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The Impact of Local Taxes and Public Services on Property Values

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Abstract

Attempts to measure the capitalization of local taxes into property prices, starting with Oates (1969), have suffered from a lack of local public service controls. We revisit this vast literature with a novel dataset of 947 time-varying local characteristic and public service controls for all municipalities in Sweden over the 2010-2016 period. To make use of the high dimensional vector of controls, as well as time and geographic fixed effects, we employ a novel empirical approach that modifies the recently-introduced debiased machine learning estimator by coupling it with a deep-wide neural network. We find that existing estimates of tax capitalization in the literature, including quasi-experimental work, may understate the impact of taxes on house prices by as much as 50%. We also exploit the unique features of our dataset to test core assumptions of the Tiebout hypothesis and to estimate the impact of public services, education, and crime on house prices.

Keywords: Local Public Goods, Tax Capitalization, Tiebout Hypothesis, Machine Learning, Property Prices
JEL-Classification: C45, C55, H31, H41, R3

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1 Introduction

The choice and acquisition of housing is one of the most important financial decisions people encounter throughout their lives. Beyond providing direct utility, housing services are associated with a range of local public goods that are provided on a neighborhood or district level. As such, apart from individual preferences, one can expect that the price of housing will not only be a function of its physical attributes, but also the characteristics of surroundings and local services. In his seminal paper, Tiebout (1956) argued that households, while choosing where to live, "vote with their feet," revealing their preferences for a mix of local public goods and taxes. He sketched out a theory that suggested that this act of preference revelation was sufficient to solve the free-rider problem for local public goods. Prior to Tiebout (1956), Musgrave (1939) and Samuelson (1954) had developed a theory of public goods provision, but did so with respect to aggregate public goods, and found that no market-type mechanism could determine the efficient level of provision.

Oates (1969) provides the first empirical test of the Tiebout hypothesis (1956) and linked property values in the community to the local public budgets, focusing on the effects of property taxation and local expenditure on housing values. He documents that approximately two thirds of changes in property taxation are capitalized into property values. Furthermore, Oates (1969) also started a vast empirical and theoretical literature on the capitalization of local taxes into housing values, which has focused primarily on property taxes.[1] The biggest problem the early literature encountered was that high quality data on local public services was typically not available. Most early papers, starting with Oates (1969), which used public school expenditure per pupil in his regression, had at most two public services as controls (Pollakowski, 1973; King, 1977; Rosen and Fullerton, 1977; Cebula, 1978; and Brueckner, 1979). Since local public services and local taxes are likely to comove, this suggests that omitting a wide range of public services may lead to a biased estimate of the tax capitalization effect. This problem has been widely acknowledged in the

[1]See Pollakowski (1973), Edel and Sclar (1974), Hamilton (1976), Meadows (1976), King (1977), Rosen and Fullerton (1977), Epple et al. (1978), Cebula (1978), Brueckner (1979), Reinhard (1981), Goldstein and Pauly (1981), Yinger (1982), Rosen (1982), Mieszkowski and Zodrow (1989), Palmon and Smith (1998), Bai et al. (2014), and Elinder and Persson (2017).

literature (Wales and Wiens, 1974; and Palmon and Smith, 1998).

Early empirical work tried to improve on Oates' original regressions by including more public services in the regression specification. Reinhard (1981), for instance, included recreational expenditures per capita, crime rates, and expenditures on streets and highways, contrasting with much of the literature, which used only one measure of public expenditures. Still, as Palmon and Smith (1998) note, the literature following Oates (1969) made only incremental improvements over the years and the omission of local public service controls remained a problem due to data availability. Palmon and Smith (1998) overcame the potential bias in the tax capitalization estimates created by an inadequate measurement of public services (or a lack thereof) and the comovement between public services and local taxes by focusing on local areas where public services were essentially fixed, while local taxes varied.

Recent work has increasingly used new data on housing and new econometric methods to estimate the tax capitalization effect. These methods, such as the boundary discontinuity design introduced by Black (1999), try to overcome the aforementioned problems in the literature by using a specification that does not require extensive data on local public services (e.g. Basten et al., 2017). While these recent approaches improve upon earlier estimates of the tax capitalization effect, they can not comment on the importance of local public services that lie at the heart of the Tiebout (1956) hypothesis. Furthermore, they also unable to control for the underlying drivers of the local tax changes, which may include shifts in political preferences and public finance choices. In contrast to recent empirical work, we contribute to the literature by assembling an exhaustive dataset on local public services and local characteristics for Sweden. Given that the property tax is set at the national level in Sweden, we focus on income taxes instead, which vary at the municipal level.[2] We also modify the recently introduced debiased machine learning estimator by Cher-

[2] The tax capitalization literature mostly focuses on estimating property tax capitalization, since most of the work uses U.S. data, where local variation in taxes comes primarily from property taxes; however, a number of papers also focus on capitalization of local income taxes (Rosen, 1979; Stull and Stull, 1991; Morger, 2017; Basten et al., 2017). The last two papers focus on Swiss data. Switzerland grants its municipalities substantial autonomy in determining their finances, and being one of the most prominent examples of fiscal federalism, it provides for a good testing ground for the tax capitalization effect. However, as Morger (2017), p. 247, notes "for Switzerland, information on the quantity and quality of public goods provided at the local level is nonexistent."

nozhukov et al. (2017, 2018) to handle a large set of time-varying controls and fixed effects simultaneously. This allows us to obtain unbiased estimates of tax capitalization and also assess the impact of local public services on house prices. Furthermore, our modification of Chernozhukov et al. (2017, 2018) can be used in many other settings where the specification calls for a high number of controls and fixed effects.

Our main exercise estimates local income tax capitalization into house prices in the presence of an exhaustive dataset on local public services and local characteristics. We first use an OLS specification that includes municipal fixed effects, annual fixed effects, and the set of time-varying controls that is most commonly used in the literature. We find that a one standard deviation increase in local income taxes is associated with a 0.13 standard deviation decrease in property prices. We then apply a modified version of the double machine learning (DML) estimator by Chernozhukov et al. (2017, 2018) to take into account our granular public service and local characteristic data. The original algorithm enables the estimation of a parameter of interest in the presence of a high-dimensional nuisance parameter. In our case, we attempt to measure the capitalization of local income taxes in the presence of a high-dimensional vector of local public services and local characteristics, which may have a nonlinear relationship with house price prices. We adapt this algorithm by combining it with the deep-wide neural network architecture introduced by Cheng et al. (2016) that allows for the joint estimation of a "wide" linear model and a "deep" neural network. This allows us to account for public service and local characteristic variables (the deep, nonlinear part of the network) and both time and geographic fixed effects (the wide or linear part of the network). Using the deep-wide version of DML algorithm (DML-DW), we find that a one standard deviation increase in local income taxes decreases property prices by 0.26 standard deviations. As such, proper accounting for public services more than doubles our estimate of the tax capitalization effect by reducing the downward bias "plaguing even the most recent literature" (Palmon and Smith, 1998, p. 1108). Beyond quantifying the size of the bias, we are also able to demonstrate that it appears to arise primarily from the omission of housing variables (including supply), public finance measures, and public service outputs. This has problematic implications for estimates in the literature, including otherwise well-identified quasi-experimental work, since the tax changes themselves are likely to depend on such variables.

Apart from our methodological contribution, we show how this new econometric approach can be used to test different aspects of the Tiebout (1956) model. Given the Tiebout (1956) theory's prediction that voting with one's feet depends on households being highly mobile and having low moving costs, our tax capitalization results should be stronger in densely populated counties with many municipalities, where households can move at a low cost and without changing jobs. Our results confirm this claim: In urban areas, a one standard deviation increase in the municipal income tax reduces property prices by 1.04 standard deviations. In rural areas, where people's choice of municipalities is limited, the tax capitalization result is almost non-existent: A one standard deviation increase in income taxes decreases property prices by merely 0.01 standard deviations. Following Cebula (1978) and Banzhaf and Walsh (2008), we also test whether people indeed move in response to higher income taxes and we document that a one standard increase in income taxes has a small, but negative impact of net migration. Again, this effect is almost twice as large for high density counties with many municipalities.

Lastly, our rich dataset and new econometric approach can be jointly used to test for the capitalization of different public services into property prices and for examining differential impacts of public service inputs and outputs on house prices. A large literature (Haurin and Brasington, 1996; Black, 1999; Downes and Zabel, 2002; Barrow and Rouse, 2004; Cheshire and Sheppard, 2004; Bayer, Ferreira, and McMillan, 2007; Ries and Somerville, 2010) studies the impact of schooling on house prices. We show that once public services are controlled for properly, schooling inputs (expenditure) have a negative impact on house prices and outputs (test scores and other quality measures) typically have a positive impact. The literature generally finds a positive association between schooling expenditures and house prices; however, this is likely because schooling outputs, such as grades, are correlated with schooling inputs. Using DML-DW and controlling exhaustively for public services, including schooling outputs, we find a negative association between spending per pupil and house prices, which suggests that the positive association documented in the literature is likely to be driven by its relationship with outputs. This supports the sub-literature that emphasizes the use of public service outputs, rather than inputs (Rosen and

Fullerton, 1977; Hanushek, 1986; and Hanushek, 1996).[3]

A closely related literature looks at the effect of crime rates on property prices or expenditures, documenting a negative relation between the two (Thaler, 1978; Reinhard, 1981; Blomquist et al., 1988; Haurin and Brasington, 1996; Gibbons, 2004; Linden and Rockoff, 2008). In line with the literature, we also find a negative relationship between crime and house prices; however, we find that the magnitude of the effect falls by more than 50% when we include exhaustive time-varying local controls. We also find that the effect is larger in urban areas.

