Kicking You When You're Already Down: The Multipronged Impact of Austerity on Crime*

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Abstract

The UK Welfare Reform Act 2012 imposed a series of deep welfare cuts, which disproportionately affected ex-ante poorer areas. In this paper, we provide the first evidence of the impact of these austerity measures on two different but complementary elements of crime – the crime rate and the less-studied concentration of crime – over the period 2011-2015 in England and Wales, and document four new facts. First, areas more exposed to the welfare reforms experience increased levels of crime, an effect driven by a rise in violent crime. Second, both violent and property crime become more concentrated within an area due to the welfare reforms. Third, it is ex-ante more deprived neighborhoods that bear the brunt of the crime increases over this period. Fourth, we find no evidence that the welfare reforms increase recidivism, suggesting that the changes in crime we find are likely driven by new criminals. We probe a set of mechanisms to explain our findings, finding no support that either policing or the labor market are important in mediating these effects. With clinic-level drug prescribing data, we provide evidence in favor of worsening psychological welfare in areas harder hit by austerity, notably in neighborhoods that were more deprived prior to the welfare cuts. Combining these results, we document an increase in place-based welfare inequality in response to the UK government's austerity program on social welfare, which reinforced the direct inequality-worsening effect of this program. Guided by a hedonic house price model, we calculate the welfare effects implied by the cuts in order to provide a financial quantification of the impact of the reform. We document an implied welfare loss of the policy – borne by the public – that far exceeds the savings made to government coffers.

Keywords— Austerity, Crime, Crime Concentration, Hedonic Price Models
JEL Codes— H31, I38, R38.

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1 Introduction

In the aftermath of the Great Recession, the majority of OECD countries imposed some form of fiscal consolidation measures. These consolidation measures typically resulted in fiscal austerity policies[1], although there was considerable heterogeneity in how countries achieved fiscal consolidation.

In a speech in 2009, the year prior to his election as UK Prime Minister, David Cameron issued a clarion call for fiscal austerity in the UK, proclaiming "the age of irresponsibility is giving way to the age of austerity". Three years later, the center-right Conservative-Liberal Democrat Government, led by Cameron, enacted the Welfare Reform Act 2012, which introduced a raft of cuts to the social security system in the UK.[2] These reforms came in addition to a series of other curtailments to both central (including a 20% cut to police funding) and local government spending (which impacted myriad local services including Sure Start – an initiative akin to Head Start in the US – youth services, and libraries).

Scholars from diverse fields have studied the impact of these austerity-imposed cuts on several socioeconomic dimensions, providing evidence that this intervention generated adverse consequences, particularly for more vulnerable populations. A small selection of the outcomes studied includes a rise in excess mortality (Watkins et al., 2017), increased use of food banks (Cooper et al., 2014; Lambie-Mumford and Green, 2017) and worsening mental health (Sarginson et al., 2017). Austerity has also been linked to changes in political outcomes (Fetzer, 2019). To date, however, the impact of austerity on crime has remained largely unstudied.[3]

In this paper, we fill this lacuna. Specifically, we consider how the Welfare Reform Act 2012 – the flagship piece of legislation of the austerity program – impacted crime in England and Wales. To do so, we harness district-level variation in exposure to the welfare reforms, using a measure for austerity incidence developed by Beatty and Fothergill (2013).[4] This measure takes into account district-level benefit claimant counts across the ten key areas of the welfare system impacted by the welfare cuts, prior to the imposition of the austerity reforms, and then simulates the impact of the Welfare Reform Act based on detailed information of the decrease in funding from various government departments. This measure is particularly useful in our setting, as it rules out any possibility of crime affecting welfare take-up, thus preventing any concerns of reverse causality.

We use street-level crime data that spans all of England and Wales in order to study the causal effect of austerity on two distinct dimensions of crime: (i) the district crime rate – to measure changes *across* districts – and (ii) the concentration of crime – which sheds light on how crime changes *within* a district. We conduct analysis at the neighborhood level, combining our crime data with a measure of ex-ante neighborhood deprivation, in order to better understand which neighborhoods bore the brunt of crime changes. We supplement our core crime data with a second crime series that spans further back in time, as well as with data on recidivism, in order to understand *who* is driving the changes in crime that we document.

Our baseline empirical strategy involves the use a (non-staggered) difference-in-differences (DD) approach. We provide a battery of evidence in support of the key identifying assumption of parallel trends

[1] The OECD average fiscal consolidation mix was two-thirds fiscal austerity measures, one third tax increases (OECD, 2012).

[2] The UK was placed in category C of the OECD's four-tier need-for-fiscal-consolidation categorization, with category A being the highest and comprising Greece, Ireland and Portugal. As of 2012, The UK had the sixth largest 2012-2015 consolidation plan of OECD countries, and the highest for category C. The UK consolidation plan focused primarily on fiscal austerity measures. Social protection was a sector that experienced significant expenditure cuts, whereas health expenditure was protected (OECD, 2012).

[3] A recent paper by Bray et al. (2022) investigates the impact of welfare cuts on hate crime in the UK. While hate crime provides an interesting angle of study for understanding the impact of welfare cuts, it accounts for less than 1% of total crime, and therefore provides a very limited perspective on how austerity impacts the type, the scale and the distribution of crime at the local level — which is precisely the scope of our paper.

[4] In the paper, the term "districts" refers to the local governments (local authority districts and unitary authorities) in England and Wales. For more details, see https://www.ons.gov.uk/methodology/geography/ukgeographies/administrativegeography/england and https://www.ons.gov.uk/methodology/geography/ukgeographies/administrativegeography/wales.

in this setting, taking into account the recent critique of pre-trends testing by Roth (2022). We use two different crime data series to provide three streams of evidence in support of parallel trends: (i) placebo regressions based on the pre-reform period, (ii) graphical evidence of the pre-trends in the raw crime data and (iii) an application of the recent work by Rambachan and Roth (2023), which provides bounds on our key treatment effects under the assumption of parallel trend violations. Taken together, the evidence we present is strongly supportive of parallel trends in crime outcomes across areas of different exposure to austerity measures.

In investigating the impact of the UK austerity program on crime outcomes, we document four interrelated findings.

First, we find that the welfare reforms lead to an increase in the rate of crime – higher austerity-exposed districts experience a 3.5% increase in total crime, an effect driven by violent crime, which increases by 3.9%. We provide further evidence that this effect is being driven not by sexual offenses but rather physical violence resulting in injury, as well as harassment.

We next document that the concentration of crime within districts rises due to austerity exposure. This is the case for both violent and property crime, again with the impact of austerity most pronounced in the first two years. Through an augmented specification that combines the two crime measures, we find that districts that experience higher crime rates due to the austerity-imposed welfare reforms are the same districts that endure increased concentration of crime. To our knowledge, we are the first to study how a policy change can impact the concentration of crime. That we find that changes in the concentration of crime is especially notable given the inertia of crime concentrations both across areas, and within areas over time – a phenomenon that Weisburd (2015) dubs the law of crime concentration.

Third, we implement a triple-differences specification at the neighborhood level – interacting our baseline DD specification with a neighborhood-level, ex-ante deprivation measure – to show that it is the ex-ante more deprived neighborhoods that experience the largest crime rises over our analysis period. This is true for both violent and property crime, and the relationship between the change in crime and ex-ante deprivation is not only positive but convex – the most deprived experience the disproportionate burden of the increases in crime. Combined with the previous two findings, we can conclude that austerity led to a widening of place-based welfare inequality – the austerity cuts led to crime increases that disproportionately hit already poor areas.

Fourth, we use district-level data on reoffending to provide evidence that the likely cause of crime increases in higher austerity-exposed areas is not existing criminals committing more crimes, but rather an increase in the number of those committing crimes i.e., a response on the extensive margin of crime. This suggests a further (indirect) cost of the austerity program – more individuals being drawn into crime, which is likely to have long-term ramifications for these individuals and their families. A striking aspect that emerges by adding this last piece of evidence together with the findings described above is that new offenders commit crime in precisely the same neighborhoods as where crime was committed prior to the Welfare Reform Act.

Once we document the impact of the welfare system cuts to place-based crime outcomes, we then conduct a series of additional analyses to understand the mechanisms that drive the effects we document. To do so, we additionally use data police numbers, police effectiveness (as measured by crime clearance rates), local labor market outcomes, and clinic-level drug prescribing information. We provide three pieces of evidence to suggest that neither police numbers nor police effectiveness are driving our results. We then consider a battery of labor market outcomes, and find no evidence that those individuals in more austerity-exposed areas respond to the less generous welfare system by changing their labor market behavior.

Finally, we use our clinic-level prescribing data to measure changes in prescribing levels of specific categories of drugs, which we use to proxy elements of the psychological models we present in Section 3.2. Specifically these are (i.) Hypnotics (prescribed for sleep problems), (ii.) Anxiolytics (prescribed for anxiety), (iii.) Antidepressant Drugs, and (iv.) drugs for Alcohol Dependence. The last category of drugs is particularly interesting, as alcohol misuse may reflect not only increased negative mindset (frustration, strain among others), but may itself act as a accelerant for violence (Nutt et al., 2010; Carpenter and Dobkin, 2010, 2015). We document two pieces of evidence, both of which point to psychological channels being the key mechanism by which the welfare reforms impacted crime. First, we find large increases in district-level prescribing of drugs for alcohol dependence – a 4% increase in the number of prescriptions and and 8.5% increase in the number of alcohol dependence drug quantities. Second, we make use of the spatial richness of the prescribing data, and conduct analysis at the neighborhood level, interacting our baseline DD with neighborhood deprivation. In addition to a substantial within-district deprivation gradient in alcohol dependence drug prescribing, we also find increases in both hypnotic and anxiolytic prescribing in the most deprived neighborhoods in districts with higher austerity exposure. Such findings are consistent not with the standard, Becker-Ehrlich (Becker, 1968; Ehrlich, 1973) model of crime, but rather with with the psychological models we consider Section 3, which suggest the austerity reforms would lead to increases in frustration and psychological strain.

In order to provide a sense of the welfare loss induced by the policy, we conclude the analysis by using a hedonic house price model following the approach by Adda et al. (2014). The starting point is a set of property-type-specific house price regressions, implementing both difference-in-difference (DD) and triple-difference (DDD) specifications.

Our house price regression specifications are highly flexible across both space and time, in order to account for the current best practice when using DD specifications in a hedonic house price setting (Kuminoff et al., 2010; Kuminoff and Pope, 2014; Bishop et al., 2020). Notably, we allow the coefficients on all housing characteristics to differ in the pre and post periods, thereby allowing the hedonic price function to shift post-policy. We do so in order to avoid conflation bias (Kuminoff and Pope, 2014; Banzhaf, 2021). We note the recent work by Banzhaf (2021), which confirms the suitability of using a difference-in-differences approach with a hedonic house price model in order to study welfare effects of policy changes.

We use the DD and DDD parameters as inputs into an implied loss equation that multiplies the associated house price penalty due to the Welfare Reform Act by the pre-policy average house prices by the quantity of housing in the post period. We discuss each element of this equation, and the underlying assumptions involved, in Section 7.2.

Our preferred estimate (very much a lower bound of the true loss, given it is based only on losses in urban areas, whereas the benefit is based on the entire country) implies a welfare loss of £92.8bn, an amount that significantly exceeds the savings made by reducing welfare generosity. This large net welfare loss clearly suggests that complex policy decisions such as the Welfare Reform Act – when purely driven by fiscal convenience principles and that are myopically unaware of the multifaceted ramifications of their socioeconomic consequences – are at risk of generating adverse effects that might well counterbalance the positive ones.

Our work provides novel contributions to three different literatures. First, our study of the impact of an austerity-induced welfare reform on crime contributes to a body of work that investigates the link between the welfare system and crime. Key papers in this literature include Deshpande and Mueller-Smith (2022), who study the impact of SSI benefit loss on subsequent criminal charges in the US, Britto et al. (2022) who document the importance of unemployment insurance on preventing criminal participation after job loss in Brazil, and Jácome (2020), who studies the effect of Medicaid eligibility loss on crime, with a particular focus on individuals with mental health conditions. Our place-based approach complements these papers by documenting the consequence of welfare reforms at both the neighborhood and district-level, thus informing both academics and policymakers on the consequences for spatial inequality of welfare reform that has uneven impact across areas. Our work also relates to studies on the criminogenic effects of changes to the timing of welfare payments (Foley, 2011; Carr and Packham, 2019; Watson et al., 2020), of welfare structure reform (Machin and Marie, 2006; D'Este and Harvey, 2020), and of police numbers (Draca et al., 2011; Chalfin and McCrary, 2018).

Second, we contribute to a small, but growing, body of literature that documents violent crime responses to income shocks or changes in income inequality (Kelly, 2000; Fajnzylber et al., 2002; Enamorado et al., 2016; Freedman and Owens, 2016; James and Smith, 2017). Interestingly, several of these papers also appeal to theories outside of the domain of economics in order to rationalize their respective findings, underscoring the views we express in this work regarding the need for richer economic models of crime.

Finally, we make a novel contribution to the literature using hedonic house price models in conjunction with quasi-experimental research designs to study the welfare consequences of policy changes (Davis, 2004; Chay and Greenstone, 2005; Linden and Rockoff, 2008; Adda et al., 2014; Currie et al., 2015; Banzhaf, 2021). With the hedonic house price model-informed approach we take in this work, we are able quantify in monetary terms the losses experienced by the public due to the austerity reforms, thereby enabling us to compare these losses to the gains in public spending from a more austere welfare system.

The paper is organized as follows. Section 2 provides an overview of both the data we employ and the reform that we study in this paper. Section 3 describes models that relate austerity with crime, highlighting both the workhorse economic model and those from other disciplines. Section 4 outlines our empirical specification, and provides evidence for the related identifying assumptions. Section 5 examines the impact of the Welfare Reform Act on our main measures of crime. Section 5.4 investigates the link between ex-ante neighborhood deprivation and crime rises over the study period. Section 5.5 examines how the policy impacts recidivism, in order to understand what margin of crime is driving our core results. Section 7 quantifies the implied welfare loss of the cuts. Section 8 concludes.

2 Data and Setting

Our main dataset is a district-by-month-level panel that spans the five-year period from April 2011 to March 2016 (the fiscal years of 2011-2015). The starting point is determined by data availability, whilst the end-point is determined by the scope of the key austerity measure we use in the paper.

In Figure 1 we set the scene for this paper, plotting an extended time series for both total recorded crime and policing numbers for England and Wales, with the two gray segments representing our analysis period. Two things are immediately apparent. First, after over a decade of declining crime, we see crime begin to rise from 2014 onward (rising by 39% from 2014-2018), just as the austerity measures of the Welfare Reform Act are starting to bite. Second, one can see clearly the impact of the October 2010 Comprehensive Spending Review (CSR) on police numbers, which included a 20% real cut in the central government police funding grant police forces in England and Wales. This translated into 8 consecutive years of police numbers declining, a 15% decline from the peak in 2010 to the nadir in 2018.[5] A final point of note is that the two series appear to follow a similar temporal pattern, with police strength lagging crime by roughly five years.

2.1 Crime Series 1: Street-Level Crime Data

The main data we use is street-by-month-by-crime-type level data, published by the Single Online Home National Digital Team (SOHNDT hereafter), which gathers data from the 43 police forces in England and Wales and the British Transport Police.[6] One of the key elements of the data for our work is the micro-level geographical information. The SOHNDT provides coordinates for each location based on a list created in 2012. This list takes the mid-point of every road in England and Wales, and appends these with the location of locally relevant points of information, e.g., parks, train stations, nightclubs, shopping centers.[7] The SOHNDT conducts various quality assurance measures prior to publishing the data.

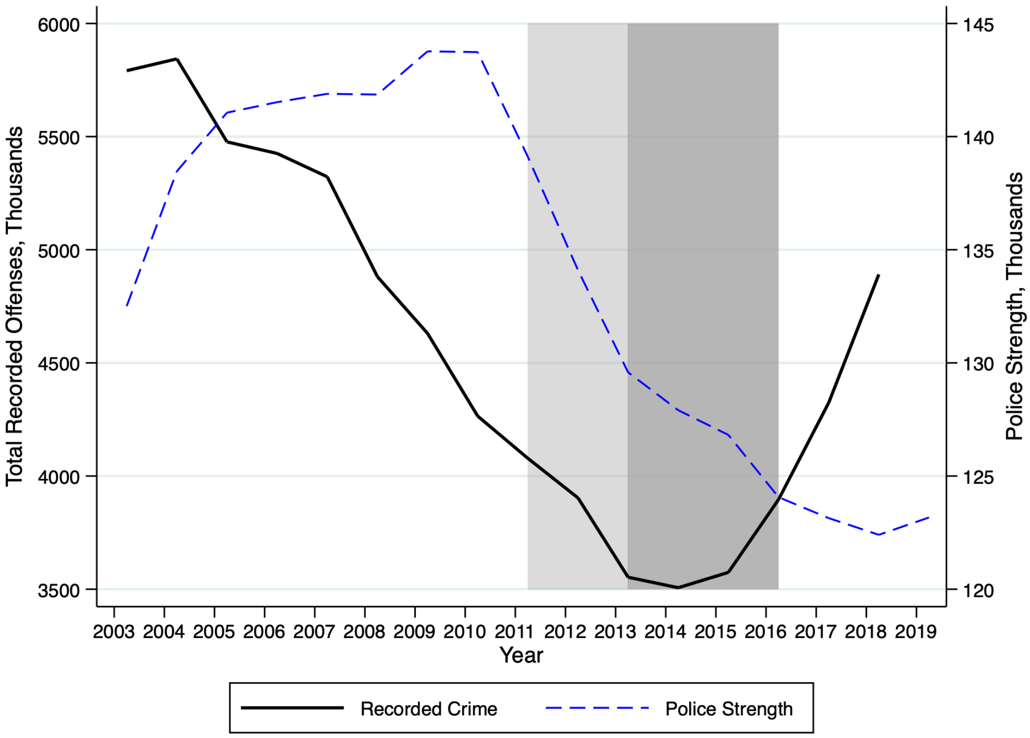
Our key treatment effect variable – exposure to austerity – is measured at the district level. We thus aggregate the street-level crime data to district-level using GIS software. Secondly, we aggregate the crime types into categories, namely property crime (bicycle theft, burglary, criminal damage and arson, other theft, shoplifting, theft from the person, and vehicle crime) and violent crime (possession of

[5] https://www.politics.co.uk/reference/police-funding.

[6] Using the data archive (https://data.police.uk/data/archive/) one can obtain data from December 2010 to present.

[7] For a full list got to https://data.police.uk/about/#location-anonymisation.

Figure 1: The Evolution of Crime and Police Strength



Notes: The figure plots full-time equivalent police officer strength and total recorded crime (excluding fraud and computer misuse) for the 43 Police Force Areas in England and Wales as of 31 March of each year. We cut the crime series a year early in order to keep the sample homogeneous - data for Greater Manchester Police (one of the largest police force areas) is not available from 2019 onward due to "the implementation of a new IT system". The light grey section highlights the pre-period for our analysis sample, the darker grey the post period for our analysis sample. Sources: Crime - ONS Crime in England and Wales: Appendix tables - year ending March 2020 edition. Police Strength - Home Office, Police Workforce, 2003-2019.

weapons, robbery, public order offenses, violence and sexual offenses, and public disorder and weapons offenses).

2.1.1 The Concentration of Crime

There is a nascent, but growing, literature that considers another dimension of crime – how crime is spatially concentrated within a given area (e.g., city).[8] Concentration of crime within a district provides a useful complementary measure to district-level crime rates. The crime rate provides an important snapshot of crime *across* areas. It does not, however, give any information of the distribution of crime *within* an area. The concentration measure does just this.

By considering how crime changes within an area, we can give a more complete characterization of how the geographical incidence of crime changed due to the austerity measures imposed as part of the Welfare Reform Act. In Section 5.4, we link the unequal incidence in crime to the prosperity of areas experiencing the crime, in order to get a sense of the deeper welfare implications.

Crime concentration is typically measured as the proportion of streets that account for 25% or 50% of crime in a district/city – the measure captures the spatial concentration of crime within an area. In this work we calculate the concentration measure at the district level, reiterating that we only consider urban districts. If crime is extremely concentrated, it may be that only 1% of streets in a city account

[8] Weisburd (2015), in his 2014 Sutherland Lecture to the American Society of Criminology, notes that even though crime levels vary greatly across areas, that crime concentration is extremely stable across both space and time. Weisburd dubs the narrow range of crime concentration across cities "the law of crime concentration". This makes clear that the null hypothesis in our setting is firmly that austerity will not shift the seemingly ironclad crime concentration.

for 25% of the crime in the city. On the other extreme, if there are many more crimes than streets and crime is uniformly spread across a city, then 25% of streets would account for 25% of crime.

There is an issue with this metric in that if the number of streets is far greater than the number of crimes, then even if crime is not spatially concentrated, it may look like it is when using a naïve concentration measure. As an example, consider a city where there are 100 murders, each occurring on a different street, and 10,000 streets. In this case 1% of streets account for 100% of all murders – a reflection not of spatial concentration, but rather the difference in magnitude of the number of crimes and streets. This issue makes it difficult to compare crime concentration both within crime, across cities of different sizes, and with city, across crime types of different levels of prevalence.[9]

A recent paper by Chalfin et al. (2021) proposes to use the marginal crime concentration (MCC) as a metric to solve the above-mentioned issue.[10] This metric takes crime concentration for crime share k in an area a in period t, cc^k_{at}, as the starting point. In order to account for differing ratios of crimes to streets in an area, Chalfin et al. (2021) simulate crime concentration based on randomly allocating each crime to a street. Randomization is implemented with a uniform distribution and streets are allocated crimes with replacement i.e., some streets will have many crimes, whilst some will have none. We run 10,000 simulations, each time calculating the simulated concentration of crime, and then take the average across the 10,000 runs to form $\overline{cc}^{k,\text{sim}}_{at}$. With these two concentration measures, we can calculate the MCC for share k of crime in $area$ in time period t as:

$$mcc^k_{at} = \overline{cc}^{k,\text{sim}}_{at} - cc^k_{at} \tag{1}$$

Given our interest in the unequal exposure of areas to crime, we focus our attention on the more spatially concentrated crime neighborhoods within a district, which maps to a crime concentration measures based on the proportion of streets that experience 25% of the crime in an area i.e., $k = .25$. A graphical representation of (1) can be seen for total crime, property crime and violent crime in Figure B1, where we present the average of the concentration measures for our sample.

