

Welfare Cuts and Crime

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WELFARE CUTS AND CRIME: EVIDENCE FROM THE NEW POOR LAW*

Eric Melander and Martina Miotto

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, highlighting the particularly criminogenic combination of welfare cuts and precarious work opportunities for the economically vulnerable.

Fiscal consolidation—*austerity*—is used by governments worldwide to overcome periods of macroeconomic 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 foresee (Accetturo *et al.*, 2014). Budget cuts may have adverse effects on the already economically vulnerable (Watkins *et al.*, 2017), and can provoke socio-political reactions (Fetzer, 2019; Ponticelli and Voth, 2020; Galofré-Vilà *et al.*, 2021). Remarkably, relatively little is known about the impact of austerity-induced welfare cuts on criminal behaviour.²

In this paper, we document the criminogenic effects of the largest welfare cut in British history: the Poor Law Amendment Act of 1834 that 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. These substantial shocks to counties' poor relief spending 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

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¹ For summaries of theoretical and empirical work, see Alesina *et al.* (2017) and Alesina *et al.* (2018).

² Important exceptions studying UK welfare reform and crime in the context of twenty-first-century austerity are recent papers by Giulietti and McConnell (2021) and d'Este and Harvey (2022).

payments, and thus with differential reductions in welfare generosity in the wake of the reform.³ Concretely, in our empirical strategy we instrument for spending in a given county-year using a difference-in-differences first stage, in which the New Poor Law induces differential shocks to poor relief across previously high- and low-spending counties.

With a novel panel dataset constructed using rich historical data from primary sources, we show that the welfare cuts imposed by the New Poor Law caused large increases in criminal activity. This result holds in simple OLS regressions, and when using the asymmetric reductions in poor relief spending that resulted from the reform to instrument for welfare expenditures. Our estimates suggest that a one SD decrease in spending caused a 0.20–0.34 SD increase in criminal activity, an economically sizeable effect.

What explains the relationship between reductions in poor relief and subsequent increases in criminal activity? To answer this question, we digitise the universe of over 250,000 individual-level criminal charges recorded between 1828 and 1840. These records provide information on the type and timing of crimes. Welfare cuts mainly affected non-violent property crimes (such as larcenies and poaching). Effects are heterogeneous across seasons and stronger during the winter months of high agricultural unemployment, particularly in years with low farm wages or high agricultural prices. The seasonal link between agriculture and crime suggests that precarious seasonal workers were most affected by the New Poor Law cuts, and committed more acquisitive crimes as a result. Across England and Wales, we calculate that the New Poor Law created approximately 2,700 additional non-violent property crimes in each post-reform year, a 17.2% increase compared to yearly pre-reform levels.

With these findings, we make three important contributions. First, we highlight an unintended consequence of austerity-induced retrenchments of the welfare state. Alongside the debates on the economic impact of austerity measures (Alesina *et al.*, 2017; 2018; House *et al.*, 2020), recently attention has turned to other socio-political outcomes. Political backlash against austerity may rouse support for extreme or populist parties (Fetzer, 2019; Galofré-Vilà *et al.*, 2021) and discontent may manifest in social unrest (Ponticelli and Voth, 2020). Work on recent episodes of austerity in the United Kingdom highlights the deleterious effects of welfare reform on the already economically vulnerable (Watkins *et al.*, 2017; Fetzer *et al.*, 2019; Giulietti and McConnell, 2021; d'Este and Harvey, 2022). Our findings underscore the criminogenic impact of cuts to welfare spending, particularly its interaction with the precarious position of the seasonally unemployed.

Second, we bring new historical evidence to bear on the relationship between welfare and crime, which has received considerable attention across the social sciences. Descriptive work has provided evidence of correlational relationships between modern welfare and crime.⁴ In historical settings, studies of the economic determinants of crime typically leverage climate- or weather-derived income shocks (Mehlum *et al.*, 2006; Bignon *et al.*, 2017; Chambru, 2020). Explicit reforms to the welfare state (however rudimentary) are less commonly considered (Fishback *et al.*, 2010, studying the expansion of US welfare during the Great Depression, being a notable exception). Using the natural experiment arising from the New Poor Law, we offer complementary

³ A rich literature on the history of the Poor Laws, summarised in Section 1, 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 2), and control for such differences throughout our analysis.

