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Welfare cuts and crime: Evidence from the New Poor Law

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Eric Melander
Martina Miotto

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Eric Melander   Martina Miotto

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Abstract

The New Poor Law reform of 1834 induced dramatic and heterogeneous reductions in welfare spending across English and Welsh counties. Using the reform in a difference-in-differences instrumental variables strategy, we document a robust negative relationship between the generosity of welfare provision and criminal activity. Results are driven by non-violent property crimes and are stronger during months of seasonal agricultural unemployment, indicating that a combination of welfare cuts and precarious work opportunities lowered the opportunity cost of crime for economically vulnerable individuals. We use data on county police forces and individual-level criminal records to rule out alternative mechanisms related to changes in policing and sentencing.

Keywords: welfare spending, austerity, crime, poor laws
JEL Codes: H53, I38, K42, N33

1   Introduction

Fiscal consolidation – austerity – is used by governments worldwide to overcome periods of macro-economic instability. The macroeconomic effects of austerity measures have been investigated in a large body of literature.[1] However, fiscal policy can affect other key outcomes that policy-makers do not necessarily foresee (Accetturo, De Blasio and Ricci (2014)). Budget cuts may have particularly adverse effects on the already economically vulnerable (Watkins et al., 2017), and can provoke socio-political reactions (Ponticelli and Voth (2020); Fetzer (2019); Galofré-Vilà, Meissner, McKee and Stuckler (2021)). Remarkably, relatively little is known about the impact of austerity-induced welfare cuts on criminal behaviour.[2]

In this paper, we study the effect of reductions in welfare spending on criminal activity in the wake of substantial budget cuts. To this end, we consider a well-known, sudden and dramatic historical policy reform: the implementation of the Poor Law Amendment Act of 1834 which enacted the New Poor Law in England and Wales. The Act centralised the administration of welfare and reduced welfare payments to the poor – or, to use the historical term, poor relief – by deterring any but the most destitute from applying for relief. A key feature of these shocks to counties' poor relief spending is that they were heterogeneous across England and Wales. We exploit this variation to identify a causal relationship between relief generosity and crime rates by comparing counties with different levels of pre-reform welfare payments, and thus with differential reductions in welfare generosity in the wake of the reform.[3]

This paper relies on an extensive collection of novel historical data from primary sources. We gather yearly data on poor relief expenditures between 1820 and 1842 for all fifty-two English and Welsh counties. We link them to county-level rates of different types of crime during the same period. Additionally, we digitise the universe of over 250,000 individual-level criminal charges recorded between 1828 and 1840 to explore mechanisms by exploiting information on perpetrators and punishments. We complement these data with a host of additional data to rule out alternative mechanisms and explore possible confounders.

We present three main findings. First, we find that reductions in per capita poor relief spending are associated with increases in criminal activity. This result holds when using a simple OLS framework and when using the asymmetric reductions in poor relief spending that resulted from the New Poor Law to instrument for welfare expenditures. Our estimates suggest that a one standard deviation decrease in poor relief spending causes a 0.20-0.34 standard deviation increase in criminal activity, an economically sizeable effect. Importantly, these results are robust to controlling for drivers of between-county differences in poor relief spending before the reform.

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[1]For summaries of theoretical and empirical work on this topic, see Alesina, Barbiero, Favero, Giavazzi and Paradisi (2017) and Alesina, Favero and Giavazzi (2018).

[2]Important exceptions studying UK welfare reform and crime in the context of twenty-first century austerity are recent working papers by d'Este and Harvey (2020) and Giulietti and McConnell (2020).

[3]A rich literature on the history of the Poor Laws, which we summarise in Section 2, indicates that previously high-spending counties were characterised by an agrarian economy and proximity to London. We confirm these descriptive patterns using our newly-collected data (described in Section 3), and control for such differences throughout our analysis.

What explains the relationship between reductions in poor relief and subsequent increases in criminal activity? Our second set of results exploits the granularity of our data to investigate a possible channel: the less generous system of relief introduced by the reform lowered the opportunity cost of some crimes (committed to ensure individuals' basic needs) and induced a greater number of individuals to take the risks associated with committing them. The risky gains from criminal activity substituted for what had previously been afforded through receipt of poor relief. This interpretation is supported by qualitative work on the "economy of makeshifts" by Ager (2014). Regressions of different types of crime on poor relief spending reveal that welfare reductions mainly affect non-violent property crimes (such as larceny, receiving stolen goods and cattle theft). Effects are heterogeneous across seasons and are stronger during the winter months of high agricultural unemployment. The seasonal link between agriculture and criminal activity suggests that precarious seasonal workers are most affected by the New Poor Law welfare cuts, and commit more petty crimes as a result.

Last, we provide evidence that our main results are not driven by plausible alternative mechanisms. The nineteenth century saw the gradual professionalisation of police forces throughout the country. We explore the possibility that latent county characteristics can be related to both the introduction of police forces and the introduction of the New Poor Laws in a manner that would confound the effects on crime that we document. Flexibly controlling for the size of county police forces and the timing of their introduction does not statistically change results compared to baseline estimates. Additionally, we provide evidence that results are not driven by systematic changes in sentencing (which could alter the expected returns to criminal activity). Using detailed data on individual trials we show that, within broad categories of crime, the outcome of a trial does not vary systematically with levels of poor relief spending.

With this paper, we speak to several strands of the economics literature. First, we contribute to a large body of work on the consequences of austerity. Alongside the traditional debate on the economic impact of austerity measures (Alesina, Barbiero, Favero, Giavazzi and Paradisi (2017); Alesina, Favero and Giavazzi (2018); House, Proebsting and Tesar (2020)), more recently attention has turned to other key socio-political outcomes. Galofré-Vilà, Meissner, McKee and Stuckler (2021) study the political consequences of austerity, documenting an increased vote share for the Nazi party in Germany as a result of spending cuts and tax increases throughout the final years of the Weimar Republic. Fetzer (2019) shows how austerity-induced welfare reforms after the 2009 financial crisis increased popular support for the UK Independence Party and eventually led to Brexit. Health and social care spending constraints imposed after 2010 in England additionally are associated with a substantial mortality gap (Watkins, Wulaningsih, Da Zhou, Marshall, Sylianteng, Rosa, Miguel, Raine, King and Maruthappu, 2017). Meanwhile, Ponticelli and Voth (2020) explore another important social outcome: social unrest and violent protest. Looking at European countries throughout the entire twentieth century, they find

a positive correlation between fiscal retrenchment and instability. We contribute to this literature by expanding its scope to another adverse social consequence of austerity-induced welfare reform, namely criminal behaviour.

Second, we contribute to a rich literature on the link between welfare and crime. Descriptive work has provided evidence of correlational relationships between modern welfare cuts and crime, but lacks causal identification (Zhang (1997); DeFronzo and Hannon (1998a,b)). Additionally, a large part of this literature focuses on the effects of expansions – as opposed to reductions – of welfare spending.[4] Therefore, we make a number of important contributions. By focusing on a different, earlier period at the height of the industrial revolution, we offer complementary evidence to that provided by the literature on more modern welfare reforms, from a time when the welfare state was in its relative infancy. We also focus on welfare cuts instead of increases in welfare spending, which potentially induce different responses in criminal behaviour. Finally, we provide a robust and well-identified estimate of the relationship between welfare cuts and crime, using a natural quasi-experiment which allows for a causal interpretation of our results.[5]

Third, we contribute to a broader literature on historical poor relief reforms, which spans a range of qualitative and descriptive studies (Williams (2005); Cousins (2011); Ager (2014); Ciprian (2016)) and quantitative empirical analyses (Rushton and Sigle-Rushton (2001); Presbitero (2009)). The English and Welsh poor laws – the setting of this paper – have received considerable interest in recent years. A number of papers explore their impact on a range of outcomes, such as the livelihoods of the poor (Richardson (2017)), social costs (Clark and Page (2019)), unemployment rates and wages (Yamamoto (2014)), charitable activities (Boberg-Fazlić and Sharp (2017)), fertility rates (Wrigley and Smith (2020)) and social mobility (Boberg-Fazlić and Sharp (2018)). Chapman (2020) studies the determinants of poor relief spending within the context of the New Poor Law during the late nineteenth century. In this paper, we add to this literature in several ways. We examine the as-yet unexplored link between crime rates and changes in poor relief spending as a result of the implementation of the New Poor Law. Additionally, by combining the natural experiment provided by the New Poor Law with modern empirical methods, we are able to uncover causal effects of the reform.

Last, a central contribution of this paper is to compile and digitise a host of historical data directly

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[4]See, for example, Meloni (2014), Loureiro (2012), Chioda, De Mello and Soares (2016) and Camacho and Mejía (2013) on Latin America and Fishback, Johnson and Kantor (2010) and Foley (2011) on the United States.

[5]This paper also complements a growing literature on the economics of crime. This literature has its theoretical roots in the pioneering work by Becker (1968), which models potential criminals as rational agents. This work launched a rich theoretical literature, including contributions by Stigler (1970), Ehrlich (1973) and Block and Heineke (1975). Meanwhile, a recent and growing empirical literature has focused on the causes of criminal behaviour. In addition to the already cited studies investigating the relationship between welfare expenditures and crime, other works have looked at the relationship between crime and poverty (Mehlum, Miguel and Torvik (2006); Iyer and Topalova (2014); Chambru (2020)), labour market changes (Schmidt and Witte (1984); Grogger (1998), minimum wages (Hashimoto (1987); Beauchamp and Chan (2014); Braun (2019)), exposure to chemical elements (Feigenbaum and Muller (2016)), policing (Di Tella and Schargrodsky (2004); Draca, Machin and Witt (2011)) and, particularly relevant for the context of our paper, professional police activities in nineteenth-century England (Bindler and Hjalmarsson (2019)). Other recent papers have looked at determinants of incarceration, including migration (Eriksson (2019)) and education (Lochner and Moretti (2004); Eriksson (2020)).

from primary sources, covering all of England and Wales, bringing to light yearly county-level data on poor relief expenditures, the rates of different types of crime, and individual-level data on criminal activities for more than two decades.

In the next Section we outline the historical background, describe the system of poor relief in place before and after the New Poor Law Amendment Act of 1834, and illustrate crime trends for the first half of the nineteenth century. We describe the data we hand-collected from primary sources in Section 3, and we lay out our empirical strategy in Section 4. In Section 5, we show that the reductions in poor relief spending following the introduction of the New Poor Law in 1834 caused an increase in criminal activity, particularly in non-violent property crimes and during the agricultural off-season when seasonal unemployment was high. Finally, in Section 6 we offer some concluding remarks.

2 Historical Background

2.1 The New Poor Law

The New Poor Law Amendment Act of 1834 was intended to centralise and standardise the administration of poor relief and to reduce welfare payments to the poor by deterring any but the most destitute from applying for relief. One of the main reasons for its introduction was the rapid and sustained increase in the cost of poor relief under the Old Poor Law system. Historical accounts report, indeed, that by 1830 it accounted for one-fifth of national expenditure (Ager (2014)). Rising expenditures were the consequence of sustained population growth from the mid-eighteenth century onwards and a fall in rural wages which necessitated the relief of the poor (Englander (1998)). One of the principal advocates for the cut in poor relief was Thomas Malthus, whose ideas and theories influenced the political discourse on the poor laws and inspired the design of the New Poor Law. Malthus believed that the cause of pauperism was rooted in the excessive procreation of the lower classes and urged for a new, more austere poor relief system in an attempt to alleviate the obligation of the rich to mitigate the conditions of the poor and to deter population growth (Malthus (1798, 1803)).[6]

The Poor Law Amendment Act had several mechanisms for achieving its goal of reducing overall poor relief expenditure. First, the establishment of poor law unions and a system of indoor relief to be administered inside workhouses.[7] Workhouses were total institutions in which paupers could receive relief – in cash and in kind – in exchange for their work. However, the work and living conditions were

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[6]Many studies have tried to assess whether Malthus's theories were rooted in fact, reaching the overall conclusion that he was mistaken (Griffith (1926); Blackmore and Mellonie (1927); Krause (1958); Huzel (1969); Wrigley and Smith (2020)). However, many of his contemporaries were profoundly influenced by his thinking, which is evident in a number of official government papers published in the early nineteenth century (Huzel (1969)). The Poor Law Report of 1834, which sought to evaluate spending under the Old Poor Law system, supported Malthus's view. However, historical research suggests that the negative narrative presented against the old system was heavily influenced by contemporaries' opinions rather than real evidence (Blaug (1964)).

[7]Workhouses were not a new feature introduced by the New Poor Law, but from 1834 they assumed a new central role in the administration of indoor relief and each union was required to have at least one.

purposely chosen to make those of an independent labourer of the lowest class more attractive. This was the basis for the so-called workhouse test: whoever accepted relief in the repellent workhouse must be a member of the "undeserving poor", lacking moral determination and therefore distinguishable from the "deserving poor". By making relief accessible only through workhouses, outdoor relief was officially outlawed in most cases. However, historical accounts show that, in practice, relief administrators had substantial autonomy over relief policies and often continued to issue outdoor relief when necessary. The second mechanism was removal under the settlement laws. Following this principle, poor individuals would be removed from the parish in which they claimed relief, unless that was their birth-parish. This measure served as a further deterrent to apply for relief.

These mechanisms – removal under settlement laws and exclusive indoor relief inside repugnant workhouses – were met with resistance and opposition. Popular campaigns protesting against workhouse conditions took place leading to social unrest throughout the country.[8] However, the reforms were eventually implemented throughout England and Wales and caused a drop in poor relief expenditure as reported in Figure 1.[9,10] The figure also shows that the reduction was disproportionally large in counties where levels of pre-reform poor relief spending were higher, highlighting how the newly introduced measures had differential impacts across the country. The spatial pattern of poor relief reductions can be seen in Figure 2a: the agrarian counties in the south-east were hardest hit by the reductions in welfare spending, relative to the more industrial north.