The paper proceeds as follows. We describe the data used in Section 2. We proceed with the estimation strategy in Section 3. Section 4 presents and discusses the empirical results. Finally, we conclude in Section 5.

2 Data

In our analysis, we combine three main data sources: data on local house prices, announcements of changes in local income taxes, and a database of public good provision and municipal characteristics. The housing data is scraped from an exhaustive online source of housing transactions.[4] Yearly announcements in local income taxes are provided by Statistics Sweden. The public good provision and municipal characteristics data is scraped from an online aggregator of local and regional statistics.[5] The data spans the 2010-2016 period and includes 947 potential covariates. Figure 1 shows the number of categories of series that are available by type. A finer disaggregation of the series types is given in the Appendix in Section A.2. Note that we have standard controls, including local economic, housing, and labor market conditions. We also have less commonly used controls, such as local public services, demographics, schooling, politics, infrastructure, and migration.

We conduct our analysis at the municipality level. Sweden is divided into 21

[3]Note that Tiebout (1956) writes about local public expenditures. However, Oates (1969) already admits that having a measure of public service outputs would be ideal for testing the Tiebout hypothesis. He chooses to work with a measurement of expenditure per pupil due to data availability, but considers it an imperfect variable for the purpose of the test.

[4]The microdata is collected from booli.se, which aggregates housing transactions.

[5]We scraped the control variables from kolada.se, which is an online aggregator of local and regional statistics for Sweden.

counties and 290 municipalities, which have discretion over the determination of local income taxes. The density of municipalities is highest in the south of Sweden and around the largest cities. For example, Stockholm County is divided into 27 municipalities with different local tax values. As of 2018, it is estimated that the incomes of 64% of people working in Sweden were only taxed at the municipal level (Lidefelt, 2017). In past years, this percentage has exceeded 70%, indicating that the level of income taxes is the most important determinant of the after-tax income of the majority of individuals.[6] Municipality boundaries sometimes run through the middle of the street, so that it suffices to move from one side of the street to the other in order to face a much lower tax burden.[7] Figure 2 shows municipal tax levels for 2010 and 2016, with darker shades indicating higher income tax rates. Notice that southern Sweden is much more densely populated than its northern part, so we might expect tax capitalization to have a stronger effect there. We specifically test for this in Section 4.1.

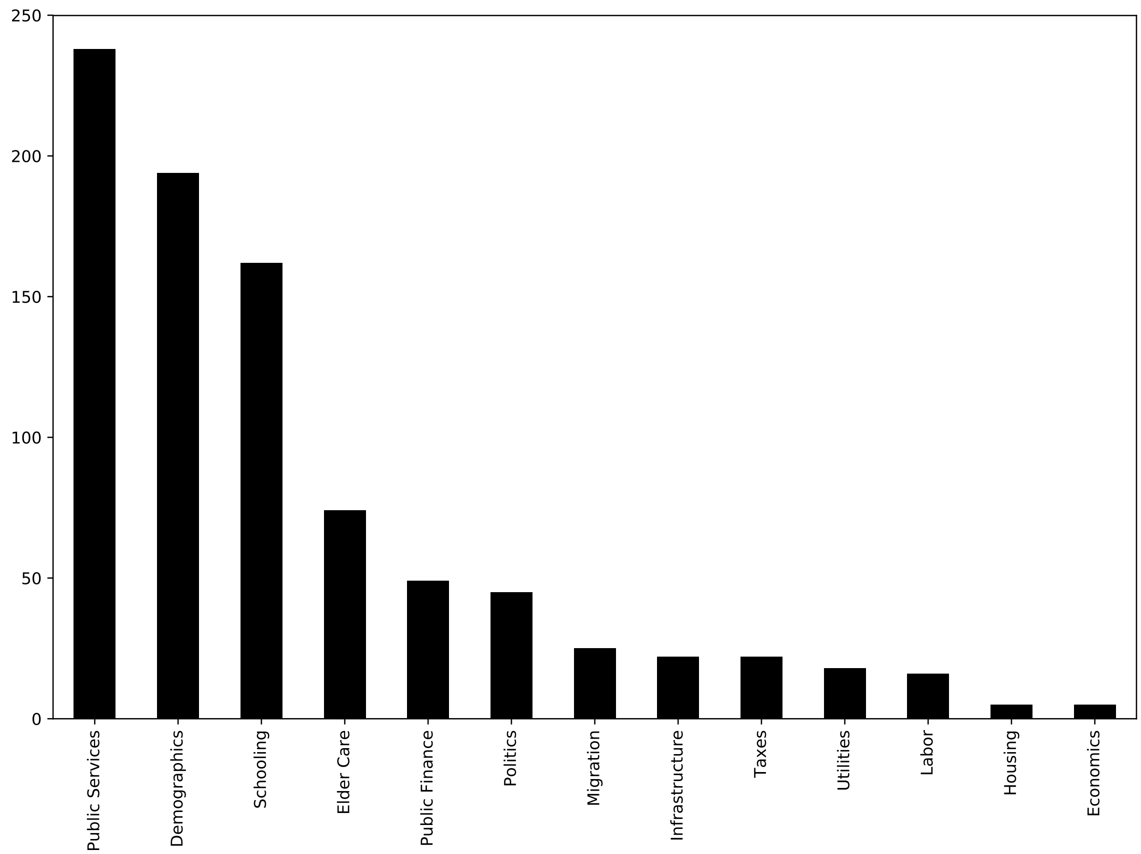
The distribution of taxes across municipalities is shown in Figure 3. Each plot displays the kernel density for a given year. While our set of controls is restricted to the years 2010-2016, tax data is available between 2001 and 2018. We show one plot for every two years, starting in 2010 and ending in 2016. Over this period, the distribution shifted to the right, but only slightly, suggesting that taxes tended to increase on average.

A rightward shift in the tax distribution does not imply that all municipalities tended to see an increase in taxes. In Figure 4, we can see the distribution of all municipal tax changes within a given year. We have both tax increases and decreases in all years. There are also differences in how numerous tax changes are across years. For instance, 2010 witnessed relatively few tax changes in comparison to 2012.

[6]Above a certain income threshold – apart from the income tax levied on the municipality level – individuals are also expected to pay a federal tax on marginal earnings.

[7]It is important to stress that, in Sweden, property taxes are set on a national level. A study testing the implications of a change in property taxes found that the effect of a decrease in property tax has approximately no effect for most properties (Elinder and Persson, 2017).

Figure 1: Frequency of municipal-level control variables by type

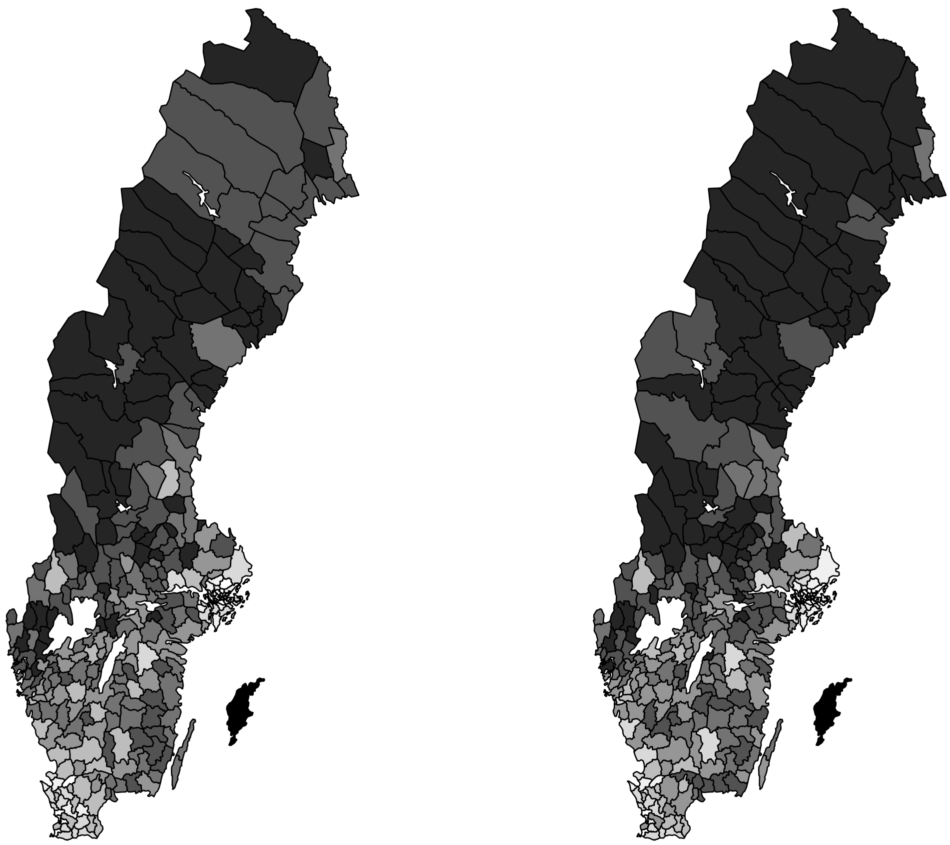


Notes: Our dataset contains 947 municipal-level control variables. This spans categories from demographics and migration to public service inputs and outputs. We assembled the dataset using webscraping and then manually classified the variables into categories using variable titles and descriptions.

