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IZA DP No. 13484

Universal Credit and Crime

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ABSTRACT

Universal Credit and Crime*

We evaluate the criminogenic effects of Universal Credit (UC), a monumental welfare reform designed to radically change the social security payment system in the United Kingdom. We exploit the UC rollout across constituencies using monthly data from 2010 to 2019 for England and Wales. We find UC has caused around 45,000 burglaries, with criminogenic effects lasting until the end of the sample period and expected to grow considerably by the time UC is fully rolled out when 6.5 million more individuals will use the system. The analysis suggests the worsening of benefit recipients' financial conditions as the key mechanism.

JEL Classification: K14, K42

Keywords: universal credit, benefits, welfare system, crime

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* This paper builds upon, improves, and supersedes Alex Harvey's MSc in Economics Dissertation at The University of Sussex (Academic Year 2018/2019).

1. Introduction

Universal Credit (UC) is a monumental welfare reform that has drastically changed the social security payment system in the United Kingdom. The main objective of Universal Credit is to reduce welfare dependency through greater incentives for claimants to enter employment and take greater responsibilities for their finances. UC is designed to replace six benefits for working-age people with low-income, currently administered separately. It has introduced major changes to the previous benefit schemes. Including a fully digitised service, monthly payments in arrears rather than prospectively each week or fortnight, increased conditionality, a tougher regime of sanctions, and reduced payments to some claimant groups (Brewer et al., 2019). The impact of Universal Credit has been widely criticized and it has generated enormous controversy. Mounting evidence in sociology, political science, and medical literature suggests Universal Credit has led to increased food bank usage, consultations in general practices, landlord repossession rates, and created mental health difficulties amongst claimants (Arie, 2018; Loopstra et al., 2018; Cheetham et al., 2019; Hardie, 2020; Wickham et al., 2020).

This paper contributes to the Economics literature by providing the first empirical evaluation of the criminogenic effects of Universal Credit based on quasi-experimental methods and high-frequency data. In particular, we exploit the staggered rollout of the welfare reform across parliamentary constituencies using monthly data for all constituencies in England and Wales from December 2010 to February 2019. We conduct difference-in-differences and instrumental variable (IV) analysis that allow to identify credible estimates of the impact of Universal Credit on crime.

We argue our analysis is of timely importance for two reasons. Firstly, UC will directly affect the lives of around eight million low-income benefit recipients by the time it is fully rolled out in 2024 (Kennedy and Keen, 2018). Therefore, even minor individual criminogenic effects will likely have major societal implications due to the growing number of recipients soon to be exposed to the new system. Second, other countries are evaluating possible reforms to their benefit schemes mirroring the new practices adopted by Universal Credit (Wickham et al., 2020). Arguably, the need for such reforms has recently intensified due to the extreme pressure that welfare systems are experiencing worldwide because of the health pandemic. Thus, a credible evaluation of the criminogenic impacts of Universal Credit is critical as it can inform policy makers across the globe regarding possible criminal consequences of analogous welfare reforms, and it can help designing *ad hoc* measures that minimize these unwarranted effects.

We find Universal Credit has caused around 45,000 burglaries since its implementation. Effects are precisely estimated and hold against numerous robustness checks such as including an extensive set of socio-economic covariates, constituency-specific linear and quadratic trends, constituency-by-month fixed effects, changes to the sample of analysis and the functional forms used. Also, a placebo test randomly permutes the month of adoption across constituencies and confirms that the criminogenic effects we detect are not driven by spurious correlations.

An event study analysis provides further reassurance regarding the validity of our difference-in-differences design and it reduces the concerns 'pre-trends' in crime might be confounding the causal interpretation of our estimates, or inflating the results. This analysis also shows the criminogenic effects of the reform emerge 'on impact', grow over time, and last until the end of the sample period. These long lasting effects are not surprising given the increase in the number of benefit claimants exposed to the new reform that has grown from 220,000 in April 2016, when Universal Credit was first offered in all constituencies but only to a selected number of applicants, to over 1.5 million claimants in February 2019, the final month in our sample.

This evidence leads us to investigate the criminogenic effects at the intensive margin, that is the number of benefit recipients enrolled in the new welfare system. A simple Ordinary Least Square (OLS) regression could be biased because of the possible correlation between changes in the number of UC claimants and unobservable crime determinants. We address endogeneity concerns by implementing an IV design exploiting quasi-experimental variation in the number of claimants provided by the staggered rollout of the program across constituencies. Compared to the findings in the literature, we find moderate crime elasticities, precisely estimated (Draca and Machin, 2015). This suggests small individual criminogenic effects driven by a minority of Universal Credit recipients. Results are robust to the inclusion of an extensive set of socio-economic covariates and fixed effects, reducing the concerns our IV estimates may be biased upward due to violation of the exclusion restriction.

Overall, the evidence points toward a unique conclusion. The criminogenic effects of Universal Credit are likely due to a substantial worsening of the financial conditions of some UC recipients: This has increased their incentives to commit crime to supplement their income. The income loss is likely due to the regressive distributional effects of Universal Credit (Brewer et al., 2019); the payment delays that arguably hit harder poorer recipients at the bottom of the distribution; and the increased number and severity of sanctions towards young males reducing income available to a subpopulation of claimants likely characterized by high criminal propensity (Grogger, 1998).

Our findings are important for policy makers and academics alike. The estimated crime elasticity and the upward trajectory of the criminogenic impacts highlight the urgency of addressing underlying issues responsible for the rise in crime. In fact, around 6.5 million more low-income individuals will use the new system by 2024. Arguably, this increases the likelihood of observing larger criminogenic impacts in the near future. It also outlines the importance of considering measures aimed at re-balancing the incentives of benefit recipients, particularly of those belonging to categories at risk of high criminal propensity. This could be achieved by smoothing the distributional impact of Universal Credit as it is currently affecting recipients at the bottom of the income distribution to a greater extent (Brewer et al., 2019). Undoubtedly, the decision to implement such measures will have to take into account other possible individual responses, such as those along the employment margin, not examined in this paper. However, we are convinced that our findings provide impetus for government measures aimed at minimizing the inappropriate operational practices of Universal Credit, such as payment delays, that are unnecessarily affecting the financial position of poor beneficiaries at the margins of crime.

Related Literature. By providing the first empirical evaluation of the criminogenic effects of Universal Credit, our paper contributes to the existing literature examining the effects of welfare systems on crime. Our results are consistent with Machin and Marie (2006) who study the impact of Jobseeker's Allowance (JSA) in the UK labour market in 1996. JSA simplified the employment benefit system and brought a more stringent, tougher regime. The researchers use quarterly data for 45 areas in England and Wales and employ both quasi-experimental designs and qualitative evidence to show the reform generates an increase in crime. Foley (2011) exploits plausibly exogenous variation in the timing of benefit payments from the Food Stamp Program across twelve US cities. Temporal patterns in crime are observed in jurisdictions where disbursements happen with monthly-frequency, but not in jurisdictions in which disbursements are relatively more staggered, suggesting benefit recipients consume welfare related income quickly and then commit crime to overcome hardship later in the month. Carr and Packham (2019) look at the impact on crime of the Supplemental Nutrition Assistance Program (SNAP)—an in-kind transfer providing food-purchasing assistance for around 45-million low income Americans each year. Similar to Foley (2011), they find the timing of nutritional aid disbursement has an effect on crime, and staggering benefits disbursement over the month leads to large reductions in theft crimes at grocery stores. Universal Credit has also altered the frequency of payments, from weekly or fortnightly, to monthly. Therefore, this can be a possible channel behind our results. However, we note UC still allows payments to be staggered during the month in a wide variety of circumstances. In fact, this happens in the most problematic cases, when the 'work coach' identifies the claimant has addiction problems, rent arrears, was previously homeless, or any situation where the claimant request it with a valid reason (Department for Work and Pensions, 2020). This leads us to believe that the change in payment frequency may not be the major force behind our results.[3]

Our analysis detects the criminogenic effects of Universal Credit, a welfare program directly affecting the lives of low-income individuals. We also show effects are stronger when only unmarried, unemployed, younger claimants without children are admitted to the program, and in constituencies with house prices below the national median. Therefore,

[3] Another compelling analysis is conducted by Bindler (2016) who studies the relationship between unemployment benefits, labour market conditions and crime in the light of increasing unemployment durations and temporary benefit extensions in the US. Interestingly, she detects a longer time in benefit leads to an increase in criminal activity. This is because longer benefits increase unemployment durations that, in turn, contributes to the increased criminal propensity.

our paper contributes to the literature examining economic incentives affecting individuals at the bottom of the income distribution with high-criminal propensity, such as young unskilled individuals or individuals with criminal records. Grogger (1998) presents a complete picture of the choice to supply labour to either the illegal or the formal labour market. He focuses on the National Longitudinal Survey of Youth (NLSY) cohort data and finds a high-proportion of individuals committing crime are also employed in the labour market, and are therefore sensitive to changes in legal wages. Gould et al. (2002) investigate the effects of unemployment and wages on the criminal behaviour of less educated young males using a Bartik type of approach. They find both wages and unemployment are significantly related to crime, but wages have larger criminogenic effects. Machin and Meghir (2004) examine the extent to which crime is related to the worsening of labour market conditions of less skilled workers in England and Wales from the mid-1970s to the mid-1990s. They use a wage measure based on the 25th percentile of the distribution for the retail trade sector as this sector is a major employer of low-skill workers and show changes in wages at the bottom of the distribution lead to significant changes in crime. Our findings, showing an increase in acquisitive crimes likely due to the worsening of benefit recipients' financial conditions associated with the introduction of Universal Credit, are consistent with all these results.[4]

This paper unfolds as follows: Section 2 describes the institutional background; Section 3 presents the data; Section 4 discusses the research designs; Section 5 reports the results; Section 6 discusses underlying channels; Section 7 concludes.

