

For sector level data, the synthetic control method is available to evaluate the impact of the policy, as introduced by Abadie and Gardeazabal (2003) and Abadie et al. (2010). For a given treated unit, this method constructs a synthetic control group based on a weighted combination of untreated sectors. This synthetic control group approximates the characteristics of the treated sector and can proxy the counterfactual evolution of the treated unit. An advantage of this approach is that it does not require arbitrary choices for the set of sectors in the control group. Furthermore, it allows for a good approximation of the counterfactual by estimating the optimal weights associated with each untreated sector.

Using this method, I analyze sector level employment for each treated sector[28]. To this end, I aggregate the data across sectors for each year. Unlike in the previous subsection, however, the method used here does not allow for data to be aggregated across several dimensions. The donor pool here consists of all sectors unaffected by the intervention. To construct the synthetic control group, I use past employment for the years 2010 and 2011[29], as well as pre-treatment averages of EBIT, hours worked by paid employees and wages per employee, as predictor variables. In order to improve the pre-treatment fit, the EBIT and wages per employee for the year 2012 are also included as predictors, but similar results are obtained without these two variables as well. While the results are robust across specification choices for predictor variables, the chosen specification seemed to yield the best pre-treatment fit, which is crucial for the validity of the synthetic control results.

Due to the large number of treated sectors, this section only shows the results for important industries, while the remaining ones can be found in Appendix E. Unfortunately, it was not always possible to construct a reasonable synthetic control group for each industry by using the same predictor variables for all of them. This may be because the predictive power of the variables differs across industries. Adjusting the predictors separately for each industry can lead to better synthetic control groups. Nevertheless, for consistency, all results are presented using the same aforementioned predictor variables.

Figure 3 shows the evolution of employment over time compared to the counterfactual for four important industries. The three treated sectors with the largest employment, in order of importance, are traditional restaurants[30] (panel A), hotels with restaurants (panel B) and the maintenance and repair of motor vehicles (panel C). Panel D shows the results for cafés, which represent another important and typical industry for the treatment group. In all four cases, the synthetic control groups mirror the treated sectors in the pre-treatment period very closely. Furthermore, the results indicate a strong positive treatment effect on employment.

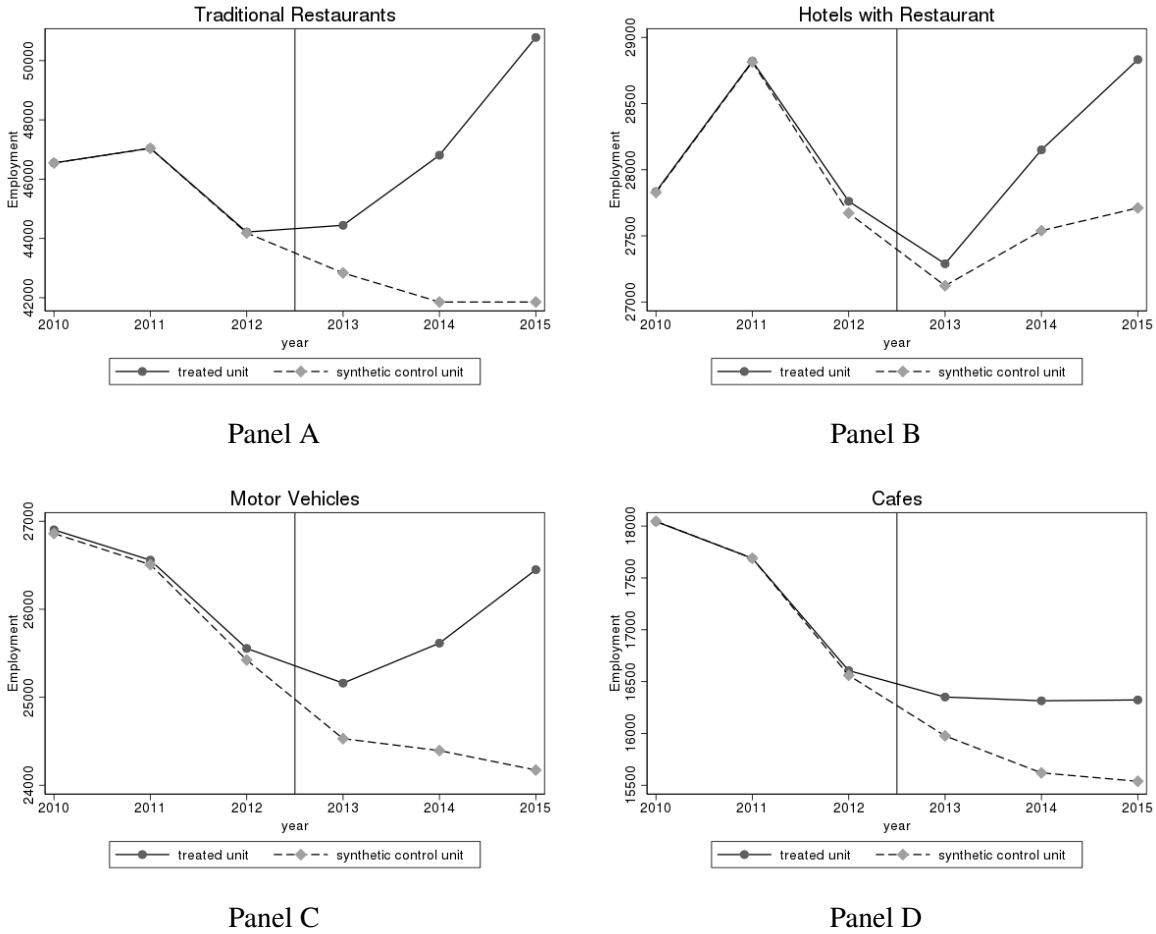
[28]The estimation uses the Stata package synth. For details, see Abadie et al. (2011).

[29]Using employment data for each pre-treatment year would render the other predictors irrelevant and may cause bias, as shown in Kaul et al. (2015).

