

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

Jonathan Chapman\*

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## Abstract

This paper tests whether inequality mediates the effect of democratization on government redistribution. 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 theories arguing that inequality strengthens elite opposition to democratization: more unequal districts experienced greater increases in government expenditure following the democratic reform.

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\*Division of Social Science, New York University Abu Dhabi. I thank Robert Allen, Brian Beach, Dan Bogart, Marc Goñi, Bernard Harris, Philip Hoffman, Jeffrey Jensen, Matthew Kovach, Samantha Myers, Cormac O’Grada, Kevin O’Rourke, 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: jchapman@nyu.edu.

# 1 Introduction

Classic political economy models suggest that voters desire greater government redistribution when inequality is high (Benabou and Ok, 2001; Meltzer and Richard, 1981). Democratic reform is thus more attractive to the masses—and less desirable to the elites—in more unequal countries (Acemoglu and Robinson, 2000, 2001, 2006a; Boix, 2003). Yet empirically testing whether the level of redistribution is higher in more unequal societies is complicated by the fact that most redistribution policy is undertaken at a national level. As a result, many studies are limited to cross-country regressions which struggle to distinguish different institutional factors that may affect support for welfare policy (Milanovic, 2000). Further, it is difficult to identify whether inequality mediates the effect of democratization because—if these theories are correct—democratic reform is itself endogenous to the extent of inequality in a society.

This paper overcomes these challenges by analyzing variation in welfare policy within a single country, Britain, during the nineteenth century. Prior to the advent of the modern welfare state in 1909, support for the destitute—including the sick, the aged, and the unemployed—was controlled by locally elected councils under the (New) Poor Law. I take advantage of an 1894 democratic reform to these councils to test the theoretical prediction that democratization leads to greater increases in welfare spending in more unequal districts. The results support the hypothesis: expenditure increased in all districts after the reform, but the increases were greater where inequality was higher.

These findings offer empirical support to theoretical models of democratization based on elite opposition to greater redistribution. As predicted by these models, elites in more unequal districts had a greater incentive to oppose reform to avoid subsequent increases in taxation. The results also support the hypothesis that British elites accepted national-level democratization in 1832 because of institutional constraints holding back redistribution

(Lizzeri and Persico, 2004). Acemoglu and Robinson (2006b) emphasize how elites can use *de facto* power to cling onto influence post-democratization; the findings here highlight how elites can use formal institutions to constrain the effects of democratization.

Nineteenth-century Britain offers a valuable empirical setting to test political economy models because much government spending was controlled by local authorities, leading to considerable intra-country policy variation. Expenditure on poor relief was controlled by locally elected councils known as the “Guardians of the Poor”, governing districts known as “Poor Law Unions”. These bodies operated within a common legal and cultural framework, meaning that the effects of institutional change can be isolated from other factors in a way that is not possible with cross-country analysis. Further, the scope of expenditure controlled by these councils was limited to the provision of poor relief, meaning that we can isolate the effects of democratization on social insurance.<sup>1</sup>

The empirical analysis in this paper focuses on an 1894 democratic reform that altered the way in which the guardians of the poor were elected. Prior to the reform, implemented as part of the Local Government Act, elites benefited from four important institutional advantages: significant property qualifications required for election as a guardian, a graduated voting franchise, the absence of a secret ballot, and the addition of unelected landowners as *ex officio* guardians. After 1894, all of these advantages were removed, and councils were elected based on a one-man-one-vote system. In the words of one historian: “democracy had come to the boards of guardians” (Brundage, 1975, p.215).

To investigate whether the effects of democratic reform vary according to the level of inequality, I implement a difference-in-difference analysis using data from 1885 to 1905. The treatment event here is the 1894 reform, and the treatment status refers to the degree of inequality in a poor law union. To isolate the effects of inequality from the effects of variation

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<sup>1</sup>Chapman (2018) investigates the effects of democratic reform on the bodies with responsibility for urban public goods expenditure.

in occupational structure, I focus on rural poor law unions (N=225). I then test whether democratization increased spending by a larger margin in high inequality unions than in their low inequality counterparts.

The analysis uses a new dataset that details poor law expenditure and the extent of pauperism across England and Wales at the level of the poor law union. I link the poor law data to census data in order to identify the demographic characteristics of each union and to construct my measure of local inequality—the share of all servants in each union in the top 5% of households (split into terciles). This measure serves as a proxy for wealth inequality, building on the fact that the number of servants was used by contemporaries as a measure of social class (Booth, 1903).

The results show greater spending increases in more unequal poor law unions (both medium and high inequality) after the 1894 reforms. This effect is robust to the inclusion of controls for demographic characteristics capturing the demand for poor relief and financial characteristics that capture potential constraints on union spending. The effect remains when allowing for a flexible response to the reform and accounting for union-specific linear time trends. A number of robustness checks demonstrate that the analysis captures the effects of different degrees of inequality rather than other correlated characteristics, and reflects economic rather than political inequality. Further, additional specifications suggest spending increased more in unions with both a relatively poor median voter and a relatively wealthy elite—consistent with the Meltzer-Richard hypothesis.

In the penultimate section, I turn to the political economy of the poor law, discussing the effects of the 1894 reform on the institutions that determined poor law policy. The reform was followed by significant changes to the composition of the boards of guardians that controlled expenditure. Rural landowners were much less represented in these bodies after 1894—opting not to stand for election in the newly democratic system. However, with rare exceptions, limited time and financial resources meant that the poorest citizens were

unable to replace them: post-1894 boards were composed mainly of farmers. The post-reform system was not a perfect democracy; if it had been, the reform may have led to more radical redistribution.

The paper concludes by placing the findings in the broader context of British political history—the motivating example for many theoretical models of democratization. The results suggest that decentralization allowed rural elites to limit the extent of redistribution long after Britain’s 1832 Great Reform Act. 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.

## 1.1 Related Literature

Several studies have found that democratization leads to greater national government expenditure but few have investigated potential heterogeneity in the effect according to country characteristics. Generally, these papers have found a positive relationship between democratization and both general government spending and tax revenue (e.g., Brown and Hunter, 2004; Aidt et al., 2006; Aidt and Jensen, 2013; Acemoglu et al., 2015). Results for spending on specific components of expenditure are more mixed but largely support the hypothesis that democratic reform led to growth in social expenditure (e.g., Lindert, 1994, 2004).<sup>2</sup> However, there has been little empirical investigation of the theoretical proposition that inequality mediates the effect of democratization.

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<sup>2</sup>See Acemoglu et al. (2015, Section 21.3) for a review of the literature examining the relationships between democracy, redistribution, and inequality.

Acemoglu et al. (2015) do investigate heterogeneous effects of democratization at the national level. Their results regarding inequality are somewhat mixed, but suggest that higher inequality may be associated with elite capture and hence lower taxation post-democratization—contrary to the predictions of the Meltzer-Richard model. The contrasting results in this paper may be explained by the fact that the local democratic reforms discussed here are more likely to have reduced elite capture and thus to have avoided this counterbalancing effect. Alternatively, the difference could reflect selection into democratic transitions if elites allow democratization only when effects on taxation will be limited. The exogenous reform exploited in this paper avoids such complications.

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 et al., 2010; Chapman, 2018, 2020; Cascio and Washington, 2014; Fujiwara, 2015). Similar to the results presented here, Husted and Kenny (1997) find that the franchise expansion resulting from the Voting Rights Act led to greater welfare expenditure in U.S. states. Consistent with my results, Martinez-Bravo et al. (2014) find that introducing elections in Chinese villages between 1982 and 2005 harmed elites—in their case identified by lower income—but they do not identify a mechanism through which this occurred.

The paper also contributes to the economic history of the New Poor Law. Most previous studies have focused on understanding local practices and have particularly emphasized the degree of continuity (or otherwise) between the 1834 New Poor Law and the previous Old Poor Law (e.g., Digby, 1978). The most consequential work within the economic history literature is Boyer (2006), which explains the system of outrelief before 1850 as a method through which employers could maintain a surplus labor force in off-seasons or industrial downturns. A handful of papers have quantitatively analyzed aspects of the operation of the poor law after 1850 but have not examined the institutional changes that are the focus of this

paper (see in particular MacKinnon, 1986, 1987, 1988). I refer to this historical literature at relevant points throughout the paper, starting in the next section with a brief overview of the historical context for the empirical analysis.

## 2 Historical Background

This paper focuses on the boards of guardians established by the 1834 New Poor Law.<sup>3</sup> These boards were responsible for supporting the destitute within their jurisdictions and had a great deal of autonomy over how they did so—an empirical setting that allows me to isolate institutional change from nationwide cultural or political factors that affect the provision of social insurance. Landed elites seized control of the newly established authorities and used this opportunity to restrict the provision of poor relief. Institutional protections for these elites were removed sixty years later; only then did spending on the poor law grow significantly again.

The Poor Law was the main form of social insurance in Britain for several centuries, providing support to a wide variety of destitute citizens. Established in the Elizabethan era, the Old Poor Law required local parishes to provide for any citizens unable to work and to arrange work for those who could not find it. As detailed below, poor relief was thus provided to groups as diverse as the unemployed, orphans, and the aged. Prior to 1909, when old-age pensions were introduced, there was no national-level support for those unable to work: poor relief was thus the only form of government support for the destitute.<sup>4</sup>

After 1834, responsibility for providing poor relief moved to the newly established boards of guardians, significantly strengthening the ability of the elite to restrict poor relief. Under the Old Poor Law, poor relief was administered by parishes, which were often governed

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<sup>3</sup>Formally, the New Poor Law refers to the system established by the 1834 Poor Law Amendment Act.

<sup>4</sup>The institutional framework established by the 1834 New Poor Law remained in place for almost one hundred years—it was not until 1930 that Boards of Guardians were abolished.

relatively democratically (Lizzeri and Persico, 2004).<sup>5</sup> Following a rapid rise in real expenditures in the early nineteenth century, landowners sought to regain control over the provision of relief—leading to the 1834 Poor Law Amendment Act.<sup>6</sup> The Act grouped parishes into “Poor Law Unions” governed by the new boards of guardians. Local landowners seized a great deal of control over these bodies through their influence on the drawing of union boundaries and via a series of institutional provisions that increased their representation on the boards (Brundage, 1972). The removal of these institutional safeguards in the 1890s is the focus of this paper.

The majority of the guardians of the poor were elected in local elections under a graduated franchise designed to protect local landowners.<sup>7</sup> Votes for the guardians were given to all property occupiers—essentially heads of household that owned or rented property—who had paid local taxes and had not received poor relief for one year. However, the number of votes given to individuals varied depending on the value of the property owned or occupied. Individuals in the highest category received six votes as either an owner or an occupier, and individuals could vote separately as both occupiers and owners. As a result, an owner-occupier could receive up to twelve votes.<sup>8</sup>

In addition to the graduated voting system, three further provisions limited the extent to which policy was democratically determined. There was no secret ballot in place, potentially providing elites with the ability to pressure poorer citizens. Significant property qualifications prevented poorer citizens from standing as poor law guardians. Further, to ensure

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<sup>5</sup>Parish authorities continued to play a role in collecting taxes after 1834 but lost nearly all of their policy making power.

<sup>6</sup>See Boyer (2006, Ch 7.1) for a detailed discussion of the logic behind the 1834 Act.

<sup>7</sup>See Appendix A.1 for a detailed description of the system under which guardians were elected, including voter registration and voting procedure. Section 6 and Appendix A.2 discuss the politics and practice of poor law elections. Under the system created in 1834, guardians were elected for a period of only one year. However, these terms were often 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).

<sup>8</sup>Individuals who held property in many different parishes within the same union could vote in each of the parishes.



that the interests of the gentry were represented, the elected guardians were supplemented by unelected *ex officio* guardians, including all magistrates—typically local landowners—residing in the poor law union. The 1894 Local Government Act—which I describe in detail in Section 4.1—removed these institutional safeguards for the elites, providing an exogenous change that I exploit in the empirical analysis.

Boards of guardians were responsible for both determining expenditure on poor relief and raising local taxes to fund that expenditure. They did not, however, hold authority over other forms of government expenditure. In particular, 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).<sup>9</sup> As such, the institutional setting allows me to isolate the effects of democratization on redistribution—the focus of the many models of democratization— from many other types of government expenditure.

To fund their expenditure, guardians predominantly had to rely on taxes collected from local citizens. Parishes, and hence poor law unions, were expected to support their own paupers, meaning the burden of poor relief fell on local citizens (and therefore voters, since failure to pay taxes meant losing the right to vote). Taxes were thus levied on owners and occupiers of land and buildings in each parish based on a tax that was proportional to the value of the property. While the guardians could control the level of taxation, they had no ability to alter this tax structure—nor could they tax income or profits. Many of the political economy models discussed in Section 1 assume a linear tax structure; the guardians were constrained in a similar way.

Guardians did, however, have a great deal of autonomy in administering poor relief; Figure 2 demonstrates the variation in policy over time and across the country. The most contentious aspect of provision throughout the nineteenth century was the extent to which

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<sup>9</sup>The guardians sometimes held other roles that involved spending on public goods—see Section 4.1 and Appendix A.3 for detailed discussion.

recipients should be forced to enter the workhouse to receive poor relief—the infamous “workhouse test”—or whether they could receive it outdoors (“out-relief”). As we can see in the bottom-left panel, guardians varied in how strictly they imposed such conditions. Guardians could also determine who was eligible for relief, the generosity of any relief payments, and whether payments should be monetary or in-kind. As a result, the level of spending both per capita and per pauper differed substantially across poor law unions.

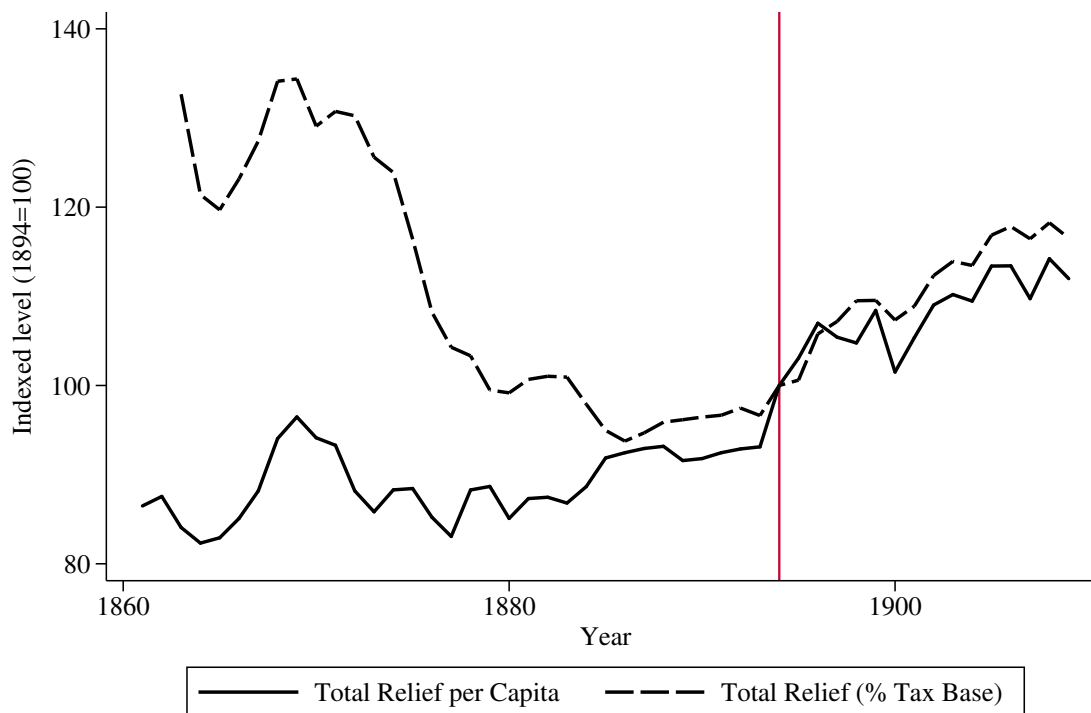
The newly established boards of guardians significantly decreased expenditures on poor relief, with reductions in support for all groups of paupers (Boyer, 2006, p.209). Across the unions in this paper, average per capita relief fell 30% between the reforms and 1861.<sup>10</sup> As shown in Figure 1, per capita spending then remained at a similar level—with a small increase during the 1860s cotton famine caused by the American Civil War—for the next thirty years. As a percentage of the tax base (the rateable value), expenditure actually declined precipitously after the 1860s as increases in land value were not reflected in increases in nominal expenditure. Only from the 1890s onward did expenditure significantly increase again—suggesting a role for the democratic reforms discussed in this paper.

The poor relief administered by the guardians continued to provide a broad range of different kinds of social insurance despite these reductions in expenditure. While poor relief was often considered a form of unemployment benefit, only around 5% of paupers were classified as “able-bodied” men—the clearest group of wage earners. In fact, the available evidence suggests that relatively few paupers were able to work, as paupers generally consisted of the temporarily sick, the mentally ill, the permanently disabled, pregnant women, the elderly, and orphaned children. One-third of paupers were classified as children, while even

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<sup>10</sup>Pre-reform expenditures reflect the average expenditure on poor relief in the three years prior to union formation (obtained from House of Commons *Parliamentary Papers*, 1842)) divided by the 1831 population (as reported in the appendices to the second and third annual reports of the poor law commissioners). Data on 1861 expenditures is obtained from the dataset introduced in Section 3. These nominal values are converted into real terms using the cost of living index from Crafts and Mills (1994). The comparison should be treated as indicative due to potential boundary changes between 1840 and 1861.

