Fiscal Capacity and Inequality: Evidence from Brazilian Municipalities*

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July 17, 2018

Accepted for Publication at the Journal of Politics

*Supplementary material for this article is available in the appendix in the online edition. Replication files are available in the JOP Data Archive on Dataverse (http://thedata.harvard.edu/dvn/dv/jop).

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Abstract

We argue that in young democracies, wealthy elites can limit their taxes by constraining the fiscal capacity of the state. Corrupting local officials and undermining fiscal capacity are some of the mechanisms by which high-income earners can lower their own tax liabilities, even when voters favor higher *de jure* levels of taxation. The incentive to undermine fiscal capacity is especially compelling when inequality is high, as the median voter is likely to support higher progressive taxation and redistribution. Using data from over $5,500$ Brazilian municipalities, we show that localities with higher levels of inequality accrue less revenue from local property taxes. These results are robust to estimating a number of cross-sectional models, as well as panel models with time and municipal fixed effects. Moreover, we show that municipalities with high levels of inequality are less likely to apply to a federal grant program to increase their capacity to collect taxes.

Key Words: Fiscal Capacity, Taxation, Inequality, Democracy

Scholars often presume that governments can enforce their preferred fiscal policies. This assumption has been empirically proven to be false, as governments' ability to collect taxes varies dramatically around the world. What explains these differences across countries, and who might have an interest in maintaining low levels of tax capacity that make evasion easier?

One of the key research questions in political economy is why some countries redistribute more than others (e.g., Acemoglu et al., 2015). In particular, why do many democracies with high levels of inequality redistribute far less than the Meltzer-Richard-Romer models would lead us to expect (Romer, 1975; Meltzer and Richard, 1981)?

Most of the research on redistribution starts with the assumption that states are capable of efficiently collecting taxes and redistributing income, and thus focuses on examining the timing and impact of government decisions to implement redistributive policies. More recent studies have argued that political and economic elites in formerly autocratic regimes may undermine future political processes and limit political choices through institutional designs (Ardanaz and Scartascini, 2013; Albertus and Menaldo, 2014) or low state capacity (Acemoglu et al., 2015). Therefore, even if democratic polities are firmly in favor of redistributive policies, institutions and bureaucratic legacies may undermine the political and administrative process to *de facto* block redistribution.

In this paper, we investigate the idea that economic elites in democracies can undermine the state's ability to collect revenues and that they to do so when levels of inequality are high. Specifically, we ask whether local economic and political elites can undermine efforts to increase taxation in democracies by inhibiting the ability to collect taxes.

We think of the state's capacity to enforce tax policies as endogenous and argue that when citizens vote for higher taxes, economic elites (the wealthy) have incentives to undermine the state's ability to collect taxes. The higher the equilibrium level of redistribution would be in a world with perfect tax collection, the stronger is the incentive for economic elites to erode the state's fiscal capacity. Weakening the state's administrative and tax capacity gives economic elites a mechanism with which to constrain policy choices and *de facto* levels of taxation outside the political system.

To investigate the theoretical argument, we use data on tax revenues from over $5,500$ Brazilian municipalities. We show that, controlling for a variety of other factors, localities with higher levels of inequality raise less revenue from local property taxes. These results are robust to estimating a variety of cross-sectional models for 2000 and 2010, as well as panel models with time and municipal fixed effects. We also show that municipalities with high levels of inequality were less likely to apply to a federal grant program to increase their local tax capacity.

Fiscal Capacity & Public Spending

Research in political science and economics often starts with the premise that in democratic polities, higher economic inequality ought to be associated with political demands for redistribution. Much of this work builds on the seminal model developed by Meltzer and Richard (1981), who showed that as the difference in mean income and income of the median voter increases, levels of taxation and redistribution should rise. The idea that democracy can and would be used for redistribution when inequality exists is not new, however, and goes at least as far back as Marx. While the Meltzer-Richard model is only one specific formalization, we expect rational voters in democracies to vote for higher taxation and redistribution as long as their marginal benefit from higher rates is positive. When taxes are linear or progressive, poorer citizens ought to prefer higher taxes than the rich. More so, if the benefit of government spending is higher for poor than rich voters, the optimal tax rate for the poor increases. Contrary to these expectations, empirically there is little evidence that inequality is associated with higher redistribution in democracies (e.g., Benabou, 1996; Perotti, 1996; Kenworthy and Pontussen, 2005).

The lack of empirical support for the Meltzer and Richard (1981) model at the cross-national level is frequently noted. Some factors that possibly condition the relationship between inequality and redistribution are differences between social insurance and redistributive policies (e.g., Moene and Wallerstein, 2001), institutional structures (e.g., Persson and Tabellini, 2003; Iversen and Soskice, 2006), religion (Scheve and Stasavage, 2006), and ethnicity (Alesina and Glaeser, 2004). More recently, scholars have argued that politics in authoritarian regimes can have lasting effects on fiscal policies, potentially long after the transition to democracy. Albertus and Menaldo (2014), for example, argue that autocratic elites can shape the institutional design of subsequent democracies to influence and shape future politics – i.e., by influencing the "rules of the game" (Albertus and Menaldo, 2014). Ardanaz and Scartascini (2013) contend that higher inequality leads to more legislative malapportionment, which makes enacting redistributive policies more difficult once the democratic regime is established.

While the design of political institutions with many veto points is one strategy to inhibit redistribution in democracies, undermining state capacity with the goal to keep the state from collecting revenue may be an equally compelling strategy. Economic elites may cripple the political process by stifling the state's ability to raise revenue. Theoretical models show that non-democracies with higher levels of income inequality should see lower investment in state capacity (Besley and Persson, 2011).

Similarly, Acemoglu and Robinson (2008) argue that possible changes in *de jure* political institutions give economic elites reasons to invest in subverting the state, to "capture democracy" and gain influence over policy decisions. An inefficient state with corrupt ("captured") bureaucrats may be a valuable strategy for economic elites to safeguard themselves against the political power of the masses (Acemoglu, Vindigni and Ticchi, 2011).

In line with these explanations, we argue that economic elites in democracies can exploit and further weaken the state's ability to collect revenue in an effort to block taxation demanded by voters. We contend that in democratic systems, rich or wealthy citizens can keep levels of taxation low, using both democratic and undemocratic means. The wealthy have incentives to ensure that their interests are (over) represented and that taxation is limited. One way to do so is by undermining the state's ability to collect taxes, i.e., by constraining its fiscal capacity. Raising taxes is a complicated undertaking that involves collecting large amounts of data and requires a functioning and efficient bureaucracy (Besley and Persson, 2009). Yet many governments cannot enforce the tax policies chosen by their governing bodies (Bird and Zolt, 2008; Gordon and Li, 2009). In such settings, wealthy residents may have strong incentives to undermine the state and limit their personal tax payments by lowering the state's ability to collect taxes.

To illustrate our argument, consider a theoretical society with rich (r) and poor (p) citizens, in which the median voter sets the *de jure* tax rate and is a member of the poor. Both wealth and income are taxable. Assume all revenue is used to finance a public good, such as education, or used as direct transfers. Assuming the median voter is decisive, she should vote for higher taxes until the marginal benefit from the financed public good is equal to her marginal cost of taxation. If taxes are not regressive and revenue is used for public goods or transfers, then the optimal tax rate at which the marginal benefit equals the marginal cost for the poor rises with increasing inequality.

As the tax becomes more progressive and spending benefits poor citizens more than the rich, the effect of inequality on the tax rate ought to be more pronounced. Thus, in accordance with the standard theory, if citizens vote rationally and based on income, we should see higher levels of *de jure* taxation in states with higher levels of inequality. On the other hand, the difference between pre- and post-tax income of the wealthy elite would increase with higher levels of inequality. With this standard argument in mind, one could hypothesize that higher inequality leads to higher taxation (i.e., *de jure* tax rates) in democracies.

The distinction between *de jure* and *de facto* taxation is important for our theoretical argument. As taxes have to be administered and collected, *de jure* tax rates must not translate into the same *de facto* level of taxation. For example, with a *de jure* tax rate of 15%, even the most efficient and effective tax administration does not achieve 15% realized revenue. We define the *de facto* tax rate as the actual share of the tax base that is collected in taxes. As the capacity of the tax administration decreases, the difference between *de jure* and *de facto* tax rates becomes greater.

In a democracy with weak administrative capacity and firm entrenchment of the wealthy in the political process, elites have strong incentives to undermine the state's ability to collect taxes. As outlined above, when inequality is higher, the *de jure* tax rate is likely to rise. When *de jure* tax rates increase, however, it becomes more profitable for economic elites to combat the state's ability to assess their tax liabilities or to influence the political process through other means. Alternative avenues for influence could include bribing local tax officials who are responsible for tax assessment, placing cronies in essential positions in the local bureaucracy, or impeding the purchase of necessary tools to make tax collection more efficient. Thus, in sufficiently weak states, we contend that economic elites can undermine tax collection, and the motivation to do so increases with higher levels of inequality.

We expect these tactics to be more likely in the context of highly progressive taxes. As a given tax becomes more progressive, the rich pay a higher share of tax revenue, which increases their motivation to fight tax collection. The difference between *de jure* and *de facto* rates should thus be more significant for more progressive taxes. Similarly, as spending benefits the poor more, we expect the relationship between inequality and the *de jure* taxation to become stronger, again raising incentives for elites to fight taxation.

Based on this theoretical argument, we develop our central hypothesis. Specifically, we expect that higher inequality is associated with less fiscal capacity, and therefore less *de facto* tax revenue. Our approach contrasts with the above outlined traditional hypothesis that higher inequality is associated with more tax revenue.

Research Design: The Case of Brazil

In this paper, we use data on tax collection from over $5,500$ Brazilian municipalities to investigate the empirical argument. There are several reasons for using the case of Brazil and its municipalities as the unit of analysis.

The democratization of Brazil in the mid-1980s advanced the country socially and politically (Oliven, Ridenti and Brandão, 2008). There are now few barriers to voter registration

(Limongi, Cheibub and Figueiredo, 2015), and compulsory voting ensures a turnout close to 80% (Nicolau, 2012). Since its transition to democracy, Brazil has been known for its high levels of income inequality, making it one of the most unequal democracies in the world. Inequality has been surprisingly resilient and stable throughout the transition from the military dictatorship (1964–1985) to the new democratic regime (Barros, Henriques and Mendonça, 2000; Souza and Medeiros, 2015).

The relatively recent transition to democracy and the persistence of inequality are two reasons that make it an intriguing case with which to investigate our argument. If the *standard* arguments were correct, we would have expected a stark increase in redistribution and taxation after Brazil's democratization in the 1980s. The argument we make above is one possible explanation for why this has *not* been the case.

The Case for Studying Municipalities

The Brazilian federative union is composed of 26 states and the federal district. Brazil has 5,570 municipalities, its lowest level of government, which have more political autonomy than localities in any other Latin American country (Nickson, 1995; Rodríguez and Velásquez, 1995). Most political responsibilities lie with the federal union or states, yet the 1988 constitution gave substantial autonomy to the municipalities (Andrade, 2007; Baiocchi, 2006; Samuels, 2004). In line with the increase in political authority, municipalities can institute and collect taxes within their jurisdiction and use the revenue to implement local policies (Arretche, 2004; Andrade, 2007).

The municipalities are largely funded by transfers from the federal and state governments. These transfers have significantly declined, however, leading to budget shortfalls and low revenues in many municipalities. One of the most critical local tax sources is the taxation of property and land in urban areas, the *Imposto Predial e Territorial Urbano* (IPTU): the urban land and building tax. This tax is solely available to municipalities, and its importance as a local revenue source has increased significantly (De Cesare and Ruddock, 1999).

We aim to investigate whether elites use low levels of administrative capacity, as well as undermine it further, to limit their taxation. To do so, we focus on the case of the property tax in Brazilian municipalities. While the IPTU is one of the principal sources of local revenue in Brazil (property taxes represent an average of 30% of the local tax revenue) (Smolka and Furtado, 1996; De Cesare and Ruddock, 1999), comprehensive studies of this tax indicate that it is still overlooked and has unrealized potential (De Cesare and Ruddock, 1999; Afonso and Araújo, 2006; Afonso, Araújo and Nóbrega, 2013).

While property taxation is a tax on wealth, we believe our theoretical argument, which is primarily about income inequality, still applies here. The IPTU is the second most important local revenue source available to municipalities (Afonso, Araújo and Nóbrega, 2013) and has the potential to be highly progressive. Therefore, if voters observe high levels of inequality and as a result demand more taxation and spending, the IPTU is the primary local mechanism to raise these funds. Moreover, administration of the property tax requires high administrative capacity (Bahl and Martinez-Vasquez, 2008; Kelly, 2013), making it a worthwhile endeavor for elites to engage in actions to undermine the collection of these taxes.

The distributive effects of the tax and relevant spending instruments are similarly important. We have strong reason to believe that the property tax is progressive by design, and that municipal spending largely benefits the poor. First, after the new constitution was enacted in 1988, a progressive property tax system was considered a potential policy mechanism to overcome urban social inequalities and attain equity (De Cesare, 2012; De Cesare and Smolka, 2004; Carvalho, Jr., 2015). After a period of legal ambiguity, a constitutional amendment was passed in 2000, that explicitly allowed progressive tax rates for the IPTU (Carvalho, Jr., 2013). In reality, however, the IPTU has been found to be a regressive tax (Carvalho, Jr., 2006, 2015; Afonso, Araújo and Nóbrega, 2013).

Several causes for the regressivity of the IPTU have been suggested. Directly in line with our argument, one significant reason for its regressive nature is the poor collection of the IPTU. This is due to administrative mismanagement, administrative inefficiency, the high cost of maintaining the property register, and the discrepancy between the government's real estate evaluations and their market value (De Cesare, 2005; Carvalho, Jr., 2006, 2015). Tax exemptions for large companies and tax evasion are also responsible for the high regressivity (De Cesare and Smolka, 2004; Carvalho, Jr., 2006).

De Cesare (2005) and Afonso, Araújo and Nóbrega (2013) found that changes in IPTU rates depend on the approval of councilors in the municipal legislature. Not surprisingly, property owners in wealthier areas regularly resist higher rates, and even more so if the revenue will be invested in poorer areas of the municipality (De Cesare, 2005; Afonso, Araújo and Nóbrega, 2013). Similarly, organized groups of landowners tend to pressure public authorities to minimize their fiscal burden (Afonso, Araújo and Nóbrega, 2013). This is exacerbated by the fact that new valuations of properties have to be approved by the municipal legislatures, giving the wealthy an avenue to undermine the administrative process of tax collection (Carvalho, Jr., 2013). Thus, at least part of the regressivity of the IPTU is due to differences in the *de jure* and *de facto* tax rates.

If properly enforced, the IPTU has the potential to be redistributive and the exact mechanisms outlined in this manuscript, i.e., elite resistance against higher taxes, are at least partially responsible for its regressivity. In addition to the potential progressivity of the tax itself, government spending at the municipal level primarily benefits the poor. In other words, the marginal benefit of additional spending is higher for the poor than the rich. For example, the most significant share of local budgets is spent on education, with health spending being second. Municipalities primarily finance preschools and primary schools as well as education infrastructure and school lunches (Gadenne, 2017).[1] While not directly redistributive transfers, we contend that spending on these goods is redistributive in nature and has greater benefits to poorer segments of society.

In line with our argument, Gadenne (2017) finds that investments allocated to modernize local tax administrations do increase tax revenue. The additional income is spent on the provision of public goods, with three-quarters of the extra revenue going towards public education. This results in an eight percent increase in locally-funded school infrastructures and six percent more children enrolled in municipal schools (Gadenne, 2017).

Measuring Fiscal Capacity Using the Property Tax

Property taxes are difficult to enforce for both administrative and political reasons (Bahl and Martinez-Vasquez, 2008; Kelly, 2013). According to Kelly (2013), we can decompose total property tax revenue into two parts. First, the total level of potential revenue, which equals the tax rate applied to the total tax base, i.e. *de jure* tax rate above. The second, equally

[1] According to data from the Brazilian Ministry of Finance (National Treasury (DFOFM), 2017), the share of public goods spending that goes to education and health grew from 25% and 11% in 1990 to 34% and 17% in 2000, and 41% and 32% in 2010, respectively.

important, determinant of total revenue is made up of "administration-related variables." These variables are the coverage ratio, i.e., the share of properties captured in the municipality's registry; the valuation ratio, i.e., the ratio of valuation in the taxpayer registry to the market valuation of properties; and the collection ratio, i.e., the percent of levied taxes that are collected. While tax rates and the base are both relevant determinants of the tax revenue collected by the state, the administrative capacity is fundamental for property taxes to raise significant revenue (Kelly, 2013; Bahl and Martinez-Vasquez, 2008).

Calculating IPTU liability (i.e., the valuation) requires several types of information, such as property size, location of the property, property use, front and backyard area, property construction standard, etc. (Carvalho, Jr., 2006). Before valuation, properties must be registered in the municipal cadaster. Carvalho, Jr. (2006) estimates that only 60% of the urban real estate in Brazil is registered. Another important aspect of property tax collection is the frequency of assessment, i.e., how often does the administration update/assess the value of properties? The Brazilian central government recommends evaluating property values every five years, with yearly adjustments. The guidelines do not seem to be regularly followed, however. For example, while Porto Alegre in the 1990s had more regular assessments than other municipalities, the assessed values of residential properties were only 19.2% of their sales prices (De Cesare, 2012).

While it is almost impossible to accurately and reliably measure fiscal capacity, we use realized property tax revenue as a proxy for local fiscal capacity. We assume that given the control variables included in the regression models below, at least some of the variation in the *policy-related variables* are held constant across our cases. For example, we include controls for local GDP, population size, and share of the rural population, which ought to explain differences in the tax base. We add controls for revenue needs (i.e., transfers from the federal government, oil revenue) and political determinants (left-leaning mayors), which should at least partly account for differences in tax rates.[2] Lastly, we discuss some robustness checks based on smaller samples with more direct measures of administrative capacity.

Kelly (2013, 147) identifies the incompleteness of property registries (cadasters) as the most pressing administrative issue when it comes to property tax collection in developing countries, with a lack of "necessary political will to *collect and enforce the property tax*" (emphasis added) as an additional major hurdle. Anecdotal evidence suggests that municipalities in Brazil find it difficult to increase their administrative capacity. As De Cesare and Ruddock (1999) point out, wherever localities aim to increase the quality of assessment and revenue of the property tax, they are met with strong opposition. Qualitative evidence of tax fraud and incompetence in local government tax collection is easy to find. For example, in 2014, the public prosecutor's office of São Paulo was investigating companies suspected of carrying out a fraud scheme in the city's IPTU collection in partnership with tax collectors (IPTU inspectors). The inspectors calculated the correct tax, but recorded only half the area when visiting buildings. The other half of the tax was paid as a bribe to the inspectors. While the bribe was paid once, the scheme guaranteed a tax bill that was 50% of the *de jure* amount for all subsequent years (Estadão, 2014).

Similarly, a group of employees in the São Paulo City Hall was accused of fraud and irregularities concerning charges of the Service Tax and the IPTU. Members of the group defrauded the IPTU, by making changes to the cadaster, which was estimated to have cost

[2] Unfortunately, complete data on tax rates at the local level are not available.

city hall about half a billion Brazilian reais (approximately 160 million $US in today's value) (G1-Globo, 2013).

Other examples of fraud and local difficulties with tax collection include charges of public servants making improper changes to the collection system (G1-Globo, 2012), fraud schemes in the city of Campinas (collection of less than 10% of property values), and the municipality of Taboão da Serra (Folha de São Paulo, 2011). These tax evasion schemes cost at least R$ 15 million for Campinas (Folha de São Paulo, 1999) and caused a minimum loss of R$10 million to Taboão da Serra, a municipality with more than 250,000 inhabitants (Folha de São Paulo, 2011).

Some reader may question the use of property tax at the local level as the unit of analysis. The majority of taxes are levied at the federal level, which raises the question whether elites would try to undermine local capacity. We believe that the collection of local property taxes is nevertheless highly relevant for this study. First, these taxes, if properly enforced, are likely to be progressive. Based on the theoretical argument, all else equal, elites ought to prefer paying lower property taxes. Additionally, undermining the local property tax administration in the respective municipality is most likely easier and less costly than attempting to do so at the federal level. Thus, the marginal benefit of undermining tax capacity may be highest at the local level. While we lay out a general argument above, we believe that if it holds true, we should find evidence of these processes at the local level. Given the large variation in inequality and tax revenues in municipalities across Brazil, we think these represent an excellent test case for our argument.

Empirical Strategy: Data & Models

To investigate whether high-income earners use low levels of fiscal capacity to limit redistribution and taxation in high-inequality municipalities, we collected data on tax revenues, political, and socioeconomic variables for the years 1990, 2000, and 2010 from different sources. The dependent variable, our proxy for fiscal capacity at the local level, is the property tax revenue collected by municipalities. The measure of revenue collection comes from the Brazilian Ministry of Finance, released by the National Treasury Secretariat, and is made available by the Institute of Applied Economic Research (IPEA, 2016).[3]

Brazil exhibits high geographic variation in both inequality and tax collection. Our preferred measure of income inequality in the municipalities, the Gini coefficient, ranges from 0.28–0.8 in Brazil for 2010. The use of subnational data allows us to hold many variables constant across observations. For example, we do not have to worry about differences in the political system affecting our results.

We include several control variables in the regression model to account for possible confounders and partial out tax rates and tax base. First, we add a control for municipal GDP to account for the fact that higher inequality may be caused by increasing incomes, while more affluent municipalities have a larger tax base, and are more likely to be more efficient at

[3]Based on personal communication with IPEA, some ambiguity about the meaning of zeros in the IPTU revenue data exists. It is possible that some observations with a value of zero are actually missing data, while for other observations the zeros are meaningful values that indicate zero revenue. This issue mostly applies to the panel model. We use the original data in the main text but undertake additional robustness checks in the online Appendix in section F.

revenue collection. We also control for population size. Brazilian municipalities are heterogeneous regarding their size, economic condition, and capacity to tax. Studies have shown that municipal size is positively correlated with property tax revenue (Gomes, Alfinito and Albuquerque, 2013; Avellaneda and Gomes, 2014). Both of these measures were gathered from the Brazilian Institute of Geography and Statistics (IBGE, 2016).

Since municipalities are only allowed to collect property taxes from urban areas, it is pertinent for us to account for differences in urbanization. Hence, we control for the share of the population living in rural areas. We also include a measure of municipal spending on housing and urbanization. The inclusion of this variable is important, as spending on housing and urban development affects real estate evaluations and increases the base for calculating the IPTU tax. A second relevant fiscal variable included in our models is the level of transfers from both the federal and state governments to each of the municipalities (Brollo et al., 2013; Litschig and Morrison, 2013). Data on transfers and housing spending was gathered from the Institute of Applied Economic Research (IPEA, 2016). Additionally, we control for municipal revenue from oil exploration (royalties). Royalty payments made to municipalities in which oil has been discovered and explored increased from R$167 million in 1997 to R$4.7 billion in 2008 (Monteiro and Ferraz, 2012). Royalty payments are associated with an increase in the number of municipal employees (Monteiro and Ferraz, 2012) and municipal revenues (Caselli and Michaels, 2009). Similar to intergovernmental transfers, we expect that royalties from oil exploration undermine local governments' incentives to increase their own revenue capacity and may also affect inequality.

In addition, in our cross-sectional models, we include an indicator variable with a value of 1 if the mayor of the municipality is from a left party, and 0 otherwise. The inclusion of this variable is an attempt to understand whether left-leaning parties are more likely to raise the fiscal capacity/redistributive taxation and whether they are able to achieve this goal. Given our theoretical argument, we do not expect left-leaning party governance to have a strong effect on *de facto* tax revenue. Additionally, this control may partial out some of the differences due to *de jure* tax rates. Political data were collected from the Superior Electoral Court (TSE do Brasil, 2016), and leftist parties were classified based on surveys and roll-call vote studies of Brazilian legislators (Power and Zucco Jr., 2009, 2012; Samuels and Zucco Jr., 2014; Saiegh, 2015).

We were able to collect these variables for the years 2000, 2010, and approximately 1990. We first estimate cross-sectional models for both 2000 and 2010. We estimate standard ordinary least squares (OLS) regressions for the cross-sectional models, but calculate standard errors clustered by states. The dependent variable (*IPTU revenue*) and the independent variables *housing*, *GDP*, *transfers*, *oil revenue*, and *population* were log transformed to reduce the right-skewness of their distributions.[4]

In addition to the cross-sectional models for two time periods (2000 and 2010), we also estimate a panel model for 1991, 2000, and 2010, in which we include municipal and year fixed effects. Using the unit-specific intercepts, we aim to control for unobserved confounders that do not vary over time or across units.

[4]To avoid creating missing values, prior to taking the log we add 1 to the values of *IPTU*, *housing*, *oil revenue*, and *transfers* variables.

Empirical Analysis: Results and Discussion

Figure 1 illustrates our general findings in the cross-sectional models. The plot displays the coefficient estimates for our cross-sectional model for 2010 with standard errors clustered by state.[5]

Our results consistently lend support to our hypothesis. Particularly, the coefficient for inequality (*Gini*) is estimated to be negative and is statistically significant in all models. Higher inequality is associated with lower property tax revenue, i.e, as inequality rises a municipality's ability to collect IPTU from its citizens decreases. For example, according to the results displayed in Figure 1, holding all covariates at their median value and increasing inequality from the 25th percentile value (0.45) by one standard deviation (to 0.52) is associated with a decrease in logged IPTU revenue from 10.92 to 10.49.

In line with our expectations, the coefficient for GDP is precisely estimated and positive, which indicates that richer municipalities can raise more revenue from property taxes. In contrast, the larger the share of the population living in rural areas, the lower the revenue from the IPTU.

The results for population size are somewhat surprising. Higher population size may be associated with lower revenues. The estimates for intergovernmental *transfers* are also

[5] Table A.1 in the Appendix presents the estimation results for six different models for the 2000 and 2010 data. All models were estimated using OLS. Models 2 and 4 were estimated computing robust standard errors, and Models 3 and 6 were estimated computing standard errors clustered at the state level. We also estimate all models based on data that is multiple imputed using Gaussian copulas (Hoff, 2007). The results are shown in Table A.2 in the Appendix and suport the results presented here.

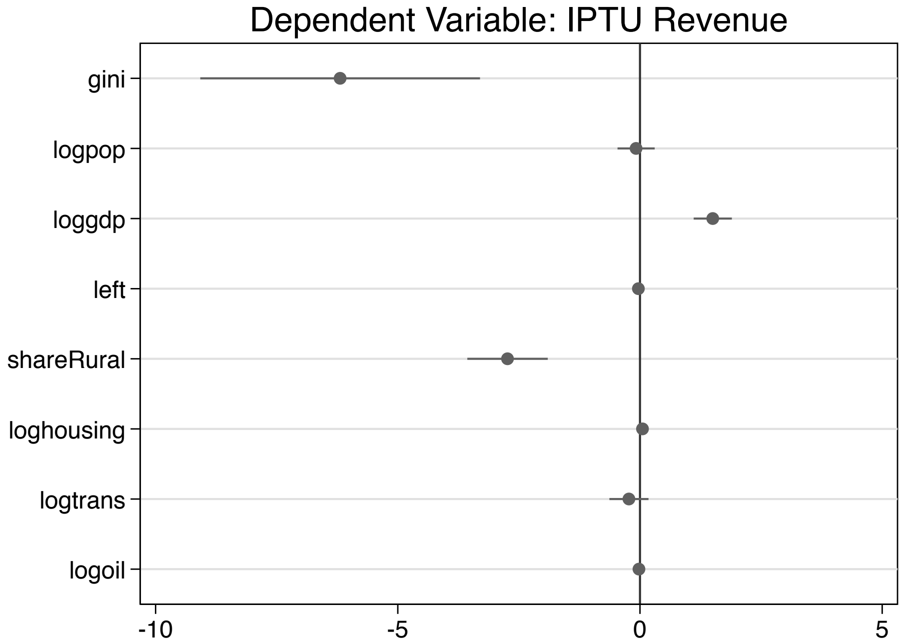

Figure 1: Coefficient Estimates from Model 6 of Table A.1 in Appendix A. Cross-Sectional Model for 2010 with non-imputed data. Standard errors clustered by state. Dependent variable: IPTU revenue in Brazilian reais (logged). The negative and significant estimate for Gini indicates that, as inequality increases the state's ability to raise revenue from citizens decreases substantially.

not precisely estimated in models with clustered standard errors. The results do indicate that municipalities that are more dependent on transfers collect lower revenues from the IPTU. These results are similar to our findings for *oil revenue*. Throughout all models, the coefficient for oil revenue is estimated to be negative, but the precision of the estimates varies across the different models. Also as expected, mayors from left-leaning political parties are not associated with higher revenues: the coefficient for leftist party mayor is very small, inconsistent, and estimated with high uncertainty.[6]

In the Supplementary Online Appendix in section B, we provide additional evidence for

[6]As an additional robustness check, Table A.3 in the Appendix displays the results from four spatial autoregressive models. Overall, the results from the spatial models are consistent with the findings presented above.

the robustness of these results by adding several potentially relevant controls and estimating bivariate models without controls. The results do not change subtantially for any of these specifications. The effect of inequality remains negative and significant when we add controls for voter turnout, competitiveness of the mayoral race, other municipal tax revenues, share of the population vulnerable to poverty[7], share of municipal GDP produced in the industrial sector, number of families that benefit from the cash transfer program (Bolsa Família), or the size of the cash benefits. The estimated effect of inequality is negative and statistically significant in all of these specifications, except when we include total logged cash benefits paid out and cluster standard errors by state. In that particular model, the coefficient on inequality is significant only at the 10% level. Lastly, we can add GDP growth over the previous decade to our cross-sectional models and the results remain substantially the same.