⁴ Zhang (1997) and DeFronzo and Hannon (1998a,b) are early examples; see Rudolph and Starke (2020) for a recent summary.

evidence to that provided by the literature on more modern welfare reforms (Foley, 2011; Carr and Packham, 2019; Tuttle, 2019; Watson *et al.*, 2020; Deshpande and Mueller-Smith, 2022), from a time when the welfare state was in its relative infancy.

Third, we contribute to a broader literature on historical poor relief, 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 have recently received considerable interest, with work exploring both determinants of spending (Chapman, 2020) and its impact on 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). We examine the as-yet unexplored link between crime and changes in poor relief as a result of the New Poor Law. With extensive novel data (both pre- and post-reform) and variation deriving from the reform, we uncover its causal impact on criminal behaviour.

1. Historical Background

1.1. *The New Poor Law*

The 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 that by 1830 it accounted for one-fifth of national expenditure (Ager, 2014). One of the principal advocates for the cut in poor relief was Thomas Malthus (1798), whose theories influenced the political discourse on the poor laws and inspired the design of the New Poor Law.⁵

The Act had several mechanisms for achieving its goals. First, it established poor law unions and a system of indoor relief to be administered inside workhouses.⁶ 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 purposely chosen to make those of an independent labourer of the lowest class more attractive. The second mechanism relied on the long-established system of removal under the settlement laws. Following this principle, paupers would be removed from the parish in which they claimed relief, unless it was their birth parish or they had acquired a settlement certificate. This measure served as a further deterrent to apply for relief.

These mechanisms were met with resistance. Popular campaigns protesting against workhouse conditions spurred social unrest throughout the country. However, the reforms were eventually

⁵ Many studies have tried to assess whether Malthus's theories were rooted in fact, concluding that he was mistaken (Griffith, 1926; Blackmore and Mellonie, 1927; Krause, 1958; Huzel, 1969; Wrigley and Smith, 2020). Contemporaries were influenced by his thinking, however, as evidenced by official government papers published in the early nineteenth century (Huzel, 1969). The Poor Law Report of 1834—which evaluated spending under the Old Poor Law system—supported Malthus's view, but historical research suggests that its negative assessment was influenced by contemporaries' opinions rather than real evidence (Blaug, 1964).

⁶ 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.



Fig. 1. *Poor Relief Spending before and after the Poor Law Amendment Act, 1834.*

Notes: Timeline showing per-capita poor relief spending in English and Welsh counties over the period 1820–42. Solid line plots the average level of spending each year. Dashed and dash-dot lines plot average spending by two sub-samples of counties: those with above-median (dashed) and below-median (dash-dot) average pre-reform spending. Vertical line indicates the year of the Poor Law Amendment Act 1834.

implemented, causing a drop in poor relief expenditure as reported in Figure 1.^{7,8} The figure shows the disproportionately large reductions in counties with high levels of pre-reform spending, highlighting the differential impact of the new measures across the country. The spatial pattern of poor relief reductions can be seen in Online Appendix Figure A1a: the agrarian counties in the South-East were hardest hit by the reductions in welfare spending, relative to the more industrial North.

1.2. Crime, Policing and Punishment

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

⁷ Figure 1 shows the drop in poor relief *per capita*, as available data only report county-level poor relief expenditure and total population. Spending levels *per recipient* cannot be found in the available returns (Baugh, 1975). Lindert (1998), for example, who specifically compared poor relief benefits per recipient across Europe, only reported this information at the national level, with three data points covering the whole first half of the nineteenth century.

⁸ 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 to stabilise at its new minimum level. This could reflect both delays in implementation and counties' opposition to the new measures, as suggested by qualitative historical literature (Fraser, 1976; Englander, 1998). This gradual reduction is also reflected in the timing of the impact of the New Poor Law on crime, which we present in Section 4.

However, data from primary sources exist on charges and convictions, showing a constant increase (Hart, 1955). Rising criminality 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).

Partly in response to these trends, policing reforms took place throughout the nineteenth century. Beginning with the Metropolitan Police Act of 1829, which established the first professional police force in London, a long series of reforms sought to create a nationwide statutory police force. This was achieved only after the 1870s, when the number of constables (outside London) rose to almost modern levels (Jones, 1983).⁹ This is important, as Bindler and Hjalmarsson (2021), looking specifically at nineteenth-century England, found that police forces significantly reduced crime only when their size was close to the nationally recommended threshold. Given these findings, 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 Online Appendix E, where we control for the introduction of county police forces.

