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Article (Accepted for Publication)
(Refereed)

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Higher taxes, more evasion?

Evidence from border differentials in TV license fees [*]

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Abstract

This paper studies the evasion of TV license fees in Austria. We exploit border differentials to identify the effect of fees on evasion. Comparing municipalities at the low- and high-fee side of state borders reveals that higher fees trigger significantly more evasion. Our preferred estimator indicates that a one percent increase in fees raises the evasion rate by 0.3 percentage points. The positive effect of fees on evasion is confirmed in different parametric and non-parametric approaches and survives several robustness checks.

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1 Introduction

Identifying the link between taxes and evasion is an equally difficult as important task for empirical research: it is difficult, because measuring evasion requires us to work with 'evidence of the invisible' (Slemrod and Weber, 2012); at the same time, it is important to quantify evasion responses to taxation, in order to predict revenue consequences of tax reforms and to design optimal tax policies.[1] In contrast to studies that exploit exogenous variation in enforcement (e.g., Kleven et al., 2011), however, causal evidence on the impact of taxes on evasion is still scarce. The early literature on income tax evasion provides conflicting evidence (e.g., Clotfelter, 1983; Slemrod, 1985; Feinstein, 1991). Recent studies point to a positive effect: Gorodnichenko et al. (2009), who study a major tax reform in Russia, find a huge positive elasticity of evasion with respect to the tax rate.[2] Kleven et al. (2011) examine bunching at kinks in the Danish income tax schedule. Comparing bunching of pre- and post-auditing incomes, they identify a small positive effect of tax rates on evasion. Our paper indirectly contributes to this literature by studying the evasion of TV license fees. Based on unique cross-sectional data from Austria, we examine whether higher fees result in more evasion.

License fees are a widespread tool to finance public broadcasting: two thirds of all European, half of all African and Asian, and a few countries in the Americas collect license fees. In 2005, a total of € 20 billion on fees were collected in Europe (see Fellner et al., 2013). Households have an incentive to evade fees because public broadcasting programs can be received without paying fees. Rincke and Traxler (2011) demonstrate that households trade off the gains from evasion against the costs of detection. Beyond this similarity to tax evasion, the institutional framework is attractive as it offers a good measure of evasion: 99 percent of all Austrian households own a radio or TV (ORF Medienforschung, 2006), which makes them liable to register for license fees, according to federal law. Relating the number of registered to all households thus gives a reasonable proxy for evasion. In addition, the set-up allows us to apply a border based identification strategy in the spirit of Holmes (1998).

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[1]Note that taxable income is not a sufficient statistic to evaluate the efficiency cost of income taxation when behavioral responses generate externalities (Saez et al., 2012). As tax evasion is associated with fiscal externalities (Chetty, 2009), optimal income taxation depends on whether the elasticity of taxable income is mainly driven by evasion rather than, say, labor supply responses (Piketty et al., 2014).

[2]Their results might be influenced by a simultaneous reform in the tax administration. Further evidence on large behavioral responses in a high evasion context are provided by Kopczuk (2012) and, in the context of tariff evasion, by Fisman and Wei (2004).

Total license fees include a specific state tax. While the collection and enforcement of the fees is harmonized at the federal level, variation in the state tax creates significant border differentials in license fees. We exploit these discontinuities – or 'border notches' (Slemrod, 2010) – by comparing evasion rates among municipalities on the high- and low-tax side of state borders. In addition, we compute the driving distance of each municipality to the nearest state border and implement a regression discontinuity design (Lee and Lemieux, 2010). Before doing so, we carefully discuss the identifying assumptions that allow us to exploit the border differentials in a quasi-experimental way. Among others, we document that – within the tightly constrained framework of Austria's federalism – other fiscal policies are uncorrelated to the specific state tax. Moreover, we show that a large set of relevant municipality characteristics (including enforcement rates) are balanced and smoothly distributed around the borders.

The analysis of border differentials identifies a precisely estimated, positive effect of fees on evasion. This result is confirmed in different parametric and non-parametric approaches and survives several robustness checks. On average, license fees increase by around 18 percent – from € 206 to € 243 – at the state borders. This differential is accompanied by a discontinuous increase in the evasion rate of 5 percentage points. Putting these numbers together, our central estimate indicates that a one percent increase in fees raises the evasion rate by about 0.3 percentage points.

Given that this semi-elasticity reflects a binary response – evasion at the extensive margin – it is hard to directly compare the effect size with the intensive margin responses analyzed in the literature. However, finding a large evasion response is consistent with the huge elasticities of evasion with respect to tax rates documented in the few other studies: Fisman and Wei (2004), for instance, find that a one percent increase in taxes and tariffs increases import tax evasion by more than 3 percent. Fack and Landais (2016) document that the elasticity of overreporting tax deductions (charitable contributions) is large and above 2. Similarly strong income reporting effects in equally weak enforcement contexts are provided by Gorodnichenko et al. (2009) and Kopczuk (2012). The large effect identified in our institutional set-up seems consistent with these findings.

We make several contributions to the literature. First and foremost, our evidence strongly supports the intuition that higher taxes trigger more evasion. This is important for two reasons. On the one hand, the relationship between taxes and evasion is theoretically ambiguous (Yitzhaki, 1974). We introduce a simple model to study the binary evasion decision which is relevant in our case. Although

our set-up differs from the classical income tax evasion theory in several important ways, we show that the ambiguous comparative static from the literature also applies to our context. On the other hand, empirical evidence on the causal link between taxation and evasion is scarce and conflicting (see the survey in Andreoni et al., 1998). In light of this scarcity and in the absence of a clear theoretical prediction, the result that higher fees trigger more evasion marks a valuable contribution. Moreover, by studying a binary evasion decision, we provide a rare piece of evidence on the extensive margin of evasion.

On a more general account, our study provides evidence that further corroborates the rational model of evasion which stresses the economic incentives to cheat. The relevance of these incentives was often questioned in the past. Over the last years, however, several studies convincingly demonstrated that the expected *costs* from evasion play a significant role in shaping non-compliance (Kleven et al., 2011; Fellner et al., 2013; Dwenger et al., 2016). The present paper contributes to this literature by documenting the impact of the potential *gains* from cheating.

In terms of methods, the present study is the first to use discontinuities at borders – in the tradition of Holmes (1998) and Black (1999) – to identify the effect of taxes on evasion. Our approach is closely related to recent work that exploits state tax differentials to analyze cigarette tax avoidance (Merriman, 2010) and the role of the internet as a tax haven (Agrawal, 2014).[3] More generally, we contribute to the growing literature on border based identification (e.g., Bayer et al., 2007) and spatial regression discontinuity designs (e.g., Lalive, 2008).

The remainder of the paper is structured as follows. Section 2 introduces the institutional background and describes our data. In Section 3 we discuss a simple theoretical model with a binary evasion decision. Section 4 briefly discusses the outcome from a naive cross-sectional regression and highlights the identification problem. Section 5 discusses our identification strategy and presents the results from a border notch and a spatial RD design. The last section concludes.

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[3]For other studies that work with border tax differentials, see Eugster and Parchet (2013), Agrawal (2015), and Agrawal and Hoyt (2014).

2 Set-up and Data

2.1 TV License Fees

Many countries in the world use obligatory TV and radio license fees to finance public broadcasting. A typical system of license fees can be found in Austria, where the Broadcasting License Fee Act stipulates that every 'household' (broadly defined, including apartment-sharing communities, etc.) must register its broadcasting equipment with Fee Info Service (FIS). FIS, a subsidiary of the public broadcasting company, is responsible for collecting and enforcing the fees. Each year, one license fee has to be paid per household, independently of the number of household members, TVs and radios.[4] In 2005, the relevant year for our study, the annual public broadcasting contribution was €182. In addition to this contribution, the total fee due included federal taxes (€24) plus a state tax. This state tax ('*Landesabgabe*') considerably differed between the states. As a consequence, the total annual fees ranged from €206 to €263.[5] On average, 25% of the total fee represent state or federal taxes (see below). The license fee system thus includes a non-trivial tax component.

Public broadcasting programs can be received without paying license fees. Households therefore face an incentive to evade license fees (and thereby the included taxes) by not registering their broadcasting equipment. FIS takes several actions to enforce compliance. It sends mailings to unregistered households (see Fellner et al., 2013) and runs an enforcement division whose members inspect potential evaders at their homes (see Rincke and Traxler, 2011). Detected evaders have to pay evaded fees and authorities may impose a fine of up to €2,180. The deterrent threat from these fines and FIS' enforcement activities is reflected in a fairly high level of compliance: In 2005, 7.9 percent of all Austrian households were not registered with FIS, whereas only one percent of households neither owned a radio nor a TV (ORF Medienforschung, 2006). In total, FIS collected revenues of about €650 million (roughly 0.3% of GDP). About 10% of this sum were federal taxes and provided revenues for the national budget (non-earmarked); roughly 15% were based on state taxes and got assigned to the different states (earmarked for the promotion of art and culture); only the remaining 75% serve the

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[4]An additional fee is due for secondary residences and holiday homes with broadcasting equipment. The fee does not depend on whether one actually uses the broadcasting equipment. For further institutional details see Fellner et al. (2013).