To provide further evidence for the robustness of our results and alleviate concerns about the dependent variable, we also estimate several models with other potential measures of fiscal capacity at the municipal level. For some of these, however, the sample size is reduced significantly. The results are presented in section C in the Appendix. First, we show that the cross-sectional results are robust to calculating our dependent variable as the ratio of IPTU revenue to municipal GDP or as a ratio to total municipal tax revenue. We also provide the results when using revenue from a different local tax source (ITBI, a tax on property transfers) as the dependent variable. The results do not change substantially.

Lastly, we also create a variable measuring the ratio of registered properties for which the property tax was paid to total registered properties (collection rate). These data are collected

[7] Variable is defined as the share of the population with incomes less than R$255.00 a month.

for 1998. While imperfect, we use this measure as an alternative dependent variable for our cross-section of 2000 (the closest year for which we have data). Again, the relationship with inequality is estimated to be negative and significant.

Panel Model Estimation

So far, we have shown that across different municipalities, higher inequality is robustly associated with less municipal revenue collected from property taxation. These findings lend support to our theoretical argument that in higher-inequality districts, wealthy elites undermine the state's ability to collect taxes. The results are robust to including many potential confounders as controls.

Nevertheless, other potential factors may affect both tax capacity and inequality. In this section, we present evidence based on a simple panel model at the municipal level for 1991, 2000, and 2010, with both municipal and year fixed effects.[8] By including both municipal and time fixed effects we can control for unobservables at the municipal level that do not vary over time, as well as shocks in time that do not change across the different municipalities.[9] Given these additional parameters, the results from the three-period panel model can serve as an additional check on the results presented above.

[8]Since several variables are not available for 1990, we use 1991 as our earliest observation. In addition, we could not find data for municipal GDP for the early 1990s. We thus have to rely on a GDP measurement from 1985 in the panel data for 1991.

[9]Since inequality within a municipality may also create incentives to redraw municipal boundaries, we conduct an analysis using a sub-sample based on municipality age. The results, presented in Appendix E, indicate that a possible split of municipalities due to high inequality does not seem to be driving our results.

We specify the following model for the three-period panel data:

$$y_{it} = \alpha_i + \gamma_t + \beta \mathbf{X_{it}} + \delta G_{it} + \epsilon_{it}, \tag{1}$$

where α_i and γ_t are municipality- and year-specific intercepts, $\mathbf{X_{it}}$ is a matrix of time-varying covariates, and β is a vector of the corresponding estimated coefficients. G_{it} is the main variable of interest, the Gini coefficient for municipality i at time t. Based on our theoretical argument, we expect its coefficient δ to be negatively signed. We present the results based on standard errors clustered at the state level.

Figure 2 displays the results from the three-period panel model. Growth in population and transfers over time are associated with higher levels of tax revenue and the 95% confidence intervals do not include zero. The coefficients for GDP, share of the rural population, and logged spending on housing are very close to zero and not significant at conventional levels. Most importantly, the coefficient for inequality is negative, and its 95% confidence interval does not cover zero. An increase in inequality over time is associated with less municipal revenue from property taxes. This finding gives additional credence to the theoretical argument.[10]

As a robustness check, we estimate the same model in a two-period panel for 2000 and 2010.[11] Surprisingly, once we add year fixed effects, the coefficient for inequality is estimated

[10]Some of the municipalities in our sample were created after 1990. We, therefore, subset the data to those municipalities created prior to 1985. The results remain the same if we do not subset.

[11]For the two-period panel model, we subset the data to municipalities created before 2000 (results shown in Table D.1 in the Appendix).

to be positive in the two-period model with controls (2000 and 2010). This suggests that a something changed in high inequality municipalities between 2000 and 2010. It is possible that the introduction of the federal cash benefits program Bolsa Família in 2003 led to these changes, though there is no clear way to test this. Since Bolsa Família was started in 2003, we can not include it as a covariate in the panel models. As we discussed above, however, the results in the cross-section for 2010 are robust even when controlling for Bolsa Família benefits. [12]

As with the cross-sectional model, we estimate the three-period panel model as a bivariate model with unit and year fixed effects. We also add a linear time trend and a quadratic time-trend to the three-period panel model. The results remain the same. Lastly, we estimate the two-period panel model using data on the collection rate (i.e., the ratio of paid to levied taxes) for 180 municipalities. These data were originally collected by Carvalho, Jr. (2017). Our general finding: a significant and negative relationship of inequality with fiscal capacity remains. On average, the greater the inequality, the smaller the IPTU collection rate. The results of these robustness checks are presented in section D of the Appendix.

[12] We thank an anonymous reviewer for alerting us to the possible effects of the Bolsa Família program. Table D.1 in the Appendix also displays the results for both panel models when the data are multiple imputed using Gaussian copulas (Hoff, 2007). The results are mostly unchanged, and in fact, the effect of inequality on property tax revenue is estimated to be stronger.

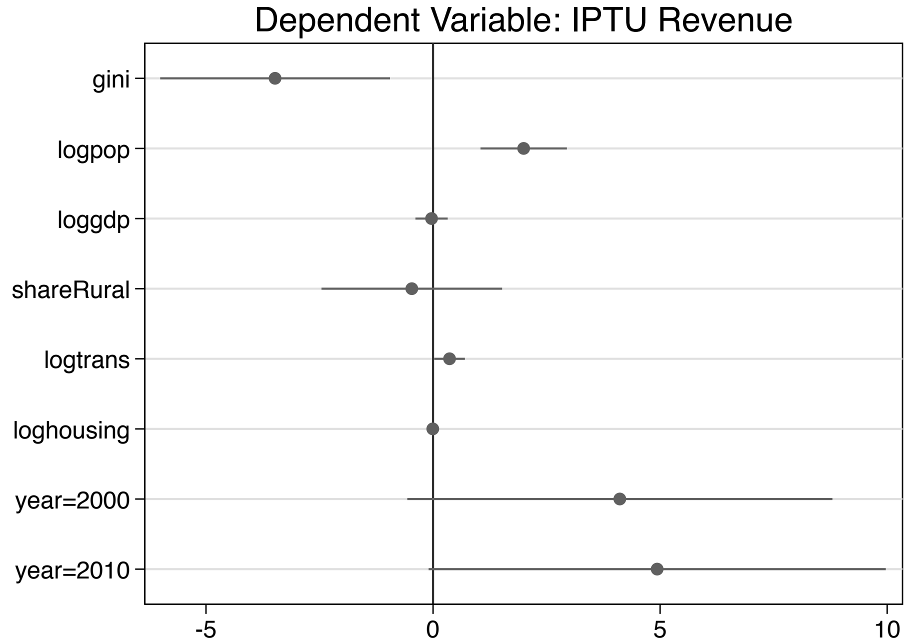

Figure 2: Coefficient Estimates from Model 1 of Table D.1 in Appendix D. Panel Model (1991, 2000, 2010) with year and municipal fixed effects, standard errors clustered at the state level. Dependent variable: IPTU revenue in Brazilian reais (logged). The results are consistent with the cross-sectional model, indicating that increases in inequality are associated with lower capacity to collect taxes.

Selection on Unobservables

In this section, we briefly discuss a sensitivity analysis of the regression results, as suggested by Oster (2017). We estimate how strong selection on unobservables compared to observables would have to be if the effect of inequality is due to bias. Two concepts are required. The first is the "relative degree of selection on observed and unobserved variables" (δ), i.e., how much more important are the variables included in the regression models compared to unobservables. Generally, Oster (2017) suggests considering results to be robust if $\delta > 1$. Secondly, R_{max} is defined as the maximum attainable R^2 for the particular regression, if all relevant variables were included. Of course, the most conservative test is with R_{max} set to one, the highest possible R^2. Based on empirical evidence using the results of randomized experiments, Oster (2017) suggests that a R_{max} of 1.3 times the R^2 from the relevant regression might be more appropriate. We estimate δ for each of three regression models of interest using the highest possible values of R_{max}, $R_{max} = 1$.

Table 1: Selection on Unobservables

	2000	2010	Panel Model
$R_{max} = 1$	$\delta = 1.92$	$\delta = 2.62$	$\delta = 4.82$

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).
Test for 2000 from Cross-Sectional Model 3 of Table A.1 in Appendix A.
Test for 2010 from Cross-Sectional Model 6 of Table A.1 in Appendix A.
Test for Panel Model from Panel Model 1 of Table D.1 in Appendix D.

The relevant values are displayed in Table 1. The results imply that it is unlikely that our results are due to selection on unobservables, as the estimated δ for all three models are above the critical value of 1, even when we use the maximum possible value of one for R_{max}.

Applications to Capacity-Building Program

The empirical analyses and the robustness checks in the previous section have provided evidence in line with our theoretical argument. Nevertheless, questions may remain with regards to our dependent variable and the identification of the theoretical mechanism. In this section, we investigate if inequality levels influenced whether municipal governments applied for grants to improve their tax administration.

In 1997, the Brazilian federal government initiated the Modernization Program of the Tax Administration (PMAT), with the goal of improving municipalities' tax administration. The foremost objective of the program was to increase municipalities' revenues by improving tax registration and collection processes, modernizing taxpayer services and enhancing municipalities' fiscal responsibility and capacity (Afonso et al., 1998; Guarneri, 2002). The program focuses on the modernization of information technology, computer equipment, training of human resources, specialized technical services, and the physical infrastructure of municipalities' public administration (Guarneri, 2002; Corrêa, 2009).

The financial funds of the program are provided to the municipalities by the Brazilian Development Bank (BNDES) through credit lines opened by BNDES financial partner institutions. The current financing amount limit is either a maximum of R$60 million per municipality or R$36 per capita (the financing accepted is based on the lower value of these criteria) (Corrêa, 2009).

Gadenne (2017) has taken advantage of the program to show that higher levels of fiscal capacity – and, ergo, local tax revenue – cause positive changes in municipal education infrastructure. If our argument is correct, we should find that municipalities with higher levels of inequality are less likely to apply to the program (even though their revenues are lower). We, therefore, estimate the probability that a municipality joins the PMAT program until 2010 as a function of its inequality level (Gini coefficient) and controls included in our previous models (all measured in 1991). We also include municipal revenue raised from IPTU collection as a control. According to our argument, the elites' constraint on the state should be stronger under higher levels of inequality. Thus, we expect that the greater the municipality's inequality, the lower the likelihood it will apply to PMAT.

As shown in Table 2, the results support this expectation. Across linear probability, logit models, and when we cluster standard errors by state (Models 2 and 4), the coefficient on inequality is negative and precisely estimated. Greater inequality appears to be associated with a lower likelihood of application to PMAT, a finding that is also reflected in the work

Table 2: Municipal Applications to the Capacity-Building Program (PMAT)

	Dependent variable: PMAT Application			
	(Model 1) OLS	(Model 2) OLS	(Model 3) Logit	(Model 4) Logit
Gini	-0.242***	-0.242***	-2.786**	-2.786**
	(0.050)	(0.070)	(1.166)	(1.230)

Notes: Dependent variable: Binary variable PMAT (1 = municipality applied to PMAT, 0 = municipality didn't apply to PMAT).

All four models include controls for IPTU revenue (logged), population (logged), GDP (logged), rural share, transfers (logged). Full Table is displayed in Table C.6 in Appendix C. Model 1 and Model 3 with robust standard errors. Model 2 and Model 4 with standard errors clustered by state.

Standard errors in parentheses. Two-tailed test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

by Gadenne (2017). For space reasons we omit the control variables from the Table, but full results are presented in Appendix C.

These results are consistent with our expectation that more unequal municipalities will have a lower capacity to collect taxes. Although PMAT currently reaches all regions of Brazil, the program is heavily concentrated in the less unequal south and southeast regions of the country (Corrêa, 2009; Grin, 2014). While the south and southeast have received 73.4% of all established contracts in 2009, municipalities in the north and northeast regions of Brazil (more unequal) account for only 3.8% of the contracts (Grin, 2014). After 13 years, the fact that only 369 municipalities (6.63% of the Brazilian municipalities in 2011) participate in the PMAT reveals a low acceptance of the program among municipal governments in general (Grin, 2014).

Conclusion

Some of the most famous formal models in political economy make the prediction that taxation ought to increase with inequality in democracies (Romer, 1975; Meltzer and Richard, 1981). Yet in many cases, scholars do not find the stated relationship to be true. We argue that this may be explained by wealthy elites undermining the state's ability to collect taxes in highly unequal democracies, especially when the state's capacity is already limited.

To investigate this proposition, we use data on property tax revenue, inequality, and other economic variables from over 5,500 municipalities in Brazil. Using cross-sectional, as well as panel models, and undertaking a variety of robustness checks, we show that municipalities with higher levels of inequality have lower levels of fiscal capacity/raise less revenue from the local property tax. The evidence is consistent with our theoretical argument. We do acknowledge, however, that we can not yet identify the exact causal mechanism and that other potential explanations are possible. On the other hand, our results are strengthened by the fact that municipalities with higher inequality were also significantly less likely to apply for federal programs that could aid their tax collection efforts.

If wealthy elites do actively undermine tax administration in highly unequal societies, this should have consequences for how we view democratic policy-making and the delivery of public goods. A democratic political system is no panacea: even if the will of the voters may be translated into policies, the state is not always able to properly enforce the policy choices made. On the other hand, it may be that as democracies stabilize and become further removed from their authoritarian origins, they can slowly diminish the influence of elites and increase capacity. This possibility should be further investigated in future cross-national work. Similarly, as we argue in the paper, we think that our findings are generalizable to national level politics. Yet, subsequent studies ought to investigate whether the lack of evidence in line with the Meltzer and Richard (1981) model cross-nationally can be explained by the theoretical argument made here.

Lastly, future research should further consider the exact mechanisms by which economic elites can undermine the state's capacity to collect revenues and enforce policies. Better understanding of these processes will help us gain a better grasp of the difficulties of policy-making in (young) democracies and thus the threats to their existence. Additionally, further research ought to investigate how limited state capacity can influence the nexus between voters and politicians. For example, low levels of capacity may impact voters' preferred policies and evaluation of politicians, especially when it comes to taxation and public goods.

Acknowledgements

We thank Pedro de Carvalho Júnior and Eduardo Grin for their valuable help with the data. We also thank Pablo Beramendi, Jose Cheibub, Eunyoung Ha, Jan Pierskalla, and Ken Scheve for valuable feedback. We also thank three anonymous reviewers and the editorial team at the Journal of Politics for their feedback and help on this paper. Previous versions of this manuscript were presented at the 112th Annual Meeting of the American Political Science Association, Philadelphia, PA and the workshop in Political Economy and Political Violence at Texas A&M University.

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Supplementary Online Appendix: Fiscal Capacity and Inequality: Evidence from Brazilian Municipalities

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July 17, 2018

A Appendix: Additional Models

Table A.1: Inequality and Fiscal Capacity in Brazilian Municipalities (Cross-Sectional Models for 2000 and 2010) – Non-imputed Data

	Dependent variable: IPTU Revenue (log)					
	Model 1 2000	Model 2 2000 (robust)	Model 3 2000 (cluster)	Model 4 2010	Model 5 2010 (robust)	Model 6 2010 (cluster)
Gini	-3.657***	-3.657***	-3.657***	-6.190***	-6.190***	-6.190***
	(0.579)	(0.619)	(0.922)	(0.493)	(0.624)	(1.396)
Population (log)	-0.994***	-0.994***	-0.994***	-0.080	-0.080	-0.080
	(0.087)	(0.105)	(0.349)	(0.075)	(0.081)	(0.185)
GDP (log)	2.230***	2.230***	2.230***	1.503***	1.503***	1.503***
	(0.074)	(0.091)	(0.226)	(0.059)	(0.068)	(0.190)
Left Party	-0.100	-0.100	-0.100	-0.033	-0.033	-0.033
	(0.087)	(0.086)	(0.122)	(0.058)	(0.059)	(0.059)
Rural Share	-2.823***	-2.823***	-2.823***	-2.734***	-2.734***	-2.734***
	(0.197)	(0.209)	(0.488)	(0.158)	(0.182)	(0.401)
Housing and Urbanization (log)	0.008	0.008	0.008	0.052***	0.052**	0.052*
	(0.017)	(0.018)	(0.027)	(0.015)	(0.023)	(0.027)
Transfers (log)	-0.071	-0.071	-0.071	-0.228*	-0.228*	-0.228
	(0.140)	(0.181)	(0.365)	(0.123)	(0.134)	(0.195)
Oil Revenue (log)	-0.033***	-0.033***	-0.033	-0.020***	-0.020***	-0.020
	(0.011)	(0.012)	(0.031)	(0.007)	(0.008)	(0.018)
Constant	-0.679	-0.679	-0.679	2.391**	2.391*	2.391
	(1.250)	(1.532)	(2.883)	(1.162)	(1.226)	(1.864)
N	4845	4845	4845	4269	4269	4269
R^2	0.507	0.507	0.507	0.641	0.641	0.641

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).
Model 2 and Model 5 with robust standard errors.
Model 3 and Model 6 with standard errors clustered by state.
The negative and significant estimates for Gini in all models indicate that, as inequality increases, the state's ability to raise revenue from citizens decreases substantially.
Standard errors in parentheses. Two-tailed test.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Inequality and Fiscal Capacity in Brazilian Municipalities (Cross-Sectional Models for 2000 and 2010) – Imputed data

	Dependent variable: IPTU Revenue (log)					
	Model 7 2000	Model 8 2000 (robust)	Model 9 2000 (cluster)	Model 10 2010	Model 11 2010 (robust)	Model 12 2010 (cluster)
Gini	-3.736***	-3.736***	-3.736***	-6.491***	-6.491***	-6.491***
	(0.584)	(0.615)	(0.894)	(0.519)	(0.639)	(1.388)
Population (log)	-1.117***	-1.117***	-1.117***	-0.366***	-0.366***	-0.366*
	(0.076)	(0.084)	(0.278)	(0.068)	(0.079)	(0.189)
GDP (log)	2.070***	2.070***	2.070***	1.327***	1.327***	1.327***
	(0.063)	(0.073)	(0.220)	(0.055)	(0.063)	(0.183)
Left Party	-0.097	-0.097	-0.097	-0.055	-0.055	-0.055
	(0.088)	(0.087)	(0.122)	(0.062)	(0.063)	(0.073)
Rural Share	-2.876***	-2.876***	-2.876***	-2.912***	-2.912***	-2.912***
	(0.197)	(0.208)	(0.475)	(0.168)	(0.188)	(0.386)
Housing and Urbanization (log)	0.011	0.011	0.011	0.061***	0.061**	0.061**
	(0.017)	(0.019)	(0.028)	(0.018)	(0.024)	(0.028)
Transfers (log)	0.345***	0.345***	0.345***	0.396***	0.396***	0.396***
	(0.073)	(0.080)	(0.086)	(0.083)	(0.102)	(0.099)
Oil Revenue (log)	-0.032***	-0.032***	-0.032	-0.023***	-0.023***	-0.023
	(0.011)	(0.012)	(0.029)	(0.007)	(0.008)	(0.018)
Constant	-4.193***	-4.193***	-4.193***	-3.383***	-3.383***	-3.383***
	(0.749)	(0.790)	(0.993)	(0.830)	(0.996)	(1.088)
N	5114	5114	5114	4580	4580	4580
R^2	.	.	.	.	.	.

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).
Model 8 and Model 11 with robust standard errors.
Model 9 and Model 12 with standard errors clustered by state.
The results are consistent with the models using non-imputed data: The negative and significant estimates for Gini in all models indicate that as inequality increases, the state's ability to raise revenue from citizens decreases substantially.
Standard errors in parentheses. Two-tailed test.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Results from Spatial Autoregressive Models

	Dependent variable: IPTU (log)			
	Model 1	Model 2	Model 3	Model 4
	2000 Binary	2000 Row-standardized	2010 Binary	2010 Row-standardized
Gini	-2.859^{***}	-1.437^{***}	-5.304^{***}	-3.446^{***}
	(0.563)	(0.551)	(0.488)	(0.474)
Population (log)	-0.805^{***}	-0.277^{***}	-0.077	0.255^{***}
	(0.083)	(0.084)	(0.073)	(0.071)
GPD (log)	1.844^{***}	1.535^{***}	1.358^{***}	1.094^{***}
	(0.074)	(0.074)	(0.058)	(0.058)
Left Party	-0.049	-0.116	-0.038	-0.070
	(0.084)	(0.082)	(0.057)	(0.055)
Rural Share	-2.770^{***}	-2.426^{***}	-2.776^{***}	-2.546^{***}
	(0.170)	(0.166)	(0.155)	(0.151)
Housing and Urbanization (log)	0.001	0.003	0.047^{***}	0.046^{***}
	(0.016)	(0.016)	(0.015)	(0.014)
Transfers (log)	-0.053	-0.103	-0.198^{*}	-0.251^{**}
	(0.136)	(0.133)	(0.120)	(0.116)
Oil Revenue (log)	-0.019^{*}	-0.036^{***}	-0.013^{**}	-0.028^{***}
	(0.011)	(0.011)	(0.006)	(0.006)
Intercept	-0.438	-3.961^{***}	2.281^{**}	-0.368
	(1.208)	(1.176)	(1.139)	(1.099)
N	4838	4838	4261	4261
Log-Likelihood	−11,220.330	−11,124.920	−8,278.976	−8,151.819
σ^2	6.027	5.693	2.849	2.642
Akaike Inf. Crit.	22,462.660	22,271.840	16,579.950	16,325.640
Wald Test (df = 1)	239.216^{***}	479.604^{***}	132.134^{***}	416.207^{***}
LR Test (df = 1)	238.898^{***}	429.717^{***}	131.314^{***}	385.629^{***}

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).
This table shows the results from four spatial autoregressive models with neighbors based on contiguous boundaries between the municipalities, using 2000 and 2010 cross-sectional data. The results in Model 1 and Model 3 are based on a binary neighbor matrix, while the results in Model 2 and Model 4 are based on a row-standardized weights matrix. The results are in line with our findings: the coefficients for inequality (*Gini*) are still substantively meaningful, negative, and precisely estimated.
Standard errors in parentheses. Two-tailed test.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Bivariate Cross-Sectional Models: Benchmark to Compare the Results

	Model 1 2000	Model 2 2000 (robust)	Model 3 2000 (cluster)	Model 4 2010	Model 5 2010 (robust)	Model 6 2010 (cluster)
Gini	-5.402***	-5.402***	-5.402**	-7.468***	-7.468***	-7.468***
	(0.745)	(0.735)	(2.417)	(0.594)	(0.623)	(2.377)
Constant	12.085***	12.085***	12.085***	14.607***	14.607***	14.607***
	(0.410)	(0.399)	(1.533)	(0.295)	(0.297)	(1.160)
N	5304	5304	5304	5211	5211	5211
R^2	0.010	0.010	0.010	0.029	0.029	0.029

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).

Model 2 and Model 5 with robust standard errors.

Model 3 and Model 6 with standard errors clustered by state.

To increase confidence in the results, we present OLS estimations without any controls, as a benchmark to compare the results. The results are consistent with our previous models including controls.

Standard errors in parentheses. Two-tailed test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B Appendix: Additional Controls

Table B.1: Original Cross-Sectional Models Including the Control Variable "Turnout"

	Dependent variable: IPTU Revenue (log)			
	Model 1 2000 (robust)	Model 2 2000 (cluster)	Model 3 2010 (robust)	Model 4 2010 (cluster)
Gini	-3.202***	-3.202***	-6.019***	-6.019***
	(0.627)	(0.851)	(0.632)	(1.375)
Turnout	3.343***	3.343***	1.316**	1.316*
	(0.789)	(1.067)	(0.566)	(0.752)
Population (log)	-0.833***	-0.833**	-0.014	-0.014
	(0.109)	(0.339)	(0.084)	(0.187)
GDP (log)	2.162***	2.162***	1.497***	1.497***
	(0.093)	(0.223)	(0.068)	(0.186)
Left Party	-0.109	-0.109	-0.025	-0.025
	(0.086)	(0.118)	(0.059)	(0.060)
Rural Share	-2.807***	-2.807***	-2.689***	-2.689***
	(0.207)	(0.467)	(0.181)	(0.393)
Housing and Urbanization (log)	0.006	0.006	0.052**	0.052*
	(0.018)	(0.026)	(0.023)	(0.028)
Transfers (log)	-0.081	-0.081	-0.272**	-0.272
	(0.180)	(0.360)	(0.135)	(0.200)
Oil Revenue (log)	-0.035***	-0.035	-0.020***	-0.020
	(0.012)	(0.031)	(0.008)	(0.019)
Constant	-4.466**	-4.466	1.322	1.322
	(1.769)	(3.146)	(1.296)	(1.598)
N	4844	4844	4250	4250
R^2	0.509	0.509	0.642	0.642

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).
$Turnout = \frac{\text{total number of voters in the municipal election}}{\text{total number of the electorate in the municipal election}}$
Model 1 and Model 3 with robust standard errors.
Model 2 and Model 4 with standard errors clustered by state.
The results from models including the independent variable *turnout* are consistent with our previous models: more unequal municipalities have a lower capacity to collect taxes.
Standard errors in parentheses. Two-tailed test.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Electoral Competition (Cross-Sectional Model for 2010)

	Model 1	Model 2 (robust)	Model 3 (cluster)
Gini	-6.305***	-6.305***	-6.305***
	(0.509)	(0.645)	(1.410)
Electoral Competition	0.310	0.310	0.310
	(0.203)	(0.192)	(0.191)
Population (log)	-0.049	-0.049	-0.049
	(0.078)	(0.083)	(0.185)
GDP (log)	1.533***	1.533***	1.533***
	(0.061)	(0.070)	(0.195)
Left Party	-0.030	-0.030	-0.030
	(0.059)	(0.060)	(0.061)
Rural Share	-2.700***	-2.700***	-2.700***
	(0.163)	(0.188)	(0.392)
Housing and Urbanization (log)	0.049***	0.049**	0.049*
	(0.016)	(0.024)	(0.028)
Transfers (log)	-0.294**	-0.294**	-0.294
	(0.128)	(0.139)	(0.195)
Oil Revenue (log)	-0.017***	-0.017**	-0.017
	(0.007)	(0.008)	(0.019)
Constant	2.916**	2.916**	2.916
	(1.201)	(1.261)	(1.834)
N	4074	4074	4074
R^2	0.642	0.642	0.642

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).

$\textit{Electoral Competition} = \frac{\text{elected candidate's vote share - runner up candidate's vote share}}{\text{total number of the electorate in the municipal election}}$

Model 2 with robust standard errors, and Model 3 with standard errors clustered by state.

The results for the models that include the independent variable *electoral competition* are consistent with our previous results: more unequal municipalities have a lower capacity to collect taxes.

Standard errors in parentheses. Two-tailed test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Vulnerability to Poverty (%), Cross-Section and Panel Models

	Model 1 2000 (robust)	Model 2 2000 (cluster)	Model 3 2010 (robust)	Model 4 2010 (cluster)	Model 5 1991-2000-2010 (FE & cluster)
Gini	-2.982***	-2.982***	-5.302***	-5.302***	-3.142**
	(0.617)	(0.883)	(0.631)	(1.358)	(1.167)
Vulnerability to Poverty (%)	-0.021***	-0.021***	-0.015***	-0.015***	-0.014***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)
Population (log)	-0.751***	-0.751**	0.069	0.069	2.024***
	(0.106)	(0.337)	(0.079)	(0.173)	(0.464)
GDP (log)	1.950***	1.950***	1.304***	1.304***	-0.043
	(0.096)	(0.223)	(0.070)	(0.169)	(0.171)
Left Party	-0.103	-0.103	-0.006	-0.006	
	(0.086)	(0.119)	(0.058)	(0.058)	
Rural Share	-2.596***	-2.596***	-2.479***	-2.479***	-0.412
	(0.205)	(0.424)	(0.181)	(0.336)	(1.004)
Housing and Urbanization (log)	0.014	0.014	0.052**	0.052*	-0.005
	(0.018)	(0.026)	(0.023)	(0.027)	(0.015)
Transfers (log)	-0.029	-0.029	-0.162	-0.162	0.371**
	(0.178)	(0.352)	(0.131)	(0.187)	(0.161)
Oil Revenue (log)	-0.024**	-0.024	-0.017**	-0.017	
	(0.012)	(0.030)	(0.008)	(0.017)	
2000					3.878*
					(2.213)
2010					4.440*
					(2.349)
Constant	0.143	0.143	2.171*	2.171	-16.439***
	(1.508)	(2.723)	(1.195)	(1.790)	(3.223)
N	4845	4845	4269	4269	8138
R^2	0.518	0.518	0.650	0.650	0.878

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).

Vulnerability to Poverty (%) = The proportion of individuals with a per capita household income equals to or less than R$255.00 per month, in Brazilian reais as of August 2010, which is equivalent to half of the average minimum salary in Brazil as of that date. The sample of individuals is limited to those who live in permanent private households.

Model 1 and Model 3 cross-sectional models with robust standard errors. Model 2 and Model 4 cross-sectional models with standard errors clustered by state. Model 5 Panel model (1991-2000-2010) with Year and Municipal fixed-effects and standard errors clustered by state.

The results for models including the independent variable *vulnerability to poverty (%)* are consistent with previous results: more unequal municipalities have a lower capacity to collect taxes.

