Rags to Rags: The Multi-Generational Effects of Ending Cash Transfers in Victorian Britain*

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Abstract

We study the intergenerational impacts of ending cash transfers using the 1834 ("New") Poor Law, which drastically cut the income support that was provided to 15 percent of the population in England and Wales and cost 2 percent of GDP. We show that in 1861, cohorts more exposed to income-support declines in childhood held lower-skilled jobs as adults and had fewer of their children in school. Linking these sons to the 1901 census, we find similar results for them as adults and also for their children, highlighting the importance of accounting for multi-generational effects in cost-benefit analyses of social programs.

Keywords: Social protection programs, austerity, intergenerational mobility, Poor Laws
JEL Codes: H53, I14, I24, I38, J62, N33, O15

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1. Introduction

Budget cuts are common during times of fiscal austerity and often include substantial reductions in welfare spending. In developed economies, the withdrawal of income support for poor families has been shown to have wide-ranging consequences in the short run, including increased poverty and criminal activity as well as worsened health and educational outcomes (Blundell et al., 2016; Bray, Braakmann and Wildman, 2022; Pilkauskas et al., 2022). However, as some of the oldest of these programs benefiting poor families – e.g., Food Stamps and the Earned Income Tax Credit in the United States – only began around a half-century ago, our understanding of their long-run effects is generally limited to those on adults who were children when the programs began (Hoynes, Schanzenbach and Almond, 2016; Bastian and Michelmore, 2018; Manoli and Turner, 2018; Bailey et al., 2023).[1]

Our lack of knowledge is especially acute in the developing world. Only in recent years have countries begun supplementing household incomes to combat persistent poverty and improve health, especially among families with children, but such programs have quickly spread across the globe (Parker and Todd, 2017; Banerjee et al., 2023). Therefore, despite the prevalence of income support programs across developed and developing economies, we generally lack the data to study their effects on the health and well-being of the low-income population across its life course and into subsequent generations.

The implementation of the Poor Law Amendment Act of 1834 ("New Poor Law") in England and Wales offers a unique opportunity to fill this gap in our knowledge. As we describe in Section 2, for over two centuries before this point, individual parishes had determined eligibility requirements and the generosity of income support ("poor relief") as well as the tax rates to fund it, leading to wide geographic heterogeneity.[2] But starting in the 1780s, poor relief expenditures and the number of recipients began to increase – especially in Southeastern parishes with seasonal demand for wheat-farming labor – reaching 15 percent of the population and costing 2 percent of GDP by 1830 (Lindert, 1998; Boyer, 2019).[3] The backlash was led in large part by the founders of modern political economy (Ricardo, 1817; Malthus, 1826; Bentham, 1843; Mill, 1909) and culminated in the New Poor Law, which centralized the administration of poor relief and dramatically restricted its provision. Within a few years, expenditures fell to 1 percent of GDP and recipient rolls fell to 10 percent of the population (Boyer, 2019).

The New Poor Law thus caused considerable declines in income support to poor families

[1] The Mothers' Pensions that Aizer et al. (2016) analyze began much earlier (the Progressive Era), but data availability similarly limits the study to the long-run effects on adults who were children when their mothers received the cash transfers.

[2] To be more precise, there were two types of poor relief: "outdoor," in the form of weekly stipends or subsidies, and "indoor," in the form of the workhouse. However, we focus on the former and use "poor relief" to refer to it because the latter was not widely used before the New Poor Law.

[3] For context, other countries in Europe – and even Scotland and Ireland – spent at most around 1 percent of GDP on analogous programs in this period (Lindert, 1998).

in some regions but not others, which is the variation we exploit in Section 3 to study (the withdrawal of) poor relief's determinants and short-run, long-run, and multi-generational effects. We begin in Section 3.1 by quantifying declines in poor relief spending across English and Welsh counties and splitting them at the median into more- (treatment) and less-exposed (control) groups. We also test the hypothesis that wheat farming was an important factor in the pre-1834 generosity – and thus the post-New Poor Law decline – of poor relief spending per capita. Our results show that land suitability for wheat cultivation was indeed an extremely strong predictor of decreases in income support to poor families after the law's implementation.

Next, we turn to its short-run effects at the county level in Section 3.2. We compare trends in poor relief spending per capita over time and find that on average, treatment counties experienced a £0.08 per capita greater sustained decline in poor relief spending through 1841 (17 percent of the pre-1834 rate). We then examine trends in data on the number of burials in a sample of parishes and find this additional decrease resulted in a 4-percent greater increase in this measure of mortality in treatment counties, though our estimate is imprecise. In Section 3.3, we study the long-run effects on adults who were children around 1834.[4] Using detailed microdata from the 1861 census, we find around 4-percent increases in the following outcomes for ("directly-treated") individuals who were born in treatment counties and were no older than 15 in 1834: holding low-skilled jobs (men and women) and living on farms (men), though only the male occupational-skill result is precisely estimated.

We then examine the effects on children living with these adults in Section 3.4 by assigning them their father's degree of childhood exposure to declines in poor relief. As children, the sons and daughters of directly-treated adults were around 4 percent less likely to be in school. Linking these sons to their adult records in the 1901 census using the Abramitzky, Boustan and Eriksson (2012, 2014, 2019) method, we find noisy but substantive evidence that they were more likely to hold low-skilled jobs (9 percent) and live on farms (2 percent), although the latter is an imprecise estimate.[5] Lastly, we study the effects on children in their households – i.e., the grandchildren of those directly treated – in Section 3.5. We estimate that these boys were 4 percent less likely to be in school and girls were 6 percent less likely, though only the former is statistically significant.

As such, our main contribution is some of the most detailed and longest-time-horizon estimates of the effects of income support during childhood on the health and socioeconomic outcomes of affected individuals and their descendants. These findings underscore the importance of accounting for the multi-generational consequences of government policy in addition to its long-term impacts (Aizer et al., 2016; Hoynes, Schanzenbach and Almond, 2016; Bailey et al., 2023; East et al., 2023). We also contribute to our understanding of the social consequences of this

[4] The noisy mortality estimate suggests that these results, if anything, are lower bounds on the true effects due to survivorship bias (e.g., Vaupel, Manton and Stallard, 1979).

