Gradual Franchise Extensions and Government Spending in Nineteenth-Century England

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Short title: Gradual Franchise Extensions and Government Spending

Abstract

This paper investigates the effect of extending the franchise on government spending on public goods in nineteenth-century England and Wales. The effect of franchise extension is identified by exploiting extensive regional and temporal variation in the right to vote in municipal elections between 1867 and 1900. Semi-parametric regressions show robust evidence of an inverted-U-shaped relationship: extending the right to vote from the rich to the middle class led to increased spending, but further franchise extensions—beyond around 50% of the adult male population—led to lower expenditure. Further, government spending was lower in towns where the poor were enfranchised by national reforms. A simple model shows that the inverted-U-relationship can be explained by the trade-offs between public spending and private consumption faced by poor voters. These results suggest that enfranchising the poor may lead to smaller government, in contrast to the predictions of many theoretical models.

Keywords: Democratization, Public Goods, Infrastructure, Franchise, Nineteenth-century Britain, Poor voters

*I am grateful for financial support from NSF grant No. 1357995. Supplementary material for this article is available in the appendix in the online edition. Replication files are available in the JOP Dataverse (https://dataverse.harvard.edu/dataverse/jop). The empirical analysis has been successfully replicated by the JOP replication analyst.

1 Introduction

Investment in critical public goods such as clean water and sewage systems remains insufficient in many countries (Günther and Fink, 2011). It is often argued that democratization, decentralization, and increased political participation can help solve these issues, through increased support for redistribution or overcoming elite capture. But severe public health challenges remain even in long established democracies, and in practice bringing public service provision "closer to the people" has hindered sustainable investment in sanitation (Herrera and Post, 2014).

This paper argues that enfranchising the poor may reduce government spending on public goods. In classic models of democratization, granting the right to vote to poorer citizens leads to higher government spending due to demands for redistribution from the wealthy (Boix, 2003; Acemoglu and Robinson, 2006). However, the same argument may not apply to government provision of public goods, where voters must trade off higher government spending with lower private consumption. Poorer citizens may value additional private consumption particularly highly—if, for instance, they require extra food—and so may vote for lower taxation and government spending. A simple model, presented in Appendix A, formalizes this intuition and predicts that the relationship between the franchise and government spending on public goods is inverted-U-shaped: extending the right to vote from the rich to the middle class increases spending, but further extensions lead to lower expenditure.

The paper investigates the effect of extending the franchise in nineteenth-century British towns and presents two empirical analyses supporting the theoretical prediction.[1] First, semi-parametric regressions show clear evidence that the relationship between the extent of the franchise and the size of government is inverted-U-shaped. Second, both tax receipts per capita and expenditure on public goods were lower in towns where national reforms led to poor citizens gaining the right to vote. There is thus clear evidence that extending voting rights to extremely poor households can inhibit investment in critical public infrastructure.

[1]The empirical data has been successfully replicated by the JOP replication analyst.

2 Empirical Context and Data

The empirical analysis investigates the effect of extending the franchise using a new annual dataset of British towns between 1867 and 1900. These towns were governed by autonomous, locally elected councils. Importantly, these councils held responsibility for spending on urban public goods and services but could not legally provide education or welfare.[2] Further, in the absence of central government grants, town spending had to be funded locally, predominantly through taxes. Town councils were locally elected, with extensive cross-sectional and temporal variation in the municipal franchise. I construct a dataset combining town-level information on the franchise with annual data from town accounts, and flexibly model the effect of gradually extending the right to vote.[3]

The detailed franchise data offers an important advance on a previous study, Chapman (2018), offering an alternative test of the theoretical mechanism analyzed here. The earlier study introduces and tests a simplified version of the model in Appendix A, and finds that an 1894 democratic reform led to lower levels of town council spending on public goods, relative to towns that were democratized at an earlier date—with the effect strongest where pre-reform local elites were predominantly middle class. The franchise data that is used in this paper allows me to isolate the effect of enfranchising the poor, and enables a direct test of the hypothesis that the relationship between the extent of the franchise and the size of government is inverted-U-shaped.[4] More generally, the data offers a rare opportunity to

[2]Sanitation—spending on water supply, sewers, and streets—was by far the largest component of public goods expenditure. These public goods contributed significantly to Britain's mortality decline (Chapman, 2019, 2022).

[3]See Appendix A for detailed discussion of how the historical context matches the theoretical argument. Appendix B provides full details of data sources and variable construction.

[4]Appendix A addresses potential alternative mechanisms for an inverted-U-shape, such as party politics or apathy from the poor, using evidence from political debates reported in local newspapers. Aidt, Daunton, and Dutta (2010) use similar data to mine but find different results. A replication analysis in Appendix F demonstrates that the difference is

study the effect of gradually extending voting rights, rather than a reform that enfranchises large swathes of the population simultaneously (e.g., Husted and Kenny, 1997).

The empirical analysis exploits extensive heterogeneity in the extent of the municipal franchise over time and across towns—see the left-hand panel of Figure I. This heterogeneity resulted from a combination of local and national factors. National legislation established the basic framework for voting rights. From 1835 onward all resident male heads of household could, in principle, vote in municipal elections. In practice, however, many citizens were disenfranchised by a combination of lengthy residence and tax-paying requirements, filtered through idiosyncrasies in local customs and implementation of voting law. Over time, a series of reforms lightened the conditions on voting rights, broadening the franchise nationally, and reducing—but not removing—the cross-sectional variation in the franchise.

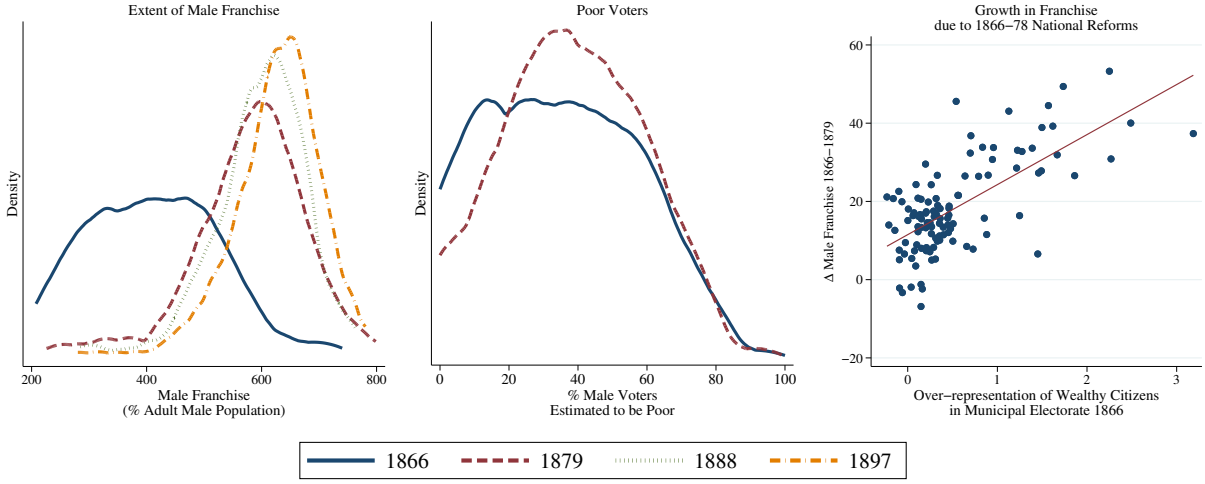
Critically for interpreting the results, a broader male franchise reflects the enfranchisement of relatively poor voters, as shown in the middle panel of Figure I. Poor citizens were more likely to fail head of household and residency requirements, due to living in cramped accommodation and moving frequently. Further, poor renters were often asked to pay taxes indirectly through their landlords—a practice known as "compounding"—leading to legal ambiguity in whether they had met the tax-paying conditions required to vote. As a result, whether these so-called "compounders" were enfranchised depended on the decisions of local authorities who, importantly, were governed independently of the town councils that determined municipal spending.

The share of the poor in the electorate increased significantly due to a series of reforms between 1869 and 1878—see the right-hand panel of Figure I. Reforms in 1869, consolidated in 1878, reduced the length of residence and tax-paying requirements by two years, and enshrined compounders' voting rights in law. Women also gained the right to vote in 1869, although the restriction to heads of household meant that they remained a small proportion of the electorate. Smaller reforms continued throughout the nineteenth century, but the

explained by the larger, more comprehensive, dataset used in this paper.

basic framework for voting rights remained unchanged until the first world war.

Figure I: The extent of the franchise varied over time and within individual cross-sections.



Note: The franchise grew considerably between 1866 and 1879 due to reforms to national legislation governing voting rights. The reforms led to poor voters becoming a larger share of the electorate (middle panel), with larger franchise expansions occurring where the "wealthy" were over-represented in the pre-reform municipal electorate (right-hand panel). Wealthy is defined by having the right to vote in Parliamentary elections, which generally required meeting property value thresholds. See Appendix B for detailed variable definitions.

The following two sections take advantage of this historical context to test the theoretical predictions. I first use the whole panel dataset, and flexibly model the relationship between the extent of the franchise and the size of government. Section 4 then isolates the effect of enfranchising very poor citizens, exploiting the "shock" of the 1869–1878 reforms.

3 The Extent of the Franchise and Government Spending

This section tests the theoretical prediction that the relationship between the extent of the franchise and government spending on public goods is inverted-U-shaped. To do so, I allow for a flexible relationship between the franchise and the size of government while assuming a linear relationship with other town characteristics. That is, I estimate:

$$y_{i,t} = \alpha + g(franchise_{i,t}) + \beta X_{i,t} + \gamma_0 Z_i + \delta T_t + \epsilon_{i,t} \quad (1)$$

where i indexes towns, t indexes year, $g(\cdot)$ is a function to be estimated, and ϵ is an error term. Dependent variables are (per capita) tax receipts and public goods expenditure. X is a vector of time-varying controls. Z and T refer to town- and year- fixed effects. In order

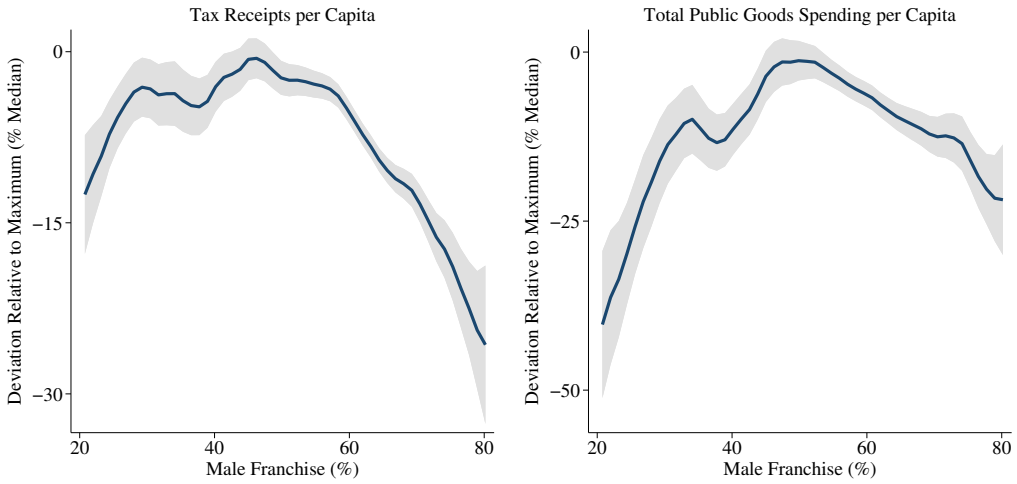
to focus on extensions of the right to vote to poorer citizens, and avoid possible confounds from changes in the gender composition of the electorate, the main franchise variable is the male franchise.

The identification assumption underpinning this analysis is that the variation in the franchise is exogenous, conditional on controlling for town- and year-fixed effects and demographic characteristics. These controls account for factors that may affect the franchise through the national regulations governing voting rights and also be associated with the demand for public goods—for instance, rapid population growth may require urgent investment in sanitation, while also leading to a low franchise due to citizens' failing residence requirements. However, after controlling for these factors, the residual variation in the extent of the franchise is plausibly exogenous since it resulted from a mixture of nationally-imposed reforms and idiosyncratic decision-making by local authorities that were governed separately to town councils. This identification strategy is justified in detail in Appendix C.

Figure II presents the results of estimating $g(\cdot)$ semi-parametrically. That is, the relationship between the male franchise and the dependent variables is shown after "purging" both fixed effects and time-varying controls, using the method of Baltagi and Li (2002). There is clear evidence of the inverted-U-shape relationship predicted by the model, with spending and taxation highest when approximately 50% of the adult male population held the right to vote. This level of the franchise represents approximately the median level prior to the reforms of 1869 and approximately the 25th percentile immediately following the reforms. The magnitude of the relationship is quite large, with extensions beyond the maximum leading to a reduction of up to around 15% of the median tax receipts per capita, and 25% of the median spending per capita.

Figure III shows that the inverted-U-shaped relationship is robust to the inclusion of alternative sets of controls. The shape of the relationship is clear even when including only year-fixed-effects to account for the general trend of increased spending (left-hand panel). Once town-fixed-effects are included the effect of extending the franchise beyond the 50%

Figure II: Semi-parametric regression shows inverted-U-shaped relationship between franchise and both per capita tax receipts and expenditure.



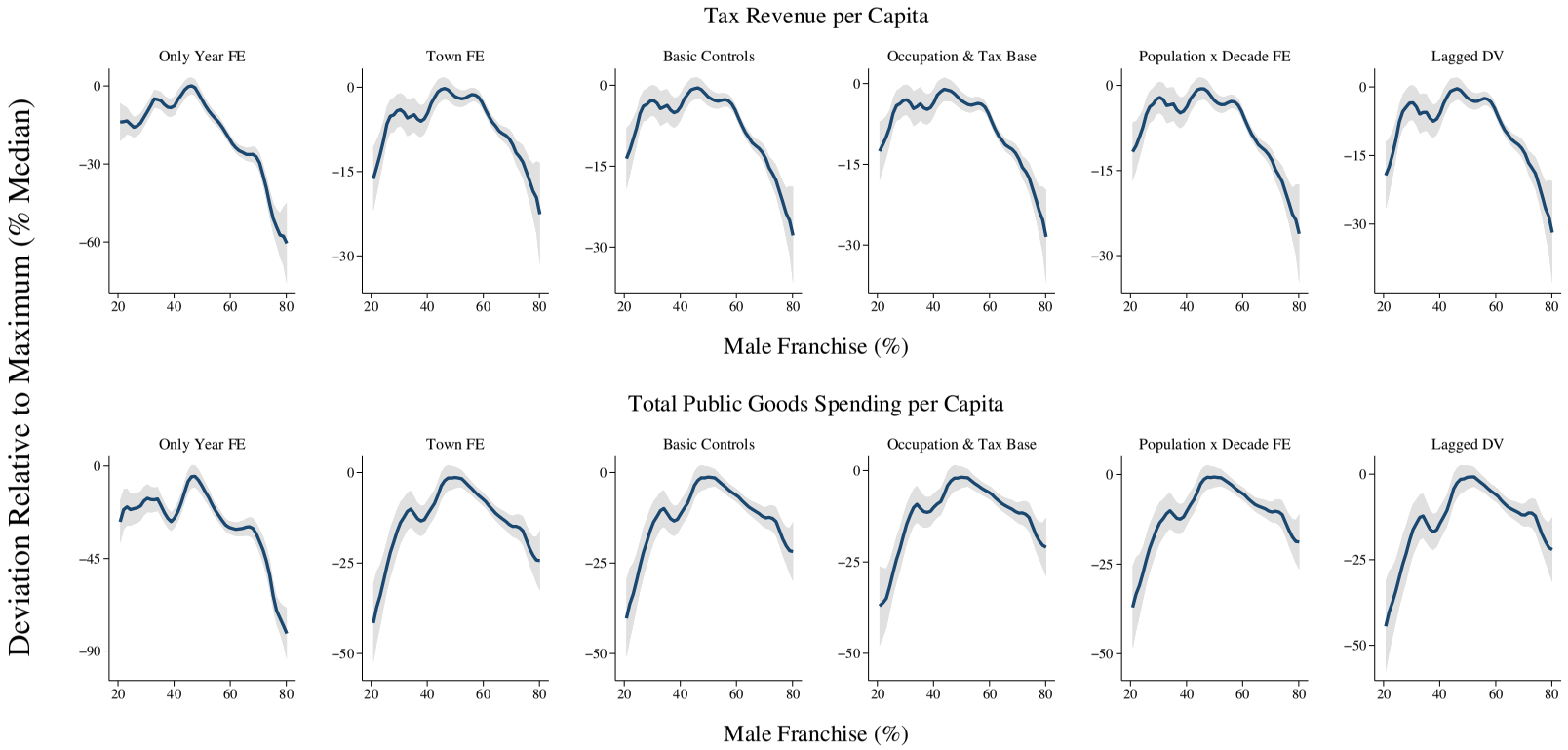
Note: Figure displays the nonlinear part of the partially-linear model estimated using method of Baltagi and Li (2002), plotted with Nadaraya-Watson regression. The sample includes 4,810 observations across 150 towns. Each specification controls for year- and town-fixed effects, population, population growth, urban crowding, and the female franchise. The y-axes reflect the reduction in taxation/spending at each level of the franchise, relative to the maximum point, displayed as a percentage of the median.

mark is less marked (notice the differing y-axis scale) but still large. The fourth panels in each row add town tax base per capita (a measure of wealth) and town occupational structure, both of which could affect the demand for public goods. The remaining panels include population–decade fixed effects to capture any possible differential take-up of new public goods according to town size and, finally, lagged dependent variables to account for historical investment and potential persistence in expenditure. The estimated effect of the franchise is similar when incorporating these additional variables, with the theoretical prediction strongly supported in all instances.

4 The Effect of Enfranchising the Poor

The results in the previous section show clear support for the inverted-U-shaped hypothesis, but they do not pin down the attitudes of the poorest citizens to increased public spending. The next analysis thus directly tests the prediction that enfranchising the poor reduced the size of government, exploiting the shock to the municipal electorate caused by the 1869–1878 national reforms to the franchise regulations.

Figure III: Inverted-U-shaped relationship is robust to inclusion of alternative controls.



Note: This figure displays the nonlinear part of the partially linear model estimated using the procedure of Baltagi and Li (2002) (Robinson (1988) when town-fixed-effects are not included). The sample includes 4,810 observations across 150 towns. The y-axes reflect the reduction in taxation/spending at each level of the franchise, relative to the maximum point, displayed as a percentage of the median of the relevant dependent variable in the sample. "Occupational controls" include the % in agriculture, the % in commerce, and the % of adult men that are heads of household. "Population x Decade FE" allows for differential time trends for each population category.

Specifically, I compare the size of government in towns where the reforms enfranchised additional poor voters, to those where no new poor voters were added to the voting register. To do so, I estimate the proportion of poor voters in each town, using information on the distribution of household rental values in 1866. I then define a variable $poorEnfranchisedByReform$, which equals one for towns where the estimated share of poor voters in the electorate increased between 1866 and 1879, and 0 otherwise. I then estimate:

$$y_{i,t} = \beta_1 poorEnfranchisedByReform\_x\_1[year \geq 1873]_{i,t} + \phi X_{i,t} + \gamma_0 Z_i + \delta T_t + \epsilon_{i,t} \quad (2)$$

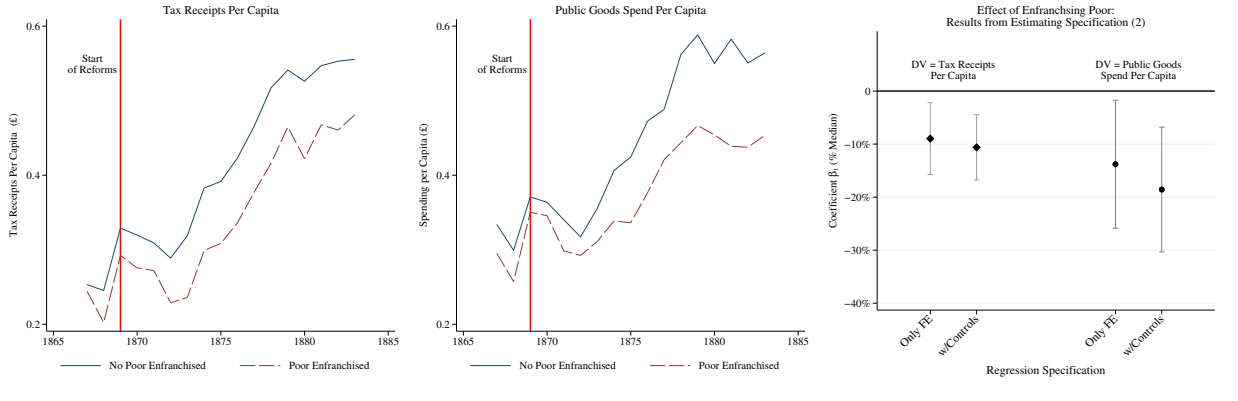
where indices and variables are defined as in Specification 1. If, as predicted, enfranchising the poor reduced spending then coefficient $\beta_1 < 0$—tax and spending will be lower once the reforms take effect, relative to the group of towns without any new poor voters.