3 Estimation Strategy

In this section, we describe our empirical strategy for measuring tax and public service capitalization into house prices. We start by describing the tax capitalization estimation problem and how existing work in the literature has dealt with it. After that, we discuss the estimation technique we use, including our refinement of the original method, which may be useful for empirical work in settings where the econometrician has a high number of time-varying covariates and fixed effects.

Figure 2: Geographical distribution of municipal taxes in Sweden



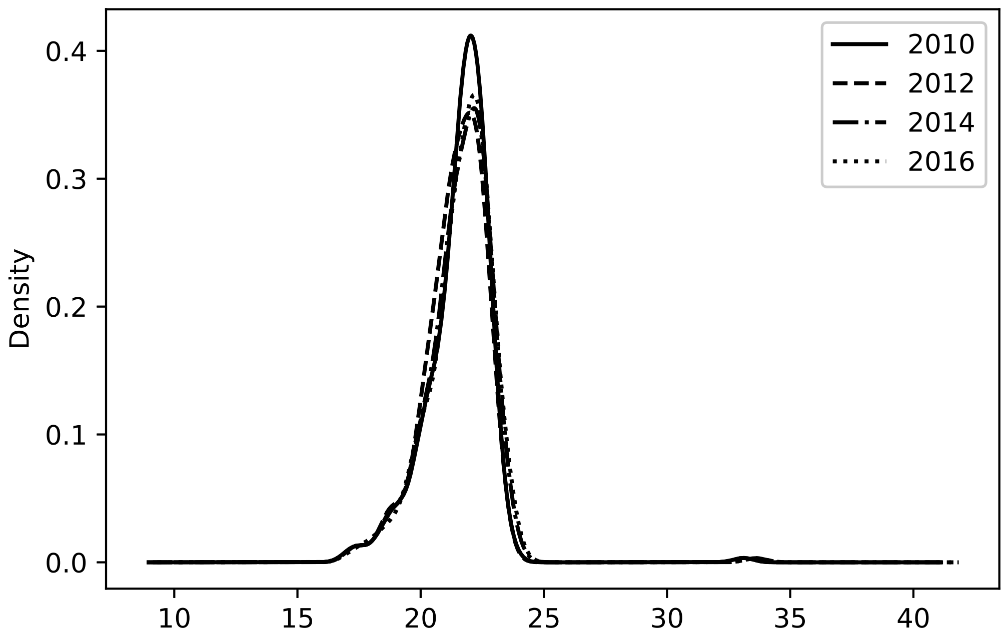
(a) Municipal Tax Level: 2010 (b) Municipal Tax Level: 2016

Notes: The subfigures above show tax rates in the 290 Swedish municipalities. A darker shade indicates a higher rate. Note that subfigure (a) shows the tax rates in 2010 and subfigure (b) shows the tax rates in 2016. Large and geographically isolated municipalities often have higher rates.

3.1 The Estimation Problem

Much of the empirical literature on tax capitalization, including Oates (1969), uses no more than two public services as controls (e.g. Pollakowski, 1973; King, 1977; Rosen and Fullerton, 1977; Cebula, 1978; Brueckner, 1979). Since higher taxes are likely to be associated with higher public service provision, omitting them from the regression specification will lead to biased estimates of tax capitalization. Furthermore, if local taxes tend to capitalize negatively into house prices and local public service provision

Figure 3: The municipal tax distribution for selected years

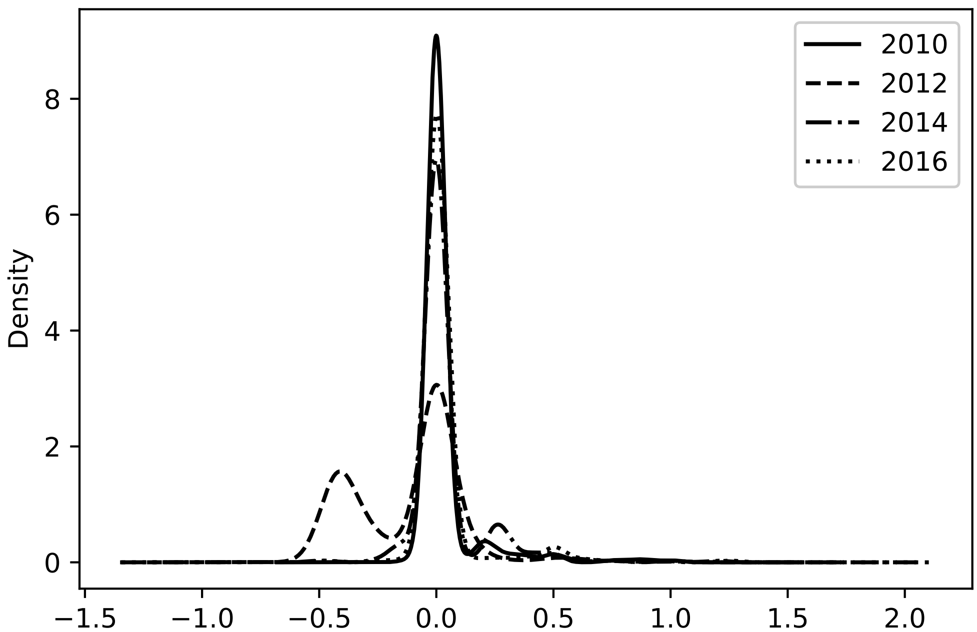


Notes: The figure above shows kernel density plots of the tax distributions for selected years: 2010, 2012, 2014, and 2016. While the plots display tax rates for every second year, we use annual tax rates in the regression analysis. Note that most rates are concentrated between 15% and 25%. Additionally, between 2010 and 2016, we observe a slight rightward shift in the distribution of taxes.

tends to capitalize positively, the direction of the bias will be positive. If the true effect of tax capitalization is negative, then the positive bias will reduce the estimated magnitude.

The empirical literature has also suffered from limitations in time and geographic variation. Most work has examined a small number of municipalities in a particular region and year. Consequently, such estimates of tax capitalization were identified purely off of cross-sectional variation. Given the lack of geographic fixed effects, these estimates were likely to have confounded tax capitalization with permanent features of geographic locations. The unchanging components of housing supply elasticity, such as natural barriers to construction (e.g. Saiz, 2010), can be captured through

Figure 4: The municipal tax change distribution for selected years



Notes: The figure above shows the distribution of municipal-level tax changes in selected years. The tax rate change for 2010, for instance, is computed as the rate that applied in 2010, less the 2009 rate. Note that there are substantial differences across years in the share of municipalities with tax rate changes.

the use of fixed effects. If taxes tend to be higher in areas with low supply elasticities, such as coastal cities, then we might expect the omission of geographic fixed effects to create a positive bias in tax capitalization measurements. Overall, however, the impact of the omission of geographic fixed effects is directionally ambiguous.

In addition to the inclusion of public service controls and adequate time and geographic variation, much of the empirical literature also lacks local characteristic controls. Demographics, migration, labor markets, political preferences, housing supply, and economic conditions all comove cross-sectionally and over time with both taxes and house prices. Even in a specification with sufficient time variation to include geographic fixed effects, biases will still emerge from failing to properly control for movements in local characteristics. As with the omission of geographic fixed effects, the direction of this bias is unclear ex-ante.

As Palmon and Smith (1998) point out, such biases contaminated estimates in the empirical tax capitalization literature up until the late 1990s. More recent work has dealt with these biases by making use of better data and newer econometric methods. Notably, Basten et al. (2017) and Morger (2017) make use of microdata on apartment rentals in Switzerland with substantial time and geographic variation. Basten et al. (2017) sets up a regression discontinuity design, exploiting the fact that income taxes depend discretely on residency, but public service access depends continuously on the distance from the service provided. Morger (2017) uses a hedonic regression and exploits time and geographic variation to control for unobserved public services. In both cases, the authors use high quality rental data, coupled with substantial time and geographic variation, to obtain unbiased estimates of tax capitalization without including data on local public service provision, which is unavailable in Switzerland. Earlier work in the literature, including Black (1999), made use of a border discontinuity design, but for the purpose of estimating the impact of public services on house prices. We contribute to the literature by introducing a new dataset that contains an exhaustive set of local public services and local characteristics. We also propose an alternative way to estimate the tax capitalization effect that does not rely on the assumption that local public services are fixed. This approach can also be used for a variety of different empirical questions.

