Public Spending in Autocracies: Evidence from Prussian Cities*

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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

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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

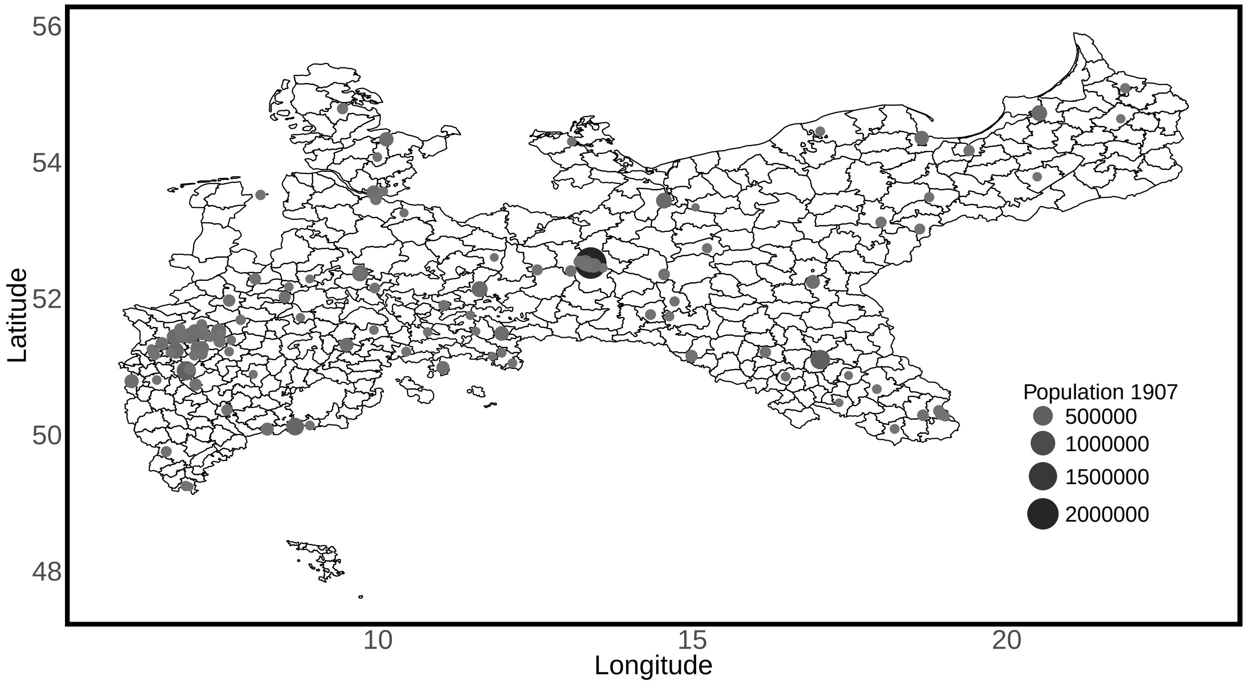


Figure 2: The plot shows the location of all cities (observations) in the data set and their respective populations in 1907.

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.

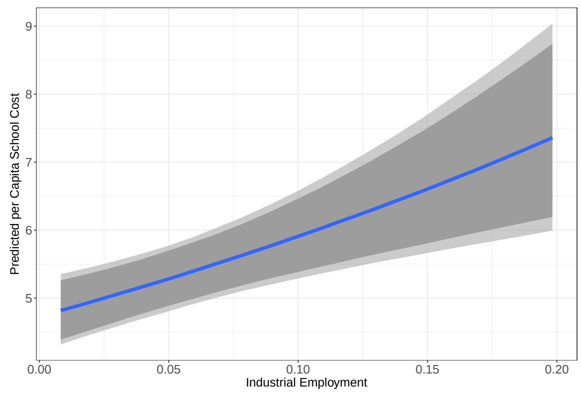
Based on the theoretical argument, we would expect industrial employment to have a positive association with school expenditure (model 1) and school enrollment (model 2), as well as a negative association with class size (model 3). As Table 1 shows, as expected, the estimated effect of industrial employment 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.

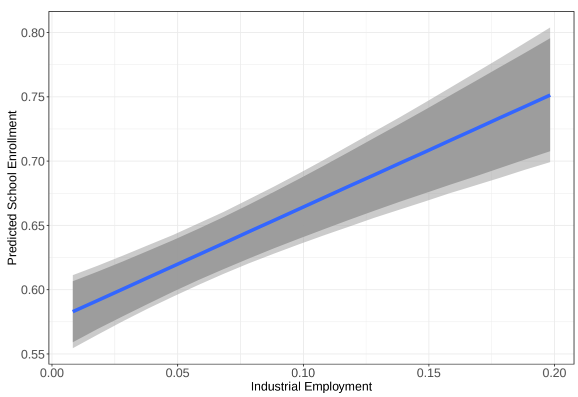
Figure 3 shows the marginal effects of industrial employment for each of the models displayed in Table 1 above. To calculate the marginal effect, all other covariates are held constant at their sample medians. The left-most plot shows the marginal effect of industrial employment on per capita school 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

