# Democracy, Redistribution, and Inequality: Evidence from the English Poor Law

Jonathan Chapman*

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**Abstract**

This paper tests whether inequality shapes the redistributive impact of democratization by examining changes to the governance of councils providing poor relief—rudimentary social insurance—in nineteenth-century Britain. An 1894 reform removed institutional features—a graduated franchise, property qualifications, the absence of a secret ballot, and the participation of unelected magistrates—that allowed landowners to control spending on poor relief after the 1832 Great Reform Act. The empirical analysis uses a new annual dataset of poor law spending from 1884–1905 to test whether higher pre-reform inequality amplified the effect of democratic reform on redistribution. The results support the Meltzer–Richard hypothesis: higher local income inequality led to higher poor relief spending after 1894. Areas where landed elites held political power saw smaller increases in expenditure, indicating that de facto elite influence muted the effect of democratization. These findings provide empirical support for models of democratization that focus on demands for redistribution.

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*Faculty of Economics, University of Bologna. I thank Robert Allen, Brian Beach, Dan Bogart, Marc Goñi, Philip Hoffman, Jeffrey Jensen, Elena Korchmina, Matthew Kovach, Samantha Myers, Cormac O'Grada, Kevin O'Rourke, Giuliana Pardelli, Didac Queralt, Jean-Laurent Rosenthal, Jeffery Timmons, Richard Trainor, and Patrick Wallis for helpful comments. I also thank four anonymous reviewers, and participants in seminars and conferences. Jayasutha Ramraj provided excellent research assistance. All errors remain my own.
e-mail: jonathan.chapman@unibo.it.

# 1 Introduction

The hypothesis that higher income inequality leads to greater government redistribution is extremely influential throughout the social sciences, but has received little empirical support. In the canonical model of Meltzer and Richard (1981), a poorer median voter demands greater spending as they face a relatively low tax burden. More recently, this insight has underpinned models of democratization (Acemoglu and Robinson, 2000, 2001; Boix, 2003). Yet testing these theories is complicated by the endogenous relationships between inequality, redistribution, macroeconomic factors, and political institutions: high inequality may, for example, make elites more capable of blocking post-democratization reforms, and more incentivized to do so (Acemoglu and Robinson, 2006a). Consequently, there remains little causal evidence that inequality increases government spending and few studies have investigated whether and how inequality moderates the effects of democratic reform.[1]

This paper overcomes these endogeneity issues by exploiting democratic reforms imposed upon locally elected councils in nineteenth-century Britain. These local councils, known as boards of guardians, controlled spending on poor relief—the primary form of government redistribution before the modern welfare state. Elites benefited from four important institutional advantages: significant property qualifications, a graduated voting franchise, the absence of a secret ballot, and the presence of unelected landowners on councils. In 1894, all of these advantages were simultaneously removed by Parliament. I find that this exogenous shock to the political power of the poor led to greater increases in spending in areas of high income inequality, consistent with the Meltzer–Richard hypothesis. At the same time, I find that post-democratization spending also responded to local economic need and was reduced by elite de facto power. Government redistribution was thus shaped by a variety of factors beyond income inequality.

These findings provide clear empirical evidence for the widespread theoretical proposition that, under certain conditions, democracies redistribute more in the presence of greater

[1] Results regarding how inequality affects government redistribution in general have been somewhat mixed, with observational studies finding a mixture of positive and negative results—see De Mello and Tiongson (2006, Table 1) for a summary.

income inequality. In particular, the paper studies a setting where, consistent with the assumptions of the Meltzer–Richard model, taxation was approximately proportional to income and spending was confined to redistribution. A significant literature has argued that those conditions rarely hold in modern-day political institutions (Moene and Wallerstein, 2001; Iversen and Goplerud, 2018). Yet those institutions are themselves endogenous, and likely shaped by elites, who may act to head off redistribution through either resisting democratization (Boix, 2003), shaping a society's constitution (Acemoglu et al., 2012), or investing in de facto power (Acemoglu and Robinson, 2006b). Such responses may explain the limited empirical support for the Meltzer–Richard hypothesis in previous studies, and hence point to income inequality as an important determinant of institutional development.

The paper begins, in Section 2, by presenting a conceptual framework that both clarifies the conditions under which the Meltzer–Richard hypothesis should hold, and distinguishes it from other theoretical channels through which inequality may affect spending. The hypothesis predicts specifically that inequality leads to higher redistribution by increasing the share of the tax burden borne by the rich. A simple model demonstrates that this prediction is more likely to hold when the political power of the poor is limited. Section 3 then explains that these conditions were plausibly met in nineteenth-century England: local councils spent money only on redistribution, raised revenue using a tax that was approximately proportional to income, and democratization remained incomplete. The setting thus offers many of the advantages of testing the Meltzer–Richard model in the laboratory (Agranov and Palfrey, 2015), while incorporating the complexities of a real-world setting.

I test how local inequality shaped reactions to democratic reform using a new annual dataset of spending on poor relief between 1884 and 1905. As explained in Section 5, I use the 1894 democratic reform to poor law governance as an exogenously imposed "treatment" that affected all poor law unions. The effect of inequality on spending per capita is then identified by including interactions between pre-reform inequality and an indicator variable for the post-reform period, and controlling for both time-invariant and time-varying factors. To test the Meltzer–Richard mechanism, I estimate income inequality as the ratio of mean

rental value to the median wage in each poor law union. Because local taxation was based on rental values, this measure directly corresponds to the relative tax burden of the rich versus the poor—the channel through which inequality affects demand for redistribution in the Meltzer–Richard model. Further, I account for other dimensions of rural inequality, allowing me to disentangle the Meltzer–Richard mechanism from other competing channels.

The results, presented in Section 6, provide robust support for the Meltzer–Richard hypothesis. Democratic reform was followed by greater increases in spending in areas with higher income inequality. The relationship holds when controlling for demographic characteristics affecting the demand for poor relief, financial factors constraining spending, and for union-specific time trends. Further, the estimated effect of income inequality remains strong after controlling for measures of land inequality, economic need, and the power of local elites. Overall, these findings are consistent with inequality affecting spending through the relative tax burden on rich and poor, as predicted by Meltzer–Richard, rather than through alternative channels.

Three additional findings emerge from the empirical analysis. First, the effect of income inequality was particularly pronounced for the provision of out-relief—the most clearly redistributive aspect of poor law policy—consistent with the Meltzer–Richard mechanism. Second, post-reform spending was lower in areas in which local elites held political power before 1894, consistent with them retaining de facto power after reform (Acemoglu and Robinson, 2006b). This effect is most evident for less publicly-salient items of expenditure, suggesting that elites could blunt the impact of democratization when the poor were unable to easily scrutinize policy. Third, spending increased by more in areas with arable agriculture, suggesting that democratization responded to economic need.

Several studies find that democratization increases government expenditure at both national and local level, but few examine whether inequality moderates the effect. Consistent with the idea that newly enfranchised voters push for redistributive expenditure, prior work generally finds positive effects of democratization on both tax revenue (Aidt et al., 2006), and social expenditure (Lindert, 1994, 2004).[2] The focus on redistributive expenditure in

[2]Reforms such as the 1894 Local Government Act analyzed in this paper help explain how expansions

this paper broadens the small literature investigating the effects of democratization within a country, which has mostly focused on expenditure on public goods or education (Husted and Kenny, 1997; Aidt et al., 2010; Cascio and Washington, 2014; Fujiwara, 2015; Chapman, 2018, 2024). Similarly to the findings here, Chacon and Jensen (2020) find that greater elite de facto power led to lower taxation in the U.S. following the enfranchisement of black voters during Reconstruction, without testing the role of income inequality.

Two recent papers have investigated how heterogeneity in inequality moderates the effects of democratization at national level. Acemoglu et al. (2015) find mixed results, but suggest that higher inequality may be associated with elite capture and hence lower taxation post-democratization. Dorsch and Maarek (2019) find that post-democratization income inequality dynamics are affected by initial inequality, but fiscal redistribution does not appear to be an important channel. Both sets of results are consistent with the findings in this paper that local elites were able to repress redistributive spending post-reform. However, the analysis here provides a more nuanced story in which elites could only reduce less publicly-salient forms of expenditure. Moreover, I observe a separate effect through which income inequality positively affected spending. The difference in findings may reflect the endogeneity problem inherent in national-level studies—elites may only democratize when they can limit the effects on redistribution. The exogenous democratic reform exploited in this paper avoids such complications.

This paper also contributes to the large literature on the history of the English Poor Law. Much of that literature has focused on understanding the effects of the 1834 New Poor Law (Boyer, 2006; Melander and Miotto, 2023), and the continuities between the Old and New Poor Law (Digby, 1978). A handful of papers have quantitatively analyzed aspects of the operation of the poor law after 1850 (MacKinnon, 1986, 1987; Brueckner, 2023), but do not focus on the governance of poor relief. In contrast, this paper investigates the democratization of poor law unions in the 1890s and demonstrates how poor relief policy

of the national franchise, emphasized by Lindert (2004), translated into increased redistribution by local governments. Studies of tax incidence have been less supportive of the link between democratization and redistribution (Aidt and Jensen, 2009a,b; Scheve and Stasavage, 2010, 2012). See Acemoglu et al. (2015, Section 21.3) for a detailed review of the literature.

was shaped by the interaction between local political institutions and economic structure.

The paper concludes by considering the implications of these results for theories of democratization, and placing the findings in the broader context of British political history—a motivating example for many of those theories. The empirical analysis suggests that decentralization allowed rural elites to limit the extent of redistribution long after Britain's 1832 Great Reform Act—an example of "elite-biased" democracy (Albertus and Menaldo, 2014). By taking control of the institutions established by the 1834 New Poor Law, landowners were able to escape the auspices of the increasingly democratic Westminster Parliament; it was not until agricultural laborers obtained the Parliamentary franchise that rural local governments were democratized. Maintaining control of poor law institutions thus enabled elites to reduce the cost of democratization for a further sixty years.

# 2 Conceptual Framework

This section introduces a simple theoretical framework that informs the empirical analysis. The framework identifies conditions under which income inequality may shape the effect of democratization on redistribution, and also identifies other factors that may confound this relationship. Here I sketch the main insights of the model, with a full formal treatment provided in Appendix A. Section 3 then describes how the historical setting fits the theoretical assumptions, and Section 4 explains how the hypotheses are operationalized.

Consider a population normalized to size 1, consisting of poor ($P$, share $\lambda > 0.5$) and rich ($R$, share $1 - \lambda$) citizens. The government provides poor relief $r$ funded by a linear tax, $t$, on income. Rich citizens always receive an income and never receive relief. Poor citizens are employed and receive income with probability $\pi$, and always receive at least partial relief. All agents have concave utility over individual consumption, $u(\cdot)$, with $u'(0) \to \infty$. I also assume, following Moene and Wallerstein (2001) that $u(c)$ exhibits constant relative risk aversion (CRRA), with $\gamma = -c\frac{u''}{u'} > 1$.[3]

[3]Appendix A presents the model without the CRRA restriction. The condition $\gamma > 1$ underpins the result (here and in Moene and Wallerstein (2001)) that higher inequality may not lead to higher government

Utilities of each group are then given by:

$$U_R = u\left(y_R(1-t)\right)$$
$$U_P = \pi u\left(y_P(1-t) + \psi r\right) + (1-\pi)u(r) + \kappa t,$$

where $y_P$ and $y_R$ are incomes of the relevant group if employed. $\psi > 0$ represents the value of relief received by employed poor citizens, either through income support or payments to dependents. If $\pi < 1$ and $\psi > 0$, then government spending has two effects. First, it redistributes from rich to poor. Second, it provides insurance against negative income shocks, such as unemployment. $\kappa \geq 0$ captures instrumental utility from taxing the rich due to fairness concerns, inequality aversion, or class conflict.

The amount of relief provided in equilibrium is determined by maximizing the following social welfare function, subject to a balanced budget constraint:

$$\max_r W = \phi U_P + (1-\phi)U_R$$

such that $r\lambda(\pi\psi + (1-\pi)) = (t - D(t))\bar{y}$. $D(t)$ is a strictly convex function representing administrative costs and potential tax base erosion due to higher taxation.[4] $\phi$ reflects the political weight placed on the welfare of the poor in the institution determining the level of relief.[5] Because the poor constitute a numerical majority ($\lambda > 0.5$), the extremes of $\phi = 0$ and $\phi = 1$ are equivalent to median voter models with voting rights restricted to the rich or extended to all citizens, respectively.

This framework is sufficient to reproduce the central insights of the Meltzer–Richard model. Setting $\pi = \psi = 1$ and $\kappa = 0$, and interpreting $D(t)$ as labor-supply distortion from taxation, essentially reduces the model to the canonical Meltzer–Richard (1981) setup: a linear tax financing a universal lump-sum transfer chosen by the median voter. This leads

spending if relief is targeted at the unemployed.

[4]Specifically, similarly to Acemoglu and Robinson (2006a), I assume that $D' > 0$, $D'' > 0$, $D(0) = D'(0) = 0$, and $D'(1) = 1$.

[5]In a standard utilitarian welfare function, $\phi$ would embed the share of citizens in each group.

to the standard prediction that a higher mean–median income ratio raises redistribution. Specifically, higher inequality lowers the poor's relative tax burden from providing relief and so their preferred level of relief increases.

The model also illustrates how the Meltzer–Richard predictions rely on a number of strong assumptions. The tax rate is linear, meaning that the poor always share the tax burden. Further, the direction of the relationship between inequality and the size of government is dependent on the nature of government spending. If spending purely provides insurance—$\psi = 0, \pi < 1$—then greater inequality leads poor voters to desire lower government spending. This latter finding is a key insight of Moene and Wallerstein (2001). The model also abstracts from other potential factors mentioned in the literature that may mean that income inequality does not lead to higher redistribution—such as the potential for middle class control of government, multi-dimensional policy space, or party alignments.[6] A core contribution of this paper is to identify a setting where these assumptions plausibly hold, meaning that the Meltzer–Richard hypothesis can be tested empirically.

A distinctive feature of this framework is the role of democratization, captured by the political weight parameter $\phi$. Notably, the standard Meltzer–Richard prediction—that higher inequality increases government spending—is less likely to hold when the poor have substantial political weight. Specifically, when $\phi$ is high, a mean-preserving spread (an increase in $y_R/y_P$ while keeping $\bar{y}$ fixed) raises spending only if the redistributive, rather than the insurance, motive for government spending is sufficiently strong. In contrast, if the poor's political weight is limited ($\phi$ is low), then higher inequality increases spending without these conditions.[7] Intuitively, weaker democracy reduces the weight of those citizens—the employed poor—for whom higher inequality may translate into lower desired government spending. A mean-preserving spread involves a reduction in $y_P$ and an increase in $y_R$. This reduces the demand for the insurance element of government spending among the poor, but also weakens

[6] See the review by Acemoglu et al. (2015) for a discussion of these possible factors.

[7] More precisely, there exists $\bar{\phi} \in (0, 1]$, such that an increase in inequality strictly increases equilibrium government spending if $\phi \in (0, \bar{\phi})$. Government spending is also increasing in income inequality if employed citizens are marginal beneficiaries of more relief or, as in Moene and Wallerstein (2001), when risk aversion is not too strong. See Proposition 1 in Appendix A for details.

the opposition to higher expenditure among the wealthy because their marginal utility cost of higher taxation is lower.[8] When $\phi$ is low, the preferences of the rich dominate.

The framework also highlights other channels that can confound empirical tests of the Meltzer–Richard predictions. $\phi \in (0, 1)$ reflects the scenario in which the poor have some political power, but are constrained by institutional factors. In models of democratization, such as Acemoglu and Robinson (2006a), $\phi$ is endogenous to inequality: the wealthy may grant less political power to the poor in unequal societies, or maintain *de facto* influence even after losing *de jure* control (Acemoglu and Robinson, 2006b). The employment probability, $\pi$, reflects the need for social insurance, while $\psi$ reflects the extent to which relief reaches employed households through, for instance, provision to dependent family members. $\kappa$ captures motives for redistribution not tied to consumption, such as resentment toward the rich. Each of these parameters represents a factor that may be correlated with income inequality, and that can affect government spending, without capturing the Meltzer–Richard mechanism.

In summary, the model identifies conditions under which income inequality—measured by the mean-to-median income ratio—should increase government expenditure, as in Meltzer and Richard (1981). The empirical analysis tests this prediction in a setting where key theoretical assumptions were plausibly satisfied: taxation was roughly proportional and fell partly on the poor, government spending benefited a broad swathe of households, and political institutions were imperfectly democratic. I also test alternative channels, allowing me to distinguish the Meltzer–Richard mechanism from other channels—such as class resentment, elite capture, or economic need—through which inequality may affect government spending.

# 3 Institutional Context

To test the effect of inequality on government spending, I exploit the fact that redistributive government expenditure in nineteenth-century England was provided by autonomous, locally

[8]Note that Moene and Wallerstein (2001)'s median voter framework captures the effects of the reduction in the income of the poor, but excludes any counterbalancing effect of increasing the income of the rich.

elected councils. Poor relief served as the main safety net against poverty in Britain for several centuries, providing rudimentary social insurance against a range of economic shocks. This section first provides a general overview of the New Poor Law. The second subsection argues that the median voter would likely benefit from higher poor relief expenditure. The third subsection discusses the politics of the poor law and the democratic reform exploited in the empirical analysis.

## 3.1 The New Poor Law

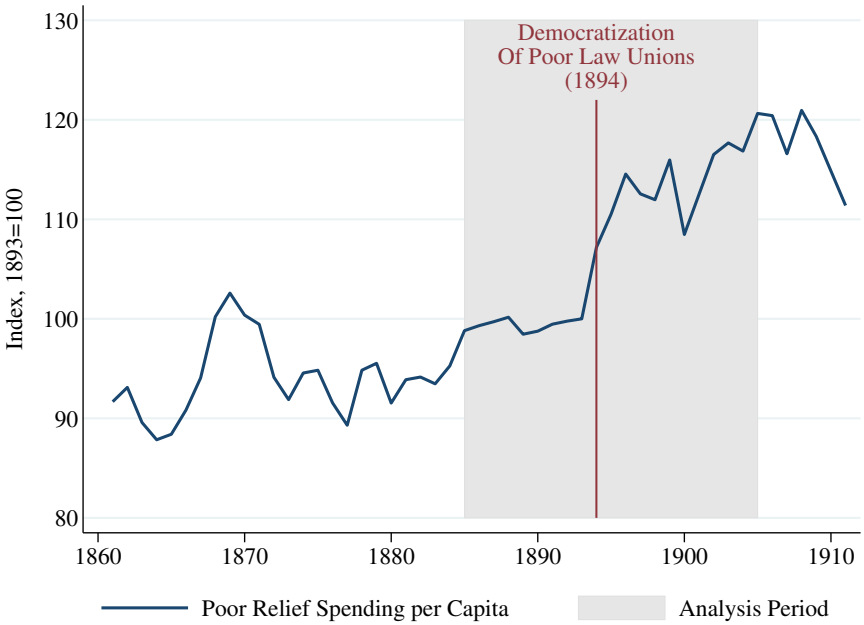
Under the infamous 1834 "New Poor Law," poor relief was controlled by a set of approximately 600 autonomous local authorities—"Boards of Guardians of the Poor". These councils determined both the level and type of relief within their respective "Poor Law Unions," raising funds through local taxation. While the guardians administered poor relief, they did not have responsibility for spending on infrastructure, local public services (provided by town councils), or education (controlled by local school boards).[9]

Spending on poor relief decreased after the introduction of the New Poor Law and did not recover until the 1890s, as shown in Figure 1. The newly-established boards of guardians significantly decreased expenditures after 1834 (Boyer, 2018, Table 2.1), with per capita spending falling approximately 30% by 1860. Spending then remained at a similar level—fluctuating in response to economic crisis—for the next thirty years. Importantly for our purposes, there was little growth in spending in the ten years prior to democratic reform but a sharp increase immediately afterwards.

The most contentious aspect of the New Poor Law was the attempt to force recipients to enter the workhouse—the infamous "workhouse test"—rather than receive relief outdoors ("out-relief"). Guardians were prohibited from offering outdoor relief to able-bodied men and, in the 1870s, vigorous efforts were made to prevent any pauper receiving support outside a workhouse—a policy known as the "Crusade Against Outrelief". The number of outdoor paupers fell by more than a third on average, causing considerable resentment as elites were

[9]The guardians sometimes held other roles that involved spending on public goods—see Appendix C.3.

Figure 1: Poor law spending increased immediately after 1894 democratic reform.



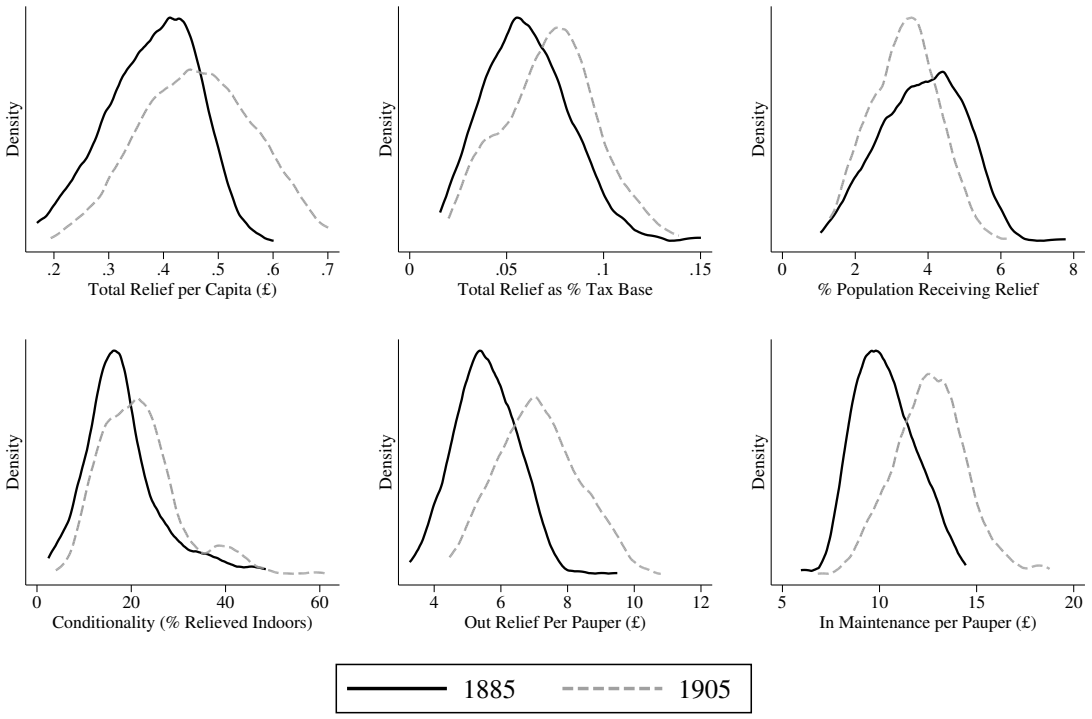

Note: The figure displays annual average real spending per capita across the 208 poor law unions included in the regression sample. A supremum Wald test estimates that a structural break in poor relief spending per capita occurred in 1894. The null hypothesis of no structural break is rejected at a 1% significance level. See Appendix Table B.2 for pre- and post-reform annual growth in spending by census division.

seen to be abandoning their paternalistic role in the community (Hurren, 2015).

Guardians held a great deal of autonomy in administering poor relief, including in the implementation of the Crusade, as we can see in Figure 2. Guardians could determine who was eligible for relief, the generosity of payments, and whether support should be monetary or in-kind. Consequently there was considerable variation across unions in the generosity of spending and the repression of outdoor relief (bottom-left panel).

Guardians had to finance their spending locally, predominantly through a proportional tax on the rental value of occupied property. Taxes had to be levied on owners and occupiers of land and buildings, meaning that all households, in principle, shared the cost of relieving the poor. The guardians decided the level of taxation, but had no ability to alter this tax structure or tax other sources of income such as profits. The tax rate was thus approximately proportional to income, and hence consistent with the framework in Meltzer and Richard

Figure 2: Poor law policy varied considerably across poor law unions.




Note: The figure displays kernel density for 208 poor law unions included in regression analysis. Three poor law unions with 1905 in-maintenance per pauper of over £20 are excluded for display purposes.

(1981) and many other political economy models. The reliance on property taxation also implies that the local tax base was relatively inelastic in the short run, meaning that higher taxes were unlikely to have significantly eroded the tax base.

