

Does crime type matter in understanding the nexus between universal credit and crime? Evidence from England and Wales

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ABSTRACT

Motivated by a seemingly negative correlation between universal credit and crime in England and Wales, we present a novel theoretical framework of crime and welfare spending, where crime-specific human capital-induced heterogeneity exists between criminal activities. This provides a theoretical basis to three empirically testable propositions. We evaluate these using county-level data for 10 different crime types. We find significant heterogeneity across different crime types in affecting the crime-universal credit nexus. Notably, criminal damage and arson exhibit both positive level and introductory effects, implying these to be human capital dependent, whereas public disorder and weapons possession exhibit a negative crime-universal credit nexus.

JEL Classifications: C26, H53, H75, K42.

Keywords: Crime Heterogeneity, Crime-welfare spending nexus, England and Wales, Universal credit, Welfare spending.

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1. INTRODUCTION

1.1. Background

Since the studies of Becker (1968) and Ehrlich (1973), the economics of crime literature, which examines criminal activities as behavioural choices made by individuals driven by economic incentives, has blossomed into a distinct field of its own. Many recent contributions highlight the significance of structural policies and institutions in explaining variations in crime (Machin and Marie 2006; Draca and Machin 2015), underlining the significance of Weberian style structural forces in explaining the economics of crime. Specifically, if policy changes or streamlining of institutions create unintended economic incentives for agents to engage in illegal activities, then there can be disproportionate effects on crime incidence. Nevertheless, there is rarely a consensus on the impact of most institutions on criminal activities.

For instance, welfare spending, or generally the welfare system, is one such institutional mechanism that can either directly or indirectly affect crime. At their core, social welfare systems are designed to help disadvantaged individuals make ends meet, whilst also incentivising proactive means, such as job-searching. These incentive mechanisms have obvious ties to the labour market participation decisions of individuals and households alike, which include the choice of whether to engage in illicit activities.

Of note, unemployment-crime relationships have been well researched and documented in early studies such as Cantor and Land (1985), and Chiricos (1987). Insofar as unemployed individuals find it difficult to sustain their end needs, then the logical choice would be to become involved in income-generating illicit activities. In such instances, the welfare system would play a key role in potentially altering the dynamics within the unemployment-crime nexus. By extension, any policy change to the structure and operation of social welfare schemes can then have material effects on crime, therefore making both potentially complementary policies.

The relationship between the welfare payment system and crime remains poorly understood. On the one hand, studies such as Hannon and Defronzo (1998), Farrall (2006), Chioda *et al* (2016) and Breckin (2019) found various temporary assistance and cash transfer programmes to facilitate crime reduction. On the other hand, studies such as Burek (2005), Foley (2011) and Cameron and Shah (2014) find that welfare payments can lead to the unintended consequence of an increase in crime, be it because of misallocation or mis-targeting of the programme, or because the welfare beneficiaries simply treat welfare-related income as a supplement to their main criminal income. Further, as found by Liebertz and Bunch (2018) when evaluating the impact of the Temporary Assistance for Needy Families programme in the United States, these effects could differ across different types of crime. Indeed, on-the-surface evidence following the phased roll-out of the Universal Credit (UC) system across England and Wales post-2013 seems to suggest a positive crime-welfare spending nexus, where there was a notable expansion in the aggregate number of recorded crime cases, rising from approximately 4.2 million in 2014 to 6 million in 2018.

Following a recent reform to the Jobseekers Allowance, essentially the unemployment benefit system of the United Kingdom, the UC system is a more overarching change made to the fundamental structure of the British social welfare system. According to the Department for Work and Pensions, the legacy benefits system had provided cash aid to some 20 million individuals in the United Kingdom under various benefit classifications, as of February 2020. Of these, 13 million are state pensions. In light of the growing fiscal pressures faced by the British government in the post-Brexit economy, aggravated further by the COVID-19 pandemic and the ongoing support made to the Ukraine-Russian war, drastic reform to the welfare system is therefore necessary to improve its allocation efficiency.

In the context of the UC programme in England and Wales, one might argue that it is merely a re-purposed welfare system in its strict technical sense, since it was launched to replace the prior roles of six 'legacy' welfare instruments (Child Tax Credit, Employment and Support Allowance, Housing Benefit, Income Support, Jobseeker's Allowance, and Working Tax Credit) (Department for Work and Pensions 2020). Nevertheless, this reform has a natural exogeneity to household and individual decision-making processes, even if they were existing claimants.

First, despite some existing claimants being transferred directly to the new system through 'managed migration', the UC programme is treated as an entirely new 'start-up' system. The bulk of registered claimants are through 'natural migration' in that these are newly enrolled individuals with new circumstances. Second, the UC system also comes with a new set of much more stringent requirements to claimant eligibility, which includes a 'benefit cap' for maximum claimable amount, so as to increase the incentive for claimants to not perpetually stay on the system. Third, by virtue of the British government adopting a 'phased roll-out' mechanism, different counties across England and Wales have seen vastly different dates for the actual implementation of the system, with some counties seeing its launch in early 2013 and others late into 2015. Further, due to its unique design, significant lags have been widely reported between announcements.