3.2 Double Machine Learning

Our empirical strategy attempts to overcome the problems identified by the literature and, in doing so, makes two contributions. First, we introduce a novel and comprehensive dataset on house prices, public services, taxes, and local characteristics for Sweden. The dataset spans all 290 municipalities and covers the time period between 2010 and 2016. To our knowledge, it contains the most comprehensive set of public services and local controls of any paper in the tax capitalization literature. Second, we adopt an empirical strategy that is capable of handling a high number of covariates that may have a nonlinear relationship with the dependent variable. As Athey et al. (2017) points out, this is precisely where machine learning (ML) algorithms are most useful in economics. They allow for a flexible functional form assumption, but require the researcher to impose discipline through the use of out-of-sample prediction. The standard model training process involves splitting the sample into training and validation sets, where the validation set is used to detect overfitting.

One problem with using ML for causal inference is that models typically do not produce consistent parameter estimates (Mullainathan and Speiss, 2017). This is because ML models are designed for prediction accuracy, rather than inference. Individual parameter estimates are typically not objects of interest. Recently, however, the econometrics literature has started modifying ML methods for use within economics (Varian, 2014; Mullainathan and Speiss, 2017; Athey, 2017; Athey et al., 2017; Athey and Wager, 2017). Notably, the debiased machine learning (DML) estimator, which was recently introduced by Chernozhukov et al. (2017, 2018), allows for the unbiased estimation of a parameter of interest in the presence of a high dimensional and potentially nonlinear nuisance parameter. We will make use of DML to estimate the tax capitalization effect. Furthermore, we will modify the DML approach by employing a neural network architecture that was recently introduced in the machine learning literature by Cheng et al. (2016): the "deep-wide" network. The benefit of this modification is that it allows for arbitrary nonlinear interactions between time-varying controls, such as public services and local characteristics, but uses a parsimonious specification for fixed effects that prohibits unintended interactions. The same cannot be achieved with standard deep neural network architectures, random forests, lasso regression, or elastic net regressions.

More formally, we treat the problem of estimating tax capitalization as a partially linear model, as described by Robinson (1988) and Chernozhukov et al. (2017). We follow the exposition and notation introduced in Chernozhukov et al. (2017). In Equation (1), p_{jt} is the square meter price of housing, τ_{jt} is the level of municipal taxes, x_{jt} is a k-dimensional vector of confounders, $(x_{jt}^1, ..., x_{jt}^k)$, and u_{jt} is the disturbance term.

$$p_{jt} = \tau_{jt}\theta_0 + g_0(x_{jt}) + u_{jt} \tag{1}$$

The functional form, $g_0(x_{jt})$, allows for the high-dimensional vector of confounders to have a direct and potentially nonlinear impact on p_{jt}. We assume that the disturbance term is zero in expectation, conditional on x_{jt} and τ_{jt}:

$$E[u_{jt}|x_{jt}, \tau_{jt}] = 0 \tag{2}$$

This setup also allows for the possibility that confounders may influence the determination of municipal tax rates:

$$\tau_{jt} = m_0(x_{jt}) + v_{jt} \tag{3}$$

We might expect, for example, that demographic variation across municipalities and time, captured in x_{jt}, will affect both τ_{jt} and p_{jt}. We allow for the confounders to do this through $m_0(x_{jt})$ and $g_0(x_{jt})$ separately. Finally, we assume that the disturbance term is zero in expectation, conditional on the vector of confounders:

$$E[v_{jt}|x_{jt}] = 0 \tag{4}$$

If τ_{jt} is conditionally exogenous, then θ_0 may be interpreted as the unbiased treatment effect. In order to capture all plausible sources of confounding, k must be large; however, k being large is also problematic, as Chernozhukov et al. (2017) show, because k is typically assumed to be growing slowly in the sample size. In our case, k is large, which violates standard assumptions about nuisance parameter complexity.

Chernozhukov et al. (2017) show that estimating the nuisance parameter, $\eta_0 = (g_0, m_0)$, with machine learning techniques and then adding it to the estimating equations directly will lead to a bias in θ_0. This arises from the use of regularization techniques, which allow machine learning methods to use a high number of covariates without overfitting. Chernozhukov et al. (2017) demonstrate how to correct for this bias using orthogonalization and sample splitting. We start by splitting the dataset into two equal parts: I and I^c. We use I^c to estimate $\hat{m}_0$ and $\hat{g}_0$. We can then estimate the parameter of interest for a given split:

$$\hat{\theta}_0(I^c, I) = \left(\frac{1}{\bar{n}\bar{t}}\Sigma_{jt\in I}\hat{v}_{it}\tau_{jt}\right)^{-1} \frac{1}{\bar{n}\bar{t}}\Sigma_{jt\in J}\hat{v}_{jt}\Big(p_{jt} - \hat{g}_0(x_{jt})\Big) \tag{5}$$

Note that $\bar{n}$ and $\bar{t}$ are the number of municipalities and time periods in I. Furthermore, notice that $\hat{\theta}_0(I^c, I)$ uses $\hat{m}_0$ and $\hat{g}_0$ estimated on the auxiliary sample, I^c, and p_{jt} and x_{jt} from the main sample, I. The final step swaps the main and auxiliary samples, computes $\hat{\theta}_0(I, I^c)$, and calculates the mean:

$$\hat{\theta}_0 = \frac{1}{2}[\hat{\theta}_0(I^c, I) + \hat{\theta}_0(I, I^c)] \tag{6}$$

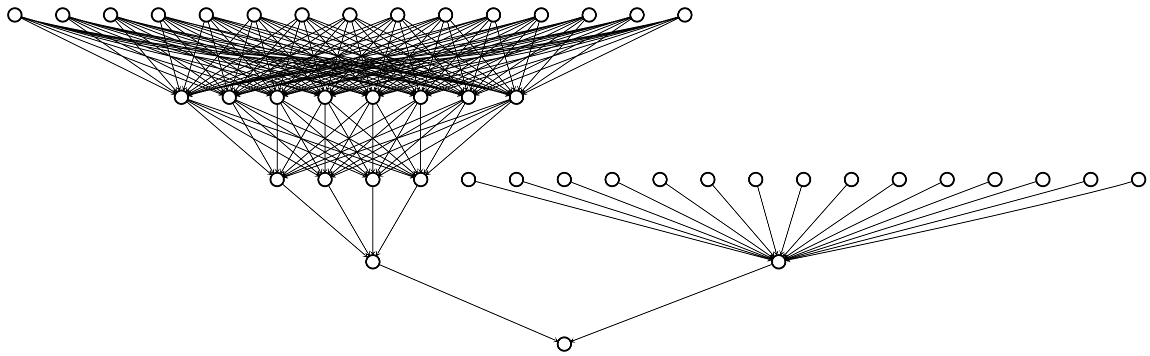
Following Chernozhukov et al. (2017), we repeat this procedure and then compute the median parameter value. In the following subsection, we will discuss how we modify this specification to allow for the use of fixed effects.

3.3 Deep-Wide Network Architecture

Chernozhukov et al. (2017) point out that DML can be performed with a wide variety of machine learning estimators, including random forests, penalized linear regression, and neural networks. We make use of a neural network to construct the DML estimator in this paper, but we use an atypical network architecture that is ideally suited to our problem and a large class of estimation problems in economics. We do this by importing the "deep-wide" network that was recently introduced in the machine learning literature by Cheng et al. (2016). Its architecture consists of two subnetworks: 1) a deep neural network; and 2) a "wide" or linear subnetwork. Figure 5 depicts the network's architecture. The deep subnetwork is shown on the left and the wide subnetwork is shown on the right.

In our implementation, the high dimensional vector of municipal characteristics and public services are used as inputs to the deep subnetwork, allowing for arbitrary nonlinear interactions between the control variables. This would not be possible in a penalized linear regression model, such as a lasso, which does not permit nonlinearities. Furthermore, while other machine learning methods, such as random forests and standard neural network architectures do allow for nonlinearities, they also allow for unintended interactions between fixed effects. In a deep-wide network, the fixed effects are handled as inputs to the linear subnetwork, allowing for an efficient parameterization and also preventing unintended interactions. Finally, our choice of architecture also allows for joint estimation of the deep and wide subnetworks, which provides an advantage over using an ensemble of separately-estimated fixed effect and