Other features of the historical context suggest that the assumptions of the Meltzer–Richard model are likely to hold in this setting. Contemporary debates surrounding the Poor Law focused on normative questions regarding the appropriate level and form of relief, rather than on efficiency concerns, such as potential disincentive effects of higher taxation. Unions did not have to consider whether more generous benefits would stimulate in-migration, both because migrants would not automatically be eligible for relief, and because the levels of relief were relatively small.[10] The focus of poor relief on relieving the destitute, and the ac-

[10]The poor law was still governed at this time by the "law of settlement" that meant residence was necessary to be eligible for relief. By the late nineteenth century the practical significance of these laws was beginning to fade, but still provided clear legal grounds to refuse to provide relief.

companying stigma, meant that transfers could not be directed towards a middle class as in the "Modified Director's Law" introduced by Acemoglu et al. (2015). Poor law politics was not affected by social cleavages, such as ethnic fractionalization, that could undercut support for redistribution among the poor (Alesina et al., 1999). Collectively, these institutional features provide a setting that is well aligned with the Meltzer–Richard framework, and that avoids several possible countervailing influences of inequality on government spending.

## 3.2 Recipients of Poor Relief

The poor law was the most important safety net against economic shocks, and many citizens were at risk of relying on relief each year. The proportion of the population receiving relief in the 1870s was generally around 6%, but could rise above 20%.[11] Relatively few paupers were able to work: one-third of paupers were children, and most adult paupers were classified as "non-able-bodied," a category which included the temporarily sick, the mentally ill, the permanently disabled, pregnant women, and the elderly. By the 1890s much poor relief was targeted at the elderly—in 1892 30% of those over 60 received relief (Boyer and Schmidle, 2009). Further, these statistics likely underestimate the number of those requiring support, as dislike of the workhouse deterred many eligible individuals from even applying. MacKinnon (1986) suggests that around 10–20% of the population were sufficiently poor to be at real risk of requiring *indoor* relief, much lower than the proportion seeking relief at all.

More generous poor relief policy also benefited citizens indirectly, by removing the need to financially support their relatives. Apart from moral obligation—amplified by the fear of the workhouse—individuals could be legally forced to financially support their relatives before relief was granted, or to repay the cost of relief ex post (King, 2000, p20). Moreover, the concept of "relative" was sufficiently broad that even friends could be called upon to pay for the support of the elderly (Thomson, 1984). Fewer restrictions on who received outrelief thus allowed citizens to pass the burden of supporting the destitute to the community.

[11] The "stock" of paupers was reported on two days each year; the proportion receiving support over a year is estimated using estimates from MacKinnon (1988) that the 1892 ratio of paupers receiving relief over the year to those relieved on January 1 was 2.24. Appendix C.4 includes a detailed classification of paupers.

Reliance on poor relief was particularly high in the rural poor law unions studied in this paper. Poverty in this period was extremely high—Gazeley and Newell (2011) estimate that nutrition levels were lower than in 1980s rural India—and agricultural laborers were particularly vulnerable, being unable to "make ends meet at all if it were not for charitable gifts—sometimes of coal, sometimes of food or clothing" (Rowntree and Kendall, 1913). In the poor law unions I study, an average of 59% of male occupations were agricultural laborers, implying that the typical household was likely very poor.[12] Not only were wages low, the seasonal nature of agriculture provided an additional need for short-term income protection. As such, it is likely that the median voter in these areas would directly benefit from more generous poor relief policy.

## 3.3 The Democratization of Poor Law Institutions

Under the 1834 New Poor Law boards of guardians were elected, but elites held four institutional protections which allowed them to maintain control over spending.[13] First, elected guardians were supplemented by unelected *ex officio* guardians, including all magistrates—typically local landowners—residing in the poor law union. Second, property qualifications prevented poorer citizens from standing as poor law guardians. Third, guardians were elected under a graduated franchise under which the wealthy held multiple votes. In principle all property occupiers—essentially heads of household that owned or rented property—could vote unless they had received relief in the previous year. However, the number of votes varied depending on the value of the property owned or occupied: a single voter could receive up to twelve votes. Finally, there was no secret ballot in place. These provisions were openly acknowledged as protections for landholders (House of Commons, 1878, paras 864–866, 5076). This undemocratic electoral system remained in place for sixty years, until the wide-ranging 1894 Local Government Act ("LGA") removed all four protections for the elite.

The LGA marked the culmination of two long-running movements in Victorian legislation—

[12]Further, analysis of the 1881 100% census sample shows that the median head of household's occupation, according to the Hiscam occupational score metric (Lambert et al., 2013), was in farming in 89% of unions. In the remaining 11% of unions the score was at a similar level.

[13]See Appendix C.1 for a detailed description of the election system.

the rationalization of the system of local government, and democratization of government at all levels. Britain's national institutions had been democratized through the three Reform Acts of 1832, 1867, and 1884. In each case, national reform was followed by reforms to local governments: municipal boroughs in 1835, 1869, and 1878, and then the establishment of county councils in 1888. The primary purpose of the 1894 Act was to complete the task of local government reform in rural areas through establishing a system of elected parish and district councils across England and Wales. Reform of the poor law was a secondary consideration, but once the boards of guardians were included in the provisions of the Act even those MPs concerned that democratization would lead to higher expenditure struggled to defend the status quo as "when the franchise in their other institutions was democratic it was quite impossible much longer to keep the Poor Law system on a different basis" (House of Commons, 1893, para 91). The decision to democratize the boards thus resulted from a reluctant acceptance that the logic of democracy precluded leaving patently undemocratic structures in place (Hurren, 2015, p.215).

To understand how the LGA affected local politics I analyze the evidence provided to the 1909 Royal Commission on the Poor Laws—Appendix Table C.1 summarizes the evidence for each of thirty unions, and Appendix C.2 provides further discussion—combined with historical literature. Delving into local detail is necessary because rural poor law politics was predominantly local, with national parties holding little presence. The content and nature of political debate consequently varied considerably across the country, reflecting both the needs of local communities and social relations between landlord and tenant.

This analysis indicates that the LGA led to a marked shift in political power towards the poor. After 1894 guardians had to account for the views of the poor, since "guardians were not now nominees of large ratepayers but men...who know the wants and miseries of the poor" (Channing, 1918, cited in Hurren, 2015, p.239). As usual in nineteenth-century British politics, few poor law elections were contested, but the perception that relief had been denied unfairly could lead to protests and trigger electoral contests—often with high turnout. In some areas, local agricultural unions provided organizations through which demands could

be formulated, even in the absence of political parties. Further, "the publicity given to [the guardians'] work by means of newspaper reports [made] them even more liable to undue pressure from their constituents"(House of Commons, 1909, p.101). Consequently the 1894 democratic reforms led to significant changes in the composition of boards of guardians, enabling, at least in some cases, dramatic changes in policy. In the words of an assistant commissioner to the Local Government Board: "It was complete revolution...some boards of guardians, certainly for a time, made radical changes and in some cases gave outdoor relief in the most lavish way" (House of Commons, 1909, Appendix I, para 2045).

Nevertheless, the poor continued to face political barriers after 1894. Despite the removal of property qualifications, poorer citizens did not have the time or the financial resources to sit as guardians. While landowners were largely removed from boards, it was farmers that tended to control decision-making as "the country gentleman is unable to secure election, or holds aloof because he is not willing to seek election, and on the other hand, the country laborer is missing because he cannot afford the time" (House of Commons, 1909, p.105). Malapportionment of electoral districts within Poor Law unions could prevent even broad popular support from translating into control of the board of guardians. Consequently, sharp spending increases post-reform tended to be short-lived. Thus, while the LGA increased the political influence of the poor, it did not allow them to fully control poor law policy. In fact, the empirical analysis provides evidence that the elite were able to reduce less politically salient spending.

Hurren (2015) provides a detailed history of the Brixworth union that highlights the political dynamics at play. Brixworth, controlled by the wealthy and politically-influential Earl Spencer, was a "model" union for the Crusade against Outrelief, cutting both the number of outdoor paupers and outdoor spending by over 95% between 1871 and 1894. To achieve these cuts, guardians prosecuted the adult children of elderly paupers for the costs of relief, refused relief to those receiving charity, reduced the number of relieving officers and dealt with claims directly, and referred elderly applicants to private charity. After the Third Reform Act, rural areas became increasingly politicized, leading to the 1893 foundation of a

pressure group demanding the reintroduction of outrelief. Prior to 1894, however, they were unable to gain control over the board of guardians—the LGA enabled them to do so, and consequently to both reintroduce outrelief and upgrade the union workhouse.

In summary, poor law unions were autonomous political units, with control over spending on poor relief. Boards of guardians were elected locally, with contests centred around local issues that were of relevance to a large swathe of the population. The 1894 Local Government Act swept away the remaining vestiges of protection for landed elites. In the following sections I analyze the consequences of that democratic reform.

# 4 Data

This section summarizes the data used in the empirical analysis. I first discuss the sample of unions analyzed, and then the datasets used.[14]

## 4.1 Sample

The paper focuses on rural poor law unions for both conceptual and practical reasons. Conceptually, a focus on rural areas means that we compare unions that are similar in economic structure, but differ in inequality. Specifically, these unions represent settings with a small landed elite and a homogeneous mass of agricultural workers, as in the model of Acemoglu and Robinson (2000). Further, as discussed in the previous section, the median voter likely viewed poor relief as an important safety net. Practically, the main measure of inequality (the mean–median ratio) relies on the agricultural wage, which is only appropriate in rural areas. Restricting the sample to unions that were rural in character throughout the period also ensures that changing poor relief expenditure does not reflect trends in urbanization.

Rural poor law unions are identified using a classification as "rural or mainly rural" by the 1909 Royal Commission on the Poor Laws. This classification was undertaken long after

[14]Full details and summary statistics are provided in Appendix B. The analysis combines newly collected data with several existing datasets, including Southall et al. (1998, 2023); Schürer and Higgs (2014); Southall et al. (2004); and Reid et al. (2018).

the LGA, ensuring stability through the analysis period. I remove poor law unions that underwent substantial boundary changes during the period: those that were established or abolished between 1860 and 1905, and those where the cumulative change in boundaries over the same period exceeded 15% of the 1881 population. Seventeen unions were excluded as the income inequality measure cannot be calculated due to missing wage data.[15] The main regression sample then consists of 208 poor law unions.

## 4.2 Data Sources

The empirical analysis uses two new datasets. The first measures union-level poor law activity using parliamentary papers. The second identifies the location and characteristics of elites across England and Wales. These data are then combined with information on local demographic structure and geographic characteristics.

**Poor Law Data:** This dataset includes annual information on unions' spending, revenue sources, tax base, and the number of paupers relieved in each year 1860–1913. I use these data to construct my main dependent variable—the total spending on poor relief per capita—and several control variables. An 1893 parliamentary paper identifies whether the pre-reform chairman of each union was elected or appointed ex officio.

**Local Elites:** I geolocate the residences and landholdings of rural elites (gentry and aristocracy) in each union, using information from Bateman (1883) and Walford (1886). This process allows me to identify the presence of peers of the Realm and the very wealthy in each union, and also to estimate the share of land owned by these elites.

**Other Union Characteristics:** Demographic and occupational characteristics are obtained using census data, and are interpolated geometrically to construct annual series for use in the panel regressions. The suitability of land for cereal agriculture in each union is

[15] Data was missing for three counties—Huntingdonshire, Rutland, and Suffolk. These missing values do not appear to bias the results: coefficients for other measures of inequality are similar when using the full sample ($N = 225$) and the smaller sample ($N = 208$) used in the main analysis.

estimated using data from the Global AgroEcological Zones project of the Food and Agriculture Organization and 1881 boundary maps. Annual county-level data on the percentage of land devoted to different crops was collected from the *Annual Agricultural Returns*.

# 5 Empirical Approach

This section first outlines the empirical framework used to investigate the relationship between income inequality and the effects of democratic reform, and then introduces the inequality measures used to operationalize the theoretical hypotheses from Section 2.

## 5.1 Empirical Specification

I test whether the impact of the 1894 Local Government Act varied with the level of pre-reform inequality. Critically, democratization was imposed on local councils, avoiding endogeneity issues inherent in national-level democratizations. While national elites shaped the LGA, the democratic reforms it imposed were exogenous to each poor law union, allowing the effect of inequality on post-reform spending to be causally identified.

Specifically, I estimate:

$$y_{it} = \beta \text{inequality}_i \times \text{post1894}_t + \gamma_0 X'_{it} + T_t + \alpha_i + \epsilon_{it} \tag{1}$$

where $i$ indexes poor law unions and $t$ indexes years. $y$ is poor relief spending per capita. $X$ is a vector of control variables, $T$ represents year fixed effects, and $\alpha$ represents poor law union fixed effects. $\epsilon$ is an error term. Union fixed effects control for time-invariant differences in the level of relief in each union (e.g., culture). Year fixed effects capture events common to all unions, such as national economic shocks. As the level of inequality is measured at a single point in time pre-reform, the level variable is absorbed by the union-fixed effects. The measures of inequality are introduced in the following subsection.

The main coefficient of interest is $\beta$, which relates to the interaction between pre-reform

inequality and the post-reform period.[16] For estimates of $\beta$ to be interpreted as capturing a causal effect of the interaction between inequality and democratic reform, two key identifying assumptions must hold. First, that in the absence of democratic reform, and after adjusting for control variables, there would have been no relationship between inequality and changes in poor law spending. This assumption is similar to the "conditional parallel trends" assumption in a difference-in-difference framework. Second, any reaction to the 1894 democratic reform must reflect a response to local inequality rather than another characteristic correlated with inequality. Both assumptions require careful justification given that the level of inequality is not a randomly-assigned treatment and is, in particular, strongly correlated with the type of agriculture—a point discussed in detail in Section 5.3 below.

I use four approaches to address the concern that the level of inequality may be correlated with underlying trends in poor law spending. First, I include a large set of control variables including (i) a vector of "crop controls" capturing county-level land usage to account for differences in agricultural type; (ii) "demographic controls" to account for differences in demand for poor relief, including population density, population, age structure, and decadal variation in the number of paupers per capita; and (iii) "financial controls", including the log tax base per acre and the percentage of revenue from taxation, to account for differences in ability to fund poor relief. Second, the results remain similar when incorporating union-specific linear trends, accounting for any (linear) differences in the trend of poor law spending before 1894. Third, following Gentzkow (2006), I flexibly control for differences in the time path of poor law spending driven by the endogeneity of inequality by including interactions between county-level observable characteristics in a base year and quartic time trends. Finally, I test whether union-level inequality is associated with pre-trends in either local spending or other observable characteristics—there is little evidence of either, as shown in Figure 4. Consequently, it does not appear that poor relief would have evolved differently in more unequal unions had the 1894 reform not occurred.

To address the second concern—that the estimated $\beta$ could capture the effect of some

[16] To use terminology more common in political science, inequality acts as a "moderator" of the reform's effect (see, for instance, Hainmueller et al., 2019).

other variable correlated with inequality—I allow for heterogeneous reactions to the democratic reform according to other district characteristics. In particular, I allow for interactions between the post-1894 dummy and various measures of economic need, pre-reform economic decline, the ease of out-migration, characteristics of urban surroundings, and the importance of seasonal agriculture. The main findings are robust to including these controls, providing further evidence that the results capture a causal effect of inequality on the impact of democratic reform.

The causal interpretation of the results would be threatened if the broader changes enacted by the LGA affected poor relief through channels other than the democratization of boards of guardians. The creation of parish councils poses little concern, as they had limited spending or tax-raising powers and failed to inspire voter interest (Keith-Lucas, 1952, p.42). However, the establishment of rural district councils is more problematic because guardians also served as representatives on these councils. Although the bodies were organizationally distinct, this overlap means that guardians could plausibly have been elected based on considerations other than poor relief. Further, the LGA expanded the range of spending controlled by these bodies, meaning the guardians' role as rural district councilors changed alongside electoral reform. Appendix C.3 discusses this potential issue in depth and demonstrates that these other changes are not a major concern in practice.

One final complication is that the 1894 reforms were likely, at least to some extent, anticipated. The bill itself was introduced in the House of Commons in March 1893, more than eighteen months before the first elections under the new electoral system. Further, in late 1892, an intermediate step of lowering (but not removing) property qualifications for elected guardians was taken by the Local Government Board. This change likely had a limited direct effect on policy, both because of the remaining institutional protections for landowners and because only some of the guardians on the boards were elected each year. However, it is possible that guardians adjusted their policy to increase the chances of winning elections later. Such a response would mean that the effects of reform are underestimated.

## 5.2 Measures of Inequality

The key challenge for the empirical analysis is to disentangle the effects of different aspects of inequality, and in particular isolate the channel highlighted by Meltzer–Richard. To do so, I construct several variables capturing different dimensions of inequality. To test the Meltzer–Richard hypothesis I construct a measure of income inequality that is directly related to the relative tax burden of the rich and poor. Further measures capture possible fairness concerns, elite de facto power, and the presence of economically vulnerable groups. Including these measures together in the same regression provides a robust test of whether inequality impacts government spending through fiscal incidence, as predicted by Meltzer–Richard, or through alternative channels.

Table 1: Operationalization of Channels Linking Inequality to Government Spending

| Channel | Predicted Effect post-Democratization | Empirical Measures |
|---|---|---|
| Relative tax burden / Meltzer–Richard ($\bar{y}/y_P$) | Income Inequality $\longrightarrow g \uparrow$ | Mean rental cost/ Median wage |
| Elite De Facto power ($1-\phi$) | Elite power $\longrightarrow g \downarrow$ | Presence of a peer Unelected chairman |
| Inequality aversion / Fairness concerns ($\kappa$) | Salient class differences $\longrightarrow g \uparrow$ | % Land owned by economic elite<br>% Large farms<br>Cuts to outdoor relief 1866–85 |
| Economic need ($\pi, \lambda$) | Need $\longrightarrow g \uparrow\downarrow^{\dagger}$ | Cereal suitability index<br>% Agricultural laborers<br>% Over 64 |

Notes: $g$ is the spending on poor relief per capita. † Theoretically, the prediction is ambiguous: greater economic need mechanically increases total spending due to a higher number of recipients, but also raises the marginal cost of relief, potentially reducing the optimal level of relief provided.

Table 1 connects each measure to the conceptual framework presented in Section 2, and summarizes the predicted relationship with post-reform government spending per capita ($g$).

**Relative Tax Burden / Meltzer–Richard** To test the Meltzer–Richard hypothesis, I construct a measure of income inequality derived from household spending on property. In Meltzer and Richard (1981), as well as in the simple model in Section 2, government spending is determined by differences in the relative tax burden between the mean and median income earners, implying that the difference between mean and median income ratio is a predictor of redistributive pressure. In the historical context I study, taxes were levied not on income but on the rental value of occupied property—that is, the value at which a property could be rented. As such, as shown formally in Appendix A, the theoretical predictions in this setting relate to the ratio of mean to median household spending on property. I estimate this ratio as follows:

$$meanMedianRatio = \frac{Mean\ rental\ value}{Median\ wage} \tag{2}$$

The gross rental value for each district is available from the *Local Taxation Returns* and the median wage is estimated using county-level data on the 1869 agricultural wage from Collins and Thirsk (2000, Table 42.3). The median wage proxies for the median spending on property if the median voter spends the same proportion of their income on property across unions. The measure in (2) captures income inequality if spending on property increases monotonically with income (i.e., it is a normal good).[17]

[17]Available evidence supports these assumptions. First, the agricultural wage reflected not only the wage paid to workers, but also provides a guide to local wages more generally (Hunt, 1973). Second, for typical agricultural households the income elasticity of housing appears to have been close to one: Horrell (1996) reports that households in low-wage agricultural regions (1840–1854) spent 10.4% of income on housing, compared to 9.9% in high-wage regions. This small variation supports the assumption that the median wage is highly correlated with median rental expenditure. Third, at higher income levels, income elasticity of housing was likely less than one, but still positive: the 1852 Select Committee on Income Taxation (Qs 5383) estimated that the share of income spent on rent declined from around one-eighth to one-tenth between annual incomes of £160 and £1,500 per annum—well above the typical agricultural wage (generally under £50). Similarly, Engels' original estimates (reported in Chai and Moneta, 2010) suggest an income elasticity of housing of around 1 at low income levels, falling to approximately 0.7 at higher levels.

In Appendix D.1, I present a number of robustness tests to ensure that the results are not driven by measurement assumptions. First, I replace the rental value in the numerator of the inequality measure with a direct measure of the tax base (the "rateable value"). Second, I replace the denominator with wage data from 1892 (county-level) and 1894 (union-level), to test sensitivity to the choice of year and the geographic unit.[18] Third, I use the HISCAM occupational score to proxy the status of the median household without relying on wage data. The main results are robust to using these alternative inequality measures.

The mean–median ratio is particularly valuable because, in contrast to other inequality measures, it directly captures the share of the tax burden borne by the poor—the mechanism at the core of the Meltzer–Richard hypothesis. As shown in Appendix Table B.3, a higher mean–median ratio is associated with several of the other inequality measures discussed below, including inequality in land ownership, the share of agricultural laborers, and the share of large farms. This demonstrates that the mean–median ratio captures some of the variation in these other measures, as we would expect given that they also reflect aspects of the income distribution. However, in this setting, these alternative measures are only indirectly related to the relative tax burden—a large landowner would not, for example, pay tax if they rented out their land. Consequently, they provide a less precise test of the Meltzer–Richard channel. As such, I use them to test the broader hypothesis that inequality shapes post-reform government spending. Further, including these other variables alongside the mean–median ratio in a regression allows me to separate the importance of the relative tax burden from these other theoretical channels.

**Elite De Facto Power** Even if higher inequality generates redistributive pressure, it may not translate into higher spending if the wealthy retain effective control over policy (the welfare weight on the poor, $\phi$, is low). I capture potential elite power using two measures. National political influence is measured using a binary indicator for whether a peer of the realm had a seat within the union. Local political control is measured using a binary indicator

[18]The 1892 and 1894 wage data provide a less precise measure of income and are available for fewer unions than the 1869 measure. Using the 1869 inequality measure also avoids potential endogeneity if inequality itself reacted to political changes in the 1890s.

for whether the pre-reform chairman of the board of guardians was an unelected (appointed *ex officio*) member of the local gentry. Because the chairman was elected by the board members, this indicator identifies whether the local elite commanded the support of other guardians.

**Fairness Concerns / Inequality Aversion** Inequality may also increase pressure for redistribution due to fairness concerns, such as an inherent aversion to inequality—summarized by the parameter $\kappa$ in the model. To investigate this channel empirically, I use three measures that may affect the salience of differences between classes in this particular setting.[19] To capture the importance of wealthy landowners I use land inequality, estimated as the share of land owned by the very wealthy in each union. High land concentration may increase the salience of economic differences between classes, generate resentment, or skew understandings of the income distribution, and associated tax burdens. This mechanism could confound the test of the Meltzer–Richard mechanism, as a high mean–median ratio could reflect the presence of a small number of very salient large landowners, leading to a high tax for reasons unrelated to the tax base. Second, I use the share of large farms (those with more than five employees), which could increase the salience of agrarian class inequality. Third, I directly measure a source of grievances between classes—the harshness of policy during the "Crusade Against Outrelief". A harsher crusade could generate resentment as the poor felt the landed elites had reneged on their paternalistic responsibilities (Hurren, 2015). I measure harshness as the percentage decline in the number of outdoor paupers between pre-crusade (1866–70) and post-crusade (1881–85) years.

**Economic Need** The final row of the table reflects the fact that the existence of economically vulnerable groups may drive government spending. In the model, this is captured by the probability of being unemployed $(1-\pi)$ and the share of the population that is poor $(\lambda)$. These groups have low incomes, and hence indirectly contribute to income inequality. How-

[19] Agricultural economies often featured stark hierarchies, with large landowners and economically dependent landless laborers. Such conditions can fuel resentment and demands for redistribution (Domènech and Sánchez-Cuenca, 2022), particularly if inequality is visible, or landowners are perceived to have broken a paternalistic social contract (Scott, 1977).

ever, the theoretical prediction regarding the effect of greater need for poor relief on spending is ambiguous. On one hand, more recipients mechanically increase total relief spending; on the other, they raise the marginal financial cost of more generous relief. Economic need could, in principle, also affect spending through other channels such as altruism (operating through $\kappa$) or through increasing the number of dependents in a household and hence relief received by households in employment ($\psi$).