[5] Because higher-socioeconomic-status individuals are more likely to be linked by this algorithm and false matches produce measurement error, we view the intergenerational estimates as suffering from additional downward biases (Bailey et al., 2020).

highly influential historical law (Williams, 2005; Kelly and Ó Gráda, 2011; Ager, 2014; Yamamoto, 2014; Boberg-Fazlić and Sharp, 2017; Clark and Page, 2019; Wrigley and Smith, 2020; Melander and Miotto, 2023), the impacts of welfare programs more broadly (Blundell et al., 2016; Baker, Messacar and Stabile, 2021; Galofré-Vilà et al., 2021; Pilkauskas et al., 2022), and mobility across several generations (Long and Ferrie, 2013, 2018).

2. Historical Background

2.1. Old Poor Law

After poor harvests in the 1590s led to concerns over famine and social disorder, Parliament passed the Elizabethan ("Old") Poor Law of 1601, which required parishes to support residents that could not support themselves (Collinge and Falcini, 2022). Each parish could determine eligibility for and the generosity of the relief it provided, with the elderly, widows, the sick, disabled, working families with multiple children, and the unemployed being the most common recipients. However, some parishes even supported workers when they were ill or when their earnings fell below a predetermined subsistence threshold. Relief mostly took the form of weekly stipends and rent and clothing subsidies funded by levies on occupiers of land and housing.[6]

This social spending comprised 1 percent of English and Welsh GDP through the mid-eighteenth century, which was in line with rates in Scotland, Ireland, and the rest of Europe (Lindert, 1998). However, according to Boyer (1990), several important changes led it to rise substantially in England and Wales between the 1750s and 1830 but remain mostly constant elsewhere. First, a sustained rise in grain prices led to specialization in wheat production, especially in England's Southeast, and labor demand in this process was highly seasonal. In addition, higher wheat prices increased land prices in these areas, which raised the opportunity cost to farmers of providing workers with their traditional subsistence plots. Finally, the simultaneous decline of cottage industry eliminated a source of slack-season income for men and year-round income for women and children.

Boyer (1990) argues that an implicit labor contract emerged in the Southeast as a result. Specifically, farmers would hire agricultural workers when they needed them, and when they did not, the seasonally unemployed would receive poor relief to tide them over and keep them from migrating away. Likely in combination with other factors such as regional tastes for welfare provision, there was wide variation in the provision and generosity of relief under the Old Poor Law: some parishes provided none, while in others, almost 30 percent of the population received it regularly. Furthermore, payments accounted for over 20 percent of household budgets in some parishes but less than 5 percent in others (Clark and Page, 2019).

[6] Parishes assessed taxes on the estimated rental value of property and determined land value by considering soil fertility and investments in farmhouses, buildings, roads, fences, drainage, and soil amendment. The tax rate on property was as high as 40 percent in some rural parishes.

2.2. *New Poor Law*

By 1830, around 15 percent of the English population was receiving relief payments, which cost 2 percent of GDP (Boyer, 1990, 2019). As a result, many – including founders of the new field of political economy, such as Bentham (1843), Malthus (1826), Mill (1909), and Ricardo (1817) – denounced the Old Poor Law, claiming it reduced incentives to work and save, discouraged investment in areas with high unemployment, increased the birth rate, and impeded labor mobility. This backlash led to the establishment of the 1832 Royal Commission into the Operation of the Poor Laws, culminating in the Poor Law Amendment Act of 1834 ("New Poor Law"). This reform maintained the parish's obligation to provide relief, but administration was centralized, and payments reduced so that only the most destitute applied.

The Poor Law Commission was responsible for the national operation of the system, including combining parishes into larger local units called Poor Law Unions. Eventually, the 15,000 parishes of England were combined into 600 Poor Law Unions administered by elected officials ("guardians"). The Board of Guardians was composed of the magistrates resident in the Union, along with an elected representative from each parish (though large occupiers and large landowners were given more votes in the election of guardians). Unlike parish vestries, the Boards of Guardians could, in theory, be compelled to adhere to the Poor Law Board.

Under the new system, outdoor relief was restricted to the sick and elderly, as the able-bodied were no longer eligible to receive it. In addition, workhouses became a central element of the administration of indoor relief, with each Poor Law Union required to have at least one. Although its operation was more expensive, the hope was that the new regime would push able-bodied workers to migrate in search of work. However, it was met with strong local opposition, particularly in the North, where the old system helped ease cyclical unemployment. Furthermore, the high cost of building workhouses incurred by rate payers meant that outdoor relief continued to be a popular alternative. But despite resistance, the 1834 reform led to sharp reductions in income support: between 1829-1833 and 1838-1841, per capita payments declined 6 percent in the Southwest and 33 percent in the Southeast (Clark and Page, 2019). As a result, the share of the population receiving poor relief declined to 10 percent within a few years, reducing the amount spent to 1 percent of GDP (Boyer, 2019).

3. Results

We now present our results regarding the determinants of poor relief spending across counties (Section 3.1), the New Poor Law's short-run effects on counties (Section 3.2), long-run effects on individuals who were children around 1834 (Section 3.3), and intergenerational effects on these individuals' children (Section 3.4) and grandchildren (Section 3.5).

3.1. Determinants of Declines in Poor Relief

Our first outcome of interest – the main explanatory variable in subsequent sections – is the decline in poor relief generosity in England and Wales following the law's implementation, as it is important to understand what explains its distribution. Using data digitized by Melander and Miotto (2023), we measure it as a county's change in poor relief spending between 1833 and 1834 divided by its population in the 1831 census.[7] Figure 1a shows counties grouped into quartiles of this measure. Consistent with the discussion in the previous section, almost all counties with the greatest declines were in the Southeast, and nearly all others in this region were in the next-highest quartile.

To test the hypothesis that poor relief generosity – and thus the declines in it induced by the New Poor Law – was determined by wheat farming, we use values of the wheat suitability index from FAO and IIASA (2022) as the main explanatory variable. To best approximate conditions in the early 19th century, we use climatic data from 1961-90 and assume that the water supply is low-input rainfall and no CO_2 fertilizer is used. We then take the value for all land covered by a gridcell and calculate the average for those within a county's borders. In Appendix A1, we present a map of these values grouped into quartiles, making it analogous to Figure 1a.

