Do fiscal transfers alleviate business tax competition? Evidence from Germany

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A B S T R A C T

According to theory, capacity equalization grants cause local governments to internalize the effects of their tax policies on revenues of neighboring jurisdictions and so raise equilibrium tax rates. This paper empirically analyzes the incentive effects of equalizing transfers on business tax policy by exploiting a natural experiment in the state of Lower Saxony which changed its equalization formula as of 1999. We resort to within-state and across-state difference-in-difference estimates to identify the reform effect on municipalities’ business tax rates. Confirming the theoretical prediction, the reform had a significant impact on the municipalities’ tax policy in the 4 years after the reform with the effect stabilizing in the fourth to fifth years. The finding is robust to various alternative specifications.

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1. Introduction

It is a familiar shibboleth among public finance economists that decentralization of business taxation to lower-level governments can give rise to undesirable competition for mobile tax bases, and a “race to the bottom” in tax rates. Despite such concerns, a number of authors have recently observed that a system of intergovernmental transfers similar to those existing in many countries may in principle serve as a corrective device for local business tax competition, discouraging beggar-thy-neighbor tax policies and even under some conditions guaranteeing the efficiency of decentralized policies.

Under such transfer systems, known as capacity equalization or foundation grants, each government receives a transfer equal to the difference between a benchmark level of spending and the revenues it is deemed able to raise from its own tax bases at standard tax rates. Thus a capacity equalization grant is an equity-enhancing device that insures that each jurisdiction can achieve some target level of spending determined by federal authorities. As Smart (1998) and Koethenbuerger (2002) observed, however, an increase in local tax rates causes measured tax bases to decline, as taxpayers shift to other regions of the country or to other, more lightly taxed activities—and so causes capacity equalization transfers to rise. Thus the grants in effect subsidize tax increases and penalize tax cuts by local governments, and the effect is larger the greater equalization rate (sometimes referred to as “taxback rate”) at which deficiencies in local fiscal capacity are compensated through the transfer formula.

In this paper, we look for empirical evidence of the effects of equalization grants on local tax policy using data on a large set of German municipalities. The German case is an especially interesting one to examine, since municipalities there levy a tax on resident businesses, known as Gewerbesteuer, at rates that average about 16% of incomes. Since interjurisdictional mobility and the pressures of tax competition in such a setting should in principle be high, the equalization grant system may play an important, if unintended, role in maintaining current rates of taxation.

Our empirical approach relies on differences in tax-setting incentives facing municipalities that qualify for the systems of “regular” and “supplementary” equalization transfers, and in particular the court-

1. This effect is clearest when considering a receiving region with a tax rate equal to the target tax rate at which capacity deficiencies are compensated: at this point, further increases in the rate will appear to create no deadweight loss to the region, as the increase in equalization transfers exactly compensates for marginal losses in private consumption. Thus equalization tends to drive tax rates above the target tax rate.
ordered reforms in eligibility for supplementary transfers that occurred in the state of Lower Saxony (Niedersachsen) in 1999. Regular equalization transfers are available to municipalities whose fiscal capacity falls below a target level, while supplementary transfers are targeted at municipalities with considerably lower than average fiscal capacity. About three-quarters of the 1022 municipalities in Lower Saxony receive one or both types of equalization transfers. The effect of the 1999 reform was to reduce the equalization rate facing municipalities eligible for supplementary transfers, while increasing the equalization rate for other, ineligible municipalities. The former group of municipalities is therefore hypothesized to levy lower tax rates than the latter one in response to the reform.

Since the equalization formula itself implicitly defines the sets of municipalities that are eligible and ineligible for supplementary grants, identification of incentive effects of equalization must address the inherent problems of self-selection. The reason is that a jurisdiction can to some extent influence its fiscal capacity and thus the program type it is eligible for. The corresponding equalization transfer eligibility and tax policy are then both endogenous. This may generate a bias with simple mean comparison estimates of a transfer reform on the tax setting of the eligible municipalities relative to the non-eligible ones. To avoid this bias, we address the problem of self-selection by applying switching regression and matching procedures in the empirical analysis.

Two sources of heterogeneity enable the identification of the reform effect on tax rates. First, we compare the change in tax rates following the 1999 reform of municipalities eligible for supplementary transfers to that of a control group. The construction of the latter group should pay attention to the problem of self-selection into supplementary transfer eligibility. Second, since municipal equalization schemes differ among German states (Länder), one can identify the effect of a transfer reform by comparing the change in tax policy between municipalities in the reforming and a non-reforming state. In our analysis, we estimate the reform effect on municipalities in Lower Saxony by a comparison with the 2056 municipalities in the state of Bavaria, which experienced no reform in the equalization rates over our 1994–2004 sample period.

We find a significant effect of the reform on local business tax rates. As hypothesized, tax rates in eligible municipalities fell gradually in the 4 years following the reform, relative to ineligible municipalities’ rates. The overall impact on the gap in business tax rates amounted to about 1% point, about 6% of their original level.

The magnitude of the effect appears to be roughly comparable to that estimated in the most closely related previous study, Buettner (2006). Buettner’s estimates imply that a 10% point increase in the marginal contribution rate of the transfer system—the parameter α in the theoretical model of Section 2—associates with an increase in local business tax rates of about 1.5% points. In our data, the share of tax base equalized decreased by 50% in the treatment group, relative to the control group, following the reform, implying a relative change in marginal contribution rates of about 6% and so an estimated impact of marginal contribution rates on taxes that is broadly similar to Buettner’s estimate.

In contrast, Smart’s (2007) estimates that a 50% increase in the inclusion rate of the Canadian equalization system are associated with an increase in affected tax rate of about 30% of base levels—much larger than the 6% increase estimated here. One explanation for this difference, consistent with the theory proposed by Smart (2007), is that the greater degree of horizontal tax competition among German municipalities tends to mitigate the tax-raising effect of equalization grants there.

Our estimated average treatment effect however masks important heterogeneity in response to the reform. According to our estimates, the magnitude of the estimated exogenous treatment effect is downward biased by about 83%. On the basis of the between-state difference in difference analysis, moreover, we conclude that the reform raised the average level of taxes in the reform state. These results are robust to alternative choices of the estimation procedures and the inclusion of alternative control variables.

The paper is organized as follows. Section 2 develops the theoretical hypotheses. Section 3 reviews the features of the German municipal equalization system and explains the reform effects from a theoretical perspective. The data are presented in Section 4, while Sections 5 and 6 summarize the empirical strategy and the results. The last section concludes with a summary of the major findings.