1.2. Our contributions

To study whether different crime type matters in influencing the welfare spending-crime nexus, we develop a novel yet simple theoretical framework of crime and cash transfer that distinguishes between the *introductory* and the *level* effect of welfare spending, as well as a crime-specific human capital-induced heterogeneity between criminal activities. Theoretically, our criminal human capital-based explanations of crime heterogeneity are similar in spirit to Mocan *et al* (2005), Mocan and Bali (2010) and Jia and Lim (2021).²

This provides a theoretical basis to three empirically testable propositions. Notably, the crime-cash transfer relationship can either be positive or negative, depending on whether a certain crime activity is criminal human capital-dependent or not. To investigate this, we exploit the large number of street-level reported crime cases in England and Wales, and construct county-month level measures for ten different types of crime. Combining this with the UC claimant rate at the same county-month level, we test for the differential effects across all individual crime types, and then infer whether certain crime types contribute to a positive or negative crime-welfare spending nexus based on the theory. Specifically, the propositions state that a crime type that empirically exhibits both positive (negative) *level* and *introductory* effects, then the specific crime type is human capital (non-human capital) dependent, therefore unlikely (most likely) to benefit from the targeted use of welfare spending.

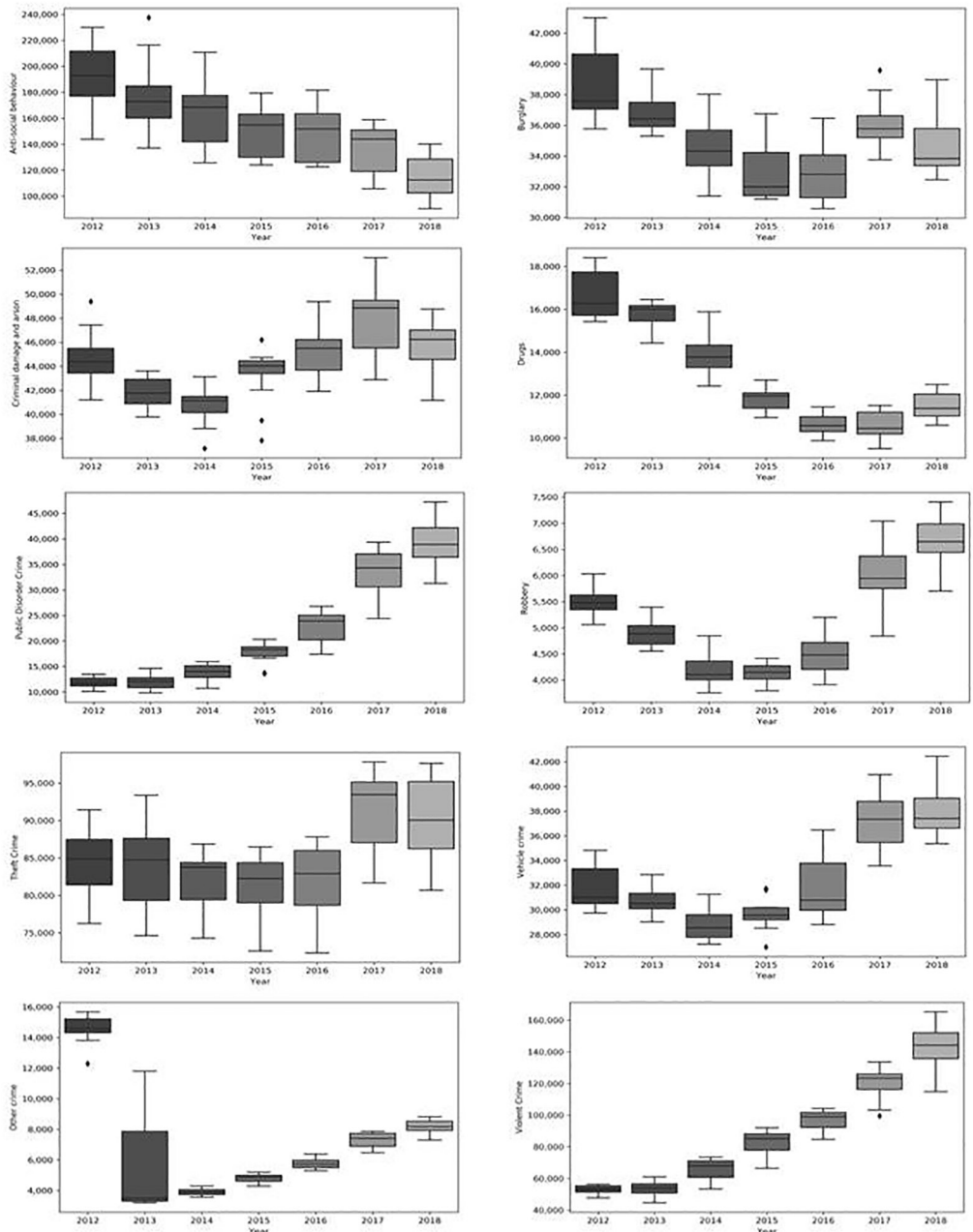
The rich dataset in England and Wales, coupled with the UC programme being a ‘phased roll-out’ welfare system, is characterised by a single, more efficient centralised payment system. But the centralisation has resulted in the levels of benefits having decreased significantly, enables us to apply it as an empirical proxy in examining the crime-welfare spending nexus. As argued above, given the clear exogeneity of the rollout process (to households’ decision-making processes), we are able to adequately control for the standard policy rollout effect, whilst evaluating evidence of differential human capital in affecting the crime-welfare spending nexus across different crime types.