Standard errors in parentheses. Two-tailed test. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: Bolsa Família Cash Transfer Program, Cross-Sectional Models for 2010

	Model 1	Model 2 (robust)	Model 3 (cluster)	Model 4	Model 5 (robust)	Model 6 (cluster)
Gini	-3.134***	-3.134***	-3.134*	-2.521***	-2.521***	-2.521*
	(0.500)	(0.683)	(1.587)	(0.501)	(0.670)	(1.463)
Number of Families (log)	-1.277***	-1.277***	-1.277***			
	(0.067)	(0.069)	(0.200)			
Cash Benefits Amount (log)				-1.221***	-1.221***	-1.221***
				(0.058)	(0.057)	(0.159)
Population (log)	1.727***	1.727***	1.727***	1.732***	1.732***	1.732***
	(0.118)	(0.116)	(0.373)	(0.112)	(0.104)	(0.322)
GDP (log)	0.753***	0.753***	0.753***	0.686***	0.686***	0.686***
	(0.068)	(0.073)	(0.204)	(0.068)	(0.072)	(0.194)
Left Party	-0.011	-0.011	-0.011	-0.018	-0.018	-0.018
	(0.055)	(0.057)	(0.053)	(0.055)	(0.056)	(0.053)
Rural Share	-2.356***	-2.356***	-2.356***	-2.242***	-2.242***	-2.242***
	(0.153)	(0.179)	(0.230)	(0.152)	(0.178)	(0.230)
Housing and Urbanization (log)	0.045***	0.045**	0.045*	0.043***	0.043*	0.043*
	(0.015)	(0.023)	(0.026)	(0.015)	(0.022)	(0.025)
Transfers (log)	-0.072	-0.072	-0.072	-0.031	-0.031	-0.031
	(0.118)	(0.127)	(0.206)	(0.117)	(0.126)	(0.193)
Oil Revenue (log)	0.001	0.001	0.001	0.002	0.002	0.002
	(0.006)	(0.007)	(0.017)	(0.006)	(0.007)	(0.016)
Constant	-1.870	-1.870	-1.870	2.946***	2.946***	2.946
	(1.137)	(1.188)	(2.084)	(1.106)	(1.132)	(1.851)
N	4269	4269	4269	4269	4269	4269
R^2	0.669	0.669	0.669	0.675	0.675	0.675

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).

Model 2 and Model 5 with robust standard errors.

Model 3 and Model 6 with standard errors clustered by state.

The results when including the number of families that receives the Bolsa Família cash transfer (*number of families*) or the amount of cash benefits in Brazilian reais (cash benefits amount) are consistent with our previous results: more unequal municipalities have a lower capacity to collect taxes.

Standard errors in parentheses. Two-tailed test. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: GDP Growth

	Model 1 2000	Model 2 2000 (robust)	Model 3 2000 (cluster)	Mode 4 2010	Model 5 2010 (robust)	Model 6 2010 (cluster)
Gini	-2.286***	-2.286***	-2.286**	-5.798***	-5.798***	-5.798***
	(0.661)	(0.702)	(0.909)	(0.492)	(0.630)	(1.326)
GDP Growth	-0.143***	-0.143***	-0.143***	-0.240***	-0.240***	-0.240**
	(0.027)	(0.043)	(0.046)	(0.024)	(0.075)	(0.088)
Population (log)	-0.750***	-0.750***	-0.750**	-0.221***	-0.221***	-0.221
	(0.098)	(0.130)	(0.279)	(0.076)	(0.083)	(0.181)
GDP (log)	2.316***	2.316***	2.316***	1.629***	1.629***	1.629***
	(0.081)	(0.101)	(0.214)	(0.060)	(0.075)	(0.196)
Left Party	-0.085	-0.085	-0.085	-0.015	-0.015	-0.015
	(0.091)	(0.091)	(0.110)	(0.057)	(0.058)	(0.059)
Rural Share	-2.939***	-2.939***	-2.939***	-2.563***	-2.563***	-2.563***
	(0.229)	(0.241)	(0.463)	(0.158)	(0.188)	(0.377)
Housing and Urbanization (log)	0.002	0.002	0.002	0.057***	0.057**	0.057**
	(0.020)	(0.022)	(0.031)	(0.015)	(0.023)	(0.027)
Transfers (log)	-0.504***	-0.504**	-0.504	-0.219*	-0.219	-0.219
	(0.148)	(0.228)	(0.320)	(0.122)	(0.135)	(0.179)
Oil Revenue (log)	-0.036***	-0.036***	-0.036	-0.020***	-0.020**	-0.020
	(0.011)	(0.013)	(0.031)	(0.006)	(0.008)	(0.017)
Constant	2.293*	2.293	2.293	2.018*	2.018	2.018
	(1.294)	(1.880)	(2.649)	(1.155)	(1.228)	(1.648)
N	3695	3695	3695	4243	4243	4243
R^2	0.546	0.546	0.546	0.649	0.649	0.649

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).
$GDP\ Growth = \frac{GDP - GDP_{t-1}}{GDP_{t-1}}$
Model 2 and Model 5 with robust standard errors.
Model 3 and Model 6 with standard errors clustered by state.
Results for models including the independent variable *GDP growth* are consistent with those reported previously: more unequal municipalities have a lower capacity to collect taxes.
Standard errors in parentheses. Two-tailed test.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Adding ITBI and Total Tax as Independent Variables into the Original Cross-Sectional Model for 2010

	Model 1 IPTU	Model 2 2010 (robust)	Model 3 2010 (cluster)	Model 4 2010	Model 5 2010 (robust)	Model 6 2010 (cluster)
Gini	-6.190***	-6.190***	-6.190***	-5.154***	-5.154***	-5.154***
	(0.493)	(0.624)	(1.396)	(0.442)	(0.534)	(0.998)
ITBI (log)				0.288***	0.288***	0.288***
				(0.012)	(0.026)	(0.037)
Total Tax (log)				0.673***	0.673***	0.673***
				(0.042)	(0.059)	(0.127)
Population (log)	-0.080	-0.080	-0.080	0.051	0.051	0.051
	(0.075)	(0.081)	(0.185)	(0.066)	(0.067)	(0.136)
GDP (log)	1.503***	1.503***	1.503***	0.535***	0.535***	0.535***
	(0.059)	(0.068)	(0.190)	(0.061)	(0.074)	(0.153)
Left Party	-0.033	-0.033	-0.033	-0.040	-0.040	-0.040
	(0.058)	(0.059)	(0.059)	(0.051)	(0.052)	(0.049)
Rural Share	-2.734***	-2.734***	-2.734***	-1.701***	-1.701***	-1.701***
	(0.158)	(0.182)	(0.401)	(0.144)	(0.172)	(0.345)
Housing and Urbanization (log)	0.052***	0.052**	0.052*	0.006	0.006	0.006
	(0.015)	(0.023)	(0.027)	(0.014)	(0.021)	(0.018)
Transfers (log)	-0.228*	-0.228*	-0.228	-0.426***	-0.426***	-0.426**
	(0.123)	(0.134)	(0.195)	(0.112)	(0.125)	(0.167)
Oil Revenue (log)	-0.020***	-0.020***	-0.020	-0.015**	-0.015**	-0.015
	(0.007)	(0.008)	(0.018)	(0.006)	(0.007)	(0.016)
Constant	2.391**	2.391*	2.391	2.518**	2.518**	2.518
	(1.162)	(1.226)	(1.864)	(1.046)	(1.094)	(1.521)
N	4269	4269	4269	4265	4265	4265
R^2	0.641	0.641	0.641	0.715	0.715	0.715

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).

ITBI = Tax Revenue on Real Estate Transfers in Brazilian reais.

Total Tax = Total taxes revenue collected by the municipality.

Model 2 and Model 5 with robust standard errors.

Model 3 and Model 6 with standard errors clustered by state.

Results for models including the independent variable *ITBI* are consistent with previous models: more unequal municipalities have a lower capacity to collect taxes.

Standard errors in parentheses. Two-tailed test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.7: Industry Value Added as Percentage of GDP

	Model 1 2000 (robust)	Model 2 2000 (cluster)	Model 3 2010 (robust)	Model 4 2010 (cluster)	Model 5 1991-2000-2010 (FE & cluster)
Gini	-4.229***	-4.229***	-6.529***	-6.529***	-3.443**
	(0.629)	(0.979)	(0.627)	(1.422)	(1.261)
Industry (% GDP)	-2.908***	-2.908***	-1.960***	-1.960***	-1.778***
	(0.394)	(0.804)	(0.241)	(0.341)	(0.524)
Population (log)	-1.140***	-1.140***	-0.198**	-0.198	1.788***
	(0.103)	(0.359)	(0.080)	(0.178)	(0.452)
GDP (log)	2.475***	2.475***	1.668***	1.668***	0.066
	(0.099)	(0.264)	(0.074)	(0.200)	(0.175)
Left Party	-0.140	-0.140	-0.037	-0.037	
	(0.086)	(0.117)	(0.059)	(0.057)	
Rural Share	-2.947***	-2.947***	-2.750***	-2.750***	-0.818
	(0.210)	(0.461)	(0.180)	(0.397)	(0.858)
Housing and Urbanization (log)	0.016	0.016	0.054**	0.054*	-0.003
	(0.018)	(0.028)	(0.023)	(0.028)	(0.015)
Transfers (log)	-0.112	-0.112	-0.227*	-0.227	0.370**
	(0.180)	(0.367)	(0.133)	(0.189)	(0.159)
Oil Revenue (log)	-0.023*	-0.023	-0.017**	-0.017	
	(0.012)	(0.030)	(0.008)	(0.017)	
2000					3.884*
					(2.209)
2010					4.673*
					(2.376)
Constant	-0.566	-0.566	2.153*	2.153	-15.652***
	(1.517)	(2.898)	(1.210)	(1.752)	(2.909)
N	4845	4845	4269	4269	8138
R^2	0.513	0.513	0.646	0.646	0.879

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).

Industry (% GDP) = Industry value added, as % of GDP.

We could not find data for *municipal GDP* and *Industry (% GDP)* for the early 1990s. We thus have to rely on a GDP measurement from 1985 in the panel data for 1991.

Model 1 and Model 3 cross-sectional models with robust standard errors. Model 2 and Model 4 cross-sectional models with standard errors clustered by state. Model 5 Panel model (1991-2000-2010) with Year and Municipal fixed-effects and standard errors clustered by state.

Results for models including the independent variable *Industry (% GDP)* are consistent with our results reported in the manuscript: more unequal municipalities have a lower capacity to collect taxes. The results are consistent when dropping the 10 observations below 0 and 4 observations above 1 for *Industry (% GDP)*.

Standard errors in parentheses. Two-tailed test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C Appendix: Additional Dependent Variables

Table C.1: IPTU as a Ratio DV, Cross-Sectional Models for 2010

	Model 1 $\frac{IPTU}{GDP}$	Model 2 $\frac{IPTU}{GDP}$ (robust)	Model 3 $\frac{IPTU}{GDP}$ (cluster)	Model 4 $\frac{IPTU}{Tax}$	Model 5 $\frac{IPTU}{Tax}$ (robust)	Model 6 $\frac{IPTU}{Tax}$ (cluster)
Gini	-5.156**	-5.156***	-5.156*	-0.432***	-0.432***	-0.432***
	(2.213)	(1.766)	(2.911)	(0.034)	(0.035)	(0.073)
Population (log)	0.418	0.418	0.418	0.017***	0.017***	0.017
	(0.344)	(0.299)	(0.399)	(0.005)	(0.006)	(0.012)
Left Party	-0.439*	-0.439*	-0.439	-0.002	-0.002	-0.002
	(0.266)	(0.254)	(0.365)	(0.004)	(0.004)	(0.004)
Rural Share	-9.056***	-9.056***	-9.056***	-0.178***	-0.178***	-0.178***
	(0.689)	(1.028)	(2.947)	(0.011)	(0.011)	(0.045)
Housing and Urbanization (log)	0.206***	0.206***	0.206*	0.002*	0.002*	0.002
	(0.070)	(0.048)	(0.120)	(0.001)	(0.001)	(0.002)
Transfers (log)	0.520	0.520	0.520	-0.083***	-0.083***	-0.083***
	(0.439)	(0.429)	(0.569)	(0.009)	(0.009)	(0.019)
Oil Revenue (log)	0.083***	0.083	0.083	-0.002***	-0.002***	-0.002
	(0.029)	(0.064)	(0.136)	(0.000)	(0.000)	(0.001)
GDP (log)				0.068***	0.068***	0.068***
				(0.004)	(0.004)	(0.014)
Constant	-6.608	-6.608	-6.608	0.888***	0.888***	0.888***
	(4.563)	(5.012)	(6.577)	(0.081)	(0.089)	(0.196)
N	4269	4269	4269	4267	4267	4267
R^2	0.113	0.113	0.113	0.344	0.344	0.344

Notes: Dependent variables: IPTU Revenue/GDP (Model 1, Model 2, and Model 3); IPTU Revenue/Total tax revenue (Model 4, Model 5, and Model 6)
Model 2 and Model 5 with robust standard errors.
Model 3 and Model 6 with standard errors clustered by state.
Results when using an alternative measures of tax capacity (IPTU as a ratio of GDP and as a ratio of total tax) are consistent with previous results: more unequal municipalities have a lower capacity to collect taxes.
Standard errors in parentheses. Two-tailed test.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Alternative Local Tax: Original Cross-Sectional Model for 2010 and Model Using ITBI as Dependent Variable

	Model 1 IPTU	Model 2 IPTU (robust)	Model 3 IPTU (cluster)	Model 4 ITBI	Model 5 ITBI (robust)	Model 6 ITBI (cluster)
Gini	-6.190***	-6.190***	-6.190***	-4.378***	-4.378***	-4.378***
	(0.493)	(0.624)	(1.396)	(0.566)	(0.673)	(1.502)
Population (log)	-0.080	-0.080	-0.080	-0.341***	-0.341***	-0.341*
	(0.075)	(0.081)	(0.185)	(0.085)	(0.093)	(0.173)
GDP (log)	1.503***	1.503***	1.503***	1.865***	1.865***	1.865***
	(0.059)	(0.068)	(0.190)	(0.067)	(0.077)	(0.219)
Left Party	-0.033	-0.033	-0.033	0.062	0.062	0.062
	(0.058)	(0.059)	(0.059)	(0.066)	(0.064)	(0.079)
Rural Share	-2.734***	-2.734***	-2.734***	-1.782***	-1.782***	-1.782***
	(0.158)	(0.182)	(0.401)	(0.181)	(0.193)	(0.354)
Housing and Urbanization (log)	0.052***	0.052**	0.052*	0.062***	0.062***	0.062*
	(0.015)	(0.023)	(0.027)	(0.017)	(0.021)	(0.030)
Transfers (log)	-0.228*	-0.228*	-0.228	-0.685***	-0.685***	-0.685**
	(0.123)	(0.134)	(0.195)	(0.141)	(0.150)	(0.271)
Oil Revenue (log)	-0.020***	-0.020***	-0.020	-0.019**	-0.019**	-0.019
	(0.007)	(0.008)	(0.018)	(0.007)	(0.008)	(0.020)
Constant	2.391**	2.391*	2.391	6.917***	6.917***	6.917**
	(1.162)	(1.226)	(1.864)	(1.332)	(1.397)	(2.520)
N	4269	4269	4269	4267	4267	4267
R^2	0.641	0.641	0.641	0.524	0.524	0.524

Notes: Dependent variables: IPTU Revenue (logged) (Model 1, Model 2, and Model 3);
ITBI Revenue (logged) (Model 4, Model 5, and Model 6)
ITBI = Tax Revenue on Real Estate Transfers in Brazilian reais.
Model 2 and Model 5 with robust standard errors.
Model 3 and Model 6 with standard errors clustered by state.
Results using an alternative local tax *(ITBI)* as our dependent variable are consistent with our previous models: more unequal municipalities have a lower capacity to collect tax.
Standard errors in parentheses. Two-tailed test.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: Correlation Matrix

	IPTU	**ITBI**
IPTU	1.0000	
ITBI	0.9702 0.000	1.0000

Considering the high positive correlation between IPTU and ITBI (Pearson's r = 0.97) it is not surprising that the results using ITBI as the dependent variable are consistent with the main results from our original models.

Table C.4: IPTU Collected by Number of Buildings as DV, 2000

	DV: IPTU Collected (By Buildings)	
	Model 1 (robust)	Model 2 (cluster)
Gini	-0.391***	-0.391***
	(0.063)	(0.076)
Population (log)	-0.163***	-0.163***
	(0.010)	(0.022)
GDP (log)	0.149***	0.149***
	(0.008)	(0.021)
Left Party	0.002	0.002
	(0.009)	(0.009)
Rural Share	0.124***	0.124**
	(0.021)	(0.057)
Housing and Urbanization (log)	0.003*	0.003
	(0.002)	(0.003)
Transfers (log)	0.001	0.001
	(0.015)	(0.013)
Oil Revenue (log)	-0.007***	-0.007***
	(0.001)	(0.002)
Constant	0.526***	0.526***
	(0.130)	(0.101)
N	4005	4005
R^2	0.175	0.175

Notes: Dependent variables: $\frac{\text{total number of buildings that paid IPTU}}{\text{total number of buildings that could be charged}}$

Model 1 with robust standard errors. Model 2 with standard errors clustered by state.

The results when using *IPTU Collected by buildings* as our dependent variable are consistent with those in our previous models: more unequal municipalities have a lower capacity to collect taxes.

Standard errors in parentheses. Two-tailed test. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: IPTU Collection Rate as DV

	Dependent variable: IPTU Collection Rate						
	Model 1 2000	Model 2 2000 (robust)	Model 3 2000 (cluster)	Model 4 2010	Model 5 2010 (robust)	Model 6 2010 (cluster)	Model 7 Panel (2000-2010) (FE & cluster)
Gini	-0.735**	-1.047**	-1.047*	-0.613**	-0.676**	-0.676*	-1.460**
	(0.315)	(0.428)	(0.556)	(0.250)	(0.317)	(0.392)	(0.544)
Population (log)		-0.092	-0.092		-0.088**	-0.088***	0.085
		(0.056)	(0.055)		(0.042)	(0.024)	(0.192)
GDP (log)		0.098	0.098*		0.162***	0.162***	-0.134***
		(0.061)	(0.048)		(0.046)	(0.044)	(0.032)
Left Party		-0.039	-0.039		-0.055*	-0.055**	-0.054***
		(0.035)	(0.029)		(0.028)	(0.025)	(0.016)
Rural Share		-0.237	-0.237		-0.349**	-0.349**	-0.975*
		(0.325)	(0.379)		(0.158)	(0.159)	(0.506)
Housing and Urbanization (log)		0.013	0.013		-0.003	-0.003	0.003
		(0.018)	(0.017)		(0.005)	(0.005)	(0.002)
Transfers (log)		0.003	0.003		-0.042	-0.042	0.043
		(0.083)	(0.086)		(0.079)	(0.069)	(0.053)
Oil Revenue (log)		-0.001	-0.001		-0.007***	-0.007***	0.010***
		(0.004)	(0.004)		(0.002)	(0.002)	(0.002)
2010							0.018
							(0.058)
Constant	0.910***	0.606	0.606	0.937***	0.633	0.633	1.421
	(0.176)	(0.532)	(0.652)	(0.130)	(0.609)	(0.556)	(1.968)
N	180	142	142	180	142	142	238
R^2	0.030	0.201	0.201	0.033	0.352	0.352	0.498

Notes: Dependent variables: IPTU Collection Rate as measured by Carvalho, Jr. (2017).
Model 2 and Model 5 with robust standard errors.
Model 3 and Model 6 with standard errors clustered by state.
Model 7 with year and municipality fixed-effects and standard errors clustered by state.
Results when using *IPTU Collection Rate* as our dependent variable are consistent with the results presented in the manuscript: more unequal municipalities have a lower capacity to collect taxes.
Standard errors in parentheses. Two-tailed test. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.6: Full Table: Municipal Applications to the Capacity-Building Program (PMAT)

	Dependent variable: PMAT Application			
	Model 1 OLS (robust)	Model 2 OLS (cluster)	Model 3 Logit (robust)	Model 4 Logit (cluster)
Gini	-0.242***	-0.242***	-2.786**	-2.786**
	(0.050)	(0.070)	(1.166)	(1.230)
IPTU Revenue (log)	0.057***	0.057***	0.449***	0.449***
	(0.007)	(0.010)	(0.084)	(0.126)
Population (log)	0.032***	0.032***	0.231**	0.231
	(0.006)	(0.009)	(0.114)	(0.196)
GDP (log)	0.019***	0.019**	0.489***	0.489***
	(0.005)	(0.008)	(0.099)	(0.138)
Rural Share	-0.035*	-0.035	-0.699*	-0.699
	(0.019)	(0.026)	(0.423)	(0.526)
Transfers (log)	-0.017***	-0.017**	-0.209***	-0.209
	(0.004)	(0.008)	(0.069)	(0.131)
Constant	-0.270***	-0.270***	-8.390***	-8.390***
	(0.057)	(0.067)	(1.042)	(1.560)
N	4047	4047	4047	4047
R^2	0.193	0.193		
Log-likelihood			-755.391	-755.391

Notes: Dependent variable: Binary variable PMAT (1 = municipality applied to PMAT, 0 = municipality didn't apply to PMAT).
Model 1 and Model 3 with robust standard errors.
Model 2 and Model 4 with standard errors clustered by state.
Results when *PMAT* as our dependent variable indicate that greater inequality is associated with a lower likelihood of application to PMAT.
Standard errors in parentheses. Two-tailed test.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

D Appendix: Panel Models

Table D.1: Panel Fixed Effects Models (1991, 2000, and 2010)

	Dependent variable: IPTU Revenue (log)			
	Model 1 1991-2000-2010 (non-imputed)	Model 2 1991-2000-2010 (imputed)	Model 3 2000-2010 (non-imputed)	Model 4 2000-2010 (imputed)
Gini	-3.479***	-5.118***	0.805	0.084
	(1.220)	(1.113)	(0.622)	(0.665)
Population (log)	1.996***	1.455***	0.642	0.042
	(0.459)	(0.435)	(0.494)	(0.511)
GDP (log)	-0.033	0.026	0.654**	0.697***
	(0.171)	(0.168)	(0.238)	(0.207)
Rural Share	-0.468	-0.801	-2.809**	-2.662**
	(0.959)	(0.789)	(1.012)	(1.089)
Housing and Urbanization (log)	-0.006	-0.080***	0.013	0.033*
	(0.015)	(0.025)	(0.015)	(0.016)
Transfers (log)	0.363**	0.349***	0.188	0.293***
	(0.163)	(0.081)	(0.305)	(0.078)
2000	4.113*	5.404***		
	(2.257)	(1.023)		
2010	4.936*	6.271***	1.107***	0.902***
	(2.427)	(1.072)	(0.376)	(0.132)
Constant	-17.067***	-11.379***	-6.170	-2.545
	(3.167)	(3.704)	(5.916)	(4.685)
N	8138	9706	8599	9154
R^2	0.878		0.358	

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).
Year and Municipal fixed-effects included in all models. Standard errors clustered by state.
The results of Model 1 and Model 2—including all the data—are consistent with the cross-sectional models, indicating that more unequal municipalities have a lower capacity to collect tax. The estimates for *Gini* in Model 3 and Model 4—2000 and 2010 data only—are not significant.
We dropped municipalities that were founded after 1985, resulting in a smaller number of observations in Model 1, i.e., those municipalities that did not exist for part of the time period used in the panel. The results do not change significantly if we do not drop these municipalities.
Standard errors in parentheses. Two-tailed test.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D.2: Bivariate Fixed Effects Panel Models: Benchmark to Compare the Results

	IPTU (1991-2000-2010)	IPTU (1991-2000-2010)	IPTU (2000-2010)	IPTU (2000-2010)
Gini	-16.745**	-3.479***	-13.298***	0.805
	(6.696)	(1.220)	(1.083)	(0.622)
Population (log)		1.996***		0.642
		(0.459)		(0.494)
GDP (log)		-0.033		0.654**
		(0.171)		(0.238)
Rural Share		-0.468		-2.809**
		(0.959)		(1.012)
Transfers (log)		0.363**		0.188
		(0.163)		(0.305)
Housing and Urbanization (log)		-0.006		0.013
		(0.015)		(0.015)
2000		4.113*		
		(2.257)		
2010		4.936*		1.107***
		(2.427)		(0.376)
Constant	16.149***	-17.067***	16.921***	-6.170
	(3.518)	(3.167)	(0.565)	(5.916)
N	9378	8138	8655	8599
R^2	0.028	0.878	0.128	0.358

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).
Year and Municipal fixed-effects included in all models. Standard errors clustered by state.
The coefficient on inequality is larger in the bivariate models and remains significant. For the bivariate model, even the two-period panel model (2000-2010) results are in line with our argument.
Standard errors in parentheses. Two-tailed test.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D.3: Original Panel Models (1991-2000-2010) with Municipality-specific Linear and Quadratic Time Trends

	DV: IPTU Revenue	
	Model 1	Model 2
	(Time Trend)	(Time Trend^2)
Gini	-3.317**	-3.479***
	(1.296)	(1.220)
Population (log)	1.829***	1.996***
	(0.427)	(0.459)
GDP (log)	-0.100	-0.033
	(0.202)	(0.171)
Rural Share	-0.426	-0.468
	(1.015)	(0.959)
Transfers (log)	0.685***	0.363**
	(0.062)	(0.163)
Housing and Urbanization (log)	0.018	-0.006
	(0.021)	(0.015)
Time Trend	0.425**	9.050
	(0.172)	(5.395)
Time Trend^2		-1.646
		(1.047)
Constant	-16.929***	-24.472***
	(3.177)	(6.540)
N	8138	8138
R^2	0.876	0.878

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged). All models with municipality fixed-effects and standard errors clustered by municipality.

This table shows the results for a panel model with both linear and quadratic time trends instead of year fixed effects. The results do not change substantially. Our independent variable of interest is still substantially large and significant.

Standard errors in parentheses. Two-tailed test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

E Appendix: Sub-sample Analysis

Between 1982 and 2007, the number of municipalities in Brazil increased by 41 percent. It could be problematic for our analysis if the split of municipal units was somehow associated with the level of inequality. In general, the increase in the number of municipalities has been attributed to a number of different factors.

Since income is often concentrated geographically, it is possible that a redrawing of municipalities could split the older municipality into two very different new municipalities. For example, one high inequality municipality could be split into two low inequality units. Or it could be split into one high inequality and one low inequality municipality. To rule out the possibility that our results are affected by these splits, we first show the densities of our main independent variable of interest, inequality, for subsamples of municipalities split up by on municipality age (until 2010).

Figure E.1 depicts the distribution of years since each municipality in our dataset was created. The trimodal distribution reveals the thee most often values in our data: 1. municipalities over 70 years old; 2. municipalities between 40 and 60 years old, and; 3. municipalities between 10 and 20 years old.

Figure E.2, in turn, shows the distribution of the GINI coefficient by municipality ages in decades of age. The non-relationship between age and inequality (captured by the relatively similar distributions in each graph) indicates that a possible split of municipalities due to high inequality are most likely not driving our results.

In addition, we run our original model on a sub-sample of those municipalities that were created prior to 1970. The results are presented in column 2 in Table E.1 and are consistent

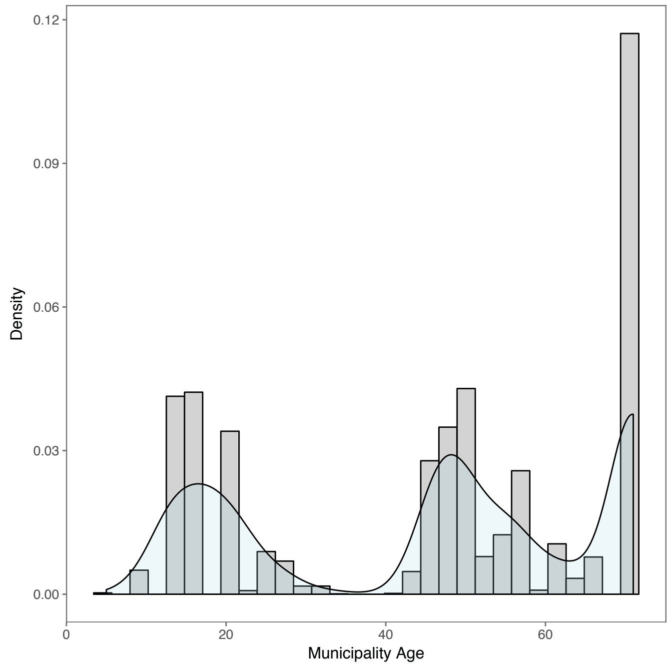

Figure E.1: Municipality Age in 2010: This plot shows the distribution of years since each municipality in the data set was created.

with those in the original sample (column 1). The results for the sub-sample analysis are consistent with the results using our original sample: a consistent negative effect of inequality on municipal IPTU revenue.