Figure 5: Deep-wide neural network architecture

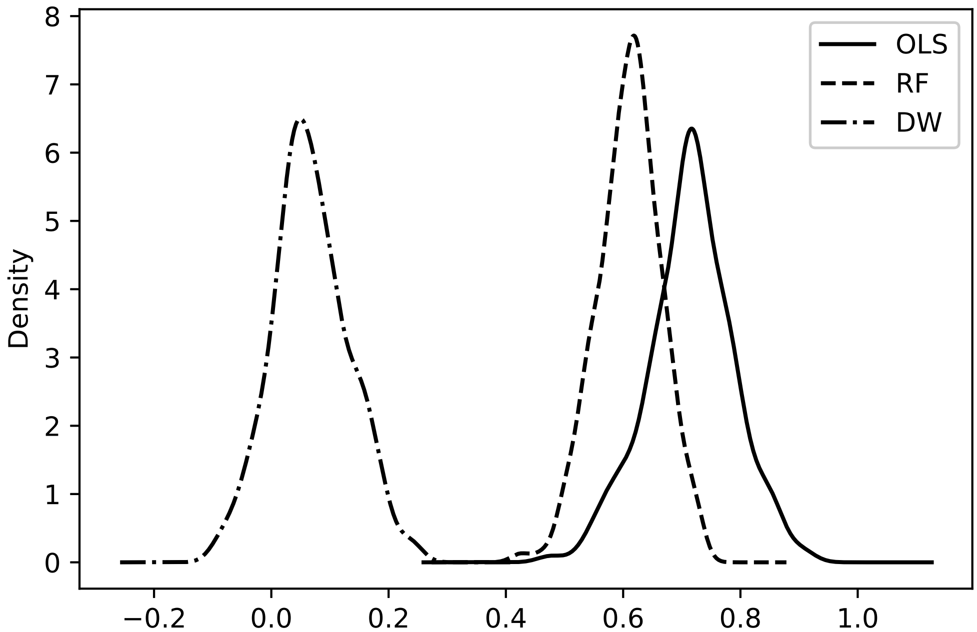


Notes: The figure above shows a deep-wide neural network. The "deep" part of the network is shown on the left and the "wide" part is shown on the right. Note that the deep component allows for arbitrary nonlinear interactions between control variables, which are captured by activation functions that are applied at each node. The wide part of the network efficiently parameterizes the fixed effects by embedding them in a linear model. The deep and wide components of the model are then trained jointly using the adaptive moment optimizer.

neural network models.

Beyond our application to tax capitalization, the deep-wide network refinement of DML could serve as a tool for estimating average treatment effects in any setting with a high number of covariates and fixed effects. We perform a Monte Carlo simulation to evaluate its performance relative to OLS and a random forest model. In particular, we specify data generating processes for both the dependent variable (house prices) and the variable of interest (municipal taxes) that consist of time fixed effects, municipal fixed effects, and time-varying controls. The processes are consistent with the aforementioned partially linear model setup; however, we assume that there is no nonlinear dependence on controls, which advantages OLS over DML. We then draw 2000 observations for each of 500 simulations. Figure 6 shows kernel density plots of the coefficient estimate distributions for OLS, a random forest with DML (RF), and a deep-wide network with DML (DW). For OLS, we exclusively use the set of fixed effects; however, including a random subset of the covariates yields similar results. For the RF and DW specifications, we use the full set of fixed effects and covariates. The OLS model yields a large and positive bias. The RF model, which includes the full set of covariates, reduces the bias slightly. Finally, the DW model is approximately unbiased. For the full details of the Monte Carlo simulation, see Section A.3.

Figure 6: Kernel density plots of coefficient estimates



Notes: The figure above shows kernel density plots of estimated coefficient biases from a Monte Carlo simulation. In each case, we randomly generate data and then use it to estimate the parameter of interest using OLS, DML with a random forest, and DML with a deep-wide network. Note that double ML with a deep-wide network is approximately unbiased; whereas, both OLS and the DML specification with a random forest yield a substantial positive bias.

We implement a custom version of the DML-DW algorithm in TensorFlow. Our code allows for flexibility in the choice of network architecture, including the number of hidden layers, the number of nodes in each layer, the type of activation functions, the type of regularization employed, and the optimization algorithm employed. We also allow for automatic selection of the number of training epochs[8] based on out-of-sample prediction performance to prevent overfitting. The technique and code could be employed to estimate parameters of interest in a large class of problems

[8]The training process in machine learning typically divides the sample into batches, which are fed into the optimizer in sequence. Training a model entails stepping over the complete sample tens or hundreds of times. Each pass over the entire sample is referred to as an epoch.

with a high number of covariates and fixed effects. Furthermore, our code also allows for the estimation of a deep-deep network, shown in Figure 7 of Section A.1 in the Appendix. While we do not employ the deep-deep network in this paper, it has useful properties that could also enhance the DML estimator. Namely, it could be used to allow geographic fixed effects to interact with all controls in one deep network and time fixed effects to interact with all controls in another deep network, but could prohibit unintended interactions between time and geographic fixed effects.[9]

4 Results

In this section, we discuss the empirical results, starting with our findings for tax capitalization. We then move on to migration, which was emphasized by Tiebout (1956) as the mechanism through which tax changes capitalize into house prices. We then discuss how public services and crime affect house prices. Finally, for each empirical exercise, we provide separate estimates for urban areas only. Within these areas, municipal competition is higher, which should lead to increased tax capitalization according to Tiebout (1956).

4.1 Tax Capitalization

We first examine tax capitalization in a simple empirical setting with OLS. Our baseline specification, shown in Equation (7), regresses the square meter price of housing at the municipal level, p_{jt}, on the municipal tax rate, τ_{jt}; municipal fixed effects, γ_j; and yearly time fixed effects, η_t. Notice that we do not use time-varying controls in this specification.

$$p_{jt} = \tau_{jt}\theta_0 + \gamma_j + \eta_t + u_{jt} \tag{7}$$

We also use a separate specification that includes a set time-varying controls, x_{jt}, that is standard in the literature. This includes spending per pupil, a measure of grades, and the number of violent crimes per 100,000 inhabitants. This specification

[9]The code for both the DML deep-wide and DML deep-deep estimators will be made available on the authors' websites.

is shown in Equation (8). Throughout this section, we use standardized versions of the square meter house price, the municipal tax rate, and time-varying controls. All results should be interpreted as the change in the dependent variable in standard deviations associated with an increase in the variable of interest in standard deviations.

$$p_{jt} = \tau_{jt}\theta_0 + \gamma_j + \eta_t + x_{jt} + u_{jt} \tag{8}$$

Table 1 shows our baseline set of estimates for tax capitalization. Column (1) uses the specification in Equation (7) and column (2) uses the specification in Equation (8). Both columns report standard errors that are clustered at the municipal level. Note that we cannot use a specification with municipality-year fixed effects, since the variation in the dependent variable is at the municipality-year level. The only confounders omitted are the time-varying public goods and services, and time-varying municipal characteristics that are novel to this paper.

Next, we employ the DML algorithm to produce estimates that incorporate both the full set of fixed effects and also the time-varying controls. We follow the approach outlined in Section 3. The results given in column (3) are for the random forest estimator (DML-RF). Column (4) uses DML, coupled with the deep-wide architecture refinement we import from the machine learning literature (DML-DW). Notice that the checkmarks (✓) indicate whether a group of time-varying controls is included. Both (3) and (4) incorporate the full set of control group categories, which includes housing supply, migration, politics, labor, demographics, economics, public finance, public service inputs, and public service outputs.

We modify the setup described in Section 3.2 by incorporating the municipality and year fixed effects into the nuisance parameter, as shown in Equation (9). The original process is given by Equation (1). Furthermore, we still allow the time-varying controls to have a potentially nonlinear impact on the square meter price through g_0, while restricting the fixed effects to enter linearly into the model through γ_j and η_t. For the DML-RF setup, however, the municipal and time fixed effects enter through g_0, potentially generating undesired interactions.

$$p_{jt} = \tau_{jt}\theta_0 + g_0(x_{jt}) + \gamma_j + \eta_t + u_{jt} \tag{9}$$

Table 1: Impact of municipal income tax level on house prices

	(1) (OLS)	(2) (OLS)	(3) (DML-RF)	(4) (DML-DW)
municipal_tax$_{it}$	-0.1145**	-0.1236***	-0.1303***	-0.2626***
	(0.051)	(0.051)	(0.011)	(0.031)
Standard Controls	✗	✓	✓	✓
Housing Supply	✗	✗	✓	✓
Migration	✗	✗	✓	✓
Political	✗	✗	✓	✓
Labor	✗	✗	✓	✓
Demographic	✗	✗	✓	✓
Economic	✗	✗	✓	✓
Public Finance	✗	✗	✓	✓
Public Service Inputs	✗	✗	✓	✓
Public Service Ouputs	✗	✗	✓	✓
Schooling	✗	✗	✓	✓
Year FE	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES
Standard Errors	CL	CL	-	-
Adj. R-squared	0.9531	0.9535	-	-
N	1764	1764	1764	1764

Notes: The dependent variable is the standardized square meter price of a villa in municipality (kommun) i and year t. The regressor of interest is the standardized municipal-level income tax rate, which is observed annually. Standard controls include measurements of spending per pupil, education, and violent crime. Fixed effects are applied at the municipal level. DML refers to double-debiased machine learning. DW indicates that a deep-wide network was used. Note that a ✓indicates that the referenced group of control variables was included. CL indicates that standard errors are clustered at the municipal level. * $p < .1$, ** $p < .05$, *** $p < .01$.

Similarly, we modify the estimating equation for τ_{jt}, given in Equation (3) by incorporating municipality and time fixed effects, as is shown in Equation (10).

$$\tau_{jt} = m_0(x_{jt}) + \delta_j + \xi_t + v_{jt} \tag{10}$$

The remainder of the DML algorithm is executed as described in Section 3. The "deep" side of the network contains two hidden layers, one with 16 nodes and one with 8 nodes. We use rectified linear unit activation functions for all hidden layers. The architecture for the deep side is also designed to prevent overfitting. We accomplish this in two ways. First, we apply regularization to each hidden layer of the network. Second, we divide the dataset into training (80%) and validation (20%) splits, and select the number of epochs at which the train and validation samples have approximately the same loss function value. We use a mean absolute error loss function and the adaptive moment optimizer.[10] Furthermore, we apply the DML step 51 times and select the median estimate, similar to what is done in Chernozhukov et al. (2017, 2018).