As well as being of intrinsic interest, this analysis also provides reassurance that we are identifying causal effects of franchise extension. The identification strategy underpinning the analysis in the previous section relies on the assumption that local decision-making affecting the franchise was (conditionally) exogenous to contemporaneous town spending decisions. By studying the national reforms, which are plausibly exogenous to each town, we need assume only that local authorities could not shape the *expansion* of the electorate caused by the reforms, and that the decisions affecting the pre-reform franchise were exogenous to *future* tax and spending outcomes.[5]

Towns where the poor were enfranchised by the reforms of 1869–1878 saw lower growth in both tax revenue and expenditure, as shown in Figure IV. There is clear evidence, in the first two panels, that the paths of taxation and spending diverged after the reforms started in 1869. The right-hand panel provides a formal test of this claim, by displaying the results from estimating Specification 2. Towns in which poor citizens were enfranchised experienced

[5]Appendix C presents further evidence that the relationship is causal, including an analysis in which the interaction between the pre-reform treatment of compounders and the reduction in the length of residence requirements serves as a source of exogenous variation in whether the 1869–1878 reforms increased the share of poor voters.

Figure IV: Enfranchisement of poor voters led to slower growth of government.



Note: The first two panels display the mean values within each group. The right-hand panel displays the coefficient β_1 from estimating Specification 2—see Appendix E for further details—with variables scaled as a percent of the sample median for comparability with Figure II. "Only FE" refer to specifications with town- and year-fixed effects, while the specifications "w/Controls" add controls for population, population growth, and urban crowding. Bars represent 90% confidence intervals.

lower growth in both tax receipts and government spending on public goods after the reforms, with similar results with and without including sociodemographic control variables.

5 Implications

These findings have important implications for our understanding of the relationship between democratization and government spending. In contrast to the predictions of leading models in political economy, the extension of the franchise in this context cannot be explained by demands for greater government spending by either the poor (Acemoglu and Robinson, 2006) or the middle class (Lizzeri and Persico, 2004). Instead, the results suggest that the poor may align with the wealthy to oppose larger government, and so provide a possible explanation for elites implementing mass, rather than gradual, enfranchisement—Disraeli's support for the Second Reform Act provides a particularly apposite example.

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Online Appendix For Gradual Franchise Extensions and Government Spending in Nineteenth-Century England

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A Model

This appendix presents a simple model predicting the inverted-U-shape relationship between the extension of the franchise and the size of government. Specifically, if local governments impose linear taxes and cannot utilize transfer payments, then both poor and the rich will desire lower government expenditure on public goods than the middle class. In contrast to many previous models, I assume that towns controlled expenditure over public goods, but could not undertake redistributive transfer payments.

The key insight is that an inverted-U-relationship can be generated through a simple, and intuitive, assumption over the utility for consumption. Previous authors have noted that demand for public services may be increasing in income, but have either not discussed the possibility of an inverted-U-relationship (Husted and Kenny, 1997; Kenny, 1978), or have focused on the role of private provision in explaining the opposition of the rich to expenditure (Epple and Romano, 1996a,b). Other papers have argued that poor voters may vote against higher spending because of the composition of public services (Ansell and Samuels, 2010; Llavador and Oxoby, 2005), or due to their social identity (Seghezza and Morelli, 2019). The model here, in contrast, shows that low income is sufficient to predict that franchise extensions will reduce government spending—even in the presence of progressive taxation—in certain settings.

The first subsection introduces the theoretical framework, assuming a proportional tax rate. The second subsection then presents two theoretical propositions, predicting the inverted-U-shaped relationship included in the main text. The third subsection presents an extension to a situation of progressive taxation, demonstrating that the insight from the model could extend to a broader range of contexts. The remaining fourth subsection explains why the historical context is a close match with the theoretical assumptions. The fifth subsection provides historical evidence that the poor opposed spending on public goods, and the final subsection addresses alternative possible mechanisms for the finding of an inverted-U shape relationship.

A.1 Theoretical Framework

Consider an individual i who receives utility from private consumption and from expenditure on a local public good G. Utility from the public good is dependent on the per capita level of expenditure $g = \frac{G}{N}$, where N is the town population. Individuals receive an income y_i, with aggregate income denoted by Y. The tax rate and government spending are set by a politician chosen through a standard two-candidate simple majority election, in which candidates' promises are binding.

A critical assumption in the model is that all voters pay the taxes that fund the public good. Taxes are implemented through a linear tax rate—an assumption that meets the historical context—leading to a government budget constraint of $G = \tau Y$. Appendix A.3 shows that the main proposition holds with progressive taxation. Consequently, the model has implications for any setting in which the poorest bear some of the burden of paying for public goods.

The utility of individual i is given by:

$$U_i = u(c_i) + v\left(g\right)$$

where c_i denotes i's private consumption. Assume u and v are strictly concave, twice continuously differentiable, $\lim_{x\to 0} u'(x) = \lim_{x\to 0} v'(x) = \infty$, and that the returns to the public good are exhausted at some point: there is some $\hat{G} < Y$ such that $v'\left(\frac{\hat{G}}{N}\right) = 0$.

In addition, assume the following conditions on the coefficient of relative risk aversion for $u(c)$, $r_R(c,u) = -c\frac{u''(c)}{u'(c)}$.

1. $\frac{\partial r_R(c,u)}{\partial c} < 0$.
2. $\lim_{c\to 0} r_R(c,u) > 1$ and $\lim_{c\to\infty} r_R(c,u) < 1$.

These assumptions state, essentially, that poor individuals are very sensitive to reductions in consumption, but that this is less true of the wealthy. Intuitively, poor households may be unwilling to gamble since any loss means more to them.

A.2 Theoretical Predictions

These assumptions are sufficient to give the following proposition:[1]

Proposition 1. *Denote g_i^* as the optimal level of government public goods expenditure per capita for an individual with income y_i. Then there exists $\tilde{y}$ such that*

1. $\frac{\partial g_i^*}{dy_i} \geq 0$ *for* $y_i \leq \tilde{y}$

[1] All proofs are contained in Appendix A.

2. $\frac{\partial g_i^*}{dy_i} < 0$ *for* $y_i > \tilde{y}$

This proposition states that the optimal tax rate is inverted U-shaped in income: the rich and poor desire lower government spending per capita compared with those with medium levels of income. The preferred level of spending is increasing in income until a point, $\tilde{y}$, after which the preferred amount of spending decreases in income.

Proof of Proposition 1

Proof. Individuals consume whatever remains after taxation $c_i = y_i(1-\tau)$. Denote average income as $\bar{y} = \frac{Y}{N}$. Then the individual's problem is

$$\max_{\tau_i} U = u(y_i(1-\tau_i)) + v(\tau_i \bar{y})$$

First note that this problem has a unique maximum since $U(c_i, G)$ is strictly concave. The assumptions that $\lim_{x\to 0} u'(x) = \lim_{x\to 0} v'(x) = \infty$ ensures an interior solution.

Since $g_i^* = \tau_i^* \frac{Y}{N}$ I proceed by identifying the optimal tax rate as a function of individual income. Taking the first-order conditions, the optimal τ^* is implicitly defined by the equation:

$$y_i u'(c_i^*) = \bar{y} v'(\tau_i^* \bar{y}) \tag{3}$$

where $c_i^* = y_i(1-\tau_i^*)$.

As y_i increases, it must be the case that c_i^* increases. To see this, consider otherwise. Since consumption is lower, the value of the left hand side would increase relative to the right hand side. Further for consumption to fall, the tax rate must be higher. But then the right-hand side of the equation will decrease, meaning there is no equilibrium.

Using implicit differentiation to identify $\frac{d\tau^*}{dy}$ yields:

$$\frac{d\tau^*}{dy_i} = -\frac{u'(c_i^*) + \frac{dc_i^*}{dy_i} y_i u''(c_i^*)}{-y_i^2 u''(c_i^*) - \bar{y}^2 v''(\tau_i^* \bar{y})}$$

The denominator of this expression is strictly positive, since both $u(\cdot)$ and $v(\cdot)$ are strictly concave by assumption. Then $\frac{d\tau^*}{dy_i} \geq 0$ when the numerator is negative:

$$\begin{aligned} -u'(c_i^*) - y_i(1-\tau_i^*)u''(c_i^*) &\geq 0 \\ -c_i u''(c_i^*) &\geq u'(c_i^*) \\ r_R(c_i^*, u) &\geq 1 \end{aligned}$$

where $r_R(c_i^*, u)$ denotes the coefficient of relative risk aversion. Implicitly define $\tilde{y}$ by $r_R(\tilde{y}(1-\tilde{\tau}_i^*)) = R_R(\tilde{c^*}) = 1$. Then by assumption 2, for any $y_i < \tilde{y}$ $R_R(c_i^*) > 1$. Further, since r_R is

monotonically decreasing, it is sufficient to show that there is j with $c_j^* \geq \tilde{c^*}$. Consider an individual j with $y_j > \tilde{c}^* + \hat{G}$. Since $v'(\hat{G}) = 0$, j will consume strictly more than $\tilde{c}^*$ This completes the proof. □

The second proposition translates these preferences into the level of spending implemented by the government. Denote the initial electorate as E_0 and suppose the right to vote is extended sequentially in decreasing order of income, i.e., a citizen i is only enfranchised once all citizens with $y_j > y_i$ are already enfranchised. Further, assume that the distribution of income in the town is such that $y_i \neq y_j$ for $i \neq j$, $|\{i|y_i \geq \tilde{y},\ i \notin E_0\}| \geq 2$ and $|\{i|y_i < \tilde{y},\ \tau_i < \tilde{\tau}\}| \geq 2$, where $\tilde{\tau}$ denotes the median level of τ_i^* for all individuals for whom $y_i \geq \tilde{y}$ (the decreasing part of the optimal tax function). These latter assumptions ensure that the median voter in the initial electorate is sufficiently wealthy and that there are some individuals who are sufficiently poor to want a lower tax rate than the rich. Finally, assume that N and E_0 are odd.

The key testable implication for the empirical analysis is then:

Proposition 2. *The tax revenue and amount of government spending per capita will be inverted U-shaped in the level of the franchise: extensions of voting rights will initially lead to higher public goods spending and taxation, but will eventually result in lower levels of spending on the public good.*

Intuitively, this proposition reflects the fact that at low levels of income citizens cannot "afford" spending on the public good, since an increase in taxation moves them to very low levels of disposable income. As income rises, this cost is reduced, increasing the preferred tax rate. However, the marginal cost of taxation is also increasing in income; once income is high enough this effect dominates and the demand for government spending declines.

Proof. First, note that preferences over τ are single peaked, since $U(\cdot)$ is strictly concave. Then for a given electorate we can apply the standard Median Voter Theorem. (Note that the median voter here is not necessarily equivalent to the voter with the median income). From Proposition 1, we know that τ_i^* reaches a unique maximum at $y_i = \tilde{y}$, and the optimal tax rate is decreasing in y_i for $y_i > \tilde{y}$.

Define τ^0 as the median tax rate under E_0, and τ_i^m as the median optimal tax rate when i is the poorest enfranchised citizen. Order the voters in order of income. That is voter $i+1$ is the next richest voter after voter i. For all citizens $\{i|y_i \geq \tilde{y},\ i \neq E_0\}$, $\tau_i^* > \tau_{i+1}^* \geq \tau^0$. Thus as each of these citizens are enfranchised τ^m (weakly) increases. Further, this increase is strict at some point since $|\{i|y_i < \tilde{y}, i \neq E_0\}| \geq 2$. By proposition 1, the optimal tax rate is increasing in y_i for $y_i < \tilde{y}$. Then all citizens $\{i|y_i < \tilde{y}\}$, $\tau_i^* > \tau_{i-1}^*$. As a result, if the

median tax rate decreases as the franchise is increased, it will always decrease for further extensions.

Now suppose τ^m never decreases as the electorate increased. Then $\tau_i^m \geq \tilde{\tau}\ \forall i$ with $y_i < \tilde{y}$. But this is not the case, since by assumption there are at least two citizens for which $\tau_i^* < \tilde{\tau}$.

To complete the proof, note that the level of the tax rate directly maps to the level of public goods expenditure per capita, since $g = \tau \frac{Y}{N}$

□

A.3 Extension to Progressive Tax System

In the main text I present the model with a proportional tax rate both for simplicity and because it closely matches the historical setting of the empirical analysis. However, the result of proposition 1 holds for a more general, progressive, tax structure where the consumption of individual i is:

$$c_i = y_i - t(y_i)$$

and $t(y_i)$ is a tax burden varying according to income, characterized by

$$t(y_i) = s(y_i)T$$

where T is the total tax revenue (and hence public goods spending) and $s(\cdot)$ is a function identifying the share of the total taxation paid by an individual. Note that if $s(y_i) = \frac{y_i}{Y}$ then this simplifies to a proportional tax system.

I consider tax systems that are (weakly) progressive as defined by constraints on the tax elasticity $\epsilon(y)$:

$$\epsilon(y) = \frac{t'(y)}{t(y)}y$$

A tax system is, as usual, defined as progressive if $\epsilon(y) > 1$, and regressive if $\epsilon(y) < 1$. For a proportional tax system $\epsilon(y) = 1$. I assume that there is some y such that the tax system is progressive at y and that, in addition, the tax system is increasingly progressive at higher incomes: $\epsilon' \geq 0$ with weakly increasing marginal tax rates: $s''(y) \geq 0$.[2] To ensure marginal tax rates of between 0 and 1 I assume that $s'(y) \in [0, \frac{1}{\tilde{G}+1}]\ \forall y$. Finally, I assume that $s(y) > 0\ \forall y > 0$—that is, all citizens bear some of the tax burden.

With these assumptions I re-state the proof of proposition 1 as follows.

Proof. The proof proceeds by first characterizing the conditions under which the optimal

[2]Note that increasing marginal tax rates are implied by an increasing tax elasticity if the tax is progressive but not if the tax is regressive.

level of taxation is increasing in income and showing that this function has a single turning point.

Individuals face the following optimization problem:

$$\max_T U = u(c_i) + v\left(\frac{T}{N}\right)$$

This problem has a unique maximum since $U(\cdot)$ is strictly concave. Taking the first-order conditions, the optimal T^* is implicitly defined by the equation:

$$\begin{aligned} F(T^*; y, Y) &= -\frac{dc}{dT}u'(c^*) + \frac{1}{N}v'\left(\frac{T^*}{N}\right) = 0 \\ &= -s'(y)u'(c^*(y)) + \frac{1}{N}v'\left(\frac{T^*}{N}\right) = 0 \end{aligned} \tag{4}$$

where for simplicity I drop the i subscripts on y_i and c_i and denote i's consumption at their optimal level of taxation as $c^* = (y - s(y)T^*)$. The assumptions that $\lim_{x\to 0} u'(x) = \lim_{x\to 0} v'(x) = \infty$ ensure an interior solution.

In the remainder of the proof I only display the arguments of the c^*, s, s', T^* if needed for clarification. **Lemma 1** The optimal level of consumption is increasing in y:

$$\frac{dc^*}{dy} > 0 \ \forall y$$

Proof. First note that we can write:

$$\frac{dc^*}{dy} = 1 - s'T^* - \frac{dT^*}{dy}s$$

Since, by assumption, $1 - s'T^* > 0$ then $\frac{dc^*}{dy} < 1$ if and only if $\frac{dT^*}{dy}s > 0$. Now consider $y_2 > y_1$ with associated consumption $c_2^* < c_1^*$ and $T_2^* > T_1^*$. But then $u'(c_2^*) > u'(c_1^*)$ and $v'(T_2^*) < v'(T_1^*)$ violating equation 4. □

Using implicit differentiation to identify $\frac{dT^*}{dy}$ yields:

$$\begin{aligned} \frac{dT^*}{dy} &= -\frac{F_y(T^*; y)}{F_{T^*}(T^*; y)} \\ &= -\frac{[-s'u'(c^*) - s(1 - s'T^*)u''(c^*)]}{s^2u'' + \frac{1}{N}v''(\frac{T^*}{N})} \end{aligned}$$

The denominator of this expression is negative since both $u''(\cdot)$ and $v''(\cdot)$ are strictly negative and $s > 0$. Then the sign of this derivative is determined by the sign of the

numerator and $\frac{dT^*}{dy} \geq 0$ if and only if:

$$-[-s'u'(c^*) - s(1 - s'T^*)u''(c^*)] \leq 0$$

Denoting the coefficient of absolute risk aversion and the coefficient of relative risk aversion at c^* as R_A^* and R_R^* respectively, then the optimal tax rate is increasing if:

$$R_R^* \geq \frac{(\epsilon(y) - 1)}{(1 - s'T^*)} + 1 \tag{5}$$

Note that in this sequence we rely on lemma 1 to show that $s(1 - s'T^*) > 0$.

Inequality 5 establishes the conditions under which the optimal tax rate will be increasing in individual income. To complete the proof I proceed in two steps. First I show that there is at least one income y_1 where the inequality holds strictly (the optimal tax rate is rising in income) and some point $y_2 > y_1$ where the inequality strictly fails to hold. In the second step, I then show that there is no point $y_3 > y_2$ where the tax rate is again increasing in income.

Now, consider any income $\hat{y}$ such that $\epsilon(\hat{y}) > 1$ (i.e., the tax system is progressive). Then the right-hand side of (5) is greater than 1, since $(1 - s'T^*) > 0$ by Lemma 1. By assumption there exists $\hat{c}$ such that $R_R(\hat{c}) < 1$. An individual with income $\hat{\hat{y}} = \hat{c} + \hat{G}$ will consume at least $\hat{c}$ and so will have relative risk aversion less than one, and so inequality (5) will strictly fail to hold.

I now show there is $\underline{y}$ such that (5) is strictly satisfied. By assumption 2 there exists y such that $R_R(c^*(y)) > 1$ and so it is sufficient to show that for low enough y the right-hand side of (5) is less than or equal to 1. First, consider the case where the tax schedule is at some point regressive. Then the right-hand side of 5 is always less than one. Now consider the case where the elasticity $\epsilon(y) \geq 1 \ \forall y$. It is sufficient to show that:

$$\lim_{y \to 0} \frac{ys'}{s} = 1 \tag{6}$$

since $(1 - s'T)$ is bounded above by our assumption on s' and the fact that $T^*(y) < \hat{G} \ \forall y$. As such, if (6) holds then the right-hand side of (5) will tend to 1 as $y \to 0$.

We can write 6 as follows

$$\lim_{y \to 0} \frac{f(y)}{g(y)} \left(\frac{f'(y)}{g'(y)} \right)^{-1}$$

where $f(y) = y$ and $g(y) = s(y)$.

If we can apply L'Hopital's rule then $\lim_{y\to 0} \frac{f(y)}{g(y)} = \lim_{y\to 0} \left(\frac{f'(y)}{g'(y)}\right)$ and we are done. To apply this rule, three conditions need to be met i) $\lim_{y\to 0}\ y = 0$ ii) $\lim_{y\to 0}\ s(y) = 0$ and iii) $s'(y) > 0$ if $y > 0$. The first condition is trivial. To see the second, suppose otherwise that $\exists L$ such that $lim_{y\to 0}\ s(y) = L > 0$. Then $\lim_{y\to 0}\ \epsilon(y) < 1$ and the tax system is regressive at some point. Similarly for the third condition, consider that $s'(y) = 0$. Then $\epsilon(y) = 0$ and the tax system is regressive.

So far I have shown that there is some point at which the optimal tax function is increasing and a point with higher income at which it is decreasing. However, to complete the proof I must show that T^* cannot not again increase after it has begun to fall. Since T^* is continuous, it is sufficient to show there is not a point $y_3 > y_2$ such that $\frac{dT^*(y_3)}{dy} = 0$.

Suppose otherwise that such a point (or points) exists and consider the lowest such point. Define the following function:

$$h(y) = R_R(c^*(y)) - \frac{(\epsilon(y) - 1)}{(1 - s'T^*(y))} - 1 \tag{7}$$

Then $h(y_2) < 0$, $h(y_3) = 0$ and $h'(y_3) \geq 0$. Differentiating:

$$h'(y) = \frac{R_R^{'}(c^*(y))}{dy} - \frac{\epsilon^{'}(y)}{(1 - s'T^*(y))} + \frac{\epsilon(y)(-s'\frac{dT^*}{dy} - s''T^*)}{(1 - s'T^*)^2} \tag{8}$$

By assumption, $R_R^{'}(c^*(y)) < 0$, $\epsilon'(y) > 0$ and $s'' > 0$. But then if $\frac{dT^*}{dy} = 0$ then $h'(y_3) < 0$ and we have a contradiction. This completes the proof. □

A.4 Historical Background and Theoretical Assumptions

This subsection explains the links between the institutional setting and each of the theoretical assumptions. To do so I draw on secondary literature and original research from local newspapers regarding the content of local political campaigns. Specifically, I carried out a systematic search of local plebiscites regarding public goods spending and investigated twenty-six in detail (based on source availability). Second, I systematically counted the topics discussed during hustings for election to the Liverpool town council between 1860 and 1880.[3]

Governance Councils were governed under a common legal framework, under which councilors were elected every three years. Councils determined public goods policy, both directly and via confirmatory plebiscites, leading to considerable variation in investment across boroughs and over time.

