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Property Taxation as a Determinant of School District Efficiency*

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Abstract

Recent theoretical contributions have emphasized the favorable incentive effects of property taxation. The object of this paper is to confront these theories with Norwegian data on student performance. The institutional setting in Norway is well suited to analyzing the effects of property taxation because we can compare school districts with and without property taxation. In addition, we focus on an alternative incentive mechanism – competition between school districts. The empirical results indicate that students in school districts that levy residential property taxes perform better at the national examination than students in comparable school districts. Strategic interaction in school quality is present, but the magnitude of the interaction effect is modest.

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1. Introduction

Brennan and Buchanan (1977, 1978, 1980) have emphasized that if governments are not entirely benevolent, it is in the interest of voters to design tax systems to control a ‘Leviathan’ government. Glaeser (1996) applied the ideas of Brennan and Buchanan to local property taxation. He showed that when local government bureaucrats act as revenue-maximizing agents, then property taxation provides the correct incentives. Higher school quality increases the tax base, generating additional tax revenue, which in turn benefits the public sector officials. Property taxation reduces waste because public sector officials take into account the feedback via property values. Glaeser showed that when housing demand is inelastic, property taxation provides stronger incentives for local governments than lump sum taxes. Hoxby (1999) provided a theoretical framework to analyze costs and efforts in schools where property taxation works as a disciplining device. Property taxation links school quality to school financing and helps control costs and efforts in schools. Even if the theories of Glaeser (1996) and Hoxby (1999) are correct in principle, it has yet to be shown that these mechanism are important in practice.[1] The object of this paper is to confront these theories with Norwegian data on student performance. The Norwegian case is well suited to empirically testing the kind of hypothesis put forward by Glaeser (1996) and Hoxby (1999) because we can compare school districts with and without residential property taxation.

Wilson and Gordon (2003) developed the argument of Glaeser (1996) in a richer model that emphasizes competition between jurisdictions. In their model, interjurisdictional competition reduces waste, raises the utility of residents, and increases the desired supply of public goods. The argument is analogous to the property tax mechanism, as public officials benefit from ‘taking a smaller slice out of a larger pie’ through the Tiebout process (Wilson and Gordon 2003: 401).[2] We extend the analysis of school district efficiency to capture competition

1 An exception is Borge and Rattsø (2004a), who found that property taxation is able to hold down costs in Norwegian local governments.

2 The beneficial effect of interjurisdictional competition is not necessarily due to the Tiebout process where school districts face out-migration if they provide low quality schooling. It can also be due to yardstick competition. Yardstick competition models argue that voters look to other similar jurisdictions to help judge whether their local government officials are wasting resources and deserve to be voted out of office. Since self-

among school districts. We utilize spatial econometric methods to investigate strategic interaction in school quality and solve the endogeneity problem with Maximum Likelihood methods. With this approach, we are able to detect strategic interaction among school districts, but we cannot establish the welfare consequences. However, on theoretical grounds, it is reasonable to argue that competition among school districts will be welfare improving.

The econometrical analysis finds strong support for the hypothesis that residential property taxation works as a disciplining device. Students in school districts that levy residential property taxation perform better at the national examination than students in comparable school districts. Due to rich data availability at the individual level, we are able to control properly for students' family background characteristics as well as school district characteristics. We carry out several robustness checks and find a robust positive relationship in all specifications. We interpret this as evidence of property taxation working as an incentive mechanism. A school district that is able to increase school quality faces increased demand for housing because the school district is regarded as more attractive.[3] This is accompanied by a rise in housing prices, yielding increased revenue to the school district that utilizes residential property taxation as a local income source. Hence, local property tax gives the local bureaucrats/leaders stronger incentives to maintain a high quality education system. In addition, we find evidence of strategic interaction among the school districts, but the magnitude of the effect is quite modest.

In a related paper, Grosskopf et al. (2001) focused on school district competition and voter monitoring. Although they used a different methodology to the current paper, they found that technical inefficiency is lower when monitoring, as represented by several proxies, is high. Our contribution is to introduce property taxation as another explicit incentive mechanism.

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interested government officials choose policies knowing that voters make such comparisons, they will tend to mimic the policies implemented by other comparable local governments.

3 Black (1999) showed that school quality, measured by test scores, is capitalized into housing prices, controlling for neighborhood characteristics, school spending, and property taxation. Note that capitalization of public sector quality into housing prices will take place also in the absence of residential mobility, if public sector services and housing are complementary goods and the housing stock is fixed in the short run.

The results below are interpreted as showing a favorable incentive effect on school officials. An alternative understanding, more consistent with Grosskopf et al. (2001), would be that property taxation gives the residents incentives to increase monitoring.

The structure of the paper is as follows. Section 2 introduces the empirical context, while Section 3 presents the empirical specification. We discuss the main results in Section 4 and carry out a robustness check based on matching on propensity scores in Section 5. Section 6 concludes the paper.