1.1. Previous literature

Empirical work on the incentive effects of equalization programs has evolved only recently. Baretto et al. (2002) provide evidence that the equalization system among German states implicitly taxes tax revenues allocated to states through revenue-sharing arrangements. States do not have explicit taxing powers. Thus, the effect of fiscal equalization on tax policy cannot be identified there-
in. Hayashi and Boardway (2001) report empirical results consistent with the idea that Canadian provinces conform in their tax rate setting to the tax rate of the province of Ontario which predomin-
antly determines the average provincial tax rate used to compute the standard fiscal capacity in the Canadian equalization formula. Smart (2007) extends their approach and finds a robust effect of equalization on the tax policies of grant-receiving gov-
ernments in Canada. Dahlby and Warren (2003) find a similar in-
centive effect for Australia. Buettner (2006) examines the combined effect of vertical (revenue-sharing) and horizontal (equalization) grants on the tax policies of municipalities in the German state of Baden-Württemberg, and finds significant responses. The identification of this effect however relies on year-to-year changes and within-state/year cross-sectional variation in the equalization rate, which may be subject to the self-selection problems just discussed. More precisely, in the German system of fiscal arrangements, municipalities are required to levy a tax on their business tax bases that is transferred to higher-level (county and state) governments, as well as receiving payments from or contributing to the equalization grant system. Formally, vertical revenue sharing has an effect that is analogous to horizontal equalization, to the extent that county-level taxes are “passed on” to firms by municipalities. Buettner (2006) estimates the combined effect of the two. In contrast, our paper looks at the effect of horizontal equalization alone. As well, Buettner’s approach to identifying the tax-raising effect relies on variation in county-level tax rates, which may be correlated with other determinants of tax rates of municipalities within the county. In contrast, our estimates are identified solely from the asymmetric changes in equalization rates induced by a state-wide policy reform, which entails a natural experiment.

The problem of self-selection has not yet surfaced at the heart of existing research on the incentive effects of equalization programs. Furthermore, previous work has not exploited information on a large-scale reform of the equalization system in one state of a federal union to identify the effect on tax rates. Only the latter enables an identification of the reform-induced effects on different groups of municipalities according to their transfer eligibility status. The reason is that, with the eligibility status being endogenous, intra-
state variation in the outcome variable is required to identify the differential impact on treatment and control in the reform state. Lower Saxony, while inter-state variation is required to identify the average effect in the reform state (across-state difference in difference analysis).

2. Equalization and tax competition

To understand the incentive effects of equalization grants, consider a version of the model in Koethenbuerger (2002) and Bucovetsky and
A federation consists of two jurisdictions, labelled $i$ and $j$, each with a single resident and a single tax base. Jurisdiction $i$ levies tax rate $\tau_i$ on its own base $B_i$. Let the tax base in jurisdiction $i$ be a linear function of tax rates in the federation,

$$B_i = B_0 + c\tau_i - \alpha\tau_i$$  \hspace{1cm} (1)

where $B_0$ and $a > c > 0$ are parameters. Thus the model incorporates a fiscal spillover (e.g., tax competition where $B_i$ is the capital tax base) among jurisdictions when $c > 0$, since a rise in the tax rate in one jurisdiction causes a rise in the other jurisdiction’s tax base.

Each jurisdiction receives from the central government an equalization grant that compensates for differences in the size of the local tax bases. For jurisdiction $i$, the transfer formula is

$$T_i = \alpha_i(N_i - B_i),$$  \hspace{1cm} (2)

where $N_i$ is a parametric lump-sum component to the grant representing the jurisdiction’s deemed “fiscal need”, and $\alpha_i$ is the rate at which the grant is reduced for each unit of local fiscal capacity or tax base $B_i$. The (variables for jurisdiction $j$ are defined analogously.) Note that we allow marginal equalization parameters $\alpha_i$ to differ between jurisdictions, capturing the possibility that jurisdictions operate on different segments of a non-linear equalization scheme.

Consider the problem of a government in region $i$ that seeks to maximize the sum of tax revenue and federal equalization transfers, and which takes the parameters of the equalization formula and the tax rate of the other jurisdiction as given. In this model, the optimal tax rate $\tau_i$ solves

$$\max \tau_i B_i + \alpha_i(N_i - B_i)$$

for which the first-order (necessary and sufficient) condition is

$$B_i - a(\tau_i - \alpha_i) = 0.$$  \hspace{1cm} (3)

The first-order condition defines the optimal tax rate in region $i$ as a function of the neighbor region’s tax rate and its marginal equalization rate, $\tau_i(\tau, \alpha_i)$. Using Eqs. (1) and (3), we can show that $\tau_i$ is increasing in region $i$’s marginal equalization rate $\alpha_i$.

In the empirical analysis below, we examine the impact of the equalization formula on the reduced form tax rates of affected governments, without regard for the structural interactions among tax rates as embodied in the reaction function $\tau_i(\tau, \alpha_i)$. To motivate this approach, therefore, we may solve for the Nash equilibrium tax rates of the jurisdictions that constitute a fixed point of the reaction functions. This can be interpreted as a reduced form relationship between tax rates and equalization parameters, $\tau_i(\alpha_i, \alpha_j)$. Our empirical work employs a “difference-in-difference” strategy that examines how the tax rates of jurisdictions change relative to each other in response to a change in their relative marginal rates of equalization. To see this relationship in the theoretical model, we may therefore compute the difference in equilibrium tax rates

$$\tau_i^* - \tau_j^* = \frac{B_0^* - B_j^*}{2a + c} + \frac{a}{2a + c} (\alpha_i - \alpha_j)\beta + \gamma(\alpha_i - \alpha_j).$$  \hspace{1cm} (5)

The tax differential is positively related to the differential in equalization rates pertaining to the respective jurisdictions.

### 3. Equalization transfers in Germany

Our analysis is of the municipal equalization scheme in Lower Saxony where a reform has been implemented as of 1999.

The core of the municipal transfer system is: (i) a system of “regular” equalization grants, which compensates for a fraction of the amount by which each municipality’s measured taxation capacity falls short of its targeted spending level or “fiscal need”, and (ii) a system of supplementary equalization grants, which establishes a floor level of spending in each municipality, and equalize 100% of deficiencies up to the floor. In algebraic terms, let $B_i$ denote the measured tax capacity in municipality $i$; that is, the revenues that would be available for local spending purposes if the jurisdiction were to levy a centrally determined reference tax rate on its measured tax bases. Let $N_i$ denote the “fiscal need” or target spending level of the municipality—again, a centrally determined parameter that depends only on the current population of the municipality. Let $\alpha$ be the rate at which capacity deficiencies are compensated under the regular equalization transfer. Finally, let $\beta N_i$ denote the spending floor below which deficiencies are fully compensated under the supplementary equalization transfer, where $\beta$ is a centrally (state-level) determined parameter that is common to all municipalities within the same state. The aggregate equalization transfer to the municipality may then be written:

$$T_i(B_i) = \alpha \max\{N_i - B_i, 0\} + \max\{\beta N_i - B_i - \alpha(N_i - B_i), 0\}$$

where the first term corresponds to the regular equalization transfer and the second to the supplementary transfer (which itself depends on the fiscal capacity resulting from the regular transfer). Observe that the second term in the transfer formula, the supplementary equalization component, is positive if and only if $(\beta - \alpha)N_i - (1 - \alpha)B_i > 0$ or

$$B_i - \frac{\beta - \alpha}{1 - \alpha} N_i \equiv \theta N_i.$$  \hspace{1cm} (7)

The municipal transfer system also comprises transfers to the county government. Transfers are a fraction (county sharing rate) of the standardized fiscal capacity of its member municipalities, where the sharing rate is chosen by county governments. County revenue sharing is equivalent to a system of “piggybacking” county-level taxes on the municipal Gewerbesteuer, and so should exert an effect on the combined tax rate that is analogous to the incentive effect of horizontal equalization that we are studying. In our analysis, we examine the effects on tax rates of changes in the horizontal transfer system alone. In contrast, Buettner (2006) examines the combined effect of the horizontal and vertical components; see above, Section 1.1, for a discussion.

