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

Jonathan Chapman*

July 31, 2023

Abstract

This paper tests whether higher income inequality leads to higher government redistribution following democratization. An 1894 democratic reform to councils that provided social insurance in Britain is used as the treatment event in a difference-in-difference analysis. The reform removed institutional features—a graduated franchise, property qualifications, the absence of a secret ballot, and the participation of unelected magistrates—that helped landowners seize control of spending on poor relief after the 1832 Great Reform Act. The results support the Meltzer-Richard hypothesis, with a higher mean-median income ratio leading to more redistribution following the democratic reform. De facto elite power limited the effects of democratization, with areas where landed elites held local political power experiencing lower growth in spending. However, there is limited evidence that grievances between social classes were an important determinant of post-democratization expenditure. These findings provide support to theories of democratization focusing on demands for redistribution.

Keywords: Democratization, Inequality, Redistribution, Meltzer-Richard, De Facto Power

*Faculty of Economics, University of Bologna. I thank Robert Allen, Brian Beach, Dan Bogart, Marc Goñi, Philip Hoffman, Jeffrey Jensen, Matthew Kovach, Samantha Myers, Cormac O'Grada, Kevin O'Rourke, Didac Queralt, Jean-Laurent Rosenthal, Richard Trainor, and Patrick Wallis for helpful comments. I also thank 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, mean elites are both more capable of blocking post-democratization reforms, and more incentivized to do so (e.g. Acemoglu and Robinson, 2006b). 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 only source of social welfare before the modern welfare state—within a common legal framework. 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 identity of the median voter led to greater increases in spending in areas of high income inequality, consistent with the Meltzer-Richard hypothesis. At the

[1] Observational studies have found a mixture of positive (e.g., Milanovic, 2000; Boustan et al., 2013) and negative (e.g., Islam, Madsen, and Doucouliagos, 2018; Perotti, 1996) relationships between inequality and government spending across and within countries—see De Mello and Tiongson (2006, Table 1) for a summary. Empirical evidence regarding the link between inequality and democratization is also mixed, perhaps reflecting these endogoneity difficulties: several papers have found that inequality hinders democratization (e.g., Ansell and Samuels, 2010; Engerman and Sokoloff, 2005; Ziblatt, 2008, 2009), but a number of cross-national studies have found no relationship. See Haggard and Kaufman (2012) for a review.

same time, I find that post-democratization spending was also affected by both the need for poor relief (positively) and elite de facto power (negatively). The generosity of government redistribution was thus shaped by a variety of factors as well as 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 income inequality. A significant literature has argued that those conditions may rarely hold in practice, as the assumptions of the Meltzer-Richard model fail to match the reality of modern-day political institutions (e.g., Moene and Wallerstein, 2001; Iversen and Goplerud, 2018).[2] Yet those institutions are themselves endogenous, and likely shaped by elites, who may act to head off redistribution through either resisting democratization (e.g. Boix, 2003), shaping a society's constitution (Acemoglu, Egorov, and Sonin, 2012), or investing in de facto power (e.g., Acemoglu and Robinson, 2006a). 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 historical context studied in this paper provides a near-ideal environment to test the Meltzer-Richard model. Focusing on variation across rural areas within a single country allows comparison of observational units that were similar culturally, economically, and institutionally—isolating the effect of inequality from other confounding factors. Further, the government structure studied here closely meets the model's theoretical assumptions, with government spending limited to redistribution (social insurance) and town councils restricted to raising funds using a tax that was approximately proportional to income. The setting thus offers many of the advantages of testing the Meltzer-Richard model in the laboratory (Agranov and Palfrey, 2015), while accounting for the complexities of a real world setting.

To test the effect of inequality on government expenditure post-democratization I im-

[2]Harms and Zink (2003) provide an early survey of this theoretical literature.

plement a difference-in-difference analysis using a new annual dataset of poor law union spending between 1884 and 1904. The 1894 democratic reform then serves as a treatment event and the degree of pre-existing income inequality as the treatment status. I focus on rural poor law unions (N=208), minimizing heterogeneity across the sample, and allowing me to use data on the agricultural wage as a proxy for median income. The rich dataset allows me to control for both time-invariant (union fixed effects) and time-varying (demographic characteristics, financial constraints) factors that may affect the demand for poor relief and hence confound the estimated effect of inequality. Further, I can account for alternative dimensions of rural inequality (Ardanaz and Mares, 2014)—need for social insurance, class conflict, and elite de facto power—providing further evidence that the relationship between income inequality and government spending is causal. Consistent with the Meltzer-Richard hypothesis, pre-existing inequality—the estimated mean:median income ratio—moderated the effects of democratic reform on welfare spending: poor relief expenditure increased by more in more unequal districts.

Two additional findings emerge from the empirical analysis. First, the results provide a nuanced understanding of how elites can limit the effect of democratization on government spending. Post-reform spending was lower in areas in which local elites held political power pre-reform, consistent with the hypothesis that they held de facto power even after 1894 (Acemoglu and Robinson, 2006a). Further investigation suggests that elites were able to reduce spending despite democratization by abandoning austerity measures in publicly-salient policy areas—who was granted poor relief—and instead reducing less observable areas of expenditure. Such an approach was possible in an institutional environment in which the poor could not afford to sit on boards and scrutinize policy, despite a *de jure* right to do so. Second, there is limited evidence that class conflict moderated the effects of democratic reform, once differences in the type of agricultural production are accounted for. I find some evidence that democratic reform increased spending by more in areas with high inequality

in land ownership, a high share of rural laborers, and previous political conflicts between classes. However, these effects disappear after accounting for differences in agricultural production. These results suggest that demand for redistribution was driven by economic needs rather than social division.

Several studies at both national- and local-level have found that democratization leads to greater government expenditure but few have investigated whether inequality moderates the effect.[3] These papers have generally found a positive relationship between democratization and government spending and tax revenue, consistent with the idea that newly enfranchised voters push for redistributive expenditure (e.g., Brown and Hunter, 2004; Aidt, Dutta, and Loukoianova, 2006; Aidt and Jensen, 2013).[4] The focus on redistributive expenditure in 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 (e.g., Aidt, Daunton, and Dutta, 2010; Chapman, 2018, 2023; Cascio and Washington, 2014; Fujiwara, 2015; Husted and Kenny, 1997). 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 moderating effect 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 ap-

[3]See Acemoglu et al. (2015, Section 21.3) for a detailed review of the literature examining the relationships between democracy, redistribution, and inequality.

[4]The few studies examining the relationship between democratization and tax incidence have been less supportive of the link between democratization and redistribution (see Aidt and Jensen, 2009a,b; Scheve and Stasavage, 2010, 2012). The focus of this paper is the effect of democratization on expenditure because, as discussed in Section 3, Poor Law Guardians could not affect the structure of taxation.

pear 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.

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 Theoretical Framework

The rich dataset used in this paper allows me to disentangle multiple plausible theoretical pathways through which rural inequality may affect redistribution following democratic reform. This section outlines four theoretical hypotheses to be tested in the empirical analysis. Section 3 then describes the historical setting, before Section 4 explains how the hypotheses are operationalized.

Income Inequality The primary measure of inequality used in this paper relates to the classic model of Meltzer and Richard (1981), in which the level of government spending is determined by the preference of the voter with the median income. Poorer voters desire more redistribution, and hence a democratic reform that enfranchises the poor increases government redistribution. Specifically, the level of government spending is determined by the ratio of the mean income in society to the income of the median voter. The model assumes a tax rate that is proportional to income and that government spending is only on redistribution—making it difficult to test the model using cross-country data. However, as we will see in the following section, these theoretical assumptions are closely matched in the historic setting analyzed in this paper.

H1 (Meltzer-Richard): Democratization leads to higher spending on poor relief. The effect is moderated by income inequality, as captured by the mean:median income ratio.

Appendix B.1 presents a simple formal model deriving this prediction when accounting for the specifics of poor law institutions.

Need for Social Insurance Citizens' demands for social insurance may be motivated not by income inequality, but rather by the presence of demographic groups that are highly likely to require assistance from social insurance. In contrast to redistributive transfers, citizens' expected benefit from social insurance varies according characteristics such as age (in the case of pensions) or occupation (income support). A higher proportion of citizens in such categories may mean that the median voter is either more likely to benefit from more generous provision themselves or cares about vulnerable citizens for altruistic reasons. Alternatively, such groups may be able to organize and lobby for more generous provision after reform.

H2 (Need for social insurance): Democratization leads to larger increases in expenditure where a higher proportion of citizens are likely to benefit from social insurance.

Class Conflict Rural areas dominated by agriculture are typically characterized by strongly hierarchical relationships with large landowners at the top and landless laborers at the bottom. In such a setting, economic dependence on landlords may lead to resentment amongst such laborers, generating demands for redistribution (Domènech and Sánchez-Cuenca, 2022). Moreover, such resentment may be stronger in areas with more salient inequality between social classes, and in areas where a social contract with landowners is perceived to have been broken (Scott, 1977). Democratization provides the poor with the opportunity to redress such grievances, with higher taxes a channel through which poor can obtain redress from landowners.

H3 (Class Conflict): Democratization leads to larger increases in spending in areas with greater inequalities between social classes, and in areas with previous political conflict between laborers and landed elites.

Elite De Facto Power While the above three arguments have focused on the role of ordinary citizens, elites may also shape responses to democratization, through holding *de facto* power even after their *de jure* power has been removed (Acemoglu and Robinson, 2006a). For instance, large landlords may be able to compel their tenants to vote as they wish or, alternatively, elites may hold significant local lobbying power through networks of influence. In either case, they may be able to limit—or even prevent entirely—any effects of democratic reform.

H4 (De Facto Elite Power): Democratization leads to smaller increases in spending where elites have more informal control over decisions through political or economic influence.

After examining the effects of inequality on total spending, I examine heterogeneous effects on different dimensions of relief, distinguishing between aspects of policy that were publicly observable—pauper maintenance and the number of paupers—and those that were not—fixed costs of poor relief. It may be easier for elites to cut types of expenditure that are

harder to scrutinize either because the public are unaware that spending has been reduced or because a lack of salience makes it more difficult to organize opposition. If so, we would anticipate a more pronounced negative effect of elite de facto power on less salient spending types.

3 Institutional Context

To test the theoretical hypotheses I exploit the fact that social insurance in nineteenth-century England was provided by autonomous, locally-elected councils. Poor relief was the main form of social insurance in Britain for several centuries, providing support to a wide variety of destitute citizens. This section outlines the key features of this historical setting, starting with a general overview of the working of the New Poor Law. The second subsection discusses the recipients of poor relief, and argues that the median voter would likely benefit from higher spending. The third subsection discusses the politics of the poor law and the democratic reform exploited in the empirical analysis.