[30]Notably, traditional restaurants is the sector with 8th biggest employment in the sample.

The values of the predictor variables for these industries as compared to the synthetic control group are shown in Table 11. For the most part, the control group seems to approximate the characteristics of the treated sectors very well, which indicates a reasonable approximation of the counterfactual evolution of employment.

Figure 3: Synthetic Control for Selected Industries



Note: The figure plots the evolution of paid employees (solid line) compared to an estimated synthetic control group (dotted line) for selected industries. Source: Own calculations based on the data described in section 3.

Table 11: Predictor Balance

Predictor Variable	Restaurants	Synthetic	Hotels	Synthetic	Vehicles	Synthetic	Cafés	Synthetic
Employment in 2010	46549	46547	27834	27828	26903	26861	18044	18046
Employment in 2011	47045	47042	28821	28815	26559	26508	17689	17690
EBIT (millions)	-69.7	-59.7	-128	26.4	-10.1	-10.3	-29.7	-3.2
Hours (millions)	78.9	78.8	52.7	52.5	47.1	47	30.5	30.5
Wages	6428	7827	12825	12311	7286	7090	4829	4837
EBIT (2012, millions)	-150	-91.7	-226	0.399	-33.4	-33.8	-54.1	-54.1
Wages (2012)	6343	8004	12899	13089	7223	7027	4775	4782

Note: The table displays the values of the predictor variables for selected industries and their synthetic control group, used for the results displayed in Figure 3.

4.3 Robustness

The main message of the results is that tax enforcement can increase employment. I have argued that the mechanism driving the effects is a decrease in diversion by employees. Another explanation could be that the intervention increased the real sales of goods, due to the possibility for consumers to deduct a portion of the VAT from their personal income taxes. Nevertheless, it seems unlikely that the VAT rebate alone could increase employment. One of the most important targeted sectors in Portugal were food services such as restaurants. Harju and Kosonen (2013) analyze the effects of a VAT cut on restaurants from 22% to 13% in Finland and find no effect on the quantity of sales or the total wages paid to employees. Furthermore, another targeted sector in Portugal were hairdressers. Kosonen (2013) analyzes a VAT cut for this sector from 22% to 8% in Finland and also finds no effect on sales or employment. While demand elasticities may differ between Portugal and Finland, the implied drop in VAT for Portugal was much smaller than the cut in Finnish VAT in these cases.

Appendix A discusses the possibility of the VAT deduction to increase employment in the context of the model without diversion. A back-of-the-envelope calculation shows that this is unlikely, because it would imply low compliance increases from the enforcement policy. Furthermore, the tax deductions were capped at €250, which may not imply a strong increase in real sales. Lastly, if the increases in employment are only due to increased real sales, then the results should not vary by the level of social trust in a district, as discussed.

Another margin through which the intervention can affect employment, besides through the number of employees, is also the amount of hours worked. In Appendix B I take the logarithm of hours worked as the dependent variable and find similar effects. Furthermore, in the results presented so far, I analyze the number of paid employees. This is because a firm is possibly unconstrained in hiring unpaid employees and the most policy relevant effect is on the number of paid employees. One concern, however, could be that employees may simply be registered as unpaid in order to avoid taxes. Nevertheless, Appendix B shows that the results using the log number of total (paid and unpaid) employees as a dependent variable are similar to those

presented so far.

Appendix C considers the effects under alternative control groups, in order to provide more reasonable comparisons for the treatment group. I first consider the case without the six largest sectors in the control group, as done for the previous matching estimates and find similar results. Furthermore, the treated firms are in service sectors that do not actively export, or are not necessarily aware that they are exporting when serving tourists and therefore may not report any exports. Moreover, exports are not affected by the tax enforcement reform, as non-Portuguese residents do not pay any income tax in Portugal and therefore cannot deduct the VAT. Hence, a plausible sample could consist only of non-exporting firms. For this case, I find somewhat larger firm level employment effects, with coefficients of 5.1% and 4.9% with and without propensity score matching, respectively. At the sector level, the results with these alternative samples are also similar to those already discussed.

I have argued that such tax enforcement reforms can simultaneously increase employment and tax compliance, but have so far only focused on employment effects. As discussed, Fooken et al. (2014) and Pavia (2017) already provide evidence that this reform did in fact reduce tax evasion in Portugal. Nevertheless, as a robustness check, Appendix D examines the effects of the intervention on reported sales in my data set. As in Naritomi (2015), an increase in sales proxies an increase in compliance, which cannot be observed directly. Reassuringly, I find positive effects, which would be consistent with a stronger increase in the tax burden for the treatment group than for the control group.

5 Conclusion

This paper analyzes the employment effects of increased tax enforcement in the form of increased incentives for consumers to demand receipts. I find that such a policy had a positive employment effect in Portugal. While the net effect of enforcement may not always be positive in other cases, the results show that increased enforcement can have positive side-effects, which

can mitigate the negative employment effects of an increased tax burden. This would allow policymakers to reduce tax evasion through this form of enforcement at a lower employment cost than previously thought.

I argue that sales receipts help business owners monitor their employees, similarly to how receipts help the government monitor businesses. In this context, receipts can reduce the amount that employees divert from the firm, which reduces effective wages and increases employment. The intuition is that an employee effectively imposes a tax of 100% on each sale that is not reported to the business owners. For each receipt, this "employee tax" is removed, while a lower tax is imposed by the government. Therefore, such an enforcement policy can increase tax compliance, while improving the profitability of the firm as well. The empirical evidence also seems to support this diversion channel.

An avenue for future research could be the potentially positive spillover effects of such enforcement policies on overall compliance. As Kleven et al. (2016) discuss, increased firm size can make collusion on other tax avoidance schemes, such as for payroll taxes, more difficult. Enforcement through receipts may therefore provide an employment push to increase compliance for other taxes as well, while simultaneously raising tax revenues.