We note that for a given value of simulated concentration ($\overline{cc}^{k,\text{sim}}_{at}$), the more concentrated crime in a district is, the smaller is the raw concentration value (cc^k_{at}), but the higher is the marginal crime concentration value (mcc^k_{at}). Thus if we document that austerity exposure leads to an increase in the marginal crime concentration, this will mean a positive coefficient in a regression of marginal crime concentration on austerity exposure.

2.2 Crime Series 2: Community Safety Partnership (CSP) Crime Data

For some of our analyses, we also used police recorded crime data at the Community Safety Partnership-by-quarter level (supplied by the Home Office). Both this and our primary crime data are police recorded crime, but the two data series differ in the dimensions in which they are collated. Community Safety Partnerships (CSPs) are equivalent to districts in almost all cases, although in certain cases one CSP will correspond to multiple local authority districts, generally in rural areas. During the period of study, there were 348 districts in total, and 315 CSPs. The CSP level data is coarser both spatially and temporally, but it has two distinct advantages over our primary crime data. First, it extends further back in time. We make use of this when studying pre-policy trends in crime. Second, the data is considerably more detailed at the offense level. One can identify the offense code level, whereby as an example, one can separately identify "arson endangering life" from "arson not endangering life" offenses. We make use of this dimension of the data in order to understand what types of offenses are behind the rises we document

[9]This is important given the range of district sizes we have in our sample. For example, in 2011, the least populated district was Purbeck, with a population of 45,165, 1,821 crimes and 903 street segments (crimes/streets = 2.02). The most populous district was Birmingham, with a population of 1,061,074, 86,935 crimes and 8,836 street segments (crimes/streets = 9.84). In absence of the adjustment inherent in the marginal crime concentration approach (that we introduce in the next paragraph), the discrepancy in the crime to street ratio between these districts would create difficulties in comparing concentration between districts.

[10]The method of Chalfin et al. (2021) builds upon previous work of Levin et al. (2017); Hipp and Kim (2017).

at the crime type level.

2.3 Recidivism Data

In order to explore the channels through which austerity impacts crime, we use data on recidivism from the Ministry of Justice's Proven Reoffending Statistics Series. These data allow us to empirically test whether we see an increase in crime because the pre-existing pool of offenders are committing more crimes (i.e., an increase on the intensive margin) or because more individuals are committing crimes (i.e., an increase in the extensive margin).

The recidivism data are structured in quarterly cohorts. In order to "enter" a given quarterly cohort one must either (i) be released from custody, (ii) receive a non-custodial conviction at court or (iii) receive a caution in a given three-month period. The cohorts are followed for a year and a half, where re-offending is measured in the first year, with an additional 6-month period included to allow the offense to be heard in court. The data we have provide information on the number of offenders, the number of reoffenders, the number of re-offenses, and the number of previous offenses for a four-quarter rolling panel at the district-level. Due to a change in the way the data was recorded in October 2015, we start with the April 2010-March 2011 cohorts and end with the October 2014-September 2015 cohorts, thereby ensuring consistency across the cohorts within our sample.

2.4 Clinic-Level Prescription Data

To provide better insight into the predictive power of the psychological models we present below in Section 3.2, we take advantage of a rich data source available from the National Health Service (NHS) in England – the Practice Level Prescribing (PLP) data series. This data contains information on every prescription from every General Practitioner practice (or clinic in the US). The data is available at the clinic-month-BNF code level. A BNF code is a 9 character code and uniquely identifies each drug prescribed[11]. We have the full address of every clinic, so we are able to match each clinic to a neighborhood (LSOA) and a district (LAD).

2.5 Housing Data

In order to quantify the financial impact of the reform, we obtained data for housing transactions from the Land Registry Price Paid Data for the fiscal years of 2010-2015 (04/2010 - 03/2016). These data contain the near universe of all residential property sales in England and Wales. They include housing characteristics such as property type (detached, semi-detached, terraced and flats), and indicators for new-build and leasehold status. In order to enrich the set of property characteristics, we merge in data from Energy Performance Certificates (EPC) which includes a more extensive set of characteristics.[12] The EPC variables we use are floor area of the property, number of habitable rooms, and indicators for double-glazed windows, triple-glazed windows and gas being the main fuel.

2.6 Other Data

We match in a variety of additional data that we use as control variables in our regressions and for further analyses. From the Police Workforce England and Wales Statistics, we obtain police force area

[11]See https://openprescribing.net/bnf/ for a full list of all BNF codes and https://www.bennett.ox.ac.uk/blog/2017/04/prescribing-data-bnf-codes/ for an overview of the BNF code structure.

[12]The way that the Price Paid and EPC data record street address – the variable we use to merge the two datasets – is not identical. In order to match the two data sources, we hence standardize the way in which addresses are recorded in both datasets and then match over several different variants of address specification. We specify an extremely high minimum match score coupled with the restriction that matches can only occur if postcodes match. In doing so, we sacrifice some potential true matches that would be accompanied by many false matches. This ensures that we are truly matching the correct properties in the two datasets. We obtain a match rate of 90.4%.

(PFA)-level information on the number of (full-time equivalent) police officers. From the Annual Population Survey and The Annual Survey of Hours and Earnings we obtain district-level labor market data. From the Office for National Statistics we obtain district-level and PFA-level population counts, and age-specific breakdown of population. From the Department for Communities and Local Government we obtain neighborhood-level Indeces of Multiple Deprivation (IMD) for 2010.

Table 1 presents summary statistics for the key variables in our main analyses. Note that the crime rates are monthly crime rates. Given our focus on both crime rates and crime concentration, we restrict our attention to the 234 urban districts in England and Wales.

Table 1: Descriptive Statistics

	Mean	Standard Deviation	Min.	Max.
Crime Rate:				
Total Crime	5.60	1.78	2.21	12.04
Crime Categories:				
Property	3.40	1.06	1.13	7.51
Violent	1.53	0.58	0.44	3.09
Crime Types:				
Theft	1.04	0.58	0.36	4.65
Burglary	0.69	0.22	0.23	1.31
Criminal Damage and Arson	0.78	0.23	0.35	1.54
Robbery	0.10	0.11	0.00	0.56
Violence and Sexual Offences	1.19	0.40	0.38	2.53
Marginal Crime Concentration:				
Total Crime	0.12	0.02	0.07	0.17
Category - Property	0.10	0.02	0.04	0.15
Category - Violent	0.07	0.02	0.02	0.12
Recidivism Rate	0.30	0.04	0.19	0.43
Reoffences per Offender	1.03	0.23	0.50	1.88
Reoffences per Reoffender	3.41	0.36	2.48	4.55
Reoffences per Reoffender / Offences per Offender	0.25	0.06	0.15	0.51
Simulated Austerity Impact (£)	479.59	118.64	247.20	914.01
Police Officers per 1000 Population	2.37	0.76	1.50	3.78
Median Weekly Wage (£)	524.85	72.03	382.31	803.55
Population Share: Males aged 10–17	0.05	0.00	0.03	0.06
Population Share: Males aged 18–24	0.05	0.01	0.03	0.11
Population Share: Males aged 25–30	0.04	0.01	0.02	0.10
Population Share: Males aged 31–40	0.07	0.01	0.04	0.12
Population Share: Males aged 41–50	0.07	0.01	0.06	0.08

Notes: Data at district level, with 234 districts. Summary statistics weighted by district-level population. The sample period covers 04/2011-03/2016. Crime Rates denote district average monthly crime rates per 1000 population. Marginal Crime Concentrations denote district average annual concentration measures. Simulated austerity impact denotes the simulated impact of austerity per working age person.

2.7 The Welfare Reform Act 2012

In the aftermath of the great recession of the late 2000s, the coalition government chose to implement a program of austerity as a means to reduce the budget deficit. This program included cuts to local government budgets, the cancellation of school building programs and reductions in welfare spending, the latter implemented in large part through the Welfare Reform Act 2012, which came into force on 1 April 2013. The austerity program reforms to the welfare system, implemented via the Welfare Reform Act and that lead to large cuts to the generosity of the welfare system across a number of individual benefit transfers, are the primary focus of this paper.

In order to study the impact of the Welfare Reform Act, we use the simulated austerity impact (SAI) measure of Beatty and Fothergill (2013), which provides district-level variation in exposure to the welfare reforms.[13] The SAI measures the annual (simulated) financial loss per working age adult (ages 16-64) for each district, calculated as the sum of financial losses across ten major welfare reforms, all except one of which were implemented as part of the Welfare Reform Act.[14] As Beatty and Fothergill (2013) note, these cuts – which average £480 per person per year in our sample – disproportionately impact areas that were poorer before the Welfare Reform Act. The direct human effect of the Welfare Reform Act was to increase income inequality, by driving down the lower end of the income distribution. What we document below is that these poorer areas were further negatively impacted by the Welfare Reform Act, this time indirectly, by the increase in crime experienced.

A particularly attractive characteristic of the Beatty and Fothergill (2013) measure is that it was calculated using specific benefit claimant counts on the eve of the reform, i.e., district-specific counts of welfare recipients (the share component of the measure) measured in the 2012 fiscal year, prior to the Welfare Reform Act coming into effect. The shift component was dictated by the Welfare Reform Act itself, with the budget cuts related to each component coming from HM Treasury. Given this, there is no scope for any simultaneity concerns caused by endogenous feedback over time, where for instance the benefit reforms lead to a change in crime, which further leads to a change in the local welfare claimant count, which then impacts the treatment measure.

In our analysis we consider three years post-Welfare Reform Act. This is for two reasons. First, it aligns with the Beatty and Fothergill (2013) measure. Several of the components come into full effect in the 2014 fiscal year, whilst two of the largest components come into full effect in 2015. Given this, it seems reasonable to consider three years as the post-period. We explore the sensitivity of our results to the length of the sample period in our robustness tests. The results are not sensitive to the precise length of the post-period. Second, we do not extend beyond the 2015 fiscal year, given that (i) a second round of welfare reforms were announced in May and November of 2015 and implemented from April 2016 onward and (ii) due to the expanded roll-out of Universal Credit (which implied a substantial change in the way welfare transfers are administered.)[15]

3 Models Linking Austerity with Crime

In this section, we briefly review the economic model of crime, in order to get a sense of how a shock to the welfare system may impact crime. The Becker-Ehrlich model (Becker, 1968; Ehrlich, 1973), with its emphasis on a rational consideration of criminal engagement, offers some traction when considering property crime. It does not, however, feel apt when thinking about violent crime. In order to glean insights regarding how a negative shock to benefit income may affect violent crime outcomes, we complement our economic model with theories developed in the areas of psychology and criminology. When presenting models from these disciplines, we discuss the models and then outline the relevant causal pathways they propose.

3.1 Economic Model

We follow the approach of both Edmark (2005) and Draca et al. (2019) in outlining the standard economic model of crime à la Becker (1968) or Ehrlich (1973). According to this approach, an individual will (rationally choose to) commit crime if the expected value of crime exceeds that of engaging in the legal

[13] Fetzer (2019) recently used this same measure to consider the impact of the welfare reforms on political outcomes, including most notably the Brexit vote.

[14] Part of one of the ten reform categories - the incapacity benefit reforms - were implemented by the previous government, but come into force during the period of study. The results that we document below are not systematically different if we remove the incapacity benefit component from the main SAI measure. This can be seen most clearly by comparing the results based on the full SAI measure (Table 2) with the equivalent estimates based on an augmented SAI measure that excludes the incapacity benefit component (Table B3).

[15] In a recent working paper D'Este and Harvey (2020) study how this change to the structure of welfare payments impacted crime, and document an increase in property crime as areas transition to the new payment method.

labor market:

$$E(V_C) > E(V_W). \tag{2}$$

The expected value of crime is a weighted average of the benefits of crime (P) and the costs of being caught ($-S$), which occur with probability π:

$$E(V_C) = (1 - \pi)P - \pi S. \tag{3}$$

Similarly, the expected value of engaging in the legal labor market is a weighted average of obtaining wage W when employed, and benefits B when not employed. Unemployment occurs with probability u:

$$E(V_W) = (1 - u)W + uB. \tag{4}$$

Building on the approach of Draca et al. (2019) we rewrite $\pi = \kappa_1 C + \kappa_2 O + \kappa_3$ where $\kappa_1 > 0$, $\kappa_2 > 0$, O is the strength of the police force and C the quantity of crime. We add the term κ_3 to allow for the fact that individuals may be exposed to different apprehension or detection technologies in different areas. We write down an equation for the equilibrium of crime as:

$$(1 - \kappa_1 C - \kappa_2 O - \kappa_3)P - (\kappa_1 C + \kappa_2 O + \kappa_3)S = (1 - u)W + uB. \tag{5}$$

Rearranging yields:

$$C = \frac{P - (1 - u)W - uB - (P + S)(\kappa_2 O + \kappa_3)}{\kappa_1 (P + S)}. \tag{6}$$

By partially differentiating Equation 6 with respect to B and multiplying by B/C, we obtain the crime-benefit elasticity:

$$\frac{\partial C}{\partial B}\frac{B}{C} = \frac{-u}{\kappa_1 (P + S)}\frac{B}{C} < 0. \tag{7}$$

The elasticity of crime with respect to benefits is negative. The austerity measures imposed by the Welfare Reform Act unambiguously cut the value of benefits, thus lowering B. Based on the Becker-Ehrlich model, and given the previous two points, we expect crime to increase in response to austerity measures. We can aggregate the supply of crime at the local level to obtain a district-level measure for the supply of crime.

The demand for crime will depend on local factors that relate to the gains from crime. This may involve local wages, house prices, levels of conspicuous consumption, levels of risk aversion, demographic composition, and many other factors. We may be able to proxy for a subset of these factors, but it is unrealistic to account for all relevant demand factors. Given the short time range that we consider (the five years from 2011 to 2015), we argue that district fixed effects along with regional time effects will adequately subsume and account for all relevant demand-side factors. The key assumption we make here is that benefit income, B, does not impact the demand for crime.[16]

The economic models of crime target the levels, rather than the concentration, of crime. As Freeman (1999) notes, when discussing the aggregate supply and demand equations for crime that one can derive from the Becker-Ehrlich model: "the simple demand-supply framework fails to explain some important phenomenon, such as the concentration of crime in geographic areas or over time".

3.2 Psychological Models

In this section we briefly survey key models from psychology and criminology that provide us a better understanding of how austerity measures may impact violent crime. We take this foray into other disci-

[16] If this assumption is incorrect, then when we estimate a crime equation of the form outlined in Section 4, the coefficient related to austerity will represent a lower bound for the impact of a cut to benefit income, B, on the supply of crime. This is because if B does impact the demand for crime, via the gains from crime, then an austerity-induced fall in B will lead to lower demand for crime.

plines given that the majority of violent crime is unlikely to be best considered under rational decision making, and the focus of the Becker-Ehrlich Model is on why a *rational* agent may engage in crime.

Frustration-Aggression Hypothesis Berkowitz (1989) reformulates the original frustration-aggression hypothesis (FAH) of Dollard et al. (1939). The starting point in this hypothesis is a "frustration" - an obstacle to the attainment of an expected gratification. In the revised frustration-aggression hypothesis there is a multi-stage, causal pathway that leads from (i) frustration, to (ii) a negative emotional response ("negative affect"), to (iii) an aggressive inclination which could finally lead to (iv) an act of aggressive behavior. Breuer and Elson (2017) provide a concise overview of this hypothesis. Of relevance to this study, when reviewing the original formulation of Dollard et al. (1939), Berkowitz notes that poverty per se would not be viewed as a frustration, but rather "keeping people from some attractive goal was a frustration only to the extent that these persons had been anticipating the satisfactions they would have obtained at reaching this objective" (Berkowitz, 1989, p. 60).

General Strain Theory Agnew (1992) develops general strain theory (GST) and expands on this in Agnew (2001). There is a large degree of overlap between this criminological theory and the psychological frustration-aggression hypothesis. A strain in GST is broadly defined, more so than in the FAH, and may be either (i) the failure to achieve positively valued goals (ii) the removal of positively valued stimuli from the individual or (iii) the presentation of negative stimuli. We may think of the impact of the Welfare Reform Act studied in this paper as relating best to the second of these three sources of strain.

Strain results in negative emotions, one of which may be anger. Anger "increases the individual's level of felt injury, creates a desire for retaliation/revenge, energizes the individual for action, and lowers inhibitions, in part because individuals believe that others will feel their aggression is justified [...] The experience of negative affect, especially anger, typically creates a desire to take corrective steps, with delinquency being one possible response. Delinquency may be a method for alleviating strain, that is, for achieving positively valued goals, for protecting or retrieving positive stimuli, or for terminating or escaping from negative stimuli." (Agnew, 1992, p. 60).

Agnew (2001) further characterizes the types of strain most likely to lead to a criminal response, including strain that is seen as unjust, and strain that is seen as high in magnitude, and strain that is caused by or associated with low social control. One could argue that all three of these apply to the welfare reform measures imposed by the austerity program.

Low-Status Compensation Theory Henry (2008, 2009) outlines the low-status compensation theory (LCST), which links status or shocks to status to violence. For the purposes of this paper, we think of a distribution of socioeconomic status, and the Welfare Reform Act creating a negative status shock to those receiving welfare payments. The first step in the proposed pathway here starts with low socioeconomic status, and the need to control or compensate for the negative shock to self worth induced by the welfare reforms. The next step is to note the increased vigilance of lower (socioeconomic-) status individuals to status-related threats to the self. The final step involves a link between vigilance towards self-protection and violence. Henry (2009) applies this theory, and attempts to test steps of the causal pathway that are outlined above.

4 Empirical Specification

4.1 Main Specifications

In our main specification, we estimate the impact of austerity on crime using a regression-adjusted difference-in-differences (DD) model of the form:

$$c_{it} = \beta Post_t \times Austerity_i + X_{it}^{'}\gamma + \pi_{r\times t} + \theta_i + \epsilon_{it} \ , \tag{8}$$

where c_{it} is either the log of the crime rate per 1,000 population or the marginal crime concentration for district i and time period t, $Austerity_i$ is the ex-ante simulated exposure of the district to the austerity package of the Welfare Reform Act (measured in £100s per working age person) and $Post_t$ is an indicator that takes the value of 1 from April 2013 onward (when the majority of the components of the Welfare Reform Act come into effect) and 0 otherwise. X_{it} is a vector of control variables that includes police officers per 1,000 population, median district wage, and the district population shares of males in the following age groups: 10-17, 18-24, 25-30, 31-40 and 41-50.[17] The first two control variables are motivated by a Becker-Ehrlich model of crime, whereas the population shares are intended to mimic the age-crime profile, and thus proxy the likely demographic structure of the offender sub-population within the district.[18] We also present the results for a binarized version of austerity, where we replace $Austerity_i$ in Equation (8) with $\mathbb{1}[Austerity_i \geq median]$ – an indicator that equals 1 if district i has austerity exposure above the (population-weighted) median, and 0 otherwise.

The t subscript denotes time, which is at the monthly level for the crime rate data, and the annual level for the crime concentration data. $\pi_{r \times t}$ are region-by-time fixed effects (specifically region-by-month-by year fixed effects and region-by-year fixed effects for crime rates and crime concentration, respectively) and θ_i are district fixed effects.[19] We cluster ϵ_{it} by district.

Given the short time span of our study, the district fixed effects will capture the lion's share of local unobserved heterogeneity. On top of these are the region-by-time fixed effects allowing us to account non-parametrically for region-specific time effects at the level of variation in the data (month-by-year for crime rates, year for crime concentration). The local wage variable captures temporal variation in local district labor market conditions, and the police numbers account for changes in policing numbers of the five-year period, which as seen in Figure 1 appear to lag crime changes by four to five years, thus ruling out any contemporaneous simultaneity issues.[20] These variables are included in all specification below, unless otherwise stated, in order to capture relevant local conditions, and thus enable us to isolate the direct impact of the austerity-imposed welfare cuts. Given the myriad policy changes occurring within this period, and the fact that certain areas were more likely to bear the brunt of these changes, it is critical that we individually account for all relevant channels.

In order to consider dynamic treatment effects, we supplement our baseline DD estimates with an event study design. We use our crime data series 2 for this (the CSP-by-quarter level crime data) in order to extend further back in time. The estimating equation for this design is of the form:

$$c_{iyq} = \sum_{\substack{y=2010, \\ y \neq 2012}}^{2015} \beta_y \left(FiscalYear_y \times Austerity_i \right) + X_{iq}^{'} \gamma + \pi_{r \times q} + \theta_i + \epsilon_{iq} \ , \tag{9}$$

[17] We did not include local unemployment in addition to wages, given that we can think of local wages being a function of local unemployment, as per the wage curve argument of Blanchflower and Oswald (1994, 1995). If we ignore this argument and enter local (district) unemployment in addition to district wages, the coefficient on unemployment is both small and statistically insignificant. The inclusion of local unemployment does not alter the estimated treatment effect parameter.

[18] See Hansen (2003) and Britto et al. (2022) for recent examples of the age-crime relationship, or O'Brien and Stockard (2002) for evidence on the age-crime victimization relationship

[19] There are a total of 10 regions. England comprises the following 9 regions: North West, North East, Yorkshire and the Humber, West Midlands, East Midlands, East of England, London, South West, South East. Wales is a self-contained region.

[20] There are several papers that focus on the possible simultaneity biases between crime and policing, and use quasi-experimental approaches to measure the causal impact of policing on crime (e.g., Draca et al. (2011)). Chalfin and McCrary (2018) provide an interesting counter to these papers, arguing that what we should be concerned about is more the correct measurement of policing numbers rather than simultaneity bias. In this study, we do not instrument for policing. We argue that over the five year time frame, district fixed effects and regional-by-time fixed effects will capture a local levels effect of both the local crime and policing environment. In addition, during this period the key change to policing was driven by large-scale, universal budget cuts due to austerity measures. Third, policing numbers per se appear to be unresponsive to crime in the short run, at least based upon the time series evidence we present in Figure 1. Finally, we obtain policing numbers directly from the Home Office police workforce statistics series, hence are not overly concerned about measurement error. To be conservation, we lag police numbers by one calendar year.

where we note for clarification that the temporal variable y denotes fiscal year, and q denotes quarter-by-year.

4.2 Identification

The key identifying assumption underpinning our empirical approach is that, irrespective of the intensity of exposure to austerity and conditional on control variables and fixed effects, districts experience common trends in crime.