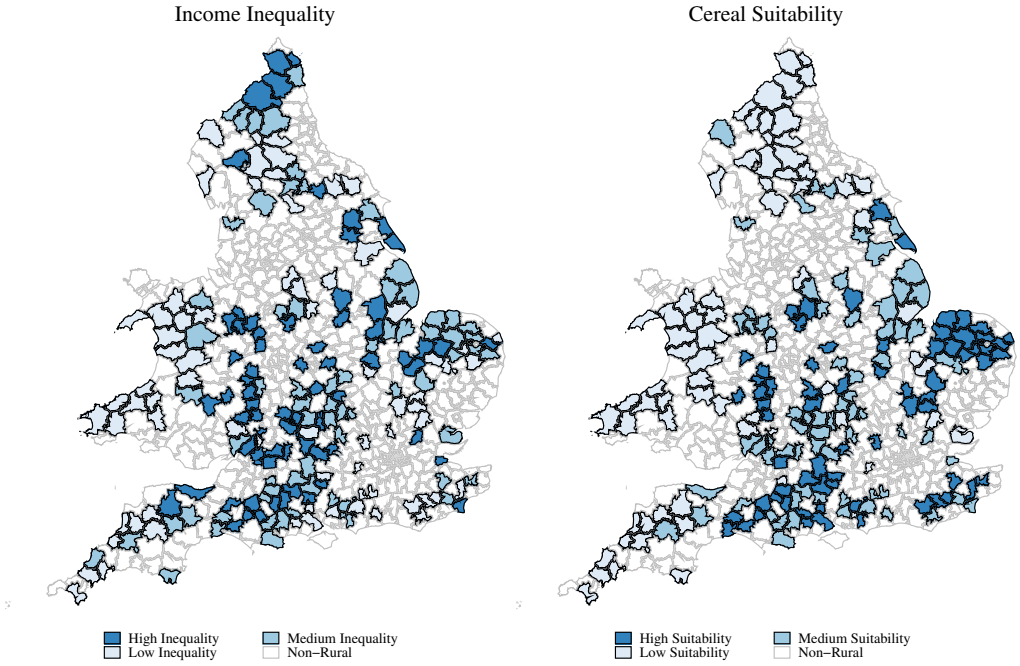
I use three measures to capture variation in economic need. First, higher suitability for cereal production is expected to be correlated with a higher proportion of highly-seasonal cereal agriculture and hence a need for poor relief to support households outside of harvest season relative to areas dominated by pastoral agriculture. Second, I use census data to measure the share of male rural laborers amongst the population employed in agriculture. Third, the percentage of the population over 64 identifies areas with a high number of elderly citizens likely to rely on poor relief prior to the advent of a national pension system.

## 5.3 Inequality and Agriculture Type

The data show a clear pattern whereby areas with high cereal suitability—and hence arable agriculture—tend to be more unequal, as shown in Figure 3. There is clear regional clustering according to the level of income inequality, with more unequal areas generally used for higher value crops: income inequality is positively correlated with the 1885 percentage of county land used for wheat (r= 0.15, p-val=0.03) and negatively correlated with that used for oats (r= $-0.31$, p-value=0.00). Cereal suitability is strongly correlated with all the inequality measures except for local elite de facto power (see Appendix B.3). This pattern may be explained by the fact that higher value agricultural production affects patterns of land settlement and land value, and hence inequality (Cinnirella and Hornung, 2016). Cereal agriculture also requires more farm labor than pastoral agriculture, leading to a larger mass of rural laborers.

The connection between agriculture type and inequality poses two challenges for the empirical analysis. First, as discussed above, workers in arable agriculture were more reliant

Figure 3: Poor law unions with high inequality are geographically clustered.




Note: The left-hand panel displays the estimated mean–median ratio from Equation (2). The right-hand panel displays FAO soil cereal suitability. High, medium, and low refer to terciles.

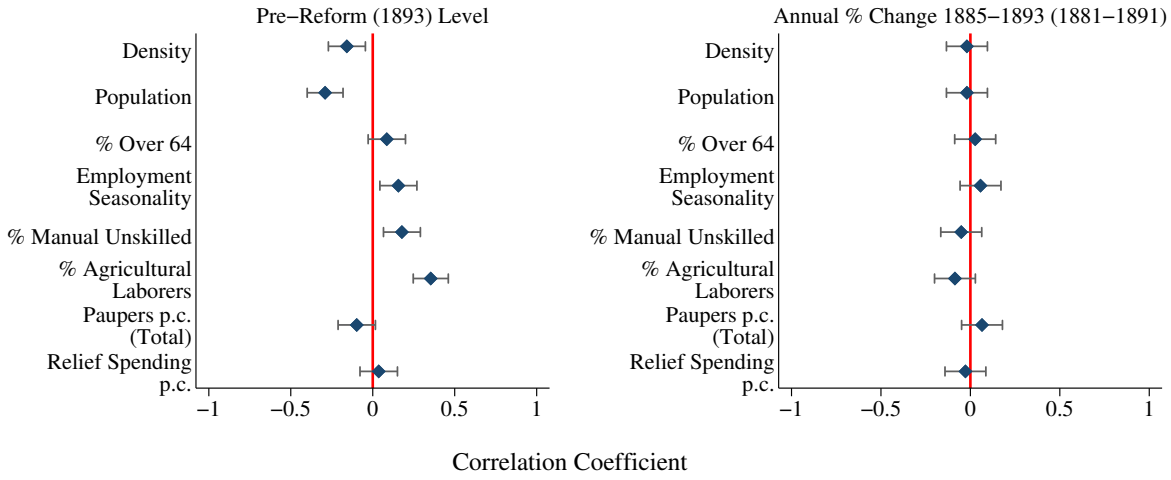
on poor relief to support their families outside harvest season.[20] As a result, it is important to test whether other inequality measures represent an independent causal channel, or instead simply capture greater need for poor relief in cereal-producing unions. Second, during this period cereal production in Britain declined rapidly due to the "grain invasion" from Eastern Europe and the United States—an economic shock that could have created growing demand for poor relief in more unequal areas, threatening the conditional parallel trends assumption. While the main impact of the shock was before the analysis period, I account for this concern by controlling for agriculture type in the main specifications and adding robustness tests allowing for different time trends according to land type.

Figure 4 provides evidence that differences in union characteristics are not a concern in practice. Despite the clear correlation between income inequality and various other variables across unions (left-hand panel), inequality does *not* appear correlated with the changes in

[20]Seasonality in the number of outdoor paupers was correlated with the cereal suitability of a union ($r = 0.17$; p-value $= 0.00$).

observable characteristics pre-reform. This result suggests that agricultural type did not lead to changing demand for poor relief in the years leading up to the reform, supporting the validity of the identifying assumption. A more formal examination follows in Section 6.3.

Figure 4: Income inequality is not correlated with trends in other union characteristics.




Note: Figure displays regression coefficient for a one standard deviation increase in income inequality on the level in 1893 (left-hand panel) or change 1885–1893 (right-hand panel). Bars represent 90% confidence intervals. For variables reported in the decennial census, levels are from 1891 and the change is for 1881–1891.

The geographic clustering of inequality also raises the concern that spatial autocorrelation could lead to spurious regression results—an issue I discuss in detail in Appendix D.4. First, as recommended by Conley and Kelly (2025), I calculate Moran's I (Moran, 1948) for both the inequality measure and the change in relief spending. There is little evidence of spatial autocorrelation after accounting for agriculture type and the other regression controls. Second, I re-estimate the main regressions allowing for arbitrary spatial correlation between unions for a range of bandwidths (Conley, 1999). The estimated standard errors are similar to standard errors clustered by poor law union, suggesting that spatial dependence is not inflating estimated precision. As a conservative measure, I use standard errors allowing for spatial autocorrelation within a bandwidth of 100 kilometers in the regression analysis. Finally, Table 2 demonstrates that the results are robust to allowing for flexible time trends according to location.

# 6 Results

The empirical analysis provides strong and robust evidence that more unequal areas experienced greater increases in government spending following the 1894 democratic reform. The first subsection demonstrates strong support for the Meltzer–Richard hypothesis. The second subsection tests all the theoretical hypotheses presented in Table 1, disentangling the effects of different types of inequality. The third subsection investigates the dynamic effects of inequality post-reform. The final subsection then disaggregates the effects of inequality on different components of poor relief expenditure.

## 6.1 Support for the Meltzer–Richard Hypothesis

Democratic reform led to larger increases in poor relief expenditure in areas with higher income inequality, as shown in Table 2. All variables are standardized, so that the regression coefficients represent the effect of a one standard deviation increase in each explanatory variable in terms of standard deviations of the outcome variable—per capita expenditure on poor relief. The coefficient on income inequality is positive and statistically significant across all specifications—areas with higher income inequality experienced greater increases in spending following the Local Government Act, consistent with the Meltzer–Richard hypothesis.

Importantly, the estimated coefficient on income inequality remains robust after including various sets of control variables. The coefficient remains stable across specifications after adding vectors of "crop controls" in (2), demographic controls (3), and financial controls (4). Specifications (5)–(7) allow flexibly for potential confounds by including quartic time trends interacted with union characteristics observed before the analysis period. First, to allow for trends in the demand for poor relief to differ according to agriculture type, I include a trend interacted with the suitability for cereal agriculture. In the second specification I allow for different time paths according to the ease of outmigration, proxied by the size of the nearest large town (over 25,000 population). Specification (7) allows spending of poor relief to evolve based on location, accounting for possible spatial trends in poor relief. Appendix Table D.8

Table 2: High inequality districts had greater increases in expenditure following 1894 democratic reforms.

| | DV=Relief Expenditure per Capita (Standardized) | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Income Inequality | 0.12*** | 0.09*** | 0.11*** | 0.11*** | 0.11*** | 0.11*** | 0.11*** |
| x post1894 | (.030) | (.028) | (.027) | (.026) | (.026) | (.027) | (.026) |
| Population | | | 0.35 | 0.30 | 0.25 | 0.32 | 0.18 |
| | | | (.453) | (.460) | (.463) | (.464) | (.474) |
| Population Density | | | −0.20 | −0.26 | −0.25 | −0.26 | −0.24 |
| | | | (.435) | (.446) | (.447) | (.447) | (.448) |
| Decadal Variance | | | 0.02 | 0.02 | 0.02 | 0.02 | 0.03 |
| in Pauperism | | | (.021) | (.020) | (.019) | (.020) | (.019) |
| % Popn Male | | | 0.02 | 0.02 | 0.00 | 0.02 | 0.01 |
| Age Over 64 | | | (.070) | (.068) | (.068) | (.068) | (.068) |
| % Popn Female | | | 0.34*** | 0.32*** | 0.30*** | 0.32*** | 0.26*** |
| Age Over 64 | | | (.092) | (.090) | (.090) | (.090) | (.091) |
| % Revenue | | | | 0.13*** | 0.12*** | 0.13*** | 0.12*** |
| from Poor Rate | | | | (.026) | (.026) | (.026) | (.025) |
| Log Tax Base | | | | −0.11 | −0.08 | −0.11 | −0.03 |
| Per Acre | | | | (.114) | (.117) | (.116) | (.117) |
| Estimated Effect of Inequality as % of pre-Reform Spending: | | | | | | | |
| 10th Percentile | 6% | 5% | 6% | 6% | 5% | 6% | 6% |
| Median | 10% | 8% | 9% | 9% | 9% | 9% | 9% |
| 90th Percentile | 14% | 11% | 13% | 13% | 12% | 13% | 13% |
| Average | 10% | 8% | 9% | 9% | 9% | 9% | 9% |
| % Δ Spend post-1894 | 70% | 56% | 63% | 63% | 63% | 63% | 63% |
| Crop Controls | N | Y | Y | Y | Y | Y | Y |
| Demographic Controls | N | N | Y | Y | Y | Y | Y |
| Quartic Time Trend | | | | | % Wheat 1885 | Nearest Town Size | Longitude &Latitude |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y | Y |
| No. Observations | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 |
| No. PLUs | 208 | 208 | 208 | 208 | 208 | 208 | 208 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All variables are standardized. "Crop Controls" is the county-level percentage of land used for different crops. "Demographic Controls" include the percentage of population of each sex in different age groups. "Quartic time trend" is a quartic polynomial in time interacted with the variable named in that row. Nearest Town Size is the log population of the nearest town with more than 25,000 inhabitants in 1891. The first four rows in the second panel report the estimated effect of different levels of inequality relative to spending in 1893. "% Δ Spend post-1894" is the estimated average effect of inequality post-reform relative to the actual change in spending after 1894. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses.

shows similar specifications allowing for time trends according to the 1869 wage, population density, tax base per acre, the fall in tax base per acre 1875–1885, the distance from a major city, and seasonality of agriculture. The estimated coefficient on income inequality is of a similar magnitude and statistically significant across all specifications.

The second panel of Table 2 investigates the magnitude of the effect of higher income inequality, both in absolute terms and relative to the overall growth in spending after 1894. The estimated effects are modest relative to pre-reform spending levels. At the median level of inequality in the sample, the estimated effect is approximately a 9% increase in per capita spending relative to spending in 1893. At the 90th percentile, the estimated effect is around 13%. These muted effects align with the limited impact of democratization we have seen in Figure 1. Overall, per capita spending increased by just 15% in the decade following the reform, likely reflecting the remaining barriers to political participation of the poor discussed in Section 3.3. The estimates in the final row of the panel indicate that more than half of this growth was due to the presence of inequality: if unions had been entirely equal, increases in expenditure would have been markedly lower. Income inequality thus appears to have exerted an important influence on the—relatively small—effect of democratic reform.[21]

## 6.2 The Effect of Alternative Dimensions of Inequality

I now investigate other potential mechanisms linking inequality and government spending listed in Table 1. I first re-estimate specification (4) from Table 2 using each inequality measure separately, and then include multiple measures together to disentangle different channels. To allow comparisons across specifications, each non-binary inequality measure is again standardized.

The results, presented in Table 3, provide some evidence for all four causal channels. When examining each hypothesis independently, I find (sometimes weakly) statistically sig-

[21] As a comparison, Husted and Kenny (1997) find that eliminating each of poll taxes and literacy tests in the United States in the 1960s raised state welfare spending by between 10% and 20%. Although these estimates are not directly comparable, they suggest that the reforms here had a smaller effect despite starting from a lower pre-reform base level of taxation, and involving a reform that had much broader electoral consequences.

nificant coefficients in the expected direction for all measures except the presence of an aristocrat, which proxies national political influence. The analysis thus provides some support for various theoretical channels suggested in previous studies, with elite de facto reflecting local rather than national influence.

The final two columns of Table 3 suggest that the Meltzer–Richard model identifies a distinct mechanism through which inequality affects government spending. The coefficient on income inequality remains similar in size and statistical significance when controlling for all the other inequality measures. These specifications thus provide reassurance that the mean–median rental ratio—and hence the relative tax burden—captures an effect of inequality that is distinguishable from local economic structure and other inequality measures.

There is also consistent evidence that spending grew more in arable-intensive unions and less in unions controlled by local elites. Elite de facto power $(1 - \phi)$ thus appears to have played an important role in shaping the effect of democratic reform. In contrast, the coefficients relating to fairness concerns $(\kappa)$ shrink considerably and become statistically indistinguishable from zero once cereal suitability is controlled for. The apparent effects of these variables may thus capture other, correlated, local characteristics—particularly economic structure—rather than indicating an independent causal mechanism. Alternatively, cereal suitability may capture multiple mechanisms through which inequality affected government spending, and our measures are insufficiently nuanced to tease them apart.

Appendix D.1 examines alternative operationalizations of the different dimensions of inequality. This analysis uses alternative definitions of some measures—the percent of wheat or percent of old-aged men or women to capture the need for poor relief, and the drop in adult paupers to capture harsh relief policies. For other inequality measures I introduce alternative variables entirely. Specifically, as alternative sources of fairness concerns I use land inequality in terms of land value (not size), wealth inequality (estimated using the number of servants), and the number of gentry in a union. As an alternative to aristocratic presence, I use the presence of a great landowner in case wealth, rather than national prestige, provided de facto power. The conclusion that the effect of democratic reform was shaped by

income inequality, economic structure, and elite de facto power is robust to these alternative specifications. Further, the results are similar when allowing for alternative definitions of the mean–median income ratio—see Appendix Table D.4.

Appendix D reports a number of additional robustness tests. First, I allow for the effect of the reform to vary according to a range of characteristics that could affect the demand for relief, including both variables to capture differences in economic need (land value, wages, and the seasonality of pre-reform relief; Appendix Table D.6) and the ease of outmigration (distance to nearby towns, population density, and change in demographic structure; Appendix Table D.7). The coefficients on these variables are sometimes statistically distinguishable from zero, but they do not disturb the main findings regarding inequality. Second, I allow for different time trends according to the observable characteristics of different poor law unions—a further check that omitted variable bias does not explain the results (Appendix Table D.8). Third, I show that the results are not driven by particular sub-samples of poor law unions (Appendix Table D.9). Fourth, I account for differences in urban surroundings, which could plausibly drive migration and wage dynamics. In particular, I control for the population, population growth, and tax base of the nearest town to each union (Appendix Table D.10), as well as allowing for the reaction of the reform to differ according to these characteristics (Appendix Table D.11). The magnitude and statistical significance of the coefficients relating to inequality remains similar across these specifications.

In summary, the analysis provides robust evidence that the effect of democratic reform on spending was shaped by three dimensions of inequality. Spending increased more in areas of higher income inequality, consistent with the Meltzer–Richard model. It also grew more in unions with high cereal suitability, which may reflect greater need for income support. In contrast, spending grew less where elites controlled poor law boards before reform. However, after accounting for differences in economic structure, there is little evidence that fairness concerns played a significant role in shaping post-reform spending. The findings thus provide evidence for both median voter models of redistribution and theories emphasizing the importance of elite de facto power.

Table 3: Effect of democratic reform was conditioned by multiple dimensions of inequality.

| | DV=Relief Expenditure per Capita (Standardized) | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Meltzer-Richard ($\bar{y}/y_P$) | Elite De Facto Power ($1-\phi$) | | Fairness Concerns ($\kappa$) | | Economic Need ($\pi$, $\lambda$) | | | Multiple Channels | | |
| Post 1894 x | | | | | | | | | | | |
| Income Inequality | 0.11*** | | | | | | | | | 0.09*** | 0.10*** |
| | (.026) | | | | | | | | | (.028) | (.026) |
| Local Elite Control | | −0.13** | | | | | | | | −0.11** | −0.11** |
| | | (.057) | | | | | | | | (.053) | (.054) |
| Aristocratic Elite | | | −0.00 | | | | | | | −0.00 | |
| | | | (.054) | | | | | | | (.052) | |
| Land Inequality | | | | 0.05* | | | | | | 0.03 | |
| | | | | (.026) | | | | | | (.025) | |
| % Large Farms | | | | | 0.06** | | | | | −0.04 | |
| | | | | | (.029) | | | | | (.038) | |
| Outrelief Cuts 1866–85 | | | | | | 0.06* | | | | 0.02 | |
| | | | | | | (.031) | | | | (.030) | |
| Cereal Suitability | | | | | | | 0.11*** | | | 0.08** | 0.10*** |
| | | | | | | | (.031) | | | (.035) | (.030) |
| % Agricultural Laborers | | | | | | | | 0.10*** | | 0.04 | |
| | | | | | | | | (.026) | | (.039) | |
| % Age over 64 | | | | | | | | | 0.08** | 0.06* | |
| | | | | | | | | | (.032) | (.030) | |
| Controls | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| No. Observations | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 |
| No. PLUs | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 |

Notes: All non-binary variables are standardized. "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level.

## 6.3 Dynamic Effects

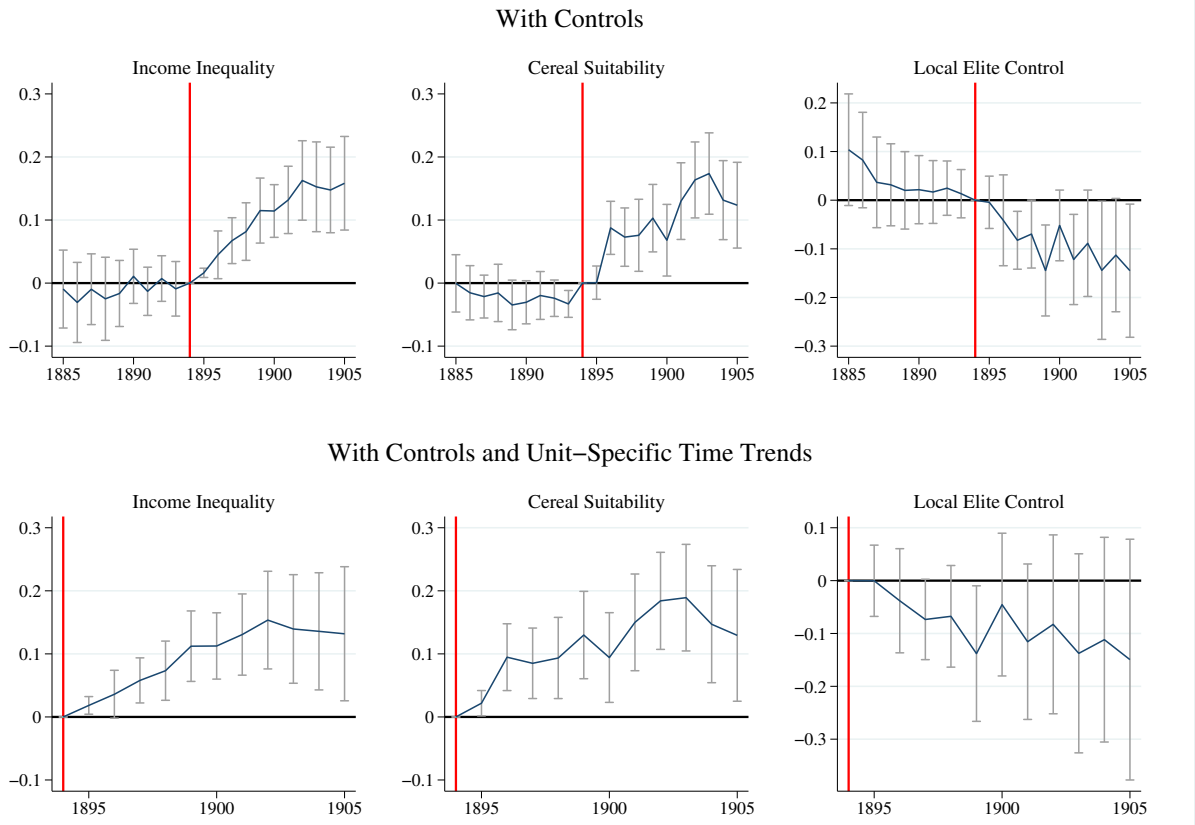
This subsection investigates the dynamic effect of the democratic reform, focusing on the three forms of inequality—income inequality, elite de facto power, and economic need—which emerged as important previously. To do so I estimate flexible regression specifications allowing the effect of each inequality measure to differ every year. Doing so both investigates the timing of the effects identified in Table 3—whether they emerged immediately post reform, and whether they persisted over time—and also checks whether there is any evidence of pre-trends before 1894 that would suggest the identifying assumptions are violated. This set-up also allows a more stringent test of causal identification through the inclusion of union-specific linear time trends that capture any pre-reform trends in spending.

The dynamic analysis, presented in Figure 5, provides further evidence that spending increased in areas of high income inequality (left-hand side) and those of higher economic need (central column). There is little evidence of any relationship between either variable and spending pre-reform—supporting the conditional parallel trends assumption—but a clearly increasing trend immediately after. Notably, areas with more cereal suitability experienced an upward jump in spending after 1894, consistent with this variable capturing the presence of needy families that would immediately benefit from more generous provision. Further, the bottom panel of Figure 5 demonstrates robustness to the inclusion of union-specific linear time trends—further evidence that the effects are not driven by pre-reform differences.[22]

We see a different pattern when examining elites' de facto control. Areas with ex officio chairmen appear to have had falling spending even before the 1894 reform, particularly before 1890. Such a result makes sense both historically, given the Crusade Against Outrelief, and theoretically—if elites had control, why would they not use it? However, this pattern suggests that the results in Table 3 may partly reflect elite efforts to reduce spending pre-reform. Nevertheless, Figure 5 indicates that democratization strengthened elites' attempts to reduce expenditure.

[22]These specifications allow for interactions between inequality and the year dummies only post-reform, so that the union-specific trends do not capture part of the dynamic response to the reform (see Wolfers, 2006). In the dynamic specifications, I use an 80 km bandwidth to account for spatial autocorrelation, ensuring sufficient spatial variation to estimate standard errors for the individual year coefficients.

Figure 5: The relationship between inequality and relief spending emerges only after the democratic reform.