[5]Several states apply the same (round number) tax rates on the broadcasting contribution basis to determine the *Landesabgabe*: in 2005 the state tax was €17.6 in Burgenland and Tirol, €36.7 in Vienna, €37.2 in Lower Austria and Salzburg, €49.2 in Carinthia, and €56.5 in Styria. Upper Austria and Vorarlberg did not impose this tax. Possible explanations for this variation are discussed below (see Section 5).

public broadcasting system. Compliance is in permanent flux. An easy opportunity to start evading fees emerges in case of moving. Broadcasting registrations are attached to the place of residence and moving households often de-register at the old place without registering at the new residence. In principle, this could be due to forgetfulness. However, households constantly receive nudges which remind them about their legal obligation to register. During the second half of 2005, FIS placed an average of three daily spots in countrywide broadcasted channels. In addition there are campaigns in newspapers and billboards. Not registering after the first few months at a new home thus seems like a very deliberate choice.

2.2 Data

Our analysis exploits data on the number of households that had registered any broadcasting equipment in the fourth quarter of 2005. The raw data provide this number for each of the 2,380 Austrian municipalities. Following FIS' method to compute a proxy for the evasion rate, we compare the number of households with registered broadcasting equipment, R_i, to the number of households with a residence in that municipality, H_i. We then compute the evasion rate

$$\text{Evasion}_i = \frac{H_i - R_i}{H_i}$$

for each municipality i. Since only one percent of households do not own any broadcasting equipment (see above), Evasion_i is a reasonable proxy for a municipality's evasion rate. Nevertheless, evasion is measured with error. First, H_i refers to primary places of residence whereas R_i also includes some registrations of broadcasting equipment at secondary residences (see fn. 4).[6] Registrations of the latter type are very infrequent and only account for 1.3 percent of all broadcasting registrations. For municipalities with a significant share of secondary residences, we could nonetheless observe $R_i > H_i$. In response to this point, we deviated from the FIS' standard and used the sum of primary and secondary residences as basis for computing an alternative evasion rate. All results reported below are robust to using this alternative measure. However, to avoid problems related to the underreporting of secondary residences in the official residency register we focus on the evasion rate as defined above.

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[6]Note further that R_i also includes registrations that emerged from past enforcement activities.

Second, there are no municipality level data that would allow us to correct for variation in the number of households without broadcasting equipment. This measurement error could become problematic if it were correlated with the level of the fees. To asses this concern we studied TV ownership using data from a large, representative survey (see Online Appendix). The analysis shows that the correlation between TV license fees and ownership is statistically and economically insignificant: a one percent increase in the fee is associated with less than a 0.01 percentage point lower chance of owning a TV (see Table III.1 in the Online Appendix). We are therefore confident that the variable captures evasion rather than real economic responses.

Table 1 about here.

Table 1 shows that the average evasion rate across all 2,380 Austrian municipalities is 4.5 percent. If we weight each municipality's evasion rate by the number of households, we obtain a weighted average of 7.9 percent (which corresponds to the total evasion rate in Austria, i.e., $\sum_i (H_i - R_i)/\sum_i H_i$). FIS also provided us with data on license fees and on the number of registrations stemming from the enforcement division's door-to-door checks. Based on the latter, we compute municipality level enforcement rates as the sum of enforced registrations during 2005 relative to H_i. As displayed in Table 1, the average enforcement rate was 1.2 percent.

We complement the data from FIS with an extensive set of municipality characteristics.[7] Our data include, among others, information on labor income, age, education, occupational structure, household size, religion and voting outcomes. The descriptive statistics indicate that municipalities are fairly small, with an average of 1,500 households. As discussed in more detail in Section 5.2, we also computed the driving distance from each municipality to the nearest state border. On average, a municipality is located a 41 minute drive from the closest state border.

3 Model

To set the stage for our empirical analysis, we first study the role of fees for a household's decision to either pay or evade fees. We model this binary choice in the spirit of Allingham and Sandmo (1972). An agent with an exogenous (after-tax) income y_i faces a license fee t. If he pays the fee, his available income is $y_i - t$. If he evades the fee, he is detected with probability p, $0 < p < 1$. In case of detection,

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[7]Detailed information on data sources and further summary statistics are provided in the Online Appendix.

he has to pay the license fee and a fine, $s > 0$, resulting in an available income of $y_i - t - s$. In case the evasion remains undetected, the agent avoids any payment.

Preferences over available income are described by a twice differentiable function $U_i(.)$, with $U_i' > 0 \geq U_i''$. Utility is given by the deterministic $U_i(.)$ plus a random utility component η for the case of compliance.[8] The agent will choose to evade if and only if

$$p\, U_i(y_i - t - s) + (1 - p)\, U_i(y_i) \geq U_i(y_i - t) + \eta. \tag{1}$$

Let η be distributed according to the cdf $F(.)$. The probability of evasion is then given by

$$F(x) \quad \text{with } x := p\, U_i(y_i - t - s) + (1 - p)\, U_i(y_i) - U_i(y_i - t). \tag{2}$$

Note that this model deviates from the classical theory of income tax evasion in two important ways: First, the fee t is not a rate but a fixed payment. Second, the fine s is neither proportional to the evaded fee (Yitzhaki, 1974) nor to the income. The comparative statics for our set-up are therefore not at all obvious. Based on (2) one can analyze how the probability of evasion responds to an increase in t. Differentiating $F(x)$ w.r.t. t we obtain

$$F'(x)\frac{\partial x}{\partial t} = F'(x)\left[U_i'(y_i - t) - p\, U_i'(y_i - t - s)\right]. \tag{3}$$

For a *risk-neutral* agent ($U_i'' = 0$), U_i' is constant and $\partial x / \partial t > 0$ since $p < 1$. The probability that a risk-neutral agent evades is therefore increasing in the fee (for $F'(x) > 0$). For the case of *risk-aversion* ($U_i'' < 0$), the sign of $\partial x / \partial t$ is ambiguous. As long as the degree of risk aversion (captured by the curvature of the utility function) is sufficiently small or, equivalently, if p is sufficiently small, one obtains $\partial x / \partial t > 0$. Hence, for $p < \bar{p} := U_i'(y_i - t)/U_i'(y_i - t - s)$ (where $U_i'' < 0$ implies $0 < \bar{p} < 1$), the probability of evasion is again increasing in the fee. Although the enforcement rate is quite low in

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[8]One might think of η as the 'net' effect from different random utility terms that separately enter the (expected) utility from evasion (say η^-) and from compliance (η^+). These terms might, for instance, reflect heterogenous levels of intrinsic motivation to comply.

our context (see Table 1), it is hard to judge whether the condition from above is met. The empirical analysis will shed further light on this point.[9]

4   Cross-sectional Analysis

As a starting point, we analyze the cross-sectional variation in evasion. We estimate the model

$$\text{Evasion}_i = \alpha^{cs} + \beta^{cs} \log(\text{Fees}_i) + \mathbf{X}_i \boldsymbol{\gamma}^{cs} + \epsilon_i^{cs}, \tag{4}$$

where $\mathbf{X}_i$ includes a large set of control variables that account for municipality differences in, e.g., population size and density, age, educational, religious, household and occupational structure as well as voting outcomes. In addition, we control for the local enforcement rate and average labor income. As license fees only vary at the state level, we compute clustered standard errors. To account for the small number of cluster units (Austria has nine states), we bootstrap the standard errors following Cameron et al. (2008)'s wild cluster bootstrap-t procedure.

The results from OLS estimates of equation (4) are reported in Table 2.[10] We find a positive correlation between the level of license fees and the evasion rates. The coefficient indicates that a one percent increase in fees is correlated with a 0.13 percentage point increase in the evasion rate. The estimate, however, is statistically insignificant as the (bootstrapped) clustered standard errors are fairly large.

Table 2 about here.

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[9]Two further comparative statics are worth noting. First, it is straightforward to demonstrate that the probability of evasion is decreasing with a higher detection risk, p, and increasing in risk aversion. Second, the effect of income on evasion is less clear-cut. Taking the derivative of $F(x)$ w.r.t. y_i we arrive at $F'(x)\frac{\partial x}{\partial y_i} = F'(x)\left[p\,U_i'(y_i - t - s) + (1-p)\,U_i'(y_i) - U_i'(y_i - t)\right]$. For risk-neutrality we get $\partial x / \partial y_i = 0$ and there would be no income effect on evasion. For the case of risk-aversion, $\partial x / \partial y_i$ is positive whenever $p > \underline{p} := \left[U_i'(y_i - t) - U_i'(y_i)\right] / \left[U_i'(y_i - t - s) - U_i'(y_i)\right]$. If this condition is satisfied, the probability of evasion increases in income. It is worth noting that the latter condition, $p > \underline{p}$, does not conflict with $p < \bar{p}$ from above. One can easily show that $0 < \underline{p} < \bar{p} < 1$. (To do so, rewrite $\underline{p} < \bar{p}$ as $U_i'(y_i - t - s)\left[U_i'(y_i - t) - U_i'(y_i)\right] < U_i'(y_i - t)\left[U_i'(y_i - t - s) - U_i'(y_i)\right]$. Simplifying yields $U_i'(y_i - t - s) > U_i'(y_i - t)$, which holds due to $U_i'' < 0$.) Hence, for the case $\underline{p} < p < \bar{p}$, the model would predict that the evasion probability increases in the fee *and* in income.