Punishment also underwent a profound transformation during the nineteenth century. Possible punishments included capital sentences, non-custodial sentences (such as whipping) 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 this evolution is important for the careful interpretation of our results.¹⁰ In Online Appendix E, we show that sentencing behaviour within broad crime categories did not change systematically with the new system of poor relief introduced in 1834.

2. Data

We construct a novel dataset from multiple sources, creating a yearly panel of 52 English and Welsh counties from 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.

2.1. *Data on Poor Relief*

The main source for our ‘treatment’ of interest—poor relief spending—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 years). For each county, we digitise yearly information about poor relief expenditure from 1820 to 1842. Summary statistics for variables related to poor relief are presented in panel A of Online Appendix Table A1. In the average county year, total poor relief expenditure amounted to approximately £111,850. There was considerable variability in spending: while the average county year saw expenditures of £0.47 per capita, this figure varies significantly, from £0.14 to £1.23.

Importantly for our empirical strategy—described in Section 3—expenditure levels changed heterogeneously with the implementation of the New Poor Law, as shown in Figure 1. The spatial patterns of poor relief reductions are evident in Online Appendix Figure A1a: spending

⁹ Online Appendix E provides more detail on these reforms.

¹⁰ Bindler and Hjalmarsson (2018) showed, for example, the impact of the abolition of capital punishment on jury decisions in the context of London’s Old Bailey.

fell most in the agrarian counties near London. To account for this spatial pattern, we control for occupational structure and proximity to London throughout our analysis.

2.2. *Data on Crime*

We assemble data on criminal activity from two sources. First, we use the *Porter's Tables* to obtain information on annual county-level criminal charges from 1820 to 1842 (Board of Trade, [various years](#)). Our main measure is the number of charges per thousand inhabitants. Panel B of Online Appendix Table A1 reports summary statistics for our county-level crime variables. The average county year saw 1.14 charges per thousand inhabitants, again displaying considerable variation.

We expect a differential impact of poor relief reductions across various types of crime, with non-violent property crimes (such as larceny and poaching) responding most strongly. To this end, we collect data on criminal charges broken down into broad types. We digitise archival records of the universe of individual-level charges recorded in England and Wales between 1828 and 1840 (National Archives, [various years](#)). With over 250,000 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 Online Appendix Table A1, we provide summary statistics for charges per thousand inhabitants, broken down into six broad categories.¹¹ Non-violent property crimes are the most common.

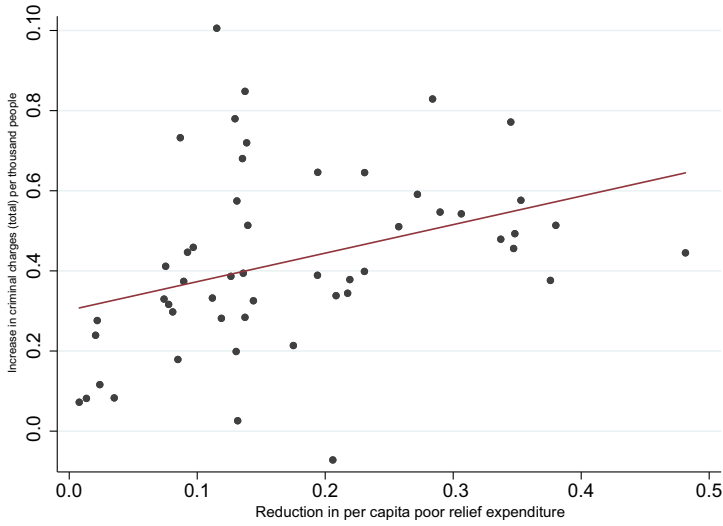
Summary statistics based on the 276,962 individual-level records are reported in panel C. The proportions of various types of crime for which individual defendants were charged naturally reflect county-level totals (see panel B). Additionally, we report information on the outcomes of these trials. A quarter of all defendants were acquitted, around half were sentenced to imprisonment and one-fifth transported to a penal colony. Death sentences and other miscellaneous sentences were less common.

The spatial patterns of the evolution of crime before and after the New Poor Law are shown in Online Appendix Figures A1b and A1c. 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 (Online Appendix Figure A1a). In Figure 2, we show correlations between reductions in per capita poor relief expenditures and increases in charges per thousand inhabitants. Counties where the drop in spending was greatest following the New Poor Law subsequently saw the largest increases in crime. We estimate this relationship more rigorously below, using a difference-in-differences instrumental variables strategy.