To underscore the importance of the parallel trend in allowing us to estimate to parameter of interest – the average treatment effect on the treated (ATT) – we present a simplified version of our binarized treatment in absence of covariates or fixed effects. The exposition below follows Cunningham (2021). Comparing crime outcomes for areas with high (H) and low (L) austerity exposure, the ATT estimate $\hat{\beta}$ can be written as:

$$\hat{\beta} = (\bar{c}_{H,1} - \bar{c}_{H,0}) - (\bar{c}_{L,1} - \bar{c}_{L,0}) \, , \tag{10}$$

where the subscripts 0 and 1 refer to the pre and post periods. Rewriting the term above with conditional expectations, we have:

$$\hat{\beta} = (E[C_H \mid Post = 1] - E[C_H \mid Post = 0]) - (E[C_L \mid Post = 1] - E[C_L \mid Post = 0]) \, . \tag{11}$$

Making use of the potential outcomes framework, with C^0 and C^1 respectively representing untreated and treated potential outcomes, where the treatment in this case is high austerity exposure, and adding and subtracting the term $E[C^0_H \mid Post = 1]$ we can recast Equation (11) as

$$\begin{aligned} \hat{\beta} = {} & (E[C^1_H \mid Post = 1] - E[C^0_H \mid Post = 0]) - (E[C^0_L \mid Post = 1] - E[C^0_L \mid Post = 0]) \\ & + (E[C^0_H \mid Post = 1] - E[C^0_H \mid Post = 1]) \, . \end{aligned} \tag{12}$$

Rearranging Equation (12), we can write:

$$\begin{aligned} \hat{\beta} = {} & (E[C^1_H \mid Post = 1] - E[C^0_H \mid Post = 1]) \\ & + \left[(E[C^0_H \mid Post = 1] - E[C^0_H \mid Post = 0]) - (E[C^0_L \mid Post = 1] - E[C^0_L \mid Post = 0]) \right] \, . \end{aligned} \tag{13}$$

The first term of Equation (13) is the ATT. The second term, based on untreated counterfactual outcomes, is the non-parallel trends bias. This formulation of $\hat{\beta}$ in Equation (13) is particularly useful, as it underscores the importance of the parallel trend assumption. If satisfied, the parallel trends assumption means we can use our DD estimator to recover the ATT as the second, "bias" term is equal to zero.

Keeping this in mind, we take into account the recent critique to canonical pre-trends testing made by Roth (2022), who highlights issues with standard pre-trend testing, including the limited power of such tests, and as a practical recommendation, suggests the use of the bounding approach of Rambachan and Roth (2023). We provide a battery of evidence, using both our crime data series and multiple approaches, in support of parallel trends in our setting.

We first use our primary crime data, focusing on the two years of pre-policy data, and implement placebo DD regressions. Specifically, we perform augmented versions of Equations 8 and **??** above, with the sole difference that in the placebo specifications $Post_t$ takes the value of 1 for the year 2012, and 0 for the year 2011.

The results for both crime rates and crime concentration from these placebo regressions can be seen in Table A1 and Table A4, respectively. Table A1 shows that there is no evidence of a violation of the parallel trends assumption. This is the case for crime as a whole, for both violent and property crime categories, and for the five individual crime types of interest. It also holds for both the continuous and the binary treatment specifications. Table A4 presents the crime concentration placebo regression results. Mirroring what we find for crime rates, there is no evidence of parallel trends violation for crime concentration as whole, or for violent or property crime categories. This is true for both implementations of treatment definition.

We then turn to our alternative crime data (the CSP level data series), and provide further support for parallel trends in our key crime rate specifications.[21] We use the data to provide three complementary pieces of evidence in support of parallel trends: (i) placebo regressions based on a longer time period that extends back to 2009 (Table A2), (ii) graphical evidence of the pre-trends in the raw data in the extended pre-period (Figure A1) and (iii) an application of the recent work by Rambachan and Roth (2023), which provides bounds on our key treatment effects under the assumption of parallel trend violations (Table A3 and Figure A2).

Taken together, our evidence is strongly supportive of parallel trends in crime outcomes across areas of different exposure to austerity measures.

5 District Crime Outcomes

5.1 Crime Rates

Table 2 presents DD estimates for our key crime rate outcomes. We turn first to Panel A – our baseline DD specification estimates. Higher austerity exposure leads to higher district crime rates, an effect driven not by property crime, but rather by violent crime. Given that austerity primarily has financial repercussions, this first result suggests that we need to look beyond the standard economic model of crime to understand why this occurs. A one standard deviation increase in a district's exposure to austerity measures is associated with a 1.7% increase in total crime, whilst the increases for property and violent crime are 0.8% and 1.7% respectively.

Panel B presents the estimates based on a binarized austerity measure, thus we can interpret these results as the changes in crime in high austerity exposure areas. In high exposure districts, total crime increased by 3.5% during the first three years of austerity, and violent crime by 4.8%.

Table 2: DD Evidence of the Impact of Austerity on Crime

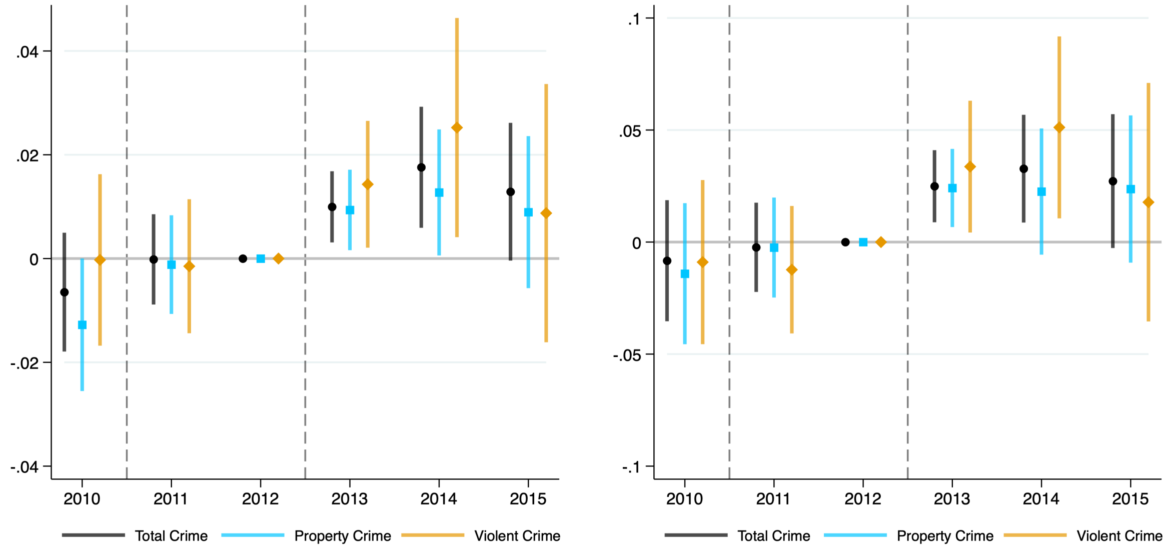
		Crime Categories		**Crime Types**				
	Total	Property Crime	Violent Crime	Theft	Burglary	Criminal Damage and Arson	Robbery	Violence and Sexual Offences
A. Continuous								
Post × Austerity	.0139***	.00641	.0143*	.0158*	-.0029	.0151***	.0172	.0174*
	(.00441)	(.00479)	(.00863)	(.00874)	(.0074)	(.00477)	(.0138)	(.00944)
$1\sigma_{Aus}\times$ Post × Austerity	.0165***	.0076	.017*	.0187*	-.00344	.0179***	.0205	.0206*
	(.00523)	(.00568)	(.0102)	(.0104)	(.00878)	(.00566)	(.0163)	(.0112)
B. Binary								
Post × $\mathbb{1}$[Austerity Impact Above Median]	.0345***	.0223**	.0392**	.0243	.0106	.0318***	.0306	.0476**
	(.00996)	(.011)	(.0178)	(.0177)	(.0176)	(.011)	(.0335)	(.0194)
$\overline{Y}_0$	5.8	3.28	1.33	1.09	.761	.819	.122	1.03
Districts	234	234	234	234	234	234	234	234
Observations	14,040	14,040	14,040	12,870	14,040	12,870	12,840	14,040
Proportion of Total Crime	1	.65	.28	.19	.12	.14	.018	.22

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at district level. The dependent variable is log Crime Rate per 1000 Population in all specifications. The Post variable takes value 1 for 04/2013 onwards, and 0 otherwise. Austerity is the simulated impact of austerity in *£100s* per working age person. Observations are weighted by district-level population. District fixed effects and region-by-year fixed effects are included in all specifications. Additional control variables - all district-level unless otherwise specified - include (Police Force Area-level) police officers per 1000 population, the median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50. Data: District×Month-level Crime Data. 04/2011-03/2016.

[21] We do not provide further support for the crime concentration outcomes, as the alternative CSP-level data series does not allow us to calculate crime concentration in the same way that our main crime data series (which in its raw form is at the street-level) does.

The results from our event study analysis, which we present in Figure 2, highlight a slightly more nuanced story of crime changes. It is the first two years of the austerity reforms that we see the significant increases in crime, with the pattern of treatment effects following an inverse-U shape over time. For instance, relative to the pre-policy year, violent crime in higher austerity-exposed areas increases by 3.3% in the first year, 5.1% in the second year of austerity, and then by 1.8% in the third and final year that we consider in this analysis.

Figure 2: Event Study Graphs for Crime Rate



(a) Continuous Treatment

(b) Binary Treatment

5.1.1 What Types of Offenses are Behind the Rise in Violent Crime?

We turn to our alternative crime series in order to understand what types of offenses are behind the increase in violent crime in areas more exposed to austerity-based cuts. The regression specifications are the same as before, except the key spatial unit is now the CSP, and the temporal unit is quarter.[22]

Table B1 presents our DD estimates for a set nested crime outcomes, where as one moves from the left to the right, one is moving successively to more detailed level of offense. We note here that the estimates presented in the first three columns serve as a cross-validation exercise between our two crime data series. Columns 1, 2 and 3 of Table B1 replicate columns 1, 3 and 8 respectively of Table 2, displaying very similar parameter estimates.

Columns 4 and 5 display separate estimates for Violence (column 4) and Sexual Offences (column 5). We can see from these two columns that it is violence, and not sexual offenses, that rises more in auserity-hit areas in the post period. Columns 6-8 present more detailed results, with estimates from offense-specific regressions. From these three columns (noting the relative rarity of homicides in England and Wales) we see that both crimes classified as "violence with injury" and "violence without injury" both rise in areas harder hit by austerity cuts, the latter more so.

5.1.2 Sensitivity Analyses

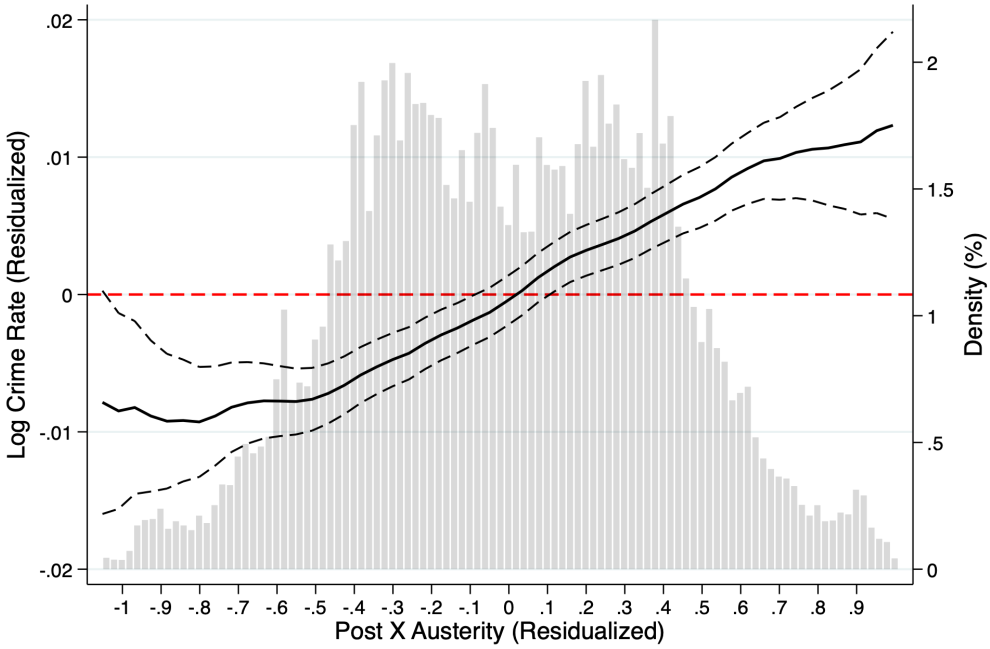
To probe the robustness of our core results, we implement a variety of sensitivity analyses. We start with a series of specification checks, where we: vary the functional form of our dependent variable;

[22]For variables that vary at the district level, including our measure of austerity exposure, we collapse the data from district to CSP level, and take population-weighted averages for the few cases where districts are nested. We use average population in the district for the 5 years prior to the Welfare Reform Act as the population-based weight.

experiment with different specifications of our control variables; explore the importance of district trends and lower spatial levels of area-by-time fixed effects. We outline these sensitivity analyses in Section B.3.1. Figure B2 presents our key estimates across the 8 specifications we consider, and highlights the stability of our results to alternative specifications.

We next consider whether the imposition of a linear functional form on the austerity measure could be driving our estimates. In one sense we know that this is not the case – in Table 2 we estimate a binarized version of the austerity measure, replacing $Austerity_i$ with $\mathbb{1}[Austerity_i \geq median]$. In Section B.3.2, we relax the linear function form assumption, and estimate a non-parametric version of (8) using local linear regression for all key crime types. Section B.3.2 provides details of the procedure. Based on the evidence that we document in Figure B3, we conclude that the linear functional form specified in (8) is not driving the results and is thus appropriate. We show the graph for total crime in Figure 3 below.

Figure 3: The Assumption of the Linearity of the Austerity Term in (8) is Valid



Notes: The log crime rate is plotted against our DD term $Post_t \times Austerity_i$. The plotted values are residuals from regressions on all controls and fixed effects as in Equation (8). The solid black line is the shows the local linear regression of log crime rates on $Post_t \times Austerity_i$. The dashed lines are 95% confidence intervals. In the background is the density of (residualized) $Post_t \times Austerity_i$.

In Section 2.7, we noted that several of the components came in to full effect in 2014/5 fiscal year, whilst two of the largest components did so a year later. In Section B.4 we re-run our main analysis on a restricted two year post-period, instead of the three year post-period we use in the main analysis. Given the temporal patterns that we see in Figure 2 – where we find larger effects in the first two post-Welfare Reform Act years compared to the third – it is not surprising that the coefficient estimates for the baseline DD specifications are slightly larger than (but qualitatively similar to) our main results.

We probe the austerity measure itself based on two concerns. First, given that one of the ten components of the measure incorporates welfare reforms enacted prior to the Welfare Reform Act, yet came into effect in our analysis period, one may be concerned that our austerity measure is not reflecting the Welfare Reform Act precisely enough, even though it provides an accurate measure of the austerity measure impacting households around the country during our sample period. To allay such concerns, we modify our main austerity measure, stripping it of the incapacity benefit reform component, and repeat our key analyses. We discuss this in Section B.5.1 and present the results of our robustness tests in Table B3. Our key results are robust to this recalculation of the austerity measure.

Next, we use an updated version of our main austerity measure, produced by Beatty and Fothergill

(2016) in their follow-up paper to Beatty and Fothergill (2013). The updated measure produced by Beatty and Fothergill (2016) differs from the original in one key way. Instead of being an ex-ante projection of the financial impact of the austerity measures imposed by the Welfare Reform Act, the update is now an ex-post estimate of the impact, accounting for outturn. It is precisely this difference that makes us skeptical about using the updated measure as our main austerity variable: it opens the door to the possibility of reverse causality issues, where the aggregate supply of crime in a district impacts the district claimant count. From this perspective, a slightly less accurate, but pre-(policy-)determined, austerity measure feels like the right choice. That said, the measures are extremely similar: the correlation between the ex-ante and ex-post measures is 0.982. It is therefore not surprising that the estimates presented in Table B4 are very similar to our main results.

5.2 Crime Concentration

The analysis presented in Section 5.1 enables us an understanding of how austerity impacts the level of crime in an area. This is important, but does not paint the full picture of how crime changes. In order to enrich our understanding of the response of crime to a shock to the welfare system generosity, we now turn to consider crime concentration. Two points are worth noting here. First, the ability to consider another dimension of crime – one that measures location of crime within districts, rather than across – is key to developing a full understanding of how crime responds to a policy, in this case welfare reform. Second, it is at this stage that we are able to maximize the potential of our street-level crime data.

Table 3: Austerity Increases the Concentration of Crime in Districts, Notably Property Crime

	(1)	(2)	(3)	(4)	(5)	(6)
	Continuous Treatment			**Binary Treatment**		
		Crime Categories			**Crime Categories**	
	Total	Property Crime	Violent Crime	Total	Property Crime	Violent Crime
Post × Austerity	.000564*	.00102***	.000119	.00121**	.00199***	.000723
	(.00029)	(.000344)	(.000346)	(.000576)	(.000679)	(.00073)
Post × Austerity as Proportion of $\overline{Y}_0$	.00541*	.013***	.00202	.00976**	.0215***	.0104
	(.00278)	(.0044)	(.00589)	(.00466)	(.00733)	(.0105)
$\overline{Y}_0$	.124	.0927	.0697	.124	.0927	.0697
Districts	234	234	234	234	234	234
Observations	1,170	1,170	1,170	1,170	1,170	1,170

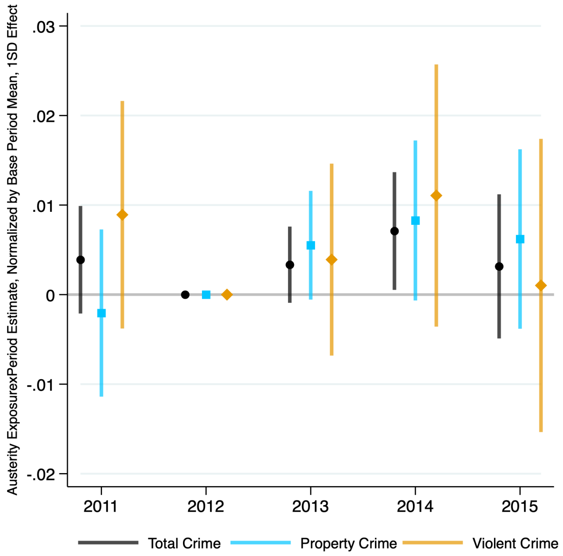
Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at district level. The dependent variable is the Marginal Crime Concentration. The Post variable takes value 1 for 2013 onwards, and 0 otherwise. Austerity is the simulated impact of austerity in £100*s* per working age person. In order to give a better sense of the magnitude of the effects we find, we present the DD terms as proportions of $\overline{Y}_0$. For the continuous treatment specifications, we present the effect of a one standard deviation increase in austerity exposure. Observations are weighted by district-level population. District fixed effects and year fixed effects are included in all specifications. Additional control variables - all district-level unless otherwise specified - include (Police Force Area-level) log one-quarter lagged police officers per 1000 population, the log one-quarter lagged median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

The first piece of evidence we provide on the impact of the welfare reforms on crime concentration can be seen in Table 3. Here we document that the reforms lead to total crime becoming significantly more concentrated. A one standard deviation increase in austerity exposure leads to a 0.5% rise in crime concentration compared to the pre-reform base level. This effect is more pronounced for property crime than for violent crime. The results from the binarized version of the austerity measure tell a similar story. High-austerity exposure areas see a 1% increase in crime concentration relative to the pre-Welfare Reform Act time period, and a 2.2% increase in property crime.[23]

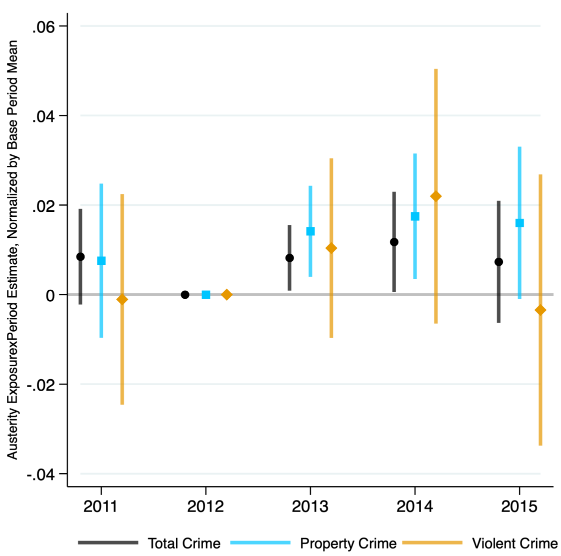
[23] In Section B.5.3 we present a series of sensitivity analyses, where we explore different cutoffs for the concentration measure.

The second piece of evidence we provide on crime concentration comes in the form of event study analysis, which we present in Figure 4 below. What is interesting to note here is that violent crime also appears to become more concentrated in the first two years post-Welfare Reform Act implementation (mirroring what we find in the event study analysis for district crime rates), although this effect is noisily estimated.

Figure 4: Event Study Graphs for Crime Concentration



(a) Continuous Treatment



(b) Binary Treatment

Taking stock of what we have found so far, we see a picture emerging where crime both (i) increases and (ii) becomes more concentrated as a consequence of the cuts implemented by the Welfare Reform Act. Districts more exposed to austerity measures experience a rise in crime, and certain neighborhoods within those districts bear the brunt of these rises. We already know that austerity-exposed districts are ex-ante poorer areas, but in order to trace the full welfare consequences of the austerity measures on crime, we would need to know more about the neighborhoods experiencing the sharp end of the rise in crime concentration. We return to this point in Section 5.4.

A second aspect relates to the paper at the center of the renewed focus on crime concentration: Weisburd (2015). In this paper, based on his Sutherland Address to the American Society of Criminology, Weisburd notes the remarkable consistency of crime concentration across space, and within areas over time, and goes on to label this "the first law of the criminology of place — *the law of crime concentration*" Weisburd (2015, p.151). This consistency of crime concentration is a useful point from which to view the results in Table 3. Although statistically significant, they are somewhat small in magnitude. However, when viewed against the backdrop of the law of crime concentration, it is notable that we find that the austerity measures of the Welfare Reform Act impacted crime concentration. To our knowledge, we present the first evidence of the malleability of crime concentration to policy changes.