Lastly, the enactment of the New Poor Law had a political dimension. First, as a political institution it granted power and patronage to those in charge of its administration (Fraser (1976)). Second, it took place simultaneously with the shift of electoral power at the national and local levels from the labour-hiring landlords and farmers – who had a major interest in keeping poor relief generous enough to retain otherwise migrating labourers – to merchants and artisans (Boyer (1990); Lindert (1998)). These developments are important potential confounders, and we provide checks to confirm that they do not explain our findings in Section 5 below.

2.2 Crime, Policing and Punishment

Real crime rates for England and Wales during the first half of the nineteenth century are not possible to recover, as records of offences *known* to have been committed were not kept before 1857. However, data

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[8]A popular critique of the New Poor Law can also be found in the narrative of Charles Dickens's famous novel *Oliver Twist*, published as a serial from 1837 to 1839.

[9]Figure 1 shows the drop in poor relief *per capita*, as available data only report county-level poor relief expenditure and total population. It would be highly valuable to know spending levels *per recipient*, but the consensus of historians is that such data cannot be found in the available returns (Baugh (1975)). Lindert (1998), for example, who specifically tries to make a comparison of poor relief benefits per recipient across Europe, only reports this information at the national level, with three data points covering the whole first half of the nineteenth century.

[10]Figure 1 also displays the gradual implementation of the reform. While the drop in relief spending can be seen from the first year after the reform, it takes three years for relief per capita to stabilise at its minimum level for the time period considered. This could reflect both delays in implementation and counties' opposition to the new measures, as suggested by qualitative historical literature (Fraser (1976); Englander (1998)).

are available both on charges and convictions from primary sources and show a constant increase (Hart (1955)). Not only was criminality rising, but it was increasingly seen as a sign of instability against the backdrop of Chartist activities (Hart (1955)) and the reductions in welfare spending resulting from the New Poor Law (Jones (1983); Ager (2014)).[11]

Partly in response to these trends, many innovative policing reforms took place to establish a nation-wide statutory police force.[12] Beginning with the Metropolitan Police Act of 1829, which established the first professional police force in London, police forces were given a formalised structure in boroughs and counties with the Municipal Corporation Act of 1835 and the County Police Act of 1839, respectively. The newly formed police units replaced an inadequate and ineffective locally administered system, where constables usually worked part-time, unpaid and for limited periods, employing neither preventive nor detective policing methods (Emsley (2013)). While these acts provided guidance, they were not promptly or widely implemented.[13] A full roll-out was only reached thanks to the County and Borough Police Act of 1856. But even then, the number of policemen per capita was very heterogeneous among counties and generally very low (Hart (1955)). It was only after the 1870s that the number of constables (outside London) rose to almost modern levels (Jones (1983)). This is important, as Bindler and Hjalmarsson (2019), looking specifically at nineteenth-century England, find that police forces significantly reduced crime only when their size was close to the nationally recommended threshold. Given the results of Bindler and Hjalmarsson (2019), we do not expect the smaller police forces operating before their formal institutionalisation to have significant effects on our results. Nevertheless, we do address this concern in Section 5 where we control for the introduction of county police forces.

While policing was evolving, punishment also underwent a profound transformation during the nineteenth century. Possible punishments included capital sentences, non-custodial sentences (such as mutilation of the body, flogging, whipping, the pillory or the stocks) and custodial sentences (such as imprisonment or transportation to a penal colony). Whereas an exhaustive list of all the changes is beyond the scope of this paper, acknowledging the evolution of such legislation is important for the careful interpretation of our heterogeneity analysis of individual-level criminal activities and sentencing. In Section 5, we show that changes in sentencing behaviour within broad categories of crime do not change systematically with the new system of poor relief provision introduced in 1834.

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[11] Ager (2014) examines the different unlawful or socially unacceptable practices which arose as a consequence of the changes to the system of poor relief and which contributed to the perceived sense of instability. While in rural districts petty theft and poaching became increasingly common, especially given progressing land enclosure throughout the country, prostitution proliferated in urban centres with developing commercial ports (Walkowitz (1999)).

[12] Other historical factors need to be taken in consideration to understand the birth and development of the policing machinery in England and Wales during this period. Jones (1983) lists, for example, the country-wide decreasing opposition to preventive policing and Westminster's intervention on the one hand, and the pressure for peace-keeping from the central government on the other.

[13] For instance, only 93 of the 171 boroughs without police forces had created them during the two years after 1835, and others had only carried out the statutory obligations (Hart (1955)). Further, the 1839 Act gave freedom to Quarter Sessions' justices to decide whether and when to establish their professional constabularies, and by the mid-1850s only two-thirds of counties had done so (Emsley (2013)).

3 Data

We construct a novel dataset from multiple sources, covering a yearly panel of fifty-two English and Welsh counties over the period 1820 to 1842, for a total of 1,196 county-year observations. Using historical documents and archival material, we collect data on poor relief expenditures, criminal activity as well as demographic and economic characteristics.

3.1 Data on Poor Relief

The main source for our "treatment" of interest – the generosity of poor relief – are the so-called *Porter's Tables*. These were statistical tables compiled from official returns by G. R. Porter (the head of the Board of Trade's statistical office) and presented annually to Parliament (Board of Trade, various). For each county, we digitise yearly information about poor relief expenditure over the period 1820 to 1842. Summary statistics for variables related to poor relief are presented in Panel A of Table 1. In the average county-year, total poor relief expenditure amounted to approximately £111,850. There was considerable variability in total expenditure: ranging from £6,180 to £688,160 in the most extreme county-year observations.

Part of the variability in total expenditure can, of course, be explained by differences in population: the populous county of Middlesex mechanically spent more than the sparsely populated Welsh county of Radnorshire, for example. Even when expressing expenditure in per capita terms, however, significant variation remains. While the average county-year saw expenditures of £0.47 per capita, this figure ranges from £0.14 to £1.23. We discussed the historical determinants of such differences in Section 2 above. Importantly for our empirical strategy – which we describe in detail in Section 4 below – expenditure levels changed heterogeneously with the implementation of the New Poor Law in 1834, as shown in Figure 1. The spatial patterns of poor relief reductions are evident from Figure 2a: spending fell most sharply in the agrarian counties near London. To account for this spatial pattern, we control for occupational structure and proximity to London throughout our analysis.

3.2 Data on Crime

We assemble data on our outcomes of interest – measures of criminal activity – from two sources. First, we again use the *Porter's Tables* to obtain information on annual county-level criminal charges for the period 1820 to 1842 (Board of Trade, various). We normalise the absolute number of charges by population; our main measure of criminal activity is the number of charges per 1,000 inhabitants. Panel B of Table 1 reports summary statistics for our county-level crime variables. The average county-year saw 1.14 charges per 1,000 inhabitants – again displaying considerable variation.

We expect a heterogeneous impact of poor relief reductions across various types of crime. In par-

ticular, we expect the prevalence of low-level, non-violent property crimes, such as larceny and cattle theft, to respond most strongly. To this end, we collect data on criminal charges broken down into broad types of crime. We digitise archival records of the universe of individual-level criminal charges recorded in England and Wales between 1828 and 1840 (Home Office, various). With over a quarter of a million observations obtained from individual records, we reconstruct yearly county-level totals of different types of crime. These are otherwise not available in official returns. In the remaining rows of Panel B of Table 1, we provide summary statistics for charges per 1,000 inhabitants, broken down into six broad categories.[14] The type of crime we expect to respond most strongly is also the most prevalent: non-violent crimes against property. Summary statistics based on the 276,962 individual-level records are reported in Panel C. The proportions of various types of crime for which individual defendants were charged naturally reflect county-level totals (see Panel B). Additionally, we report information on the outcomes of these trials. Just over a quarter of all defendants were acquitted, around half of were sentenced to imprisonment, one-fifth to transportation to a penal colony. Death sentences and other miscellaneous sentences, such as fines, were less common.

The spatial patterns of the evolution of crime before and after the New Poor Law reform in 1834 are shown in Figures 2b and 2c, respectively, for all crimes and for non-violent property crimes. The post-reform increase, particularly of non-violent property crimes, follows a spatial pattern very similar to that of the post-reform decrease in poor relief spending in Figure 2a. In Figure 3, we show correlations between the reduction in per capita poor relief expenditures and the increase in charges per 1,000 inhabitants. Counties where the drop in poor relief expenditure was greatest following the New Poor Law reform in 1834 subsequently saw the largest increases in criminal activity. We estimate this relationship more rigorously below, using a difference-in-differences instrumental variables strategy which we describe in more detail in Section 4.

3.3   Other Data

In addition to the data we amass on poor relief and crime, we construct a number of demographic and economic control variables. The proximate source of our data are the *Porter's Tables* (Board of Trade, various), which in turn collate this information from the population censuses of 1811, 1821 and 1831. We have information on population, the number of families in broad occupational categories as well as measures of the housing stock. Summary statistics for these variables are reported in Panel D of Table 1.

From the same source we also collect county-level data on savings banks. In particular, for the last pre-reform year (1833), we calculate the total value of deposits held in savings banks, as well as a Gini

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[14]These are: (i) crimes against the person (such as assaults, manslaughter and murder), (ii) violent crimes against property (such as housebreaking, burglary and robbery), (iii) non-violent crimes against property (such as larceny, sheep stealing and receiving stolen goods), (iv) malicious crimes against property (such as arson, cattle maiming and setting fire to crops), (v) crimes against the currency (such as forgery of bank notes, counterfeiting of gold and silver coin), (vi) other crimes (such as rioting, keeping "disorderly" houses and other misdemeanours).

coefficient of deposits. These measures serve as proxies for wealth and inequality. We also gather data on the number of electors registered as qualified to vote for Members of Parliament in the year 1834-35. We use these data when discussing the robustness of our results to potential confounders.

Additionally, we use the cereal suitability index of Caprettini and Voth (2020) as an additional proxy for counties' differential involvement in agriculture. This index is provided at the parish level, so we create counties' weighted average suitability using parish areas as weights. Lastly, we use data on the date of establishment and the initial size of the first county police forces from Stallion and Wall (1999). These data are useful to address potential concerns that our results are driven by differential professionalisation of county police forces – this does not appear to be the case, as we show in Section 5 below. Summary statistics for these data and other variables we use at different points in our analysis are reported in Panel E of Table 1.

4 Empirical Strategy

We are interested in the impact of the generosity of poor relief on criminal activity. To identify this effect, we begin by estimating equations of the following form:

$$\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \, \text{PoorRelief}_{it} + \text{X}'_{it} \, \beta + \epsilon_{it} \tag{1}$$

The outcome of interest, Crime_{it}, is a per capita measure of criminal activity in county i in year t. In our baseline specification, Crime_{it} is the number of criminal charges per 1,000 inhabitants across all types of crime. We then proceed to decompose this effect into broad crime types. PoorRelief_{it} is the per-capita poor relief expenditure of the county. The vectors α_i and γ_t are county and year fixed effects, and X_{it} is a vector of controls which we describe as we introduce it in the analysis below.

Estimating equation 1 by OLS is useful to obtain conditional correlations between poor relief spending and criminal activity, which are themselves informative. A causal interpretation of estimates of ϕ from this equation alone, however, is not possible. In particular, a county's poor relief spending is determined simultaneously with other local budgeting decisions – such as education and health spending – which may in turn reduce criminal behaviour. This simultaneity may introduce a spurious positive relationship between poor relief spending and criminal activity. We therefore exploit the asymmetric reductions in poor relief spending in the wake of the New Poor Law to generate exogenous variation in PoorRelief_{it} in a difference-in-differences first stage given by:

$$\text{PoorRelief}_{it} = \alpha_i + \gamma_t + \delta \, \text{SpendingPre1834}_i \times \text{Post}_t + \text{X}'_{it} \, \beta + u_{it} \tag{2}$$

PoorRelief_{it} is the, presumably endogenous, per-capita poor relief spending of the county. Fixed

effects α_i and γ_t, and controls X_{it} are as in equation 1. SpendingPre1834$_i$ × Post$_t$ is our excluded instrument, which is an interaction of a cross-sectional measure of average poor relief spending before the New Poor Law in 1834 with an indicator for the timing of the reform.[15] We make use of the fact that counties with high pre-reform poor relief spending saw disproportionately large decreases in spending following the New Poor Law in 1834, leading us to expect a large and significant negative first-stage estimate of δ.

When using pre-reform spending in first-stage equation 2, it becomes crucial to account for the drivers of between-county differences in poor relief spending before the reform. If these characteristics also predict crime in a manner that interacts with the timing of the reform, then the exclusion restriction of the instrument will be violated. In our reading of the literature on the Old Poor Law, we identify two key determinants of pre-reform spending. First, spending tended to be higher in more agrarian counties in order to support seasonally unemployed agricultural workers. Second, spending was higher near London in order to discourage large migration flows to the metropolis. Controlling for occupational structure and proximity to London will therefore be crucial to ensure the validity of our instrument.[16]

We run a number of balance checks, which we report in Figure 4. In particular, we regress pre-reform county characteristics on our instrument in a series of bivariate regressions.[17] As the historical literature on the Poor Laws leads us to expect, we find that our instrument is correlated positively with the number of families in agriculture and negatively with distance to London. Reassuringly, across a range of other covariates, we find no other significant correlation with our instrument. This gives us confidence that, conditional on occupational structure and proximity to London, our instrument is as good as randomly assigned.

5   Main Results: Poor Relief and Crime

5.1   First-Stage and Reduced-Form Results

We begin by reporting results from first-stage and reduced-form regressions of the form specified in equation 2. With these difference-in-differences regressions, we estimate the differential impact of the 1834 New Poor Law reform on poor relief spending and criminal activity across counties with different levels of pre-reform poor relief spending.