2. Empirical context

Norway has around 3000 public schools, which are distributed over 434 school districts.[4] As each school district corresponds to one local government, we use the two terms interchangeably. Local governments in Norway are responsible for primary and lower secondary education, in addition to care for the elderly, preschool education, and some other local services, such as infrastructure. Spending on primary and lower secondary education accounts for about 30% of total local government spending. The responsibility for financing and organization lies with the individual school district. However, unlike school districts in the US, Norwegian school districts are largely financed through block grants and regulated income tax (all school districts apply the maximum income tax rate). Around 90% of local governments' revenues are generated from central government grants and regulated income taxes. However, user charges and property taxes represent important sources of marginal revenue for the Norwegian school districts. Borge and Rattsø (2004b) analyzed determinants of the tax structure, the mix of revenues from property taxation and fees. School districts can levy two sorts of property taxation: commercial and residential property taxation. The former kind of taxation is not interesting in our setting because it is basically a tax on power stations and primary industries, which are typically immobile, and does not have any impact on most citizens. Furthermore, residential property taxation in Norway is regulated by law (as of June

4 About 1000 of these schools are pure primary schools (grade 1 to 7). Around 98% of the students are enrolled in public schools. Hence private schools are not an alternative to public schools and they are not a part of this analysis.

6, 1975). The law describes the property that can be taxed, the tax base assessment, and restrictions of the tax rate. In particular, the law restricts residential property tax to areas that completely or partially have the characteristics of a town or areas where such characteristics are developing. This definition is not very clear and it is hard to separate school districts that have opted out of property taxation and those that are not allowed to have property taxation. This is sometimes decided by court.[5] Figure 1 shows the geographical distribution of residential property taxation in 2001. Approximately, one out of four school districts levies residential property taxation. Our key explanatory variable is a dummy that is equal to one if the school district levies residential property taxation.

Figure 1 about here.

Our measure of school quality is based on a nationally decided written external examination that all Norwegian students have to undertake at the end of tenth grade. Although the curriculum includes many different subjects, both elective and compulsory, a written examination is undertaken only in mathematics, English, and Norwegian.[6] Further, each student is examined in only one of the three subjects, this one being decided centrally shortly before the exam. In addition, students are graded by their teachers. Both the exam results and the grade points matter for entrance to upper secondary school. Because exam grades are set by external examiners, we believe the exam results provide the most accurate picture of actual school district quality. The grades set by teachers may be severely biased because of relative grading within the school, grade inflation etc. The final data on student achievement are based on 107,841 students in 425 school districts who were finishing lower secondary school in the school years 2001/2002 and 2002/2003.[7] The grading goes from one to six, where six is the

5 In the empirical analysis, we experiment with excluding the least populated school districts, which are unlikely to be allowed to levy residential property taxation.

6 In Norway, we have two official written languages (Norwegian and New Norwegian), and an exam has to be undertaken in both of them.

7 The total number of students finishing in 2001/2002 and 2002/2003 is 122,281 students. Because of missing data on some explanatory variables and because we exclude the capital, Oslo, which is differently organized than the rest of the local governments, we are left with 107,841 observations.

top score and one is fail. The grades are fairly normally distributed, with a mean of 3.44 and a standard deviation of 1.07.

3. Empirical specification

When aiming to isolate the incentive effect of property taxation on public sector efficiency, it is clearly important to control for all other features of the school districts that affect student achievement (and might be correlated with property taxation). In particular, it is well known that family background characteristics are important determinants of student achievement. A strength of our analysis is that we are able to control for family characteristics at the individual level. Our estimation strategy contains two steps. First, we estimate the impact of student family background characteristics on individual student exam performance and obtain fixed effects for 425 school districts. These school district fixed effects can be interpreted as the variation in exam performance when family background is controlled for. Second, we utilize the school district fixed effects to investigate whether school districts that levy residential property tax have a higher level of student achievement.[8]

The following education production function is estimated at the school district level (step 2):

$$\begin{aligned} A_i &= \beta_0 + \beta_1 dptax_i + \beta_2 R_i + \sum_{k=1}^{K} \beta_k CONTROLS_{ki} + u_i, \\ u_i &\sim N(0, \sigma_u^2) \end{aligned} \tag{i}$$

where A_i is the school district fixed effects measuring average student achievement in district i, $dptax_i$ is a dummy for residential property taxation, and R_i is a variable capturing resource use in the school sector. In addition, K other controls and an i.i.d. error term, u_i, are included.

8 Because of the discrete nature of the student achievement variable, there is a rationale for utilizing the ordered probit model. However, when we are interested in the school district fixed effects we need to use the standard OLS approach.

The parameter of interest is β_1, which we expect, from Glaeser (1996), to be positive and statistically significant. Since districts are not randomly selected into levying property taxation, we face some econometrical challenges in obtaining reliable estimates of β_1. If we fail to include any variable that is correlated with the decision to have residential property taxation and also with student achievement, β_1 will be biased. Consequently, we include a vast number of controls, both on the individual and on the school district level. In the second step, we control in particular for resources spent in the education sector. It is crucial not to mix up the effects of increased resources spent and the incentive mechanism caused by property taxation. We utilize teacher education hours per student to take into account resource spending in the education sector. In order to reduce the chances of spuriously attributing an effect to property taxation when it is really caused by other factors, we include a large battery of controls consisting of school characteristics (enrollment, enrollment^2, and teacher experience), the political and structural characteristics of the school district that are important determinants of property taxation (socialists in the local council, party fragmentation of the local council, population size, and settlement pattern) and finally, we acknowledge that the controls for family background in step 1 may not completely capture the effect of school district background characteristics. Differences between school districts in family background may still remain in the error term. Therefore, we also provide specifications where we include several school district level characteristics (median private income, educational level in the population, the unemployment rate, the share of the population that is divorced or separated, and the share of the population that is disabled). Descriptive statistics of all variables are presented in Appendix Table 1.[9]