The fiscal capacity is the jurisdiction’s tax base multiplied by a “standard” tax rate. In the sequel we assume a “standard” tax rate equal to unity. In practice, it depends on the lagged average of tax rates actually levied by municipalities. To circumvent problems of endogeneity in our empirical analysis, we define the reform as the exogenous component of the change in the price effect—the legislated change in equalization rates.

In the transfer system described by Eq. (6), equalization is on a “gross” basis: each transfer is positive if the corresponding deficiency is positive, and zero otherwise; municipalities with capacity in excess of fiscal need are not taxed under the formula. In fact, in the 1999 Lower Saxony reform discussed below, municipalities with excess capacity were required to pay 20 per cent of the difference to the state government, converting the actual formula to a partial “net” equalization basis. We return to this issue below.
where $\theta < 1$. Thus $\theta$ in Eq. (7) expresses the fraction of target spending below which capacity deficiencies are fully equalized.

This describes the state's horizontal equalization system in general terms throughout our sample period. We turn now to the numerical values assumed by the parameters in the formula and the changes in the parameters following the 1999 reform—which is the key to our identification strategy. The reform was initiated by a ruling of the state supreme court in November 1997 which declared the initial system unconstitutional and requested the implementation of a new system as of 1999. The reform prescribed changes in the different equalization rates. Prior to the reform, the regular equalization transfer compensated 50% of deficiencies in capacity below the target level, and the spending floor was established at 80% of the target level; thus $\alpha^0 = 1/2$ and $\beta^0 = 4/5$ in the pre-reform period, and $\theta^0 = (4/5 - 1/2)/(1/2) = 3/5$ was the threshold fraction of the target below which supplementary equalization was paid. In the 1999 reform, the regular equalization was increased to 75%, while other parameters of the formula remained unchanged; thus $\alpha^1 = 3/4$, $\beta^1 = 4/5$, and $\theta^1 = (4/5 - 3/4)/(1/4) = 1/5$. As we will see, this resulted in substantial changes in municipal government incentives.

To understand the incentives for local tax policy induced by the transfer system, it is useful to consider the graph of the equalization formula. Fig. 1 expresses the relationship between a municipality's own fiscal capacity $B_i$ and its equalization transfers $T_i(B_i)$ in both the pre-reform and post-reform periods. The kinked line segment ADNG is the constraint which obtains in the pre-reform period: capacity deficiencies are fully compensated by transfers when $B_i \leq \theta^0 N_0$, so the constraint has slope $-1$ in this interval; 50% of capacity deficiencies are compensated when $\theta^0 N_0 < B_i \leq N_0$, so the slope of the constraint is $-0.5$ in this interval; and no equalization transfers are paid when $B_i > N_0$, the slope of the constraint is thus zero to the right of $N_0$. The post-reform budget constraint is represented by the kinked line segment ACNH. The effect of the reform was to increase the fraction of capacity deficiencies compensated by regular equalization transfers to 75% and so to increase the slope of the constraint by 0.25 (in absolute value) in the intermediate interval, while reducing the threshold at which supplementary equalization was paid commensurately to $\theta^1 N_0$. For governments with tax capacity in excess of need, operating on segment NG, no equalization payments were received before or after the reform. In the post-reform period, however, such municipalities were required to pay 20% of excess tax capacity to the state government, operating now on the segment NH with slope $-0.2$. Such a payment operates exactly like a negative equalization grant with an equalization fraction of one-fifth.

Thus the reform resulted in a rather stark change in the extent to which marginal changes in local resources $B_i$ are compensated through the formula. Municipalities may be classified into three groups based on their equalization status prior to the reform. Group 1, corresponding to segment CD of the pre-reform budget constraint, faced a decrease in equalization fraction of 25 percentage points following the reform, while Groups 2 and 3, corresponding to segments DN and NG, faced increases in the equalization fraction of 25 and 20 percentage points, respectively. According to our theory then, tax rates among the former group of governments are predicted to fall, compared to those of the other two groups.

It is this shock to incentives that is the key to our identification strategy, described in further detail below. An obvious concern with this approach is that factors that determine the initial equalization status of a municipality may be related to the unobservable determinants of subsequent innovations in tax rates, so that assignment to supplementary transfer treatment and control groups is not ignorable in our analysis. We describe our strategy for dealing with this endogeneity issue in Section 5.

Before turning to data, we will briefly describe further elements of the 1999 reform, unrelated to the change in the equalization rate. The 1999 reform involved changes in the definition of fiscal needs for the equalization system—the parameter $N_i$ in Eq. (6). In particular, fiscal need is defined as population size times a qualifier which is a non-linear, increasing function of population size times a per-capita grant (Grundbetrag). The reform entailed a different functional form of the qualifier; a change which is of relevance when assessing the income effect of the reform. We will turn to this issue in Section 6.4.

The reform also entailed the abolition of the state-wide unification levy (Einheitsumlage) under which municipalities had been required to contribute in proportion to their fiscal capacity under the local business tax. Abolition of the levy is therefore predicted also to have reduced business income taxes, but the effect was common to all municipalities, unrelated to the change in relative taxing incentives identified in our empirical analysis. As well, municipalities in all German states previously could levy a tax on the stock of capital employed locally (known as Gewerbekapitalsteuer). The local capital tax was abolished nation-wide as of 1998 and municipalities received a share in value-added taxes (VAT) to compensate for the loss in revenues. As is the case for the abolition of the unification levy, the change affected all municipalities in Lower Saxony alike, and our empirical strategy should be robust to its effects. In the main part of our empirical analysis we do not account for the two aforementioned changes, but return to them in our sensitivity analysis in Section 6.4.