[10]The complete estimation output for all control variables is reported in the Online Appendix.

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The cross-sectional analysis further shows a negative correlation between the enforcement and the evasion rate[11] as well as an economically and statistically insignificant income effect. At the same time there is a strong, positive correlation between the share of self-employed and the evasion rate. Previous research has found that receiving self-employed (i.e., not third-party reported) income crucially shapes the opportunity to evade income taxes (Kleven et al., 2011). In our case, there is no 'technological' difference in the opportunity to evade license fees between different occupational groups. A possible interpretation of the evidence is that more self-employment within a municipality is correlated with less risk aversion (Ekelund et al., 2005). In turn, this might produce more evasion.

Given the lack of experimental variation in license fees, it is questionable whether the positive correlation between license fees and evasion captures a causal effect. The state level taxes that drive the differences in the fees might be set according to unobserved factors (e.g., risk-attitudes) that shape evasion. To the extent that our control variables do not (fully) account for these factors, the OLS estimate for β^{cs} might be downward biased.[12] In the following, we discuss two approaches to this identification problem.

5 Identification and Results

5.1 Border Notches

Our first approach to identify the effect of fees on evasion relates to the notion of 'border notches' (Slemrod, 2010), i.e., the idea that borders create discontinuous changes in a certain treatment. In our context, there are border tax differentials (similar as, e.g., in Agrawal, 2015) which produce discontinuous changes in license fees at state borders (see Section 2). The comparison of evasion rates between municipalities on the 'high tax'- and 'low tax'-side of a state border then captures the effect of interest, as long as other factors that shape evasion do not change discontinuously at the border. Several institutional aspects suggest that our application gets quite close to this ideal design.

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[11]Due to the obvious simultaneity between evasion and enforcement, the coefficient is potentially misleading. Identifying the causal effect of enforcement on evasion is beyond the scope of the present paper (see Rincke and Traxler, 2011). If we run instrumental variable estimations that follow a similar identification strategy as Rincke and Traxler, 2SLS estimates indicate a substantially larger deterrent effect from enforcement. The estimated β, however, remains unaffected.

[12]Consider a hypothetical variable that measures local risk aversion, v_i which would enter with coefficient $\gamma_v < 0$ in equation (4). As long as fees are higher in states with more risk averse taxpayers, $\mathrm{Cov}\left(\log(\mathrm{Fees}_i), v_i\right) > 0$, omitting v_i implies that the OLS estimate for β^{cs} is biased downwards.

First, the public broadcasting service and its quality attributes do not depend on the variation of the fee. The revenues from the (federal and state) taxes, which FIS collects together with the broadcasting contributions, are not invested into broadcasting. Public broadcasting service is almost identical across all of Austria. The state specific content in TV programs, for instance, accounts for only four out of 336 weekly hours of public broadcasting.

Second, Austrian fiscal rules provide little incentives for households to sort on the low-fee side of a state border. For one, the border tax differentials *per se* are too small to plausibly influence a household's residential choice.[13] In addition, other local fiscal parameters that may be correlated with the level of the state tax are likely to play a limited role, too. In Austria, essentially all important fiscal and welfare policies are set at the level of the central government. In principle, states do have spending responsibilities in several domains (e.g., health care, primary and secondary education). However, the states (and municipalities) have hardly any taxing power and rely largely on inter-governmental grants and shared federal tax revenues for which the central government has full legislative responsibilities (OECD, 2005).[14] Moreover, there exists a Fiscal Equalization Law, which regulates inter-governmental fiscal relations and explicitly aims at achieving equal living conditions in all regions. As a consequence, a substantial share of the grants to the states is earmarked and the central government heavily constraints the framework under which sub-central governments can maneuver (Fuentes et al., 2006). Consistent with the objective of the Fiscal Equalization Law, mobility rates in Austria are quite low.

Third, a serious threat to identification could arise if enforcement activities endogenously respond to the tax differentials: if higher fees trigger more evasion this could in turn stipulate more enforcement on the high-fee side of a border. Institutional arrangements should again prevent this from happening, as the allocation of enforcement resources is centralized and based on the overall population size rather

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[13]Note that the state taxes which induce the variation in the license fees changed over time. Prior to 2005, reforms were rare and maintained the 'high- vs. low-fee' ranking between neighboring states, but more recent reforms reverted some of these rankings. If households rationally anticipated the possibility of such reforms they should not put much emphasis on the current level of license fees in their location choice. If there still was sorting according to fees, one might argue that any endogenous mobility responses would bias the estimated β^b *downwards*. Recall from above that moving offers an opportunity to start evading. When households systematically move into 'low-fee' municipalities at a border, the higher population influx should ceteris paribus increase the evasion rate – despite lower license fees. Hence, we would obtain a lower bound on the effect of fees on evasion.

[14]In 2005, the central government collected 95.15% of general tax revenue; the respective shares for states and municipalities were only 1.58% and 3.26%, respectively. In the same year, central government expenditure of total general government expenditure was 69.23%, as compared to 16.93% and 13.84% for the states and municipalities (see OECD Fiscal Decentralization Database).

than the level of evasion.[15] In addition, the second important parameter of enforcement, the fine s, is harmonized between states. Note further that, beyond enforcement, the procedure to voluntarily register for license fees is the same in all states. Hence, there is now border differential in compliance costs.

Finally, concerning the specific location of the borders, one might question whether municipality characteristics change at a border for topographical reasons. This concern is based on the fact that several Austrian state borders – especially those separating the 'northern' from the 'southern' states – are defined along Alpine mountain chains. It seems plausible that such natural borderlines could be associated with differences between bordering municipalities.[16]

Balancing Tests. Motivated by the discussion from above, we first study correlations between the specific state tax ('*Landesabgabe*', which drives the variation in licence fees) and different state level expenditures and revenues. Even though the state tax is earmarked for promoting art and culture, we do not find any significant correlation with the states' cultural expenditures ($r = -0.357$, $p = 0.385$). We obtain similar results – with either insignificantly positive or negative correlations – for other expenditure categories (e.g., health and education) as well as for overall expenditures and revenues. However, we do observe that states with higher debts impose a higher state tax: More indebted states seem to more actively exploit the rare chance to set a decentralized tax, even if this does not translate into higher overall revenues. Since the debt at the state level is relatively small (state debts accounted for 7% of total public debt in 2013) and states have only limited fiscal autonomy (see fn. 12), we do not expect the level of state debt to directly affect license fee evasion. The variation in state debts is therefore unlikely to threaten identification.

In a second step, we examine whether enforcement rates, household mobility and other municipality characteristics are balanced between the two sides of each state border. To do so, we run linear regressions of the form

$$x_i = \mu + \rho D_i + \nu_i \tag{5}$$

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[15] FIS' headquarter in Vienna assigns – depending on a county's population – one or two enforcement officers to each county. Working under a piece-rate contract, these local officers then choose independently when and where to monitor households in one of the county's municipalities (see Rincke and Traxler, 2011).

[16] It is worth noting that basically none of the state borders overlaps with important historical borderlines. In fact, the precise line of Austria's state borders are fairly young in historical terms: the borders result from transforming the law from Habsburg Monarchy, together with the provisions of the State Treaty of St. Germain (1919) and the Venice Protocol (1921), into Austrian constitutional law past WWI. Between 1938-45, the states of Tyrol and Vorarlberg were unified, and Burgenland was separated into two formerly non-existing states. Past WWII, the state borders of 1937 were reestablished.

for the sample of border municipalities; x_i denotes the variable that is compared and D_i is a dummy indicating whether a border municipality is located on the high-fee side of a state border. The coefficient of interest, ρ, reflects differences between the two sides of the border. Using 41 different municipality characteristics as dependent variables we separately estimate equation (5) for each of the 12 Austrian state borders listed in Table 3.[17] The estimated ρ's for several key municipality characteristics are reported in Table A.1. (Results for all 12×41 regressions are discussed in the Online Appendix.)

Consistent with the centralized allocation of enforcement resources, we do not find any systematic differences in enforcement rates.[18] In 10 out of 12 borders, there are no significant differences in enforcement activities across borders. In one case, the enforcement rate is slightly *lower* on the high-fee side of the border, in one case it is higher. The balancing tests also fail to detect evidence on systematic household sorting according to fees. The evidence is consistent with our conjecture that the fairly small differences in license fees do not influence residential choices. Beyond these primary characteristics of interest, the balancing tests do reveal several significant differences. However, for none of these variables we detect any systematic heterogeneity that is correlated with the level of license fees. Moreover, and in line with the discussion from above, the observed differences are primarily concentrated at state borders that are defined along the Alps.