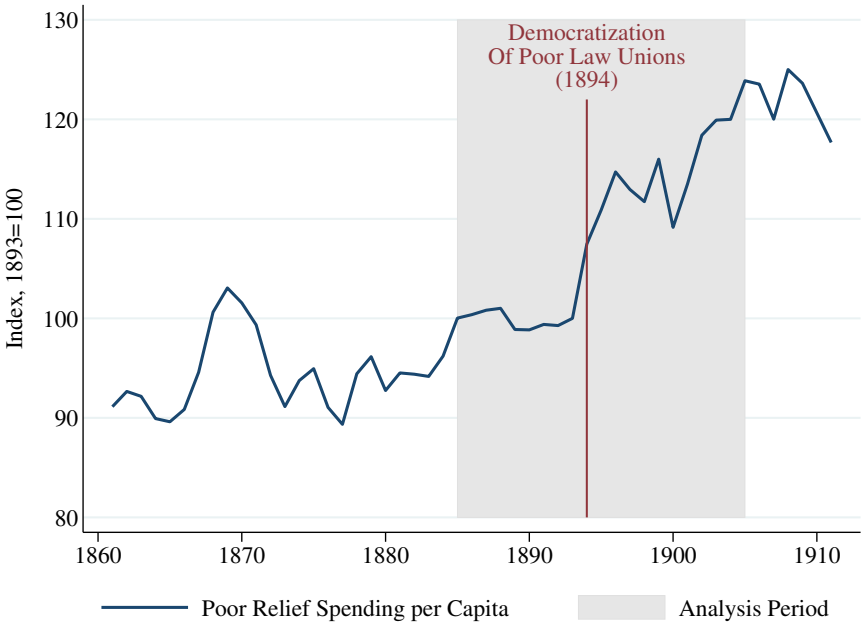
3.1 The New Poor Law

Under the provisions of the infamous 1834 "New Poor Law", poor relief was provided by a set of autonomous local authorities—"Boards of Guardians of the Poor"— who controlled the level and type of relief in their "Poor Law Union", of which there were approximately 600 in England and Wales. The main task of these councils was to provide social insurance, with funds raised through local taxes. While the guardians held authority over spending on poor relief, they did not have responsibility for spending on infrastructure, local public services (which were provided by town councils), or education (which was provided by local school boards).[5]

[5]The guardians sometimes held other roles that involved spending on public goods—see Appendix C.2.

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, 2006, p.209), 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. A more formal examination of possible pre-trends is presented in Section 6.3.[6]

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.

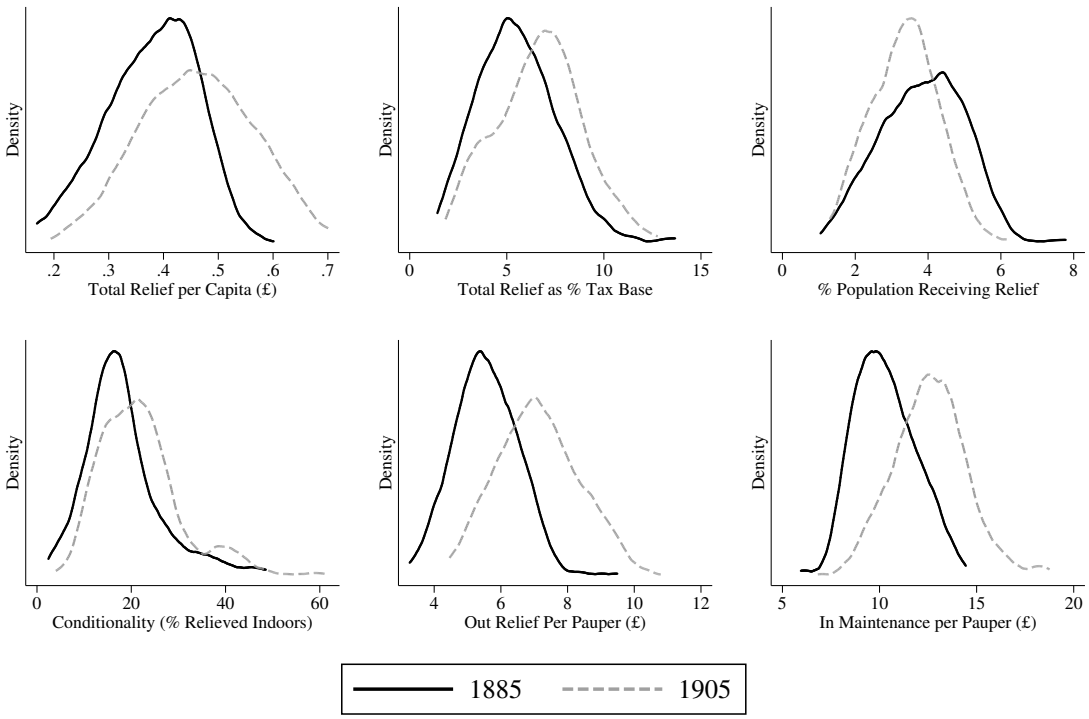
The most contentious aspect of the New Poor Law was the attempt to force recipients

[6]Table A.2 displays correlations between all eight inequality measures and the pre-reform trend in poor relief spending before 1894.

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 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).

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



Note: 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.

Importantly, 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 linear to income, as assumed in Meltzer and Richard (1981) and many other political economy models.

3.2 Recipients of Poor Relief

The poor law served as the main form of social insurance in Britain, 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%.[7] Relatively few paupers were able to work: one-third of paupers were children, and most adult paupers were classified as "not-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 old aged—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,

[7]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.3 includes a detailed classification of paupers.

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.

Reliance on poor relief was particularly large 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). Not only were wages low, the seasonal nature of agricultural 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 higher spending on poor relief.

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.[8] First, elected guardians were supplemented by unelected *ex officio* guardians, including all magistrates—typically local landowners—residing in the poor law union. Second, significant 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 poor 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—see House of Commons (1878b, e.g., paras 864–866, 5076). This undemocratic electoral system remained in place for

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

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—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 similar 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". 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, including statements provided by witnesses relating to thirty unions, combined with historical literature.[9] 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 thus consequently varied considerably across the country, reflecting both the needs of local communities and social relations between landlord and tenant.

[9]Appendix C.1 provides further discussion of this evidence. A table summarizing the evidence for each of the thirty unions individually is available upon request

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, p101). 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 simply 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, p105). Guardians continued to be elected at parish-level, forming a further barrier to broad political movements. Consequently, extreme 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 salient types of 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. Four strategies were introduced to do so: prosecuting the adult children of elderly paupers for the costs of relief, refusing relief to those in receipt of charity, reducing the number of relieving officers so that guardians dealt with claims directly, and referring elderly pauper applications to private charity. After the Third Reform Act, rural areas became increasingly political, leading to the 1893 foundation of a pressure group seeking to reintroduce 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 implement upgrades to the 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. Full details and summary statistics are provided in Appendix A.

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 correspond to a setting with a small landed elite and a homogenous mass of agricultural workers, as in the model of Acemoglu and Robinson (2000). Further, as discussed in the previous section, we can be confident that the median voter benefited from more generous poor relief in these areas. Practically, the main measure of inequality (the mean-median ratio) relies on the agricultural wage, and so is only appropriate in rural areas. In addition, changes in poor relief expenditure within urban areas could capture the differing pace of urbanization in different districts since the nature of poor relief varied considerably between town and country. By restricting the sample to only those unions that were rural in character throughout the period, I avoid this complication.

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 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 population in 1881. Seventeen unions are excluded as the income inequality measure cannot be calculated due to missing wage data. 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. This data is then combined with existing data from the census and geographic information.

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 this 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 chairman of each union was elected or *ex officio* pre-reform.

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 percentage of land owned by these elites.

Other Union Characteristics: Demographic and occupational characteristics are constructed using census data. The suitability of land for cereal agriculture in each union is 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 is collected from the *Annual Agricultural Returns*.

5 Empirical Approach

This section discusses the empirical framework used to test whether the effect of democratic reform on expenditure varied according to the level of inequality in a poor law union. I then discuss the inequality measures used to operationalize the theoretical hypotheses from Section 2.

5.1 Empirical Specification

To test the effects of the democratization of poor law unions as part of the 1894 Local Government Act, I use a difference-in-difference approach, where the "treatment" is the degree of inequality in the union. Critically, democratization was imposed on poor law unions by the national government—as discussed in Section 3.3. This approach thus avoids the endogeneity issues inherent to national-level democratizations, the timing of which is clearly dependent on elite decision-making. 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:

$$y_{it} = \beta inequality\_\text{x}\_post1894 + \gamma_0 X'_{it} + T_t + \alpha_i + \epsilon_{it} \tag{1}$$

where i indexes poor law unions and t indexes years. y is the total poor relief spending per capita. X is a vector of control variables, T represents year fixed effects, and α represents poor law union fixed effects. ϵ 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. The key variable of interest is pre-reform *inequality*, with the interaction term testing whether the relationship between inequality and poor law spending changed after the 1894 reforms. 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.

Underpinning this approach is the "conditional parallel trends" identifying assumption: conditional on the control variables, the average spending per capita would have followed parallel paths in the absence of the democratic reform. This assumption allows for the

fact that inequality could be correlated with observed covariates that themselves may affect the level of spending. I include a vector of "crop controls" capturing the usage of land at county-level to tackle issues about agricultural type discussed below. To capture other potential sources of time-varying heterogeneity in the demand for poor relief I control for "demographic controls"—population density, population, the percentage of the population in different age categories and the decadal standard deviation in the number of paupers per capita—and "financial controls": the tax base per acre and the percentage of revenue from taxation. I also implement a range of robustness tests allowing for differing time trends according to the levels of covariates pre-reform.

The causal interpretation of the results would be threatened if the broader changes wrought by the LGA affected poor relief through channels other than the democratization of boards of guardians. The creation of parish councils is of little concern as they had limited spending or tax-raising powers, and failed to inspire voter interest (Keith-Lucas, 1952, p.42). However, the creation of rural district councils is more troubling because guardians also sat as representatives on these councils. While the bodies were organizationally distinct, this overlap means that guardians could plausibly have been elected based on considerations other than poor relief. Further, although guardians held essentially the same role before 1894, the Local Government Act expanded the range of spending controlled by these bodies, meaning the guardians' role as rural district councilors changed alongside the reform to the electoral system. Appendix C.2 discusses this potential issue in depth, showing 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 into 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 would likely have

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 in any case. It is possible, however, that guardians moderated their policy to increase the chances of winning elections post-reform. Such a response would mean that the effects of reform are underestimated.