To preview, we find significant heterogeneity in different crime types affecting the crime-welfare spending nexus. Specifically, criminal damage and arson are found to exhibit a robust positive welfare spending-crime nexus, with much weaker evidence on vehicle crimes and robbery. In contrast, public disorder and weapon possessions as a crime type display a consistent negative welfare spending-crime nexus. Based on our theoretical propositions, we therefore theorise the former as exhibiting the characteristics of being criminal human capital-dependent, whilst for the latter, welfare spending would serve as an effective secondary policy measure to aid in crime reduction. These findings are reaffirmed when we apply an alternative estimation technique based on using non-UC real benefit expenditure as an instrumental variable.

In terms of the existing literature, our study is closest to Machin and Marie (2006), Tiratelli *et al* (2020), and D’Este and Harvey (2024). The former found that the toughening of the Jobseekers Allowance (basically, the unemployment benefit regime) led to an increase in crime rates in the UK, whereas the latter two share the same policy focus as ours, i.e. the crime-UC nexus at a local level across England and Wales. There are nonetheless many fundamental differences in our study. For instance, Tiratelli *et al* (2020) have documented a positive crime-UC relationship at the aggregate level, but failed to identify the significant heterogeneity across different crime types, which can be observed in Figure 1.

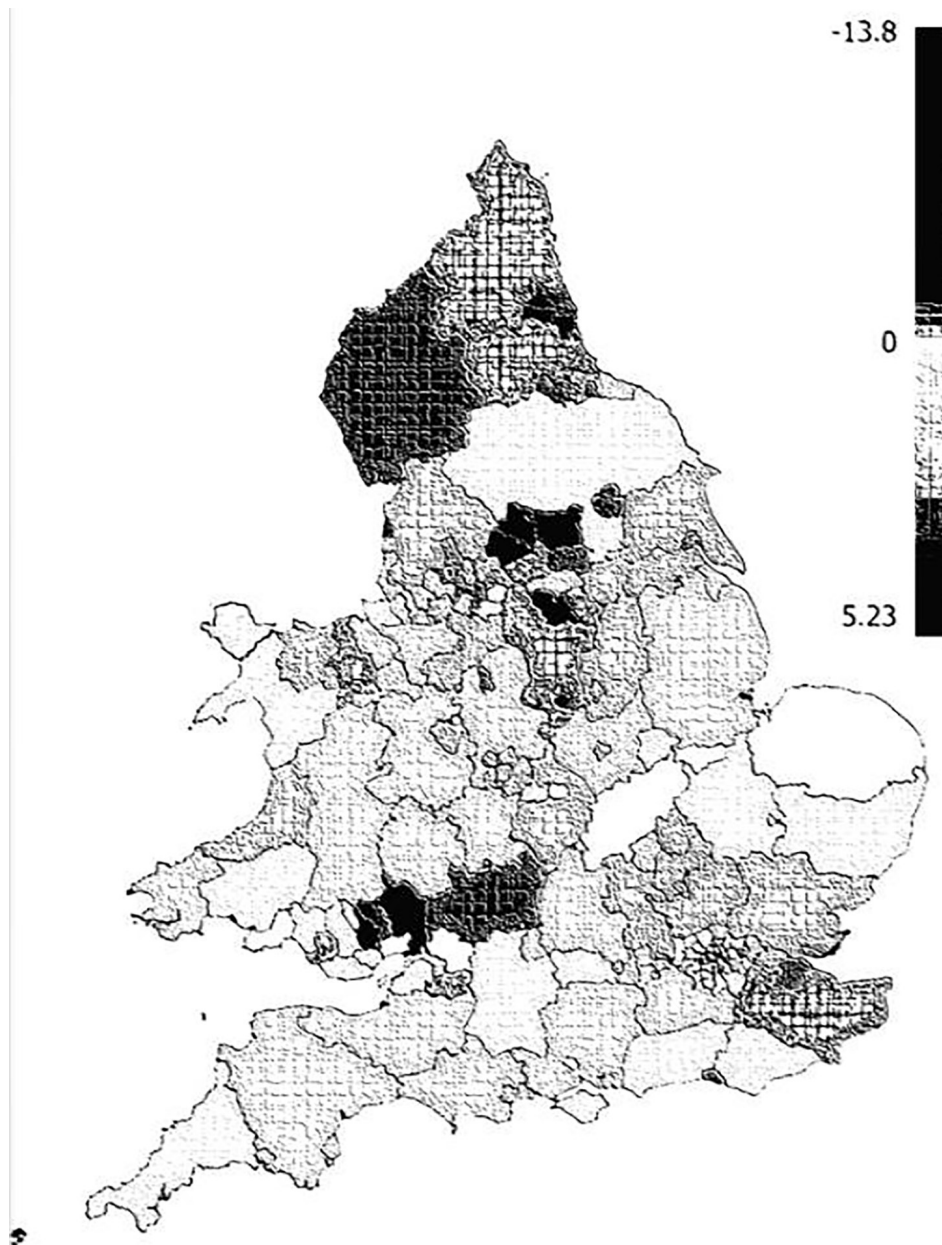
D’Este and Harvey (2024) undertake a more thorough policy-specific evaluation, by applying a quasi-experimental methods (difference-in-difference, triple differences, event studies), to evaluate the UC reform as a time-varying policy event in affecting crime and court repossession orders. While they did focus on similar county- and monthly-level data as ours and evaluated across different crime types, the lack of a theoretical foundation prevent them from being able to identify and explain why only acquisitive crime such as burglaries is found to be responsive to UC. In contrast to these two studies, our main objective involves attempting to evaluate the more general nexus between cash transfer/welfare spending on criminal activities, when the latter is distinguished between basic and criminal human capital-dependent activities –a differential concept initially introduced by Mocan *et al* (2005). To distinguish between the policy effect (as being the core interest of the two aforementioned studies) and our focus, we distinguish between an *introductory* effect and a *level* effect in our empirical evaluations.