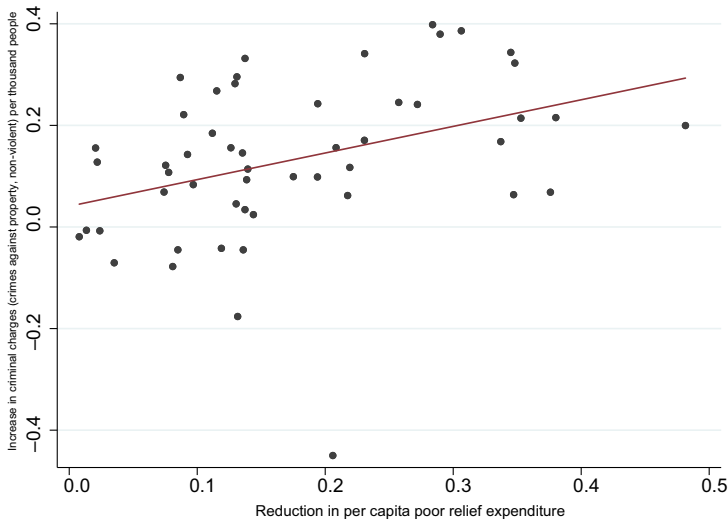
2.3. *Other Data*

We additionally construct a number of demographic and economic control variables. The proximate source of our data are the *Porter's Tables* (Board of Trade, [various years](#)), 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

¹¹ These are: (i) crimes against the person (such as assaults and murder), (ii) violent crimes against property (such as burglary and robbery), (iii) non-violent crimes against property (such as larceny and poaching), (iv) malicious crimes against property (such as arson and cattle maiming), (v) crimes against the currency (such as forgery and counterfeiting), (vi) other crimes (such as rioting and other misdemeanours).



(a) All crimes



(b) Non-violent property crimes

Fig. 2. *Correlation between Poor Relief Decreases and Subsequent Increases in Crime.*
 Notes: 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 thousand inhabitants. Panel (a) shows all crimes, panel (b) non-violent property crimes only. Each dot represents one county.

measures of the housing stock. Summary statistics for these variables are reported in panel D of Online Appendix Table A1. We describe additional data, used for robustness checks, in Online Appendix B.

3. Empirical Strategy

We are interested in the impact of poor relief on criminal activity. We begin by estimating equations of the following form:

$$\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \text{PoorRelief}_{it} + \mathbf{X}'_{it} \beta + \epsilon_{it}. \quad (1)$$

In our baseline specification, Crime_{it} is the total number of criminal charges per thousand inhabitants in county i in year t . We then proceed to decompose charges into broad crime types. Here PoorRelief_{it} is the per capita poor relief expenditure of the county, α_i and γ_t are county and year fixed effects, and \mathbf{X}_{it} is a vector of controls that we describe as we introduce it below.

Estimating (1) using OLS would, however, not allow for a causal interpretation of ϕ . In any given year, a county's poor relief spending was determined simultaneously with, and subject to the same constraint as other budgeting decisions, that also may have reduced criminal behaviour. We therefore exploit the asymmetric reductions in poor relief spending induced by the New Poor Law to generate exogenous variation in PoorRelief_{it} in a difference-in-differences first stage given by

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

where $\text{SpendingPre1834}_i \times \text{Post}_t$ is our excluded instrument, an interaction of a cross-sectional measure of average spending before 1834 with an indicator for the timing of the reform.¹² Counties with high pre-reform poor relief spending saw disproportionately large decreases following the New Poor Law, leading us to expect a large negative first-stage estimate of δ . In the presence of county and year fixed effects, identifying variation comes from changes in poor relief within counties over time. We leverage the fact that the drop in welfare spending was larger in counties with high initial spending levels, even if levels remained higher (in an absolute sense) after the reform in 1834.

When using pre-reform spending in first-stage equation (2), accounting for the drivers of between-county differences in poor relief before the reform becomes crucial. If these characteristics also predict crime in a manner that interacts with the timing of the reform, 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 was higher in more agrarian counties to support seasonally unemployed agricultural workers. Second, spending was higher near London in order to discourage migration flows to the metropolis. Controlling for occupational structure and proximity to London is therefore important to ensure the validity of our instrument.

We report balance checks in Online Appendix Figure A2, regressing pre-reform county characteristics on our instrument in a series of bivariate regressions.¹³ As the historical literature on the Poor Laws suggests, 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

¹² Concretely, SpendingPre1834_i is the average spending of county i over the years 1820–33 and Post_t equals one from 1834 onwards.

¹³ Variables have been standardised to have a mean of zero and an SD of one.