Our estimating equation is

$$y_c = \alpha + \tau \cdot \text{WheatSuitability}_c + \kappa_{k(c)} \cdot f(\text{lat}_c, \text{lon}_c) + \epsilon_c, \tag{1}$$

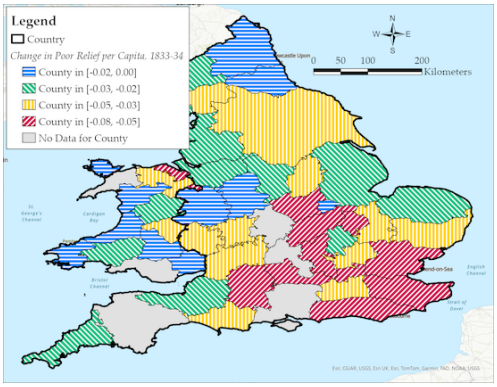
where y_c is the outcome of interest in county c, $\text{WheatSuitability}_c$ is the index value described above, $\kappa_{k(c)}$ is a fixed effect for the country k containing c, $f(\text{lat}_c, \text{lon}_c)$ is a quadratic polynomial in the latitude and longitude coordinates of c's centroid, and ϵ_c is the idiosyncratic error term. The coefficient of interest is τ: it measures the cross-sectional relationship between counties' mean wheat suitability index values and their 1833-34 declines in poor relief per capita after controlling for England- and Wales-specific spatial trends. We weight observations by 1831 population and exclude Middlesex County (most of Greater London) because of its low suitability and high weight.[8] For inference, we calculate robust standard errors.

Figure 1b presents the added-variable plot showing the partial effect of wheat suitability on 1833-34 changes in poor relief spending per capita. Consistent with the historical discussion, these measures are very strongly related: a one-standard deviation increase in the suitability index is associated with a decline in poor relief generosity of nearly one-half of a standard deviation. This relationship is also very precisely estimated.

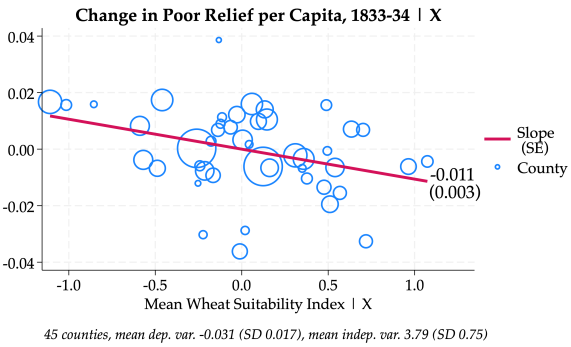
[7] We use absolute rather than relative changes in county-level spending because we want to capture absolute changes in household budgets. In other words, high- and low-poor relief counties may have experienced identical percentage declines in their per-capita expenditures, but households in the former would likely have lost far larger shares of their budgets as a result.

[8] See Appendix A2 for a plot showing Middlesex County's outlier status.

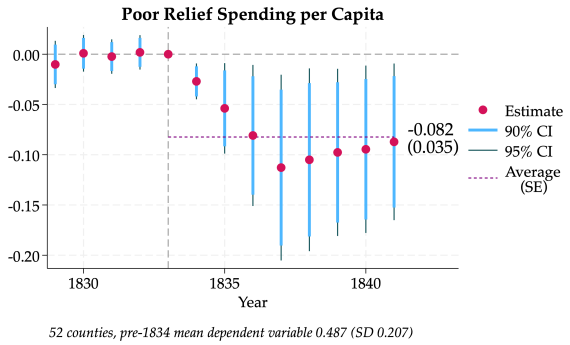
Figure 1: Determinants and Short-Run Effects of Declines in Poor Relief



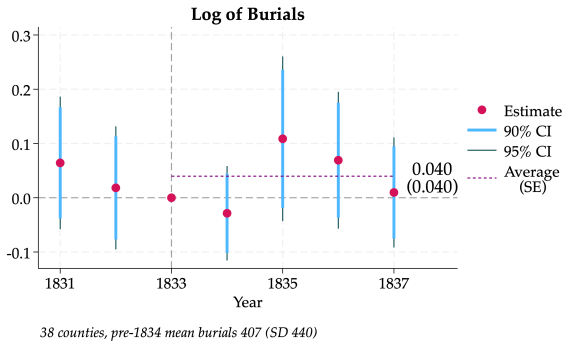
(a) Change in Poor Relief per Capita, 1833-34



(b) Wheat Suitability and Change in Poor Relief



(c) Poor Relief Spending per Capita, 1829-41



(d) Log of Burials, 1831-37

Notes: The top left panel contains a map of English and Welsh counties grouped into quartiles of 1833-34 changes in poor relief spending per capita. The top right panel contains an added-variable plot showing the partial effect of counties' mean wheat suitability index value on these changes, with counties weighted by population. The regression controls for country-specific quadratic polynomials in latitude and longitude, excludes Middlesex County (Greater London), and uses robust standard errors. The bottom panels contain event-study plots comparing outcomes across counties with above- versus below-median 1833-34 declines in poor relief spending per capita. Regressions control for country-specific non-parametric trends and cluster standard errors by county. See the text for data sources and variable definitions.

3.2. *Short-Run Effects on Counties*

3.2.1. *Poor Relief Expenditures per Capita*

We then turn to whether the 1833-34 changes are also informative about the evolution of broader trends in poor relief expenditures. To do so, we split the sample of counties at the median 1833-34 change, a decline of £0.03 per capita. Our treatment group consists of the counties with £0.03 to £0.08 per capita declines – those in Figure 1a with vertical yellow or southwest-to-northeast red lines – in which 1829-33 mean poor relief per capita was £0.56. Counties with 1833-34 declines from £0.00 to the median value – those with horizontal blue or northwest-to-southeast green lines – thus comprise our control group. The 1829-33 mean in this group was £0.38 per capita.

Our baseline estimating equation is the static two-way fixed effects (TWFE) specification

$$y_{c,t} = \alpha_c + \gamma_t + \tau \cdot \left(\mathbf{1}[t \geq 1834] \cdot \mathbf{1}\left[|\Delta_{c,1833-34}| \geq |\Delta_{\text{median},1833-34}|\right]\right) + \gamma_t \cdot \kappa_{k(c)} + \epsilon_{c,t}, \quad (2)$$

where α_c and γ_t are fixed effects for county and year, $\mathbf{1}[t \geq 1834]$ indicates whether an observation is from 1834 or later, $\mathbf{1}\left[|\Delta_{c,1833-34}| \geq |\Delta_{\text{median},1833-34}|\right]$ indicates whether c's 1833-34 decline in poor relief was above the median, and the other terms are identical or analogous to those in equation (1). The coefficient of interest is τ, which measures the average treatment-control difference in outcomes after the New Poor Law took effect relative to the average difference in the pre-1834 period, after controlling non-parametrically for England- and Wales-specific trends. To incorporate recent developments in the TWFE literature, we follow de Chaisemartin and D'Haultfœuille (2020) in estimating τ. We also weight each county by its population and cluster standard errors at this level.