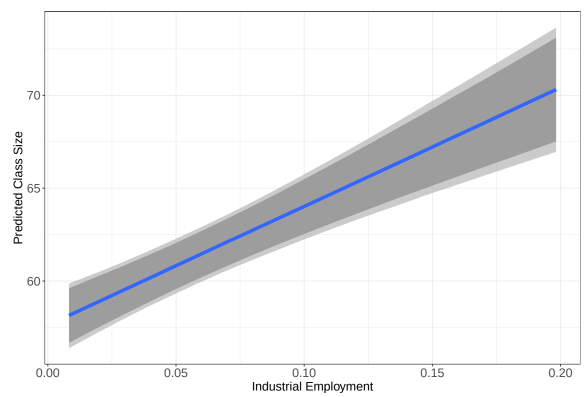
	Dependent variable:		
	per Capita School Cost (1895)	School Enrollment (1905)	Class Size (1896)
	(1)	(2)	(3)
Income Gini	6.526***	1.013	15.258
	(2.463)	(0.635)	(39.921)
Avg Income	−0.004***	−0.001**	−0.026
	(0.001)	(0.0004)	(0.023)
Log Pop	0.064	0.002	−0.791
	(0.047)	(0.012)	(0.757)
Taxes pC	0.045*	0.002	−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

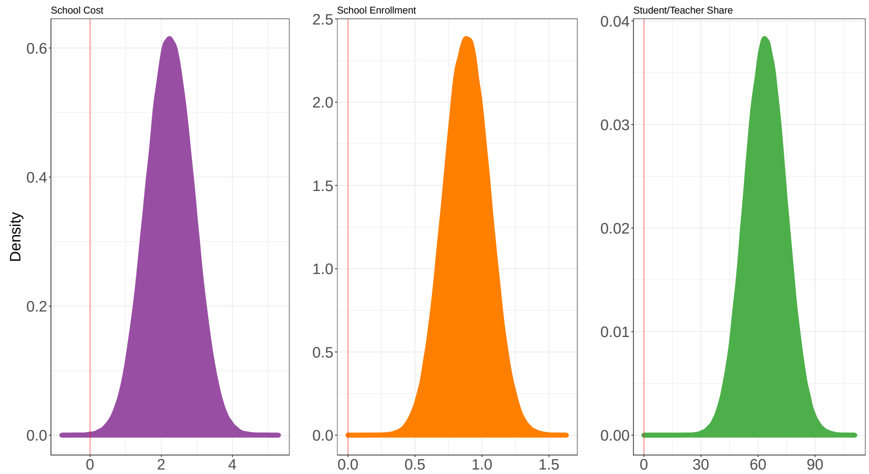


Figure 4: The plot shows the distribution of the coefficient estimates for industrial employment based on 500 sampled data sets. For counties with multiple cities, only one observation is drawn to be included in the data for each of the 500 models. Based on the coefficient estimate and the standard error, $1,000$ 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.

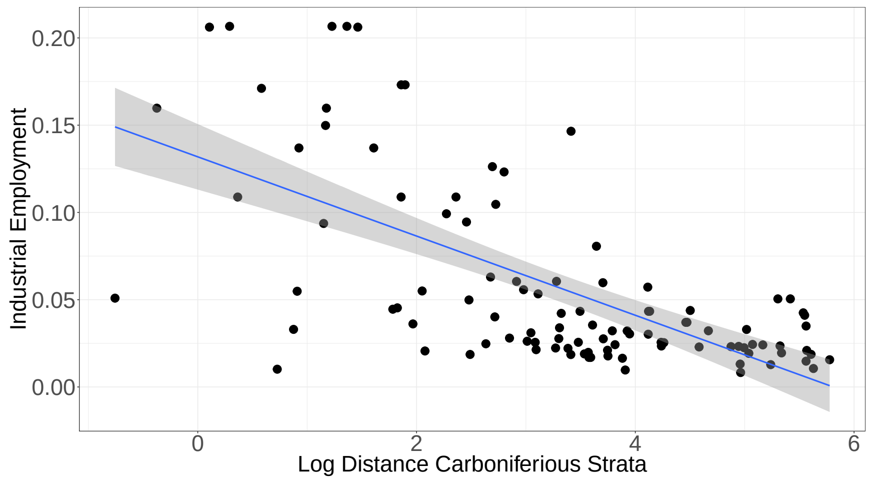


Figure 5: The plot shows the relationship between the potentially endogenous variable (industrial employment) and the instrument used (logged distance to closest carboniferous area).

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		