5.2 Operationalization of Dimensions of Inequality

I construct several variables capturing different dimensions of inequality, and use them to test the theoretical hypotheses—see Table 1 for a summary. I discuss each of the four hypotheses, and associated variables, in turn.[10] However, the cross-cutting nature of inequality means that the classification of each variable to a single hypothesis is not always clear cut. For example, land inequality may generate resentment (H3), but also mean elites have more de facto power (H4). Moreover, even if conceptually distinct, different dimensions of inequality may be highly correlated making it difficult to identify the causal channel through which inequality affects spending.[11] Disentangling the different hypotheses thus requires considering *in totum*, rather than testing each hypothesis sequentially.

H1: Income Inequality The primary measure of inequality is an approximation of the *mean-median income ratio*, derived from household spending on property. This measure is theoretically-motivated, following Meltzer and Richard (1981), and hence directly corresponds to *H1*. In Meltzer and Richard (1981), taxes are proportional to income and consequently the theoretical results relate to the ratio of the mean to the median income. In the current setting, in contrast, taxes were proportional to the rental value occupied property—the value at which a property could be rented. As such—shown formally in Appendix B.1—the theoretical predictions relate to the ratio of the mean to the median

[10] Appendix D.2 presents analysis with alternative definitions of each measure.
[11] Appendix Table A.2 displays correlations between the inequality measures.

Table 1: Hypothesized moderating effects on post-democratization government spending.

Hypothesis	Measure	Operationalization	Predicted Effect
H1 (Meltzer-Richard)	H1: Income Inequality	Mean:Median Ratio	↑
H2 (Need for Social Insurance)	H2a: Need for Income Support	Cereal Suitability	↑
	H2b: Need to Support Old-aged	% Population Aged> 64 1891	↑
H3 (Class Conflict)	H3a: Land Inequality	% Land Owned by Great Landowners	↑
	H3b: Agrarian Inequality (poor)	% Male laborers in agricultural workforce	↑
	H3c: Agrarian Grievance	Strength of Crusade vs outrelief (% fall in outdoor paupers 1866–1885)	↑
H4 (Elite De Facto Power)	H4a: Aristocratic Elite	Presence of peer in union	↓
	H4b: Local Elite Control	Unelected chairman of guardians 1893	↓

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. With this assumption, the proportional tax rate allows a direct test of the Meltzer-Richard hypothesis in this historical setting. Further, we can consider the measure in (2) as capturing income inequality if we also assume spending on property increases with income (i.e., it is a normal good). See Appendix B.1 for further discussion and historical evidence justifying these assumptions.

H2: Need for Social Insurance Two variables reflect major sources of demand for poor relief. First, to capture the need for poor relief as income support I use the cereal suitability of soil within the union. 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 laborers outside of harvest season relative to areas dominated by pastoral agriculture. Second, the % of the population over 64 identifies areas with a high number of old-aged citizens likely to rely on poor relief prior to the advent of a national pension system.

H3: Class Conflict I use two approaches to capture sources of class conflict in rural areas. The first approach uses two variables capturing the structure of social relationships. To capture the importance of landowners I use land inequality, estimated as the share of land owned by the very wealthy in each union. Second, following Domènech and Sánchez-Cuenca (2022), I measure agrarian inequality using the share of male rural laborers amongst the population employed in agriculture.

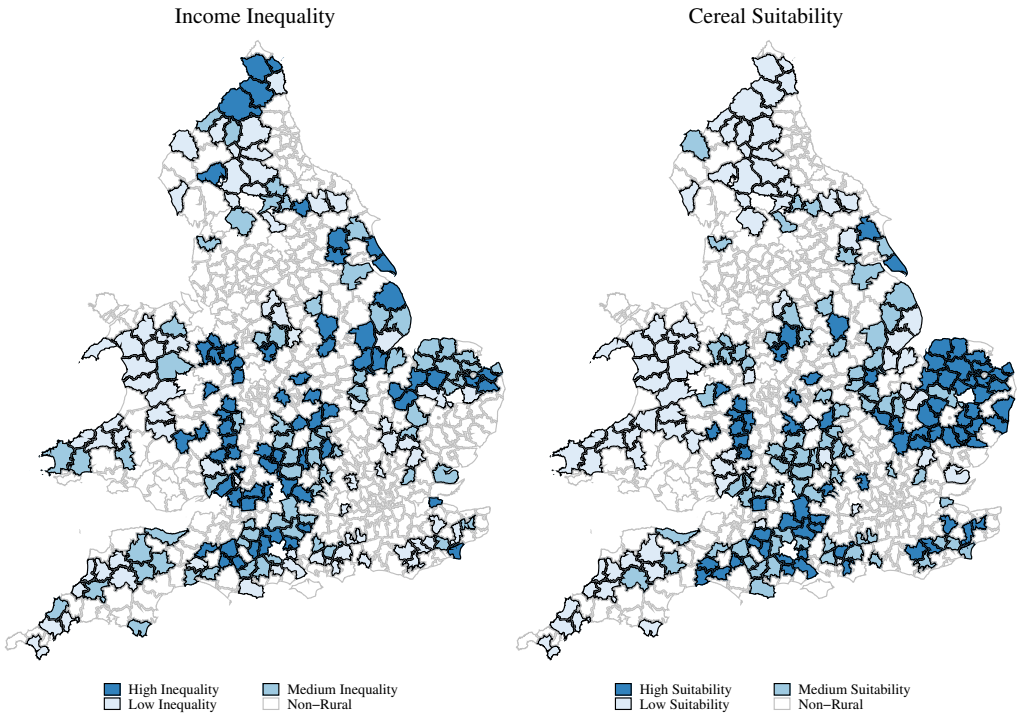
The second approach directly measures a source of grievances between classes—the harshness of policy during the "Crusade Against Outrelief". A harsher crusade could generate resentment as the poor felt the landed elites had reneged on their paternalistic responsibilities (Hurren, 2015). I define harshness as the percentage fall in the number of outdoor paupers between pre-crusade (1866–71) and post-crusade (1881–86) years.

H4: Elite De Facto Power I measure elites' political power in two ways. National political influence is captured by a binary variable capturing whether a peer of the realm had a seat within the union. Local political control is identified by whether the chairman of the board of guardians pre-reform was *ex officio*—i.e., an unelected member of the local gentry. The chairman of each board was chosen by the votes of the board, and so this measure identifies whether the local elite could garner support from other guardians.

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 we can see in Figure 3. There is clear regional clustering according to the level of income inequality, with more unequal areas generally in areas used to grow higher value agricultural crops: income inequality is positively correlated with the 1885 percentage of county land used for wheat (r=0.22, p=0.00) and negatively correlated with that used for oats (r=-0.33, p-value=0.00). Appendix A.3 shows that cereal suitability is strongly correlated with nearly all the inequality measures—the exceptions being the two measures of de facto power (H4). 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.

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



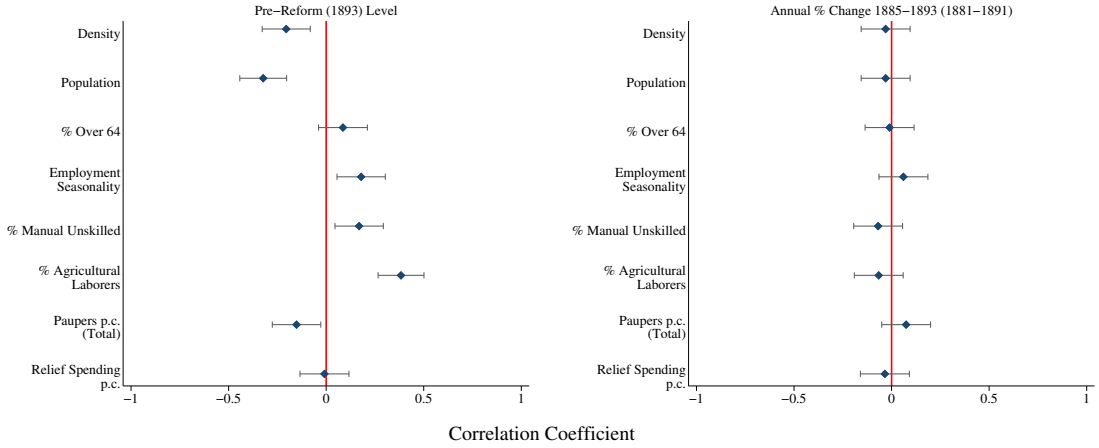
Note: The left-hand panel displays the estimated mean-median ratio from Equation (2). The right-hand panel displays FAO soil cereal suitability. High, medium, and low refer to terciles.

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 on poor relief to support them outside harvest season. As a result, it is important to test whether other inequality measures represent an independent causal channel, or instead simply capture the need for income support in cereal-producing unions (Hypothesis 2a). 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 to allow 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 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, and hence that the identifying assumption is appropriate. 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 mean-median 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 change is 1881–1891.

6 Results

The empirical analysis shows strong and robust evidence that more unequal areas experienced greater increases in government spending following the 1894 democratic reforms. 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. Finally, the third section investigates the dynamic effects of inequality post-reform, and disaggregates the effects on different components of poor relief expenditure.

6.1 Support for the Meltzer-Richard Hypothesis

Democratic reform led to greater increases in poor relief expenditure in areas with higher income inequality, offering empirical support to the Meltzer-Richard hypothesis, 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. As predicted by Hypothesis 1, the coefficient on income inequality is positive and statistically significant in all specifications—areas with higher inequality experienced greater increases in spending following the Local Government Act.