Figure 1 about here.

Table 3 about here.

To account for the unbalanced municipality characteristics, our analysis will focus on the most balanced borders. We define a primary sample that excludes all borders which display significant differences (with $p \leq 0.05$) in more than 2 out of the 41 variables. With this cutoff, the main sample is composed of the four most balanced borders, indicated in Figure 1 and Table 3. The first two of these borders – Upper/Lower Austria and Upper Austria/Salzburg – are predominantly flat and non-mountainous. The two other borders – Salzburg/Styria and Vorarlberg/Tyrol – are more mountainous

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[17]Our analysis does not include the border between Vienna's outer districts and Lower Austria, as Vienna's jurisdictions differ systematically (and substantially) from the much smaller, neighboring municipalities. This reflects Vienna's special status as capital city and state.

[18]Below we will show that there are more evaders on the high-fee side of a border. Together with the constant enforcement rate this seems to suggest that the detection rate among evaders is actually lower on the high fee-side. However, this conclusion would be drawn too quickly as it mixes flow (detections during the last year) and stock variables (evaders at a given point in time). If we empirically compute a proxy for the average rate of detections among the population at risk during the last year, we find again no significant differences at the borders.

but expand from North to South and are thus orthogonal to the East-West stretch of the Alps.[19] Given that the choice of the cutoff is somewhat arbitrary, one might question the composition of the primary estimation sample. In what follows below, we address this concern by replicating each step of analysis for the full sample that includes all state borders.

Graphical Evidence. A first illustration of the change in the evasion rate at the borders is provided in Figure 2. The figure displays the evasion rates among border municipalities at the four borders from the main sample (see Table 3). At the first border (Upper/Lower Austria), annual license fees increase from €206.16 to €243.36. Evasion rates also increase from 1.4 to 4.2 percent, with the difference being significant ($p = 0.055$, according to a two-sided t-test). For the second border (Upper Austria/Salzburg), which is characterized by the same differential in license fees, we again observe a significant increase in evasion rates (from 4.0 to 10.1 percent; $p = 0.001$) when we move from the low to the high-fee side of the border. At the third border (Salzburg/Styria), fees jump from €243.36 to €262.56 and evasion increases from 5.2 to 8.8 percent ($p = 0.588$, due to a larger variance and a smaller sample). At the fourth border (Vorarlberg/Tyrol), the increase in fees from €206.16 to €233.76 is accompanied by a major increase in evasion from 2.0 to 21.1 percent ($p = 0.094$).[20] Hence, the observed differences in evasion rates are all positive and statistically significant at three out of four borders. While the analysis also illustrates a fairly large variation in evasion rates between different borders, one has to keep in mind that the samples at the last two borders are fairly small. Overall, the figure provides a first piece of evidence suggesting that higher fees trigger more evasion.

Figure 2 about here

Parametric Results. In a second step, we estimate the model

$$\text{Evasion}_i = \alpha^b + \beta^b \log(\text{Fees}_i) + \mathbf{X}_i \boldsymbol{\gamma}^b + \mathbf{B}_i \boldsymbol{\phi}^b + \epsilon_i^b \tag{6}$$

for all municipalities i that are located directly at a state border. $\mathbf{B}_i$ is a vector of border fixed effects which account for common differences in evasion rates between municipalities from different borders (see Figure 2). We augment this model by replacing the border with border-municipality group fixed

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[19]All our results are robust when we exclude the two latter borders from the main sample.

[20]The corresponding *p*-values for *one-sided* t-tests of the hypothesis that evasion is higher among municipalities on the high-fee side of the border are $p = 0.027$, $p = 0.000$, $p = 0.294$, and $p = 0.047$, respectively.

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effects. The latter fixed effects (which are similar to the boundary dummies used in, e.g., Black, 1999; Bayer et al., 2007) absorb any unobserved heterogeneity across groups of municipalities along each border.[21] The augmented model thus estimates β^b only from the variation in fees within the different border-municipality groups.

The outcome from estimating equation (6) for the sample of municipalities located at the state borders from the main sample is provided in columns (1)–(3) of Table 4. For the first specification, the estimated coefficient is 0.33 and highly significant. The result hardly changes when we omit border fixed effects. In column (2), we include the full set of border-municipality group fixed effects (41 dummies). The estimate slightly increases to 0.36 and remains highly significant. When we add the full vector of controls from the cross-sectional regression, the coefficient and standard error in column (3) remain again fairly stable. The last estimate suggests that a one percent increase in fees results in a 0.29 percentage point increase in the evasion rate. Hence, the effect is sizable and almost three times the correlation observed in the cross-sectional analysis.

Table 4 about here

To assess whether these findings are sensitive to the specific definition of the sample, we re-run the specifications for the full sample, i.e., for the border municipalities from *all* state borders (see Table 3). The estimates, reported in columns (4)–(6) of Table 4 again indicate a significant and stable positive effect of fees on evasion. The point estimates are quite precisely estimated at 0.28, only slightly below the coefficients obtained for the main sample.

5.2 Spatial Regression Discontinuity

While the analysis of border tax differentials provides clear evidence on a positive effect of fees on evasion, it is limited to a fairly small sample of municipalities located right at the state borders. A natural extension leads to a spatial regression discontinuity design (RDD), in which we will make use of a broader set of municipalities and a different metric to measure distance. The basic idea behind the spatial RDD is to interpret the distance to the closest state border as an assignment variable that

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[21]This approach can be motivated by the observation that tangential municipalities from different sides of a state border are indeed very similar in terms of observable characteristics (see above). In contrast to this similarity *within a group* of tangential border municipalities, there are often pronounced observable differences *between* municipality groups. To account for this heterogeneity along a state border, we assign all municipalities that 'touch' each other at one side of the state border into different groups. The procedure is described in more detail in the Online Appendix.

decides about the high vs. the low-fee 'treatment' (Imbens and Zajonc, 2011). Controlling for distance, one can then exploit the discontinuous change in fees at the borders. In implementing this design, we follow the recent literature and compute the driving time to the nearest state border (e.g., Lalive, 2008; Agrawal, 2015).[22] This measure of distance is preferable to the simple Euclidian distances, as driving time better reflects the topography at state borders (in particular, mountains and rivers).

The assumptions for the regression discontinuity to identify the effect of license fees on evasion are similar to those discussed in Section 5.1. First, given that treatment assignment in a spatial RDD is non-random (see Lee and Lemieux, 2010), households must not sort conditional on license fees. Second, beyond license fees, no other relevant variable changes discontinuously at the border. The discussion as well as the evidence reported above suggest that these assumptions should be met. To further assess the validity of the identifying assumptions, we provide graphical evidence and run parametric and non-parametric placebo estimations that explore possible discontinuities in municipality characteristics (see the Online Appendix). The results from this analysis suggest that we are not far from an ideal situation with smoothly distributed municipality characteristics around the borders, in particular, the borders from main estimation sample. For both, the main and the full sample, we do not detect any discontinuities in, e.g., the enforcement rate or any other variables that turned out to be correlated with the evasion rate in the cross-sectional analysis (see Figure A.1, Table A.2 and the Online Appendix). Moreover, and in line with the results from above, we do not find evidence on systematic sorting into treatment. We are therefore confident that the identifying assumptions are fulfilled.

Graphical Evidence. Figure 3 illustrates the average differential in license fees at the borders of our main sample. Municipalities with a negative [positive] distance to the border are located on the low [high] fee side of the respective state borders. The dots in the figure indicate the average level of license fees in bins of 5 minutes driving distance.[23] The figure shows that, on average, license fees increase by roughly € 37 at the borders. Relative to the level at the low-fee side of the border this approximately corresponds to a 18 percent increase. The key question is now whether this differential is accompanied by a discontinuous increase in evasion rates.

Figures 3 and 4 about here.

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[22]More specifically, we either compute the shortest driving time from each municipality to the closest point at one of the four state borders from the main sample or one of the 12 state borders from the full sample.

[23]The 5 minutes bin size is supported by the F-test procedure proposed by Lee and Lemieux (2010, p.309).

Figure 4, which depicts the discontinuity in evasion, suggests that the answer to the question is yes. In line with the border differential in license fees, there is a significant jump in the evasion rate right at the border.[24] The fitted line from local linear regressions (with a bandwidth chosen according to Imbens and Kalyanaraman, 2012) suggests that, on average, the evasion rate increases by 5.2 percentage points at the state borders. Relative to the 18 percent border differential in license fees (see Figure 3 above), the observed discontinuity translates into a semi-elasticity of 0.29 – which is identical to the central estimate from the border notch analysis.

In addition to the discontinuity, the figure also reveals that there is quite some variation in the evasion rate between the municipalities on either side of the borders. Part of this variation can be explained by observable municipality characteristics (see Section 4). Other factors (e.g., heterogeneity in the intrinsic motivation to comply) remain unexplained and will be absorbed by the distance functions introduced below.