[4] Other work in the area includes Bell et al. (2014) who show recessionary conditions at the point of school exit influence participation in crime by comparing outcomes across cohorts using both US and UK data. Schnepel (2018) finds increases in construction and manufacturing working opportunities at the time of release from prisons are associated with significant reductions in recidivism. For a comprehensive review of the impact of economic incentives on criminal activity see Draca and Machin (2015).

2. Institutional Background

UC Main Features. Universal Credit is a UK social security payment system legislated in the Welfare Reform Act 2012. It is designed to fully replace the still functioning 'Legacy system', by combining six benefits for working-age people with low income that are currently administered separately.[5] The integration of many benefits into one single payment aims to simplify the payment process by avoiding multiple claims to different government departments, also arising when claimants' circumstances change.

Other than attempting to create a unified, more flexible system, Universal Credit aims to incentivise claimants to take greater responsibilities for their finances and, whenever possible, enter employment and reduce welfare dependency. To achieve these objectives, Universal Credit has radically transformed various key features of the Legacy system, which is scheduled to disappear by the end of 2024 (Kennedy and Keen, 2018).

First, it has reduced the marginal deduction rate: As claimants start to earn or increase earnings, the benefit payments reduce at a slower rate compared to the Legacy system. This aims to ease the employment transition and to ensure that taking on even a small amount of working hours could be financially rewarding. Solving a known issue with the Legacy system.

[5] These are: Jobseekers Allowance, a transfer for unemployed individuals looking for work; Employment and Support Allowance, a transfer for individuals that cannot find work full-time; Child Tax Credit, a means tested transfer to families with children below an income threshold; Income Support, a means tested transfer to individuals working fewer than 16 hours a week; Housing Benefit, a means tested transfer to support individuals living in rented accommodation, typically claiming other benefit transfers alongside; Working Tax Credit, a means tested transfer for individuals on low income working part time or full time.

Second, it has introduced the Claimant Commitment, a contract between the benefit recipient and the State, which reflects individual circumstances and defines the essential actions the claimants must undertake to continue receiving benefits. These actions range from not being required to find work at all (e.g. severely disabled claimants and carers) to those required to spend up to 35 hours a week job searching. Importantly, the payment conditionality introduced by the Claimant Commitment is complemented with a tougher regime of sanctions. For instance, the maximum sanction of disqualification from receiving any benefits for up to three years can be applied to those who persistently refuse to comply with the Claimant Commitment.

Third, to mimic a typical salary, payments are made in arrears once a month directly into the claimants bank account. Also, Housing Benefit granted as part of the overall benefit calculation is included in the monthly payment. In the Legacy system, payments are made weekly or fortnightly and Housing Benefit is paid directly to the landlord. In section 6, we will discuss in more depth some of the UC measures that disproportionally affect the financial condition of poorer households and are therefore likely to be key determinants of the increase in criminal activity.

UC Rollout. UC was introduced as a pilot in April 2013 in selected Jobcentres in the North West. Jobcentres, one for constituency, are government funded employment agencies run by the Department for Work and Pensions (DWP). Jobcentres included in the pilot scheme were selected on an administrative basis, to allow for the refinement and the evaluation of the best practices to be exported when scaling up the program nationally.[6] The policy then spread to the rest of the country following the capacity of Jobcentres,

[6] Universal Credit was launched as a Pathfinder in areas of the North West commencing in April 2013. The four initial Pathfinder offices are Ashton-under-Lyne, Oldham, Warrington, and Wigan. Six further sites rolled out between October and the spring of 2014.

administrators, and employees to take up and absorb new methods and IT practices connected with the implementation of Universal Credit.[7] To facilitate the transition to the new welfare system, the initial eligibility criteria for claiming UC benefits focused on the simplest cases: New claimants, single, unemployed, non-home-owning and without children.[8] By April 2016, the national rollout of Universal Credit was completed. That is, UC was available for new claims from single unemployed individuals in Jobcentres across the country. In May 2016, the DWP started the expansion of Universal Credit to new claims taken from all claimants groups, including people with health conditions, disabilities, parents, carers, and those in employment (Kennedy and Keen, 2018). This has led to a large increase in the number of UC recipients in each constituency.

Panel A of Figure I shows the monthly rollout of Universal Credit as well as the number of Universal Credit claims in England and Wales. As discussed, there were no UC recipients prior to the initial rollout in the Spring of 2013. In April 2016, when Universal Credit was offered in all constituencies for a selected category of applicants, around 220,000 claimants were enrolled in the system. After that date, UC started to accept new applications from a wider range of claimants, reaching over 1.5 million claimants in February 2019.[9]

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[7] The date of adoption in each jobcentre was mainly due to administrative reasons, reducing the concerns that the introduction of Universal Credit was driven by pre-existing crime trends in the area. This hypothesis is further supported by estimates that are robust to the inclusion of constituency-specific linear and quadratic trends, and by the event study analysis showing that the increase in burglaries is emerging 'on impact' and it is not inflated by upward crime trajectories prior to the program implementation.

[8] New claimants are defined as claimants who were not enrolled in the Legacy system when UC was operating in a constituency. It is possible these claimants were receiving benefits before the introduction of Universal Credit but that—for any reason—left the welfare system some time prior its start.

[9] The number of Legacy claims dropped from around 6.2 million in April 2013 when UC was first introduced to around 4 million at the end of the sample period. The six Legacy benefits are administered separately and recorded separately in our data. For this reason, we cannot calculate the exact number of individuals claiming Legacy benefits, given that the same individual can claim multiple benefits (e.g. Housing Benefit and Employment Support Allowance). Instead, Universal Credit data identifies the number of claimants, given UC is a unified benefit disbursement.

Panel B of Figure I shows the monthly change in the share of constituencies where UC was in operation and the number of burglaries. We note the larger adoption of Universal Credit started in 2015. Coincidentally, this was also the time when the downward trend in the number of burglaries stopped, appearing to rise afterward.[10] Importantly, the paper will closely examine this motivating descriptive evidence using the staggered rollout of the new welfare systems across constituencies to identify the causal impact of Universal Credit on crime while controlling for common time trends across the country.

3. Data

Universal Credit and Crime Data. We retrieve the date (month and year) when Universal Credit was initially rolled out in every UK constituency from The House of Common Library, a research and information service based in the UK Parliament, which also provides the number of UC individual claimants at the constituency-month level. We match this information with data from UKCrimeStats, an open platform of the Economic Policy Centre, which takes crime data obtained from official police forces and aggregates it the constituency-month level for all constituencies in England and Wales. UKCrimeStats data starts in December 2010 and includes data up to February 2019.[11] The match between UC and crime data generates a balanced sample of all 573 constituencies in England and Wales from December 2010 to February 2019 (99 year-by-month periods).

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[10] Monthly crime displays seasonality. Our analysis includes year-by-month fixed effects, hence controls for changes in crime due to seasonal patterns in the data. Also, we show that results are practically unchanged when including constituency-by-month fixed effects, which controls for crime seasonality at constituency level.

[11] Date of last access (April 2019). Data on crime are divided in the following categories: Anti-social behaviour, bicycle theft, burglary, criminal damage and arson, drugs, other crime, other theft, possession of weapons, public disorder and weapons, robbery, shoplifting, theft from the person, vehicle crime, violence and sexual offenses.

Other Data. UKCrimeStats also includes information on constituency population, and land in hectares we use to construct population density. The DWP provides the number of claims for Jobseekers Allowance, Employment and Support Allowance, Income Support, and Housing Benefit through Stat-Xplore, a browser based client to explore benefit data. HM Revenue & Customs (HMRC) provides the number of Child and Working Tax Credit claimants.[12] We also include data on median house prices provided by the Office for National Statistics (ONS) and data on unemployment and median weekly wages provided by the House of Common Library. Given the set of controls is potentially endogenous to the adoption of Universal Credit, we only use it to test the sensitivity of the baseline estimates and conduct heterogeneity analysis based on pre-reform constituency characteristics measured in the initial sample period (December 2010).

Sample Selection. Our preferred baseline sample excludes 29 constituencies (out of 573) where Universal Credit was rolled out prior to April 2014. We make this choice for two reasons. First, this subset of early adopter constituencies includes sites (known as Pathfinders) where the implementation of Universal Credit was first piloted to test claimants' behaviour and evaluate the provisional program before national expansion (NAO, 2013). Second, the Claimant Commitment, a central component of Universal Credit that intensified the level of sanctions and that we will also argue may play an important role in explaining its criminogenic effects, was officially rolled out from April 2014 (Timmins, 2016). Importantly, we show the estimates are similar, but slightly smaller compared to the baseline and still precise under conventional significance levels, when this subset of 29 constituencies is included in the sample. This has to be expected given the

[12] Tax credits are administered by HMRC, while other benefits are administered by the DWP.

welfare reform in this very early stage had likely less 'bite' because it was piloted and did not include the Claimant Commitment clause. Table 1 provides summary statistics for the baseline sample. These are not discussed for brevity considerations.

4. Research Designs

Difference in Differences. We use the following baseline model to estimate the effects of Universal Credit on crime:

$$y_{c,t} = \beta_1 UC_{c,t} + \alpha_c + \gamma_t + \epsilon_{c,t} \qquad (1)$$

where $y_{c,t}$ is the crime outcome in constituency *c* at time *t* (year-by-month level, e.g. July 2017). The variable of interest, $UC_{c,t}$, is an indicator set to zero in the months prior to the adoption of Universal Credit in a constituency, it takes the value of one in the month when UC was rolled out and afterwards. Constituency fixed effects α_c absorb unobservable time-invariant differences across constituencies. Year-by-month fixed effects γ_t control for uniform changes in criminal activity in England and Wales, fitting a different intercept for each of the 99 periods in the sample (e.g. intercept for July 2017, intercept for August 2017). We employ a weighted least squares (WLS) estimator, using the annual population of the constituency as weight.[13] Standard errors are clustered at the constituency level and permit valid inference in the presence of within-constituency autocorrelation in the errors.

[13] WLS consistently estimates the population linear projection of the impact of Universal Credit on crime experienced by the population in a constituency affected by the reform. Also, when population varies across clusters, the group average error term is heteroskedastic, and the OLS estimation may be inefficient and lead to inconsistent standard errors. The WLS estimator is the minimum variance linear unbiased estimator that produces consistent standard errors correcting for heteroskedasticity (Solon et al. 2015).