References

Abadie, A., Diamond, A., and Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American Statistical Association*, 105(490):493–505.

Abadie, A., Diamond, A., and Hainmueller, J. (2011). Synth: Stata module to implement synthetic control methods for comparative case studies. Statistical Software components, Boston College Department of Economics.

Abadie, A. and Gardeazabal, J. (2003). The economic costs of conflict: A case study of the Basque country. *The American Economic Review*, 93(1):113–132.

Almunia, M. and Lopez-Rodriguez, D. (2018). Under the radar: The effects of monitoring firms on tax compliance. *American Economic Journal: Economic Policy*, 10(1):1–38.

Autor, D. (2003). Outsourcing at will: The contribution of unjust dismissal doctrine to the growth of employment outsourcing. *Journal of Labor Economics*, 21(1):1–42.

Besley, T. and Persson, T. (2014). Why do developing countries tax so little? *Journal of Economic Perspectives*, 28(4):99–120.

Best, M. C., Brockmeyer, A., Kleven, H. J., Spinnewijn, J., and Waseem, M. (2015). Production versus revenue efficiency with limited tax capacity: Theory and evidence from Pakistan. *Journal of Political Economy*, 123(6):1311–1355.

Bird, R. M. (1992). Tax reform in Latin America: A review of some recent experiences. *Latin American Research Review*, 27(1):7–36.

Brockmeyer, A., Hernandez, M., Kettle, S., and Smith, S. (forthcoming). Casting a wider tax net: Experimental evidence from Costa Rica. *American Economic Journal: Economic Policy*.

Bruhn, M. (2011). License to sell: The effect of business registration reform on entrepreneurial activity in Mexico. *Review of Economics and Statistics*, 93(1):382–386.

Carrillo, P., Pomeranz, D., and Singhal, M. (2017). Dodging the taxman: Firm misreporting and limits to tax enforcement. *American Economic Journal: Applied Economics*, 9(2):114–164.

Cowell, F. (2004). Carrots and sticks in enforcement. In Aaron, H. J. and Slemrod, J., editors, *The crisis in tax administration*. Brookings Institution Press.

de Mel, S., McKenzie, D., and Woodruff, C. (2013). The demand for, and consequences of, formalization among informal firms in Sri Lanka. *American Economic Journal: Applied Economics*, 5(2):122–50.

Desai, M. A., Dyck, A., and Zingales, L. (2007). Theft and taxes. *Journal of Financial Economics*, 84(3):591 – 623.

European Commission, Directorate-General for Economic and Financial Affairs (2011). The economic adjustment programme for Portugal.

Fabbri, M. (2015). Shaping tax norms through lotteries. *International Review of Law and Economics*, 44:8 – 15.

Fajnzylber, P., Maloney, W. F., and Montes-Rojas, G. V. (2011). Does formality improve micro-firm performance? Evidence from the Brazilian SIMPLES program. *Journal of Development Economics*, 94(2):262–276.

Fooken, J., Hemmelgarn, T., and Herrmann, B. (2014). Improving VAT compliance - random awards for tax compliance. *European Commission Taxation Papers, Working Paper N. 51*.

Gordon, R. and Li, W. (2009). Tax structures in developing countries: Many puzzles and a possible explanation. *Journal of Public Economics*, 93:855–866.

Guiso, L., Sapienza, P., and Zingales, L. (2004). The role of social capital in financial development. *American Economic Review*, 94(3):526–556.

Harju, J. and Kosonen, T. (2013). Restaurant VAT cut: Cheaper meal and more service? *Government Institute for Economic Research VATT Working Papers No. 52*.

Heckman, J. J., Ichimura, H., and Todd, P. E. (1997). Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. *Review of Economic Studies*, 64:605–654.

Heckman, J. J., Ichimura, H., and Todd, P. E. (1998). Matching as an econometric evaluation estimator. *Review of Economic Studies*, 65:261–294.

Kaplan, D. S., Piedra, E., and Seira, E. (2011). Entry regulation and business start-ups: Evidence from Mexico. *Journal of Public Economics*, 95:1501–1515.

Kaul, A., Klößner, S., Pfeifer, G., and Schieler, M. (2015). Synthetic control methods: Never use all pre-intervention outcomes together with covariates. *MPRA Paper No. 83790*.

Kleven, H. J., Knudsen, M. B., Kreiner, C. T., Pedersen, S., and Saez, E. (2011). Unwilling or unable to cheat? Evidence from a tax audit experiment in Denmark. *Econometrica*, 79(3):651–692.

Kleven, H. J., Kreiner, C. T., and Saez, E. (2016). Why can modern governments tax so much? An agency model of firms as fiscal intermediaries. *Economica*, 83(330):219–246.

Knack, S. and Keefer, P. (1997). Does social capital have an economic payoff? A cross-country investigation. *The Quarterly Journal of Economics*, 112(4):1251–1288.

Kosonen, T. (2013). More haircut after VAT cut? On the efficiency of service sector consumption taxes. *Government Institute for Economic Research VATT Working Papers 49/2013*.

La Porta, R. and Shleifer, A. (2014). Informality and development. *Journal of Economic Perspectives*, 28(3):109–26.

Marchese, C. (2009). Rewarding the consumer for curbing the evasion of commodity taxes? *FinanzArchiv / Public Finance Analysis*, 65(4):383–402.

Monteiro, J. C. and Assunção, J. J. (2012). Coming out of the shadows? Estimating the impact of bureaucracy simplification and tax cut on formality in Brazilian microenterprises. *Journal of Development Economics*, 99(1):105–115.

Nannicini, T., Stella, A., Tabellini, G., and Troiano, U. (2013). Social capital and political accountability. *American Economic Journal: Economic Policy*, 5(2):222–50.

Naritomi, J. (2015). Consumers as tax auditors. *Working Paper, Harvard University*.