RDD Estimates. To estimate the effect of fees on evasion, we run the following two equations:

$$\log(\text{Fees}_i) = \alpha^f + \delta^f D_i + g_H^f(dist_i)D_i + g_L^f(dist_i)(1 - D_i) + \mathbf{B}_i\phi^f + \epsilon_i^f \tag{7}$$

and

$$\text{Evasion}_i = \alpha^e + \delta^e D_i + g_H^e(dist_i)D_i + g_L^e(dist_i)(1 - D_i) + \mathbf{B}_i\phi^e + \epsilon_i^e \,, \tag{8}$$

where the dummy D_i indicates if a municipality is on the high-fee side of a border, $\mathbf{B}_i$ captures border fixed effects, and $g^e(dist_i)$, $g^f(dist_i)$ are functions of $dist_i$, the driving distance to the nearest border. These functions, which are allowed to differ between the low- and the high-fee side of the border, take up any unobserved factors that vary with distance and potentially influence evasion (or the fees). We use local linear regressions to non-parametrically estimate these equations.[25]

Equations (7) and (8) deliver the average discontinuity in the fees, δ^f, and the average discontinuity in evasion rates, δ^e, at the border. In the spirit of an instrumental variable approach, δ^f captures the 'first-stage' variation induced by the border discontinuity and δ^e gives the 'reduced form' effect of the 'treatment', i.e., the effect from moving from the low- to the high-fee side of a border on the average

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[24]Note that the consistency between Figures 2 and 4 is not at all trivial. The former figure is based on the *spatial* location at the border, the RDD graph is based on distance in terms of *driving time* to the border.

[25]The Online Appendix presents results from regressions that parametrically estimate polynomial functions for $g^e(.)$ and $g^f(.)$.

evasion rate. By comparing the first-stage and the reduced form effect, we obtain the Wald estimator for the local average effect of license fees on evasion (Hahn et al., 2001):

$$\beta^{\text{RD}} = \delta^e / \delta^f. \tag{9}$$

Columns (1) and (2) in Table 5 report estimates for δ^f, δ^e and β^{RD} for the main sample. We set the bandwidth following the procedures proposed by Imbens and Kalyanaraman (2012) and Calonico et al. (2014), respectively. The results for the two bandwidths (51 and 27 minutes) corroborate the findings from Section 5.1. The estimates indicate a discontinuity in evasion rates of 4.4 and 5.4 percentage points, respectively. Together with the estimated jumps in the fee, we obtain highly significant Wald estimates of 0.28 and 0.34, respectively.[26] Hence, a one percent increase in the fee results in an approximately 0.3 percentage point increase in the evasion rate. These semi-elasticities are again remarkably close to those obtained from the border notch analysis (compare columns 1–3 in Table 4).

Table 5 about here.

It is worth noting that these estimates are very stable for a broad range of bandwidths. This point is illustrated in Figure 5, which plots the Wald estimates and the corresponding 95% confidence intervals for bandwidths ranging from 20 to 80 minutes around the state borders. The dashed, horizontal line indicates the estimate from column (1) in Table 5, which is based on a bandwidth of 51 minutes. When we increase the bandwidth, the precision of the estimates increases slightly. Moreover, we obtain almost identical point estimates for all bandwidths between 30 and 80 minutes driving distance around the border. In a further sensitivity test, we also considered parametric RDD estimations (see the Online Appendix). The parametric analysis yields Wald estimates in the range of 0.25 to 0.33, which again confirms our results from above.

Figure 5 about here.

Finally, we examine whether our results are specific to the main sample or whether the effect generalizes to all state borders. Columns (3) and (4) of Table 5 present the results from local linear regressions with bandwidths of 36 and 33 minutes, respectively (again chosen according to Imbens and

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[26]We also implemented Calonico et al. (2014)'s procedure for bias correction and robust inference. The estimates hardly change quantitatively and remain highly significant.

Kalyanaraman, 2012; Calonico et al., 2014). The estimates confirm a highly significant discontinuity in evasion rates at the state borders. As compared to the main sample, however, the Wald estimates are much larger. The estimates reach 0.62 and are somewhat less precisely estimated than those reported in columns (1) and (2). The larger variance in the full sample is also documented in parametric RDD estimations, which yield semi-elasticities between 0.38 and 0.73 (see the Online Appendix). Moreover, a bandwidth sensitivity analysis similar to the one presented in Figure 5 suggests that the large point estimates from columns (3) and (4) considerably shrink for slightly higher bandwidths.

Summing up, the results from the spatial RDD confirm the main finding from the border notch analysis: higher fees trigger more evasion. For the main sample, the point estimates from the different designs and specifications are all very close to a semi-elasticity of 0.3. For the full sample, the spatial RDD delivers the same qualitative result, but a larger and less precisely estimated effect size. Given the similarity in the results for the main and the full sample noted in Section 5.1, this gap might appear surprising. It is important to recall, however, that one cannot directly compare the RDD and the border notch analysis. In the latter, the location at the border is defined in geographic terms. The spatial RDD uses driving time to the nearest border. For the state borders defined along the Alps – which are excluded in the main but included in the full sample – these two measures differ quite a bit and seem to drive the difference in the results.

Placebo Borders. The spatial RDD provides consistent evidence on a positive effect of fees on evasion. One might nevertheless wonder whether it is by chance that we observe a discontinuity in evasion rates at state borders. To address this concern, we present a placebo test that studies discontinuities at virtual, randomly generated borders. To do so, we first consider virtual borders that resemble those from our main sample. As illustrated in Figure 1, these state borders run predominantly from the north to the south. In a simple approach to mimic this north-south stretch, we introduce random borders along longitudinal lines. In particular, we randomly draw three longitudes (in the range $[10.5°, 11.5°E]$, $[13.5°, 14.5°E]$, and $[15°, 16°E]$) that split the states from our main sample roughly in the middle. Municipalities are then assigned to 'random states' depending on wether their midpoints are to the east or the west of these longitudes. In addition, we randomly assign the high-fee dummy D_i to the resulting states. We then iterate this process, compute the distance of each municipality to

the closest of the randomly drawn borders, and estimate (analogously to equation (8)) whether there is any discontinuity δ^e in evasion at these virtual borders.[27]

As it is computationally very time consuming to repeatedly derive our main distance variable (i.e., the minimum driving distance), we focus on simple Euclidean distances from each municipality to the closest border. This raises the question whether our results are robust to using distance as the crow flies rather than the driving distance. To answer this question, we replicated the spatial RDD analysis using the alternative distance measure. The results demonstrate that our results are robust to using the Euclidean distance (see the Online Appendix, Table III.5).

The distribution of the results from 1000 iterations of estimating border discontinuities in evasion, δ^e, for the randomly generated borders are presented in Figure 6. It illustrates the c.d.f. for estimates from local linear regressions (for a bandwidth which is determined, separately for each random border draw, according to Imbens and Kalyanaraman, 2012). The figure shows that all estimates for the virtual borders are below the evasion discontinuities from the 'true' borders (indicated by the dashed red line). In fact, the average placebo estimate exactly coincides with zero. Hence, the results strongly reject the idea that we observe discontinuities in evasion at the true borders by chance.

Figure 6 about here.

To assess whether the outcome from this placebo exercise is sensitive to the details of the implementation, we tested a broad set of alternative approaches. We considered both parametric and non-parametric estimates, varied the specification and estimation samples, used more than three borders (drawn from different ranges of longitudes), latitudinal borders, as well as a mixture of latitudinal and longitudinal borders. For all these approaches we obtain distributions that are similar to the one presented in Figure 6.

6 Conclusions

Based on unique cross-sectional data that offer a proxy for the evasion of TV license fees in all 2,380 Austrian municipalities, we study the effect of higher fees on evasion. While the collection and enforcement of license fees is harmonized at the federal level, the total fee due includes federal and

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[27]In principle, one could also derive a Wald estimator, δ^e/δ^f. However, as license fees are constant within states, we would obtain estimates for δ^f that are very close to zero. Despite small levels of δ^e, one would mechanically produce Wald estimators with a large variance in absolute terms. We therefore focus on δ^e, the 'reduced form' effect.

state taxes. Variation in the state taxes creates border differentials in fees. Exploiting these border discontinuities, we identify a robust, positive effect of fees on evasion. Our preferred estimate suggests that a one percent higher fee increases the evasion rate by 0.3 percentage points.

The effect captures a large extensive margin response which is consistent with the few studies that identify large (intensive margin) evasion responses (e.g. Fisman and Wei, 2004; Gorodnichenko et al., 2009). Based on our central estimate one can also derive the revenue maximizing 'Laffer fee' which would be roughly twice the average fee observed in our data (see Berger et al., 2015). This observation renders our result different from those in Fisman and Wei (2004), who find such a large evasion response which imply that lower taxes (and, in their case, tariffs) could in fact increase revenues. From a more general point of view, our results strongly support the intuition that higher taxes trigger more evasion and that these evasion responses are quantitatively sizable.