**Figure 1: Poor law spending fell after 1870 and increased after the 1894 reforms.**



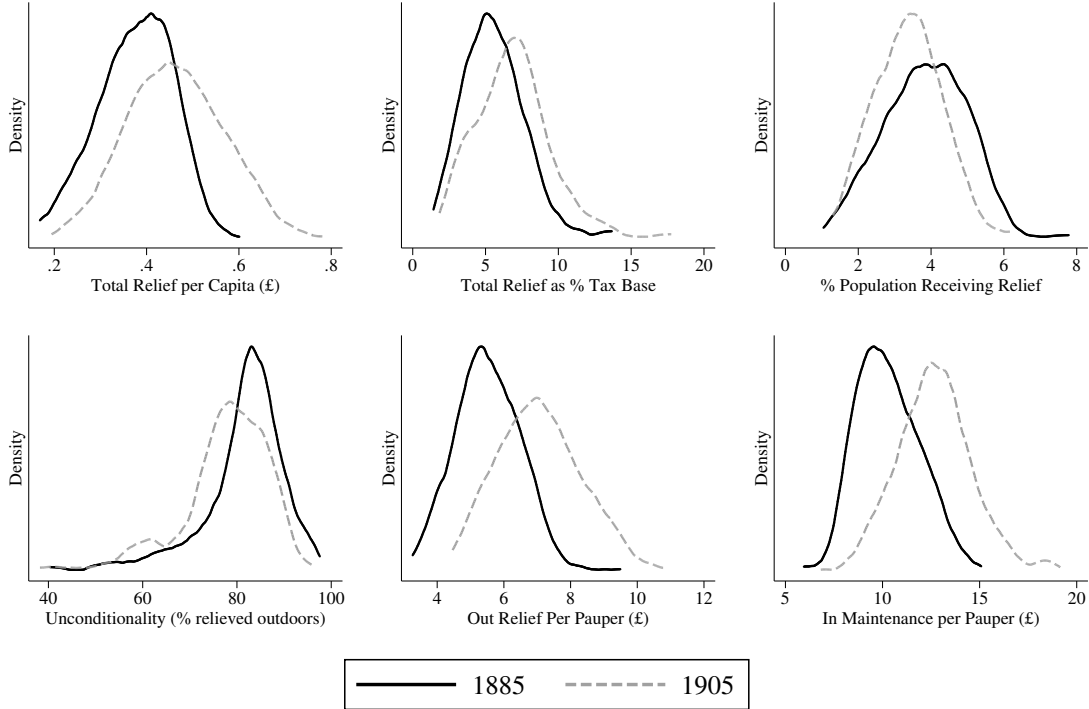
Note: Figure displays annual average across the 225 rural poor law unions included in the empirical analysis—see Section 3 for details of sample and variable definition. Financial variables are in real terms.

within the adult categories, most paupers were classified as “not-able-bodied,” indicating some form of temporary or permanent physical affliction. Further, many of those classified as “able-bodied” were also temporarily affected by ill health (MacKinnon, 1988).<sup>11</sup>

The breadth of support provided by the poor law meant that a significant proportion of the population would receive support through poor relief during their lifetime—even though the number of people receiving poor relief at any given point was relatively small. The proportion of individuals receiving relief on a single day in rural poor law unions ranged from 3%–6% between 1860 and 1911, implying that around 10% of the population received

<sup>11</sup>See Appendix A.4 for further discussion of these categories and a detailed breakdown of the classification of paupers in 1865 and 1895.

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



Note: Figure displays kernel density for the 225 rural poor law unions included in the empirical analysis—see Section 3 for details of sample and data sources. “Unconditionality” is the percentage of paupers relieved outside the workhouse, while “out relief per pauper” is the spending per outdoor pauper and “in maintenance” per pauper is spending per pauper in the workhouse. Financial variables are in real terms. Three poor law unions with in maintenance per pauper of over £20 in 1905 are excluded for display purposes.

support over the course of a year.<sup>12</sup> Support was particularly prevalent among the aged—over a one-year period, 30% of those over 60 were received poor relief (Boyer and Schmidle, 2009).<sup>13</sup> Even those in no danger of destitution themselves could thus benefit from more generous policy since magistrates had the power to compel relatives to provide monetary support to their destitute family members instead of providing poor relief (King, 2000, p20).

A large proportion of voters could thus benefit from a more generous poor law policy.

<sup>12</sup>As explained in Section B, the “stock” of paupers was reported on only two days each year; the proportion receiving support over a year is estimated based on data from MacKinnon (1988) indicating that in 1892, the nationwide proportion of paupers receiving relief over the year to those relieved on January 1 was 2.24.

<sup>13</sup>These estimates refer to all unions, as opposed to the 225 rural unions in the regression sample. Boyer and Schmidle (2009) also report that on a single date in 1890, 13.4% of those 60 and older received poor relief, and the number increases to 30% for those 80 and over.

Democratization should, according to a standard theoretical model of redistribution, have enabled voters to expand the provision of poor relief. Poorer voters were more likely to require relief and paid less tax—under the proportional tax system described above—than their wealthier counterparts. Further, the desired level of spending should be greater in more unequal districts where the benefit derived from taxing the rich was greater.<sup>14</sup> The remainder of the paper uses an 1894 democratic reform to test this theoretical proposition. I discuss the reform in detail in Section 4, but I first introduce the datasets used in the empirical analysis.

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<sup>14</sup>Or, alternatively, where the likelihood of requiring poor relief is greater.

## 3 Data

This section introduces the three main data sources used in the empirical analysis. Two of these sources relate to the operation of the Poor Law: one includes information on the number of paupers while the other provides information on the spending by poor law unions. These datasets are then matched with census information I use to construct the poor law union-level inequality measure.<sup>15</sup>

### 3.1 Sample

I analyze the effects of the 1894 reforms on a subset of 225 poor law unions that were rural in nature at the end of the analysis period.<sup>16</sup> Limiting the analysis to this group of poor law unions addresses two possible issues. First, differing levels of inequality may capture differences in occupational structure or some other aspect affecting the “type” of elite. A similar concern may be that changes in poor relief expenditure could capture the differing pace of urbanization in different districts—since the nature of poor relief varied considerably between town and country (both historians and contemporaries distinguish between the rural and urban poor law). By restricting the sample to only those unions that were rural in character throughout the period, I avoid this complication.

I trim the sample further by removing poor law unions that underwent substantial boundary changes during the period. In particular, I drop 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.<sup>17</sup> Fortunately, the boundaries of poor law unions were relatively stable across the second half of the nineteenth century—particularly when compared to most British local government areas at this time—meaning that relatively

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<sup>15</sup>Additional discussion of the data construction and summary statistics for the variables used in the regression analysis are provided in Appendix B.1.

<sup>16</sup>Specifically, these unions were identified as “wholly rural” in the 1909 Royal Commission on Poor Relief”.

<sup>17</sup>Boundary changes were identified using information reported at [www.ukbmd.org.uk](http://www.ukbmd.org.uk).

few (22) unions were dropped due to this restriction.

## 3.2 Pauperism and Poor Relief

The empirical analysis is based on part of a new dataset of poor law activity for the period 1860–1913. This dataset was collected from a long series of Parliamentary reports on the poor law which contained a range of information on the workings of the poor law in each of the poor law unions. Reports summarizing the financial accounts of each union were produced yearly, while the number of paupers in each union was reported biannually and distinguished between paupers relieved inside and outside the workhouse.<sup>18</sup>

The dependent variables in the regression analysis include 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.<sup>19</sup> 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).<sup>20</sup> The nominal figures are adjusted into real terms using the Bowley Cost of Living index (Mitchell, 1971, p.738).

I use the expenditure series to construct two dependent variables: expenditure per capita and expenditure as a percentage of the total district tax base—i.e., the value of the property eligible for taxation (the total “rateable value”). The former measure best captures the

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<sup>18</sup>The reports state the number of paupers relieved on January 1 and July 1; I average the two to create an annual figure. 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.

<sup>19</sup>Expenditure defrayed out of loans is not included, but the amortized cost of this expenditure is captured through spending on loan charges.

<sup>20</sup>In addition, the guardians were responsible for collecting taxes on behalf of other bodies, such as municipal boroughs.

generosity of the relief provided since it captures the amount each citizen would receive on average in the district. The latter, on the other hand, accounts for the differing financial constraints that poor law unions would have faced. It also has the advantage of being measured more accurately, as there is no need to interpolate population or to be concerned about using the correct price index.

Finally, I use the information on the number of paupers in each poor law union to construct a measure of volatility in the local labor market. In particular, I measure volatility as the standard deviation in the annual number of paupers per capita for each decade within each poor law union.

### **3.3 Inequality and Other PLU Characteristics**

One advantage of the poor law union data is that district boundaries were nearly always the same as those used for registration purposes. This fact allows me to link the poor law dataset to census data—and hence to construct my measure of inequality and to identify a number of demographic characteristics of the unions.

I measure inequality by analyzing the distribution of servants within households in 1881. 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 can therefore proxy the distribution of wealth within the elite.

I use the 100% census sample of the 1881 census (Schürer and Woollard, 2003) to identify the number of servants in each household. I then order the households in descending order according to the number of servants and identify the proportion of servants in the top 5% of households in each union. A higher share of servants implies that the union was more unequal. I then define three groups of poor law unions—*Low Inequality*, *Medium Inequality*,



and *High Inequality*—based on the terciles of this measure.

To identify the number of servants in the households, I start by identifying those whose relationship to the household head was reported as being a servant by census enumerators and those listed as employees whose occupation explicitly identified them as domestic servants (for example, governesses or coachmen). I then remove individuals whose occupational code identified them as non-servants, for instance clerks, or cases in which the “household” appeared to be an institution (such as a prison or boarding house). Full details of this procedure are provided in Appendix B.2.

Reassuringly, the servants inequality measure is strongly correlated with other measures of inequality based on the presence of elites within the poor law union. I geolocate the residences and landholdings of rural elites (gentry and aristocracy) in each poor law union, using information from Bateman (1883) and Walford (1886).<sup>21</sup> The correlation between the servants inequality and the number of country residences per acre is 0.46. Restricting to properties of “great landowners” identified by Bateman, the correlation is 0.38. The servants measure is also correlated ( $r=0.22$ ) with a rough measure of the proportion of the district owned by large landholders.<sup>22</sup> The servants measure thus captures the landholdings of elites in each poor law union.

Finally, I construct variables capturing other demographic characteristics of poor law unions using summary census data collected by Schürer and Woollard (2003). These datasets report the population disaggregated by age group for each registration district for each census year. I then interpolate geometrically between census years and construct annual measures of population, the share of the population aged over 65, the percentage of the workforce engaged

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<sup>21</sup>See Appendix B.3 for further details of the geolocation process. All but three of the unions contained at least one member of the gentry. All correlations in this paragraph are statistically distinguishable from zero at the 1% level.

<sup>22</sup>High measurement error likely means that this correlation is biased towards zero. The measure is constructed based on acreage reported by Bateman (1886). Specifically, I assign the acreage at county level to individual poor law unions, which is clearly imprecise, as many landholdings may have crossed union boundaries and could have been located separately from the residences of elites.

in agriculture, and population density. For the latter, I use the area of the poor law unions in 1881, estimated using GIS software.<sup>23</sup> These boundaries were also used to estimate union-level cereal suitability using data from the Global AgroEcological Zones (GAEZ) project of the Food and Agriculture Organization (FAO).

## 4 Empirical Specification

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 start by discussing the 1894 poor law reform that serves as the treatment event in the analysis. I then introduce the difference-in-difference specification before discussing potential threats to the identification strategy.

### 4.1 Democratic Reforms to Poor Law Governance

To identify the effects of democratic reform, I take advantage of an 1894 reform to the governance of poor law unions, implemented as part of the Local Government Act. Prior to the reform, elites benefited from four important institutional advantages: the presence of unelected officials on the boards determining policy, property qualifications for election as a guardian, a graduated voting system, and the absence of a secret ballot.<sup>24</sup> After 1894, all four of these advantages were removed, and councils were elected based on a one-man-one-vote system. The reform affected all poor law unions—including those in urban areas, which are not discussed here—hence it was exogenous to the preferences of either local elites or other local citizens.

The 1894 Local Government Act was part of a wide-ranging reform to local government;

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<sup>23</sup>Registration district maps were obtained from <https://www.visionofbritain.org.uk/data/>.

<sup>24</sup>These provisions were openly acknowledged as protections for landholders—see House of Commons (1878, e.g., paras 864–866, 5076).

its primary purpose was to establish a system of parish and district councils across England and Wales. Six years previously, the 1888 Local Government Act had established a system of democratically elected county councils, and by 1891, the need for further local government reform was accepted by the Liberal party (Keith-Lucas, 1952, p.40). New parish councils were established in villages, and a network of elected urban and rural district councils that controlled expenditure on public goods was established throughout the country. The effect on poor law unions was a secondary concern—if anything, it was only reluctantly accepted that the logic of democracy necessitated reforming these bodies as well (Hurren, 2015, p.215). The reform was thus in no way shaped by the anticipated effects on individual boards of guardians.

The broad changes to local government mean that, in principle, the reform could have 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 and tax-raising powers—and failed to inspire voter interest (Keith-Lucas, 1952, p.42). However, the creation of rural district councils was more troubling because guardians also sat as representatives on these councils (and the bodies that preceded them, the rural sanitary authorities).<sup>25</sup> 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.

In practice, however, the other changes wrought by the 1894 Act do not appear to be a major concern. Spending by these other bodies generally only amounted to around half of

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<sup>25</sup>See Appendix A.3 for a more detailed discussion of the relationship between the boards of guardians and the rural sanitary authorities, including the analysis discussed in the following paragraph. There were very few urban district councils situated within the rural poor law unions that are analyzed in this paper, so the changes to these bodies are not a major concern.

what was spent on poor relief each year, as discussed in detail in Appendix A.3. Further, the identification strategy relies on differences across inequality groups—and there is no evidence that the amount spent by these bodies varied according to the level of inequality. Finally, the main results are robust, both in magnitude and statistical significance, to including measures of spending by these other bodies, indicating that any effect of the reform on poor relief spending does not reflect alternations in the composition of expenditure.

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, the intermediate step of lowering property qualifications for elected guardians was taken by the Local Government Board.<sup>26</sup> 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. More generally, however, it is plausible that guardians would moderate their policy to increase their chances of winning an election under the new system implemented by the Local Government Act. Such a response would mean that the effects of reform are underestimated by the specifications here.

## 4.2 Empirical Specification

To test the effects of the 1894 democratic reform, I use a difference-in-difference approach, where the “treatment” is the degree of inequality in the union in 1881. That is, I test whether the reforms led to greater spending increases in more unequal districts—in other words, those areas which were (potentially) subject to greater elite control prior to 1894.

Specifically, I estimate the following specification:

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<sup>26</sup>Specifically, the qualification was reduced to £5 in all poor law unions—a level that would allow all but the poorest to serve as guardians. Rather than occurring through national legislation, this change was implemented by an order from the central Local Government Board in November 1892.

$$y_{it} = \beta_1 post1894\_x\_MediumInequality + \beta_2 post1894\_x\_HighInequality + \gamma_0 X'_{it} + T_t + \alpha_i + \epsilon_{it} \quad (1)$$

where  $i$  indexes poor law unions and  $t$  indexes years. “Medium Inequality” and “High Inequality” are indicator variables identifying whether the town was in the second or third tercile of the distribution of the “top 5% share of servants,” as discussed above.  $y$  refers to the two measures of poor law expenditure discussed above: the total poor relief spending per capita and the total relief as a percentage of the tax base (the rateable value of property).  $X$  is a vector of control variables,  $T$  represents year fixed effects, and  $\alpha$  represents poor law union (district) fixed effects.  $\epsilon$  is an error term.

The interaction terms in this specification test whether the relationship between the inequality measures and poor law policy changed after 1894, the year governance of poor law unions was reformed to reduce elites’ control. If democratization is followed by greater redistribution in more unequal societies, as suggested by the theoretical models discussed above, then  $\beta_1$  and  $\beta_2$  will be positive.

The fixed effects model controls for time-variant factors affecting the extent of poor relief across districts. Specific concerns here may include differences in the economic structure across unions, such as cultural differences affecting the type or generosity of poor relief; the occupational structure of the district; or the existence of alternative sources of relief, such as through charity. Year fixed effects capture any events common to all poor law unions, such as changes in guidance offered by the Local Government Board or widespread changes in economic conditions.

I include a number of control variables relating to the characteristics of poor law unions. A measure of population density is included to account for any variation in the degree of union

urbanization. To capture variation in demand for poor relief, I control for the percentage of the population aged over 65 and the extent to which pauperism (paupers per capita) varies within each decade—the latter measure addresses the fact that some unions may have been more exposed to unemployment shocks.

Policy towards poor relief would likely also have been affected by the financial resources of the each poor law union. I include two measures of these resources. First, I use the tax base (rateable value) per capita. Second, I control for the percentage of the union’s revenue raised through taxation, in case external sources of funding (such as property holdings, or government grants) affect the degree of expenditure.