Inconsistent estimates of β_1 can arise if the quality of the school system affects the decision to levy property taxation. It is not clear in what direction this would bias our results. We do not believe that our results, presented below, are driven by 'reverse causality'. The school

9 The model is tested in several specifications not reported. We have tried including several variables that came out as insignificant and consequently are not included in the presented analysis. The main results are robust for the inclusion of the following variables: the share of minority students, the share of students with special needs, the share of female teachers, the average length of teacher education, a dummy variable for combined schools, the share of teachers on temporary contracts, the crime rate in the school district, and refugees in the school district.

districts typically do not opt in and out of property taxation. To address this potential endogeneity problem, we need an instrumental variable that is correlated with the decision to impose a property tax, but has no independent impact on student achievement. We have experimented with utilizing the exogenous variation in property taxation caused by the property tax law. Because the settlement pattern is an important determinant of the decision to levy property taxation, it has the potential to work as an instrumental variable. However, the settlement pattern is correlated with several other characteristics of the school district that may affect performance and we are not completely confident that we are able to wash out all these differences with our controls. Therefore, we base our inference on a standard ordinary least squares (OLS) estimation. It is a challenge for future work to establish a valid instrumental variable.

In principle, it would be interesting to include data for residential property taxation for more than one year for two reasons. First, because we do not expect the incentive mechanisms caused by property taxation to have a sudden impact on student achievement and second, because knowledge production is a cumulative process that starts earlier than the year in which the students have their final exam. However, time series data on residential property taxation are not available. Nonetheless, we know that school districts typically do not opt in and out of property taxation. In fact, account data show that 93 of the 109 school districts that levied residential property taxation in 2001 also levied some sort of property taxation (residential or commercial) in the seven previous years.[10] Equivalently, it would be interesting to check the impact of other lagged variables, in particular the resource input variable. We have attempted to do this, but we do not find a significant improvement in our model specification by including lags. It is not surprising that lagged resource use does not improve our model specification because the variable is highly correlated over time.

In an extension of the baseline model (i), we focus on interdistrict school competition. There is a large empirical literature that examines the effects of competition on school quality. Grosskopf et al. (2001) provide an overview of the literature. Similarly to the property tax

10 In 2001, 120 school districts levied only commercial property tax.

mechanism, competition provides incentives for school officials to be productive.[11] The beneficial effect of competition can arise either because school districts face out-migration if they provide low quality schooling or because of yardstick competition. Yardstick competition models argue that voters look to other similar jurisdictions to help judge whether their local government officials are wasting resources and deserve to be voted out of office. As self-interested government officials choose policies knowing that voters make such comparisons, they will tend to mimic the policies implemented by other comparable local governments. The yardstick competition mechanism was introduced by Salmon (1987).

Here, we follow the empirical literature on interjurisdictional competition and relate the value of the dependent variable at a given location to its value at other locations, thus extending (i) to:[12]

$$\begin{gathered} A_i = \beta_0 + \rho \sum_{j=1}^{N} w_{ij} A_j + \beta_1 dptax_i + \beta_2 R_i + \sum_{k=1}^{K} \beta_k CONTROLS_{ki} + u_i \\ u_i \sim N(0, \sigma_u^2) \end{gathered} , \tag{ii}$$

where the w_{ij} are elements in a symmetric 425×425 weight matrix. The w_{ij} are different from zero if the two school districts are considered to be neighbors. The rest of the variables are described above. The spatial weights are determined *a priori* and can be considered as part of school district i's basic characteristics. We define neighbors to be school districts with a common border. The weight matrix is row-standardized, ensuring that $\sum_{j=1}^{N} w_{ij} A_j$ can be interpreted as the weighted average of neighboring student achievement (controlled for family background). If there is competition among public school districts, we would expect school district quality to be dependent on neighboring school districts' quality, making ρ different from zero. This can either arise because of (mobility) competition over students or yardstick competition. The basis for the interaction is not the issue here. Blair and Stanley (1995)

11 Productive in the sense that achievement can be increased for a given resource input.

12 Brueckner (2003) presented an overview of different studies utilizing this approach and discussed econometric issues.

followed the above outlined approach when utilizing US data to study strategic interaction in school district achievement. Although they found a positive spatial correlation ($\rho > 0$), they neglected the endogeneity problem apparent in (ii), which biases OLS upward. Another recent study by Millimet and Rangaprasad (2005) found that public school districts interact strategically in schooling-input decisions.

An observed spatial pattern in student achievement is not necessarily the result of competition among school districts. In addition, common shocks and unobserved correlates will appear as spatial auto-correlation. With spatially correlated omitted variables, we have a pattern of spatial error of the form: $u_i = \lambda \sum_{j=1}^{N} w_{ij} u_j + \varepsilon_i$, where ε is an i.i.d. error term. Estimating (ii) when the error term is spatially correlated can in principle lead to a false conclusion of strategic interaction ($\rho \neq 0$) when $\rho = 0$ holds in the true model. Note that the rigorous controls used in stage one make it highly unlikely that we are capturing spatially correlated family background variables. In Section 4, we estimate both the spatial lag model and the spatial error model. The endogeneity is handled with Maximum Likelihood (ML) methods.

4. Results

Utilizing approximately 108,000 observations at the individual level, we find the expected effects of family background characteristics on student achievement. In particular, we find that the achievement level increases with the parents' income and educational levels. Table 1 presents the results from the first stage analysis.

Table 1 about here.