Pavia, R. (2017). Tax evasion by domestic and foreign-owned Portuguese firms: A bunching analysis. *CORE Discussion Paper 2017/20*.

Pomeranz, D. (2015). No taxation without information: Deterrence and self-enforcement in the value added tax. *American Economic Review*, 105(8):2539–69.

PORDATA (2018). Income and household expenditure. `https://www.pordata.pt/en/Portugal/Average+household+income-2098`. Last Accessed: 2018-10-11.

PricewaterhouseCoopers (2018). PricewaterhouseCoopers tax guide. https://www.pwc.pt/en/pwcinforfisco/tax-guide/2018.html. Last Accessed: 2018-10-11.

Schneider, F. and Enste, D. H. (2002). Shadow economies: Size, causes, and consequences. *Journal of Economic Literature*, 38(1):77–124.

Slemrod, J., Collins, B., Hoopes, J. L., Reck, D., and Sebastiani, M. (2017). Does credit-card information reporting improve small-business tax compliance? *Journal of Public Economics*, 149:1–19.

Slemrod, J. B. (2016). Tax compliance and enforcement: New research and its policy implications. *Ross School of Business Paper No. 1302*.

Villa, J. M. (2016). diff: Simplifying the estimation of difference-in-differences treatment effects. *The Stata Journal*, 16(1):52–71.

Wan, J. (2010). The incentive to declare taxes and tax revenue: The lottery receipt experiment in China. *Review of Development Economics*, 14(3):611–624.

Yesegat, W., Vorontsov, D. E., Coolidge, J., and Corthay, L. O. (2015). Tax compliance cost burden and tax perceptions survey in Ethiopia (English). *Washington, D.C.: World Bank Group*.

Appendix A: The Model without Diversion

This section extends the model by considering the VAT deduction included in the intervention and analyzes the counterfactual employment effects in the absence of employee diversion. A competing hypothesis could be that the VAT deduction increases real sales and therefore employment, instead of reduced diversion. A rough calculation shows that this is unlikely.

The VAT deduction acts as a consumer subsidy, the incidence of which depends on the elasticities of demand and supply for the consumption good. As reliable data for these elasticities

in the treated sectors in Portugal were not available, I maintain the assumption of a perfectly elastic demand. Therefore, the full incidence of the subsidy is on the producers. In this case, the subsidy has the largest effect on employment, but the firm also carries the full tax burden of increased enforcement.

Let s denote the implied subsidy. Since the producers always offer the same price, whether a receipt is requested or not, the subsidy increases the effective producer price for all sales. Profits before and after the reform are:

$$\Pi_0 = (1-\psi)\Big[\alpha_0 \frac{F(L_0)}{1+\tau} - wL_0\Big] + (1-\alpha_0)F(L_0) \tag{15}$$

$$\Pi_1 = (1-\psi)\Big[\alpha_1 \frac{F(L_1)(1+s)}{1+\tau} - wL_1\Big] + (1-\alpha_1)(1+s)F(L_1) \tag{16}$$

Since labor supply is independent of tax enforcement in the absence of diversion, equilibrium employment is determined by labor demand. The inverse labor demand functions are:

$$F'(L_0) = \frac{(1-\psi)w}{(1-\psi)\alpha_0 \frac{1}{1+\tau} + 1 - \alpha_0} \tag{17}$$

$$F'(L_1) = \frac{(1-\psi)w}{(1+s)[(1-\psi)\alpha_1 \frac{1}{1+\tau} + 1 - \alpha_1]} \tag{18}$$

The VAT deduction can increase employment if and only if:

$$F'(L_1) < F'(L_0) \iff 1+s > \frac{1-\alpha_0 \frac{\tau+\psi}{1+\tau}}{1-\alpha_1 \frac{\tau+\psi}{1+\tau}} \approx 1 + (\alpha_1 - \alpha_0)\frac{\tau+\psi}{1+\tau} \tag{19}$$

The compliance change is crucial here. If there is no compliance increase, i.e. $\alpha_0 = \alpha_1$, then the enforcement policy simply introduces a subsidy without increasing (effective) taxes, which always leads to more employment. To evaluate whether equation (19) is likely to hold, I now turn to determining the expression for s. Recall that the consumer price before the subsidy

is normalized to one, which means the subsidy implies a consumer price of $\frac{1}{1+s}$. The policy allows consumers to deduct a percentage ϕ of the VAT paid from their personal income tax. I abstract here from any caps on the deductible amount, which makes the deduction likelier to increase employment. Furthermore, I assume a constant marginal income tax rate T for all consumers. The subsidy can then be written as:

$$\frac{1}{1+s} = 1 - \frac{\tau}{1+\tau}\alpha_1 \phi T \tag{20}$$

Inserting this expression into equation (19) yields the following condition:

$$A \equiv \Big(\frac{\alpha_1 - \alpha_0}{\alpha_0}\Big)\frac{\alpha_0}{\alpha_1} < \frac{(1+\tau)\tau\phi T}{(\tau+\psi)(1+\tau-\tau\phi T\alpha_1)} \tag{21}$$

It is possible to use equation (21) to check the plausibility of a real sales increase driving the increase in employment. To this end, I use tax rates from the annual tax report for Portugal by PricewaterhouseCoopers (2018). The standard CIT on continental Portugal is $\psi = 0.21$, the standard VAT rate is $\tau = 0.23$ and the deductible fraction is $\phi = 0.15$. Average annual household income in Portugal, according to PORDATA (2018) is €30 534, which corresponds to a marginal personal income tax rate of $T = 0.37$. The left hand side of equation (21) is a function of the percentage increase in compliance. In their discussion of the Portuguese reform, Fooken et al. (2014) report a 2.5%-9.5% increase in reported growth for the treated sectors and an increase of the number of companies issuing receipts of almost 40%. Following them, I set $\frac{\alpha_1}{\alpha_0} \in [1.025, 1.095]$. A caveat here, however, is that these compliance figures could potentially include increased real sales. On the other hand, compliance may have increased even further over time since the publication of their report. The right hand side of condition (21) is largest for $\alpha_1 = 1$. Since I do not have reliable data on α_1, I set it to unity, as this makes the condition likeliest to hold. Using these numbers, the condition for the VAT deduction to increase employment in the absence of diversion is:

$$A < 2.93\%,\ A \in [2.44\%, 8.68\%] \tag{22}$$

Therefore, even when imposing generous assumptions that raise the likelihood for the deduction to increase employment, it seems unlikely that the results can be (completely) explained by this effect.