In addition, we estimate the dynamic TWFE specification

$$y_{c,t} = \alpha_c + \gamma_t + \sum_{j \neq 1833} \left(\tau_j \cdot \mathbf{1}[t = j] \cdot \mathbf{1}\left[|\Delta_{c,1833-34}| \geq |\Delta_{\text{median},1833-34}|\right]\right) + \gamma_t \cdot \kappa_{k(c)} + \epsilon_{c,t}, \quad (3)$$

where $\mathbf{1}[t = j]$ indicates whether an observation is from year j and all other variables are as in equation (2). The coefficients of interest are the τ_j, which measure the average treatment-control difference in outcomes in year j relative to this difference in 1833, the omitted year. Therefore, this strategy allows us to examine the evolution of trends in the outcome over time. We follow the same estimation and inference procedures as above.

Our results in Figure 1c confirm that our treatment-control categorization captures sustained differences in poor relief declines. Specifically, while trends evolved in parallel prior to the law's implementation, spending per capita fell an average of £0.08 more in the treatment group (£0.18 versus £0.09). Therefore, these counties experienced larger declines in both absolute and relative terms (32 versus 24 percent of 1829-33 levels), though we consider the former to be far more important.

3.2.2. *Mortality*

Given that poor households in treatment counties likely experienced larger negative shocks to their budgets, it is plausible that mortality increased as a result. To test this hypothesis, we aggregate data on burials in a sample of 404 English parishes (Smith et al., 2020) to the county level. Due to apparent inconsistencies in the data across years, we restrict our attention to 1831-37 because burial counts appear consistent across this years.

Before proceeding with the analysis, there are several important caveats to consider. One is that this measure of mortality is a severe undercount of the true number of deaths: we have data from an average of just 11 parishes in 38 counties, each of which in fact had hundreds of parishes. It is also not clear that burials in these 404 parishes are representative of broader

trends in mortality within their counties. Additionally, effects on mortality might not have been apparent until at least a year after the New Poor Law was implemented: as the 1833-34 decline was much smaller than subsequent ones (see Figure 1c), possibly because the law did not take effect until later in the year, the first major shock to poor households' budgets might not have occurred until 1835.

With these important caveats, we estimate equations (2) and (3) with the log of burials in a county as the outcome. Figure 1d shows that burials increased an average of 4 percent from 1834 to 1837, although this estimate is not precise. However, it appears that adjusting for the pre-trend in the style of Rambachan and Roth (2023) would actually increase the estimated effect size. And in line with the discussion above, the dynamic coefficient for 1835 is quite large (10 percent) and almost statistically distinguishable from zero. Because this estimate contrasts with the one for 1834 (an imprecise -3 percent), it suggests that the New Poor Law might not have affected mortality until the year after it was implemented.

Therefore, we re-estimate these equations using 1834 as the omitted year. Because the results of this ad-hoc adjustment can be intuited from Figure 1d, we present the results in Appendix A3. They show that relative to 1834, mortality increases substantially in 1835 and 1836 (14 and 10 percent), and their average is statistically significant (not shown). Individually, these estimates are close to this threshold, and the same is true of the average effect for 1835-37 (9 percent), which would also be larger after adjusting for the pre-trend. Taken together, we interpret these results as weak but suggestive evidence of increases in mortality resulting from larger declines in poor relief expenditures per capita.

3.3. Long-Run Effects on Adults Exposed as Children

Next, we examine the long-run effects of the New Poor Law on individuals who were children around the time it was implemented using the full-count 1861 census of England and Wales from IPUMS. This dataset contains information on all members of a household, including their occupations, counties and years of birth, and whether they live on a farm. Detailed occupational information allows us to determine whether an individual has a low- or high-skilled job according to the 1988 International Standard Classification of Occupations.[9] We use this skill variable and the household's farm status as our outcomes of interest, which we view as proxies of adult income and accumulated human capital. Table 1 presents summary statistics of the 1861 and 1901 censuses, the latter of which is described in Section 3.4.2.

To measure childhood exposure to declines in poor relief, we first assign individuals a treatment status using their counties of birth and the previous section's above- versus below-median declines in poor relief per capita between 1833 and 1834. We then define childhood as ending at age 15, as nearly all children in the 1861 census had left school by that point. Therefore, we

[9] For example, laborers, messengers, service workers, and clerks are classified as low-skilled, and civil servants, teachers, and, medical professionals are high-skilled.

Table 1: Summary Statistics

	Household Heads		Children	
	Mean (1)	SD (2)	Mean (3)	SD (4)
Panel A. 1861 Census				
Female	0.52		0.50	
Student			0.43	
Single	0.18			
Number of children	2.11	2.17		
Employed	0.88			
Low-skilled job	0.37			
Living on farm	0.09			
Observations	4,359,148		5,107,407	
Panel B. 1901 Census				
Female	0.52		0.49	
Employed (not in school)			0.74	
Single	0.13			
Number of children	2.46	2.36		
Employed	0.94			
Low-skilled job	0.45			
Living on farm	0.06			
Observations	4,872,267		3,232,180	
Panel C. 1901 Matched Sample				
Female			0.49	
Employed (not in school)			0.71	
Single	0.13			
Number of children	2.46	2.33		
Employed	0.95			
Low-skilled job		0.49		
Living on farm	0.08			
Observations	165,012		158,377	

Notes: We use the ABE algorithm to match sons in 1861 to their adult observations in 1901.

consider the 1819 birth cohort (i.e., age 15 in 1834) to have 1 year of childhood exposure to the New Poor Law, whereas those born in 1818 (i.e., age 16 in 1834) had none. Our hypothesis is that more childhood years of exposure resulted in worse adult outcomes because poor households that lost income support would have had fewer resources to invest in their children. Therefore, our results should have cohort-based patterns similar to those in, e.g., Bleakley (2007, 2010).