Figure 1: Box plots of ten different crime types, 2012–2018



Sources: data.police.uk; authors' calculations

Figure 2: Absolute difference of aggregate crime rate between year 2014 and 2018 for different counties in England and Wales



Sources: data.police.uk; authors' calculations

Note: The darker intensity areas reflect drastic change in crime rates, whereas lighter tone reflects relative stability.

In addition to these two, our study is also related to other similar studies on crime that found the effects of policies or shocks to differ markedly across different types of crime. For example, in examining crime rates at the local administrative level in nineteenth-century France, Bignon *et al* (2017) found a negative income shock to cause an increase in theft and property crime, but a significant decrease in violent crime. The Colombia-based study of Cortés *et al* (2016) found violent crime and robbery to respond differently to negative economic shocks.

Similarly, when assessing the effect of military enlistment on crime in the United States during the Vietnam War era, Lindo and Stoecker (2014) found that draft eligibility increased incarceration for violent crimes but decreased incarceration for non-violent crimes. Moreover, these different effects appeared to vary by region, with results obtained often overturning findings based on more highly aggregated data (Cook and Winfield 2015). Indeed, studies such as Raphael and Winter-Ebmer (2001) and Arnio *et al* (2012) found significant state- or region-specific effects in their empirical analysis. For our study, even at an aggregate level, Figure 2 clearly illustrates that the absolute difference between crime rate in 2013 and 2018 can be markedly different across the different counties. In addition to crime heterogeneity, this therefore highlights the equal importance of county-level empirical analysis.

The remainder of the paper is structured as follows. Section 2 presents a theoretical model of crime and cash transfer, which includes the derivation of three theoretical propositions explaining the mechanism linking welfare spending and crime. Section 3 discusses the empirical strategy and data in detail. Section 4 presents the results and interprets these results according to the theory. Finally, Section 5 concludes the paper.

2. A SIMPLE MODEL OF DIFFERENTIAL CRIME AND CASH TRANSFER

We provide a theoretical basis by developing a simple economic model of crime heterogeneity (two broadly-defined types of criminal activity with different natures) and cash transfer/welfare spending. Although in reality the British UC programme is a more sophisticated scheme than a cash transfer especially when, post-reform, the level of benefits has decreased for individual recipients, we believe it adequately captures the salient feature of welfare spending as an exogenous income level earned by the individuals, compared to both market work and illegal/criminal activities.

There is a continuum of identical infinitely-lived individuals $i \in (0,1)$, in the economy living in time t , with a typical individual i deriving utility from consumption, $C_{i,t}$, and leisure, $l_{i,t}$. Each individual has a unit endowment of time, and in addition to leisure allocates time to legal employment, $n_{i,t}$ (earning real market wage rate, w_t), basic criminal activities, $y_{B,i,t}$, and criminal human capital-dependent crime, $y_{C,i,t}$. Hence, $l_{i,t} + n_{i,t} + y_{B,i,t} + y_{C,i,t} = 1$. Consistent with evidence documented in studies such as Pi and Zhang (2020), the illegal 'earnings' from criminal human capital-dependent crime, are determined in

effective terms, $h_{i,t}y_{C,i,t}$ where $h_{i,t}$ is akin to a crime-specific human capital level which represents the accumulated stock of ‘investment’ made by individual i into building up criminal human capital, with an initial endowment equals to unity.³ In contrast, basic crime, $y_{B,i,t}$, does not depend on this. Given that it is peripheral to our main focus, for convenience, we also assume that the human capital level of legal employment is normalised to unity.

