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Article

The Political Economy of Crime: Did Universal Credit Increase Crime Rates?

Matteo Tiratelli, Ben Bradford and Julia Yesberg*

*Matteo Tiratelli, Social Research Institute, University College London, 27 Woburn Square, London, WC1H 0AA, UK; m.tiratelli@ucl.ac.uk; Ben Bradford and Julia Yesberg, Department of Security and Crime Science, University College London, 35 Tavistock Square, London, WC1H 9EZ, UK.

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Interest in the political economy of crime goes back to sociology's founding fathers, but the nature of the relationship between restrictive social security systems and crime remains contested. This paper exploits exogenous variation in the introduction of Universal Credit (UC) to local areas across England and Wales to address this question. We first use fixed effects models, with a range of controls, to show that as UC enrolments increase in a given area, so does the crime rate. We then use interrupted time series analysis to show that, despite UC being rolled out at different times in different places, its introduction in each local area coincides with a positive shift in the trend in crime. These findings hold for total crime, property crime and violent crime. Borrowing from Strain Theory and Routine Activities Theory, we suggest that changes to the pool of motivated offenders may help to explain these correlations.

KEY WORDS: political economy, routine activities theory, strain theory, Universal Credit, welfare

INTRODUCTION

Questions about the role of political economy in the production of crime have troubled sociologists for nearly two centuries. In recent years, research in this area has focussed particularly on whether restricting the generosity of social security increases crime. But different pieces of evidence seem to point in different directions and the relevant theoretical mechanisms remain contested. One reason for this ongoing disagreement is the challenge of finding ways to disentangle the specific impact of social security from the complex network of relations between the state, the economy and crime.

The introduction of Universal Credit (UC) in the United Kingdompresents a valuable opportunity to attempt that difficult task. There are three reasons for this. The first is the scale of the policy. UC was arguably the most far-reaching reform to Britain's social security system since World War II: it merged the six largest welfare programmes in the United Kingdom, and

introduced a Benefit Cap that limited the maximum amount anyone could claim. Second, the complex, chaotic and politicised implementation of the policy (Timmins 2016) means that the timing of its introduction at a local level was, we argue, largely exogenous. Third, UC is representative of a wider trend in social security policy. As the centrepiece of the then Conservative government's policy agenda, UC was bound up with austerity and with a longstanding neoliberal desire to shift responsibility away from the state and towards individuals (Esping-Andersen 1990; Piven 2015). Trying to answer the question of whether UC has been associated with higher rates of crime should therefore allow us to add a rich case study to the growing body of evidence concerning the political economy of crime.

In what follows we start by explaining the central importance of political economy to sociological theories of crime, particularly Mertonian strain theory, and introduce our case study. We then describe the data and the analytic strategy, which involves exploiting exogenous variation in the rollout of UC at a local level across England and Wales[1] to test whether crime rates were affected by this new form of social security. Analysis then proceeds in two stages. We first use a two-way fixed effects framework to show that increased UC enrolments are associated with higher crime rates. Second, we use interrupted time series analysis to show that, in each local area, the introduction of UC coincides with a positive shift in the long-run trend in crime. These findings hold across various different types of crime (total crime, property crime and violence against the person), suggesting that the effects of strain extend beyond the purely economic. While this analysis cannot definitively prove that UC *caused* an increase in crime, it does provide preliminary evidence of a relationship between restricted social security and increased crime. We conclude by considering the various limitations of this study and suggesting an agenda for future research unpicking the different causal mechanisms which might link crime and social security.

THE POLITICAL ECONOMY OF CRIME

Interest in the political economy of crime goes back to sociology's founding fathers. It appears in Durkheim's work on the anomic effects of the industrial revolution (Durkheim 1893) and Marx's writings on the theft of wood and the act of primitive accumulation (Marx 1842). This interest continued for much of the twentieth century, with many theories of crime putting economic factors at their heart (e.g. Becker 1968). But, while there might be *prima facie* reasons to think that restricted welfare regimes like UC would lead to increased criminality, the empirical evidence suggests a more complex picture. Much of this work has concentrated on the United States, where there is some evidence that restrictions to social security provision have increased crime. Liebertz and Bunch (2018), evaluating the impact of *Temporary Assistance for Needy Families*, found that more stringent welfare policies were associated with higher levels of violent crime but not property crime. Worrall (2009) found that lower levels of 'general relief' were associated with higher homicide rates. Foley examined the temporal patterns of crime in twelve American cities to show that 'welfare beneficiaries consume welfare-related income quickly and then attempt to supplement it with criminal income' (Foley 2010: 97). Foley *et al.* (2010) found that, during the Great Depression, cities with higher levels of relief spending saw significantly lower levels of crime.[2] There is also evidence that non-receipt of welfare increases women's risk of offending (Monte and Lewis 2011; Thompson and Uggen 2013).

On the other hand, Corman and colleagues (2013, 2014, 2018) have shown that *The Personal Responsibility and Work Opportunity Reconciliation Act*, which aimed to reduce single parents' dependency on government benefits, led to a decline in illicit drug use and arrests for property

1 Scotland has significant numbers of devolved powers and so was excluded from the analysis.
2 Meloni (2014) found a similar pattern using data from recessions in Argentina.

crime among women. Brown *et al.* (2004), looking at a sample of social security recipients during the same period, found that, despite reductions in their benefit payments, criminal activity and substance use declined. Meanwhile, Burek (2005) found that increased levels of welfare spending were associated with an *increase* in serious property crimes. Looking at rates of recidivism, Orrick *et al.* (2011) also found that social support on its own had little impact on reoffending rates.

In the United Kingdom, the most significant work in this field has concentrated on the legacies of Margaret Thatcher's reforms to the social security system. Time series analyses have shown that reduced welfare spending at a time of crisis was associated with higher rates of property crime (Jennings *et al.* 2012; Farrall *et al.* 2017—for similar findings from the USA see Fishback *et al.* 2010).[3] More recent work using cohort studies suggests that children growing up in areas that experienced economic restructuring were more likely to have truanted from school and been in contact with the criminal justice system later in life (Farrall *et al.* 2020). Most interestingly, Thatcherism seems to have increased a whole generation's fear of crime via a political socialisation effect (Farrall 2006; Gray *et al.* 2019). This suggests an intriguing feedback loop where neoliberal economic and social policies led to increased crime, which then triggered an 'authoritarian populist' reaction (Hall 1979, 1985) and more 'conservative' law and order policies (Farrall and Jennings 2012; Jennings *et al.* 2017).