Notes: The top panels plot $\beta_j$ and $\beta_k$, and associated 90% confidence intervals, from the specification:

$$y_{it} = \sum_{j<1894} \beta_j \left(Inequality_i * year_j\right) + \sum_{k\geq 1895} \beta_k \left(Inequality_i * year_k\right) + \delta X_{it} + \alpha_i + \mu_t + \epsilon_{it}$$

where *Inequality* refers to the variable in the title of each panel. Similarly, the bottom panels plot $\gamma_k$ and associated 90% confidence intervals, from the specification:

$$y_{it} = \sum_{k\geq 1895} \gamma_k \left(Inequality_i * year_k\right) + \delta X_{it} + \alpha_i + \mu_t + T_{it} + \epsilon_{it}$$

where $T_{it}$ are union-specific linear time trends. The excluded category in both cases relates to 1894, so results are relative to the year of reform. All specifications include controls from specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 80km using the approach of Colella et al. (2019).

## 6.4 Disaggregating the Effects on Poor Relief Expenditure

Table 4 investigates the ways in which inequality shaped poor relief expenditure. I investigate both different spending categories and also the number of recipients of relief. In both cases I distinguish between relief provided outside of the workhouse ("outdoor")—at the heart of many political debates about poor relief—and in the workhouse ("indoor"). I also examine spending, such as salaries of poor law officers, that was not directly related to pauper maintenance.

The results suggest that the effect of higher income inequality was predominantly on spending on outdoor relief, consistent with both the theoretical predictions and historic evidence. Conceptually, we can consider the provision of outrelief as capturing $\psi$, the fraction of relief received by households while employed. The minimal evidence of any effect of income inequality on spending on indoor relief is consistent with the Meltzer–Richard prediction only applying if $\psi$ is sufficiently high. Contemporary evaluations of the Act, summarized in Appendix C.2, also emphasized the effect of the reform on outrelief. In Stow Union, for example, prior to 1894 the guardians "were very particular as to the grant of outrelief, and if the least doubt existed as to the condition of the applicant, an order for admission to the workhouse only was given. In the year 1895, the new qualification admitted a majority of laborers to the board, and [their] policy was immediately subverted...outrelief jumped from £45 to £92 a week, and later to £100."

The results relating to the other inequality measures are also compatible with the theoretical framework. The negative effect of elite control is focused on other expenditure and, more weakly, spending in the workhouse. This pattern suggests that reform may have forced elites to be more generous in publicly-salient—and hence high $\phi$—elements of relief after 1894, while using their political influence to reduce less observable forms of expenditure. The impact of greater economic need is clearest on other expenditure, which is consistent with scaling up the overall provision of relief, rather than expanding the generosity of relief for each recipient. The overall pattern is thus consistent with the model in Section 2, although it is important to note that the mapping from the theoretical parameters to these

Table 4: The primary effect of income inequality was increasing outdoor relief.

| | Dependent Variable (Standardized) | | | | | |
|---|---|---|---|---|---|---|
| | Expenditure p.c. | | | Paupers p.c. | | |
| | Outdoor | Indoor | Other | Total | Outdoor | Indoor |
| Post 1894 x | | | | | | |
| Income Inequality | 0.09*** | 0.00 | 0.06** | 0.11*** | 0.08*** | 0.04 |
| | (.027) | (.033) | (.027) | (.027) | (.022) | (.025) |
| Local Elite Control | 0.00 | −0.08 | −0.15*** | 0.02 | 0.02 | −0.08 |
| | (.058) | (.067) | (.054) | (.055) | (.050) | (.051) |
| Cereal Suitability | 0.03 | 0.06* | 0.11*** | 0.02 | 0.03 | 0.03 |
| | (.028) | (.037) | (.033) | (.029) | (.026) | (.027) |
| Controls | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y |
| No. Observations | 4,368 | 4,368 | 4,368 | 4,368 | 4,366 | 4,364 |
| No. PLUs | 208 | 208 | 208 | 208 | 208 | 208 |

Notes: "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level.

disaggregated outcomes is not clear-cut, and partly reflects ex post interpretation.

Further analysis, presented in Appendix D.5, provides suggestive evidence that the main effect of the 1894 reforms was to increase support for the elderly and women, rather than the adult male workforce. Appendix Table D.12 estimates the effect of the reforms on different categories of paupers, classified by age (adult versus child), sex, and ability to work ("able-bodied" versus "non-able-bodied," a category that in practice included many elderly paupers). These latter categories should be treated with some caution as they were not well-defined and likely manipulable to justify the provision of relief. Income inequality leads to a clear increase in the number of non-able-bodied outdoor paupers as well as outdoor able-bodied adult women, but there is no clear effect on indoor pauper numbers, or on able-bodied men. Higher economic need (cereal suitability) is associated with greater support to

non-able-bodied paupers, suggesting that democratic reform did not expand the use of poor relief as a means of insuring against seasonal unemployment.

# 7 Conclusion

This paper provides empirical support for Meltzer and Richard (1981)'s prediction that higher income inequality leads to greater government spending on redistribution. I test the effects of democratization on government redistributive spending in a setting that closely aligns with the assumptions underpinning the Meltzer–Richard model—autonomous, locally-elected councils funding welfare through an approximately proportional tax levied on all households. This institutional context allows me to isolate the core theoretical mechanism—the relative tax burden borne by rich and poor—while sidestepping many confounding factors present in national-level analyses. The results demonstrate that national democratic reform in 1894 led to greater increases in spending in areas of higher pre-reform income inequality, consistent with theoretical predictions.

These findings illustrate the importance of separating the precise predictions of the Meltzer–Richard model from a more general hypothesis that inequality will increase government spending. Common measures of inequality, such as the Gini coefficient, may be poorly aligned with actual tax burdens and therefore weakly capture this mechanism. Indeed, in this paper, the estimated mean–median income ratio predicts post-reform spending more strongly than alternative inequality measures. Such empirical challenges may explain why many previous studies have found weaker evidence in support of the Meltzer–Richard hypothesis.

The results also support theories emphasizing the importance of inequality in shaping the results of democratization. Wealthy citizens had a greater incentive to resist democratization of the bodies controlling spending on poor relief in areas of high inequality. The results also add empirical support for elite de facto power dampening the effects of democratic reform. Moreover, a simple model suggests that Meltzer–Richard may be particularly relevant in settings where, as in nineteenth-century Britain, the poor have relatively little political power even after democratization.

The paper also sheds light on an understudied period in the history of the English Poor Law. The 1894 democratization of poor law boards marked a turning point in both the level

and composition of expenditure, including a shift towards greater provision of relief outside the workhouse and, perhaps, increased support for the elderly. This finding contributes to our understanding of the nascent welfare state and the steps towards the introduction of national pensions in 1911. Further research is required to understand the broader consequences of the Local Government Act, and particularly whether it had similar effects on expenditure in urban areas. Those areas were also highly unequal, but may have exhibited different political dynamics. In particular, urban areas contained a lower middle class that was disinclined to support public spending (Lindert, 2004), but also lacked the powerful landed elites that continued to constrain expenditure in rural areas after 1894.

Finally, the paper demonstrates how decentralization can constrain the redistributive impact of national democratization. Britain's 1832 Great Reform Act expanded the national franchise, but left the local political structures governing poor relief largely undisturbed (Lizzeri and Persico, 2004). In fact, Parliament reformed these undemocratic bodies only after the 1884 Third Reform Act enfranchised poor rural households.[23] By securing control of local institutions in 1834, landed elites insulated themselves from redistributive pressures even as their national influence waned. Understanding when and how elites use decentralization to blunt the effects of democratic reform remains an important question for future research.

[23]The three national Reform Acts were each followed by reforms to local bodies in the areas where newly enfranchised voters were concentrated. The Great Reform Act enfranchised urban middle classes, and was followed by the restructuring of municipal councils (Lizzeri and Persico, 2004). The 1867 Second Reform Act extended voting rights to the urban lower middle classes and was followed by further changes to municipal governance. The 1884 Third Reform Act enfranchised agricultural laborers, and was followed by the creation of elected county councils in 1888 and the reforms discussed in this paper.

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# Online Appendix—Not Intended for Publication

# Table of Contents

# A Model

This appendix introduces a simple model in the spirit of Meltzer and Richard (1981), which forms the basis of the canonical models of democratization of Acemoglu and Robinson (2006) and Boix (2003).

## A.1 Framework

The government chooses a relief payment $r$ to maximize a weighted social welfare function. There is a unit mass of citizens, consisting of two classes: poor ($P$) and rich ($R$). Let the share of the population that is poor be $\lambda$, and denote by $\pi \in [0,1]$ the probability that a poor individual is employed. Assume $\pi\lambda > 0.5$, so that the employed poor are a majority of the population—the income of the employed poor is thus the median household (pre-tax and benefit) income.

Rich individuals earn a pre-tax income $y_R$, are always employed, and never receive relief. Poor citizens earn a pre-tax income $y_P$ if employed, and zero otherwise. Incomes are such that $y_R > y_P > 0$. Poor citizens receive the full amount of relief $r$ if unemployed, and partial relief $\psi r$ if employed. The parameter $\psi \in [0,1]$ captures the extent to which households benefit from relief spending even if employed, through mechanisms such as in-work benefits or payments to dependents. I assume that if $\pi = 1$, then $\psi = 1$, as all relief flows to the employed.

The cost of providing relief depends on the share of unemployed and employed poor individuals. A composite parameter $\tilde{\lambda}$ captures the per capita burden of relief spending, defined as: $\tilde{\lambda} = \lambda\big((1-\pi) + \pi\psi\big)$.

Aggregate income in the economy, denoted $\bar{y}$, is given by:

$$\bar{y} = \lambda\pi y_P + (1-\lambda)y_R.$$

This aggregate income constitutes the total tax base. The mean-to-median income ratio, which serves as a measure of income inequality, is denoted by $\theta = \bar{y}/y_P$, and is assumed to be greater than 1.

Each group derives utility from consumption. All individuals have strictly increasing and concave utility functions $u(\cdot)$. In addition, I assume $u'(0) \to \infty$, ensuring that the poor always desire some relief. I also assume, following Moene and Wallerstein (2001), that the coefficient of relative risk aversion $\gamma(c) = -\frac{cu''(c)}{u'(c)} > 1$.

The government finances relief entirely through a proportional income tax at rate $t$. There are deadweight costs of taxation, $D(t)$. $D(\cdot)$ is strictly increasing and strictly convex, with $D(0) = D'(0) = 0, D'(1) = 1$. This implies that there is no administrative burden when $t = 0$ (and hence $r = 0$), and very high costs when $t$ approaches a 100% tax rate.

The balanced budget constraint requires:

$$(t - D(t))\bar{y} = \tilde{\lambda} r$$

which implies that per capita relief $r$ and per capita government expenditure $g$ are given by:

$$r = \frac{(t - D(t))\bar{y}}{\tilde{\lambda}}$$
$$g = (t - D(t))\bar{y}$$

The consumption of the employed poor, $c_P^e$, unemployed poor, $c_P^u$, and the rich, $c_R$ after taxes and transfers, can thus be expressed as:

$$c_P^e = y_P(1 - t) + \psi\, r$$
$$c_P^u = r$$
$$c_R = y_R(1 - t)$$

The utility of the rich is simply $U_R = u(c_R)$. The utility of the poor, $U_P$, includes three components: utility from consumption when employed, utility when unemployed, and any instrumental value from higher taxation. Specifically,

$$U_P = \pi\, u(c_P^e) + (1 - \pi)\, u(r) + \kappa\, t,$$

where $\kappa \geq 0$ captures any intrinsic value that poor citizens place on higher taxation, such as moral concerns about inequality or class conflict motives.

The social planner chooses the relief level $r$ to maximize a social welfare function:

$$W = \phi\, U_P + (1 - \phi)\, U_R,$$

where $\phi \in [0, 1]$ reflects the welfare weight placed on the poor. If $\phi = 0$, the problem

is equivalent to a median voter model where only the rich can vote. Similarly, $\phi = 1$ is equivalent to a median voter model with universal suffrage.

The first proposition addresses the relationship between income inequality and the implemented level of government expenditure per capita.

**Proposition 1.** *Consider a mean-preserving spread in the income distribution. Then:*

1. *(Political weight on the poor is zero): If $\phi = 0$, then $g^* = 0$ is unchanged as inequality changes.*
2. *(Redistribution / Meltzer–Richard): If $\phi > 0$, $\pi = 1$, $\psi = 1$, and $\kappa = 0$, then $g^*$ increases.*
3. *(Insurance): If $\phi = 1$, $\pi < 1$, $\psi = 0$, then $g^*$ decreases.*
4. *(Political weight of the poor): If $\phi > 0$, then:*
   (a) *(Redistribution dominates) If the consumption of the employed poor weakly increases with a marginal increase in taxation ($\frac{\partial c_P^e(t^*)}{\partial t} \geq 0$), then $g^*$ increases. This holds if and only if $\psi \geq \frac{(1-\pi)\lambda}{(\theta(1-D'(t^*))-\lambda\pi)}$ and $(\theta(1 - D'(t^*)) - \lambda\pi) > 0$*
   (b) *There exists $\bar{\phi} \in (0, 1]$ such that if $\phi < \bar{\phi}$ then $g^*$ increases. $\bar{\phi} > 0$*
   (c) *For any $\phi > \bar{\phi}$ then $g^*$ increases, provided that inequality ($\theta$) and the share of relief received by employed poor citizens (determined by $\pi$ and $\psi$) are sufficiently large relative to the coefficient of relative risk aversion. If relative risk aversion is sufficiently high, $g^*$ may decrease.*

The first part of Proposition 1 reflects the fact that when $\phi = 0$ the planner implements the level of government spending preferred by the rich—$g^* = 0$—regardless of the level of inequality. The second part captures the logic of Meltzer and Richard (1981): the poor have full political power, as in a median-voter model, and relief functions purely as a redistributive transfer—$\pi = 1$ means that there is no insurance component to government spending. This set-up also corresponds to Proposition 1(ii) in Moene and Wallerstein (2001), where benefits are targeted purely to the employed. In this case, higher inequality increases the demand for redistribution and hence government spending.

The third part of the proposition shows that the relationship between inequality and government spending depends on the purpose of relief. When relief is primarily redistributive (inequality, $\psi$ and $\pi$ are high) then higher inequality increases equilibrium expenditure—the classic Meltzer–Richard mechanism. However, this may not hold if relief also provides

insurance against unemployment ($\pi < 1$). Specifically, the mean-preserving spread in income reduces the income of poor voters ($y_P$) and hence their demand for insurance. If risk aversion is sufficiently high then this insurance effect may outweigh the redistributive effect, and higher inequality may lead to lower government spending. This result mirrors Proposition 1(i) in Moene and Wallerstein (2001).

The fourth part of the proposition considers the case in which the poor have some, but not complete, political power. When $\phi$ is small, the implemented level government spending is significantly below the level desired by an employed poor citizen. Consequently, the marginal value of redistribution is relatively high, meaning that the redistributive motive dominates the insurance motive. As such, inequality will increase spending.

The second proposition derives implications for the effects of democratic reform, as studied in the main text.

**Proposition 2.** *Suppose an economy initially places full political weight on the rich ($\phi = 0$). A democratization is any increase of $\phi$ from 0 to some $\phi_1 \in (0, 1]$. Define $\Delta g^*(\phi_1, \theta) \equiv g^*(\phi_1, \theta) - g^*(0, \theta) = g^*(\phi_1, \theta)$. Then*

1. *Democratization leads to higher government spending, $\Delta g^*(\phi_1, \theta) > 0$.*
2. *The effect of democratization on spending is increasing in inequality if any of the following conditions are met:*
   (a) *The employed poor are marginal beneficiaries from relief at the post-reform optimum: $\frac{\partial c_P^e(t^*(\phi_1,\theta))}{\partial t} \geq 0$, where $t^*(\phi_1, \theta)$ is the implemented tax rate at $\phi_1$. This condition is met if $\pi = 1$ and $\psi = 1$.*
   (b) *The political weight of the poor ($\phi$) remains low even after democratization: $\phi_1 \leq \bar{\phi}$ for some $\bar{\phi} > 0$.*
   (c) *$\phi_1 \geq \bar{\phi}$ and the insurance motivation for government spending is sufficiently weak at the post-reform optimum $t^* = t^*(\phi_1, \theta)$. This is more likely to be met if relative risk aversion, $\gamma(c_P^e(t^*))$, is small relative to $\psi$.*
3. *The post-democratization change in $g^*$ is increasing in the instrumental value of taxation ($\kappa$): $\frac{\partial \Delta g^*}{\partial \kappa} > 0$*
4. *The signs of $\frac{\partial \Delta g^*}{\partial \lambda}$ and $\frac{\partial \Delta g^*}{\partial \pi}$ are ambiguous.*

The first part of this proposition captures the fact that the poor prefer higher taxes than the rich: as their political weight increases the level of spending increases. The second part

is a corollary of Proposition 1. Part 3 is a straightforward implication of the fact that a higher $\kappa$ increases the demand for taxation, and hence spending. Part 4 reflects the fact that more need has two effects: on one hand, it mechanically increases the demand for relief. On the other hand, it reduces the marginal value of relief to each individual citizen.

This model can be extended to account for the fact that, in nineteenth-century England, taxation was proportional to expenditure on housing rather than income. Suppose housing spend is given by $h_P = \alpha_P y_P$ and $h_R = \alpha_R y_R$. $\alpha_P, \alpha_R > 0$. The unemployed do not spend money on housing and hence, as before, do not pay tax. Then we have the following corollary:

**Corollary 1.** *Define $\hat{\theta} = \frac{\bar{h}}{\alpha_P y_P}$: the ratio of the mean to the median housing expenditure. Then the change in $g^*$ induced by democratization is increasing in $\hat{\theta}$ under the same conditions as in Proposition 2, with $\hat{\theta}$ in place of $\theta$.*

## A.2 Proofs

**Proof of Proposition 1.** The proof proceeds by identifying the planner's optimal tax rate $t^*$. This is sufficient to identify effects on $g^*$ because $\frac{\partial g}{\partial t} = \bar{y}(1 - D'(t)) > 0$ for $t \in [0, 1)$, as $D(t)$ is increasing and $D'(t) \leq 1$. To do so, substitute the expression for $r$ in terms of $t$ into the consumption of the poor and the rich, and define the planner's objective function as $F(t) = \frac{\partial W}{\partial t}$. The first-order condition is then:

$$F(t) = \phi \left[ \pi \, u'\left(c_P^e\right) \, \frac{\partial c_P^e}{\partial t} + (1 - \pi) \, u'(c_P^u) \, \frac{\partial c_P^u}{\partial t} + \kappa \right] + (1 - \phi) \, u'\left(c_R\right) \, \frac{\partial c_R}{\partial t} = 0 \tag{3}$$

The marginal changes in consumption with respect to the tax rate are:

$$\begin{aligned} \frac{\partial c_P^e}{\partial t} &= -y_P + \psi \frac{(1 - D'(t))\bar{y}}{\tilde{\lambda}} \\ \frac{\partial c_P^u}{\partial t} &= \frac{(1 - D'(t))\bar{y}}{\tilde{\lambda}} \\ \frac{\partial c_R}{\partial t} &= -y_R < 0. \end{aligned}$$

Taking the second order conditions, and using $u'' < 0$, $D'' > 0$ verifies that this is a unique maximum. There is an interior solution for $\phi > 0$: this is guaranteed for $\pi < 1$ by the fact that $u'(0) \to \infty$ and $D'(1) = 1$. For $\pi = 1$, $\left.\frac{\partial c_P^e}{\partial t}\right|_{t=0} > 0$. This is true if $-y_P + \psi \frac{(1 - D'(t))\bar{y}}{\tilde{\lambda}} > 0 \implies \theta > \lambda$, which is guaranteed by the fact $\theta > 1$. For $\phi = 0, F(t) = u'\left(c_R\right) \cdot (-y_R) < 0$, so the planner strictly prefers $t^* = 0$.

Next, we analyze the comparative statics on $t^*$ with respect to the parameter $\phi$, which captures the effective political weight of the poor. The second-order conditions imply that $\frac{\partial F}{\partial t} < 0$ at $t^*$. By the implicit function theorem, this implies that $\frac{dt^*}{d\phi} > 0 \iff \frac{\partial F}{\partial \phi} > 0$.

Writing $F(t) = \phi U'_P + (1-\phi)U'_R$, we have:

$$\frac{\partial F}{\partial \phi} = U'_P - U'_R$$

so $\frac{dt^*}{d\phi} > 0 \iff U'_P(t^*) > U'_R(t^*)$. This inequality holds under our assumptions for any $t^* \in [0, 1)$. To see this, first note that at an interior solution the optimal tax rate $t^*$ satisfies $\phi U'_P(t^*) = -(1-\phi)U'_R(t^*)$. Second, $U'_R(t) < 0$ for any $t$, so $U'_P(t^*) > 0$ if $\phi < 1$, and $U'_P(t^*) = 0$ if $\phi = 1$. At $t = 0$ $U'_P(0) \to \infty$ because the unemployed poor have zero consumption and $u'(0) \to \infty$. So $U'_P(t^*) \geq 0 > U'_R$ for any $t^* < 1$. This ensures $\frac{\partial t^*}{\partial \phi} > 0$

We now apply the same approach to analyze comparative statics with respect to inequality, $\theta = \frac{\bar{y}}{y_P}$. From above, if $\phi = 0$, then $t^* = 0$, and $\frac{dt^*}{d\theta} = 0$. Consequently, we consider the case $\phi > 0$. Applying the first order conditions $\mathrm{F}(t^*) = 0$, and using the implicit function theorem: $\frac{dt^*}{d\theta} = -\frac{\frac{\partial F}{\partial \theta}}{\frac{\partial F}{\partial t}}$. Since $\frac{\partial F}{\partial t} < 0$, it follows that the sign of $\frac{dt^*}{d\theta}$ is determined by the sign of $\frac{\partial F}{\partial \theta}$.

To analyze the effect of a mean-preserving increase in income inequality, we treat $\theta = \frac{\bar{y}}{y_P}$, and express consumption levels in terms of $\theta$, keeping $\bar{y}$ constant. From this point forward, expressions $c_P^e$, $c_P^u$, and $c_R$ are evaluated at the optimum $t^* = t^*(\theta)$, and asterisks are omitted for notational simplicity. Taking the derivative of $F(t, \theta)$ with respect to $\theta$, and substituting the coefficient of relative risk aversion, $\gamma(c)$ for $u''$ gives:

$$\frac{\partial F}{\partial \theta} = \phi\pi \frac{\bar{y}}{\theta^2} u'(c_P^e) \left[ \left( \frac{\partial c_P^e}{\partial t}(1-t)\frac{\gamma(c_P^e)}{c_P^e} + 1 \right) \right] + (1-\phi) \left[ u'(c_R) \frac{\partial^2 c_R}{\partial t \partial \theta} \left(1 - \gamma(c_R)\right) \right]. \quad (4)$$

By assumption $\gamma > 1$ and a simple calculation shows that $\frac{\partial^2 c_R}{\partial t \partial \theta} < 0$, so the part of (4) relating to the rich is strictly positive. The sign of the part of (4) relating to the poor is determined by the sign of $\frac{\partial c_P^e}{\partial t}$, which is ambiguous. We can then work through the different cases in the proposition.

*Part 2: Meltzer–Richard case:* Assume $\phi > 0$, $\pi = 1$, $\psi = 1$, $\kappa = 0$. In this case the FOC (3) implies $u'(c_P^e)\, \partial c_P^e / \partial t > 0$, and so $\frac{\partial c_P^e}{\partial t} > 0$. Thus $dt^*/d\theta > 0$.

*Part 3: Insurance case:* If $\phi = 1$, then the rich term in (4) disappears, and the sign of $\frac{\partial F}{\partial \theta}$ is determined by the poor term. If $\psi = 0$, then $\partial c_P^e / \partial t = -y_P$, and the part of the poor term in parentheses becomes $-\gamma(c_P^e) + 1 < 0$, as $\gamma(c_P^e) > 1$ by assumption.

*Part 4(a):* $\frac{\partial F}{\partial \theta} > 0$ if $\frac{\partial c_P^e}{\partial t} \geq 0$. The threshold in the proposition is then given by rearranging this partial derivative.