Concerning the external validity of our findings, one should note that we analyze the binary choice to evade a fee. As highlighted by our theoretical framework, the way that economic incentives shape this choice resembles the familiar income tax evasion context. We therefore think that our result tells something generally, i.e., that evasion *does* respond to the potential gains from cheating. One might nevertheless argue that the evasion of license fees is specific, as the fees are associated with one specific public service. Such usage fees, however, are quite common and apply to a vast array of publicly provided goods and services which are not fully excludable (think of, e.g., free-riding on public transport). Moreover, many of these goods and services are at least partially funded through the general tax pool. Whether a more or less salient link between the payment of fees or taxes and the 'service in return' affects the responsiveness of evasion with respect to the level of the payment, remains an open question for future research.

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Tables and Figures

Table 1: Basic Summary Statistics

Variable	Mean	S.D.
Evasion Rate	0.045	0.077
Enforcement Rate	0.012	0.025
Annual Fees	238.122	19.916
Households (H_i)	1,521	5,802
Labor Income	30,496	3,274
Distance (minutes)	40.980	24.408

Notes: The table reports descriptive statistics for the evasion rate, annual license fees (nominal Euro values), the enforcement rate, and selected municipality characteristics (see Online Appendix). Number of observations: 2,380.

Table 2: Cross-Sectional Estimation

	Coefficients	Clustered SEs	Robust SEs
log(Fees)	0.129	[0.087]	[0.022]
Enforcement Rate	−0.273	[0.169]	[0.072]
log(Income)	−0.017	[0.034]	[0.028]
Self-Employed	0.215	[0.084]	[0.046]
Observations	2,378		
R^2	0.298		

Notes: Results from OLS regressions of equation (4). Additional control variables are included. The full estimation output is reported in the Online Appendix. Bootstrapped clustered standard errors (based on Cameron et al. (2008)'s Wild Cluster Bootstrap-t procedure; 2,000 replications) and robust standard errors are presented in parentheses.

Table 3: Austrian state borders

Border (low/high-fee)	Number of municipalities	Border-municip. groups	Variables with significant differences $p \leq 0.01$	$p \leq 0.05$	Included in main sample
Upper/Lower Austria	46	18	0	2	Yes
Upper Austria/Salzburg	39	14	0	2	Yes
Salzburg/Styria	20	6	1	2	Yes
Vorarlberg/Tyrol	9	3	1	1	Yes
Tyrol/Salzburg	28	12	1	5	No
Lower Austria/Burgenland	50	22	2	3	No
Upper Austria/Styria	27	7	3	7	No
Tyrol/Carinthia	17	5	3	7	No
Salzburg/Carinthia	17	5	3	8	No
Lower Austria/Styria	32	12	3	12	No
Burgenland/Styria	36	16	6	12	No
Carinthia/Styria	32	11	6	13	No

Notes: The table shows the number of municipalities and municipality groups at each border. It further displays the results from balancing tests, indicating the number of variables (out of 41) that show significant differences, i.e., an estimated ρ that is significant at the 1%- or, at least at the 5%-level, respectively.

Table 4: Border Notch Estimations

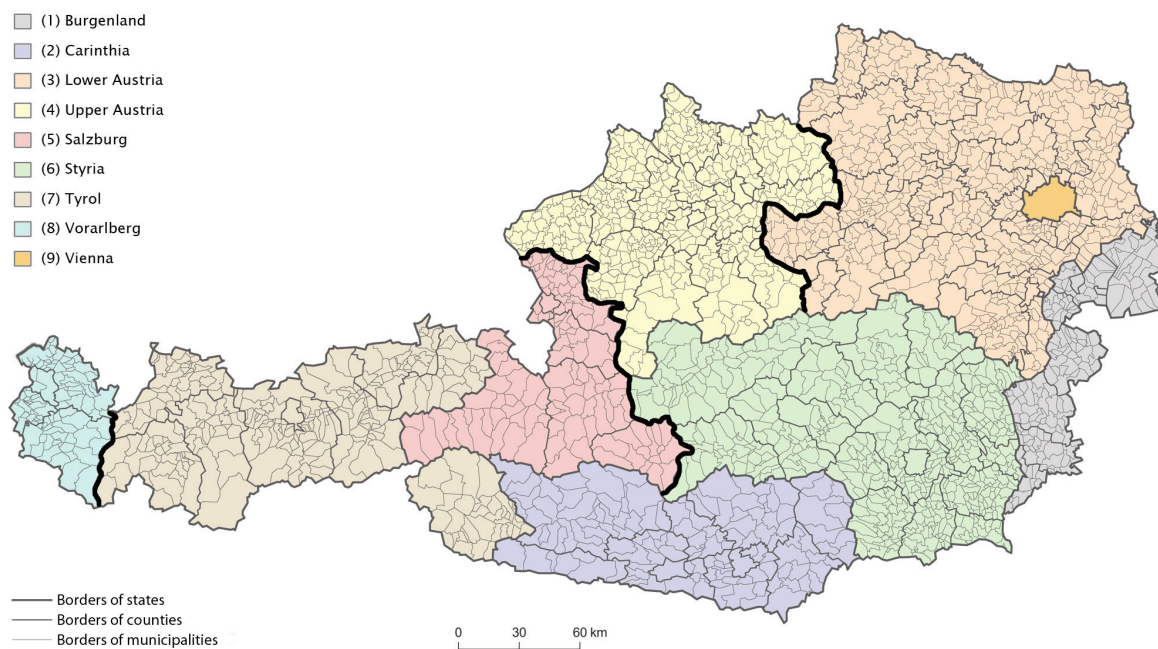| Sample	Main Sample			All Borders		
	(1)	(2)	(3)	(4)	(5)	(6)
log(Fees)	0.329***	0.363***	0.290**	0.276***	0.289***	0.278***
	[0.083]	[0.087]	[0.128]	[0.064]	[0.064]	[0.067]
Border FEs	Yes	No	No	Yes	No	No
Border-municip. group FEs	No	Yes (41)	Yes (41)	No	Yes (123)	Yes (123)
Control variables	No	No	Yes	No	No	Yes
Observations	113	113	113	342	342	342
R^2	0.146	0.422	0.752	0.110	0.400	0.551

Notes: Results from OLS regressions of equation (6). The sample in columns (1)–(3) includes all municipalities located at the borders of the main sample (see Section 5.1). Columns (4)–(6) includes all bordering municipalities from the full sample. Robust standard errors are in parentheses. ***, **indicates significance at the 1%, 5%-level, respectively.

Table 5: Local Linear Regressions

| | (1) | (2) | (3) | (4) |
	Main sample		Full sample	
Discontinuity in Evasion Rate	0.044	0.054	0.060	0.063
	[0.012]	[0.016]	[0.012]	[0.013]
Discontinuity in log(Fees)	0.156	0.160	0.099	0.100
	[0.001]	[0.001]	[0.006]	[0.006]
Wald Estimator	0.283	0.336	0.611	0.625
	[0.074]	[0.101]	[0.131]	[0.138]
Bandwidth (in minutes)	51.44	26.79	35.59	33.04
Observations	1,133	1,133	2,277	2,277

Notes: Estimates from local linear regressions using a triangle kernel. Columns (1) and (2) consider the main border sample, columns (3) and (4) the full sample. In columns (1) and (3), the bandwidth choice follows Imbens and Kalyanaraman (2012). Columns (2) and (4) set the bandwidth according to Calonico et al. (2014). Regressions include border fixed-effects. Standard errors in parenthesis. All estimates are significant at the 1%-level.

Figure 1: Austrian State Borders

Notes: The state borders in bold indicate the 'most balanced' borders.

Figure 2: Evasion Rates at Borders
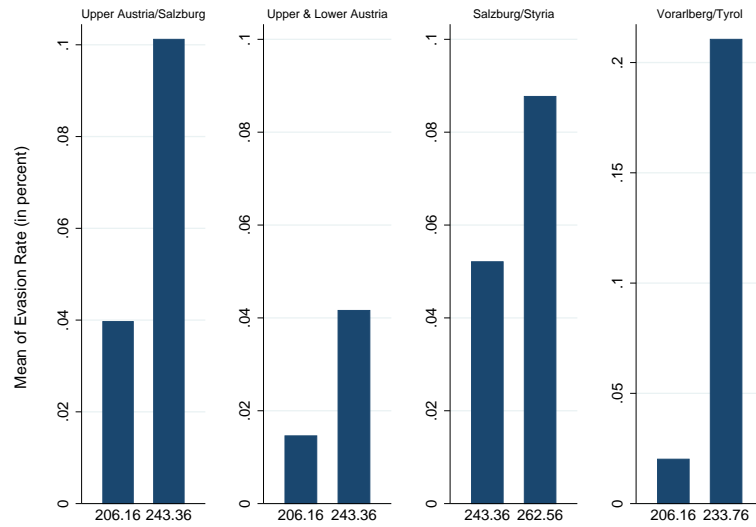
Notes: Average evasion rates among the bordering municipalities at the state borders in the main sample. The level of annual license fees (in nominal Euro values) is presented on the horizontal axis. The graph employs a different scale for the fourth border.