The analysis here assumes that the price is not contingent on whether a consumer asks for a receipt. It may be that firms offer discounts to consumers who do not request receipts to match the incentive offered by the government. Nevertheless, this should not influence employment any differently than already discussed, as such discounts also impose a kind of tax on the firm and imply a transfer to consumers similarly to the subsidy.

The model has also not considered the lottery aspect of the intervention, which was introduced in later years. Nevertheless, the lottery applied to the whole economy, which is unlikely to cause stronger real sales increases in some sectors than in others. Furthermore, the lottery payments may not produce income effects, because they must be financed through taxes and because the expected payout is small. Therefore, it is unlikely that the lottery increased real sales in the treatment group compared to the control group, which would invalidate the diversion hypothesis.

Appendix B: Alternative Dependent Variables

As a robustness check, this section analyzes the treatment effects on two alternative dependent variables, namely the log number of hours worked as well as the log of the total number of (paid and unpaid) employees.

The results using hours worked are shown in Table 12. Column (1) shows a baseline regression, whereas column (2) controls for district fixed effects. I find an increase of hours worked due to the intervention of about 3.3%, which is similar to the effect on the number of paid employees reported in section 4.1. Columns (3) and (4) examine the leads and lags. The leads are all insignificant, which provides some evidence in favor of the parallel trends assumption. Similarly to before, I find that the positive treatment effects increase over time. Column (5)

shows the effects by the level of social trust. Firms in districts with low trust, i.e. a high abstention rate, increase hours worked by about 4.8% and experience larger effects than firms in high trust districts. In this case the effect on firms in high trust districts is positive and small, but statistically insignificant.

Table 12: Firm Level Hours Worked Effects

	(1) Log Hours	(2) Log Hours	(3) Log Hours	(4) Log Hours	(5) Log Hours
DD (Post*Treatment)	0.0331***	0.0329***			
	(0.0115)	(0.0115)			
Treatment_{2010}			-0.0060	-0.0057	
			(0.0128)	(0.0129)	
Treatment_{2011}			0.0022	0.0027	
			(0.0096)	(0.0095)	
Treatment_{2013}			0.0147**	0.0147**	
			(0.0066)	(0.0066)	
Treatment_{2014}			0.0308***	0.0309***	
			(0.0118)	(0.0116)	
Treatment_{2015}			0.0498***	0.0498***	
			(0.0193)	(0.0192)	
DD * High abstention rate					0.0479***
					(0.01590)
DD * Low abstention rate					0.0194
					(0.01496)
Sector FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
District FE	NO	YES	NO	YES	YES
Observations	1650744	1650744	1650744	1650744	1650696
Adjusted R^2	0.031	0.021	0.018	0.021	0.034

Note: Standard errors in parentheses, clustered by sector (793 clusters). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using firm level data and firms from all untreated sectors as the control group. The dependent variable is the log of the hours worked by paid employees of the firm. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015. The Treatment_t variables are indicators for treatment in year t. The variable "high abstention rate" is a dummy for districts with an abstention rate in the 2009 European Parliamentary Election that was above the median. The variable "low abstention rate" is a dummy for districts with below median abstention. In the last column both variables are interacted with the treatment assignment DD.

Similar results are also found using the number of paid and unpaid employees, denoted total employment, as the outcome variable. The results are shown in Table 13. Column (1) shows a baseline regression, whereas column (2) additionally controls for district fixed effects. I find an effect on the total number of employees of about 3.1%. Columns (3) and (4) examine the leads and lags. Again, the leads are all insignificant, thereby providing support in favor of the parallel trends assumption. Column (5) shows how the effects vary by the level of social trust.

As before, firms in low trust districts experience stronger positive effects.

Table 13: Firm Level Total Employment Effects

	(1) Log Total Employment	(2) Log Total Employment	(3) Log Total Employment	(4) Log Total Employment	(5) Log Total Employment
DD (Post*Treatment)	0.03119***	0.03132***			
	(0.01112)	(0.01110)			
Treatment_{2010}			-0.005559	-0.005809	
			(0.009470)	(0.009569)	
Treatment_{2011}			0.001363	0.001411	
			(0.006546)	(0.006589)	
Treatment_{2013}			0.01499***	0.01497***	
			(0.005186)	(0.005148)	
Treatment_{2014}			0.02617***	0.02629***	
			(0.008156)	(0.008071)	
Treatment_{2015}			0.04803***	0.04813***	
			(0.01533)	(0.01517)	
DD * High abstention rate					0.03872**
					(0.01505)
DD * Low abstention rate					0.02471*
					(0.01345)
Sector FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
District FE	NO	YES	NO	YES	YES
Observations	1773600	1773600	1773600	1773600	1773534
Adjusted R^2	0.017	0.019	0.017	0.019	0.019

Note: Standard errors in parentheses, clustered by sector (793 clusters). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using firm level data and firms from all untreated sectors as the control group. The dependent variable is the log of all (paid and unpaid) employees of the firm. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015. The Treatment_t variables are indicators for treatment in year t. The variable "high abstention rate" is a dummy for districts with an abstention rate in the 2009 European Parliamentary Election that was above the median. The variable "low abstention rate" is a dummy for districts with below median abstention. In the last column both variables are interacted with the treatment assignment DD.