In each period t , a typical individual i solves his/her dynamic intertemporal optimisation problem by choosing sequences of consumption ($C_{i,t}$), time allocated to legal employment ($n_{i,t}$), basic criminal activities ($y_{B,i,t}$), criminal human capital-dependent criminal activities ($y_{C,i,t}$), and next-period stock of crime-specific human capital ($h_{i,t+1}$) from time zero to infinity, so as to maximise the lifetime expected utility function:

$$u_i^t = E_t \sum_{t=0}^{\infty} (1 + \Lambda)^{-t} [\eta^C \ln(C_{i,t}) + \eta^L \ln(1 - n_{i,t} - y_{B,i,t} - y_{C,i,t})] \quad (1)$$

where E_t is the expectation operator, $\Lambda \in (0, 1)$ is the discount factor, η^C and η^L are the preference parameters for consumption and leisure respectively.

In standard fashion, the budget constraint is state-dependent. Specifically, assuming a two-state Markov process, in each period at a constant probability $\pi \in (0, 1)$, the individual receives net income from formal market work (similar to studies such as Imrohoroğlu *et al* (2004), an assumption in which a fraction $\varrho \in (0, 1)$ of this income is lost to crime is specified, which means individuals become victims of crime too), a cash transfer/welfare payment, $Z_{i,t}$, and the illicit income from crime, $X_{i,t}$. Further, in line with Neanidis and Papadopoulou (2013), we assume that the amount of resources that are lost to crime, $X_{i,t}$, corresponds only to the fraction of the victim’s legal income. The illegal income, $X_{i,t}$, is therefore not at risk to crime. In contrast, at a probability $1 - \pi$, the individual is caught when committing crime, resulting in zero income for the period.⁴ The period-specific income of the individual is therefore:

$$Income_{i,t} = \begin{cases} (1 - \varrho)n_{i,t}w_t + X_{i,t} + (1 - \varrho)Z_{i,t}, & \text{with prob } \pi \\ 0, & \text{with prob } 1 - \pi \end{cases} \quad (2)$$

which is spent on consumption, $C_{i,t}$, and a contribution/investment made to crime-specific human capital, $J_{i,t}$, where for analytical convenience, the latter is assumed to be a small fraction diverted from the net cash transfer received, $J_{i,t} = \kappa(1 - \varrho)Z_{i,t}$, $\kappa \in (0, 1)$, as in those mis-targeting instances described in Cameron and Shah (2014). To ensure that the cash transfer positively affects consumption, $\partial C_{i,t} / \partial Z_{i,t} > 0$, $\kappa < \pi$ must also hold.

Further, following Lochner (2004) and Neanidis and Papadopoulou (2013), the illegal income from crime, $X_{i,t}$, is increasing in the time individuals spend in these activities, and is therefore given by:

$$X_{i,t} = (y_{B,i,t} + h_{i,t}y_{C,i,t})\varrho(n_{j,t}w_t + Z_{j,t}), \quad (3)$$

which is a linear function of the total resources lost to crime by another individual j , $j \neq i$. Finally, the growth of crime-specific human capital over time

depends positively on the resources diverted to building up the criminal human capital, as well as the raw time spent in the activities:

$$\frac{h_{i,t+1}}{h_{i,t}} = [\kappa(1 - \varrho)Z_{i,t}]^\beta (y_{C,i,t})^\psi, \text{ where } \beta \geq 0, \psi \geq 0. \quad (4)$$

Assumption: $\psi = 1$. For analytical tractability, we assume that the growth of crime-specific human capital linearly depends on raw time spent on these criminal activities. Then, by imposing the terminal conditions, $\lim_{T \rightarrow \infty} (1 + \Lambda)^{-T} E_t(\lambda_{t+T} h_{i,t}) = 0$ and let $\sum_{k=1}^{\infty} (1 + \Lambda)^{-k} E_t(Z_{i,t+k}) = Z_{i,t}/\Lambda$, solving the individuals' intertemporal optimisation problem yields the first-order conditions of individual i (see Appendix A).

In a symmetric equilibrium, given that all individuals would behave optimally in a similar manner, we know that on aggregate, $n_{i,t} = n_{j,t} = n_t$, $y_{B,i,t} = y_{B,t}$, $y_{C,i,t} = y_{C,t}$, $h_{i,t} = h_t$, $C_{i,t} = C_t$, $Z_{i,t} = Z_{j,t} = Z_t$. The optimal time allocations to criminal human capital-dependent and basic criminal activities in the economy are therefore given by:

$$y_{C,t} = \frac{C_t}{C_{t-1}} [(1 + \Lambda)^{-1} \varrho \pi n_t w_t + (1 - \varrho)^\beta (\kappa Z_t)^\beta]^{-1}, \text{ and,} \quad (5)$$

$$y_{B,t} = \frac{1}{2} \left[1 - \frac{C_t}{\eta^c} \frac{(h_t - 1) \eta^l}{h_t [\kappa(1 - \varrho) Z_t]^\beta} - \frac{(1 - \varrho) w_t}{\varrho} - y_{C,t} \right]. \quad (6)$$