4. Data

We use data on municipalities in the state of Lower Saxony over the time period 1994–2004. The described reform in Lower Saxony was effective as of 1999. We use data on the business tax rate which is set at the municipal level in Germany, information on the transfer formula as given in Eq. (1) before and after the reform for each municipality in Lower Saxony, and socio-economic characteristics of the respective municipalities such as population (inhabitants, age 10 Prior to 1999 municipalities transferred roughly 170 million Euro per year to the state government. To put it into perspective, this amounts to approximately 17% of equalizing transfers received by municipalities in these years.
11 Municipalities in Lower Saxony lost approximately 230 million Euro per year in tax revenues, whereas the compensation in VAT revenues amounted to roughly 225 million Euro per year (which is the average payment received over the years 1998–2004).

For governments initially operating on segment AC of the pre-reform constraint, there was no change in marginal incentives. Observe however that the new threshold level was extremely low, at 20% of the target spending level. In consequence, only one municipality has qualified for supplementary transfers in the post-reform period. We categorize this municipality as part of the treatment group 1 despite the fact that there was no change in marginal incentives; this can only lead to attenuation bias in our results.
structure, and population density), income per capita, and the unemployment rate. Also, we account for geographical characteristics of a municipality (land used for agriculture, forests, water sheds, and size of the road network each of which is measured in hectares). Finally, we employ political characteristics such as the party composition of the local government (social democrats, liberals, conservatives, and the greens). All data except unemployment rates are available from the respective statistical office (Statistisches Landesamt), most of it is available in an on-line data base.\footnote{The link is \url{http://www1.nhs.niedersachsen.de/Statistik/}.} Data on the number of unemployed at the municipal level are taken from the Federal Labor Office (Bundesagentur für Arbeit)—also available online.\footnote{The link is \url{http://www.pub.arbeitsamt.de}.}

In the subsequent analysis, we consider all municipalities in Lower Saxony as supplementary transfer eligible if they have actually received such transfers in at least 1 year in the pre-reform period (1994–1998). In all estimations, the remaining Lower Saxony municipalities belong to the control group. Table 1 provides details on the number of eligible and non-eligible municipalities, the average business tax rate, the average fiscal capacity per capita as defined by the equalization formula, and the average income per capita across years and municipalities in the respective group.\footnote{The income per capita seems low, but note that the denominator includes all inhabitants of a municipality, irrespective of whether they are working, unemployed, or not in the labor force at all.}

<table>
<thead>
<tr>
<th>Number of municipalities</th>
<th>Eligible</th>
<th>Non-eligible</th>
<th>All</th>
</tr>
</thead>
<tbody>
<tr>
<td>Business tax rate in percent (mean)</td>
<td>15.51</td>
<td>15.63</td>
<td>15.54</td>
</tr>
<tr>
<td>Fiscal capacity per capita in Euros (mean)</td>
<td>333.54</td>
<td>466.59</td>
<td>369.21</td>
</tr>
<tr>
<td>Income per capita in Euros per annum (mean)</td>
<td>9169</td>
<td>10698</td>
<td>9579</td>
</tr>
</tbody>
</table>

Descriptive statistics Lower Saxony.

In Fig. 2, the gap opens up in 1999 (hence, there is no indication of anticipation effects in 1998), reaching a level of about — 0.25 percentage points from 2003 onwards. The regression representation will be useful for a comparison with the results in the subsequent analysis, where we account for a possible self-selection into supplementary transfer eligibility. The latter seems particularly important for municipalities operating next to the kinks of the budget constraint depicted in Fig. 1. For these, a small change in tax policy may significantly change the local slope of the non-linear budget constraint.

5. Empirical strategy and results

It will be useful to start with a few definitions for portraying the problem of identification of the supplementary transfer reform effect on business tax rates at the municipal level and the outline of the empirical strategy. For convenience, we will refer to the case of supplementary transfer recipient status by index 1 (treatment status) and to the non-recipient status by index 0 (no-treatment status). The corresponding business tax rate of municipality \( i \) with and without treatment status is \( \tau_{i,1} \) and \( \tau_{i,0} \), respectively. Since we will focus on differences in differences of the reform effect, let us define the pre-to-post-reform change in business tax rates of the supplementary transfer eligible units as \( \Delta \tau_{1,i} \), and that of the ineligible ones as \( \Delta \tau_{0,i} \). The empirical strategy will be useful for a comparison with the natural experiment as in Table 1. Before the reform, there was not much change in this relationship. But the figure suggests that the gap in the two tax rates increased immediately after the reform year. The adjustment in the gap is somewhat sluggish and a new ‘steady-state’ in the gap seems to be reached only after 4 years.

To give a first impression of the possible response of tax rates to the reform, we illustrate the development of the business tax rates in Lower Saxony in Table 2. The average business tax rate is significantly lower in eligible municipalities than in non-eligible ones. This is consistent with the income given in Table 1. Before the reform, there was not much change in this relationship. But the figure suggests that the gap in the two tax rates increased immediately after the reform year. The adjustment in the gap is somewhat sluggish and a new ‘steady-state’ in the gap seems to be reached only after 4 years.

The information in Table 2 may also be inferred from a descriptive comparison along the lines of regression analysis. In Table 2 we consider the average business tax rate in the period 1994–1997 in eligible versus non-eligible municipalities and compare it to any later year, capturing supplementary transfer eligibility by a dummy variable that is set at one for eligible municipalities and at zero for non-eligible ones. Of course, since 1998 was actually not covered by the reform the change in the eligible regions should be as large as in the non-eligible ones so that the gap does not increase. Hence, the supplementary transfer reform effect for a randomly drawn municipality; the expected treatment effect on a municipality is influenced by a random vector \( \Delta \tau_{1,i} \) and that of the ineligible ones as \( \Delta \tau_{0,i} \). The corresponding business tax rate of municipality \( i \) with and without treatment status is \( \tau_{i,1} \) and \( \tau_{i,0} \), respectively. Since we will focus on differences in differences of the reform effect, let us define the pre-to-post-reform change in business tax rates of the supplementary transfer eligible units as \( \Delta \tau_{1,i} \), and that of the ineligible ones as \( \Delta \tau_{0,i} \). Therein, \( \Delta \) is the difference operator across periods.

Notice that there are two treatments of interest, here. First, the supplementary transfer reform effect—as a natural experiment—is exogenous from the viewpoint of a municipality in Lower Saxony. Second, supplementary transfer status may be partly influenced by a municipality and, hence, is endogenous. Let us refer to the binary supplementary transfer eligibility status, the change in business tax rates between the pre- and post-reform periods will depend on this endogenous selection.

Define the random vectors \( \Delta \tau_{1} \) and \( \Delta \tau_{0} \) from the population to indicate changes in business tax rates for the average municipality with and without supplementary transfer treatment, respectively. Furthermore, define the vector \( \Delta \) indicating treatment status for all municipalities. We will deliver estimates of two types of treatment effects: the constant treatment effect (ATE; see Wooldridge, 2002) of supplementary transfer eligibility is defined as the unconditional, expected change in business tax rates associated with the reform effect for a randomly drawn municipality; the average treatment effect on the treated (see Rosenbaum and Rubin, 1983) of supplementary transfer eligibility is the expected treatment effect on a municipality that is randomly drawn only from the sub-population of the actually supplementary transfer eligible units.