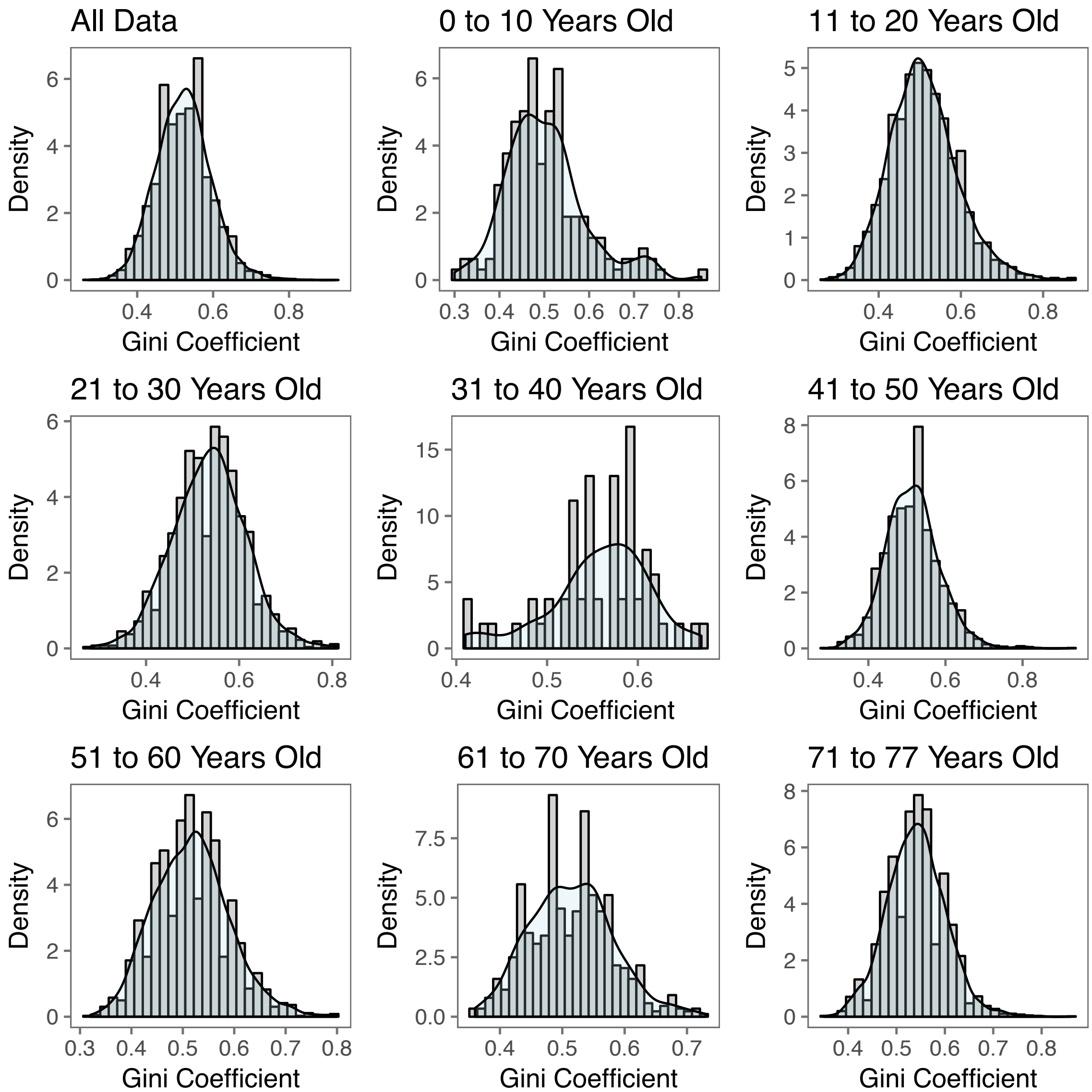

Figure E.2: GINI Coefficient by Municipality Age in 2010: This plot shows the distribution of the GINI coefficient by municipality ages (in decades of age). The non-relationship between age and inequality (captured by the relatively similar distributions in each graph) indicates that the split of municipalities due to high inequality is most likely not driving our results.

Table E.1: Sub-Sample Analysis Based on Municipality Age (until 2010)

	DV: IPTU Revenue (log)	
	Model 1 (Original 2010 Cross-Sectional Model)	Model 2 (Sub-Sample Model)
Gini	-6.190***	-4.880***
	(1.396)	(1.336)
Population (log)	-0.080	0.023
	(0.185)	(0.154)
GDP (log)	1.503***	1.570***
	(0.190)	(0.208)
Left Party	-0.033	-0.006
	(0.059)	(0.061)
Rural Share	-2.734***	-2.874***
	(0.401)	(0.390)
Housing and Urbanization (log)	0.052*	0.049
	(0.027)	(0.031)
Transfers (log)	-0.228	-0.437**
	(0.195)	(0.186)
Oil Revenue (log)	-0.020	-0.028
	(0.018)	(0.018)
Constant	2.391	3.690**
	(1.864)	(1.650)
N	4269	3337
R^2	0.641	0.677

Notes: Dependent variable: IPTU Revenue in Brazilian reais (logged).

Models with standard errors clustered by state.

The results for the sub-sample analysis are consistent with the results using our original sample: a consistent negative effect of inequality on municipal IPTU revenue.

Standard errors in parentheses. Two-tailed test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

F Appendix: Zero Values in the Dependent Variable

Our measure of IPTU revenue collection, made available by the Institute of Applied Economic Research (IPEA), is in the current Brazilian currency called *real* (in plural, *reais*). The *real* was introduced on July 1, 1994. The data for 1991, therefore, was converted by IPEA from the former currency *cruzeiro* to *reais*, and also deflated to controlling for the high inflation in Brazil in 1991. In personal correspondence with the IPEA staff responsible for the data collection, they acknowledged that zero values in the revenue data should mean that no revenue was collected, but could not rule out the possibility that some of these zero values should actually be missing values. This is in addition to the missing values that do exist in the data.

We therefore decided to conduct an additional robustness check to our results. In an attempt to identify observations with actual zero revenue, we used the data on IPTU revenue in nominal values (in their original currencies), as originally released by the Brazilian Ministry of Finance through the National Treasury Secretariat. For the robustness check we created a corrected version of the IPTU revenue variable. We set values in the original IPTU data to NA (missing value) if the corresponding observation in the nominal IPTU data is NA.

This correction reduces the number of zero values in the data significantly—specifically, by about two-thirds for 1991. The original data have $1,527$ zeros in 1991, 458 in 2000, and 71 in 2010. The replacement of zeros with NA when the nominal data is missing, results in 443 zeros for 1991.

Given that only the data for 1991 is affected, we present the panel model with these changes in Table F.1. We are calling this changed dependent variable *IPTU_corrected (log)*. The first model is the fixed effects panel model (1991-2000-2010) using the corrected dependent variable with non-imputed data, i.e., dropping the missing observations. The second column shows the results when the corrected dependent variable is also imputed, i.e., the original zero values that were set as missing observations are replaced with imputed values.

While the magnitude of the coefficient for *Gini* in the panel model is smaller than from our original panel model, the results from the analysis using our new dependent variable *IPTU_corrected (log)* are consistent with the results we found originally: a consistent negative effect of inequality on municipal IPTU revenue in both models (either using non-imputed or imputed data). In addition, the selection on unobservables test results in a δ value of 3.34 for the non-imputed panel model.

Table F.1: Replacing the observations in our original dependent variable (IPTU revenue) to NA (missing value) when the observation is NA in the original data using nominal values

	Dependent variable: IPTU_corrected (log)	
	Model 1	Model 2
	FE Panel Model	FE Panel Model
	(1991-2000-2010)	(1991-2000-2010)
	Non-Imputed Data	*Imputed Data*
Gini	-2.851***	-3.614***
	(0.994)	(0.945)
Population (log)	2.283***	2.096***
	(0.481)	(0.398)
GDP (log)	0.081	0.120
	(0.127)	(0.120)
Rural Share	-0.927	-1.033
	(0.765)	(0.673)
Housing and Urbanization (log)	0.007	-0.068***
	(0.014)	(0.022)
Transfers (log)	-0.581	-0.841***
	(0.484)	(0.279)
2000	13.796**	17.562***
	(5.472)	(3.037)
2010	15.881**	20.080***
	(6.057)	(3.344)
Constant	-16.369***	-13.076***
	(2.868)	(3.073)
N	8102	9221
R^2	0.879	.

Notes: Dependent variable: *IPTU_corrected (log)*.

Both models with standard errors clustered by state and fixed-effects. The results for the analysis using *IPTU_corrected (log)* as the dependent variable are consistent with the results using our original dependent variable: a negative effect of inequality on municipal IPTU revenue in both models.

Standard errors in parentheses. Two-tailed test. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Public Spending in Autocracies: Evidence from Prussian Cities*

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First Draft: December 2016
This Draft: September 13, 2018

Abstract

When do authoritarian governments invest in public goods and services? The prevailing view is that non-democratic governance is generally associated with low levels of government spending and taxation. Yet autocracies exhibit significant variation in levels of government spending; the causes of these discrepancies have thus far not been thoroughly examined. I argue that where authoritarian elites own capital that is conducive to government spending, authoritarian regimes make larger state investments. I test this argument using newly collected data on government spending, investment, and individual wealth for 110 cities in 19th century Prussia. I combine these data with protest event locations and show that local government decisions on public spending were largely driven by the economic needs of the local autocratic elite.

Key Words: Public Spending; Revenue; Autocracies; Industrial Demand, Fiscal Policy

*I thank Joshua Alley for excellent research assistance. The paper has been presented at the Meeting of the European Political Science Association, Milan, Italy, June 2017. I thank Franziska Keller and participants at the Texas A&M Conference on Taxation, Revenue, and Fiscal Capacity for helpful comments on this project. All remaining errors are my own.

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1 Introduction

Education is one of the most important long term determinants of a country's development path and growth trajectory (e.g., Sala-i Martin, Doppelhofer and Miller, 2004). Yet, we still know relatively little about the origins of public education across the world and especially in non-democratic regimes[1]. What determines when non-democratic leaders invest in public education? Much of the literature in this area is concerned with spending patterns in democracies (Boix, 1998; Steinmo, 1993; Swank and Steinmo, 2002; Persson and Tabellini, 2003; Iversen and Soskice, 2006; Hall and Soskice, 2001) or differences between regime types (Ansell, 2008; Baum and Lake, 2003; Boix, 2003; Acemoglu et al., 2013; Stasavage, 2005). We know much less about the origins of spending on education in non-democracies. Even though first investments generally occurred prior to the development of fully democratic regimes. What leads authoritarian leaders to invest in public goods and what explains the differences in public investments within autocracies. In contrast to the empirical variation we observe, much work on non-democracies assumes that authoritarian elites are generally opposed to public goods spending (e.g. Boix, 2003; Bueno de Mesquita et al., 2005).[2] In an attempt to enhance our understanding of fiscal policy within autocracies, I empirically investigate a theory of when self-interested autocratic elites prefer higher levels of public spending.

In this paper I make use of a theoretical model developed by Galor and Moav (2006) and argue that differences in factor endownments by political elites can lead to different preferences over levels of government spending. Economic elites who own capital that directly benefits from higher government spending on public services demand investment in these public goods. I contend that,

[1] But see recent work by Paglayan (2017, 2018).

[2] Gift and Wibbels (2014) make a similar point with regards to research on investments in education.

depending on the type of capital they own, higher government investment can increase elites' return on capital ownership. For example, the return on public education spending is high for capital owners when skilled workers are needed. Therefore, while government spending may also benefit the poorer and disenfranchised population, the political and economic elites in this context have an incentive to increase public goods spending, even if it is costly in the short term. On the other hand, owners of capital that is limited in its complementarity to government spending are likely to oppose such investments.

To investigate the theoretical argument, I have collected new data on economic and political characteristics in Prussian cities in the latter half of the 19th century. This period, in Prussia and Germany more generally, is marked by profound economic change (Pierenkemper and Tilly, 2004), state development, and a growing fiscal development: the introduction of the general income tax (Mares and Queralt, 2015; Hollenbach, 2018; Mares and Queralt, 2018). Most importantly, these data allow me to calculate measures of income inequality and investment in public education that are not available for other subnational, or even national, administrative units in this time period. The data are based on a census of all Prussian cities with more than 25,000 inhabitants at the time (Silbergleit, 1908) and allow me to directly test several arguments about the relationship between economic characteristics and political outcomes. In addition to data availability, using data at the municipal level has several advantages from a research design perspective. First, the design allows me to control for several confounding factors, such as external war and the political system. Moreover, the Prussian case enables me to undertake a very direct test of the proposed argument. The design of the Prussian electoral system, explained in more detail below, guaranteed an extreme overrepresentation of the economic elites, which effectively linked economic and political power. Whereas many theoretical arguments in political science and economics assume the congruence between political and economic elites, in reality and in empirical tests this link is often rather tenuous. In contrast, in the Prussian setting the political and economic elites strongly overlap.

As I show in the empirical analysis, contrary to common models in political economy, inequality at the local level has very little effect on city-level spending on public goods. If at all, I recover a slight positive relationship. More importantly, in line with the theoretical argument, I show that areas with high levels of industrial employment, which I argue are cities where elites demand skilled labor, had much greater investment in public education. In the first part of the empirical section I show these findings to be true for standard regression analysis, even when controlling for a number of possible confounders. For example, the results are robust to including the occurrence of political protests by the working class, as well as modeling spatial dependence. In the second part of the empirical analysis I focus on estimating the a more precise causal effect of industrial employment on education investments. I introduce a new instrumental variable for industrial employment that is based on the underlying rock strata that lead to the development of coal beds. The instrumental variable estimation shows that the effect of industrial employment on public investments in education is substantially important and precisely estimated. Lastly, I undertake a bounding exercise with respect to omitted variable bias, which lends additional credence to these findings (Oster, 2017).

Whereas the theoretical argument in this paper is largely based on Galor and Moav's (2006) theoretical model, the paper contributes to the literature in several ways. First, to my knowledge, this is the first direct empirical test of the general theoretical argument. Second, I introduce new data at the city level in Prussia in the 19th century and undertake an instrumental variable analysis that ought to increase confidence in these results.

2 Public Spending in Autocracies

Throughout history, authoritarians have ruled the vast majority of societies. Only since 1991 has democracy been the most prevalent political system in the world; even in 2007, 46 percent of the worlds population lived in non-democratic regimes.[3] Moreover, a non-democratic government is, in essence, the original regime type, since *all* modern states were once under authoritarian rule. Nevertheless, our understanding of politics and what explains differences in public policy is much more detailed for democratic political systems.

In contrast to a vast literature on the differences between democracies and autocracies, much less research has attempted to explain what determines the differences in fiscal and other public policies *within* autocracies. The conventional wisdom is that there is little variation in public finance within authoritarian regimes, and that spending levels are generally low. The prevailing view is that exclusive institutions or non-democratic governance is generally associated with low levels of government spending and taxation (e.g. Acemoglu and Robinson, 2001; Boix, 2003; Bueno de Mesquita et al., 2005; Acemoglu and Robinson, 2006). Yet, the data shows that among autocracies there is considerable empirical variation in levels of public spending and the provision of public services.

Figure 1 shows the empirical densities for total tax revenue as a percentage of GDP by regime type, averaged from 2000 to 2007 to increase data coverage.[4] The plot shows that on average,

[3] See Mulligan, Gil and Sala-i Martin (2004) and personal calculation based on population data by the World Bank (World Bank Group, 2013) and regime type coded by Boix, Miller and Rosato (2013).

[4] I use tax revenue as a percentage of GDP to proxy for public expenditure here, since data coverage across both regime types is much better than for any government spending variables. The data on tax revenues is taken from Wilson, Cobham and Goodall (2014). The sample only includes countries with a constant regime type over the period 2000-2007. Total tax revenue refers to all revenue from taxation plus social security, excluding any revenues from taxes on natural resources. Countries are classified as democratic and non-democratic based on (Boix, Miller and Rosato, 2013).

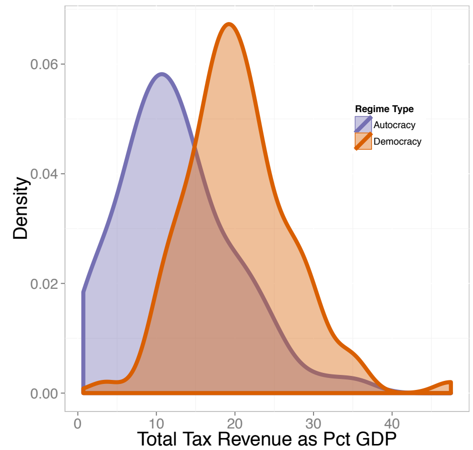


Figure 1: The plot shows the density for total tax revenue as a percentage of GDP by regime type. Democracies do have, on average, higher levels of taxation, but the variation is larger within autocratic regimes. Therefore, while many expect authoritarian elites to oppose taxation, we can see that they vary significantly in the amount that they utilize taxation.

democracies (orange) have higher levels of taxation than authoritarian regimes (purple). Yet there is greater variation among autocracies (52.4) than among democracies (45.1).

As Figure 1 indicates, autocratic regimes exhibit large differences in their fiscal policies, and often exhibit levels of taxation equal to those of democracies. While our understanding of the differences among autocracies is limited, one common explanation is based on the level of institutionalization among them (Escribà-Folch, 2009; Gehlbach and Keefer, 2011; Jensen, Malesky and Weymouth, 2013; Boix and Svolik, 2013). Another strand of the literature contends that as the size of the politically pivotal share of the population (or selectorate (Bueno de Mesquita et al., 2005)) increases, governments spend more on public versus private goods and vice versa (Bueno de Mesquita et al., 2005).

While other scholars have investigated the provision of public goods such as education in authoritarian regimes, much of the focus has been on how the political power of the poor or inequality affects their provision. For example, Go and Lindert (2010) find that the American North strongly outperformed the South in school enrollment rates in the 19th century, most likely due to greater local autonomy and voting power for the poor. Yet Galor, Moav and Vollrath (2009) and Kourtellos, Stylianou and Ming Tan (2013) show that higher land inequality is associated with a delay in the expansion of primary schooling, both in the US context in the 20th century and cross-nationally. By contrast, Cinnirella and Hornung (2016) use data on Prussian counties in the 19th century to show an initial negative relationship between land inequality and primary school enrollment that becomes weaker as labor coercion decreases. Contrary to prevalent theories, however, Cinnirella and Hornung (2016) find that land concentration does not seem to affect the supply of education, but instead peasants demand for primary education.

I propose a theory about when political elites have incentives to invest in public education and provide public spending that benefits the politically less powerful masses. Independent of the institutional structure and size of the ruling coalition, the economic activity of elites matters, and can induce different levels of government spending.

This idea builds heavily on Galor and Moav (2006) and is somewhat similar to the argument made by Lizzeri and Persico (2004). Lizzeri and Persico (2004) contend that the franchise was not extended in England due to political pressure from the disenfranchised, but because increasing the number of poorer voters would create a larger political majority for the preferred policies of elites who benefited from more public goods spending over private rents. Thus, in Lizzeri and Persico's (2004) view, the expansion of the franchise in England was not driven by the masses' redistributive pressures ("or threat of revolution") but instead by intra-elite conflict over public vs. private goods spending. Urban elites demanded more investment in (health) infrastructure and saw that a larger pool of voters would allow them to pursue these policies against the opposition of the landed elites.

In a similar vein, Galor and Moav (2006) argue that the demise of class conflicts in the 19th and 20th centuries in England was not due to the higher redistribution associated with democratization, but instead because industrialists in the second phase of industrialization demanded increased investment in public goods. “The capitalists found it beneficial to support publicly financed education, enhancing the participation of the working class in the process of human and physical capital accumulation, leading to a widening of the middle class and to the eventual demise of the capitalist-workers class structure” (Galor and Moav, 2006, 1). Brown (1989, 1988) shows that cities in more democratic countries (UK, USA) lagged in their investments in sanitation compared to cities with smaller ruling coalitions in Prussia, and argues that these investments were again spurred by the demands of the wealthiest for public goods spending. Brown (1989) contends that as workers became more valuable, investment in public health became more profitable for elites as it significantly reduced sick days and increased life expectancy.

Others have argued that the externalities to education can become large enough for autocrats to invest in education even if this may be associated with an expansion of the franchise and the loss of political power (Bourguignon and Verdier, 2000).

As in Galor and Moav (2006), I contend that when the capital–skill complementarity is high, economic elites can directly benefit from government investment in skill formation. When elites own capital that relies on physical and human capital, higher government spending in health and education directly benefit them by increasing the return on their capital. In these cases, the capital owners prefer higher taxes on all citizens – even themselves – to increase investment in public goods.

Cross-country econometric analyses have shown that government investments, specifically in education and infrastructure, can have positive effects on economic growth (e.g Cashin, 1995; Devarajan, Swaroop and Zou, 1996; Easterly and Rebelo, 1993). Building on endogenous growth models (Romer, 1989; Barro, 1990), I argue that at the individual level, government investment in specific public goods can directly affect the production function and increase returns on capital. The effect of public spending on individual-level returns, however, depends on the complementarity between the physical and human capital.

If the supply of skilled labor is low but the demand is rising, increased public investment in public education can become highly profitable for elites for several reasons. First, it raises the productivity of the workforce. Second, it increases the supply of skilled labor for capital owners. Similarly, public spending on health care or sanitation can raise the life expectancy of workers and reduces the number of sick days, promoting their reliability and longevity.

The supply of public education is especially profitable if the beneficiaries are poor and lack access to credit. In this case, without public investment education will be under-supplied given that private investments are limited (Benabou, 2002).

By contrast, owners of capital with low skill complementarity have much less interest in publicly financing education and/or other public goods. When labor supply is high and capital owners demand low-skilled labor, such as in agriculture, there are fewer benefits of public investment: in such situations labor is easily replaceable and education is thus unnecessary. Similarly, according to Lizzeri and Persico (2004), elites in rural and less dense areas were less concerned about the public provision of sanitation since they were less affected by the illnesses of the poor.

Similar to the work by Engerman and Sokoloff (2002), I believe that factor endowments are an important part of the story, as they strongly influence elite economic activity. Given an abundance of land and a high supply of unskilled labor, economic elites (or owners of large estates) have little reason to push for higher taxation and government spending. However, owners of industrial capital who lack adequate labor supply and require a more educated workforce can benefit directly from the state providing these public goods. Industrial elites therefore benefit from government spending on health and education, as it increases the return on their private investments. Ergo, these capital owners have incentives to demand higher levels of public spending on education and other productive public goods. Galor, Moav and Vollrath (2009) point out that a conflict exists between large landowners who prefer abundant and cheap unskilled labor and elites who benefit from increasing the productivity of the workforce. Therefore I expect that autocratic elites who own industrial capital are more likely to demand higher public spending on education.

3 Research Design & Data

In this paper, I use a unique, extraordinarily rich data set with observations from Prussian cities in the 19th and early 20th centuries to investigate the argument made above at the local level. There are several advantages to using city-level data for the empirical testing. First, the data set comes from a census of "large Prussian cities" with over 25,000 inhabitants (Silbergleit, 1908). This guarantees some level of comparability in terms of density, size, and political organization. Second, the data allow me to control for several confounding factors, such as the political system or the threat of war. The political system is very similar across the sample of cities, making it unlikely that it would cause changes in spending levels. Similarly, the demand for other spending items, such as defense, is quite low at the local level and should not differ significantly across cities, thus allowing for a cleaner investigation of how local demands for "domestic spending" differ. Lastly, using only city-level data avoids introducing rural-urban differences.

Figure 2 shows the unit of analysis, 110 Prussia cities, as they are distributed across the country. County (Kreis) borders are marked in black, and cities are depicted as gray dots; darker shading and larger point size represent larger populations in 1907. The largest and darkest point shows

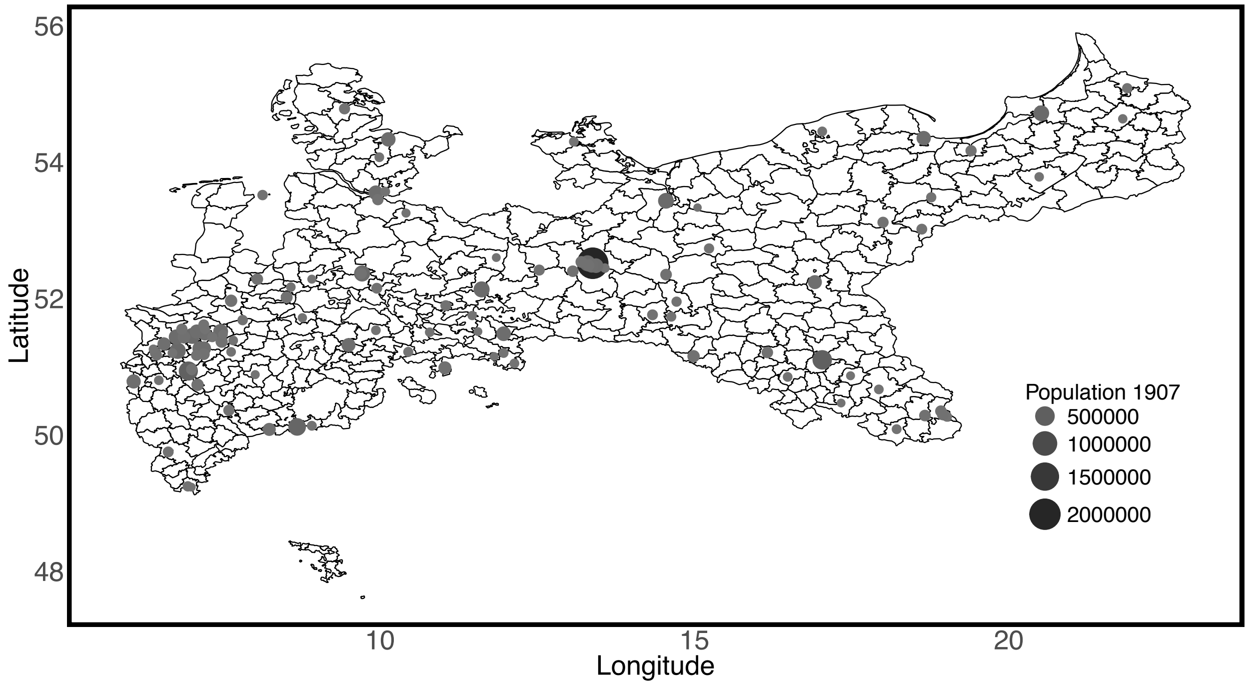


Figure 2: The plot shows the location of all cities (observations) in the data set and their respective populations in 1907.

Berlin. While the majority of observations are clearly concentrated in the western, more industrial part of the country, a number of observations are located in the more agrarian, eastern parts of the country.

The data is gathered from the 1907 city census of all Prussian cities with more than $25,000$ inhabitants (Silbergleit, 1908). This data was transcribed and then merged with county-level data for variables that are not available at the city level.[5]

During the study period, the political system across cities in Prussia was quite similar. The electoral rules – the *Dreiklassenwahlrecht* – across the country ensured the continued significant influence of wealthy voters, directly linking economic and political power. Male citizens were separated into three classes of voters, each of which had the same voting power. However, the

[5]The county-level data is originally based on Prussian censuses and statistical yearbooks, but here is based on Becker et al. (2014).

size of each group was markedly different. The first group contained the richest tax payers, who paid for one-third of the local tax revenue. The second group contained the next-richest tax payers, again paying for one-third of the local tax revenue. The last group contained all other male citizens. Thus, the richest citizens paying for one-third of the tax revenue had as much voting power as the larger group of poorest voters who paid for one-third of the revenue. Moreover, elections were far from free and secret; poorer voters were often subject to pressure from their employers and could rarely choose freely (Thier, 1999; Hallerberg, 2002).

Local administrative units were generally responsible for funding local public goods. For example, cities, rural communities (*Gutsbezirke*), or even local manorial lords were the administrative units that were responsible for funding local schools. The schools were financed via school fees, local taxes, or directly by local estates. Only after 1888 was some level of state assistance allowed, yet urban areas were generally disadvantaged in assistance from the Prussian state (Hühner, 1998, 32f.).

This local-level data provides a unique opportunity to investigate the circumstances under which economic elites were in favor of providing public services to the general public. In this paper, I use investment in education as the primary outcome of interest. I create three dependent variables that measure both the supply and demand for local public education. First, I calculate the cost of schooling per capita for each city (or school expenditure). The second measure is the number of students in *Volkschule* divided by the number of 5 to 15 year olds in a given city, to approximate the share of eligible children that attended school. Third, I calculate the average class size in a given city, i.e. the ratio of students to teachers.

I use the share of industrial employment as the main independent variable of interest; it proxies for the complementarity between elite-owned capital and skilled labor. Since owners of industrial capital in this context are elites who profit from increased government spending, they ought to have a strong interest in increasing public spending on education and other public goods. Since data on industrial employment is not available at the city level, I use county-level data on industrial employment and divide it by the total number of workers in a given county. To do so, I geocode every city in the data set and match it to the county in which it is located. This is not a perfect measure, but I believe it is reasonable to assume that the share of industrial workers at the county level is highly correlated with that in cities, and that most industrial workers lived in cities. Nevertheless, this is not an optimal solution, given that some counties contain multiple cities in the data set. To deal with this issue, I undertake several robustness checks below.

In addition to the main independent variable, the empirical models include several control variables. First, I control for city size or population level, i.e., logged population. I also create two measures of inequality, one for income inequality at the city level and one for wealth inequality. Both are measured as a Gini coefficient based on the number of city inhabitants in different income or wealth groups.[6] Not surprisingly, however, the correlation between the two measures of inequality is very high at 0.68. Therefore I generally only include the measure of income inequality in the models presented here; the results do not change significantly if wealth inequality is included.