In our setting, it is not of special interest to evaluate the effects at the individual level. More interesting are the school district fixed effects. The school district fixed effects come out as statistically significant. Leaving them out reduces the R^2 from 0.1898 to 0.1744. There is substantial variation in the school district fixed effects. Compared to the average school district, the 'worst' and 'best' school districts have average achievement levels of around one grade lower and one grade higher, respectively. Our second stage endogenous variable has a mean of 0.02 and a standard deviation of 0.2.

The second stage of our analysis is to explain what differences at the school district level can explain the school district fixed effects. The main results are presented in Table 2. Specifications (1), (2), and (3) are based on the whole sample, whereas specification (4) excludes small school districts and specification (5) excludes small and large school districts. Specification (3) is augmented with a spatial lag and spatial error and estimated with ML to solve the endogeneity. The ML estimations are reported as specifications (6) and (7).

We begin by discussing the baseline results obtained from the whole sample, shown in columns (1), (2), and (3). In column (1), we regress student achievement on the property tax dummy only. Thereafter, we add school district characteristics in two steps in columns (2) and (3). The overall picture shows that, when adding controls, school districts with property taxation have average grades of around 0.05 higher than school districts without property taxation. The estimates are significant at conventional levels. We interpret this to mean that property taxation has a favorable incentive effect on school officials, stimulating them to provide effective and efficient schooling. An alternative interpretation is that property taxation gives the residents stronger incentives to monitor the school officials. Monitoring may be more intense when the school district is (partly) financed through property taxation rather than from central government grants. We do not seek to distinguish whether the effect of property taxation on school district efficiency is caused by favorable effects on school leaders, residents, or both, but conclude that our results corroborate the theoretical framework put forward by Glaeser (1996) and Hoxby (1999).

Table 2 about here

Figlio (1997) estimated an education production function utilizing US data from the National Education Longitudinal Survey (NELS). He found evidence that students in schools subject to local property tax limitations perform worse in reading, science, and social studies, than students in schools without such limits. These results might indicate that “money matters” for knowledge production to take place. However, one weakness with this analysis is the restriction that tax limits affect only the resource level, which means that Figlio failed to separately control for the disciplining effect caused by property taxation, as outlined in Glaeser (1996) and Hoxby (1999). A strength of our analysis is that we take this matter seriously. Contrary to Figlio, we condition on the resource use, which implies that we aim to separate the impact of money and incentives. However, if 'resource use' is endogenous, we should be careful in interpreting the effect on 'teacher education hours' as a causal effect. This may be the reason why we fail to find any robust effect of resource use on student achievement.[13] A potential critique of our central result might be that revenue from property taxation yields increased spending in the education sector, which we fail to capture in our resource variable. To test whether this is a relevant critique, we have run an additional regression where we have tried 'spending per pupil' as an alternative resource measure. The measure of fit of the model is reduced, but the property tax coefficient is basically unaltered. In addition, in Appendix Table 2, we present results where we have regressed 'spending per pupil' on a dummy for property taxation and other potentially important determinants of resource use. We control for several characteristics that have been shown to be important determinants of local government spending and find that property taxation does not seem to increase spending per pupil. On the contrary, we find a negative but statistically insignificant correlation between the property tax dummy and spending per pupil.

Of the other control variables, we find that large schools seem to perform worse than small schools and that experienced teachers increase student achievement. The only other statistically significant variables are the shares of the population with nine and twelve years of education. Neither the political variables nor the structural characteristics of the population have any (partial) statistically significant impact on student achievement. However, a joint F-test of the impact of the political and structural characteristics does suggest that the inclusion of these variables significantly increases the explanatory power of the model. These variables

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13 A disputed topic in the education literature is whether additional resource use will improve performance in schools. We are by no means the first to find no effect of resources. See for instance Hanushek (2002). Note that the incentive effect of property taxation is unaltered if 'teacher education hours' are excluded from specification (3).

are included because they are expected to be important determinants of property taxation and local government priorities in general.[14]

An important question is how large the disciplining effect of property taxation is. A 0.05 increase in average student achievement corresponds to a quarter of a standard deviation increase in the school district fixed effects. The effect is of approximately the same magnitude as a one-year increase in mother's education for all students in the school district (estimated to be 0.07 in Table 1). We conclude that the incentive effect of property tax has a considerable impact on the achievement level.

Residential property taxation is a voluntary choice among school districts that completely or partially have the characteristics of a town or where such characteristics are developing. Our OLS estimates compare communities that choose to levy property taxation with those that choose not to levy property taxation, together with those that are not allowed to (rural school districts). We recognize that this might cause problems. By failing to exclude the rural school districts from our sample we might falsely conclude that property tax yields higher student achievement, when the driving force behind the pattern may simply be differences between urban and rural school districts. Note, however, that neither the proxies for settlement pattern or population size are statistically significant at conventional levels in model (3). In principle, the solution to this problem is simple – compare only the school districts that are allowed to levy property taxation. The problem with this remedy is that we are not able to perfectly separate between those that are allowed to utilize property taxation and those that choose not to.[15] However, as an attempt to investigate this problem, we check whether the main results are robust to the exclusion of school districts that are more likely not to be allowed to levy residential property tax. For this purpose, we exclude school districts where the population

14 Several previous Norwegian studies have shown that political strength (measured by the Herfindahl index), and ideology have a strong impact on the priorities of local governments. Falch and Rattsø (1999), for example, argued that political strength, measured by the party fragmentation of the county council, holds down the costs and allows for more student enrollment in upper secondary education.