Town Spending as a Public Good Councils had autonomy over spending decisions but could only spend money on infrastructure and other public goods. They did *not* have legal authority to undertake transfer payments and did not control spending on either welfare (poor relief) or education.

The assumption of a non-excludable public good with equal benefits to all citizens is justified by the fact that most expenditure was on sanitation, and other items that it was difficult to obtain privately. Sanitary public goods—spending on water supply, sewers, and streets—were by far the largest component of public goods spending at this time. In 1884, for example, on average sanitary expenditure was 53% of current spending on public goods. The next most important individual categories were gas (13%), lighting (12%), and markets (3%). Baths, hospitals, and libraries each accounted for 1%, while a composite "other public works" category covers 15%. This expenditure led to significant improvements in public health (Chapman, 2019, 2022).

The model assumes that utility is from the per capita level of spending on the public good because the amenities provided by town councils generally need to scale with the size of the city—a sewer system needs more capacity, and streets need more maintenance. Sewer systems, for example, needed to grow in order to cope with increased capacity and effectively remove waste. Similarly, bigger cities require more household connections to the water system, and larger volume of water being provided. Further, in the second half of the century sanitary systems were in danger of being overwhelmed, meaning additional investment was needed to keep pace with population growth—as displayed in Figure B.4, expenditure grew throughout this period.

[3]See Appendix B.8 for full detail. Liverpool was chosen due to its substantive interest and the availability of source material.

The model also assumes that utility from the public good is constantly increasing—that is, individuals do not reach a satiation "point" in their demand. This assumption is justified by the continued poor sanitary environments in England and Wales even at the end of the century (see, for example, Thompson, 1984). Even at the end of the century, large towns had not invested in chlorinated water and did not offer constant water supply Troesken, Tynan, and Yang (2021)—meaning continued demands for further improvements.

Linear Tax Rate Councils faced restrictions on the taxes they could impose, meaning that in practice tax burdens were approximately proportional to household income. Taxes could only be raised on the value of property occupied, meaning that renters as well as owners were charged tax. Councils could levy only a single proportional tax rate—there was no possibility of a progressive tax rate. Nor was it possible to impose other taxes, such as business or income taxation, that could have led to a more progressive schedule.

The argument that property taxation implemented fell proportionally on all occupiers assumes that the full value of the tax was passed on tenants and not absorbed by landlords through lower rents. There was some debate about where the burden actually fell (for instance, see *Hansard*, 20 February 1850 col 1118-27). However, for the purposes of the theoretical prediction, it is sufficient that part of the cost was passed on to tenants since the model extends to progressive taxation.

Unidimensional policy Consistent with the single policy dimension in the model, the high cost of public goods was the principal focus of local politics. Politicians stood—and won—elections on the basis of their resistance to spending, often as part of a local "economy" party or with the support of a "shopocracy" of small property owners or local ratepayers' associations (Hennock, 1973). Town spending and taxation was a constant theme in the Liverpool elections—it was a major topic in 92% of hustings held by political candidates. Frequently (in 62%) this discussion specifically addressed sanitation and water supply. But the need for "economy" or a lighter tax burden was consistently emphasized as well—in 49% of meetings and in all of the campaigns surrounding local plebiscites. Other topics were mentioned much less frequently.[4] The most common examples included discussions around temperance and liquor licenses (around 20% of meetings) and the position of Catholics (around 13% of reports).

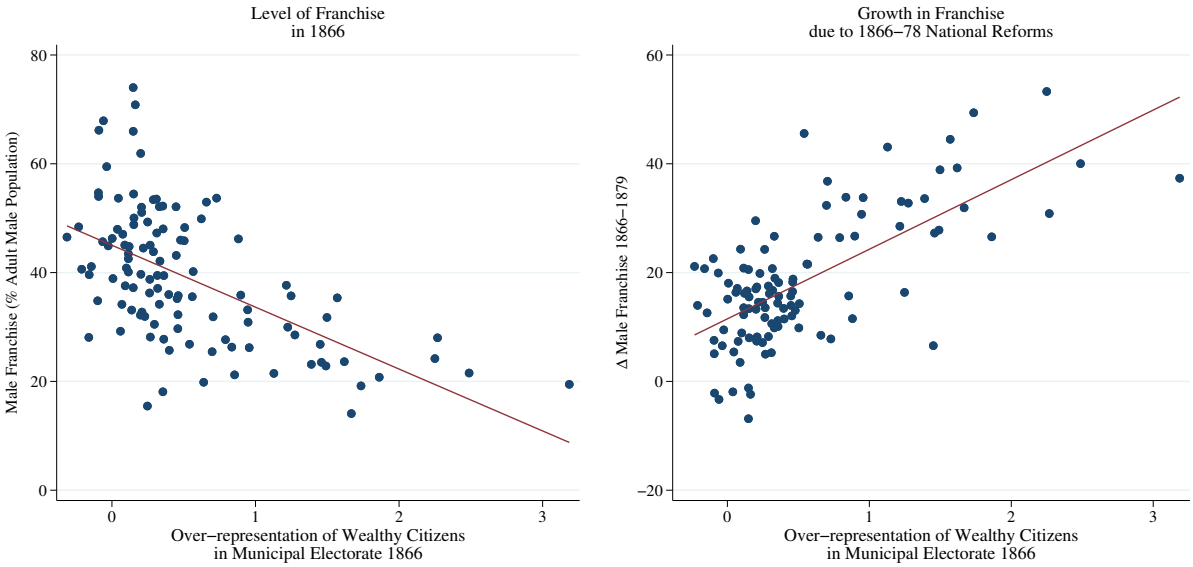
Voting Rights and the Theoretical Assumptions This voting system meets the theoretical assumptions in that, first, all voters paid taxes and, second, the restrictions on the franchise fell predominantly on the poor. Non-taxpayers lost to the right to vote automatically. The main groups of newly enfranchised citizens during our period were compounders

[4]Reports of hustings were generally short, so the absence of a topic does not necessarily mean it was not mentioned at all.

and those failing the strict residency requirements—both groups that would be paying tax even while disenfranchised, as assumed in the model. Further, both groups would predominantly consist of poor voters, who would move more regularly and be more likely to live in low-value housing.

Figure A.1 provides empirical evidence for this claim using information on the composition of the municipal electorate in 1866. Unfortunately, no information regarding the income of municipal voters exists. However, for 1866 we can identify the share of municipal voters who also had the right to vote in Parliamentary elections (see Section B.5). Parliamentary voters were relatively rich because, unlike in municipal elections, they generally had to meet property requirements (specifically, occupying a property of at least £10 annual rental value) to gain the right to vote. A higher share of Parliamentary voters in the municipal electorate than in the population as a whole thus indicates that the wealthy were disproportionately likely to have municipal voting rights. The left-hand side of the figure shows that in 1866 the franchise was smaller in towns where Parliamentary voters were over-represented in the municipal electorate. Further, the right-hand side of the figure demonstrates that these towns saw larger franchise expansions between 1866 and 1878, consistent with national reforms during this period disproportionately enfranchising the poor.

Figure A.1: Over-representation of wealthier citizens associated with smaller pre-reform franchise and larger franchise expansions during 1866-1878 reforms.



Over-representation of parliamentary voters denotes the extent to which citizens qualifying to vote in Parliament were over-represented in the municipal electorate (see Equation 9).

High Marginal Consumption of the Poor The assumptions regarding voter utility are difficult to test directly but fit with empirical evidence for modern developing countries: Ogaki and Zhang (2001) find declining relative risk aversion in Pakistan and India with low income households. Such households were likely richer than the poor in nineteenth-century England; Logan (2009) finds that even industrial workers—far from the poorest group—in 1888 were worse nourished than the poor in rural Indian households in 1983. Specifically, nineteenth-century British households consumed around 40% fewer calories and had higher income-calorie elasticities—i.e., a greater proportion of income increases were spent on additional calories. Further, a smaller proportion of additional food spending in Britain consisted of higher quality food, indicating a greater degree of hunger. In Britain of a 1% increase in food spending around three-quarters went into more food, and 25% into better food (e.g., dairy, meat, vegetables and fruit); in India around half went into better food. These results are particularly striking given that the industries represented in the British sample mean that the average earnings are much higher than in the population and are "not generally representative of the laboring poor" (Horrell and Oxley, 1999, p. 499). Working class households thus faced trade-offs between improved sanitary environments and better nutrition—consistent with the proposition that the costs of public goods were simply too high for the poor to bear.

The poor turned out to vote Unfortunately no data on turnout in municipal elections exists, and it is difficult to identify turnout by class even in better-documented Parliamentary elections. However, there is no clear evidence that the poor were considerably less likely to vote than their richer counterparts: Berlinski, Dewan et al. (2011) find only weak evidence that franchise extension reduced turnout between 1865 and 1868, and no evidence of any effect by 1874. These findings suggest that the poor turned out at least in these elections. This conclusion is supported by the single available data point from the town plebiscites (Birmingham in 1860), where ordinary voters represented 62% of those who voted.

Moreover, for the theoretical predictions to hold it is not necessary that the poor turnout at the same rate as wealthier voters. The critical factor is that franchise extensions reduce the income of the median voter: this would be the case as long as some newly enfranchised citizens vote, and there is no counter-mobilization whereby turnout increases among richer voters due to the poor being enfranchised.

The political debates also make it clear that the poor were an important political constituency, and that class represented the key cleavage in town politics. Both in plebiscites and in Liverpool elections, candidates' arguments stressed the interests of the "working man" or the poor. In a Birmingham plebiscite, for example, supporters of higher investment claimed it "was of great importance to the poor, who paid rates and received nothing", while

opponents hoped that "working men...would see to their own interests and throw out the measure".[5]

A.5 Historical Evidence of Poor's Opposition to Public Spending

I analyze the reasons for opposition (and support) to public spending using secondary literature and the newspaper reports of political debates in both town council elections and plebiscites. We cannot directly measure voter preferences during this period, but we can gain insight into them through the arguments used by the politicians seeking their support. To do so, I collect data from two sets of local newspaper reports of political debates.[6] First, I investigate the debate surrounding local plebiscites regarding public goods expenditure between 1855 and 1905.[7] Second, I undertake a systematic analysis of the topics discussed in the "ward meetings" of candidates seeking election to the Liverpool town council between 1860 and 1880. At these meetings candidates met supporters and canvassed support—I categorize the major topics of discussion in each.[8] .

A range of historical evidence supports the conclusion that the poor themselves opposed spending on public goods. In 1869-71, the Royal Sanitary Commission concluded that following the 1869 enfranchisement of the compounders, "sanitary reforms [were] in many cases rendered impossible by the hostility of the...poorest class" (House of Commons *Parliamentary Papers*, 1871, p.30)—and suggested a graduated franchise, in which wealthier voters had multiple votes, as a means to fix this. Furthermore, in one Welsh town "workers were willing enough to admit they were killing themselves, but they saw immediate income as more important than environmental quality" (Hamlin, 1998, p.298). Importantly, citizens did not need detailed—or even correct—scientific knowledge to understand the benefits of clean water or improved sanitation. Sanitary reformers were proposing correct solutions to sanitation issues even before the germ theory of disease was properly understood (Williamson, 2002). The reports of plebiscites and the Liverpool election meetings do not indicate that politicians were questioning the responsibility of town councils to maintain sanitary environments or public health—we see no equivalent of, for instance, an anti-vaccination movement.

The history of Birmingham provides a case study of how local politics was shaped around

[5]See *Aris's Birmingham Gazette*, Sat 08/12/1860, p.10 and p.5.

[6]Local newspapers were an important way in which local politicians could reach their electorate: "many readers, including increasing numbers of newly-enfranchised working-class men, wished to follow carefully the actions of local politicians and the columns of the local paper allowed them to do so in relative comfort" (Walker, 2006, p.383). The existence of subscription reading rooms meant that purchase price was not a barrier for the working classes—a rough estimate suggests copies of newspapers were read by around twenty-five people on average (Aspinall, 1946, p.29–30).

[7]See Appendix B.8 for a list of the plebiscites, and details of how they were identified.

[8]Liverpool was chosen for this exercise due to both the availability of archival material, the size of the city, and because the need for improved water and sanitation was constantly discussed during this period, culminating in the passing of a plebiscite in favor of the Vyrnwy water scheme in 1879.

questions of local spending, and how opposition to spending was driven by poorer voters.[9] After 1851, the Birmingham council embarked on a program of sanitary improvements. As costs increased, however, a group of "Economists" emerged on the council, blocking the purchase of the town waterworks and opposing the expansion of the council's borrowing powers. In the latter case, the Economists were outvoted in the council, at which point they instigated a plebiscite—in which the population overwhelmingly refused to sanction greater expenditure. The Economists then took charge of the council; blocking street improvements and halting drainage works.

The results of these plebiscites in Birmingham provide direct evidence of the opposition of the poor to increased expenditure. These plebiscites allowed occupiers of more valuable property up receive up to six votes; the results thus provide some evidence as to the support for the bill among different classes of voters. In 1860, those voting for expanding town investment held 2.4 votes per voter, compared to 1.5 among opponents—similarly in 1874 the results were 3.3. votes amongst supporter and 2 votes for those against. In 1874, 80% of the "ordinary" voters with just one vote opposed increasing the size of government. The middle classes were willing to pay for greater government spending, but the poor were not.

A.6 Alternative Explanations

The support of the middle class for higher public goods spending is contrary to the predictions of models that explain limited government through the voter with median income (the "median voter") aligning with the rich. In Benabou and Ok (2001), for example, such a coalition forms because the median voter believes her income will be above the mean in the next period. The results in Section 3 show, in contrast, that it is the poor that form a coalition with the rich to resist higher taxation.

Models allowing for multiple policy dimensions could explain the results, however they are inconsistent with the empirical context. The model's assumption of a single policy dimension fits well with the legal restrictions on council activities. In particular, town councils' spending powers were limited to spending on public goods and they could not, for instance, engage in redistribution. Low public goods spending could thus not be compensated by reallocation of resources. This rules out for instance, the situation analyzed in Levy (2005), whereby lower overall government spending is compensated by a shift of resources from redistribution towards education. Further, taxation was the prominent issue at local elections and, as we have seen above, local plebiscites allowed the electorate to be heard even on specific expenditure projects.

Further, there is little evidence that attitudes toward public goods expenditure were capturing affiliations to national political parties. Political parties did operate at municipal

[9]This paragraph draws extensively on Hennock (1973, p.31–33).

level during the nineteenth-century, but local—rather than national—issues dominated when it came to matters of town improvement. There was not a clear mapping between national and local party politics on the issue of public goods provision—the two major political parties (the Tories and the Liberals) were on different sides of the debate in different cities, and in general "divisions could be both across and along party lines" (Fraser, 1976, p175). Opposition to growing expenditure was centered around the detail of local issues, and not party attachment.

More generally, the analysis of the political debates makes it clear that class was the key cleavage in town politics. There are frequent mentions of class differences in the newspaper reports, and particular reference to the working classes or the poor (in 15% of reports). Other demographic differences are, in contrast extremely rare—the main exception being a small number of wards where the "Irish-catholic" vote was emphasized in the mid-1870s. Consistent with the theoretical prediction, the key characteristic of the new voters was their low income.

An alternative set of explanations for the inverted-U-relationship allows for the benefit of public goods to vary across groups. The model in this paper assumes a non-excludable public good with equal benefits to all citizens—meaning that variation in demand for public spending is purely due to the differences in tax payments. In those other models, in contrast, the rich prefer private to public provision (Epple and Romano, 1996a,b), or public policies are beneficial only for specific industries (Llavador and Oxoby, 2005).

Such explanations do not, however, fit well with the fact that sanitation investment led to significant health improvements for all economic classes[10]. Urban infrastructure led to major reductions in mortality not only from waterborne diseases, but from airborne diseases as well (Chapman, 2019). Moreover, the rich could not purchase public health in the way they could buy better education (Epple and Romano, 1996a,b): even wealthy citizens were continually exposed to the unsavory conditions around them, as "even with the growing separation of the classes, many elements of sanitary condition—water supply, drains, muck in the streets, odors, facilities for relieving oneself, complexion and stature of the people—were truly public" (Hamlin, 1998, p.281). The life expectancy of different social classes moved closely over time (Lizzeri and Persico, 2004), and differences in life expectancy between inner and outer portions of cities remained relatively constant between 1851 and 1900 (Szreter and Mooney, 1998, Table 2). The wealthy could not obtain these public health benefits privately.

For similar reasons, the focus on urban infrastructure means that other commonly-discussed cleavages, such as between agriculture and manufacturing interests (Llavador and

[10]Table D.8 demonstrates that the inverted-U-shape relationship holds when restricting to public goods of a clear sanitary nature.

Oxoby, 2005), caste identity (Suryanarayan, 2019), or ethnicity (Alesina, Baqir, and Easterly, 1999), were not important. This paper's focus is ethnically homogeneous urban settings, with few citizens working in agriculture. While some sanitation public goods, such as water supply, had industrial as well as health benefits, few of the towns analyzed here had important agricultural interests: the median town only had 12% of the population engaged in agriculture.[11] The most important ethnic minority were Irish immigrants, but even in Liverpool (a center for Irish immigration) the "Irish-Catholic" vote was emphasized in only a small number of electoral wards. The poor benefited from public goods, but they preferred not to pay for them.

Another set of concerns could be that the results are driven by changes in the composition of the electorate due to in- or out-migration. Rapid in-migration could lower the franchise through leading to more citizens failing residence requirements, while also changing the demand for public goods. If the wealthy fled cities in response to deteriorating sanitary environments, then the franchise could fall along with the funds to pay for public goods. The main specifications control for this possibility through controlling for tax base, occupational make-up and population growth while, Appendix D presents the results of additional specifications showing that the inverted-U shape is not explained by the presence of fast-growing or shrinking towns, or towns where the share of the wealthy occupations fell dramatically. The relationship is also robust to removing very large or small towns and removing towns that had high or low tax bases. Thus it does not appear that the results are driven by the behavior of towns with extreme values of these characteristics.

Appendix D also includes additional tests addressing the concern that the results could be driven by economies of scale or scope in public goods provision. Economies of scale could be problematic if they led to concavity in spending on public goods, with major investments followed by minimal maintenance costs. In that case, early increases in the franchise could be associated with increasing spending, while later increases would be associated with slower growth—and the inverted-U shape would be purely an artefact of the temporal nature of the data. The main results look to account for economies of scale by allowing a flexible relationship with town scale (population), and also allowing for the relationship to vary (by interacting population with decade fixed effects).[12] Town scale provides an imperfect measure of economies of scale, and so Table D.9 presents additional specifications accounting for the

[11] Moreover, that percentage includes some surrounding areas of towns, meaning that this figure is likely an over-estimate.

[12] Millward and Sheard (1995) finds little evidence of economies of scale in this setting using population as a proxy. Ideally we would examine economies of scale by looking at the costs per unit produced by each type of municipal service. In the absence of such data, town scale serves as a proxy for the amount of services provided.

level of investment in urban infrastructure in various ways. The evidence of the inverted-U-shape remains strong after doing so.

B Data Appendix

To test the theoretical predictions I use a new dataset of town expenditure and the extent of the franchise in England and Wales between 1867 and 1900.

The majority of the data used in the paper are drawn from reports to Parliament downloaded from the House of Commons Parliamentary Papers Database[13]. A full list of the reports used is available upon request. Other sources are discussed below.

The first subsection in this Appendix provides a brief overview of the dataset and main variable definitions. I then present the descriptive statistics before moving onto detailed description of the data construction.

B.1 Summary of Dataset

Sample The analysis focuses on incorporated towns in England and Wales that had control of sanitary expenditure in 1867. Incorporated towns included nearly all of the largest towns in the country (the major exception was London), as well as small market towns. A total of 214 towns had been incorporated by 1867; however, only 154 had control of sanitary expenditure at this point. A further four towns are excluded due to either franchise data that appeared implausibly high or difficulties identifying boundary changes.

The Extent of the Franchise

I measure the municipal franchise for each sex as follows:

$$\text{Male (female) franchise} = \frac{\text{Number of male (female) electors}}{\text{Male (female) population of voting age}}$$

The numerator is calculated using the number of municipal electors reported in a number of parliamentary papers for ten cross sections between 1864 and 1897. Intervening values are interpolated using a compound average growth rate. The denominator is calculated using total population, adjusted by the estimated proportion of citizens that were voting age.

To concentrate on the effect of extending voting rights to poorer citizens—the focus of the theoretical model—the main franchise measure is the male franchise. Using the total franchise could conflate two (potentially very different) sources of changes in the franchise: the broadening of the male franchise and the extension (for the first time) of the franchise to women. It is reasonable to assume that growth in the male franchise—due to enfranchising compounders and reducing residence requirements—involved extensions of the right to vote to include poorer citizens. However, this is not necessarily the case for women, since their right to vote depended on being a head of household, and it is not clear how the proportion of women heading households varied across income groups.

[13] See http://parlipapers.chadwyck.co.uk/

To account for delays between the date of registration and actual change in expenditure, I lag the franchise by three years. This lag is chosen to reflect the fact that municipal councils were elected across a three-year period. The results are robust to alternative lags, and the lagged franchise is preferred to the current franchise in a "horse race". To ensure that the results are not driven by extreme values, I exclude the top and bottom 1% of franchise values. The results are, if anything, stronger when these observations are included.