Unfortunately, data on total city income, such as GDP, is not available. In an attempt to control for city income levels I add a variable of the average taxes paid by city residents. While not perfect, this variable should capture income levels. I also use data on income groups and create an average city income based on assuming that each resident earns the average of the income group they are

[6]To calculate the Gini coefficient, some assumptions have to be made. First, I assume that for a given category of income or wealth, all persons in that group have the average income or wealth of that bin. For example, for incomes between 900 and 3000 marks, I assume all people in this bin earn 1,950 marks. For the last bin, e.g. incomes above 100,000 marks, I assume all persons in this bin earn the lower limit, i.e. 100,000 marks. This is more likely to underestimate inequality, assuming that most persons in the last category earn more than the lower limit. For income the data provides seven categories listed in the city census, while the wealth Gini coefficient is based on nine categories.

in.

Lastly, a competing argument might be that it is easier for protesters to organize in very urban areas with industrial production, especially if factories further enhance the ability for collective action. I therefore add data on protest events in 19th century Germany. This data was originally compiled by Tilly (1980, 1990) based on newspaper articles and was coded at the city level. While incomplete, it is as comprehensive as possible for the time period covered and should have captured all major protest events (Tilly, 1980). I geocode the data based on city location and then create a dummy variable of any protest events occurring within a 15km radius of each city.

Unfortunately, not all measures are available for the exact same point in time. The measure of industrial employment, the main independent variable, is only available for 1882. I therefore use all other variables measured at the time point closest to 1882, which generally means 1893. The enrollment rate is unfortunately only available for 1905/06 and is therefore measured for that year. Both other dependent variables are also available for 1905/06, which allows me to estimate a longer-run effect and to include a lag-dependent variable. The main results do not change significantly depending on which year the measures of schooling use. To measure the effect of protest events, I use an indicator variable of whether any protest events were recorded between 1865 and 1885, i.e., 20 years prior to the measurement of the dependent variables.

4 Empirical Analysis

As a first step I estimate three different ordinary least squares (OLS) models with the three dependent variables: per capita school cost (logged), school enrollment (i.e., share of 6-15 year olds in school), and average class size (i.e., students per teacher). As explained above, the main variable of interest is industrial employment (measured at the county level). I add controls for protest events in a 15km radius, average income in 1893, income inequality in 1893, logged population size in 1893, and taxes per capita in 1893. Table 1 shows the results from the three basic regression models.

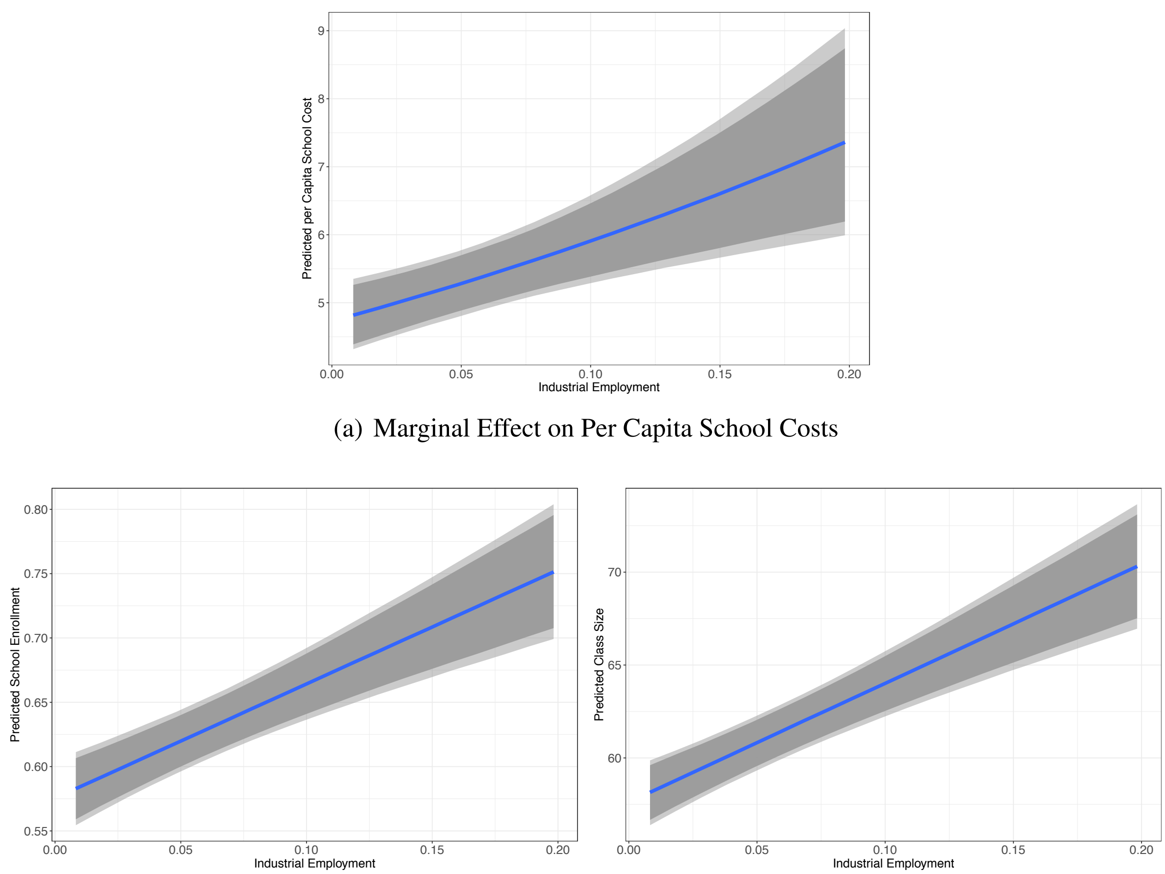
Based on the theoretical argument, we would expect industrial employment to have a positive association with school expenditure (model 1) and school enrollment (model 2), as well as a negative association with class size (model 3). As Table 1 shows, as expected, the estimated effect of industrial employment on school costs is positive and quite large. Based on the log-transformed dependent variable, the interpretation is quite simple. With a one-percentage-point increase in industrial employment (i.e., an increase of 0.01 on the scale of the variable), we expect an increase in per capita school costs of 2.24%. Importantly, the 95% confidence interval does not include zero.

Similarly, for school enrollment, the coefficient for industrial employment is quite precisely estimated (i.e., the 95% confidence interval does not cover zero) and has a substantially large effect. Lastly, somewhat surprisingly, the effect of industrial employment on school class size (or student-to-teacher ratio) is also positive and quite large. This might indicate that whereas cities with high levels of industrial employment have invested more in education, i.e., increased spending and enrollment, the higher spending is not large enough to overcome the increased demand for education. Thus while more young city dwellers are receiving education, class sizes are also getting bigger.

Figure 3 shows the marginal effects of industrial employment for each of the models displayed in Table 1 above. To calculate the marginal effect, all other covariates are held constant at their sample medians. The left-most plot shows the marginal effect of industrial employment on per capita school costs. It shows that when industry was more important in the local economy, cities invested significantly more money in local schools. The middle plot illustrates the effect of increases in

Table 1: Simple Linear Models - Public Education

	Dependent variable:		
	per Capita School Cost (1895)	School Enrollment (1905)	Class Size (1896)
	(1)	(2)	(3)
Income Gini	6.526^{***}	1.013	15.258
	(2.463)	(0.635)	(39.921)
Avg Income	-0.004^{***}	-0.001^{**}	-0.026
	(0.001)	(0.0004)	(0.023)
Log Pop	0.064	0.002	-0.791
	(0.047)	(0.012)	(0.757)
Taxes pC	0.045^{*}	0.002	-0.023
	(0.024)	(0.006)	(0.382)
Industrial Empl	2.239^{***}	0.885^{***}	63.795^{***}
	(0.646)	(0.167)	(10.476)
Protests	0.142^{**}	0.059^{***}	2.524^{**}
	(0.068)	(0.018)	(1.103)
Constant	1.502^{**}	0.776^{***}	81.061^{***}
	(0.601)	(0.155)	(9.746)
Observations	105	105	105
R^2	0.317	0.409	0.470
Adjusted R^2	0.275	0.373	0.437
Note:	*p<0.1; **p<0.05; ***p<0.01		



(a) Marginal Effect on Per Capita School Costs

(b) Marginal Effect on Enrollment

(c) Marginal Effect on Class Size

Figure 3: The plot shows the marginal effect of industrial employment for each of the models displayed in Table 1 above, holding all other covariates at their sample medians. Note the different scales of the y-axes for each plot.

industrial employment on school enrollment. Again, more industrial workers are associated with a significantly higher share of 5-15 year olds enrolled in school. Lastly, the right plot shows the effect of industrial employment on class size. The results indicate that the increased spending was insufficient to overcome the higher demand, thus leading to larger classes in areas with more industry.

Looking at the results for the other variables presented in Table 1, income inequality is estimated to have a positive effect on per capita school costs. One explanation for this finding could be that areas with higher inequality more closely represent the will of industrial elites, since higher inequality also meant more political power for the rich. This effect, however, is generally surprising and not what one would expect given the extant political economy literature. Average income has a small but negative effect on school costs. Logged population and per capita taxes are both positively associated with school costs. As one might expect, protest events have a substantial and positive effect on school spending, suggesting that local collective action may have also driven higher investment in education. While the results for school enrollment are essentially the same, all estimated coefficients are smaller in magnitude. Lastly, the results are again similar in the model with class size as the dependent variable. In this model, however, logged population is negatively associated with class size (though with high uncertainty), which is somewhat surprising. All other coefficients are in the same direction as with enrollment or spending as the dependent variable.

4.1 Robustness Checks

The results above indicate increased supply and demand for public education in cities with high industrial employment. However, to address the possibility that additional channels could produce these results, I add three additional control variables. First, I calculate a measure of land concentration in the cities' surrounding counties, as land inequality is likely to be correlated with industrial development and has been shown to affect school enrollment (Cinnirella and Hornung, 2016). I also include a measure of distance to the closest river or coastline. The results are displayed in Appendix Table 4 but do not change the substantive interpretation with regard to industrial employment. It is noteworthy, however, that land concentration has a significant and substantially large negative effect on per capita school costs.

As discussed above, one problem with the data is that industrial employment is measured at the county level, whereas all other variables are measured at the city level. In addition, 28 cities in the data come from counties with more than one city (i.e., these observations would have the same value with respect to industrial employment). Therefore as a simple robustness check I include indicator variables for the cities in the OLS regression models estimated above. The results are displayed in Appendix Table 5. Second, I drop all observations of cities that are not unique to their county. The results for these models, presented in Appendix Table 6, do not substantially change based on either of these strategies.

Lastly, as an additional robustness check I estimated a unique bootstrap model. The regression models shown in Table 1 above are each estimated 500 times. For each of the 500 regressions I use a randomly drawn sample. Specifically, for each county that contains multiple cities in the data, only one city observation is sampled to be included in the data; cities from unique counties are always included. Each of the 500 estimated regressions thus includes only one case from each of the counties with multiple cities. To take the uncertainty for each of the 500 regressions into account I draw $1,000$ values from a normal distribution based on the coefficient and standard error in each regression. This results in a distribution of $500,000$ observations, which represents the coefficient of industrial employment and its uncertainty with respect to changes in the sample and regression

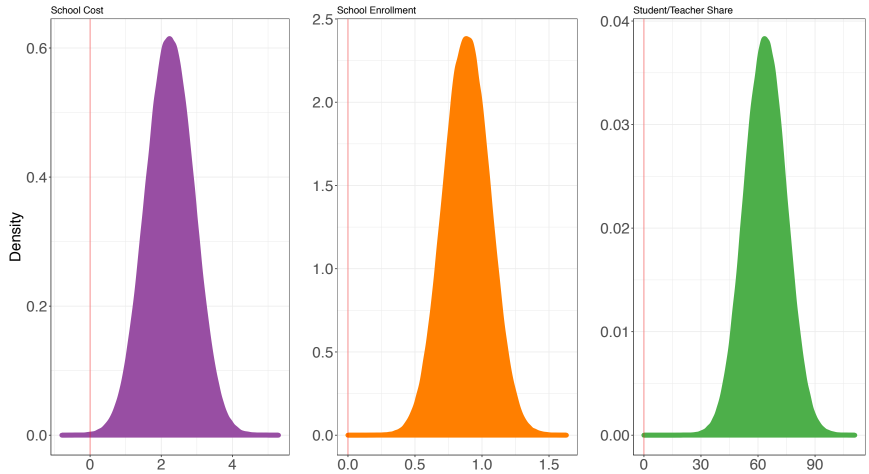


Figure 4: The plot shows the distribution of the coefficient estimates for industrial employment based on 500 sampled data sets. For counties with multiple cities, only one observation is drawn to be included in the data for each of the 500 models. Based on the coefficient estimate and the standard error, $1,000$ estimates are drawn for each regression result. Each density shows the distribution of $500,000$ coefficient estimates accounting for the uncertainty associated with each regression. The left-most plot shows the estimates when per capita school cost is the dependent variable. The middle plot shows the results when school enrollment is the dependent variable. Lastly, the right plot shows the results for class size or students per teacher. Overall, this confirms the results found above and lends additional confidence that these results are not due to the multiple cities in certain (more industrial) counties.

models. Figure 4 shows density plots for the coefficient of industrial employment for each dependent variable. The results further show that for essentially all 500 estimated models, even with the random sampling of multi-city observations, the estimated effect of industrial employment is positive and associated with little uncertainty across all models.

The results from the basic regressions are in line with the theoretical expectation that industrial capital leads authoritarian elites to invest more in public goods, specifically education. Cities in counties with higher levels of industrial employment spent more on education and had a higher rate of school enrollment. In contrast, these cities also had larger student-to-teacher ratios, indicating that the additional supply (higher spending) was insufficient to overcome the additional demand (higher enrollment). Nevertheless, several concerns with regards to the inference remain based on the standard OLS models estimated above.

4.2 Spatial Regression Model

One concern with this data is potential spatial spillovers. For example, industrial employment in one county/city may increase the demand for education spending in neighboring cities. Similarly, investment in education in one city may allow for free-riding and less investment in nearby cities. Using Moran's I test I am unable to reject the null hypothesis of no spatial correlation in the residuals for each of the OLS models. To account for the spatial dependence in the data I estimate spatial autoregressive models by including a spatial lag for the dependent variable. The model is estimated via maximum likelihood in R (Bivand, Pebesma and Gómez-Rubio, 2008).

Appendix Table 7 presents the results from the spatial autoregressive models. These show that there is quite a bit of evidence of spatial dependence in the data, as for all three dependent variables the estimates for ρ are quite large (0.24, 0.45, and 0.51, respectively) and we can reject the null of no spatial dependence for all three models.

The overall results of the spatial autoregressive model are quite similar to those of the standard OLS regression. There is strong evidence that industrial employment is positively associated with school expenditure, enrollment, and class size. In addition, the marginal effects now ought to take into account the indirect (or feedback) effect of independent variables. First, the direct effect of a one-unit increase in industrial employment on school expenditure is 2%; however, due to the spatial lag there is an additional indirect effect of 0.6, meaning the total estimated effect is 2.6%. Similarly, for enrollment the direct effect of industrial employment 0.61, but the indirect effect is 0.48, leading to a total estimated marginal effect of 1.12, i.e., for each one-unit increase in industrial employment, enrollment increases by 1.12 percentage points. Lastly, for class size the total effect of industrial employment is 39.65, whereas the direct effect is only 26.03. Overall, the total effects of industrial employment are quite similar to those estimated in the OLS regression and all are precisely estimated, strengthening the evidence for the theoretical argument.

In addition to the results for industrial employment, the model again suggests that protest events have a positive impact on educational investment. The coefficients for protest events are positive and significant for all three dependent variables, suggesting that local politics did react to protests and collective action. Again, in line with previous results, the spatial autoregressive model provides support for the theoretical argument made above. Cities with more industrial employment seemed to have invested more in education, but not enough to overcome the increased demand. Even though the data exhibit spatial dependence, controlling for its presence does not change these conclusions.

4.3 Instrumental Variable Models

Despite the strong results from the OLS models and the spatial autoregressive models, concerns remain with regards to establishing the hypothesized relationship, let alone causality. For example, the results may suffer from omitted variable bias, measurement error, or reverse causality. Thus, to further check the robustness of the results, I estimate an instrumental variables model treating industrial employment as potentially endogenous.

To instrument for industrial employment I use a geographic variable – the presence of rock strata that developed during the carboniferous era (more than 3 million years ago). Carboniferous (literally "coal bearing") rock strata were mapped by the Federal Institute for Geosciences and Natural Resources in Germany (Asch, 2005). As Fernihough and O'Rourke (2014) show, these carboniferous areas are highly correlated with later coal discoveries.

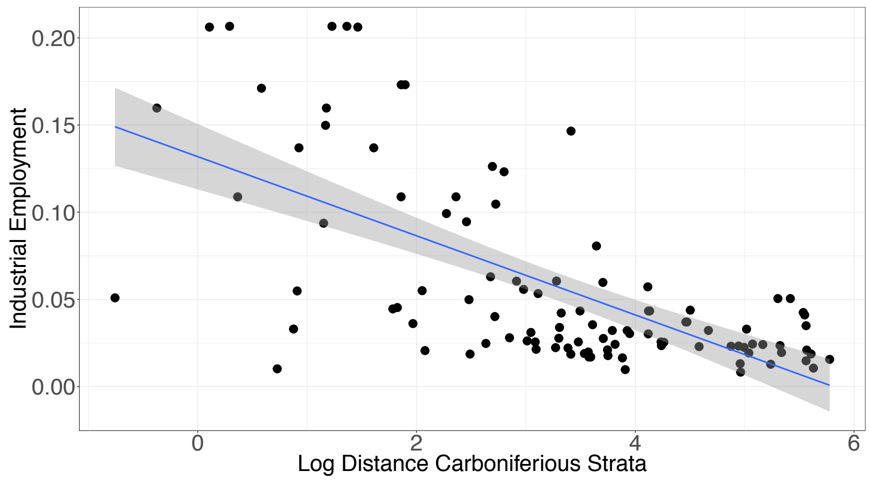


Figure 5: The plot shows the relationship between the potentially endogenous variable (industrial employment) and the instrument used (logged distance to closest carboniferous area).

These rock strata are therefore especially likely to contain coal, which was one of the most important natural resources during industrialization and a major driver of economic progress (Fernihough and O'Rourke, 2014). Indeed, the industrial take-off in Europe would have been impossible without the large coal deposits in England (Pomeranz, 2002; Wrigley, 2010; Gutberlet, 2013). The availability of raw materials is imperative to industrial development and manufacturing, especially at a time when transport costs were still very high. Close location to coal mines therefore ought to be important to industry location. I expect distance to carboniferous areas to be negatively correlated with industrial employment. Specifically, I use the natural log of distance to the closest carboniferous rock strata as an instrument for industrial employment for each city.

Figure 5 shows the bivariate relationship between the potentially endogenous variable of interest, industrial employment, and the instrument, logged distance to the closest carboniferous rock strata. As expected, there is a strong negative relationship between the two variables: the R^2 for the bivariate regression is 0.42.

A second necessary assumption for the IV estimation to be valid is the exclusion restriction, i.e., that, conditional on the included control variables, distance to carboniferous areas has no effect on the dependent variables. First, it seems quite likely that rock strata are exogenous to political processes, not least because they precede these processes by millions of years. Second, apart from economic development (average income is included as a control in the model), it is hard to imagine other factors through which rock strata or the likelihood of coal discovery could affect educational outcomes/investments.

One concern, however, even in the IV model is again spatial dependence. This can be especially problematic if both the dependent variable and instrument exhibit spatial correlation (Betz, Cook and Hollenbach, 2017), which is likely the case here. Instead of the standard two-stage least squares model, I therefore estimate a spatial IV model. The main difference is that this model also estimates a spatial lag of the dependent variable, which in turn is instrumented by spatial lags of the regressors (Drukker, Egger and Prucha, 2013; Betz, Cook and Hollenbach, 2017).

Table 2 shows the results for the spatial IV regressions. Two things stand out. First, across all parameters the results are again quite similar to the OLS regression results. Second, industrial employment is estimated to be positively associated with all three dependent variables. Compared to the OLS results, the estimated effect of industrial employment on per capita school spending is slightly smaller, indicating that it may have been biased upwards.

Based on the spatial IV model, a one-percentage-point increase in industrial employment is associated with a 1.98% increase in per capita school spending (compared to 2.24% in the OLS model). The estimated effect of industrial employment for both other dependent variables is almost identical.

An additional interesting result that stands out is that protests in and around the cities of interest still have a positive impact on all three dependent variables, which lends further confidence to the results from the OLS model. The estimated parameter on the spatial lag (λ) is quite large in all three spatial IV models, again providing evidence that spatial dependence is present in the data.

Table 2: Spatial Instrumental Variable Models

	Dependent variable:		
	per Capita School Cost (1895)	School Enrollment (1905)	Class Size (1896)
	(1)	(2)	(3)
Industrial Empl	1.975**	0.886***	64.60***
	(0.828)	(0.209)	(13.44)
Income Gini	5.680**	0.727	10.31
	(2.345)	(0.588)	(38.18)
Avg Income	-0.00373***	-0.000623*	-0.0243
	(0.00133)	(0.000334)	(0.0217)
Log Pop	0.0812*	0.00466	-0.746
	(0.0443)	(0.0111)	(0.720)
Taxes pC	0.0464**	0.00201	-0.0211
	(0.0225)	(0.00567)	(0.369)
Protests	0.123*	0.0507***	2.338**
	(0.0655)	(0.0164)	(1.069)
Constant	1.119*	0.651***	77.17***
	(0.577)	(0.145)	(9.445)
ρ	0.0330***	0.0275***	0.00949*
	(0.0110)	(0.00788)	(0.00552)
Observations	105	105	105
Note:	*p<0.1; **p<0.05; ***p<0.01		

4.4 Effect of Unobservables

As a last robustness check I follow Oster (2017) and calculate how strong unobservables would have to be to invalidate the results regarding the effect of industrial employment found above. In essence, this method provides an estimation of how influential unobserved factors would have to be to make the effect of industrial employment disappear because of omitted variable bias. Oster's (2017) method defines two important terms: δ and R_{max}. δ is defined as the "relative degree of selection on observed and unobserved variable," i.e. what is our belief about the importance of controls that are not included in the regression compared to those that are. In general, the results are seen to be robust to unobservables if $\delta \geq 1$. R_{max} is defined as the maximum R-squared that would be the result of the hypothetical regression that includes all relevant variables, both observed and unobserved.

This method provides an estimate of how large δ would have to be in order to essentially invalidate the estimated effect of industrial employment on each of the outcomes, given an assumed R_{max}. I estimate the δ for the main variable of interest for each of the three dependent variables for two suggested values for R_{max}. The largest possible value it could take, or the absolute upper bound, is 1. This would be the most conservative test possible. Based on empirical evidence using the results of randomized experiments, Oster (2017) suggests that a R_{max} of 1.3 times the R^2 from the relevant regression might be more appropriate. I therefore estimate δ for each of the regression models displayed in Table 1 using both possible values of R_{max}. The relevant values are displayed in Table 3.

Table 3: Selection on Unobservables

	per Capita School Cost	School Enrollment	Class Size
$R_{max} = 1$	$\delta = 0.93$	$\delta = 0.64$	$\delta = 0.89$
$R_{max} = 1.3 \times R^2$	$\delta = 4.02$	$\delta = 1.66$	$\delta = 1.91$

As Table 3 shows, when R_{max} is set to the most conservative value of 1 the estimated δ for industrial employment is close to one for the dependent variables of school cost and class size.

The evidence is weaker for the model concerning school enrollment. When R_{max} is set to 1.3 times the respective R-squared value, the estimated δ for all three models is substantially larger than one. These results indicate that the selection on unobservables would have to be at least as strong as the selection based on the observables included in the models in order to invalidate the findings presented above.

5 Conclusion

When do authoritarian elites invest in public goods provision? How can differences in public spending within different autocracies be explained? In this paper, I use data from Prussian cities at the end of the 19th century to investigate these questions. I argue that authoritarian elites may have had an interest in increasing government spending on public services if it increased the return on capital they owned. Specifically, when the complementarity between physical capital and human capital is high, capital owners have strong interests in getting the state to invest in the provision of human capital. I argue that this was the case for owners of industrial capital in 19th century Prussia.

I use data from a census of Prussian cities to investigate the theoretical argument. To do so, I collected data on educational investment in 110 Prussian cities, income inequality in those cities, as well as industrial employment in the counties around them. Using standard regression techniques and spatial autoregressive models, I show that industrial employment is robustly associated with higher local spending on education. At the same time, however, industrial employment is also associated with larger class sizes. This suggests that the industrial elite increased investment in education, but not by enough to overcome the higher demand in cities near industrial areas. The results are strengthened by an IV analysis in which I instrument industrial employment using distance to carboniferous rock strata, in which coal discovery is more likely. Lastly, I undertake a bounds exercise to show that the results presented here are unlikely to be the artifact of omitted variable bias.

While this paper shows the effect of industry location on educational investment, three potential avenues for further research stand out. First, the effect of different types of capital ownership on other public goods could be investigated. For example, data on other budget items, such as police budgets and health spending, could be collected. Second, future research could examine the relationship between political and economic inequality and types of elite capital ownership to determine whether the findings with regards to inequality are more than a result of a correlation with income. Third, future research should further investigate how inequality affects spending at the local level.

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6 Appendix

Table 4: Simple Linear Models - Additional Controls

	Dependent variable:		
	per Capita School Cost (1895)	School Enrollment (1905)	Class Size (1896)
	(1)	(2)	(3)
Land Concentration	-8.903^{***}	-1.032	-68.296
	(2.756)	(1.003)	(63.479)
Income Gini	1.390	0.637	-21.797
	(2.267)	(0.825)	(52.212)
Avg Income	-0.001	-0.001^{**}	-0.031
	(0.001)	(0.0005)	(0.031)
Log Pop	0.056	-0.006	-0.404
	(0.042)	(0.015)	(0.963)
Taxes pC	0.025	0.017^{*}	0.704
	(0.025)	(0.009)	(0.579)
Industrial Empl	3.738^{***}	1.035^{***}	63.542^{***}
	(0.845)	(0.307)	(19.456)
Protests	0.026	0.048^{**}	1.964
	(0.057)	(0.021)	(1.315)
Distance to Rivers	0.0003	-0.001	-0.022
	(0.001)	(0.0003)	(0.019)
Distance to Coast	-0.0004	-0.00000	-0.004
	(0.0003)	(0.0001)	(0.006)
Constant	1.269^{**}	1.126^{***}	89.949^{***}
	(0.634)	(0.231)	(14.608)
Observations	81	81	81
R^2	0.528	0.371	0.332
Adjusted R^2	0.468	0.291	0.247
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table 5: Simple Linear Models - Public Education

	Dependent variable:		
	per Capita School Cost (1895)	School Enrollment (1905)	Class Size (1896)
	(1)	(2)	(3)
Income Gini	6.538**	1.167*	18.952
	(2.496)	(0.632)	(40.356)
Avg Income	−0.004**	−0.001**	−0.031
	(0.001)	(0.0004)	(0.024)
Log Pop	0.063	−0.001	−0.849
	(0.047)	(0.012)	(0.763)
Taxes pC	0.046*	0.007	0.096
	(0.026)	(0.007)	(0.417)
Industrial Empl	2.227***	0.721***	59.834***
	(0.735)	(0.186)	(11.883)
Protests	0.141**	0.054***	2.384**
	(0.069)	(0.018)	(1.123)
Multiple Cities	0.004	0.048*	1.153
	(0.100)	(0.025)	(1.618)
Constant	1.510**	0.882***	83.623***
	(0.644)	(0.163)	(10.412)
Observations	105	105	105
R^2	0.317	0.430	0.472
Adjusted R^2	0.267	0.389	0.434
Residual Std. Error (df = 97)	0.320	0.081	5.172
F Statistic (df = 7; 97)	6.420***	10.442***	12.411***
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table 6: Simple Linear Models - Public Education

	Dependent variable:		
	per Capita School Cost (1895)	School Enrollment (1905)	Class Size (1896)
	(1)	(2)	(3)
Income Gini	4.058*	1.068	2.669
	(2.198)	(0.767)	(48.034)
Avg Income	−0.002	−0.001**	−0.038
	(0.001)	(0.0005)	(0.030)
Log Pop	0.069	−0.004	−0.145
	(0.042)	(0.015)	(0.928)
Taxes pC	0.013	0.016*	0.614
	(0.026)	(0.009)	(0.571)
Industrial Empl	4.307***	1.078***	66.154***
	(0.863)	(0.301)	(18.863)
Protests	0.045	0.054**	2.254*
	(0.059)	(0.021)	(1.297)
Constant	0.748	1.036***	83.302***
	(0.632)	(0.220)	(13.805)
Observations	81	81	81
R^2	0.456	0.334	0.308
Adjusted R^2	0.412	0.280	0.252
Residual Std. Error (df = 74)	0.240	0.084	5.241
F Statistic (df = 6; 74)	10.341***	6.179***	5.481***

Note: *p<0.1; **p<0.05; ***p<0.01

Table 7: SAR Models

	Dependent variable:		
	per Capita School Cost	School Enrollment	Class Size
	(1)	(2)	(3)
Income Gini	5.248**	0.309	−17.987
	(2.372)	(0.565)	(33.261)
Avg Income	−0.003**	−0.0004	−0.002
	(0.001)	(0.0003)	(0.019)
Log Pop	0.083*	0.007	−0.701
	(0.045)	(0.011)	(0.630)
Taxes pC	0.047**	0.00002	−0.192
	(0.022)	(0.005)	(0.318)
Industrial Empl	2.009***	0.618***	41.500***
	(0.634)	(0.157)	(9.326)
Protests	0.123*	0.044***	1.853**
	(0.065)	(0.016)	(0.924)
Constant	0.972	0.405***	43.978***
	(0.621)	(0.155)	(10.114)
Observations	105	105	105
ρ	0.24	0.45	0.51
Akaike Inf. Crit.	65.166	−231.223	627.452
Note:	*p<0.1; **p<0.05; ***p<0.01		

Spatial Interdependence and Instrumental Variable Models

Timm Betz *   Scott J. Cook†   Florian M. Hollenbach‡

Abstract

Instrumental variable (IV) methods are widely used to address endogeneity concerns in research using observational data. Yet, a specific kind of endogeneity – spatial interdependence – is regularly ignored in this research, threatening claims of causal identification. We show that ignoring spatial interdependence results in asymptotically biased estimates, even when instruments are randomly assigned. The extent of this bias increases when the instrument is also spatially distributed, which is the case for most widely-used instruments (such as rainfall, natural disasters, economic shocks, regionally- or globally-weighted averages, etc.). We demonstrate the extent of these biases both analytically and via Monte Carlo simulation. Finally, we discuss a simple estimation strategy that can be employed to recover consistent estimates of the desired effects.