15 It is hard to exclude the school districts that are not allowed to have property taxation because of the vague formulation of the property tax law.

size is below 2,500 inhabitants.[16] As a further robustness check, we exclude school districts that have a population size above 30,000. The results are reported in columns (4) and (5) in table 2. We find that the effect of the property tax increases to around 0.06 and is statistically significant at the 5% level. The control variables show a similar pattern as in specification (3).

The Moran test is a natural starting point for evaluating a potential spatial pattern in the school district fixed effects. Based on the OLS residuals from specification (3) in Table 2, the Moran test rejects the H_0 of absence of spatial auto-correlation at the 10% level. The Moran test suggests that the model should be estimated with a spatial lag or a spatial error model, but it cannot point to the model that best fits the data. The LM tests based on Burridge (1980), and their robust counterparts developed by Anselin et al. (1996), point in the direction of the spatial lag model rather than the spatial error model.[17] The test results are reported in Appendix Table 3. For completeness, we estimate both the spatial lag model and the spatial error model. The simultaneity problem is solved by estimating the models with maximum likelihood methods (ML). The results are reported in columns (6) and (7) in Table 2. Both ρ and λ come out as statistically significant. The log likelihood is somewhat higher for the spatial lag specification. The positive ρ is consistent with the school district competition hypothesis. However, the magnitude of the effect is quite modest. A coefficient of 0.14 suggests that if all neighbors increase their (family background-corrected) student achievement by 0.2 of a grade (one standard deviation), then the school district under study will increase its grade by 0.028.[18]

Fiva and Rattsø (2005) showed that property taxation is itself spatially correlated because of fiscal competition (see also Figure 1). Consequently, one might worry that the estimated effect of property taxation on student achievement is simply caused by spatially correlated

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16 Only 2% (three out of 130) of the school districts below 2,500 inhabitants levy property taxation. For school districts with a population above 2,500 and below 5,000, the share is 0.23.

17 The robust LM tests are robust to the presence of local misspecification of the other form of spatial dependence. I.e. they test for spatial lag dependence that is robust to spatial error dependence (and vice versa).

18 Note that ignoring simultaneity and estimating (ii) with OLS gives a ρ of 0.26 (s.e.=0.10). Blair and Staley (1995) for comparison found an effect of approximately 0.4 on American data utilizing OLS.

omitted variables and therefore the spatial auto-regressive estimations provide a relevant robustness check of the effect of property taxation on student achievement. The estimated effect of property taxation is basically unaltered when we control for strategic interaction in school quality. We conclude that the property tax dummy does not act as a proxy for school district competition or other spatially correlated omitted variables.

5. Matching analysis

The main purpose of our analysis is to study the causal effect of property taxation on the student achievement level. Since school districts are not randomly selected into having property taxation, our challenge is to reduce the potential bias in the estimates generated by unobservable confounding factors. The central idea behind matching methods is to find a way to compare causal effects *as if* we were facing a controlled experiment.

Let $Y_i(D_i)$ denote student achievement level in school district i. D_i equals one (treatment) if the school district levies property tax and zero otherwise. The causal treatment effect for school district i is given by: $\Delta_i = Y_i(1) - Y_i(0)$. Δ_i is clearly not observable. We do not observe what would have happened to the treated in the counterfactual situation of no treatment (and vice versa). However, with a clean natural experiment we could easily have obtained a consistent estimate for the average effect of treatment by simply finding the difference in average outcome for treatment and non-treated units. The beauty of controlled experiments is that they create balance (or comparability) between treated and non-treated units on all covariates except treatment. With non-experimental data, treatment selection yields imbalance on some covariates that might give rise to biased estimation. Matching methods try to overcome this problem by creating balance on a set of observable characteristics. With no omitted imbalanced confounders, the average treatment effect is then consistently estimated by averaging within-match differences in the outcome variable between treated and non-treated units.

To avoid problems connected with multi-dimensionality, we will concentrate on Rosenbaum and Rubin's (1983) matching on propensity scores method. Matching school districts with the same probability of selecting property tax, given the relevant controls, is equivalent to matching directly on the controls. Conditioning on the propensity score, those school districts that do not levy property tax serve as a valid control group to those that do levy property tax.

The first step in implementing the matching on propensity scores method is to estimate the propensity score. Any standard probability model can be used to estimate the propensity score. In this analysis, we use a logit model. To what extent matching on propensity scores is able to obtain reliable estimates depends crucially on the richness and quality of the control variables on which the propensity score is computed and the matching performed (Becker and Ichino, 2002:358). We should not fail to include any variable correlated with the decision on whether to levy property tax that may simultaneously influence the student achievement level. We started out by including all control variables that are incorporated in specification (3) in Table 2 in the logit regression. In the final analysis, we excluded variables that did not have any statistically significant effect in the logit regression.[19] The results from estimating the propensity score function are presented in Appendix Table 4. As expected, we find that more populated and urban school districts are associated with a higher probability of having property taxation.