Financial Data The two main dependent variables are the per capita town-level tax revenue and total public goods expenditure between 1867 and 1900. These variables are drawn from an annual panel dataset constructed from town accounts reported in the *Local Taxation Returns*, translated into real terms. In addition, I construct a measure of expenditure on sanitation public goods—water supply, sewers, and street expenditure—however, unfortunately this variable is available only from 1872, when public goods expenditure became disaggregated.

Tax revenue is drawn directly from the accounts; to estimate expenditure on public goods I construct a measure of ongoing spending by adjusting the raw data for large one-off expenditures that could skew the estimates. Specifically, the raw expenditure figures do not differentiate between investment and ongoing (e.g., maintenance) expenditure on public goods. As a result, it is clear from inspection of the dataset that there are a large number of extremely high one-time expenditures (see Figure B.2). To isolate ongoing expenditure, I first identify "investment periods" by analyzing deviations in trend expenditure for each type of spending. In non-investment periods, the level of ongoing expenditure is simply the per capita expenditure in that period. In investment periods, the level of ongoing expenditure is the level of expenditure in the next non-investment period. For instance, if 1873 and 1874 were investment periods but 1875 was not, then the level of per capita expenditure in 1873 and 1874 is set equal to that in 1875.

Investment periods are identified using both the level and year-on-year increase in expenditure. An investment period is identified as starting either when a town begins spending for the first time, when year-on-year expenditure increases by more than 100%, or if the town's per capita expenditure is higher than twice the median of per capita expenditure in the town in future years. An investment period is then identified as continuing until expenditure falls significantly again, relative to both other towns and future expenditure in the same town. Prior to the existence of disaggregated data in 1872, investment periods are also identified if expenditure is more than twice the aggregated 1872 ongoing expenditure. The results are robust to alternative ways of identifying these periods. Appendix B.3, below, contains full details of the methodology, and plots the unadjusted versus adjusted spending data.

Other Variables Population, number of houses, and occupational data were collected using

census data (Appendix B.7). Party of town mayors was identified from local newspapers (Appendix B.8).

B.2 Descriptive Statistics

Table B.1 summarizes the main variables used in the semi-parametric plots and regression analysis in Section 3.

Table B.1: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Male Franchise (% Adult Male Population)	4,810	55.94	11.41	20.77	80.18
Total Public Goods Spending per Capita (£ p.c.)	4,810	.57	.39	0	3.03
Sanitation Spending per Capita (£ p.c.)	4,181	.29	.19	0	1.75
Tax Receipts per Capita (£ p.c.)	4,810	.58	.31	0	2.06
Property Receipts per Capita (£ p.c.)	4,221	.09	.15	0	2.04
Population (10,000s)	4,810	4.87	8.07	.1	67.92
Crowding (Population/Houses)	4,810	5.19	.89	3.86	11.37
Annual Population Growth (%)	4,810	.97	1.1	-2.29	7.61
Female Franchise (% Women over 30)	4,810	10.23	5.43	0	28.79
Tax Base per Capita (£ p.c.)	4,749	3.92	1.38	.12	9.69
% Population In Agriculture	4,778	15.41	12.69	.51	50.35
% Population In Commerce/Professions	4,778	5.65	1.68	2.24	14.09
% Men over 20 Heads of Household	4,798	35.86	2.22	27.92	42.56

Note: see text in prior subsections.

B.3 Financial Data

Information is collected from the annual financial accounts reported to Parliament and collated in the *Local Taxation Returns* contained in the Parliamentary Papers collection. These accounts contain detail on the sources of revenue and types of expenditure in each town annually. Each town reported separately as both a municipal borough and as a sanitary authority (as a local board, improvement commission or urban sanitary authority): these accounts are aggregated together. This information is used to construct an annual panel dataset between 1867 and 1910. Financial values are translated into current prices using the Rousseaux Price Index (Mitchell, 1971, pp. 723–4) following Millward and Sheard (1995).

Defining ongoing public goods expenditure

Prior to 1884 the financial data does not distinguish between one-off and ongoing expenditure items: as such the accounts include a number of very high expenditures, reflecting investment activities. To separate ongoing expenditure from investment expenditure for different types of public good, I first identify "investment periods" by analyzing deviations in trend expenditure in each of the following categories "sewerage and sewer systems", "water supply", "highways, watering and scavenging", and "other public works".

The first three of these categories are defined separately in the financial reports (albeit with some changes over time in the name). However, from 1890 onward some towns (those that were not made County Boroughs) began to receive much higher transfer funding for spending on roads from their County Council. As such, I adjust expenditure on "highways, watering and scavenging" to remove the amount received from this source. To do so, I separate between revenue from County Councils from the "Exchequer Account" and "Other", since it was the latter that was predominantly consisting of payments for main roads. The "other public works" series is the aggregate of (loan and nonloan) expenditure on "other public works", "markets", "lighting", "lighting and sewers", "electric lighting", "tramways" "municipal buildings", "bridges", "housing", "asylums", "libraries", "burial", "baths", "hospitals", and "other". In non-investment periods, the level of ongoing expenditure is simply the per capita expenditure in that period. In investment periods, the level of ongoing expenditure is the level of expenditure in the next non-investment period. For instance, if 1873 and 1874 were investment periods, but 1875 was not, then the level of expenditure in 1873 and 1874 is set equal to that in 1875.

For the period following 1871, a year is identified as the beginning of an investment period for each good if:

1. Expenditure per capita exceeds the median percentile of expenditure per capita (across all towns and years) in the relevant category; and:

- the town started expenditure on the relevant good in that period (the spending in the previous period was 0); or
- there is a 100% year-on-year growth in expenditure on the good, and the expenditure p.c. exceeds the median future per capita spending for the town; or
- the two previous years of data are missing, and the expenditure p.c. exceeds the median future per capita spending for the town; or
- the level of expenditure p.c. is higher than the previous year and twice the median future per capita spending for the town.

The years following the start of an investment period are identified as investment periods if either:

1. expenditure p.c. is greater than the previous period; or
2. the expenditure p.c. exceeds the median future per capita spending for the town; and either:
   - the expenditure is twice the town's average expenditure over the period; or
   - the level of expenditure exceeds the median percentile of expenditure per capita (across all towns and years) in the relevant category.

Between 1867 and 1871, public goods expenditure is not disaggregated in the financial reports, and so I cannot use the process above. Instead, investment periods are identified as being twice the level of ongoing expenditure in 1872, and the above process is then applied to total public goods expenditure in those towns.[14]

Figure B.2 displays the unadjusted and adjusted spending on each of the three components of the sanitation public goods measure, and also the total public goods expenditure. In each case, the plots indicate how, in the absence of adjustment, there are a number of extreme values which could skew the analysis. The percentage of observations identified as investment periods was as follows: Water supply, 13% sewers, 18%; streets 10%; and for total public goods, 36%—where the latter identifies that any of the subcomponents had an investment period.

Figures B.3 and B.4 show that there was an increasing trend over the analysis period in both town council expenditure and revenue. That growth did not, however, remove the considerable variation in the size of government across towns.

[14]For a small number of towns the first period that disaggregated data was available is later than 1872: in this case investment periods are defined relative to the first period data is available.

Definition of financial variables

Tax receipts: Aggregation of all different "rates" collected by towns as municipality and sanitary authority.

Sanitary public goods expenditure: Sum of ongoing expenditure per capita on "sewerage and sewer systems", "water supply", "highways, watering and scavenging". See previous subsection for details of construction of series.

All public goods expenditure: After 1872, sum of "sanitary public goods expenditure" and ongoing expenditure on "other public works" series (see previous subsection for details). Prior to 1872, total of expenditure on "public works" and on sewerage and lighting.

Tax base per capita: Information on the value of the tax base (the "rateable value" of the district) is reported annually in the *Local Taxation Returns* from 1872 onward, with the exception of 1883. For many years, the tax base is reported separately for the town as a sanitary district, and as a municipal borough—I use the *maximum* tax base reported by the town in each year. Before 1872 information regarding the annual value of the tax base was not reported alongside the financial accounts. However, two additional parliamentary papers do provide information regarding the size of the tax base in sanitary districts (but not municipal boroughs) in 1866 and 1870. In addition, for towns with similar municipal and parliamentary boundaries, I can use information on the rateable value in the parliamentary boroughs in 1866. I then linearly interpolate per capita values for missing years.

B.4 Electoral Data

Information as to the number of electors was collected from returns to Parliament supplemented by information for 1879 reported in Vine (1879). Information for the total number of electors in each town was collected for years 1850, 1852, 1854, 1852–1866, 1869, 1871, 1873, 1879, 1883, 1885 and 1897. Information broken down by gender was collected for 1871, 1885, and 1897. Values relating to the number of electors in Shaftesbury (for all years), Carlisle (1854) and Buckingham (1866, 1869, and 1873) were excluded, since there were clear discrepancies in the returns (for instance, where the number of parliamentary electors was reported rather than the number of municipal electors).

The time series for total number of electors was estimated as follows. First, the franchise is calculated as a percentage of the total population, using the series relating to the number of electors above. The missing years are then interpolated using a constant compound growth rate—with the exception of the years 1867 and 1868 which are replaced with the 1866 value, since reforms in 1869 led to a large jump in the level of the franchise. Missing values for 1864 and 1865 are replaced with the value from 1866. A compound growth rate is used in order to match the assumption made on the growth of population between decennial censuses. Linear interpolation between periods could bias the results toward finding a downward relationship

between a high level of the franchise and spending since it leads to higher estimated values of the franchise in later periods while, at the same time, the estimated population is also higher.

To estimate the male / female franchise used in the main specifications, I first estimate the proportion of male electors in 1871, 1885, and 1897. This series is then interpolated at a constant growth rate for the intervening years (the proportion did not tend to change substantially between periods). Multiplying these two series provides an estimate of the number of male and female electors in each year. The franchise measure is then estimated using the estimated adult male population discussed in the following two subsections.

The key franchise variable used in the paper is calculated using an adjustment factor relating to proportion of males and females that were of voting age (21 and 30 respectively). The main measure uses individual-level census data obtained from the North Atlantic Population Project (Minnesota Population Center, 2008; Schürer and Woollard, 2003). The individual-level data is aggregated to identify the age distribution of voters at the level of administrative sub-districts.[15] Each town was then matched to the relevant sub-districts using the 1881 census: often each municipal borough was spread across several of these sub-districts (the boundaries did not, unfortunately, overlap directly). To estimate the town-level age distribution I then average across the different sub-districts, weighted by the proportion of 1881 population in each of the sub-districts (which is also identified in the 1881 census).

While this measure should accurately account for variation in the age distribution across towns, one potential concern is the use of a constant adjustment factor for every year. To check whether this is an issue, I compare the estimated proportion to data from the period 1861–1870 collected from the decennial reports of the Registrar General. Unfortunately, this data is only available at the level of the registration district rather than sub-district, and so can be matched to towns less precisely.[16] The resulting comparison shows a very high degree of correlation over time in the town age distribution, providing confidence that our use of a constant adjustment factor is appropriate. Further, the results are robust to these different measures of the franchise.

B.5 Over-representation of Wealthy Voters in 1866

I measure the over-representation of the wealthy through comparing the percentage of parliamentary voters in the municipal electorate to the percentage of parliamentary voters in the entire population. This is possible only for 1866, when a Parliamentary paper (House of Commons, 1866) lists the number of municipal voters who also have the right to vote

[15] More precisely, these are the registration sub-districts used by the Registrar General.

[16] Smaller boroughs were often only a small part of a registration district. As such this measure combines urban and rural areas.

in Parliamentary elections. Most Parliamentary voters were relatively wealthy—qualifying to vote through occupying a property of sufficiently high value—with only one third of the population qualifying to vote.

Specifically, I estimate the measure of over-representation in Figure I as:

$$\text{Over-representation} = \frac{\%\text{ Parliamentary electors in municipal electorate}}{\%\text{ Parliamentary electors in population}} - 1 \qquad (9)$$

where "population" refers to the number of male occupiers in the town—that is the potential electorate under the male household franchise. If the electorate were entirely representative, the measure would equal zero. If the wealthy were over-represented, on the other hand, then the number will be greater than zero.

B.6 Estimating the Proportion of Poor Voters

I estimate the proportion of "poor" households in the voting population using data on the distribution of housing values in the towns represented in Parliament, reported in the 1866 Electoral Returns and subsequent Parliamentary Papers (House of Commons *Parliamentary Papers*, 1866a,c,b). Specifically, these papers report the number of houses at different gross rental values—that is, the value at which it was estimated the property could be rented for a 12 month period (*communibus annis*). The rental value information is highly disaggregated, with information on the number of occupiers renting property split into 27 bins: under £4, at £1 intervals to £20 (and also at £10 exactly), at £10 intervals to £100 and over £100. I assume that rental values were uniformly distributed within each bin.

The distribution of rental value in the reports is particularly apposite for our purposes, since it relates to male occupiers—precisely the category of potential voters in the main franchise measure. The data was collected to examine the potential effect of altering the rental value at which the Parliamentary vote was gathered, and so was based on identifying possible additions to the electorate. Individuals occupying two properties were, for instance, included only once (at the maximum of the two values).

The way in which these values were assessed means that they provide an accurate estimate of property values. Property values in Britain were assessed at least once per year, since they formed the basis of local taxation. Tax assessments were carried out by local parish officials, and then confirmed by an Assessment Committee at the level of the Poor Law Union. This latter stage was implemented in the early 1860s to address concerns that parishes were distorting values to reduce the tax burden. Further, to the extent that values were distorted prior to this date, it was through allowing considerable deductions (for repairs, insurance and other expenses) in assessing the "rateable" value on which tax was

determined.

To estimate the proportion of the population that are working class, I use information from household budgets in Horrell (1996, particularly Tables 1 and 5). These budgets report the household expenditure and rents of 7 occupational groups across a diverse mix of geographical locations between 1840 and 1854. Two of the occupational groups are agricultural: I remove these, and then estimate the average rent paid by working classes, weighted by occupation.[17]—providing an estimate of £7.2 per annum. I then define poor households as those paying a rent lower than this value.

The proportion of these poor households in the town electorate is then calculated on the basis of two assumptions. First, that all compounders are poor (the percentage of the town voters that are compounders in 1866 is reported in House of Commons *Parliamentary Papers* (1866a)). Second, I assume that, apart (potentially) from compounders, the franchise is extended in descending order of income. In other words, I assume the poor are enfranchised only when the extent of the franchise is greater than the share of non-poor voters in the town. As the franchise grows over time, this threshold is crossed in an increasing proportion of towns.

As a sanity check, I re-estimate the poverty measure re-defining "poor" as those below the median household income in 1860 estimated by MacKenzie (1921). She estimates that 18% of households worked in agriculture, so this value translates into approximately the sixth decile of urban households. Her standard household at this income level included 3 children, with an income of £53 and rental spending of £7.8 per annum. This alternative definition of the poverty line is within 10% of the main estimates—despite being constructed on a very different basis.

Further reassurance that these estimates are reasonable is that living in a dwelling of over £6 rateable value was frequently used as a potential "lower limit" for obtaining the Parliamentary franchise in the 1850s and seen as a bulwark against providing the working classes with democratic control (Seymour, 1915). On average across Parliamentary boroughs the rateable value was around 18% less than the rental value (calculations using House of Commons, 1866, Return F); and so the poverty definition above is in line with this threshold.

B.7 Census Data

Information regarding the population and number of inhabited houses for each town were gathered from census reports between 1861 and 1901, and from the parish-level statistics for the 1911 census gathered by Southall et al. (August 2004). An "inhabited house" in this context was defined as a distinct building which was inhabited, including "all space within the external and party walls of a building" (Newman, 1971, p.11). Between censuses the

[17]Occupational shares are given in Horrell (1996, fn. 38).

population is interpolated at a constant annual growth rate.

In several cases towns underwent boundary changes between census years. To adjust for this, I have identified the towns that underwent boundary changes using the census and the year of the boundary changes using both the census reports themselves and the annual reports of the Local Government Board. The population series is adjusted to the revised population (provided in the census reports) at this date and binned into six categories: less than 10,000 citizens, 10,000-25,000, 25,000-50000, 50,000-100,000, 100,000-250,000 and more than 250,000 citizens.

Occupational structure, and the percentage heads of household, are measured at the level of the Registration Districts used for registration purposes. Occupational data was obtained from the Integrated Census Microdata service through the UK Data Archive, while the percentage heads of household was constructed using the 100% census samples available at Minnesota Population Center (2019).[18] The registration districts changed over time, and so I create a set of synthetic districts with standardized boundaries that accounted for mergers and splits. Each town is then linked to one or more of these districts as explained in the discussion of the franchise data above.

B.8 Local Politics

Data on local politics was collected from national and local newspaper collections relying, in particular, on the digital collection of the British Newspaper Archive[19]

Party Mayor Affiliation The political affiliation of mayors was identified from newspaper reports. Incomplete lists were published in national newspapers from 1871 onward—prior to this date, newspapers that reported lists of mayors reported little (at most) information regarding party affiliation. This information was then supplemented, including for earlier years, with information from local newspaper reports.

Liverpool Ward-Level Politics A search was undertaken for details of local election debate in each of Liverpool's sixteen wards annually between 1860 and 1880 using the local newspaper collection of the British Newspaper Archive. Specifically, this involved finding newspaper reports of local "ward meetings", where candidates would address the public. Reports were found for a total of 230 candidates (reports were less common in the many cases where no contest was held).

Local Plebiscites A search was undertaken of the British Newspaper Archive[20] for local

[18]Occupational data could not be obtained for one town due to ambiguity in the place names used in the ICEM data.

[19]See http://britishnewspaperarchive.co.uk/. The BNA provides scans of millions of pages of local newspapers from the British Library's collection.

[20]The primary search term used was "ratepayers' polls", with addition of other terms, such as "elections" to identify most relevant results.

plebiscites between 1848 and 1905. Over three hundred polls of various nature were identified (including some in Scotland and Ireland). Of those polls, 112 appeared to relate to town expenditure on public goods—such as the adoption of new powers, undertaking specific projects, or purchasing private providers or water or gas.[21] The scale of the projects ranged from improvements to a single street, to enabling a £1 million loan to the Manchester Ship Canal Company. On average, around 5 articles were identified for each poll.

Twenty-six of these plebiscites were chosen for more detailed investigation.[22] Polls were selected based on town and project size (in the expectation that more information would be available), and with a primary focus on sanitation. However, to ensure coverage of a broad range of experiences, some smaller projects and alternative topics were also selected. For each of these polls, additional searches were undertaken to find reports of town meetings or other debates. Those articles were then used to provide more detail on the subject of each poll, and the key arguments used by both proponents and opponents of the motion.

Table B.2 provides a brief description of each of these plebiscites. The topics considered were often complex, particularly if they related to a parliamentary bill—here the topic highlights the elements most relevant to this paper. We can see that the plebiscites were often keenly contested, and that motions were often lost: only 50% in this list were successful. For a few locations we can glean some insight regarding the preferences of different classes by comparing the number of votes and number of voters for and against each proposal. In these locations a graduated franchise was used, where voters owning or occupying valuable property would have multiple votes.[23] In four out of the six polls, the votes per voters is notably higher for supporters; in the remaining two (Reading and Sunderland), the groups are very similar. These results then provide further evidence that the poor could be part of the opposition to growing expenditure.

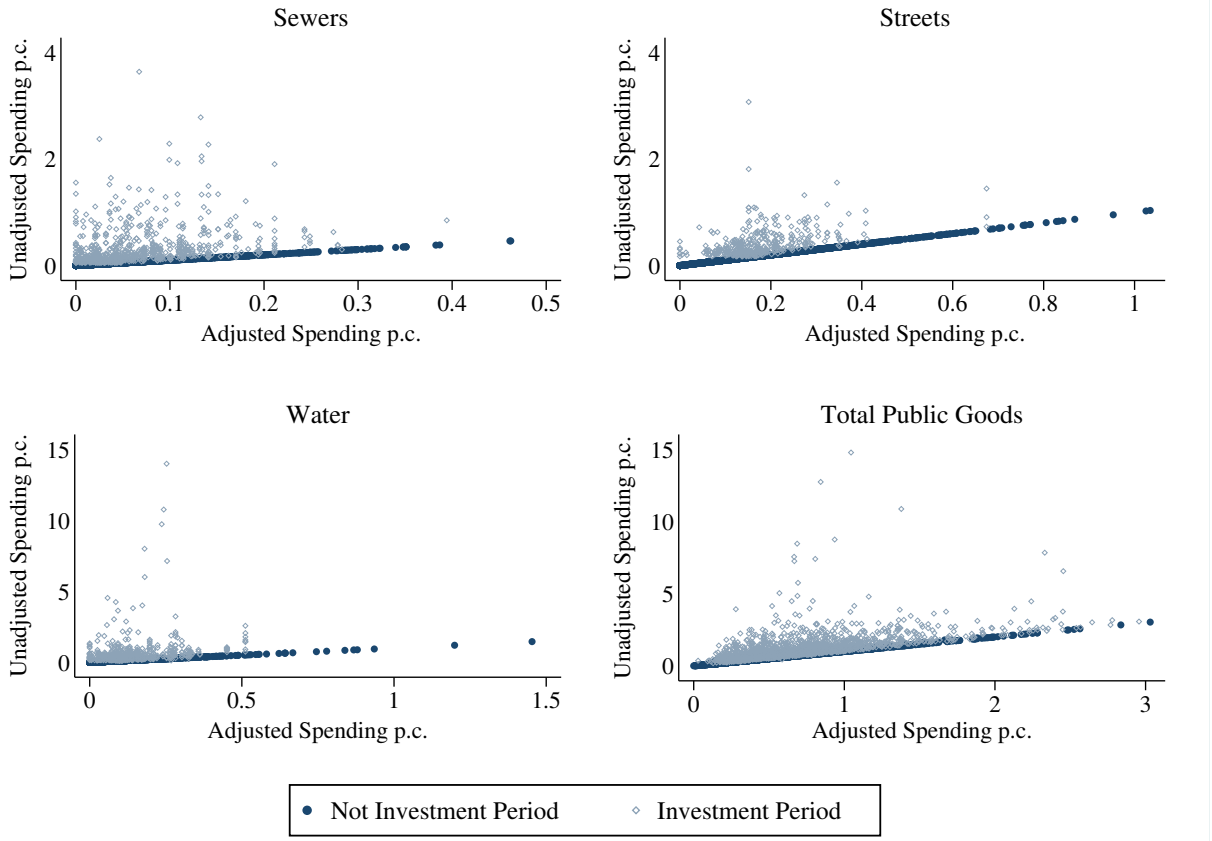
The main text refers to twenty-four polls because little or no information was available for the polls in Warrington and Filey.