*Part 4(b):* First note that $t^*(\phi) \to 0$ as $\phi \downarrow 0$. To see this, suppose by contradiction that $t^*(\phi) \geq \epsilon$ for fixed $\epsilon \in (0,1)$ for a sequence $\phi \downarrow 0$, with $\epsilon > D(\epsilon)$. At $t = \epsilon$, $r_\epsilon = r(\epsilon) > 0$. Then $c_P^u > 0$ and $c_P^e > 0$. Evaluating the First Order Conditions at $\epsilon$ gives $F(\epsilon) = \phi U_P'(\epsilon) + (1-\phi)U_R'(\epsilon)$, which is strictly negative for small enough $\phi$ because $U_R'(\epsilon) < 0$ and $\phi U_P'(\epsilon)$ can be made arbitrarily small. Since $\partial F/\partial t < 0$, for such $\phi$, the FOC will be satisfied for $t^* < \epsilon$—a contradiction. Next, decompose Equation (4) into the components relating to the poor and the rich:

$$\frac{\partial F}{\partial \theta} = \phi \mathcal{P}(\phi) + (1-\phi)\mathcal{R}(\phi) \tag{5}$$

$$\tag{6}$$

where $\mathcal{P}(\phi) = \pi \frac{\bar{y}}{\theta^2} u'(c_P^e) \left[ \left( \frac{\partial c_P^e}{\partial t}(1-t)\frac{\gamma(c_P^e)}{c_P^e} + 1 \right) \right]$ and $\mathcal{R}(\phi) = \left[ u'(c_R)\frac{\partial^2 c_R}{\partial t \partial \theta}\left(1 - \gamma(c_R)\right) \right]$. As $t^* \to 0$, $c_P^e \to y_P > 0$, and so $\mathcal{P}(\phi)$ is bounded for $t$ close to zero. Further, $R(\phi) \to \mathcal{R}(0) = u'(y_R)\left[ -\left(\frac{\bar{y}}{1-\lambda}\right)\left(\frac{\pi\lambda}{\theta^2}\right) \right](1-\gamma(y_R)) > 0$. As such, we can choose $\bar{\phi}$ such that $\phi\mathcal{P}(\phi)$ is arbitrarily close to zero, and $(1-\phi)\mathcal{R}(\phi) > 0$.

*Part 4(c):* Rearranging the part of Equation (4) relating to the poor, and substituting $(c_P^e)$ and $\frac{\partial c_P^e}{\partial t}$, and rearranging gives the condition:

$$\frac{\partial c_P^e}{\partial t}(1-t)\frac{\gamma(c_P^e)}{c_P^e} + 1 \geq 0$$

$$\gamma(c_P^e) \leq \frac{y_P(1-t) + \psi\frac{(t-D(t))\bar{y}}{\tilde{\lambda}}}{\left(y_P - \psi\frac{(1-D'(t))\bar{y}}{\tilde{\lambda}}\right)(1-t)}$$

Note that the right-hand of this inequality is increasing in $\psi$, and the inequality is never satisfied for $\psi = 0$ as $\gamma(c_P^e) > 1$.

**Proof of Proposition 2.** As above, $F(t;\cdot) = \partial W/\partial t$. At any interior optimum, $\partial F/\partial t < 0$ (strict concavity), and $\partial g/\partial t = \bar{y}(1 - D'(t)) > 0$.

*Part 1.* From Proposition 1, $\partial F/\partial \phi = U_P' - U_R' > 0$ at any interior $t$. By the implicit function theorem, $\frac{dt^*}{d\phi} = -\frac{\partial F/\partial \phi}{\partial F/\partial t} > 0$, hence $g^*(\phi)$ is strictly increasing in $\phi$. Since $g^*(0) = 0$, any democratization ($\phi_1 > 0$) raises $g^*$.

*Part 2.* This follows directly from Proposition 1. $g^*(0,\theta) = 0$, so $\Delta g^*(\phi_1,\theta) = g^*(\phi_1,\theta)$. Applying the conditions outlined in Proposition 1 gives the proof.

*Part 3.* $\kappa$ enters linearly in $F$ via the poor's taste for taxes: $\partial F/\partial\kappa = \phi > 0$. With $\partial F/\partial t < 0$, the implicit function theorem gives $dt^*/d\kappa > 0$, hence $\partial\Delta g^*/\partial\kappa > 0$.

*Part 4.* Consider separately a change in unemployment risk $\pi$ and a change in the poor's population share $\lambda$. For convenience, write $\tilde{\lambda} \equiv \lambda\big[(1-\pi) + \pi\psi\big]$.

We can decompose the effect of changes in these variables into an effect on aggregate income (and hence the tax base) and on the optimum tax rate. That is, for $x \in \{\pi, \lambda\}$, the total derivative at the interior optimum $t = t^*(x)$ is

$$\frac{dg^*}{dx} = \bar{y}\left(1 - D'(t^*)\right)\frac{dt^*}{dx} \;+\; \left(t^* - D(t^*)\right)\frac{\partial\bar{y}}{\partial x}. \tag{7}$$

where $1 - D'(t^*) > 0$ because $t^* < 1$ and $\left(t^* - D(t^*)\right) > 0$ because $r^* > 0$ for $\phi > 0$.

The second term in this equation identifies the mechanical effect of changing the tax base available to fund poor relief. In particular, holding $t = t^*$ fixed, we have:

$$\frac{\partial\bar{y}}{\partial\pi} = \lambda y_P > 0, \qquad \frac{\partial\bar{y}}{\partial\lambda} = \pi y_P - y_R < 0,$$

so that an increase in the share of the employed leads to upward pressure on government spending, and a greater share of the poor leads to downward pressure. To identify the effect on the optimal tax rate, we again apply the implicit function theorem. The sign of the resulting expressions depends on components of varying, and sometimes ambiguous, signs. As a result the signs of $\frac{dg^*}{d\pi}$ and $\frac{dg^*}{d\lambda}$ are, in general, ambiguous.

□

**Proof of Corollary** Average housing spending is $\bar{h} = (\pi\lambda\alpha_P y_P + (1-\lambda)\alpha_R y_R)$. The budget constraint is $\tilde{\lambda}r = (t - D(t))\bar{h}$. Define $\hat{\theta} = \frac{\bar{h}}{\alpha_P y_P}$: the ratio of the mean to the median housing expenditure. Then we can write:

$$\begin{aligned}
c_P^e &= y_P(1-\alpha_P t) + \psi\frac{(t-D(t))\bar{h}}{\tilde{\lambda}} \\
&= \frac{\bar{h}}{\alpha_P\hat{\theta}}(1-\alpha_P t) + \psi\frac{(t-D(t))\bar{h}}{\tilde{\lambda}} \\
c_R &= \left(\frac{\bar{h}}{(1-\lambda)\alpha_R}\right)\left(1 - \frac{\pi\lambda}{\hat{\theta}}\right)(1-\alpha_R t)
\end{aligned}$$

It follows that

$$\frac{\partial c_P^e}{\partial t} = -\frac{\bar{h}}{\hat{\theta}} + \psi \frac{(1 - D'(t))\,\bar{h}}{\tilde{\lambda}}, \quad \frac{\partial c_P^u}{\partial t} = \frac{(1 - D'(t))\,\bar{h}}{\tilde{\lambda}}, \quad \frac{\partial c_R}{\partial t} = -\frac{\bar{h}}{1-\lambda}\Big(1 - \frac{\pi\lambda}{\hat{\theta}}\Big),$$

Consequently, the expressions in Proposition 1 and Proposition 2 carry over from the main model set-up, replacing $\bar{y}$ with $\bar{h}$ and $\theta$ with $\hat{\theta}$. □

# B Data

This appendix includes additional information regarding the data introduced in Section 4. The first subsection details the main data sources. The second gives a brief definition of the variables used in the main text. The third subsection presents descriptive statistics, including correlations between the inequality measures.

## B.1 Main Data Sources

This subsection presents additional information regarding the construction of the variables used in the main analysis.

**Boundaries of Poor Law Unions** There were approximately 630 Poor Law Unions in England and Wales throughout the nineteenth century. Around 250 of these unions were still defined as rural in 1909, and so fall within the scope of this paper. To avoid possible complications with boundary changes I exclude any unions that were established or abolished between 1860 and 1905, and those where the cumulative change in boundaries over the same period exceeded 15% of the population in 1881—boundary changes were identified using information from www.ukbmd.org.uk. The boundaries of poor law unions were relatively stable across the second half of the nineteenth century and so only twenty-two unions were dropped due to this restriction. A further seventeen unions do not appear in the main regressions due to a missing inequality measure, reflecting either missing wage data or for two unions due to missing tax base data in 1869. The main regression sample thus consists of 208 unions.

**GIS Data:** I estimate a number of union characteristics using GIS software. To do so, I use the boundaries of the poor law unions in 1881, estimated using GIS software based on registration district maps obtained from Southall et al. (2023). Suitability of soil for cereal agriculture is estimated using data from the Global AgroEcological Zones (GAEZ) project of the Food and Agriculture Organization (FAO). Information is provided for 5 arc-minute

grid cells. Average terrain ruggedness is estimated using the data provided by Nunn and Puga (2012).

**Poor Law Policy** Annual data regarding poor law spending and the number of paupers relieved was collected from papers included in the House of Commons *Parliamentary Papers* collection. Reports summarizing the financial accounts of each union were produced yearly as part of the *Local Taxation Returns*. The number of paupers in each union was reported biannually at the start of January and July. The financial year end for the spending reports is the end of March; as such, for each year I construct the annual number of paupers by averaging the figure for January of the present year and July of the previous year. MacKinnon (1988) argues that the January and July figures are in fact good approximations of the respective six-month averages.

The dependent variable in the regression analysis includes all expenditure related to poor relief. The sub-categories included in this measure are spending on paupers, both inside the workhouse ("in-maintenance") and outside ("out-relief"); salaries; loan repayment and interest; payments for buildings and repairs; and other. Expenditure defrayed out of loans is not directly included, but the amortized cost of this expenditure is captured through spending on loan charges.

Poor relief expenditure formed by far the largest component of the unions' direct spending; other categories included costs associated with vaccination, parliamentary registration, and other administrative activities. In addition, the guardians were responsible for collecting taxes on behalf of other bodies, such as municipal boroughs.

The nominal figures for spending and also tax base are adjusted into real terms using the Bowley Cost of Living index (Mitchell, 1971, p.738).

**Sociodemographic Characteristics** I use decennial census data at registration-district level to construct a number of union-level demographic variables. The boundaries of the poor law unions were similar to those of registration districts meaning that it is straightforward to link the poor law and census data reported in several different sources.

*Population:* Registration district population disaggregated by age was obtained from Southall et al. (1998). Annual variables were constructed by geometric interpolation between census years.

*Houses:* The parish-level number of houses are taken from Southall et al. (2004). I aggregate this information to the level of poor law union.

*Occupational structure:* Data on the percentage of agricultural laborers is taken from Reid et al. (2018). The total population working in agriculture was constructed using the

100% census sample, available from I-PUMS.

**Annual Agricultural Returns:** Annual county-level information on agricultural land devoted to different crops between 1885 and 1905 taken from reports of the Board of Agriculture and Fisheries in the Parliamentary Papers collection.

## B.2 Definition of Main Variables

*Income Inequality:* Estimated mean–median ratio of household spending on property. See Appendix B.4 below.

*Cereal Suitability*: Estimates are for rain-fed, low-input agriculture.

*% Male (female) population within age group*: The number of men (women) within each group as a share of the union population.

*Land Inequality*: The estimated share of acres in a union owned by a "great landowner" as identified by Bateman (1883). See Appendix B.5 below.

*% Agricultural Laborers*: (Male agricultural laborers aged 15-64 as % of all men aged 15-64 years (from Reid et al., 2018) / share of men in agriculture (calculated using 1881 census sample (Schürer and Higgs, 2014))).

*Strength of Crusade Against Outrelief*: The % fall in the number of outdoor paupers between 1866–1870 and 1881–1885.

*Aristocratic Elite*: A binary variable equalling one if a peer of the realm had a country seat in the union. See Appendix B.5 below.

*Local Political Control*: A binary variable equaling one if the chairman of the board of guardians in 1893 was unelected.

*Distance from London*: Straight-line distance from centroid of each union to the center of London.

*Distance from Major Cities*: Straight-line distance from centroid of each union to the center of four cities with 1901 population over 500,000—London, Birmingham, Manchester, and Liverpool.

*% in Wheat (and other crops)*: Percentage of agricultural land within a county devoted to each crop.

*% Revenue from Poor Rate*: Percentage of all revenue obtained from the poor rate.

*Decadal Variance in Pauperism*: Standard deviation of total paupers per capita over the decade.

*Tax Base Per Acre*: The total rateable value of the union, divided by area, excluding any land defined as having "medium" or "high" terrain ruggedness.

*Population Density*: Total population divided by area in 1881, excluding any land defined as having "medium" or "high" terrain ruggedness.
*Terrain Ruggedness*: Average terrain ruggedness using the data provided by Nunn and Puga (2012).
*Relief Expenditure per Capita* : All expenditure related to poor relief divided by total union population.
*Spending on Pauper Maintenance per Capita* : Expenditure on out-relief or in-maintenance, divided by total union population.
*Spending on Other Relief per capita*: All expenditure on relief except out-relief or in-maintenance, divided by total union population. Includes spending on salaries, loan repayments, and other spending.

## B.3 Descriptive Statistics

Table B.1 displays the descriptive statistics for the variables included in the main regressions.

Table B.1: Descriptive Statistics

| | Obs | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| Total Relief Spend per Capita (£) | 4368 | .41 | .1 | .16 | .75 |
| | | | | | |
| Inequality Measures: | | | | | |
| Income Inequality | 208 | 10.39 | 2.64 | 3.26 | 21.89 |
| Local Elite Control | 208 | .46 | .5 | 0 | 1 |
| Aristocratic Elite | 208 | .48 | .5 | 0 | 1 |
| Land Inequality | 208 | 38.35 | 25.06 | 0 | 100 |
| % Large Farms | 208 | 34.5 | 21.8 | 0 | 82.5 |
| Outrelief Cuts 1866–85 | 208 | 38.19 | 18.33 | -36.62 | 81.33 |
| Cereal Suitability | 208 | 3508.29 | 1248.66 | 0 | 7090.25 |
| % Agricultural Laborers 1891 | 208 | 59.19 | 14.43 | 13.63 | 85.71 |
| % Age over 64 1891 | 208 | 7.63 | 1.03 | 4.3 | 10.34 |
| | | | | | |
| Control Variables: | | | | | |
| Population ('000s) | 4368 | 14.86 | 7.74 | 2.47 | 69.07 |
| % of Population Men >64 years | 4368 | 3.61 | .51 | 1.93 | 4.87 |
| % of Population Women >64 years | 4368 | 3.99 | .65 | 1.98 | 6.66 |
| Log Tax Base Per Acre | 4368 | .31 | .46 | -1.21 | 1.62 |
| % Revenue from Poor Rate | 4368 | 84.33 | 7.17 | 59.28 | 97.92 |
| 100*Std Dev of Paupers p.c. | 4368 | .26 | .14 | .04 | .9 |

Note: Financial variables are in 1914 £. See Section 4 for discussion of data sources and variable construction.

Table B.2: Growth in Spending by Census Division.

| Census Division | Number of Unions | Annual Growth in Relief Expenditure per capita | | |
|---|---|---|---|---|
| | | Pre-Reform (1885-1893) | Post-Reform (1895-1905) | Total (1885-1905) |
| South Eastern | 31 | 0.1% | 1.1% | 1.0% |
| South Western | 35 | 0.3% | 0.5% | 1.1% |
| South Midlands | 24 | -0.1% | 1.8% | 1.4% |
| Eastern | 20 | 0.9% | 1.4% | 1.7% |
| East Midlands | 31 | 0.7% | 1.6% | 1.8% |
| West Midlands | 29 | -0.2% | 1.7% | 1.4% |
| North Western | 1 | -1.0% | -0.0% | 0.4% |
| Northern | 16 | 0.5% | 1.1% | 1.3% |
| Yorkshire | 15 | 0.5% | 0.9% | 1.2% |
| Wales | 23 | -0.0% | 0.1% | 0.5% |
| England and Wales | 225 | 0.3% | 1.1% | 1.3% |

Notes: Annual growth is calculated as the total percentage change over the period divided by the number of years.

Table B.3: Correlations between inequality measures and pre-reform spending trends.

| | Income Inequality | Local Elite Control | Aristocratic Presence | Land Inequality | % Large Farms | Outrelief Cuts 1866–85 | Cereal Suitability | % Agri. Laborers | % Age over 64 |
|---|---|---|---|---|---|---|---|---|---|
| Local Elite Control | −0.08 | | | | | | | | |
| | (.069) | | | | | | | | |
| Aristocratic Presence | −0.00 | 0.13* | | | | | | | |
| | (.070) | (.069) | | | | | | | |
| Land Inequality | 0.10 | −0.07 | 0.31*** | | | | | | |
| | (.069) | (.069) | (.066) | | | | | | |
| % Large Farms | 0.34*** | −0.08 | 0.14** | 0.23*** | | | | | |
| | (.066) | (.069) | (.069) | (.068) | | | | | |
| Outrelief Cuts 1866–85 | 0.22*** | −0.09 | −0.04 | 0.01 | 0.21*** | | | | |
| | (.068) | (.069) | (.070) | (.070) | (.068) | | | | |
| Cereal Suitability | 0.24*** | −0.03 | 0.14** | 0.22*** | 0.49*** | 0.19*** | | | |
| | (.068) | (.070) | (.069) | (.068) | (.061) | (.068) | | | |
| % Agricultural Laborers | 0.32*** | 0.06 | 0.14** | 0.23*** | 0.74*** | 0.12* | 0.60*** | | |
| | (.066) | (.070) | (.069) | (.068) | (.047) | (.069) | (.056) | | |
| % Age over 64 | 0.09 | −0.06 | −0.11 | −0.05 | 0.10 | 0.37*** | 0.16** | 0.21*** | |
| | (.069) | (.070) | (.069) | (.070) | (.069) | (.065) | (.069) | (.068) | |
| Δ Relief Spend. p.c. 1885–93 | −0.03 | −0.06 | −0.01 | −0.01 | −0.04 | 0.01 | −0.03 | −0.04 | 0.06 |
| | (.070) | (.070) | (.070) | (.070) | (.070) | (.070) | (.070) | (.070) | (.070) |

*Notes:* The table presents univariate correlations between measures. Δ Relief Spending p.c. 1885–1893" is the percentage change in per capita spending on poor relief between 1885 and 1893. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels.

## B.4 Income Inequality

I estimate the mean:median ratio of expenditure on housing as:

$$Mean : Median\ Ratio = \frac{Mean\ rental\ value}{Median\ wage} \tag{8}$$

The gross rental value for each district is available from the *Local Taxation Returns*—this is divided by the number of houses (given in the 1871 census) to estimate the average per household. The way in which rental value was assessed means that this source provides 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—rather than the gross rental value used for the construction of the measure used here.

The median wage is estimated using county-level data on the 1869 agricultural wage from Collins and Thirsk (2000, Table 42.3). To check that the agricultural wage can capture the income, and hence the household spending, of a representative voter, I use the 1881 100% census sample to estimate the median head of household's occupation according to the hiscam occupational score metric (Lambert et al., 2013). In 89% of cases the occupation is involved in farming, and in the remaining 11% the score is at a similar level.

The median wage proxies for the median spending on property if we assume that voters spend a fixed proportion of their income on housing across unions. While it is not possible to measure this in practice, some supporting evidence is provided by Horrell (1996), who investigates household budgets and finds that households in low-wage agriculture 1840–1854 spent 10.4% of their income on housing, while those in high-wage agriculture spent 9.9%. This data suggests that the income elasticity of housing was quite low at these income levels and so regional variation in wages would not be associated with major variations in the share of income spent on housing.

A final question is whether the mean:median ratio of spending on housing can be considered a measure of income inequality. This question is not important for the operationalization of the variable, since the empirical measure directly matches the theoretical prediction in this

context—where taxation fell on housing spending. However, it affects the potential interpretation of the finding If we accept that spending by the poor was a constant share of income, as discussed in the previous paragraph, then the key assumption is whether higher incomes of wealthy (and hence higher average income) translate into a higher mean spending on property. This assumption will hold if property spending is a "normal good"—i.e., household expenditure increases with income. That is, the total amount spent on property increases even if the share of income spent by the wealthy is lower than the poor. This assumption seems reasonable, and is supported by contemporary evidence to the 1852 Select Committee on Income Taxation: (Qs 5383) which stated "Supposing house rent...to be on the average £150 and that no person could occupy any house there with an income of less than £1,500 paying therefore one-tenth of the income in house rent, are you not aware that persons occupying houses in the surrounding districts pay £20 or £25 a year rent for those houses, which is equal to paying one-seventh or one-eighth of their incomes..." (emphasis added). This quote demonstrates that the share of income spent on housing fell only slightly between an income of around £150 and ten times that amount. Further, to the extent that occupied property also included agricultural land, occupation of more land would translate directly into higher income. For these reasons, it is reasonable to think that a higher mean–median ratio in housing expenditure reflects a higher mean–median ratio in income.

## B.5 Geolocating Elites

Local elites were identified using two nineteenth-century sources: Bateman (1883) and Walford (1886).

**Construction of Dataset**

Bateman (1883) is well-known among economic historians and lists the size and value of landholdings of the largest landowners in Great Britain and Ireland (excluding London). This information was drawn from an 1873 census of landownership, with many revisions and corrections. The first primary category of landholders included individuals with aggregate possessions consisting of more than 3,000 acres and more than £3,000 per annum gross rental value—in this case, landholdings were disaggregated by county. In addition, Bateman reported landholders with more than 2,000 acres worth more than £2,000 per annum gross rental value, but in this case the amounts were not disaggregated by county.

While Bateman (1883) provides a valuable source of these "great" landowners, for the purposes of this paper, it suffers from two deficiencies. First, it may miss important local landholders who do not fit the criterion of "great" identified by Bateman. Second, it limits

the potential to geolocate their land because at most one address is listed for each landholder. Frequently landholders owned large properties in multiple counties—the Bateman's information would only allow us to locate one of these holdings at the poor law union level.

To overcome these issues, I combine the information from Bateman with information from Walford (1886). This source identifies a list of owners of the "principal seats" in England and Wales, and in many cases, identifies multiple properties for each individual. As such, I can identify a much larger number of landowners and allocate a much higher proportion of the estates reported by Bateman to individual poor law unions.

The database was then constructed as follows:

1. Identify all individuals either listed in Bateman (1883) or included in the list of Principal Seats in Walford (1886).

2. Identify relevant characteristics of these individuals, particularly their titles and whether Bateman mentions them as part of a family that held land in England back to the time of Henry VIII.

3. For each individual, identify all properties mentioned in either source.

4. Geolocate these properties and assign each property to the relevant poor law union. Two major sources were used for the geolocation: www.britishlistedbuildings.co.uk and the gazette provided by Vision of Britain. Where matching was unclear, additional internet research was done to ensure the correct property had been identified. In some (relatively few) cases, it was not possible to identify the precise location of the property but only that of a nearby landmark or relevant parish.

5. Where information is available, assign the land owned by an individual within each county to the property (or properties) in that county. For the group of smaller landholders in Bateman (those with over 2,000 but under 3,000 acres or £3,000 rental value), counties are grouped together—the acreage is split evenly between these counties.

Nearly all properties in both sources were geolocated and matched with poor law unions. Between the two sources, a total of 5,804 properties were identified in England and Wales, of which 88% were successfully geolocated and 52% (3,020) were matched to the landholdings in Bateman. Those unmatched reflect properties owned by families not qualifying for inclusion in Bateman's list—in other words, they were relatively small landowners.[1]

[1]There were only 27 landholders in Bateman for whom we were unable to identify any property.

One aspect that is potentially more problematic is the fact that there are several large estates which could not be matched to any property. A total of 5,049 owner-county pairs were identified in Bateman, of which 2,669 (53%) were successfully matched to properties (note that this matched total is lower due to owners having multiple properties within the same county). These unmatched estates were small compared to those that were matched—median size 783 versus 3,235 acres—but still included some very large properties, with the largest such property being almost 35,000 acres.

It does not appear, however, that the lack of matching biases the inequality estimates. Properties were unmatched as a result of landowners not having residences in these areas rather than an inability to locate the properties—95% of listed residences of Bateman's landowners are matched to an acreage value. Potentially, elites could still project power without having a formal residence in a specific union—potentially biasing our estimates of "low-inequality," which merely captures the lack of a residence. In practice, however, the correlation between our county-level servants inequality measure and the share of landholdings matched is close to zero ($\rho = -0.06$,p-val=0.65).[2]

### B.5.1 Measures of Elite Presence and Land Inequality

Using this procedure, I identify the total number of seats per acre and the number of seats owned by the "great landowners" that appeared in Bateman. This provides me with measures of the size of the landholding elite, and—using the proportion of these landholders that were "great"—the extent of inequality within that elite. I can also estimate the proportion of both the land and the total property value within the district owned by the great landholders—although this is less precise due to the boundary issues noted above.

Out of 225 rural poor law unions, all but three contained at least one county seat. Just under half (44%) of the unions contained a seat owned by a peer, with just over 10% containing multiple seats owned by peers.