Looking across the available international evidence, a recent systematic review by Rudolph and Starke found that empirical studies 'overwhelmingly show that more generous welfare policies are associated with lower levels of crime' (Rudolph and Starke 2020: 1) - something echoed in their own work which showed that higher unemployment benefits were associated with lower homicide rates (based on panel regressions with country fixed effects for 18 OECD countries between 1990 and 2011).[4] However they also point to a weakness in these studies: they display varying levels of theoretical clarity about the causal mechanisms involved, with some focussing on social control, others on strain and others leaving the mechanism unspecified.

We argue that a promising starting point can be found in Routine Activities Theory (RAT – Cohen and Felson 1979), which argues that a crime occurs when there is a physical convergence in time and space of three elements: a motivated offender; a suitable target; and the absence of a capable guardian. RAT forms the basis for one of the most durable explanations of the long decline in crime from the 1990s to the mid 2000s (for the United Kingdom, see ONS 2019; for other countries, see Tseloni *et al.* 2010). The 'security hypothesis' argues that technological advances in the quantity and quality of security were responsible for the change in crime patterns because they reduced the number of suitable targets and altered the behaviour patterns of those who might previously have become involved in crime (Clarke and Newman 2006). By, for example, making theft of and from cars more difficult, not only were these particular crimes prevented, but the routine activities of those who might have been tempted into crime were altered, meaning they did not progress onto other forms of offending (Farrell *et al.* 2011).

The security hypothesis focusses on changes in the supply of suitable targets, but it seems plausible to suggest that the introduction of UC might have increased the supply of motivated offenders, thereby leading to an increase in crime. The most basic explanation of 'motivation' here would be economic – people are more motivated to commit crime when they can no longer afford goods they are used to purchasing. Yet, the relationship between individual poverty and crime is not always direct (Sharkey *et al.* 2017). Rather, economic factors may promote

3 However, Kim and Pridemore (2005), looking at the effect of welfare spending during recessions in Russia, find no evidence of such an association.

4 New additions to the studies covered in their systematic review should include: Rivero-Cantillano *et al* (2022) who use data from Chile to argue that increased social spending reduced homicides rates; Melander and Miotto who make a similar argument using a difference-in-difference analysis of the New Poor Law in England; and Stam et al. who apply a regression discontinuity approach to new data from the Netherlands and find that more generous social security provision reduces crime.

other situations or circumstances that increase people's propensity to offend. Indeed, the psychological literature shows that variables such as anger and negative mood increase an individual's immediate risk of committing crime (Serin *et al.* 2016).

Strain theory helps us to unpack these varied conceptions of 'motivation' and also links back to some of the earliest work on the political economy of crime. Durkheim's work on anomie (1893) made two central contributions here: first, the idea that crime was a quasi-inevitable side effect of social change, rather than a result of individual failings; second, that these side effects were generated via a weakening of social norms. However, Durkheim believed that this period of anomie would be short-lived, as a new social and political-economic order emerged to replace the old. This ran counter to the notion of a modern capitalist society as one in which there was, in Marx and Engels's famous words, 'constant revolutionising of production, uninterrupted disturbance of all social conditions, everlasting uncertainty and agitation' (Marx and Engels 1992 [1848], p. 6). But it also made it difficult to deploy strain theory as a general explanation for non-normative social actions.

Responding to these challenges, Robert K. Merton (1938) translated Durkheim's theory into a much narrower focus on the discrepancy between what society promises and what is actually available. *Strain*, therefore, arises due to failure to achieve positively valued goals. Although Merton focussed on illegal means of obtaining culturally valued ends, later theorists also acknowledged the broader emotional effects of strain (Agnew 1992). Either way, 'the basic argument… [is] that structural-level variables prevent individuals from achieving what they desire, and so they turn to illegal activities to achieve these [aims] or to express their frustrations' (Farrall *et al.* 2017, p. 223). The state has a particularly important role to play in this process. As Pat Carlen (1988, 1996) and others (e.g. Messner and Rosenfeld 1997) have suggested, a whole range of different branches of the welfare state can promote or inhibit this feeling of strain, leading to a wide range of criminal responses from basic survivalism to criminality as a source of excitement.

In what follows, we distinguish between two interpretations of strain, one direct and the other indirect. The direct, Mertonian mechanism sees people to turning to crime to achieve positively valued goals (or basic survival) that are not available to them in other ways. We assess this by investigating the effects of UC on property crime (robbery, burglary, theft and shoplifting). The indirect, Agnewian mechanism sees strain creating situations of emotional distress and psycho-social alienation, which increase the chances of an individual committing a crime. This is measured by looking at the impact of UC on violent crime (all violent crimes against the person).

THE CASE STUDY: UC

UC merged the six main existing social security payments in the United Kingdom (Jobseeker's Allowance, Employment and Support Allowance, Income Support, Working Tax Credit, Child Tax Credit and Housing Benefit). Instead of being administered separately, these benefits became part of one streamlined system delivering a single monthly payment. Those designing the policy argued that this would lead to reduced administrative costs, a simpler system, and help ensure that 'work always paid' (quoted in *The Daily Telegraph* 15 Febraury 2015). The policy was also embedded in a wider push to reduce government spending. This meant tying UC to a new 'Benefit Cap', which limited the maximum amount anyone could claim.