The coefficient of interest, β_1, measures the effect on crime of Universal Credit. Crime is log transformed using log(1+z), where z is the crime rate per 100,000 people.[14]

Event Study. The consistency of the results is further examined by conducting an event-study analysis. This enriches our understanding of the impact of UC on crime in at least two ways. First, it investigates possible 'pre-trends' potentially confounding the estimates of β_1. Second, it identifies the dynamics of the effects, whether the crime responses emerge on 'impact' of the reform, and their persistence over time. We estimate the following equation:

$$Y_{c,t} = \theta_1 1(\tau_{c,t} < -36) + \sum_{m=2}^{7} \sum_{j=-3}^{2} \theta_m 1[12(j+1) > \tau_{c,t} \geq 12j] + \theta_8 1(\tau_{c,t} \geq 36) + \alpha_c + \gamma_t + \epsilon_{c,t} \quad (2)$$

The indicator $\tau_{c,t}$ measures the month relative to the introduction of Universal Credit in a constituency. We define $\tau_{c,t} = 0$ if constituency *c* introduced Universal Credit at any time during month *t*. To maximize the power of the analysis, we estimate eight θ_m coefficients associated with indicator variables running for the entire sample of analysis. The excluded coefficient is the indicator measuring the impact of Universal Credit in the year prior to the UC launch. All other details are equal to estimating equation (1).

Difference in Differences in Differences. We estimate heterogenous impacts of the reform across constituencies using the following equation:

$$y_{c,t} = \beta_2 UC_{c,t} + \beta_3 [UC_{c,t} \times CH_{c,t=0}] + \alpha_c + \gamma_t + \epsilon_{c,t} \quad (3)$$

[14] We use a log transformation to generate a normal distribution of the outcome. See appendix Figure A1.

where $CH_{c,t=0}$ is a dummy variable indicating whether a constituency characteristic measured in period 1 of our sample (December 2010) is above the median. We measure constituency characteristics in the initial sample period because these characteristics cannot be affected by a welfare reform adopted more than three years later. The β_2 coefficient measures the criminogenic impacts of Universal Credit in the subpopulation of constituencies below the median of the relevant pre-adoption characteristic. The β_3 coefficient tests whether Universal Credit generates significantly different criminogenic effects above the sample median. The sum of β_2 and β_3 therefore represents impact of Universal Credit in constituencies above the median of the corresponding constituency characteristic presented in the results section. All other details are equal to estimating equation (1).

Instrumental Variable. The last empirical exercise aims to identify the causal impact of the number claimants subject to the new welfare system on crime. We use the following IV estimating equation:

$$y_{c,t} = \beta_4 \widehat{UC\_Claimants}_{c,t} + \alpha_c + \gamma_t + u_{c,t} \quad (4)$$

$$UC\_Claimants_{c,t} = \beta_5 UC_{c,t} + \alpha_c + \gamma_t + v_{c,t} \quad (5)$$

Where $UC\_Claimants$ is the number of Universal Credit claimants in a constituency-month. All other details are identical to estimating equation (1). The logic of this empirical approach is as follows. We are interested in quantifying the causal impact of the number of claimants enrolled in this new welfare system on criminal activity (β_4). A simple OLS regression is likely to produce biased estimates given within-constituency changes in the number of UC claimants are likely correlated with unobservables crime determinants. We therefore address endogeneity concerns by exploiting quasi-experimental variation in the number of claimants provided by the staggered rollout of the program across constituencies, as shown in Figure I.

The main identifying assumption for the exclusion restriction to be valid is that the rollout of the new welfare system affects the proliferation of crime only through changes in the number of benefit recipients exposed to the new system, and it is not correlated to other possible crime determinants omitted from the empirical model. In the related empirical part, we will discuss the reliability of this assumption and show the robustness of the IV estimates to the inclusion of an extensive set of fixed effects and covariates.

5. Results

Difference in Differences Results. Table 2 reports the estimates of Universal Credit on burglary, the main outcome of the analysis. Column 1 displays the baseline estimate obtained using estimating equation (1). We find Universal Credit increased burglaries by around 3.4% with (p<.01). Column 2 includes median weekly earnings, median house prices, unemployment rate, population density, number of claimants for: Housing Benefit, Jobseeker Allowance, Employment and Support Allowance, Income Support, Child and Working Tax Credit. This inclusion attempts to capture local crime determinants potentially correlated with the adoption of Universal Credit in a constituency. The estimate is similar to the baseline, 3.2%, and is significant at the 1% level. Column 3 adds 544 constituency-specific linear trends to the baseline. Results are similar in terms of magnitude, 3.4%, and precision (p<.01). Column 4 adds 544 constituency-specific linear trends and 544 constituency-specific quadratic trends. This specification reduces the magnitude of the estimate to 2.9% with ($p<.05$). Results in columns 3-4 indicate unobserved trends correlated with UC adoption and the propagation of burglaries in a constituency are not driving the results. Column 5 includes 6,528 constituency-by-month fixed effects. Results are slightly larger than the baseline, 3.6%, and are significant at the 1% risk level, showing crime seasonality in a constituency does not influence our estimates.

We now discuss robustness checks that modify the sample of analysis. Column 6 excludes the month when Universal Credit was adopted in a constituency. Crime is reported monthly, and the day of the month when Universal Credit was launched is not available to the researchers. For these two reasons, we cannot identify which crimes lead and which follow the launch of Universal Credit during the month of adoption. The estimate is identical to the baseline, with slightly larger standard errors ($p<.05$). Column 7 excludes 70 constituencies in London, the capital and largest city in the United Kingdom. Estimates are slightly smaller, around 3%, and significant at the 5% level. Column 8 limits the sample to pre-April 2016, the date after which Universal Credit was rolled out to a broader category of individuals. Compared to the baseline, the estimate is 30% larger, 4.4% with ($p<.01$). This suggests the criminogenic effects of the reform are stronger when only the single, unemployed, non-home-owning and childless were enrolled in Universal Credit, a subpopulation that likely has relatively higher criminal propensity (Grogger 1998).[15] Column 9 limits the sample to 40 months before and 34 months after the adoption of Universal Credit. This ensures a balanced sample of constituencies around the date of Universal Credit adoption. That is, the estimates shown in this column are obtained observing all 544 constituencies in each pre and post adoption period. Estimates are slightly

[15] We also note that in the initial years of adoption UC recipients were in high majority males. The male share was around 70% when the program started in 2013 and reached 50% only in 2018 (DWP, 2019).

larger than in the baseline, around 3.7%, and are significant at the 1% level. Column 10 includes the 29 constituencies where Universal Credit was rolled out before April 2014. Estimates are smaller, around 2.7%, with ($p<.0.5$). This suggests the reform in the very early stages likely had less 'bite' because of the piloting issues discussed above and the fact the Claimant Commitment and tougher regime of sanctions were not part of Universal Credit.

Column 11 reports estimates obtained using a Poisson count data regression. Column 12 shows results obtained using a negative binomial count data regression. Marginal effects in both columns are similar in terms of magnitude compared to the baseline, with ($p<0.05$), indicating that the findings are not driven by the choice of a particular functional form. Column 13 shows estimates not weighted by population. Column 14 reports the estimate obtained with standard errors clustered two-way at the constituency and period (year-by-month) level to permit valid inference in presence of both within-constituency and within-month cross-constituencies autocorrelation in the errors (Cameron et al. 2011). Estimates in columns 13-14 are similar to the baseline.

Other Crimes. Results for other crimes are reported in table A1. We find some evidence of similar criminogenic effects on shoplifting, vehicle theft, anti-social behaviour, and possession of a weapon. However, these estimates do not hold against all robustness specifications, therefore they should be interpreted with caution and have been disregarded from the main analysis.[16] Universal Credit has worsened its recipients' financial conditions, likely incentivising some of them to revert to crime to compensate for the related income loss. This may explain why our estimates do not reveal significant effects of UC on violent

[16] See Appendix B for descriptive statistics of all crimes and crime categories definitions.

crimes. We also note burglary is the second most frequent acquisitive crime. Therefore, compared to other less frequent categories of theft (e.g. shoplifting, bike theft, theft from person) it is more likely affected by the reform due to the higher baseline number of criminal episodes, which also enhances the power of our analysis. In terms of occurrences rates, burglary is only preceded by the 'other theft' category that includes thefts by an employee, blackmail and making off without payment. These acquisitive crimes are unlikely to be committed by unemployed individuals, such as the majority of those enrolled in Universal Credit. Hence, if anything, the estimates for other thefts may be considered as a plausible falsification test for the analysis. These estimates are close to zero and not significant under conventional significance levels across specifications (see Panel D of table A1).

Placebo Test. We now present the results of a placebo test where the primary analysis conducted using model (1) is replicated with a pseudo treatment variable drawn from the same distribution of the original treatment variable that, crucially, is not supposed to affect the outcome of interest. We randomly permute the month of UC adoption across constituencies and subsequently generate a placebo $UC_{c,t}$ variable. This is an indicator set to 0 in the months prior to the placebo UC adoption in a constituency, and 1 in the month of placebo adoption and afterwards. We generate 1,000 permutations of the independent variable and run 1,000 placebo regressions to minimize the likelihood that a single distributed placebo variable could affect the results by chance.[17] Figure II shows the Kernel distribution of placebo difference in differences estimates, which is normal and centred

[17] In practice, the true value of the estimand is zero, and the goal of the placebo analysis is to provide support for the identification strategy behind the primary analysis by assessing whether the pseudo treatment leads to estimates that are close to zero, taking into account the statistical uncertainty (Athey and Imbens 2017).

around zero. The vertical solid line identifies the baseline estimate of the impact of Universal Credit on burglaries as in table 2 column 1 (0.0343). This lies to the right of the 99th percentile of the placebo distribution, rejecting at the 1% level the possibility our results are obtained purely by chance. Reassuringly, this placebo analysis provides further support for the identification strategy behind the primary analysis, indicating that the estimates of the effects of UC on burglary detected using model (1) are indeed reliable.