### B.5.2 Top 5% Wealth Inequality

The top 5% wealth inequality measure in the paper is constructed using the 100% 1881 census sample (Schürer and Higgs, 2014). The idea behind this measure is to proxy household wealth using the number of live-in servants.[3] The number of servants was used by contemporary

[2]This correlation excludes Yorkshire, as the reporting of counties in the Bateman (1883) list does not break down different ridings in Yorkshire.

[3]Initially, I attempted to use the measure of servants constructed as part of the IPUMS data set, which is based on the relationship to household head and occupation. However, investigation indicated that this

scholars as a measure of class; Charles Booth, in his classic surveys of London identified the "middle class" as those with up to two servants and the "upper class" as those with more than two servants (Booth, 1903). The distribution of number of servants within the servant-keeping class can therefore proxy the distribution of wealth within the elite.

The first step in constructing the variable was to define the "houses" to be included. Many servants listed in the census worked in institutions (such as military barracks, schools, or prisons) or in boarding houses or hotels, rather than in domestic settings. I thus excluded these institutions as follows. First, institutions were identified as such if they contained individuals whose relationship to the household head was listed as "military," "student," "member of religious order," "institutional inmate," "family of inmate,", or "foundling / orphan." I also excluded households if they contained more than 10 individuals not related to the household head—of which more than 5 were boarders, students, or soldiers. Finally, I excluded hotels—identified as places where the head of household's occupation was related to service in hotels or boarding-houses and where there was at least one other hotel worker in the household.

The second stage involved identifying individual servants in the census. Servants were primarily identified using their relationship to the household head in the census. Specifically, they were categorized as servants a) if their relationship to the household head was as a servant, a housekeeper, or another probable domestic employee or b) their relationship to the household head was stated as "other employee" and their occupation code identified them as a domestic servant (e.g., a governesses or coachmen). Individuals were not classified as servants if their occupation indicated they were not live-in domestic servants. This included, for instance, "governesses (non-resident)" and day-servants, as well as a number of occupations that appear to have been misclassified as servants (e.g., blacksmiths). I then added those listed as employees whose occupation explicitly identified them as domestic servants (for example, governesses or coachmen) and removed those whose occupational code identified them as non-servants, for instance clerks, or those listed in a "household" that appeared to be an institution (such as a prison or boarding house). As a robustness test, I then construct an additional measure excluding those labeled "farm servants;" the resulting measure is correlated 0.97 with our main variable.

measure included a large number of individuals who were not the domestic servants of interest here but different types of employees, such as clerks and hospital or hotel workers.

# C Additional Historical Background

This appendix provides further historical background regarding the poor law. First, I provide additional details of the governance system used in poor law unions, including the qualifications necessary to vote and to be a guardian, as well as the electoral procedure used in poor law elections. The second subsection discusses the historical evidence relating to electoral behavior and election outcomes discussed in Section 3.3. Following this, I discuss the guardians' role in the bodies providing rural public goods and present evidence that the spending by these authorities does not confound the main findings. Finally, I draw on the dataset outlined in Section 4 to describe the type of support provided through the poor law and the trends in poor law policy over time.

## C.1 Governance of Poor Law Unions

The poor law system analyzed in this paper was established by the 1834 Poor Law Amendment Act (i.e., the "New Poor Law"). Under this Act, a central Poor Law Commission was created to supervise the work of local officials, and local administrative responsibility was passed from parishes to the newly created poor law unions. In total, approximately 630 poor law unions were created in England and Wales by grouping parishes together. Each union was governed by a locally elected council—the "Board of Guardians." Despite ongoing reforms to the poor law (some of which are discussed below), this basic structure remained in place until after World War I.

Guardians held considerable discretion over the implementation of the poor law, and differences in policy explain much of the variation in poor law support shown in Figure 2. In theory, at least, the 1834 New Poor Law established centralized control to remove local discretion over poor law generosity. However, evidence provided to the 1909 Royal Commission on the poor law demonstrates the continuing importance of guardians' attitudes:

> "a study of the relief given in different Unions shows how great a discretionary power is still exercised by local administrators, and, undoubtedly, the wideness of that power does make it possible for an elected Guardian to fulfill to some extent his election pledges. The class of applicants who have no chance of receiving outdoor relief in one Union may get it easily and as a matter of course in another. In one Union out-relief will not be granted unless the applicants have sufficient independent means to pay their rent, while in another...earning or possessing any independent resources will be considered a bar to relief. Some Guardians will not

> relieve the sick dependents of an able-bodied man at all unless the man himself comes into the workhouse, while other Guardians make no effort whatever to exclude even the well-to-do classes from receiving gratuitously both indoor and outdoor medical relief. Innumerable instances of the existing lack of uniformity will be found throughout our evidence, and this diversity applies to every form of relief and to every class of applicant. In no cases is the varying interpretation placed on the claim to relief better illustrated than in the case of the widow. In one part of the country a widow with one child would get no relief whatever unless she came into the workhouse; in another part of the country she would, indeed, get out-relief, but nothing for herself; in a third district she would get as much as 5 [shillings] for herself and 4 [shillings] for her child; and in a fourth district she would get relief only if she consented to part with her child and send it into a Poor Law School. Or again, in one Union outdoor medical relief may be freely given to all applicants without any inquiry as to means, whilst in another Union it is refused to all persons unless they are in receipt of ordinary outdoor relief. (House of Commons, 1909, pp101–102)

The majority of guardians in each union were elected at the parish level. The number of guardians varied considerably across poor law unions due to differences in the number of parishes within the union and in the number of guardians representing each parish—with the latter determined by the Poor Law Commission based on the parish population. In addition, each board included local justices of the peace—typically local landowners—as *ex officio* guardians. In 1908, the number of guardians per union thus ranged from eight to 104, with an average of 38 (House of Commons, 1909, Appendix XXV, p.650).

The focus of this paper is on the changes to the way guardians were elected created by the 1894 Local Government Act. This act affected multiple aspects of how elections were conducted, including, most importantly, implementing a secret ballot, removing *ex officio* guardians, replacing a system of a graduated franchise with a one-man-one-vote system, and removing the property qualifications required to act as a guardian. Each of these aspects are discussed in detail below.[4]

[4]The administrative procedures for the poor law were very detailed, and it is only possible to provide a high-level overview here. For additional detail, readers are referred to the parliamentary papers and secondary sources cited in the text below. In particular, the discussion of the pre-1894 system draws heavily on the 1878 Select Committee on the Election of the Poor Law Guardians (House of Commons, 1878b), and that of the 1894 changes on MacMorran and Colquhuon Dill (1907). For changes in the franchise, see Keith-Lucas (1952). The practice of poor law elections is discussed extensively in both the report of the 1878 Select Committee and the report of the 1909 Royal Commission on the Poor Laws (House of Commons,

**Election Procedure**

Voting for the guardians occurred in April each year, except for one-off November elections as part of the transition from the pre-1894 system. Prior to 1894, the entire board was elected annually; after the Local Government Act, guardians were instead elected for three years, with one-third of guardians leaving office each year (if not re-elected). These terms could be extended to three years by the poor law commissioners. By 1894, this had been done, at the request of the guardians, in over 100 unions, including all of the largest ones (Keith-Lucas, 1952, 137-138). Prior to 1894, elections took place on the 7th-9th of April each year; the Local Government Act changed this date to April 15. The first elections under the Local Government Act took place on 8 November 1894 (except with special exceptions). After 1894, there was the potential for all of the guardians to retire every three years due to an order of the relevant county council (on application of the Board of Guardians), or because of an order of the Local Government Board. The Local Government Act was implemented by electing whole boards of guardians under the new regulations in November 1894. After that point, one-third could continue until 1896, a further third until 1897, and the final third until 1898.

There were also unelected guardians on the boards, both before and after 1894. The 1834 New Poor Law allowed all county magistrates (Justices of the Peace) to sit as *ex officio* guardians on the boards. These *ex officio* guardians were removed by the 1894 act, but instead boards could co-opt up to four persons from outside of the body—including a chairman or vice-chairman—as additional guardians to gain outside expertise. However, in practice, few did so. In 1908, only 370 of a possible 2,570 members had been co-opted, and two-thirds of boards (425 out of 643) had not co-opted anyone at all (House of Commons, 1909, p100).

In the pre-1894 system, voting occurred via voting papers that were left at voters' houses and then collected the next day (voters could also deliver the votes themselves). There was no way for a vote to be kept secret: all voting papers had to be signed. Owners not resident in a parish could appoint a proxy voter (if resident in a parish, they were required to vote themselves).

The criteria for a proper ballot were somewhat stringent:

> "each voter shall write his initials in the proper column of the voting paper delivered to him, against the name or the names of the person or persons (not exceeding the the number of guardians to be elected...for whom he intends to

1909).

> vote, and, shall sign his name at the foot of the voting paper; and when any person votes as a proxy, he shall in like manner write his own initials and sign his own name...but if the voter cannot write then he may affix his mark at the foot of the voting paper in the presence of a witness, and that witness has to attest it, and he has to write the name of the voter against the mark, and the initials of the voters against the name of the person for whom he votes; *and there is an express provision that if those regulations are not complied with in every respect, the voting paper shall be void and rejected in the calculation of votes.*" Evidence of Mr. Fry, Assistant Secretary of the Local Government Board House of Commons (1878b, para 15) (emphasis added).

These restrictions could be enforced rigorously, meaning that a number of votes were found to be invalid each year. For instance, recording a cross or the number of votes rather than the initials next to the name of the person could cause a paper to be invalidated in some areas. Further, it was not made clear on the voting paper that recipients of relief should not vote, which may also have inflated the number that were rejected (House of Commons, 1878b, paras 114, 334, 15229).

After 1894, elections involved a secret ballot were implemented in the same way as voting for parliamentary elections (as per the 1872 Ballot Act). Under these provisions, voters had to attend a polling station to submit their ballot.

**Voting Qualifications**

The basic framework determining the right to vote in poor law elections remained the same throughout our period of interest. Individuals had the right to vote in a parish as long as they paid local taxes (the "rates") on property within that parish, were resident in that parish, and had not received poor relief or alms prior to the election. Voters also had to meet requirements that they had been resident and rated to the poor rates for the previous twelve months, and paid all relevant rates due in the previous twelve months, excluding those made within the six months before the election. Those in receipt of poor relief, or other alms, in the twelve months before the election were also disqualified from voting.[5]

The most important changes to voting qualifications involved the number of votes that each voter held rather than the right to vote itself. Prior to 1894, owners and occupiers

[5]The exact implementation of these restrictions depended on local practice, particularly regarding the date of making rates. Over the course of the nineteenth century, the requirements became more codified, however, and under the 1888 County Electors Act (implemented as part of the 1894 LGA), the rate-paying requirement was met if by the 20th of July, all rates up to the 5th of January had been paid. Voters were disqualified from voting if they had received union, parochial, or other alms (e.g., from charity) in the 12 months prior to July 15.

received up to six votes in each capacity (if rated over £250).[6] As a result, they could assign multiple votes to each guardian in an election—a system explicitly designed to protect large property holders (House of Commons, 1878a). After 1894, this graduated franchise was removed, and each eligible voter received only one vote within a parish; voters could still qualify to vote in multiple parishes, depending on their property holdings.

Two less significant changes to voting rights are worthy of note. The first relates to the female franchise. Women, if rated as taxpayers, could vote throughout the period (McClaren, 1987, p480). However, married women were potentially disenfranchised on the basis of coverture, under which husband and wife became one person in law. This disqualification was largely removed by the 1882 Married Women's Property Act, but some legal ambiguity remained. A stipulation in the 1894 LGA ensured there was no disqualification as a result of marriage. However, the group affected by this part of the legislation was very small; since property was generally rated to the husband, only married women either living away from their husbands or keeping a separate business would have been affected.[7]

A further set of changes in the franchise relates to the right to vote of recipients of poor relief or other charities.[8] At the beginning of the nineteenth century, paupers were disqualified from the poor law franchise under Common Law rather than under statute (the disqualification is not mentioned in the 1834 Poor Law Amendment Act). Local practice thus depended on both local custom and a patchwork of legal decisions; however, as a general rule receiving relief in the 12 months prior to creation of the electoral register meant losing the right to vote—a rule codified in the 1894 Local Government Act. The exact qualification for being considered a pauper, however, was ill-defined and became increasingly complicated as the scope of the poor law expanded. A body of statutes, therefore, expressly excluded forms of relief as leading to disqualification. It was not until 1917 that the disqualification was removed entirely.

Individuals could also be *de facto* disenfranchised by the decisions of parish officials (the overseers) regarding who was officially listed as paying the rates. Legally, overseers were expected to enter the names of all occupiers into a ratebook—which, until 1894, essentially

[6]This voting scale was established in the 1844 Poor Law Amendment Act. Previously, the 1834 Poor Law Amendment Act had imposed a scale of up to 6 votes for owners (if they owned over £150), and up to three votes for ratepayers (if they were rated over £400).

[7]See Keith-Lucas (1952, pp 166–167) for a discussion of the history of the female franchise in local government. For further details of the rationale underlying the 1894 stipulation, see the Parliamentary debate recorded in Hansard, House of Commons, 21 November 1893, vol. 18 cols. 1380–1472.

[8]The pauper disqualification the local level is discussed in Keith-Lucas (1952, p161–163) and at the national level in Briggs (1979).

served as the electoral register.[9] However, this may not have happened in practice in all areas, particularly in the case of tenants ("compounders") who paid their rates through their landlord.[10] However, compounding was rare in rural areas (House of Commons, 1909, Appendix I, para 2155). There was no legal requirement that overseers seek out occupiers (if, for instance, they were not at home at the time of rating), and hence may have depended on the landlord to supply their names. If people were not entered in the ratebook, they could claim to be; in practice, however, this may not have been done frequently. (House of Commons, 1878b, paras 731–741, 763–765, 839).

Individuals would also lose the right to vote if they moved between the time the ratebook was compiled and the date of the election. Poorer voters were more likely to move, and hence this particularly affected them (House of Commons, 1878b, para 2204). It also meant that the timing of the ratebook was important in determining the level of enfranchisement—the further ahead of the election it was compiled, the more likely people were to have moved and become disenfranchised. In Oldham and Chorlton, for example, the ratebook was complied in November; since an individual had to be registered for over a year, this meant that qualifying for an April election meant relying on a ratebook compiled nearly eighteen months earlier (House of Commons, 1878b, paras 1666–67, 2167).[11]

**Guardian Qualifications**

An additional change in the 1894 Local Government Act was the removal of all qualifications, except residence, required for individuals to be elected as guardians. The 1834 Poor Law Amendment Act had allowed the poor law commissioners to impose qualifications based on the value of property held within the union. The limit was largely fixed at the point a union was formed, and it was allowed to vary to account for differences in property values: in 1878, the value ranged from £15 to £40 (House of Commons, 1878b, para 496). In 1893, this limit was reduced to £5 and then removed entirely in the 1894 Local Government Act.[12]

The 1894 Local Government Act also removed any ambiguity regarding the right of women (particularly married women) to act as guardians, but this does not seem to have had a large impact on the boards analyzed in this paper. Women had been elected as

[9] After 1894, poor law elections used the register as defined in the 1888 County Electors Act.

[10] Evidence to the 1878 Select Committee indicated variation across areas; for example, it was reported that all occupiers were entered into the ratebook in Wolverhampton and Spalding (paras 1822, 2462) but not in Eastbourne (para 834–840).

[11] In contrast, municipal elections occurred in November, meaning that the ratebook was complied a year in advance; this was given as the principal explanation for differences in turnout between the elections for the two different bodies.

[12] See *Essex Standard*, Saturday 3 December 1892, p.5 for discussion of the initial reduction in property qualifications.

guardians long before 1894, but the exact legal situation was complicated for the same reasons as the female franchise discussed above [13]. Following the 1894 Act, the number of female guardians in urban areas grew substantially; however, female representation remained low in rural districts. In 1907, only 146 of the 16,001 Rural District Councilors were female (House of Commons, 1909, Appendix XV, p651). Therefore, it does not appear that the reform had a major effect in terms of increasing the representation of women on councils.

## C.2 Elections in Poor Law Unions

Although quantitative data on poor law elections is scarce, a great deal of qualitative evidence is available through the reports of Parliamentary enquiries in 1878 and 1909. Table C.1 summarizes the relevant evidence provided by witnesses in a total of thirty rural poor law unions to the 1909 Royal Commission on the Poor Laws and Relief of Distress, including information on the composition of the boards of guardians, electoral contests, and the effect of the 1894 Local Government Act. The information available varies considerably for different unions, predominantly reflecting the fact that witnesses supplied both written and oral evidence, with the latter providing much greater detail according to the Commissioners' questions. Thus, the absence of a comment should not be seen as indicative.

**Composition of Boards**

Boards of guardians were consistently dominated by farmers, both before and after 1894. While there is some variation in the size of the property farmed, farmers—and specifically tenant farmers—appear to be in the majority in all thirty unions. This is particularly true in rural parishes (the majority of parishes in the unions we study), where farmers tended to be elected alongside clergymen and a few landowners or land agents. In urban parishes, on the other hand, tradesmen and professionals served as guardians, while in several cases women were elected.

We can also glean some information regarding the composition of poor law boards by examining the tenure of chairmen and vice-chairmen of the boards of guardians—positions chosen by the guardians each year. I investigate these using two contemporary sources: first, an 1893 parliamentary paper identifying whether these officials were *ex officio* or elected, as well as their length of tenure, and second, directories from 1893 and 1898 listing the names of individuals in each position for every poor law union. These sources show that 46% of

[13] See Webb and Webb (1929, p234). The number of female guardians across the entire country increased significantly over our period of interest: In 1885, there were 50 female guardians; in 1895, there were 839; and in 1907, there were 1141.

boards within the regression sample had *ex officio* chairmen in 1893 (36% across all poor law unions)—37% of whom were still in place five years later—indicating either that they had chosen to stand for election or had been co-opted onto the board by the other guardians.

**Contested Elections and Turnout**

Few poor law union elections were contested, particularly in rural areas. Of the thirteen unions in Table C.1 with relevant evidence, only Spalding reports a considerable number of contests. In others, it appears only around one-tenth of elections were contested—and sometimes even fewer. This lack of contests was characteristic of the poor law long before 1894: nationwide there were contests for fewer than 4% of guardianships between 1873 and 1875. Further, these contests were mostly in urban areas rather than the rural unions on which this paper focuses House of Commons (1878b, paras 24–25, 263). For example, in the predominantly agricultural Spalding union, there were around 40 contests—for 30 guardianships—over 40 years (House of Commons, 1878b, para 2413).

The lack of contests is reflected in slow turnover in board leadership. In 1893, the average chairmen had been in office for almost 10 years—the longest serving chairmen had held their roles since the 1840s. These extended terms were only partly a result of unelected *ex officio* guardians sitting on the boards: the average elected chairman had been in place for 8 years and the longest serving had held the position for 46 years. Overall, the positions were marked by stagnancy: despite the fact that officers were elected annually, only just over a third (37%) of unions had experienced a change of chairman between 1888 and 1893, and only 46% between 1893 and 1898.[14]

Where elections did take place, turnout in rural areas appears to have been quite high. The witnesses listed in Table C.1 suggest turnouts of around 85%–90% were the norm. These high turnouts contrast with the general consensus on poor law turnouts, which appear to be based on the (much more frequent) contests in urban parishes. In general, turnout was around 20%–30% of the electorate (House of Commons, 1878b, paras 702, 845–847,1423), House of Commons (1909, p101–102), Webb and Webb (1929, p233)), although it could be much higher —within London in 1904, ward-level turnout ranged from 13%–97% of the population (House of Commons, 1909, Appendix I, para 14043).[15] The reason for these differences is likely a mixture of voters in smaller rural parishes having greater personal knowledge of candidates and fewer registration difficulties due to frequent movement between

[14]These statistics relate to the 225 unions in the regression sample. For all poor law unions, the figures are 48% and 52%, respectively.

[15]Voter turnout also varied widely across the parishes in Chorlton Union in the 1870s (House of Commons, 1878a). See also House of Commons (1878b, paras 1005, 1726).

addresses.

Evidence regarding the level of public interest in the poor law is mixed, but there is no indication that the poor were particularly disengaged. While in some cases, the public are seen as apathetic, in others the public are seen as engaged, particularly after 1894. The differences do not appear to be based on the poor being uninterested in politics: in Staffordshire, for instance, the poorest voters were reported to take the greatest interest in the election in the early twentieth century (House of Commons, 1909). More broadly, there is little evidence to suggest that the poor were *less* interested in the poor law than other classes of citizen.

**Changes following the 1894 Local Government Act**

The qualitative evidence suggests that the most common effect of the 1894 reforms was to reduce the representation of landowners on boards of guardians and to increase expenditure in the short term. However, there is noticeable variation in these effects: in some cases there appears to have been very little change in either board personnel or subsequent policy. Further, while there are drastic policy changes in some unions, in others there appears to be a more subtle movement towards greater expenditure.

Several of the reports in Table C.1 commented on the loss of *ex officio* guardians and the consequent unwillingness of country gentlemen to engage in poor law politics. Prior to 1894, landowners could serve on the board without election as justices of the peace, but once this right was removed, it appears they were often unwilling to stand for election.[16] In some other cases, smaller farmers also appear to have replaced large farmers after 1894. Notably, however, in some unions the reported composition of the board hardly changed at all.

The Act was not followed, however, by considerable representation of the poor on boards of guardians. With the exception of Stow, where agricultural laborers formed a majority, there were at most a few working class men or agricultural laborers serving as guardians. The cost of acting as a guardian—in terms of both the financial and time commitment—is cited as an explanation for this. The effect of the Act was thus to proportionally increase the representation of the tenant farmers at the expense of landowners and gentlemen in rural areas.

Changes in the boards of guardians tended to be associated with increases in spending,

[16]Unfortunately, it does not appear that there was any detailed record made of the number of *ex officio* guardians. In general, it appears to have been less than half (although there were some exceptions, at least in London). In two rural unions discussed in the 1878 Select Committee, the number stood at six out of twenty-nine and five out of thirty-five guardians. See House of Commons (1878a, paras 272, 2408, 2469, 5009-5010). In Brixworth, 16 out of 60 guardians were *ex officio* in 1889 (Hurren, 2015, Table 6).

particularly in the provision of outrelief. This effect is most marked in Stow, where agricultural laborers were able to form a majority on the board and more than double weekly expenditure. Similarly, in Mitford and Launditch, the new board quickly reversed a policy of reducing outrelief. In Madeley and North Witchford, however, the move to a laxer outrelief policy appears to have been more moderate.

Notably, witnesses frequently reported that the initial upsurge in outrelief was temporary. In general, new guardians are reported to have quickly fallen into line as they recognized the financial burden of more generous expenditure—the conclusion arrived at by the chief inspector of the Local Government Board (House of Commons, 1909, Appendix I, para 2046). Alternatively, some witnesses suggest the spending increases engendered a reaction from local taxpayers, and consequently spending fell again, although not necessarily to previous levels.

Finally, there is also limited evidence that the Act led to greater engagement with poor law politics. Some witnesses report either growing political interest or greater enthusiasm on the part of guardians following 1894. However, this is not consistently true, and it appears to have been limited in magnitude, thus supporting the conclusions of Webb and Webb (1929) that both the number of contests and popular interest in elections increased after 1894, but "only...relatively to the almost complete deadness that prevailed during the generation preceding the Act" (p233).