The implementation of UC has been beset by problems since its inception. In November 2011, an initial schedule was released according to which UC would be rolled out for new claimants between April 2013 and April 2014; and all legacy claimants would then be migrated across to the new system by the end of 2017. This was a hugely ambitious timetable, especially given

that the primary legislation setting out the policy and its regulation had yet to be passed by Parliament (Kennedy 2016). By early 2013, it was clear to the government that preparations were far behind schedule (Timmins 2016). They decided to scale back the launch to a small set of pilot sites chosen from the north west of England (the criteria for that selection have not been made public). At the last minute, three of these sites were again delayed, meaning that UC began in April 2013 in just one local authority: Ashton-under-Lynne. At first UC was only offered for simple cases where claimants met a number of 'gateway conditions' (essentially single, unemployed claimants with no children, housing claims or significant savings) (Kennedy and Keen 2018). In July 2013, this slimmed down version of UC (known as 'Live Service') was introduced to Wigan, Warrington and Oldham (also in the north west of England) and, in October, to a further ten sites around the country.

Further delays were announced in December and, by the end of the following year, the 'Live Service' version of UC only covered the north west and a few other areas of the United Kingdom. This pattern of repeated delays and amendments has continued up to the present (National Audit Office 2018). By May 2016, the 'Live Service' version of UC was offered universally (Timmins 2016), but the rollout of 'Full Service' UC for all new claimants, regardless of the complexity of their case, is still ongoing and the forced migration of legacy claimants onto the new system is not scheduled to be complete until late 2024.

For the purposes of this study, there are two crucial facts about the rollout of UC. First, the number of UC claimants in an area is almost entirely determined by the timing of the rollout in that area. By the end of the period of our study (2018), there was significant geographical variation around the country: in some areas nearly 40 per cent of claimants were on UC, in others the figure was less than 5 per cent (the United Kingdom average was 11 per cent - Kennedy and Keen 2018). This variation depends on both the order in which areas received UC (the rationale for which has never been made public) and decisions about delays or amendments to the process. Second, those repeated delays and changes to the schedule were political decisions. Rollouts are governed by pieces of secondary legislation known as 'Commencement Orders' which specify where and to whom UC should be offered (Kennedy and Keen 2018). Decisions have therefore often been made in response to political pressure, for example the political imperative to get a flag-ship scheme 'up and running' in the face of practical and bureaucratic barriers, as well as for administrative reasons (Timmins 2016). In what follows, we will argue that this complex, inconsistent and highly politicised rollout justifies our treatment of UC as an exogenous intervention, at least when it comes to its potential effect on crime.

Over the years, UC has proved controversial. The reforms impacted different groups in different ways. The Resolution Foundation calculated that, although 2.2 million working families potentially eligible for UC would be on average £41 a week better off under the new system, 3.2 million working families would lose an average of £48 a week - largely because of a substantial reduction in payments for working, single-parent families (Brewer *et al.* 2017). But despite this variation, it is clear that the introduction of UC has led to lower incomes for most claimants and higher rates of sanctioning and therefore constituted a significant constriction in the generosity of Britain's social security system (Tucker 2019).

It has also been criticised for: reducing the income of disabled people (The Children's Society 2012); triggering a rise in food bank use (Trussell Trust 2017); increasing domestic violence (Work and Pensions Committee 2018); creating disincentives to work (House of Commons Library 2016); forcing women into sex work (Work and Pensions Committee 2019); and failing on its own cost-cutting terms (National Audit Office 2018). However, the government has defended its record, arguing that the social security system needed to be simplified and that, under UC, 'people are moving into work faster and staying in work longer' (quoted in *BBC News* 6/1/2019).

At a theoretical level, it is worth noting that UC embodies a series of common neoliberal tropes that have informed policies in other parts of the world: an emphasis on reducing state assistance, encouraging work and personal and familial responsibility (Grimshaw and Rubery 2012; Cooper 2017). But its peculiarities are also salient to our argument. As well as restricting the overall generosity of social security, anyone applying for UC - as either a new claimant or someone 'transferring' from the previous system(s) – had to wait at least five weeks for their first payment and, in some trial areas, this was extended up to 12 weeks (Work and Pensions Committee 2017, Q18). As UC combines a number of major benefits, under which no payment can be made during this lead-in period, the policy can leave people with very little or no income. UC's design therefore prevents the already poor from claiming social security for at least five weeks (Schmuecker 2017: 4).

Unpicking the wider effects of UC will require a huge amount of careful research. Here we focus on just one element: the link between UC and crime. Apart from a Scottish Police Authority (2018) report, which suggested anecdotally that the reforms were fuelling robberies, there has been no published research in this area. This is somewhat surprising given the long tradition of work examining the link between political economy and crime. Nevertheless, simple descriptive analysis suggests that early adopters of UC have seen a larger increase in crime rates than later adopters (see Figure 1, which plots the percentage change in crime between 2013, when UC was first introduced, and 2018[5] against the length of time each CSP has spent under UC). Testing that association is the focus of this paper.

DATA

In order to test the hypothesis that UC led to increased crime rates, data was collected from a variety of United Kingdom government sources and compiled into a panel dataset covering each Community Safety Partnership (CSP) in England and Wales, for every month from

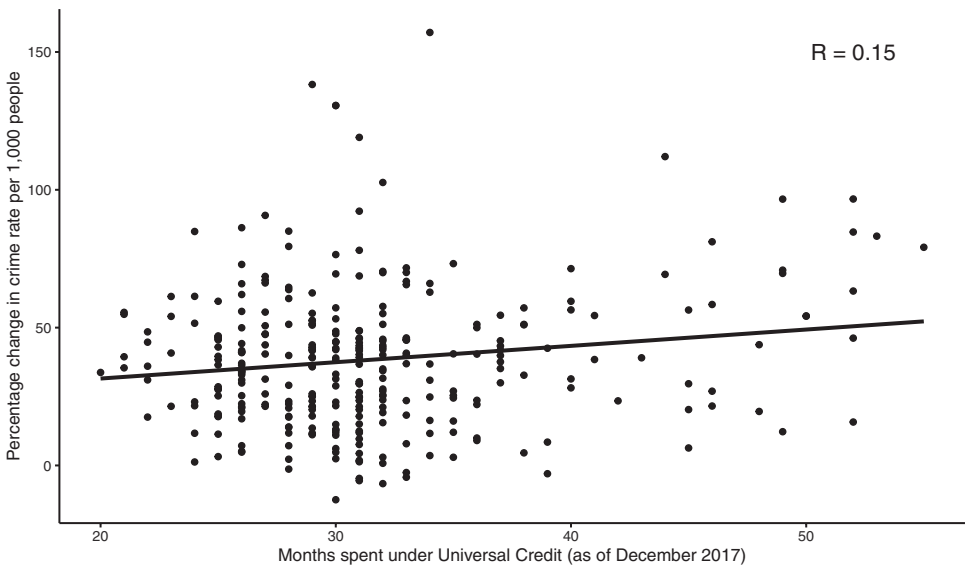

Fig. 1 The change in crime rates by months spent under UC.