[21] Other common topics related to the adoption of church rates, the adoption of the free libraries act, adoption of school boards, or boundary changes.

[22] An additional five polls, implemented simultaneously as part of the debate over the Cardiff Corporation Bill in 1900, were initially selected. However, it became clear that the sprawling nature of the bill meant that the issues of relevance to this paper were little reported, and so these polls were excluded.

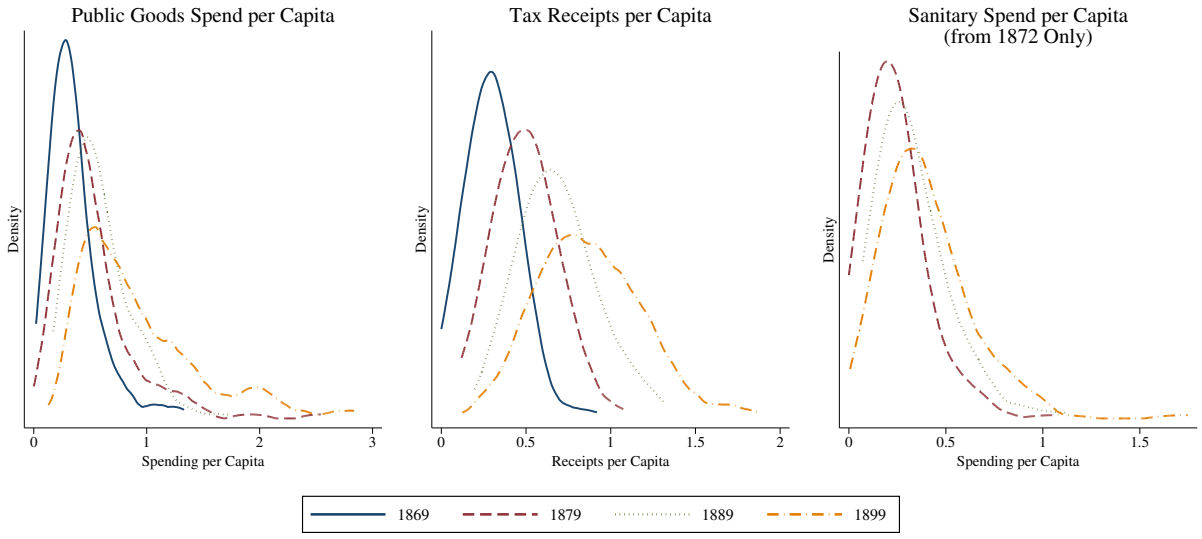
[23] In general limited information regarding the way in which polls were undertaken is provided, and so it is possible that other towns used a similar system without results being reported in this way.

Figure B.2: Unadjusted and adjusted financial data.



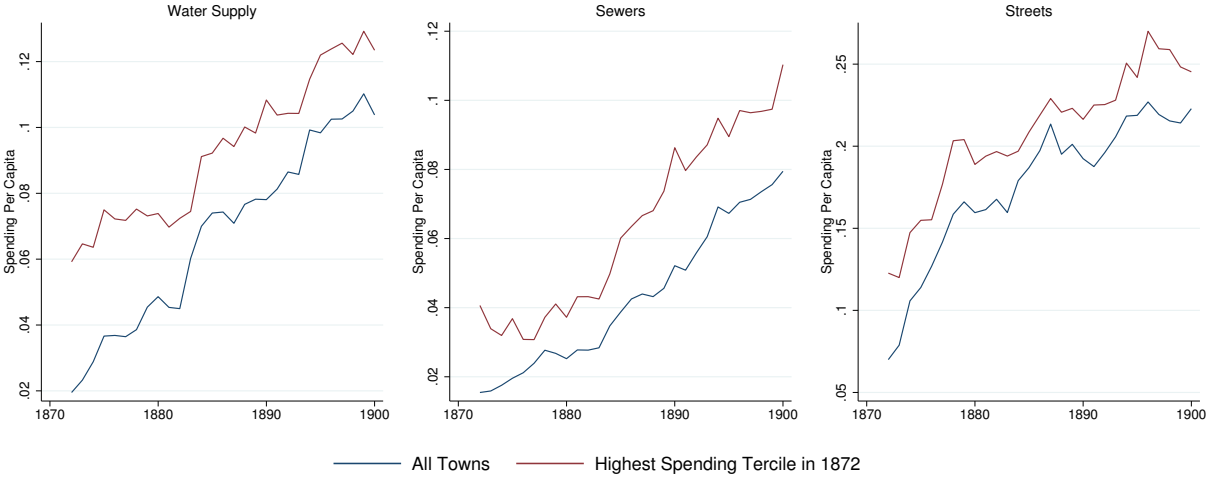
Note: The data used in the regression analysis is plotted on the x-axis in each panel and the unadjusted (apart from conversion to real terms) per capita spending on the y-axis. Investment periods are those where the data was adjusted—see text for details. All values are in £ per capita. Water, sewers, and streets are only available for the period after 1872 and are aggregated into combined sanitation spending in the analysis. Total public goods is available for the entire period, consisting of these three categories as well as "other public goods"—a period is identified as an investment periods for this variable if any of the subcomponents was identified as being in investment period.

Figure B.3: Town council spending and tax revenue grew over time.



Note: See Appendix B.3 for details of series construction.

Figure B.4: Growth in Sanitation Spending over Time



Note: The figure displays the average per capita ongoing spending on each item each year within the main regression sample. "Top tercile in 1872" includes the top tercile of towns according to the level of ongoing spending in the first year for which data is available.

Table B.2: Details of Local Plebiscites

Town	Year	Topic	Result
Birmingham	1855	Borrowing for improvement purposes	votes: 171 for, 3,425 against
Birmingham	1860	Borrow money for sewage and other improvements	votes: 6,531 for, 3,802 against; voters: 2,681 for, 2,586 against
Birmingham	1874	Increase tax for sanitary purposes	votes: 2,894 for, 5,410 against; voters: 869 for, 2,664 against
Birmingham	1882	Consolidate previous acts, and added powers re infectious disease	votes: 5,340 for, 5,074 against; voters: 3,216 for, 2,597 against
Bristol	1900	Drainage of sewage	votes: 8,678 for, 19,205 against
Cheltenham	1903	Sewage and sanitation	votes: 2,555 for, 5,904 against; voters: 1,685 for, 4,785 against
Chester	1882	Increase borrowing powers	votes: 3,194 for, 3,307 against
Croydon	1890	General improvements	votes: 10,618 for, 2,722 against
Driffield	1882	Control of water works	votes: 406 for, 834 against
Eastbourne	1885	General improvements, including sanitation	votes: 1,553 for, 2,129 against
Filey	1904	Improvements, including water and gas	votes: 295 for, 215 against
Frome	1874	Water supply	votes: 625 for, 823 against
Leicester	1886	Water supply	votes: 14,357 for, 6,218 against
Liverpool	1874	Water supply	votes: 9,835 for, 34,424 against
Liverpool	1879	Water supply	votes: 21,455 for, 18,853 against
Lowestoft	1896	Purchase water, gas and market company	votes: 2,001 for, 895 against
Maidstone	1896	Purchase of water company	votes: 1,427 for, 3,489 against
Melton Mowbray	1885	Water supply	votes: 309 for, 525 against
Reading	1886	Enlarge boundaries and more powers for water / sewers	votes: 3,885 for, 2,005 against; voters: 3,008 for, 1,548 against
Sheffield	1873	Water supply	votes: 11,401 for, 5,979 against
Sheffield	1878	General improvements	votes: 11,770 for, 15,390 against
Sheffield	1887	Water supply	votes: 25,641 for, 3,604 against
Sunderland	1885	Improvement bill, including sanitary measures	votes: 6,363 for, 5,004 against; voters: 5,443 for, 4,444 against
Tynemouth	1891	Purchase water company	votes: 2,117 for, 2,206 against
Warrington	1890	Purchase water company	votes: 2,488 for, 1,685 against
Wigan	1905	General improvements	votes: 2,128 for, 1,864 against

Note: See text for discussion of how information was collected and plebiscites selected.

C Identification

The identification assumption underlying the empirical analysis in Sections 3 and 4 is that the residual variation in the franchise was exogenous to town spending decisions, conditional on controlling for fixed effects and a set of control variables. This appendix discusses potential threats to that assumption, drawing on a range of historical and empirical evidence

There are three major threats to the identification strategy. First, the variation in the franchise may capture variation in town demographic characteristics, due to differential effects of the national regulations. Second, there may be reverse causality if the bodies determining the franchise were motivated by town spending decisions. Third, the decisions of poor law authorities may have indirectly been correlated with spending choices of municipal councils due to some common local characteristic. To address these concerns, the appendix delves more deeply into the process that determined the franchise, and shows that there is no evidence that—after accounting for our demographic controls—the residual variation in the franchise is correlated with other observable town characteristics.

The first subsection provides a historical discussion of the sources of variation in the franchise. The second subsection investigates the correlates of the level of the franchise and the change in the franchise during the 1866–1878 reforms, providing reassurance that the set of control variables account for the endogenity in the franchise variable. The third subsection argues that one important driver of variation in the franchise—the treatment of compounders—was exogenous to town spending decisions, drawing on both historical evidence and empirical analysis of parish-level decision-making. The final subsection presents an instrumental variables analysis, providing further evidence that franchise extension caused reductions in government spending and taxation.

C.1 Sources of Variation in the Franchise

As a starting point, this sub-section explains three sources of the observed variation in the franchise. First the national regulations that governed voting rights at the start of the analysis period. Second, reforms to those regulations, particularly between 1869 and 1878. Third, the decisions of local authorities—not town councils—that implemented those regulations.

Regulations Governing the Franchise According to the 1835 Municipal Corporations Act, the right to vote in municipal elections was subject to five major conditions. Individuals had to be heads of household and male. They also had to meet residence requirements, have paid taxes, and have not received poor relief in the year prior to each election. Unlike in Parliamentary Elections there was no restriction according to the value of property occupied, but many—generally poor—citizens were nevertheless disenfranchised. Both residence and

tax-paying requirements were severe: voters must have been resident for at least three years (rather than one year in Parliamentary elections) and have paid local taxes for two and a half years (rather than 6 months). As a result, in the 1830s the Parliamentary franchise tended to be wider than the municipal franchise despite having a property qualification. (Keith-Lucas, 1952, p.61).

Voters were sometimes disenfranchised if they paid taxes indirectly through their landlord. Collecting tax from poor renters was expensive and so in some cases the taxation authorities collected tenants' taxes from their landlord. This practice was known as "compounding", and the renters paying taxes in this way thus were "compounders". Whether compounders had the right to vote was legally ambiguous, leading to considerable variation in whether these renters were actually enfranchised across and within towns.

Reforms to the National Regulations Major reforms in 1869, consolidated in 1878, eased the voting restrictions, and led to significant expansions of the male franchise—the median level grew by 20% between 1866 and 1885.[24] This growth resulted from two major changes. First compounders' right to vote was, in principle, enshrined in law although lawyers continued to fight over the issue until at least the late 1870s (Keith-Lucas, 1952, p.74). Second, both the length of residence and tax-paying requirements were reduced by two years. Women also gained the right to vote in 1869, although the restriction to heads of household meant that they remained a small proportion of the electorate.

It is clear from this discussion that some of the variation in the franchise is driven by the interaction between town demographic characteristics and the national regulations. Head-of-household and residence requirements would be more stringent in fast-growing, overcrowded cities. The male franchise will also be mechanically limited by the extent to which woman are heads of household, and hence able to vote. Population is linked to the franchise by definition and might also be related to economies of scale in the provision of sanitation. Controlling for these characteristics is thus an important part of the identification strategy.

Local Decision-Making over Voting Rights A significant potential threat to the identification strategy is reverse causality resulting from local authorities controlling who had the right to vote. Although this concern is mitigated by the national reforms—which took away much control over these decisions—it is possible that local authorities varied in how rapidly or effectively they implemented these changes. Further, if these factors determined the level of the franchise before 1869, they could also affect the change in the franchise caused by the reforms.

[24]Since the right to vote was only given to heads of households—rather than to individuals—these figures indicate that by the 1880s a very high proportion of households had the right to vote in these towns. This also explains the low level of the female franchise since few women were household heads (less than 7% in 1881).

Concern over reverse causality is alleviated, however, by the fact that the level of the franchise was not determined by municipal councils, but instead by the authorities responsible for poor relief. In particular, decisions over who to tax and how taxation was implemented were made by a combination of officials of Poor Law Union and local vestries.[25] Local decision-making was particularly important in determining whether poor tenants were compounded, and whether any compounders gained the right to vote. Such decisions were in the hands of local vestries, who had to obtain authority from Parliament before engaging in compounding.

Historical evidence shows that these bodies did have some control over who was allowed to vote (Fraser, 1976; Salmon, 2002). However, there is little reason to believe that these authorities were particularly concerned with municipal concerns when making these decisions. Not only were these authorities governed separately to town councils, they were also elected separately and under a different franchise. Poor Law Guardians, for example, were elected under a graduated franchise whereby the wealthy could receive up to 12 votes each (Lizzeri and Persico, 2004). Further, the poor law authorities governed jurisdictions with boundaries that were generally very different to the areas governed by town councils. Most towns formed only a small part of a much larger poor law union: in 1881 88% of municipal boroughs fell within a single Poor Law Union, with the median town comprising only approximately 36% of the population of that Poor Law Union.[26] The second set of relevant poor law authorities, local vestries, governed parishes which comprised only parts of municipal boroughs. Only 22% of towns were comprised of a single complete parish in 1871, with almost 50% containing more than 3 parishes. Several larger towns, including as Norwich and York, contained over twenty parishes.[27]

In summary, local decisions over the franchise were made independently of considerations about town spending. Of particular importance, local authorities varied in how they treated compounders without considering the effects on municipal spending. Further, both ambiguity of national legislation and heterogeneity in the overlap of parish and town boundaries provided added randomness into the effects of those decisions on voting rights. The following two sub-sections offer further support for these claims, through first discussing the correlates of the franchise and then analyzing parish decisions over compounding in further detail.

[25]Essentially, the municipal councils would set a tax rate that sat on top of the poor law taxes that were set and collected by the poor law authorities. Once the Poor Law Guardians had set a tax rate, vestries were charged with assessing property and collecting the tax.

[26]Poor Law Union population is estimated using the average population of Registration Districts 1871-1880, which were almost always coextensive with the Poor Law Unions.

[27]Calculations using House of Commons (1872).

C.2 Correlates of the Franchise

This subsection directly analyzes the residual variation that is at the center of the identification strategy. I investigate the correlates of the franchise, testing whether a higher franchise is associated with factors that could determine town spending. As anticipated, the franchise is correlated with some demographic characteristics of towns. However, after controlling for those characteristics, neither the level of nor change in the franchise is associated with possible confounding variables. While we cannot test the identification assumption directly, this analysis offers reassurance that the variation in the franchise is idiosyncratic.

Table C.3 displays regressions of two franchise variables against a number of town-level characteristics. In columns (1)–(3) the dependent variable is the level of the franchise in 1866 (the start of the analysis period); in the remaining columns the dependent variable is the *change* due to the 1866–1878 national reforms. Three sets of independent variables are included. First, demographic characteristics, which we might expect to be related to the franchise due to the regulations regarding head of household characteristics or residency, or due to variable construction. Second, a number of other characteristics that could be related to parish decision-making and also the demand for public goods: town tax base, town spending in the early 1850s, the share of the population working in agriculture (and hence industrial demand for public goods), or similarity between parish and town boundaries. Finally, some specifications include the percentage of the electorate that were compounders before 1866 to demonstrate how enfranchsing this group of voters affected the franchise.

The results show, as expected, that the municipal franchise is associated with both the proportion of compounders in the municipal electorate and town demographic characteristics. The proportion of compounders is positively correlated with the franchise before 1866, demonstrating that these poor citizens gaining the right to vote was associated with a broader franchise. We can also see that the 1869–1878 reforms had a bigger effect in towns where these citizens had not already been enfranchised in 1866, because part of their effect was to standardize the treatment of compounders across the country. Finally, we can see that the coefficient on this variable is robust to controlling for other municipal characteristics—further evidence that parishes' decisions were uncorrelated with municipal factors.

As expected, the franchise variables are correlated with demographic factors, likely reflecting the effect of national reforms discussed above. Urban crowding is negatively associated with the franchise, reflecting the head of household and residence requirements, while the relationship with population may capture both the construction of the franchise variable, and the fact the poor could move around within big cities and hence fail the residence requirements. For this reason, my preferred regression specifications include these characteristics as controls.

In contrast, the coefficients on the other town characteristics are consistently small and insignificant (individually and jointly), offering support that the residual variation is exogenous to town spending. One threat to the identification is that—even if they did not deliberately manipulate the franchise—local-decisions over the franchise may have indirectly been correlated with municipal spending due to local characteristics. For example, poorer areas may have been more willing to extend the franchise and also spent less on public goods. The results here suggest that this was not the case, with no evidence that the variation in the franchise is correlated with a range of variables that could affect demand for municipal spending. We also do not see any relationship between local politics—having a local Conservative mayor—and the change in the franchise during the 1869–1878 reforms (specification 7), consistent with local elites being unable to control the effect of the reforms.

C.3 Adoption of the Small Tenements Rating Act

The previous subsections have argued that since parish authorities were independent of town councils we can consider their decisions exogenous to municipal spending and taxation decisions. This subsection provides further evidence for that claim, by discussing one major source of exogenous variation in the franchise—variation in how different parishes treated compounders. In particular, I analyze the adoption of one particular piece of legislation relating to compounding—the 1850 Small Tenements Rating Act (henceforth STRA). The STRA is important because it led to a significant expansion in both the use of compounding, and consequently the extent of the male franchise—analyzing its adoption can thus inform us about the motivations of parish vestries making decisions that could affect voting rights.

The STRA facilitated compounding by allowing vestries to simply "opt-in" to gain necessary legal authorization from Parliament. Prior to 1850, parishes needed to obtain a Local Act of Parliament—a process that was both costly and difficult—in order to engage in compounding. The STRA removed this requirement and was rapidly adopted in many areas.

Most importantly for our purposes, the adoption of the STRA led to an unanticipated and heterogeneous expansion of the the municipal franchise. If compounding was authorized by other acts it was legally ambiguous whether compounders had the right to vote. In practice, local authorities had to turn to the courts, who could come to differing decisions over similar questions in different parts of the country. In contrast, due to a late amendment, compounding under the STRA qualified indirect taxpayers to vote in municipal elections—and as a result the adoption of the STRA led to significant expansions in the expansion of the municipal franchise: across towns where it was (at least partially) adopted, the electorate grew from 101,338 to 254,118 by 1866 (Keith-Lucas, 1952, p.67).

The primary motivation for STRA adoption appears to have been simply to maximize revenue: "as [the STRA] bestows no parochial votes on the tenement holders, it is not

Table C.3: Variation in the franchise is idiosyncratic after controlling for urban crowding, population growth and population.