To test these hypotheses, we estimate a static TWFE specification analogous to equation (2):

$$y_{i,c,b} = \alpha_c + \gamma_b + \cdot\left(\mathbf{1}[b \geq 1819] \cdot \mathbf{1}\left[|\Delta_{c,1833-34}| \geq |\Delta_{\text{median},1833-34}|\right]\right) + \gamma_b \cdot \kappa_{k(c)} + \epsilon_{i,c,b}, \quad (4)$$

where subscripts denote individual i from county of birth c and birth cohort b, and all other

variables are as before. The coefficient of interest is τ, which captures the average treatment-control difference in outcomes for cohorts who were exposed to declines in poor relief relative to the average difference for unexposed cohorts, net of non-parametric country-specific trends. Mirroring equation (3), we also estimate the dynamic TWFE analogue to equation (4),

$$y_{i,c,b} = \alpha_c + \gamma_b + \sum_{b \neq 1818} \left(\tau_j \cdot \mathbf{1}[b = j] \cdot \mathbf{1}\left[|\Delta_{c,1833-34}| \geq |\Delta_{\text{median},1833-34}|\right]\right) + \gamma_b \cdot \kappa_{k(c)} + \epsilon_{i,c,b}, \quad (5)$$

in which the τ_j measure this treatment-control difference in birth cohort j relative to this difference in the 1818 birth cohort. We estimate both specifications separately by sex and cluster standard errors by county of birth.

For our empirical strategy to accurately estimate these effects, it must be that the likelihood of an individual who was a child around the New Poor Law's implementation was not different across treatment and control groups. Given the weak but suggestive evidence of greater mortality in treatment counties after 1834, this assumption is potentially a bad one. However, if the increase in mortality was greatest among the poorest households, the treatment group should be more skewed toward those whose parents were of higher socioeconomic status. The impact on our results should be a bias against finding significant effects of the declines in poor relief during childhood (e.g., Vaupel, Manton and Stallard, 1979).

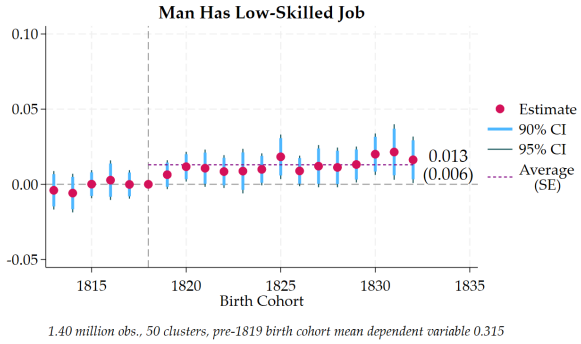
Figure 2 presents the long-run results, which are quite consistent with our hypothesis. In particular, Figure 2a shows that men exposed to the New Poor Law as boys had a 1.3-percentage point (p.p.) greater likelihood of holding a low-skilled job in 1861, which was 4 percent of the mean for pre-1819 birth cohorts. This average effect and almost all of the dynamic coefficients are precisely estimated. The effect on women's likelihood of holding low-skilled jobs in Figure 2b is larger in absolute terms (2.3 p.p.), but it is effectively the same size relative to the mean for pre-1819 cohorts and is much noisier. However, several of the dynamic coefficients for later cohorts are (close to being) statistically significant, and it is possible that adjusting for the slight positive pre-trend would make more of them or the average effect distinguishable from zero. Lastly, Figure 2c shows a 0.4-p.p. increase in exposed men's likelihood of living on a farm (4 percent of pre-1819 cohorts' mean), although it is also imprecise and fewer of the dynamic coefficients are close to statistical significance. But taken together, these results suggest that declines in poor relief had substantially negative impacts on (proxies of) adult income and human capital for those more exposed in childhood.

3.4. Effects on the Next Generation

3.4.1. As Children in 1861

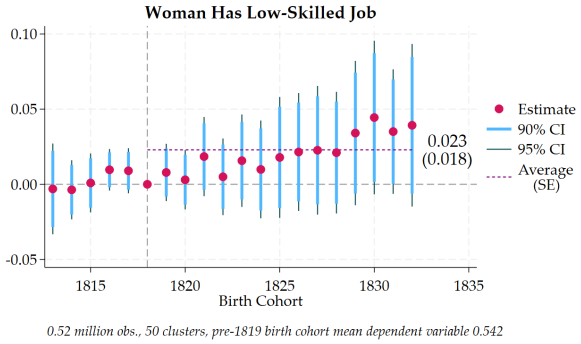
We also use the 1861 census to study the childhood human capital accumulation of the next generation. We look within the households headed by the adults examined above (birth cohorts 1813-32) and examine the school attendance status of the children present, as completed

Figure 2: Long-Run Effects on Adults Exposed as Children, 1861



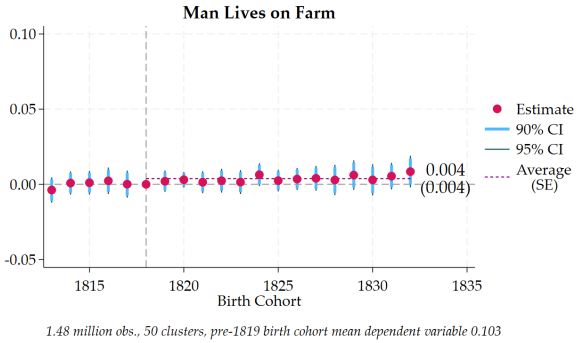
1.40 million obs., 50 clusters, pre-1819 birth cohort mean dependent variable 0.315

(a) Low-Skilled Job: Men



0.52 million obs., 50 clusters, pre-1819 birth cohort mean dependent variable 0.542

(b) Low-Skilled Job: Women



1.48 million obs., 50 clusters, pre-1819 birth cohort mean dependent variable 0.103

(c) Lives on Farm: Men

Notes: The panels contain event-study plots comparing outcomes across individuals from counties of birth with above- versus below-median 1833-34 declines in poor relief spending per capita. Regressions control for country of birth-specific non-parametric trends and cluster standard errors by county of birth. See the text for data sources and variable definitions.

schooling was not recorded. We define attending school using the University of Essex Integrated Census Microdata occupational classifications of "other students" and "scholars etc," and we set our age range of interest as 3 to 15 years old because this lower bound was the first age at which substantive numbers of children were listed in this category.