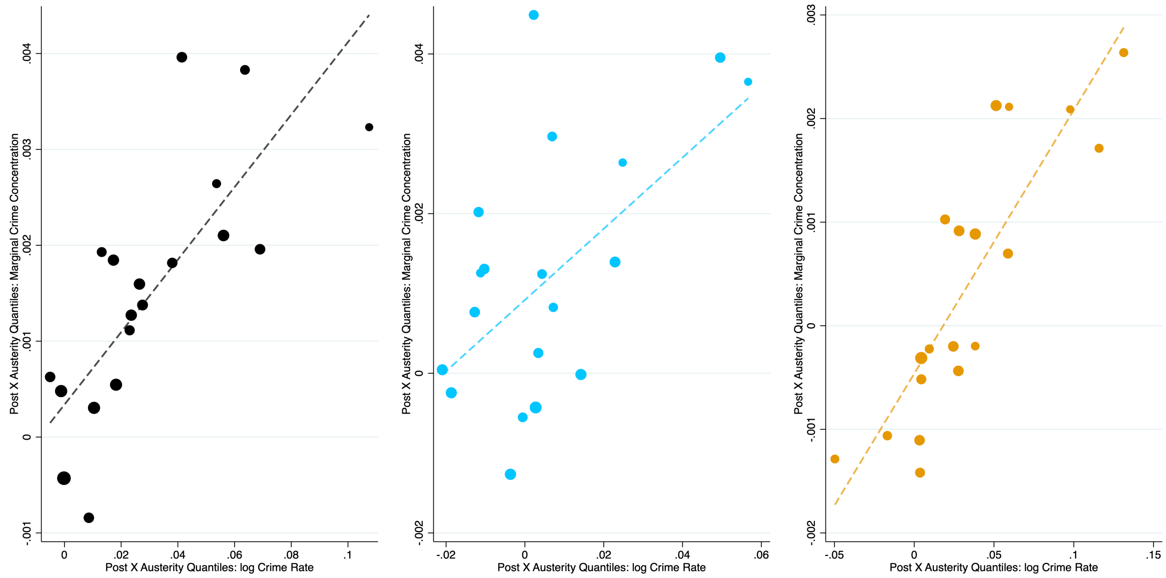
5.3 Combining the two crime measures

We have so far documented that areas with higher exposure to the austerity measures experience: (i) an increase in total crime, due to a rise in violent crime and (ii) an increase in the concentration of crime. In order to understand whether it is the same areas that experience both the rise in crime *and* crime concentration we estimate a variation of Equation 8, where we split our austerity measure into ventiles. This equation takes the form:

$$c_{it} = \sum_{q=1}^{20} \beta_q Post_t \times Austerity\,Ventile_{iq} + X_{it}^{'}\gamma + \pi_{r\times t} + \theta_i + \epsilon_{it} \ , \tag{14}$$

which we estimate for both crime rates and crime concentration. Given our specification of region by time fixed effects, we cannot identify β_1 – the coefficient related to Austerity Ventile 1, and all coefficients are relative to this base category. We scatter plot the respective coefficients from both a crime rates and a crime concentration variant of Equation 14, weighting each point by the inverse combined variance for each ventile. We present these graphs in Figure 5. Districts that experience increases in crime rates also experience increases in crime concentration.

Figure 5: Districts That Experienced Larger Increases in Concentration of Crime Also Experienced Higher Rates of Crime



(a) Total Crime (b) Property Crime (c) Violent Crime

Notes: We estimate $c_{it} = \sum_{q=1}^{20} \beta_q Post_t \times Austerity\,Ventile_{iq} + X'_{it}\gamma + \pi_{r\times t} + \theta_i + \epsilon_{it}$ for both crime rates (CR) and crime concentration (CC). We then scatter plot the resulting austerity coefficients for the two crime outcomes – that is, $\hat{\beta}_2$ – $\hat{\beta}_{20}$ – against one another. We weight each point by the inverse of the combined variance. That is for the point relating to decile q, the weight is $[var(\hat{\beta}_q^{CR}) + var(\hat{\beta}_q^{CC})]^{-1}$.

The message from Figure 5 is clear – districts that experience increases in crime rates are the selfsame districts that also experience increases in crime concentration.

5.4 Ex-ante Deprivation and Neighborhood Crime Changes

Beatty and Fothergill (2013) show clearly that the austerity-imposed welfare reforms of the Conservative-Liberal Democrat government hit areas that were ex-ante poorer.[24] We document above an additional negative shock to more austerity-exposed districts in the form of a rise in crime rates. At the district-level it is unambiguous that the welfare system reforms negatively impacted social welfare, increasing between-district inequality. We also show that the welfare reforms led to an increase in crime concentration i.e. the reforms impacted the within-district distribution of crime. Without knowing which neighborhoods were hit by the increase in crime concentration, we cannot say anything further regarding changes to within-district inequality. It is this point that we focus on in this section, thus completing our understanding of how the Welfare Reform Act affected inequality.

Just prior to our sample period, the Department for Communities and Local Government produced Indeces of Multiple Deprivation (IMD) for 2010. This neighborhood-level index comprises seven different components, which measure different dimensions ("domains") of local deprivation.[25] Once aggregated,

[24]This can clearly be seen in Figure 2 of Beatty and Fothergill (2013).

[25]These domains, along with their contribution weights listed in parentheses are: Income Deprivation Domain (22.5%), Employment Deprivation Domain (22.5%), Health Deprivation and Disability Domain (13.5%), Educa-

the IMD is typically presented as a percentile score of deprivation.

With the domain-level data in hand, we construct an adjusted, four-domain, version of the IMD.[26] We do so, as the income and employment domains relate too closely to our austerity measure, and the crime domain captures our key dependent variable. The correlation between our adjusted measure and the original is 0.951.

Next we return to our street-level data, and aggregate these to the neighborhood-by-year level. We then estimate a DDD regression at the neighborhood level, where we interact our baseline DD with quintiles of the distribution of the adjusted index of baseline deprivation (2010) and present the resulting estimates in Figure 6 below. What the figure shows is that neighborhoods with higher ex-ante deprivation are the areas that experience larger post-policy rises in crime if in high austerity districts. This is particularly the case for the most deprived neighborhoods. Whilst there is a crime-deprivation gradient for both crime categories, the effect is more pronounced for property crime.

Our findings from this neighborhood-level analysis connect closely with our crime concentration results. In Section 5.2 we document that at the district level, crime, and in particular property crime, becomes more concentrated. In our neighborhood-crime analysis, we find that more deprived neighborhoods in high-austerity exposure districts experience the largest increases in crime, notably property crime. That it is the most deprived neighborhoods in districts hardest hit by austerity that experience the largest crime increases paints a very clear picture of how the austerity cuts widened place-based welfare inequality.

5.5 Who drives the impact on crime? An analysis of recidivism data

Having documented that the welfare cuts due to the Welfare Reform Act led to an increase in crime, particularly violent crime, a natural question to ask relates to the source of the increased crimes in high austerity-exposed areas. Are the same group of offenders committing more crimes, or do the welfare reforms instigate an inflow of new individuals into the offender pool? Put another way, is this increase in the crime rate driven primarily by the intensive margin of crime supply, or the extensive margin?

To make progress on this, we estimate our baseline specification on a battery of recidivism outcomes based on our reoffending data. To recap, these data follow district-specific cohorts of previous offenders over a year-long period, recording any new (re-)offenses. The primary measure of interest is the recidivism rate, but we also consider the number of reoffenses per offender (the intensive margin of reoffending relative to the baseline pool of previous offenders), the number of reoffenses per reoffender (the intensive margin of reoffending) and the ratio of reoffenses per reoffender to offenses per offender (in order to get a sense if the intensity of reoffending has increased).

Table 4 below presents the resulting parameter estimates based on Equation (8), along with the proportion that each group represents of the total number of prior offenders. Whether we look at adults or juveniles (ages 10-17), if we split by gender, or we break down the offender pools into age categories, the message is overwhelmingly clear. Recidivism, at least recorded recidivism, is *not* driving the increase in crime.

This is a striking finding, particularly when coupled with the evidence presented in Section 5.1 of rising crime, and in Section 5.2 of an increase in the concentration of crime. The rise in crime, and the rise in crime concentration, in high austerity exposure areas appears to be driven by an increase in the extensive margin of crime supply. That new criminals i.e., the "compliers" to the austerity measures are choosing to commit crime in the *same* areas as existing criminals is a novel insight that we draw from the analyses presented.

tion, Skills and Training Deprivation Domain (13.5%), Barriers to Housing and Services Domain (9.3%), Crime Domain (9.3%) and Living Environment Deprivation Domain (9.3%).

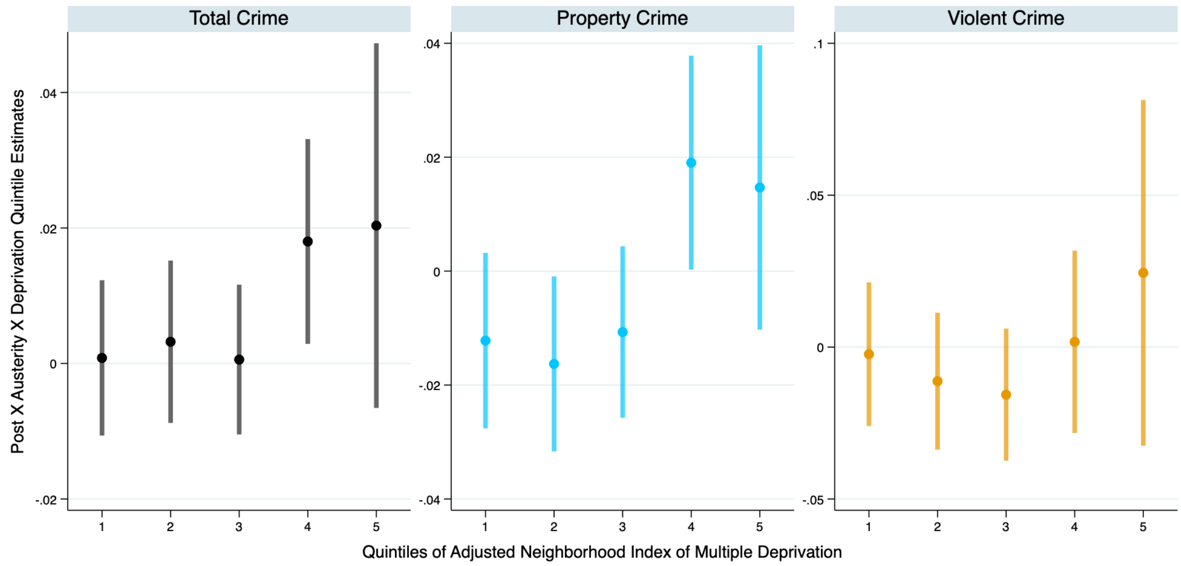
[26] Specifically we use the Health Deprivation and Disability Domain (13.5%), Education, Skills and Training Deprivation Domain (13.5%), Barriers to Housing and Services Domain (9.3%) and Living Environment Deprivation Domain (9.3%), and rescale the weighted combination of these by $1/(0.135 + 0.135 + 0.093 + 0.093)$ to get a consistent level to the original IMD.

Table 4: Austerity has a Near Universal Null Effect on Various Recidivism Measures

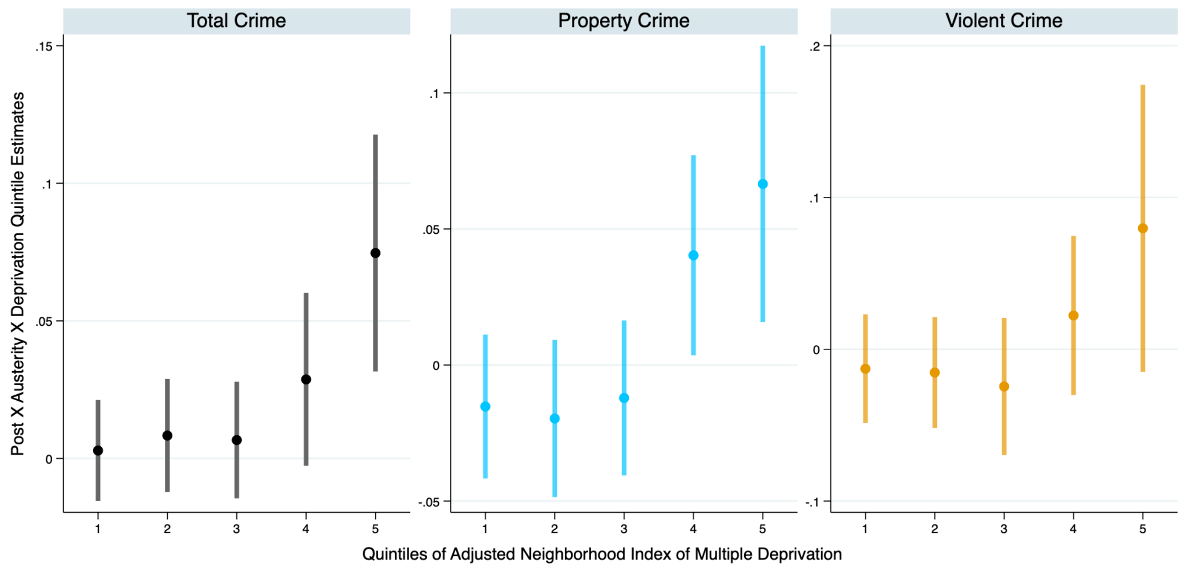
	Recidivism Rate	Reoffences per Offender	Reoffences per Reoffender	Reoffences per Reoffender / Offences per Offender
A. Adults	.0009	.00733	-.00534	-.00109
	(.00152)	(.00904)	(.0198)	(.00157)
$\overline{Y}_0$	.29	.96	3.27	.233
Proportion of Total	.897	.897	.897	.897
B. Juveniles:	-.00091	-.0162	-.0346	-.0272
	(.00365)	(.0247)	(.045)	(.0208)
$\overline{Y}_0$	.404	1.27	3.1	.956
Proportion of Total	.103	.103	.103	.103
C. Gender:				
Female	.00404*	.0197	.00627	-.00032
	(.00211)	(.0139)	(.0492)	(.00761)
$\overline{Y}_0$	.22	.719	3.2	.439
Proportion of Total	.174	.174	.174	.174
Male	.00057	.00276	-.0135	-.00042
	(.00166)	(.00926)	(.0191)	(.00161)
$\overline{Y}_0$	.322	1.06	3.26	.236
Proportion of Total	.826	.826	.826	.826
D. Age Groups:				
10–14	.00348	-.00853	-.0346	-.216**
	(.00653)	(.0574)	(.103)	(.105)
$\overline{Y}_0$	.39	1.26	3.12	2.15
Proportion of Total	.0241	.0241	.0241	.0241
15–17	-.00314	-.0208	-.0316	-.0211
	(.00411)	(.0252)	(.0468)	(.0203)
$\overline{Y}_0$	.408	1.27	3.08	.827
Proportion of Total	.0793	.0793	.0793	.0793
18–20	-.00056	-.0166	-.0464	-.0182**
	(.00284)	(.0144)	(.0333)	(.00914)
$\overline{Y}_0$	.345	1.01	2.89	.492
Proportion of Total	.119	.119	.119	.119
21–24	.00299	-.00751	-.0683**	-.00717
	(.00226)	(.0128)	(.034)	(.00454)
$\overline{Y}_0$	.309	.92	2.95	.345
Proportion of Total	.161	.161	.161	.161
25–29	.00031	.0147	.03	.00211
	(.00256)	(.0162)	(.0361)	(.00335)
$\overline{Y}_0$	.306	1.04	3.35	.257
Proportion of Total	.164	.164	.164	.164
30–34	-.00249	-.0231	-.0678	-.00164
	(.00273)	(.0218)	(.0499)	(.00348)
$\overline{Y}_0$	.313	1.14	3.58	.202
Proportion of Total	.129	.129	.129	.129
35–39	.00287	.0457**	.0813	-.00213
	(.00303)	(.021)	(.0501)	(.00319)
$\overline{Y}_0$	.297	1.06	3.49	.169
Proportion of Total	.0987	.0987	.0987	.0987
40–44	-.00105	.0235	.0898	.00598
	(.0031)	(.0223)	(.0657)	(.00429)
$\overline{Y}_0$	.257	.879	3.33	.166
Proportion of Total	.0839	.0839	.0839	.0839
45–49	.00104	-.00145	-.0392	-.00038
	(.00344)	(.0262)	(.0909)	(.00434)
$\overline{Y}_0$	.222	.755	3.28	.176
Proportion of Total	.0627	.0627	.0627	.0627
50+	-.00024	.00946	.0541	.0133*
	(.00252)	(.0194)	(.0855)	(.0068)
$\overline{Y}_0$	.157	.579	3.52	.259
Proportion of Total	.0785	.0785	.0785	.0785

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at district level. The Post variable takes value 1 for rolling quarters entirely later than 01/2013 onwards, 0 for rolling quarters full before 12/2012, .25, .5 and .75 for the cohorts 07/2012-06/2013, 10/2012-09/2013, and 01/2013-12/2013 respecitively. Austerity is the simulated impact of austerity in *£100s* per working age person. Observations are weighted by district-level population. District fixed effects and region-by-rolling four quarter time fixed effects are included in all specifications. Additional control variables - all district-level unless otherwise specified - include (Police Force Area-level) police officers per 1000 population, the median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

Figure 6: Neighborhood Deprivation and Crime



(a) Continuous Treatment



(b) Binary Treatment

Notes: We estimate $c_{int} = \sum_{q=1}^{5} \beta_q Post_t \times Austerity_i \times IMD\, Quintile_{nq} + X'_{it}\gamma + \pi_{r \times t} + \theta_n + \epsilon_{nit}$. c_{int} is the neighborhood crime rate per 1,000 population. $IMD\, Quintile_{nq}$ are quintiles of the adjusted, four-domain, version of the 2010 IMD. We present the DD terms as proportions of $\overline{Y}_0$. For the continuous treatment specifications, we present the effect of a one standard deviation increase in austerity exposure.

6 Mechanisms

As we document in Table 2, it is violent crime that responds to the welfare cuts, not property crime. A related literature documents that income inequality might affect property and violent crime in different ways. As Kelly (2000) notes, in his work on income inequality and crime, "the pattern of property crime is in line with the predictions of the economic theory of crime. However, when it comes to explaining violent crime, the role of inequality and race are in keeping with strain theory" (Kelly, 2000, p. 530). A body of more recent work provides evidence to suggest that income inequality can impact both property and violent crimes (Fajnzylber et al., 2002; Enamorado et al., 2016; Freedman and Owens, 2016; James and Smith, 2017). Several of these papers appeal to theories outside of the domain of economics in order to rationalize their respective findings.

In this section we consider a variety of potential mechanisms that may help to shed light on the evidence we document in Section 5.

6.1 Police Numbers and Police Effectiveness

As noted in Section 2, not only did the Conservative-Liberal Democrat coalition government implement a series of welfare reforms, they also cut other public services, including a 20% cut to the grant for police funding. This was outlined in the CSR of October 2010, and as one can see in Figure 1, the effect of this was immediately apparent, with police numbers falling steeply. Given the importance that policing plays in impacting crime, one may wonder about changes in policing numbers of police effectiveness in driving our results. The first point to allay such a concern is to note that we account for local policing numbers over time in our vector X_{it}. The second point is to note that policing numbers do not appear to respond to crime very rapidly, at least based on what we see in Figure 1.

To address the point of policing more directly, we provide three additional pieces of evidence, all of which suggest that changes in police numbers or police behavior are not what is driving our results. First, we implement an augmented, DDD, version of the DD specification of (8), where the additional difference dimension relates to policing. We detail the specifics of our approach in Section B.7. We present the results of this analysis in Figure B4. The key lesson we learn from this analysis is that there is no systematic pattern in our estimated treatment effect across different levels of either (i) pre-policy policing levels or (ii) the change in policing levels over our period of analysis.

Second, we create an austerity exposure measure we use throughout this paper at the police force level, and consider the relationship between police numbers and austerity exposure. We find that police numbers do not co-vary with Welfare Reform Act-induced austerity exposure, presenting this evidence in Table B7.

Third, we use data on the clearance rate of crimes at the district-month level and investigate if the police are less effective at clearing crimes in areas more exposed to welfare cuts. We find no substantive evidence that the clearance rate is lower in more austerity-exposed areas. We present the results of this analysis in Table B8. We note that these results additionally assuage concerns that differential reporting of crimes to the police may be driving our results. In the unlikely event that reporting patterns changed in a manner that correlated spatiotemporally with the incidence of austerity cuts, we would expect this to translate into differential clearance rates.

6.2 The Labor Market

We next move to the labor market, noting that individuals may respond to the less generous welfare and benefits system in place from 2013, by changing their behavior in the labor market. A standard job search model would predict that with a fall in benefits leading to a decrease in the utility value of non-employment, individuals would lower their reservation wage in order to increase their job acceptance rate. Table B6 estimates regressions specifications analogous to Equation (8) above, where we consider a battery of labor market outcomes at the district level. We do not find support for this labor market response hypothesis. There is no change in the hourly wage nor in the intensive labor supply margin. We thus conclude that labor market responses are not driving this pattern.

6.3 Returning to the Psychological Models of Crime

Given that our core analysis points to a response in violent crime, and not property crime, to austerity exposure, we believe it is fruitful to look beyond standard economic models of crime to help explain our findings. The canonical Becker model provides suitable settings for a rational agent soberly weighing up the costs and benefits of a property crime, but makes little sense when considering violent crime. This is where the psychological models – which link worsening financial situation of welfare recipients to stress, frustration, or concerns of social status – may provide insights into the channel through which the austerity shock impacts violence.

To make progress on this front, we use our clinic-level prescribing data to measure changes in prescribing levels of specific categories of drugs, which we use to proxy elements of the psychological models we present in Section 3.2. Specifically these are (i.) Hypnotics (prescribed for sleep problems), (ii.) Anxiolytics (prescribed for anxiety), (iii.) Antidepressant Drugs, and (iv.) drugs for Alcohol Dependence. The last category of drugs is particularly interesting, as alcohol misuse may reflect not only increased negative mindset (frustration, strain among others), but may itself act as a accelerant for violence (Nutt et al., 2010; Carpenter and Dobkin, 2010, 2015).