Event Study Analysis. The dynamics of the effects are now examined in an event-study analysis. Figure III shows point estimates and 95% confidence intervals of θ_m estimates obtained using equation (2). We note the effects on burglary are well timed, appearing the year after the UC adoption. The criminogenic effects are durable and increasing until the end of the sample period. The magnitude ranges from 3.5% the year after the adoption ($p<.01$), 4.1% in the second year ($p<.05$), 4.4% in the third year with a p-value of .11 not significant under the conventional level, 7.9% in the fourth year and after with ($p<.05$). The growth in the criminogenic effects is not surprising given the number of UC claimants has consistently risen over time (Figure I). We also note the estimates for all pre-intervention periods are not significantly different from zero and do not reveal any increasing crime trajectory prior to the UC adoption. This provides further reassurance about the validity of our research design and it reduces the concerns crime pre-trends might be confounding the causal interpretation of our estimates.[18]

Heterogeneity Analysis. The purpose of this part of the analysis is to examine possible differences in the criminogenic impacts of Universal Credit associated with pre-

[18] Figure A2 show monthly estimates from an event study analysis on a sample spanning two years before and after the initial implementation of the reform and excluding the dummy indicating the month prior to the adoption. Similarly to equation (2), this specification shows the absence of pre-trends and criminogenic effects emerging on impact. Monthly estimates are however less precise, plausibly because of the high-demanding specification that disaggregates a moderate criminogenic effect into 48 event study dummies.

intervention characteristics of a constituency. This analysis is helpful to locate areas where the criminogenic effects are more pronounced and, therefore, to shed some light on the underlying channels. Results obtained using estimating equation (3) are presented in Table 3. The variable 'Characteristic' refers to a pre-adoption constituency characteristic measured in December 2010 (the initial period of the sample). It takes the value of 1 if the related variable is above the median, 0 otherwise. Columns 1-5 focus respectively on: Median house prices; median weekly earnings; unemployment rate; share of people claiming benefits; population density.

The analysis reveals the criminogenic effects of Universal Credit are concentrated in constituencies below the median of national house prices: The effect in this subpopulation is around 6% with ($p<.01$) as measured by β_2 and it vanishes for constituencies above the median ($\beta_2 + \beta_3$) with a β_3 estimate of around -5% and significant at the 1% level. We do not find any evidence UC has significantly different criminogenic effects across constituencies when considering median weekly earnings, median unemployment rate and share of the population receiving benefits (columns 2-4, respectively). Column 5 shows some evidence the criminogenic effects of the new welfare system are stronger in more rural constituencies below the population density median. The effect in this subpopulation is around 5% with ($p<.01$) and it decreases by more than half for constituencies above the median ($\beta_2 + \beta_3$) with a β_3 estimate of around -3%, marginally significant at the 10% level.

Instrumental Variable Design. We now examine the causal impact of the number of UC recipients on burglaries. Figure IV shows the maps of constituencies in England and Wales and the geographical distribution on UC claimants and burglaries. The two figures suggest a positive correlation between UC beneficiaries and crime that is now closely examined using an IV analysis. Table 4 reports the results obtained using estimating equation (4). Column 1 shows the first stage estimate. The effect is large and significant at the 1% level. This is consistent with the fact that the rollout of the reform has increased the number of UC recipients, and it suggests our instrument causes an average monthly increase of around 350% in the number of UC recipients across the sample period. Reduced form estimates capturing the impact of the policy on burglary are reported for readers' convenience in column 2. The results are equal to our baseline estimate: 3.4% with $p<.01$.

Column 3 displays the IV estimate. The detected elasticity is around 0.01 with ($p<.01$). To put results into perspective, this suggests that moving from the 25th to the 75th percentile of the number of UC recipients in a constituency (from 252 to 990, almost a 300% increase in UC beneficiaries) would lead to an increase in burglaries of around 3%.

Column 4 shows OLS estimates, which are of slightly smaller magnitude, compared to the IV estimates, and are significant at the 1% level. We explain the negative bias in the OLS because of the Local Average Treatment Effect generated by our instrument. In particular, the rollout of the reform across constituencies—that we use as instrument—has larger predictive power when the indicator variable 'switches on' capturing variation in the number of claimants driven by the entry of young individuals, unemployed, unmarried and without children. Most likely, this subpopulation has an higher propensity to commit crime compared to the average benefit recipient covered by our analysis (Grogger, 1998).

In this empirical exercise, the main identifying assumption is that the rollout of Universal Credit affects crime through changes in the number of benefit recipients exposed to this new welfare system (our endogenous variable) and it is not correlated to other possible crime determinants excluded from the empirical model. We have discussed that the rollout of Universal Credit was mainly driven by the administrative capacity of local Jobcentres to handle the introduction of the new payment system. However, one could argue this may be related to unobservable crime determinants, hence violating the exclusion restriction of our IV design. For this reason, in column 5, we saturate the model adding to the baseline: Median weekly earnings, median house prices, unemployment rate, population density; number of claimants of: Housing-Benefit, Job Seekers Allowance, Employment and Support Allowance, Income Support, and Child Tax Credit and Working Tax Credit; constituency-specific linear trends and constituency-by-month fixed effects. This high-demanding specification generates IV estimates that are 10% smaller compared to the baseline in column 3. Results are still significant at the 1% level.

We conclude the Instrumental Variable analysis corroborates the hypothesis that the criminogenic effects of Universal Credit are closely related to the increase in the number of benefit recipients enrolled in this new welfare system. The size of the elasticity is rather small, if compared with other elasticities estimated in the literature examining the impact of legal market opportunities and wages (Draca and Machin, 2015). However, one aspect is worth highlighting. As of February 2019 UC enrolled 1.5 million individuals and, by the time is fully rolled out, it will administer payments to around 8 million individuals (Kennedy and Keen, 2018). Therefore, even if UC produces small individual criminogenic effects because only a minority of its recipients commit crime in response, it can lead to major societal implications given the large number of claimants exposed to the new welfare system.

Interpretation of the Estimates. A conservative interpretation of our estimates indicates that Universal Credit has produced around 45,000 burglaries at a societal cost of £270 million. This estimate is obtained by considering a (pre-adoption) average monthly number of burglaries per constituencies of 64.8, a 3.4% crime increase lasting for 36 months, and a unit cost of burglary due to anticipation, consequence, and response of £5,930 (Heeks et al. 2018). These are conservative estimates, considering that the event study analysis shows larger effects than 3.4%, growing overtime, and persistent after 36 months of UC adoption. Moreover, the unit cost of commercial burglaries (£15,460) is higher than the cost for domestic burglaries (Heeks et al. 2018). Our data do not allow to differentiate between the two. Hence, we opt for the smaller value when estimating the social costs associated with the increase in crime caused by Universal Credit.

6. Discussion

This section discusses the potential reasons behind the impact of UC on crime. UC generated a cut in benefit entitlement of £2 billion per annum. This has disproportionally affected those at the bottom decile of the income distribution (Brewer et al., 2019). While there are various new measures introduced by Universal Credit, one of the most obvious candidates for this regressive impact is the 'Minimum Income Floor' (MIF) for self-employed people. Under this new measure, if earnings are below the MIF, the government computes the benefit amount as if the recipient was earning an amount equal to the MIF (for most people this is the minimum they would earn by working full time as employees). As a result, a single, self-employed person could see their benefit entitlement reduced by up to £8,250 per annum (Brewer et al., 2019).

The regressive distributional impact of UC may have been amplified by the delays in benefit payments.[19] To mimic a typical salary, UC payments are made in arrears once a month directly into the claimants' bank account. Given that payments take one week to reach the bank account of the recipient, the first payment should be received around six weeks after the original application. This, *per se*, may be enough to create higher financial instability for relatively poorer recipients. To exacerbate matters, as for 2017 (hence almost four years after the start of the program) 40% of claimants received the initial payment 11 weeks or more after the date of original application, and 20% received the initial payment around 20 weeks or more after the date of original application (NAO, 2018).

Another possible explanation for the increase in crime relates to the change in conditionality and sanctions regime legislated in Welfare Reform Act 2012, that also officialised the implementation of Universal Credit. The Act established new conditionality rules for both UC and the Legacy system to ensure a broader alignment between the two. In doing so, it increased the length and severity of sanctions in both regimes.[20] The higher volume of sanctions has decreased the amount of benefits received by subgroup of individuals that failed to meet the requirements imposed by the Claimant Commitment.[21] We note the larger majority of recipients being sanctioned are male and in the 16-24 age

[19] Late payments can arise because the verification process is not completed on-time (either by DWP or the claimant), claims are amended at a late stage, or any other sort of technical difficulty.

[20] Prior to the Welfare Reform Act 2012, the maximum period for which a claimant's benefit could be stopped for a breach of the rules was six months. The new rules increased the maximum period of benefit sanction to three years. Sanctions are applied according to the seriousness of the infraction. For more information see: https://publications.parliament.uk/pa/cm201719/cmselect/cmworpen/955/955.pdf

[21] As of December 2016, 8.2% of UC claimants required to search for work were being sanctioned. The percentage of sanctions for JSA, prior to the Welfare Reform Act 2012, was around 2% (DWP, 2017). This suggests a large increase in the number of sanctions received under UC. We note however that the two percentages are not directly comparable. Under JSA, payments can be stopped to claimants who miss an advisor interview without good reason, under the assumption that the claimant has found work without informing the Jobcentre. Under UC, which supports claimants for a range of needs beyond unemployment (e.g. rental costs), the jobcentre is more likely to sanction the claimant instead of stopping the benefit entirely. In addition, JSA payments were fortnightly, therefore restarting them would not lead to the same payment delay as UC.

range (Webster, 2017). These individuals are also likely to be characterized by a higher baseline criminal propensity (Grogger, 1998).