Table C.1: Summary of evidence from the 1909 Royal Commission.

| Union | Source | Board Composition | Elections | Effect of Act |
|---|---|---|---|---|
| Atcham | 69924, 70511, Apps 46, 101, 115, 119 | Rural parishes: mainly farmers, a few clergymen and country gentlemen. In towns: tradesmen, publicans, etc., and of late 3 labour candidates. | No contests (out of possible 18) last year. Where there are contests in Shrewsbury (town) turnout around 50%. Sometimes party political, sometimes not. | Laxer outrelief policy, due to loss of ex officio guardians with "culture and education"—who do not want to contest elections—and influence of labour members. |
| Bosmere and Claydon | 73628, 73805, 74034, 74127, App 199 | 2 landowners, 8 landowners farming their land, 17 tenant farmers 17, 1 miller, 1 harness maker, 1 grocer, 5 clergy, 1 solicitor. No labour representative. | Contests exceptional: last time four elections (out of 38) was more than usual. Mean turnout 87%. No political tinge. | Fewer landowners and more farmers on board. Guardians show more enthusiasm in work. |
| Bridgwater | 67905, Apps 41, 45, 54, 73, 81 | Rural parishes: 35 retired farmers and farmers, 3 clergymen, 7 magistrates / county gentlemen, 1 solicitor, 3 land agents; urban parishes: 1 married lady and 6 tradesmen. | Not many contests; last election: only 5 out of 49. | Main policy was "economy" before and after LGA—no effect of Act. Accepted premise that labourers "practically hold supreme power." |
| Church Stretton | 72138, Apps 97, 142, 160, 162 | 20 guardians, majority farmers, few clergymen and professionals. | | |
| Dulverton | 68878, App 21 | Almost entirely farmers. | | Resident gentry stand aloof, main reason "delicacy of feeling" |
| Ellesmere | 71323, 71522, 71697, 71938, Apps 111, 132, 153 | Majority are important tenant farmers, also a few country gentlemen, tradesmen, land agents, and 2–3 clergymen. | | Large class of lower status than formerly now seeking election. Loss of ex officio guardians undermines policy stability. |
| Ely | 74604, App 167 | 34 guardians, around four-fifths farmers. 2 clergymen, barrister, brewer, hotel keeper, 2 ladies (in town), 3 co-opted. Has never been any labourers. | Few contests. Around 3 this time. One guardian (a landowner) has been opposed twice in approximately 12 years, with turnout around 85%–90%. | Made practically no difference to constitution of board. |
| Freebridge Lynn | 75246, App 171 | 16 farmers, 8 clergymen, 15 various occupations. | | 30 years previously, the large occupiers of land used to serve the office in turn, but at this point, there were only 6-8, replaced small farmers and a few tradesmen. More interest by inhabitants and better attendance of guardians in recent years. |

Table C.1: Summary of evidence from the 1909 Royal Commission (continued).

| Union | Source | Board Composition | Elections | Effect of Act |
|---|---|---|---|---|
| Henstead | 72976 | Mostly farmers with small holdings, 2–3 clergymen, landlords' agents, and a doctor. No representation of labouring classes. | No contests since joined board in 1899-1900. Parishes very small with few people to stand: agricultural laborers cannot afford time to attend meetings. | |
| Langport | Apps 56,57,65,71 | 34 guardians. Chiefly tenant farmers; others include tradesmen, clergymen, and gentlemen of independent means (Justices of the Peace). Usually large ratepayers. | Very few contests, with no political element. | |
| Leominster | 72681, Apps 124, 136, 138 | 30 guardians, nearly all tenant farmers, except in town which has 1 lady, 2 innkeepers, and 1 farmer. | Contests rare—one guardian had 3 contests within 14 years. Public apathetic at time of election. | Ex officio were better class of men, and more conscientious. People of education and position do not come on to the board of guardians because board quite "rough"—if a gentleman disagreed with some farmers, he would be insulted. |
| Linton | Apps 163, 168, 195 | 12 farmers holding over 300 acres, 3 farmers holding over 100 acres, 3 farmers holding over 50 acres, 1 clergyman, 1 merchant. 1 retired farmer, 1 shopkeeper, 1 major, 1 mill manager. 3 seats vacant | | |
| Madeley | App 94 | Farmers, manufacturers, tradesmen, insurance agents. | | Union used to give little outdoor relief, but changed with LGA when outrelief increased to alarming extent. Last year or two efforts have been made to reduce it again. |
| Malpas | App 95 | Generally farmers and tradesmen; occasionally country gentlemen and clergymen. | | |

Table C.1: Summary of evidence from the 1909 Royal Commission (continued).

| Union | Source | Board Composition | Elections | Effect of Act |
|---|---|---|---|---|
| Mildenhall | 75948 | 18 guardians in total, farmers, 2–3 clergymen, 3 land agents, 1 professional. No labor representatives. | One parish where there is generally a contest. Purely a political matter. If there is a contest, poorer classes will take part: turnout of 87–90%. | |
| Mitford and Launditch | 74414 | 67 guardians, mostly farmers (large and small), 10 clergymen, 3 ladies, a few landowners, and a few shopkeepers and small tradesmen. No insurance agents. | Have been contests since 1894, but public interest has not increased. Elections not on political lines; sometimes triggered by individual claims to relief being turned down. | New board undid previous policy of cutting down outrelief, leading to large expansion in number receiving outrelief. Then gradual return to old policy after 1896, but still many people receiving relief that should not be.<br>Change in class of guardians: many "not so well educated." Not trade unionists but working men and small shopkeepers—small tradespeople a little bit above the agricultural laborer.<br>Most of board farmers, before entirely so. But now some larger farmers replaced by smaller farmers. E.g., present chairman of tho House Committee is the son of a man who was a laborer a few years ago. |
| Newton Abbot | Apps 43, 46, 49, 50, 84 | | Rural parishes: chiefly farmers; urban districts: tradesmen, middle class, ladies. | No difference in position of persons seeking election as guardians, but advantage of ladies on the board every year. |

Table C.1: Summary of evidence from the 1909 Royal Commission (continued).

| Union | Source | Board Composition | Elections | Effect of Act |
|---|---|---|---|---|
| North Witchford | 74985, Apps 177, 188, 196 | Mainly large and small farmers with a few clergy, tradesmen, and men of independent means. | Very few changes in board, not a single contested election in 40 years in main parish. | Indoor and outdoor relief quite stationary before 1894, but since then outdoor relief given more freely. |
| Oswestry | Apps 102,120, 151 156, 207, 218 | Clergymen, property owners, farmers, shopkeepers, colliery proprietors, land agents and two labor members. | | |
| Risbridge | 75894, App 184 | Majority of farmers, with clergy, tradesmen and retired tradesmen. | | Increase in relief costs after 1891, due to laxer administration - in turn due partly to reform. Abolition of qualifications to be guardian regrettable in some cases. |
| Sherborne | Apps 38, 39, 80, 90 | Chiefly farmers, 3–4 gentlemen, 2–3 tradesmen, 2 ladies. | | |
| Shifnal | Apps 122, 162 | 9 farmers, 2 clergy, 1 mining engineer, 1 gentleman, 3 landowners, 1 manufacturer, 1 postmaster, 1 estate agent, 2 insurance agents and 1 builder; total, twenty-two. | | |
| St Thomas, Exeter | 68237, Apps 22, 30, 32, 48, 68, 83 | County squires, retired army officers, 16–17 clergymen, tradesmen and farmers, 5 ladies. | | |

Table C.1: Summary of evidence from the 1909 Royal Commission (continued).

| Union | Source | Board Composition | Elections | Effect of Act |
|---|---|---|---|---|
| Stow | 75345 | Farmers, a few clergymen, tradesmen and laborers. Country gentlemen show little interest. | LGA led to many contests—45 in one year. Normally around three-quarters of electors take part in contests. | In 1894, poor relief had been around £45 for 30 years. Guardians were chiefly substantial farmers, with a few landlords and clergy. They were very particular about granting of outrelief. In 1895, new qualification allowed a majority of laborers on the board and within weeks outrelief jumped to £92 per week, and later £100. Almost everyone that applied given relief, and people even invited to apply. In subsequent elections (1898, 1901, 1904) the labor representation fell from 23 to 8—some representatives defeated, many retired—and outrelief slowly being diminished—now £68 per week. |
| Tenbury | 71061, Apps 110, 125, 158 | Mostly superior tenant farmers, 3–4 clergymen, 4 landowners, 1 lady, 2 tradesmen from the town. | | |
| Thingoe | 76117 | Tenant farmers, land agents of residential estates, and clergy. | | |
| Wellington, Somerset | 67742, Apps 25, 35, 36, 78, 89 | Rural parishes: 4 clergymen, 2–3 magistrates, and the rest farmers; urban: tradesmen, retired tradesmen, shopkeeper. No one of labouring class has been put forward. | | |
| Whitchurch, Shropshire | Apps 125, 129, 132 | Larger ratepayers. Rural parishes: farmers in almost all cases, otherwise landholders; urban parish of Whitchurch: 6 tradesmen, 1 solicitor, 1 insurance agent, 1 no occupation. | | |

Table C.1: Summary of evidence from the 1909 Royal Commission (continued).

| **Union** | **Source** | **Board Composition** | **Elections** | **Effect of Act** |
|---|---|---|---|---|
| Williton | 69292, App 51 | 49 guardians, 34 farmers, also 1 labor representative and several ladies. | Generally not contests, when they occur they are on "political" grounds. | Main reason election is sought is for rural district council, not to be guardian. |
| Yeovil | Apps 23, 53, 68 | Majority farmers, also country gentlemen, businessmen, a few clergymen, working men and 3 ladies. | In Yeovil (likely town) run on political lines. | Several working men have been elected in Yeovil (likely town). |

Note: Table summarizes the evidence given to the 1909 Royal Commission on the Poor Laws (House of Commons, 1909, Appendix VII).The number in the source column refers to the starting paragraph for oral evidence, while "App" refers to the written statements included as Appendices.

## C.3 Rural Sanitary Authorities and Highway Districts

While the duties of the boards of guardians were restricted predominantly to the provision of poor relief, the guardians themselves held responsibility for spending on public goods when acting in other capacities. After the 1870s, the guardians in rural areas also constituted the rural sanitary authorities that were responsible for maintaining local sanitary environments. After 1894, these bodies gained additional responsibility for maintaining local road networks, as they became the highway authorities for their districts.[17] These additional responsibilities pose a potential complication for the paper's analysis in two ways. First, they mean that the link between the empirical context and the theoretical models focused on redistribution are less clear—since the guardians had some control over public goods spending. Second, the fact that the range of spending controlled by sanitary authorities changed in 1894 means that the electoral system changed at the same time as the range of authority of the rural guardians.

In this appendix, I provide evidence that the additional powers of the guardians are not a major concern. First, the spending controlled by these other authorities was far less than the provision of relief administered by the guardians. Second, the results are robust to controlling directly for this additional spending, demonstrating that the main hypotheses are not in some way capturing an interaction between poor relief and these other government activities.

I analyze the importance of the additional roles of the guardians using two additional sets of data. First, to assess the relative size of this additional spending I combine the poor law union data with spending data for rural sanitary authorities and highway authorities for the 1894 cross-section. The fragmented nature of the highway authorities makes identifying their total spending difficult, particularly because "highway boards" (groups of parishes) could have different boundaries than poor law unions. I thus combine the actual expenditure of "highway parishes"—individual parishes acting as highway authorities—aggregated at the poor law union level, with estimated expenditure for the "highway boards" based on the total revenue raised under precept within each poor law union. Second, I construct a time-varying measure of the revenue burden associated with the rural and highway authorities, using the total amount raised under precept for these bodies in each year, and assuming per capita

[17] Prior to 1894, the control of highways was distributed across a morass of "highway boards" and "highway parishes" controlled by "waywardens" elected by the parish. See Webb and Webb (1913, Chapter IX, especially pp. 209–213) for discussion of the history of the highway boards. In addition a small number of Rural Sanitary Authorities held such responsibility prior to 1894. See Dodd (1890) for a detailed discussion of the powers and responsibilities of the rural sanitary authorities prior to 1894.

spending by highway parishes is fixed before 1894. Here I take advantage of the fact that many local taxes in Britain, including the majority of revenue for these bodies, was routed through (collected under precept by) the poor law guardians.[18]

The data shows that spending on poor relief was significantly greater than that of either rural sanitary authorities or the highway boards prior to 1894. Expenditure on rural public goods was extremely small—accounting for 9% of relief expenditure in the median poor law union in 1894.[19] Spending by highway boards was higher, but still comparatively small—with tax revenue accounting for less than 30% of relief expenditure in the median poor law union. On average, total spending by these bodies in 1894 was thus approximately 52% of poor relief in the median poor law union. Poor relief was thus by far the largest item of expenditure controlled by the guardians.

Even if the level of spending is relatively low, it remains plausible that the changes wrought by the 1894 Local Government Act confound the analysis. Most of the changes to these rural sanitary bodies were cosmetic, with the powers of rural sanitary authorities transferred to the new rural district councils.[20] Prior to 1894, the guardians acted as the rural sanitary authority; after 1894, the rural district councilors acted as the guardians. However, more significantly, the fact that the highway boards were combined with rural sanitary authorities meant that the amount of non-poor-relief expenditure controlled by the guardians increased. If the magnitude of that change was correlated with the level of inequality in a district, then the main results could be capturing something other than the effects of democratization in each union—such as trade-offs between different forms of expenditure.

Additional results, however show that the changes in the guardians' responsibilities do not affect the key findings—see Table C.2. Here, to check that the results are not capturing

[18]Consequently, these revenues are reported in the poor law accounts until 1903. The revenue that was not collected in this way was largely not determined at the union level, so it would not have been controlled by the guardians in the same way. First, part of the tax revenue raised for rural sanitary purposes was raised for "special expenses" that related to only part of the district. Second, highway authorities received funding from the county councils that were responsible for maintaining main roads. Finally, the "highway parishes" discussed above raised funds directly.

[19]This value includes both current (not funded out of loans) and investment (funded out of loans) expenditure.

[20]Specifically, the 1894 Act defined the rural sanitary districts controlled by rural district councils as consisting of the parts of a poor law union that were not contained in an urban sanitary district (the exact boundaries could change later under orders from county councils, (MacMorran and Colquhuon Dill, 1907, p.101). In principle, this means the composition of the Rural Sanitary Authority could have changed over time if towns were designated as "urban sanitary districts." However this is of not of great consequence to the analysis in this paper due to the focus on rural poor law unions; 86% of unions in the sample contained 0 or 1 towns (or part of a town).

these changing responsibilities, I re-estimate the main specifications including the estimated tax revenue raised for these bodies as a control variable. I allow public goods spend per capita to enter both as a level, and allow for a change in trend after 1894—this data is available only until 1903, and so the number of observations is lower in these specifications. To assess how including these extra variables affects the coefficients relating to inequality, the first column presents results for this smaller sample without adding measures of public goods expenditure. The remaining columns then include each inequality measure separately and, finally, together. As we can see, the coefficients relating to other forms of spending on poor relief are small and not robust suggesting little if any spillover in the spending decisions of the different bodies. Further, the coefficient on the inequality variables are very similar when including these additional variables. Thus any effect of these other forms of government spending on the provision of poor relief does not appear to confound the observed relationships with inequality.

Table C.2: Robustness to controlling for other types of local government spending in poor law unions.

| | DV=Relief Expenditure per Capita (Standardized) | | | | |
|---|---|---|---|---|---|
| Post 1894 x | | | | | |
| Income Inequality | 0.08*** | 0.09*** | | | 0.08*** |
| | (.025) | (.027) | | | (.026) |
| Local Elite Control | −0.10* | | −0.11** | | −0.10*** |
| | (.052) | | (.053) | | (.052) |
| Cereal Suitability | 0.09*** | | | 0.10*** | 0.09* |
| | (.029) | | | (.029) | (.029) |
| PublicGoods_x_post1894 | | 0.01 | 0.04* | 0.04* | 0.01 |
| | | (.024) | (.024) | (.023) | (.023) |
| Public Goods Spend p.c. | | −0.04 | −0.05** | −0.05** | −0.03 |
| | | (.023) | (.023) | (.023) | (.022) |
| Controls | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y |
| No. Observations | 3,952 | 3,952 | 3,952 | 3,952 | 3,952 |
| No. PLUs | 208 | 208 | 208 | 208 | 208 |

Notes: "Public Goods Spend p.c." is the estimated spend per capita of the rural sanitary and highway authorities. These variables are only available until 1903, so the total number of observations is less than in the main regressions. "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level.

## C.4 Categorization of Paupers in 1895

The breadth of support provided by the poor law is demonstrated by the range of pauper "types" included in the annual reports. The two major categories in these reports were "able-bodied" and "non-able-bodied" paupers. The distinction between these categories is

not, unfortunately, as obvious as it may seem, particularly because these terms had no legal definition—so the classification between the two could vary both across areas and over time. In fact, "able-bodied" did not necessarily imply good health: many able-bodied paupers were acutely ill. Over half of able-bodied men receiving outdoor relief towards the end of the century were classified as "sick" (MacKinnon, 1988).[21] In fact, "non-able-bodied" largely referred to elderly paupers, with nearly all paupers over 60 classified as non-able-bodied in 1890 (MacKinnon, 1988, p.9).

Table C.3 displays the classification of paupers in January 1895, showing that nearly three-quarters of paupers were relieved outside the workhouse. Unemployed men—proxied by the number of able-bodied men and vagrants—were a relatively small share of relief recipients.[22] The majority of paupers were, in fact, women and children—both in the workhouse and outside—together accounting for over 70% of paupers. More than half of paupers were not-able-bodied which, as per the previous paragraph, reflects the importance of poor relief in supporting the elderly prior to the advent of old-age pensions.

Table C.3: Classification of paupers in 1895

| | | Able-bodied | Not Able-bodied | Lunatics | Vagrants | Total |
|---|---|---|---|---|---|---|
| **Indoor** | Men | 0.9% | 5.7% | 0.5% | n.a. | 7.2% |
| | Women | 1.3% | 2.3% | 0.8% | n.a. | 4.5% |
| | Children | 1.7% | 2.3% | 0.0% | n.a. | 4.0% |
| | Total | 4.0% | 10.3% | 1.4% | 1.9% | 17.5% |
| **Outdoor** | Men | 1.5% | 14.5% | 3.3% | n.a. | 19.3% |
| | Women | 6.0% | 32.6% | 4.1% | n.a. | 42.7% |
| | Children | 15.7% | 4.6% | 0.1% | n.a. | 20.3% |
| | Total | 23.2% | 51.8% | 7.4% | 0.1% | 82.5% |
| **Total** | | **27.1%** | **62.1%** | **8.8%** | **1.9%** | **100%** |

Note: Table displays the breakdown of the population receiving poor relief in 1895 (average of January and July figures) in the regression sample of 208 rural poor law unions. No breakdown by sex or age was provided for vagrants. "Lunatics" refers to those treated in asylums managed under the poor law.

[21] MacKinnon (1988) explains that the able-bodied and non-able-bodied categories were largely distinguished by diet. As a result, even those with disabilities could be categorized as able-bodied.

[22] "Vagrants" refers (roughly speaking) to the homeless poor—this category was particularly affected by economic conditions and so has been used as a measure of male unemployment by Boyer (2004).

# D Additional Empirical Analysis

## D.1 Alternative Measures of Inequality

This appendix shows that the main results are robust to alternative ways of measuring inequality. First, Table D.4 demonstrates that the results are similar using alternative measures of the mean–median ratio. Second, Table D.5 presents results for alternative measures of other dimensions of inequality discussed in the main text.

The mean–median ratio measure in the main text (Equation (2)) is constructed using a ratio of the mean rental to median wage, measured in 1869. I check robustness to alternative variable definitions, as follows:

*1869 Mean Rateable Value-Median Wage Ratio:* This measure is equivalent to the primary measure, but with the denominator the mean rateable—rather than rental—value in the district. That is, it is a direct measure of the average tax base after accounting for deductions due to, for example, maintenance of a property.

*1892 Mean Rental Value-Median Wage Ratio:* This measure is constructed equivalently in the main text, but using 1892 data for both the numerator and denominator. This checks I am also capturing inequality immediately prior to the reform. However, using this measure reduces the number of observations as wage data is not available for all counties. Further, unlike variables for other years, the 1892 wage data are based on only a few selected unions within each county, meaning they are relatively noisy measures of the wage for the county as a whole (Bowley, 1900; Hunt, 1973).

*1894 Mean Rental Value-Median Wage Ratio:* This measure uses data from 1894, including union-level wage data to estimate the denominator. The numerator is the gross rental value in 1894. Specifically, I use information from Board of Trade (1900) regarding summer agricultural wages, which includes information for approximately 80% of the unions included in the main analysis. This provides reassurance that my results are not an artefact of imposing the same median wage on every union in a county. However, there are drawbacks. First, in contrast to my main wage measure, this information does not incorporate allowances (such as free food, or fuel) into the wage, and thus it provides a less complete measure of income. Second, it comes at the cost of fewer observations. Third, in principle the wage could be endogenous to the changes to poor relief surrounding the 1894 Act.

*1881 Mean-Rental Value:Median Hiscam Occupational Score* Here I use the median hiscam occupational score as the denominator of the measure. The hiscam score provides a measure of the status of an occupation and so is, at best, an imperfect proxy for income. However, this

measure provides reassurance that the results are robust to allowing for potential differences in the occupation of the median (or decisive) voter in a union—relaxing the assumption that the agricultural laborer's wage is appropriate in every case.

The results show consistent evidence that income inequality shaped the effect of the democratic reform. The three measures relating to 1869 produce very similar results, indicating that the findings are not a result of assumptions over functional form or the use of the rental value rather than the tax base. The results using data from other years are smaller in magnitude, consistent with the smaller sample size and less precise measurements, as documented above.

Table D.4: Robustness to alternative definitions of mean-median ratio.

| | DV=Relief Expenditure per Capita (Standardized) | | | | | |
|---|---|---|---|---|---|---|
| Post 1894 x Mean-Median / Ratio | 0.11*** | 0.11*** | 0.11*** | 0.06** | 0.06** | 0.05* |
| | (.026) | (.025) | (.026) | (.029) | (.030) | (.025) |
| Controls | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y |
| No. Observations | 4,368 | 4,368 | 4,368 | 3,696 | 3,843 | 4,347 |
| No. PLUs | 208 | 208 | 208 | 176 | 183 | 207 |
| Definition of Mean-Median Ratio: | | | | | | |
| Numerator | Rental Value | Rental Value | Rateable Value | Rental Value | Rental Value | Rental Value |
| Denominator | County Wage | County Wage | County Wage | County Wage | Union Wage | Union Occ. Score |
| Year | 1869 | 1869 | 1869 | 1892 | 1894 | 1881 |
| Transform | None | Log | None | None | None | None |

Notes: Each column includes the interaction between post-1894 and a different definition of the mean-median ratio—explained in the bottom panel of the table. The numerator is always a mean at union level. The denominator is a median, using either a county-level or union-level variable. "Occ. Score" refers to the Hiscam occupational score. "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level.

Table D.5 presents results for alternative measures of many of the inequality measures analyzed in the main text. The first row investigates whether the presence of a great landowner—rather than a peer—in the union led to more elite power. The following two rows include alternative measures of land inequality: i) the share of value (not acres) owned by great landowners ii) a wealth inequality measure, estimated using a domestic servant to capture a unit of wealth (see Appendix B.5.2 for details), and iii) the number of any gentry (not only aristocrats) in the union. The fifth row uses a measure of the Crusade against Outrelief based on the fall in adult outdoor paupers—the main target of the Crusade. The following three rows use alternative definitions of measures relating to the need for poor relief—the estimated share of land used for wheat replaces cereal suitability, and the percentage of males over 64 replaces the percentage of the whole population in the same age group. The final column then includes multiple measures simultaneously.

The results for each of these variables are consistent with the comparable coefficients in the main text. We find positive and statistically significant (sometimes weakly) coefficients relating to income inequality, measures of need for outrelief, and the Crusade against Outrelief. The coefficient relating to inequality is again positive, although not statistically significant in this case—possibly reflecting the fact that land value, measured in 1873, may be less persistent than the number of acres owned. There is little evidence that other measures of inequality are important moderators, or that the presence of a great landowner affected spending over 1894. Further, the last column shows that again it is income inequality, cereal suitability, and local elite power that emerge as the most important dimensions of inequality. Overall, then, the table supports the conclusions from Section 6.2.

Table D.5: The main findings are similar when using alternative definitions of inequality measures.

| | DV=Relief Expenditure per Capita (Standardized) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Elite Power | Fairness Concerns | | | | Economic Need | | | Multiple Channels |
| Post 1894 x | | | | | | | | | |
| Great Landowner | −0.06 | | | | | | | | −0.15 |
| in Union | (.099) | | | | | | | | (.109) |
| Land Inequality | | 0.04 | | | | | | | 0.05* |
| (Value) | | (.028) | | | | | | | (.029) |
| Wealth Inequality | | | 0.02 | | | | | | −0.03 |
| (Servants) | | | (.030) | | | | | | (.032) |
| Number of | | | | 0.02 | | | | | −0.01 |
| Gentry | | | | (.029) | | | | | (.030) |
| Crusade vs | | | | | 0.06* | | | | 0.03 |
| Outrelief (Adults) | | | | | (.032) | | | | (.031) |
| % Wheat 1885 | | | | | | 0.07** | | | 0.03 |
| | | | | | | (.028) | | | (.035) |
| % Old Aged Males | | | | | | | 0.06** | | −0.04 |
| | | | | | | | (.031) | | (.042) |
| % Old Aged Females | | | | | | | | 0.07** | 0.10*** |
| | | | | | | | | (.031) | (.035) |
| Income Inequality | | | | | | | | | 0.10*** |
| | | | | | | | | | (.026) |
| Chairman Ex Officio | | | | | | | | | −0.10* |
| | | | | | | | | | (.053) |
| Cereal Suitability | | | | | | | | | 0.10*** |
| | | | | | | | | | (.037) |
| Controls | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| No. Observations | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 |
| No. PLUs | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 |

Notes: "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level.