We first implement a DD specification for the percentage share of total clinic prescribing accounted for by each drugs category of interest:

$$\%\, share^{d}_{ct} = \beta Post_t \times Austerity_i + \pi_{r\times t} + \theta_c + \epsilon_{ct} \ , \tag{15}$$

where the outcome variable is the percentage of prescription items/quantity for a given drug type with reference to all prescription items for practice c in month t, the practice fixed effect θ_c captures all time-invariant unobservables (which will account for the general physical and mental health of the local population), and the region-by-year-by-month fixed effects $\pi_{r\times t}$ will absorb any regional-specific time shocks in prescribing behavior. Given we are estimating a DD specification, we impose the additional restriction that each clinic must be seen at least once in the pre-period and once in the post-period. This leads to us dropping a small number of clinics that close in the pre-period or open in the post-period. We present these results in Table 5.

The resulting estimates point to no response to austerity exposure in the prescribing of medication for sleep problems or anxiety. We document slight increases in the prescribing of antidepressant drugs (the key statistic to look at here is the DD estimate as a proportion of the pre-period prescribing level, expressed as a one standard deviation increase in austerity – $(1\sigma_{Aust} \times Post \times Austerity)/\overline{\mathrm{Y}}_0$ – which is a very small effect). The key finding from this exercise is the evidence we document in Columns 7 and 8. Prescribing of drugs for alcohol dependence increases substantially in response to austerity exposure. A 1 standard deviation increase in austerity exposure leads to a 4% increase from baseline in the number of prescriptions for alcohol dependence, and an 8.5% increase in the quantity of prescription drugs in this category.

We next exploit the spatial richness of the PLP data, and estimate a DDD variant of Equation 15, where the third difference is the quartile of neighborhood deprivation. The idea behind this specification is to drill down within districts to explore the extent to which prescribing behavior is different across more and less deprived neighborhoods. The DDD specification takes the form:

$$\%\, share^{d}_{cnit} = \sum_{q=1}^{4} \beta_q Post_t \times Austerity_i \times Deprivation\, Quartile^{q}_{n} + \pi_{r\times t} + \theta_c + \epsilon_{cnit} \ , \tag{16}$$

We present the results in graphical form in Figure 7. We first note that the DD analysis misses important lower-level heterogeneity – we find increases in both hypnotic and anxiolytic prescribing in the most deprived neighborhoods in districts with higher austerity exposure. For each drug category, we present in the title bar of the figure the p-value of a test that $\beta_1 = \beta_4$ – that is we test the null hypothesis that the DDD coefficients for the top and bottom quartile of neighborhood deprivation are equally sized. These p-values indicate a significant difference in austerity exposure-induced prescribing behavior between the top vs the bottom quartile of neighborhood deprivation for both hypnotic and anxiolytic prescribing. The DDD estimates also point to a substantial within-district deprivation gradient in alcohol dependence drug prescribing.

The evidence we provide in section highlights the possibility of worsening mental health and increasing misuse of alcohol in response to the welfare cuts (Table 5), notably in more deprived areas (Figure 7).

Table 5: Austerity Exposure Leads to an Increase in Alcohol Dependence Drug Prescribing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hypnotics		**Anxiolytics**		**Antidepressant Drugs**		**Alcohol Dependence**	
	Percent Items	Percent Quantity	Percent Items	Percent Quantity	Percent Items	Percent Quantity	Percent Items	Percent Quantity
Post × Austerity	-.00253	.00118	–9.2e-05	.00179	.0254***	.00568*	.00059**	.00124***
	(.00241)	(.00092)	(.00199)	(.00119)	(.00404)	(.00308)	(.00026)	(.00021)
$\overline{\text{Y}}_0$	1.02	.338	.673	.289	4.95	2.37	.0174	.0173
σ_{Aust}	1.19	1.19	1.19	1.19	1.19	1.19	1.19	1.19
$(1\sigma_{Aust}$ × Post × Austerity) / $\overline{\text{Y}}_0$	-.00294	.00414	-.00016	.00734	.00609***	.00285*	.0403**	.0851***
	(.0028)	(.00322)	(.00351)	(.0049)	(.00097)	(.00155)	(.0178)	(.0144)
Practices	8,126	8,126	8,126	8,126	8,126	8,126	8,119	8,119
Observations	454,115	454,114	454,115	454,114	454,115	454,114	400,125	400,124

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at practice level. The dependent variable is the percentage of prescription items for a given drug type with reference to all prescription items for that practice-month in odd columns, and the percentage of prescription quantity for a given drug type with reference to all prescription quantities for that practice-month in even columns. The Post variable takes value 1 for 04/2013 onwards, and 0 otherwise. Austerity is the simulated impact of austerity in *£100s* per working age person. Practice fixed effects and region-by-year-by-month fixed effects are included in all specifications. Data used: Practice level prescribing data, fiscal years 2011-2015.

This evidence is more compatible with frustration or psychological strain acting as mediating channels through which the welfare cuts led to increased violent crime rates. The evidence we provide regarding an increase in alcohol misuse in response to austerity exposure is consistent with existing work documenting the social harms of alcohol (Nutt et al., 2010; Carpenter and Dobkin, 2010, 2015). We believe that our evidence we document here additionally underscores the value of looking beyond the standard rational agent model of crime when aiming to understand the distinct impact of policies on different types of crime.

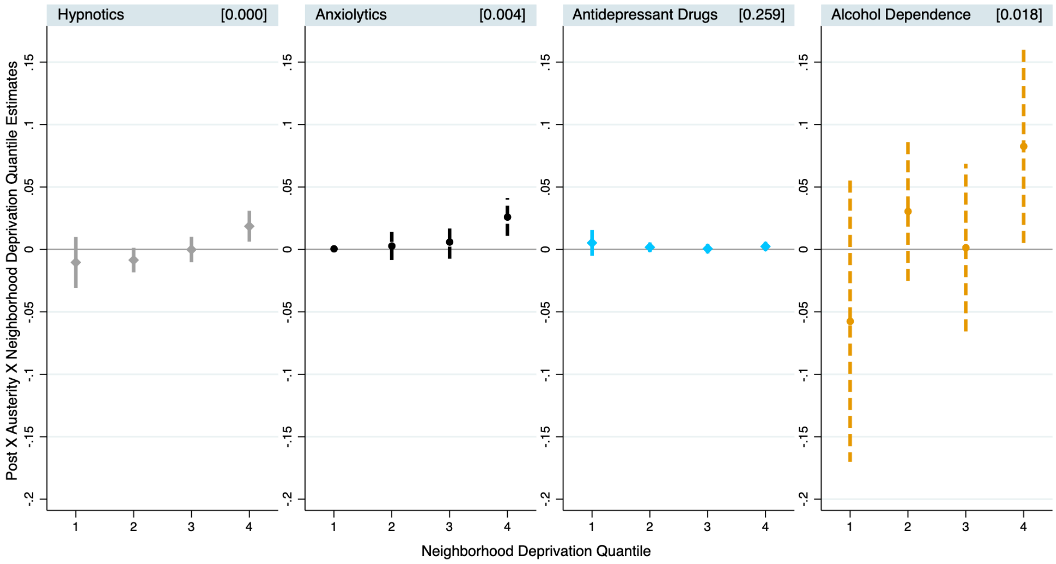
7 The Implied Welfare Loss due to the Reform

The evidence provided so far shows an increase in crime in districts exposed to larger austerity-induced cuts, as well as increases in the concentration of crime that occurs predominantly in more deprived areas. These findings suggest negative welfare effects for areas exposed to the cuts, both across and within districts. In this section, we aim to quantify the welfare implications of the Welfare Reform Act. To do so, we use the insights of Rosen (1974), and specify a hedonic house price model.[27] This approach enables us to estimate the total welfare effects of the reform – as measured by the house prices changes due to the policy.[28]

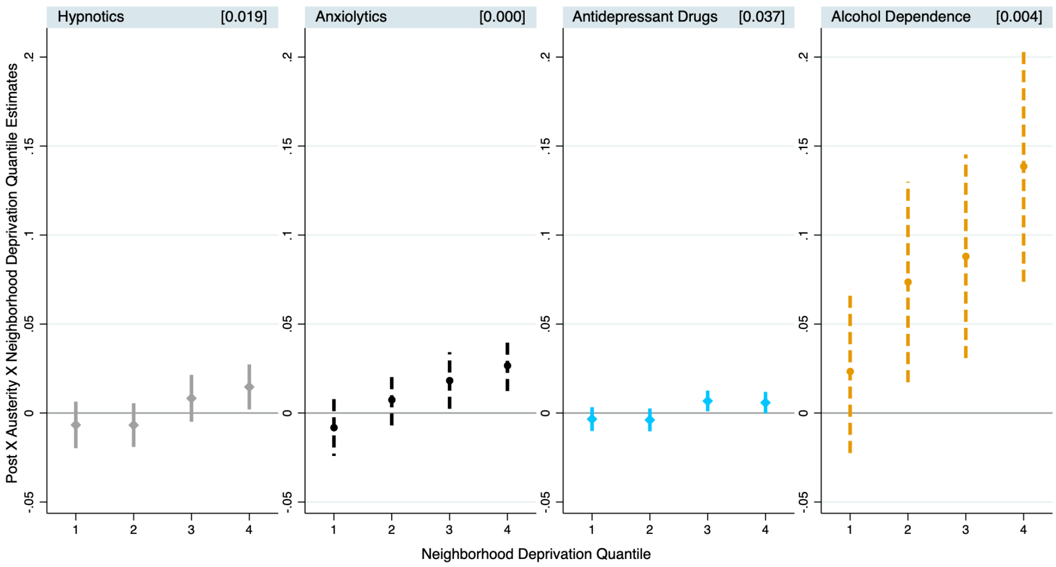
[27] The hedonic house price model is widely used to quantify the social welfare consequences of neighborhood characteristics, including crime (Gibbons, 2004; Linden and Rockoff, 2008; Adda et al., 2014), schools (Black, 1999; Gibbons and Machin, 2003) and pollution (Davis, 2004; Chay and Greenstone, 2005).

[28] The following approach, which uses house prices in order to capitalize the non-market impacts of the Welfare Reform Act, is likely to underestimate the true societal cost as it ignores the cost borne by areas where residents live predominantly in either social housing or rented accommodation, both of which the house price analysis does not incorporate. In addition, those most impacted by the austerity program are likely to be under-represented in the house purchasing subset of the population, which again points to our approach providing a lower bound of the cost of the policy.

Figure 7: The DDD Evidence Highlights That Deprived Neighborhoods in Higher Austerity Exposure Districts see the Largest Increase in Prescribing For Alcohol Dependence and Other Conditions



(a) Percent of Prescription Items



(b) Percent of Prescription Quantities

Notes: Standard errors are clustered at practice level. The p-Value of a test of the DDD term for Q1 vs Q4 is presented in brackets in the title for each sub-graph. The dependent variable is the percentage of prescription items for a given drug type with reference to all prescription items for that practice-month in Figure 7a , and the percentage of prescription quantity for a given drug type with reference to all prescription quantities for that practice-month in Figure 7b. The Post variable takes value 1 for 04/2013 onwards, and 0 otherwise. Austerity is the simulated impact of austerity in £100s per working age person. Practice fixed effects and region-by-year-by-month fixed effects are included in all specifications. Data used: Practice level prescribing data, fiscal years 2011-2015.

7.1 Empirical Specification

The first step to empirically quantify the welfare effects at the district level is to estimate a property-type-specific difference-in-differences house price regression of the form:

$$Price_{indrt} = \beta_p Post_t \times Austerity_d + \sum_{r=1}^{R} Region_r \times Post_t \times X_i^{'}\gamma_p + \pi_{p,r\times t} + \theta_{p,n} + \epsilon_{indrt} \ , \qquad (17)$$

for p = 1, ..., 4 and where $Price_{indrt}$ is the log house price of house i, in neighborhood n, in district d, in region r, sold in period t (measured at the month-level). In order to be internally consistent, we consider the same subset of urban districts used in the first part of our analysis. β_p is the key treatment effect parameter, and captures the district-level impact of the welfare cuts. X_i is a vector of property characteristics including dummies for new-build and leasehold, deciles of floor area of the property and number of habitable rooms categories. $\pi_{p,r\times t}$ captures month-by-year regional shocks to house prices, $\theta_{p,n}$ is a neighborhood fixed effect and ϵ_{indrt} is an error term that we cluster at the district level. This choice of clustering level is informed by the recent work of Abadie et al. (2022).

In order to reconcile the welfare analysis more closely with our focus in this paper on crime, we also consider a triple difference (DDD) specification. This allows us to investigate whether there are within-district differences in the main treatment effect that are driven by the distribution of crime. This analysis maps to the within-district findings we document in the latter part of Section 5 and in Section 5.4. The equation we estimate takes the form:

$$\begin{aligned} Price_{indrt} = & \sum_{q=2}^{4} \alpha_q Post_t \times Crime_0\, Quartile_{nq} \\ & + \beta_{p,1} Post_t \times Austerity_d + \sum_{q=2}^{4} \beta_{p,q} Post_t \times Austerity_d \times Crime_0\, Quartile_{nq} \\ & + \sum_{r=1}^{R}\sum_{q=1}^{4} Region_r \times Crime_0\, Quartile_{nq} \times Post_t \times X_i^{'}\gamma_p + \pi_{p,r\times t} + \theta_{p,n} + \epsilon_{indrt} \ , \end{aligned} \qquad (18)$$

for p = 1, ..., 4 and where the triple difference parameters capture the extent to which prices are differentially impacted by the welfare reforms depending on the pre-policy level of neighborhood crime. We cluster ϵ_{indrt} at the district-quartile level, again to reflect the level of variation of the key treatment variable. Other than the additional terms related to the third difference, all other terms in Equation 18 are the same as in Equation 17.

There are three aspects of the hedonic house prices regressions above that are worth highlighting. First, we interact the vector of housing characteristics, X_i, with region dummies in order to respect the "law of one price function" (Bishop et al., 2020). This allows the valuation of key property characteristics to vary across regional markets.

Secondly, we allow the coefficients on all housing characteristics to differ in the pre and post periods, thereby allowing the hedonic price function to shift post-policy. We do so in order to avoid conflation bias (Kuminoff and Pope, 2014; Banzhaf, 2021). Given this flexibility, the regression specifications in (17) and (18) are, in the nomenclature of Kuminoff et al. (2010), generalized DD and generalized DDD estimators respectively. As Kuminoff et al. (2010) note: "the generalized DID estimator appears to be the best suited to hedonic estimation in panel data. The interactions between time dummies and housing characteristics control for changes in the shape of the equilibrium price function over time; the spatial fixed effects control for omitted variables in each time period".

Finally, the recent work by Banzhaf (2021) shows that we are able to use a difference-in-differences approach with a hedonic house price model in order to study welfare. Our generalized DD and DDD models enable us to estimate a lower bound on policy-induced (general equilibrium) welfare changes (Banzhaf, 2021).

We present the results from this analysis of house prices in Table B9 and Table B10.

7.2 Quantifying the Total Welfare Loss

In order to expand upon the welfare implications of the estimates in Table B9 and Table B10, we follow the approach taken by Adda et al. (2014).[29]. This approach takes the estimates of our key DD and DDD parameters as inputs into formulae that detail the implied welfare loss of the policy:

$$Loss^{DD} = \sum_{d=1}^{D} \sum_{p=1}^{4} \hat{\beta}_p^{DD} \times Austerity_d \times \overline{Price}_{0,pd} \times quantity_{1,pd} \tag{19}$$

$$Loss^{DDD} = \sum_{d=1}^{D} \sum_{q=1}^{4} \sum_{p=1}^{4} \hat{\tau}_{pq}^{DDD} \times Austerity_d \times \overline{Price}_{0,pqd} \times quantity_{1,pqd} \ , \tag{20}$$

where $\hat{\tau}_{pq} = \hat{\beta}_{p1}$ for $q = 1$, $\hat{\tau}_{pq} = \hat{\beta}_1 + \hat{\beta}_{pq}$ otherwise, p denotes property type, d the district and q the crime quartile. In addition, $\overline{price}_{0,pd}$ and $\overline{price}_{0,pqd}$ are pre-reform mean prices for each property type-district cell and property type-district-crime quartile cell respectively, and $quantity_{1,pd}$ and $quantity_{1,pqd}$ are the post-reform quantity of housing in the same cell configurations.

We use two different inputs for our measure of quantity. The first is a flow-based measure – for each cell, we calculate the number of housing transactions in the post period. We take this approach as an ultra-conservative lower bound for the welfare loss of the policy, as this method uses *only* the properties that are sold to calculate the austerity-associated penalty. The second approach – our preferred estimate – is stock-based. For this estimate, we obtain data on the (private sector) stock of housing for each district-year for England and Wales separately, and take the average over the three post-reform years.[30] We assume the proportion of sales by property type is representative of the stock of housing, and for each district and district-quartile cell, we calculate the relevant stock of housing, as the sales-based proportion of the total stock. This is the same approach taken by Adda et al. (2014) to calculate property type-specific housing stocks. We elaborate on this in Section B.10.

With all the necessary components in hand, we are able to calculate the total implied welfare loss of the Welfare Reform Act. Table 6 presents these losses (in £billions) for each of the main specifications, broken down by property type. Column 5 presents the total welfare loss.

Taking the total crime-based DDD estimates as a benchmark, we see that over the three years after the reform, the lower bound estimate of welfare loss is £12.1bn, and our preferred stock-based measure implies a welfare loss of £92.8bn. Whichever estimate one chooses here, the resounding conclusion is that the welfare loss of the austerity reform package is large.

To put these welfare losses in perspective, it is useful to compare to the savings made due to the austerity measures. It is worth noting here that our welfare loss estimates are based *only* on the 234 urban districts in England and Wales. The cost savings noted below relate to all 348 districts. Based on 2012 population estimates from the Office for National Statistics, the 234 urban districts account for 77% of the population in England and Wales. Thus, if we assume stability of the parameter estimates across urban and rural districts, we can rescale our welfare loss estimates by a factor of 1.3 (i.e. 1/.77) in order to make them nationally representative.

According to Beatty and Fothergill (2016), by the end of March 2016 (which coincides with the end of our sample period) the reforms associated with the Welfare Reform Act amounted to a saving to the government of £14.49bn per year, or £43.47bn for the three post-reform years. Using our preferred

[29] The reason why we use property type-specific regression specifications is due to the fact that both the average prices, and the quantities (measured as either stocks or flows), of the different property types differ considerably both at the national and, more importantly for us, the district level.

[30] Data were obtained at https://opendatacommunities.org/data/housing-market/dwelling-stock/tenure for England and https://statswales.gov.wales/Catalogue/Housing/Dwelling-Stock-Estimates/dwellingstockestimates-by-localauthority-tenure for Wales

Table 6: Irrespective of the Specification, the Welfare Losses due to the Reforms are Sizable

	(1)	(2)	(3)	(4)	(5)
	Total Welfare Loss (£Billions) for Urban Districts				
	Loss Based on Property Type:				
A. DD	Detached	Semi-Detached	Terraced	Flats	Total
Sales-Based	–2.284	–3.491	–3.774	–0.392	–9.941
	[–4.4, -0.2]	[–5.6, -1.4]	[–6.4, -1.2]	[–4.1, 3.3]	[–20.5, 0.6]
Stock-Based	–17.108	–27.323	–28.882	–2.965	–76.277
	[–32.9, -1.3]	[–43.6, -11.1]	[–48.8, -9.0]	[–31.2, 25.2]	[–156.4, 3.9]
B. DDD					
Total Crime					
Sales-Based	–2.466	–4.122	–4.428	–1.077	–12.093
	[–5.0, 0.1]	[–6.4, -1.8]	[–7.3, -1.6]	[–5.3, 3.2]	[–24.1, -0.1]
Stock-Based	–18.458	–32.260	–33.916	–8.123	–92.757
	[–37.5, 0.6]	[–50.2, -14.3]	[–56.0, -11.9]	[–40.2, 24.0]	[–183.9, -1.6]
Property Crime					
Sales-Based	–2.441	–4.041	–4.300	–0.971	–11.753
	[–5.0, 0.1]	[–6.4, -1.7]	[–7.2, -1.4]	[–5.1, 3.2]	[–23.7, 0.2]
Stock-Based	–18.233	–31.648	–32.940	–7.381	–90.202
	[–37.3, 0.9]	[–49.9, -13.4]	[–55.3, -10.6]	[–38.7, 23.9]	[–181.3, 0.9]
Violent Crime					
Sales-Based	–2.492	–4.159	–4.404	–0.967	–12.023
	[–5.1, 0.1]	[–6.4, -1.9]	[–7.3, -1.5]	[–5.3, 3.4]	[–24.2, 0.1]
Stock-Based	–18.669	–32.551	–33.738	–7.234	–92.192
	[–38.2, 0.9]	[–50.4, -14.7]	[–55.7, -11.8]	[–40.2, 25.7]	[–184.6, 0.2]

Notes: 95% confidence intervals of the welfare loss are given in square brackets below the main welfare loss estimate.

welfare effect estimate of -£92.8bn, we conclude that the public suffer welfare losses that exceed the gains made to government coffers based on these reforms. The net loss would be even greater if we use the rescaled loss estimate for all of England and Wales (-£119.8bn).

8 Conclusion

In this work, we empirically explore for the first time the crime consequences of the flagship austerity policy implemented in the early 2010s – the Welfare Reform Act. We document that these welfare reforms increased both the level and the concentration of crime. We note that ex-ante poorer districts were more exposed to the sharp end of these benefit cuts, thus at a district level, the Welfare Reform Act imposed both a direct negative consequence, and as we find, an additional indirect negative effect of rising crime. We see this inequality-worsening effect mirrored at the neighborhood level, where ex-ante poorer areas saw the largest rises in crime over this period. Our evidence suggests that it is these neighborhoods that lead to the increased concentration of crime. Our final main finding – that it is not existing offenders driving this crime rise – points to a further negative consequence of austerity of increasing the pool of those committing crime. Although not an absorbing state, committing a crime for the first time today will likely have future negative consequences on the lives of new offenders in the future, even in absence of being apprehended, thus casting a longer, darker shadow of austerity on the future.

In exploring potential mechanisms behind our findings, we find no support for policing or the labor market as the core forces mediating the effects of the austerity program on crime. Rather, we document evidence consistent with inhabitants' worsening mental health and increasing misuse of alcohol in response to the welfare cuts, with more pronounced effects in more deprived neighborhoods. This evidence is more compatible with frustration or psychological strain acting as mediating channels through which the welfare cuts led to increased violent crime rates. Our work here underscores the benefit of looking beyond the standard economic model of crime.