First, notice that our baseline OLS estimate of tax capitalization is negative, which is consistent with the previous literature. Column (1) indicates that a one standard deviation increase in the level of the municipal tax is associated with a 0.1145 standard deviation decrease in the square meter house price. In column (2), the inclusion of standard controls from the literature increases the magnitude of the estimate from -0.1145 to -0.1236 and increases its significance from the 5% level to the 1% level. Furthermore, including the full set of controls in the DML-RF specification generates a further increase in magnitude to -0.1303. Finally, our baseline DML-DW estimate, shown in column (4), yields a tax capitalization impact that is roughly twice as large as our estimate using OLS with fixed effects and standard controls or DML-RF using a full set of fixed effects and time-varying controls. In particular, we find that a one standard deviation increase in the income tax at the municipal level is associated with a 0.26 standard deviation decrease in the square meter price of housing.

Beyond our baseline estimate, we can also see that selective inclusion of control groups results in range of coefficient estimates from -0.194 to -0.34. This is shown in Tables 2 and 2, which provide the tax capitalization estimates using just one of the control groups in each of the columns. Furthermore, notice that we divide public

[10]The adaptive moment estimator or "adam" was introduced by Kingma and Ba (2014). It is one of the most commonly-used optimization algorithms in machine learning and has several attractive properties for the class of problems we try to solve. First, it applies different step sizes to each component of the gradient, which is useful for high-dimensional optimization problems. Second, it has a parsimonious set of hyperparameters, which can easily be interpreted and tuned. Third, it has good convergence properties. And fourth, it has been demonstrated to perform well empirically in a large variety of applications.

services into two control groups: inputs and outputs. Oates (1969) was the first to point out that using public expenditures (inputs), as Tiebout (1956) suggested, would only imperfectly capture outputs, which was the true object of interest. While Oates (1969) did not use measures of public service and good outputs due to data availability, later work in the literature (Rosen and Fullerton, 1977; Hanushek, 1986; and Hanushek, 1996) confirmed the importance of outputs. We find that the inclusion of outputs leads to a larger increase in the magnitude of the impact of tax capitalization, which supports the claim that inputs are an imperfect proxy for outputs. We will revisit this question again when we measure the impact of public goods and services.

Furthermore, the selective inclusion of control groups allows us to identify which confounders are likely to have the largest effect on the bias in tax capitalization estimates. In particular, we find that housing variables (including measures of supply), public service outputs, and public finance variables have the largest impact on tax capitalization estimates. Thus, failure to properly control for such variables may suggest that estimates in the literature will tend to be positively biased. Even in quasi-experimental settings, the exclusion of public finance variables and public service variables could be problematic, since they could be partially responsible for the shifts in local tax rates.

We next split our sample along urban-rural lines. The urban subsample contains all observations associated with municipalities in Sweden's most populous counties: Stockholm, Skåne, and Västra Götaland. These counties are also more population-dense and municipality-dense than the remaining 18. Furthermore, all variables are re-standardized within the urban subsample. Table 4 provides results for the DML-DW estimator with municipality and time fixed effects, as well as a complete set of time-varying controls. Note that the estimated impact for rural areas is approximately zero; whereas the impact for urban areas is roughly four times greater in magnitude than the baseline estimate for the full sample. This accords well with Tiebout (1956), which argues that the impact should be greatest where moving costs are low. In this case, the cost of moving between municipalities should be lowest in dense urban areas, where many alternatives are available and where moving will not typically require a change in employment.

4.2 Migration

Following Cebula (1978) and Banzhaf and Walsh (2008), we further examine the claim in Tiebout (1956) that households will "vote with their feet." We do this by directly measuring the impact of municipal taxes on net migration into the municipality. A positive rate of net migration indicates that more individuals are entering the municipality than are leaving it in a given year. Our results are given in Table 5. We first show OLS with two different specifications in columns (1) and (2). Column (1) uses year and municipal fixed effects and column (2) includes spending per pupil, grades, and violent crimes per 100,000 inhabitants as controls. In both OLS specifications, the measured impact of municipal income taxes on net migration is negative, but insignificant. In column (3), we switch to DML-DW with time and municipality fixed effects, as well as the full set of time-varying controls. This yields a substantially lower impact of -0.062, which is significant at the 1% level. When we limit ourselves

Table 2: Impact of control groups on tax capitalization estimates (II)

	(1) (DML-DW)	(2) (DML-DW)	(3) (DML-DW)	(4) (DML-DW)	(5) (DML-DW)
municipal_tax$_{it}$	-0.3438***	-0.1942***	-0.2173***	-0.1933***	-0.2092***
	(0.0483)	(0.0311)	(0.0280)	(0.0348)	(0.0374)
Housing Supply	✓	✗	✗	✗	✗
Migration	✗	✓	✗	✗	✗
Political	✗	✗	✓	✗	✗
Labor	✗	✗	✗	✓	✗
Demographic	✗	✗	✗	✗	✓
Economic	✗	✗	✗	✗	✗
Public Finance	✗	✗	✗	✗	✗
Public Service Inputs	✗	✗	✗	✗	✗
Public Service Ouputs	✗	✗	✗	✗	✗
Schooling	✗	✗	✗	✗	✗
Year FE	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES
N	1764	1764	1764	1764	1764

Notes: The dependent variable is the standardized square meter price of a villa in municipality (kommun) i and year t. The regressor of interest is the standardized municipal-level income tax rate, which is observed annually. Fixed effects are applied at the municipal level. DML refers to double-debiased machine learning. DW indicates that a deep-wide network was used. Note that a ✓ indicates that the referenced group of control variables was included. * $p < .1$, ** $p < .05$, *** $p < .01$.

to urban areas only in column (4), this estimate nearly doubles to -0.1063, suggesting that the underlying mechanism in Tiebout (1956) for tax capitalization may, indeed, be supported empirically. Furthermore, it is plausible that the long-run effect could be larger, since many households might be unable to move within a year of the tax change's announcement.

4.3 Education

We next make an attempt to directly measure the impact of public services on house prices. We start by examining education, since this is one of the most frequently tested public services in the literature (see, e.g., Haurin and Brasington, 1996; Black, 1999; Downes and Zabel, 2002; Barrow and Rouse, 2004; Cheshire and Sheppard, 2004; Bayer et al., 2007; Ries and Somerville, 2010). In most work, either spending per pupil is used an input or grades are used as an output. Table 6 shows our results

Table 3: Impact of control groups on tax capitalization estimates (II)

	(1) (DML-DW)	(2) (DML-DW)	(3) (DML-DW)	(4) (DML-DW)	(5) (DML-DW)
$\text{municipal\_tax}_{it}$	-0.2176***	-0.2982***	-0.2587***	-0.3173***	-0.2184***
	(0.0336)	(0.0353)	(0.0335)	(0.0393)	(0.0323)
Housing Supply	✗	✗	✗	✗	✗
Migration	✗	✗	✗	✗	✗
Political	✗	✗	✗	✗	✗
Labor	✗	✗	✗	✗	✗
Demographic	✗	✗	✗	✗	✗
Economic	✓	✗	✗	✗	✗
Public Finance	✗	✓	✗	✗	✗
Public Service Inputs	✗	✗	✓	✗	✗
Public Service Ouputs	✗	✗	✗	✓	✗
Schooling	✗	✗	✗	✗	✓
Year FE	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES
N	1764	1764	1764	1764	1764

Notes: The dependent variable is the standardized square meter price of a villa in municipality (kommun) i and year t. The regressor of interest is the standardized municipal-level income tax rate, which is observed annually. Fixed effects are applied at the municipal level. DML refers to double-debiased machine learning. DW indicates that a deep-wide network was used. Note that a ✓ indicates that the referenced group of control variables was included. * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 4: Impact of municipal income tax level on house prices by population density

	(1) (DML-DW)	(2) (DML-DW)
municipal_tax$_{it}$	-1.043***	-0.008***
	(0.0216)	(0.003)
Year FE	YES	YES
Municipal FE	YES	YES
Time-Varying Controls	YES	YES
Counties	Urban	Rural
N	658	1106

Notes: The dependent variable is the standardized square meter price of a villa in municipality (kommun) i and year t. The regressor of interest is the standardized municipal-level income tax rate, which is observed annually. We use two subsamples: 1) Urban and 2) Rural. DML refers to double-debiased machine learning. DW indicates that a deep-wide network was used. * $p < .1$, ** $p < .05$, *** $p < .01$.

for spending per pupil and grades.