ATT and ATE can be estimated consistently in a simple regression as in Table 2 only if municipalities do not self-select into supplementary transfer eligibility status. However, if municipalities internalize their possible influence on the slope of the budget constraint and adjust their tax rates accordingly (referred to as self-selection into treatment), the simple difference-in-means estimates in Table 2 are biased and inconsistent. The micro-econometrics literature on program evaluation suggests several alternative avenues of how to recover consistent estimates of ATE and ATT under self-selection into treatment (see Wooldridge, 2002, for an overview). We make use of various available techniques to address the problem in the sequel.
6. Results

6.1. Regression in a sub-sample of municipalities without selectivity

A straightforward way of dealing with potential self-selection is to limit the econometric analysis to those municipalities for which self-selection is very unlikely. In our application, those are the municipalities which operate not too close to the kink $D$ of the transfer formula in Fig. 1. For this, we exclude all municipalities with a lower ratio of the critical-to-actual fiscal capacity ratio than the average transfer-receiving municipality from the group of supplementary transfer recipients (the treated). Moreover, we exclude all municipalities with a higher ratio than the average non-receiving municipality from the group of supplementary transfer non-recipients. After excluding these municipalities in each year, we are left with a sub-sample of 440 municipalities.

Table 3 summarizes the regression results based on this restricted sample, where the dependent variable is again the difference in tax rate from the 1994–1997 average for the municipality. Since the fiscal adjustment to the reform might be sluggish, we estimate the corresponding ATE for each post-reform year separately. Also, we check for possible anticipation effects by assigning 1998 to the post-reform period. Recall, the reform became effective as of 1999 after a supreme court ruling in November 1997. In the block at the top of the table, referred to as Estimates unconditional on other covariates, we report results that are fully comparable to the ones in Table 2. The only difference between the estimates in Tables 2 and 3 is that the ones in the former are obtained from all Lower Saxon municipalities while the ones in the latter are obtained from the sub-sample which excludes possibly self-selecting units. The estimates in Table 3 are slightly higher by the end of the sample period and slightly lower at the beginning than the ones in Table 2.

However, excluding municipalities around the supplementary transfer eligibility threshold may result in a sub-sample of observations, where the included treated municipalities are virtually incomparable to the included untreated ones due to differences in principally observable characteristics. To avoid that problem, we control for other covariates in the lower block of results in Table 3. In particular, apart from a constant, we include the following control variables there: change in population since 1993–1997; population density; streets (in hectares); change in per-capita income since 1993–1997; change in share of elderly people (age 65 and above).

It turns out that the supplementary transfer treatment effects in the bottom block of Table 3 are somewhat higher that the ones at the top. However, they are of a comparable magnitude. According to the findings at the bottom of the table, the average treated municipality changed its tax rate by about 0.28 percentage points less than the average untreated one by 2004 as compared to the average 1994–1997 level. This treatment effect is about 0.05 percentage points lower in absolute value than the one in Table 2. However, one concern that remains with the findings in Table 3 is that controlling for covariates in a linear fashion may not establish full comparability between the treated and the untreated municipalities. Accordingly, we apply alternative methods in the sequel which may account for that problem.

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Table 2
Descriptive comparison of change in business tax rates for supplementary transfer eligible municipalities versus non-eligible ones in Lower Saxony (reported estimates are with respect to changes as compared to 1994–1997 average levels).

<table>
<thead>
<tr>
<th>Year</th>
<th>Supplementary Transfer Eligible</th>
<th>Supplementary Transfer Non-Eligible</th>
</tr>
</thead>
<tbody>
<tr>
<td>1998</td>
<td>-0.026</td>
<td>0.042</td>
</tr>
<tr>
<td>1999</td>
<td>-0.116**</td>
<td>0.051</td>
</tr>
<tr>
<td>2000</td>
<td>-0.188***</td>
<td>0.057</td>
</tr>
<tr>
<td>2001</td>
<td>-0.184***</td>
<td>0.070</td>
</tr>
<tr>
<td>2002</td>
<td>-0.224***</td>
<td>0.078</td>
</tr>
<tr>
<td>2003</td>
<td>-0.258***</td>
<td>0.080</td>
</tr>
<tr>
<td>2004</td>
<td>-0.249***</td>
<td>0.080</td>
</tr>
</tbody>
</table>

Standard errors are robust to heteroskedasticity of unknown form and based on the correction by White. ** Significant at 1%. *** Significant at 5%.

---

15 Note that the critical fiscal capacity is centrally determined and, hence, not endogenous to a municipality.
6.2. Switching regression model

We first apply a switching regression model, which eliminates a possible bias from self-selection by controlling for a variable—referred to as the inverse Mills’ ratio—which captures the selectivity bias. A consistent two-step estimator of ATE can be obtained as follows. First, estimate the probability of being eligible for supplementary transfers depending on a set of instruments based on a non-linear probability model. Second, determine the expected value of business tax rates conditional on supplementary transfer eligibility, a set of exogenous variables in the second stage model and on the set of instruments in the first stage which are collected into the inverse Mills’ ratio (see Heckman, 1978). We allow for different inverse Mills’ ratios for the treated and the untreated observations, following Wooldridge (2002). The Mills’ ratios control for the selection bias, rendering the estimates of ATE as captured by $\beta$ consistent and asymptotically normal.

We assume that the set of instruments determining the probability of supplementary transfer eligibility consists of the following socio-economic and geographical characteristics: a municipality’s area of agricultural land in hectares (as a measure of its degree of industrialization); the area of forest space in hectares; the area of watersheds in hectares (as a measure of remoteness); the area of paved streets in hectares (as a measure of a municipality’s infrastructure endowment); per-capita GDP as of 1993 (i.e., in the pre-treatment period); population size as of 1993; the change in population density between the pre- and post-treatment periods; the change in share of elderly people (age>65) depending on a set of instruments based on a non-linear probability model helps determining the latter in a structural way. For instance, the probability of being treated as supplementary transfer eligible declines with the share of people below working age, and it declines in the pre-treatment level of per-capita income. The likelihood of being treated is higher for regions with large water sheds. The reason is that these regions are relatively remote. For both models, the pseudo R² is fairly high regarding the sample size of 1022 observations and the fairly large number of treated ones, amounting to about 0.26. The log-likelihood of the logit model is slightly higher than that one of the probit. However, the difference is not significant according to a likelihood ratio test (this test is suggested by Davidson and Mackinnon, 2004). In the subsequent analysis, we use the slightly preferable logit model according to the log-likelihood.