Proposition 1: *The cash transfer, Z_t , reduces the rate of criminal human capital-dependent crime, $y_{C,t}$, if the direct marginal consumption gained from the transfer is larger than the marginal gains from diverting some welfare payment into criminal human-capital investment. In contrast, the crime-cash transfer relationship is positive if the latter outweighs the marginal consumption gain.*

$$\frac{\partial y_{C,t}}{\partial Z_t} > 0, \text{ if } (\pi - \kappa)^{-1} C_{t-1} < [(1 - \varrho)^{\beta-1} \kappa^\beta \beta (Z_t)^{\beta-1} y_{C,t}]^{-1}; \quad (7)$$

$$\frac{\partial y_{C,t}}{\partial Z_t} < 0, \text{ if } (\pi - \kappa)^{-1} C_{t-1} > [(1 - \varrho)^{\beta-1} \kappa^\beta \beta (Z_t)^{\beta-1} y_{C,t}]^{-1}. \quad (8)$$

Proposition 2: *Assuming $\partial y_{C,t} / \partial Z_t \neq 0$, the relationship between the level of welfare payments and basic criminal activities is positive if the cash transfer reduces criminal human capital-dependent crime, and that the consumption level in the previous period is higher than the probability-adjusted cash transfer received in the current period, and vice versa.*

$$\frac{\partial y_{B,t}}{\partial Z_t} > 0, \text{ if } C_{t-1} \geq (\pi - \kappa)(1 - \varrho) Z_t \text{ and } \frac{\partial y_{C,t}}{\partial Z_t} < 0; \quad (9)$$

$$\frac{\partial y_{B,t}}{\partial Z_t} < 0, \text{ if } C_{t-1} < (\pi - \kappa)(1 - \varrho) Z_t \text{ and } \frac{\partial y_{C,t}}{\partial Z_t} > 0. \quad (10)$$

The proofs for Propositions 1-2 are straightforward differentiation (see Appendix A).

In addition to the ‘level’ relationship between crime and cash transfer, we also examine the *introductory effect* of the UC policy to crime, based on the asymmetric characteristics of $y_{B,t}$ and $y_{C,t}$. Specifically, by supposing that time $t=0$ corresponds to the first disbursement of the UC payments, we can expect the policy *introductory effect* to have a comparatively short-term context. Indeed, based on (7)–(8), we would expect the sign of the *introductory effect* to depend largely on the strength of the term associated with the marginal consumption gained from cash transfer, vis-à-vis the strength of the second term, which is associated with the marginal gains from diverting the reduced level of welfare payments away from criminal human capital-investment. Specifically, we propose the following.

Proposition 3: *The introductory effect of the UC scheme is positive for criminal activities that are criminal human capital-dependent in nature, whereas the introduction of the UC scheme reduces basic crime.*

The proof is Appendix A. In short, given that

$(\pi - \kappa)(C_{t-1})^{-1} > (1 - \varrho)^{\beta-1} \kappa^{\beta} \beta (Z_t)^{\beta-1} y_{C,t}$, $\frac{\partial y_{C,t}}{\partial Z_t}$, evaluated at the beginning of time ($t=0$) will be greater than zero, which also imply the opposite effect for basic criminal activities, i.e. $\frac{\partial y_{B,t}}{\partial Z_t} < 0$ evaluated at time 0.

Finally, it is worth noting that, the two types of crime in the theory are broadly defined. In practice, based on a penal code-based classification, a number of different types of crime may fit the characteristics of $y_{C,t}$, some other $y_{B,t}$, and the remainder not explainable by this crime-specific human capital framework. It is part of our research objectives to let the empirical results inform these. In other words, we do not make a prior assumption on which type of crime belongs to $y_{B,t}$ or $y_{C,t}$, but instead let the empirical results speak. Specifically, we would expect a crime type that empirically exhibits both **positive level** and *introductory* effects to be crime human capital-dependent. In contrast, for a crime type that exhibits both **negative level** and *introductory* effects empirically, it is likely to be basic in nature and therefore most likely to be effectively reduced by welfare spending.

3. EMPIRICAL STRATEGY AND DATA

As mentioned, to examine the effects of welfare spending on crime rates empirically, we focus on the case of the UC system in England and Wales. Specifically, the benchmark form of the empirical model is given by:

$$y_{jt}^k = \alpha_0 + \alpha_1 Z_{jt} + \alpha_2 U_{jt} + \alpha_3 \tau_{jt} + \sum_{l=1}^L \psi_l X_{l,jt} + \epsilon_j + \epsilon_t + u_{jt}, \quad (11)$$

where $j(t)$ is the county/unitary authority (time) index, y_{jt}^k refers to the logarithm of the crime rate of type k in county j and time t , Z_{jt} is the logarithm of UC rate, U_{jt} and τ_{jt} measure the *introductory* effect of the UC policy and the associated time trend post-occurrence of the first payments respectively, $\{X_{l,jt}\}_{l=1}^L$ denote

the set of L county-specific control variables that are well-recognised determinants of crime, ϵ_j (ϵ_t) is the time (county)-invariant county (year-by-month) fixed effects, and is u_{jt} the random error terms uncorrelated with the regressors.^{5,6}