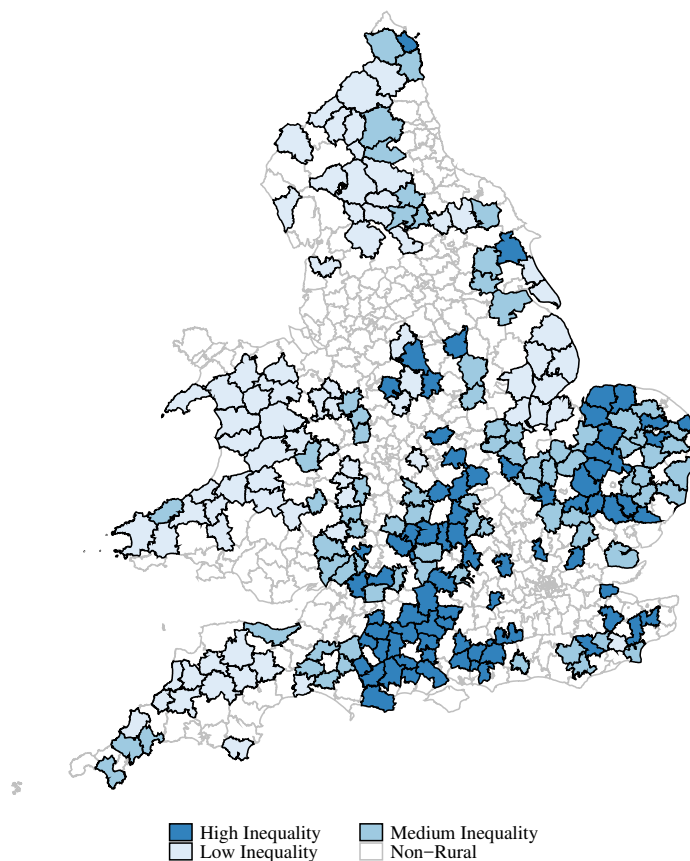
### **4.3 Threats to Identification**

There are two major threats to this identification strategy, both reflecting the fact that the level of inequality is not a randomly assigned treatment. First, the parallel trends assumption—that changes in relief across inequality groups would have been the same in the absence of reform—could be violated. Second, any treatment effect could be picking up on a reaction to some characteristic correlated with inequality rather than the level of inequality itself. In this section, I discuss these issues in more detail, highlighting in particular the characteristics correlated with the level of inequality. Section 5 then reports a number of robustness tests addressing remaining concerns.

First, we can see that there is clear geographic clustering of poor law unions with similar levels of inequality, as shown in Figure 3. Poor law unions in Wales, and northern and south-western England were, in general, much more equal. Unions in the east and south-east of England, in contrast, tended to be more equal. This might raise concerns that the results are driven not by inequality, but by other differences, such as culture, between regions. Additional tests, contained in Appendix C.3, demonstrate that this is not the case: the results are robust to both excluding individual regions and to constraining the analysis only

to regions with considerable variation in the degree of inequality.

**Figure 3: Rural poor law unions with similar levels of inequality are geographically clustered.**



Note: The figure presents inequality data for the sample of 225 rural poor law unions described in Section 3.1 and used in the regression analysis. Low, Medium, and High Inequality poor law unions are defined according to the distribution of domestic servants in each union, as discussed in Section 3.3.

A related concern is that the degree of inequality is highly correlated with the degree to which the land was suitable for cereal agriculture. The correlation between the servant inequality and the GAEZ measure of high-input, rain-fed cereal agriculture is 0.48, suggesting that the type of agriculture may well have differed substantially across the groups—with pastoral agriculture being more common in low inequality areas and arable farming being more common in high inequality regions. While any level differences in spending will be

addressed by the inclusion of fixed effects in our model, differences in the form of agriculture could plausibly have led to different trends in relief due, for instance, to changing patterns of demand. Further, the pattern of need for relief could vary according to agriculture type—leading to different responses to democratic reform.<sup>27</sup> I thus check that the findings are robust to both allowing for time trends and reactions to the reform that vary according to soil suitability.<sup>28</sup>

There are also some clear differences in other observable characteristics across the different groups, as shown in Panel A of Table 1. The most notable difference is that low inequality unions were considerably less dense on average. The average value of land per acre—although not per capita—was also lower in the most equal unions, suggesting that they may have contained a substantial portion of less habitable (e.g., mountainous) land. There are, however, no statistically significant differences in the extent to which the districts are agricultural (as measured by the percent of the population having occupations in agriculture), or the percentage aged over 64.

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<sup>27</sup>For instance, suppose post-reform voters cared only about providing a minimum level of support to all paupers. If more seasonal arable agriculture had significantly more paupers, we could observe higher post-reform spending in high inequality areas due purely to agricultural type.

<sup>28</sup>It would, of course, be preferable to measure agriculture type directly rather than relying on cereal suitability measures. Unfortunately, however, I am unaware of any data capturing these differences at the poor law union level.



**Table 1: High inequality districts were denser and aging more slowly.**

	Means			Differences		
	Low Inequality	Medium Inequality	High Inequality	Medium - Low	High - Low	High- Medium
Panel A: Level in 1881						
Population ('000s)	15.03 (7.399)	17.03 (6.705)	13.92 (7.639)	2.00* (1.153)	-1.11 (1.228)	-3.11*** (1.174)
Area ('000 Acres)	98.84 (42.171)	68.99 (28.393)	56.66 (22.124)	-29.85*** (5.870)	-42.18*** (5.499)	-12.33*** (4.156)
Density (Popn per Acre)	0.16 (0.065)	0.25 (0.075)	0.24 (0.103)	0.09*** (0.011)	0.09*** (0.014)	-0.01 (0.015)
% Aged over 64	7.11 (0.979)	7.32 (0.769)	7.02 (0.836)	0.20 (0.144)	-0.09 (0.149)	-0.29** (0.131)
Occupation Agriculture (%)	40.63 (8.595)	38.78 (8.172)	38.25 (8.831)	-1.86 (1.384)	-2.38* (1.432)	-0.52 (1.404)
Tax Base per Capita	6.81 (2.358)	7.08 (1.870)	6.59 (1.224)	0.27 (0.348)	-0.22 (0.307)	-0.49* (0.258)
Tax Base per Acre	1.04 (0.521)	1.69 (0.396)	1.56 (0.454)	0.64*** (0.076)	0.52*** (0.080)	-0.13* (0.070)
% Revenue from Poor Rate	94.28 (1.811)	93.23 (2.139)	93.39 (1.741)	-1.05*** (0.324)	-0.89*** (0.290)	0.15 (0.318)
Panel B: Change 1881-1901						
Δ Population	-0.81 (1.769)	-1.08 (1.620)	-0.04 (2.239)	-0.26 (0.277)	0.78*** (0.329)	1.04*** (0.319)
Δ Density	-0.01 (0.020)	-0.02 (0.024)	-0.00 (0.038)	-0.01*** (0.004)	0.01 (0.005)	0.02*** (0.005)
Δ % Aged over 64	0.25 (0.847)	0.72 (0.723)	0.63 (0.749)	0.47*** (0.129)	0.38*** (0.131)	-0.09 (0.120)
Δ Occupation Agriculture	-5.84 (5.370)	-5.22 (4.435)	-7.19 (3.675)	0.63 (0.813)	-1.34* (0.756)	-1.97*** (0.671)
Δ Tax Base per Capita	0.37 (0.947)	-0.31 (1.297)	-0.63 (0.929)	-0.68*** (0.185)	-1.00*** (0.153)	-0.32* (0.184)
Δ Tax Base per Acre	0.15 (0.249)	0.03 (0.336)	0.07 (0.368)	-0.12*** (0.048)	-0.08 (0.051)	0.04 (0.058)
Δ Revenue from Poor Rate	-15.23 (4.699)	-14.61 (4.842)	-14.94 (4.740)	0.62 (0.779)	0.29 (0.771)	-0.33 (0.782)

Notes: Standard deviations (columns 1–3) and standard errors (columns 4–6) are presented in parentheses. Asterisks refer to t-tests of the difference in means across the respective groups: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

More problematic than these level differences is the fact that, as shown in Panel B, the trends in some of these characteristics differed across the groups of poor law unions.<sup>29</sup> In particular, low inequality is associated with slower growth in the percentage of those over the age of 64 and faster growth in the tax base. The former could be expected to lead to slower growth in poor relief expenditure—as discussed in Section 2, the aged were particularly likely to receive poor relief. The latter could potentially be associated with more rapid increases in expenditure to the extent it drives higher wealth; however, this could also be associated with lower growth in expenditure as a percentage of rateable value, as it serves as the variables’ denominator. I therefore control for these differences in observable characteristics in the main specifications.

Examining the trends in expenditure (Figure 4) directly provides some, although limited, evidence of divergent trends before 1894. In general, it appears that average expenditure—both per capita and as a percentage of the tax base—was fairly constant prior to 1894, increasing rapidly in all three groups after the reforms. We observe a jump in expenditure, particularly in per capita terms, between 1893 and 1894. This largely reflects falling prices rather than increases in nominal spending but could also reflect anticipation of the reform, as discussed above.<sup>30</sup> There is some evidence that expenditure, as a percentage of rateable value, was increasing before 1894 in high inequality unions. However, as we will see in the following section, this difference largely disappears when we control for other characteristics. The findings are also robust to the inclusion of union-specific linear time trends.

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<sup>29</sup>Ideally we would assess trends up to the treatment date because, in principle, 1901 characteristics could be influenced by the treatment event. However, this is not possible since census data is only available decennially.

<sup>30</sup>See Appendix Figure A.2 for nominal series.

**Figure 4: Poor law expenditure increased after 1894 in all groups of poor law unions.**



Note: Figure displays data for sample of 225 poor law unions described in Section 4.1. Low, Medium, and High Inequality refer to the terciles of the % servants in the top 5% of households, described in Section 3. Expenditure data in left-hand panel are adjusted for inflation.

## 5 Results

The empirical analysis shows strong and robust evidence that more unequal areas experienced greater increases in government spending following the 1894 democratic reforms. I first show the results from the difference-in-difference specifications discussed in Section 4.2. The second sub-section then tests the effects of different measures of inequality, providing further evidence that the differential effects of the reforms were a result of heterogeneity in economic, rather than political, inequality.

## 5.1 Inequality Mediated the Effect of Democratic Reform

Democratic reform led to much greater increases in poor relief expenditure in more unequal poor law unions, as shown in Table 2. Relative to the most equal unions, expenditure in medium and high inequality unions increased by around 0.2–0.3 standard deviations in the eleven years following the reform. The magnitude of this effect is similar across both medium and high inequality unions (the coefficients are statistically indistinguishable), suggesting that the key to this change may have been the existence of an elite that could limit expenditure before the reforms occurred.

The estimated effect of the reforms is similar when controlling for both demographic (specifications (2) and (5)) and financial (specifications (3) and (6)) characteristics of the poor law unions. As expected, given the high proportion of citizens receiving relief in old age, there is a strong relationship between the percentage of the population aged over 64 and expenditure on poor relief. Perhaps surprisingly, there is little evidence that wealthier unions spent more on poor relief.

These findings suggest that inequality can play an important role in mediating the impact of democracy on welfare expenditure. The estimates imply that, relative to low inequality unions and measured in terms of the pre-reform median, the reforms increased spending in high inequality unions by approximately 6%–9% in per capita terms and by 9%–18% as a percentage of the tax base. Further, while these effects are not exceedingly large, it is important to recognize that they are not capturing the entire effect of democratization. Rather, they reflect the change in expenditure relative to the control group of “low inequality” unions, which were themselves likely quite unequal and which, in principle, would also have experienced growing expenditure following the reforms.<sup>31</sup>

The difference-in-difference design does not causally identify the entire effect of the re-

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<sup>31</sup>As discussed in the following section, we see qualitative evidence that the reform led to greater spending in Stow, which is classified as low inequality in the regression analysis.

**Table 2: More unequal districts had greater increases in expenditure following 1894 reforms to the governance of poor law unions.**

	DV=Relief per Capita			DV=Relief as % tax base		
	(1)	(2)	(3)	(4)	(5)	(6)
Medium Inequality_x_post1894	0.35*** (0.080)	0.24*** (0.072)	0.21*** (0.072)	0.38*** (0.072)	0.32*** (0.070)	0.19*** (0.056)
High Inequality_x_post1894	0.34*** (0.079)	0.26*** (0.071)	0.24*** (0.074)	0.49*** (0.069)	0.44*** (0.064)	0.24*** (0.057)
post1894	0.66*** (0.067)	0.57*** (0.068)	0.85*** (0.091)	0.28*** (0.059)	0.22*** (0.062)	0.55*** (0.070)
Population		0.18 (0.596)	0.17 (0.606)		0.17 (0.463)	0.07 (0.451)
Population Density		-0.41 (0.562)	-0.56 (0.574)		-0.20 (0.419)	-0.32 (0.427)
% of Population Age Over 64		0.33*** (0.082)	0.33*** (0.081)		0.21*** (0.067)	0.22*** (0.065)
Decadal Variance in Pauperism		0.00 (0.016)	0.00 (0.016)		0.01 (0.016)	0.01 (0.013)
% Revenue from Poor Rate			0.11*** (0.029)			0.08*** (0.024)
Tax Base p.c.			-0.06 (0.066)			-0.83*** (0.088)
No. Observations	4725	4725	4725	4725	4725	4725
No. PLUs	225	225	225	225	225	225
Year Fixed Effects	Y	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y	Y

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . All variables are standardized. See Section 3 for variable definitions. Standard errors are clustered by poor law union and presented in parentheses.

forms, but we can see that spending also increased in low inequality poor unions after 1894. Spending in these unions increased by between 9% and 23% of the pre-reform median (the “post1894” coefficient), suggesting that democratization could also have significantly affected those areas. In turn, this would imply that inequality considerably magnified the effect of the reforms: the coefficient related to being “high inequality” is between 25% and 200% of that capturing the post-reform growth in low inequality unions. While these are not

causal estimates, they provide suggestive evidence that the effects of democratic reform were magnified considerably by the presence of greater inequality.

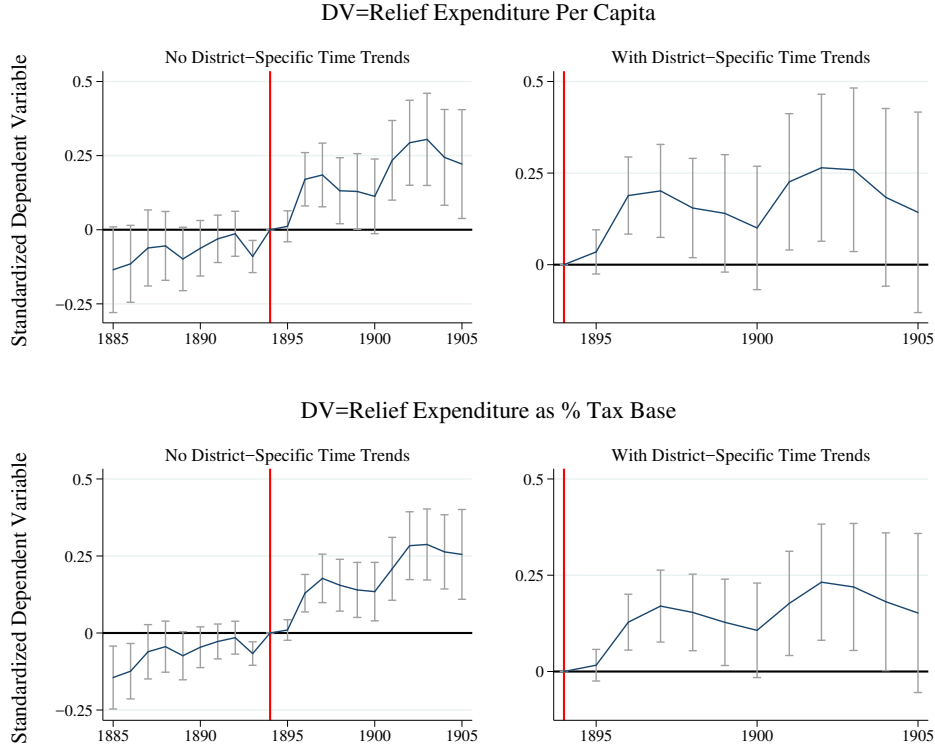
Figure 5 shows the results from a specification allowing for a flexible response to the reform.<sup>32</sup> The top and bottom panels relate to the two dependent variables, while the left- and right-hand sides correspond to differing specifications. On the left-hand side, I allow for differences in expenditure between the groups of unions (relative to 1894) to emerge in any year before or after the reform, while on the right-hand side, I include linear unit-specific time trends and allow for a flexible post-1894 reform response. We can see that there is limited evidence of any differences in the pre-trends between the groups, particularly in the years immediately preceding the reform.<sup>33</sup> Further, the results are similar (albeit noisier) after controlling for unit-specific time trends: in all four panels we see some evidence that the response to the reform grew over time. However, the size of the standard errors means we cannot distinguish these differences precisely.

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<sup>32</sup>For display purposes, only the coefficients for high inequality poor law unions are displayed. Those for medium inequality unions are similar in magnitude and statistical significance; they are presented in Appendix D.

<sup>33</sup>Appendix C shows that the results are similar when restricting the sample to the years after 1890.