Since spending per pupil is an input, we might expect that the positive effect found in the literature arises from the omission of variables for schooling output, such as grades and quality measures, which comove with spending. In columns (1) and (2), we make use of our extensive set of time-varying municipal controls to estimate the impact of spending per pupil in isolation. In particular, in column (1), we use DML-DW and include year and municipal fixed effects, as well as time-varying controls. This includes educational outputs, such as grades. We find that a one standard deviation increase in spending is associated with a 0.021 standard deviation decrease in the square meter price of housing. Furthermore, when we use a subsample that is limited to urban areas only – where moving costs are low and municipal competition is high – the magnitude of the effect increases further from -0.021 to -0.039. This is what we might expect, given that we are able to include controls for educational outputs and other highly correlated public services.

We next measure the impact of outputs, rather than inputs, using grades as a

Table 5: Impact of taxes on net migration

	(1) (OLS)	(2) (OLS)	(3) (DML-DW)	(4) (DML-DW)
municpal_tax$_{it}$	-0.0576	-0.0535	-0.062***	-0.1063***
	(0.0659)	(0.0600)	(0.005)	(0.009)
Year FE	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES
Standard Controls	NO	YES	YES	YES
Time-Varying Controls	NO	NO	YES	YES
Counties	ALL	ALL	ALL	URBAN
Standard Errors	CL	CL	-	-
Adj. R-squared	0.888	0.888	-	-
N	1764	1764	1764	658

Notes: The dependent variable is standardized net migration in (kommun) i and year t. The regressor of interest is the standardized spending per pupil at the municipality-level, which is observed annually. DML refers to double-debiased machine learning. DW indicates that a deep-wide network was used. CL indicates that standard errors are clustered at the municipal level. * $p < .1$, ** $p < .05$, *** $p < .01$.

measure. The results are shown in Table 6. Column (3) uses DML-DW, coupled with year and municipal fixed effects, and time-varying controls, but yields no significant effect. If we again restrict ourselves to urban areas, the size of the effect rises to 0.021 and becomes significant at the 1% level. This effect, however, remains quite small, even in urban counties, suggesting that much of the apparent positive association between educational outcomes and house prices might actually be capturing comovement with omitted municipal characteristics. It is also plausible, of course, that the long-run impact of improvements in grades could be substantially higher.

4.4 Crime

In addition to measuring tax and public service capitalization, we also examine the impact of crime on house prices in a final exercise. While this question differs slightly from the core aim of this paper, it is closely related and allows us to contribute to a large literature on the subject (see, e.g., Thaler, 1978; Reinhard, 1981; Blomquist et al., 1988; Haurin and Brasington, 1996; Gibbons, 2004; Linden and Rockoff, 2008) using our novel dataset, coupled with the DML-DW approach. Our estimates are given in Table 7. The dependent variable is the standardized square meter price of housing and the variable of interest is the standardized number of violent crimes per 100,000 inhabitants. Column (1) provides results for an OLS specification with year and municipal fixed effects. Here, we find that one standard deviation increase in crime is associated with a -0.0385 standard deviation decrease in house prices. When we add spending per pupil and grades as controls, we get a slight increase in the magnitude of the estimate to -0.389. Furthermore, when we use DML-DW and include the full set of time-varying controls, and year and municipal fixed effects, we find that the impact of crime is reduced by more than half to -0.0154. Limiting the sample to urban areas increases the magnitude of the effect to -0.054. All estimates are significant at the 1% level. Additionally, all variables are re-standardized in the urban subsample. Overall, our findings suggest that the impact of violent crime on property prices is negative, but small in the short run. As with educational outputs, it is plausible that the impact of crime could be more substantial in the long-run.

Table 6: Impact of educational inputs and outputs on house prices

	(1) (DML-DW)	(2) (DML-DW)	(3) (DML-DW)	(4) (DML-DW)
spending_per_pupil$_{it}$	-0.0208***	-0.039***	-	-
	(0.002)	(0.006)	-	-
grades$_{it}$	-	-	0.0011	0.0211***
	-	-	(0.005)	(0.009)
Year FE	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES
Time-Varying Controls	YES	YES	YES	YES
Counties	ALL	URBAN	ALL	URBAN
N	1764	658	1764	658

Notes: The dependent variable is the standardized square meter price of a villa in municipality (kommun) i and year t. The regressor of interest is the standardized spending per pupil at the municipality-level and a measure of grades at the municipal-level, both of which are observed annually. DML refers to double-debiased machine learning. DW indicates that a deep-wide network was used. * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 7: Impact of crime on house prices

	(1) (OLS)	(2) (OLS)	(3) (DML-DW)	(4) (DML-DW)
crime_{it}	-0.0385***	-0.0389***	-0.0154***	-0.0540***
	(0.0142)	(0.0140)	(0.003)	(0.008)
Year FE	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES
Standard Controls	NO	YES	YES	YES
Time-Varying Controls	NO	NO	YES	YES
Counties	ALL	ALL	ALL	URBAN
Standard Errors	CL	CL	-	-
Adj. R-squared	0.9532	0.9532 -	-	
N	1764	1764	1764	658

Notes: The dependent variable is the standardized square meter price of a villa in municipality (kommun) i and year t. The regressor of interest is the standardized number of violent crimes per 100,000 inhabitants. DML refers to double-debiased machine learning. DW indicates that a deep-wide network was used. * $p < .1$, ** $p < .05$, *** $p < .01$.

5 Conclusion

Tiebout (1956) first argued that the free-rider problem for local public goods could be resolved entirely through preference revelation. That is, households could "vote with their feet" by moving to a community that offered their preferred bundle of tax rates and public goods. Oates (1969) followed Tiebout (1956) by providing a first empirical test of the theory by measuring the impact of local taxation and expenditures on housing values. He found that approximately two thirds of changes in property taxation were capitalized into prices. In addition to this, his work spawned a large empirical literature on the capitalization of local taxes into housing values.[11]

Since Oates (1969), the empirical literature has struggled to obtain unbiased estimates of tax capitalization. The main problem is that measures of public services are typically not available at the local level. Consequently, the omission of public

[11] See Pollakowski (1973), Edel and Sclar (1974), Hamilton (1976), Meadows (1976), King (1977), Rosen and Fullerton (1977), Epple et al. (1978), Cebula (1978), Brueckner (1979), Reinhard (1981), Goldstein and Pauly (1981), Yinger (1982), Rosen (1982), Mieszkowski and Zodrow (1989), Palmon and Smith (1998), Bai et al. (2014), and Elinder and Persson (2017).

service controls has likely lead to a substantial bias in estimates, as mentioned in the literature and documented in Wales and Wiens (1974) and Palmon and Smith (1998). More recent work, such as Morger (2017) and Basten et al. (2017), make use of high-quality microdata on rental contacts and local income taxes in Switzerland to obtain well-identified estimates of tax capitalization without public service data. The high quality microdata also enables Basten et al. (2017) to employ a border discontinuity design in measuring tax capitalization.

We contribute to the literature in three ways. First, we assemble a novel and exhaustive dataset of house prices, local public goods and services, local characteristics, and local taxes for Sweden. In total, we have 947 time-varying local controls. Our dataset spans the full set of 290 municipalities and the period between 2010 and 2016. In addition to the time-varying controls, the high degree of time and geographic variation allows us to include a large number of fixed effects to sweep out confounding variation. Second, we use the recently introduced "debiased machine learning" estimator from Chernozhukov et al. (2017, 2018), which enables the estimation of a parameter of interest in the presence of a potentially nonlinear and high dimensional nuisance parameter. We also modify the method by using a novel neural network architecture that was introduced in Cheng et al. (2016), called a "deep-wide" network. The combination of DML and a deep-wide network enables us to estimate tax capitalization in a specification where we allow for nonlinear dependence on controls, but restrict dependence on fixed effects to be linear. This refinement is likely to be generally useful for estimation problems that involve a high number of controls and fixed effects. And third, we use our novel dataset and econometric technique to estimate the impact of taxes, public service inputs, public service outputs, and crime on house prices. We also test the impact of taxes on migration.

Overall, we find that excluding public service and local characteristic controls leads to a substantial downward bias the magnitude of tax capitalization estimates. The inclusion of time-varying public service controls, coupled with econometric methods that are capable of handling a high vector of covariates, yields a doubling of the estimated reduction in house prices in response to an increase in taxes. We also show that this effect is four times as large in urban areas, where municipal competition is highest, as Tiebout (1956) suggests. We also test the underlying mechanism in Tiebout

(1956) more directly by estimating the impact of municipal taxes on migration. We find a small effect using the entire sample, but it nearly doubles when we exclusively use the subsample of urban municipalities. We also show that public service inputs, such as spending per pupil, actually have a negative effect when outputs are properly controlled for; whereas the impact of outputs remains positive. This builds upon the sub-literature that emphasizes the importance of public service outputs (see Oates, 1969; Rosen and Fullerton, 1977, Hanushek, 1986; and Hanushek, 1996). Finally, we show an application of our methodology to the estimation of crime effects on house prices, following vast literature on that topic (see, e.g., Thaler, 1978; Reinhard, 1981; Blomquist et al., 1988; Haurin and Brasington, 1996; and Gibbons, 2004; Linden and Rockoff, 2008.)