Key Words: Instrumental Variables, Spatial Analysis, Spatial Modeling, Two-Stage Least Squares

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1 Introduction

As political scientists increasingly focus on the identification of causal effects, the use of instrumental variable (IV) models is becoming commonplace (e.g., Sovey and Green, 2011). The efficacy of IV models in addressing endogeneity concerns hinges on the validity of the instrument. While researchers are usually aware of conditional independence and relevance as general requirements for valid instruments, we identify a specific threat that is frequently ignored: spatial interdependence in the outcome variable. A review of the use of IV models in top journals reveals that scholars rarely discuss and never empirically address this threat to inference (see Figure 1), even as researchers have articulated theories of spatial interdependence and diffusion across political science (see, e.g., Siverson and Starr 1990; Starr 1991; Ward and O'Loughlin 2002; Ward and Gleditsch 2002; Simmons, Dobbin and Garrett 2006; Franzese and Hays 2007; Plümper and Neumayer 2010).[1]

This is is not a trivial oversight. We show that failing to model this interdependence produces estimates that are asymptotically biased, even when the instrument is randomly assigned. When, additionally, the instrument exhibits spatial dependence similar to that of the outcome, the bias in IV estimates increases and can surpass that of ordinary least squares. This concern applies to many popular instruments, including geographic, meteorologic, and economic variables (see, e.g., Ramsay 2011; Hansford and Gomez 2010; Ahmed 2012). Because these instruments are not randomly distributed across space, they risk increased bias even when they are otherwise plausibly exogenous. This is also true for *any* instrument measured at a higher level of aggregation than the outcome, such as regional or global economic, political, and institutional shocks (see, e.g., Stasavage 2005; Büthe and Milner 2008; Boix 2011; Ramsay 2011), which are some of the most frequently used instruments.

Our results connect more general findings in the otherwise distinct literatures on spatial in-

[1]We analyzed each article on the basis of whether prior theories of spatial interdependence or diffusion had been established for and could reasonably apply to the outcome of interest. The articles using IV models that are not at risk of the issues we discuss here include pure time-series analyses, survey experiments, most field experiments, etc.

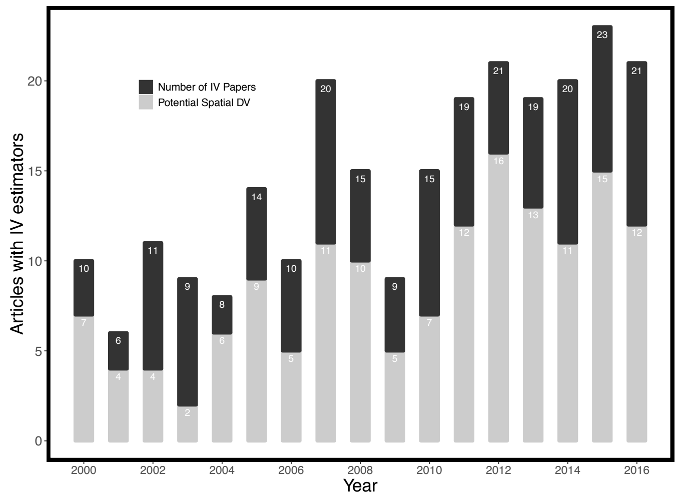

Figure 1: The plot shows the number of articles published in the APSR, AJPS, JOP, IO, BJPS, and World Politics between 2000 and 2016 that use IV models (light grey bars), and the number of those articles at risk of spatial interdependence in the dependent variable (dark grey bars).

terdependence and instrumental variables, respectively. Ignored spatial interdependence constitutes an omitted variables problem (e.g., Franzese and Hays 2007). While IV models are commonly thought to be immune to omitted variable bias, and indeed frequently used to overcome it (Wooldridge, 2002), our results demonstrate that this intuition does not always hold and that, indeed, IV models can augment this omitted variable bias in the case of spatial interdependence. Ignored spatial interdependence constitutes a special type of omitted variable which, due to its reciprocal relationship with the outcome, causes a violation of the exclusion restriction.

As is well known, even mild violations of the exclusion restriction can produce substantial bias (Bartels, 1991; Bound, Jaeger and Baker, 1995). When these violations are due to spatial interdependence, however, solutions are available to recover asymptotically unbiased estimates if one is willing to make assumptions about the nature of spatial relationships in the outcome variable. Recent work in the spatial econometrics literature has generalized spatial models to allow for endogenous predictors (e.g., Kelejian and Prucha 2004; Anselin and Lozano-Gracia 2008; Fingleton and Le Gallo 2008; Drukker, Egger and Prucha 2013; Liu and Lee 2013). These same methods – hereafter spatial-two stage least squares (S-2SLS) – are useful when addressing endogenous predictors even when researchers are otherwise uninterested in spatial dependence theoretically.[2] In short, with S-2SLS researchers instrument for both the endogenous predictor and the spatial-lag of the outcome, thereby obtaining consistent estimates of the desired causal effect.

In addition to accounting for possible outcome interdependence, this approach has two attractive features. First, it nests a standard spatial-autoregressive model and a standard instrumental variables model, allowing researchers to explicitly test restrictions rather than proceed by assumption.[3] Second, because it is an instrumental variables approach, it should be straightforward to understand and implement for those already pursuing IV strategies. Our simulations demonstrate that this approach consistently outperforms estimation strategies that neglect interdependence – even under conditions unfavorable to spatial models.

We therefore advocate that researchers consider S-2SLS as a general, conservative strategy when confronting endogenous predictors and existing theories suggest the possibility of interdependence in the outcome variable. In the conclusion, we discuss some of the implications for the use of instrumental variable models in applied research.

2 OLS and Multifarious Endogeneity

In order to better understand the problems that arise from neglecting spatial interdependence in IV estimation, it is useful to first clarify that unmodeled interdependence is itself an omitted variables problem. Consider a simple linear-additive model

$$\mathbf{y} = \beta\mathbf{x} + \mathbf{e}, \tag{1}$$

[2]To clarify, Franzese and Hays (2007) and others have previously used S-2SLS to indicate a spatial autoregressive (SAR) model estimated via 2SLS. Here, we use this term more broadly to include instances where at least one of the non-spatial predictors is also endogenous.

[3]We focus on the spatial-autoregressive model (SAR) for two reasons. First, it is the most widely-used spatial model in political science. Second, it is the interdependence in the outcome – as in the SAR – that induces the simultaneity that is at the heart of the problem we discuss. Other models that contain a spatial lag of the outcome and additional features, such as autoregressive disturbances (the SAR-AR model), are extensions of the SAR model and could also be estimated. Drukker, Egger and Prucha (2013) discuss the estimation of a SAR-AR model with an endogenous predictor, which can be estimated using the same software routines we discuss below.

where $\mathbf{y}$ is an n-length vector of outcomes, $\mathbf{x}$ the predictor, and $\mathbf{e}$ the disturbance. The OLS estimator of β is the sample covariance of $\mathbf{x}$ and $\mathbf{y}$ over the sample variance of $\mathbf{x}$,

$$\hat{\beta}_{ols} = \frac{\widehat{\text{cov}}(\mathbf{x}, \mathbf{y})}{\widehat{\text{var}}(\mathbf{x})}. \tag{2}$$

Substituting the right-hand side of equation (1) in for $\mathbf{y}$ yields the probability limit

$$\text{plim}_{n\to\infty}\, \hat{\beta}_{ols} = \beta + \underbrace{\frac{\text{cov}(\mathbf{x}, \mathbf{e})}{\text{var}(\mathbf{x})}}_{\text{endogeneity bias}}, \tag{3}$$

showing that $\hat{\beta}_{ols}$ is asymptotically unbiased if $\text{cov}(\mathbf{x}, \mathbf{e}) = 0$, that is, if $\mathbf{x}$ is exogenous.[4] This result should be familiar to readers. It is presented in any introductory econometrics textbook along with common sources of bias: confounding due to omitted variables, simultaneity or reverse causality, and measurement error in the variable of interest.

We are concerned with a special case of confounding: unmodeled interdependence between outcomes. Spatial, or cross-sectional, interdependence occurs when a unit's outcome affects the choices, actions, or decisions of other units (Kirby and Ward, 1987; Ward and O'Loughlin, 2002; Beck, Gleditsch and Beardsley, 2006; Franzese and Hays, 2007; Plümper and Neumayer, 2010). Theories of interdependence are "ubiquitous, and often quite central, throughout the substance of political science" (Franzese and Hays, 2007, p. 141): the contagion of conflict and crises, the spread of domestic institutions and ideologies, economic integration and resulting policy coordination, and participation in international agreements all provide examples. Ignoring this spatial interdependence induces both cross-sectional correlation in the residuals and, more problematically, covariance between the predictors and the disturbances. As a consequence, coefficient estimates are both inefficient and biased; in the following, we focus on the latter concern.

To distinguish confounding due to spatial interdependence from other sources of endogeneity

[4]When we discuss bias, we refer to asymptotic bias. All IV estimators have small-sample bias.

of $\mathbf{x}$, we decompose the error term in equation (1) as

$$\mathbf{e} = \rho \mathbf{W}\mathbf{y} + \mathbf{u}, \tag{4}$$

where ρ is the effect of outcomes $\mathbf{y}$ in surrounding units j on unit i, weighted by $\mathbf{W}$, an n-by-n connectivity matrix which identifies the relationship between units i and j. As usual in spatial econometrics, we refer to $\mathbf{W}\mathbf{y}$ as the spatial lag, with $\mathbf{W}$ determining which other-unit outcomes y_j are likely to influence the choices, actions, behaviors of unit i..

Then, we can rewrite equation (3) as

$$\text{plim}_{n\to\infty}\,\hat{\beta}_{ols} - \beta = \underbrace{\left[\frac{\text{cov}(\mathbf{x},\mathbf{u})}{\text{var}(\mathbf{x})}\right]}_{\substack{\text{Non-spatial}\\ \text{endogeneity bias}}} + \underbrace{\rho\left[\frac{\text{cov}(\mathbf{x},\mathbf{W}\mathbf{y})}{\text{var}(\mathbf{x})}\right]}_{\substack{\text{Spatial}\\ \text{endogeneity bias}}}. \tag{5}$$

Equation (5) separately identifies spatial and non-spatial endogeneity as two potential sources of bias in the OLS estimator.[5] First, bias can result from more familiar, non-spatial sources of endogeneity of $\mathbf{x}$, that is, correlation between $\mathbf{x}$ and $\mathbf{u}$. This is represented by the first term in equation (5), which drops out if $\text{cov}(\mathbf{x},\mathbf{u})$ is zero. Second, bias can arise from spatial interdependence in $\mathbf{y}$. As indicated by the second term on the right-hand side of equation (5), this bias drops out if $\rho = 0$; that is, when there is no spatial interdependence.[6] In what follows, we show that addressing the former while neglecting the latter not only fails to recover unbiased estimates of the effect, but, in many cases, can magnify the bias relative to ordinary least squares.

[5]This derivation of the bias is only approximate, as $\mathbf{W}$ also increases in n. In scalar form, the bias is $\frac{\text{cov}(x_i,u_i)}{\text{var}(x_i)} + \rho\left[\frac{\text{plim}_{n\to\infty}\sum_{j=1}^{n} w_{ij}\,\hat{\text{cov}}(x_i,y_i)}{\text{var}(x_i)}\right]$.

[6]It is only when ρ is zero that this term drops out. $\text{cov}(\mathbf{x},\mathbf{W}\mathbf{y})$ is non-zero, because $\mathbf{W}\mathbf{y}$ is a function of $\mathbf{x}$. While the most obvious solution to address the bias from interdependence may be including $\mathbf{W}\mathbf{y}$ as a variable, this would not be sufficient, because $\mathbf{W}\mathbf{y}$ itself is endogenous in the outcome equation; see, e.g., Franzese and Hays (2007).

3 Spatial Bias in IV Models

Following Sovey and Green (2011), we introduce IV estimation using familiar notation from structural equation models, assuming linear-additive relationships between the variables. Suppose a suitable instrument $\mathbf{z}$ is available, resulting in the following system of equations:

$$\mathbf{y} = \beta\mathbf{x} + \mathbf{e}, \tag{6}$$

$$\mathbf{x} = \gamma\mathbf{z} + \mathbf{v}. \tag{7}$$

As before, suppose that the disturbance can be decomposed as $\mathbf{e} = \rho\mathbf{W}\mathbf{y} + \mathbf{u}$ and interdependence is ignored in the estimation. Then, non-spatial endogeneity arises if $\text{cov}(\mathbf{u}, \mathbf{v}) \neq 0$ and therefore $\text{cov}(\mathbf{x}, \mathbf{u}) \neq 0$. We assume in the following that the variable $\mathbf{z}$ satisfies the usual assumptions for a valid instrument – $\text{cov}(\mathbf{z}, \mathbf{x}) \neq 0$ and $\text{cov}(\mathbf{z}, \mathbf{u}) = 0$ – such that $\mathbf{z}$ is correlated with the endogenous predictor $\mathbf{x}$ but uncorrelated with the disturbance $\mathbf{u}$.

The IV estimator is obtained as two-stage least squares (2SLS), such that

$$\hat{\beta}_{2sls} = \frac{\text{cov}(\mathbf{y}, \mathbf{z})}{\text{cov}(\mathbf{x}, \mathbf{z})}. \tag{8}$$

Inserting the expression for $\mathbf{y}$ yields

$$\text{plim}_{n\to\infty}\, \hat{\beta}_{2sls} - \beta = \frac{\rho \times \text{cov}(\mathbf{W}\mathbf{y}, \mathbf{z})}{\text{cov}(\mathbf{x}, \mathbf{z})} + \frac{\text{cov}(\mathbf{u}, \mathbf{z})}{\text{cov}(\mathbf{x}, \mathbf{z})}, \tag{9a}$$

$$= \underbrace{\rho \left[\frac{\text{cov}(\mathbf{W}\mathbf{y}, \mathbf{z})}{\text{cov}(\mathbf{x}, \mathbf{z})}\right]}_{\substack{\text{Spatial} \\ \text{endogeneity bias}}}, \tag{9b}$$

which shows that, by assumption, 2SLS does not suffer from the non-spatial endogeneity bias of OLS: because $\text{cov}(\mathbf{u}, \mathbf{z}) = 0$ and $\text{cov}(\mathbf{x}, \mathbf{z}) \neq 0$, the second term on the right-hand side of equation (9a) disappears. This result, of course, is well appreciated and motivates the use of 2SLS where $\mathbf{x}$ is suspected to be endogenous.

Less appreciated is that 2SLS is biased in the presence of (ignored and hence unmodeled) interdependence. In short, the instrument violates the exclusion restriction, because it is related to the outcome disturbances via the omitted interdependence term $\mathbf{Wy}$. To see why, note that after substituting and rearranging terms, equation (6) can be written as

$$\mathbf{Wy} = \mathbf{W}(\mathbf{I} - \rho\mathbf{W})^{-1}[\beta\gamma\mathbf{z} + \beta\mathbf{v} + \mathbf{u}]. \tag{10}$$

That is, we can re-express the spatial lag, $\mathbf{Wy}$, in terms of the spatially weighted instrument $\mathbf{z}$ and stochastic terms $\mathbf{u}$ and $\mathbf{v}$. Substituting this expression into the definition of the spatial bias in 2SLS and rearranging, we obtain

$$\text{plim}_{n\to\infty}\, \hat{\beta}_{2sls} - \beta = \beta\left[\rho\frac{\text{cov}(\mathbf{Wz},\mathbf{z})}{\text{var}(\mathbf{z})}\right] + \beta\sum_{k=2}^{\infty}\left[\rho^{k}\frac{\text{cov}(\mathbf{W}^{k}\mathbf{z},\mathbf{z})}{\text{var}(\mathbf{z})}\right]. \tag{11}$$

2SLS is biased unless the terms on the right-hand side are zero. For clarity in the following exposition, we have split the bias into two terms – the first representing the first-order bias and the second representing higher-order terms. Both terms disappear if $\rho = 0$, such that no interdependence exists. If interdependence in the outcome does exist, such that $\rho \neq 0$, however, 2SLS is biased.

Notably, this bias persists even when $\mathbf{z}$ is randomly assigned and, therefore, independently distributed and otherwise exogenous. It is in this case that the two-term expression of the bias in equation (11) becomes useful. When $\mathbf{z}$ is independently distributed, the first term drops out, because independence in $\mathbf{z}$ implies that any specification of $\mathbf{W}$ yields $\text{cov}(\mathbf{Wz},\mathbf{z}) = 0$.[7] That is, the value of $\mathbf{z}$ on unit i is uncorrelated with the value of $\mathbf{z}$ on any other unit (and their weighted-sum $\sum_j w_{ij}z_j$). However, this is not true of the second term in equation (11). While $\mathbf{W}$ is a hollow matrix – all elements along the diagonal equal zero – higher-order multiples of $\mathbf{W}$ are not hollow matrices as ties between units are not uni-directional.[8] Because $\mathbf{W^k}$ has non-zero diagonal

[7]Recall that $\mathbf{W}$ is the connectivity matrix of the outcome – based on, e.g., contiguity, neighbors, or inverse distance – defining how y_i is related to all $y_{j\neq i}$. In connectivity matrices like $\mathbf{W}$ the diagonal elements are always zero, that is, you can not be a direct neighbor of yourself.

[8]If spatial ties were unidirectional – $\mathbf{W}$ is upper- or lower-triangular – the higher-order multiples would remain independent of z_i. However, interdependence rules out unidirectional ties. The

elements, it follows that $\mathbf{W^k z}$ is, for unit i, a function of z_i, and therefore correlated with $\mathbf{z}$, regardless of the distribution of $\mathbf{z}$.

To gain more intuition for why this is the case, recall that $\mathbf{W}$ can be thought of as defining 'neighbors': non-zero entries indicate which units on the outcome variable are related to one another. Then, for each unit, the respective row of $\mathbf{W}$ defines a set of neighbors. Heuristically, powers of $\mathbf{W}$ then represent neighbors-of-neighbors. For example, the i^{th} row of $\mathbf{W}^2$ indicates i's neighbors' neighbors. This is important because, intuitively, a unit always is a neighbor of its own neighbors. Consequently, if $\mathbf{W}$ links unit i to j and unit j to unit i, then $\mathbf{W}^2$ (and higher powers of $\mathbf{W}$) links unit i back to itself. Therefore, even under independence of $\mathbf{z}$, some $\mathbf{W}^k\mathbf{z} \not\perp \mathbf{z}$ as long as $\mathbf{W}$ is non-triangular. Put simply, even if unit i is not related to any of the neighbors defined by $\mathbf{W}$, unit i is always related to itself through these higher powers of $\mathbf{W}$.

That is, for $\rho \neq 0$, any instrument that is randomly assigned is (only) first-order unbiased, providing a lower bound on the spatial bias. While the bias is relatively mild, spatial interdependence on the outcome variable renders IV models biased, even under conditions most favorable to IV models, such as experimental or quasi-experimental designs.

RESULT 1 *With unmodeled spatial interdependence in the outcome, 2SLS is asymptotically biased.*

However, the instruments often used in practice are not independently distributed, risking greater bias still. Specifically, the more the values z_i are similar to neighboring values $z_{j\neq i}$ (where neighboring values are defined by $\mathbf{W}$, the matrix defining relationships among units for the outcome), the greater the bias will be: the first term in equation (11) no longer drops out, and all of the terms in the expression increase in magnitude.

To understand this result, it helps to think of 2SLS broken down into two stages. The first stage is a regression of the endogenous predictor $\mathbf{x}$ on the instrument $\mathbf{z}$, which yields fitted values

importance of reciprocal relationships between units – i.e., interdependence, rather than dependence – for our results can also be seen in the contrast to temporal dependence. With temporal dependence, the current value of the outcome is a function of past values of the outcome, but past outcomes are not a function of the current value. Hence, a randomly assigned instrument poses no problems under temporal dependence.

$\widehat{\mathbf{x}}$. The second stage is a regression of the outcome variable $\mathbf{y}$ on the fitted values $\widehat{\mathbf{x}}$. We make two observations. First, if $\mathbf{z}$ follows a spatial distribution, the projected values $\widehat{\mathbf{x}}$ inherit some of that spatial pattern. Second, in a regression with an (erroneously) omitted spatial lag, the bias in coefficient estimates is reinforced for variables that have a spatial distribution similar to that of the outcome (see, e.g., Franzese and Hays 2007). It follows that the bias in 2SLS becomes most severe if the fitted values $\widehat{\mathbf{x}}$ have a spatial distribution similar to that of the outcome – which, in turn, is the case if the instrument has a spatial distribution similar to the outcome.[9]

It is not crucial that the instrument and the outcome follow identical spatial patterns, merely that the instrument and the outcome have some similarity in their spatial patterns. That is, the bias in 2SLS increases if the $\mathbf{W}$ that characterizes the relationships in $\mathbf{y}$ also can be *thought of* as partially characterizing the relationships in $\mathbf{z}$. In practice, when considering the extent of the spatial bias in 2SLS, one can therefore remain largely agnostic about the nature of the spatial relationships on the instrument – in particular, it is not necessary to determine whether $\mathbf{z}$ and $\mathbf{y}$ are truly governed by identical $\mathbf{W}$s or even which specific $\mathbf{W}$ applies to the instrument (and our empirical approach, detailed in the next section, is consistent with this view). Our point is much simpler: if the outcome is spatially interdependent, then the bias in 2SLS will be more severe for instruments with spatial patterns similar to that of the outcome.

These concerns apply to a large set of common instruments. Researchers often draw on geographic, meteorologic, or economic variables, such as natural disasters (Ramsay, 2011), rainfall data (Hansford and Gomez, 2010), or commodity price shocks (Ahmed, 2012), where spatial dependence among units is likely – natural disasters, rainfall, and price shocks do not stop at territorial borders. The same problem arises for instruments that are measured at a higher level of aggregation than the endogenous predictor. These instruments are increasingly popular in political science and economics. If, for instance, the instrument is based on regional political or institutional shocks, such as waves of democratization (Stasavage, 2005) or membership in international institutions in neighboring countries (Büthe and Milner, 2008), the instrument is by construction

[9]Regardless of the instrument, this is also the case, of course, if $\mathbf{x}$ has a spatial distribution similar to the outcome.

measured at a higher level of aggregation than the endogenous predictor: the value of the instrument is identical or nearly identical for each of the observations within the cluster. At the same time, because these instruments rely on the argument that units are connected to each other, the resulting model is a strong contender for spatial interdependence in the outcome: it is difficult to argue convincingly for spatial patterns in the instrument while ruling out the same for the outcome variable (Betz, Cook and Hollenbach, 2018).[10] Similarly, many of the outcome variables of interest to political scientists cluster in regional patterns, such as democratization, economic growth, or policy choices. In these cases, z_i is correlated or identical for some units in a way that is similar to the spatial pattern in the outcome, reinforcing the bias in 2SLS.

To illustrate, consider the use of meteorological variables as instruments for democratization ($\mathbf{z}$) in models of economic development ($\mathbf{y}$). Contiguous states (a widely used $\mathbf{W}$) likely have both similar levels of development ($\mathbf{y}$) and common weather patterns ($\mathbf{z}$), where the former implies $\rho > 0$ and the latter implies $\text{cov}(\mathbf{Wz}, \mathbf{z}) > 0$. It is under these conditions that the bias will be most severe; as can be seen in equation (11), the bias increases in the strength of the interdependence in the outcome (ρ) and the strength of the spatial dependence in the instrument ($\text{cov}(\mathbf{Wz}, \mathbf{z})$).

RESULT 2 *With unmodeled spatial interdependence in the outcome, the more similar are the spatial distributions of the instrument and the outcome, the greater is the bias in 2SLS.*

We add three additional observations. First, these biases are usually inflationary, which can be seen from equation (11). The bias terms are multiplied by powers of ρ, which is positive in most applications (Franzese and Hays, 2007). And, if $\mathbf{z}$ is governed by a similar pattern of spatial dependence as the outcome, the covariances between $\mathbf{W^k z}$ and $\mathbf{z}$ are non-negative. Consequently, the right-hand side of equation (11) should have the same sign as β and be proportional to β. Thus, in most applications the bias in 2SLS that arises from spatial interdependence exaggerates the true parameter value – where β is negative, 2SLS produces smaller coefficient estimates, and where β is positive, 2SLS produces larger coefficient estimates.

[10] See Betz, Cook and Hollenbach (2018) for a separate set of concerns with the identifying assumptions underlying instruments that rely on spatial interdependence.

Second, the spatial bias induced from the instrument can exceed the spatial bias in ordinary least squares. Consider the relative spatial bias of OLS (the left-hand side) and 2SLS (the right-hand side):

$$\frac{\mathrm{cov}(\mathbf{Wy}, \mathbf{x})}{\mathrm{var}(\mathbf{x})} \lessgtr \frac{\mathrm{cov}(\mathbf{Wy}, \mathbf{z})}{\mathrm{cov}(\mathbf{x}, \mathbf{z})}. \tag{12}$$

To focus on the comparison of the spatial bias between 2SLS and OLS, suppose that no non-spatial endogeneity exists. Re-expressing both terms, condition (12) becomes

$$\sum_{k=1}^{\infty} \left[\rho^k \frac{\mathrm{cov}(\mathbf{W}^k\mathbf{x}, \mathbf{x})}{\mathrm{var}(\mathbf{x})} \right] \lessgtr \sum_{k=1}^{\infty} \left[\rho^k \frac{\mathrm{cov}(\mathbf{W}^k\mathbf{z}, \mathbf{z})}{\mathrm{var}(\mathbf{z})} \right]. \tag{13}$$

Simply put, differences in the spatial distribution of the instrument and the endogenous variable inform the relative degree of spatial bias. This is similar to Bartels's (1991) recognition that, because $\mathbf{x}$ can be considered its own instrument, when using an invalid instrument $\mathbf{z}$ the gains relative to OLS are a function of the relative difference in how $\mathbf{z}$ and $\mathbf{x}$ covary with the disturbance of $\mathbf{y}$. Again thinking of the second stage in 2SLS as a regression of $\mathbf{y}$ on the projection $\widehat{\mathbf{x}}$ further clarifies the role of spatial dependence in the instrument: the bias of 2SLS relative to OLS increases as the spatial distribution of the instrumented predictor, $\widehat{\mathbf{x}}$, becomes more similar to the spatial distribution of the outcome than the original predictor, $\mathbf{x}$. Then, IV models augment the spatial bias, because $\widehat{\mathbf{x}}$ is more similar to the omitted spatial lag than $\mathbf{x}$ is. The reverse, of course, also holds: if the instrument is randomly assigned, then the similarity between the spatial pattern of the instrumented predictor, $\widehat{\mathbf{x}}$, and the outcome decreases, and the bias of 2SLS relative to OLS declines. Nonetheless, even in that case, as we emphasize in Result 1, 2SLS remains biased.

Finally, because spatial and non-spatial endogeneity biases may attenuate or reinforce each other, ignoring spatial interdependence in the outcome risks unpredictable and possibly greater overall bias than OLS. When the endogenous variable, $\mathbf{x}$, is spatially less clustered than the instrument, $\mathbf{z}$, the severity of the difference in the spatial biases may be sufficiently large to surmount the gains from addressing non-spatial endogeneity. And because the spatial and non-spatial bias may have different directions, resolving one of the biases may easily produce results further from the truth than resolving none. Perhaps most problematically, these offsetting effects mean that the OLS and 2SLS estimates will not even be sufficient to obtain bounds on the true parameter value.

4 Spatial Models with Additional Endogenous Predictors

What, then, can researchers concerned with endogeneity in a key predictor and spatial interdependence in the outcome do? The solution is actually quite simple: estimate a modified instrumental variables model. While early work in spatial econometrics assumed exogenous predictors, methods for estimating models with additional endogenous predictors have become increasingly common (Kelejian and Prucha, 2004; Anselin and Lozano-Gracia, 2008; Fingleton and Le Gallo, 2008; Drukker, Egger and Prucha, 2013; Liu and Lee, 2013). To date, however, these models have not received much attention in applied spatial work in political science, and even less so in contexts where researchers are not theoretically interested in spatial relationships.