Importantly, the estimated coefficient relating to income inequality is robust to including various sets of control variables. After adding the vector of "crop controls" in (2) the coefficient size is stable across specifications, offering reassurance that the effect of income inequality is not confounded by these other union characteristics. Specifications (5)–(7) allow flexibly for potential confounds by including quartic time trends interacted with union characteristics 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

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

	DV=Relief Expenditure per Capita (Standarized)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
IncomeInequality_x_post1894	0.11***	0.09***	0.11***	0.11***	0.10***	0.10***	0.10***
	(0.030)	(0.028)	(0.026)	(0.026)	(0.025)	(0.026)	(0.025)
Population			0.40	0.35	0.21	0.36	0.38
			(0.454)	(0.461)	(0.482)	(0.465)	(0.467)
Population Density			-0.25	-0.31	-0.24	-0.32	-0.24
			(0.441)	(0.454)	(0.467)	(0.453)	(0.472)
Decadal Variance in Pauperism			-0.00	0.00	0.00	0.00	0.00
			(0.015)	(0.014)	(0.014)	(0.014)	(0.014)
% Popn Male Age Over 64			0.02	0.02	-0.02	0.02	0.04
			(0.071)	(0.069)	(0.070)	(0.069)	(0.076)
% Popn Female Age Over 64			0.34***	0.33***	0.32***	0.32***	0.33***
			(0.093)	(0.090)	(0.093)	(0.091)	(0.092)
% Revenue from Poor Rate				0.12***	0.12***	0.11***	0.12***
				(0.026)	(0.026)	(0.026)	(0.027)
Log Tax Base Per Acre				-0.10	-0.07	-0.10	-0.09
				(0.112)	(0.115)	(0.112)	(0.112)
Estimated Effect of Inequality on post-Reform Spending:							
10th Percentile	5%	4%	5%	5%	5%	5%	5%
Median	9%	6%	9%	8%	8%	8%	8%
90th Percentile	13%	9%	13%	12%	11%	12%	12%
Average	9%	7%	9%	9%	9%	9%	9%
Avg. Δ Spending Post-Reform	15%	15%	15%	15%	15%	15%	15%
No. Observations	4368	4368	4368	4368	4368	4368	4368
No. PLUs	208	208	208	208	208	208	208
Crop Controls	N	Y	Y	Y	Y	Y	Y
Demographic Controls	N	N	Y	Y	Y	Y	Y
Quartic Time Trend					% Wheat 1885	Distance Major City	% ¿64 1881
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y	Y	Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All variables are standardized. "Crop Controls" includes the county-level percentage of land used for different crops. "Demographic Controls" include the percentage of population of each sex in different age groups. Standard errors are clustered by poor law union and presented in parentheses.

for cereal agriculture. The second specification allows for different time paths according to the ease of outmigration, proxied by the distance from a major city. Specification (7) allows spending of poor relief to evolve based on the proportion of old-aged citizens—those most likely to require relief in the district. Appendix Table D.4 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 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 relatively small. The median level of inequality in the sample led to an 9% increase in per capita spending relative to pre-reform levels. At the 90th percentile, the estimated effect is around 13%. These muted effects reflect 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. Counterfactual estimates suggest that if unions had been entirely equal (or democratic reform had not occurred), the increase in spending would have been approximately halved. Income inequality thus appears to have been a major moderator of the—relatively small—effect of democratic reform.

Appendix D.2 reports a number of additional robustness tests. The results are very similar when logging the mean-median ratio to account for outliers or using a mean-median ratio constructed using the 1892 wage. The effect magnitude and statistical strength are also similar when restricting the sample to exclude outliers or to remove regions of very high or low inequality. The results thus show strong and robust evidence supporting the Meltzer-Richard hypothesis.

6.2 Other Dimensions of Rural Inequality

I now test all the theoretical hypotheses listed in Table 1 simultaneously. To do so, I first re-estimate specification (4) from Table 2 with each of the measures of inequality independently. I then run specifications in which I include multiple measures simultaneously to disentangle different channels through which inequality may affect spending. Each non-binary inequality measure is again standardized, so that regression coefficients are comparable across specifications.

The results, presented in Table 3, provide some evidence for all four theoretical hypotheses. When examining the hypotheses independently, we see statistically significant (at least weakly) coefficients in the expected direction for all measures except the presence of an aristocrat—a measure of national political influence. The analysis thus provides some support for many of the theoretical channels suggested in previous studies, with de facto power based on local rather than national influence.

However, the pattern is more complex when examining multiple hypotheses together, as shown in the final two columns of Table 3. The coefficients relating to income inequality (H1), demand for poor relief (H2), and elite de facto power (H4) remain similar when in these specifications—consistent with them capturing distinct dimensions of inequality. However, the coefficients relating to class conflict (H3) both shrink considerably and become statistically indistinguishable from zero. The seeming effects of these variables thus appear to capture other, correlated, measures of inequality—particularly high correlations with agricultural type (see Appendix Table A.2)—rather than representing a true causal effect of class conflict on post-democratization spending.

The main conclusions are similar when using alternative operationalizations of the theoretical hypotheses—see Appendix D.1 for results. To test robustness to the way in which each dimension of inequality is defined, I use alternative measures of inequality to account for alternative ways of conceptualizing the theoretical hypotheses. Specifically, I use three

alternative measures of class conflict (H3a, H3b): land inequality measured in terms of land value (not acreage), wealth inequality (measured using the distribution of household servants), and the number of gentry in a union. As an alternative to aristocratic presence, I use the presence of a great landowner in a union—in case wealth, rather than national prestige, provided de facto power (H4a). For those dimensions of inequality where alternative variables are not available, I check that the results are robust to using alternative definitions of the variables reported in Table 3. This includes capturing the need for social insurance (H2) using the percent of union land used for wheat and the percent of old-aged men (as opposed to cereal suitability and the percent of all old-aged paupers), and capturing agrarian grievances (H3c) using the fall in adult paupers (rather than all paupers) during the Crusade. All results are consistent with those in Table 3, with variables relating to class conflict consistently demonstrating the weakest relationship with post-reform spending.

Appendix D presents additional robustness tests. Tables D.4 and D.5 show that the results in specification (8) are similar when including alternative complex time trends or allowing reactions to interactions with other union characteristics. Similarly, Table D.6 investigates robustness to subsamples—again results are similar (in one case the p-value relating to the coefficient on local political control is above 0.1).

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

	DV=Relief Expenditure per Capita (Standarized)									
Hypothesis	1	2a	2b	3a	3b	3c	4a	4b	All	1, 2a, 4b
Income Inequality	0.11***								0.09***	0.09***
	(0.026)								(0.027)	(0.025)
Need for Income Support		0.12***							0.09**	0.10***
		(0.030)							(0.034)	(0.029)
Need to Support Old Aged			0.06*						0.06*	
			(0.033)						(0.032)	
Land Inequality				0.06**					0.03	
				(0.026)					(0.025)	
Agrarian Inequality (Poor)					0.10***				0.02	
					(0.025)				(0.032)	
Agrarian Grievance						0.06**			0.01	
						(0.031)			(0.029)	
Aristocratic Elite							0.00		-0.01	
							(0.056)		(0.053)	
Local Elite Control								-0.13**	-0.10*	-0.11*
								(0.059)	(0.054)	(0.055)
No. Observations	4368	4368	4368	4368	4368	4368	4368	4368	4368	4368
No. PLUs	208	208	208	208	208	208	208	208	208	208
Demographic Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Financial Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Crop Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All non-binary variables are standardized. Controls are the same as in Specification (4) of Table 2. Standard errors are clustered by poor law union and presented in parentheses.

In summary, the analysis provides robust evidence that the effect of democratic reform on spending was moderated by three dimensions of inequality. Spending was higher in areas of higher income inequality, as suggested by the Meltzer-Richard hypothesis, and those with greater need for social insurance, particularly the need for income support in areas with arable farming. On the other hand, areas in which elites held local political influence, specifically controlling poor law boards before 1893, experienced relatively lower spending after 1894. However, there is little evidence that class conflict was important in shaping the effect of democratic reform, after differences in agricultural production are accounted for. The findings thus provide evidence for both median voter models and theories emphasizing elite de facto power. The following subsection provides further detail of the ways in which these inequality channels shaped spending outcomes.

6.3 Dynamic Effects

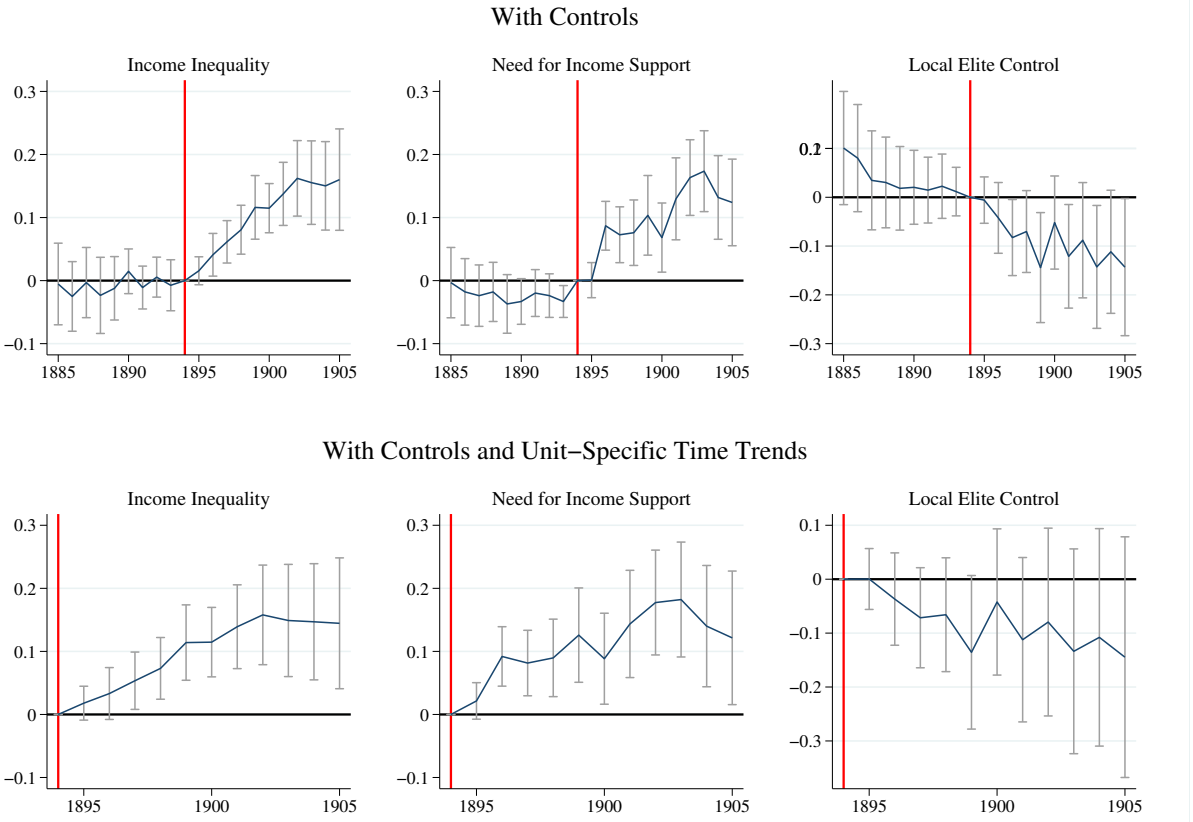
This subsection investigates the dynamic effect of the democratic reform, focusing on the three measures—income inequality, elite political power, and the share of rural laborers—for which there was evidence of a causal relationship in the previous subsection. To do so I estimate flexible regression 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 assumption is 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.[12]

The dynamic analysis, presented in Figure 5, provides further evidence that spending in-

[12]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).

creased in areas of high income inequality (left-hand side) and those needing income support due to the presence of arable agriculture (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 requiring more income support experienced a clearer upward jump after 1894, consistent with the idea that this variable captures the short-run spike for laborers' demands for more outrelief following reform (see Section 3.3). Further, the bottom panel of Figure 5 demonstrates that the main conclusions are robust to the inclusion of union-specific linear time trends—further evidence that the effects are not driven by pre-reform differences.

Figure 5: The effect of income inequality and need for social insurance clearly emerges post-reform, but there is evidence of pre-trends in unions controlled by local elites.