	Dependent Variable (% Adult Male Population)						
	1866 Male Franchise			Δ Male Franchise 1866-79			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Compounders (% Electors)		4.22***	4.29***		-3.39***	-3.80***	-4.75***
		(0.989)	(0.999)		(1.126)	(1.157)	(1.203)
Main Regression Controls							
Population (Log)	-1.93	-2.80*	-2.67*	2.73	3.19*	3.55*	3.70*
	(1.795)	(1.587)	(1.587)	(2.063)	(1.906)	(1.902)	(1.953)
Population Growth	-0.15	0.09	0.35	0.68	0.56	0.17	0.39
	(1.400)	(1.280)	(1.423)	(1.461)	(1.427)	(1.552)	(1.682)
Urban Crowding	-5.30***	-4.23***	-3.73***	0.06	-0.77	-1.71	-2.17*
	(0.979)	(0.923)	(0.863)	(1.239)	(1.354)	(1.146)	(1.161)
Other Town Characteristics							
% Agriculture	0.18	-0.30	0.09	-0.49	-0.28	-0.10	-0.63
	(1.707)	(1.559)	(1.563)	(1.782)	(1.688)	(1.712)	(1.791)
1850 Tax Base p.c.	-0.13	-0.03	0.24	-0.99	-1.08	-1.04	-1.17
	(0.924)	(0.766)	(0.792)	(0.931)	(0.856)	(0.882)	(0.869)
No. Parishes	-0.09	-0.02	0.01	0.05	-0.00	-0.01	-0.04
	(0.168)	(0.154)	(0.157)	(0.190)	(0.172)	(0.170)	(0.167)
Coterminous with Parish	-0.81	-0.44	-0.63	1.86	1.73	1.87	1.27
	(3.090)	(2.808)	(3.111)	(2.853)	(2.747)	(3.206)	(3.129)
pre-STRA Spend p.c.			-0.89			0.81	1.05
			(0.843)			(0.844)	(0.816)
Had Conservative Mayor							0.95
							(2.621)
No. Towns	146	146	139	141	141	134	123
R^2	0.25	0.36	0.35	0.10	0.18	0.20	0.26
Joint Significance of other town characteristics	0.98	1.00	0.93	0.79	0.72	0.68	0.63

Robust standard errors in parentheses. Coefficients for all continuous independent variables are standardized. The dependent variable in specifications 1–3 is the level of the male franchise in 1866, and in specification 4–7 is the change between 1866 and 1879. *Compounders (% Electors)* is the proportion of municipal electors that were compounders in 1866. *No. Parishes* is the number of parishes in the town; *Coterminous with Parish* is a dummy variable that the town consisted of a single parish. The table only includes towns that were also Parliamentary Boroughs, due to data availability.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

surprising that the vestries should look at the question of its adoption merely in a financial point of view"(House of Lords, 1859, p.vii). Parishes could probably not have foreseen the effect on voting rights in any case, given that Parliament itself failed to anticipate the effect of the STRA on the franchise (Keith-Lucas, 1952). In fact, parishes often came to different decisions within a town: across a sample of 85 towns, in only half (53%) were parishes' STRA decisions homogeneous. This heterogeneity across parishes thus provides a further source of variation in the franchise at town-level.

The evidence in Table C.4 supports the claim that parishes were not motivated by municipal concerns when deciding whether to adopt the STRA. Here, I analyze the take-up of the Act across all parishes in towns represented in Parliament—most, but not all, of which were also represented in Parliament. I first simply consider whether we see different patterns of adoption in parishes within municipal boundaries, then test directly whether adoption is associated with either the pre-STRA burden of municipal spending[28] or two municipal characteristics that could be correlated with spending, occupational structure and the tax base per capita.

None of the six specifications suggest that municipal factors influenced the decisions determining the extent of the franchise. As discussed above, the STRA enfranchised compounders for municipal, but not other, elections; thus we would expect a different adoption pattern within municipal boundaries if the STRA's effect on voting rights was an important concern. There is no evidence of such an effect: the coefficient regarding being within a municipal boundary is statistically insignificant and close to zero, both across the whole sample (specifications 1 and 2) and when focusing on the variation within towns where the parliamentary and municipal boundaries differed (specifications 3 and 4). Further, specifications 5 and 6 show no evidence that the willingness to take-up the STRA was unaffected by the burden of municipal spending or town characteristics associated with the demand for public goods.

These results, together with those results in the previous subsection, offer support for the identification strategy used in the main text. While we cannot specify the exact factors that determined local franchise decisions, there is very little evidence that those decisions were correlated in any way with a number of town-level factors that would determine the demand for public goods expenditure.

[28]The *Local Taxation Returns* used to construct the main dataset are not available for this earlier period; data on municipal spending was collected from a set of parliamentary papers that sporadically report municipal borough spending in the 1840s and 1850s.

Table C.4: Parish decisions affecting franchise were independent of town characteristics.

	DV = Implemented Small Tenements Rating Act					
	(1)	(2)	(3)	(4)	(5)	(6)
Within MB boundary	-0.01	-0.04	-0.05	-0.02		
	(0.071)	(0.071)	(0.083)	(0.055)		
Parish Population (Log)		0.06*	0.10*	0.14**		0.15***
		(0.032)	(0.052)	(0.055)		(0.042)
Parish Crowding		-0.02*	0.01	0.00		-0.01
		(0.012)	(0.015)	(0.005)		(0.012)
Parish Popn <100		-0.13*	0.07	0.04		-0.09
		(0.073)	(0.096)	(0.072)		(0.120)
Town Spend p.c. pre-STRA					0.01	0.04
					(0.071)	(0.063)
Town Tax Base p.c. pre-STRA					-0.03	-0.02
					(0.050)	(0.046)
Town % Agricultural					-0.03	-0.05
					(0.082)	(0.074)
Town Population (Log)					-0.10	-0.17**
					(0.071)	(0.067)
Sample	All	All	Different Bounds	Different Bounds	MBs Only	MBs Only
No. Observations	1370	1370	420	420	702	702
R^2	0.00	0.04	0.03	0.50	0.02	0.12
Town Fixed Effects	N	N	N	Y	N	N

The unit of observation in this table is the parish, with the dependent variable equaling one if the parish chose to implement the Small Tenements Rating Act. "All" includes parishes in all parliamentary boroughs, while "different boundaries" includes only boroughs with differing municipal and parliamentary boundaries. "MBs only" excludes parishes in parliamentary boroughs which were not also municipal boroughs. Coefficients for all continuous variables are standardized. Standard errors are presented in parentheses and clustered by town. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.4 Instrumental Variables Analysis

This subsection presents the results of an instrumental variables analysis providing further additional reassurance that the relationship between enfranchising the poor and government spending is causal. The primary identification strategy argues that complexity of the franchise and tax-paying regulations led to quasi-random variation in who exactly was enfranchised. Even if local authorities had some control over who was enfranchised pre-reform, they could not control or anticipate who would be enfranchised by the reforms. The analysis in this subsection goes further by isolating one part of that complexity—the interaction between the pre-reform treatment of compounders and the reduction in the length of resi-

dence requirements—as a source of exogenous variation in whether the 1869–1878 reforms increased the share of poor voters in the electorate.

To do so, I estimate specifications of the form:

$$\Delta y_i = \alpha + \beta poorEnfranchisedByReform_i + \gamma X_i + \epsilon_i$$

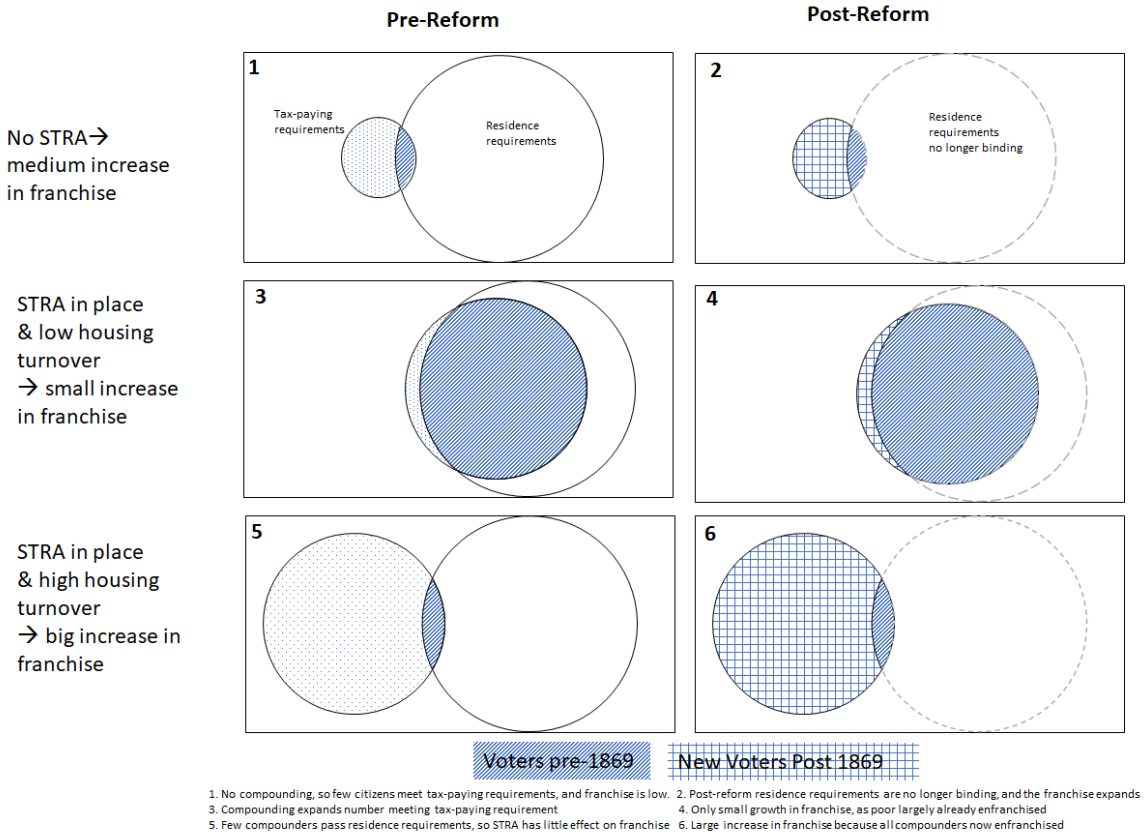
where i are towns, and $poorEnfranchisedByReform$—which is potentially endogenous—is defined in the same way as in Specification (2) in Section 4. Δy_i is the change in the average tax or government spending per capita between the pre-reform (1867–1872) and post-reform periods (1873–1879).

The IV strategy is based around two town features that would have shaped the effects of the 1869–78. First, the effect of the reform would have been greater where a large number of citizens failed the pre-reform residence requirements. Second, the presence of compounding meant that, following the reform, more citizens met the tax-paying requirements and hence could potentially be enfranchised. The identification strategy in the main analysis assumes that the first of these factors is endogenous (and accounted for by control variables) but that the second is exogenous to town decisions over government spending. The analysis in this section exploits the interaction between the two as a source of exogenous variation in whether poor citizens were enfranchised—we would expect the reforms to enfranchise more poor voters where *both* compounding was in place and many citizens had been disenfranchised by the long residence requirements.

Figure C.5 illustrates the argument using a simplified version of the 1869 reforms. Pre-reform (the left-hand side of the figure) citizens have to meet both tax-paying and residence-requirements to be able to vote. Post-reform, the residence requirements are removed and all those meeting the tax-paying requirement have the right to vote. For simplicity the figure ignores the parallel reduction in the length of tax-paying requirements.

The second and third rows demonstrate the effect of adopting compounding using the provisions of the Small Tenements Rating Act—under which compounders qualified as municipal voters—before 1869. The STRA expanded the number of citizens meeting the tax-paying requirements pre-reform, and so increased the pool of potential voters. However, as illustrated in panels 3 and 5, the extent to which the actual franchise was affected would depend on the proportion of voters also meeting residence requirements. If most voters met the residence requirements (panel 3) the pre-reform franchise would expand considerably. On the other hand, if many voters failed (panel 5) then the pre-reform franchise would be little affected by adopting STRA. The effect of the STRA on the franchise thus depends on

Figure C.5: Illustration of IV Strategy.



how binding the residence requirements are.

Similarly, the effect of the reform is shaped by both whether the STRA is in place, and whether many voters are disenfranchised by the residence requirements. The reform has little effect on the franchise where the STRA is in place and most voters meet the pre-reform residence requirements (second row)—the change in the franchise may be lower than in the non-STRA scenario (first row). When the residence requirements have a large impact, in contrast, the reforms lead to a significant expansion of voting rights (bottom row).

I construct a set of instruments to capture the interaction between the pre-reform treatment of compounders and how binding residence requirements were. Unfortunately, it is not possible to measure precisely how many households were compounded; instead I identify the treatment of compounders using a 1867 Parliamentary paper identifying the share of male occupiers that could legally be compounded under different types of legislation adopted in each town. Importantly, this distinguishes voters compounding under the STRA (who should, in principle have had the right to vote) and those compounded under other par-

liamentary acts. This provides four measures of different forms of compounding—to proxy for the strength of residence requirements I interact each of these with the level of town crowding and population growth.

Table C.5 presents the first stage estimates for both the main measure of poor voters (left-hand column) and the continuous measure of the increase in the share of poor voters. We can see that the 1869–78 reforms were less likely to enfranchise poor voters where a high proportion of the population could be compounded under the STRA—consistent with the STRA having enfranchised some poor voters even prior to the reform. However, the negative relationship is ameliorated in towns with high urban crowding—in line with the predictions in Figure C.5. The relationships with the proportion of citizens compounded under non-STRA Acts is less clear cut, which could reflect ambiguity in these Acts, or simply noise in the data given the relatively few towns with such Acts. Most importantly for our purposes, the instruments are strong, with Kleibergen-Papp statistics of over 10.

Table C.6 presents the second stage results. The first and fourth column present simple OLS specifications—as we would expect, the results are in line with those in Figure B.3. The second and fifth columns include specifications where the four variables regarding pre-reform treatment of compounders are treated as exogenous—i.e., they are included only in the first stage regressions. The third and sixth columns include only the interaction terms between the treatment of compounders and urban growth as instruments—here the effect of enfranchising the poor is identified only through the way pre-reform treatment of compounders interacted with residence requirements (as proxied by town characteristics). We see consistent evidence that increasing the share of the poor in the electorate led to small town government.

Table C.7 presents the results of the IV regressions using the robustness measure of poor voters. We can see that the results are similar in magnitude, although statistically slightly weaker.

Table C.5: First Stage Regressions.

	DV = Poor Enfranchised By Reform	
	Indicator	Continuous
% Compounders All-STRA	-0.07***	-0.10***
	(0.011)	(0.024)
x Crowding	0.01***	0.02***
	(0.002)	(0.004)
x Pop. Growth	-0.18	-0.64
	(0.198)	(0.456)
% Compounders Part-STRA	-0.07***	-0.11***
	(0.014)	(0.036)
x Crowding	0.01***	0.02***
	(0.002)	(0.007)
x Pop. Growth	-0.32	-0.57
	(0.270)	(0.730)
% Compounders No-STRA Low-Value	0.02	0.10
	(0.044)	(0.077)
x Crowding	-0.00	-0.01
	(0.011)	(0.018)
x Pop. Growth	-0.50	-2.38***
	(0.353)	(0.476)
% Compounders No-STRA High-Value	-0.22**	-0.07
	(0.105)	(0.175)
x Crowding	0.03	0.01
	(0.018)	(0.038)
x Pop. Growth	3.41*	-1.08
	(1.902)	(4.974)
Controls	Y	Y
No. Observations	98	98
Kleibergen-Papp	10.3	60.2

The four variables relating to percentage of compounders reflect the proportion of male occupiers in each town that could be compounded under various Acts. "% Compounders All-STRA" refers to towns in which all parishes had implemented the Small Tenements Rating Act, and reflects the percentage of occupiers that could be compounded under that Act. % Compounders Part-STRA" refers to towns in which all parishes had implemented the Small Tenements Rating Act, and reflects the percentage of occupiers of housing valued at under £10 that could be compounded under that Act or other Acts. % Compounders No-STRA refers to towns where no parishes had implemented the STRA, with the two variables referring to houses of under or £10 in rental value. Controls include 1871 crowding, population growth, and town population (in six categories). Robust standard errors are presented in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Parametric Tests of Inverted-U-Shaped Relationship

This appendix presents additional robustness tests of the inverted U-shaped relationship shown in Section 3. First quadratic regression specifications show that the results are robust to the inclusion of alternative control variables, and to varying the sample of towns included

Table C.6: 2SLS Regressions provide further evidence that enfranchising poor reduced size of government.

	DV = Δ Tax Receipts per Capita			DV = Δ Public Goods Spend per Capita		
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
Poor Enfranchised By Reform	-0.09**	-0.20***	-0.20***	-0.13*	-0.23**	-0.26**
	(0.043)	(0.070)	(0.075)	(0.071)	(0.104)	(0.103)
% Compounders All-STRA			0.00			-0.00
			(0.001)			(0.002)
% Compounders Part-STRA			-0.00			0.00
			(0.001)			(0.001)
% Compounders No-STRA Low-Value			0.00			0.00
			(0.002)			(0.002)
% Compounders No-STRA High-Value			-0.01			-0.02
			(0.009)			(0.018)
Controls	Y	Y	Y	Y	Y	Y
No. Observations	98	98	98	98	98	98
Kleibergen-Papp		8.5	10.3		8.5	10.3

Note: The four variables relating to percentage of compounders reflect the proportion of male occupiers in each town that could be compounded under various Acts. "% Compounders All-STRA" refers to towns in which all parishes had implemented the Small Tenements Rating Act, and reflects the percentage of occupiers that could be compounded under that Act. % Compounders Part-STRA" refers to towns in which all parishes had implemented the Small Tenements Rating Act, and reflects the percentage of occupiers of housing valued at under £10 that could be compounded under that Act or other Acts. % Compounders No-STRA refers to towns where no parishes had implemented the STRA, with the two variables referring to houses of under or £10 in rental value. Controls include 1871 crowding, population growth, and town population (in six categories). Robust standard errors are presented in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in the analysis. The quadratic specification is useful due to ease of interpretation; however, the second subsection demonstrates that the inverted-U-shaped relationship is also strongly supported when using fractional polynomials to allow for a more flexible functional form.

D.1 Quadratic Specifications

The existence of an inverted U-shaped relationship is also demonstrated when modeling the function $g(x)$ in specification (1) with a standard quadratic specification, as shown in the first six columns of Table D.8. These specifications support the graphical results in Figure III and add additional results with sanitation expenditure per capita—available from 1872 onwards—as the dependent variable. The quadratic terms are nearly always both individually and jointly statistically significant, and meet Lind and Mehlum (2010)'s "U-test" of the joint restriction that the relationship should be positive at lower franchise levels and negative at higher levels. The coefficient on the linear term in specification (6) is narrowly insignificant at conventional levels, reflecting limited data at low levels of the franchise in these specifications, because sanitation data is not available in the early years of the dataset. These parametric estimates thus provide further evidence that extending voting

Table C.7: IV results are robust to using continuous measure of change in share of poor voters.

	DV = Δ Tax Receipts per Capita			DV = Δ Public Goods Spend per Capita		
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
Δ Poor Enfranchised	-0.03*	-0.10***	-0.12***	-0.05*	-0.11**	-0.10*
	(0.018)	(0.034)	(0.038)	(0.027)	(0.051)	(0.052)
% Compounders All-STRA			-0.00		-0.00	
			(0.001)		(0.002)	
% Compounders Part-STRA			-0.00		0.00	
			(0.001)		(0.002)	
% Compounders No-STRA Low-Value			0.00		0.00	
			(0.002)		(0.002)	
% Compounders No-STRA High-Value			-0.00		-0.01	
			(0.011)		(0.021)	
Controls	Y	Y	Y	Y	Y	Y
No. Observations	98	98	98	98	98	98
Kleibergen-Papp		67.1	47.9		47.9	67.1

Note: Coefficients for "Δ Poor Enfranchised" represent the effect of a 10% increase in the share of poor voters. See notes to Table C.6 for further details. Robust standard errors are presented in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

rights led to significant declines in the town spending after approximately half of the male population was enfranchised.

The final two columns of Table D.8 present the results of a placebo test, showing that the inverted-U relationship is not driven by the structure of the data. The dependent variable here is the per capita town receipts from property sales and rents—a variable over which the town councils had limited control because it was determined by the value of their estates, which varied considerably across towns (Millward and Sheard, 1995). As we can see, there is no evidence of a relationship.

Table D.9 demonstrates the robustness of the results to different sets of control variables. Specificiation (1) repeats the main results for ease of comparison. Specifications (2)–(4) are similar to semi-parametric specifications reported in Figure III. Specifications (2) and (3) for each dependent variable control for population (either as a quartic, while or as town size–decade fixed effects), while specification (4) includes the lagged dependent variable. The final two specifications include additional tests that the results are not driven by the temporal element of infrastructure investment—for instance, the fact that large investments in infrastructure could have happened when the franchise was low, meaning less investment was needed later once the franchise had increased. Specification (5) directly controls for the level of loans outstanding—a proxy for infrastructure investment used by contemporaries (Wohl, 1983)—while specification (6) allows for a more flexible relationship by including

an interaction between the level of investment early in the period and decade fixed effects. Controlling for the lagged dependent variable also leads to lower estimated effect sizes: which could reflect the franchise variable (which is lagged three years) affecting spending in previous years, or the fact that this leads to the loss of observations, particularly before the 1869 reforms when the franchise was low. Overall, however, there is clear and consistent evidence of the inverted-U-shape in all specifications.

Table D.10 repeats the main specifications within subsamples of the dataset in order to test possible alternative explanations for the results. Specification (1) for each dependent variable limits the sample to observations below the estimated turning points identified in Table D.8. In two-way fixed effects models it is not straightforward to disentangle the contribution of over-time versus cross-sectional variation (Kropko and Kubinec, 2020), and so we cannot rule out that the inverted-U-shape is driven by heterogeneity across towns, rather than the effect of expanding the franchise within a town. Limiting the sample in this way reduces the extent of cross-sectional variation and ensures that the inverted-U-shape involves towns moving "over the turning point". Both specifications support the inverted-U-shape, although the evidence is weaker in the case of taxation per capita—likely reflecting the limited data available at low levels of the franchise in this case leading to noisy estimates.