First-stage results are reported in columns 1 and 2 of Table 2. Focussing on the parsimonious specification without controls in column 1, the reform had a marked differential impact on post-reform

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[15]Concretely, SpendingPre1834$_i$ is the average spending of county i over the years 1820-1833. Post$_t$ equals one from 1834 onwards.

[16]Indeed, a LASSO selection model to determine the most important correlates of pre-reform spending selects distance to London and the number of families in agriculture as the top two correlates. Other, less important, candidate correlates include population size, other occupation variables and measures of urbanisation.

[17]All variables have been standardised to have a mean of zero and standard deviation of one for ease of comparability between coefficients.

poor relief spending. Counties which had higher per capita poor relief spending before the reform see significantly sharper reductions to spending after 1834. Concretely, for every additional pound spent per capita before the reform, spending falls by an additional 0.5 pounds after the reform, which speaks to the post-reform compression of the gap between high- and low-spending counties that we documented descriptively in Section 2. This effect remains unchanged when controlling for measures of occupational structure, demographics, urbanisation and distance to London in column 2. Recall from Sections 2 and 4 that proximity to London and an agrarian economy were the main predictors of pre-reform levels of spending. Accounting for these characteristics is therefore important to ensure the validity of our instrument. Reassuringly, the estimated coefficient on the difference-in-differences interaction changes only marginally from the more parsimonious specification in column 1.

Turning to reduced-form results in columns 3 to 6 of Table 2, we report results from estimating equation 2 with a measure of criminal activity – our ultimate outcome of interest – on the left hand side. The reported coefficients should be interpreted as the differential impact of the New Poor Law reform on criminal activity in counties which were relatively harder-hit by the reform (as measured by pre-reform levels of spending). Taking all crimes together in columns 3 and 4, the positive and statistically significant coefficient indicates that harder-hit counties see sharper increases in criminal activity after the reform. We expect low-level property crimes to be particularly affected, and confirm this by using only non-violent property crimes on the left hand side in columns 5 and 6.[18]

A simple comparison of pre- and post-reform outcomes can disguise significant dynamics of the reform. More importantly, it does not allow for an evaluation of pre-reform trends. For identification in our difference-in-differences instrumental variables strategy, we require the assumption that post-reform trends would have been parallel in the absence of the reform. To assess whether this assumption appears to be satisfied, we report generalised difference-in-differences estimates of our first stage and reduced form in Figure 5. This amounts to estimating an event-study specification where the cross-sectional measure $SpendingPre1834_i$ is interacted with year fixed effects rather than a simple post-reform indicator. Throughout, we take 1833 – the last pre-reform year – as the baseline year.

Figures 5a and 5b show these estimates for the first stage. The sharpness of the reform is evident. High- and low-spending counties evolve on similar trends prior to 1834: the effect of $SpendingPre1834_i$ is not systematically different in 1820 to 1832 compared to the baseline year 1833. Immediately following the reform, however, there is a marked drop for previously high-spending counties. The effect magnifies over time as the reform is rolled out and implemented before stabilising in the late 1830s.

In Figures 5c to 5f we repeat the same exercise for the reduced form, taking criminal activity as outcomes. A consistent pattern emerges: previously high-spending counties do not evolve differentially prior to the reform, but see an immediate increase in criminal activity after 1834. These estimates are

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[18]Note that we have breakdowns by the type of crime only for a sub-sample of years, 1828 to 1840, which is reflected in the reduced number of observations in these regressions. We still keep the full set of 52 counties.

naturally more noisy than those for the first stage, but nevertheless clearly demonstrate the differential level shift in criminal activity that takes place in the wake of the reform.

5.2   OLS and IV Results

We now return to our main relationship of interest: the impact of poor relief spending on criminal activity. Beginning with OLS estimations of equation 1, we then proceed to report instrumental variables regressions in which we estimate equations 1 and 2 using two-stage least squares. With this exercise, we obtain causal estimates of the impact of poor relief reductions on criminal activity following the New Poor Law reform.

Main results are reported in Table 3. In columns 1 and 2, we report OLS results which confirm our hypothesis: decreases in per capita poor relief spending are significantly associated with increases in criminal activity. The relationship holds both with and without the inclusion of controls for occupational structure, urbanisation and proximity to London. Our findings in the OLS specification suggest that a one standard deviation reduction in poor relief spending is associated with a 0.12–0.18 standard deviation increase in criminal activity.[19]

As discussed in Section 4, however, the endogeneity of poor relief spending makes it difficult to give these estimates a causal interpretation. In particular, since spending decisions for a range of budgeting items are determined locally subject to the same budget constraint, a spurious positive relationship is introduced between poor relief and criminal activity. We therefore suspect that the estimates reported in columns 1 and 2 of Table 3 are upward-biased (towards zero), leading us to underestimate the true effect of poor relief. To circumvent this issue, we now report instrumental variables regressions using the New Poor Law reform as a shock to local spending in the first stage described above.

We present the results of this exercise, with and without controls, in columns 3 and 4 of Table 3. As shown in Table 2 and in Figure 5, the sharpness of the reform yields a strong first stage, which is reflected in the high F-statistic on the excluded instrument in Table 3. Results are qualitatively in line with those from the OLS: reductions in poor relief spending predict statistically significant and economically meaningful increases in criminal activity. Using this approach, the estimated effect sizes are twice as large as before, which is not surprising given the nature of the simultaneity problem that our instrument solves. Since identification now comes from an exogenous shock to poor relief spending, we can interpret these estimates causally. Once again turning to standardised coefficients, a one standard deviation decrease in poor relief spending causes a 0.20–0.34 standard deviation increase in criminal activity.[20]

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[19]The standard deviation of per capita poor relief spending is 0.205, and that of criminal charges per 1,000 inhabitants is 0.662. This translates into standardised coefficients of $\frac{-0.389 \times 0.205}{0.662} \approx -0.12$ and $\frac{-0.606 \times 0.205}{0.662} \approx -0.18$ for the estimates in columns 1 and 2 of Table 3, respectively.

[20]Using a similar calculation, $\frac{-0.634 \times 0.205}{0.662} \approx -0.20$ and $\frac{-1.112 \times 0.205}{0.662} \approx -0.34$ for the estimates in columns 3 and 4 of Table 3, respectively.

5.3   Breakdown by Crime Type

What explains the relationship between reductions in poor relief spending and subsequent increases in criminal activity? Our proposed mechanism is that the less generous system of relief introduced by the reform lowers the opportunity cost of crime, particularly for crimes committed to ensure individuals' basic needs. As a result, a greater number of individuals are willing to take the risks associated with committing crimes to substitute for what had previously been afforded through poor relief. This interpretation is supported by qualitative work on the "economy of makeshifts" by Ager (2014).

To investigate this hypothesis formally, we run additional OLS and IV regressions of the form introduced above. Instead of considering all criminal charges together, we divide crimes into five broad types. We consider separately crimes committed against the person, against property (sub-divided into violent, non-violent and malicious property offences), and against the currency. See Section 3 for a more detailed description of the five crime categories.[21] If our proposed mechanism is correct, we expect to detect an impact of poor relief spending only on non-violent property crimes (which include, for example, larceny and cattle theft). Crimes in other categories should not be systematically affected, since we do not expect the margin of selection into these more severe crimes to be near the margin of selection into petty property crimes. We report the results of this exercise in Table 4. In columns 1 to 5, we present estimates separately for each of the five broad crime categories. A clear pattern emerges: the negative relationship between poor relief and crime is explained predominantly by non-violent property crimes (column 3). The effect of other crimes are statistically or economically insignificant in comparison.[22,23]

Note that malicious property offences appear to be positively related with poor relief spending (column 4). This is an artefact of the Swing Riots, which took place in southern and eastern England in 1830-31. During this period of unrest, agricultural workers demolished agricultural machinery in protest of increasing mechanisation, and offenders were charged with malicious property offences.[24] The affected counties were agrarian, and for this reason had high levels of poor relief spending, as described in Section 2. This introduces a spurious positive relationship between poor relief spending and malicious property crimes. In Appendix Table A2 we exclude the years 1830-31 from the analysis to abstract away from the impact of the Swing Riots. This markedly attenuates the effect on malicious property crimes (the effect vanishes completely in the IV specification) while the strong effects for non-violent property

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[21]Note that we exclude the residual "other crime" category from this analysis. This group of crimes is very heterogeneous (and results therefore difficult to interpret) and constitutes only two percent of all crimes.

[22]Despite the effect on the category of crimes against the person in Table 4 column 1 being small, we investigate which particular crime is driving these results exploiting the granularity of our individual-level data. We document a spike in assaults on peace officers (a pre-professionalisation, rudimentary form of policing) starting in 1834. We hypothesise that such crimes are likely committed in relation to being apprehended for a different crime. The 1834 increase of crimes against the person suggests a possible connection to the Poor Law reform, and the results in column 3 of Appendix Table A1 confirm this hypothesis. Moreover, excluding these assaults from overall offences against the person flips the sign of its coefficient, as shown in column 2 of Table A1. This suggests that assaults on peace officers are the crimes driving the significant negative relationship between poor relief and crime reported in Table 4 column 1.

[23]The significant coefficient on crimes against the currency in column 5 of Table 4 is very small in size and, further, this felony represents only around two percent of total crimes in this period (see Table 1).

[24]A recent literature in economics has given the Swing Riots considerable attention, exploring their causes (Caprettini and Voth, 2020), spread (Aidt, Leon and Satchell, 2017) and political consequences (Aidt and Franck, 2015).

crimes remain. In a separate exercise, we show in Appendix Table A3 that we can reach the same conclusion by excluding from our sample all crimes involving the destruction of threshing machinery.[25]

The literature on the living standards of women during the industrial revolution has documented that a majority of female convicts were employed in domestic service (see, for example, Nicholas and Oxley (1993)). Domestic servants would not, as a rule, be recipients of poor relief. Therefore, we now provide evidence that the relationship between poor relief spending and non-violent property crime is not driven by this subcategory of perpetrators. While our individual-level crime data does not list the occupation of each defendant, we can exploit the fact that larcenies (the most common crime) are recorded distinctly for servants and non-servants. First, in column 1 of Appendix Table A4, we show that a strong negative relationship exists between poor relief spending and the total number of larcenies per 1,000 inhabitants. Then, we break down the total number of larcenies into those committed by servants and non-servants (columns 2 and 3, respectively). Reassuringly, the effect on larcenies (which constitute the majority of non-violent property crimes) is driven entirely by crimes committed by non-servants. The absence of an effect for non-recipients of poor relief is precisely what our proposed interpretation would predict: the margin at which these individuals select into crime is unaltered by the New Poor Law.

5.4 Mechanisms

5.4.1 Poverty and the Seasonality of Crime

Our analysis so far brought to light two important results: the opportunity cost of criminality appears to be lower following the poor law reform in 1834, and even more so for petty property crimes, suggesting that crime is a substitute for the sudden lack of poor relief for the economically vulnerable. But how can we be sure that it is indeed the poorest who react this way? Unfortunately, the criminal records do not provide information on individuals' income or occupation. However, we can test this hypothesis in an alternative way, by exploiting the cyclical nature of poor relief claims and crime rates.

First, poor relief spending tended to be higher in more agrarian counties, in order to support out-of-work seasonal agricultural labourers, making relief claims countercyclical with respect to unemployment. Indeed, it was mainly precarious agricultural labourers who relied on poor relief during the winter months (Boyer (2002); Clark and Page (2019)) when wages were at their annual lowest (Clark (2001)). Second, the seasonal pattern of crime in nineteenth-century England and its link to poverty is well-established in the historical literature (Osborne (2000); Ager (2014)). Thus, we can test whether petty property crime responded to the New Poor Law disproportionally during the off-season winter months, when the poor faced the compounding effects of unemployment and cuts to welfare spending.

We test this hypothesis by estimating equation 1 separately for crimes in summer (higher employment)

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[25]Other less frequent crimes were associated with the Swing Riots, such as burning ricks and tithe barns. Dropping these crimes as well confirms the results in Table A3.

and non-summer (lower employment) months, focusing on non-violent property crimes.[26] We use three different definitions of "agricultural summer". The first is based on the astronomical definition of summer (July to September). For the second definition we follow what historians have identified as agricultural summer in nineteenth-century England. Specifically, we combine the definitions of Collins (1976), Goose (2006) and Clark (2007), to arrive at a definition of summer as May to September. Last, Clark (2001) hints at a third way to think about agricultural summer, and suggests that the peak of employment was reached only during harvest, which we define as September. We report results for all three pairs of definitions of summer and non-summer in Table 5. These strongly support our hypothesis. Across all pairs of definitions, the impact of the New Poor Law-induced cuts to poor relief is much stronger in the non-summer months, when agricultural workers were most likely unemployed and therefore poorer than usual. Throughout, effect sizes in non-summer months are at least twice as large as in summer.[27,28]

We additionally perform the same analysis for other types of crime, focusing on the first historical definition of agricultural summer. In our discussion of the effect on different categories of crime, we saw that non-violent property crimes are the most responsive to the spending cut induced by the New Poor Law. The impact of the New Poor Law can be seen on other crimes as well, although the effects are much smaller. We notice a very similar pattern in Table A6, where also violent crimes against property and against the person display bigger effects during non-summer months. However, their magnitudes remain very small. For malicious crimes against property and currency crimes, the coefficients on poor relief for the summer and non-summer months are not statistically different from each other. Since we do not expect these two categories of crime to exhibit seasonality, this null result provides a reassuring check that the differences we detect for non-violent property crimes are indeed driven by economic forces and not by particularities in the timing of sentencing, for example.