Given the estimated propensity score and that there are no omitted imbalanced confounders, the average treatment effect is consistently estimated by averaging within-match differences in the outcome variable between treated and non-treated units. Therefore, the second step is to establish the suitable control(s) for each treated unit. The optimal approach would be to match treated units and controls with exactly the same (estimated) propensity score. However, as it is impossible (or rare) to find two units with precisely the same propensity score, we have to rely on alternative procedures to work out step two. Different matching estimators have been suggested in the literature. The original Rosenbaum and Rubin (1983) approach was to utilize

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19 We also ran a specification where we included 'grants per capita' instead of teacher hours (teacher hours and grants per capita have a raw correlation of 0.73), but the specification with teacher hours gives a slightly higher fit and when adding both variables in the same regression, grants per capita turns out to be insignificant.

the same stratification method used for estimating the propensity score. Provided that the exposure to treatment is random within each strata, this approach yields consistent estimates of the average treatment effect on the treated. An alternative approach to matching within blocks is to match each treated unit with the closest control unit(s) in terms of estimated propensity scores. How 'neighborhood' should be defined is not clear and hence, we present several estimators.[20] The kernel estimator is obtained by matching every treated unit with a weighted average of all the controls; it gives a smaller weight to school districts that are far from the treated unit (in terms of probability of treatment) than it does to school districts that are closer to the treated unit.[21] Smith and Todd (2005) utilized experimental data and argued in favor of the kernel estimator. In addition, we provide estimators based on simple 'nearest neighbor' matching and estimators based on matching within a radius in the propensity score.

Table 3 reports the results of property taxation on average student achievement utilizing different matching estimators. We find a positive causal effect of property taxation on the student achievement level. The ATT coefficients are estimated to be in a range from 0.040 to 0.070. Thus, the size of all the estimated ATT coefficients is of the same magnitude as the OLS coefficients reported in Table 2.

Table 3 about here

We conclude that there is a robust difference in student achievement between school districts with and without residential property taxation.

6. Conclusion

Several researchers have asserted that inefficiency in the school sector arises from a lack of incentives to behave efficiently. When output is not well defined, as in the education sector, it

20 To estimate the propensity scores and the average effect of treatment on the treated, we obtained STATA programs pscore.ado, atts.ado, attk.ado, attnd.ado, and attr.ado, which are downloadable at http://www.iue.it/Personal/Ichino/Welcome.html

21 For a definition of the kernel estimator see Becker and Ichino (2002:363-364).

may be hard to detect inefficient production. Monitoring may simply be too difficult. In this paper, we empirically evaluated the relevance of two proposed incentive mechanisms suggested in the theoretical literature, property taxation and interjurisdictional competition. Utilizing a well-suited data set from the educational sector in Norway, we found evidence that students in school districts levying residential property tax perform better at the nationally decided external examination. The main result is robust to several different specifications, and in particular to Rosenbaum and Rubin's (1983) matching on propensity score method. We did not seek to distinguish whether the effect of property taxation on school district efficiency is caused by favorable effects on school leaders, residents, or both, but concluded that our results corroborate the theoretical framework put forward by Glaeser (1996) and Hoxby (1999). Moreover, we found evidence of strategic interaction among school districts. Such school district competition is likely to contribute to higher school quality. An interesting elaboration of this analysis would be to evaluate how these two mechanisms interact. Another challenge for future work is to take a closer look at the possible endogeneity problem related to the choice of having residential property tax. We cannot rule out the possibility that the quality of the school system might affect the decision to levy property taxation. This could have biased our results, but it is not clear in what direction the bias would have gone.

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Figure 1 The geographical distribution of the property tax dummy, where shaded areas indicate that the school districts levies residential property taxation.