**Figure 5: Effects of reform remain after controlling for union-specific time trends.**



Note: The left-hand panels plot  $\beta_j$  and  $\beta_k$ , and associated 95% confidence intervals, from the specification:

$$\begin{aligned}
 y_{it} = & \sum_{j < 1894} \beta_j (High\_inequality * year_j) + \sum_{k \geq 1895} \beta_k (High\_inequality * year_k) \\
 & + \gamma_j (Medium\_inequality * year_j) + \sum_{k \geq 1895} \gamma_k (Medium\_inequality * year_k) \\
 & + \delta X_{it} + \alpha_i + \mu_t + \epsilon_{it}
 \end{aligned}$$

The right-hand panels plot  $\beta_j$  and  $\beta_k$ , and associated 95% confidence intervals, from the specification:

$$\begin{aligned}
 y_{it} = & \sum_{k \geq 1895} \beta_k (High\_inequality * year_k) + \sum_{k \geq 1895} \gamma_k (Medium\_inequality * year_k) \\
 & + \delta X_{it} + \alpha_i + \mu_t + T_{it} + \epsilon_{it}
 \end{aligned}$$

where  $T_{it}$  are district-specific time trends. The excluded category in both cases relates to 1894, so all results are relative to the year immediately prior to the reform.  $X_{it}$  includes the controls included in specification (3). Coefficients  $\gamma_j$  and  $\gamma_k$  are displayed in Appendix Figure D.5. Standard errors are clustered by poor law union.

Given the noisiness of the individual year coefficients, I carry out additional specifications allowing for the democratic reform to have changed both the level and the trend in spending on poor relief. This approach allows me to test whether the effect of democratization grew over time, while accounting for union-specific time trends. The flexibility of the specification also allows me to check whether any part of the effect we observe in Table 2 could be driven by the other observable differences between the inequality groups discussed in Section 4.3. In particular, I check whether the results are similar when allowing for the reaction to the reforms to differ by tercile of cereal suitability, 1881 population density, the 1881 proportion of the population aged over 64, or the land value (the tax base per acre).<sup>34</sup>

The results of these specifications, displayed in Table 3, suggest that the reform had an immediate impact on spending in high inequality unions, but took longer to take effect in the medium inequality group. We can see that, in general, there is statistically significant evidence of a change in the intercept (“x\_post1894”) for high inequality groups post the 1894 reform, but the findings are much weaker for the medium inequality group. When looking at the change in trend (“x\_time\_x\_post1894”), in contrast, the results are stronger for the medium inequality group. Further, the size of the coefficients is consistent across specifications, supporting the hypothesis that the coefficients capture responses to the level of inequality in each poor law union, and not pre-existing trends or other correlated characteristics. Relative to the low inequality poor law unions, both medium and high inequality groups experienced an increase in spending—but it happened more quickly where inequality was higher.<sup>35</sup>

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<sup>34</sup>Full results of the specifications, including interactions with the percentage of the population engaged in agriculture, are displayed in Appendix Table C.5.

<sup>35</sup>There is no statistically significant difference in the combination of the change in trend and change in intercept across the two groups in 1905.



**Table 3: The effect of democratic reform was more immediate in high inequality unions.**

	DV=Relief per Capita				DV=Relief as % Tax base							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<b>Medium Inequality</b>												
x_post1894	0.02	0.05	0.02	0.05	0.03	0.05	0.05	0.06*	0.06	0.06*	0.08**	0.09**
	(0.070)	(0.054)	(0.059)	(0.054)	(0.057)	(0.062)	(0.049)	(0.036)	(0.041)	(0.036)	(0.038)	(0.041)
x_time_x_post1894	0.03***	0.04***	0.05***	0.04***	0.04***	0.04***	0.02***	0.02**	0.03**	0.02***	0.02**	0.03**
	(0.009)	(0.013)	(0.015)	(0.013)	(0.014)	(0.015)	(0.007)	(0.010)	(0.011)	(0.009)	(0.010)	(0.011)
<b>High Inequality</b>												
x_post1894	0.13*	0.11**	0.09	0.13**	0.10*	0.11*	0.11**	0.08**	0.07*	0.09**	0.10**	0.10**
	(0.069)	(0.057)	(0.061)	(0.055)	(0.059)	(0.065)	(0.048)	(0.039)	(0.042)	(0.038)	(0.039)	(0.044)
x_time_x_post1894	0.02*	0.01	0.02	0.01	0.01	0.02	0.02***	0.01	0.01	0.01	0.01	0.01
	(0.010)	(0.015)	(0.016)	(0.015)	(0.015)	(0.016)	(0.008)	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)
PLU-Specific Trends	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y
<b>Interactions</b>												
Population Density	N	N	Y	N	N	N	N	N	Y	N	N	N
% Over 64	N	N	N	Y	N	N	N	N	N	Y	N	N
Cereal Suitability	N	N	N	N	Y	N	N	N	N	N	Y	N
Tax Base per Acre	N	N	N	N	N	Y	N	N	N	N	N	Y
No. Observations	4725	4725	4725	4725	4725	4725	4725	4725	4725	4725	4725	4725
No. PLUs	225	225	225	225	225	225	225	225	225	225	225	225
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: All variables are standardized. See Section 3 for variable definitions. PLU-specific trends refer to union-specific linear trends. “Controls” are those reported in specifications (3) and (6) in Table 2. Standard errors are clustered by poor law union and presented in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

A number of additional robustness tests are reported in the Appendix. The specifications in Table 3 are repeated, allowing for quartic time trends according to union characteristics (rather than post-1894 interactions); again, the results are similar. Table C.7 shows that the results are robust to restricting the sample to unions within the common support of each of the characteristics. Appendix C.3 demonstrates that the results here are not driven by the geographic clustering of unions described in Section 4.2. In particular, the results are similar when removing each of the ten census regions one by one, as well as when restricting the sample only to the three regions (South-West, East Midlands, and West Midlands) that contain multiple unions in each of the inequality groupings. Thus, the relationship with inequality does not appear to capture other regional differences, such as cultural attitudes to poor relief.

## 5.2 Distinguishing Economic and Political Inequality

The analysis so far has provided robust evidence that inequality mediated the effects of the 1894 reforms; the remainder of the paper investigates the mechanisms through which inequality affected spending outcomes. This subsection introduces empirical specifications with alternative measures of inequality and tests whether the results are driven by economic inequality—the distribution of income or wealth—or by political inequality—the presence of politically influential elites. The following section then uses historical evidence to understand the political effects of reform.

The servants measure of inequality offers limited scope to disentangle different mechanisms because it focuses on a relatively well-off group—households with servants.<sup>36</sup> Under the hypothesis of (Meltzer and Richard, 1981), greater redistribution should occur where the median income is low (relative to the mean). However, the servants measure cannot dis-

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<sup>36</sup>In this respect, the servants measure is similar to all measures of wealth inequality, with a large proportion of the population having zero wealth—in this case proxied by servants.

tinguish this form of inequality from inequality purely among elite households, such as the presence of a few very wealthy individuals. The latter may offer an easy target for high taxation<sup>37</sup> or, alternatively, could hold a great deal of political power. A wealthy elite may have been able to dominate Boards of Guardians due to the graduated voting system, because they could serve as *ex-officio* Guardians, or through informal pressure on other constituents. The economic inequality captured by the servants measure would then be capturing heterogeneity in elites' political power before the 1894 reforms, and the results could reflect elites in high inequality areas having more control pre-reform—rather than being taxed more afterwards.

To disentangle these mechanisms, in Table 4 I implement additional specifications including interactions with alternative measures of inequality. In each case, I split the measure into terciles, and then start by including the measure on its own (analogous to specifications (3) and (6) in Table 2), before adding the servants inequality measure. The first two rows of the table then relate to the servants measure—our main measure of inequality. The next two include the wage of an agricultural laborer, as an approximation to the post-reform median income in the district.<sup>38</sup> The following four rows then measure political inequality: first through the size of the elite (the number of gentry residences) in the union per acre, and then the presence of a peer in the district.<sup>39</sup>

The results suggest that it was differences in economic inequality that explain the heterogeneous effect of democratization. The first two columns show that the reform had less effect where the median voter was wealthier—consistent with a Meltzer-Richard mechanism. Further, this effect remains when including the servants' inequality measure: the effect of

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<sup>37</sup>The restrictions on proportional taxation system in place in Poor Law Unions meant that it was not possible to implement progressive taxation, or to simply expropriate the wealthy. However, in principle, it could still have been easier to extract revenue from a few very wealthy individuals because they had a larger taxable surplus or because their income was more observable.

<sup>38</sup>Wage data is measured at county-level in 1898, and was obtained from Collins and Thirsk (2000, Table 42.3). Data for Rutland is unavailable, and so the number of observations is slightly lower in these specifications.

<sup>39</sup>A landowner was identified as a peer if they had a title of Count, Marquis, Duke, Earl, Lord, Viscount, or Baron.

democratization was greater in districts with both high inequality amongst the elite and a poor median voter.<sup>40</sup>

In contrast, the simple presence of elites does not appear to explain the results. The coefficients for the measures capturing the number of gentry per acre, or the presence of a peer, are only sometimes positive, smaller in magnitude than the main estimates, and not always statistically significant. Further, the servants measure clearly dominates when both sets of interactions are included in a “horse race.” Additional regressions in Appendix C.4 show similar results controlling for the simple number of elites (rather than normalized per acre), or including only very wealthy landowners as elites.

This section has provided robust evidence that higher inequality areas experienced greater increases in poor relief expenditure following democratic reforms. Further, the effect of the reforms was mediated by economic inequality, rather than the presence of either the very wealthy or politically influential individuals. I now turn to the political changes wrought by the Local Government Act, to understand how higher taxation was implemented in practice.

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<sup>40</sup>As shown in Table C.11, the results are similar when replacing the servants inequality measure with the estimated share of land in the union owned by Bateman’s “great landowners”—those owning 3,000 acres with a rental value of at least £3,000—reported in Bateman (1883)..

**Table 4: Economic, rather than political economy, mediated effects of democratic reform.**

	DV=Total Relief per Capita				DV=Total Relief as % Tax Base							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
% Servants in Top 5% of households (Main Inequality Measure):												
Middle Tercile		0.17** (0.076)		0.19** (0.079)		0.22*** (0.074)		0.14** (0.059)		0.16*** (0.058)		0.19*** (0.056)
Top Tercile		0.17** (0.081)		0.22** (0.088)		0.25*** (0.075)		0.16*** (0.062)		0.20*** (0.067)		0.24*** (0.058)
Median Income (Wage of Farm Laborer):												
Middle Tercile		-0.23*** (0.080)		-0.19** (0.084)				-0.23*** (0.064)		-0.19*** (0.066)		
Top Tercile		-0.26*** (0.081)		-0.20** (0.086)				-0.26*** (0.065)		-0.21*** (0.068)		
Elite Residences per Acre:												
Middle Tercile				0.16** (0.075)						0.18*** (0.060)		0.10 (0.064)
Top Tercile				0.13* (0.075)						0.13** (0.059)		0.03 (0.066)
Peer Resident in the Union:												
Yes						0.02 (0.063)		-0.02 (0.064)			0.02 (0.050)	-0.01 (0.049)
No. Observations	4683	4683	4725	4725	4725	4725	4683	4683	4725	4725	4725	4725
No. PLUs	223	223	225	225	225	225	223	223	225	225	225	225
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . All variables are standardized. "Controls" include variables in specification (3) and (6) of Table 2. Standard errors are clustered by poor law union and presented in parentheses.

## 6 The Political Economy of Poor Relief

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. There was also, however, significant continuity in political representation and continuing constraints on mass politics. While the poor significantly gained political power, there were thus constraints on their ability to implement enduring radical increases in expenditure.

The reforms led to major changes in the composition of boards of guardians: across a sample of 93 unions, 46% of 3,690 guardians had not served before 1894. 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).<sup>41</sup> Much of this change reflected the removal of *ex officio* Guardians and consequently the gentry from the boards. While these individuals could have stood for election, they often chose not to do so—a fact explained by contemporaries as occurring because they shied away from a contest, or because they did not wish to mix with less refined board members.<sup>42</sup> As a consequence, the gentry were a much reduced force on the boards of guardians after 1894.

The changing composition of the boards of guardians could have dramatic effects on poor relief spending, and particularly on the provision of outrelief. In Stow Union, prior to 1894 the guardians “were very particular as to the grant of outrelief, and if the least doubt existed as to the condition of the applicant, an order for admission to the workhouse only

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<sup>41</sup>We cannot, unfortunately, examine the make-up of whole boards as no comprehensive records exist: Unions generally did not even inform the Local Government Board as to election results. The discussion in this section thus draws on Parliamentary Papers, particularly evidence to the 1909 Royal Commission on the Poor Laws, and Local Government Manuals from 1893 and 1898, which listed the Poor Law Chairmen. Further discussion is included in Appendix A.2, with Appendix Table A.1 providing a summary of the evidence provided to the 1900 Royal Commission for each of thirty unions.

<sup>42</sup>The decision of the gentry not to stand after democratic reform is in line with the findings of Berlinski et al. (2011), who find that incumbent Members of Parliament were less likely to run for election in areas most affected by the 1867 Second Reform Act.

was given. In the year 1895, the new qualification admitted a majority of laborers to the board, and [their] policy was immediately subverted. In a few weeks, the outrelief jumped from £45 to £92 a week, and later to £100.” By 1896, in Brixworth Union, famous for its restriction of outdoor relief, the newly elected board had not only reintroduced outrelief, but also implemented upgrades to the workhouse (Hurren, 2015). In both cases, democratic reform had allowed decades of policy to be rapidly overturned.

At the same time, there were also important continuities between the pre- and post-reform periods. While the role of landowners was diminished, farmers were predominant on boards before and after 1894.<sup>43</sup> More generally, 54% of chairmen of the boards stayed the same between 1893 and 1898, with a similar proportion even of *ex officio* chairmen. Neither of these continuities preclude changing policy—indeed it was changing votes within the farming bloc that led to more liberal policy in Brixworth—but they do suggest limits on the ability to implement radical reform.

These continuities are explained in part by the remaining barriers to mass politics even after 1894. Despite the removal of property qualifications, poorer citizens simply did not have the time or the financial resources to regularly take part in elections. In the Stow Union, discussed above, the initial movement of agricultural laborers was funded by an agricultural union—and dwindled as interest and financial resources began to run out (House of Commons, 1909, paras, 75382, 75427). Further, guardians continued to be elected at parish-level, forming a further barrier to broad political movements—in Brixworth the pro-outrelief movement won a majority of votes across the Union but less than a quarter of available seats on the Board of Guardians (Hurren, 2015, Table 9, p.235).

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<sup>43</sup>The 1909 Commission concluded that in rural areas “there is an overwhelming mass of testimony to show that the tone and policy of the Boards of Guardians is largely determined by the tenant farmers, who are in a great majority. Interspersed among the mass of farmers are clergymen, still fewer country gentlemen, and a sprinkling of land agents and small tradesmen. On the one hand, as a rule, 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).

Nevertheless, it appears that the increases in poor relief observed in the previous section benefited the destitute rather than the farmers sitting as guardians. Before the New Poor Law, farmers used poor relief to essentially subsidize rural wages in off-season (Boyer, 2006)—in which case the expansions above could be capturing something other than a desire for greater redistribution. The qualitative evidence, discussed above and in detail in Appendix A.2, suggests otherwise—growing outrelief was sought by the poor, and reflected laxer policy to all citizens rather than being targeted at healthy male laborers. Further, as detailed in Appendix D.2, there is no evidence of any seasonal effect on the number of outdoor paupers after 1894. Finally, additional specifications suggest that the longer term effects of reform were due to improved spending on relief within the workhouse—expenditure that is clearly targeted at the needs of paupers rather than employers.

Poorer citizens were, in fact, able to influence policy even without being elected to boards themselves. The 1909 Royal Commission on the Poor Laws expressed concern that “in many places Guardians are...interested chiefly in administering relief to their near neighbors, while the publicity given to their work by means of newspaper reports makes them even more liable to undue pressure from their constituents” (House of Commons, 1909, p101). Several witnesses to the Commission testified that the poor took an interest in the poor law, particularly after 1894, while turnout in rural poor law elections appears to have often been high. The perception that relief had been denied unfairly could lead to protests and trigger electoral contests, creating implicit pressure on guardians to act in accordance with their electorate’s wishes. As a result, in the eyes of a contemporary Liberal MP, the act ensured that “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).

More generally, the Local Government Act did not lead to the emergence of a democracy in a modern sense. As was the case throughout British politics, few poor law elections were



actually contested. Nor is there much evidence of party politics in rural poor law elections.<sup>44</sup> While the reforms substantially increased the political power of the poor, the institutions that would allow their full representation would take longer to develop. If such institutions had existed, the effect of democracy on welfare expenditure could have been even greater.

## 7 Discussion

This paper has presented a new test of the relationship between democratization, inequality, and redistribution in nineteenth-century England and Wales. This context offers a valuable setting to test theories about redistribution since responsibility for welfare policy, in the shape of poor relief, was held by a system of locally elected government bodies. These bodies were tasked only with administering the poor relief system, meaning that demand for redistribution can be isolated from the demand for other types of government spending.

The results offer support for the hypothesis that greater inequality should make elites more wary of granting democratic reform. Difference-in-difference estimates indicate that government spending increased by between 5% and 18% of the pre-reform median in high inequality areas, relative to their low inequality counterparts. In turn, this suggests that inequality could substantially magnify the effects of democratic reform—non-causal estimates suggest that high inequality areas experienced spending growth between 25% and 200% of the change in low inequality areas. Poor law politics was by no means a mass democracy after 1894, but the reforms did reduce the ability of elites to control poor law expenditure—and that loss of control was most costly where inequality was higher.