Table 1. *First Stage and Reduced Form.*

Dependent variable:	First stage		Reduced form			
	Poor relief per capita		Charges per thousand (all crimes)		Charges per thousand (non-vio. prop. crimes)	
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-1834 poor relief \times post	-0.500*** (0.025)	-0.497*** (0.022)	0.317** (0.120)	0.552*** (0.133)	0.308*** (0.077)	0.504*** (0.101)
Observations	1,196	1,196	1,196	1,196	673	673
Counties	52	52	52	52	52	52
Outcome mean	0.472	0.472	1.135	1.135	0.961	0.961
County FEs	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y
Controls		Y		Y		Y

Notes: First-stage and reduced-form regressions of the form $Y_{it} = \alpha_i + \gamma_t + \delta \text{ SpendingPre1834}_i \times \text{Post}_t + \mathbf{X}'_{it} \beta + u_{it}$. 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 thousand 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 are reported in parentheses. ** and *** indicate significance at the 5% and 1% levels, respectively.

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.

4. Results: Poor Relief and Crime

4.1. First-Stage and Reduced-Form Results

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

First-stage results are reported in columns (1) and (2) of Table 1. Focussing on the parsimonious specification without controls in column (1), the reform had a marked differential impact on post-reform poor relief spending. Counties with higher pre-reform levels saw significantly sharper reductions to spending after 1834. For every additional pound spent per capita before the reform, spending fell by an additional 0.5 pounds after the reform. This speaks to the post-reform compression of the gap between high- and low-spending counties that we documented descriptively in Section 1. This effect remains unchanged when controlling for measures of occupational structure, demographics, urbanisation and distance to London in column (2). Recall from Sections 1 and 3 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.

Turning to reduced-form results in columns (3) to (6) of Table 1, we estimate (2) with measures of criminal activity as outcomes. The reported coefficients capture the differential impact of the New Poor Law on crime in counties that were relatively harder hit by the reform. Taking all crimes

together in columns (3) and (4), the positive and statistically significant coefficients indicate that harder-hit counties saw sharper increases in criminal activity after the reform. We expect low-level acquisitive crimes to be particularly affected, and confirm this by using only non-violent property crimes in columns (5) and (6).¹⁴

A comparison of pre- and post-reform outcomes can disguise significant dynamics, and does not allow us to assess whether post-reform trends would have been parallel in the absence of the reform. To check whether this assumption appears valid, we estimate an event-study specification where we interact the cross-sectional measure SpendingPre1834_i with year fixed effects. We report this exercise, for our first stage and reduced form, in Online Appendix Figure A3. Throughout, we take 1833—the last pre-reform year—as the baseline year.

Online Appendix Figures A3a and A3b show these estimates for the first stage. The sharpness of the reform is evident: high- and low-spending counties evolved on similar trends prior to 1834. Immediately following the reform, there was a marked drop for previously high-spending counties. The effect magnifies over time as the reform was rolled out before stabilising in the late 1830s. In Online Appendix Figures A3c to A3f we repeat this exercise for the reduced form. A consistent pattern emerges: previously high-spending counties did not evolve differentially prior to the reform, but saw an immediate increase in criminal activity after 1834. These estimates are naturally more noisy than those for the first stage, but nevertheless clearly demonstrate the differential shift in criminal activity that took place in the wake of the reform.¹⁵

4.2. OLS and IV Results

We now return to our relationship of interest: the impact of poor relief spending on criminal activity. We begin with OLS estimations of (1); results are reported in columns (1) and (2) of panel A in Table 2. These estimates confirm our hypothesis: decreases in per capita poor relief spending are significantly associated with increases in criminal activity, both with and without controls.¹⁶ As discussed in Section 3, however, the endogeneity of poor relief spending precludes a causal interpretation of these estimates. In particular, since spending decisions for a range of budgeting items were determined locally subject to the same budget constraint, a spurious positive relationship is introduced between poor relief and criminal activity. The estimates reported in panel A of Table 2 are thus likely upward biased (towards zero), leading us to underestimate the true effect of poor relief on crime. We therefore report instrumental variable regressions using the New Poor Law as a shock to local spending in the first stage described above.

We present the results of this exercise in columns (1) and (2) of panel B in Table 2. The sharpness of the reform yields a strong first stage, which is reflected in the high F -statistic on the excluded instrument. Results are qualitatively in line with those from the OLS: reductions in poor relief spending cause statistically significant and economically meaningful increases in criminal activity. The estimated effect sizes are twice as large as before, which is not surprising

¹⁴ 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.