5.4.2 Alternative Mechanism: The Introduction of County Police Forces

The world's first professional police force – the London Metropolitan Police – was introduced in 1829, and a roll-out of county police forces followed across England and Wales over the period 1839–1857. Bindler and Hjalmarsson (2019) document the deterrent effect on crime of the introduction of these forces. Can the professionalisation of police explain the results we document? A direct confounding effect is unlikely:

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[26]In this analysis we use the temporal information on criminal activity that we can systematically aggregate. Using our individual-level sentencing data, we focus on individual cases for which the exact month of trial is known. We exclude cases for which we can not assign a precise month.

[27]We report the p-values of a test for the difference between the summer and non-summer coefficients in the final row of Table 5. For the astronomical and second historical definitions of summer, these differences are highly statistically significant. For the first historical definition of summer, the difference is just shy of conventional levels of significance.

[28]The information we rely on to address the seasonality of criminal activity come from the Criminal Records, and thus refer to the month in which people are brought to trial. To take into account a possible delay in prosecution, as a robustness check we impute to each crime trialled a delay in sentencing of one month (in the period covered by our data monthly trial sessions were typical). We assess whether one month is a realistic approximation of delays in prosecution by digitising ca. 1,250 entries from the Proceedings of the Old Bailey (the Central Criminal Court of London and the County of Middlesex). These records report detailed and systematic information on both dates of trial *and* of the crime itself. For simple larcenies sentenced between January 1828 and December 1840, the average trial delay amounts to 32 days. Results are reported in Table A5, and are very similar to those in Table 5.

neither the shock of the introduction of the Metropolitan Police nor the later roll-out of county police forces overlap temporally with the New Poor Law reform.

County police forces were only permitted following the County Police Act of 1839. It is therefore plausible that latent county characteristics caused some counties to introduce county forces relatively early (in 1839, rather than 1857, for example). Additionally, such counties may have mobilised comparatively large police forces, as measured by the number of officers per inhabitant. Latent characteristics driving these margins could plausibly interact with the New Poor Law reform in a manner that would confound the effects we document and our proposed mechanism. Counties that were early to professionalise their police forces may have also had particularly vigilant informal policing before the County Police Act, for example.

To investigate this alternative channel, we use data on the year of introduction and initial size of county police forces (Stallion and Wall, 1999). We interact these flexibly with year fixed effects and include them as controls in our main specification, and report the results in Appendix Table A7. The baseline effects of per capita poor relief on all crimes and non-violent property crimes are reported in columns 1 and 4, respectively. We then additionally control for the year of introduction of county police in columns 2 and 5, and for the size of the initial force (per capita) in columns 3 and 6. Coefficients change marginally, but are not statistically distinguishable from our baselines estimates. This gives us confidence that the relationship we find between the generosity of poor relief and criminal activity is not driven by latent characteristics related to the eventual introduction of county police forces.

5.4.3 Alternative Mechanism: Changes in Sentencing and Real Crime

As discussed in Section 2, real crime rates for England and Wales during the first half of the nineteenth century are not available. To measure criminal activity in this paper we use the number of people charged with criminal offences, which could potentially capture not only a change in real crime but also in sentencing. Therefore, the changes in crime rates we document could be due, at least partially, to changes in the judiciary system related to the introduction of the New Poor Law.

To explore this channel we exploit the individual-level crime data, which include each trial's final sentence. The possible outcomes are acquittal, imprisonment, transportation to a penal colony, death and a residual category for other sentences (for example, fines, whipping, and so on). We use the same empirical strategy outlined in Section 4 and study the relationship between the generosity of welfare provision and the outcomes of individual trials. Importantly, we add to our regression specification a vector of broad crime-type fixed effects which allow us to exploit within-crime variation in sentences. Reassuringly, Table A8 shows that punishment is not systematically affected by the reductions in relief spending.

Another way to confirm that the effects we find in our main analysis are driven by real crime instead

of potential crime is to drop from our sample all charges that resulted in acquittals. We present these results in Table A9. Although the coefficients on non-violent crimes against property are reduced in size, they remain quantitatively sizeable and statistically significant, indicating a strong negative relationship between spending on the poor and real crime rates.[29]

5.5 Robustness

We now turn to a range of checks to probe the robustness of our results. These are reported in the Appendix.

Constant population. In our main analysis we do not fully account for migration. It is possible that large reductions in poor relief spending encouraged outmigration from certain counties, which would increase crime per 1,000 inhabitants even with a constant absolute level of crime. While such an argument is theoretically plausible, in practice such migration patterns were unlikely given that poor relief administration under the New Poor Law followed the principle of removal under the settlement laws. This meant that poor individuals would be removed from the parish in which they claimed relief, unless that was their birth-parish, thus limiting the mobility of the poor (see Section 2 for details). However, to address this concern, in Table A10 we adjust our crime variables and poor relief measure using fixed population figures from the 1821 census – the baseline census for our sample period – and the coefficients do not qualitatively change.

Soil suitability. The set of controls we use in our regressions includes the number of families involved in agriculture. However, while agriculture was a widespread activity throughout the whole country, southern counties were endowed with more arable land suitable for cereals, the main crops at the time. In Table A11 we show that our results are robust to the inclusion of additional proxies for agricultural activity. We use the cereal suitability index constructed by Caprettini and Voth (2020) which incorporates suitability information for barley, oat, rye and wheat. We add to our regressions the interaction of year fixed effects with the continuous index and with a dummy variable for whether a county's index is above the median. Our results are robust to these additional controls.

Wealth and inequality. To explore whether our results can be explained by between-county differences in wealth and inequality, in Table A12 we include two additional controls to our benchmark analysis of total and non-violent property crime. Columns 1 and 4 report baseline estimates. First, in columns 2 and 5 we control for each county's wealth in 1833 (the last pre-reform year), proxied by the value of deposits held in saving banks. This allows us to take into account localised – rather than nationwide – shocks related to levels of economic activity that are not absorbed by year fixed effects.[30] Second, in columns 3 and 6 we include as an additional control a Gini coefficient calculated on the deposits held

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[29]In Figure A1 we show reduced-form coefficient plots from these regressions to validate our identification strategy.
[30]We use data on savings bank deposits, as these are available at the county level. More direct measures of local GDP are not available for this period.

in saving banks. This rough measure of inequality is computed using the shares of each county's total deposits under £20, £50, £100, £150, £200 and above £200 in 1833. When adding these two controls, both results remain unchanged.

The Great Reform Act 1832. As discussed in Section 4, for our empirical strategy to hold it is crucial to account for the drivers of between-county differences in poor relief spending before the reform. One possible determinant of pre-reform spending could be the shift in political power following the Great Reform Act of 1832. The Act granted enfranchisement to small landowners, tenant farmers, shopkeepers, householders who paid a yearly rental of £10 or more, as well as some lodgers. This decreased the relative power of big labour-hiring farmers and landlords (the most adamant supporters of generous relief spending aimed at retaining labourers during the off-season), led to a decrease in relief spending, and likely played a role in the push for the 1834 Poor Law reform (Boyer (1990); Lindert (1998)). To mitigate this possible concern, in Table A13 we show that our results are robust to adding the number of electors registered as qualified to vote in the year 1834-35 interacted with year fixed effects (both in absolute numbers and normalised by the county's population).

The industrialising North of England. Counties in the North of England historically had low levels of per capita poor relief expenditure, as shown by Boberg-Fazlić and Sharp (2018) and verified in our descriptive evidence above. These same counties experienced accelerating industrialisation during the nineteenth century and could plausibly better absorb surplus labour in their growing cities. To the extent that such opportunities reduced crime, and to the extent that our reform-induced shock to welfare spending depends on a North-South comparison, our results may be partly explained by such differences. We perform two checks to show that this does not appear to be the case. First, we include an interaction of a "North of England" indicator with year fixed effects as controls in our regressions.[31] Second, we report results excluding the North of England completely. Both exercises are reported in Table A14; results are robust both to controlling flexibly for Northern counties and to excluding them completely and focussing on variation from Southern England and Wales.

Time-varying impact of pre-reform characteristics. Throughout, we have controlled for time-varying factors, such as the number of families in various sectors of the economy, population size, and so on. As a further check, we additionally take the pre-reform values of these variables and interact them with a post-reform indicator.[32] Including these as controls in our regressions allows us to capture potential differential crime trends of counties with different pre-reform characteristics. Reassuringly, the results of this exercise, reported in Table A15, indicate that accounting for such differences change coefficient sizes only marginally, while their economic and statistical significance remains unchanged.

Spatial standard errors. We correct standard errors for spatial dependence following Conley

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[31]Concretely, we follow Boberg-Fazlić and Sharp (2018) and define the North as the following counties: Cheshire, Cumberland, Derbyshire, Durham, Lancashire, Lincolnshire, Northumberland, Nottinghamshire, Shropshire, Staffordshire, Westmorland and Yorkshire.

[32]In particular, we take the values from 1831, the year of the last pre-reform census.

(1999). We allow for arbitrary spatial correlation of errors up to distances of 100, 200 and 300 kilometres. Results are robust to this adjustment, and are reported in Table A16.

Outliers. Last, we perform a jackknife exercise aimed at checking whether our results are driven by particular outliers. We show that removing counties in turn from our sample does not produce coefficients significantly different from our baseline specification for total crime (Figure A2) or for non-violent property crimes (Figure A3).

6   Concluding Remarks

Austerity measures continue to be controversial, particularly when accompanied by shrinkages of the welfare state. In this paper, we combine a novel dataset with a natural experiment from history to document a potential unintended consequence of reductions in welfare payments: an increase in criminal activity. Using the heterogeneous drop in per capita poor relief expenditure across English and Welsh counties following the New Poor Law reform in 1834 in a difference-in-difference instrumental variables strategy, we find a robust negative effect of the generosity of poor relief on crime.

By disaggregating total crime into different categories, we show that results are driven by an increase in non-violent property crime in the counties hit hardest by the reform. The effect is stronger during the winter months, a particularly austere period for precarious seasonal labourers in agriculture. These facts together support our proposed mechanism behind the aggregate effect on crime: following a reduction in the generosity in poor relief, the opportunity cost of crime is lowered, causing economically vulnerable individuals to self-select into criminal activity. We rule out competing mechanisms operating through policing and sentencing to further strengthen our preferred interpretation. While a full welfare analysis is beyond the scope of this paper, our findings underscore a key trade-off that must be faced by policy-makers: fiscal savings from austerity measures must be weighed against their direct and indirect social costs.

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Tables

Table 1: Summary statistics

	Obs.	Mean	St. Dev.	Min.	Max.
Panel A. Poor relief					
Total poor relief expenditure (£1000)	1196	111.85	109.44	6.18	688.16
Expenditure per capita (£)	1196	0.47	0.21	0.14	1.23
Pre-reform expenditure per capita (£)	728	0.54	0.21	0.19	1.23
Panel B. Crime – county level					
Charges per 1000 inhab. (all)	1196	1.14	0.66	0.03	3.36
Charges per 1000 inhab. (against person)	673	0.09	0.07	0.00	0.41
Charges per 1000 inhab. (against property, violent)	673	0.10	0.08	0.00	0.59
Charges per 1000 inhab. (against property, non-violent)	673	0.96	0.53	0.00	2.68
Charges per 1000 inhab. (against property, malicious)	673	0.04	0.09	0.00	1.06
Charges per 1000 inhab. (against currency)	673	0.02	0.02	0.00	0.17
Charges per 1000 inhab. (other)	673	0.03	0.04	0.00	0.51
Panel C. Crime – individual level					
Charge: crime against person	276962	0.07	0.25	0	1
Charge: crime against property, violent	276962	0.07	0.26	0	1
Charge: crime against property, non-violent	276962	0.79	0.41	0	1
Charge: crime against property, malicious	276962	0.03	0.16	0	1
Charge: crime against currency	276962	0.02	0.14	0	1
Charge: other crime	276962	0.02	0.14	0	1
Sentence: acquittal	276962	0.28	0.45	0	1
Sentence: death	276962	0.02	0.14	0	1
Sentence: imprisonment	276962	0.49	0.50	0	1
Sentence: transportation	276962	0.19	0.39	0	1
Sentence: other	276962	0.03	0.17	0	1
Panel D. Census control variables					
Total population (thousands)	156	267.64	305.34	18	1667
Total no. families (thousands)	156	53.21	59.66	4	314
Families in agriculture (thousands)	156	16.09	11.33	2	64
Families in trade (thousands)	156	23.12	35.79	1	174
Families in other occupations (thousands)	156	13.97	19.72	0	130
No. inhabited houses (thousands)	156	48.11	51.54	4	315
No. uninhabited houses (thousands)	156	2.32	3.45	0	24
No. other buildings (thousands)	156	0.46	0.66	0	4
Panel E. Other variables					
Distance to London (IHS)	52	5.71	1.04	0.00	6.80
Value of deposits in savings banks, 1833 (IHS)	52	11.99	2.72	0.00	15.23
Gini of deposits in savings banks, 1833	50	0.42	0.05	0.28	0.51
Electors in 1834-35 (log)	52	8.62	0.82	6.55	10.40
Electors per 1000 inhab. in 1834-35	52	34.71	12.95	5.89	84.37
Year of introd. of prof. police force	52	1848.62	8.67	1829.00	1857.00
Size of first prof. police force (officers per 1000 inhab.)	51	0.53	0.37	0.07	2.62

Note: Summary statistics for key variables. See Section 3 for a detailed description of data sources. **Panel A** summarises poor relief expenditure data for 52 English and Welsh counties over the period 1820-1842. Pre-reform period defined as before the Poor Law Amendment Act 1834. **Panel B** summarises county-level crime data. For total crime figures, these are available for the entire period 1820-1842. Breakdowns by crime type rely on archival individual-level data which we collapse on the level of the county. These are available for the period 1828-1840. **Panel C** summarises crimes and sentences using this individual-level data. **Panel D** summarises county-level control variables from population censuses conducted in 1821, 1831 and 1841. **Panel E** summarises remaining variables used in our analysis.