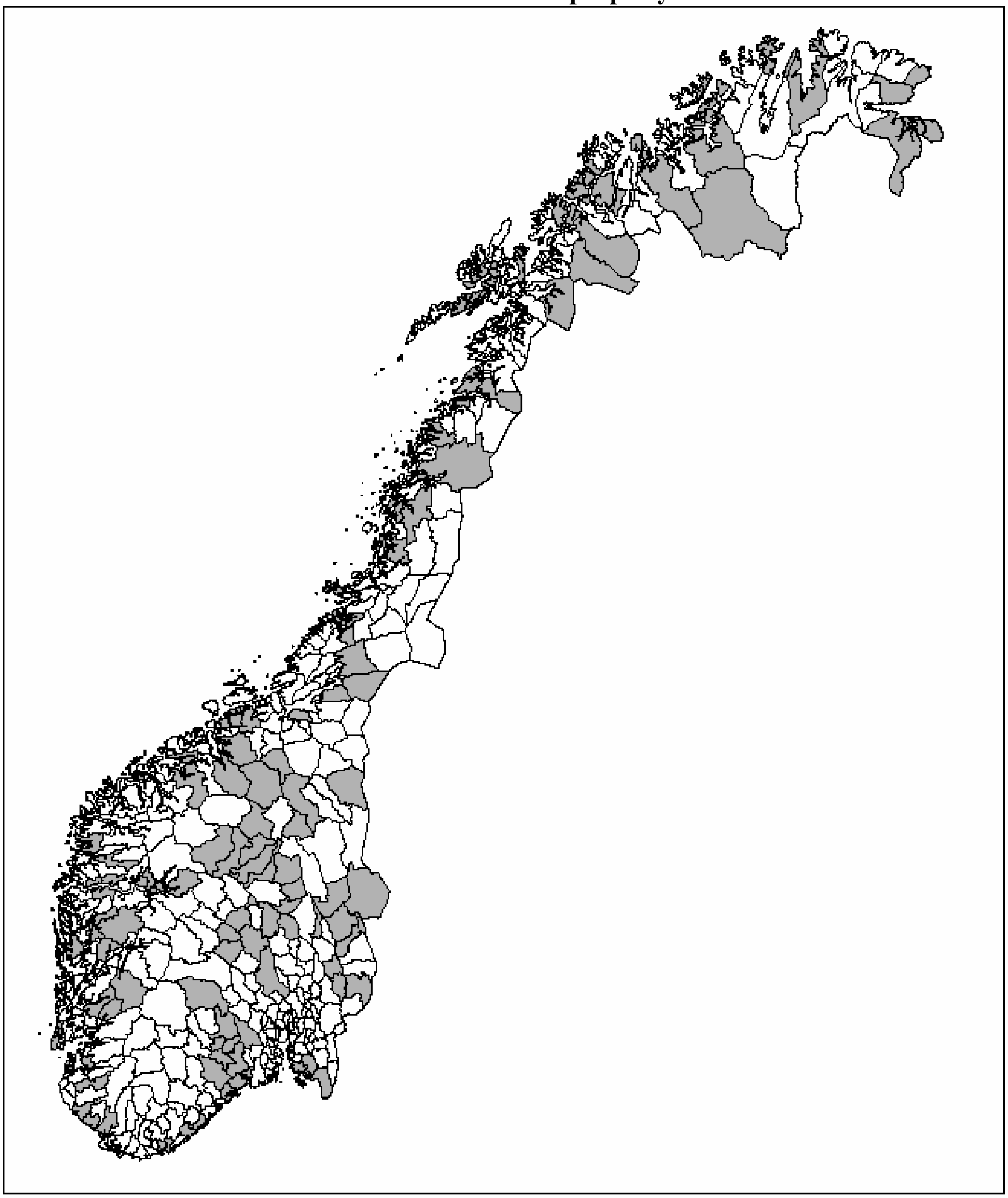

Table 1: Computation of 425 school district fixed effects. The endogenous variable is the individual exam result.

	Coefficient (st. error)
Girl	0.347 (0.010)***
Age (years)	0.075 (0.011)***
ln (family income)	0.124 (0.008)***
Education mother (years)	0.076 (0.002)***
Education father (years)	0.068 (0.002)***
Foreign-born parents	–0.034 (0.025)
Parents non-cohabiting	–0.118 (0.017)***
Parents married	0.090 (0.016)***
R^{2+}	0.1898
# observations	107841
School district fixed effects	Yes
Estimation method	OLS

Note: Included in the regression, but not reported are a constant term, a year dummy, dummies for different subjects, and 425 school districts fixed effects. Standard errors (corrected for within school correlation between years) are in parentheses. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 2: Determinants of corrected school district performance

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Property tax	0.004	0.050	0.044	0.059	0.061	0.042	0.045
	(0.022)	(0.025)**	(0.025)*	(0.023)**	(0.025)**	(0.025)*	(0.025)*
Teacher education hours per student		0.000	0.001	–0.002	–0.002	0.001	0.001
		(0.001)	(0.001)	(0.001)*	(0.001)*	(0.001)	(0.001)
Enrollment		–0.003	–0.003	–0.003	–0.002	–0.002	–0.002
		(0.001)**	(0.001)**	(0.001)**	(0.001)*	(0.001)*	(0.001)*
Enrollment^2/1000		0.015	0.012	0.011	0.006	0.013	0.013
		(0.008)*	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Teacher experience (years and months)		0.012	0.011	0.011	0.011	0.010	0.010
		(0.003)***	(0.004)***	(0.004)***	(0.004)***	(0.003)***	(0.003)***
Ln (school district population)		0.027	0.017	0.003	0.008	0.002	0.002
		(0.020)	(0.020)	(0.021)	(0.027)	(0.002)	(0.002)
Share of people living in rural areas		0.097	0.078	0.017	0.034	0.093	0.104
		(0.050)*	(0.060)	(0.072)	(0.076)	(0.058)	(0.059)*
Share of socialists in the local council		–0.186	–0.091	–0.055	–0.058	–0.082	–0.063
		(0.079)**	(0.084)	(0.099)	(0.108)	(0.082)	(0.084)
Party fragmentation in the local council		0.151	0.123	–0.113	–0.112	0.108	0.106
		(0.143)	(0.142)	(0.188)	(0.201)	(0.139)	(0.140)
Share of population with lower secondary education as highest education			–0.801	–1.257	–1.344	–0.762	–0.826
			(0.283)***	(0.306)***	(0.329)***	(0.277)***	(0.289)***
Share of population with upper secondary education as highest education			–0.954	–1.083	–1.243	–0.961	–0.983
			(0.350)***	(0.354)***	(0.400)***	(0.341)***	(0.353)***
Unemployment rate in the school district			–1.199	–2.245	–2.267	–1.111	–1.181
			(0.679)*	(0.742)***	(0.769)***	(0.661)*	(0.692)*
Share of the population that is disabled			–0.617	0.046	0.371	–0.598	–0.688
			(0.798)	(0.925)	(0.972)	(0.778)	(0.811)
Share of the population that is divorced			–1.148	–1.758	–2.101	–0.894	–0.974
			(0.893)	(0.924)*	(1.007)**	(0.873)	(0.899)
Median income in the school district (in 1000 NOK)			–0.001	–0.002	–0.002	–0.001	–0.001
			(0.001)	(0.001)**	(0.001)*	(0.001)	(0.001)
Spatial lag (ρ)						0.140	
						(0.069)**	
Spatial error (λ)							0.125
							(0.073)*
R^2	0.000	0.110	0.157	0.236	0.246		
Log likelihood						115.37	114.60
Included observations	All	All	All	Pop>2500	Pop<30000 & Pop>2500	All	All
# obs.	425	425	425	302	279	425	425
# obs. with dptax = 1	109	109	109	106	92	109	109
Estimation method	OLS	OLS	OLS	OLS	OLS	ML	ML

Note: Included in the regression, but not reported, is a constant term. Standard errors are in parentheses. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 3: Matching estimates, 'school district fixed effects' as dependent variables

	Kernel	Stratification	Nearest Neighbor	Radius (r=0.01)	Radius (r=0.1)
Estimate (ATT)	0.056	0.070	0.053	0.047	0.040
t- value	2.588□	2.988□	1.927	1.537	1.902
# of treated	109	89	109	78	104
# of controls	225	245	53	199	225
Common support	Yes	Yes	Yes	Yes	Yes

□Based on bootstrapped standard errors.