¹⁵ Note that the positive reduced-form effect begins to fade out after five years. Two observations can provide a speculative explanation for this pattern. First, as shown in Online Appendix Figures A3a and A3b, there is a small upward reversion in the first-stage effect on poor relief per capita from around 1838. This relative increase in spending could have alleviated some of the need to resort to crime. Second, individuals sentenced to imprisonment or transportation to a penal colony were incapacitated; these sentences were often long, even for minor offences.

¹⁶ Since poor relief spending enters in levels on the right-hand side, a welfare cut should be thought of as a decrease in this variable. Therefore, $\phi < 0$ implies that cuts are associated with increases in crime.

Table 2. *The Effect of Poor Relief Expenditures on Crime.*

	Dependent variable: charges per thousand inhabitants						
	All crimes (1)	All crimes (2)	Crimes against				Currency (7)
			Person (3)	Property (vio.) (4)	Property (non-vio.) (5)	Property (mal.) (6)	
<i>Panel A: OLS</i>							
Poor relief p.c.	-0.389** (0.174)	-0.606*** (0.208)	-0.055* (0.031)	0.045 (0.037)	-0.700*** (0.223)	0.271*** (0.069)	0.004 (0.012)
<i>Panel B: IV</i>							
Poor relief p.c.	-0.634*** (0.236)	-1.112*** (0.273)	-0.069* (0.039)	0.003 (0.041)	-1.112*** (0.231)	0.223** (0.090)	-0.027* (0.014)
Observations	1,196	1,196	673	673	673	673	673
Counties	52	52	52	52	52	52	52
Outcome mean	1.135	1.135	0.0917	0.104	0.961	0.0433	0.0217
K-P <i>F</i> -statistic	406.5	523.9	543.8	543.8	543.8	543.8	543.8
County FEs	Y	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y	Y
Controls		Y	Y	Y	Y	Y	Y

Notes: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \text{PoorRelief}_{it} + \mathbf{X}'_{it} \beta + \epsilon_{it}$, where Crime_{it} is the number of criminal charges per thousand inhabitants in columns (1) and (2), and in columns (3) to (7) is the number of criminal charges per thousand inhabitants broken down into five broad crime categories (see Section 2 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 are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

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. Turning to standardised coefficients, a one SD decrease in poor relief spending caused a 0.20–0.34 SD increase in criminal activity.

4.3. Breakdown by Crime Type

What explains the relationship between reductions in poor relief and subsequent increases in criminal activity? We propose that the reform increased individuals' willingness to take the risks associated with committing acquisitive 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 regressions in which 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 2 for a more detailed description of the five crime categories.¹⁷ If our proposed mechanism is correct, we should detect an impact of poor relief only on non-violent property crimes (including larceny and poaching). Other crimes should not be systematically affected, since we do not expect the margin of selection into these more severe crimes to

¹⁷ Note that we exclude the 'other crime' category from this analysis. This group of crimes is very heterogeneous (and results therefore difficult to interpret) and constitutes only 2% of all crimes.

be near that of petty property crimes. We report the results of this exercise in columns (3) to (7) of Table 2, separately for each of the broad crime categories. The negative relationship between poor relief and crime is explained predominantly by non-violent property crimes (column (5)).¹⁸ The effects on other crimes are statistically or economically insignificant in comparison.^{19,20}

These patterns across broad crime types—with strong and robust effects only for non-violent property crimes—is what classic models of rational crime would predict (Becker, 1968; Ehrlich, 1973).²¹ Similar patterns have been found in studies of modern welfare reforms (Foley, 2011; Carr and Packham, 2019; Tuttle, 2019; Watson *et al.*, 2020; Deshpande and Mueller-Smith, 2022; d'Este and Harvey, 2022). Findings from historical settings often document more nuanced relationships between income shocks and crime; negative agricultural income shocks tend to increase property crimes (Papaioannou, 2017), but may also depress violent crime (Mehlum *et al.*, 2006; Bignon *et al.*, 2017; Chambru, 2020). An important difference between those historical studies and our setting is the nature of the income shock; in settings where it operates through climate or weather, the same shock can increase property crime (through a neediness channel) and decrease violent crime (since prices of alcohol production inputs rise, lowering alcohol-related violence). The shock we exploit appears to operate exclusively through the neediness channel, as poor relief became more stringent and left more individuals in poverty.