Note: The top panels plot β_j and β_k, and associated 90% confidence intervals, from the specification:

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

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

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

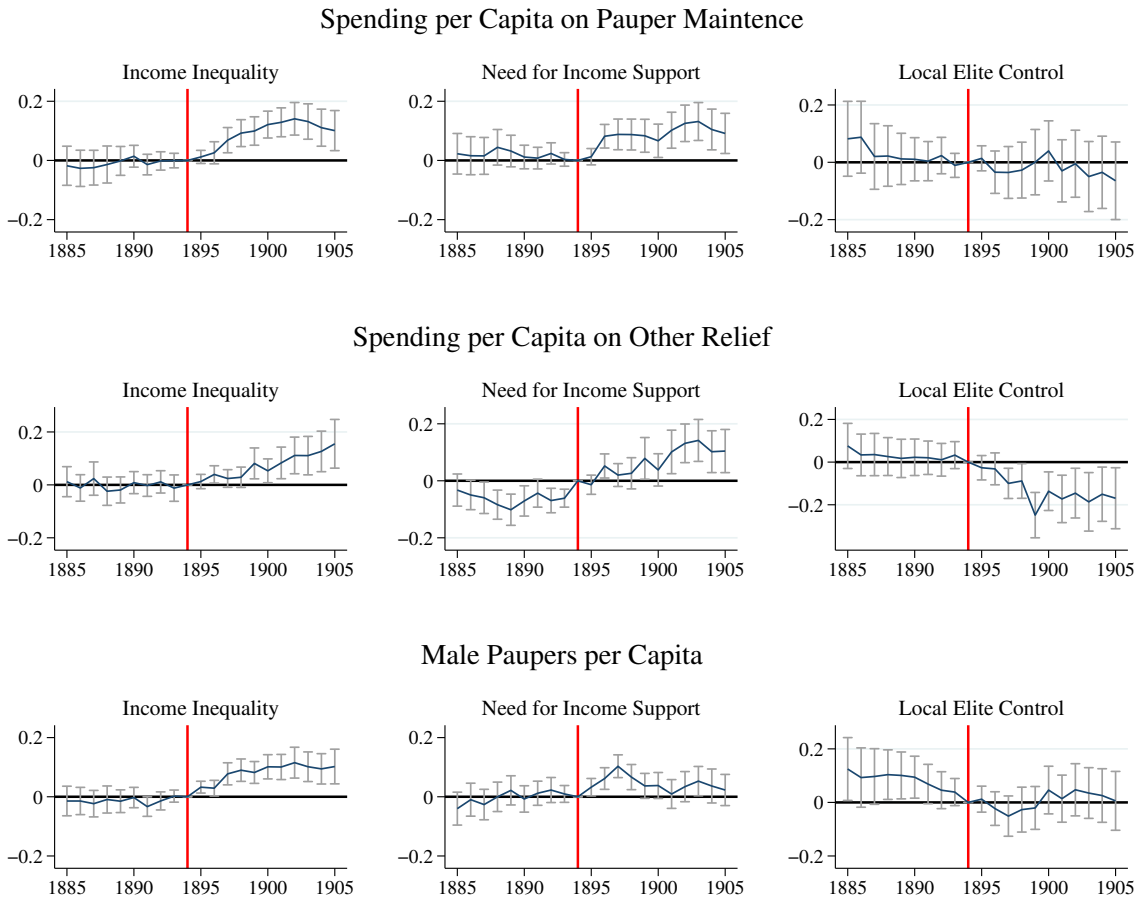
where T_{it} are union-specific linear time trends. The excluded category in both cases relates to 1894, so results are relative to the year of reform. All specifications include controls from specification (4) of Table 2. Standard errors are clustered by poor law union.

We see, however, a very different pattern when examining elites' local control. Areas with ex officio chairmen had falling spending even before the 1894 reform. 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, it suggests that the regression results above may not entirely capture an elite counter-reaction to democratization. Rather, the elite appear to have been engaged in depressing spending pre-reform, and democratization was unable to reverse—or even stop—that trend.

Figure 6 sheds further light on this pattern by investigating changes in different components of spending on poor relief. The top two panels distinguish between per capita spending on maintenance (direct costs of supporting a pauper) and other spending, such as salaries of poor law officers. The bottom panel investigates the effect on the number of male paupers—the group most susceptible to political debates about outrelief.[13] The patterns for both income inequality and needing income support are consistent with those in Figure 5, with increases in all three measures. The pattern for local elite control is, however, more nuanced. There is no evidence of any increase in maintenance spending, but a clear fall in spending on the fixed components of expenditure. The downward trend before 1894 appears to be explained by falling numbers of paupers—a trend that disappears afer 1894. Such a pattern suggests that reform may have forced elites to be more generous in publicly-salient elements of relief after 1894, while switching to use their political influence to reduce less observable forms of expenditure.

[13]Appendix D.3 shows similar figures for other groups.

Figure 6: The effect of democratic reform on different components of poor relief.



Note: The figure plots specifications as in the top panel of Figure 5 with the dependent variables listed in the panel titles (standardized).

7 Conclusion

These findings support theories in which greater income inequality causes elites to be warier of democratization. In a setting where democratic reform was exogenously imposed, post-reform government spending increased by more in areas of high inequality. The results

provide empirical support for the Meltzer-Richard model of government spending, and consequently for redistributive theories of democratization: wealthy landowners had a greater incentive to resist democratization of the bodies controlling spending on social insurance where inequality was higher. Previous literature has provided evidence that landowners opposed democratic reforms more where inequality was high (e.g., Ziblatt, 2008); this paper suggests that doing so could be motivated by a fear of higher taxation. Similarly, the results support the hypothesis that British elites accepted national-level democratization in 1832 because of institutional constraints holding back redistribution (Lizzeri and Persico, 2004).

The results also demonstrates that elites were able to limit the effects of democracy even after 1894. Spending grew relatively little in general after 1894, and growth was particularly low in areas where rural elites held political influence. Despite the removal of property qualifications, poorer citizens did not possess the financial resources to sit on poor law boards or, consequently, scrutinize policy in detail. As a result, elites were able to reduce less salient spending, even while constrained in policy areas that could spark popular protest.

Finally, the paper provides an example of how decentralization can limit the effects of democratic transitions on government redistribution. After Britain's 1832 Great Reform Act, the increasingly democratized Westminster parliament rapidly addressed the needs of growing industrial cities, but left traditional political structures in rural areas largely undisturbed. Previous literature has pointed to the potential for pre-reform elites to limit the impact of democracy through investing in *de facto* power (Acemoglu and Robinson, 2006a), patronage (Acemoglu, Ticchi, and Vindigni, 2011), the threat of a coup (Acemoglu and Robinson, 2001), or capital flight (Boix, 2003). In the British case, landed elites managed to shape redistributive institutions such that they maintained local control even while losing their national influence. Only after agricultural laborers were enfranchised by the 1884 Third Reform Act were these local institutions finally reformed. Landowners played a large role in molding the New Poor Law and in doing so they limited the redistributive impact of

democratization for a further sixty years or more. Understanding when and how elites can establish such local control to constrain national democratization is an important topic for future research.

References

Acemoglu, Daron, Davide Ticchi, and Andrea Vindigni. 2011. "Emergence and persistence of inefficient states." *Journal of the European Economic Association* 9 (2): 177–208.

Acemoglu, Daron, Georgy Egorov, and Konstantin Sonin. 2012. "Dynamics and stability of constitutions, coalitions, and clubs." *American Economic Review* 102 (4): 1446–1476.

Acemoglu, Daron, and James Robinson. 2000. "Why did the West extend the franchise? Democracy, inequality, and growth in historical perspective." *Quarterly Journal of Economics* 115 (4): 1167–1199.

Acemoglu, Daron, and James Robinson. 2001. "A Theory of Political Transitions." *American Economic Review* 91 (4): 938–963.

Acemoglu, Daron, and James Robinson. 2006a. "De facto political power and institutional persistence." *American Economic Review* 96 (2): 325–330.

Acemoglu, Daron, and James Robinson. 2006b. *Economic Origins of Dictatorship and Democracy*. Cambridge: Cambridge University Press.

Acemoglu, Daron, Suresh Naidu, Pascual Restrepo, and James Robinson. 2015. "Democracy, redistribution, and inequality." In *Handbook of Income Ristribution*. Vol. 2. Elsevier.

Agranov, Marina, and Thomas Palfrey. 2015. "Equilibrium tax rates and income redistribution: A laboratory study." *Journal of Public Economics* 130: 45–58.

Aidt, Toke, Jayasri Dutta, and Elena Loukoianova. 2006. "Democracy comes to Europe: franchise extension and fiscal outcomes 1830–1938." *European Economic Review* 50 (2): 249–283.

Aidt, Toke, Martin Daunton, and Jayasri Dutta. 2010. "The Retrenchment Hypothesis and the Extension of the Franchise in England and Wales." *The Economic Journal* 120 (547): 990–1020.

Aidt, Toke, and Pete Jensen. 2009a. "Tax structure, size of government, and the extension of the voting franchise in Western Europe, 1860–1938." *International Tax and Public Finance* 16 (3): 362–394.

Aidt, Toke, and Peter Jensen. 2009b. "The taxman tools up: An event history study of the introduction of the personal income tax." *Journal of Public Economics* 93 (1-2): 160–175.

Aidt, Toke, and Peter Jensen. 2013. "Democratization and the size of government: evidence from the long 19th century." *Public Choice* 157 (3-4): 511–542.

Albertus, Michael, and Victor Menaldo. 2014. "Gaming democracy: elite dominance during transition and the prospects for redistribution." *British Journal of Political Science*: 575–603.

Ansell, Ben, and David Samuels. 2010. "Inequality and democratization: A contractarian approach." *Comparative Political Studies*.

Ardanaz, Martin, and Isabela Mares. 2014. "Labor shortages, rural inequality, and democratization." *Comparative Political Studies* 47 (12): 1639–1669.

Bateman, John. 1883. *The Great Landowners of Great Britain and Ireland*. Harrison and Sons.

Boix, Carles. 2003. *Democracy and redistribution*. Cambridge University Press.

Booth, Charles. 1903. *Life and Labour of the People in London: First series*. Macmillan and Company.

Boustan, Leah, Fernando Ferreira, Hernan Winkler, and Eric Zolt. 2013. "The effect of rising income inequality on taxation and public expenditures: Evidence from US municipalities and school districts, 1970–2000." *Review of Economics and Statistics* 95 (4): 1291–1302.

Boyer, George. 2004. "The evolution of unemployment relief in Great Britain." *Journal of Interdisciplinary History* 34 (3): 393–433.

Boyer, George. 2006. *An economic history of the English poor law, 1750-1850*. Cambridge University Press.