## D.2 Robustness

This appendix presents additional tests that the estimates above capture a causal effect of inequality. The causal interpretation of the estimates relies on the assumption that poor law spending reacted to local inequality and not some other local characteristic. In particular, we might be concerned that inequality reflects other factors affecting the demand for poor relief—as such I first carry out robustness tests including reactions between the post 1894 period and other variables. I then present additional tests allowing for different time trends according to the observable characteristics of different poor law unions—providing further evidence that omitted variable bias does not explain the results. Third, I show that the results are not driven by particular subsamples of poor law unions. The following subsection then discusses the concern that the effects of income inequality may reflect differences in the urban surroundings of poor law union.

The post-democratic reform growth in poor law spending in areas of high inequality does not appear to capture other local characteristics driving the need for poor relief, as shown in Table D.6. These specifications include interactions between the post-1894 dummy and other poor law union observables that could reflect the post-democratic reform need for poor relief. First, I allow for interactions with the distance from London or another major city, to account for the possibility that poor relief is required less where outmigration provided an alternative reaction to unemployment. Specifications 3–5 account for the possibility that demand for poor relief could be higher in areas of low land value, declining land value, or low wages. Finally, the last two columns allow for the possibility that the reaction to reform was driven by high population density (and hence less reliance on agricultural labor), and the seasonality of the provision of poor relief pre-reform. This last variable is measured by comparing the number of paupers in the summer to those in winter, and reflects the fact that such areas could, in principle, have greater latent demand for short-term outrelief. While the coefficients on some of these variables are statistically distinguishable from zero, their inclusion does not disturb the main findings regarding inequality.

Table D.7 presents similar results allowing for other possible characteristics that might reflect ease of outmigration. Specifically, the first three columns allow for differential effects of the reform depending on the distance from towns of different sizes. The second three columns then allow interactions with demographic change—change in population, age structure, and the share in agriculture—which might themselves capture pre-reform migration. Again, these additional controls have little effect on the estimated effect of inequality.

Table D.8 shows that the main results are robust to including interactions between ob-

Table D.6: Robustness to allowing for post-reform reactions to other observable union characteristics.

| | DV=Relief Expenditure per Capita (Standardized) | | | | | | |
|---|---|---|---|---|---|---|---|
| Post 1894 x | | | | | | | |
| Income Inequality | 0.10*** | 0.09*** | 0.08*** | 0.09*** | 0.08*** | 0.11*** | 0.10*** |
| | (.026) | (.025) | (.026) | (.027) | (.028) | (.026) | (.026) |
| Local Elite Control | −0.10* | −0.11** | −0.11** | −0.11** | −0.14** | −0.12** | −0.11** |
| | (.054) | (.053) | (.053) | (.054) | (.054) | (.053) | (.054) |
| Cereal Suitability | 0.02 | −0.06* | 0.05* | −0.01 | −0.13*** | 0.06** | −0.02 |
| | (.026) | (.039) | (.028) | (.024) | (.027) | (.026) | (.029) |
| Post 1894_x_ | Distance from Major City | Distance from London | Tax Base per Acre 1893 | Δ Tax Base 1883–1893 | Wage 1892 | Density 1891 | Seasonality 1881–90 |
| | 0.02 | −0.06* | 0.05* | −0.01 | −0.13*** | 0.06** | −0.02 |
| | (.026) | (.039) | (.028) | (.024) | (.027) | (.026) | (.029) |
| Controls | Y | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y | Y |
| No. Observations | 4,368 | 4,368 | 4,368 | 4,368 | 3,696 | 4,368 | 4,368 |
| No. PLUs | 208 | 208 | 208 | 208 | 176 | 208 | 208 |

Notes: Each specification includes an interaction the characteristic (pre-reform, standardized for comparability) listed in the row "Post1894_x_" and a post1894 indicator variable. "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level.

servable pre-reform characteristics and fourth-order polynomial time trends. In particular, I allow for different trends in poor law spending according to the distance from London, land value in each union—either in levels or trends—population density or the seasonality of poor relief. The coefficients relating to inequality remain similar across all of these specifications.

Table D.9 shows that the results are robust to analyzing alternative groups of unions. The first two specifications show robustness to excluding outliers for inequality and spending respectively. The next two columns exclude the least unequal (Wales) and most unequal (West Midlands) Census divisions. The fifth specification limits the sample to unions where the median household status was in a farming occupation—as assumed by the income in-

equality measure. Finally, specification 6 studies a larger sample including unions with the missing income inequality measure. We can see that the results for the other two inequality measures are similar, providing reassurance that sample selection does not drive the results.

## D.3 Urban Surroundings

This Appendix the potential for the urban surroundings of poor law unions to influence poor relief, and potentially confound the estimated effects of income inequality. To do so I identify the closest town of different population thresholds to the centroid of each poor law union. Most migration in this period was over short distances, and so nearby towns could affect local labor market conditions, including creating an alternative option to receiving poor relief in the case of unemployment. If this were an important factor, we might expect unions nearer to larger, or fast-growing towns, to be particularly affected.

Table D.10 shows that controlling for the characteristics of the closest town to a poor law union does not affect the findings regarding inequality. Interestingly, the first two columns suggest that poor relief was lower when the nearest town above 25,000 inhabitants was growing faster—consistent with the idea that the town offered a safety valve for poor economic activities. When we investigate the nearest town over 100,000, in contrast, we see if anything the opposite pattern—faster growing towns were associated with more poor relief. In neither case, however, do the additional controls change the estimated relationship between the inequality measures and poor relief after 1894.

Table D.11 allows for the possibility that the reform interacted with characteristics of the closest town. We see very little evidence that the urban hinterland shaped the effects of democratic reform. In particular, income inequality remains a very strong predictor of the growth in spending after reform.

Table D.7: Controlling for interactions with additional measures of ease of out-migration does not affect the results.

| | DV=Relief Expenditure per Capita (Standardized) | | | | | |
|---|---|---|---|---|---|---|
| Post 1894 x | | | | | | |
| Income Inequality | 0.09*** | 0.09*** | 0.10*** | 0.10*** | 0.10*** | 0.10*** |
| | (.027) | (.026) | (.026) | (.026) | (.026) | (.026) |
| Local Elite Control | −0.11** | −0.11** | −0.11** | −0.10* | −0.10* | −0.10* |
| | (.054) | (.054) | (.054) | (.056) | (.055) | (.054) |
| Cereal Suitability | 0.10*** | 0.09*** | 0.10*** | 0.10*** | 0.11*** | 0.10*** |
| | (.032) | (.031) | (.033) | (.030) | (.031) | (.030) |
| Distance: | | | | | | |
| Town> 25$k$ | −0.01 | | | | | |
| | (.030) | | | | | |
| Town> 50$k$ | | −0.04 | | | | |
| | | (.029) | | | | |
| Town> 100$k$ | | | 0.01 | | | |
| | | | (.028) | | | |
| 1881–91 Change in: | | | | | | |
| % Working in Agriculture | | | | 0.03 | | |
| | | | | (.025) | | |
| Population | | | | | −0.03 | |
| | | | | | (.025) | |
| % Pop Age > 64 | | | | | | 0.04* |
| | | | | | | (.022) |
| Controls | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y |
| No. Observations | 4,368 | 4,368 | 4,368 | 4,284 | 4,368 | 4,368 |
| No. PLUs | 208 | 208 | 208 | 204 | 208 | 208 |

Notes: Each specification controls for an interaction between post-1894 and a union characteristic reflecting the ease of out-migration. In first three columns the interaction uses the distance from the nearest town of different sizes. In the following columns the interaction uses the change in the workforce in agriculture, population, and the % of the population aged over 64. "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level.

Table D.8: Robustness to allowing complex time trends.

| | DV=Relief Expenditure per Capita (Standardized) | | | | | |
|---|---|---|---|---|---|---|
| Post 1894 x | | | | | | |
| Income Inequality | 0.09*** | 0.07*** | 0.09*** | 0.09*** | 0.11*** | 0.10*** |
| | (.025) | (.026) | (.025) | (.025) | (.026) | (.026) |
| Local Elite Control | −0.11** | −0.11** | −0.10* | −0.11** | −0.11** | −0.11** |
| | (.053) | (.053) | (.054) | (.053) | (.053) | (.054) |
| Cereal Suitability | 0.07** | 0.08*** | 0.10*** | 0.10*** | 0.08*** | 0.10*** |
| | (.035) | (.030) | (.031) | (.030) | (.030) | (.029) |
| Quartic Time Trend | Distance London | Tax Base per Acre 1884 | % Δ Tax Base 1874–1884 | 1869 Wage | Density 1881 | Seasonality 1881–1890 |
| Controls | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y |
| No. Observations | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 |
| No. PLUs | 208 | 208 | 208 | 208 | 208 | 208 |

Notes: "Quartic time trend" indicates a quartic time trend interacted with pre-reform levels of variables. "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level.

Table D.9: Robustness to Alternative Samples.

| | DV=Relief Expenditure per Capita (Standardized) | | | | | |
|---|---|---|---|---|---|---|
| Post 1894 x | | | | | | |
| Income Inequality | $0.10^{**}$ | $0.09^{***}$ | $0.07^{***}$ | $0.13^{***}$ | $0.10^{***}$ | |
| | (.039) | (.026) | (.027) | (.029) | (.028) | |
| Local Elite Control | $-0.11^{*}$ | $-0.07$ | $-0.12^{**}$ | $-0.11^{*}$ | $-0.12^{**}$ | $-0.12^{***}$ |
| | (.060) | (.053) | (.055) | (.060) | (.057) | (.055) |
| Cereal Suitability | $0.10^{***}$ | $0.08^{***}$ | $0.11^{***}$ | $0.08^{***}$ | $0.08^{**}$ | $0.12^{*}$ |
| | (.032) | (.027) | (.031) | (.032) | (.032) | (.030) |
| Sample | Drop Extreme Inequality | Drop Extreme Spending | Drop Wales | Drop West Midlands | Median In Farming | No Income Inequality |
| Controls | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y |
| No. Observations | 3,948 | 3,948 | 3,906 | 3,780 | 3,822 | 4,725 |
| No. PLUs | 188 | 205 | 186 | 180 | 182 | 225 |

Notes: The first column excludes unions in the top or bottom 5% of income inequality. The second column excludes unions the top or bottom 5% of per capita spending in each year. The next two columns exclude the least unequal (Wales) and most unequal (West Midlands) Census divisions. The fifth specification limits the sample to unions where the median household status was in a farming occupation. The final column includes unions for which income inequality is missing. "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. $^{***}$, $^{**}$, $^{*}$ denote statistical significance at the 1%, 5%, and 10% level.

Table D.10: Controlling for the size and growth of nearest urban area does not affect the main results.

| | DV=Relief Expenditure per Capita (Standardized) | | | | | |
|---|---|---|---|---|---|---|
| | Towns $> 25k$ | | Towns $> 50k$ | | Towns $> 100k$ | |
| Post 1894 x | | | | | | |
| Income Inequality | $0.09^{***}$ | $0.09^{***}$ | $0.09^{***}$ | $0.09^{***}$ | $0.10^{***}$ | $0.08^{***}$ |
| | (.026) | (.025) | (.026) | (.026) | (.026) | (.026) |
| Local Elite Control | $-0.11^{***}$ | $-0.10^{*}$ | $-0.10^{***}$ | $-0.10^{*}$ | $-0.11^{***}$ | $-0.11^{**}$ |
| | (.056) | (.054) | (.054) | (.052) | (.054) | (.053) |
| Cereal Suitability | $0.10^{**}$ | $0.09^{***}$ | $0.10^{*}$ | $0.10^{***}$ | $0.10^{*}$ | $0.10^{***}$ |
| | (.031) | (.030) | (.030) | (.030) | (.030) | (.029) |
| Town Characteristics: | | | | | | |
| (Log) Population | $-0.21^{*}$ | $-0.20^{*}$ | $-0.05$ | $-0.06$ | $-0.15$ | 0.11 |
| | (.108) | (.111) | (.099) | (.097) | (.194) | (.189) |
| Population Growth | $-0.06^{**}$ | $-0.06^{**}$ | $-0.03$ | $-0.03$ | 0.00 | $0.05^{*}$ |
| | (.023) | (.024) | (.024) | (.023) | (.026) | (.026) |
| Tax Base Per Capita | | $-0.06$ | | $-0.03$ | | $0.05^{***}$ |
| | | (.055) | | (.042) | | (.061) |
| Controls | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y |
| No. Observations | 4,363 | 4,156 | 4,343 | 4,140 | 4,368 | 4,167 |
| No. PLUs | 208 | 208 | 208 | 208 | 208 | 208 |

Notes: Each specification controls for the characteristics of the closest town to the poor law union. The first two columns use the closest town with over 25,000 inhabitants in 1891, the third and fourth columns use the closest town with over 50,000 inhabitants, and the final two columns use the closest town with over 100,000. The series of town tax base ends in 1904, and so there are fewer observations in these specifications. "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level.

Table D.11: Results are robust to interactions between post 1894 and nearest town.

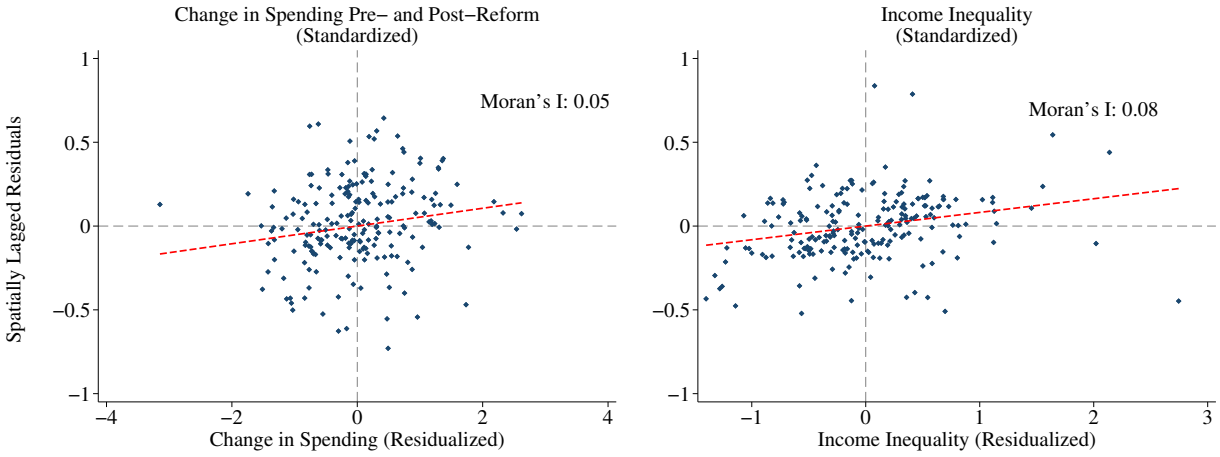
| | DV=Relief Expenditure per Capita (Standardized) | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Closest Town $> 25k$ | | | | Closest Town $> 50k$ | | | | Closest Town $> 100k$ | | | |
| Post 1894 x | | | | | | | | | | | | |
| Income Inequality | 0.10*** | 0.09*** | 0.09*** | 0.10*** | 0.10*** | 0.09*** | 0.10*** | 0.10*** | 0.09*** | 0.10*** | 0.10*** | 0.10*** |
| | (.026) | (.027) | (.026) | (.026) | (.026) | (.026) | (.026) | (.026) | (.027) | (.026) | (.026) | (.026) |
| Local Elite Control | −0.11*** | −0.11*** | −0.11*** | −0.11*** | −0.11*** | −0.11*** | −0.11*** | −0.11*** | −0.11*** | −0.11*** | −0.11*** | −0.11*** |
| | (.054) | (.054) | (.055) | (.054) | (.055) | (.054) | (.054) | (.055) | (.054) | (.054) | (.054) | (.054) |
| Cereal Suitability | 0.10** | 0.10** | 0.09** | 0.10** | 0.10** | 0.09** | 0.10** | 0.10* | 0.10** | 0.10** | 0.11** | 0.10** |
| | (.030) | (.032) | (.030) | (.030) | (.029) | (.031) | (.030) | (.030) | (.029) | (.033) | (.030) | (.030) |
| Post 1894 x Town: | | | | | | | | | | | | |
| Population | −0.01 | | | | −0.04 | | | | 0.05* | | | |
| | (.025) | | | | (.027) | | | | (.030) | | | |
| Distance | | −0.01 | | | | −0.04 | | | | 0.01 | | |
| | | (.031) | | | | (.029) | | | | (.029) | | |
| Tax Base p.c. | | | 0.04 | | | | 0.01 | | | | 0.06* | |
| | | | (.027) | | | | (.027) | | | | (.030) | |
| Δ Tax Base | | | | −0.00 | | | | −0.02 | | | | 0.01 |
| p.c. 1885–93 | | | | (.027) | | | | (.023) | | | | (.025) |
| Controls | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| No. Observations | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 |
| No. PLUs | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 |

Notes: Each specification includes an interaction between post-1894 and a characteristic of the nearest town. The first four columns use the closest town with over 25,000 inhabitants in 1891, the next four columns use the closest town with over 50,000 inhabitants, and the final four columns use the closest town with over 100,000. "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level.

## D.4 Tests for Spatial Autocorrelation

In this appendix I investigate potential spatial autocorrelation. First, I examine whether spatial autocorrelation is present in the main variables of interest—the change in spending between the pre-reform (1885–1894) and post-reform (1895-1905) period and in the income inequality measure. Specifically, I regress each variable on the controls in specification (4) in Table 2 and then estimate Moran's I test statistic. Figure D.1 plots these residuals against the spatially-lagged residuals (i.e., the residuals from neighboring locations). As we can see, there is only limited evidence of spatial autocorrelation—the two series are weakly correlated in both cases, with Moran's I statistics of less than 0.1 compared to the theoretical maximum of 1.

Figure D.1: There is little evidence of spatial autocorrelation in either the growth of spending on relief or in income inequality.

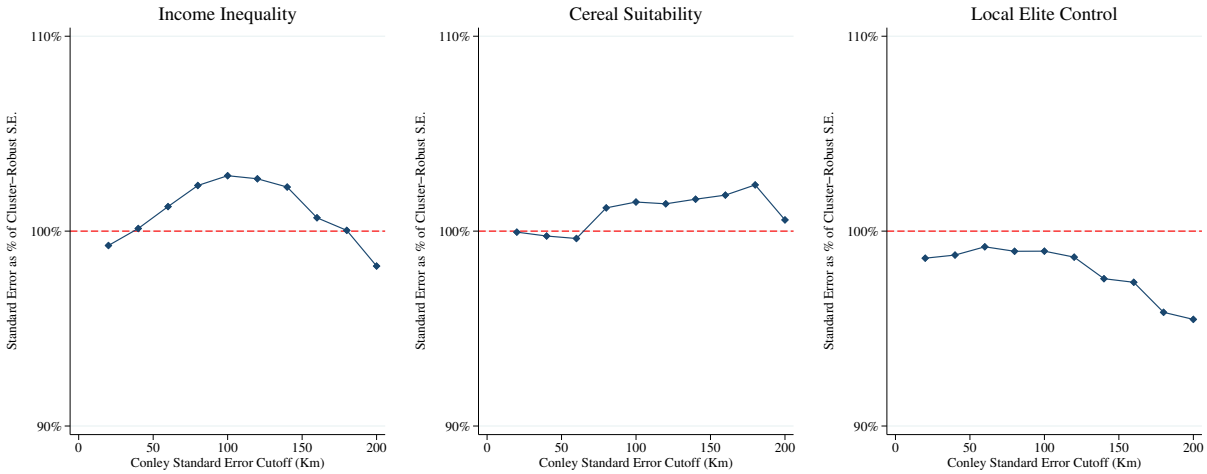



Notes: Each panel of the figure plots residuals against their spatially-lagged residuals (i.e., the residuals from neighboring locations). Red dashed lines represent linear fits. The dependent variable in the left-hand panel is the change in average spending between the pre-reform (1885-1894) and post-reform (1895-1905) period, and in the right-hand panel it is the primary measure of income inequality. In each case, I residualize the variable using the set of controls from Specification 4 in Table 2, measured in 1891. The Moran's I statistic is a measure of spatial autocorrelation, ranging from -1 to 1. Spatial lags are estimated using a 100km cut-off and a power weighting matrix with distance decay parameter of 2.

Figure D.2 investigates the impact of correcting the standard errors in the regression results for spatial autocorrelation. In particular, I use the approach of Colella et al. (2019) who allow for both arbitrary spatial autocorrelation (within a given bandwidth) and temporal autocorrelation. Following their recommendation, I examine the effect of using various bandwidths, and then implement the bandwidth which produces the most conservative result (largest standard error). Here we see that, regardless of the bandwidth chosen, the spatially-corrected standard errors are similar to (within 10%) unadjusted cluster-robust

standard errors. The difference is largest using a cut-off of around 100km, and so I use that bandwidth as the baseline in the main text.

Figure D.2: The magnitude of standard errors is similar after correcting for arbitrary spatial autocorrelation..




Notes: Each panel of the figure plots the standard error relating to the relevant variable from specification (11) in Table 3, allowing for arbitrary spatial autocorrelation within different bandwidths, scaled as a percentage of the standard errors without spatial correction. That is, the red dotted line represents the standard errors presented in specification (11) in Table 3. Standard errors are estimated using the approach of Colella et al. (2019), which allows for both spatial and temporal serial correlation.

## D.5 Disaggregating Effects on Pauper Numbers

Table D.12 investigates the effects of the reform for different groups of paupers. In particular, I separate paupers according to whether they were adult males, adult females, or children (no sex breakdown is provided for the latter), and whether they were classified as able-bodied or non-able-bodied. Some caution is required in placing too much emphasis on the categorization of able-bodied and non-able-bodied, as this was manipulable—it was easier to grant outrelief to those classified as non-able bodied. However, this provides some insight into the attitudes underpinning the provision of relief.

For income inequality, we see a positive effect on outdoor non-able-bodied paupers in all three categories, as well as outdoor able-bodied adult women. There is relative little evidence of any effect on indoor pauper numbers (although we should be careful as the difference between these coefficients and those on non-abled-paupers in the same category are not always statistically significantly different). Interestingly, elite control is associated with an increase in the share of indoor able-bodied men (and a decrease in non-able-bodied

men), which could reflect a harsher policy in forcing paupers into the workhouse.

Overall, these patterns are broadly consistent with those in the main text, in showing that the main effect of the reform was to expand outdoor relief. They also add further suggestive evidence that the main effect was to increase the support provided to dependent groups—those non-able-bodied (often the elderly), or to able-bodied women—rather than to offer unemployment insurance to younger men.

Table D.12: Effect of Reform on Different Pauper Categories.

| | DV = Paupers per Capita (Standardized) | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Adult Men | | | | Adult Women | | | | Children | | | |
| | Able-Bodied | | Not Able-Bodied | | Able-Bodied | | Not Able-Bodied | | Able-Bodied | | Not Able-Bodied | |
| | Indoor | Outdoor | Indoor | Outdoor | Indoor | Outdoor | Indoor | Outdoor | Indoor | Outdoor | Indoor | Outdoor |
| Post 1894 x | | | | | | | | | | | | |
| Income Inequality | 0.06 | 0.03 | 0.03 | 0.07** | −0.02 | 0.12*** | 0.04 | 0.06*** | 0.02 | −0.02 | 0.04 | 0.13*** |
| | (.044) | (.039) | (.030) | (.027) | (.045) | (.043) | (.037) | (.021) | (.052) | (.060) | (.053) | (.048) |
| Local Elite Control | 0.13 | 0.05 | −0.14** | −0.05 | 0.07 | 0.04 | −0.10 | 0.04 | −0.10 | 0.13 | 0.04 | 0.07 |
| | (.081) | (.079) | (.064) | (.053) | (.096) | (.079) | (.073) | (.051) | (.102) | (.103) | (.095) | (.093) |
| Cereal Suitability | −0.08 | −0.05 | 0.06** | 0.05* | 0.00 | −0.04 | 0.04 | 0.05* | −0.06 | 0.02 | −0.06 | 0.04 |
| | (.049) | (.051) | (.033) | (.027) | (.052) | (.042) | (.036) | (.027) | (.054) | (.063) | (.051) | (.051) |
| Controls | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| PLU Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| No. Observations | 4,366 | 4,368 | 4,368 | 4,368 | 4,366 | 4,366 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 | 4,368 |
| No. PLUs | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 | 208 |

Notes: The denominator for male (female) adult paupers is the total male (female) population aged over 64. For children, the denominator is total population aged under 15. "Controls" include the variables included in specification (4) of Table 2. Standard errors are corrected for temporal and spatial autocorrelation within 100km using the approach of Colella et al. (2019) and are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level.

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