5 We compared May 2013 and June 2018 and transformed those figures into 12-month rolling averages to prevent seasonality or other temporary shocks from distorting the results.

January 2013 to June 2018. In the vast majority of cases CSP boundaries map onto local authority boundaries (the areas which were tasked with introducing UC), but some CSPs represent a merging of local authorities and so we refer to CSPs throughout the analysis.[6] Ideally, we would have examined a longer time period but, unfortunately, consistent measures of unemployment and local government spending were not readily available before 2013. Nevertheless, for most CSPs, we have more than two years' worth of data before UC is introduced. More importantly, all the analysis below was replicated using data going back to January 2010 (and dropping those two control variables), producing very similar results.

Dependent variable: *Crime* - all police recorded crime excluding fraud[7]; property crime (robbery, burglary, theft and shoplifting); and violence against the person. All data from the Home Office, recorded at CSP level for each month. Police recorded crime was chosen in preference to the *Crime Survey for England and Wales* because that survey is designed to produce estimates at higher levels of geographic aggregation (national, regional and police force area), making it much harder to exploit exogenous variation in the local introduction of UC. It is also a victimisation survey and so excludes many crimes relevant in the current case (e.g. shoplifting). There is of course no perfect measure of crime, and all sources are prone to biases which may be unstable over time and space. In our analysis, steps were taken where possible to control for variations in recording practices (notably, using CSP and time period fixed effects). Nevertheless, the possibility of bias remains a serious concern for all research based on official crime statistics and this is a central limitation of our study. It is important to note, however, that measurement error would only bias our estimates if they themselves were correlated with the relationship between UC and crime at the local level (something we discuss in the conclusion). Moreover, there is an important and irreconcilable trade-off here: causal inference from pre/post studies demands long time periods; but the longer the time period, the greater the risk of significant shifts in recording practices.

Independent variable: *UC claimants*[8] - the total number of UC claimants in a given area per month; data from the Department for Work and Pensions. This allows us to examine the effect of the policy as a whole on local areas and to accurately track the level of UC enrolment as people transition into and out of the system (we are not able to measure individuals directly). This measure is also preferable to using a dummy variable because the policy is implemented gradually in each local area. In the first few months there were often less than ten people enrolled on UC in any given area, making it implausible to test for an effect at the macro-level based simply on a binary variable. (To aid with the interpretation of effect sizes in the two-way fixed effect models, we also take the proportion of all claimants using the UC system as our independent variable.)

Controls

- *Police officer numbers*: this is the best available measure of police capacity and was linearly interpolated to generate monthly figures from the Home Office's annual figures (at Police Force level).

6 Excluding the Isles of Scilly, there are 310 CSPs in England and Wales. In most cases they align with Local Authority boundaries, but some Local Authorities were merged into single CSPs. Some local government areas also only reported annual expenditure at a larger geographic level (e.g. Dorset) and so some CSPs had to be merged together. The final number of units was 305.

7 Since the early 2010s (although the precise date varied from area to area) fraud has been recorded by a separate government agency (Action Fraud). It is therefore routinely excluded from analysis of police recorded crime figures to preserve consistency over time. Our decision to exclude fraud therefore follows official guidance.

8 We are aware that UC itself has become less generous over the years. For the purposes of this analysis, we treat it as a homogenous treatment but, in this sense, we may underestimate the effect of UC *in its current form* on crime.

- *Unemployment*: the Office for National Statistics' preferred measure of unemployment (Alternative Claimant Count), recorded at monthly level for each Local Authority.
- *Legacy benefit claimants*: the Office for National Statistics' measure of Benefit Combinations is used to control for other welfare recipients (including people claiming social security through the pre-UC system), recorded quarterly and linearly interpolated to monthly data points.
- *Lagged total crime at national level*: to control for diffusion effects and wider fluctuations in crime driven by nationwide changes (e.g. austerity at national level or changes to national recording practices).
- *Local government service expenditure*: annual figures from the Ministry of Housing, Communities and Local Government, adjusted for inflation via the Office for National Statistics' GDP deflator, then linearly interpolated to create monthly figures.[9]

All control variables were transformed into rates per 1,000 people using the Office for National Statistics' mid-year population estimates (linearly interpolated to generate monthly figures for each CSP).

Statistical controls

- *CSP fixed effects*: to control for any time-invariant factors (including different police recording practices).
- *Monthly dummies*: to control for seasonality, i.e. January, February, March etc.
- *Time period fixed effects*: a non-parametric control for any changes over time which are common to all CSPs.

Crime is often referred to as being 'overdetermined', in that it responds to a huge variety of different variables (Hutson 2020). The point of this analysis, however, is not to construct a model to explain crime in general, but rather to test a specific hypothesis. In that context, missing variables are only relevant if they confound the relationship between UC and crime, in which case our estimates would be biased. Indeed, given the risks of 'bad controls' (Angrist and Pischke 2009; Elwert and Winship 2014), adding variables indiscriminately to address the issue of overdetermination might introduce more problems than it solves. Nevertheless, these all represent potential limitations of the results presented below.