Boyer, George, and Timothy Schmidle. 2009. "Poverty among the elderly in late Victorian England." *The Economic History Review* 62 (2): 249–278.

Brown, David S, and Wendy Hunter. 2004. "Democracy and human capital Formation: Education Spending in Latin America, 1980 to 1997." *Comparative Political Studies* 37 (7): 842–864.

Cascio, Elizabeth, and Ebonya Washington. 2014. "Valuing the Vote: The Redistribution of Voting Rights and State Funds following the Voting Rights Act of 1965." *Quarterly Journal of Economics* 129 (1): 379–433.

Chacon, Mario, and Jeffrey Jensen. 2020. "Democratization, De Facto Power, and Taxation: Evidence from Military Occupation during Reconstruction." *World Politics* 72 (1): 1–46.

Channing, Francis. 1918. *Memories of Midland Politics, 1885-1910*. Constable, Limited.

Chapman, Jonathan. 2018. "Does democratic reform lead to lower infrastructure investment? Evidence from nineteenth-century England." *Quarterly Journal of Political Science* 13 (4): 363–404.

Chapman, Jonathan. 2023. "Gradual Franchise Extensions and Government Spending in Nineteenth-Century England." *Forthcoming at the Journal of Politics*.

Cinnirella, Francesco, and Erik Hornung. 2016. "Landownership concentration and the expansion of education." *Journal of Development Economics* 121: 135–152.

Collins, Edward, and Joan Thirsk. 2000. *The Agrarian History of England and Wales. 7, 1850-1914: Pt. 2*. Cambridge University Press.

De Mello, Luiz, and Erwin Tiongson. 2006. "Income inequality and redistributive government spending." *Public finance review* 34 (3): 282–305.

Dodd, J.Theodore. 1890. "The powers of the rural sanitary authorities." *County and Local Government Magazine* 4 (XXII): 227-236.

Domènech, Jordi, and Ignacio Sánchez-Cuenca. 2022. "The Long Shadow of Agrarian Conflict: Agrarian Inequality and Voting in Spain." *British Journal of Political Science* 52 (4): 1668–1688.

Dorsch, Michael, and Paul Maarek. 2019. "Democratization and the conditional dynamics of income distribution." *American Political Science Review* 113 (2): 385–404.

Engerman, Stanley, and Kenneth Sokoloff. 2005. "The evolution of suffrage institutions in the new world." *Journal of Economic History* 65 (4): 891.

Fujiwara, Thomas. 2015. "Voting technology, political responsiveness, and infant health: Evidence from Brazil." *Econometrica* 83 (2): 423–464.

Gazeley, Ian, and Andrew Newell. 2011. "Poverty in Edwardian Britain." *Economic History Review* 64 (1): 52–71.

Haggard, Stephan, and Robert Kaufman. 2012. "Inequality and regime change: Democratic transitions and the stability of democratic rule." *American Political Science Review* 106 (03): 495–516.

Harms, Philipp, and Stefan Zink. 2003. "Limits to redistribution in a democracy: a survey." *European Journal of Political Economy* 19 (4): 651–668.

Horrell, Sara. 1996. "Home Demand and British Industrialization." *The Journal of Economic History* 56 (03): 561–604.

House of Commons. 1878a. *Return of the Number of Contest Elections for Guardians.*

House of Commons. 1878b. *Select Committee on Poor Law Guardians.*

House of Commons. 1909. *Report of the Royal Commission on the Poor Laws and Relief of Distress.*

Hurren, Elizabeth. 2015. *Protesting about Pauperism: Poverty, politics and poor relief in Late-Victorian England, 1870-1900.* Vol. 60 Boydell & Brewer.

Husted, Thomas, and Lawrence Kenny. 1997. "The Effect of the Expansion of the Voting Franchise on the Size of Government." *Journal of Political Economy*: 54–82.

Islam, Md, Jakob Madsen, and Hristos Doucouliagos. 2018. "Does inequality constrain the power to tax? Evidence from the OECD." *European Journal of Political Economy* 52: 1–17.

Iversen, Torben, and Max Goplerud. 2018. "Redistribution without a median voter: Models of multidimensional politics." *Annual Review of Political Science* 21: 295–317.

Keith-Lucas, Bryan. 1952. *The English local government franchise: a short history.* Blackwell.

King, Steven. 2000. *Poverty and welfare in England, 1700-1850.* Manchester University Press.

Lambert, Paul, Richard Zijdeman, Marco Van Leeuwen, Ineke Maas, and Kenneth Prandy. 2013. "The construction of HISCAM: A stratification scale based on social interactions

for historical comparative research." *Historical methods: a journal of quantitative and interdisciplinary history* 46 (2): 77–89.

Lizzeri, Alessandro, and Nicola Persico. 2004. "Why did the elites extend the Suffrage? Democracy and the scope of government, with an Application to Britain's "Age of Reform"." *Quarterly Journal of Economics* 119 (2): 707–765.

MacKinnon, Mary. 1986. "Poor law policy, unemployment, and pauperism." *Explorations in Economic History* 23 (3): 299–336.

MacKinnon, Mary. 1988. "The use and misuse of poor law statistics, 1857 to 1912." *Historical Methods* 21 (1): 5–19.

MacMorran, Alex, and Thomas Colquhuon Dill. 1907. *The Local Government Act 1894*. Fourth ed. London, England. Butterworth and Co., Shaw and Sons.

McClaren, Eva. 1987. "The Civil Rights of Women." In *Before the Vote was Won*, ed. Jane Lewis. Routledge and Kegan Paul: New York and London.

Meltzer, Allan, and Scott Richard. 1981. "A rational theory of the size of government." *Journal of Political Economy* 89 (5): 914–927.

Milanovic, Branko. 2000. "The median-voter hypothesis, income inequality, and income redistribution: an empirical test with the required data." *European Journal of Political Economy* 16 (3): 367–410.

Mitchell, Brian R. 1971. *Abstract of British historical statistics*. CUP Archive.

Moene, Karl, and Michael Wallerstein. 2001. "Inequality, social insurance, and redistribution." *American Political Science Review* 95 (4): 859–874.

Nunn, Nathan, and Diego Puga. 2012. "Ruggedness: The blessing of bad geography in Africa." *Review of Economics and Statistics* 94 (1): 20–36.

Perotti, Roberto. 1996. "Growth, income distribution, and democracy: What the data say." *Journal of Economic growth* 1 (2): 149–187.

Reid, A.M., S.J. Arulanantham, J.D. Day, E.M. Garrett, H. Jaadla, and M. Lucas-Smith. N.d. "Populations Past: Atlas of Victorian and Edwardian Population." . Forthcoming.

Rowntree, Benjamin Seebohm, and May Kendall. 1913. *How the labourer lives: a study of the rural labour problem*. Palala Press.

Scheve, Kenneth, and David Stasavage. 2010. "The conscription of wealth: mass warfare and the demand for progressive taxation." *International Organization* 64 (04): 529–561.

Scheve, Kenneth, and David Stasavage. 2012. "Democracy, war, and wealth: lessons from two centuries of inheritance taxation." *American Political Science Review* 106 (01): 81–102.

Schürer, Kevin, and Matthew Woollard. 2003. *National Sample from the 1881 Census of Great Britain [computer file]*. Colchester, Essex: History Data Service, UK Data Archive [distributor].

Scott, James. 1977. *The Moral Economy of the Peasant: Rebellion and Subsistence in South-east Asia*. Vol. 315 Yale University Press.

Southall, H.R;, P. Ell, D.A. Gatley, and I. Gregory. August 2004. *Great Britain Historical Database: Census Data: Parish-Level Population Statistics, 1801–1951 [computer file]*. Colchester, Essex: UK Data Archive [distributor] SN: 4560.

Southall, Humphrey; Gilbert, David Gregory Ian. January 1998. *Great Britain Historical Database: Vital Statistics, Mortality Statistics, 1851–1920 [computer file]*. Colchester, Essex: UK Data Archive [distributor], SN: 3707..

Thomson, David. 1984. "'I am not my father's keeper': Families and the Elderly in Nineteenth-Century England." *Law and History Review* 2 (2): 265–286.

Walford, Edward. 1886. *The County Families of the United Kingdom*.

Webb, Sidney, and Beatrice Webb. 1913. *English local government. Vol. 5: The story of the King's highway*. London ; New York : Longmans, Green.

Webb, Sidney, and Beatrice Webb. 1929. *English Poor Law History, Part II: The Last Hundred Years* . Longmans, Green & Co.

Wolfers, Justin. 2006. "Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results." *American Economic Review* 96 (5): 1802–1820.

Ziblatt, Daniel. 2008. "Does landholding inequality block democratization?: A test of the "bread and democracy" thesis and the case of Prussia." *World Politics* 60 (4): 610–641.

Ziblatt, Daniel. 2009. "Shaping democratic practice and the causes of electoral fraud: The case of nineteenth-century Germany." *American Political Science Review*: 1–21.

Online Appendix—Not Intended for Publication

Table of Contents

A Data

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

A.1 Main Data Sources

This subsection presents additional information regarding variable construction.

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 unions that were established or abolished between 1860 and 1905, or where the cumulative change in boundaries over the same period exceeded 15% of the population in 1881—identifying 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 17 unions do not appear in the main regressions due to a missing inequality measure, reflecting either missing wage data or 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 https://www.visionofbritain.org.uk/data/. 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. While there could be some concern that these dates may not accurately reflect the stock of paupers through the 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. Guardians were also responsible for collecting taxes on behalf of other bodies.

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. Conveniently, 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 the data collected by Southall (January 1998) stored at the UK data archive. Annual variables were constructed by geometric interpolation between census years.

Houses: The parish-level number of houses are taken from Southall et al. (August 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. (n.d.). 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. Published by the Board of Agriculture and Fisheries.

A.2 Definition of Main Variables

Income Inequality: Estimated mean-median ratio of household spending on property. See Appendix B.1 below.

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

% Male (female) population in Age: 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.2 below.

Agrarian Inequality (Poor): (Male agricultural laborers aged 15-64 as % of all men aged 15-64 years (from Reid et al., n.d.) / share of men in agriculture (calculated using 1881 census sample)).

Strength of Crusade Against Outrelief: The % fall in the number of outdoor paupers between 1866–1871 and 1881–1886.

Aristocratic Elite: A binary variable equalling one if a peer of the realm had a country seat in the union. See Appendix B.2 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.

Male (female) paupers per capita: Male (female) adult paupers as percentage of male population aged older than 15.