Figure 3: Discontinuity in Fees
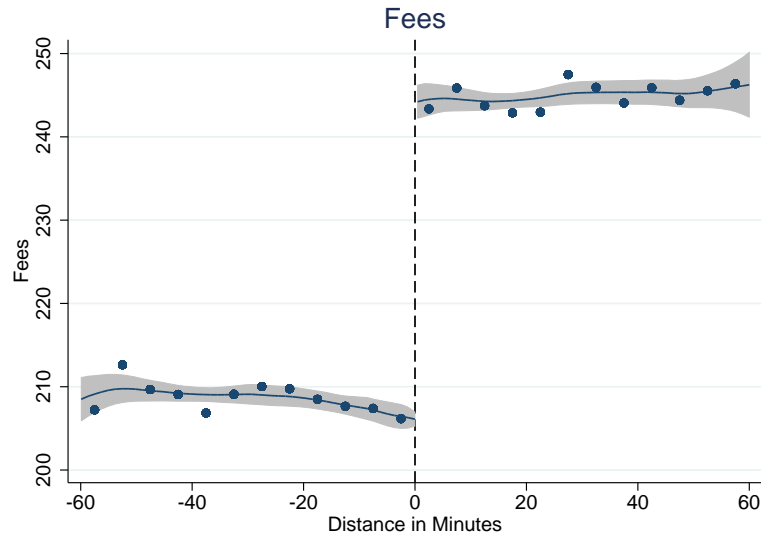
Notes: Yearly TV license fees for municipalities within a 60 minutes driving distance to the closest state border in the main sample ($N = 751$). The bin size is 5 minutes. Municipalities with a negative [positive] distance are located on the low [high] fee side of a border. Fitted lines from local linear regressions of equation (7) (excluding border fixed effects; bandwidth chosen according to Imbens and Kalyanaraman, 2012) together with the 95% confidence interval.

Figure 4: Discontinuity in Evasion Rates
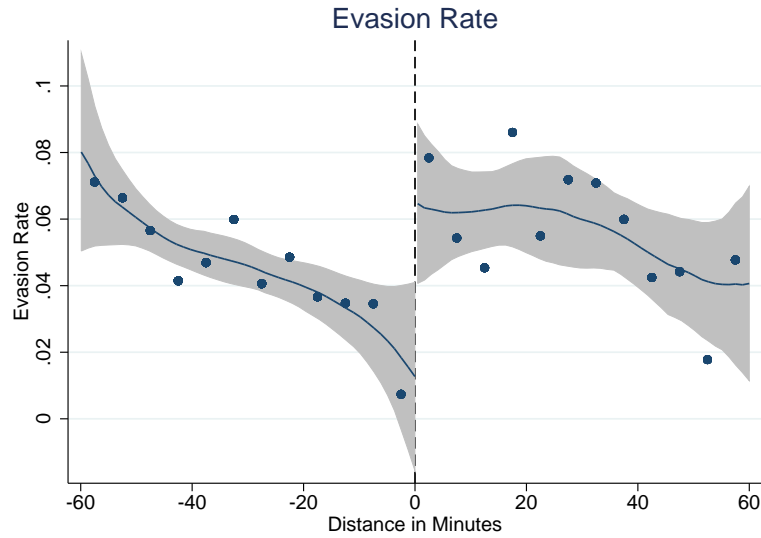

Notes: Evasion rates for municipalities within a 60 minutes driving distance to the closest state border in the main sample ($N = 751$). The bin size is 5 minutes. Municipalities with a negative [positive] distance are located on the low [high] fee side of a border. Fitted lines from local linear regressions of equation (8) (excluding border fixed-effects; bandwidth chosen according to Imbens and Kalyanaraman, 2012) together with the 95% confidence interval.

Figure 5: Local Linear Regression Outcomes for different Bandwidths
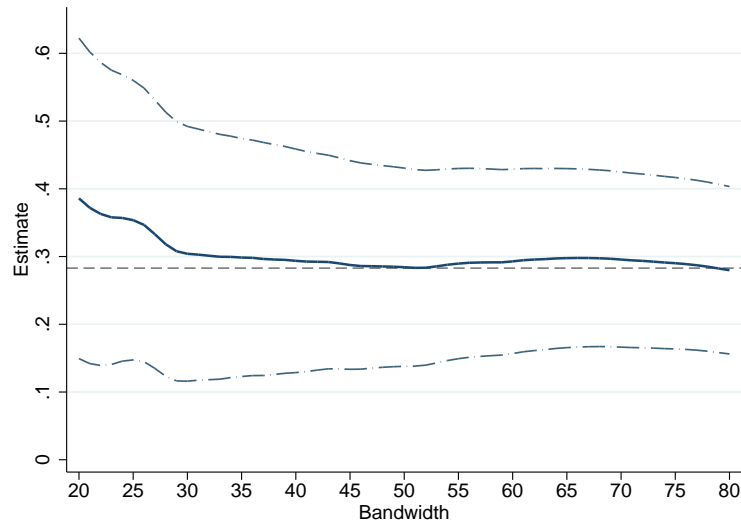
Notes: The figure plots Wald estimators and the corresponding 95% confidence intervals for local linear regressions, varying the bandwidth from 20 to 80 minuntes in one-minute steps. The dashed horizontal line illustrates the estimate from column (1), Table 5.

Figure 6: Placebo Tests for Discontinuity in Evasion Rate

Non-Parametric Estimates -- Main Sample
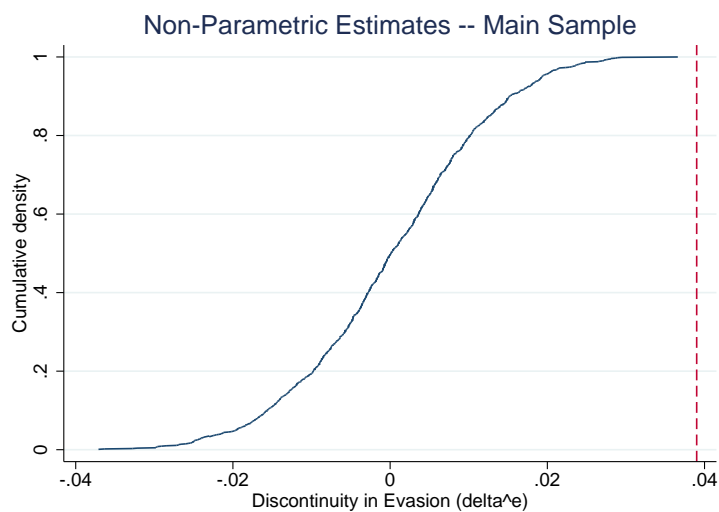
Notes: The figure plots the cumulative distribution function for 1000 estimated discontinuities in evasion rates (δ^e) at randomly generated borders. It presents the c.d.f. for non-parametric estimates, similar to those from column (1) in Table 5, where for each random border draw, the optimal bandwidth is chosen according to Imbens and Kalyanaraman (2012). All estimates are based on *Euclidian* rather than driving distances. The sample and number of observations is similar to our main sample. The dashed, vertical lines in the figure indicate the estimated discontinuity in evasion rates at the actual borders (using *Euclidian* distance).