The second stage results of the switching regression approach are summarized in Table 5. The estimates apply for a randomly selected municipality in Lower Saxony, irrespective of whether it was eligible for supplementary transfers or not. We are primarily interested in the

---

Table 3

Average treatment effect of supplementary transfer eligibility on the change in business tax rates in Lower Saxony (reported estimates are with respect to changes as compared to 1994–1997 average levels).

<table>
<thead>
<tr>
<th>Explanatory variables</th>
<th>1998</th>
<th>1999</th>
<th>2000</th>
<th>2001</th>
<th>2002</th>
<th>2003</th>
<th>2004</th>
</tr>
</thead>
<tbody>
<tr>
<td>Supplementary transfer eligibility in Lower Saxony</td>
<td>0.033</td>
<td>−0.089</td>
<td>−0.127*</td>
<td>−0.109</td>
<td>−0.166*</td>
<td>−0.290***</td>
<td>−0.25***</td>
</tr>
<tr>
<td>Supplementary transfer eligibility in Lower Saxony</td>
<td>0.055</td>
<td>0.066</td>
<td>0.069</td>
<td>0.075</td>
<td>0.093</td>
<td>0.108</td>
<td>0.113</td>
</tr>
</tbody>
</table>

Notes: reported figures are treatment effect estimates of reform effects on taxes changes as compared to 1994–1997 average levels. Supplementary transfer eligibility in Lower Saxony is exogenous, but all municipalities except the 440 ones which are most distant to (to the right or the left of) the supplementary transfer threshold have been excluded.

* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

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17 Concretely, taking the difference of the equilibrium tax rate before and after the reform (i.e. for different values of $\alpha_i$ and $\epsilon_i$), the baseline components $B_i^r$ and $B_i^t$ drop out.

18 The vector of estimated probabilities is not only useful to avoid the problem of self-selection in one or the other way. It is also informative about the problem of self-selection as such. In Section 6.1, we excluded all those municipalities from the sample which were close to the supplementary transfer eligibility threshold according to a rule of thumb. Accordingly, the number of municipalities used in the regression was as small as 440. Hence, 582 municipalities were classified as potential switchers there. The non-linear probability model helps determining the latter in a structural way. For instance, we may classify the potential switchers as the sum of two types of municipalities: those that actually received supplementary transfers but had a probability of supplementary transfer eligibility of less than 50%; and those municipalities that did not receive supplementary transfers but had a probability of supplementary transfer eligibility of at least 50%. The total number of municipalities in the two groups together is 253. Hence, the assumed number of potential switchers in Section 6.1 was too high according to the estimated selection model.
estimates pertaining to the effect of supplementary transfer eligibility. The estimates correspond to the change in the tax rate gap between eligible and non-eligible municipalities—similar to the illustration in Fig. 2.

Similar to the exogenous ATE estimates in Tables 2 and 3, there is no indication of significant anticipation effects in 1998 with the endogenous treatment approach. However, the identified causal effect on tax rate changes until 2004 between the eligible and non-eligible municipalities is more than four times as large as the corresponding estimates in Tables 2 and 3. Recall that the reference estimate was about 0.25 percentage points in Table 2 and 0.26 in Table 3, whereas it amounts to 1.12 in the switching regression—see Table 5. Hence, the selection bias is not sufficiently overcome by excluding potential switchers as in Section 6.1. Moreover, the difference in the estimates between Tables 5 and 2 mainly reflects a self-selection bias rather than an omitted control variables bias in Table 2. We illustrate this issue in Fig. 3.

In the figure, the dashed line at the top corresponds to the exogenous simple comparison estimates of the average treatment effect in Table 2. The solid grey one in the middle reflects exogenous treatment effect estimates where the same covariates as in Table 4 have been controlled for. The solid black locus at the bottom of the figure is based on the estimates of the average treatment effect in Table 5.Obviously, the selection bias rather than the omitted variables bias accounts for the lion’s share in the difference between the endogenous treatment effect estimates of Table 5 and the exogenous ones in Table 2. Recall that the average level of business tax rates amounted to about 15.5% in the pre-reform period. Accordingly, we may conclude that the reform-induced change in the gap between the eligible and non-eligible communities amounts to almost 10% of this level. Note that the constant amounts to somewhat less than 2.4 in the 2004 results in Table 5. This is an estimate of the change in the average community’s business tax rate since 1994/1997. Hence, a randomly drawn community’s tax rate is increased by about 50% less due to supplementary transfer eligibility. As an ATE (rather than an ATT), the latter is unconditional on whether the community was actually eligible or not.

We have experimented with less parsimonious specifications than the one reported in Table 4. In particular, we have checked for the possible relevance of unemployment rates, and other socio-economic and geographical characteristics accounted for in the selection model. However, it turns out that the less parsimonious model estimates are not significantly preferable to the ones in the table. In particular, population density and the size of the road network are the most important control variables according to their significance.

6.3. Matching based on the propensity score

In the previous analysis, we have focused on ATE, i.e., the treatment effect of supplementary transfers unconditional on actual treatment status. In fact, this is the (group size) weighted average of ATT and the average treatment effect of the untreated (ATU). In a next step, we address the effect on the actually treated (ATT), by employing different matching estimates of the role of supplementary transfer eligibility for the tax reform effect in Lower Saxony.

Rosenbaum and Rubin (1983) deploy an estimate of the probability of treatment given observable covariates (e.g., the ones in Table 4). The estimated response probability of supplementary transfer eligibility—also referred to as the propensity score—then serves as a metric to determine similar observations among the sub-samples of the treated and untreated observations. With matching, the vector of predicted probabilities of being treated (as supplementary transfer eligible) is used to construct an appropriate control group of ineligible municipalities with a similar probability of being eligible as the actually treated ones. Hence, tax rate changes of the supplementary transfer eligible municipalities are compared to ones of a suitable control group rather than all untreated municipalities (see Rosenbaum and Rubin, 1983; Wooldridge, 2002, for a catalogue of assumptions).

Matching may obtain estimates of either ATT or ATU. The most prominent matching procedure is nearest neighbor (or one-to-one) matching. With ATT estimation, nearest neighbor matching compares tax rate changes of the vector of supplementary transfer eligible municipalities to their closest ‘twins’ in terms of the propensity score within the sub-sample of non-eligible municipalities. The propensity scores for matching are based on the logit estimates in Table 4 throughout. The ATT estimates are given in Table 6.

According to Tables 5 and 6, ATT is much lower than ATE by 2004. In that year, ATT on the tax rate change amounts to about 0.26 percentage points, while ATE amounts to 1.12 percentage points. This means that an untreated municipality would have set a considerably smaller business tax rate, had it been treated instead.