Our hypothesis is that fathers more exposed to declines in poor relief in childhood would have had fewer resources to invest in their children's schooling, so fewer of them should have been attending school. To test it, we attach fathers' treatment statuses and birth cohorts to the children in their households and estimate a static TWFE specification similar to equation (4):

$$y_{i,\hat{c},\hat{b}} = \alpha_{\hat{c}} + \gamma_{\hat{b}} + \cdot\left(\mathbf{1}[\hat{b} \geq 1819] \cdot \mathbf{1}\left[|\Delta_{\hat{c},1833-34}| \geq |\Delta_{\text{median},1833-34}|\right]\right) + \mathbf{X}_i\beta + \gamma_{\hat{b}} \cdot \kappa_{k(\hat{c})} + \epsilon_{i,\hat{c},\hat{b}}, \quad (6)$$

where the $\hat{c}$ and $\hat{b}$ denote fathers' counties and cohorts of birth, $\mathbf{X}_i$ is a vector of individual-level controls (child i's age and age squared), and all other variables are as before. The coefficient of interest is again τ, which captures the average treatment-control difference in outcomes for children with fathers that were exposed to declines in poor relief in childhood relative to the

average difference for children of unexposed fathers, net of non-parametric father's country of birth-specific trends. We also estimate the dynamic TWFE version of equation (6),

$$y_{i,\hat{c},\hat{b}} = \alpha_{\hat{c}} + \gamma_{\hat{b}} + \sum_{\hat{b} \neq 1818} \left(\tau_j \cdot \mathbf{1}[\hat{b} = j] \cdot \mathbf{1}\left[|\Delta_{\hat{c},1833-34}| \geq |\Delta_{\text{median},1833-34}| \right] \right) + \mathbf{X}_i \beta + \gamma_{\hat{b}} \cdot \kappa_{k(\hat{c})} + \epsilon_{i,\hat{c},\hat{b}}, \tag{7}$$

where the τ_j measure the treatment-control difference for children with fathers in birth cohort j relative to this difference for children with fathers born in the 1818. We continue to estimate both specifications separately by sex and cluster standard errors by father's county of birth.

We present our results in Figures 3a and 3b. The former shows that sons aged 3 to 15 with fathers who more exposed to declines in poor relief as children were 2.1 p.p. less likely to be in school in 1861, which was 4 percent of the mean among their peers with fathers who were unexposed as children. This average effect and many of the dynamic coefficients are precisely estimated; nevertheless, adjusting for the negative pre-trend would make them even larger. The latter shows a slightly smaller impact on daughters – a decline of 1.9 p.p., or 3 percent of the mean for daughters of unexposed fathers – despite a smaller negative pre-trend. However, the average effect and several of the dynamic coefficients are more precisely estimated. We thus conclude that greater childhood exposure to declines in income support reduced the human capital accumulation of the next generation.

3.4.2. As Adults in 1901

We then turn to studying the second generation's outcomes in adulthood. Because information about fathers' places of birth was not collected, we link sons observed in 1861 to their records in the full-count 1901 census from IPUMS, which allows us to attach to them their fathers' birth counties and cohorts. To perform this linkage, we take advantage of granular identifying data available in both datasets: surname, (standardized) parish of birth, (implied) year of birth, and sex.[10] With this information, we use the Abramitzky, Boustan and Eriksson (2012, 2014, 2019) algorithm (based on Ferrie, 1996) to match sons across these censuses. Restricting the sample to linked observations whose implied years of birth in the 1861 and 1901 censuses are within 2 years of each other, we achieve a match rate of just under 5 percent (108,000 out of 2.33 million observations). When further limiting the sample to those with identical implied birth years, our match rate falls to just under 3 percent (63,000 observations).

As in Bailey, Cole and Massey (2020), a simple regression method is used to test the representativeness of the matched sample. Specifically, we start with the reference population data (the 1901 census), and create a dummy variable equal to 1 if we successfully link sons in the 1861 census to their adult records in 1901. Given that we only try to link males, the linkable popula-

[10] We do not attempt to link daughters given that surnames generally changed with marriage and those who remained unmarried would have been a highly selected sample.

Table 2: Regression Test of Representativeness

	Unweighted (1)	IP weighted (2)
Living in England in 1901	0.0029	-0.0102
	(0.0014)	(0.0165)
Born in England	0.0077	-0.0202
	(0.0013)	(0.0166)
Age	0.0002	0.0001
	(0.0001)	(0.0005)
Single	0.0163	-0.0076
	(0.0006)	(0.0057)
Any children	0.0022	-0.0089
	(0.0005)	(0.0061)
Employed	-0.0081	0.0037
	(0.0027)	(0.0188)
Employed in a low-skill job	-0.0020	0.0041
	(0.0009)	(0.0073)
Living on a farm in 1901	0.0014	0.0020
	(0.0010)	(0.0088)
Observations	538,641	538,641
R^2	0.002187	0.0002129
F-statistic	134.2	0.9773

Notes: The dependent variable is a dummy variable equal to 1 if a record is linked, and 0 if in the linkable population. Both specifications restrict attention to male household heads in 1901. Heterogeneity-robust standard errors are in parentheses.

tion is defined to be male household heads in the 1901 census, dropping all men born outside of England and Wales, as well as those living abroad in 1901. We then regress the dummy variable on a series of covariates describing the reference population using heterogeneity-robust standard errors. Table 2 summarizes the results, presenting both the individual estimates and the test of joint significance. The results displayed in the first column show that almost every covariate is predictive of match success, and that the F-test easily rejects the null of representativeness. However, although the coefficients are nearly all statistically significant, the magnitudes suggest that they may not be economically significant. Nevertheless, we use inverse propensity (IP) score reweighting to adjust for the imbalance (second column).[11] Applying these IP weights makes a meaningful difference to representativeness, with the covariates no longer predictive of match success, and the F-test failing to reject representativeness.[12]

[11] This approach requires that the propensity of being linked is properly specified and can be consistently estimated (unconfoundedness), and that the distribution of the propensity of being linked spans the same support as the reference population (common support).

[12] Given the relative paucity of information collected in historic censuses, it is possible that the resulting data may still be unbalanced in terms of unobserved characteristics (reweighting only accurately addresses bias from non-representativeness if the unconfoundedness assumption holds). However, the hope is that reweighting at least mitigates the problem of non-representativeness of linked data (DiNardo, Fortin and

The dataset of linked adult sons is used to estimate equations (6) and (7) for our outcomes of interest, which are the same as in Section 3.3: whether they are in low-skilled jobs and whether they live on farms. However, although reweighting improves representativeness, there are still two important biases that tend to arise when analyzing data using this method to link records. First, false matches create measurement error, which leads to overstated intergenerational mobility. In our context, it would create a bias toward finding null effects on adult sons of their fathers having been more exposed to declines in poor relief as boys. This bias can be further exacerbated if those from higher socioeconomic status families are more likely to be matched (Bailey et al., 2020).