Based on the theoretical propositions, the coefficients of primary interest are α_1 and α_2 . Although interpretation based solely on the sign of α_1 could indicate a crime type being either basic or criminal human capital-specific, a joint-positive estimate with α_2 would affirm its nature as being driven by criminal human capital, hence most-likely a ‘beneficiary’ of a mis-targeting of welfare payments. In contrast, a joint-negative would affirm a crime type not being influenceable by criminal human capital, therefore most-effectively reducible by welfare payments. In addition, a standalone interpretation of the sign of α_2 would also enable us to provide a more accurate picture on the role of the UC policy in affecting a certain crime type (compared to Tiratelli *et al* 2020), despite this – if without corresponding statistical significance for – not allowing us to reconcile the empirical evidence with the theory.

As an in-built means for robustness checks, we consider three specifications: (i) $L=0$ for Specification 1; (ii) $L=4$ for Specification 2, where population density, logarithm of real gross weekly wage (to reflect income differences across counties), unemployment rate, and logarithm of number of police per capita are included. The inclusion of the first two is standard in the crime literature. The exogenous effect of unemployment on crime is well-established in studies such as Mauro and Carmeci (2007) and Fougère *et al* (2009), and this is on top of it being a common indicator used as a proxy for business cycle (Detotto and Otranto 2012). The significant role of police personnel and its deterrence mechanism in general is documented in studies such as Corman and Mocan (2000) and Draca *et al* (2011). Lastly, for Specification 3, we attempt to capture the significant lags documented in Tiratelli *et al* (2020) between enrolment and disbursement of UC payments, by modifying U_{jt} not just to equal one at the time when UC payments are first recorded for a county, but also the preceding 11 months (year) in the monthly (annual)-based regression.

Two additional controls that are of slightly longer-term nature – national-level CPI growth rate and county-level conviction rate – are included in this specification to account for the lengthier consideration of the *introductory* effect. The former is attributed to the recent study of Draca *et al* (2019) who found that changes in prices leads to crime variations, whereas the latter reflects the different quality of the judiciary system across counties (Mocan *et al* 2020).

In terms of the data, for crime measures we employ a reverse geocoding technique similar to Bernasco and Block (2011) to build both our monthly and annually crime rates from the bottom-up.⁷ Specifically, from the open data source of data.police.uk, all individual street-level crime incidence reported are tagged to the different counties, based on coordinates (latitude and longitude), and then aggregated according to the nature of the crime type reported. Specifically, we examine 10 different classifications (anti-social behaviour,

burglary, criminal damage and arson, drugs, other crime, public disorder and weapon possessions, robbery, theft, vehicle crime, and violent crime). These cover the months from 2010–12 to 2019–12 (for annual-based estimation, the years of 2011–19), and a total of 173 counties. For the UC data, as well as the real benefit expenditure data we used as an instrument, we obtain the county-level information from the Department for Work and Pensions.

The policing figures are sourced from the open data site of the Home Office, while the other data are drawn primarily from the Office for National Statistics. The summary statistics of the full sample are presented in Table 1, with further information on data treatments and frequency conversion documented in Appendix B. Nevertheless, it is worth noting that for econometric estimation the effective sample size is much smaller, as this is capped by the available observations for the logarithm of the UC rate, at 9,975 for the monthly data and 923 for the annual data.

In terms of econometric strategy, we begin with a monthly-based fixed-effect estimation of the benchmark model, with the three different specifications applied to the 10 different crime types. Formal Hausman tests are implemented, but save for a negligible few of the skeletal Specification 1 estimations, test statistics are predominantly in favour of fixed-effect estimators. Endogeneity tests are then implemented, based on the chi-squared test documented in Baum *et al* (2003, 2007). To preview, in about half of the benchmark estimations the UC rate is found to be endogenous.

To provide a robustness check to the differential effects across crime types found in the benchmark estimations, we then apply a panel-instrumental variable (panel-IV) approach, using a much more exogenous monetary measure of non-UC benefit expenditure (in real terms) per claimant as an instrument. In this instance, only the estimation for three crime types suffers from endogeneity. While we do not claim that our instrument fully addresses the endogeneity issue, this additional estimation provides some degree of robustness to our attempted documentation of the differential effects across crime types.