Table 2: First stage and reduced form

| Dependent variable: | First stage | | Reduced form | | | |
| | Poor relief p.c. | | Charges per 1000 (all crimes) | | Charges per 1000 (non-vio. prop. crimes) | |
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-1834 poor relief × Post	-0.500***	-0.497***	0.317**	0.552***	0.311***	0.503***
	(0.025)	(0.022)	(0.120)	(0.133)	(0.078)	(0.102)
Observations	1196	1196	1196	1196	673	673
Counties	52	52	52	52	52	52
Outcome mean	0.472	0.472	1.135	1.135	0.962	0.962
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls		Y		Y		Y

Note: First-stage and reduced-form regressions of the form $Y_{it} = \alpha_i + \gamma_t + \delta \, \text{SpendingPre1834}_i \times \text{Post}_t + X'_{it}\beta + u_{it}$. For first-stage regressions, $Y_{it} = \text{PoorRelief}_{it}$ where PoorRelief_{it} is per-capita poor relief spending. For reduced-form regressions, $Y_{it} = \text{Crime}_{it}$ where Crime_{it} is the number of criminal charges per 1000 inhabitants, either for all crimes (columns 3 and 4) or for non-violent property crimes only (columns 5 and 6). SpendingPre1834_i is the level of average pre-reform poor relief spending, and Post_t is a post-reform indicator. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table 3: Main results: poor relief expenditure and crime

	OLS		IV	
	Dependent variable: Charges per 1000			
	(1)	(2)	(3)	(4)
Poor relief p.c.	-0.389**	-0.606***	-0.634***	-1.112***
	(0.174)	(0.208)	(0.236)	(0.273)
Observations	1196	1196	1196	1196
Counties	52	52	52	52
Outcome mean	1.135	1.135	1.135	1.135
K-P F-Stat			406.5	523.9
County FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Controls		Y		Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \, \text{PoorRelief}_{it} + X'_{it} \, \beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants and PoorRelief_{it} is per-capita poor relief spending. First-stage and reduced-form regressions reported in Table 2. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table 4: Breakdown by crime type

Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	Panel A: OLS				
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.055*	0.045	-0.701***	0.271***	0.004
	(0.031)	(0.038)	(0.226)	(0.069)	(0.011)
	Panel B: IV				
Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.070*	0.001	-1.109***	0.223**	-0.027*
	(0.038)	(0.042)	(0.234)	(0.090)	(0.014)
Observations	673	673	673	673	673
Counties	52	52	52	52	52
Outcome mean	0.0915	0.103	0.962	0.0433	0.0216
K-P F-Stat	543.8	543.8	543.8	543.8	543.8
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \text{PoorRelief}_{it} + X'_{it}\beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants broken down into five broad crime categories; see Section 3 for a detailed description. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table 5: Mechanism: poverty and the seasonality of crime

	Panel A: OLS					
	Property crimes (non-vio.)					
	Astronomical def.		Historical def. 1		Historical def. 2	
	summer (1)	non-summer (2)	summer (3)	non-summer (4)	summer (5)	winter (6)
Poor relief p.c.	-0.263*** (0.081)	-1.013*** (0.355)	-0.460*** (0.140)	-0.815*** (0.288)	-0.138*** (0.045)	-1.137*** (0.393)

	Panel B: IV					
	Property crimes (non-vio.)					
	Astronomical def.		Historical def. 1		Historical def. 2	
	summer (1)	non-summer (2)	summer (3)	non-summer (4)	summer (5)	non-summer (6)
Poor relief p.c.	-0.353*** (0.094)	-1.670*** (0.474)	-0.696*** (0.175)	-1.327*** (0.370)	-0.237*** (0.071)	-1.786*** (0.489)
Observations	502	502	502	502	502	502
Counties	52	52	52	52	52	52
Outcome mean	0.154	0.671	0.236	0.588	0.0128	0.812
K-P F-Stat	264.7	264.7	264.7	264.7	264.7	264.7
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
p-value diff. coef.	0.0065		0.1229		0.0018	

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\,\text{PoorRelief}_{it} + X'_{it}\beta + \epsilon_{it}$. Crime_{it} is the number of non-violent property crimes charges per 1000 inhabitants. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). The sample is composed of the subset of observations for which we have information on the month of trial. Columns 1 and 2 use the astronomical definition of summer (July-September); columns 3 and 4 use a combined definition coming from Collins (1976), Goose (2006) and Clark (2007), keeping May to September as summer; columns 5 and 6 use only September following Clark (2001). P-values at the bottom of the table are from tests for the difference of the coefficients on poor relief p.c. between each summer/non-summer comparison. Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Figures

Figure 1: Poor relief spending before and after the Poor Law Amendment Act, 1834
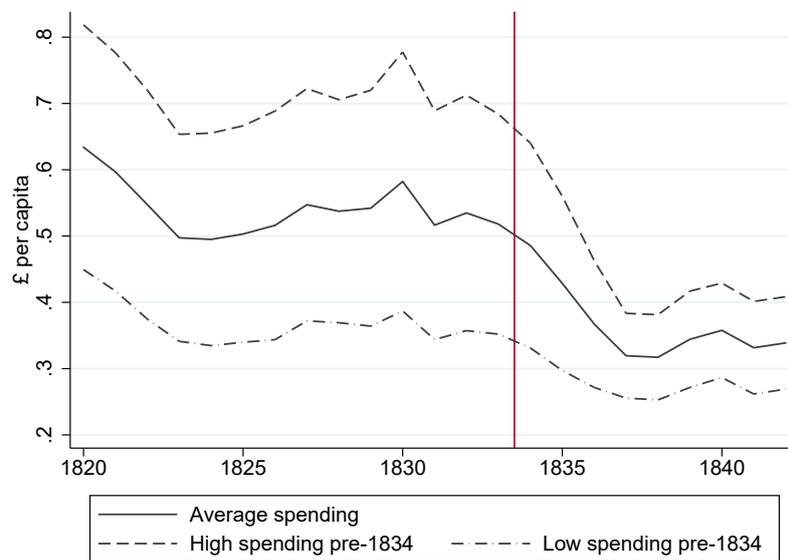
Note: Timeline showing per-capita poor relief spending in English and Welsh counties over the period 1820-1842. Solid line plots the average level of spending each year. Dashed and dot-dashed lines plot average spending by two subsamples of counties: those with above-median (dashed) and below-median (dot-dashed) average pre-reform spending. Vertical red line indicates the year of the Poor Law Amendment Act 1834.

Figure 2: Spatial pattern of poor relief reductions and crime increases

0.008 - 0.035	
0.036 - 0.087	
0.088 - 0.115	
0.116 - 0.131	
0.132 - 0.137	
0.138 - 0.194	
0.195 - 0.219	
0.220 - 0.284	
0.285 - 0.347	
0.348 - 0.481	
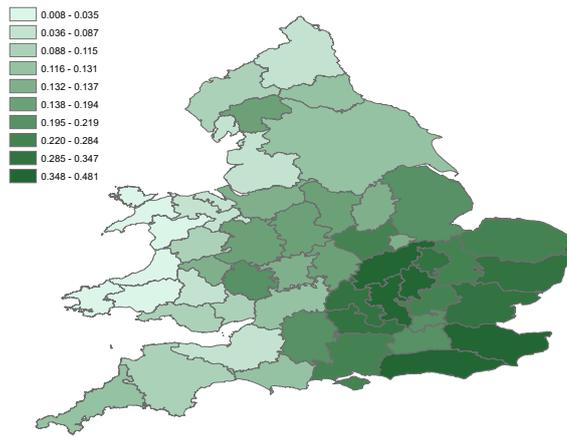
(a) Per capita poor relief reductions

-0.072 - 0.116
0.117 - 0.282
0.283 - 0.330
0.331 - 0.376
0.377 - 0.399
0.400 - 0.459
0.460 - 0.514
0.515 - 0.591
0.592 - 0.732
0.733 - 1.006

(b) Increases in crimes per 1000 inhabitants
(all crimes)

-0.391 - -0.048
-0.047 - 0.019
0.020 - 0.080
0.081 - 0.120
0.121 - 0.135
0.136 - 0.168
0.169 - 0.203
0.204 - 0.261
0.262 - 0.348
0.349 - 0.471
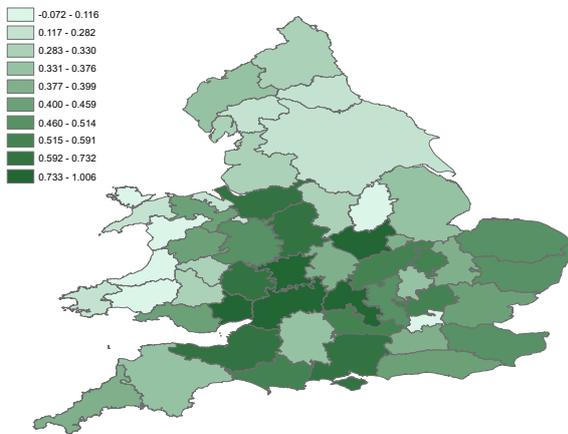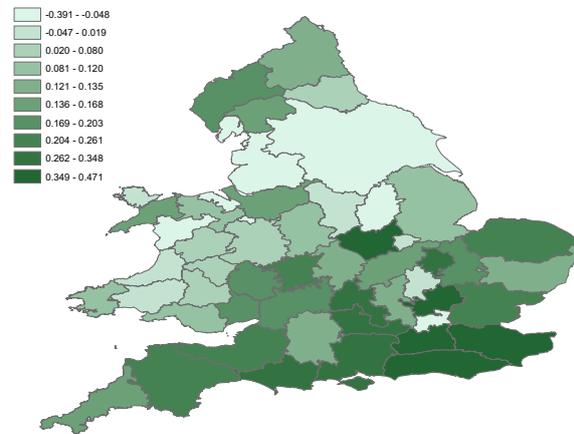
(c) Increases in crimes per 1000 inhabitants
(non-violent property crimes)

Note: Maps showing the spatial relationship between reductions in per capita poor relief spending (defined as average spending pre-1834 less average spending post-1834) and increases in criminal charges per 1000 inhabitants. Figure (b) shows all crimes, Figure (c) non-violent property crimes only. Colours indicate deciles; darker shades represent larger changes.

Figure 3: Correlation between poor relief decreases and subsequent increases in crime
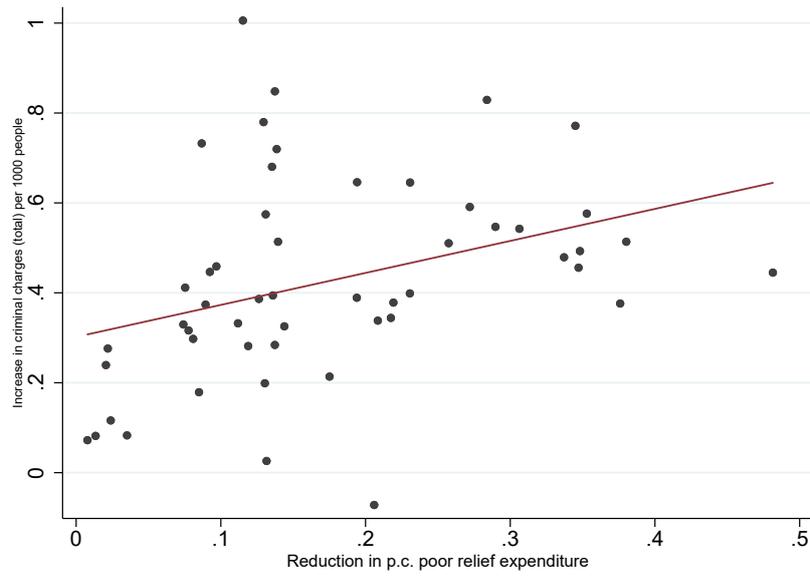
(a) All crimes
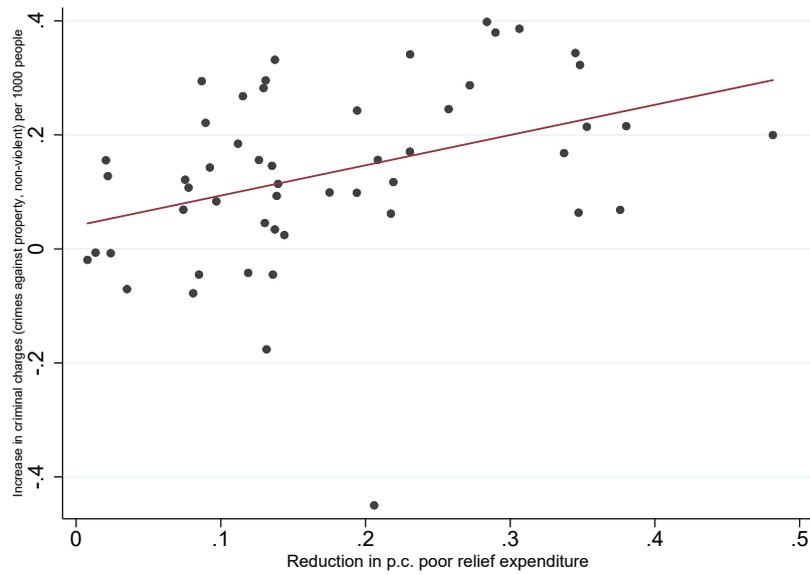
(b) Non-violent property crimes

Note: Correlational scatter plots showing the relationship between reductions in per capita poor relief spending (defined as average spending pre-1834 less average spending post-1834) and increases in criminal charges per 1000 inhabitants. Figure (a) shows all crimes, Figure (b) non-violent property crimes only. Each dot represents one county.