Table 14 shows the results for the matched sample, similarly[31] to the main specification in section 4.1.2. The results are again comparable to those for paid employment, with significant treatment effects of around $2\% - 2.5\%$. Although unreported, the leads are insignificant and the treatment effects are larger for districts with lower trust in this case.

For parsimony, I do not include the results for the aggregated sample, but they are similar to the results already presented for the number of paid employees. This is unsurprising given that I do not find an effect for the exit rate.

[31] The previous results match on the number of hours worked and the EBIT. I continue to do so for total employment. To avoid matching on pre-treatment outcomes, for the number of hours, I instead match on paid employment and EBIT.

Table 14: Firm Level Effects – Alternative Outcomes with Matching

	(1) Log Hours	(2) Log Hours	(3) Log Total Employment	(4) Log Total Employment
DD (Post*Treatment)	0.0202**	0.0200**	0.0251***	0.0250***
	(0.00994)	(0.00984)	(0.00904)	(0.00893)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
District FE	NO	YES	NO	YES
Observations	1335962	1335962	1429238	1429178
Adjusted R^2	0.155	0.156	0.179	0.178

Note: Standard errors in parentheses, clustered by sector (780 and 781 clusters for the first two and last two columns respectively). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions with propensity score matching, using firm level data and all firms from untreated sectors except the six largest sectors (in terms of employment) as the control group. Matching for all regressions is performed on EBIT and additionally the number of employees for columns (1) and (2) and the hours worked for columns (3) and (4). The dependent variable for the first two columns is the log of the hours worked by paid employees. The dependent variable for the last two columns is the log of all (paid and unpaid) employees. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

Appendix C: Alternative Control Groups

I also consider difference-in-difference results for alternative samples in order to generate a more comparable control group. Table 15 shows the firm-level results when excluding the six largest sectors in terms of employment from the control group, as done for the matching results described in section 4.1.2. The treatment effects for employment and wages are significant at the 1% level and very similar to the baseline regressions in terms of magnitude. For parsimony, I do not report the coefficients on the leads and lags or the effects by abstention rate. However, the leads are insignificant with treatment effects increasing over time and lower trust districts again exhibit stronger effects.

Since the treated firms typically do not export and exports are not subject to the enforcement reform, a plausible sample could also consist only of non-exporters. Table 16 presents firm level results with any firms that have positive levels of exports excluded from the sample. The coefficient for employment is still significant and slightly larger in magnitude, with an effect of 4.9%. The treatment effect for wages is similar to before, but not statistically significant, which may be due to a loss of observations. Although unreported, this sample is the only case where we do not observe stronger employment effects for lower trust districts. Nevertheless, for these regressions the lead coefficients for the year 2010 are significant, which may imply that these

results are less valid.

Table 15: Firm Level Effects – Excluding Large Sectors

	(1) Log Employment	(2) Log Employment	(3) Log Wages	(4) Log Wages
DD (Post*Treatment)	0.0327***	0.0326***	0.0260***	0.0253***
	(0.009907)	(0.009793)	(0.006079)	(0.006044)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
District FE	NO	YES	NO	YES
Observations	1488271	1488271	1161909	1161909
Adjusted R^2	0.019	0.021	0.020	0.029

Note: Standard errors in parentheses, clustered by sector (787 and 786 clusters for the first two and last two columns respectively). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using firm level data and all firms from untreated sectors except the six largest sectors (in terms of employment) as the control group. The dependent variable for the first two columns is the log number of paid employees and for the last two is the log of the average wage per employee. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

Table 16: Firm Level Effects – Excluding Exporting Firms

	(1) Log Employment	(2) Log Employment	(3) Log Wages	(4) Log Wages
DD (Post*Treatment)	0.04960***	0.04901***	0.02281	0.02174
	(0.01578)	(0.01566)	(0.01489)	(0.01518)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
District FE	NO	YES	NO	YES
Observations	117206	117206	90806	90806
Adjusted R^2	0.018	0.022	0.017	0.022

Note: Standard errors in parentheses, clustered by sector (651 and 637 clusters for the first two and last two columns respectively). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using firm level data on non-exporting firms. The dependent variable for the first two columns is the log number of paid employees and for the last two is the log of the average wage per employee. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

The results are also similar when using propensity score matching and excluding exporting firms, as shown in Table 17. The coefficients for paid employment and wages are again significant with respective magnitudes of approximately 5.1% and 2.7%.

I now turn to the sector level effects for these alternative samples. I omit the coefficients related to the abstention rate, but each case shows stronger effects for lower trust districts. Table 18 reports the results when excluding the six largest sectors, while Table 19 reports those when excluding exporting firms. The results are again similar to those of the main specification in section 4.2.1, with the coefficients for paid employment ranging between $8.7\% - 9.4\%$ and for

Table 17: Firm Level Effects – Excluding Exporting Firms with Matching

	(1) Log Employment	(2) Log Employment	(3) Log Wages	(4) Log Wages
DD (Post*Treatment)	0.05192***	0.05133***	0.02766*	0.02623*
	(0.01702)	(0.01675)	(0.01477)	(0.01524)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
District FE	NO	YES	NO	YES
Observations	107602	107602	74236	74236
Adjusted R^2	0.161	0.164	0.083	0.088

Note: Standard errors in parentheses, clustered by sector (622 and 600 clusters for the first two and last two columns respectively). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions with propensity score matching on pre-treatment hours worked and EBIT using firm level data on non-exporting firms. The dependent variable for the first two columns is the log number of paid employees and for the last two is the log of the average wage per employee. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

wages between $2.2\% - 4.7\%$.