The findings also suggest a channel through which elites are able to hold onto power after a democratic transition such as Britain's 1832 Great Reform Act. By decentralizing authority

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<sup>44</sup>There is more evidence that national party politics played a role in urban areas. Even there, however, there were no standardized policy positions within parties: in some areas the Tories could be against the New Poor Law, while in others they could be strong proponents of it (Fraser, 1976).

over the poor law, landowners managed to escape the control of both central authority and local scrutiny. The increasingly democratized Westminster parliament could address the needs of the growing industrial cities, while leaving traditional political structures in rural areas largely undisturbed. In fact it was only after agricultural laborers were enfranchised by the 1832 Reform Act that the local institutions controlled by rural elites were reformed—first through the 1835 Local Government Act, and then finally the 1835 Local Government Act discussed in this paper. Landowners played a large role in shaping the initial implementation of the New Poor Law: by doing so they were able to limit the redistributive impact of democratization for a further sixty years.

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# Online Appendix—Not Intended for Publication

## A Additional Historical Background

This appendix provides further historical background regarding the poor law. First, I provide additional details of the governance system used in poor law unions, including the qualifications necessary to vote and to be a guardian, as well as the electoral procedure used in poor law elections. The second subsection discusses the historical evidence relating to electoral behavior and election outcomes that underpins much of Section 6. Following this, I discuss the guardians’ role in the bodies providing rural public goods and present evidence that the spending by these authorities does not confound the main findings. Finally, I draw on the dataset outlined in Section 3 to describe the type of support provided through the poor law and the trends in poor law policy over time.

### A.1 Governance of Poor Law Unions

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

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

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

in another...earning or possessing any independent resources will be considered a bar to relief. Some Guardians will not relieve the sick dependents of an able-bodied man at all unless the man himself comes into the workhouse, while other Guardians make no effort whatever to exclude even the well-to-do classes from receiving gratuitously both indoor and outdoor medical relief. Innumerable instances of the existing lack of uniformity will be found throughout our evidence, and this diversity applies to every form of relief and to every class of applicant. In no cases is the varying interpretation placed on the claim to relief better illustrated than in the case of the widow. In one part of the country a widow with one child would get no relief whatever unless she came into the workhouse; in another part of the country she would, indeed, get out-relief, but nothing for herself; in a third district she would get as much as 5 [shillings] for herself and 4 [shillings] for her child; and in a fourth district she would get relief only if she consented to part with her child and send it into a Poor Law School. Or again, in one Union outdoor medical relief may be freely given to all applicants without any inquiry as to means, whilst in another Union it is refused to all persons unless they are in receipt of ordinary outdoor relief. (House of Commons, 1909, pp101–102)

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

The focus of this paper is on the changes to the way guardians were elected created by the 1894 Local Government Act. This act affected multiple aspects of how elections were conducted, including, most importantly, implementing a secret ballot, removing *ex officio* guardians, replacing a system of a graduated franchise with a one-man-one-vote system, and removing the property qualifications required to act as a guardian. Each of these aspects are discussed in detail below.<sup>1</sup>

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<sup>1</sup>The administrative procedures for the poor law were very detailed, and it is only possible to provide a high-level overview here. For additional detail, readers are referred to the parliamentary papers and secondary sources cited in the text below. In particular, the discussion of the pre-1894 system draws heavily on the 1878 Select Committee on the Election of the Poor Law Guardians (House of Commons, 1878), and that of the 1894 changes on MacMorran and Colquhoun 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).

## Election Procedure

Voting for the guardians occurred in April each year, except for one-off November elections as part of the transition from the pre-1894 system. Prior to 1894, the entire board was elected annually; after the Local Government Act, guardians were instead elected for three years, with one-third of guardians leaving office each year (if not re-elected).<sup>2</sup> The Local Government Act was implemented by electing whole boards of guardians under the new regulations in November 1894. After that point, one-third could continue until 1896, a further third until 1897, and the final third until 1898. There were also unelected guardians on the boards, both before and after 1894. The 1834 New Poor Law allowed all county magistrates (justices of the peace) to sit as *ex officio* guardians on the boards. These *ex-officio* guardians were removed by the 1894 act, but instead boards could co-opt up to four persons from outside of the body—including a chairman or vice-chairman—as additional guardians to gain outside expertise. However, in practice, few did so. In 1908, only 370 of a possible 2,570 members had been co-opted, and two-thirds of boards (425 out of 643) had not co-opted anyone at all (House of Commons, 1909, p100).

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

The criteria for a proper ballot were somewhat stringent:

“each voter shall write his initials in the proper column of the voting paper delivered to him, against the name or the names of the person or persons (not exceeding the the number of guardians to be elected...for whom he intends to vote, and, shall sign his name at the foot of the voting paper; and when any person votes as a proxy, he shall in like manner write his own initials and sign his own name...but if the voter cannot write then he may affix his mark at the foot of the voting paper in the presence of a witness, and that witness has to attest it, and he has to write the name of the voter against the mark, and the initials of the voters against the name of the person for whom he votes; *and there is an express provision that if those regulations are not complied with in every respect, the*

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<sup>2</sup>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). Unions could apply for guardians to be elected only every three years, but this was rare: only 26 unions, all in towns, had done so in 1878 (para 71 House of Commons, 1878). 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.

*voting paper shall be void and rejected in the calculation of votes.”* Evidence of Mr. Fry, Assistant Secretary of the Local Government Board House of Commons (1878, para 15) (emphasis added).

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

After 1894, elections involving 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.<sup>3</sup>

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).<sup>4</sup> 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, 1878). 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

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<sup>3</sup>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.

<sup>4</sup>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).

their property holdings.

Two less significant changes to voting rights are worthy of note. The first relates to the female franchise. Women, if rated as taxpayers, could vote throughout the period (McClaren, 1987, p480). However, married women were potentially disenfranchised on the basis of coverture, under which husband and wife became one person in law. This disqualification was largely removed by the 1882 Married Women's Property Act, but some legal ambiguity remained. A stipulation in the 1894 LGA ensured there was no disqualification as a result of marriage. However, the group affected by this part of the legislation was very small; since property was generally rated to the husband, only married women either living away from their husbands or keeping a separate business would have been affected.<sup>5</sup>

A further set of changes in the franchise relates to the right to vote of recipients of poor relief or other charities.<sup>6</sup> 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.<sup>7</sup> 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.<sup>8</sup> 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

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<sup>5</sup>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.

<sup>6</sup>The pauper disqualification the local level is discussed in Keith-Lucas (1952, p161–163) and at the national level in Briggs (1979).

<sup>7</sup>After 1894, poor law elections used the register as defined in the 1888 County Electors Act.

<sup>8</sup>Evidence to the 1878 Select Committee indicated variation across areas; for example, it was reported that all occupiers were entered into the ratebook in Wolverhampton and Spalding (paras 1822, 2462) but not in Eastbourne (para 834–840).

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, 1878, paras 731–741, 763–765, 839).

Individuals would also lose the right to vote if they moved between the time the ratebook was compiled and the date of the election. Poorer voters were more likely to move, and hence this particularly affected them (House of Commons, 1878, 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 compiled 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, 1878, paras 1666–67, 2167).<sup>9</sup>

### **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, 1878, para 496). In 1893, this limit was reduced to £5 and then removed entirely in the 1894 Local Government Act.<sup>10</sup>

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<sup>11</sup>. Following the 1894 Act, the number of female guardians in urban areas grew substantially; however, female representation remained low in rural districts. In 1908, only 146 of the 16,001 Rural District Councilors were female (House of Commons, 1909, p651). Therefore, it does not appear that the reform had a major effect in terms of increasing the representation of women on councils.

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<sup>9</sup>In contrast, municipal elections occurred in November, meaning that the ratebook was compiled a year in advance; this was given as the principal explanation for differences in turnout between the elections for the two different bodies.

<sup>10</sup>See *Essex Standard*, Saturday 3 December 1892, p.5 for discussion of the initial reduction in property qualifications.

<sup>11</sup>See Webb and Webb (1929, p234). The number of female guardians across the entire country increased significantly over our period of interest: In 1885, there were 50 female guardians; in 1895, there were 839; and in 1907, there were 1141.

## A.2 Elections in Poor Law Unions

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

### Composition of Boards

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

We can also glean some information regarding the composition of poor law boards by examining the tenure of chairmen and vice-chairmen of the boards of guardians—positions chosen by the guardians each year. I investigate these using two contemporary sources: first, an 1893 parliamentary paper identifying whether these officials were *ex officio* or elected, as well as their length of tenure, and second, directories from 1893 and 1898 listing the names of individuals in each position for every poor law union. These sources show that 46% of boards within the regression sample had *ex officio* chairmen in 1893 (36% across all poor law unions)—37% of whom were still in place five years later—indicating either that they had chosen to stand for election or had been co-opted onto the board by the other guardians.

### Contested Elections and Turnout

Few poor law union elections were contested, particularly in rural areas. Of the thirteen unions in Table A.1 with relevant evidence, only Spalding reports a considerable number of contests. In others, it appears only around one-tenth of elections were contested—and sometimes even fewer. This lack of contests was characteristic of the poor law long before 1894: nationwide there were contests for fewer than 4% of guardianships between 1873 and 1875. Further, these contests were mostly in urban areas rather than the rural unions on which this paper focuses House of Commons

(1878, 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, 1878, 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.<sup>12</sup>

Where elections did take place, turnout in rural areas appears to have been quite high. The witnesses listed in Table A.1 suggest turnouts of around 85%–90% were the norm. These high turnouts contrast with the general consensus on poor law turnouts, which appear to be based on the (much more frequent) contests in urban parishes. In general, turnout was around 20%–30% of the electorate (House of Commons, 1878, 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).<sup>13</sup> 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

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<sup>12</sup>These statistics relate to the 225 unions in the regression sample. For all poor law unions, the figures are 48% and 52%, respectively.

<sup>13</sup>Voter turnout also varied widely across the parishes in Chorlton Union in the 1870s (House of Commons, 1878). See also (House of Commons, 1878, paras 1005, 1726).



little change in either board personnel or subsequent policy. Further, while there are drastic policy changes in some unions, in others there appears to be a more subtle movement towards greater expenditure.

Several of the reports in Table A.1 commented on the loss of *ex officio* guardians and the consequent unwillingness of country gentlemen to engage in poor law politics. Prior to 1894, landowners could serve on the board without election as justices of the peace, but once this right was removed, it appears they were often unwilling to stand for election.<sup>14</sup> In some other cases, smaller farmers also appear to have replaced large farmers after 1894. Notably, however, in some unions the reported composition of the board hardly changed at all.

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

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

The pattern of a temporary surge in outrelief is also observed in Appendix D.2, where I repeat the difference-in-difference specification disaggregating different aspects of relief expenditure. There

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<sup>14</sup>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 (1878, paras 272, 2408, 2469, 5009-5010). In Brixworth, 16 out of 60 guardians were *ex officio* in 1889 (Hurren, 2015, Table 6).

however, we see that reduction in outrelief expenditure is marked by more generous spending in the workhouse in more unequal unions. Such an effect is hinted at in the reports to the Commission, which often comment on the fact that improved accommodation had made receiving relief within the workhouse more attractive. However, since this change was not immediate, it is not surprising that witnesses did not ascribe it directly to the 1894 Act—particularly since the provision of outrelief was often seen as the marker of “lax” policy at the time.

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

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

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

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

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

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

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

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

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

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

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

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

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

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



### A.3 Rural Sanitary Authorities and Highway Districts

While the duties of the boards of guardians were restricted predominantly to the provision of poor relief, the guardians themselves held responsibility for spending on public goods when acting in other capacities. After the 1870s, the guardians in rural areas also constituted the rural sanitary authorities that were responsible for maintaining local sanitary environments. After 1894, these bodies gained additional responsibility for maintaining local road networks, as they became the highway authorities for their districts.<sup>15</sup> These additional responsibilities pose a potential complication for the paper’s analysis in two ways. First, they mean that the link between the empirical context and the theoretical models focused on redistribution are less clear—since the guardians had some control over public goods spending. Second, the fact that the range of spending controlled by sanitary authorities changed in 1894 means that the electoral system changed at the same time as the range of authority of the rural guardians.

In this appendix, I provide evidence that the additional powers of the guardians are not a major concern. First, the spending controlled by these other authorities was far less than the provision of relief administered by the guardians. Second, neither the level nor the changes in this spending varied across the inequality groups that act as the “treatment” in the empirical analysis. Finally, 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.

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<sup>15</sup>Prior to 1894, the control of highways was distributed across a morass of “highway boards” and “highway parishes” (a small number of Rural Sanitary Authorities did have this responsibility prior to 1894), controlled by “waywardens” elected by the parish. See Webb and Webb (1913, Chapter IX, especially pp. 209–213) for discussion of the history of the highway boards. See Dodd (1890) for a detailed discussion of the powers and responsibilities of the rural sanitary authorities prior to 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.<sup>16</sup>

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 8% of relief expenditure in the median poor law union in 1894.<sup>17</sup> 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. Further, there is no evidence that spending differed across the three inequality groups. In low inequality poor law unions, average highways revenue per capita under precept was £0.19, compared to £0.20 in both medium and high inequality unions, respectively (with p-values 0.55). 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.<sup>18</sup> 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

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<sup>16</sup>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.

<sup>17</sup>This value includes both current (not funded out of loans) and investment (funded out of loans) expenditure.

<sup>18</sup>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 Colquhoun 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; unions contained less than 1 town (or part of a town) on average in 1894. This is true across the three inequality groups: there was an average of 0.85 towns in each low inequality union, 0.76 in each medium inequality union, and 0.65 in each high inequality union. None of these differences is statistically significant (p-value=0.52 for a t-test comparing low versus medium inequality unions, and p-value=0.18 comparing low to high).

different forms of expenditure.

The fact that average per capita spending by these other bodies was similar in each inequality group strongly suggests that the change in the guardians' responsibilities was orthogonal to our "treatment"; a conclusion supported by additional regressions in Table A.2. Here, to check further that the results are not capturing these changing responsibilities, I re-estimate the main specifications, including the tax revenue raised for these bodies as a control variable. As we can see, the coefficient relating to other forms of spending on poor relief is small—and statistically insignificant, once revenue variables are accounted for—suggesting little if any spillover in the spending decisions of the different bodies. Further, the coefficients on the inequality variables are extremely similar to those in Table 2. Any effect of these other forms of government spending on the provision of poor relief does not appear to confound the observed relationship with the degree of inequality.

#### A.4 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).<sup>19</sup> 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 A.3 displays the classification of paupers in January 1895, showing that nearly three-quarters of paupers were relieved outside the workhouse. Unemployed men—proxied by the number of able-bodied men and vagrants—were a relatively small share of relief recipients.<sup>20</sup> 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.

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<sup>19</sup>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.

<sup>20</sup>"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).

**Table A.2: Main results are robust to controlling for other types of spending in poor law unions.**

	DV=Relief per capita			DV=Relief as % tax base		
	(1)	(2)	(3)	(4)	(5)	(6)
Medium Inequality_x_post1894	0.32*** (0.078)	0.21*** (0.070)	0.19*** (0.070)	0.36*** (0.070)	0.30*** (0.068)	0.19*** (0.058)
High Inequality_x_post1894	0.33*** (0.076)	0.27*** (0.068)	0.24*** (0.071)	0.48*** (0.065)	0.44*** (0.063)	0.25*** (0.057)
post1894	0.50*** (0.071)	0.43*** (0.069)	0.67*** (0.082)	0.16*** (0.059)	0.11* (0.060)	0.38*** (0.059)
Public Goods Spend p.c.	0.04** (0.020)	0.04* (0.019)	0.00 (0.018)	0.05** (0.018)	0.04** (0.018)	0.01 (0.014)
Population		0.18 (0.661)	0.17 (0.672)		-0.03 (0.495)	-0.07 (0.482)
Population Density		-0.55 (0.634)	-0.70 (0.647)		-0.12 (0.449)	-0.31 (0.458)
% of Population Age Over 64		0.30*** (0.085)	0.30*** (0.085)		0.19*** (0.067)	0.20*** (0.066)
Decadal Variance in Pauperism		0.01 (0.019)	0.01 (0.019)		0.01 (0.017)	0.01 (0.015)
% Revenue from Poor Rate			0.12*** (0.027)			0.09*** (0.019)
Tax Base p.c.			-0.05 (0.065)			-0.77*** (0.084)
No. Observations	4275	4275	4275	4274	4274	4274
No. PLUs	225	225	225	225	225	225
Year Fixed Effects	Y	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y	Y

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . All variables are standardized. “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. See Section 3 for details of other variables. Standard errors are clustered by poor law union and presented in parentheses.