Universal Credit has also altered the frequency of payments, from weekly or fortnightly, to monthly. Foley (2011) and Carr and Packham (2019) have clearly shown this can significantly impact criminal propensity, as benefit recipients consume welfare related income quickly and then commit crime to overcome hardship later in the month. Therefore, this could also represent a possible channel behind our results. However, we speculate it is probably less prominent. In fact, UC still allows for payments staggered during the month in a wide variety of circumstances. In particular, this happens in the most problematic cases of individuals who likely have higher criminal propensity. Staggered disbursements are given when the 'work coach' identifies the claimant has addiction problems, rent arrears, was previously homeless, or in any circumstance where the claimant request it with a valid reason (DWP, 2020).

We conclude the criminogenic effects of Universal Credit are likely due to a substantial worsening of the financial conditions of some UC recipients, which has increased their incentives to commit crime to supplement their income. Our findings are therefore entirely consistent with the predictions arising from the model of Becker (1968), according to which—all else being equal—a reduction in legitimate income opportunities would lead to an increase in economically motivated crimes.

7. Concluding Remarks

This paper evaluates the criminogenic effects of Universal Credit, a monumental welfare reform designed to radically change the social security payment system in the United

Kingdom that will directly affect the lives of around 8 million low-income individuals by the time is fully rolled out in 2024. We exploit the staggered rollout of the reform across parliamentary constituencies and use monthly crime and UC data from December 2010 to February 2019 for all constituencies in England and Wales. We conduct difference-in-differences and IV analysis to identify credible estimates of the criminogenic effects of Universal Credit.

Our main finding is that UC has generated around 45,000 burglaries, with durable effects lasting until the end of the sample period. Effects are precisely estimated and robust to an extensive set of checks such as the inclusion of numerous socio-economic covariates, constituency-specific linear and quadratic trends, constituency-by-month fixed effects, changes to the sample analysed and functional forms. A placebo test and an event study analysis provide further support for the reliability of the identification strategy employed in this paper. We also show effects of UC are larger when only claimants with high-crime propensity, such as unmarried, unemployed individuals, are admitted to the program. In addition, the effects are significantly larger in poorer constituencies where house prices are below the national median. An IV analysis exploiting the staggered adoption of UC across constituencies as an instrument for the number of UC claimants, confirms that the intensive margin of adoption matters for the impact on crime.

A conservative interpretation of our estimates indicates Universal Credit has caused a societal cost of around £270 million. These societal costs cannot be disregarded and appear even more worrying considering that around 6.5 million more benefit claimants will be soon exposed to this new welfare system.[22] For this reason, our paper offers timely policy

[22] See, for instance: https://www.bbc.co.uk/news/business-52675084

implications and provides impetus for measures aimed at tackling these unintended criminogenic effects that are otherwise likely to grow over time.

Our findings are important for at least two other reasons. First, they corroborate the hypothesis that economic incentives, particularly those affecting individuals at the bottom of the income distribution, matter for crime (Grogger 1998; Machin and Meghir, 2004; Draca and Machin, 2015). Second, the results demonstrate how government policies can have unintended consequences (e.g. Adda et al., 2014; d'Este, 2019; Gavrilova et al., 2019). Overall, our findings strongly indicate one of the consequences of Universal Credit is the imposition of large societal cost in the form of higher crime.

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Table 1
Descriptive Statistics

	(1) Mean	(2) Std. Dev.	(3) Min	(4) Max
Burglary	61.972	32.741	0	312
Number of claimants:				
Universal credit	404.409	911.057	0	11,443
Housing benefit	539.482	285.564	69	3,933
Job seeker allowance	1,176.415	1,016.998	50	8,100
Employment and support allowance	1,670.372	1,041.613	117	6,166
Income support	1,469.92	1,003.985	154	7,852
Child and working tax credit	5,311.037	2,010.08	1,300	13,400
Population	100,000	14,917.13	56,453	185,000
Population density	21.823	27.706	.23	162.376
Median weekly earnings (£)	524.179	81.911	359.9	916.4
Median house prices (£)	213,000	112,000	58,000	1,280,000
Unemployment rate	0.034	0.023	0.004	0.174
Observations = 53,586				

Notes: Descriptive statistics at the constituency-month level for the baseline sample of analysis. The sample includes 544 parliamentary constituencies in England and Wales from December 2010 to February 2019 (544 constituencies × 99 months = 53,586 observations). Population density is computed as population divided by land in hectares. Data sources: Crime (UKCrimeStats); UC claimants (The House of Common Library); child tax credit and working tax credit (HMRC); other Legacy claimants (DWP); population and density (UKCrimeStats) ; earnings, house prices, and unemployment rate (ONS).

Table 2
The Impact of Universal Credit on Burglary
Difference in Differences Estimates: Baseline Result and Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Universal Credit	0.0343***	0.0321***	0.0340***	0.0292**	0.0360***	0.0343**	0.0296**
	(0.0124)	(0.0122)	(0.0124)	(0.0114)	(0.0127)	(0.0136)	(0.0134)
Observations	53,856	53,856	53,856	53,856	53,856	53,312	46,926
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Universal Credit	0.0443***	0.0365***	0.0266**	0.0274**	0.0301**	0.0350***	0.0343***
	(0.0132)	(0.0118)	(0.0107)	(0.0130)	(0.0128)	(0.0128)	(0.0129)
Observations	35,360	40,800	56,727	53,856	53,856	53,856	53,856

Notes: This table shows the difference in differences estimates of the impact of Universal Credit on burglary. Unless otherwise specified, burglary is transformed as log(1+z), where z is the rate of burglaries per 100,000 people in a constituency-month. The variable of interest 'Universal Credit' is an indicator set to 0 in the months prior to the Universal Credit adoption, 1 afterwards. Column 1 shows the baseline estimate. Column 2 includes as controls median weekly earnings, median house prices, unemployment rate, population density, and recipients of: number of Housing Benefit, Jobseeker Allowance, Employment and Support Allowance, Income Support, Child and Working Tax Credit. Column 3 adds to the baseline 544 constituency-specific linear trends. Column 4 adds to the baseline 544 constituency-specific linear trends and 544 constituency-specific quadratic trends. Column 5 includes 6,528 constituency-by-month fixed effects. Column 6 excludes the month when Universal Credit was adopted in a constituency. Column 7 excludes 70 London constituencies. Column 8 limits the sample to April 2016. Column 9 limits the sample to 40 months before and 34 months after the adoption of Universal Credit in a constituency. Column 10 includes the 29 constituencies where Universal Credit was rolled out before April 2014. Column 11 shows results from a Poisson count data regression. Column 12 shows results from a negative binomial count data regression. Column 13 shows estimates not weighted by population. Column 14 reports standard errors clustered two-way at the constituency-period level. All regressions include fixed effects at the constituency level and year-by-month level (99 periods). Regressions are weighted by population in a constituency (not in columns 11-13). Standard errors clustered at the constituency level (and at the constituency-period level in column 14) are shown in parentheses. *** significance at the 1% level, ** significance at the 5% level, * significance at the 10% level.

Table 3
The Impact of Universal Credit on Burglary
Difference in Differences in Differences Estimates: Heterogeneity Analysis

	(1)	(2)	(3)	(4)	(5)
Universal Credit	0.0597***	0.0425***	0.0351**	0.0419***	0.0499***
	(0.0145)	(0.0148)	(0.0145)	(0.0146)	(0.0148)
Universal Credit × Characteristic	-0.0512***	-0.0159	-0.00170	-0.0150	-0.0294*
	(0.0152)	(0.0153)	(0.0152)	(0.0152)	(0.0151)
Observations	53,856	53,856	53,856	53,856	53,856

Notes: This table shows the difference in differences in differences estimates of the impact of Universal Credit on burglary. Burglary is transformed as log(1+z), where z is the rate of burglaries per 100,000 people in a constituency/month. The variable 'Universal Credit' is an indicator set to 0 in the months prior to the Universal Credit adoption in a constituency, 1 afterwards. The variable 'Characteristic' refers to a pre-adoption constituency characteristic measured in December 2010 (the initial period of the sample). It takes the value of 1 if the related variable is above the median, 0 otherwise. Columns 1-5 focus respectively on: Median house prices; median weekly earnings; unemployment rate; share of people claiming benefits; population density. All regressions include fixed effects at the constituency level and year-by-month level (99 periods). Regressions are weighted by population in a constituency. Standard errors clustered at the constituency level are shown in parentheses. *** significance at the 1% level, ** significance at the 5% level, * significance at the 10% level.

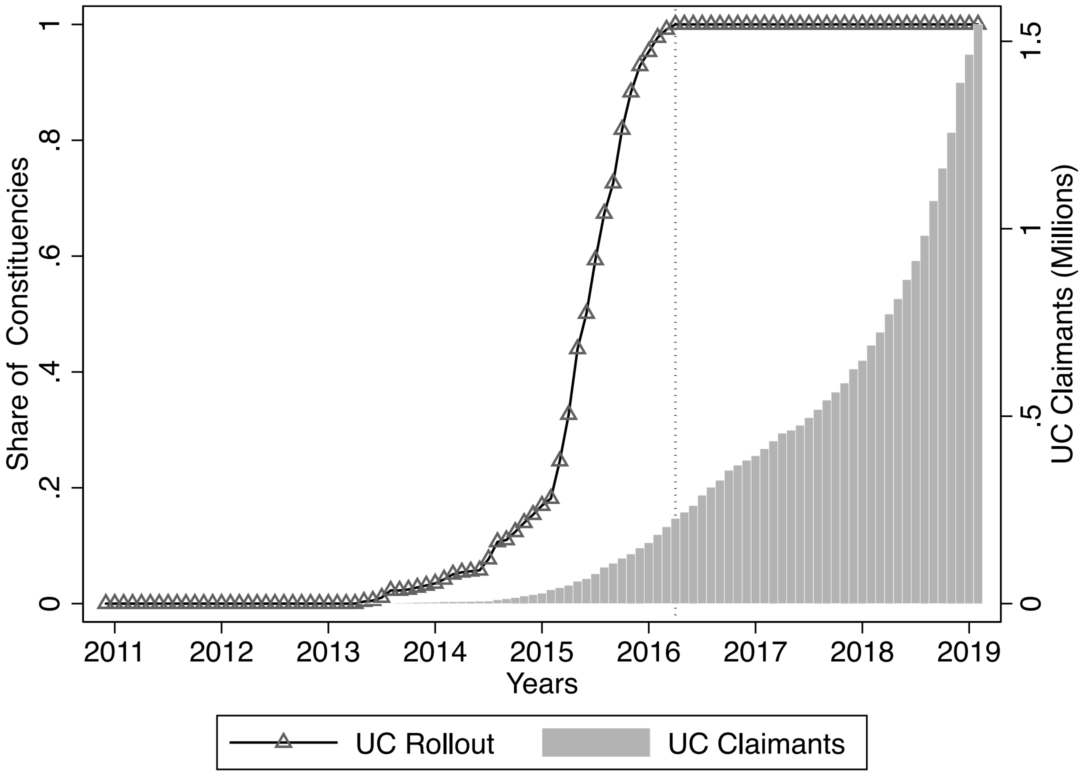
Table 4
The Impact of Universal Credit Claimants on Burglary: IV Analysis