A.3 Descriptive Statistics

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

Table A.1: Descriptive Statistics

	Obs	Mean	Std. Dev.	Min	Max
Total Relief Spend per Capita (£)	4368	.41	.1	.16	.75
Inequality Measures:					
Income Inequality	208	9.14	2.4	2.64	19.17
Need to Support Old Aged	208	66.31	59.08	-124.93	221.69
Need for Income Support	208	3508.29	1248.66	0	7090.25
Land Inequality	208	38.35	25.06	0	100
Agrarian Inequality (Poor)	208	59.19	14.43	13.63	85.71
Crusade Harshness	208	38.19	18.33	-36.62	81.33
Aristocratic Presence	208	.48	.5	0	1
Local Political Control	208	.46	.5	0	1
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
100*Std Dev of Paupers p.c.	4368	.34	.35	.04	2.91
% Revenue from Poor Rate	4368	84.33	7.17	59.28	97.92
Log Tax Base Per Acre	4368	.31	.46	-1.21	1.62

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

B Measures of Inequality

This appendix introduces a simple model capturing the key prediction to be tested in the empirical analysis: that inequality mediates the effects of democratization. This model is in the spirit of Meltzer and Richard (1981), which forms the basis of the canonical models of democratization of Acemoglu and Robinson (2006b) and Boix (2003). The second subsection then explains how the historical context fit the theoretical assumptions, before the third subsection displays trends in poor relief over the nineteenth-century.

Table A.2: Correlations between inequality measures and pre-reform spending trends.

	H1 Income Inequal.	H2b Old Aged	H3a Land Inequal.	H3b Agrarian Grievance	H3c Agrarian vs Outrelief	H4a Aristo-cratic Elite	H4b Local Elite Control	H2a Income Support
Need to Support Old-Aged	0.079 (0.259)							
Land Inequality	0.108 (0.122)	-0.059 (0.380)						
Agrarian Inequality (Poor)	0.297 (0.000)	0.209 (0.002)	0.202 (0.002)					
Agrarian Grievance	0.222 (0.001)	0.385 (0.000)	0.009 (0.892)	0.137 (0.040)				
Aristocratic Elite	0.005 (0.945)	-0.114 (0.089)	0.323 (0.000)	0.110 (0.100)	-0.039 (0.564)			
Local Elite Control	-0.082 (0.238)	-0.059 (0.378)	-0.087 (0.194)	0.097 (0.148)	-0.078 (0.244)	0.099 (0.140)		
Need for Income Support	0.235 (0.001)	0.171 (0.010)	0.210 (0.002)	0.614 (0.000)	0.202 (0.002)	0.143 (0.032)	-0.013 (0.848)	
Δ Relief Spending p.c. 1885–93	-0.031 (0.652)	0.080 (0.231)	-0.032 (0.631)	-0.070 (0.297)	0.045 (0.499)	-0.006 (0.932)	-0.067 (0.315)	-0.026 (0.697)

Note: Table shows univariate correlations between measures, with p-values shown in parentheses. The strongest pattern of correlations is with the "need for income support" which is measured using soil cereal suitability in the union.

B.1 Income Inequality

B.1.1 Model

Suppose there is a unit mass of voters, consisting of poor (P) and rich (R) citizens. The share of poor citizens is $\lambda > 0.5$. Citizens receive incomes y_P and y_R if employed. An exogenously determined fraction $1 - q$ of poor voters are unemployed and receive no employment income.

The sole government activity is to provide welfare payments—"poor relief". The government sets a level of relief, r, subject to a minimum, $\underline{r}$. Both employed and unemployed poor citizens benefit from poor relief—employed citizens receive πr, and the unemployed receive r (where $\pi > 0$, is exogenous). To ensure an interior solution, I assumed that $\pi < \frac{\lambda \mu_P y_P (1-q)}{(1-\lambda)\mu_R y_R}$. Intuitively, the relief received when employed cannot be higher than the share of tax paid by the poor—otherwise, a poor voter would rather a 100% tax rate. This requirement is necessary in the absence of assuming a convex cost of taxation, as in Meltzer and Richard (1981).

A voter's unemployment status is unknown at the point that relief is decided. Relief is funded through a proportional tax rate, τ, on citizens' spending on housing. Define the amount spent on

housing by group i as h_i=$\mu_i y_i$ where μ_i is the share of income spent on housing. There is a balanced budget constraint, so that per capita government spending, g, is given by:

$$g = \lambda(\pi q + (1-q))r = \lambda\tilde{q}r = \tau(q\lambda h_P + (1-\lambda)h_R) = \tau\bar{h}$$

where $\bar{h}$ equals mean spending on housing for the employed, and $\tilde{q} = (\pi q + (1-q))$ is the probability of receiving relief weighted by the amount received.

The goal of the model is to understand how a democratic reform affects the level of relief provided. Voters vote over r, and I assume that the level of relief preferred by the poor is always greater than $\underline{r}$. Pre-reform only the rich can vote, after reform the vote is extended to all citizens. Government policy is determined by a standard Median Voter framework, with a single policy dimension, 2 candidates, and binding campaign promises.

Voters have log utility over consumption, so that the expected utility of a poor voter is given by:

$$U_P = q\log\left(y_P - \tau h_P + \pi r\right) + (1-q)\log(r) \tag{3}$$

For notation, write $\tilde{\lambda} = \lambda\tilde{q}$. Then substitute in $\tau = \frac{r\tilde{\lambda}}{\bar{h}}$:

$$\begin{aligned} U_P = & \quad q\log\left(y_P - \frac{r\tilde{\lambda}}{\bar{h}}h_P + \pi r\right) + (1-q)\log(r) \\ = & \quad q\log\left(y_P + r(-\tilde{\lambda}\tilde{h} + \pi\right) + (1-q)\log(r) \end{aligned}$$

where $\tilde{h} = \frac{h_P}{\bar{h}}$—the inverse of the mean to median spending on housing.

Taking the first order conditions we obtain:

$$r^* = \frac{h_M(1-q)y_P}{\lambda(\pi q + (1-q)) - \pi h_M} \tag{4}$$

$$g^* = \frac{h_M(1-q)y_P\lambda(\pi q + (1-q))}{\lambda(\pi q + (1-q)) - \pi h_M} \tag{5}$$

where $h_M = \frac{\bar{h}}{h_P}$ is the mean: median ratio of spending on housing. This corresponds directly to the measure of income inequality used in the main text.

B.1.2 Empirical Measure

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

$$meanMedianRatio = \frac{\text{MeanRentalvalue}}{Median\ wage} \tag{6}$$

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 ($\mu_R < \mu_P$). This assumption seems reasonable, and is support by some 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.2 Geolocating Elites

Local elites were identified using two nineteenth-century sources: Bateman (1883) and Walford (1886). Bateman (1883) is well-known among economic historians and lists the size and value of landholdings of the largest landowners in the UK (excluding London) based on an 1873 census of landownership. This source lists individuals with aggregate possessions consisting of more than 3,000 acres and more than £3,000 per annum gross rental value—disaggregating landholdings by county. Walford (1886) identifies a list of owners of the "principal seats" in England and Wales, and in many cases, identifies multiple properties for each individual. This allows me to identify a much larger number of landowners and allocate a much higher proportion of the estates reported by Bateman to individual poor law unions.

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. Where information was available, Bateman's figures relating to the size of land owned by an individual within a county was assigned 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.

B.2.1 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.

B.2.2 Top 5% Wealth Inequality

The top 5% wealth inequality measure is constructed using the 100% 1881 census sample (Schürer and Woollard, 2003). This measure proxies household wealth using the number of live-in servants. The number of servants was used by contemporary 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 therefore proxies the distribution of elite wealth.

C Additional Historical Background

This appendix provides further historical background including i) details of the election system used in poor law unions ii) guardians' role in the bodies providing rural public goods iii) further description of the type of support provided through the poor law.

C.1 Elections of Guardians

This subsection provides further detail of the election system for Guardians, and of the operation of poor law elections in practice. 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 more detail below.[1]

Election Procedure 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).

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

[1]The administrative procedures for the poor law were very detailed, and it is only possible to provide a high-level overview. 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, 1909).

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). 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.[2]

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 received up to six votes in each capacity (if rated over £250).[3] 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).

[2]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.

[3]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).

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.[4]

A further set of changes in the franchise relates to the right to vote of recipients of poor relief or other charities. 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 served as the electoral register.[5] 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.[6] 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

[4]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.

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

[6]See evidence to the 1878 Select Committee. For example, all occupiers were entered into the ratebook in Wolverhampton and Spalding (paras 1822, 2462) but not in Eastbourne (para 834–840).

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).[7]

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.[8]

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 guardians long before 1894, but the exact legal situation was complicated for the same reasons as the female franchise discussed above [9]. 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.

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

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

[8] See *Essex Standard*, 3 December 1892, p.5 for discussion of the 1893 reduction in property qualifications.

[9] See Webb and Webb (1929, p234). The number of female guardians increased significantly over our period of interest: There were 50 female guardians in 1885, 839 in 1895, and 1141 in 1907.

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 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

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. A comprehensive review of this evidence was undertaken for this project, and is summarized here. Few poor law union elections were contested, particularly in rural areas. Of the thirteen unions discussed in the 1909 Commission providing relevant evidence, only one (Spalding) reported a considerable number of contests. In others, only around one-tenth of elections—and sometimes even fewer—were contested. 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, 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.[10]

Where elections did take place, turnout in rural areas appears to have been quite high. The witnesses listed in the 1909 Royal Commission suggested that turnouts of around 85%–90% were the norm. These high turnouts contrast with the general consensus on poor law turnouts, which appear to be

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

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).[11] 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 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 the 1909 Royal Commission 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.[12] 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

[11] 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).

[12] 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).

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, 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).

C.2 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.[13] These additional responsibilities pose a potential compli-

[13]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,

cation 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 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.[14]

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.[15]

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.

[14]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.

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

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.[16] 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.1. Here, to check that the results are not capturing 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, specification (1) 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.

[16]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).

Table C.1: Robustness to controlling for other types of local government spending in poor law unions.

	DV=Relief Expenditure per Capita (Standardized)				
	(1)	(2)	(3)	(4)	(5)
Interaction with post94:					
Income Inequality	0.09***	0.09***			0.08***
	(0.026)	(0.027)			(0.027)
Cereal Suitability	0.10***		0.11***		0.10***
	(0.029)		(0.030)		(0.030)
Local Elite Control	-0.10*			-0.11**	-0.09*
	(0.053)			(0.054)	(0.052)
Public Goods Spend p.c.		-0.03	-0.04*	-0.04**	-0.03
		(0.022)	(0.023)	(0.022)	(0.022)
PublicGoods_x_post1894		0.01	0.04*	0.04*	0.01
		(0.024)	(0.023)	(0.023)	(0.023)
No. Observations	3952	3952	3952	3952	3952
No. PLUs	208	208	208	208	208
Controls	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y

"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. Standard errors are clustered by poor law union and presented in parentheses. Controls are the same as in specification (4) of Table 2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.3 The Poor Law as Social Insurance

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).[17] 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.2 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

[17]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 if they ate the same diet as the able-bodied paupers.

able-bodied men and vagrants—were a relatively small share of relief recipients.[18] 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.2: 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%	82.3%
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.