Cognizant of these biases toward zero, we present our results in Figures 3c and 3d. The former shows that sons aged 41 to 55 in 1901 were 3.2 p.p. more likely to be in low-skilled jobs, which is just under 9 percent of the mean for the sons of men in the pre-1819 cohorts. Notably, there is substantial noise in these estimates, although a number of the dynamic coefficients are distinguishable from zero. In addition, adjusting for the positive pre-trend would likely make these results larger. In contrast, the effect on farm status is much smaller: 0.2 p.p., or 2 percent of the mean for sons of men in the pre-1819 cohorts. We thus conclude that there is evidence of a negative effect of the New Poor Law on the incomes of the sons of men whose families lost income support during their childhoods.

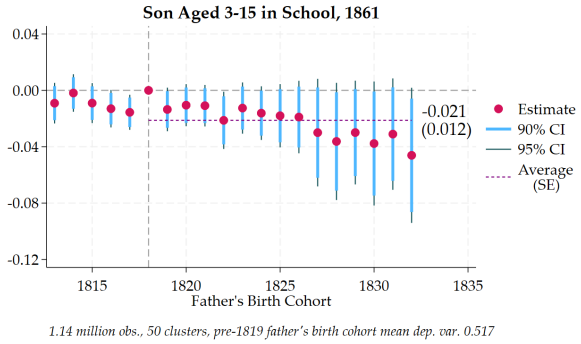
3.5. *Effects on the Third Generation*

Finally, we examine investments in the human capital of the children of linked sons in the 1901 census – i.e., the grandchildren of men who were affected by the New Poor Law as boys. We use the same approach as in Section 3.4.1 of attaching (grand)fathers' information to the records of their (grand)children. Unfortunately, these data do not appear to contain a reliable measure of school attendance. Our outcome of interest is thus whether a child was listed as being employed, which implies they were no longer in school. We focus on ages 11 to 15 because of the greater numbers of those in the labor force in this range.

Again aware of the bias toward zero discussed in the previous section, we estimate equations (6) and (7) for this outcome, where $\hat{c}$ and $\hat{b}$ are grandfathers' counties and cohorts of birth. We separate our results by sex and present them in Figure 4. For the 11- to 15-year-old grandsons of those exposed to the New Poor Law, Figure 4a shows that they were 3.2 p.p. more likely to be employed in 1901, which is just under 4 percent of the mean employment rate among peers with grandfathers from pre-1819 cohorts. While this average effect is somewhat imprecisely estimated, many of the dynamic coefficients are statistically significant. In addition, Figure 4b shows an even larger effect on granddaughters: 4.5 p.p., or 6 percent of the mean for girls whose grandfathers were in pre-1819 cohorts. However, this average estimate and the dynamic coefficients are highly

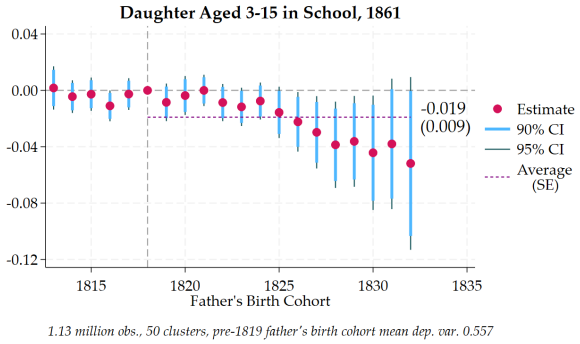
Lemieux, 1996; Heckman, 1979)

Figure 3: Effects on the Next Generation as Children (1861) and Adults (1901)



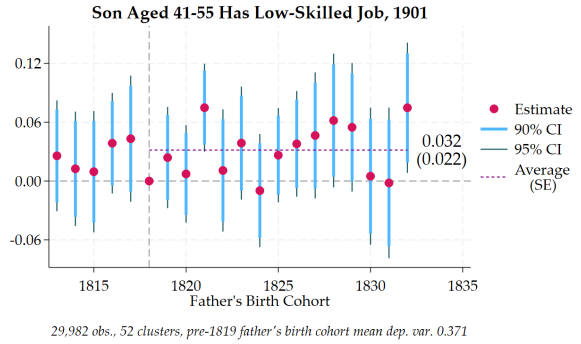
1.14 million obs., 50 clusters, pre-1819 father's birth cohort mean dep. var. 0.517

(a) In School: Son Aged 3-15, 1861



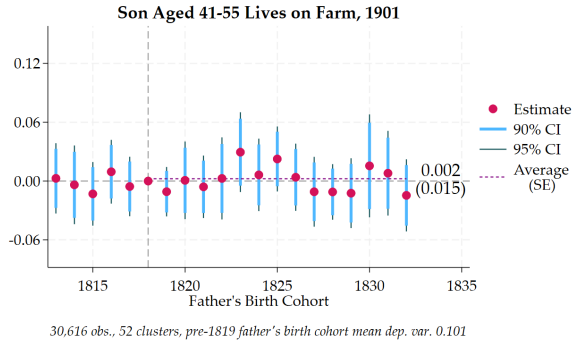
1.13 million obs., 50 clusters, pre-1819 father's birth cohort mean dep. var. 0.557

(b) In School: Daughter Aged 3-15, 1861



29,982 obs., 52 clusters, pre-1819 father's birth cohort mean dep. var. 0.371

(c) Low-Skilled Job: Son Aged 41-55, 1901



30,616 obs., 52 clusters, pre-1819 father's birth cohort mean dep. var. 0.101

(d) Lives on Farm: Son Aged 41-55, 1901

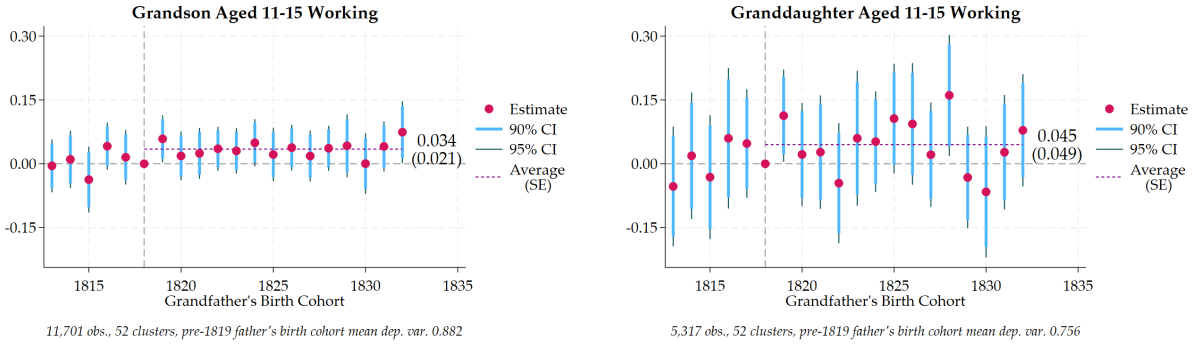
Notes: The panels contain event-study plots comparing outcomes across children in households with fathers from counties of birth with above- versus below-median 1833-34 declines in poor relief spending per capita. Regressions control for father's country of birth-specific non-parametric trends and cluster standard errors by father's county of birth. See the text for data sources and variable definitions.

imprecise. But taken together, these results suggest that the declines in income support had substantive effects on the grandchildren of those exposed during childhood.