According to Tables 5 and 6, ATT is much lower than ATE by 2004. In that year, ATT on the tax rate change amounts to about 0.26 percentage points, while ATE amounts to 1.12 percentage points. This means that an untreated municipality would have set a considerably smaller business tax rate, had it been treated instead. Hence, ATU is much larger in absolute value than ATT. ATT is about as percentage points, while ATE amounts to 1.12 percentage points.

20 We resort to alternative matching procedures in the sensitivity analysis in Section 6.4.

19 ATU is the effect a supplementary non-recipient municipality would have experienced after the reform, had it received supplementary transfers.

Table 4

<table>
<thead>
<tr>
<th>Selection into supplementary transfer eligibility (Lower Saxony).</th>
</tr>
</thead>
<tbody>
<tr>
<td>Logit</td>
</tr>
<tr>
<td>---------------------------------------------------------------</td>
</tr>
<tr>
<td>Agricultural land as of 1993</td>
</tr>
<tr>
<td>Forest as of 1993</td>
</tr>
<tr>
<td>Water as of 1993</td>
</tr>
<tr>
<td>Streets as of 1993</td>
</tr>
<tr>
<td>Per-capita income as of 1993</td>
</tr>
<tr>
<td>Population size as of 1993</td>
</tr>
<tr>
<td>Population density as of 1993</td>
</tr>
<tr>
<td>Population share with age ≤ 15 as of 1993</td>
</tr>
<tr>
<td>Population share with age ≤ 15 as of 1993</td>
</tr>
<tr>
<td>Population share with age ≤ 15 as of 1993</td>
</tr>
<tr>
<td>Square of agricultural land as of 1993</td>
</tr>
<tr>
<td>Square of forest as of 1993</td>
</tr>
<tr>
<td>Square of water as of 1993</td>
</tr>
<tr>
<td>Square of streets as of 1993</td>
</tr>
<tr>
<td>Square of streets as of 1993</td>
</tr>
<tr>
<td>Square of per-capita income as of 1993</td>
</tr>
<tr>
<td>Square of population size as of 1993</td>
</tr>
<tr>
<td>Square of population density as of 1993</td>
</tr>
<tr>
<td>Square of population density as of 1993</td>
</tr>
<tr>
<td>Square of population density as of 1993</td>
</tr>
<tr>
<td>Square of population share with age ≤ 15 as of 1993</td>
</tr>
<tr>
<td>Square of population share with age ≤ 15 as of 1993</td>
</tr>
<tr>
<td>Constant</td>
</tr>
<tr>
<td>Pseudo R²</td>
</tr>
<tr>
<td>Log-likelihood</td>
</tr>
<tr>
<td>Joint exclusion of squares</td>
</tr>
<tr>
<td>Joint exclusion of squares</td>
</tr>
<tr>
<td>P-value</td>
</tr>
</tbody>
</table>
6.4. Sensitivity analysis

We considered the robustness of the treatment effect estimates in several regards. The findings are summarized in Tables 7 and 8. In the bloc of results at the top of Table 7, we address the robustness of the ATT estimates through matching. In the center and bottom blocs of results in Table 7, we focus on switching regression and instrumental variable regression estimates of ATE, respectively. Finally, in Table 8 we illustrate that the effects identified in our analysis are not primarily income effects but indeed tax price effects.

Let us start with discussing the robustness with regard to the use of alternative estimation procedures in Table 7. First, even though the balancing property is not violated in our application,21 one concern might be that the exogenous variables in the selection model have an impact on the change in business tax rates on their own. Then, they should be controlled for after matching (see Blundell and Costa Dias, 2002). This is done in the regressions whose ATT estimates are reported at the top of Table 7. The included covariates after matching are the same as in Table 4. Note that the results are very similar to those in Table 6. Hence, we can conclude that there is no bias from omitting the corresponding covariates in the second stage regression.

Second, there could be political covariates that could play a role instead. To account for this, we include the shares of the four parties (conservatives, social democrats, liberals, the greens) in the municipal council as possible determinants of changes in the business tax rates. Although some of the political variables enter significantly in the regressions, the results are again very similar to the estimates in Table 6.

Third, we apply radius and kernel matching instead of nearest neighbor matching. Unlike nearest neighbor matching, radius matching ensures a certain quality of matching. The smaller the radius chosen around a treated municipality’s propensity score to determine a similar untreated municipality, the higher is the matching quality. Choice of a rather small radius may lead to an efficiency loss as compared to nearest neighbor matching because for some treated observations similar untreated ones may not be available given the radius. We choose a radius of 0.1 (i.e., 10% points) in Table 7. Kernel matching uses a larger number of municipalities as control units and weights them according to the difference in the propensity score between the treated and the matched untreated observations. In our application, the difference to the original estimates in Table 7 is negligible.

21 Hence, the treatment and control group are not different with respect to the explanatory variables in the selection model.
A second bloc of results provides traditional Heckman (1978) two-step estimates. These are based on a regression where the two Mills’ change in business tax rates). Especially, the geographical variables as instrumental transfer eligibility), but not directly with the outcome (the actual grants are endogenous to municipalities’ tax policy choice. To address the endogeneity problem we adopt a strategy similar to the one in Gruber and Saez (2002) by calculating a proxy for grants that is a function only of pre-reform behavior. That is, we compute transfers per-capita grant (Grundbetrug)–see Section 3.22 To calculate the proxy, we use the population size and fiscal capacity averaged over the pre-reform period 1995–1997. We refer to the corresponding variable as Equalizing transfers in our discussion of the results. Table 8 summarizes the findings for both ATE (at the top) and ATT (at the bottom). First of all, the results indicate that—after controlling for Equalizing transfers—neither ATE nor ATT of supplementary transfer status are changed a lot as compared to Tables 2 and 5, respectively. Hence, the original results can be interpreted as ones reflecting tax price rather than income effects. Moreover, the income variable (Equalizing transfers) itself is hardly significant at all with ATE estimates, where it exerts a positive effect only in 1998 (significantly different from zero at 10%). Moreover, the income effect variable exerts a negative effect (significantly different from zero at 5%) by the end of the observation period with ATT effects. The latter suggests that municipalities which would have received higher transfers without the reform increased their tax rates less than other supplementary transfer recipients.

We have addressed the sensitivity of our findings in other ways than the ones reported here in detail. However, we suppress the per-capita grant (Grundbetrug)–see Section 3.22 To calculate the proxy, we use the population size and fiscal capacity averaged over the pre-reform period 1995–1997. We refer to the corresponding variable as Equalizing transfers in our discussion of the results. Table 8 summarizes the findings for both ATE (at the top) and ATT (at the bottom). First of all, the results indicate that—after controlling for Equalizing transfers—neither ATE nor ATT of supplementary transfer status are changed a lot as compared to Tables 2 and 5, respectively. Hence, the original results can be interpreted as ones reflecting tax price rather than income effects. Moreover, the income variable (Equalizing transfers) itself is hardly significant at all with ATE estimates, where it exerts a positive effect only in 1998 (significantly different from zero at 10%). Moreover, the income effect variable exerts a negative effect (significantly different from zero at 5%) by the end of the observation period with ATT effects. The latter suggests that municipalities which would have received higher transfers without the reform increased their tax rates less than other supplementary transfer recipients.