Appendix

Figure A.1: Distribution of key municipality characteristics
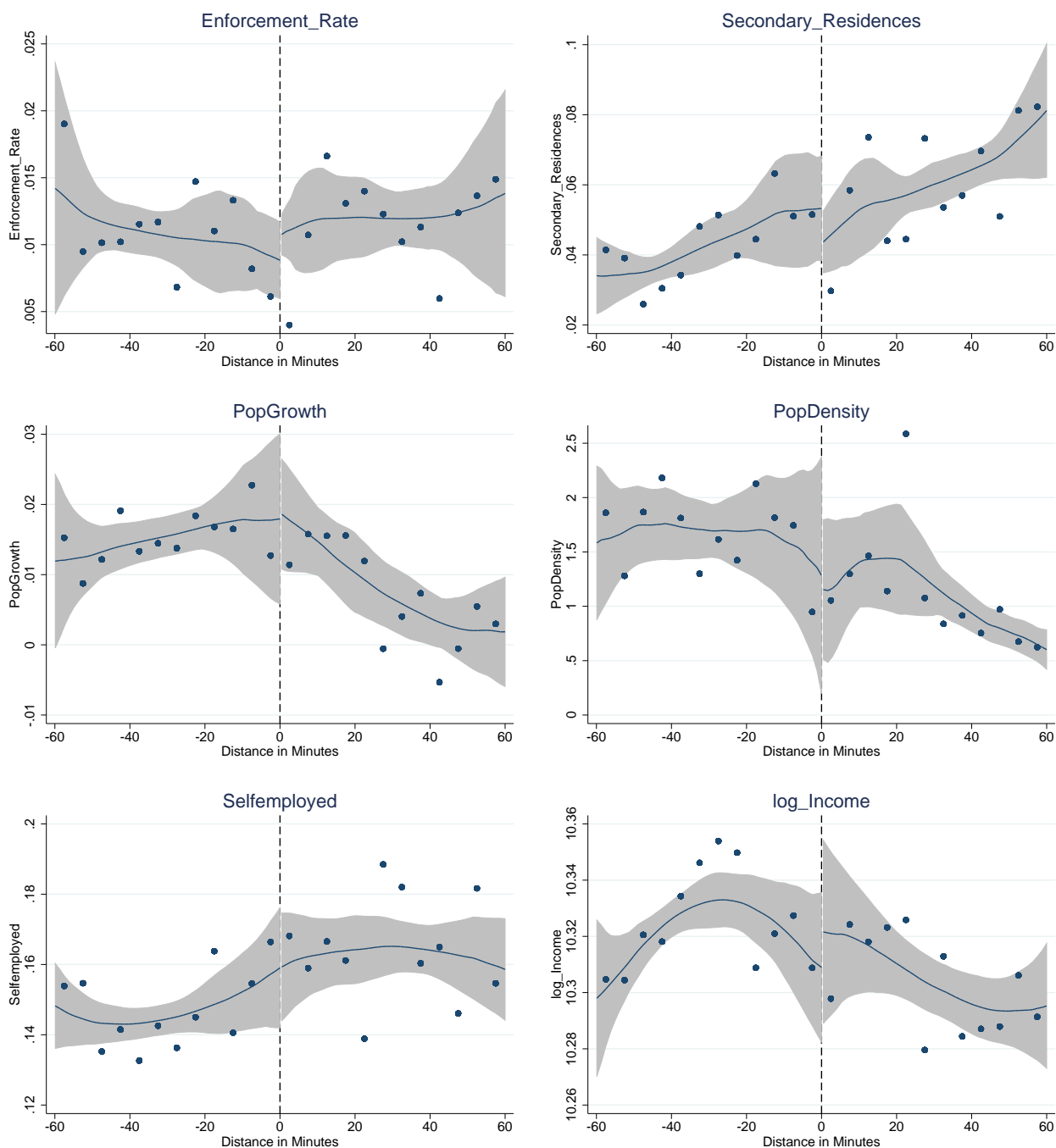
Notes: The figure explores possible border discontinuities for several key variables – in particular the enforcement rate, the rate of secondary residences (see Section 2), population growth and density as well as the rate of self-employed and average wage incomes. The figure illustrates the distribution of these variables among municipalities within a 60 minutes driving distance to the closest state border in the main sample. Municipalities with a negative [positive] distance are located on the low [high] fee side of a border. Bin size is 5 minutes. Fitted lines from local linear regressions (with a bandwidth chosen according to Imbens and Kalyanaraman, 2012) together with the 95% confidence interval.

Table A.1: Border-by-Border Balancing Tests

	(1) SOE	(2) NOE	(3) OST	(4) NST	(5) BST	(6) KST	(7) SST	(8) KS	(9) TK	(10) TS	(11) VT	(12) NB
Enforcement Rate	0.014	0.010*	−0.002	−0.005	0.004	−0.012**	−0.020	0.002	−0.025*	−0.005	−0.054***	−0.017***
	[0.010]	[0.005]	[0.003]	[0.004]	[0.004]	[0.005]	[0.016]	[0.009]	[0.012]	[0.013]	[0.008]	[0.006]
log(Income)	0.008	−0.026	0.006	−0.027	−0.017	0.011	−0.048***	0.027	0.018	0.006	−0.102	−0.025
	[0.023]	[0.024]	[0.022]	[0.020]	[0.019]	[0.021]	[0.008]	[0.022]	[0.032]	[0.025]	[0.086]	[0.019]
Self-Employed	0.002	0.026	0.003	0.006	0.029***	0.052**	0.013	0.052**	0.033	−0.032	0.013	0.021**
	[0.011]	[0.018]	[0.023]	[0.018]	[0.009]	[0.020]	[0.026]	[0.019]	[0.041]	[0.019]	[0.057]	[0.008]
Second Residences	−0.048*	0.020	−0.049	−0.084**	0.003	−0.016	0.009	−0.016	0.020**	0.007	−0.010	0.018
	[0.026]	[0.012]	[0.037]	[0.036]	[0.008]	[0.020]	[0.021]	[0.021]	[0.008]	[0.048]	[0.084]	[0.014]
log(PopSize)	0.241	−0.255	−0.231	−0.319	0.218	−1.195***	−0.191	0.019	0.464	−0.013	−0.269	0.081
	[0.224]	[0.234]	[0.343]	[0.242]	[0.221]	[0.217]	[0.392]	[0.327]	[0.286]	[0.285]	[0.824]	[0.176]
PopDensity	0.210	−1.032	−0.329	−0.286	−0.055	−0.083**	−0.082	−0.092	−0.191*	−0.014	−0.033	−0.481
	[0.188]	[0.687]	[0.416]	[0.201]	[0.208]	[0.036]	[0.088]	[0.054]	[0.100]	[0.058]	[0.040]	[0.355]
PopGrowth	0.010	−0.002	0.015*	0.001	−0.002	0.006	−0.004	−0.027**	−0.035*	−0.015	0.047	−0.003
	[0.010]	[0.007]	[0.008]	[0.014]	[0.011]	[0.008]	[0.013]	[0.010]	[0.020]	[0.009]	[0.068]	[0.010]
PopGrowthGross	−0.001	−0.016*	−0.002	−0.014	0.013*	0.004	−0.005	−0.011**	−0.000	−0.002	−0.009	−0.002
	[0.008]	[0.009]	[0.008]	[0.014]	[0.007]	[0.005]	[0.010]	[0.005]	[0.009]	[0.006]	[0.011]	[0.005]

Notes: The table reports balancing tests based on equation (5) for different state borders (SOE: Upper Austria/Salzburg; NOE: Upper/Lower Austria; OST: Upper Austria/Styria; NST: Lower Austria/Styria; BST: Burgenland/Styria; KST: Carinthia/Styria; SST: Salzburg/Styria; KS: Salzburg/Carinthia; TK: Tyrol/Carinthia; TS: Tyrol/Salzburg; VT: Vorarlberg/Tyrol; NB: Lower Austria/Burgenland). The *Enforcement Rate* measures the ratio of enforced registrations to the total number of households in a municipality; The *log(Income)* is based on average incomes from salaries and wages; *Self-Employed* captures the share of Self-Employed relative to a municipality's total population; *Second Residences* gives the number of all secondary and holiday residences relative to all residences; *PopGrowth* is the percentage increase in the population between 2001 and 2005; *PopGrowthGross* is the gross population growth (in percentage), based on the number of persons moving into a municipality from outside. Robust standard errors in parenthesis. ***,**,*indicates significance at the 1%, 5%, 10%-level, respectively.

Table A.2: Tests for Discontinuities in Municipality Characteristics

	Main Sample			Full Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
	30min	40min	50min	30min	40min	50min
Enforcement Rate	−0.000	0.001	0.002	−0.003	−0.001	−0.001
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.002]
log(Income)	0.007	0.013	0.007	−0.016	−0.006	−0.008
	[0.024]	[0.020]	[0.018]	[0.016]	[0.014]	[0.012]
Self-Employed	−0.001	−0.005	−0.002	0.016*	0.013	0.013*
	[0.018]	[0.014]	[0.012]	[0.010]	[0.008]	[0.007]
Second Residences	−0.013	−0.010	−0.009	−0.011	−0.011	−0.015*
	[0.012]	[0.012]	[0.012]	[0.012]	[0.010]	[0.009]
log(PopSize)	0.203	0.184	0.092	0.233	0.208*	0.163
	[0.234]	[0.198]	[0.175]	[0.145]	[0.120]	[0.104]
PopDensity	−0.239	−0.171	−0.014	−0.546*	−0.209	0.086
	[0.553]	[0.473]	[0.440]	[0.298]	[0.249]	[0.232]
PopGrowth	0.002	0.001	0.001	0.001	0.004	0.005
	[0.009]	[0.008]	[0.007]	[0.006]	[0.005]	[0.004]
PopGrowthGross	−0.011	−0.011*	−0.011**	−0.001	−0.001	−0.002
	[0.007]	[0.006]	[0.005]	[0.004]	[0.003]	[0.003]

Notes: The table reports local linear regression estimates for different bandwidths and samples (standard errors in parentheses). The *Enforcement Rate* measures the ratio of enforced registrations to the total number of households in a municipality; The *log(Income)* is based on average incomes from salaries and wages; *Self-Employed* captures the share of Self-Employed relative to a municipality's total population; *Second Residences* gives the number of all secondary and holiday residences relative to all residences; *PopGrowth* is the percentage increase in the population between 2001 and 2005; *PopGrowthGross* is the gross population growth (in percentage), based on the number of persons moving into a municipality from outside. Robust standard errors in parenthesis. **,* indicates significance at the 5%, 10%-level, respectively.