Figure 4: Instrument validity: balance on pre-reform characteristics
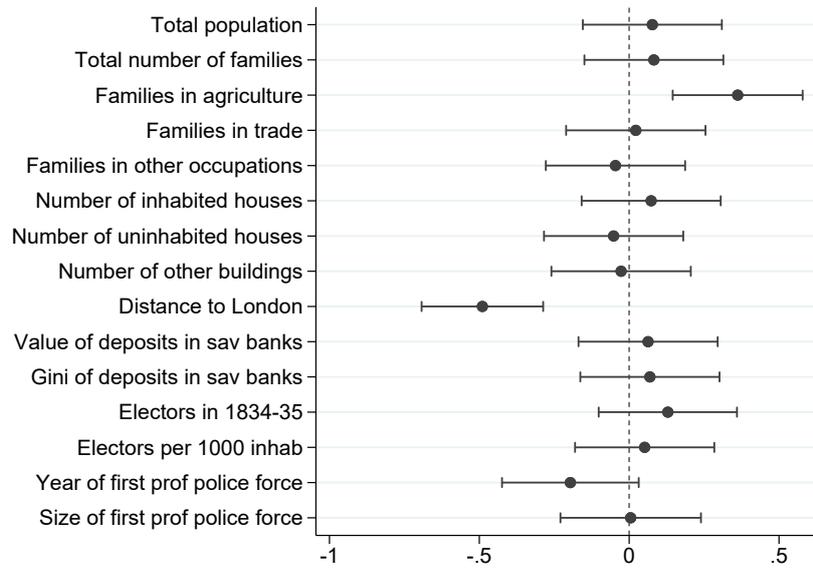
Note: Bivariate regressions of the form $\text{Variable}_i = \alpha + \omega\,\text{SpendingPre1834}_i + \epsilon_i$. Variable_i is a range of (where possible) pre-reform characteristics, as indicated on the vertical axis. SpendingPre1834_i is the level of average pre-reform poor relief spending. All variables have been standardised to have a mean of zero and a standard deviation of one. 90 percent confidence intervals shown.

Figure 5: First stage and reduced form event study plots
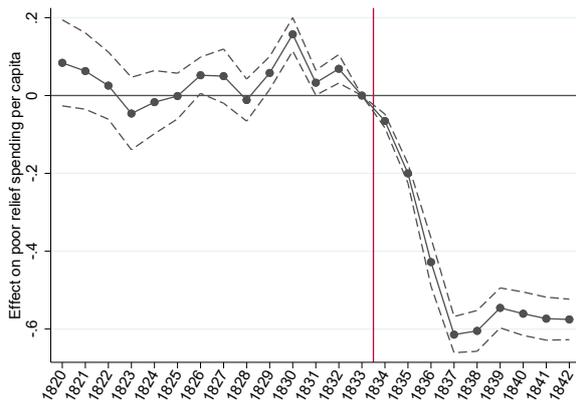
(a) First stage, no controls
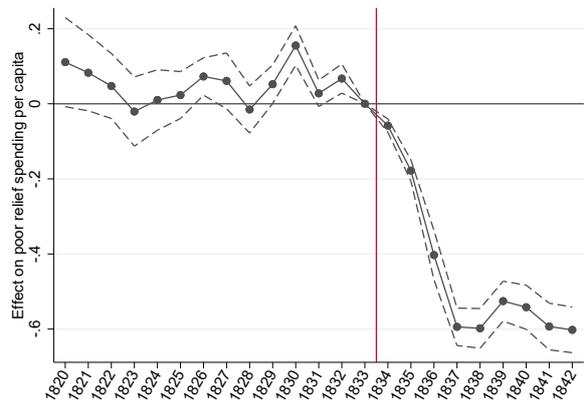
(b) First stage, full controls
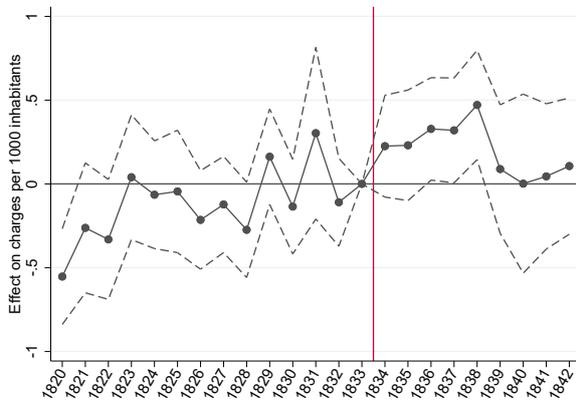
(c) Reduced form, no controls
(all crimes)
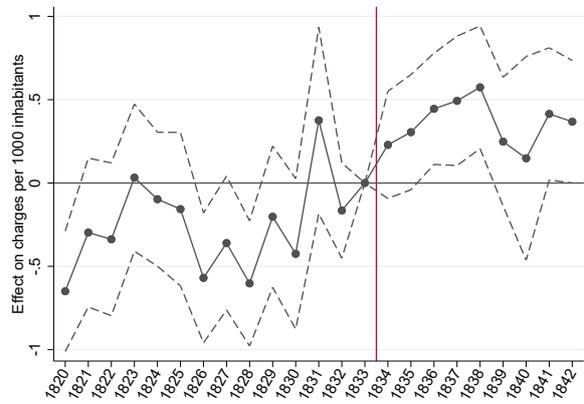
(d) Reduced form, full controls
(all crimes)
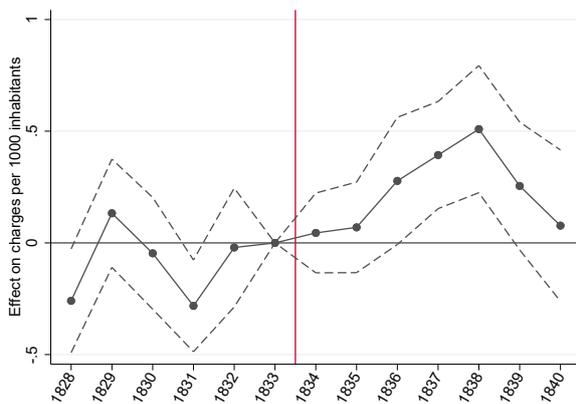
(e) Reduced form, no controls
(non-violent property crimes)
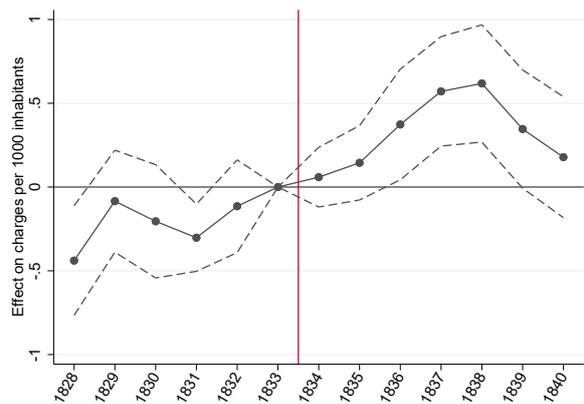
(f) Reduced form, full controls
(non-violent property crimes)

Note: First-stage and reduced-form coefficient plots from regressions of the form $Y_{it} = \alpha_i + \gamma_t + \delta_t \, \text{SpendingPre1834}_i \times \text{Year}_t + X'_{it}\beta + u_{it}$. For first-stage regressions, $Y_{it} = \text{PoorRelief}_{it}$ where PoorRelief_{it} is per-capita poor relief spending. For reduced-form regressions, $Y_{it} = \text{Crime}_{it}$ where Crime_{it} is the total number of criminal charges (Figures (c) and (d)) or the number of non-violent property crime charges (Figures (e) and (f)) per 1000 inhabitants. SpendingPre1834_i is the level of average pre-reform poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Dashed lines indicate 95 percent confidence intervals, with standard errors clustered on the level of the county.

Appendix: Additional Tables and Figures

Table A1: Disaggregating crimes against the person

	Panel A: OLS		
Crime type:	Person (all) (1)	Person (excl. assaults on officer) (2)	Assaults on officer (3)
Poor relief p.c.	-0.055* (0.031)	0.015 (0.024)	-0.078*** (0.026)
	Panel B: IV		
Crime type:	Person (all) (1)	Person (excl. assaults on officer) (2)	Assaults on officer (3)
Poor relief p.c.	-0.070* (0.038)	0.056* (0.032)	-0.143*** (0.041)
Observations	673	673	673
Counties	52	52	52
Outcome mean	0.0915	0.0735	0.0204
K-P F-Stat	543.8	543.8	543.8
County FE	Y	Y	Y
Year FE	Y	Y	Y
Controls	Y	Y	Y
Acquittals	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \, \text{PoorRelief}_{it} + X'_{it} \beta + \epsilon_{it}$. Crime_{it} is either charges for crimes against the person (column 1), the same but excluding assaults on officers (column 2) or charges for assaults against officers only (column 3), all per 1000 inhabitants. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A2: Breakdown by crime type: excluding 1830-1831

	Panel A: OLS				
Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.047	0.014	-0.640***	0.120***	-0.000
	(0.032)	(0.035)	(0.233)	(0.041)	(0.012)
	Panel B: IV				
Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.064	-0.033	-1.072***	-0.068	-0.031**
	(0.040)	(0.045)	(0.244)	(0.046)	(0.015)
Observations	570	570	570	570	570
Counties	52	52	52	52	52
Outcome mean	0.100	0.102	0.974	0.0385	0.0220
K-P F-Stat	483.2	483.2	483.2	483.2	483.2
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \text{PoorRelief}_{it} + X'_{it}\beta + \epsilon_{it}$, excluding the years 1830 and 1831 (during which the Swing Riots took place, see text for details). Crime_{it} is the number of criminal charges per 1000 inhabitants broken down into five broad crime categories; see Section 3 for a detailed description. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A3: Breakdown by crime type: excluding threshing and machinery

	Panel A: OLS				
Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.055*	0.045	-0.701***	0.099***	0.004
	(0.031)	(0.038)	(0.226)	(0.035)	(0.011)
	Panel B: IV				
Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.070*	0.001	-1.109***	-0.052	-0.027*
	(0.038)	(0.042)	(0.234)	(0.039)	(0.014)
Observations	673	673	673	673	673
Counties	52	52	52	52	52
Outcome mean	0.0915	0.103	0.962	0.0349	0.0216
K-P F-Stat	543.8	543.8	543.8	543.8	543.8
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \text{PoorRelief}_{it} + X'_{it}\beta + \epsilon_{it}$, excluding crimes involving the destruction of threshing machinery (see text for details). Crime_{it} is the number of criminal charges per 1000 inhabitants broken down into five broad crime categories; see Section 3 for a detailed description. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A4: Disaggregating larcenies: servants and non-servants

	Panel A: OLS		
Crime type:	Larceny overall (1)	Larceny by servant (2)	Larceny not by servant (3)
Poor relief p.c.	-0.649***	0.007	-0.656***
	(0.205)	(0.031)	(0.198)

	Panel B: IV		
Crime type:	Larceny overall (1)	Larceny by servant (2)	Larceny not by servant (3)
Poor relief p.c.	-1.011***	0.032	-1.043***
	(0.223)	(0.038)	(0.228)
Observations	673	673	673
Counties	52	52	52
Outcome mean	0.841	0.0365	0.805
K-P F-Stat	543.8	543.8	543.8
County FE	Y	Y	Y
Year FE	Y	Y	Y
Controls	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\,\text{PoorRelief}_{it} + X'_{it}\,\beta + \epsilon_{it}$. Crime_{it} is the number of larcenies per 1000 inhabitants (column 1), further broken down into larcenies by servants and non-servants (columns 2 and 3, respectively). PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A5: Robustness: seasonality of crime, accounting for time to prosecution

	Panel A: OLS					
	Property crimes (non-vio.)					
	Astronomical def.		Historical def. 1		Historical def. 2	
	summer (1)	non-summer (2)	summer (3)	non-summer (4)	summer (5)	winter (6)
Poor relief p.c.	-0.172*	-1.103***	-0.537***	-0.738***	-0.112	-1.164***
	(0.097)	(0.325)	(0.185)	(0.248)	(0.079)	(0.353)
	Panel B: IV					
	Property crimes (non-vio.)					
	Astronomical def.		Historical def. 1		Historical def. 2	
	summer (1)	non-summer (2)	summer (3)	non-summer (4)	summer (5)	winter (6)
Poor relief p.c.	-0.366**	-1.657***	-0.866***	-1.157***	-0.236**	-1.787***
	(0.141)	(0.402)	(0.240)	(0.326)	(0.108)	(0.451)
Observations	502	502	502	502	502	502
Counties	52	52	52	52	52	52
Outcome mean	0.189	0.636	0.374	0.451	0.153	0.671
K-P F-Stat	264.7	264.7	264.7	264.7	264.7	264.7
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
p-value diff. coef.	0.0025		0.4726		0.0009	

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\,\text{PoorRelief}_{it} + X'_{it}\,\beta + \epsilon_{it}$. Crime_{it} is the number of non-violent property crimes charges per 1000 inhabitants. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). The sample is composed of the subset of observations for which we have information on the month of trial. The definition of summer and non-summer months are as in Table 5, but corrected to account for a one month delay in prosecution times. P-values at the bottom of the table are from tests for the difference of the coefficients on poor reliefs between each summer versus non-summer comparison. Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A6: Seasonality of other crimes

Panel A: OLS

Historical definition 1

	Person		Property (vio.)		Property (mal.)		Currency	
	summer (1)	non-summer (2)	summer (3)	non-summer (4)	summer (5)	non-summer (6)	summer (7)	non-summer (8)
Poor relief p.c.	-0.011	-0.060**	-0.040*	-0.128***	0.060***	0.023	-0.006	0.015*
	(0.018)	(0.028)	(0.022)	(0.034)	(0.022)	(0.020)	(0.008)	(0.009)