In short, to redress the concerns above, researchers need to account for the spatial dependence of the outcome in the systemic part of the model. Yet, including a spatial lagged-outcome produces a system of simultaneously-determined, non-separable equations. That is, $\mathbf{Wy}$ is itself is an endogenous predictor, no different than a simultaneously-determined $\mathbf{x}$. Consequently, in spatial modeling, researchers exploit the same strategies generally used when confronting endogenous predictors (such as maximum likelihood, 2SLS, or GMM). One can simply extend the familiar IV framework, applying it to account for spatial interdependence and endogeneity in the predictor. As this is simply a special case of multiple endogenous variables, a spatial-two stage least squares (S-2SLS) model can be estimated as in standard IV analysis: instrumenting for $\mathbf{Wy}$ and $\mathbf{x}$ simultaneously.

While other solutions are also available – e.g., purging the spatial dependence of the outcome equation via eigenvector filtering – we prefer S-2SLS for several reasons.[11] First, the mechanics

[11]Limited and full information estimators allowing for both spatial and non-spatial endogeneity have been established, with Kelejian and Prucha (2004) the first to derive formal large sample results; see also Drukker, Egger and Prucha (2013) for a GMM estimator. Franzese, Hays and

of estimating this model are already familiar to researchers using IV estimation for an endogenous variable $\mathbf{x}$, because the estimator, 2SLS, is the same. Second, S-2SLS nests the non-spatial IV model a researcher would have otherwise estimated. Rather than restrict ρ – the spatial effect – to be zero by assumption, as in 2SLS, S-2SLS allows researchers to explicitly test this. As we demonstrate in simulations, this nesting helps ensure that – even if no spatial interdependence is present and $\rho = 0$ – the model recovers the same estimates as the original 2SLS, with only minimal efficiency loss due to the additional parameter. Finally, the S-2SLS model, as well as several extensions, can be estimated in both Stata (`spivreg`) and R (`sphet`).[12]

The only practical hurdles to estimating a S-2SLS are in the specification stage: i) what are appropriate instruments for the spatial lag, and ii) what is the appropriate connectivity matrix $\mathbf{W}$ for the outcome variable. The first, instrument selection, is comparatively simple. While instruments for the endogenous predictor usually require finding additional exogenous variables, instruments for the spatial lag can typically be found from transformations to the existing data. Specifically, spatial lags of the exogenous predictors serve as instruments for the spatial lag of the outcome. To see the basic intuition for this, just multiply $\mathbf{W}$ by both sides of the simple linear-additive model – i.e., $\mathbf{y} = \beta\mathbf{x} + \mathbf{e} \Rightarrow \mathbf{Wy} = \beta\mathbf{Wx} + \mathbf{We}$. Just as $\mathbf{x}$ is related to $\mathbf{y}$, $\mathbf{Wx}$ is related to $\mathbf{Wy}$, the spatial lag.[13]

The second practical hurdle, the selection of $\mathbf{W}$, is already familiar to researchers with exposure to spatial models. For those less familiar, we briefly sketch out the basics. To undertake spatial econometric modeling, researchers must pre-specify how units are related to one another

Cook (2016) discuss the complications of modeling spatial interdependence in discrete-choice models.

[12]Both programs provide additional capabilities; a S-2SLS model can also be estimated with any standard routine for estimating IV models.

[13]A more complete derivation can be seen by noting that the reduced form of the spatial-lag model discussed in section 3,

$$\mathbf{y} = (\mathbf{I} - \rho\mathbf{W})^{-1}[\mathbf{x}\beta_x + \mathbf{u}],$$

can be re-expressed using an infinite series and multiplied through by $\mathbf{W}$ to produce

$$\mathbf{Wy} = \mathbf{Wx}\beta_x + \rho\mathbf{W}^2\mathbf{x}\beta_x + \rho^2\mathbf{W}^3\mathbf{x}\beta_x + \ldots + (\mathbf{I} - \rho\mathbf{W})^{-1}\mathbf{u},$$

indicating how spatial lags of $\mathbf{x}$ (and their higher-order powers) effectively instrument for $\mathbf{Wy}$.

(i.e., the network). Geographic proximity (e.g., contiguity) is commonly used, though researchers should specify connections that are most theoretically appropriate for their data. These relational measures for 'space' are then supplied to the model as the elements in $\mathbf{W}$ – an n-by-n connectivity matrix which identifies the relationship between units i and j.[14] S-2SLS clearly performs best when $\mathbf{W}$ reflects the true network, yet gains are still likely even when researchers do not have full information on the ties between units. First, in the worst-case (and unlikely) scenario that a researcher completely mischaracterizes $\mathbf{W}$, this would still do no worse in expectation than 2SLS – S-2SLS recovers a zero estimate of ρ due to misspecified $\mathbf{W}$, while 2SLS does so by assumption. Second, due the high correlation across different possible network structures, even a mis-specified $\mathbf{W}$ has power against the truth (LeSage and Pace, 2014). We revisit this concern in the simulated experiments in the next section.[15]

Once specified, estimation of the S-2SLS model proceeds without additional complications. Because S-2SLS is estimated via the 2SLS estimator, it inherits the asymptotic and small-sample properties of 2SLS (including consistency, but finite sample bias and the sensitivity to weak instruments).[16] Similarly, standard variance estimators – robust to heteroskedasticity or non-independence, for instance – are easily applicable. We demonstrate the gains that can be realized from S-2SLS in the following sections.

[14]Because the appropriate $\mathbf{W}$ is case-specific, it is difficult to automate this step. However, useful best practices for selecting $\mathbf{W}$ can be found in Neumayer and Plümper (2016).

[15]Note that the researcher need not specify the spatial distribution of the instrument. $\mathbf{W}$ specifies connections among units with respect to the outcome variable. As we highlighted in the previous section, the extent of the bias in 2SLS depends on the similarity in the spatial pattern between the instrument and the outcome. But it is not necessary to determine the specific spatial pattern of the instrument.

[16]For an approach addressing weak instruments in IV models, see Betz (2013). Drukker, Egger and Prucha (2013) and Liu and Lee (2013) allow for both additional residual spatial error autocorrelation and/or heteroskedasticity. These extensions are GMM-plus-IV, implemented in Stata's spreg. While we do not discuss this at length here, the first step is the S-2SLS we present, which provides the initial, consistent estimates of the spatial interdependence in the outcome that can then be used in the second step estimation of the error autocorrelation, with successive iteration over both steps until convergence is obtained.

5 Simulation Experiments

To assess the performance of OLS, 2SLS, and S-2SLS, we undertake a series of Monte Carlo experiments with varying levels of spatial and non-spatial endogeneity. In particular, the data for our simulations are generated as follows:

$$\mathbf{y} = (\mathbf{I} - \rho_y \mathbf{W})^{-1}[\mathbf{x}\beta + \lambda_1 \mathbf{Q} + \mathbf{u}_1] \tag{14a}$$

$$\mathbf{x} = \gamma \mathbf{z} + \lambda_2 \mathbf{Q} + \mathbf{u}_2, \tag{14b}$$

$$\mathbf{z} = (\mathbf{I} - \rho_z \mathbf{W})^{-1}\mathbf{v}, \text{ where } \mathbf{v} \sim N(0,1) \tag{14c}$$

where $\mathbf{y}$ is the outcome, $\mathbf{x}$ is the endogenous predictor, $\mathbf{Q}$ is a matrix of exogenous predictors, $\mathbf{W}$ is a row-standardized connectivity matrix, and $\mathbf{z}$ is the instrument.[17] Consistent with our discussion above, we only consider the consequences of spatial interdependence in $\mathbf{y}$ and $\mathbf{z}$, which are the key attributes for bias in 2SLS.[18]

The extent of spatial interdependence in the outcome and the instrument is given by parameters ρ_y and ρ_z, respectively, with larger values of ρ_y and ρ_z resulting in greater spatial interdependence in $\mathbf{y}$ and $\mathbf{z}$. We do not vary the specification of the $\mathbf{W}$ that governs the spatial pattern of $\mathbf{y}$ and $\mathbf{z}$, respectively. Non-spatial endogeneity is induced through draws of $(u_1, u_2)^T = N(0, \Sigma)$, where Σ is the covariance matrix of a bivariate normal random variable. We decompose Σ such that we can specify the correlation (δ) between u_1 and u_2 directly. We vary δ to induce different degrees of non-spatial endogeneity. If $\delta = 0$, $\mathbf{x}$ is exogenous and OLS (or standard spatial) models should

[17] Locations for the units are generated by twice taking n draws from a standard uniform, with the combined results producing xy-coordinate points. Connections between the units are then generated using a k-Nearest Neighbor algorithm with $k = 5$, returning a binary n-by-n matrix with each element in a row coded as 1 for the five closest units or 0 for all others (including zeros along the diagonal). The matrix is then row-standardized.

[18] As discussed above, the relative spatial pattern of $\mathbf{x}$ and $\mathbf{z}$ only matters for the performance of 2SLS relative to OLS. For the simulations, to illustrate Result 2, we only consider scenarios where 2SLS performs relatively poorly due to the spatial pattern in $\mathbf{z}$.

be preferred. With non-zero δ and non-zero ρ_y, the assumptions of neither OLS nor 2SLS hold.

This setup allows us to consider various scenarios that correspond to our results above. ρ_y = ρ_z = 0 produces the standard IV model with an i.i.d. instrument, such that 2SLS should perform well. $\rho_y \neq 0$ but $\rho_z = 0$ implies interdependence in the outcome but an i.i.d. instrument. Following Result 1, we should still observe some bias in 2SLS in this scenario, whereas S-2SLS should perform better. As ρ_z increases, the bias in 2SLS should increase, both in absolute terms (Result 2) and relative to OLS, because the instrument becomes more similarly distributed to the outcome relative to the predictor. Finally, varying δ, the extent of non-spatial endogeneity, allows us to evaluate scenarios under which OLS – which produces spatial and non-spatial endogeneity – should perform worse than 2SLS – which produces only spatial endogeneity.[19]

The remaining parameters $\{\beta, \gamma, \lambda_1, \lambda_2\}$ are the coefficients of the predictors of $\mathbf{x}$ and $\mathbf{y}$, respectively.[20] Our main focus is on the estimate of β, which we hold constant across experiments at 2.[21] Table 1 shows the different parameter values which we use to create simulated data sets. There are 108 different combinations of the parameters shown in Table 1 (with the bolded values indicating those used in the subsequent figures). For each combination we generate $1,000$ data sets, which results in a total of $108,000$ simulation runs. On each data set we estimate β using OLS, 2SLS, and our preferred method, S-2SLS.

The results are presented in Figures 2 and 3, which report the median absolute error and coverage probabilities for the estimators, respectively.[22] The figures vary along three dimensions. First,

[19] In the Online Appendix we also present results where we vary the strength of the instrument (making it weaker).

[20] In the first stage, we specify the intercept as 2. The two exogenous predictors have coefficients 3 and -2.5. For the second stage the intercept is -2 and the exogenous predictors are -3 and 2.5. For the plots presented in the manuscript we set the coefficient on the instrument to $\gamma = 1.5$ and the number of observations to $n = 200$. Observations for the predictors are drawn from standard normal distributions. In the Online Appendix, we present how estimates vary with sample sizes and the coefficient γ.

[21] To maintain the analogy to OLS, we retain attention on the estimate of β. As is common in non-linear models (such as probit or logit), other quantities of interest can be calculated based on the coefficient estimates and the data, most notably the Average Total Direct Impact as an approximate equivalent. We return to this briefly in the replications.

[22] We prefer MAE as it limits the influence of potential outliers in the simulation results. In

Table 1: Parameter Values for Simulations

n	50	**200**	
ρ_y	**0**	**0.3**	**0.6**
ρ_z	**0**	**0.3**	**0.6**
γ		0.75	**1.5**
δ	**-0.5**	**0**	**0.5**

Note: Bold values used in Figures 2 & 3.

δ – the non-spatial endogeneity – increases across the three rows from -0.5 in the top row, to 0 in the middle row, to 0.5 in the bottom row. Second, each column shows results for a different value of ρ_z – the spatial pattern of the instrument – ranging from 0 in the column on the left over 0.3 in the middle to 0.6 on the right. Finally, within each individual plot, ρ_y – the spatial interdependence in the outcome – increases from left to right across the x-axis.

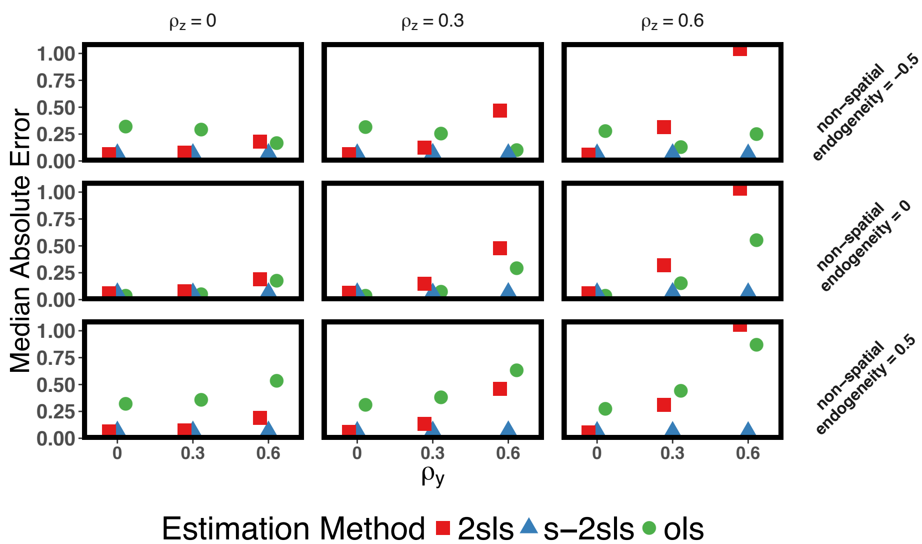

Figure 2: Median Absolute Error. Rows: δ, non-spatial endogeneity. Columns: ρ_z, spatial pattern of the instrument. Horizontal axis within each plot: ρ_y, spatial interdependence in the outcome. Vertical axis within each plot: Median Absolute Error.

Several observations stand out from the plots. Turning to the median absolute error in Figures 2

the Online Appendix we also present model performance in terms of the root mean squared error (RMSE).

first, across all levels of non-spatial endogeneity (δ), the error of 2SLS grows as ρ_y increases, dramatically so as ρ_y *and* ρ_z increase together. This is consistent with our theoretical results: under interdependence in the outcome, the 2SLS model always returns biased estimates (Result 1), with the severity of these biases increasing in the similarity of the spatial pattern in the instrument and the outcome (Result 2). Importantly, when both the instrument and outcome are characterized by spatial dependence, a situation that in our view is not uncommon in the literature, the bias in 2SLS increases quickly. Conversely, the median absolute error of S-2SLS is stable, as its performance does not suffer under high interdependence in $\mathbf{y}$, $\mathbf{z}$, or both. In fact, S-2SLS weakly dominates 2SLS, besting it when spatial interdependence is present and matching it when there is not. Thus, when non-spatial endogeneity is present and instrumental variable models may be warranted, S-2SLS performs better than or as good as 2SLS. Across all scenarios considered in the simulations, 2sls performs better in terms of the median absolute error only when $\rho_y = 0$, and even then the maximum difference in median absolute error between 2SLS and S-2SLS is 0.03. While not surprising, this bolsters our claim that S-2SLS is a useful conservative specification, robust under non-spatial and spatial endogeneity, because it nests both cases.

The OLS estimator performs poorly when either non-spatial or spatial endogeneity is present. However, and as discussed above, the bias can be larger for 2SLS than for OLS, even in the case of strong non-spatial endogeneity, where OLS should perform poorly. This occurs under higher levels of ρ_z and ρ_y – as we move from the left to the right in each box, and as we move from the left column to the right column – where the spatial and, in turn, total bias of 2SLS is greater due to the spatial interdependence of the instrument.

The top and middle rows of Figure 2 present two particularly interesting scenarios. In the top row, with negative non-spatial endogeneity and positive spatial interdependence, the relative performance of OLS improves, both in absolute terms and relative to 2SLS, as the spatial interdependence increases. The two biases are countervailing, combining to produce a result closer to the truth. Under these conditions, 2SLS produces relatively worse results, as it addresses one type (and therefore direction) of bias, while neglecting the other. As a result, 2SLS produces *more* biased estimates even while – in fact, due to – addressing one of the sources of that bias.

In the middle row, we have no non-spatial endogeneity bias, and relying on 2SLS is unnecessary. Usually, using 2SLS instead of OLS is not much of a concern, aside from a slight efficiency loss. This changes with interdependence. If the instrument is spatially more similar to the outcome than the predictor (as in the second and third column), 2SLS produces more total bias than OLS. In this case, 2SLS not only was unnecessary, but results in worse estimates than OLS. (Of course, this result hinges on the simulation setup, which consistent with our discussion allowed for a spatial pattern in $\mathbf{z}$ but not in $\mathbf{x}$ – if the reverse was the case, 2SLS would perform relatively better.)

These results are particularly problematic, as researchers relying on 2SLS over OLS estimates will be more confident about results that are further from the truth and dismissive of results that were closer to it. Frequently, a difference between 2SLS and OLS estimates is accepted as evidence of suspected non-spatial endogeneity (such as measurement error or reverse causality) that was successfully removed by 2SLS. While 2SLS removes non-spatial endogeneity, such arguments ignore that 2SLS may come with biases of its own, and that these biases need not be less pronounced than the biases in OLS. Where outcomes are interdependent, there is no guarantee that 2SLS produces better estimates than OLS. S-2SLS, by contrast, does not confront this issue and consistently outperforms both OLS and 2SLS.[23]

[23]In the Online Appendix we present findings when the instrument is weaker (i.e., $\gamma = 0.75$). As expected, IV methods perform worse, yet the overall order in performance between the different methods does not change.

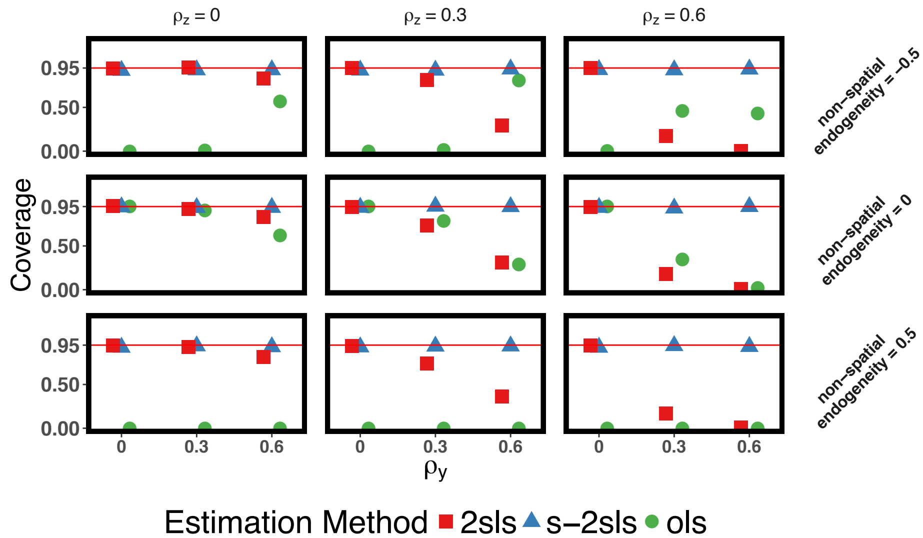

Figure 3: Coverage Probabilities. Rows: δ, non-spatial endogeneity. Columns: ρ_z, spatial pattern of the instrument. Horizontal axis within each plot: ρ_y, spatial interdependence in the outcome. Vertical axis within each plot: Coverage rate.

Figure 3 shows the coverage probabilities for each estimator. The coverage statistic measures the share of estimates for which the true parameter falls within the 95% confidence interval of the estimate. If perfectly calibrated, we would expect this to be true for 95% of cases. The results are generally consistent with our expectations. First, the coverage of OLS is generally poor under either spatial or non-spatial endogeneity. However, for the reason just discussed, when the spatial and non-spatial bias are oppositely signed (top row), the coverage of OLS improves with higher spatial interdependence. Second, with interdependence in the outcome, the 2SLS estimator under-covers, with the severity of this increasing ρ_z. Finally, S-2SLS has very good coverage throughout and is not affected by interdependence in $\mathbf{z}$ or $\mathbf{y}$. In fact, the coverage of S-2SLS is consistently around 95% , ranging between 92% and 96%.

Robustness Checks - Wrong W

What if the researcher is unsure about the spatial network underlying the modeled processes? In the simulations above, we estimated the S-2SLS model based on the correct connectivity matrix.

Effectively, this assumes the researcher has complete information on the spatial network, which is often an unrealistic assumption in applied research. Therefore, we perform the same set of simulation experiments as above but we also vary the level of misspecification of the spatial network in the estimation. To do so, we draw a second set of random spatial locations and its corresponding $\mathbf{W}$ matrix. We then create the $\mathbf{W}$ matrix for the model estimation based on binary draws from either the correct $\mathbf{W_c}$ or the false $\mathbf{W_f}$ matrix. The probability of each cell value being drawn from the false matrix is the misspecification parameter. We set this parameter to three different values: 0, 0.5 and 1. The results presented above assumed no misspecification, such that the probability of drawing from the false $\mathbf{W_f}$ matrix is 0.

As Figure A.1 in the online Appendix shows, S-2SLS generally outperforms or is equivalent to 2SLS. In this scenario we consider positive correlation between the first and second stage, i.e. sufficient non-spatial endogeneity. In the worst case (bottom row in Figure A.1), when the $\mathbf{W}$ matrix is completely misspecified, S-2SLS parallels 2SLS in performance. As the median absolute error in 2SLS increases, so does the median absolute error for S-2SLS. Similarly, as Figure A.2 in the online Appendix shows, as the coverage for 2SLS worsens, so does the coverage for S-2SLS. This demonstrates what we articulated earlier: because S-2SLS nests 2SLS, it only suffers minor efficiency losses when it is the incorrect model.[24]

Put differently, even if researchers have no knowledge of the spatial network in their data and chose a spatial matrix at random, S-2SLS does not perform significantly worse than 2SLS. Conversely, where there is spatial interdependence and some knowledge of the connectivity matrix, the gains from S-2SLS are considerable – S-2SLS provides the more robust and conservative modeling strategy.

[24]For some of the simulations we were unable to estimate the spatial model. This only occurred when the spatial matrix was drawn from two different W matrices. We drop these observations before calculating the performance statistics. The relative performance of S-2SLS does not change if we also drop the corresponding results for OLS and 2SLS.

6 Application: "Revisiting the Resource Curse"

To illustrate how failing to account for spatial interdependence when using IV models can induce bias in published research, we replicate Ramsay's (2011) "Revisiting the Resource Curse: Natural Disasters, the Price of Oil, and Democracy."[25] A long-standing literature in political science has considered the effects of natural resource revenues on political order. Ramsay (2011) identifies reverse causality as one of the main threats to inference: changes in resource revenues may cause political change, but politics may also affect resource revenues.

The main independent variable of interest is a country's annual oil income per capita (specifically the price of crude oil times annual production divided by the population). The dependent variable is a country's level of democracy, measured as a normalized score of Polity IV. A valid instrument would have sufficient power to explain oil revenues; and fulfill the exclusion restriction such that it only affects changes in democracy via the path through oil revenues. In light of these requirements, Ramsay (2011) introduces out-of-region natural disasters as the instrumental variable (where regions are defined as Europe, Middle East, North Africa, sub-Saharan Africa, Asia, or the Americas). The rationale is that natural disasters, by reducing oil production in the affected countries, change world oil prices, and therefore oil revenues of individual countries; at the same time, natural disasters should have no direct effect on oil production in remote countries.

Spatial interdependence introduces three problems with this instrument. First, we know that levels of democracy and changes thereof cluster in space (Gleditsch and Ward, 2006). As we explain above, in and of itself this raises the potential of biased coefficients. Second, natural disasters, the instrument of choice, likely correlate in space. Weather phenomena and, moreover, the *effects* of severe weather events are not constrained by geographic boundaries. As Ramsay (2011) notes, the effects of disasters are likely to spill over and directly affect neighboring states. Third, even if the instrument was not by itself spatially correlated, aggregating the variable to the regional level induces a spatial pattern by construction. By designing the instrument as "out

[25] In the online Appendix we provide an additional replication of Ashraf and Galor's article "Dynamics and Stagnation in the Malthusian Epoch" (2011).

of region disaster damage estimates" (Ramsay, 2011, 514), all countries within each of the five regions have the same value on the instrument, thus creating spatial correlation in the instrument by design. Notably, this design matches prominent theories of democratic diffusion, which often focus on regional waves of democratization (e.g., Starr 1991; Gleditsch and Ward 2006). As we demonstrate above, with a spatially interdependent outcome variable and instrument, the bias in IV estimates increases. Moreover, given the likely positive spatial correlation in the outcome in this example, we would expect IV models to overestimate the coefficient of interest when ignoring spatial interdependence.

To replicate the analysis, we first (re-)estimate a linear model (via OLS) (Model 4 in Ramsay (2011)). Aside from the variable of interest (log oil revenue per capita), the model includes controls for GDP per capita, GDP growth, a lagged polity variable, and year fixed effects. The results, in Model 1 in Table 2, reproduce those from Ramsay (2011).

Before estimating any spatial models, we have to make a decision about how to model the spatial connectivity between countries for the dependent variable. That is, we need to consider the process by which political institutions in one country may affect those of its neighbors. We opt for a common choice in the literature: a $\mathbf{W}$ matrix that identifies countries' geographically contiguous neighbors. To create the weights we row-standardize the matrix, such that the weights in each row sum to one.[26] The choice of the $\mathbf{W}$ matrix is often not clear, and while a detailed discussion is beyond the scope of this paper, we encourage scholars to think theoretically about the potential spatial relationships in their dependent variable when designing $\mathbf{W}$ (see, e.g., Neumayer and Plümper 2016). Using the spatial weights matrix, and for comparison to OLS, we first estimate a SAR model – results given in Model 2 in Table 2 – which returns a significant value for the spatial effect parameter ($\rho = 0.173$; p-value < 0.05). Oil income per capita is still negative and statistically significant.[27]

[26]We are unable to merge 4 observation to the shapefiles and therefore drop, for consistency, these observations from the 1267 original observations in all of the models.

[27]Due to the non-linear nature of the SAR model, the average direct effect – that is, the average effect of a one-unit change in x_i on y_i – is not the coefficient estimate, but instead: $\mathrm{Tr}\{(\mathbf{I} - \rho\mathbf{W})^{-1}\beta_x\}/n$. For the variable of logged oil income per capita, this results in a value of -0.042,

Turning to the IV estimates, column 3 in Table 2 replicates the two-stage least squares model presented in column 4 of Table 3 in Ramsay (2011). Based on the 2SLS results, the estimated coefficient of log oil income is -0.357 and statistically significant. That is, the 2SLS model presents an effect estimate that is almost eight times larger than the original OLS estimates.

Next, we estimate a S-2SLS model. Given that in both the SAR and S-2SLS model we are interested in modeling the spatial interdependence in $\mathbf{y}$, the choice of $\mathbf{W}$ should be the same. Thus, we again use the same row-standardized matrix of geographically contiguous neighbors. The results for the S-2SLS model are presented in column 4 in Table 2. The instrumented coefficient of logged oil income is now estimated to be -0.088. Log oil revenue per capita is still found to have a negative effect on the normalized polity score. However, the estimated effect is much smaller than in the 2SLS model that ignores spatial interdependence.[28] Furthermore, we see substantial efficiency gains in the estimate – as indicated by the standard errors – once we account for spatial interdependence.

In sum, failing to account for spatial interdependence resulted in substantial inflationary bias in the estimates of interest. We do not overturn the central finding presented in Ramsay (2011) that oil revenue is negatively associated with the polity score, but the magnitude of the effect is reduced considerably and the purported gains from IV estimation are significantly reduced.

Conclusion

Instrumental variable models are now a frequently used tool in applied political science research. IV methods are especially common in observational research, where endogeneity – induced by reciprocal causality, measurement error, or omitted variables – often threatens credible inference. Yet, observational data is also where concerns of spatial interdependence are the most salient and where, because researchers lack random assignment over treatments, instruments are not randomly distributed across space. Consequently, instrumental variable methods are most widely used where

slightly smaller than the OLS effect estimate of -0.046.

[28]Table D.1 in the Appendix displays the results comparing the 2SLS model and the S-2SLS model for the robustness checks presented in Table 5 in Ramsay (2011). Again, the differences between 2SLS and S-2SLS are stark and reflect the same pattern as shown in Table 2.

Table 2: Replication of OLS and IV results Table 1 & 3 in Ramsay (2011)

	(1) OLS Replication	(2) SAR Spatial	(3) 2SLS Replication	(4) S-2SLS Spatial
Log oil income	-0.046^{***}	-0.042^{***}	-0.358^{**}	-0.088^{***}
per capita	(0.006)	(0.004)	(0.167)	(0.011)
Log GDP	0.065^{***}	0.066^{***}	0.356^{**}	0.108^{***}
per capita	(0.009)	(0.005)	(0.155)	(0.012)
GDP growth	-0.004^{***}	-0.004^{***}	-0.012^{**}	-0.005^{***}
	(0.001)	(0.001)	(0.005)	(0.001)
Polity at entry	0.666^{***}	0.658^{***}	-0.005	0.564^{***}
	(0.028)	(0.017)	(0.373)	(0.029)
Constant	0.056	-0.237^{***}	-0.962^{**}	-0.330^{***}
	(0.064)	(0.050)	(0.439)	(0.059)
Spatial ρ_y		0.173^{***}		0.119^{***}
		(0.015)		(0.020)
Observations	1263	1263	1263	1263
Year dummies	Yes	Yes	Yes	Yes

W matrix for spatial models based on contiguous neighbors.