The remaining specifications in Table D.10 address potential concerns that the results are driven by towns with particular characteristics. Specification (2) removes towns in the top and bottom deciles of population from the sample—we might be concerned that such towns have differing needs for public goods (or benefit from economies of scale)—and also mechanically vary in the franchise since population acts as the denominator. Similarly, specification (3) removes those in the top or bottom decile of rateable value (tax base) per capita in 1873, in case differences in town resources explain both the franchise and ability to invest in public goods. Specifications (4)–(6) then test whether the inverted-U-shape could be explained by migration patterns across towns. Migration could affect the franchise (for instance due to residence requirements), while also changing the demand for public goods if, for example, wealthier citizens are more likely to leave a town. Specification (4) removes towns in the top and bottom deciles of population growth, while Specification (5) removes towns with outmigration (negative population growth). Specification (6) focuses on possible changes in the composition of the population by removing towns in the top and bottom deciles of change in the share of the population in commerce or professions—i.e., relatively wealthy citizens. As we can see, the finding of an inverted-U-shape is robust across these different subsamples with particularly strong support for the finding that enfranchising the poor reduced the size of government.

Table D.11 shows that the inverted-U-shape relationship is also evident when restricting

the sample to focus on the effects of the national reforms to the franchise regulations discussed in Section A.4. As with specification (1) in Table D.9 this focuses the estimation on the clearest source of exogenous variation in the extent of the franchise. In these specifications, the data is placed into six equally spaced five-year time periods (1867-1871, 1872-1876, etc.) occurring around the reforms. Where possible, these specifications use franchise data only from years in which the number of electors was reported in order to estimate the average level of the franchise in each 5-year period. Again, the results demonstrate an inverted U-shaped relationship with a turning point that occurs when around half of adult men were enfranchised.

Table D.8: Quadratic specifications show an inverted U-shaped relationship.

	Dependent Variable (per Capita, % of Median):							
	Tax Receipts		Public Goods Spend		Sanitation Spend (from 1872 only)		Property Receipts (Placebo, from 1872 only)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Inverted-U Shape								
Turning Point (%)	43	43	52	53	48	45	50	39
F-test (p-val)	0.00	0.00	0.00	0.00	0.00	0.01	0.90	0.91
U-test (p-val)	0.00	0.01	0.00	0.00	0.04	0.12	0.34	0.42
Increase ($fran \leq T$)	13	10	42	41	34	20	-	-
Decrease ($fran \geq T$)	36	30	36	28	50	43	-	-
Panel B: Regression Details								
Male Franchise	0.23***	0.18***	0.46***	0.41***	0.46**	0.31	-0.33	-0.16
	(0.073)	(0.067)	(0.128)	(0.124)	(0.218)	(0.218)	(0.745)	(0.651)
Male Franchise Sq	-0.03***	-0.02***	-0.04***	-0.04***	-0.05**	-0.03*	0.03	0.02
	(0.007)	(0.007)	(0.012)	(0.012)	(0.019)	(0.018)	(0.075)	(0.067)
No. Observations	4810	4705	4810	4705	4181	4142	4221	4178
No. Towns	150	149	150	149	150	149	150	149
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Town Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y	Y	Y	Y
Occupational Controls	N	Y	N	Y	N	Y	N	Y
Tax Base per Capita	N	Y	N	Y	N	Y	N	Y

Note: Panel A shows the details of the inverted-U shape estimated from the quadratic specifications using annual financial data for 1867–1900. "F-test" relates to a test of joint significance of the two franchise variables. U-test relates to the test of U-shaped relationships from Lind and Mehlum (2010). Panel B shows details of the regressions. Franchise coefficients represent the effect of a 10% increase in the franchise. Standard errors are clustered by town and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.9: Quadratic Specifications with Additional Controls

	Dependent Variable (per Capita, % of Median):											
	Tax Receipts						Public Goods Expenditure					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Inverted-U Shape												
Turning Point (%)	43	41	41	42	42	45	52	54	54	49	53	54
F-test (p-val)	0.00	0.01	0.05	0.00	0.00	0.00	0.00	0.04	0.06	0.01	0.01	0.00
U-test (p-val)	0.00	0.03	0.09	0.01	0.02	0.00	0.00	0.01	0.02	0.01	0.00	0.00
Increase ($fran \leq T$)	13	7	6	4	9	14	42	31	29	11	36	51
Decrease ($fran \geq T$)	36	27	23	12	29	29	36	20	17	13	25	32
Panel B: Regression Details												
Male Franchise	0.23***	0.15**	0.12*	0.07***	0.17**	0.21***	0.46***	0.31**	0.28**	0.14***	0.36***	0.50***
	(0.073)	(0.065)	(0.073)	(0.024)	(0.066)	(0.070)	(0.128)	(0.121)	(0.117)	(0.050)	(0.113)	(0.134)
Male Franchise Sq	-0.03***	-0.02***	-0.01**	-0.01***	-0.02***	-0.02***	-0.04***	-0.03***	-0.03**	-0.01***	-0.03***	-0.05***
	(0.007)	(0.007)	(0.007)	(0.002)	(0.007)	(0.007)	(0.012)	(0.011)	(0.011)	(0.005)	(0.011)	(0.013)
No. Observations	4810	4810	4810	4719	4795	4672	4810	4810	4810	4719	4795	4672
No. Towns	150	150	150	150	150	143	150	150	150	150	150	143
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Town Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Population Quartic	N	Y	N	N	N	N	N	Y	N	N	N	N
Population_x_Decade FE	N	N	Y	N	N	N	N	N	Y	N	N	N
Lagged DV	N	N	N	Y	N	N	N	N	N	Y	N	N
Loans Outstanding	N	N	N	N	Y	N	N	N	N	N	Y	N
1873 Loans_x_Decade FE	N	N	N	N	N	Y	N	N	N	N	N	Y

Note: See Table D.8 for details of tests and vectors of control variables. Population dummies are excluded when the population quartic is included. Standard errors are clustered by town and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.10: Quadratic Specifications in Subsamples

	Dependent Variable (per Capita, % of Median):											
	Tax Receipts						Public Goods Expenditure					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Inverted-U Shape												
Turning Point (%)	42	43	43	41	44	42	55	52	54	52	52	50
F-test (p-val)	0.24	0.15	0.01	0.02	0.00	0.01	0.01	0.01	0.11	0.01	0.01	0.08
U-test (p-val)	0.14	0.09	0.02	0.04	0.00	0.03	0.01	0.00	0.04	0.01	0.01	0.02
Increase ($fran \leq T$)	7	7	11	8	29	10	47	36	37	40	55	28
Decrease ($fran \geq T$)	23	19	33	31	42	32	25	28	21	33	31	28
Panel B: Regression Details												
Male Franchise	0.13	0.12	0.20**	0.17**	0.22***	0.18**	0.44***	0.38***	0.36**	0.43***	0.33***	0.32**
	(0.099)	(0.076)	(0.083)	(0.078)	(0.069)	(0.083)	(0.144)	(0.130)	(0.170)	(0.145)	(0.106)	(0.149)
Male Franchise Sq	-0.02	-0.01*	-0.02***	-0.02**	-0.03***	-0.02***	-0.04***	-0.04***	-0.03**	-0.04***	-0.03***	-0.03**
	(0.010)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.014)	(0.012)	(0.016)	(0.014)	(0.011)	(0.014)
No. Observations	2775	3848	3836	3910	3931	3846	3920	3848	3836	3910	3882	3846
No. Towns	86	129	148	145	119	119	122	129	148	145	119	119
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Town Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: The table replicates the analysis in Table D.8 for various subsamples of the data. Subsamples are defined as follows for each dependent variable. (1) Excludes towns with franchise in 1866 above the estimated turning point for tax receipts per capita. (2) Exclude towns with population in top or bottom decile of sample. (3) Exclude towns with 1873 tax base per capita in top or bottom decile of sample. (4) Exclude towns with population growth in top or bottom decile of sample. (5) Exclude observations with negative population growth. (5) Exclude towns with in top or bottom decile of of 1861–1901 change in share of population working in commerce or the professions. See Table D.8 for details of tests and vectors of control variables. Standard errors are clustered by town and displayed in parentheses.

Table D.11: Inverted U-shape is evident when aggregating data into 5 year periods.

	Dependent Variable (per Capita, % of Median):			
	Public Goods Spend		Tax Receipts	
	(1)	(2)	(3)	(4)
Panel A: Inverted U-Shape				
Turning Point (%)	52	53	44	44
F-test (p-val)	0.00	0.00	0.00	0.00
U-test (p-val)	0.00	0.00	0.00	0.01
Δ in Dependent Variable:				
Increase ($fran \leq T$)	54	52	17	13
Decrease ($fran \geq T$)	40	31	37	31
Panel B: Regression Details				
Male Franchise	0.58***	0.53***	0.28***	0.23***
	(0.148)	(0.147)	(0.080)	(0.076)
Male Franchise Sq	-0.06***	-0.05***	-0.03***	-0.03***
	(0.014)	(0.014)	(0.008)	(0.008)
No. Obs	976	948	976	948
No. Towns	150	149	150	149
Year Fixed Effects	Y	Y	Y	Y
Town Fixed Effects	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y
Occupation Controls	N	Y	N	Y
Tax Base per Capita	N	Y	N	Y

Note: The table shows the results from splitting the sample into seven 5-year periods and using only non-interpolated franchise data as follows: 1867–71: 1866; 1872–76: mean of 1869 and 1871; 1877–1881: mean of 1873 and 1879; 1882-1887: mean of 1879 and 1883; 1888–1892: 1885; 1892–1896: mean of 1885 and 1897; 1897–1900: 1897. "F-test" relates to a test of joint significance of the two franchise variables. U-test relates to the test of U-Shaped relationships from Lind and Mehlum (2010). See Table D.8 for details of control variables. Standard errors are clustered by town and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.2 Fractional Polynomial Regressions

The inverted-U shape is robust to allowing for a more flexible parametric form than the quadratic specification presented in the previous section. In particular, I model $g(x)$ in Specification (1) using a fractional polynomial (Royston and Altman, 1994) of up to two dimensions. This approach allows for a much wider range of non-linear shapes than achieved with the traditional quadratic relationship—not imposing symmetry and allowing for variation in the parametric form across specifications.[29] In place of the U-test of Lind and Mehlum (2010) used in the quadratic specifications, I implement a cluster bootstrap test of whether there is an internal turning point by re-estimating the fractional polynomial specification with each bootstrap sample—choosing the two-degree specification with the lowest model deviance in each case.[30] The p-value is then calculated as the percentage of bootstrap samples for which the turning point is within the observed distribution of the franchise. This approach allows for the uncertainty in the model imposed when estimating the relationship, and so provides a very strong test of the inverted U-shaped relationship.

Table D.12 shows that the inverted U-shaped relationship is supported for the three variables discussed above. In our case, the best fitting model for total public goods expenditure in our main specification (1) is actually the quadratic function used previously. The shape is slightly different for the tax and sanitation expenditure dependent variables, but the turning points and effect magnitudes remain similar to those in Table D.8. Again, there is no evidence of any relationship with the property receipts: the inverted-U shape is not a result of allowing "too much" flexibility in parametric form.

[29]Specifically, the approach compares the fit of models based on choosing from a set of fractional powers, as well as allowing for logarithmic relationships. Following Royston and Altman (1994), I allow the degrees to be chosen from the set {-2,-1,-0.5,0,0.5, 1, 2 ,3}, where a degree of 0 refers to $log(x)$, and non-zero degrees refer to exponents.

[30]Specifically, for each bootstrap sample, the turning point is classified as internal if both a linear function is rejected in favor of 2 degree fractional polynomial at a 10% significance level, and the estimated 2 degree polynomial implies a turning point within the range of the franchise data.

Table D.12: Inverted-U Shape is robust to flexible parametric form.

	Dependent Variable (per Capita, % of Median):							
	Public Goods Spend		Tax Receipts		Sanitation Spend		Property Receipts	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Inverted-U-Shape								
Turning Point (%)	52	52	44	42	45	45	40	39
F-test (p-val)	0.00	0.00	0.00	0.00	0.00	0.00	0.65	0.86
U-test (p-val)	0.01	0.02	0.01	0.03	0.12	0.20	0.47	0.59
Δ in Dependent Variable:								
Increase ($fran \leq T$)	42	43	12	12	56	48	-148	-38
Decrease ($fran \geq T$)	36	27	37	31	46	39	-27	-34
Panel B: Regression Details								
Powers of Fractional Polynomial Model								
Power: 1st degree	1	.5	.5	-.5	.5	0	-2	-.5
Power: 2nd degree	2	2	3	3	.5	0	-2	-.5
Coefficients on Male Franchise Terms:								
1st Degree	0.46***	1.17***	0.35**	-0.86**	7.77***	2.79**	19.42	-4.16
	(0.128)	(0.354)	(0.139)	(0.374)	(2.844)	(1.222)	(22.862)	(7.923)
2nd degree	-0.04***	-0.02***	-0.00***	-0.00***	-2.21***	-0.97***	-21.82	-6.39
	(0.012)	(0.007)	(0.000)	(0.000)	(0.769)	(0.363)	(28.981)	(11.751)
No. Observations	4810	4705	4810	4705	4181	4142	4221	4178
No. Towns	150	149	150	149	150	149	150	149
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Town Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y	Y	Y	Y
Occupation Controls	N	Y	N	Y	N	Y	N	Y
Tax Base per Capita	N	Y	N	Y	N	Y	N	Y

Note: Panel A shows the details of the inverted-U-shape estimated from the fractional polynomial specifications using annual financial data for 1867–1900. "F-test" relates to a test of joint significance of the two franchise variables. U-test relates to cluster bootstrap of inverted-U shape explained in text. Franchise coefficients represent the effect of a 10% increase in the franchise (lagged 3 years). See notes to Table D.8 for details of control variables. Standard errors are clustered by town and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E Robustness of Results in Section 4

E.1 Comparison of "Treatment" and "Control" Groups

Table E.13 compares the characteristics of the treatment group—towns where the share of poor voters increased—to the control group, where there was no increase in the share

of poor voters. We can see that the differences are generally small, and also statistically insignificant in most cases. Notably, there is little difference in taxes and spending pre-reform, or in the early 1850s.[31] There is a little evidence that the towns where the poor were not enfranchised were wealthier—with higher average tax base, and a higher proportion of the population involved in service and commerce. Of course, this is not a concern for the empirical specification if the effect of higher wealth doesn't change over time (and the regressions do control for these variables). However, as an additional robustness test, in Table E.14 I allow for the effect of tax base per capita to vary between the pre-reform and post-reform periods—doing so does not change the conclusions.

Table E.13: Towns with poor enfranchised by 1869–1878 reforms are similar to those where poor were not enfranchised.

	Poor Not Enfranchised		Poor Enfranchised		t-test	
	N	Mean	N	Mean	Diff.	S.E.
Population	71	44,441	29	40,356	4,085	14,130
Urban crowding	71	5.25	29	5.31	-0.06	0.18
Population growth (%)	71	1.2	29	1.2	-0.02	0.23
Occupation Agriculture (%)	71	16.5	29	17.4	-0.89	2.83
Occupation Commerce (%)	71	5.2	29	4.4	0.72	0.30**
Occupation Service (%)	71	16.6	29	14.4	2.24	1.07**
Rateable Value p.c.	71	2.76	26	2.51	0.25	0.15*
Pre-reform tax per capita	71	0.53	29	0.46	0.07	0.05
Pre-reform spend per capita	71	0.72	29	0.65	0.07	0.10
Pre-STRA spend per capita	69	0.26	28	0.20	0.06	0.04

Note: "Poor (Not) Enfranchised" indicates that the share of the poor in the electorate increased (or did not) from 1866 (pre-reform) to 1879 (post-reform). "Pre-reform" tax and spend per capita refer to the average over 1867–1872. "Pre-STRA spend per capita" refers to municipal accounts between 1848 and 1855. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E.2 Regression Results

This Appendix presents the results of the specifications reported in Figure IV, alongside additional robustness tests.

The first two specifications for each dependent variable in Table E.14 report the regression coefficients displayed in Figure IV. The first specification includes only town and year fixed effects, while the second adds the basic set of control variables—population, urban crowding, and population growth. Specifications 3 and 6 then add a full set of controls, including an interaction between the town rateable value and the post-reform period. As we can see, the results are very similar, although the coefficient for town spending is narrowly statistically insignificant at conventional levels.

[31] The pre-1860 spending data excludes sanitation infrastructure and so is directly not comparable to the post-1866 data meaning that we cannot, unfortunately, compare pre-trends.

Table E.14: Towns where the 1869–1878 reforms enfranchised the poor had lower growth in tax revenue and spending on public goods.

	Dependent Variable (per capita, % of median):					
	Tax Receipts			Public Goods Spend		
Poor Enfranchised By Reform x post1873	-0.09**	-0.11***	-0.08**	-0.14*	-0.19**	-0.10
	(0.041)	(0.037)	(0.036)	(0.073)	(0.071)	(0.070)
No. Observations	1,668	1,668	1,635	1,668	1,668	1,635
Year Fixed Effects	Y	Y	Y	Y	Y	Y
Town Fixed Effects	Y	Y	Y	Y	Y	Y
Basic Controls	N	Y	Y	N	Y	Y
Occupational Controls	N	N	Y	N	N	Y
Tax Base per Capita	N	N	Y	N	N	Y
Tax Base per Capita x post1873	N	N	Y	N	N	Y

Note: Independent variable is an indicator variable indicating that the share of poor voters in the electorate increased between 1866 and 1879. Standard errors are clustered by town and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E.15 repeats the analysis using a continuous measure of the change in the enfranchisement of the poor voters than the binary indicator used previously. The results are similar, in all cases.

Table E.15: Results are similar when using a continuous measure of the effects of the reforms.

	Dependent Variable (per capita, % of median):					
	Tax Receipts			Public Goods Spend		
Δ Poor Enfranchised x post1873	-0.04**	-0.04**	-0.03*	-0.05*	-0.06*	-0.03
	(0.016)	(0.016)	(0.016)	(0.030)	(0.031)	(0.030)
No. Observations	1668	1668	1635	1668	1668	1635
No. Towns	100	100	100	100	100	100
Year Fixed Effects	Y	Y	Y	Y	Y	Y
Town Fixed Effects	Y	Y	Y	Y	Y	Y
Basic Controls	N	Y	Y	N	Y	Y
Occupational Controls	N	N	Y	N	N	Y
Tax Base per Capita	N	N	Y	N	N	Y
Tax Base per Capita x post1873	N	N	Y	N	N	Y

Note: Independent variable is the change in the share of poor voters in the electorate between 1866 and 1879—coefficients represent the effect of a 10% change. Standard errors are clustered by town and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

F Relationship with Previous Studies

This Appendix discusses the relationship between this paper and three previous papers that have focused on the effect of local democratic reform in nineteenth-century Britain. The first subsection discusses the relationship with Chapman (2018), which draws on the theoretical model in Appendix A of this paper and tests the effect of 1894 democratic reform on town council spending. The second subsection discusses Aidt, Daunton, and Dutta (2010) who, using a subset of the data used in this paper, find evidence of a "retrenchment" effect whereby the middle class opposed expenditure on public goods. A replication analysis demonstrates that Aidt, Daunton, and Dutta (2010)'s main results are a consequence of a small, and biased, sample. The final subsection then discusses Aidt, Winer, and Zhang (2021), who investigate the effect of franchise extension using time series data on local government spending between 1820 and 1913.

F.1 Relationship with Chapman (2018)

Drawing on the model first introduced in this paper, Chapman (2018) finds that an 1894 democratic reform led to lower levels of town council spending on public goods relative to towns that were democratized at an earlier date—with the strongest effect occurring in areas where pre-reform local elites were predominantly middle class. This paper builds on that earlier study in several major ways.

First, the model here is more general and hence more straightforwardly applicable to a wider range of contexts—the model in Chapman (2018) was derived from this model for the particular needs of the empirical exercise in that paper. In particular, the model here provides a clear prediction of an inverted-U-shaped relationship with the franchise. Further, it provides a testable assumption as to the characteristics of the utility function (Assumptions 1 and 2) needed to predict the inverted-U-relationship.

Second, and most importantly, Chapman (2018) does not use the franchise data at the center of the current study, and so cannot examine the effect of extending the franchise gradually nor differentiate the effects of enfranchising different income groups. Instead, the earlier investigates how the effects of an 1894 democratic reform—extending the secret ballot and removing a graduated franchise—varied according to the characteristics of the pre-reform elite. This paper, in contrast, can directly test the inverted U-shaped relationship predicted by the theoretical model by testing the effect of enfranchising first the middle-class and then the poor.

Third, the focus on extending the franchise here provides a cleaner test of the effect of increasing the political rights of the poor. The analysis in Chapman (2018) cannot disentangle the effects of imposing a secret ballot from removing the graduated franchise, whereas

here we isolate the effect of providing voting rights—a reform which is applicable to a wider range of historical settings. As a result, the current manuscript can provide a direct test of the effect of enfranchising poor voters with political rights.