Panel B: IV

Historical definition 1

	Person		Property (vio.)		Property (mal.)		Currency	
	summer (1)	non-summer (2)	summer (3)	non-summer (4)	summer (5)	non-summer (6)	summer (7)	non-summer (8)
Poor relief p.c.	0.011	-0.092**	-0.077***	-0.247***	-0.025	-0.044	-0.016	-0.000
	(0.024)	(0.039)	(0.028)	(0.052)	(0.017)	(0.034)	(0.012)	(0.012)
Observations	502	502	502	502	502	502	502	502
Counties	52	52	52	52	52	52	52	52
Outcome mean	0.0275	0.0539	0.0231	0.0479	0.0111	0.0210	0.00618	0.0121
K-P F-Stat	264.7	264.7	264.7	264.7	264.7	264.7	264.7	264.7
County FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
p-value diff. coef.	0.0243		0.0043		0.6168		0.3678	

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\text{PoorRelief}_{it} + X'_{it}\beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants broken down into four broad crime categories; see Section 3 for a detailed description. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). The sample is composed of the subset of observations for which we have information on the month of trial. All columns use May to September as summer, a combined definition coming from Collins (1976), Goose (2006) and Clark (2007). P-values at the bottom of the table are from tests for the difference of the coefficients on poor reliefs between each summer versus non-summer comparison. Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A7: Alternative mechanisms: introduction of professional police forces

Panel A: OLS

	All crimes			Property crimes (non-vio.)		
	Baseline	Controlling for		Baseline	Controlling for	
		Year first force	Size first force (p.c.)		Year first force	Size first force (p.c.)
	(1)	(2)	(3)	(4)	(5)	(6)
Poor relief p.c.	-0.606***	-0.604***	-0.505*	-0.701***	-0.704***	-0.643**
	(0.208)	(0.210)	(0.260)	(0.226)	(0.219)	(0.262)

Panel B: IV

	All crimes			Property crimes (non-vio.)		
	Baseline	Controlling for		Baseline	Controlling for	
		Year first force	Size first force (p.c.)		Year first force	Size first force (p.c.)
	(1)	(2)	(3)	(4)	(5)	(6)
Poor relief p.c.	-1.112***	-1.116***	-1.095***	-1.109***	-1.110***	-1.137***
	(0.273)	(0.276)	(0.373)	(0.234)	(0.227)	(0.288)
Observations	1196	1196	1173	673	673	660
Counties	52	52	51	52	52	51
Outcome mean	1.135	1.135	1.126	0.962	0.962	0.953
K-P F-Stat	523.9	517.6	341.7	543.8	537.2	328.7
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\,\text{PoorRelief}_{it} + X'_{it}\,\beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants (all crimes in columns 1-3, non-violent property crimes only in columns 4-6; see Section 3 for a detailed description). PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Columns 2 and 5 additionally control for the year of the introduction of the first county police force interacted with year fixed effects. Columns 3 and 6 additionally control for the size of the first county police force interacted with year fixed effects. Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A8: Alternative mechanisms: changes in sentencing

Panel A: OLS

Sentence:	Acquittal	Imprison-ment	Trans-portation	Death	Other
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.038	0.053	-0.018	0.006	-0.001
	(0.029)	(0.050)	(0.033)	(0.018)	(0.012)

Panel B: IV

Sentence:	Acquittal	Imprison-ment	Trans-portation	Death	Other
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	0.008	0.029	-0.057	0.051	-0.029
	(0.037)	(0.059)	(0.043)	(0.035)	(0.022)
Observations	276962	276962	276962	276962	276962
Counties	52	52	52	52	52
Outcome mean	0.277	0.489	0.186	0.0190	0.0286
K-P F-Stat	392.2	392.2	392.2	392.2	392.2
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Crime Type FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Sentence}_{jit} = \alpha_i + \gamma_t + \phi\,\text{PoorRelief}_{it} + X'_{it}\,\beta + \epsilon_{jit}$. Sentence_{jit} is an indicator for whether individual trial j results in an acquittal, imprisonment, transportation, death or another sentence (corresponding, in turn, to columns 1-5). PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A9: Alternative mechanisms: excluding acquittals

Panel A: OLS

Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.052**	0.037	-0.416**	0.187***	-0.006
	(0.024)	(0.026)	(0.161)	(0.051)	(0.008)

Panel B: IV

Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.085***	0.013	-0.716***	0.123*	-0.028***
	(0.031)	(0.031)	(0.168)	(0.068)	(0.010)
Observations	673	673	673	673	673
Counties	52	52	52	52	52
Outcome mean	0.0549	0.0729	0.694	0.0276	0.0158
K-P F-Stat	543.8	543.8	543.8	543.8	543.8
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\,\text{PoorRelief}_{it} + X'_{it}\,\beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants broken down into five broad crime categories (excluding charges resulting in acquittal); see Section 3 for a detailed description. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A10: Robustness: constant population using baseline figures (1821)

	Panel A: OLS				
Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.046	0.034	-0.715***	0.281***	0.006
	(0.035)	(0.039)	(0.238)	(0.073)	(0.012)
	Panel B: IV				
Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.052	-0.008	-1.186***	0.245**	-0.027*
	(0.046)	(0.045)	(0.270)	(0.100)	(0.015)
Observations	673	673	673	673	673
Counties	52	52	52	52	52
Outcome mean	0.103	0.115	1.072	0.0484	0.0242
K-P F-Stat	401.0	401.0	401.0	401.0	401.0
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\, \text{PoorRelief}_{it} + X'_{it}\,\beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants (using fixed population figures from the 1821 census) broken down into five broad crime categories; see Section 3 for a detailed description. PoorRelief_{it} is per-capita poor relief spending (also using fixed population figures from the 1821 census). Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A11: Robustness: soil suitability for major cereals

Panel A: OLS

	All crimes			Property crimes (non-vio.)		
	Baseline	Controlling for		Baseline	Controlling for	
		Cereal suitability	Above-median suitability		Cereal suitability	Above-median suitability
	(1)	(2)	(3)	(4)	(5)	(6)
Poor relief p.c.	-0.606***	-0.666***	-0.631***	-0.701***	-0.791***	-0.724***
	(0.208)	(0.204)	(0.208)	(0.226)	(0.213)	(0.213)

Panel B: IV

	All crimes			Property crimes (non-vio.)		
	Baseline	Controlling for		Baseline	Controlling for	
		Cereal suitability	Above-median suitability		Cereal suitability	Above-median suitability
	(1)	(2)	(3)	(4)	(5)	(6)
Poor relief p.c.	-1.112***	-1.158***	-1.101***	-1.109***	-1.185***	-1.118***
	(0.273)	(0.278)	(0.286)	(0.234)	(0.229)	(0.232)
Observations	1196	1196	1196	673	673	673
Counties	52	52	52	52	52	52
Outcome mean	1.135	1.135	1.135	0.962	0.962	0.962
K-P F-Stat	523.9	510.4	518.2	543.8	519.4	517.8
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\,\text{PoorRelief}_{it} + X'_{it}\,\beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants (all crimes in columns 1-3, non-violent property crimes only in columns 4-6; see Section 3 for a detailed description). PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Columns 2 and 5 additionally control for soil suitability for four major cereals (barley, oat, rye and wheat) using the index constructed by Caprettini and Voth (2020), interacted with year fixed effects. Columns 3 and 6 include a dummy variable for whether a county has above-median suitability for cereals, interacted with year fixed effects. Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A12: Robustness: wealth and inequality (proxied using savings bank deposits)

Panel A: OLS

	All crimes			Property crimes (non-vio.)		
	Baseline	Controlling for		Baseline	Controlling for	
		Value of deposits	Inequality in deposits		Value of deposits	Inequality in deposits)
	(1)	(2)	(3)	(4)	(5)	(6)
Poor relief p.c.	-0.606***	-0.594***	-0.617***	-0.701***	-0.671***	-0.695***
	(0.208)	(0.212)	(0.185)	(0.226)	(0.231)	(0.218)

Panel B: IV

	All crimes			Property crimes (non-vio.)		
	Baseline	Controlling for		Baseline	Controlling for	
		Value of deposits	Inequality in deposits		Value of deposits	Inequality in deposits)
	(1)	(2)	(3)	(4)	(5)	(6)
Poor relief p.c.	-1.112***	-1.088***	-1.078***	-1.109***	-1.078***	-1.061***
	(0.273)	(0.278)	(0.256)	(0.234)	(0.241)	(0.223)
Observations	1196	1196	1150	673	673	647
Counties	52	52	50	52	52	50
Outcome mean	1.135	1.135	1.151	0.962	0.962	0.977
K-P F-Stat	523.9	516.8	501.1	543.8	512.2	521.3
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\,\text{PoorRelief}_{it} + X'_{it}\,\beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants (all crimes in columns 1-3, non-violent property crimes only in columns 4-6; see Section 3 for a detailed description). PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Columns 2 and 5 additionally control for the value of deposits held in savings banks in 1833 (in inverse hyperbolic sine) interacted with year fixed effects. Columns 3 and 6 additionally control for the Gini coefficient of deposits held in savings banks in 1833 interacted with year fixed effects. Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A13: Robustness: political representation following the Great Reform Act, 1832

Panel A: OLS

	All crimes			Property crimes (non-vio.)		
	Baseline	Controlling for		Baseline	Controlling for	
		Number of electors	Electors p.c.		Number of electors	Electors p.c.
	(1)	(2)	(3)	(4)	(5)	(6)
Poor relief p.c.	-0.606***	-0.474**	-0.498**	-0.701***	-0.638***	-0.637***
	(0.208)	(0.211)	(0.191)	(0.226)	(0.238)	(0.227)

Panel B: IV

	All crimes			Property crimes (non-vio.)		
	Baseline	Controlling for		Baseline	Controlling for	
		Number of electors	Electors p.c.		Number of electors	Electors p.c.
	(1)	(2)	(3)	(4)	(5)	(6)
Poor relief p.c.	-1.112***	-1.043***	-0.891***	-1.109***	-1.077***	-1.016***
	(0.273)	(0.283)	(0.276)	(0.234)	(0.247)	(0.230)
Observations	1196	1196	1196	673	673	673
Counties	52	52	52	52	52	52
Outcome mean	1.135	1.135	1.135	0.962	0.962	0.962
K-P F-Stat	523.9	480.8	591.6	543.8	484.4	562.8
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\,\text{PoorRelief}_{it} + X'_{it}\,\beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants (all crimes in columns 1-3, non-violent property crimes only in columns 4-6; see Section 3 for a detailed description). PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Columns 2 and 5 additionally control for the number of electors in the year 1834-35 (in logarithms) interacted with year fixed effects. Columns 3 and 6 additionally control for the number of electors per 1000 inhabitants in the year 1834-35 interacted with year fixed effects. Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A14: Robustness: the industrialising North of England

	Panel A: OLS					
	All crimes			Property crimes (non-vio.)		
	Baseline	Controlling for North × year FE	Excluding North	Baseline	Controlling for North × year FE	Excluding North
	(1)	(2)	(3)	(4)	(5)	(6)
Poor relief p.c.	-0.606***	-0.584***	-0.631***	-0.701***	-0.647***	-0.669***
	(0.208)	(0.202)	(0.195)	(0.226)	(0.217)	(0.224)

	Panel B: IV					
	All crimes			Property crimes (non-vio.)		
	Baseline	Controlling for North × year FE	Excluding North	Baseline	Controlling for North × year FE	Excluding North
	(1)	(2)	(3)	(4)	(5)	(6)
Poor relief p.c.	-1.112***	-0.930***	-1.003***	-1.109***	-0.921***	-0.977***
	(0.273)	(0.264)	(0.262)	(0.234)	(0.219)	(0.225)
Observations	1196	1196	920	673	673	517
Counties	52	52	40	52	52	40
Outcome mean	1.135	1.135	1.157	0.962	0.962	0.976
K-P F-Stat	523.9	889.6	848.5	543.8	697.4	748.1
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\,\text{PoorRelief}_{it} + X'_{it}\,\beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants (all crimes in columns 1-3, non-violent property crimes only in columns 4-6; see Section 3 for a detailed description). PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Columns 2 and 5 additionally control for a "North of England" indicator (see text for details) interacted with year fixed effects. Columns 3 and 6 exclude Northern counties entirely. Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A15: Robustness: time-varying impact of pre-reform characteristics

|	**Panel A: OLS**				
Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.071*	0.046	-0.429**	0.310***	0.007
	(0.042)	(0.043)	(0.209)	(0.074)	(0.011)
	Panel B: IV				
Crime type:	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.150**	-0.026	-0.767***	0.194**	-0.039**
	(0.064)	(0.074)	(0.276)	(0.091)	(0.019)
Observations	673	673	673	673	673
Counties	52	52	52	52	52
Outcome mean	0.0915	0.103	0.962	0.0433	0.0216
K-P F-Stat	484.2	484.2	484.2	484.2	484.2
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \text{PoorRelief}_{it} + X'_{it}\beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants broken down into five broad crime categories; see Section 3 for a detailed description. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). We additionally control for the pre-reform values (measured in 1831, the last pre-reform census) of total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), interacted with a post-1834 indicator. Standard errors clustered on the level of the county reported in parentheses. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Table A16: Robustness: Conley spatial standard errors (up to 100km, 200km and 300km)

Crime type:	**Panel A: OLS**				
	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.055	0.045	-0.701	0.271	0.004
	(0.030)*	(0.042)	(0.215)***	(0.105)***	(0.013)
	[0.025]**	[0.040]	[0.201]***	[0.096]***	[0.012]
	{0.024}**	{0.040}	{0.217}***	{0.102}***	{0.014}

Crime type:	**Panel B: IV**				
	Person	Property (vio.)	Property (non-vio.)	Property (mal.)	Currency
	(1)	(2)	(3)	(4)	(5)
Poor relief p.c.	-0.070	0.001	-1.109	0.223	-0.027
	(0.036)*	(0.047)	(0.224)***	(0.156)	(0.016)*
	[0.030]**	[0.041]	[0.224]***	[0.159]	[0.014]*
	{0.028}**	{0.044}	{0.247}***	{0.172}	{0.015}*
Observations	673	673	673	673	673
Counties	52	52	52	52	52
Outcome mean	0.0915	0.103	0.962	0.0433	0.0216
K-P F-Stat	33.11	33.11	33.11	33.11	33.11
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

**Note:** OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\text{PoorRelief}_{it} + X'_{it}\beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants broken down into five broad crime categories; see Section 3 for a detailed description. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Standard errors clustered on the level of the county reported under each coefficient, corrected for spatial autocorrelation by implementing Conley (1999) standard errors using the acreg package by Colella, Lalive, Sakalli and Thoenig (2019). Different distance cut-offs indicated as follows: spatial autocorrelation up to 100km in parentheses, up to 200km in square brackets, up to 300km in curly brackets. *, ** and *** indicate significance at the 10, 5 and 1 percent levels, respectively.