Over the last decade, police recorded crime rates have followed a roughly quadrilateral trend, falling from 2010 until 2014 and then slowly rising (Figure 1). However, there is also significant variation across the country, with some areas experiencing crime rates several orders of magnitude larger than others (Table 1). Meanwhile, the rollout of UC starts to escalate from the middle of 2014, reaching over 800,000 claimants by the middle of 2018 (Figure 1). There is therefore significant variation both between places and across time. Figure 2

9 We examined spending by Unitary Authorities and District Authorities (ignoring the County Councils of which they form a part). This is not ideal because Unitary Authorities and District Authorities have different powers and responsibilities, which makes it difficult to compare between different places (Institute for Fiscal Studies 2016). However, the only way to correct this would be to aggregate all District Authorities into much larger County Councils; a strategy which has its own problems. As fixed effect models look at the effect of changes *within* units, the difference between different forms of local government should not be too troubling.

ANALYTIC STRATEGY

The first key aspect of our analytic strategy is the claim that the introduction of UC to each local area was an exogenous intervention, specifically an exogenous reduction in the generosity of welfare entitlements in that area. As described above, UC was originally planned to be rolled

Table 1. Descriptive statistics per CSP per month (all CSPs in England and Wales, 2013 - 2018)

	Minimum	Median	Mean	Maximum
Crime rate per 1,000 population	0.05	5.04	5.6	84.2
Violence against the person rate per 1,000 population	0.00	1.2	1.4	15
Property crime rate per 1,000 population	0.00	3.3	3.7	75.7
Universal Credit claimants per 1,000 population	0	0.78	3.6	81.7
Police Force numbers (at Police Force Area per month) per 1,000 population	1.4	1.8	2.4	127.2
Unemployment per 1,000 population	0.007	18.5	20.5	79.6
Annual local government expenditure (£1,000s, 2017–18 prices) per 1,000 population	160.6	596.5	1,306.2	31,019.1

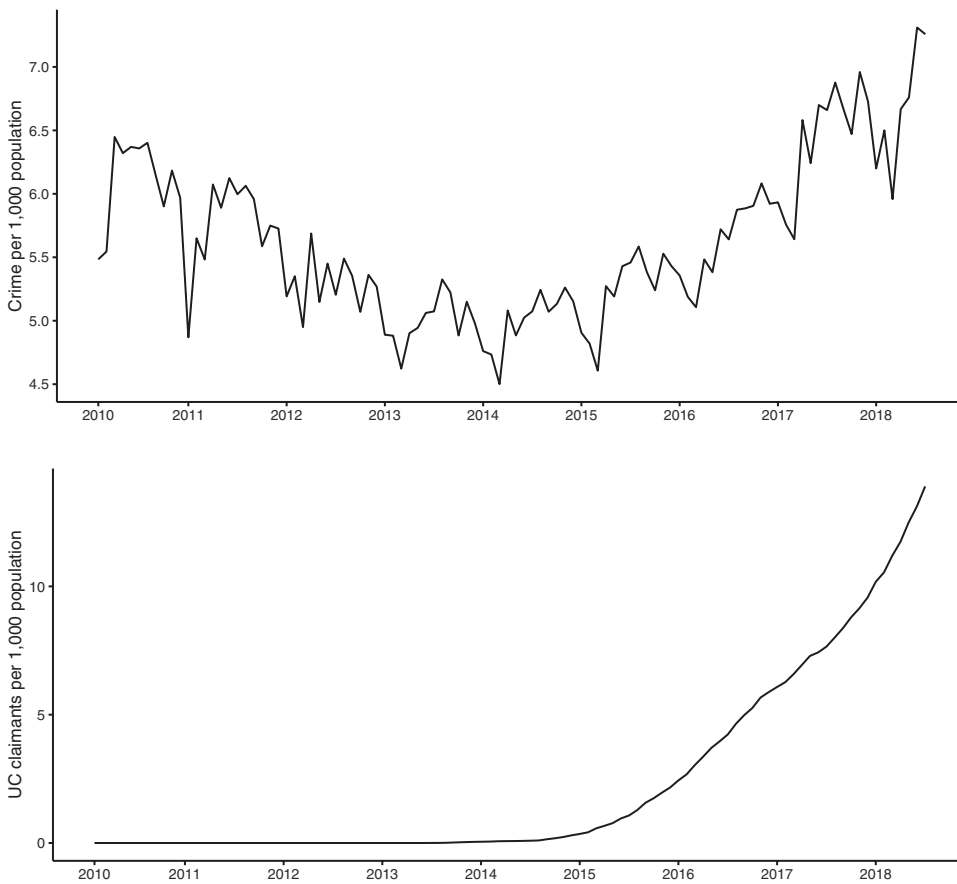

Fig. 2 Trends in crime and UC rates.

out over four years, and selected Jobcentres began offering it in 2013, focussing at first on simple cases. But the rollout was repeatedly and inconsistently delayed for political and administrative reasons. In essence, the rollout of UC has been such a complex, piecemeal process that we feel justified in treating it as an exogenous intervention, at least as far as crime rates are concerned. To further test this premise, we investigated whether the timing of the rollout in each area was correlated with underlying crime rates. We conducted a traditional survival analysis, with a linear trend and CSP-fixed effects, and found that crime rates did not predict the introduction of UC (Supplementary Appendix 1). Therefore, by exploiting exogenous variation in the introduction of UC, we should be able to offer a more robust identification of the causal effects of these reforms on local areas than simple correlations normally allow. However, this was not a perfect randomisation and, as mentioned above, the rationale behind the timetables, delays and schedules may never be known to us. Alternative analytic approaches and alternative data sources will therefore be needed to confirm or disprove our findings.

Second, this is an ecological analysis in that we look at the effect of UC on a given area, rather than directly measuring its impact on individuals. There are limitations to this ecological approach, which are discussed at greater length in the conclusion, but, unfortunately, individual-level data was unavailable. It is also important to reiterate that the introduction of UC is an exogenous shock *to each area*, not to individuals. The introduction of UC therefore represents an exogenous reduction in the generosity of the social security system in each local area. The effect of this intervention then builds up gradually as more and more people enrol on UC every month.