The most contentious aspect of poor law policy in the nineteenth century was the extent to which relief could be provided outside of the workhouse. Under the infamous “workhouse test” imposed by the 1834 New Poor Law, able-bodied male workers were, in principle, only allowed to receive poor relief within a workhouse. Workhouses were designed to be unpleasant places: the standard of living in a workhouse was expected to be lower than the lowest paying job outside; the unpleasantness

**Table A.3: Composition of paupers in January 1895**

		Able-bodied	Not Able-bodied	Lunatics	Vagrants	Total
<b>Indoor</b>	Men	1.6%	6.5%	0.7%	n.a.	8.7%
	Women	1.7%	3.5%	0.9%	n.a.	6.1%
	Children	1.9%	3.3%	0.1%	n.a.	5.3%
	Total	5.1%	13.3%	1.7%	1.5%	21.6%
<b>Outdoor</b>	Men	1.6%	12.1%	3.5%	n.a.	17.2%
	Women	6.5%	28.2%	4.4%	n.a.	39.1%
	Children	17.6%	4.2%	0.1%	n.a.	22.0%
	Total	25.8%	44.5%	8.0%	0.1%	78.4%
<b>Total</b>	<b>34.6%</b>	<b>53.6%</b>	<b>10.0%</b>	<b>1.2%</b>	<b>100%</b>	

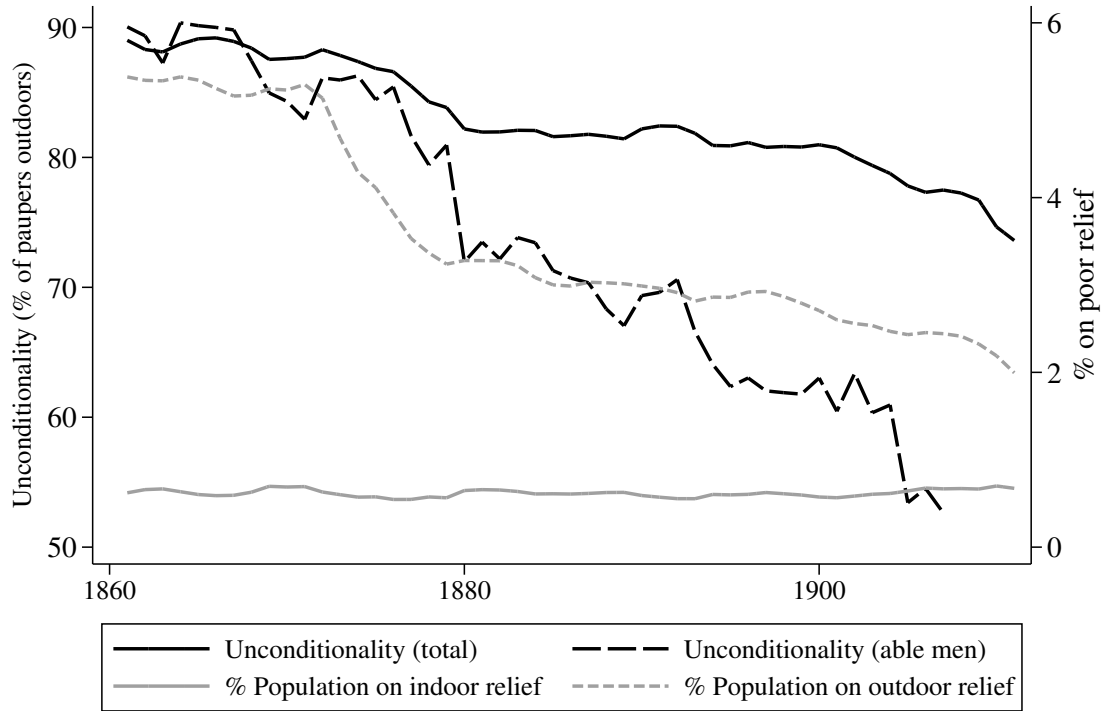
Note: Table displays the breakdown of the population receiving poor relief in 1895 (average of January and July figures) in the regression sample of 225 rural poor law unions. No breakdown by sex or age was provided for vagrants. “Lunatics” refers to those treated in asylums managed under the poor law.

was exacerbated by separating families within the workhouse. The purpose of this requirement was clearly stated as a means of reducing the tax burden, a clear parallel with more modern notions of conditionality. The percentage of able-bodied male paupers served indoors has thus been used as a measure of the strictness of poor law policy both in the modern academic literature and by contemporaries (Boyer and Schmidle, 2009; MacKinnon, 1987).

The period between 1870 and 1880 was characterized by the “Crusade against Outrelief,” as renewed attempts were made to force paupers into the workhouse, as shown in Figure A.1. Over this period the share of able-bodied men on outdoor relief fell by almost two-thirds, declining from around 0.6% to 0.2% of the population. This demonstrates the deterrent effect of the workhouse in reducing the total demand for relief (see MacKinnon (1987) for an empirical test of this deterrent effect). The Crusade years are associated with a clear drop in both the number receiving poor relief and the share of those relieved outdoors around 1870. However, after 1880, both series are relatively stable, and outrelief remained the most common form of support even after the Crusade: over three-quarters of all paupers were relieved outdoors, even at the start of the twentieth century. A majority of even the able-bodied male paupers—the group targeted specifically by the New Poor Law—continued to be relieved outside the workhouse.

The restriction of poor relief expenditure after 1870 is particularly striking when considered in

**Figure A.1: The proportion of the population receiving outdoor relief fell considerably as policy became stricter after 1875.**



Note: “Unconditionality” refers to the proportion of poor relief recipients relieved outside the workhouse (among able-bodied men or all adults, respectively) and is displayed on the left-hand axis. “% population on indoor” or “on outdoor relief” is the proportion of the population receiving the relevant form of relief, and is displayed on the right-hand axis.

nominal terms, as shown in Figure A.2. While real expenditure per capita was largely flat in real terms between 1870 and 1890, in nominal terms spending declined considerably. This decline was driven in part by the fall in pauper numbers discussed previously but also, as the right-hand panel shows, by declining maintenance expenditure per pauper. In real terms, however, expenditure continued to increase—reflecting the considerable deflation experienced during this period due, in particular, to cheap grain imports (O’Rourke, 1997).

**Figure A.2: Poor law expenditure was stagnant until the 1890s in both nominal and real terms.**

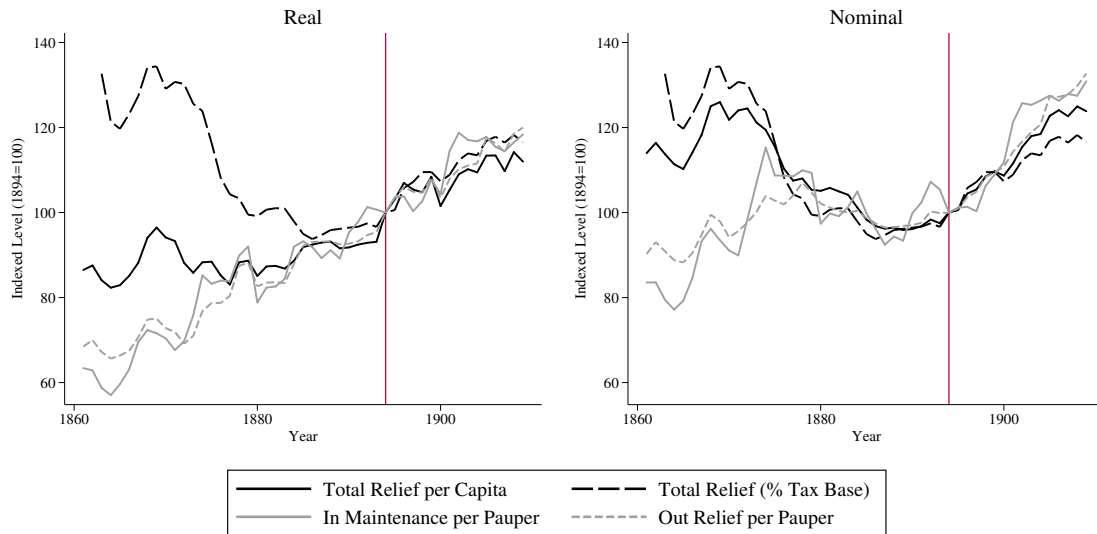


Figure displays indexed average spending for 225 rural poor law unions included in regression analysis—see Section 3 for details of sample and variable definition.

## B Data

This appendix includes additional information regarding the data introduced in Section 3. The first subsection presents descriptive statistics of the variables included in the regression analysis. I then provide more detailed discussion of the inequality variables, starting with the main measure constructed from the distribution of domestic servants; I then move on to the measures constructed by geolocating elite residences.

### B.1 Descriptive Statistics

Table B.4 displays the descriptive statistics for the variables included in the main regressions.

### B.2 Construction of Servants Measure

The main inequality measure in the paper is constructed using the 100% 1881 census sample (Schürer and Woollard, 2003). The idea behind this measure is to proxy household wealth using the number

**Table B.4: Descriptive Statistics**

Variable	Obs	Mean	Std. Dev.	Min	Max
1881 % Servants in Top 5% Households	225	60.94	9.37	35.31	84.56
Total Relief Spend per Capita (£ p.c.)	4725	.41	.1	.16	.79
Total Relief as a % of Tax Base	4725	6.05	2.23	1.43	17.75
Population ('000s)	4725	14.91	7.59	2.47	69.07
Population Density (Popn per Acre)	4725	.21	.10	.02	1.05
% Aged over 64	4725	7.64	1.05	3.92	10.83
100*Decadal Std Dev of Paupers p.c.	4725	.41	.35	.05	2.91
% Revenue from Poor Rate	4725	84.19	7.23	46.99	97.92
Tax Base per Capita (£ p.c.)	4725	6.55	1.84	2.12	14.38

Note: Table includes observations included in regressions in Table 2. Financial variables are in 1914 £. See Section 3 for discussion of data sources and variable construction.

of live-in servants.<sup>21</sup>

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

The second stage involved identifying individual servants in the census. Servants were primarily identified using their relationship to the household head in the census. Specifically, they were categorized as servants a) if their relationship to the household head was as a servant, a housekeeper, or another probable domestic employee or b) their relationship to the household head was stated as “other employee” and their occupation code identified them as a domestic servant (e.g., a governesses or coachmen). Individuals were not classified as servants if their occupation indicated they were not live-in domestic servants. This included, for instance, “governesses (non-resident)” and day-

<sup>21</sup>Initially, I attempted to use the measure of servants constructed as part of the IPUMS data set, which is based on the relationship to household head and occupation. However, investigation indicated that this measure included a large number of individuals who were not the domestic servants of interest here but different types of employees, such as clerks and hospital or hotel workers.



servants, as well as a number of occupations that appear to have been misclassified as servants (e.g., blacksmiths). I then added those listed as employees whose occupation explicitly identified them as domestic servants (for example, governesses or coachmen) and removed those whose occupational code identified them as non-servants, for instance clerks, or those listed in a “household” that appeared to be an institution (such as a prison or boarding house). As a robustness test, I then construct an additional measure excluding those labeled “farm servants;” the resulting measure is correlated 0.97 with our main variable.

### **B.3 Geolocating Elites**

The local elites discussed in Section 3 were identified using two nineteenth-century sources: Bateman (1883) and Walford (1886).

#### **B.3.1 Construction of Dataset**

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

While Bateman (1883) provides a valuable source of these “great” landowners, for the purposes of this paper, it suffers from two deficiencies. First, it may miss important local landholders who do not fit the criterion of “great” identified by Bateman. Second, it limits the potential to geolocate their land because at most one address is listed for each landholder. Frequently landholders owned large properties in multiple counties—the Bateman’s information would only allow us to locate one of these holdings at the poor law union level.

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

The database was then constructed as follows:

1. Identify all individuals either listed in Bateman (1883) or included in the list of Principal Seats in Walford (1886).
2. Identify relevant characteristics of these individuals, particularly their titles and whether Bateman mentions them as part of a family that held land in England back to the time of Henry VIII.
3. For each individual, identify all properties mentioned in either source.
4. Geolocate these properties and assign each property to the relevant poor law union. Two major sources were used for the geolocation: [www.britishlistedbuildings.co.uk](http://www.britishlistedbuildings.co.uk) and the gazette provided by Vision of Britain. Where matching was unclear, additional internet research was done to ensure the correct property had been identified. In some (relatively few) cases, it was not possible to identify the precise location of the property but only that of a nearby landmark or relevant parish.
5. Where information is available, assign the land owned by an individual within each county to the property (or properties) in that county. For the group of smaller landholders in Bateman (those with over 2,000 but under 3,000 acres or £3,000 rental value), counties are grouped together—the acreage is split evenly between these counties.

Nearly all properties in both sources were geolocated and matched with poor law unions. Between the two sources, a total of 5,804 properties were identified in England and Wales, of which 88% were successfully geolocated and 52% (3,020) were matched to the landholdings in Bateman. Those unmatched reflect properties owned by families not qualifying for inclusion in Bateman's list—in other words, they were relatively small landowners.<sup>22</sup>

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

It does not appear, however, that the lack of matching biases the inequality estimates. Properties were unmatched as a result of landowners not having residences in these areas rather than an

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<sup>22</sup>There were only 27 landholders in Bateman for whom we were unable to identify any property.

inability to locate the properties—95% of listed residences of Bateman’s landowners are matched to an acreage value. Potentially, elites could still project power without having a formal residence in a specific union—potentially biasing our estimates of “low-inequality,” which merely captures the lack of a residence. In practice, however, the correlation between our county-level servants inequality measure and the share of landholdings matched is close to zero ( $\rho = -0.06, p\text{-val}=0.65$ ).<sup>23</sup>

### B.3.2 Variables

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.

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

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<sup>23</sup>This correlation excludes Yorkshire, as the reporting of counties in the Bateman (1883) list does not break down different ridings in Yorkshire.

## C Additional Robustness Checks

### C.1 Allowing for Alternative Effects of Reform

Table C.5 presents an extended version of the results in Table 3. I report the coefficients relating to the additional interaction terms included in the table and also allow for interactions with the percentage of the workforce engaged in agriculture. We can see that the coefficients regarding inequality are similar in all specifications. Further, there is little evidence that the other characteristics have similar effects to those estimated for inequality, the only exception being the interaction with the intercept and the population aged over 64. The results thus do not appear to reflect responses to these other characteristics.

**Table C.5: Robustness to allowing interactions with other characteristics.**

	DV=Relief per Capita				DV=Relief as % Tax base					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<b>Medium Inequality</b>										
x_post1894	0.02 (0.059)	0.05 (0.054)	0.03 (0.057)	0.05 (0.062)	0.04 (0.056)	0.06 (0.041)	0.06* (0.036)	0.08** (0.038)	0.09** (0.041)	0.06 (0.037)
x_time_x_post1894	0.05*** (0.015)	0.04*** (0.013)	0.04*** (0.014)	0.04*** (0.015)	0.04*** (0.013)	0.03** (0.011)	0.02*** (0.009)	0.02** (0.010)	0.03** (0.011)	0.03*** (0.010)
<b>High Inequality</b>										
x_post1894	0.09 (0.061)	0.13** (0.055)	0.10* (0.059)	0.11* (0.065)	0.11* (0.059)	0.07* (0.042)	0.09** (0.038)	0.10** (0.039)	0.10** (0.044)	0.08** (0.041)
x_time_x_post1894	0.02 (0.016)	0.01 (0.015)	0.01 (0.015)	0.02 (0.016)	0.01 (0.015)	0.01 (0.012)	0.01 (0.011)	0.01 (0.011)	0.01 (0.012)	0.01 (0.011)
<b>Interactions</b>										
<b>Medium Tercile</b>										
x_post1894	0.08 (0.054)	0.18*** (0.056)	-0.01 (0.057)	0.03 (0.061)	0.03 (0.060)	0.03 (0.039)	0.11*** (0.038)	-0.05 (0.039)	-0.02 (0.042)	0.04 (0.040)
x_time_x_post1894	-0.02 (0.015)	-0.02 (0.015)	0.01 (0.015)	-0.02 (0.016)	0.01 (0.014)	-0.01 (0.012)	-0.01 (0.011)	0.00 (0.011)	-0.01 (0.012)	0.01 (0.010)
<b>High Tercile</b>										
x_post1894	0.07 (0.066)	0.18*** (0.056)	0.04 (0.062)	-0.00 (0.068)	-0.00 (0.060)	0.02 (0.047)	0.14*** (0.039)	-0.03 (0.042)	-0.07 (0.047)	0.02 (0.040)
x_time_x_post1894	-0.02 (0.017)	-0.03* (0.015)	-0.00 (0.015)	-0.00 (0.017)	0.01 (0.014)	-0.00 (0.012)	-0.01 (0.010)	0.01 (0.011)	0.00 (0.012)	0.00 (0.010)
<b>No. Observations</b>										
No. Observations	4725	4725	4725	4725	4641	4725	4725	4725	4725	4641
<b>No. PLUs</b>										
No. PLUs	225	225	225	225	221	225	225	225	225	221
<b>Year Fixed Effects</b>										
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
<b>PLU Fixed Effects</b>										
PLU Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
<b>Controls</b>										
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: See notes to Table C.6.

The results are also robust to allowing for differential growth paths according to observable characteristics in 1881, as shown in Table C.6. Here, rather than unit-specific time trends and interactions with observable characteristics, I include additional quartic time trends according to the tercile of each of the characteristics in 1881. By doing so, I allow for the fact that the different groups of poor law unions may have developed differently over time in a way not captured by the inclusion of our control variables. Again, the relationship remains similar in each specification.