An additional difference between the two papers is that the sample in the current paper includes largest British cities (excluding London). Identification in Chapman (2018) relied on comparing similar incorporated to unincorporated towns—leading to a focus on relatively small towns. The largest towns, that became "County Boroughs" in 1890 were directly excluded, while additional towns were excluded due to a matching exercise. Consequently, over half the towns in this study were not included in the matched dataset in Chapman (2018). In contrast, this study looks within the group of incorporated towns, and so is able to include major cities.

F.2 Relationship with Aidt, Dutta, and Daunton (2010)

Aidt, Dutta, and Daunton (2010) (henceforth ADD), using a subset of the data used in this paper, find evidence of a "retrenchment" effect, whereby the middle class opposed expenditure on public goods. That is, small increases in the franchise in England led to a fall in expenditure on public goods, but that large increases in the franchise led to increases in expenditure. They thus argue that the poorest citizens supported higher government investment and taxation. ADD use similar sources to the dataset in this paper, but with less comprehensive data collection: below I show that their results are an artefact of using a small, biased, sample of towns.

I first explain the major limitations of their data, highlighting the differences with my dataset and design choices. To do so, I draw on their published paper (including the Online Technical Appendix), as well as documentation associated with the data stored in the UK data archive, study 5024. This detailed analysis uncovered some limitations of the sample not obvious from the ADD manuscript, and also identified some transcription errors. The second subsection replicates ADD's main estimates—using their replication dataset—and shows that their results are driven by these data limitations.

F.3 Limitations of ADD Dataset

This section explains the key differences between the ADD dataset and my data, highlighting specific problems identified by the replication analysis.

Small and Imbalanced Sample

The dataset used by ADD is drawn from similar sources as this paper but is much smaller in scope. Their data includes only three time periods (1868, 1871, 1886), and 55 towns. The logic for the choice of these specific towns and years is unclear. In contrast, the regressions in this paper use data from 150 towns, with annual data between 1867 and 1900. The larger

dataset reflects collecting comprehensive annual information from the *Local Tax Returns*, and data regarding the number of voters in seven cross-sections. A key advantage of this larger dataset is that I can identify large, one-off expenditures, and hence produce less noisy estimates. The smaller ADD dataset means—as we will see in detail below—their results are more vulnerable to data errors[32] or slight changes in sample composition.

The ADD regression sample is biased towards larger towns in 1886. In all their regression specifications ADD control for the percentage of the population occupied in industry. However, for the 1886 cross-section this is only available for towns over 50,000 in population—as such the number of towns included in their regressions drops from 55 to 22 between 1871 and 1886.[33] As we will see below, the resulting bias in the sample is a key driver of the ADD results.

Excluded Financial Accounts

A major limitation of the ADD dataset is that town spending as "Improvement Commissions" is excluded. As explained in Appendix C, before 1872 towns could undertake sanitary expenditure as either a "Local Board of Health" or as an "Improvement Commission". Around one-fifth of the towns in the ADD main regressions did so as Improvement Commissions; this information is not captured in the ADD dataset. In 1868, ADD exclude Improvement Commission expenditure entirely. In 1871, they appear to take a different approach for some (although not all) towns—estimating this spending by the town spending as urban sanitary authorities in 1873.

In addition, for a number of observations, ADD assume that "missing" data implies zero expenditure. Many towns did not return their accounts as local boards of health in 1868, and as such are entered as missing values in the *Local Taxation Returns*. ADD appear to assume this means zero spending occurred, wit out discussion or justification. Together with the omission of spending by improvement commissions, this means spending is likely underestimated for more than a third of their sample in 1868.

Notably, the ADD approach biases the data towards finding the upright U-shaped relationship that they observe. Spending is underestimated in 1868 when the franchise is low, and over-estimated in 1871 after the franchise has grown. The distortions appear quite

[32]Reconstructing the ADD variables identified a small number of transcription errors for 1886, these are corrected in the following analysis. The errors appear to reflect transcription from the wrong line of the report. Specifically, part of expenditure was incorrect for Warrington, Newport (Monmouth), Winchester, and Monmouth. Borrowed funds were incorrect for Burnley, Dover, Nottingham, and Wakefield. Tax base was incorrect for Huddersfield, Bolton, Lancashire, and Newport (Monmouth). In general these errors led to under-estimates of the dependent variables; again this biases in favor of ADD's finding of an upright U-shaped relationship.

[33]Data appears to be available for a few additional towns, it is not clear why this is not included in the ADD data.

substantial: on average, the ADD spending was 58% of the true value in towns with improvement commissions. In 1871, spending in these towns is over-estimated by an average of 83%.

F.4 Differences in Definition of Major Variables

ADD use measures of the franchise and spending on public goods which are similar—but not identical—to those in this paper. To aid interpretation of the replication results in the following section, I first explain these differences.

Franchise: The main independent variable in this paper is the male franchise—the number of voters as a percentage of the adult male population. ADD's franchise variable is the "total number of individuals on the Burgess Roll divided by the number of male inhabitants in the municipal borough, aged above 20, times 100". Since women were allowed to vote after 1869, this neither defines the male nor the total franchise.

This paper uses a three-year lag of the franchise throughout (an approach which relies on some interpolation). ADD use inconsistent lags of the franchise across different periods: the franchise for the 1868 time period is from 1865, in 1871 the franchise data is contemporaneous, and in 1886 the franchise is taken from 1884.

Dependent Variables:

This paper uses two major dependent variables: tax receipts per capita, and ongoing spending on public goods per capita. ADD do not use revenue data, but do use a measure of public goods spending per capita—their "urban amenities per capita"—as well as a measure of loans received per capita.

The "urban amenities per capita" is similar to one of the main dependent variables—total public goods spend per capita—used in this paper, but with three major differences. First, I combine municipal accounts and sanitary accounts from the same year. ADD, in contrast, combine information from different years—the 1868 figure combines data from 1868 and 1869; while that for 1871 combines information from 1871 and 1872. The reason for this is not clear. Second, ADD include a slightly different set of public goods in their definition of spending. Because the categories reported in the accounts change over time, it is not straightforward to define a consistent measure of public spending. I include all public spending, but in contrast to ADD I exclude spending funded privately. I also include "Other" spending in all years to account for the fact that this category includes some items of spending that are disaggregated in later years, including some public goods.[34] In addition, in the 1886 cross-section I include spending on "Baths and Washhouses", "Hospitals", "Public Libraries and Museums", "Municipal Buildings", and "Lunatic Asylums"—excluded by ADD. Including

[34]The other category is not well defined, but on occasion items such as water supply are explicitly mentioned as being included.

these items improves the consistency of the spending variable over time: the accounts are less disaggregated before the mid-1880s, meaning that the additional categories would be included elsewhere in other categories (such as "other" or "other public works")—excluding them in 1886 thus potentially creates an artificial drop in expenditure. Further, with the arguable exception of asylums, they are public goods in nature and in some cases have a clear sanitary element. Third, I use the annual panel data to adjust for one-off expenditures that can skew the results.

F.5 Replication of ADD Results

I now use the ADD replication dataset to demonstrate that these data limitations drive their results. I first replicate the ADD closest specification closest to that in my results.[35] I then, drawing on the more comprehensive dataset, investigate how the findings are affected by addressing each of the issues discussed above in turn. Once these corrections are made, and noisiness in the data is accounted for, the inverted-U-shape is observed even within the relatively small ADD sample. I then replicate all the specifications in ADD and show that their finding of an upright U-shaped relationship disappears once we use the corrected data. The replication analysis uses multiple versions of the ADD "urban amenities per capita" variables:

1. Original: urban amenities per capita as used in ADD, taken from their replication dataset.
2. Corrected 1: "Original", corrected to account for transcription errors, and to include spending by towns as improvement commissions.
3. Corrected 2: The same as corrected 1, but with financial accounts for each town aggregated within a financial year (rather than across two financial years, as in ADD).
4. New: The public goods spending per capita variable used in the main text of this paper (see comparison to the ADD measure in previous subsection).

I also vary the regression sample:

1. ADD: regression sample used in ADD Tables 3 and 4.
2. All ADD: ADD regression sample, but including towns with missing data for *industrial employment*
3. Corrected: "All ADD", removing towns that are excluded in this paper due to missing spending data in 1868, becoming incorporated after 1867, or other reasons discussed in Appendix C.

[35]The replication dataset was downloaded from the home page of the Economic Journal on August 13, 2014.

4. Remove Outlier: "Corrected" sample, minus a single outlier (see text below)

The remainder of the section uses these variables and sample definitions to demonstrate how the various aspects of the ADD dataset affect their results.

The first three columns in Table F.16 show how ADD's results are driven by sample bias towards large towns. The dependent variable here is their "urban amenities per capita" variable. Specification (1) in the table directly replicates the ADD specification—with year and town fixed effects: the coefficients are identical to those reported in ADD (Table 3, column 2), although the standard errors are higher due to clustering by town.[36] As discussed above, only large towns are included in the ADD regressions in 1886 due to a lack of data for *industrial employment*. The inclusion of *industrial employment* alone does not affect the results—see column (2)—if the regression sample is unchanged. However, once towns for which *industrial employment* is unavailable are included—the "All ADD" sample, reported in column (3)—there is little evidence of an upright U-shaped relationship.

Specification (4) demonstrates that the ADD results are also not robust to including the full set of financial accounts for each town. Here I return to the original ADD regression sample and set of control variables but use the "corrected1" version of their dependent variables—adjusting only for transcription errors and adding excluded data by towns as improvement commissions. Again, there is now little evidence of an upright U-shaped relationship.

The remaining four columns in Table F.16 show how the inverted-U-relationship—the key finding of this paper—emerges once we use a cleaner version of the ADD dataset. Specification (5) and (6) uses the "corrected" sample, the male franchise lagged three years, and the "corrected2" dependent variable. These adjustments alone do not lead to any observable relationship between the franchise and spending per capita. However, removing a single outlier—with spending per capita twice the level in the remainder of the sample—leads to the inverted-U-relationship emerging in specification (7). Specification (8) shows that we observe a similar—but even stronger—result when using the ongoing spending variable used in the main text as the dependent variable. By removing the noisiness associated with lumpy investment, the inverted-U-shape can be observed more cleanly.

Tables F.17 and F.18 replicate all the specifications in ADD and show that the upright U-shaped relationship disappears when making minimal corrections to their dataset. The first six columns in each table replicate the results from ADD Table 3 (see Table F.17)

[36] ADD's main results report unclustered, heteroskedasicity-robust, standard errors. I have been unable to replicate these standard errors, possibly due to differing statistical packages (the estimates here were produced by the xtreg command in STATA 17). The results in Table F.16 are similar using unclustered standard errors—see Tables F.17 and Tables F.18.

Table F.16: ADD Results Driven by Imbalanced Sample and Data Anomalies.

	DV=Urban Amenities Per (1,000) Capita							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	ADD	ADD	All ADD	ADD	Corrected	Corrected	Remove Outlier	Corrected
Dependent Variable	ADD	ADD	ADD	Corrected1	Corrected2	Corrected2	Corrected2	New
Franchise	ADD	ADD	ADD	ADD	ADD	New	New	New
Franchise	-26.81*	-26.65	-14.88	-12.80	7.99	10.26	36.48**	45.68***
	(13.781)	(17.920)	(14.995)	(15.504)	(12.704)	(32.900)	(15.088)	(9.652)
Franchise squared	0.36**	0.36*	0.14	0.20	-0.13	-0.03	-0.39**	-0.47***
	(0.137)	(0.207)	(0.153)	(0.183)	(0.097)	(0.404)	(0.170)	(0.102)
Population	-0.00	-0.00	-0.01**	0.00	-0.01	-0.01	-0.01	-0.00
	(0.006)	(0.004)	(0.005)	(0.004)	(0.010)	(0.010)	(0.006)	(0.002)
Industrial employment	-7.72			3.25				
	(22.075)			(20.410)				
Rateable value	-0.02	-0.00	0.03	-0.04	-0.21	-0.25	-0.16	-0.04
	(0.251)	(0.144)	(0.072)	(0.347)	(0.170)	(0.152)	(0.119)	(0.129)
Population density	-3.94	-3.96	6.64***	-3.07	7.37**	7.24**	4.92***	0.92
	(4.647)	(4.204)	(2.482)	(4.029)	(3.106)	(2.815)	(1.344)	(0.950)
Accumulated debt	0.25**	0.24***	0.37***	0.18***	0.75**	0.75**	0.47**	0.06
	(0.100)	(0.052)	(0.111)	(0.062)	(0.360)	(0.375)	(0.221)	(0.106)
Franchise turning point (%)	37.5	37.5	54.6	32.7	31.7	147.9	47.3	48.8
Borough FE	Y	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y	Y
No. obs	127	127	180	127	149	152	151	152
No groups	55	55	64	55	61	61	61	61
F-test (p-val)	0.01	0.18	0.61	0.34	0.20	0.51	0.06	0.00

Groups in top panel are defined as follows (see text for full details). Samples: ADD=sample from ADD, Table 3, specification (2); All ADD=ADD + include towns with missing data for Industrial employment - remove towns excluded from analysis in this paper. "Corrected1"=ADD data, removing towns excluded in dataset from this paper (e.g., those with missing 1868 data). "Remove outlier"="Corrected2" - observation with highest spending in dataset. DV: "ADD"=variable from ADD dataset; "ADD′"= ADD definition with data errors corrected and using improvement commission data; "Corrected1"=ADD', but combining accounts within same year; "New"= variable from main text. Franchise: ADD=variable from ADD replication dataset; "new"=variable used in main text of this paper. Standard errors are adjusted by clustering by district and are displayed in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

and Table 4 (see Table F.18). The second half of the table replicates these specifications, making minimal corrections to their dataset: using the "corrected1" dependent variable, and the "All ADD" sample, as in specification (4) in Table F.16. In addition, in order to bias in favor of finding a statistically significant relationship, here I do not adjust standard errors to correct for heteroskedasicity—inflating the t-statistics relative to those in the initial paper. Nevertheless, as we can see, there is no statistically significant evidence of an upright U-shaped relationship.

Table F.17: Replication of ADD Table 3

	Urban Amenities per (1,000) capita											
	Replications of Original						Corrected Data and Sample					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Franchise	-28.0**	-26.8*	-62.8***	-17.8*	-24.5*	-57.4***	-9.6	-5.9	-3.8	0.6	-6.0	-30.9*
	(12.40)	(13.78)	(20.59)	(10.75)	(13.48)	(19.38)	(15.84)	(16.87)	(22.71)	(14.69)	(16.96)	(17.54)
Franchise squared	0.4***	0.4**	0.8***	0.2**	0.3**	0.4***	0.2	0.0	0.1	0.0	0.0	0.2
	(0.13)	(0.14)	(0.20)	(0.10)	(0.13)	(0.13)	(0.16)	(0.16)	(0.22)	(0.13)	(0.16)	(0.12)
Population	-0.0	-0.0	-0.0	-0.0	-0.0	-0.0	0.0	-0.0	-0.0	-0.0	-0.0	-0.0
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Industrial employment	-2.6	-7.7	-105.9**	15.5***	-6.1	-3.9						
	(17.84)	(22.07)	(48.74)	(3.60)	(21.53)	(21.64)						
Rateable value	-0.0	-0.0	0.8	0.2***	-0.0	0.1	0.1	-0.0	0.1	0.1	-0.0	0.0
	(0.24)	(0.25)	(0.49)	(0.07)	(0.24)	(0.25)	(0.14)	(0.14)	(0.17)	(0.07)	(0.14)	(0.22)
Population density	-3.3	-3.9	7.7	-0.2	-5.2	-5.1	3.2	6.7**	6.7	-0.0	6.7**	-3.7
	(4.25)	(4.65)	(9.41)	(0.42)	(4.57)	(4.62)	(2.80)	(2.81)	(4.16)	(0.54)	(2.83)	(4.18)
Accumulated debt	0.3**	0.3**	0.3***	0.2***	0.2**	0.2**	0.3*	0.3**	0.3*	0.2***	0.3**	0.2**
	(0.10)	(0.10)	(0.12)	(0.04)	(0.10)	(0.10)	(0.14)	(0.13)	(0.15)	(0.07)	(0.13)	(0.08)
Growth in housing stock					6.2**						-0.9	
					(2.96)						(3.72)	
Relative tax base x franchise						-21.8						-13.3
						(15.06)						(13.62)
Relative tax base						0.4**						0.3
						(0.20)						(0.18)
Franchise turning point (%)	39.6	37.5	41.1	40.1	36.4	42.9	29.9	80.3	20.2	-14.4	79.0	38.5
Borough FE	Y	Y	Y	N	Y	Y	Y	Y	Y	N	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Region FE	N	N	N	Y	N	N	N	N	N	Y	N	N
Shire-specific trends	N	N	Y	N	N	N	N	N	Y	N	N	N
No. obs	127	127	127	141	127	127	180	180	180	191	180	133
No groups	55	55	55		55	55	64	64	64		64	61
F-test (p-val)	0.01	0.01	0.00	0.04	0.01	0.01	0.12	0.90	0.67	0.76	0.90	0.18

Standard errors are adjusted by clustering by district, and are displayed in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.18: Replication of ADD Table 4

	Borrowed Funds per (1,000) capita											
	Replications of Original						Corrected Data and Sample					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Franchise	-40.0**	-44.5**	-111.1***	-29.4**	-42.9**	-69.4***	-18.7	-18.4	-30.1	-6.0	-18.9	-34.0*
	(16.28)	(17.87)	(29.11)	(14.20)	(17.89)	(25.68)	(14.50)	(16.41)	(21.13)	(13.86)	(16.47)	(19.43)
Franchise squared	0.5***	0.6***	1.3***	0.4**	0.5***	0.6***	0.2	0.2	0.3*	0.1	0.2	0.4**
	(0.17)	(0.18)	(0.29)	(0.14)	(0.18)	(0.18)	(0.15)	(0.16)	(0.20)	(0.12)	(0.16)	(0.13)
Population	-0.0	0.0	0.0	0.0	0.0	-0.0	-0.0	-0.0	-0.0	0.0	-0.0	0.0
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Industrial employment	44.0*	22.4	-57.3	14.0***	23.5	25.8						
	(23.42)	(28.62)	(68.90)	(4.75)	(28.58)	(28.67)						
Rateable value	0.3	0.3	0.9	0.1	0.3	0.4	0.0	0.0	0.2	0.1	0.0	0.2
	(0.31)	(0.32)	(0.69)	(0.09)	(0.32)	(0.33)	(0.12)	(0.13)	(0.16)	(0.06)	(0.13)	(0.24)
Population density	-4.7	-7.7	-1.8	-0.2	-8.6	-8.4	0.7	1.1	1.1	-0.1	1.2	-4.0
	(5.58)	(6.02)	(13.30)	(0.56)	(6.06)	(6.12)	(2.56)	(2.74)	(3.87)	(0.51)	(2.75)	(4.63)
Accumulated debt	0.3**	0.3**	0.3**	0.1*	0.3*	0.3**	0.3**	0.3***	0.4***	0.1*	0.3***	0.3***
	(0.13)	(0.13)	(0.17)	(0.06)	(0.13)	(0.13)	(0.13)	(0.13)	(0.14)	(0.06)	(0.13)	(0.09)
Growth in housing stock					4.4						-2.5	
					(3.93)						(3.61)	
Relative tax base x franchise						-23.7						-3.2
						(19.96)						(15.08)
Relative tax base						0.4						0.1
						(0.26)						(0.20)
Franchise turning point (%)	40.0	39.8	43.6	41.7	39.4	42.3	43.6	46.2	43.8	35.6	46.3	39.6
Borough FE	Y	Y	Y	N	Y	Y	Y	Y	Y	N	Y	Y
Time FE	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y
Region FE	N	N	N	Y	N	N	N	N	N	Y	N	N
Shire-specific trends	N	N	Y	N	N	N	N	N	Y	N	N	N
No. obs	127	127	127	141	127	127	180	180	180	191	180	133
No groups	55	55	55		55	55	64	64	64		64	61
F-test (p-val)	0.00	0.00	0.00	0.01	0.00	0.01	0.27	0.43	0.22	0.59	0.41	0.04

Standard errors are adjusted by clustering by district, and are displayed in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In summary, the difference between the results in this paper and Aidt, Daunton, and Dutta (2010) is explained by the more comprehensive data collection underpinning this paper. ADD's finding of an upright U-shaped relationship between the franchise and government spending is explained by a biased sample, and incomplete data collection. Once these issues are accounted for, we do not observe any relationship with the franchise and government spending on public goods. Cleaning the data further, the inverted-U-shaped relationship emerges even in the small sample of towns ADD analyze.

F.6 Relationship with Aidt, Winer, and Zhang (2021)

Aidt, Winer, and Zhang (2021) use a very different approach to tackle the effect of franchise extension. Whereas the previous two papers (and the current manuscript) use panel datasets at town-level, they use a time series approach with aggregated data on local government spending (and also, separately, central government spending). They then test the effects of franchise extension by exploring for structural breaks. The results show weak evidence of a structural break in aggregated local government spending after the 1869 franchise reforms—a finding which is not inconsistent with the results of this paper.

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