Third, both of the hypotheses listed below were applied separately to total crime, property crime and violent crime. Property crime and violent crime here reflect the two different interpretations of 'strain' that were discussed above. In the first, strain leads people to find illegal means of obtaining culturally valued ends and hence, we argue, to acquisitive crime (Merton 1938). In the second, strain leads to broader emotional and psychological stresses and hence is potentially also related to non-acquisitive, violent crimes (Agnew 1992).

Our analysis focusses on testing two hypotheses:

H1) More UC claimants leads to higher levels of crime

This tests the short-term impact of higher numbers of UC claimants on crime rates within each CSP and was assessed within a two-way fixed effects framework (Allison 2009; Wooldridge 2010; Goodman-Bacon 2021; unfortunately, more advanced matching approaches for time-series cross sectional data [e.g. Imai *et al.* 2021] can only be used with binary treatment variables, which are inappropriate in the context of the gradual increase in UC claimants over time). The resulting equation was:

$$Y_{it} = \mathbf{A}_i + b_1 X_{it-1} + \mathbf{D}_{it} + \mathbf{M}_t + \mathbf{T}_t + e_{it}$$

where Y_{it} is the crime rate in CSP *i* at time *t*, $\mathbf{A}_i$ is a vector of CSP fixed effects, X_{it-1} is the number of UC claimants per 1,000 people in CSP *i* at time *t-1*, $\mathbf{D}_{it}$ is a vector of control variables (police officer numbers, unemployment, local government expenditure, legacy benefit claimants and the lag of total national crime, all as rates per 1,000 people) in CSP *i* at time *t*, $\mathbf{M}_t$ is a vector of monthly dummies (one for e.g. January, February, March etc), $\mathbf{T}_t$ is a vector of time period fixed effects (i.e. a dummy variable for every time period) and e_{it} is the error term. b_1 is, therefore, the coefficient of interest.

The advantage of this two-way fixed effects set up is that it controls for all time-invariant unobserved factors as well as all temporal shocks which are common across units.

Our second hypothesis then examines the long run impact of UC:

H2) The rollout of UC changes the trend in crime rates

This tests whether the introduction of UC to an area coincides with a change in the trend in crime. We employed a comparative interrupted time series framework including all CSPs in England and Wales (Betts *et al.* 2010), resulting in the following equation:

$$Y_{it} = \mathbf{A_i} + b_1 X_{it} + b_2 T_{it} + b_3 (X_{it}.T_{it}) \quad + \mathbf{D}_{it} + \mathbf{M}_t + \mathbf{T}_t + \quad e_{it}$$

where Y_{it} is the crime rate in CSP *i* at time *t*, $\mathbf{A}_i$ is CSP fixed effects, X_{it} is a dummy variable indicating whether UC has been introduced to CSP *i* at time *t*, T_{it} is a linear time trend centred at zero in the month when UC was introduced to a given area, $(X_{it}. T_{it})$ represents the change to trend after UC was introduced, $\mathbf{D}_{it}$ is a vector of control variables (police officer numbers, unemployment, local government expenditure, legacy benefit claimants and the lag of total national crime, all as rates per 1,000 people) in CSP *i* at time *t*, $\mathbf{M}_t$ is a vector of monthly dummies (January, February, March etc), $\mathbf{T}_t$ is a vector of time period fixed effects (i.e. a dummy variable for every time period) and e_{it} is the error term. b_3 is, therefore, the coefficient of interest.

Due to the presence of heteroskedasticity, cross-correlation and autocorrelation, we use consistent covariance matrix estimation to generate robust standard errors in all cases (Driscoll and Kraay 1998).

RESULTS

Hypothesis 1: Short term impact

We first tested whether higher numbers of UC claimants were associated with higher crime rates (H1) using a fixed effects approach. For each of the three dependent variables there is a significant positive association (Tables 2 and 3), which persists after controlling for the potential confounders listed above. (The exception is Model 10 looking at the correlation between the UC ratio and property crimes.)

These effects are modest but not insignificant. To aid with the interpretation of the effect sizes, we also used the proportion of claimants being processed through UC as our independent variable. This suggests that the full implementation of UC would nearly double the total crime rate in a given area (see models 7 and 8). As UC has yet to fully replace the legacy social security system anywhere in the United Kingdom, this obviously represents an extrapolation outside of the range of our data. But for each ten-percentage point increase in the ratio of UC to legacy claimants, our models predict an additional 0.4–0.5 total crimes per 1,000 people per month. The effect of violent crime would be even larger in relative terms. We can also quantify the impact of UC by looking at it as a proportion of the average increase in crime from 2013 to 2018. By 2018, there were 14 people claiming UC per 1,000 in each CSP, while the crime rate had risen by 2.2 additional crimes per 1,000 people since 2013. Our models suggests that the introduction of UC might have accounted for 18 per cent of that increase.[10] Although this is by no means conclusive evidence of a causal effect, these results indicate the potential significance of political economy for the study of crime.

The main results are robust to a variety of alternative specifications including: using a much longer run of data (2010-–2018)[11]; excluding Birmingham (by far the largest CSP); excluding the Metropolitan Police Force area (which had difficulties accurately recording the location of crime from 2014–2017); and replacing time period fixed effects with a linear or quadrilateral trend.[12] See Supplementary Appendix 2 for details.

10 By the end of the period considered here (June 2018), there were on average 14 people claiming UC per 1,000 people per CSP. According to our estimates, that would equate to an additional 0.4 crimes per 1,000 people, which is 18 per cent of the average increase from June 2013 (2.2 additional crimes per 1,000 per CSP).

11 Due to issues with the availability of data this requires dropping the unemployment, legacy benefit claimants and local government spending controls.