D Additional Empirical Analysis

D.1 Alternative Measures of Inequality

Table D.3 presents results for alternative measures of many of the inequality measures analyzed in the main text. The first two rows relate to income inequality—first a logged version of the main variable, to account for possible right skew in the variable, and second a version of the variable constructed using 1892 wage data. This latter specification has fewer observations due to missing data. The third and fourth rows include alternative definitions of measures relating to the need

[18] "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).

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 following three 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.2.2 for details). Third, the number of any gentry (not only aristocrats) in the union. The eighth row uses a measure of the crusade against outrelief based on the fall in adult outdoor paupers—the main target of the crusade. The ninth row investigates whether the presence of a great landowner—rather than a peer—in the union led to more elite power.

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 conclusion in Section 6.2 that post-reform spending was moderated by income inequality, the need for income support in arable farming, and local elites' de facto power. There is only weak evidence, however, that social cleavages were important in determining poor relief spending.

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

	DV=Relief Expenditure per Capita (Standarized)									
Hypothesis	1	1	2a	2b	3a	3a	3a	3c	4a	1, 2a, 2b, 3a, 3c, 4a, 4b
Interaction with post1894:										
Log Income Inequality	0.11***									0.09***
	(0.025)									(0.024)
1892 Income Inequality		0.07**								
		(0.029)								
% Wheat 1885			0.06**							0.01
			(0.027)							(0.032)
% Males Old Aged				0.06**						0.02
				(0.031)						(0.034)
Land Inequality (Value)					0.04					0.03
					(0.028)					(0.029)
Wealth Inequality (Servants)						0.02				-0.02
						(0.029)				(0.032)
Number of Gentry							0.02			0.00
							(0.029)			(0.030)
Crusade vs Outrelief (Adults)								0.06*		0.03
								(0.031)		(0.030)
Great Landowner in Union									-0.06	-0.14
									(0.099)	(0.107)
Need for Income Support										0.10***
										(0.037)
Local Elite Control										-0.10*
										(0.054)
No. Observations	4368	3696	4368	4368	4367	4368	4368	4368	4368	4367
No. PLUs	208	176	208	208	208	208	208	208	208	208
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors are clustered by poor law union and presented in parentheses. Controls are the same as in specification (4) of Table 2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.2 Additional Robustness Checks

This appendix demonstrates that the results are robust to a number of additional robustness tests. Table D.4 reports additional specifications allowing for differential time trends based on a range of observable characteristics. Table D.5 allows for the effects of democratic reform to have been moderated by other observable characteristics as well as inequality.

Table D.6 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 inequality measure. Finally, specification 6 studies a larger sample including unions with 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.

Table D.4: Robustness to allowing complex time trends.

	Relief Expenditure per Capita (Standardized)					
	(1)	(2)	(3)	(4)	(5)	(6)
Interaction with post94:						
Income Inequality	0.09***	0.07***	0.09***	0.09***	0.11***	0.09***
	(0.024)	(0.025)	(0.024)	(0.024)	(0.025)	(0.025)
Cereal Suitability	0.07**	0.09***	0.10***	0.10***	0.09***	0.10***
	(0.035)	(0.030)	(0.030)	(0.029)	(0.030)	(0.029)
Local Elite Control	-0.11**	-0.11**	-0.10*	-0.11**	-0.11**	-0.11*
	(0.054)	(0.054)	(0.055)	(0.054)	(0.054)	(0.055)
No. Observations	4368	4368	4368	4368	4368	4368
No. PLUs	208	208	208	208	208	208
Quartic Time Trend	Distance London	Tax Base per Acre 1884	% Δ Tax Base 1874–1884	1869 Wage	Density 1881	Seasonality 1881–1890

Note: All specifications include the control variables from specification (4) of Table 2, year fixed effects, and union fixed effects. "Quartic time trend" indicates a quartic time trend interacted with pre-reform levels of variables. Standard errors are clustered by poor law union and presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.3 Disaggregating Dynamic Effects on Pauper Numbers

Figure D.1 displays similar specifications to the bottom panel of Figure 6 for different groups of paupers. The pattern for income inequality is similar to in that figure, but for cereal suitability and

Table D.5: Robustness to allowing for post-reform reactions to other observable union characteristics.

	DV=Relief Expenditure per Capita						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Income Inequality	0.10***	0.09***	0.08***	0.09***	0.08***	0.11***	0.10***
	(0.024)	(0.024)	(0.025)	(0.026)	(0.027)	(0.025)	(0.025)
Cereal Suitability	0.11***	0.08*	0.09***	0.11***	0.06*	0.08***	0.11***
	(0.031)	(0.039)	(0.031)	(0.030)	(0.036)	(0.030)	(0.030)
Local Elite Control	-0.10*	-0.11**	-0.11**	-0.11*	-0.14**	-0.11**	-0.11*
	(0.055)	(0.054)	(0.054)	(0.055)	(0.055)	(0.054)	(0.055)
Post1894_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	-0.07*	0.05*	-0.01	-0.14***	0.06**	-0.02
	(0.027)	(0.039)	(0.027)	(0.023)	(0.028)	(0.025)	(0.029)
No. Observations	4368	4368	4368	4368	3696	4368	4368
No. PLUs	208	208	208	208	176	208	208

Note: All specifications include the control variables from specification (4) of Table 2, year fixed effects, and union fixed effects. Each specification includes an interaction with the characteristic (pre-reform, standardized for comparability) listed in the row "Post1894_x_" and a post1894 indicator variable. Standard errors are clustered by poor law union and presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

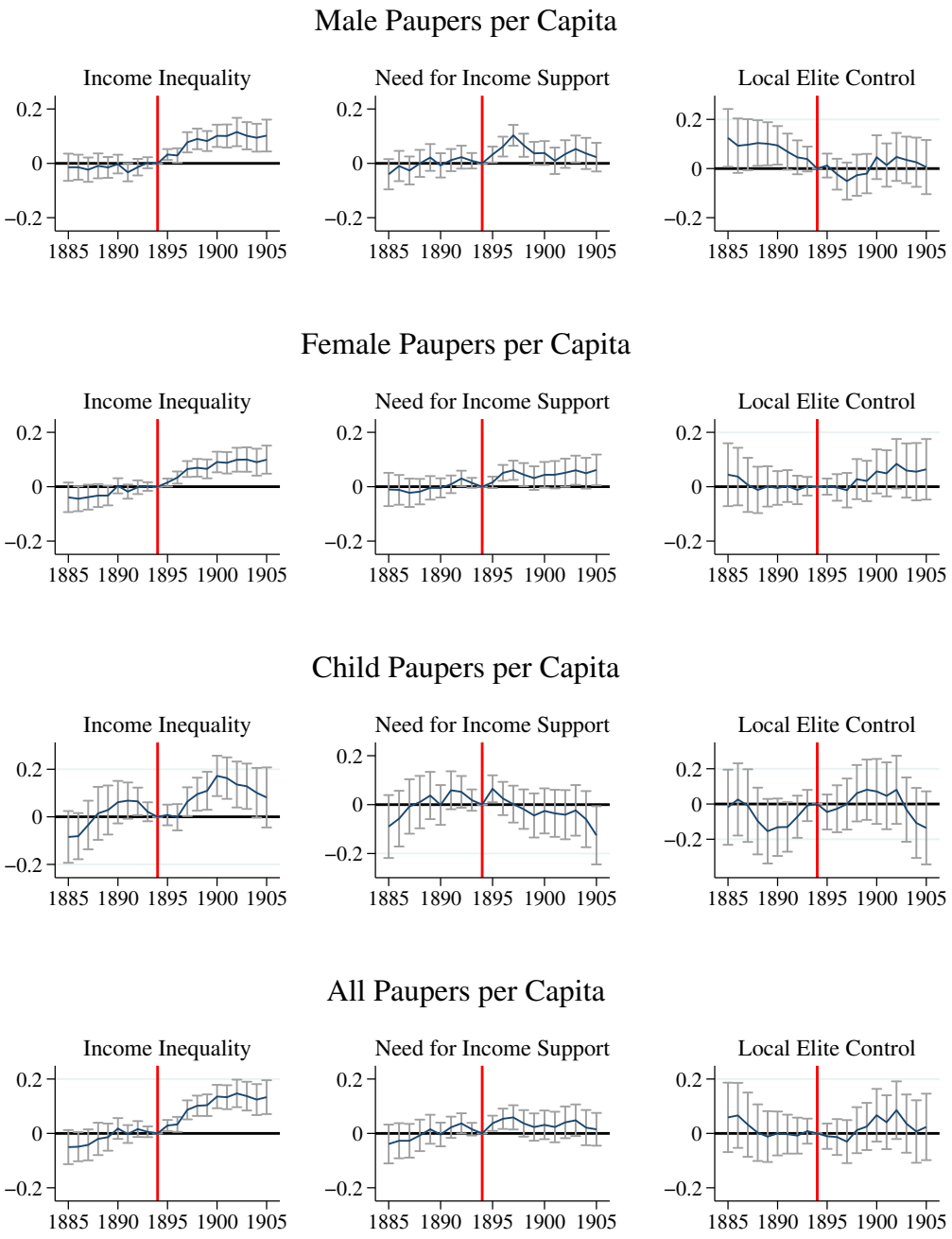
Table D.6: Robustness to Alternative Samples.

	DV=Relief Expenditure per Capita (Standardized)					
	(1)	(2)	(3)	(4)	(5)	(6)
Interaction with post94:						
Income Inequality	0.10***	0.09***	0.07***	0.13***	0.10***	
	(0.037)	(0.025)	(0.025)	(0.027)	(0.026)	
Cereal Suitability	0.09***	0.08***	0.11***	0.08***	0.08***	0.11***
	(0.031)	(0.025)	(0.029)	(0.029)	(0.029)	(0.027)
Local Elite Control	-0.10*	-0.06	-0.10**	-0.09*	-0.10*	-0.11**
	(0.054)	(0.049)	(0.051)	(0.055)	(0.052)	(0.050)
Sample	Drop Extreme Inequality	Drop Extreme Spending	Drop Wales	Drop West Midlands	Median In Farming	No Inc Inequal
No. Observations	3948	3948	3906	3780	3822	4725
No. PLUs	188	205	186	180	182	225

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered by poor law union and presented in parentheses. All specifications include the control variables from specification (4) of Table 2, year fixed effects, and union fixed effects.

local elite power the patterns are clearer for male paupers—consistent with these variables relating to the disputes relating to the need for outrelief for agricultural laborers.

Figure D.1: The effect of democratic reform on different components of poor relief.



Note: The figure plots specifications as in the top panel of Figure 5 with the dependent variables listed in the panel titles (standardized).