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### Table 6

<table>
<thead>
<tr>
<th>Year</th>
<th>Lower Saxony municipalities only</th>
<th>Supplementary transfer eligibility in Lower Saxony (incl. controls as in Table 5)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Column 1 (ATE)</td>
</tr>
<tr>
<td>1998</td>
<td>0.016</td>
<td>-0.069</td>
</tr>
<tr>
<td>1999</td>
<td>0.230***</td>
<td>-0.189**</td>
</tr>
<tr>
<td>2000</td>
<td>0.267**</td>
<td>-0.294***</td>
</tr>
<tr>
<td>2001</td>
<td>0.258***</td>
<td>-0.318**</td>
</tr>
<tr>
<td>2002</td>
<td>0.363**</td>
<td>-0.376**</td>
</tr>
<tr>
<td>2003</td>
<td>0.376**</td>
<td>-0.411</td>
</tr>
<tr>
<td>2004</td>
<td>0.376**</td>
<td>-0.508</td>
</tr>
</tbody>
</table>

Notes: reported estimates are propensity score nearest neighbor matching based with respect to changes as compared to 1994–1997 average levels. Supplementary transfer eligibility in Lower Saxony is endogenous.

* * * Significant at 1%.
* * Significant at 5%.
* * Significant at 10%.

### Table 7

<table>
<thead>
<tr>
<th>Year</th>
<th>Lower Saxony municipalities only</th>
<th>Supplementary transfer eligibility in Lower Saxony (incl. controls as in Table 5)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Column 1 (ATE)</td>
</tr>
<tr>
<td>1998</td>
<td>0.007</td>
<td>0.082</td>
</tr>
<tr>
<td>1999</td>
<td>0.059</td>
<td>0.108</td>
</tr>
<tr>
<td>2000</td>
<td>0.115</td>
<td>0.059</td>
</tr>
</tbody>
</table>

Notes: supplementary transfer eligibility in Lower Saxony is endogenous. Reported estimates are average treatment effects of the treated for changes in business tax rates as compared to 1994–1997 average levels.

* * * Significant at 1%.
* * Significant at 5%.
* * Significant at 10%.

22 Recall, fiscal need is defined as the product of population size, a qualifier which rises with population size, and a per-capita grant. The latter is annually determined to balance the budget for equalizing transfers. The functional form of the qualifier changed due to the reform. We hence calculate fiscal need as pre-reform population times the post-reform qualifier and the per-capita grant of the respective year. Post-reform population may be endogenous to the reform and would potentially bias our results.
corresponding tables for the sake of brevity and briefly mention selected results here.\textsuperscript{23} For instance, there is a possible concern that the abolition of the unification levy and the local capital tax (Gewerbekapitalsteuer) at the end of 1997 may have affected relative taxing incentives (see Section 3). To see whether our estimates are robust, we have included the associated change in the unification levy and local capital tax revenues (corrected for VAT revenues received in compensation) as a control variable. It turns out that the results of the supplementary transfer treatment effects are quantitatively insensitive to the inclusion of this control. Furthermore, we extended the control group for Bavarian municipalities to estimate the treatment effects more efficiently. Again, the findings are very stable in that regard. By including Bavarian municipalities we can also identify the overall tax rate effect of the 1999 reform by comparing the change in tax policy between municipalities in the reforming and a non-reforming state. The reform effect turns out to be positive. Finally, we assessed alternative ways of capturing income effects of the reform in Lower Saxony. Our findings are unchanged.

Overall, the bias associated with an exogenous treatment of supplementary transfer eligibility is large as becomes evident when comparing exogenous ATE estimates in Table 3 to endogenous ones in Tables 5, 7, or 8. However, ATT estimates seem preferable over ATE estimates, since ATU estimates are so different from ATT estimates (see Imbens, 2004).

7. Conclusion

Capacity equalization grants have been adopted in many countries primarily to affect interregional equity—but they may nevertheless influence local policy incentives and the efficiency of the equilibrium tax system. Equalization grants calculate transfer entitlements indirectly, using differences in revenues calculated at deemed rather than actual tax rates. But when measured tax bases respond negatively to tax rate increases, this formula may induce higher levels of taxation: increasing local tax rates causes measured tax bases to decline, as economic activity shifts to other regions of the country or to other, more lightly taxed forms—and so causes capacity equalization transfers to rise. Thus the grants in effect subsidize increases in taxes by equalization-receiving governments.

The paper empirically analyzes the incentive effects of equalizing transfers on business tax policy by exploiting a natural experiment in the state of Lower Saxony which changed its equalization formula as of 1999. Relying on within-state and across-state difference-in-difference estimates, the analysis reveals a significant impact of the reform on municipalities’ tax policy in the 4 years after the reform with the effect stabilizing in the fourth to fifth year. The finding is robust to various alternative specifications. The empirical result is in line with the theoretical prediction of a positive incentive effect of equalization grants on local tax rates.

From a policy perspective, the analysis suggests that fiscal capacity equalization implicitly acts as a tax coordination scheme, which deters municipalities from lowering taxes in order to attract a larger tax base (since it would result in a reduction in transfers) and which therefore mitigates fiscal competition.\textsuperscript{24} Our reduced form approach does not allow us to determine the extent to which the tax-raising effect of equalization is in fact mitigated by the effects of fiscal competition among German municipalities that would otherwise have reduced consumer welfare. But this interpretation is in line with the observation that the federal corporate tax rate fell from 56% on retained earnings and 36% on distributed earnings in 1980 to a uniform rate of 26.25% as of 2001 (in successive tax-cut cum base-broadening reforms), a trend that is often attributed to intensified fiscal competition. On the other hand, municipal business tax rates on average did not fall over the same period, suggesting that some mechanism—perhaps equalization grants—insulates these jurisdictions from fiscal competition.\textsuperscript{25}

Acknowledgments

We are grateful for comments by three anonymous referees and the editor. Comments by seminar participants in Bergen, Berlin, Helsinki, Paderborn, and at the 2006 IIPF Meeting in Paphos are gratefully acknowledged.

References


\textsuperscript{23} A complete set of tables and associated discussions are available from the authors upon request.

\textsuperscript{24} Indeed, Koethenbuerger (2002) and Bucovetsky and Smart (2006) show that, in the presence of horizontal tax competition, equalization grant may in a wide variety of settings induce subnational governments to independently choose tax rates that increase welfare.

\textsuperscript{25} The federal corporate tax base is nearly perfectly co-occupied by the municipalities’ business tax such that the broadening of the tax base does not account for the asymmetric response.