Table 18: Sector Level Effects – Excluding Large Sectors

	(1) Log Employment	(2) Log Employment	(3) Log Wages	(4) Log Wages
DD (Post*Treatment)	0.0945**	0.0926**	0.0224	0.0263**
	(0.0386)	(0.0420)	(0.0147)	(0.0130)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Legal Form FE	NO	YES	NO	YES
District FE	NO	YES	NO	YES
Observations	100753	100753	93527	93527
Adjusted R^2	0.153	0.506	0.186	0.390

Note: Standard errors in parentheses, clustered by sector (787 and 786 clusters for the first two and last two columns respectively). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using data aggregated across firms by sector, district and legal form and excluding the six largest sectors (in terms of employment) from the control group. The dependent variable for the first two columns is the log number of paid employees and for the last two is the log of the average wage per employee. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

Lastly, using the aforementioned alternative outcome variables with these alternative samples yields similar results, which are not reported for parsimony. This is unsurprising, since Appendix B shows that these outcome variables are largely capturing the same variation as paid employment.

Table 19: Sector Level Effects – Excluding Exporting Firms

	(1) Log Employment	(2) Log Employment	(3) Log Wages	(4) Log Wages
DD (Post*Treatment)	0.0875*	0.0869**	0.0469	0.0399
	(0.0468)	(0.0420)	(0.0285)	(0.0274)
Sector FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Legal Form FE	NO	YES	NO	YES
District FE	NO	YES	NO	YES
Observations	26316	26316	22987	22987
Adjusted R^2	0.197	0.431	0.132	0.209

Note: Standard errors in parentheses, clustered by sector (651 and 637 clusters for the first two and last two columns respectively). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using data aggregated across firms by sector, district and legal form and excluding non-exporting firms. The dependent variable for the first two columns is the log number of paid employees and for the last two is the log of the average wage per employee. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

Appendix D: Sales

This section examines the effects of the intervention on reported sales. Since many firms tend to report zero sales, I look at the level instead of the logarithm. Reassuringly, I find positive effects, which would be consistent with an increase in the tax burden for firms. Table 20 shows the effects on the sales of goods in columns (1) and (2). The value of services sold is the outcome variable in columns (3) and (4), while the last two columns use the value of all sales (goods and services) as the dependent variable. District fixed effects are included in columns (2), (4) and (6). There is a positive and significant increase in sales of around €$42\,000$ each for sales and services, with an effect of approximately €$130\,000$ for the sum of these two.

Table 20: Sales Effects

	(1) Sales	(2) Sales	(3) Services	(4) Services	(5) Turnover	(6) Turnover
DD (Post*Treatment)	42.40**	43.06**	41.99***	42.00***	132.4***	134.7***
	(19.34)	(19.22)	(11.62)	(11.66)	(25.79)	(26.02)
Sector FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
District FE	NO	YES	NO	YES	NO	YES
Observations	2262234	2262234	2263261	2263261	2206162	2206162
Adjusted R^2	0.000	0.000	0.000	0.000	0.000	0.000

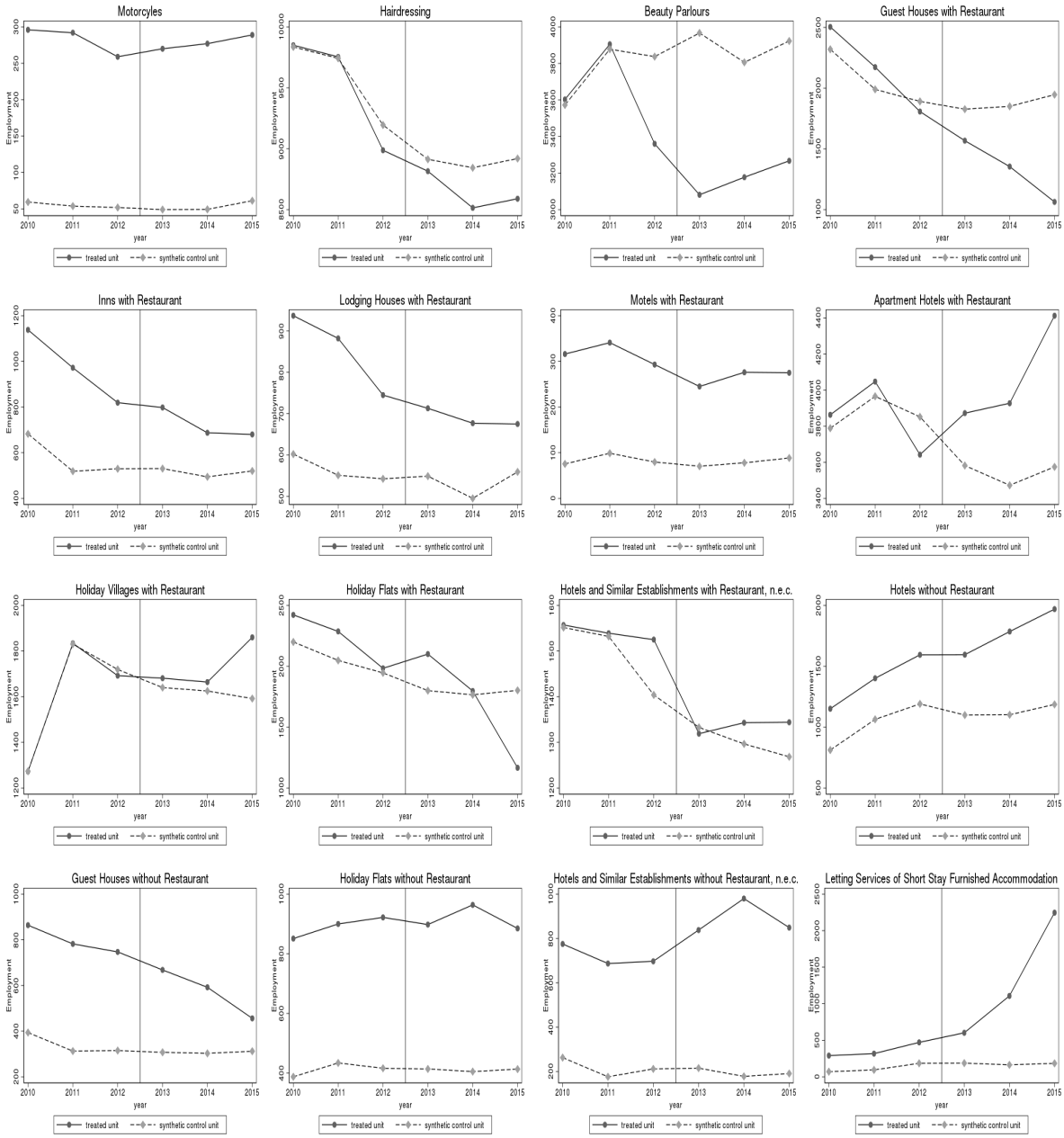
Note: Standard errors in parentheses, clustered by sector (796 clusters). Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table displays DD regressions using firm level data and all firms from untreated sectors as the control group. The dependent variable for the first two columns is the value of goods sold, for the second two is the value of services sold and for the last two it is the value of all goods and services sold, all expressed in thousands of euros. All regressions include a dummy indicating whether a firm existed before the intervention as a control variable. The DD variable is defined as the interaction between a dummy for treated sectors and a dummy that equals 1 for the treatment period 2013-2015.