Instrumental variable: out-of-region natural disasters.

Table shows coefficient estimates, standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

spatial interdependence is also most likely to occur. However, researchers using IV models frequently fail to account for spatial interdependence, resulting in asymptotic bias in IV estimates and rendering any claims to casual identification suspect.

We discuss a simple strategy researchers can and should employ to avoid these biases: S-2SLS. This estimation strategy offers few complications for researchers already pursing IV methods, inherits the properties of 2SLS that researchers using IV methods are already familiar with, and helps ensure results are robust to possible spatial interdependence. Our simulations evidence that S-2SLS performs well across a variety of situations, including contexts where only spatial or non-spatial enodgeneity are present, because it nests both 2SLS and the spatial-autoregressive model. As such, S-2SLS presents a more conservative and robust strategy.

There is good reason to believe that spatial bias appears disproportionately often in published IV research. The reason is a variant of the file drawer problem. Plausibly, researchers report IV estimates most often when they improve upon OLS results. Yet, we have shown that with (ignored and) unmodeled spatial interdependence in the outcome, IV estimators commonly produce inflated estimates, leading researchers to prefer those over OLS. We speculate that this results in substantial selection effects: biased IV estimates, driven by unmodeled spatial interdependence, are those which are most likely to be reported. We saw cursory support for this in our replications, where controlling for spatial interdependence in IV models greatly attenuated the effects. We therefore strongly encourage researchers to control for spatial interdependence in their instrumental variable models.

Finally, our results add to growing concerns over spatially dependent instruments. Cooperman (2017), for example, focuses on the difficulties in obtaining appropriate variance estimates, while Betz, Cook and Hollenbach (2018) highlights the tenuous identification assumptions underlying some of these instruments. Yet, while we have identified challenges to credible inference when using observational data – where spatial interdependence and endogeneity concerns often coincidence – we want to emphasize that we do not discourage analyses using these data. Instead, our purposes in this paper are twofold. First, we highlighted the unique problems posed by spatial interdependence for instrumental variable models. In our reading of the literature, these problems have largely been ignored by applied researchers. Second, we want to encourage researchers to consider more carefully the potential drawbacks of instrumental variables. Frequently, instrumental variable estimates are assumed to be superior to results from ordinary least squares. This assumption is often wrong. The estimates obtained from IV models can quickly, and under fairly general circumstances, be worse than what would be obtained with ordinary least squares, even with instruments that are plausibly exogenous. Instrumental variables can, under specific circumstances, identify causal effects. But these circumstances are more limiting than is often realized, which should give researchers some pause in advocating the use of instrumental variable models without careful consideration of interdependence between observations. Where this is not possible, instrumental variables may cause more problems than they solve.

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Supplementary Online Appendix:
Random? As if – Spatial Interdependence and Instrumental Variables

A Additional Plots Simulation: Misspecified W

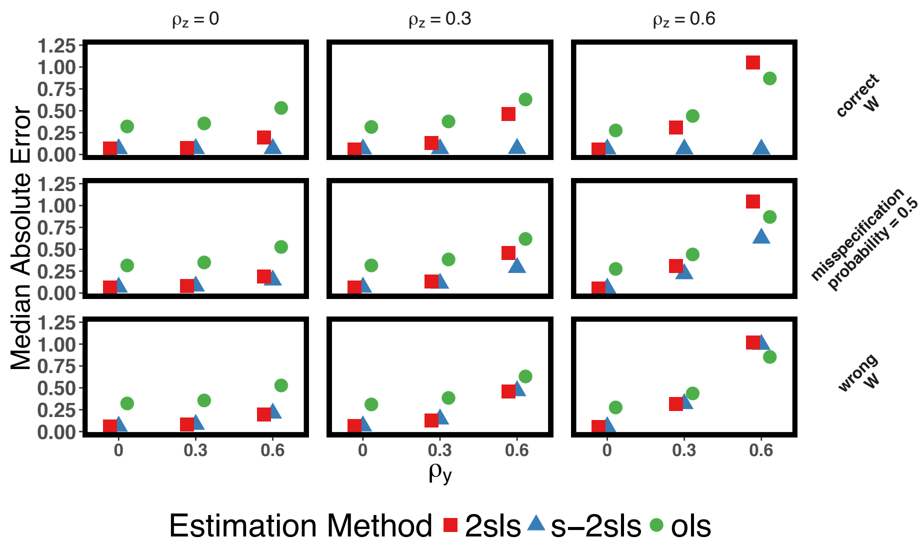

Figure A.1: Median Absolute Error over Misspecification of W ($\lambda = 1.5$ & $\delta = 0.5$ & $N = 200$)

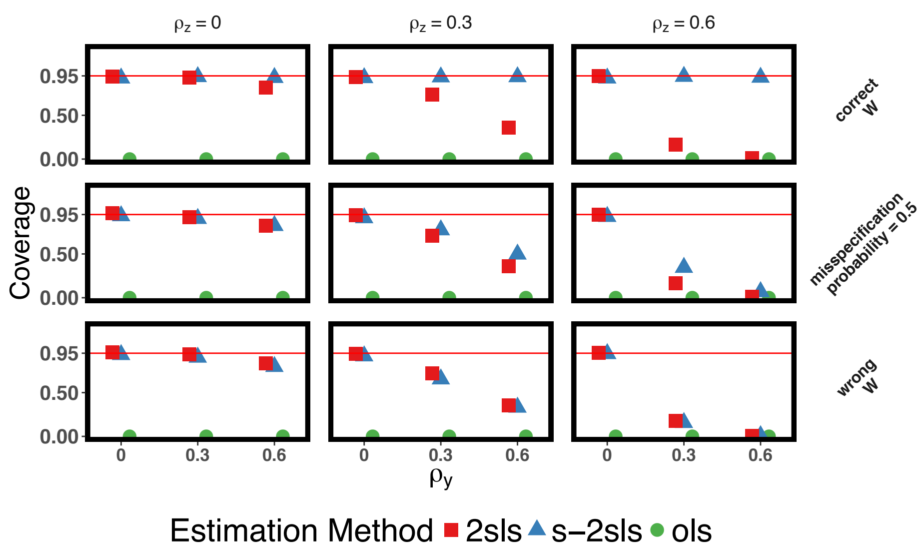

Figure A.2: Coverage over Misspecification of W ($\lambda = 1.5$ & $\delta = 0.5$ & $N = 200$)

B Additional Plots Simulation: MedAE & Coverage

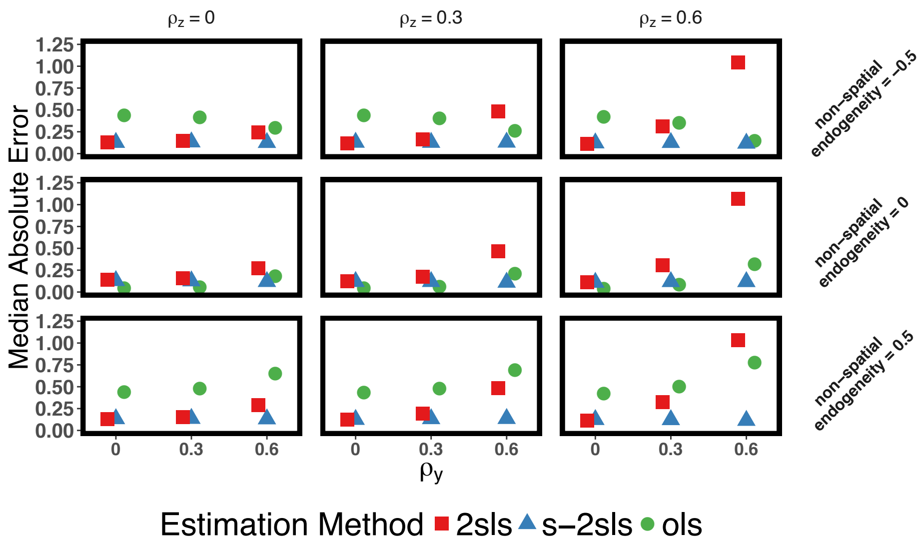

Figure B.3: Median Absolute Error over δ ($\lambda = 0.75$ & $N = 200$)

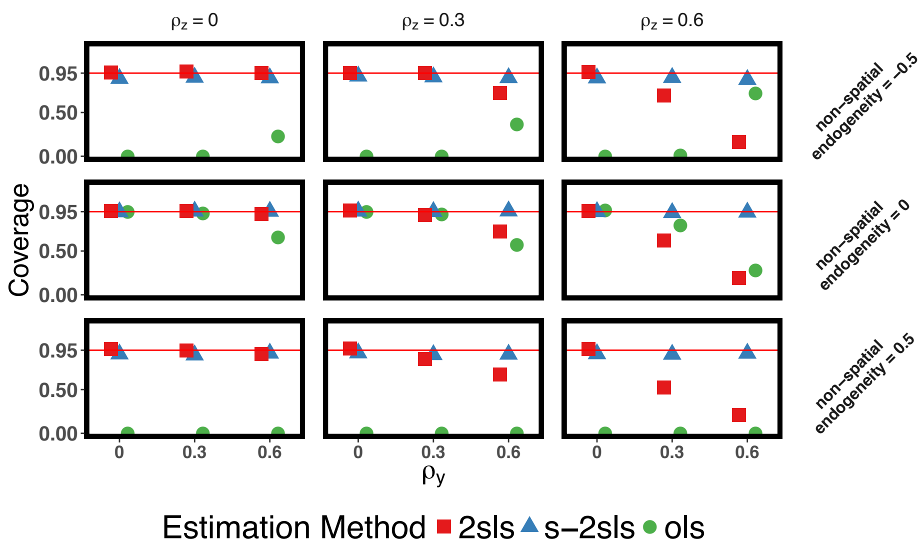

Figure B.4: Coverage over δ ($\lambda = 0.75$ & $N = 200$)

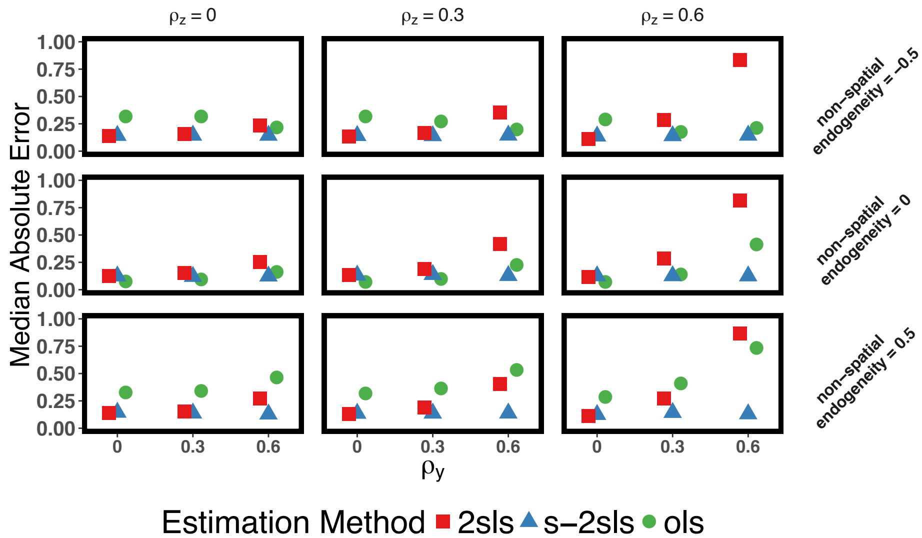

Figure B.5: Median Absolute Error over δ ($\lambda = 1.5$ & $N = 50$)

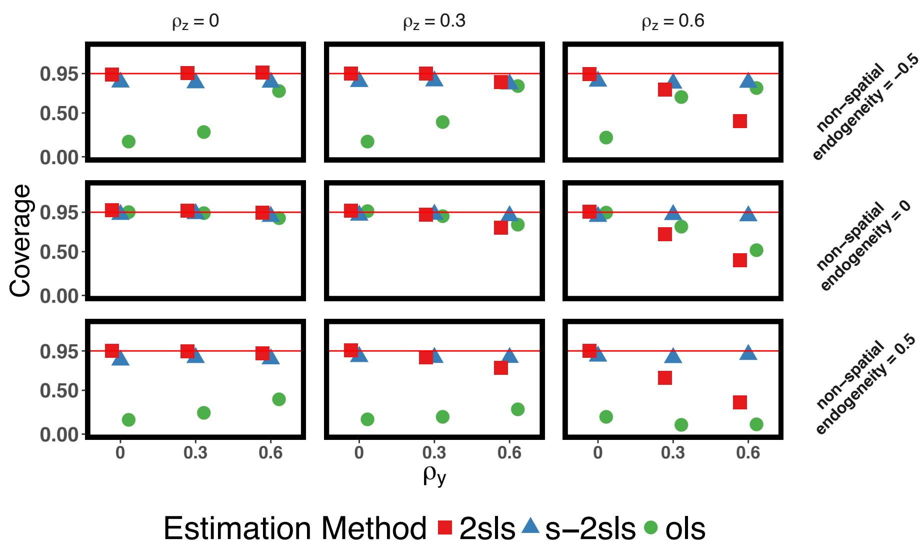

Figure B.6: Coverage over δ ($\lambda = 1.5$ & $N = 50$)

C Additional Plots Simulation: RMSE

Since the RMSE is very sensitive to outliers, we drop any simulation where the absolute error is greater than 10. The only estimation method for which this occurs is standard 2SLS, for which we drop 2217 simulated data sets. Thus, this adjustment actually improves the results for 2SLS.

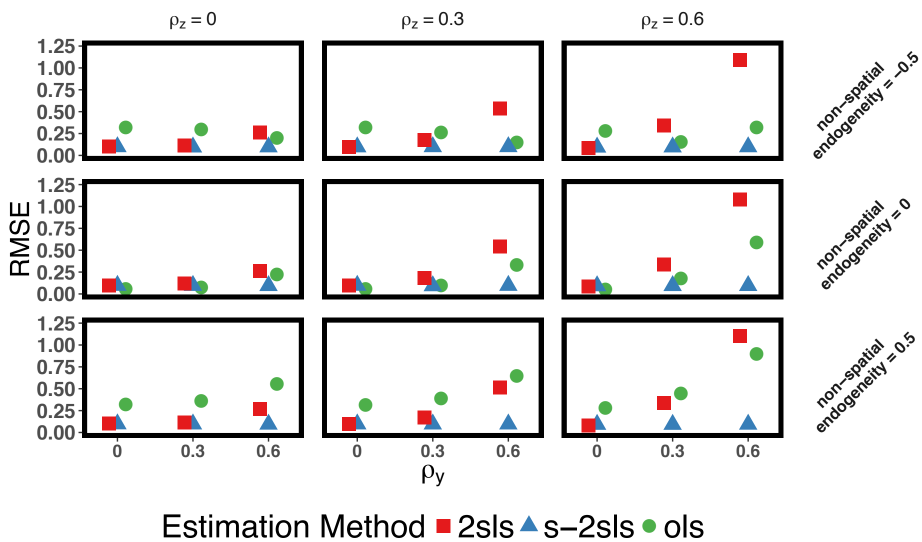

Figure C.7: Rows: δ, non-spatial endogeneity. Columns: ρ_z, spatial pattern of the instrument. Horizontal axis within each plot: ρ_y, spatial interdependence in the outcome. Vertical axis within each plot: RMSE.

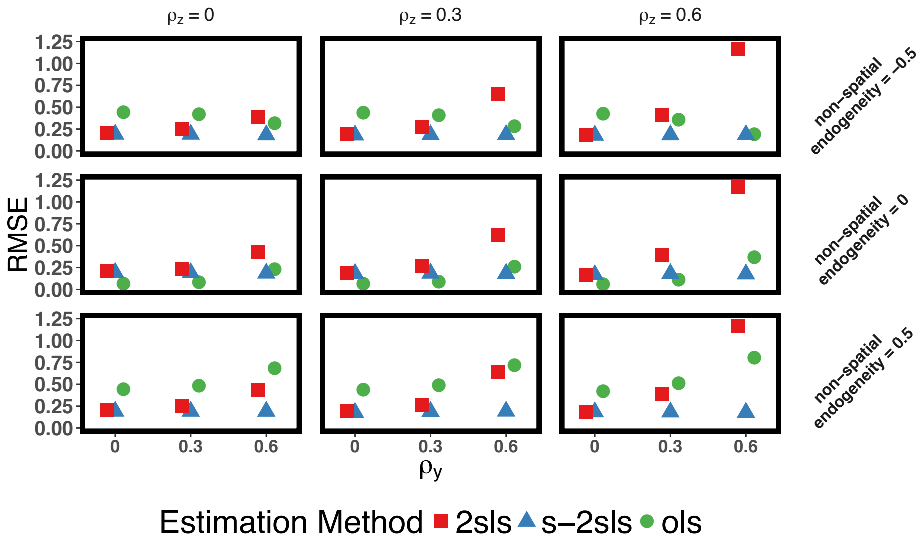

Figure C.8: RMSE over δ ($\lambda = 0.75$ & $N = 200$)

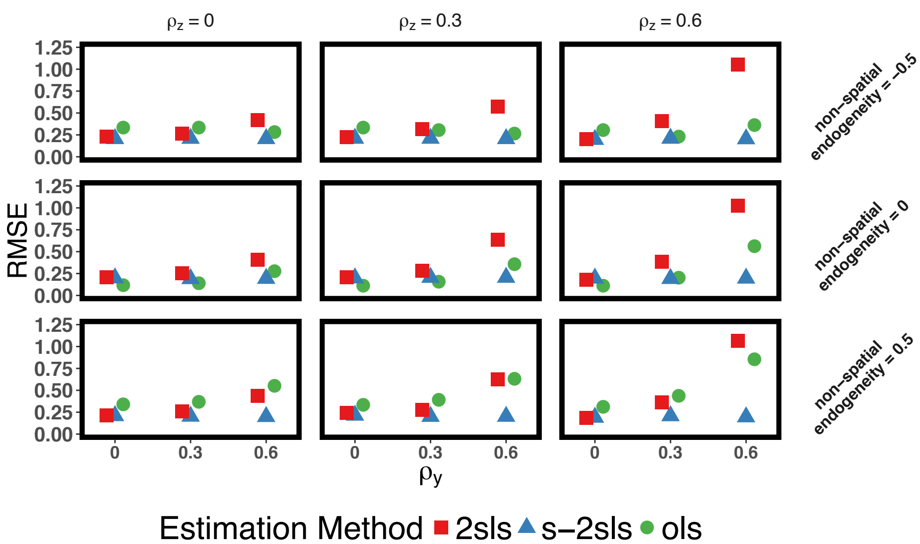

Figure C.9: RMSE over δ ($\lambda = 1.5$ & $N = 50$)

D Additional Tables for Applications

Table D.1: Replication of Robustness Checks, Table 5 Ramsay (2011)

Respective Column Table 5 in Ramsay (2011)	(1) 2SLS	(1) S-SLS	(2) 2SLS	(2) S-SLS	(3) 2SLS	(3) S-SLS	(4) 2SLS	(4) S-SLS	(5) 2SLS	(5) S-SLS	(6) 2SLS	(6) S-SLS	(7) 2SLS	(7) S-SLS
log oil income	-0.358**	-0.0878***	-0.261***	-0.0997***	-0.358**	-0.0725***	-0.358**	-0.0867***	-0.260***	-0.0897***	-0.357***	-0.177***	-0.453*	-0.0989***
per capita	(0.167)	(0.0114)	(0.0837)	(0.00970)	(0.167)	(0.00914)	(0.167)	(0.0113)	(0.0823)	(0.00883)	(0.0769)	(0.0161)	(0.252)	(0.0115)
log gdp	0.356**	0.108***	0.300***	0.129***	0.350**	0.0944***	0.356**	0.106***	0.294***	0.118***	0.371***	0.202***	0.492*	0.122***
per capita	(0.155)	(0.0115)	(0.0914)	(0.0114)	(0.151)	(0.00932)	(0.155)	(0.0113)	(0.0878)	(0.0104)	(0.0767)	(0.0172)	(0.265)	(0.0133)
gdp growth	-0.0118**	-0.00499***	-0.00851***	-0.00503***	-0.0118**	-0.00461***	-0.0118**	-0.00496***	-0.00851***	-0.00482***	-0.0112***	-0.00601***	-0.0127*	-0.00514***
	(0.00518)	(0.00102)	(0.00268)	(0.00103)	(0.00518)	(0.000975)	(0.00518)	(0.00102)	(0.00265)	(0.000998)	(0.00361)	(0.00155)	(0.00655)	(0.00103)
polity at entry	-0.00517	0.564***	0.226	0.548***	0.00684	0.595***	-0.00517	0.567***	0.239	0.567***			-0.0507	0.549***
	(0.373)	(0.0288)	(0.184)	(0.0251)	(0.363)	(0.0242)	(0.373)	(0.0284)	(0.177)	(0.0232)			(0.463)	(0.0271)
top5 oil producers					0.127	-0.0160			0.105*	-0.000276				
					(0.0948)	(0.0224)			(0.0627)	(0.0233)				
cold war dummy							0	-0.327***						
							(.)	(0.0588)						
"west" dummy											-0.101	0.201***	-0.235	0.00790
											(0.165)	(0.0402)	(0.218)	(0.0238)
sub-Saharan Africa													0.462*	0.0949***
Dummy													(0.276)	(0.0233)
Constant	-0.962**	-0.330***	-0.245*	-0.0670	-0.172	-0.0539	-0.210	0	-0.215	-0.0687	-0.318	-0.504***	-0.755	0
	(0.439)	(0.0591)	(0.149)	(0.0608)	(0.170)	(0.0500)	(0.183)	(.)	(0.140)	(0.0517)	(0.205)	(0.102)	(0.535)	(.)
Spatial ρ		0.119***		0.0989***		0.131***		0.119***		0.108***		0.113***		0.130***
		(0.0202)		(0.0214)		(0.0193)		(0.0201)		(0.0208)		(0.0346)		(0.0201)
Observations	1263	1263	1263	1263	1263	1263	1263	1263	1263	1263	1263	1263	1263	1263
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

E Additional Application: "Dynamics and Stagnation in the Malthusian Epoch"

In this section of the Appendix, we provide an additional application. In a 2011 article in the *American Economic Review*, Ashraf and Galor (2011) aim to test a central prediction of the famous Malthusian theory. Thomas R. Malthus (1798) argues that the main reason for stagnating incomes, prior to the industrial revolution, is that when incomes increase, population size rises as well. Since resources are limited, higher populations induce declining living standards. As a result, technological progress or the discovery of new resources only temporarily improves living standards, but does not produce sustained gains (Ashraf and Galor, 2011). As Ashraf and Galor (2011, p. 2004) outline, their article "exploits exogenous sources of cross-country variation in land productivity and technological levels to examine their hypothesized differential effects on population density versus income per capita during the time period 11500 CE." To test the Malthusian theory in pre-industrial societies, Ashraf and Galor (2011) investigate two predictions: 1) a country's improvements in productivity should lead to larger populations, but not higher living standards; and 2) countries with higher land productivity, or better technology, should have higher population densities, but again, should not be significantly richer.

In their empirical analysis, Ashraf and Galor (2011) use the timing of the onset of the neolithic revolution to proxy for technological change. Consistent with their expectations, the authors show that both the onset of the neolithic revolution and land productivity are positively (and significantly) associated with population density, but not with income per capita. In addition, Ashraf and Galor (2011) use instrumental variables to estimate the causal effect of technological progress on population density. They argue that "prehistoric biogeographical endownments," in particular the "availability of domesticable species of plants and animals," have had an important effect on technological progress and are otherwise exogenous (Ashraf and Galor, 2011, pp. 2029-2031). The use of the instrumental variable is primarily motivated by the authors to estimate the "causal impact of technology on population density" (Ashraf and Galor, 2011, p. 2031).

However, the authors ignore possible spatial interdependence in both the instrumental variables and the dependent variable. Both population density and natural wildlife are likely to be spatially clustered. In other words, it is likely that the animal and plant species found in one country are similar to those in adjacent regions. Likewise, in pre-historic times (i.e., 1000 CE), it is likely that some parts of the planet had higher population density than others, again reflecting positive spatial correlation. This does not mean that these variables are similarly clustered in space, but rather that by themselves they might exhibit (positive) spatial dependence. If correct, this would induce bias in their IV models for the reasons outlined above.

To test this, we first need to specify an appropriate weights matrix. Here, we create a binary-contiguity matrix, with neighbors defined as having adjacent borders.[29] As a preliminary test of spatial autocorrelation, we estimate Moran's I based on the residuals of the original OLS model – with logged population density in 1000 CE as the dependent variable (column 2, Table 9 in Ashraf and Galor (2011)). Based on a Moran's I value of 0.4 with an associated p-value smaller than 0.001, we are able to reject the null of independence of the residuals.

Table E.2 shows the results of replicating the models with population density in 1000 CE (Table 9 in Ashraf and Galor (2011)). Column 1 replicates the original OLS model on the restricted sample (column 2 in Table 9 in Ashraf and Galor (2011)). As a first step, column 2 in Table E.2 shows the results when we estimate a spatial autoregressive (SAR) model instead of the standard OLS model. As one can see, the main coefficients of interest (technological index) have the same levels of significance as in the original OLS results. The effect estimates, however, are quite different. For the linear-additive model (as in Column 1), the direct effect is simply the reported coefficient estimate on the log technology index (4.198). The average direct effect of the SAR model – calculated as above – is 3.096 – larger than the coefficient estimate for SAR (due to expected feedback effects), but still substantially smaller than the OLS effect estimates of 4.198.

Columns 3 and 4 replicate the instrumental variable model for population density in 1000 CE as presented in Table 9 in Ashraf and Galor (2011). The differences in results between the original

[29]We have also replicated the results with a k(=5) nearest-neighbor matrix or a row-standardized contiguous neighbor matrix.

2SLS model and the spatial 2SLS model are stark. The coefficient on technological progress (log of technological index) in the original 2SLS model is 14.53, almost 3.5 times as large as the OLS coefficients. Ashraf and Galor (2011) argue that the difference in estimated coefficients is “a pattern that is consistent with measurement error in the transition-timing variable and the resultant attenuation bias afflicting OLS coefficient estimates” (Ashraf and Galor, 2011, p. 2031). Column 4, however, shows the results from the model estimated with S-2SLS. Here the coefficient for technological progress is much smaller compared to 2SLS, with the average direct effect – calculated as before – being 4.810. In fact, the average effect estimate of technological progress in the spatial 2SLS model is comparable to that in the original OLS estimates. Recall, that, as we show above, the non-spatial and spatial bias in OLS can be offsetting. This may be the case here. If the non-spatial measurement bias is attenuating and the spatial bias is upward, the OLS model ends up being less biased than the 2SLS model due to the countervailing forces of both biases on the coefficient estimate.

We note that the overall conclusion of Ashraf and Galor (2011) still stands.[30] The Malthusian theory for pre-industrial times is supported by these data. On the other hand, the causal effect of technological progress on population density is smaller than the standard 2SLS model indicates and is about the same size the original estimates in the OLS models in Ashraf and Galor (2011).

[30]In the Appendix we also replicate the results using population density in 1CE as the dependent variable (Table E.3), producing similar results.

Table E.2: Replication of Table 9 (1000 CE) in Ashraf and Galor (2011)

	(1) Original OLS	(2) SAR	(3) Original 2SLS	(4) S-2SLS
Log technology index in relevant period	4.198^{***}	2.856^{***}	14.53^{***}	4.303^{***}
	(1.164)	(0.953)	(4.437)	(1.328)
Log land productivity	0.498^{***}	0.397^{***}	0.572^{***}	0.397^{***}
	(0.139)	(0.0963)	(0.148)	(0.0987)
Log absolute latitude	-0.185	-0.093	-0.209	-0.086
	(0.151)	(0.106)	(0.209)	(0.108)
Mean distance to nearest coast or river	-0.363	-0.341	-1.155^{*}	-0.462
	(0.426)	(0.360)	(0.640)	(0.368)
Percentage of land within 100 km of coast or river	0.442	0.472	0.153	0.431
	(0.422)	(0.341)	(0.606)	(0.344)
Constant	-1.820^{***}	-1.286^{**}	-5.507^{***}	-1.796^{***}
	(0.641)	(0.531)	(1.702)	(0.630)
Spatial ρ_y		0.151^{***}		0.169^{***}
		(0.0246)		(0.0334)
Observations	92	92	92	92
Continent dummies	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table E.3: Replication of Table 9 (1 CE) in Ashraf and Galor (2011)

	(1) Original OLS	(2) SAR	(3) Original 2SLS	(4) S-2SLS
Log technology index in relevant period	3.947***	3.369***	10.80***	3.010***
	(0.983)	(0.760)	(2.857)	(0.978)
Log land productivity	0.350**	0.311***	0.464**	0.294***
	(0.172)	(0.106)	(0.182)	(0.105)
Log absolute latitude	0.0834	-0.0152	-0.0521	-0.0505
	(0.170)	(0.115)	(0.214)	(0.114)
Mean distance to nearest coast or river	-0.625	-0.300	-0.616	-0.175
	(0.434)	(0.394)	(0.834)	(0.388)
Percentage of land within 100 km of coast or river	0.146	0.0986	-0.172	0.0867
	(0.424)	(0.357)	(0.642)	(0.351)
Constant	-2.719***	-1.749***	-4.770***	-1.334**
	(0.601)	(0.500)	(0.980)	(0.544)
Spatial ρ		0.182***		0.252***
		(0.0275)		(0.0358)
Observations	83	83	83	83
Continent dummies	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$