Guided by a hedonic house price model, we provide a financial quantification of the impact of the reform by calculating the welfare effects implied by the package austerity-induced welfare reforms. We document large welfare losses due to the policy, which for our preferred specification far exceed the savings made due to benefits cuts. Viewed through this lens, the policy cost significantly more to the public than it saved to the government.

Our results carry two compelling policy implications. First, by affecting crime, we demonstrate that austerity measures cause a negative externality on society – crime – that goes beyond their direct, well-documented financial implications. It is of utmost importance that policy-making takes into account these adverse spillovers effect when contemplating welfare cuts since failing to do so would – at the very least – underestimate the true cost of austerity borne by the society. Second, the finding that areas highly impacted by austerity are those experiencing a surge in crime levels and concentration provides important insight for crime prevention, as it suggests that the planning of resources devoted to crime deterrence (e.g. police strength) should take into account the unequal spatial distribution of crime effects and possibly consider ad-hoc resource allocations to more affected areas.

It is important to note that the UK's austerity program was not a singular event – the majority of OECD countries implemented fiscal consolidation policies in the wake of the Great Recession, typically doing so with fiscal austerity measures as opposed to tax increases (OECD, 2012). Hence, although the focus of this study is on England and Wales, the relevance of our work linking austerity measures to changes in crime extends beyond these countries' borders.

In their work on the unequal exposure of different parts of the country to the welfare reforms, Beatty and Fothergill (2013) note "As a general rule, the most deprived local authorities across Britain are hit hardest. The loss of benefit income, which is often large, will have knock-on consequences for local spending and thus for local employment, which will in turn add a further twist to the downward spiral." We add an extra dimension of outcomes to the list of drivers of this downward spiral: crime.

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Appendix

A Pre-Policy Placebo Analysis

In this section, we run placebo versions of our baseline specifications – Equations (8) and (9). Here the $Post_t$ term takes value zero for the first pre-policy year, and one for the second pre-policy year. We control for the same variables, and include the same fixed effects. The aim of this section is to check for pre-trends. The key assumption of the DD model is one of parallel trends, hence any significant coefficients here is a warning that this assumption is not met.

It is worth noting that the estimated coefficients are often an order of magnitude smaller in the placebo table e.g., Table A1 compared to the main estimates (Table 2). Thus the lack of significance is not merely a power issue.

A.1 Crime Rate

We implement placebo DD regressions for crime rate specifications using both our primary and secondary crime data series. Both data sources point towards the same finding – there is no pre-trend in crime rates across areas with different austerity exposure. In addition, using our secondary crime data series, which extends further back in time, we provide evidence of the raw trends in crime rates across high and low austerity exposure areas.

A.1.1 Placebo DD Regressions – Primary Data Series

Table A1: The Placebo DD Specifications Show no Evidence of a Pre-Trend for Crime Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Crime Categories		**Crime Types**				
	Total	Property Crime	Violent Crime	Theft	Burglary	Criminal Damage and Arson	Robbery	Violence and Sexual Offences
Continuous Treatment								
Post × Austerity	8.2e-05	-.00418	.00831	.00457	.00949	-.00521	-.00264	.0148
	(.00525)	(.0115)	(.00888)	(.00767)	(.00941)	(.00604)	(.0156)	(.00932)
Binary Treatment								
Post × 𝟙[Austerity Impact Above Median]	-.00421	-.00518	.0187	-.0123	.0247	-.0129	-.00232	.0319
	(.0114)	(.0236)	(.0181)	(.0167)	(.0225)	(.0141)	(.0332)	(.0197)
Districts	234	234	234	234	234	234	234	234
Observations	5,616	5,616	5,615	3,744	5,616	3,744	5,128	5,615
Proportion of Total Crime	1	.66	.23	.19	.13	.15	.021	.17

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at district level. The dependent variable is log Crime Rate per 1000 Population in all specifications. Only 2011 and 2012 data is used for the placebo analysis. The Placebo variable Post takes the value 1 for all observations in 2012, and 0 for all in 2011. Austerity is the simulated impact of austerity in *£100s* per working age person. Observations are weighted by district-level population. District fixed effects and region-by-month-by-year fixed effects are included in all specifications. Additional control variables - all district-level unless otherwise specified - include (Police Force Area-level) police officers per 1000 population, the median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

A.1.2 Placebo DD Regressions – Secondary Data Series

In order to further probe the parallel trends assumption for crime rates, we use an alternative data source to run another set of placebo regressions. This alternative data is more coarse, both spatially and temporally. The spatial unit is no longer the district, but rather the Community Safety Partnership

(CSP) level. The vast majority of CSPs are also districts, whilst 14 out of the total of 315 CSPs in England and Wales are composed of multiple districts. The temporal resolution is the quarter, not the month. The advantage of this data is that it contains crime information that extends further back in time than our main data.[31]

The results of the placebo regressions over an extended pre-period confirm the core findings documented in Section A.1.1 – there is no evidence of a violation of the parallel trends assumption for either the continuous or binary treatment specifications.

Table A2: The Alternative Data DD Specifications Also Show no Evidence of a Pre-Trend

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Crime Categories		**Crime Types**			
	Total	Property Crime	Violent Crime	Theft and Burglary	Criminal Damage and Arson	Robbery	Violence and Sexual Offences
Continuous Treatment							
Post × Austerity	.00267	.00648	-.00756	.00957	.00725	.00343	-.012
	(.00494)	(.00538)	(.00701)	(.00612)	(.00498)	(.0167)	(.00767)
Binary Treatment							
Post × $\mathbb{1}$[Austerity Impact Above Median]	-.00671	-.00157	-.0194	-.0007	.00834	.00376	-.0263*
	(.00941)	(.0112)	(.0148)	(.0125)	(.0107)	(.0353)	(.0158)
Community Safety Partnerships	226	226	226	226	226	226	226
Observations	3,616	3,616	3,616	3,616	3,616	3,592	3,616
Proportion of Total Crime	1	.71	.22	.51	.16	.02	.17

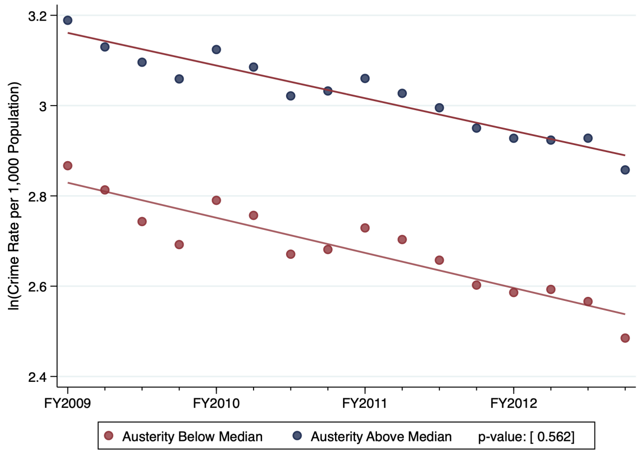
Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at CSP level. The dependent variable is log Crime Rate per 1000 Population in all specifications. The sample period for the placebo analysis is the fiscal years of 2009 to 2012. The placebo variable Post takes the value 1 for all observations in 2011 and 2012, and 0 for previous years. Austerity is the simulated impact of austerity in £100*s* per working age person. Observations are weighted by CSP-level population. CSP fixed effects and region-by-quarter-by-year fixed effects are included in all specifications. Additional control variables - all CSP-level unless otherwise specified - include (Police Force Area-level) police officers per 1000 population, the median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

[31] The CSP-by-quarter crime data extends back to April 2002, however in order to both i.) avoid issues with changing spatial resolutions for our key control variables and ii.) avoid conflating our pre-trends analysis with the worst of the financial crisis, we use a data series that extends back to April 2009.

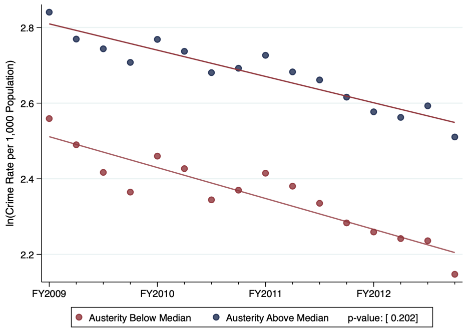
A.1.3 Graphical Evidence of Parallel Trends – CSP Crime Data Series

Figure A1 shows the pre-trends in unconditional crime outcomes for the fiscal years of 2009-2012, using the binarized measure of austerity exposure. The p-value presented in the legend of each graph is based on a test of equality of trends in the pooled data. The large p-values reinforce the visual patterns, confirming that the trends in crime rates between treatment and control areas are indeed parallel in the run-up to the policy change in 2013.

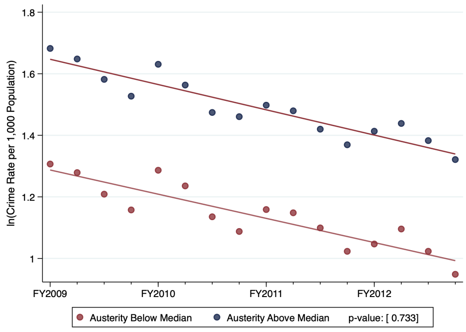
Figure A1: A Visual Inspection of the Raw Data Strongly Suggests That Trends are Parallel



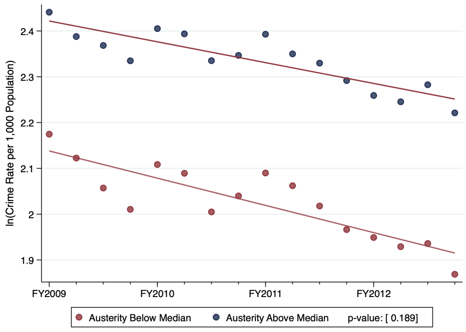
(a) Total Crime



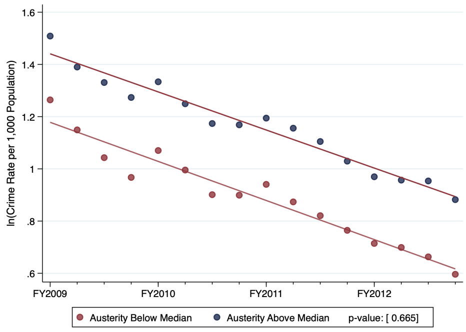
(b) Property Crime Category



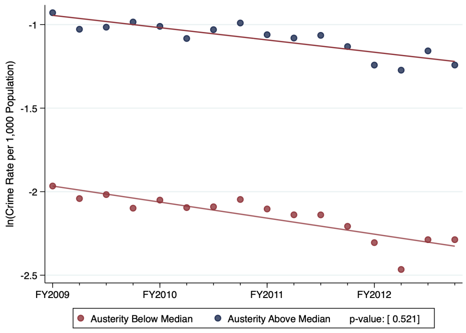
(c) Violent Crime Category



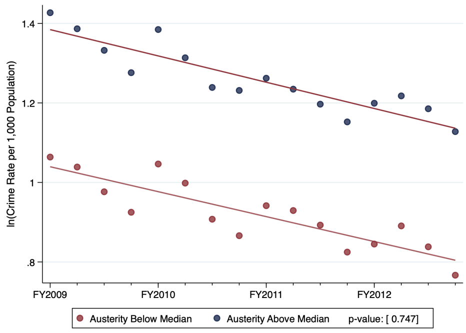
(d) Theft and Burglary



(e) Criminal Damage and Arson



(f) Robbery



(g) Violence and Sexual offenses

Notes: The p-value presented in the legend of each graph is based on a test of equality of trends between high and low austerity-exposure using pooled data. Source: CSP-level crime data, FY2009-FY2012

A.1.4 Honest Difference-in-Differences – Rambachan and Roth (2023)

Finally, we implement the honest difference-in-differences approach of Rambachan and Roth (2023), in order to create worst-case treatment effect bounds for potential violations of the parallel trends assumption, based on pre-trends. In order to operationalize this approach, we use data for fiscal years 2009-2015, and create 3 periods: 1. An initial period of 2009-2010 that is prior to the pre-period used in the main analysis, 2. the pre-period of 2011-2012 and 3. the post-period of 2013-2015. We then implement a continuous treatment and binary treatment version of our core DD model, but based on the extended data and a 3 period approach, as follows:

$$c_{it} = \sum_{j=1,\neq 2}^{3} \beta_j Period_j \times Austerity_i + X_{it}^{'}\gamma + \pi_{r\times t} + \theta_i + \epsilon_{it} \tag{21}$$

$$c_{it} = \sum_{j=1,\neq 2}^{3} \beta_j Period_j \times \mathbb{1}[Austerity_i \geq median] + X_{it}^{'}\gamma + \pi_{r\times t} + \theta_i + \epsilon_{it} \ , \tag{22}$$

The coefficients presented in Table A3 below, and accompanying variance-covariance matrices are the required inputs into the R package (HonestDiD) that implements the Rambachan and Roth (2023) approach.

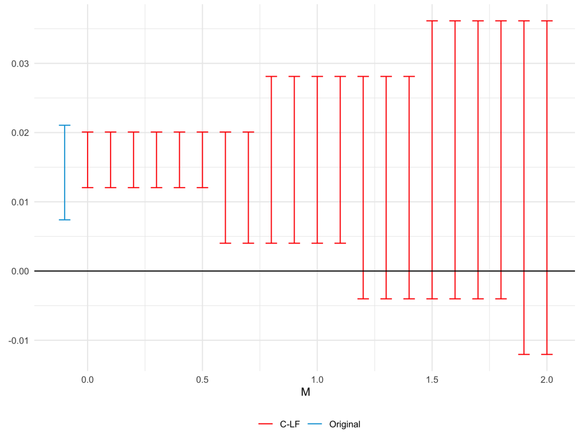
Table A3: The Inputs For the Honest DD Approach Highlight The Large Ratio Between Placebo and Actual Treatment Effects From a Pooled Estimation

	Continuous Treatment			**Binary Treatment**		
	Total Crime	Property Crime	Violent Crime	Total Crime	Property Crime	Violent Crime
$Period_1 \times$Austerity	-.00665	-.00943*	.0039	-.00509	-.0089	.00945
	(.00436)	(.00482)	(.00676)	(.00938)	(.0111)	(.0146)
$Period_3 \times$Austerity	.0142***	.0121***	.0221***	.0283***	.0246***	.0443***
	(.00349)	(.00385)	(.00654)	(.00816)	(.0088)	(.0152)
CSPs	226	226	226	226	226	226
Observations	6,328	6,328	6,328	6,328	6,328	6,328

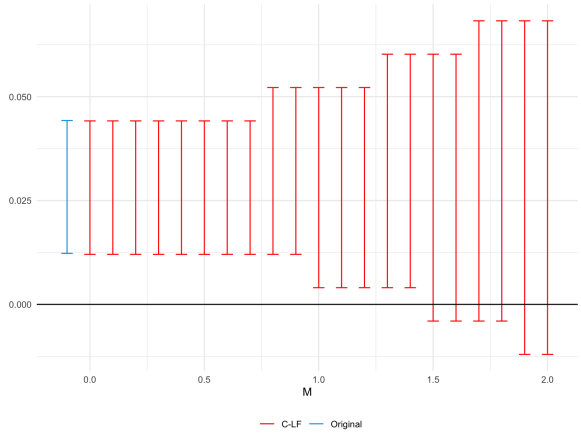
Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at CSP level. The dependent variable is log Crime Rate per 1000 Population in all specifications. The variables $Period_1$ and $Period_3$ are dummies corresponding resepctively to the earlist pre-period of 2009-2010, and the post-policy period of 2013-2015. The pre-policy period of 2011-2012 is the omitted period. Observations are weighted by CSP-level population. CSP fixed effects and region-by-month-by-year fixed effects are included in all specifications. Additional control variables - all CSP-level unless otherwise specified - include (Police Force Area-level) police officers per 1000 population, the median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

The graphical outputs from the Rambachan and Roth (2023) approach, where we use the Relative Magnitude approach for bounding, are presented in Figure A2. For total crime, and for violent crime, the "breakdown value" of $\overline{M}$ – the factor of the pre-trends at which the bounds on the estimated treatment effect overlap with zero – exceeds 1 for both continuous and binary versions of the DD specification. This means that even if post Welfare Reform Act violations of parallel trends were as large as any pre-policy violations, the confidence set for the treatment effects would not include zero. The breakdown value is below 1 for property crime, which is not surprising, as our estimated treatment effect is rather small for this crime category.

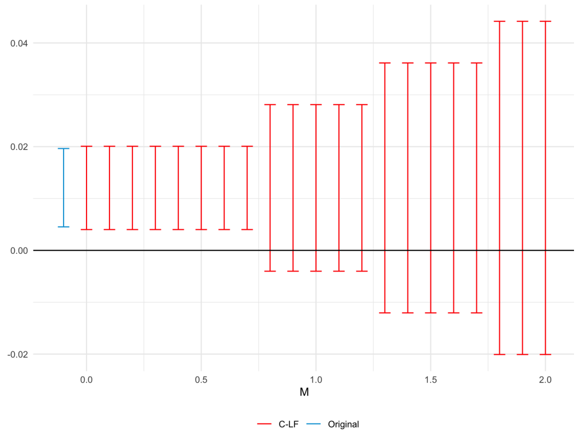
Figure A2: Our Core Results are Robust to Reasonably Large Potential Violations of Parallel Trends



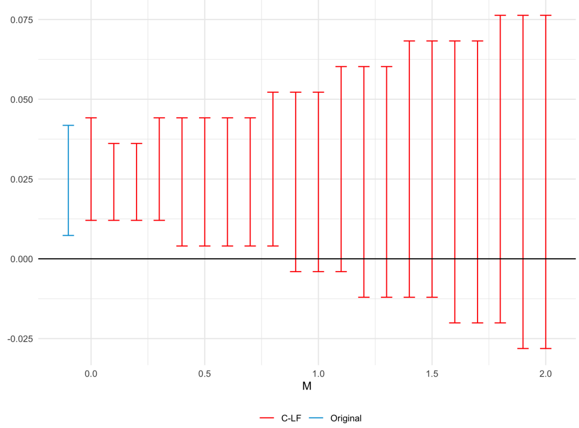
(a) Total Crime, Continuous Treatment



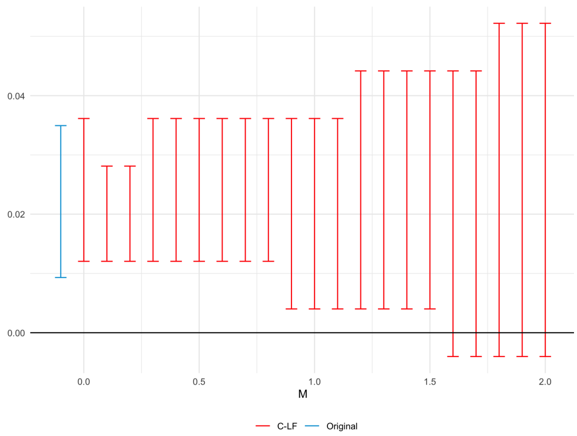
(b) Total Crime, Binary Treatment



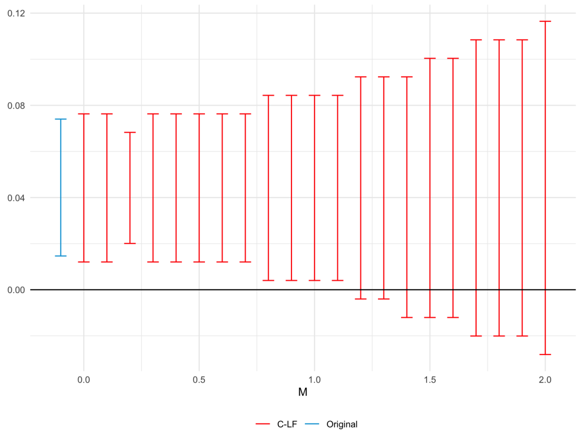
(c) Property Crime, Continuous Treatment



(d) Property Crime, Binary Treatment



(e) Violent Crime, Continuous Treatment



(f) Violent Crime, Binary Treatment

Notes: The blue band ("Original") is the 95% confidence interval of the DD treatment effect estimate ($Period_3 \times Austerity$ in Table A3). The red bands ("C-LF") are the robust confidence intervals for the Rambachan and Roth (2023) Relative Magnitude-based bounds. These vary with the x-axis – $\overline{M}$ – which designates factors of the maximum pre-treatment violation of parallel trends. Thus a confidence interval that does not intersect 0 when $\overline{M} = 1$ informs us that when we allow any parallel trend violations in the post-period to be as large as the maximum pre-treatment violation, the 95% confidence intervals for the bounded treatment effect do not include zero. Source: CSP-level crime data, FY2009-FY2015

A.2 Crime Concentration

Table A4: The Placebo DD Specifications Show no Evidence of a Pre-Trend for Crime Concentration

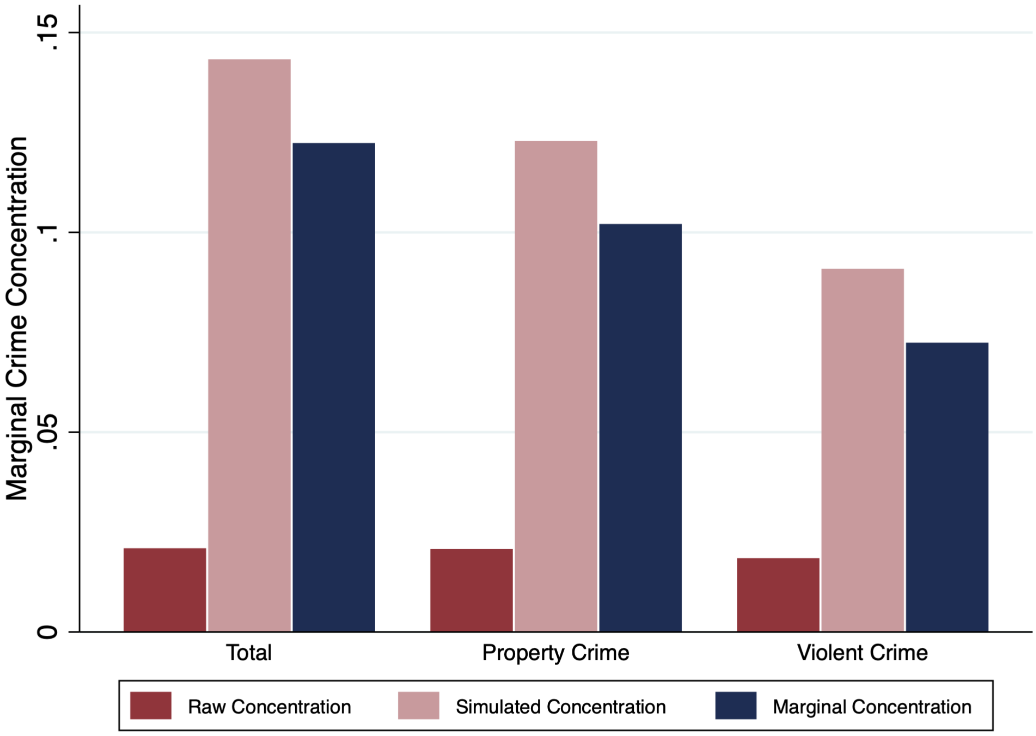
	(1)	(2)	(3)
		Crime Categories	
	Total Crime	Property Crime	Violent Crime
Continuous Treatment			
Post × Austerity	-.000282	.000137	-.000535
	(.000278)	(.000439)	(.000374)
Binary Treatment			
Post × 𝟙[Austerity Impact Above Median]	-.000656	.000022	-.000227
	(.00058)	(.000954)	(.000751)
Districts	234	234	234
Observations	468	468	468
Proportion of Total Crime	1	.66	.23

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at district level. The dependent variable is the Marginal Crime Concentration. The Placebo variable Post takes the value 1 for all observations in 2012, and 0 for all in 2011. Observations are weighted by district-level population. District fixed effects and year fixed effects are included in all specifications. Additional control variables - all district-level unless otherwise specified - include (Police Force Area-level) police officers per 1000 population, the median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

B Robustness and Ancillary Results

B.1 Visualizing how the Concentration Measure is Constructed

Figure B1: Marginal Crime Concentration is Calculated as Simulated Minus Raw Concentration



B.2 Using the Alternative Crime Series to Explore the Specific Offenses Causing the Crime increases

Using the CSP-level crime data, we can explore the specific offenses that lead to the increase in violent crime that we document in the body of the paper. Table B1 presents our DD parameter estimates for a key set of outcomes.