12 The coefficient of interest for property crime becomes non-significant when we use quadrilateral trends over time.

Table 2 The impact of UC claimants on crime rates, all CSPs in England and Wales, 2013–2018 Two-Way Fixed Effects (TWFE)

	Total crime per 1,000 population		Property crime per 1,000 population		Violent crime per 1,000 population	
	(1)	(2)	(3)	(4)	(5)	(6)
Universal Credit claimants per 1,000 population (lagged)	0.021*** (0.003)	0.026*** (0.007)	0.004*** (0.001)	0.018*** (0.004)	0.010*** (0.002)	0.007* (0.003)
CSP fixed effects	✓	✓	✓	✓	✓	✓
Time period fixed effects	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Observations	20,195	19,520	20,195	19,520	20,183	19,512
Adjusted R^2	0.447	0.518	0.081	0.253	0.654	0.662

Note: Coefficients with robust standard errors in brackets. Estimated via OLS in R version 4.1.2.
Controls: Police officer numbers, Unemployment, Legacy benefit claimants, Total crime at the national level, Local government service expenditure (all as rates per 1,000 people) and Monthly dummies.
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$

Table 3 The impact of the proportion of claimants using UC on crime rates, all CSPs in England and Wales, 2013–2018 (TWFE)

	Total crime per 1,000 population		Property crime per 1,000 population		Violent crime per 1,000 population	
	(7)	(8)	(9)	(10)	(11)	(12)
Universal Credit claimants as proportion of all claimants (lagged)	5.325*** (0.610)	3.942*** (0.636)	0.973** (0.366)	-0.380 (0.518)	2.620*** (0.403)	2.377*** (0.445)
CSP fixed effects	✓	✓	✓	✓	✓	✓
Time period fixed effects	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Observations	19,520	19,520	19,520	19,520	19,516	19,516
Adjusted R^2	0.439	0.524	0.084	0.245	0.644	0.657

Note: Coefficients with robust standard errors in brackets. Estimated via OLS in R version 4.1.2.
Controls: Police officer numbers, Unemployment, Total crime at the national level, Local government service expenditure (all as rates per 1,000 people) and Monthly dummies.
$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

Hypothesis 2: Long term impact

We then tested the second hypothesis: that the introduction of UC to an area coincides with a positive shift in the trend in crime. This allows us to assess whether UC had an effect on the long-term dynamics of crime, rather than just a month-on-month association. A visual inspection of total crime rates for the average CSP suggests there may be a connection and that the change in trend could be dramatic (Figure 3). However, in order to test this more rigorously, we employed interrupted time series analysis and added a series of control variables.

As shown in Table 4, the introduction of UC to a given area is correlated with a significant, positive (i.e. upwards) shift in the long-term trend in crime rates. This effect persists despite controlling for unemployment, total national crime, police officer numbers, local government

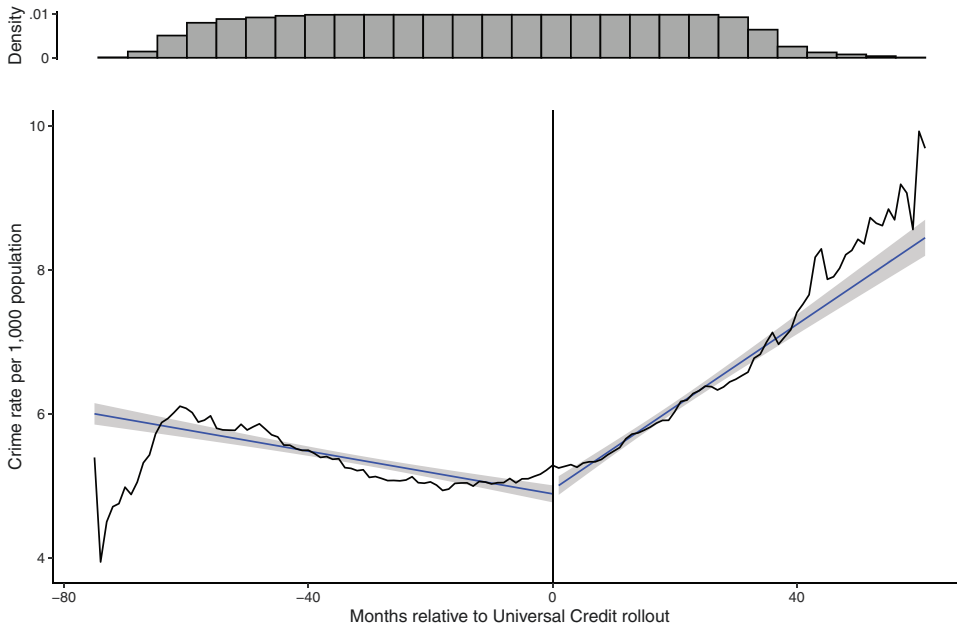

Fig. 3 The trend in crime before and after UC.*Notes*: The black line tracks the average crime rate across CSPs before/after the introduction of Universal Credit. The two blue lines fit a linear trend for all CSPs before/after the introduction of UC. The top panel shows the distribution of observations (rearranging the data by months before/after the introduction of Universal Credit results in an unbalanced dataset - there are therefore very few observations at either end of the x axis).

spending, legacy benefit claimants and calendrical time via time period fixed effects. We then add a variable to control for the rate of UC claimants in each CSP. For property crime and total crime this has the expected effect of reducing the size of the coefficient for the change to trend, showing that the correlation is mediated by the numbers enrolled in UC itself. It has no significant effect on the coefficient for violent crime. It is also worth pointing out that, although the effect of the UC dummy is strongly negative, this is an artefact of the statistical model, not a real, causal effect. It simply represents the linear extrapolation of the trend after the new policy was introduced back to the origin.[13]

These results are again robust to a variety of alternative specifications. We first experimented with shrinking the time frame of our analysis: repeating the analysis on progressively smaller subsets of the data, from the full period to eighteen months from January 2013 i.e. when UC was first introduced. The coefficient of interest remains positive and significant for all three crime types. For total crime and violence, the findings were also robust to: using data back to 2010[14]; dropping Birmingham; dropping the Metropolitan Police Force area; and applying the Bonferroni correction for multiple comparison testing (Esarey and Sumner 2018). However, the findings for property crime were less robust and only remain significant when dropping Birmingham from the analysis. See Supplementary Appendix 3 for details.

13 The model fits a straight line to the data for the outcome variable over time after the introduction of UC. The coefficient for the UC dummy therefore represents the extrapolation of that trend line backwards to the vertical line in Figure 3 (i.e. to the first moment when UC was introduced).

14 Due to issues with the availability of data this requires dropping the unemployment, legacy benefit claimants and local government spending controls.