4. Conclusion

Although governments often reduce welfare spending in an attempt to lower deficits and encourage program beneficiaries to find employment, recent work has documented the negative shorter-run consequences of making such cuts. In addition, many anti-poverty programs around the world now include direct payments to recipients, making it important to understand their long-run consequences. Our results contribute to these literatures by examining the long-run and intergenerational effects of the New Poor Law, which made the largest welfare cut in British history by cutting income support to poor families. Exploiting geographic heterogeneity in welfare-spending declines and using detailed microdata from the 1861 and 1901 censuses of England and Wales, we show that the negative effects of withdrawing income support during childhood

Figure 4: Effects on the Third Generation as Children, 1901



(a) Employed: Grandson Aged 11-15 **(b)** Employed: Granddaughter Aged 11-15

Notes: The panels contain event-study plots comparing outcomes across grandchildren of men from counties of birth with above- versus below-median 1833-34 declines in poor relief spending per capita. Regressions control for grandfather's country of birth-specific non-parametric trends and cluster standard errors by grandfather's county of birth. See the text for data sources and variable definitions.

were felt across three generations. In particular, adults exposed to welfare cuts as children were more likely to hold low-skilled jobs, their children had worse educational and labor market outcomes, and their sons – the grandsons of those directly exposed to the law's effects – were less likely to be in school nearly 70 years later. As such, this paper underscores the importance of incorporating multi-generational effects when conducting cost-benefit analyses of social policy.

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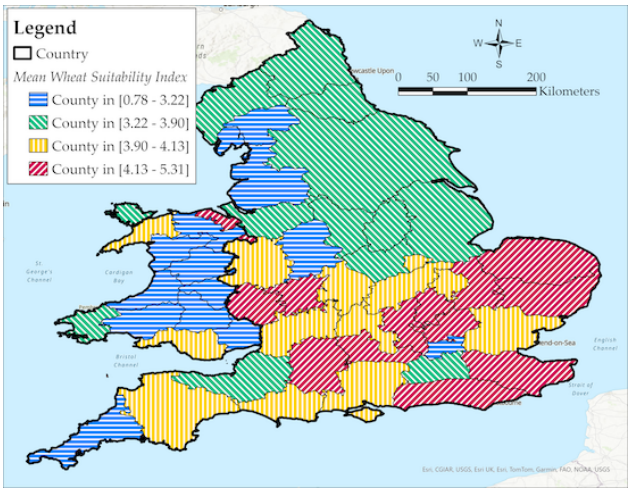
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Appendix A. Additional Figures

A1. Wheat Suitability

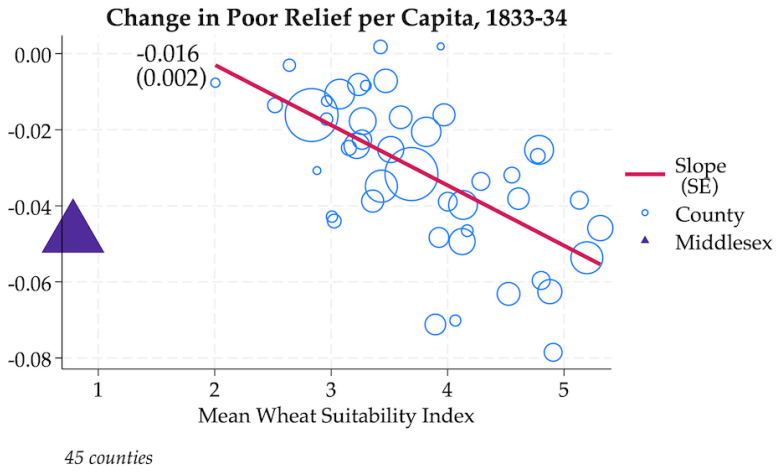
Figure A1: County-Level Wheat Suitability [5]



Notes: The map shows quartiles of mean wheat suitability index values by county. Middlesex County (Greater London) is only the southeastern county in the lowest quartile (blue horizontal stripes).

A2. Outlier in Wheat Suitability

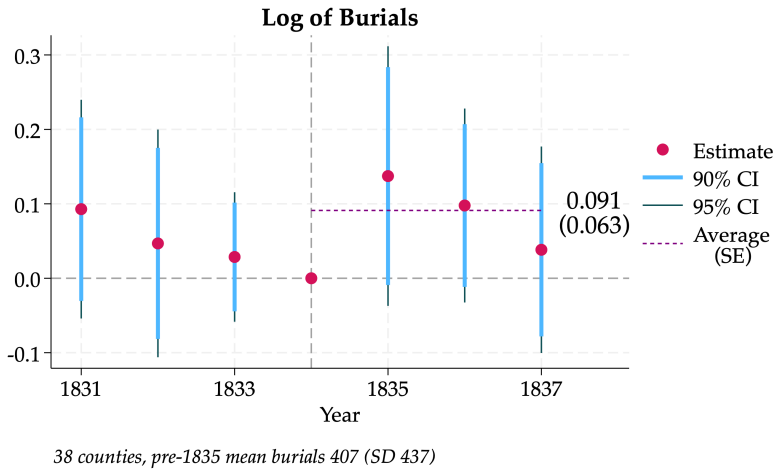
Figure A2: Highlighting Outlier in Wheat Suitability [5]



Notes: The scatterplot shows the unconditional relationship between wheat suitability and 1833-34 changes in poor relief spending per capita. Counties are weighted by population and Middlesex County (triangle) is excluding from the calculation of the fitted line. See Section 3.1 for data sources and variable definitions.

A3. Mortality

Figure A3: Event-Study Plot for Log of Burials with 1834 as Omitted Year [7]



Notes: The event-study plot compares outcomes across counties with above- versus below-median 1833-34 declines in poor relief spending per capita. The regression controls for country of birth-specific non-parametric trends and clusters standard errors by county of birth. The omitted year is 1834 instead of 1833 as in the main text. See Section 3.2.2 for data sources and variable definitions.