**Table C.6: Robustness to allowing complex time trends.**

	DV=Relief per capita				DV=Relief as % Tax Base					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Medium Inequality										
x_post1894	0.03 (0.053)	0.05 (0.054)	0.04 (0.053)	0.04 (0.053)	0.04 (0.056)	0.06* (0.036)	0.06* (0.036)	0.07** (0.035)	0.07** (0.035)	0.06* (0.037)
x_time_x_post1894	0.05*** (0.015)	0.04*** (0.013)	0.04*** (0.014)	0.04*** (0.015)	0.04*** (0.013)	0.03** (0.011)	0.03*** (0.009)	0.02** (0.010)	0.02** (0.011)	0.03*** (0.010)
High Inequality										
x_post1894	0.09* (0.056)	0.13** (0.055)	0.11* (0.056)	0.10* (0.058)	0.11** (0.058)	0.07* (0.038)	0.09** (0.037)	0.09** (0.038)	0.08** (0.039)	0.09** (0.040)
x_time_x_post1894	0.02 (0.016)	0.01 (0.015)	0.01 (0.015)	0.02 (0.016)	0.01 (0.015)	0.01 (0.012)	0.01 (0.011)	0.01 (0.011)	0.01 (0.012)	0.01 (0.011)

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Quadratic Time Trends interacted with Terciles of:

Population Density	Y	N	N	N	N	Y	N	N	N	N	N	N
% Over 64	N	Y	N	N	N	N	Y	N	N	N	N	N
Cereal Suitability	N	N	Y	N	N	N	N	Y	N	N	N	N
Tax Base per Acre	N	N	N	Y	N	N	N	N	N	Y	N	N
% in Agriculture	N	N	N	N	Y	N	N	N	N	N	N	Y
No. Observations	4725	4725	4725	4725	4641	4725	4725	4725	4725	4725	4641	
No. PLUs	225	225	225	225	221	225	225	225	225	225	221	
PLU-Specific Time Trends	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: All variables are standardized. See Section 3 for variable definitions. Standard errors are clustered by poor law union and presented in parentheses. "Controls" includes all variables included in specifications (3) and (6) in Table 2. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## C.2 Robustness in Subsamples

In this section, I repeat the main analysis, restricting the sample to unions with similar levels of major characteristics. Specifically, I limit the sample to unions within the common support of variables that are correlated with the level of inequality. By doing so, I ensure that the results are not driven by the different groups of unions being fundamentally different in other dimensions apart from inequality.

As we can see in Table C.7, the results are similar when restricting the sample in this way. In each specification, the sample is restricted to districts within the common support of the variable in the header row. In all cases, the magnitude and statistical significance of the coefficients is similar to the results in Table 2.



Table C.7: Main results when restricting sample to districts with similar 1881 characteristics.

Sample restriction:	DV=Relief per capita					DV=Relief as % tax base				
	Popn. Density	Tax Base per Acre	% Occs Agric	% Popn. ≥65	Cereal Suitability	Popn. Density	Tax Base per Acre	% Occs Agric.	% Popn. ≥65	Cereal Suitability
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Medium Inequality_x.post1894	0.09 (0.077)	0.15* (0.078)	0.18** (0.072)	0.20*** (0.070)	0.14 (0.087)	0.09* (0.054)	0.16*** (0.057)	0.18*** (0.057)	0.20*** (0.053)	0.13** (0.062)
High Inequality_x.post1894	0.18** (0.078)	0.18** (0.082)	0.21*** (0.076)	0.23*** (0.076)	0.20** (0.087)	0.18*** (0.059)	0.20*** (0.060)	0.23*** (0.060)	0.24*** (0.057)	0.25*** (0.065)
post1894	0.87*** (0.090)	0.87*** (0.102)	0.89*** (0.096)	0.87*** (0.095)	0.92*** (0.113)	0.57*** (0.064)	0.58*** (0.078)	0.57*** (0.074)	0.56*** (0.072)	0.55*** (0.080)
Population	0.50 (0.544)	-0.39 (0.595)	0.61 (0.621)	0.04 (0.518)	0.00 (0.723)	0.43 (0.408)	-0.38 (0.451)	0.38 (0.453)	-0.10 (0.369)	-0.09 (0.537)
Population Density	-1.45** (0.595)	-0.47 (0.674)	-1.17** (0.575)	-1.10** (0.556)	-0.45 (0.644)	-0.99** (0.424)	-0.25 (0.505)	-0.79* (0.411)	-0.70* (0.391)	-0.23 (0.480)
% of Population Age Over 64	0.23** (0.088)	0.21** (0.092)	0.29*** (0.090)	0.23*** (0.082)	0.26*** (0.096)	0.18** (0.068)	0.13* (0.072)	0.19*** (0.071)	0.13** (0.064)	0.17** (0.074)
Decadal Variance in Pauperism	0.03 (0.026)	0.03 (0.028)	0.02 (0.027)	0.02 (0.027)	0.01 (0.031)	0.03 (0.019)	0.03 (0.020)	0.01 (0.021)	0.01 (0.020)	-0.01 (0.022)
% Revenue from Poor Rate	0.11*** (0.024)	0.08*** (0.030)	0.12*** (0.030)	0.12*** (0.031)	0.11*** (0.035)	0.09*** (0.018)	0.06** (0.027)	0.09*** (0.025)	0.08*** (0.025)	0.06** (0.028)
Tax Base p.c.	-0.13* (0.072)	-0.06 (0.073)	-0.09 (0.069)	-0.07 (0.070)	-0.01 (0.084)	-0.94*** (0.098)	-0.81*** (0.092)	-0.85*** (0.091)	-0.83*** (0.087)	-0.75*** (0.096)
No. Observations	3843	3906	4494	4452	3402	3843	3906	4494	4452	3402
No. PLUs	183	186	214	212	162	183	186	214	212	162
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

The table reports the results of repeating the specifications (3) and (6) in Table 2 with a restricted sample. In each specification, the sample is restricted to districts within the common support of the variable in the header row. All variables are standardized. See Section 3 for variable definitions. Standard errors are clustered by poor law union and presented in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table C.8 shows the results removing the years before 1890, which show the clearest evidence of differences in pre-trends between the groups.

**Table C.8: Similar results when restricting analysis to years 1890 onward.**

	DV=Relief per Capita			DV=Relief as % Tax Base		
	(1)	(2)	(3)	(4)	(5)	(6)
Medium Inequality_x_post1894	0.34*** (0.071)	0.25*** (0.064)	0.22*** (0.062)	0.37*** (0.062)	0.31*** (0.061)	0.19*** (0.049)
High Inequality_x_post1894	0.32*** (0.072)	0.25*** (0.068)	0.22*** (0.069)	0.42*** (0.062)	0.38*** (0.058)	0.21*** (0.053)
post1894	0.67*** (0.059)	0.69*** (0.054)	0.85*** (0.073)	0.27*** (0.048)	0.29*** (0.043)	0.63*** (0.059)
Population		0.14 (0.610)	0.13 (0.616)		0.26 (0.494)	0.03 (0.451)
Population Density		-0.36 (0.545)	-0.51 (0.547)		-0.31 (0.441)	-0.29 (0.402)
% of Population Age Over 64		0.35*** (0.094)	0.34*** (0.092)		0.21*** (0.075)	0.22*** (0.073)
Decadal Variance in Pauperism		0.01 (0.008)	0.01 (0.008)		0.01* (0.007)	0.01 (0.007)
% Revenue from Poor Rate			0.10*** (0.029)			0.08*** (0.024)
Tax Base p.c.			-0.02 (0.087)			-0.82*** (0.096)
No. Observations	3600	3600	3600	3600	3600	3600
No. PLUs	225	225	225	225	225	225
Year Fixed Effects	Y	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y	Y

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . All variables are standardized. See Section 3 for variable definitions. Standard errors are clustered by poor law union and presented in parentheses.

### C.3 Geographic Clustering

There are clear differences in the pattern of geographic clustering according to the level of inequality in poor law unions, as shown in Figure 3 and Table C.9, which breaks down the location of poor law unions according to the registration divisions used in the census. Almost all poor law unions in Wales and northern England were low inequality, while almost none were in the South East.

This clustering could be a concern if, for example, there were other differences across regions—such as cultural variation—that would lead to a different reaction to the 1894 democratization. For instance, if Welsh authorities were minded for some reason to always provide low relief, then we might be incorrectly attributing the lack of reaction to the 1894 reform to low inequality rather than other factors causing antipathy to relief.

**Table C.9: Regional breakdown of rural poor law unions.**

	Low Inequality	Medium Inequality	High Inequality	Total
South Eastern	1	7	23	31
South Western	12	11	12	35
Eastern	0	11	9	20
South Midland	0	11	13	24
East Midland	10	11	10	31
West Midland	10	13	6	29
North Western	1	0	0	1
Yorkshire	9	5	1	15
Northern	12	3	1	16
Wales	20	3	0	23
Total	75	75	75	225

Note: Regional classifications relate to census divisions.

To check that the relationship with inequality is not driven by this geographic clustering, I repeat the main specifications excluding different regions. First, I limit the sample to only those regions—the South-West, West Midlands and East Midlands—that have more than one union in each inequality group. Second, to check if the results are explained by the characteristics of any particular region, I re-estimate the specifications excluding each individual region.

The main conclusions are robust to these different samples, as shown in Table C.10 and in Figures C.3–C.4. Table C.10 replicates the analysis in Table 2 with the sample restricted to only the three regions with multiple low, medium and high inequality poor law unions. We can see that the effect sizes are similar—although as a result of the smaller sample, some coefficients are marginally statistically insignificant at conventional levels. Figures C.3 and C.4 then replicate the analysis accounting for union-specific time trends, both for this group (panel 2 in each figure) and then excluding each region from the regression sample. There is no evidence that the results are being driven by any particular region, and there are similar trends in each case. Similarly, running the re-

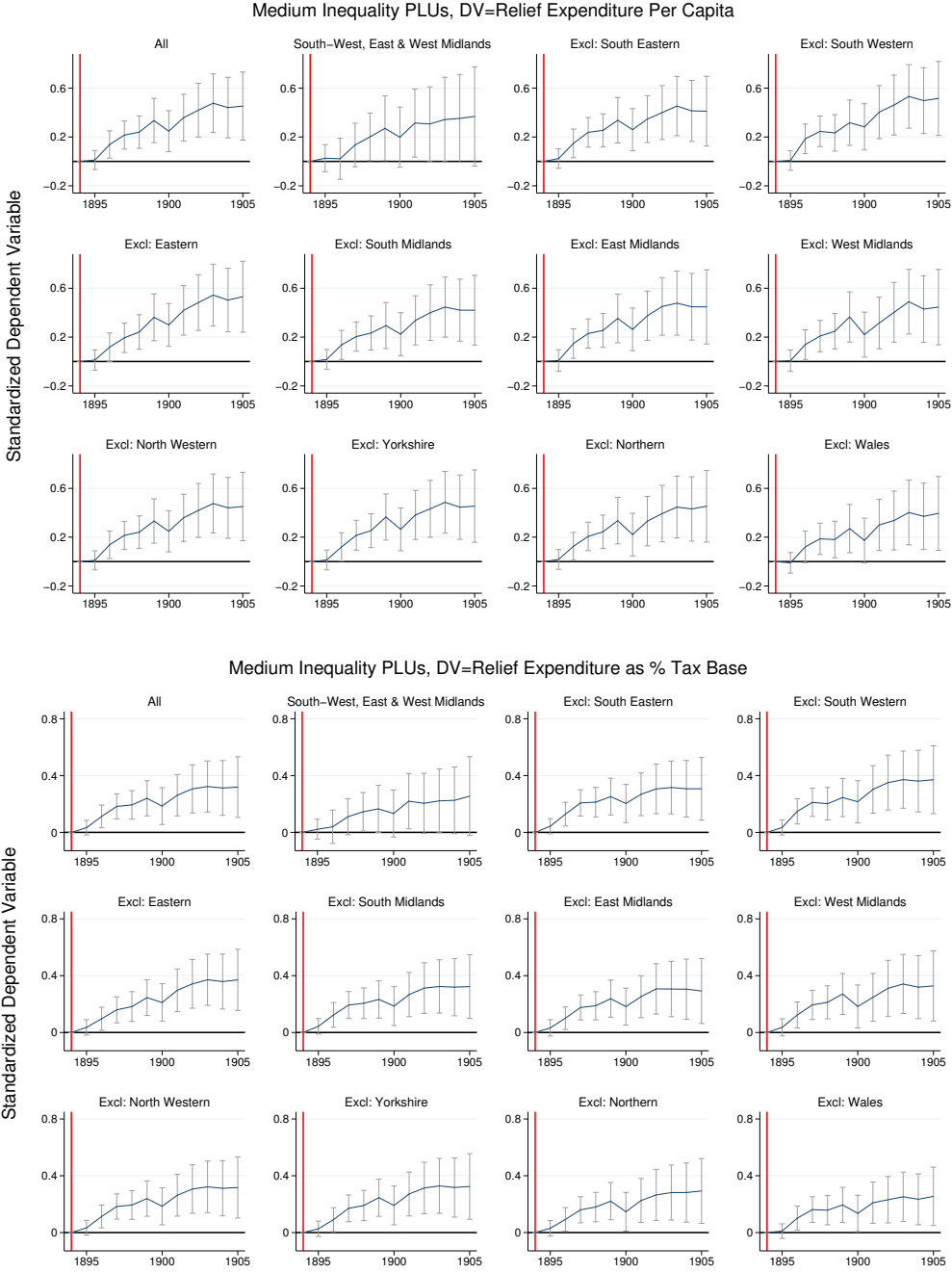
gression specifications in Table 2, column (3), the coefficient for medium inequality, varies from 0.17 to 0.24, and for high inequality, it varies from 0.20 to 0.30. For column (6), the ranges are 0.15–0.21 and 0.19–0.34, respectively. In all cases, the coefficients are strongly statistically significant.

**Table C.10: Results are similar when restricting the sample to regions with variation in the level of inequality.**

	DV=Relief per Capita			DV=Relief as % Tax Base		
	(1)	(2)	(3)	(4)	(5)	(6)
Medium Inequality_x_post1894	0.26** (0.102)	0.18* (0.098)	0.16* (0.094)	0.23** (0.087)	0.20** (0.093)	0.13* (0.071)
High Inequality_x_post1894	0.28** (0.114)	0.21* (0.113)	0.21* (0.116)	0.40*** (0.104)	0.38*** (0.104)	0.23** (0.092)
post1894	0.82*** (0.085)	0.78*** (0.085)	0.98*** (0.144)	0.50*** (0.062)	0.48*** (0.059)	0.66*** (0.128)
Population		0.86 (0.731)	0.84 (0.740)		0.24 (0.554)	0.40 (0.531)
Population Density		-1.06 (0.681)	-1.14* (0.681)		-0.33 (0.536)	-0.58 (0.477)
% of Population Age Over 64		0.20** (0.098)	0.20** (0.097)		0.10 (0.082)	0.17** (0.076)
Decadal Variance in Pauperism		0.01 (0.020)	0.01 (0.020)		0.02 (0.020)	0.01 (0.017)
% Revenue from Poor Rate			0.08 (0.050)			0.06 (0.047)
Tax Base p.c.			0.00 (0.106)			-0.89*** (0.107)
No. Observations	1995	1995	1995	1995	1995	1995
No. PLUs	95	95	95	95	95	95
Year Fixed Effects	Y	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y	Y

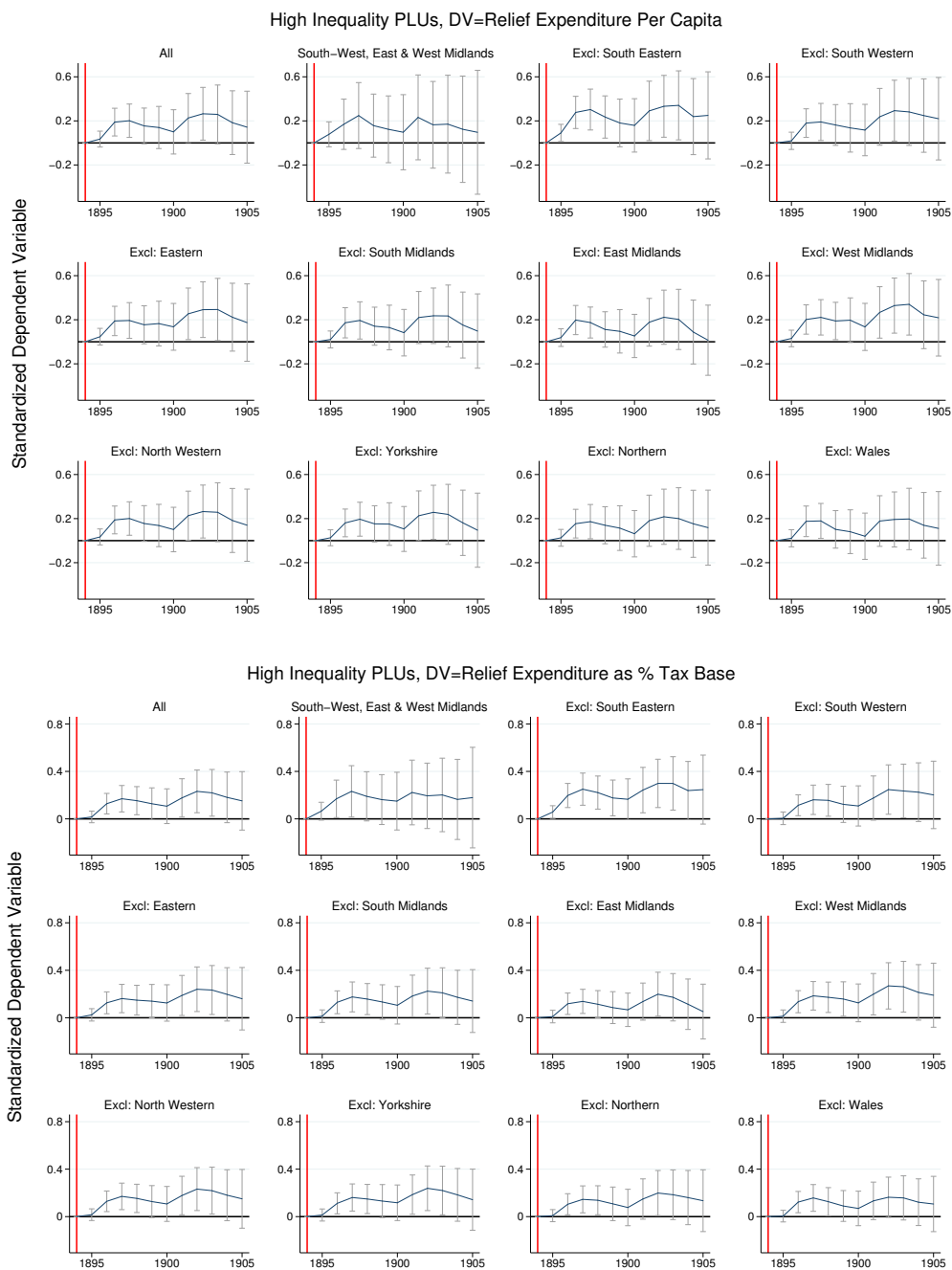
\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . All variables are standardized. See Section 3 for variable definitions. Standard errors are clustered by poor law union and presented in parentheses.