	(1) First Stage	(2) Reduced Form	(3) IV	(4) OLS	(5) IV + Controls
Universal Credit	3.593***	0.0343***			
UC Claimants			0.00955*** (0.00343)	0.00878*** (0.00305)	0.00876*** (0.00333)
Observations	53,856	53,856	53,856	53,856	53,856

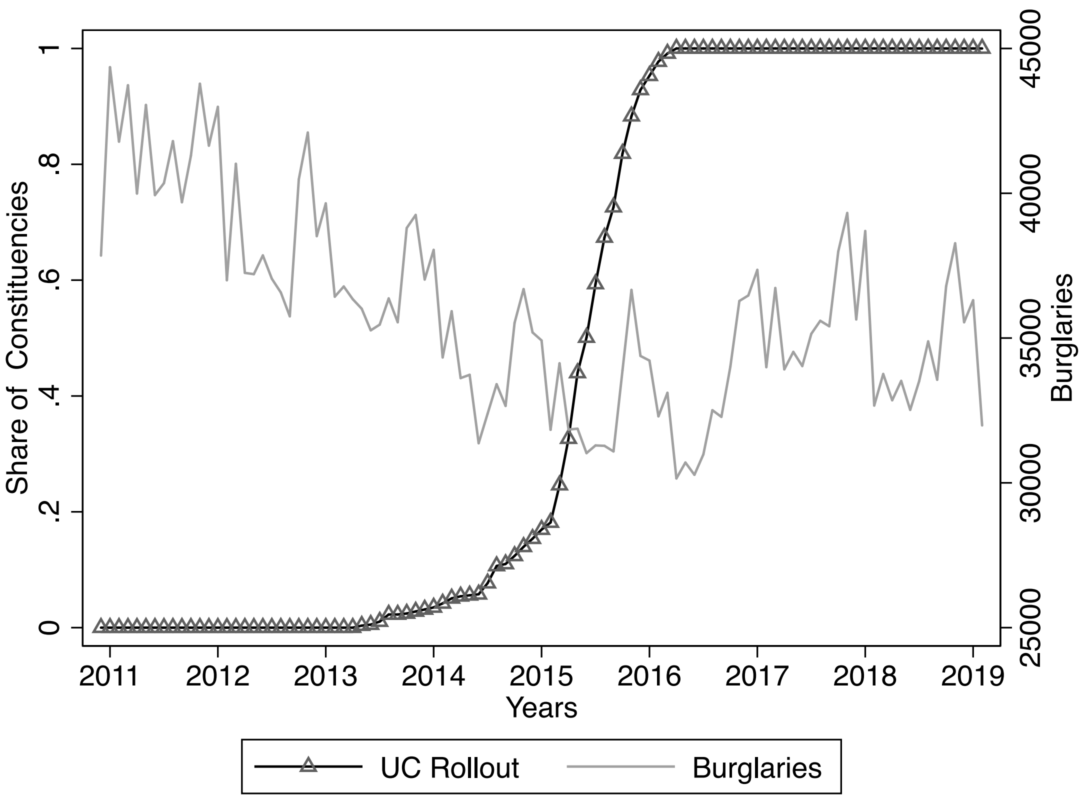
Notes: This table reports the results of an IV analysis that estimates the impact of the number of UC claimants on burglary. Burglary is transformed as log(1+z), where z is the rate of burglaries per 100,000 people in a constituency/month. UC Claimants is transformed as log(1+z), where z is the rate of Universal Credit claimants per 100,000 people in a constituency/month. The variable 'Universal Credit' is an indicator set to 0 in the months prior to the Universal Credit adoption in a constituency, 1 afterwards. Column 1 shows first stage estimates. Column 2 shows the reduced form estimates, as in table 2 column 1. Column 3 reports IV estimates. Column 4 shows OLS estimates. Column 5 shows IV estimates including: 544 constituency-specific linear trends, 6,528 constituency-by-month fixed effects, median weekly earnings, median house prices, unemployment rate, population density, and claimants of: Housing-Benefit, Job Seekers Allowance, Employment and Support Allowance, Income Support, and Child tax credit and working tax credit. All regressions include fixed effects at the constituency level and year-by-month level (99 periods). Regressions are weighted by population in a constituency. Standard errors clustered at the constituency level are shown in parentheses. *** significance at the 1% level, ** significance at the 5% level, * significance at the 10% level.

Figure I

Panel A

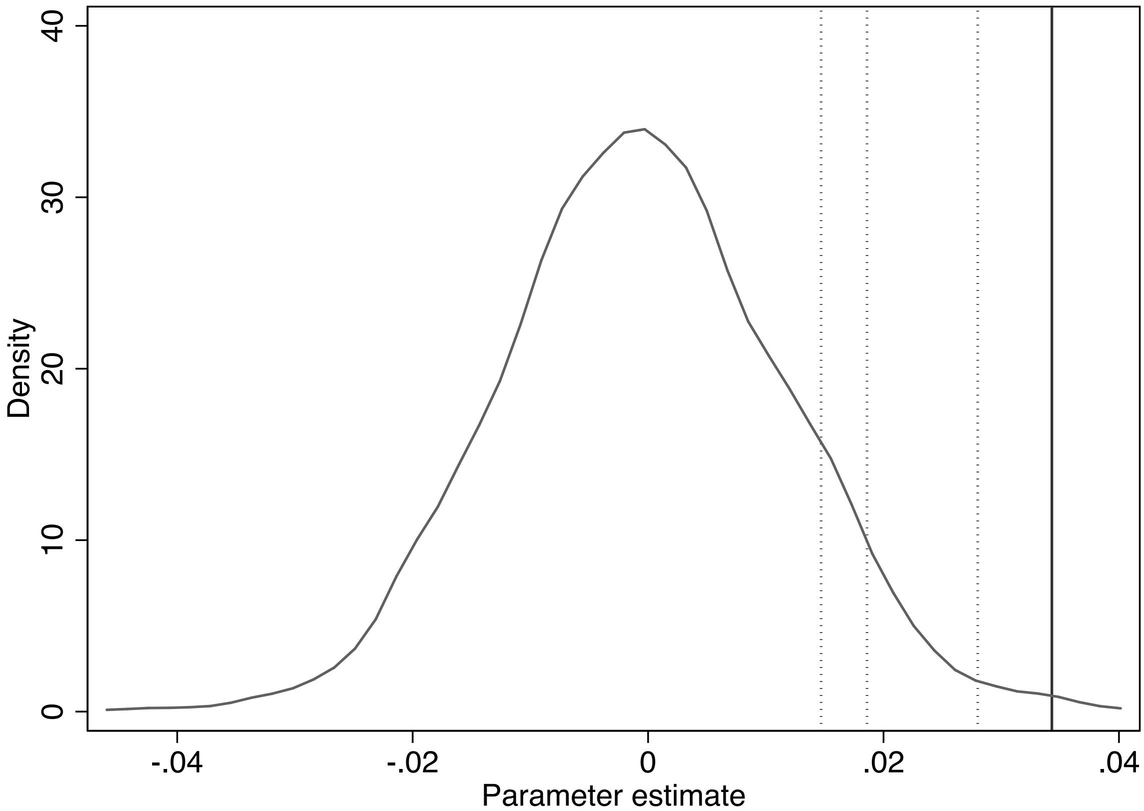


Panel B



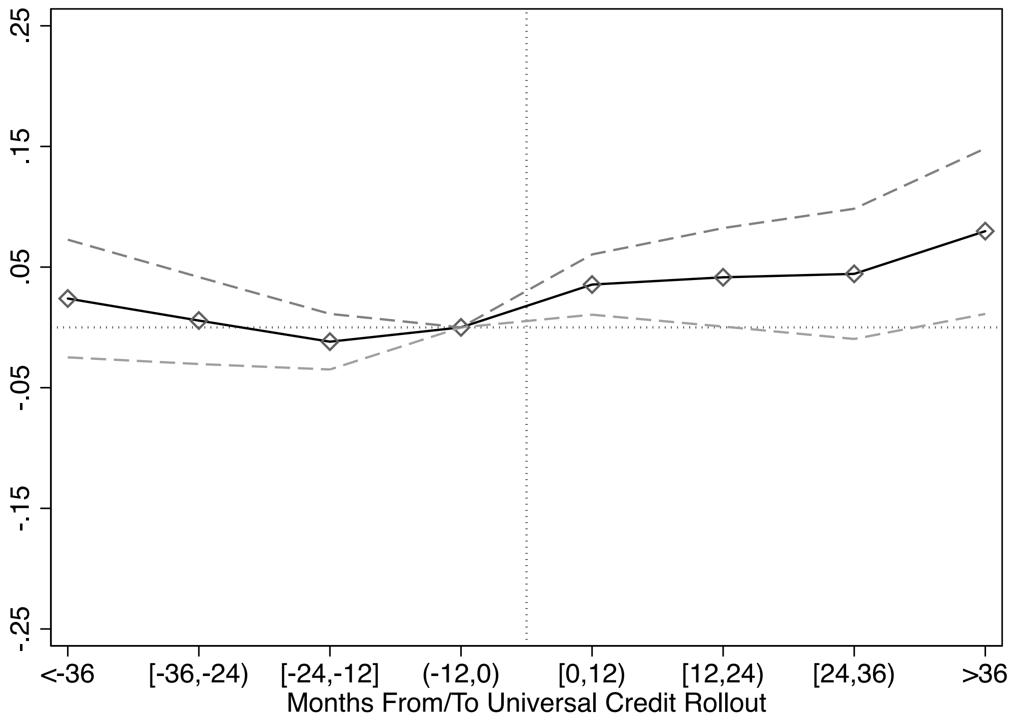
Notes: We show the monthly rollout of Universal Credit across 573 parliamentary constituencies in England and Wales (left y-axis). On the right y-axis, Panel A shows the number of Universal Credit claimants; Panel B shows and the number of burglaries.