Table B1: The Alternative Data Series Allows us to Isolate the Specific Crimes That Drive the Main Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Category	**Crime Type**	**Sub-Types**		**Specific Violent Offenses**		
	Total	Violent Crime Category	Violence and Sexual Offences	Violence	Sexual Offences	Homicide	Violence with Injury	Violence without Injury
A. Continuous								
Post × Austerity	.0138***	.0188**	.0191**	.0219***	-.00242	-.00014	.0164*	.0386***
	(.00417)	(.00763)	(.00825)	(.00835)	(.0135)	(.00015)	(.00846)	(.0103)
B. Binary Treatment								
Post × $\mathbb{1}$[Austerity Impact Above Median]	.0303***	.0409**	.0441**	.0487***	.00072	-.00042	.0284	.0906***
	(.00916)	(.0163)	(.0171)	(.0172)	(.0286)	(.00029)	(.0178)	(.0224)
$\overline{Y}_0$	16.7	4.07	3.01	2.77	.237	.00262	1.47	1.3
Community Safety Partnerships	226	226	226	226	226	226	226	226
Observations	4,520	4,520	4,520	4,520	4,520	4,494	4,520	4,520
Proportion of Total Crime	1	.28	.22	.2	.02	.00016	.098	.087

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at CSP level. The dependent variable is log Crime Rate per 1000 Population in all specifications. The sample period is the fiscal years of 2011 to 2015. The variable Post takes the value 1 for all observations in 2013 to 2015, and 0 for previous years. Austerity is the simulated impact of austerity in £100*s* per working age person. Observations are weighted by CSP-level population. CSP fixed effects and region-by-quarter-by-year fixed effects are included in all specifications. Additional control variables - all CSP-level unless otherwise specified - include (Police Force Area-level) police officers per 1000 population, the median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

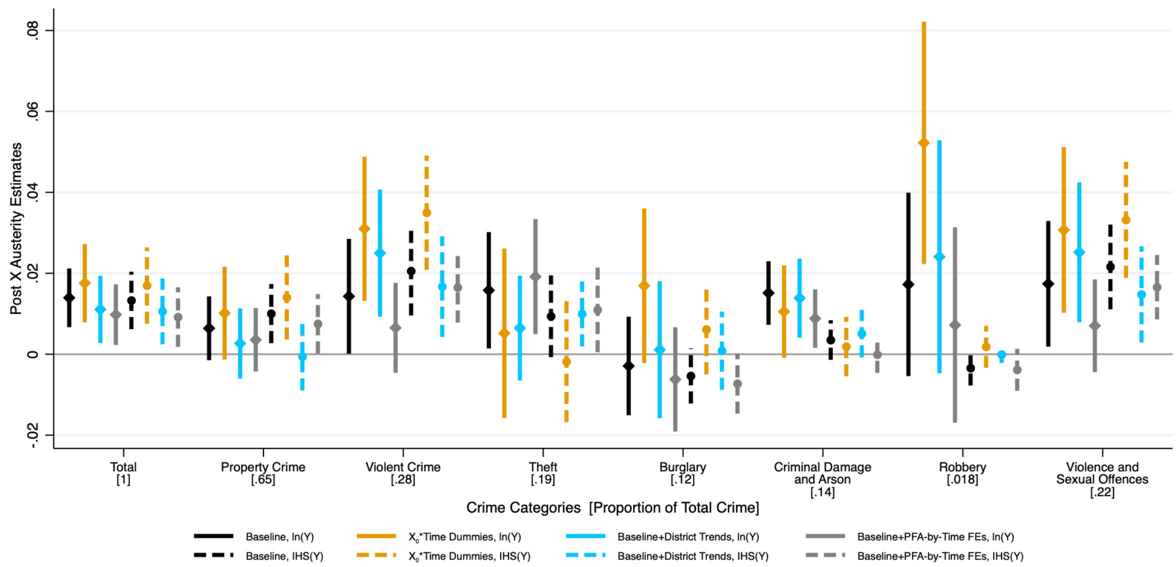
B.3 Sensitivity Analyses

B.3.1 Functional Form

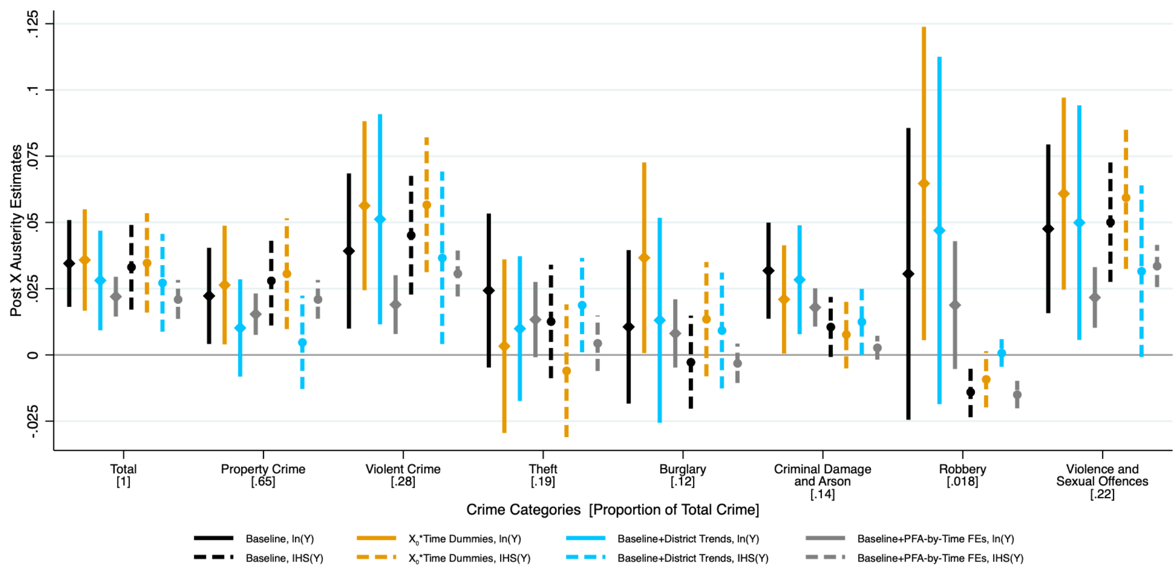
We explore a variety of variants to our baseline functional form specification. We present eight different specifications in Figure B2 below, where the first four are (1) baseline specification, (2) baseline controls interacted with time dummies, (3) baseline specification plus district specific time trends, and (4) replacing region-by-time fixed effects with police force-by-time fixed effects. The second four specifications present the same set of specification checks but where we apply the inverse hyperbolic sine instead of the log transformation.

The key message we take from these graphs is that our results are almost universally robust to different empirical specifications. The one exception is robbery, which jumps around somewhat – robbery accounts for 1.8% of total crime, and our core results for total crime and violent crime are robust to the exclusion of these smaller number of offenses.

Figure B2: Our Results are Robust to a Battery of Specification Checks



(a) Continuous Treatment

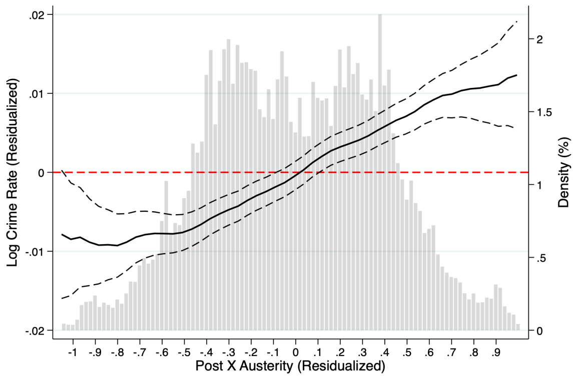


(b) Binary Treatment

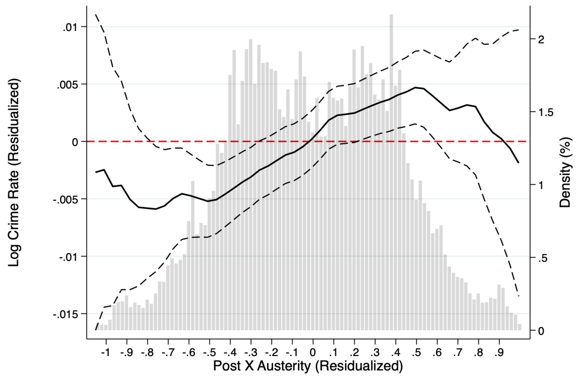
B.3.2 The Linear Functional Form Assumption of our Austerity Measure

The aim of the analysis presented in this section is to probe the validity of the functional form assumption inherent in Equation (8) – namely that austerity impacts crime in a linear fashion. We offer some leeway in the main body of the text by presenting accompanying results for a binarized version of the DD specification. Here we go further, and estimate a non-parametric version of (8). To do so, we use the Frisch-Waugh-Lovell theorem (Frisch and Waugh, 1933; Lovell, 1963), and first residualize both the dependent variable – crime rate – and our DD term. We then run a local linear regression of residualized crime on our residualized DD terms, in order to estimate a more flexible relationship between austerity exposure and crime. We graph these estimates, along with district-clustered 95% confidence intervals, in Figure B3 below.

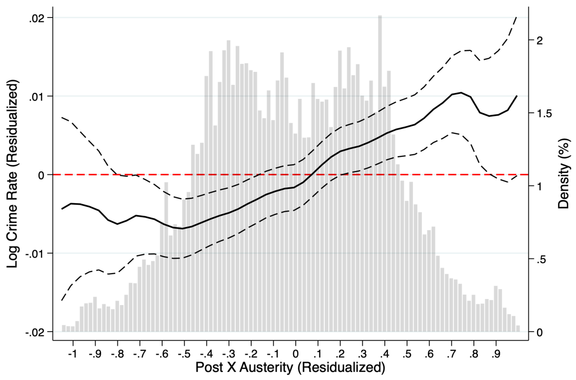
Figure B3: The Baseline Model Results are not Driven by the (Linear) Functional Form Assumption in (8)



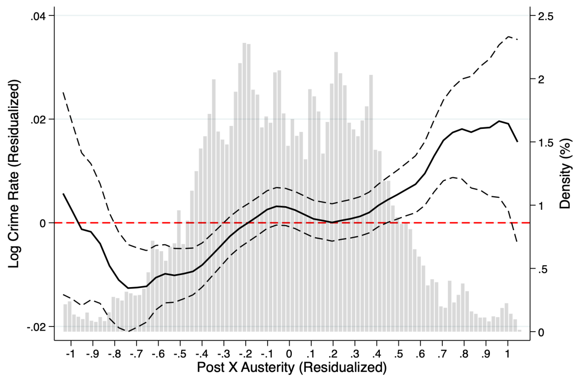
(a) Total Crime



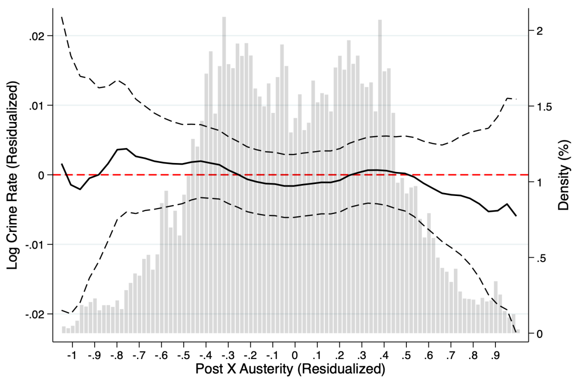
(b) Property Crime Category



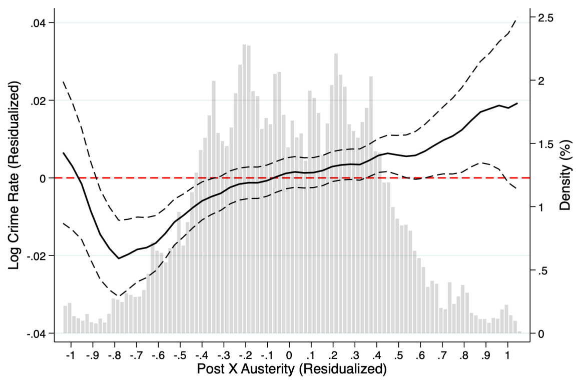
(c) Violent Crime Category



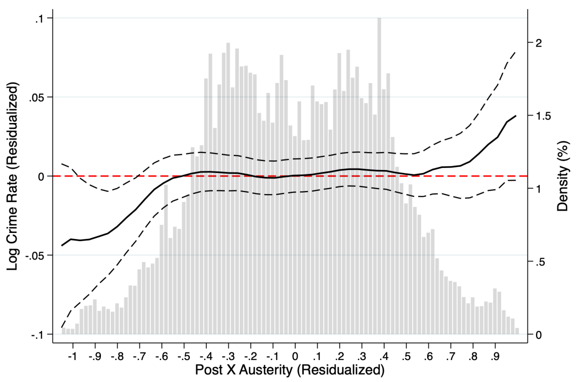
(d) Theft



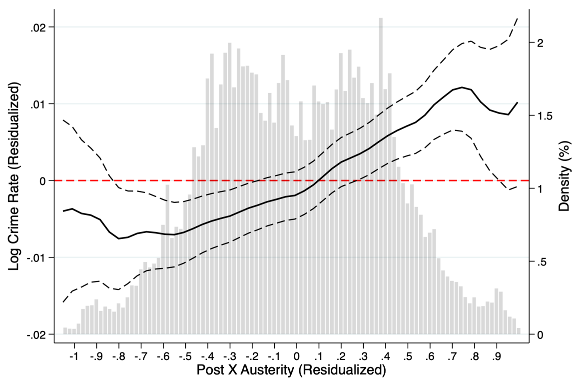
(e) Burglary



(f) Criminal Damage and Arson



(g) Robbery



(h) Violence and Sexual offenses

B.4 The Time Frame of the Beatty and Fothergill (2013) Austerity Measure

The Beatty and Fothergill (2013) austerity measure that we use in this paper is comprised of 10 individual components. With the exception of reforms to Disability Living Allowance (full impact realized in 2017/2018), 1 per cent up-rating (full impact realized in 2015/2016) and incapacity benefits (full impact realized in 2015/2016), the majority of the components, and thus the main measure reforms, come in to full effect in the 2014/2015 financial year, which ends March 2015.

Our main sample runs until March 2016 (the end of the 2015/6 fiscal year). The tables below repeat the main crime rate specifications for the shorter time period of April 2011-March 2015. The results we present below show very similar, and slightly larger, treatment effects compared the full sample. Such results are in line with what one would surmise from considering the individual year treatment effects in the post-period from our event study analysis (Figure 2 – generally, but not always, the large treatment effects are found in the earlier years in the post-period.

Table B2: Our Key Baseline Specification Results are Robust to Changing the Time Range of the Sample

		Crime Categories		**Crime Types**				
	Total	Property Crime	Violent Crime	Theft	Burglary	Criminal Damage and Arson	Robbery	Violence and Sexual Offences
A. Continuous								
Post × Austerity	.0152***	.00655	.0205**	.0167**	.00168	.0168***	.016	.0233**
	(.00436)	(.00475)	(.00826)	(.00826)	(.00726)	(.0046)	(.0133)	(.00944)
$1\sigma_{Aus}\times$ Post × Austerity	.018***	.00776	.0244**	.0198**	.00199	.0199***	.0189	.0277**
	(.00517)	(.00563)	(.0098)	(.0098)	(.00861)	(.00545)	(.0158)	(.0112)
B. Binary								
Post × $\mathbb{1}$[Austerity Impact Above Median]	.0344***	.021*	.0466**	.0246	.0127	.0323***	.0353	.0538***
	(.00987)	(.0108)	(.0182)	(.0165)	(.0171)	(.0108)	(.0319)	(.0205)
$\overline{Y}_0$	5.8	3.28	1.33	1.09	.761	.819	.122	1.03
Districts	234	234	234	234	234	234	234	234
Observations	11,232	11,232	11,232	10,062	11,232	10,062	10,250	11,232
Proportion of Total Crime	1	.65	.28	.19	.12	.14	.018	.22

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at district level. The dependent variable is log Crime Rate per 1000 Population in all specifications. The Post variable takes value 1 for 04/2013 onwards, and 0 otherwise. Austerity is the simulated impact of austerity in £100*s* per working age person. Additional control variables - all district-level unless otherwise specified - include (Police Force Area-level) police officers per 1000 population, the median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50. Data: District×Month-level Crime Data. 04/2011-03/2015.

B.5 The Robustness of the Definition of the Austerity Measure

B.5.1 Removing the Incapacity Benefit Reform Component from our Austerity Measure

As noted by Beatty and Fothergill (2013), a portion of the incapacity benefit reforms they capture in their SAI measure were enacted not in the Welfare Reform Act, but rather by the previous government. We re-estimate all our main specifications with an augmented austerity measure that removes the contribution of incapacity benefit reforms. Although these reforms contribute a sizable amount to the total austerity impact, the estimates based on the augmented austerity measure – presented below in Table B3 – follow the same pattern as those in the main body, that is, austerity leads to higher crime rates.

Table B3: The Results in our Main Analysis are Robust to Augmenting the Measure of Austerity Exposure That we use.

		Crime Categories		Crime Types				
Specification:	Total	Property Crime	Violent Crime	Theft	Burglary	Criminal Damage and Arson	Robbery	Violence and Sexual Offences
A. Continuous Treatment								
i. Baseline DD								
Post × Austerity	.0152**	.00519	.0117	.0248**	-.00836	.0207***	.00871	.0172
	(.00624)	(.00646)	(.0118)	(.0124)	(.0102)	(.00623)	(.0192)	(.0129)
ii. Dynamic DD								
Post_1 × Austerity	.0143**	.00244	.023**	.0234**	-.00791	.0193***	.0148	.0281**
	(.00615)	(.00621)	(.0112)	(.0102)	(.0109)	(.00611)	(.0188)	(.0125)
Post_2 × Austerity	.0194**	.00759	.0184	.0282*	-.00747	.0247***	.00348	.0222
	(.00768)	(.00789)	(.0144)	(.0143)	(.0121)	(.00804)	(.024)	(.0154)
Post_3 × Austerity	.0113	.00626	-.0129	.0227	-.0101	.0179*	.00637	-.0048
	(.0083)	(.00882)	(.0173)	(.016)	(.0145)	(.0092)	(.0282)	(.0175)
B. Binary Treatment								
i. Baseline DD								
Post × $\mathbb{1}$[Austerity Impact Above Median]	.0303***	.0177	.0329*	.0421**	-.00078	.0304***	.0095	.0507***
	(.00974)	(.0109)	(.0182)	(.0169)	(.0171)	(.0108)	(.0329)	(.0193)
ii. Dynamic DD								
Post_1 × $\mathbb{1}$[Austerity Impact Above Median]	.0278***	.0106	.0503***	.0377***	.00205	.0284***	.025	.0644***
	(.0094)	(.00963)	(.0187)	(.0138)	(.018)	(.00939)	(.0354)	(.0203)
Post_2 × $\mathbb{1}$[Austerity Impact Above Median]	.035***	.0183	.0436*	.0438**	-.00992	.0281**	.0127	.0598**
	(.0121)	(.0134)	(.0224)	(.0202)	(.0193)	(.014)	(.0394)	(.0233)
Post_3 × $\mathbb{1}$[Austerity Impact Above Median]	.0284**	.0263*	-.00208	.046**	.00601	.0358**	-.014	.0224
	(.0139)	(.0148)	(.0275)	(.0226)	(.0261)	(.0157)	(.045)	(.0273)
Mean Crime $\text{Rate}_{\text{pre-period}}$	5.8	3.28	1.33	1.09	.761	.819	.122	1.03
Districts	234	234	234	234	234	234	234	234
Observations	14,040	14,040	14,040	12,870	14,040	12,870	12,840	14,040
Proportion of Total Crime	1	.65	.28	.19	.12	.14	.018	.22

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at district level. The dependent variable is log Crime Rate per 1000 Population in all specifications. The Post variable takes value 1 for 04/2013 onwards, and 0 otherwise. The variables Post_1, Post_2 and Post_3 are dummies corresponding to the austerity period fiscal years of 2013, 2014 and 2015 respectively. Austerity is the simulated impact of austerity in £100*s* per working age person. Observations are weighted by district-level population. District fixed effects and region-by-month-by-year fixed effects are included in all specifications. Additional control variables - all district-level unless otherwise specified - include (Police Force Area-level) police officers per 1000 population, the median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

B.5.2 Updating the Austerity Measure Based on Beatty and Fothergill (2016)

In this section, we replace our original austerity measure with an updated version from Beatty and Fothergill (2016). Whereas the original measure was an ex-ante projection of the effects of austerity at the local level, based on pre-policy claimant counts, the updated measure is a district-level, ex-post, estimate of the financial impact, based on outturn. Table B4 presents estimates based on the updated measure for our key specifications.