Appendix E: Synthetic Control

For completeness, this section shows the results from the synthetic control estimation described in section 4.2.2 for the remaining industries. These are depicted in Figures 4 and 5. The solid line represents the evolution of the treated sector, while the dotted line is the estimated counterfactual.

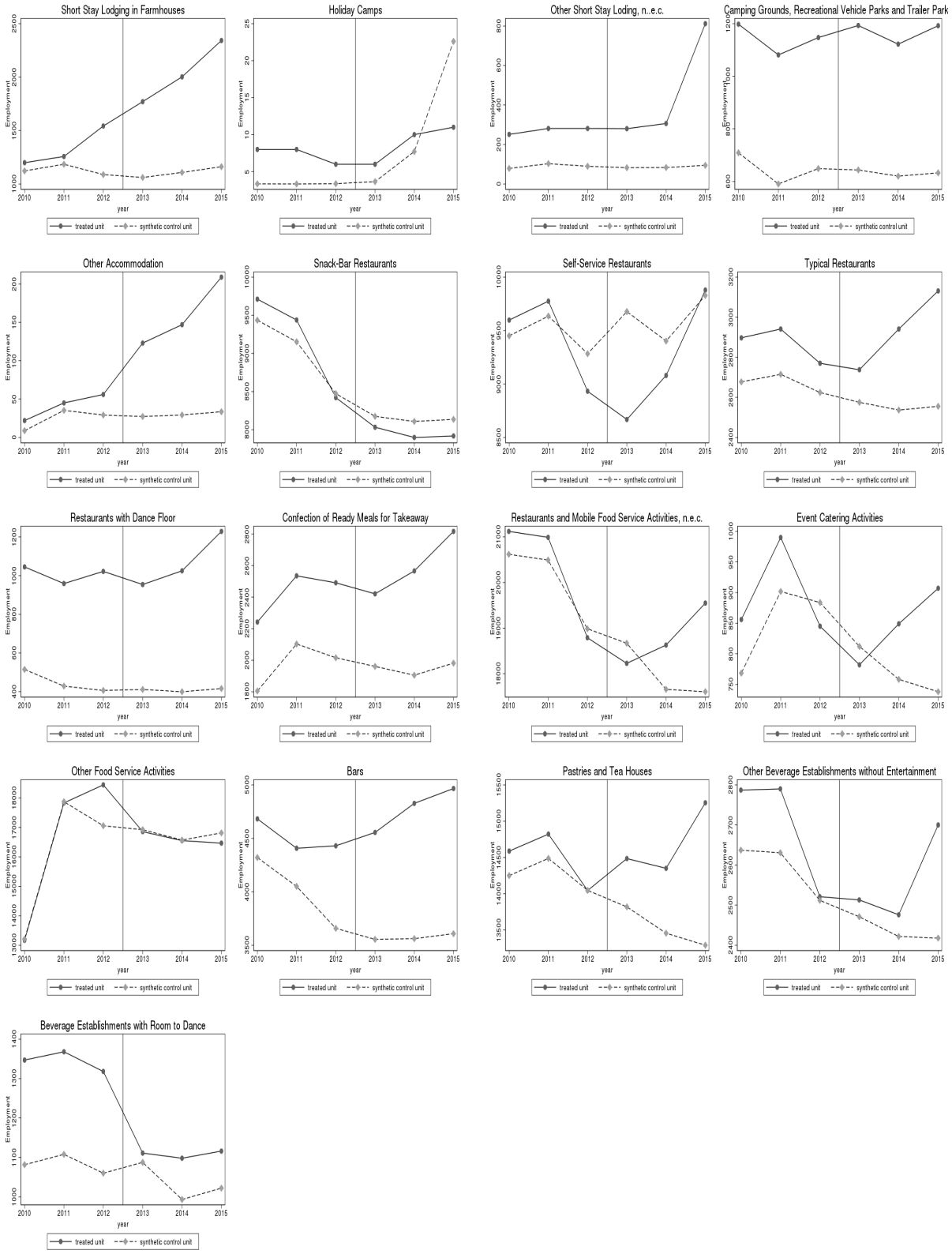
For most industries it was not possible to obtain a good pre-treatment fit for the synthetic control group, thereby making these comparisons less meaningful. This may be a result of using the same predictor variables for all industries, even though the relevant predictors may differ. Nevertheless, this approach was chosen for consistency. For sectors with a good fit, however, we can observe a positive effect on employment.

Figure 4: Synthetic Control Results I



Note: The figure plots the evolution of paid employees (solid line) compared to an estimated synthetic control group (dotted line) for selected industries. Source: Own calculations based on the data described in section 3.

Figure 5: Synthetic Control Results II



Note: The figure plots the evolution of paid employees (solid line) compared to an estimated synthetic control group (dotted line) for selected industries. Source: Own calculations based on the data described in section 3.