Table 4 The long-term impact of UC on the trend in crime, all CSPs in England and Wales, 2013–2018 (Comparative Interrupted Time Series Analysis)

	Total crime rate per 1,000 population		Property crime rate per 1,000 population		Violent crime rate per 1,000 population	
	(13)	(14)	(15)	(16)	(17)	(18)
Change to trend after UC rollout	0.020*** (0.004)	0.015*** (0.004)	0.005* (0.003)	0.002 (0.002)	0.008*** (0.001)	0.007*** (0.001)
Trend before UC rollout	0.006 (0.003)	0.009** (0.003)	0.003 (0.002)	0.005** (0.002)	0.005*** (0.001)	0.006*** (0.001)
UC dummy	-0.189*** (0.047)	-0.180*** (0.047)	-0.027 (0.020)	-0.020 (0.020)	-0.56** (0.018)	-0.054** (0.018)
UC claimants per 1,000 population (lagged)		0.022** (0.007)		0.018*** (0.004)		0.005 (0.003)
CSP fixed effects	✓	✓	✓	✓	✓	✓
Time period fixed effects	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	19,520	19,520	19,520	19,520	19,512	19,512
Adjusted R^2	0.519	0.521	0.249	0.253	0.663	0.664

Note: Coefficients with robust standard errors in brackets. Estimated via OLS in R version 4.1.2.
Controls: Police officer numbers, Unemployment, Legacy benefit claimants, Total crime at the national level, Local government service expenditure (all as rates per 1,000 people) and Monthly dummies.
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

CONCLUSION

While it is impossible to comprehensively prove causation from a single, observational study, our results provide salient and plausible evidence linking UC to an increase in recorded crime. Not only does the crime rate increase as the number of claimants rises, but the introduction of UC to each area also coincides with a shift in the long run trend in crime. Interestingly, these results seem to hold for violent crime as well as acquisitive property crime (if anything, the findings for the latter are less robust), suggesting that the effects of strain extend beyond the purely financial. This should serve to remind us of the importance of political economy in shaping the level of crime and echoes many of the empirical findings mentioned above, which showed that restricted welfare regimes can lead to increases in crime.

There are, inevitably, limitations to the empirical case study presented in this paper. Most importantly, this is an ecological study and we cannot be certain that it is UC claimants themselves who are responsible for the increase in crime rates (Selvin 1958). Indeed, we might expect the effects of the policy to ripple out through families and social networks. Unfortunately, data on individuals was not available in this case but, in the future, it may be possible to use cohort studies to probe individual-level effects. Second, despite our efforts to control for variation in recording practices, our analysis ultimately relies on police recorded crime figures, which are inevitably problematic (Public Administration Select Committee 2014). However, those sources of measurement error would only bias our estimates if they were themselves correlated with the relationship between UC and (recorded) crime. Third (and relatedly), another possibility is that UC made *victims* more likely to report crime, for example in order to claim insurance. However, it is difficult to see a plausible mechanism which would produce the large

effect we observed on non-property crimes, where insurance claims are less relevant. Fourth, our models show the average effect across different areas and so ignores local heterogeneity which may be useful from a policy perspective.

Much work also remains to be done exploring the causal mechanisms linking UC and crime. We have framed our analysis in terms of strain theory, arguing that restricting the generosity of social security payments may prevent people from achieving culturally valued goals leading, potentially, to a criminal response from some. But there are also several alternative mechanisms, many of which we could not evaluate directly because of data limitations. First, it is possible that the correlations we see are due to the disruption caused by the *rollout* of UC to Jobcentres, rather than any reduction in generosity. We cannot totally discount this possibility. However, given that the effect seems to persist beyond the first few months of the rollout, we feel it is less plausible. Second, the widespread controversy surrounding UC may have undermined the *legitimacy of the government and its agencies*, thus leading to an increase in crime without necessarily increasing strain. Although you might expect such a mechanism to play out at the national level (and thus be controlled for in our analysis), further study would be needed to fully evaluate it.

Third, it is often suggested that restricted social security systems might undermine *social support and control* (Shaw and McKay 1942; Cullen 1994). These theories suggest that crime can be prevented by dense social networks which allow behaviour to be monitored and encourage identification with and internalisation of moral codes. The implication is that policies like UC might disrupt those networks, thus undermining informal processes of social control. However, this is extremely difficult to test within the framework used here. Levels of social organisation (the key mediating variable) are normally measured through residential churn (e.g. Kawalerowicz and Biggs 2015) or through locally registered non-profits (e.g. Sharkey *et al.* 2017). However, the former can only be calculated from census statistics, which are only available every ten years, and the second is also unavailable at the monthly level.[15] Developing robust measures across time and space would help to test this relationship but, unfortunately, that lies beyond the scope of this paper. This points to a more general problem with using quantitative methods to examine the underlying mechanisms linking crime and social security: lack of suitable measures and accurate, individual-level data. We might therefore consider turning to rigorous, qualitative research to tease out these causal connections.

Despite these limitations, this paper presents robust, preliminary evidence that the introduction of UC is correlated with increased crime rates, a finding with significant theoretical and policy implications. Theoretically, our findings underline the importance of the political economy of crime. As suggested by strain theory, UC seems to have exacerbated the discrepancy between what society promises and the standard of living that is actually available, increasing the pool of motivated offenders and the level of crime. From a policy perspective, these results demonstrate the need to think about policing and crime within the larger social system. The impact of UC on crime could, according to our models, be larger thanx that for hotspot policing (Braga *et al.* 2014; Sherman and Weisburd 1995), stop and search (Tiratelli *et al.* 2018), or the use of CCTV (Welsh and Farrington 2009). This suggests that effective ways of reducing crime might be found outside of policing – an important lesson for policy makers in the United Kingdom and beyond.

SUPPLEMENTARY DATA

Supplementary data are available at *British Journal of Criminology* online.

15 Although the Charity Commission does have monthly data on the number of charities, their annual expenditure and their registered addresses, significant changes to registration practices make it impractical to use as a measure over time.

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