While our individual-level crime data do not list the occupation of each defendant, we exploit the fact that larcenies (the most common crime) are recorded distinctly for servants and non-servants. Domestic servants would not, as a rule, be recipients of poor relief; this provides us with an interesting falsification check. First, in column 1 of Online Appendix Table A4, we show that a strong negative relationship exists between poor relief spending and total larcenies. Then, we break down larcenies into those committed by servants and non-servants (columns 2 and 3). Reassuringly, the effect on larcenies is driven entirely by crimes committed by non-servants. The absence of an effect for servants (non-recipients of poor relief) is precisely what our proposed interpretation would predict: these individuals' economic circumstances were unaltered by the New Poor Law.

Note that malicious property offences appear to be positively related with poor relief spending (Table 2, column (6)). This is an artefact of the Swing Riots taking place in England in 1830–31. Distressed agricultural workers, protesting against increasing mechanisation, demolished agricultural machinery and were charged with malicious property offences.²² The affected counties were agrarian, and thus had high levels of poor relief spending, which introduces a positive relationship between poor relief and malicious property crimes. In Online Appendix Tables A5

¹⁸ We check whether results on non-violent property crimes are heterogeneous along important dimensions. We use, in turn, all the variables in our vector of controls and create indicators for counties with above-median pre-reform levels of each variable, and run specifications where we interact these indicators with our main explanatory variable (poor relief). The results of this exercise are shown in Online Appendix Table A2. No single dimension of heterogeneity appears statistically significant.

¹⁹ We show in Online Appendix Table A3 that the small effect on crimes against the person (Table 2, column (3)) is driven by assaults on peace officers (a pre-professionalisation form of policing). We hypothesise that such crimes are likely committed when being apprehended for a more minor, non-violent offence.

²⁰ The coefficient on crimes against the currency in column (7) of Table 2 is very small and, further, this category represents only 2% of total crimes (see Online Appendix Table A1).

²¹ The economic hardship amplified by the New Poor Law can additionally be conceptualised as a 'strain' within criminological-sociological strain theories (Merton, 1938; Agnew, 1992).

²² Recent work has given the Swing Riots considerable attention, exploring their causes (Caprettini and Voth, 2020), spread (Aidt *et al.*, 2022) and political consequences (Aidt and Franck, 2015).

and A6 we perform two exercises: we exclude the years 1830–31 from the analysis to abstract away from the impact of the Swing Riots, and we exclude all crimes involving the destruction of threshing machinery. Reassuringly, in both tables the effect on malicious property crimes is markedly attenuated and vanishes completely in the IV specifications.

Lastly, we perform a long battery of checks to probe the robustness of our results. These are described in Online Appendix C.

4.4. *Mechanisms: Poverty and the Seasonality of Crime*

Our analysis so far has brought to light an important result: the New Poor Law induced large increases in petty property crime, suggesting that crime was a substitute for the sudden lack of poor relief for the economically vulnerable. But how can we be sure that it was indeed the poorest who reacted this way? We test this hypothesis by exploiting the seasonality of poor relief claims and crime rates.

First, poor relief was higher in more agrarian counties to support out-of-work seasonal agricultural labourers, making relief claims countercyclical to employment. Indeed, it was mainly precarious agricultural labourers who relied on poor relief during the winter months (Boyer, 1990; 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 crimes responded to the New Poor Law disproportionately during the off-season winter months, when the poor faced the compounding effects of unemployment and cuts to welfare spending.

We estimate (1) separately for crimes in summer (higher employment) and non-summer (lower employment) months, focusing on non-violent property crimes.²³ We focus on a historical definition of agricultural summer (May to September), based on the definitions of Collins (1976), Goose (2006) and Clark (2007). In columns (1) and (2) of Table 3, we report the effects of poor relief on crime separately for crimes committed in summer and non-summer months. These strongly support our hypothesis: the impact of the New Poor Law-induced cuts to poor relief is much stronger in non-summer months, when agricultural workers were likely unemployed and poorer than usual. Effect sizes in non-summer months are approximately twice as large as in summer.²⁴

To further strengthen our interpretation of seasonal poverty and neediness as a driver of our results, we exploit yearly variation in real farm wages and agricultural prices. We construct indicators for ‘wage shock’ and ‘price shock’ years, and include these indicators in an interaction term in our main specification. This allows us to explore whether poor relief spending was particularly predictive of crime in ‘bad’ years, when seasonally unemployed labourers would be faced with particular neediness.²⁵ The results reported in columns (3) and (4) of Table 3 suggest that this was indeed the case. The large negative effect of poor relief on crime in

²³ In this analysis we use the temporal information on criminal activity that we can systematically aggregate. Using our individual-level data, we focus on cases for which the month of trial is known, excluding cases for which we cannot assign a precise month.