Figure II



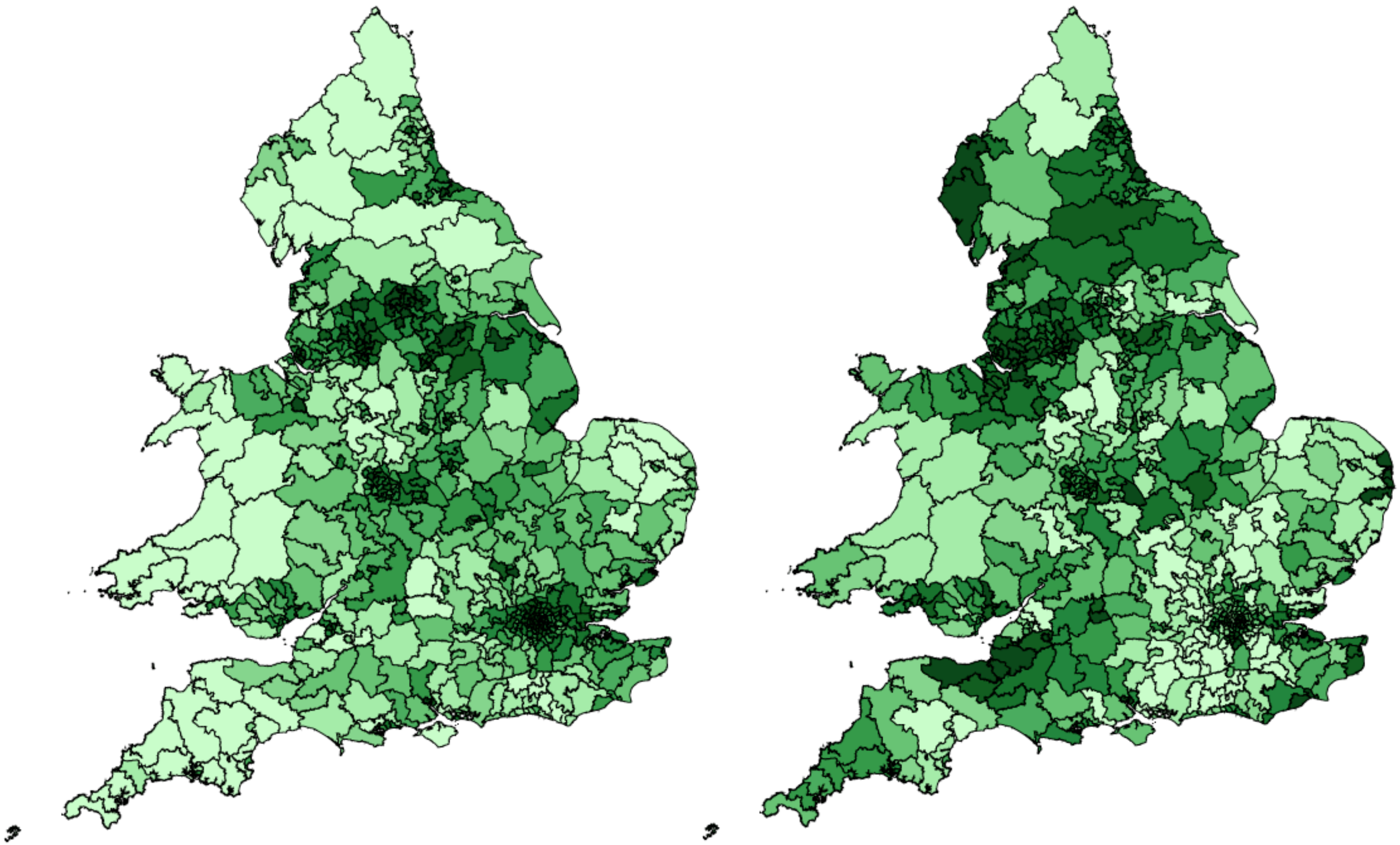
Notes: This figure shows the Kernel distribution of placebo difference in differences estimates. These are obtained by randomly permuting the month of adoption of Universal Credit across constituencies and by then generating a placebo 'Universal Credit' variable, which is an indicator set to 0 in the months prior to the placebo Universal Credit adoption in a constituency, 1 afterwards. We perform 1,000 random permutations and run 1,000 placebo regressions. Dotted lines represent 90^{th}, 95^{th}, and 99^{th} percentiles of the placebo distribution. The vertical solid line identifies the baseline estimate of the impact of Universal Credit on burglaries as in table 2 column 1 (0.0343).

Figure III



Notes: This figure reports the event study analysis of the impact of Universal Credit on burglary. We show rollout annual estimates and 95% confidence intervals obtained using equation (2). Burglary is transformed as log(1+z), where z is the rate of burglaries per 100,000 people in a constituency/month. The event study includes fixed effects at the constituency and year-by-month level and it is weighted by population in a constituency. Standard errors clustered at the constituency level.

Figure IV



Notes: This figure shows the map of 573 constituencies in England and Wales. Left figure shows burglary rates. Right figure shows Universal credit recipients rates. Colours indicate deciles in the corresponding distribution.

Table A1
The Impact of Universal Credit on Other Crimes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Violent Crimes and Sex Offences						
Universal Credit	-0.0151	-0.0162	-0.0140	-0.00639	-0.0174*	-0.00268
	(0.00967)	(0.0102)	(0.00989)	(0.00868)	(0.0100)	(0.00875)
Panel B: Robberies						
Universal Credit	-0.00204	0.00113	-0.00285	0.000371	-0.00538	-0.000442
	(0.0170)	(0.0169)	(0.0169)	(0.0162)	(0.0178)	(0.0162)
Panel C: Drug Crimes						
Universal Credit	-0.000758	-0.00820	0.000215	-0.00900	-0.00535	-0.00723
	(0.0137)	(0.0138)	(0.0139)	(0.0146)	(0.0139)	(0.0135)
Panel D: Other Thefts						
Universal Credit	-0.00276	-0.00904	-0.00244	-0.00732	-0.00469	-0.00120
	(0.00983)	(0.0100)	(0.00981)	(0.00959)	(0.0103)	(0.00931)
Panel E: Anti-Social Behaviour						
Universal Credit	0.0475***	0.0438***	0.0462***	-0.00884	0.0481***	0.0121
	(0.0124)	(0.0121)	(0.0127)	(0.00718)	(0.0128)	(0.00788)
Panel F: Shoplifting						
Universal Credit	0.0262**	0.0160	0.0238**	-0.00613	0.0258**	0.00729
	(0.0121)	(0.0117)	(0.0119)	(0.0116)	(0.0126)	(0.0105)
Panel G: Other Crimes						
Universal Credit	-0.0121	-0.0132	-0.00798	0.0163	-0.0162	0.00712
	(0.0161)	(0.0163)	(0.0163)	(0.0151)	(0.0167)	(0.0154)
Panel H: Vehicle Thefts						
Universal Credit	0.0376***	0.0299**	0.0384***	0.00871	0.0360**	0.0237*
	(0.0141)	(0.0139)	(0.0142)	(0.013)	(0.0147)	(0.0133)
Observations	53,856	53,856	53,856	53,856	53,856	40,800