The issue of tax and public service capitalization into housing values has attracted a lot of attention in the literature since Tiebout's seminal paper in 1956. On a more practical note, this literature also has important implications for public policy at the local level. Knowing what drives migration and how specific public services are valued by households may enable local governments to better meet the needs of their constituents. Furthermore, from an individual's perspective, it may be useful to know how house prices respond to changes in local taxation. This knowledge may be particularly valuable in regions where there is a high level of municipal competition, where individuals can choose from many housing locations with similar commute lengths. While past work on tax and public service capitalization suffered from either a lack or a restricted set of public service controls, advances in data collection and availability will likely alleviate this problem for many countries in the future. In this paper, we propose a new methodology that could assist those who wish to exploit the growth in data availability by enabling them to obtain unbiased estimates of the tax capitalization effect or other treatment effects in the presence of a large vector of controls and granular fixed effects.

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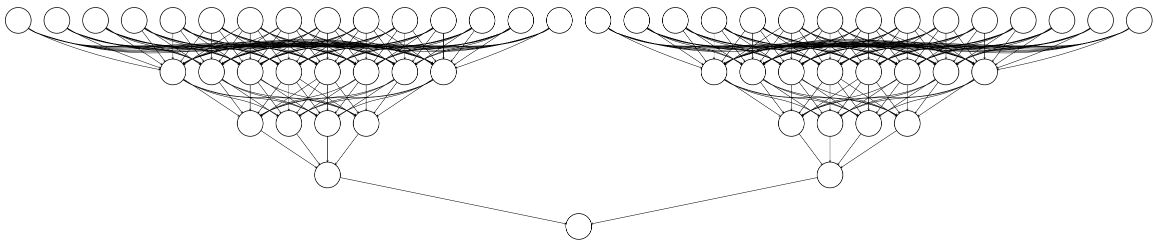
A Appendix

A.1 Figures

Figure 7: Deep-deep neural network architecture



Notes: The figure above shows a deep-deep neural network. The deep part of the network on the left allows for the interaction of all controls and one set of fixed effects (e.g. geographic fixed effects). The other side allows for the interaction of all controls and another set of fixed effects (e.g. time effects). While the networks are trained jointly, the model does not allow for the two groups of fixed effects to interact with each other.

A.2 Series List

Table 8: Control List Summary (I)

Subcategory	Category
Births and deaths	Demographics
Expenses due to population change	Demographics
Foreign-born residents	Demographics
Life expectancy	Demographics
Parental education	Demographics
Personal assistance and disability	Demographics
Residents by age (number and share)	Demographics
Resident education level by grade	Demographics
Student demographics	Demographics
Total inhabitants	Demographics
Women (share)	Demographics
Elder care compensation	Disability & Elder Care
Elder care and disability costs	Disability & Elder Care
Elder care and disability employment	Disability & Elder Care
Elder care and disability revenue	Disability & Elder Care
Elder dependence on care	Disability & Elder Care
Family care costs	Family Care
Family care employment	Family Care
Family care income	Family Care
Family care revenue	Family Care
Housing costs	Housing
Holiday houses per 1000 persons	Housing
New apartments per 1000 persons	Housing
New houses per 1000 persons	Housing
Health care employment	Health
Health care expenditures per person	Health

Notes: This table contains a list of series subcategories for the following categories: demographics, disability care and elder care, family care, housing, and health.

Table 9: Control List Summary (II)

Subcategory	Category
Air traffic costs	Infrastructure
Building structure costs	Infrastructure
Communication network costs	Infrastructure
Infrastructure revenue and cost	Infrastructure
Investment in infrastructure	Infrastructure
Physical and technical planning costs and income	Infrastructure
Port, harbor, and sea costs	Infrastructure
Road, rail, bus, and parking costs	Infrastructure
Gender wage gap	Labor
Long-term unemployment by age	Labor
Unemployment by age	Labor
Wages	Labor
Domestic occupants	Migration
Domestic relocation	Migration
Emigration	Migration
Emigration by age range	Migration
Foreign-born residents	Migration
Immigration	Migration
Immigration by age range	Migration
Net relocation (number)	Migration
Population change, 1-year	Migration
Population change, 5-years	Migration
Refugee population and costs	Migration

Notes: This table contains a list of series subcategories for the following categories: infrastructure, labor, and migration.

Table 10: Control List Summary (III)

Subcategory	Category
Audit costs	Politics
Election turnout (county)	Politics
Election turnout (EU parliament)	Politics
Election turnout (municipal)	Politics
Election turnout (parliamentary)	Politics
Employment in politics	Politics
Foreign-born politicians	Politics
Political activity costs	Politics
Political activity revenue	Politics
Politicians by age (share)	Politics
Politician income	Politics
Vote share for political parties	Politics
Women's representation in politics	Politics
Budget balance	Public Finance
Cash flow	Public Finance
Debt	Public Finance
Depreciation	Public Finance
Equity and assets	Public Finance
Expenses on property	Public Finance
Financial income	Public Finance
Financial ratios	Public Finance
Fixed assets	Public Finance
Guarantees and liabilities	Public Finance
Government grants	Public Finance
Income equalization	Public Finance
Interest expenses	Public Finance
Investment income	Public Finance
Net cost of municipal activities	Public Finance
Other budget items	Public Finance
Other finance	Public Finance
Profits	Public Finance
Provisions	Public Finance
Revenue from municipal activities	Public Finance
Self-financing rate	Public Finance
Working capital	Public Finance

Notes: This table contains a list of series subcategories for the following categories: politics and public finance.

Table 11: Control List Summary (IV)

Subcategory	Category
Cost equalization	Public Service
Cost of activities	Public Service
Cost of activity by category	Public Service
Crime rates	Public Service
Equalization system income	Public Service
Infrastructure production	Public Service
Net costs by category	Public Service
Operating expenses	Public Service
Paid staff	Public Service
Paid staff by age	Public Service
Paid staff by agency	Public Service
Paid staff by education	Public Service
Paid staff hours	Public Service
Personnel expenses (share)	Public Service
Purchase of business by counterparty	Public Service
Regulatory contribution	Public Service
Revenue by service	Public Service
School rankings	Public Service
School resources	Public Service
Spending on disability assistance	Public Service
Spending on government activities	Public Service
Student performance	Public Service

Notes: This table contains a list of series subcategories for public services.

Table 12: Control List Summary (V)

Subcategory	Category
Adult education costs	Education
Cost of education by expense	Education
Cost of education by grade	Education
Cost of education by program	Education
Cost and income from preschool	Education
Educational revenue and income	Education
Gender parity in education	Education
Immigrant education costs	Education
Library books and equipment	Education
Library costs and revenue	Education
Library debt	Education
Library staff	Education
Other education	Education
Other educational costs	Education
Other library	Education
Paid educational childcare staff	Education
Paid educational staff by grade	Education
School enrollment by grade (share)	Education
Student-teacher ratio by grade	Education
Student qualifications and test results	Education
Support for study organizations	Education
Teacher education by grade	Education
Central government taxes	Taxes
Compensation rate	Taxes
County council taxes	Taxes
County taxes	Taxes
Equalization basis	Taxes
Grossing-up factor	Taxes
Guaranteed tax power	Taxes
Municipal taxes	Taxes
Tax power	Taxes
Communication investment	Utilities
Energy, water, and waste investment and costs	Utilities
Utility prices	Utilities
Working areas and premises costs	Utilities

Notes: This table contains a list of series subcategories for the following categories: education, taxes, and utilities.

A.3 Monte Carlo Simulation

The Monte Carlo simulation exercise can be broken down into two steps: a data-generation step and an estimation step. For the data-generation step, we simulate the following two processes for house prices and taxes:

$$p_{jt} = \tau_{jt}\theta_0 + g_0(x_{jt}) + \gamma_j + \eta_t + u_{jt} \tag{11}$$

$$\tau_{jt} = m_0(x_{jt}) + \delta_j + \xi_t + v_{jt} \tag{12}$$

Since the choice of g_0 and m_0 is arbitrary, we will select linear functions, as shown in Equations (13)-(14). This will reduce the advantage of DML-based estimators relative to OLS.

$$g_0(x_{jt}) = g_0^0 x_{jt}^0 + ... + g_0^{k-1} x_{jt}^{k-1} \tag{13}$$

$$m_0(x_{jt}) = m_0^0 x_{jt}^0 + ... + m_0^{k-1} x_{jt}^{k-1} \tag{14}$$

Furthermore, we will assume the true value of θ_0 is -0.5. This is within the range of most of our parameter estimates in the paper, which are between 0 and -1.0; however, the choice of true value is arbitrary and should not impact the results of our simulation. All other processes and coefficients are drawn from a random normal distribution with a standard deviation of one and a mean of zero.

We then estimate θ_0 using OLS, DML-RF, and DML-DW, as described in Section 3. For the OLS estimates, we will exclusively use fixed effects, omitting the vector of controls; however, randomly selecting a subset of controls yields similar results. For DML-RF and DML-DW, we include the full set of controls.

We use 2000 draws of all variables for each set of estimates. This is in line with our sample size, which is 1764 for most regressions in the paper. We repeat the process 500 times, subtract the true value from the estimated values, and then and plot the distribution of coefficient estimate biases in Figure 6.

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