Appendix Table 1: Sample summary statistics data description and descriptive statistics, individual student level – means and standard deviations

Variable	mean	st. dev
Individual characteristics (N=107841)		
Girl	0.491	0.50
Age (years)	14.524	0.293
ln (family income)	13.280	0.535
Education mother (years)	11.806	2.636
Education father (years)	11.929	2.786
Foreign-born parents	0.023	0.150
Parents non-cohabiting	0.297	0.457
Parents married	0.656	0.475
School characteristics (aggregated on school district level) (N=425)		
Enrollment	51.121	33.232
Enrollment^2/1000	3.974	4.573
Teacher experience (years and months)	19.535	2.842
Teacher education hours per student	86.728	18.022
School district characteristics (N=425)		
School district fixed effects	0.027	0.202
Property tax, dummy taking the value 1 for school districts levying residential property tax	0.256	0.437
Ln (school district population)	8.485	1.061
Share of people living in rural areas	0.496	0.272
Share of socialists in the local council	0.366	0.141
Party fragmentation of the local council	0.259	0.082
The share of the population (16–74 years) with completed lower secondary education as their highest educational level	0.209	0.056
The share of the population (16–74 years) with completed upper secondary education as their highest educational level	0.621	0.041
Unemployment rate in the school district (yearly average)	0.034	0.019
The share of the population that is disabled	0.066	0.018
The share of the population that is divorced or separated	0.063	0.017
Median income in the school district for persons 17 years and older (in 1000 NOK)	195.048	21.020
Extra variables used in regression, appendix table 2 (N=422)		
Grants per capita (in 1000 NOK)	27.072	6.918
The share of the population in the school district between 0 and 7 years	0.077	0.010
The share of the population in the school district between 7 and 16 years	0.136	0.015
The share of the population in the school district above 80 years	0.154	0.035

Documentation of the variables: data are provided by the Norwegian Social Science Data Services and by Statistics Norway. Neither of these institutions is responsible for the analyses conducted or for the conclusions drawn.

Appendix Table 2: The determinants of gross spending per pupil, measured in 1000 NOK. OLS estimates.

Variable	Coefficient (st. error)
Property tax	–0.602
	(0.968)
Grants per capita	1.346
	(0.101)***
Enrollment	–0.153
	(0.057)***
Enrollment^2	0.689
	(0.343)**
Ln (school district population)	–0.333
	(0.828)
Dummy variable for small school districts (pop<2500)	–1.253
	(1.321)
Share of people living in rural areas	4.877
	(2.161)**
Share of socialists in the local council	–7.296
	(3.124)**
Party fragmentation of the local council	2.733
	(5.683)
The share of the population in the school district between 0 and 7 years	–85.497
	(51.281)*
The share of the population in the school district between 7 and 16 years	–169.851
	(32.620)***
The share of the population in the school district above 80 years	–64.093
	(19.995)***
Median income in the school district (in 1000 NOK)	–0.005
	(0.028)
R^2	0.735
# obs.	422
# obs. with dptax = 1	109
Estimation method	OLS

Note: Standard errors are in parentheses. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. A constant term is included (but not reported).

Appendix Table 3: Tests for spatial dependence

Moran's I	1.75 (0.08)
LM for spatial lag dependence ($H_0 : \rho = 0$)	3.72 (0.05)
Robust LM for spatial lag dependence ($H_0 : \rho = 0$)	5.11 (0.02)
LM for spatial error dependence ($H_0 : \lambda = 0$)	1.74 (0.19)
Robust LM for spatial error dependence ($H_0 : \lambda = 0$)	3.13 (0.08)

Note: The tests are based on the OLS residuals from specification (3) in Table 2. P-values are in parentheses. The Moran test follows the normal distribution, whereas the LM tests follow a χ^2 distribution with one degree of freedom. The critical values for the $\chi^2_{(1)}$ are 2.71, 3.84, and 6.63 for the 10%, 5%, and 1% significance levels, respectively.

Appendix Table 4: Propensity score equation, logit estimates.

Variable	Coefficient (st. error)
Enrollment	0.029 (0.020)
Enrollment^2/1000	–0.207 (0.112)*
Teacher experience (years and months)	0.164 (0.057)***
Teacher education hours per student	0.034 (0.013)***
Ln (school district population)	1.005 (0.285)***
Share of people living in rural areas	–4.373 (0.938)***
Share of socialists in the local council	7.353 (1.488)***
Party fragmentation in the local council	–5.073 (2.759)*
Median income in the school district (in 1000 NOK)	–0.040 (0.010)***
constant	–8.544 (3.842)**
Pseudo R^2	0.325
Number of observations	425
Final number of blocks	5
Common support	Yes
Balancing property satisfied	Yes

Note: Standard errors are in parentheses. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.