²⁴ In Online Appendix D we perform additional tests to check robustness to different definitions of summer (Table D1), possible delays in prosecution times (Table D2) and seasonality of other types of crime (Table D3).

²⁵ Data on farm wages are from Clark (2010) and an index of agricultural prices is from Clark (2004). Using national time series of these variables from 1800 to 1870, we calculate their quadratic trends over this period. We classify a given year as having a ‘wage shock’ if that year’s real farm wage is below trend, and we classify years with an agricultural price index above trend as ‘price shock’ years. See Online Appendix Figure A4 for details.

Table 3. *Mechanism: Poverty and the Seasonality of Crime.*

	Dependent variable: property crimes (non-vio.)			
	Summer	Non-summer		
	(1)	(2)	Wage shock (3)	Price shock (4)
<i>Panel A: OLS</i>				
Poor relief p.c.	-0.460*** (0.140)	-0.815*** (0.288)	-0.785*** (0.290)	-0.814*** (0.289)
Poor relief p.c. × shock			-0.166 (0.160)	0.009 (0.159)
<i>Panel B: IV</i>				
Poor relief p.c.	-0.696*** (0.175)	-1.327*** (0.370)	-1.134*** (0.326)	-1.415*** (0.402)
Poor relief p.c. × shock			-0.417** (0.194)	-0.515* (0.266)
Observations	502	502	502	502
Counties	52	52	52	52
Outcome mean	0.236	0.588	0.588	0.588
K-P <i>F</i> -statistic	264.7	264.7	23.38	71.49
County FEs	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Notes: OLS and IV regressions of the form $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \text{PoorRelief}_{it} + \mathbf{X}'_{it} \beta + \epsilon_{it}$ (columns (1) and (2)) and $\text{Crime}_{it} = \alpha_i + \gamma_t + \phi \text{PoorRelief}_{it} + \pi \text{PoorRelief}_{it} \times \text{Shock}_t + \mathbf{X}'_{it} \beta + \epsilon_{it}$ (columns (3) and (4)). For the IV regressions (panel B), in columns (1) and (2) PoorRelief_{it} is instrumented by $\text{SpendingPre1834}_i \times \text{Post}_t$, and in columns (3) and (4) PoorRelief_{it} and $\text{PoorRelief}_{it} \times \text{Shock}_t$ are instrumented by $\text{SpendingPre1834}_i \times \text{Post}_t$ and $\text{SpendingPre1834}_i \times \text{Post}_t \times \text{Shock}_t$. Here Crime_{it} is the number of non-violent property crime charges per thousand inhabitants and 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. We use a definition of summer combining Collins (1976), Goose (2006) and Clark (2007), keeping May to September as summer. In column (3), we use data on real farm wages from Clark (2010) and define a 'wage shock' year as one where wages are below trend. In column (4), we use an index of agricultural prices from Clark (2004) and define a 'price shock' year as one where prices are above trend. Standard errors clustered on the level of the county are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.

non-summer months was especially strong in years when farm wages were particularly low, or when agricultural prices were particularly high.²⁶

Lastly, we use data on county police forces and individual-level criminal records to explore plausible alternative mechanisms related to changes in policing and sentencing. We show in Online Appendix E that results are not driven by such forces.

5. Concluding Remarks

Austerity measures continue to be controversial, particularly when accompanied by shrinkages of the welfare state. In this paper, we combine novel data with a natural experiment from history to document a potential unintended consequence of welfare cuts: an increase in criminal activity. Using the heterogeneous drop in poor relief spending across English and Welsh counties following

²⁶ We report estimates from this exercise using alternative definitions of non-summer in Online Appendix Table D4. These are, if anything, even more striking. Across specifications, the impact of poor relief on crime is systematically greater in shock years.

the New Poor Law in a difference-in-differences instrumental variables strategy, we find a robust negative effect of welfare spending on crime.

By disaggregating 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. Effects are stronger during the winter months, a particularly austere period for precarious seasonal agricultural labourers. These findings support our proposed mechanism: a reduction in the generosity in poor relief caused economically vulnerable and precariously employed individuals to select into crime. 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: savings from austerity measures must be weighed against their direct and indirect social costs.

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Additional Supporting Information may be found in the online version of this article:

Online Appendix Replication Package

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