Figure A1: Reduced form event study plots excluding acquittals
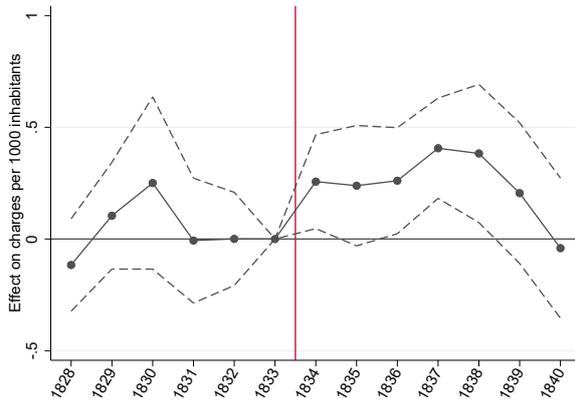
(a) Reduced form, no controls
(all crimes)
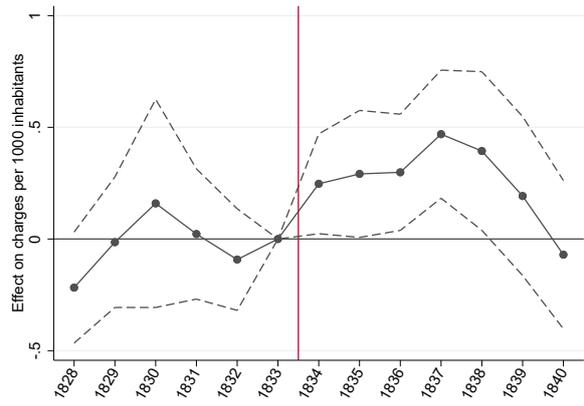
(b) Reduced form, full controls
(all crimes)
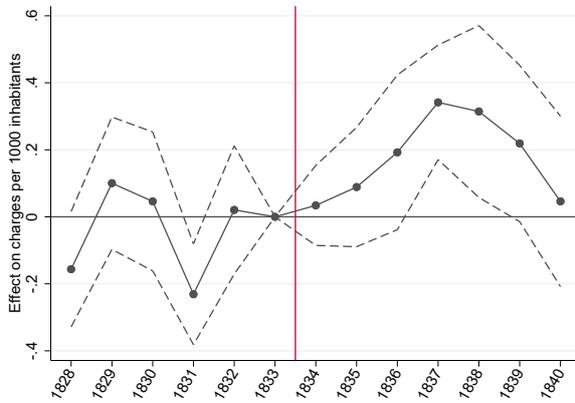
(c) Reduced form, no controls
(non-violent property crimes)
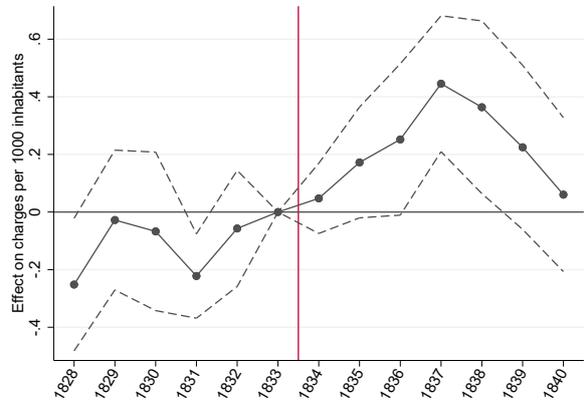
(d) Reduced form, full controls
(non-violent property crimes)

Note: Reduced-form coefficient plots from regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \delta_t \text{SpendingPre1834}_i \times \text{Year}_t + X'_{it}\beta + \epsilon_{it}$. Crime_{it} is the total number of criminal charges (Figures (a) and (b)) or the number of non-violent property crime charges (Figures (c) and (d)) per 1000 inhabitants (excluding charges resulting in acquittal). SpendingPre1834_i is the level of average pre-reform poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Dashed lines indicate 95 percent confidence intervals, with standard errors clustered on the level of the county.

Figure A2: Robustness: jackknife exercise dropping counties in turn – all crimes
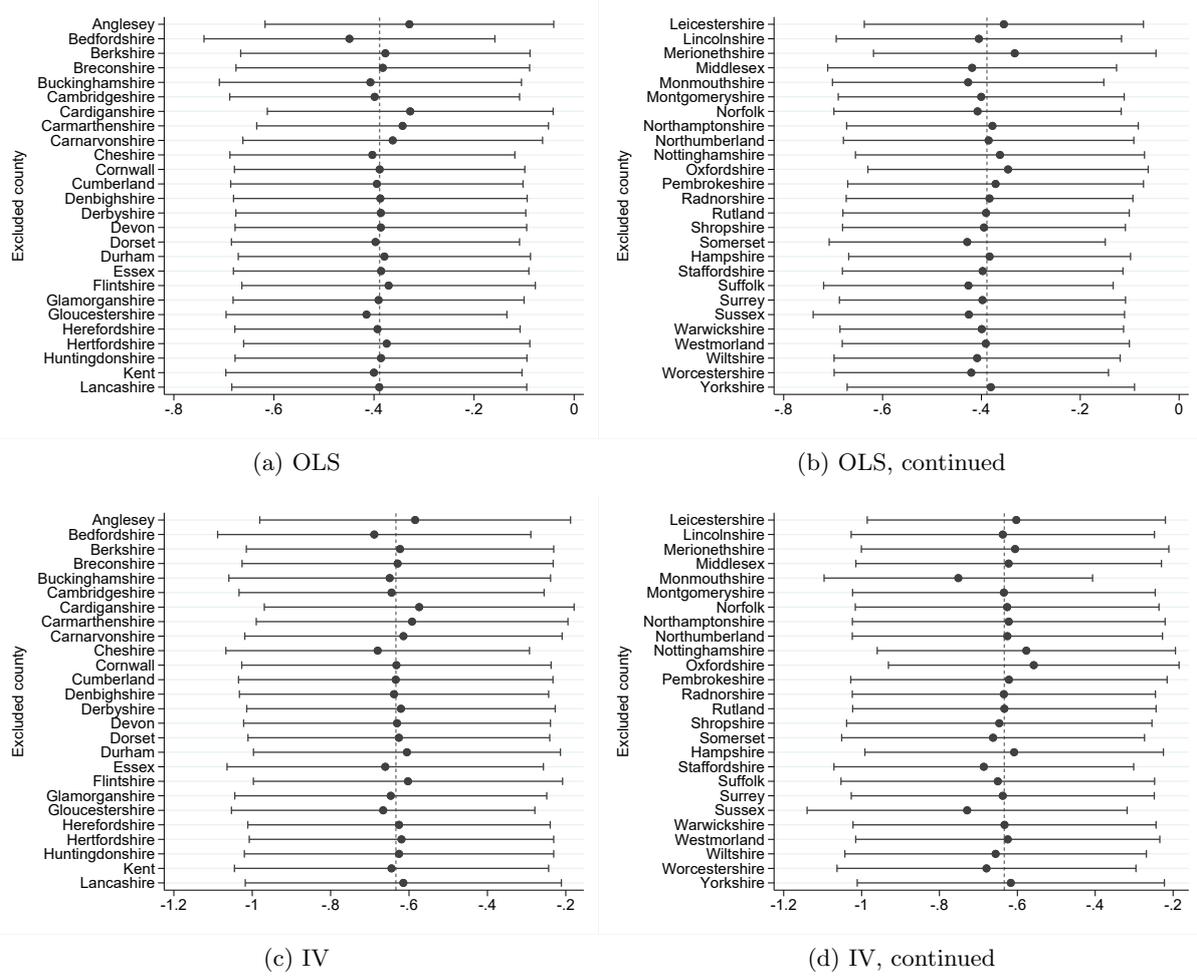
(a) OLS

(b) OLS, continued

(c) IV

(d) IV, continued

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi\,\text{PoorRelief}_{it} + X'_{it}\,\beta + \epsilon_{it}$. Crime_{it} is the total number of criminal charges per 1000 inhabitants; see Section 3 for a detailed description. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Each coefficient corresponds to a separate regression where the county indicated on the vertical axis is excluded. The vertical dashed line indicates the baseline coefficient from the corresponding regression using the full sample of counties. Standard errors clustered on the level of the county. 90 percent confidence intervals shown.

Figure A3: Robustness: jackknife exercise dropping counties in turn – non-violent property crimes
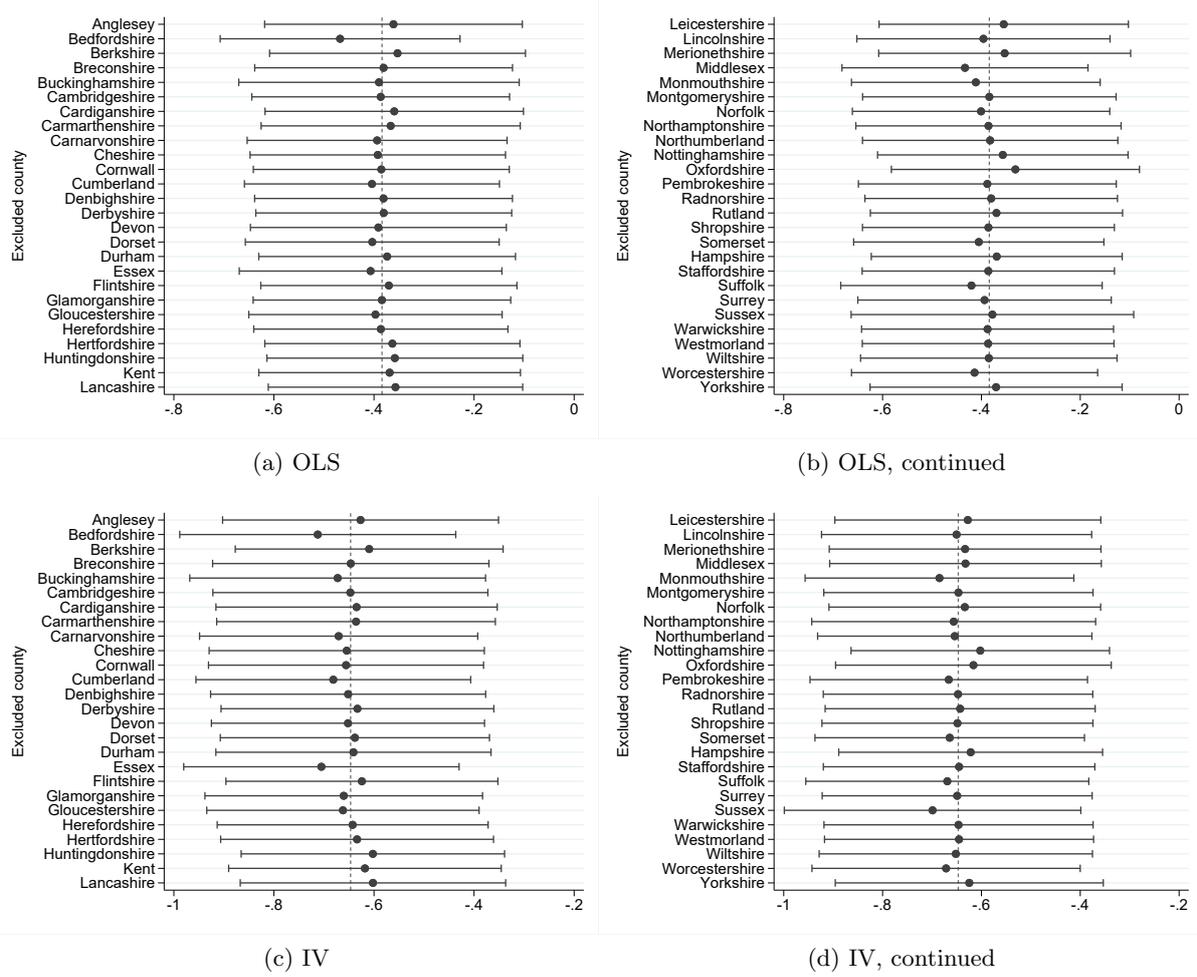
(a) OLS

(b) OLS, continued

(c) IV

(d) IV, continued

Note: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \, \text{PoorRelief}_{it} + X'_{it} \, \beta + \epsilon_{it}$. Crime_{it} is the number of criminal charges per 1000 inhabitants for non-violent property crimes only; see Section 3 for a detailed description. PoorRelief_{it} is per-capita poor relief spending. Controls include: total population, total number of families, families in agriculture, in trades/manufactures and in other occupations, total number of inhabited, uninhabited and other buildings (all in logarithms), as well as the inverse hyperbolic sine of the geodesic distance to London (interacted with year fixed effects). Each coefficient corresponds to a separate regression where the county indicated on the vertical axis is excluded. The vertical dashed line indicates the baseline coefficient from the corresponding regression using the full sample of counties. Standard errors clustered on the level of the county. 90 percent confidence intervals shown.