**Figure C.3: Results in Medium Inequality Unions are not driven by a specific region.**



Note: Figure replicates the specifications in the right-hand panel of Figure 5, excluding each of the registration divisions in turn, and presents the coefficients relating to Medium Inequality poor law unions.

**Figure C.4: Results in High Inequality Unions are not driven by a specific region.**



Note: Figure replicates the specifications in the right-hand panel of Figure 5, excluding each of the registration divisions in turn, and presents the coefficients relating to the group of high inequality poor law unions.

## C.4 Alternative Measures of Inequality

Table C.11 expands the analysis in Table 4 to include additional measures of inequality. I start by including an alternative measure of economic inequality: the estimated percentage of land in a union owned by one of the “great” landowners as defined in Bateman (1883)—those owning 3,000 acres with a rental value of at least £3,000. As discussed in the text, this measure is imperfect because it assigns all land owned in a county to a specific union where the landowner was found to have a residence. However, it provides an more direct measure of the concentration of wealth in a small elite than the main inequality measure, estimated from the distribution of domestic servants. As additional measures of political inequality, I include the raw number of gentry in the union (in terciles)—as opposed to normalizing by size of district as in the main text. I also include a measure of the number of great landowners—rather than all gentry—per acre in the union.

The results are similar to the main text in both cases. Again, there is evidence that the reforms led to greater increases in spending where the median income was low, and where wealth was very concentrated. Further, there is no evidence that political inequality is important, once the servants measure of economic inequality is included. The servants measure also dominates the measure estimating the percentage of land owned by great landowners in the district—a result consistent with higher measurement error in the latter, due to the estimation of local landholdings using county-level data.



**Table C.11: Results are similar using alternative measures of economic and political inequality.**

	DV=Total Relief per Capita						DV=Total Relief as % Tax Base							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
% Servants in Top 5% of households (Main Inequality Measure)														
Middle Tercile			0.19**		0.21***		0.21***			0.17***		0.19***		0.18***
			(0.075)		(0.077)		(0.077)			(0.058)		(0.056)		(0.058)
Top Tercile			0.22***		0.24***		0.24***			0.22***		0.24***		0.23***
			(0.077)		(0.084)		(0.079)			(0.060)		(0.064)		(0.060)
Median Income (Wage of Farm Laborer):														
Middle Tercile			-0.21**						-0.22***					
			(0.082)						(0.066)					
Top Tercile			-0.25***						-0.26***					
			(0.080)						(0.064)					
% Acres owned by Elite:														
Middle Tercile	0.09	0.08	0.05					0.05	0.04	0.02				
	(0.079)	(0.078)	(0.077)					(0.065)	(0.063)	(0.063)				
Top Tercile	0.15**	0.15**	0.08					0.12**	0.11*	0.05				
	(0.069)	(0.071)	(0.073)					(0.055)	(0.057)	(0.057)				
Great Landowners per Acre:														
Middle Tercile				0.04	-0.04						0.04	-0.04		
				(0.076)	(0.082)						(0.061)	(0.064)		
Top Tercile				0.12	0.02						0.10	0.01		
				(0.077)	(0.084)						(0.063)	(0.065)		
Number of Elite Residences:														
Middle Tercile						0.04	-0.01						0.04	0.00
						(0.075)	(0.080)						(0.059)	(0.062)
Top Tercile						0.08	0.02						0.09	0.04
						(0.084)	(0.085)						(0.067)	(0.066)
No. Observations	4725	4683	4725	4725	4725	4725	4725	4725	4683	4725	4725	4725	4725	4725
No. PLUs	225	223	225	225	225	225	225	225	223	225	225	225	225	225
Fixed Effects, Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

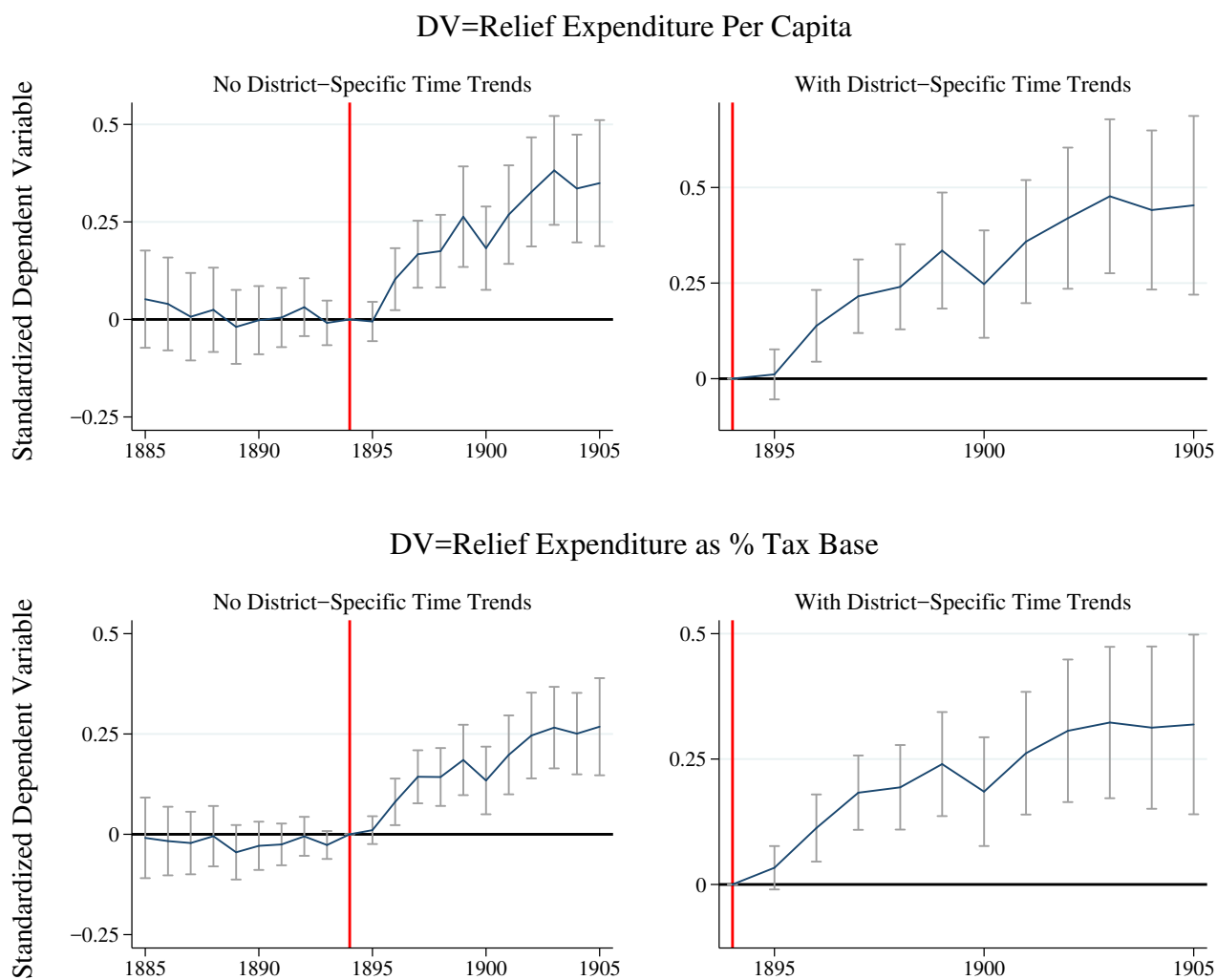
\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . All variables are standardized. All specifications include year and PLU “fixed effects. Controls” include variables in specification (3) and (6) of Table 2. Standard errors are clustered by poor law union and presented in parentheses.

## D Additional Results

### D.1 Additional Results for Medium Inequality Poor Law Unions

This appendix displays the results of the specifications reported in Figures D.5 for medium inequality poor law unions. Here again, there is no evidence of differences in pre-trends. Further, consistent with the results in Table 2, the estimated effects of the reform are, if anything, larger than those observed in the high inequality poor law unions.

**Figure D.5: Limited evidence of pre-trends in provision of relief in Medium Inequality Unions.**



See note to Figure 5 for specification details. This figure displays the coefficients  $\gamma_j$  and  $\gamma_k$  referred to there.

## D.2 Disaggregating Effects of Reform

In this Appendix, I analyze the effects of the reform on differing dimensions of poor law support. In particular, I investigate whether the increases in spending documented in the main text reflected more people receiving poor law support or a greater level of spending on each pauper. Unpacking the way in which the provision of relief altered is difficult due to the limited nature of the poor

law statistics. For instance, because we do not observe how long paupers received relief for, a higher cost per pauper could reflect either a higher pauper allowance—a more generous policy—or a determination to drive short-term recipients from the poor law rolls—a harsher policy. Nevertheless, we can shed some light on the changes that occurred: the analysis offers suggestive evidence that the effect of the reforms was due to increasing outdoor relief in the short term and providing more generous workhouse accommodation in the longer term. Further, there is no evidence that the effects were focused on the provision of relief in one particular season, providing reassurance that the results are not driven by differing forms of agriculture (and hence varying degrees of seasonality) across inequality groups.

The top panel of Figure D.6 suggests that the reform was followed by an initial, but temporary, increase in the number of paupers in more unequal unions. Immediately after the reforms, there is a jump in the number of paupers which is not observed in the low inequality group. However, this effect disappears again after a short period—consistent with the evidence given to the 1909 Royal Commission discussed in Appendix A.2. The panel also shows that the effect on the number of paupers is similar for both the January and July pauper counts, suggesting that there is no seasonal effect. More generally, different types of agriculture have different levels of seasonality, and in principle, the reforms could lead to different reactions by the newly democratic voters. For instance, a more seasonal workforce might require more support in the winter months.<sup>24</sup> We see little evidence of such a trend here, with similar patterns observed at both points of the year.

The bottom panel row displays the spending per pauper over time across the three groups. First, I plot the spending across all paupers; I then split this amount between the spending per indoor pauper (those receiving relief in a workhouse) and the spending per outdoor pauper.<sup>25</sup> As we can see, there is evidence of increasing spending per pauper in all three series for all three groups of poor law unions. The more unequal unions started spending more per indoor pauper after 1894 and also more per pauper overall, although the latter pattern is of a greater magnitude.

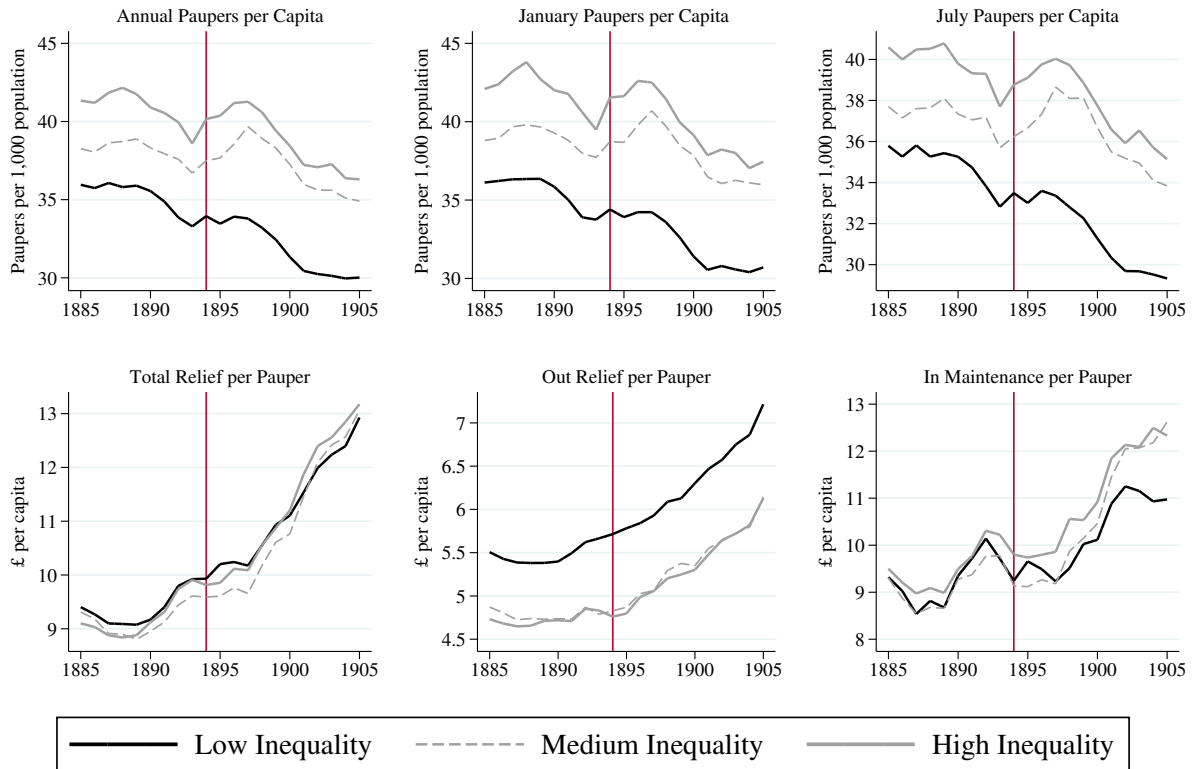
Table D.12 shows similar patterns within a regression framework, controlling for other demographic characteristics of the poor law unions, and allowing for differing effects between the immediate (1895–1899) and later (1900–1905) post-reform periods. The variables are constructed here so that the coefficient on “x\_post1900” is additional to the overall effect after 1894 (captured by “x\_post1894”). Again, we see a temporary increase in the number of paupers per capita in more

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<sup>24</sup>The classic argument, developed for a much earlier period, here is that arable farmers used the poor law to subsidize labor in the off-season.

<sup>25</sup>For display purposes I use nominal spending per pauper in the figure as opposed to real spending per pauper. The results are similar in both cases, but the real series is noisier, making it more difficult to discern trends.

**Figure D.6: Changing patterns of poor relief 1884–1905.**



Note: See Section 3 for data sources. “Annual Paupers per Capita” is the average of the January and July totals. Out Relief and In Maintenance per pauper are calculated using the number of outdoor and indoor paupers, respectively.

unequal unions, although the coefficient for high inequality unions is not statistically significant (a result of controlling for the percentage of the population aged over 64). On the right-hand side of the table, we can see that evidence of increasing spending per pauper appears only five years after the reforms and is driven by increased spending per pauper within the workhouse. Outside the workhouse, in contrast, we see evidence of falling expenditure per pauper, which is consistent with greater provision of short-term outrelief. However, the limitations of the data mean we must be wary of over-interpretation here.

While we cannot draw strong conclusions, the following interpretation is consistent with both the overall increase in spending discussed in Section 5 and the qualitative evidence presented in Appendix A.2. Immediately following the reform, new boards of guardians accepted more applications for short-term outrelief. Since these relief spells were shorter in duration, the spending per outdoor

**Table D.12: Effect of 1894 reforms on number of paupers and spending per pauper.**

	Paupers per 1,000 population			DV=Relief per Pauper (£)		
	Annual	January	July	Total	Outrelief	Inmaint.
	(1)	(2)	(3)	(4)	(5)	(6)
Medium Inequality						
x_post1894	0.15** (0.066)	0.15** (0.066)	0.16** (0.066)	-0.08 (0.067)	-0.14** (0.069)	0.02 (0.097)
x_post1900	-0.04 (0.053)	-0.06 (0.052)	-0.02 (0.054)	0.23*** (0.077)	-0.09 (0.062)	0.48*** (0.101)
High Inequality						
x_post1894	0.07 (0.063)	0.06 (0.063)	0.08 (0.063)	0.01 (0.068)	-0.17** (0.073)	0.10 (0.098)
x_post1900	-0.07 (0.061)	-0.09 (0.061)	-0.04 (0.061)	0.16** (0.068)	-0.05 (0.066)	0.24*** (0.089)
post1894	-0.59*** (0.075)	-0.51*** (0.074)	-0.66*** (0.077)	1.33*** (0.077)	1.42*** (0.079)	0.81*** (0.115)
No. Observations	4725	4725	4725	4725	4725	4725
No. PLUs	225	225	225	225	225	225
Year Fixed Effects	Y	Y	Y	Y	Y	Y
PLU Fixed Effects	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . All variables are standardized. See Section 3 for variable definitions. Standard errors are clustered by poor law union and presented in parentheses.

pauper went down. Over time, they also began to provide more comfortable workhouse accommodation, leading to greater acceptance of the workhouse as a place to receive relief and a decline in the number of outdoor paupers. In one poor law union, for example, it was explained that “there has been a large increase in the number of indoor paupers and a considerable decrease in the number of outdoor paupers...[S]ince the improvements in the workhouse and infirmary have been carried out there has not been the objection which once existed on the part of old people to come into the workhouse, where they are very much better cared for and looked after than they could be in their own homes” (House of Commons, 1909, App Volume VII, Appendix L, para 7). The

results presented here thus suggest that higher inequality unions invested more in improving their workhouses.