Table B4: The Results in our Main Analysis are Robust to Updating the Measure of Austerity Exposure (á la Beatty and Fothergill (2016)) That we use.

		Crime Categories		**Crime Types**				
Specification:	Total	Property Crime	Violent Crime	Theft	Burglary	Criminal Damage and Arson	Robbery	Violence and Sexual Offences
A. Continuous Treatment								
i. Baseline DD								
Post × Austerity	.0168***	.00716	.0123	.0248**	-.00602	.0229***	.00896	.0186
	(.00629)	(.00669)	(.012)	(.0117)	(.0104)	(.00649)	(.0189)	(.013)
ii. Dynamic DD								
$Post_1$ × Austerity	.0149**	.00359	.0231**	.023**	-.00623	.0212***	.0158	.0289**
	(.0061)	(.00632)	(.0112)	(.00969)	(.011)	(.00627)	(.0191)	(.0125)
$Post_2$ × Austerity	.0217***	.0102	.0198	.029**	-.00533	.0273***	.00291	.0243
	(.00776)	(.00809)	(.0146)	(.0135)	(.012)	(.00825)	(.0235)	(.0155)
$Post_3$ × Austerity	.0135	.00837	-.0106	.0222	-.00655	.0201**	.00696	-.00177
	(.00848)	(.00901)	(.0175)	(.0151)	(.015)	(.00944)	(.0275)	(.0176)
B. Binary Treatment								
i. Baseline DD								
Post × $\mathbb{1}$[Austerity Impact Above Median]	.0347***	.0208**	.038**	.0474***	.00704	.0274**	.0193	.0527***
	(.00944)	(.0105)	(.018)	(.0171)	(.0172)	(.0108)	(.0327)	(.0192)
ii. Dynamic DD								
$Post_1$ × $\mathbb{1}$[Austerity Impact Above Median]	.0306***	.013	.0521***	.0418***	.00666	.0235**	.0287	.064***
	(.00918)	(.00962)	(.018)	(.0138)	(.0178)	(.00961)	(.0341)	(.0197)
$Post_2$ × $\mathbb{1}$[Austerity Impact Above Median]	.0397***	.0224*	.0493**	.0492**	.0007	.029**	.0191	.0635***
	(.0116)	(.0129)	(.0219)	(.0204)	(.0193)	(.0138)	(.039)	(.0229)
$Post_3$ × $\mathbb{1}$[Austerity Impact Above Median]	.0345**	.0293**	.0064	.0528**	.0148	.0308**	.00731	.0256
	(.0136)	(.0143)	(.0271)	(.0227)	(.0258)	(.0155)	(.0459)	(.0268)
Mean Crime Rate$_{\text{pre-period}}$	5.8	3.28	1.33	1.09	.761	.819	.122	1.03
Districts	234	234	234	234	234	234	234	234
Observations	14,040	14,040	14,040	12,870	14,040	12,870	12,840	14,040
Proportion of Total Crime	1	.65	.28	.19	.12	.14	.018	.22

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at district level. The dependent variable is log Crime Rate per 1000 Population in all specifications. The Post variable takes value 1 for 04/2013 onwards, and 0 otherwise. The variables $Post_1$, $Post_2$ and $Post_3$ are dummies corresponding to the austerity period fiscal years of 2013, 2014 and 2015 respectively. Austerity is the simulated impact of austerity in *£100s* per working age person. Observations are weighted by district-level population. District fixed effects and region-by-month-by-year fixed effects are included in all specifications. Additional control variables - all district-level unless otherwise specified - include (Police Force Area-level) police officers per 1000 population, the median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

B.5.3 Different Levels of Crime Concentration

In this section we present sensitivity analyses using different cutoffs for the concentration measure (10%, 20%, and 50% in addition to our preferred cutoff of 25%, which we use as our primary measure to capture how concentrated crime was in a district at the upper end of concentration). For completeness, we also include two additional measures – the Gini and the generalized Gini as proposed by Bernasco and Steenbeek (2017) in order to deal with areas with fewer crimes than streets. The Gini provides a measure of overall inequality of crime in an area, which obviates our ability to focus on the upper end of concentration. We present our core DD estimates for these measures in Table B5.

Table B5: Alternative Concentration Metrics

	(1)	(2)	(3)	(4)	(5)	(6)
	Continuous Treatment			**Binary Treatment**		
	Total Crime	Property Crime	Violent Crime	Total Crime	Property Crime	Violent Crime
MCC 10%	.00029***	.000368***	.000167	.000609***	.000675***	.000497*
	(.000101)	(.000107)	(.000123)	(.000202)	(.00023)	(.000258)
$\overline{Y}_0$	.0465	.0351	.0252	.0465	.0351	.0252
MCC 20%	.0006***	.00079***	.000203	.00139***	.00163***	.000735
	(.000225)	(.00026)	(.000276)	(.000448)	(.000528)	(.000596)
$\overline{Y}_0$	.0979	.0736	.0545	.0979	.0736	.0545
MCC 25%	.000564*	.00102***	.000119	.00121**	.00199***	.000723
	(.00029)	(.000344)	(.000346)	(.000576)	(.000679)	(.00073)
$\overline{Y}_0$	.124	.0927	.0697	.124	.0927	.0697
MCC 50%	.000913	.0011	.000275	.0018	.00154	.00121
	(.000624)	(.00079)	(.000824)	(.00124)	(.00158)	(.00185)
$\overline{Y}_0$	.245	.184	.15	.245	.184	.15
Gini	.00106	.00297**	-.00116	.000654	.00393	-.00252
	(.000833)	(.00124)	(.000959)	(.00169)	(.00252)	(.00191)
$\overline{Y}_0$	.616	.61	.752	.616	.61	.752
Generalized Gini	.00106	.00277**	-.00172	.000654	.00351	-.00153
	(.000833)	(.00124)	(.00112)	(.00169)	(.0025)	(.00219)
$\overline{Y}_0$	.616	.61	.735	.616	.61	.735
Districts	234	234	234	234	234	234
Observations	1,170	1,170	1,170	1,170	1,170	1,170

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at district level. The dependent variable is the Marginal Crime Concentration. The Post variable takes value 1 for 2013 onwards, and 0 otherwise. Austerity is the simulated impact of austerity in £100s per working age person. In order to give a better sense of the magnitude of the effects we find, we present the DD terms as proportions of $\overline{Y}_0$. For the continuous treatment specifications, we present the effect of a one standard deviation increase in austerity exposure. Observations are weighted by district-level population. District fixed effects and year fixed effects are included in all specifications. Additional control variables - all district-level unless otherwise specified - include (Police Force Area-level) log one-quarter lagged police officers per 1000 population, the log one-quarter lagged median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

B.6 The Labor Market and Austerity Exposure

Table B6: There is no Relationship Between Austerity Exposure and a Battery of Labor Market Outcomes

Specification:	Median Hourly Wage	Median Hours Worked per Week	Participation Rate	Employment Rate	Self-Employment Rate	Unemployment Rate
A. Continuous						
Post × Austerity	-.0189	-.00434	-.00087	-.00121	.00037	–6.0e-05
	(.0294)	(.0134)	(.00142)	(.00154)	(.00114)	(.00116)
B. Binary						
Post × $\mathbb{1}$[Austerity Impact Above Median]	-.078	-.0279	-.00281	-.003	.00041	-.00051
	(.0613)	(.0327)	(.00339)	(.00336)	(.00214)	(.00238)
$\overline{\mathrm{Y}}_0$	13.1	37.6	.76	.696	.091	.0856
Districts	234	234	234	234	234	234
Observations	1,404	1,404	1,404	1,404	1,403	1,347

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at district level. The Post variable takes value 1 for 2013 onwards, and 0 otherwise. The variables Post_1, Post_2 and Post_3 are dummies corresponding to the austerity period years 2013, 2014 and 2015 respectively. Austerity is the simulated impact of austerity in £100*s* per working age person. Observations are weighted by district-level population. District fixed effects and region-by-year fixed effects are included in all specifications. In order to keep regression specification as close as possible to those in the main text, we include the district-level population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

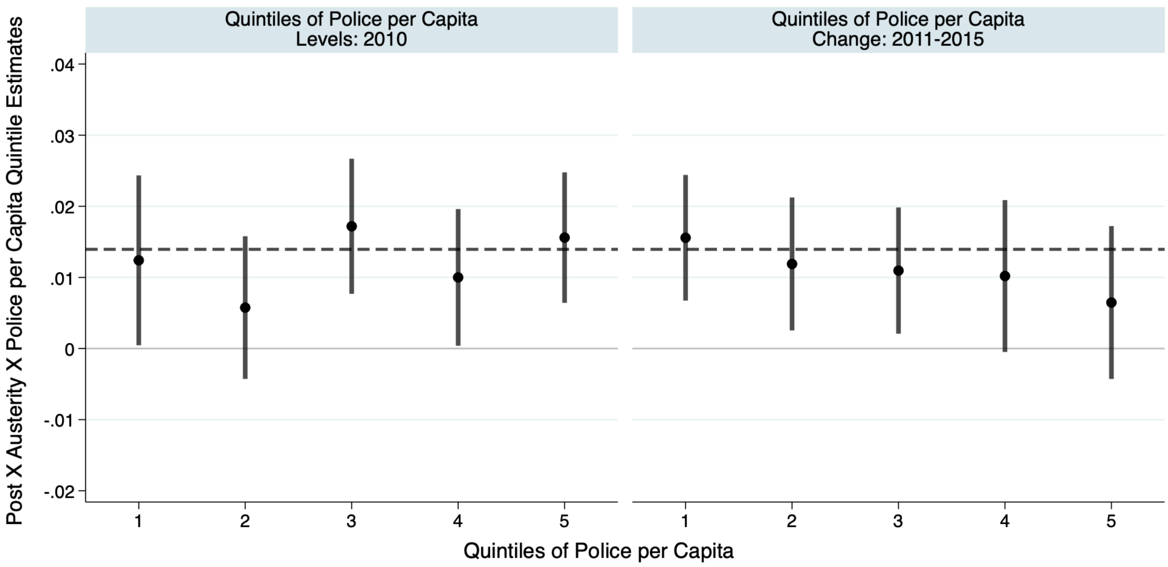
B.7 Heterogeneity by Police per Capita

In this section, we examine the possibility that treatment effect heterogeneity is correlated with the policing in the area. We consider two different but complementary aspects of policing. First, we construct quintiles based on police officers per capita in 2010 - prior to our analysis period. Second, we construct quintiles based on the change in police officers per capita from the beginning (2011) to the end (2015) of our sample period. Armed with these two quintiles, we run a series of triple difference specifications of the form:

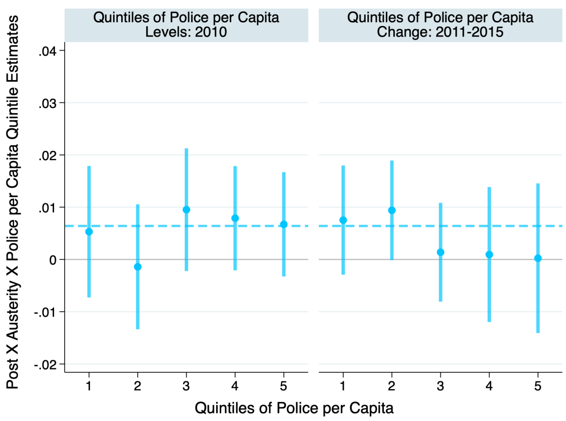
$$c_{it} = \sum_{q=1}^{5} \beta_q Post_t \times Austerity_i \times Police\,Quintile_{iq} + X_{it}^{'}\gamma + \pi_{r \times t} + \theta_i + \epsilon_{it} \ . \tag{23}$$

This specification mimics Equation (8), with the exception that we allow our difference-in-differences parameter to vary by policing quintile. We thus estimate 5 treatment effect parameters for each policy measure, and plot these below: The key lesson we learn from this exercise is that there is no statistically

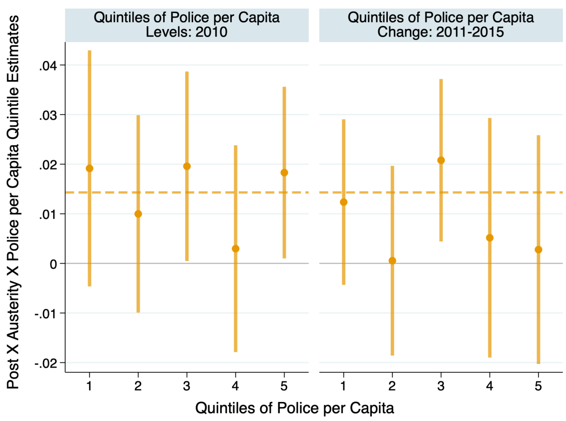
Figure B4: Treatment Effect Heterogeneity is not related to Policing



(a) Total Crime



(b) Property Crime Category



(c) Violent Crime Category

significant, systematic pattern to how the treatment effect estimates vary across policing quintiles. This is the case for total crime, as well as for the two main crime categories. We thus conclude that the estimated treatment effect heterogeneity is unrelated to both the initial stock and subsequent flow of local police officer strength. This is of relevance given that levels of policing fell substantial during our analysis period, as shown in Figure 1. More explicitly, the analysis above helps to assuage concerns that our difference-in-difference parameter estimates may reflect not just the impact of welfare cuts in the district, but also correlated differences in levels or changes in local policing.

B.8 Policing Numbers and Austerity Exposure

To examine if there is any systematic relationship between our austerity exposure measure and the size of the police force, we collapse our district-level austerity measure to the police force area (PFA) level, and then implement a regression model of the form:

$$p_{at} = \beta Post_t \times Austerity_a + X_{it}^{'}\gamma + \tau_t + \theta_a + \epsilon_{at} \ . \tag{24}$$

We present the resulting DD estimates in Table B7 below. We document no evidence that there is any systematic relationship between the welfare cuts at the core of the Welfare Reform Act and changes in police force size.

Table B7: There is no Relationship Between Police Force Size and PFA-Level Austerity Exposure

	Continuous Treatment		**Binary Treatment**	
	(1)	**(2)**	**(3)**	**(4)**
Post × Austerity	-.0104	-.00851	-.0054	-.00664
	(.00717)	(.00674)	(.0152)	(.0112)
$\overline{Y}_0$	.826	.826	.826	.826
$\sigma_{Austerity}$	.831	.831	.831	.831
$[1\sigma_{Austerity} \times DD]/\overline{Y}_0$	-.0105	-.00857		
	(.00721)	(.00678)		
$DD/\overline{Y}_0$			-.00654	-.00804
			(.0184)	(.0136)
PFAs	42	42	42	42
Observations	210	210	210	210

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at PFA level. The dependent variable in odd-numbered columns is the log of police per 1,000 population. In even columns the dependent variable is the log of police force size and we include log PFA population as a control variable. The Post variable takes value 1 for 04/2013 onwards, and 0 otherwise. Observations are weighted by PFA-level population. PFA fixed effects and year fixed effects are included in all specifications.

B.9 Clearance Rates and Austerity Exposure

We now investigate if there is any relationship between district level austerity exposure and a key measure of police effectiveness: the crime clearance rate. We estimate a specification akin to Equation (8), but replace crime with the crime clearance rate. We find no statistically significant evidence that there is an impact of district-level austerity exposure on this measure of police effectiveness, as evidenced by the estimates we present in Table B8 below.

Table B8: DD Evidence of the Impact of Austerity on the Clearance Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Crime Categories		**Crime Types**				
	Total	Property Crime	Violent Crime	Theft	Burglary	Criminal Damage and Arson	Robbery	Violence and Sexual Offences
A.) Continuous Treatment								
Post × Austerity	-.00347	-.00121	-.00431	-.00175*	-.00124	-.00116	-.0063	-.00387
	(.00229)	(.00163)	(.00382)	(.00106)	(.00175)	(.00178)	(.00545)	(.00395)
$\overline{Y}_0$	.234	.144	.38	.071	.094	.135	.227	.357
$\sigma_{Austerity}$	1.19	1.19	1.19	1.19	1.19	1.19	1.19	1.19
$[1\sigma_{Austerity} \times DD]/\overline{Y}_0$	-.0176	-.00994	-.0134	-.0292*	-.0156	-.0101	-.0329	-.0129
	(.0116)	(.0134)	(.0119)	(.0176)	(.0221)	(.0156)	(.0284)	(.0131)
B.) Binary Treatment								
Post × $\mathbb{1}$[Austerity Impact Above Median]	-.00243	-.00058	-.00159	-.0008	.00033	-.00111	-.00484	-.00118
	(.00523)	(.00378)	(.00811)	(.0023)	(.00387)	(.00395)	(.011)	(.00839)
$\overline{Y}_0$	.234	.144	.38	.071	.094	.135	.227	.357
$\sigma_{Austerity}$	1.19	1.19	1.19	1.19	1.19	1.19	1.19	1.19
$[DD]/\overline{Y}_0$	-.0123	-.0048	-.00496	-.0133	.0041	-.00972	-.0253	-.00392
	(.0264)	(.0312)	(.0253)	(.0384)	(.0488)	(.0346)	(.0572)	(.0278)
Districts	234	234	234	234	234	234	234	234
Observations	11,934	11,934	11,934	11,934	11,934	11,934	10,899	11,934
Proportion of Total Crime	1	.65	.28	.19	.12	.14	.018	.22

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at district level. The dependent variable is the crime clearance rate in all specifications. The Post variable takes value 1 for 04/2013 onwards, and 0 otherwise. Observations are weighted by district-level population. District fixed effects and region-by-month-by-year fixed effects are included in all specifications. Additional control variables - all district-level unless otherwise specified - include (Police Force Area-level) police officers per 1000 population, the median weekly wage, and the local population share of the following age groups of males: 10-17, 18-24, 25-30, 31-40 and 41-50.

B.10 House Price Regression Parameter Estimates by Property Types

The two tables below present regression estimates for Equations 17 and 18. These coefficients are then used as inputs into Equations 19 and 20.

In order to calculate the share of housing *stock* accounted for by each property type (information which we do not have), we use information on property type shares based on housing *transactions*. Specifically we calculate $prop_{1,dp}$ – the post-reform period proportion of each property type, in each district, as:

$$prop_{1,dp} = \frac{sales_{1,dp}}{sales_{1,d}} \quad \text{for } p = 1, \ldots, 4 \tag{25}$$

We then calculate an estimate of the housing stock by property type using these transaction-informed proportions, and the total housing stock at the district level:

$$stock_{1,dp} = prop_{1,dp} \times stock_{1,d} \quad \text{for } p = 1, \ldots, 4 \tag{26}$$

We calculate $stock_{1,pqd}$ in precisely the same manner, except we additionally condition on crime quartile.

Table B9: The Majority of Property Types in Districts Exposed to Austerity See House Price Drops

	(1)	(2)	(3)	(4)
DD	Detached	Semi-Detached	Terraced	Flats
Post × Austerity	-.0041**	-.0075***	-.0079***	-.00092
	(.0019)	(.0023)	(.0028)	(.0045)
Mean Price$_0$ (£)	316,407	197,322	186,927	211,172
Neighborhoods	234	234	234	234
Observations	748,964	969,238	1,018,374	747,983

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at neighborhood level. The dependent variable is log house price in all specifications. The Post variable takes value 1 for 04/2013 onwards, and 0 otherwise. Austerity is the simulated impact of austerity in *£100s* per working age person. Neighborhood fixed effects and region-by-month-by-year fixed effects are included in all specifications. Additional control variables – all property-level unless otherwise specified – include dummies for new-build and leasehold, deciles of floor area of the property and the number of habitable rooms categories.

Table B10: Districts Exposed to Austerity See House Price Drops, Particularly in Neighborhoods with High Crime Before the Policy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	$Crime_0$ Quartiles Based on:											
	Total Crime				**Property Crime**				**Violent Crime**			
DDD	Detached	Semi-Detached	Terraced	Flats	Detached	Semi-Detached	Terraced	Flats	Detached	Semi-Detached	Terraced	Flats
Post × Austerity	-.006**	-.009***	-.0085***	-.0029	-.0059**	-.008***	-.008***	-.00097	-.0047*	-.01***	-.0071**	-.0053
	(.0024)	(.0024)	(.0027)	(.0053)	(.0024)	(.0024)	(.0027)	(.0051)	(.0024)	(.0025)	(.003)	(.0057)
Post × Austerity × $Crime_0$ $Quartile_2$	.0035	-.00017	-.00023	.0029	.0024	-.0007	-.0014	-.00013	.0002	.0021	-.0032	.0037
	(.0033)	(.0034)	(.0039)	(.0067)	(.0032)	(.0034)	(.0038)	(.0065)	(.0034)	(.0034)	(.0039)	(.0072)
Post × Austerity × $Crime_0$ $Quartile_3$	.0023	.0019	-.00063	.0031	.003	.00035	.00037	.001	.00075	.0012	-.0036	.0059
	(.0031)	(.0035)	(.0041)	(.0071)	(.0031)	(.0036)	(.0041)	(.007)	(.0033)	(.0035)	(.0043)	(.0072)
Post × Austerity × $Crime_0$ $Quartile_4$	.001	-.0021	-.0016	-.0017	.0024	-.0035	-.0024	-.0031	-.00014	.0031	-.0013	.0021
	(.0039)	(.0038)	(.0044)	(.0077)	(.0041)	(.0038)	(.0045)	(.0074)	(.0036)	(.0039)	(.0045)	(.0082)
Mean $Price_0$ (£)	316,407	197,322	186,927	211,172	316,407	197,322	186,927	211,172	316,407	197,322	186,927	211,172
Neighborhoods	913	921	929	929	911	923	931	929	920	924	934	934
Observations	748,956	969,238	1,018,374	747,965	748,959	969,238	1,018,374	747,960	748,958	969,238	1,018,374	747,966

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at neighborhood level. The dependent variable is log house price in all specifications. The Post variable takes value 1 for 04/2013 onwards, and 0 otherwise. Austerity is the simulated impact of austerity in *£100s* per working age person. $Crime_0$ Quartiles are created using data from 2011 and 2012. Neighborhood fixed effects and region-by-month-by-year fixed effects are included in all specifications. Additional control variables – all property-level unless otherwise specified – include dummies for new-build and leasehold, deciles of floor area of the property and the number of habitable rooms categories.