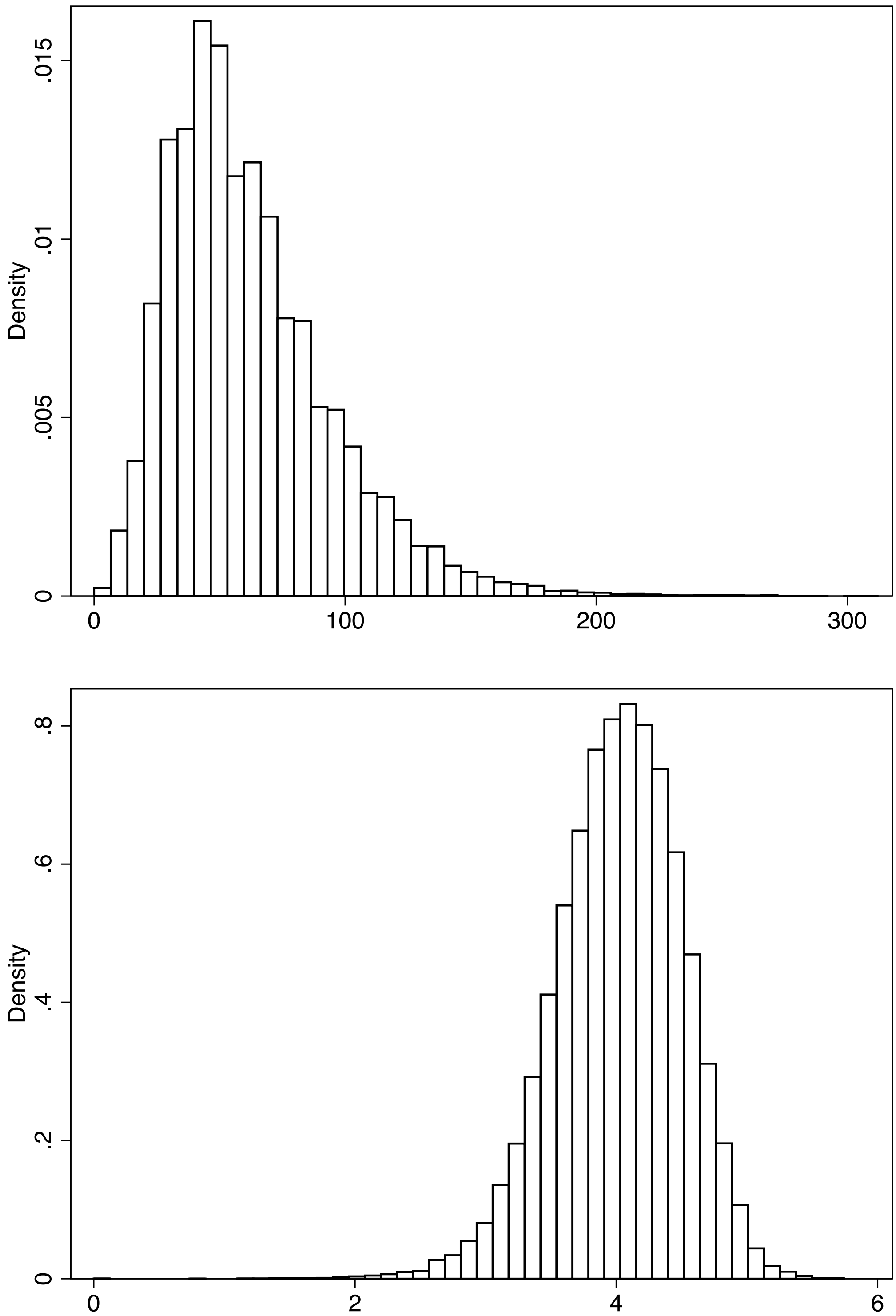
Notes: This table shows the difference in differences estimates of the impact of Universal Credit various crimes indicated in the Panel title. Crime is transformed as log(1+z), where z is the crime rate per 100,000 people in a constituency-month. The variable of interest 'Universal Credit' is an indicator set to 0 in the months prior to the Universal Credit adoption, 1 afterwards. Column 1 shows the baseline estimate. Column 2 includes control variables. Column 3 adds to the baseline constituency-specific linear trends. Column 4 adds to the baseline constituency-specific linear trends and quadratic trends. Column 5 includes constituency-by-month fixed effects. Column 6 limits the sample to 40 months before and 34 months after the adoption of Universal Credit in a constituency. All regressions include fixed effects at the constituency level and year-by-month level. Regressions are weighted by population in a constituency. Standard errors clustered at the constituency level are shown in parentheses. *** significance at the 1% level, ** significance at the 5% level, * significance at the 10% level.

Table A1 - Continued
The Impact of Universal Credit on Other Crimes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel I: Criminal Damage and Arson						
Universal Credit	-0.352	-0.997	-0.444	-2.109***	-0.290	-0.845
	(0.633)	(0.642)	(0.622)	(0.671)	(0.649)	(0.586)
Panel L: Bike Theft						
Universal Credit	0.327	0.0375	0.157	-0.279	0.291	-0.173
	(0.327)	(0.359)	(0.301)	(0.242)	(0.309)	(0.269)
Panel M: Possession of a Weapon						
Universal Credit	0.291***	0.140	0.277***	0.336***	0.283***	0.309***
	(0.0955)	(0.0988)	(0.0983)	(0.0993)	(0.0980)	(0.0966)
Panel N: Theft From Person						
Universal Credit	0.113	-0.910	-0.186	-0.485	0.138	-0.405
	(0.366)	(0.601)	(0.363)	(0.351)	(0.375)	(0.325)
Panel O: Public Disorder Weapons						
Universal Credit	-0.358	-0.0886	-0.278	-0.163	-0.389	-0.162
	(0.238)	(0.193)	(0.219)	(0.167)	(0.246)	(0.229)

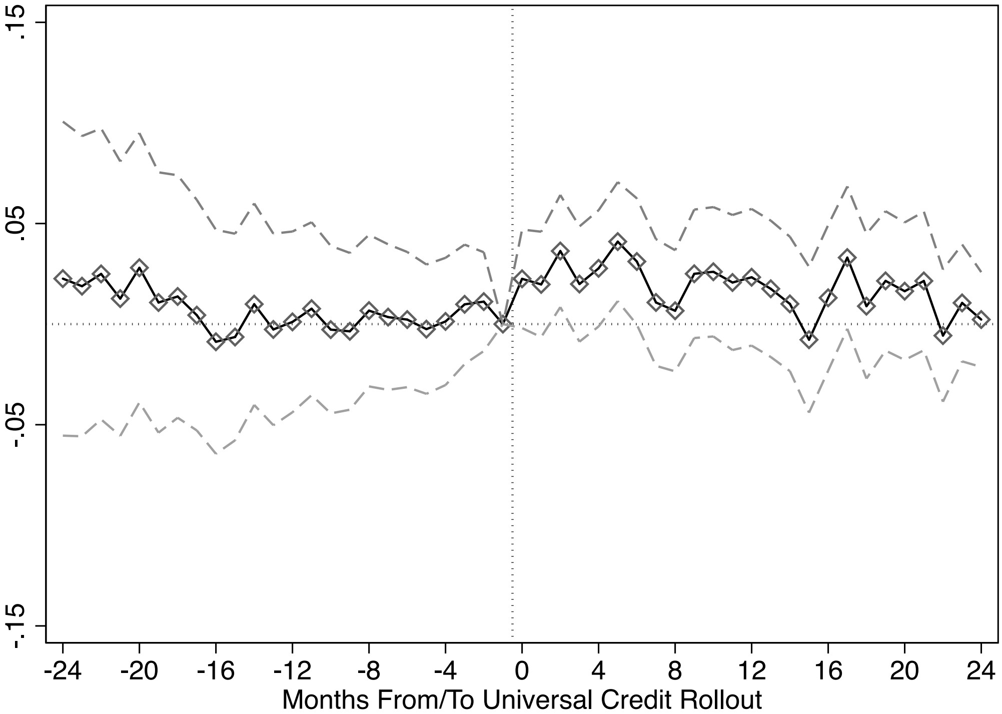
Notes: This table shows the difference in differences estimates of the impact of Universal Credit various crimes indicated in the Panel title. Crime is transformed as log(1+z), where z is the crime rate per 100,000 people in a constituency-month. The variable of interest ‘Universal Credit’ is an indicator set to 0 in the months prior to the Universal Credit adoption, 1 afterwards. Column 1 shows the baseline estimate. Column 2 includes control variables. Column 3 adds to the baseline constituency-specific linear trends. Column 4 adds to the baseline constituency-specific linear trends and quadratic trends. Column 5 includes constituency-by-month fixed effects. Column 6 limits the sample to 40 months before and 34 months after the adoption of Universal Credit in a constituency. All regressions include fixed effects at the constituency level and year-by-month level. Regressions are weighted by population in a constituency. Standard errors clustered at the constituency level are shown in parentheses. *** significance at the 1% level, ** significance at the 5% level, * significance at the 10% level.

Figure A1



Notes: Top figure shows the histogram of the number of burglaries in the baseline sample. Bottom figure shows the histogram of burglary transformed as log(1+z), the main outcome of the empirical analysis, where z is the rate of burglaries per 100,000 people in a constituency/month.

Figure A2



Notes: This figure reports estimates from an event study analysis of the impact of Universal Credit on burglary. We show monthly estimates and 95% confidence intervals obtained on sample spanning two years before and after the initial implementation of the reform and excluding the month prior to the adoption. Burglary is transformed as log(1+z), where z is the rate of burglaries per 100,000 people in a constituency/month. The event study includes fixed effects at the constituency and year-by-month level and it is weighted by population in a constituency. Standard errors clustered at the constituency level.

Appendix B: All Crimes

Table B1
Descriptive Statistics for Other Crimes

	Mean	Std.Dev.	Min	Max
Burglary	61.972	32.741	0	312
Antisocial behavior	280.744	165.886	0	1707
Robbery	9.017	15.798	0	375
Vehicle crimes	56.428	38.083	0	406
Violent crimes	146.145	96.451	1	1159
Shoplifting	43.387	37.687	0	493
Criminal damage and arson	69.66	38.692	0	373
Other thefts	70.19	83.711	0	2783
Drugs	20.493	21.68	0	517
Other crimes	39.809	108.807	0	4256
Bike theft	8.888	15.88	0	329
Weapons	3.025	4.48	0	104
Theft from person	8.331	32.271	0	1575
Public disorder	3.903	10.402	0	184

Notes: Descriptive statistics at the constituency-month level for the baseline sample of analysis. The sample includes 544 parliamentary constituencies in England and Wales from December 2010 to February 2019 (544 constituencies × 99 months = 53,586 observations). Data sources: UKCrimeStats.

Crimes Definition

Anti-social behaviour
Includes personal, environmental and nuisance anti-social behaviour.

Bicycle theft
Includes the taking without consent or theft of a pedal cycle.

Burglary
Includes offences where a person enters a house or other building with the intention of stealing.

Criminal damage and arson
Includes damage to buildings and vehicles and deliberate damage by fire.

Drugs
Includes offences related to possession, supply and production.

Other crime
Includes forgery, perjury and other miscellaneous crime.

Other theft
Includes theft by an employee, blackmail and making off without payment.

Possession of weapons
Includes possession of a weapon, such as a firearm or knife.

Public disorder and weapons
Includes offences which cause fear, alarm, distress or a possession of a weapon such as a firearm.

Robbery
Includes offences where a person uses force or threat of force to steal.

Shoplifting
Includes theft from shops or stalls.

Theft from the person
Includes crimes that involve theft directly from the victim (including handbag, wallet, cash, mobile phones) but without the use or threat of physical force.

Vehicle crime
Includes theft from or of a vehicle or interference with a vehicle.

Violence and sexual offences
Includes offences against the person such as common assaults, grievous bodily harm and sexual offences.