We then estimate the benchmark model using annual data. This serves two purposes: (i) additional robustness; (ii) the availability of county-level educational attainment data enables us to construct a proxy measure based on relative human capital level, which allows us to use additional instruments in the subsequent IV estimation to provide better reconciliation of any *introductory* effect observed empirically, to the criminal human capital-based theoretical explanations. Specifically, this is calculated as:

$$RHC_{jt} = N_{jt}^{NQ} / N_{jt}^{Q4}, \quad (12)$$

where N_{jt}^{NQ} and N_{jt}^{Q4} are the proportion of residents without any educational qualification and with tertiary education respectively.⁸ For the annual data-based estimation, this series allows us to experiment further with different instrumenting strategies: (i) using real benefit expenditure per head and the

Table 1: Summary Statistics*

Variables	Monthly (2010-12 to 2019-12)				Annually (2011 to 2019)					
	Mean	Std Dev.	Min	Max	Mean	Std Dev.	Min	Max	Obs**	
Crime rates:										
Anti-social behaviour	2.97	1.85	0	29.55	18,759	35.33	21.11	0	277.73	1,557
Burglary	0.65	0.35	0	8.33	18,726	7.72	3.91	0	54.85	1,557
Criminal damage & arson	0.80	0.34	0	5.07	17,214	8.90	4.10	0	42.50	1,557
Drugs	0.25	0.40	0	10.15	18,852	3.03	4.55	0	76.21	1,557
Other crime (includes forgery, perjury, & other white-collar crimes)	0.44	1.64	0	73.65	18,684	4.91	15.69	0	496.35	1,557
Public disorder and weapon possessions	0.41	0.51	0	11.64	18,852	4.97	5.88	0	88.05	1,557
Robbery	0.11	0.15	0	3.10	17,984	1.26	1.76	0	22.76	1,557
Theft (includes theft from the person, bicycle theft, shoplifting, & other theft)	1.66	3.68	0	72.53	18,852	20.11	43.37	0	710.57	1,557
Vehicle crime	0.60	0.33	0	4.59	18,739	7.16	3.75	0	38.03	1,557
Violent crime, including sexual offences	1.69	1.28	0	22.18	18,852	20.39	14.98	0	215.98	1,557
Welfare spending/Cash transfer measures:										
logarithm of UC rate	1.74	1.87	-5.48	4.62	9,975	1.36	2.32	-7.27	4.55	923
logarithm of real benefit expenditure per claimant	2.60	0.52	1.38	4.37	18,753	5.09	0.52	3.91	7.13	1,557
Other variables										
Proportion with NVQ4 qualification & above	36.18	11.13	15.30	100.00	18,744	36.23	11.13	15.30	100.00	1,548
Proportion with no qualification	54.77	8.97	1.10	24.80	18,648	8.94	3.52	1.10	24.80	1,540
Real gross weekly pay	535.10	82.06	379.24	1,102.49	18,525	534.89	81.88	382.69	1,083.27	1,530
Population density	24.45	31.21	0.26	160.94	18,852	24.47	31.25	0.26	160.94	1,557
Unemployment rate	0.03	0.01	0.01	0.08	18,597	0.03	0.01	0.01	0.07	1,538
Number of policing staff	8,399	11,369	694	33,820	18,852	8,391	11,365	694	32,894	1,557
CPI growth rate	0.16	0.28	-0.70	0.80	18,852	0.16	0.08	0.04	0.30	1,557
National-level conviction rate	0.79	0.04	0.14	1.00	18,852	0.79	0.04	0.14	1.00	1,557

Notes: A detailed description of the variables are presented in Table B1.

* For some of the countries, data for the second half of 2019 were not made available at the time of the analysis. ** The number of observations reported spans the entire period of the full sample. However, in econometric estimation, the effective sample size is capped by the available observations for logarithm of UC rate.

change of the relative human capital proxy in jointly instrumenting for the UC rate and *introductory* effect; and (ii) in addition to these two, using also the level measure of relative human capital to serve as joint-instruments for the UC rate, the *introductory* effect, and the post-UC trend measure.

Again, we acknowledge that none of these strategies is able to fully account for endogeneity, but for our main research question this should be sufficient in allowing us to conservatively distinguish the differential effect across crime type in influencing the crime-welfare spending nexus. Lastly, as we have exercised a rule-of-thumb, “*n instruments for n regressors*” strategy, with under-identification more of a concern than over-identification. To test this, we implement the under-identification test of Kleibergen and Paap (2006).

4. RESULTS AND INTERPRETATION

The first set of results estimated using monthly data is presented in Tables 2 and 3. Disregarding the endogeneity issue identified, on the surface we observe that most crime rates do share a positive association with the UC rate, which is consistent with the empirical estimates of Tiratelli *et al* (2020) using aggregate crime data. In contrast to their findings, with the post-UC time trend properly accounted for, and the exogeneity of UC rate as a regressor being in doubt, we find this positive UC-crime association to be largely independent of the UC policy. A quick examination of the statistically significant estimates of the other control variables commonly documented as determinants of crime in the literature indicates the importance of crime heterogeneity. As an example, the estimated coefficients for police personnel are negative for burglary, vehicle crime, criminal damage and arson, but are positive for violence, public disorder and weapons possession. In evaluating the effect of welfare spending on crime, it is therefore necessary to account for the different characteristics of crime.⁹

To address the endogeneity between the crime and UC rates (both are effectively ‘headcount’ measures), Tables 4 and 5 summarise the set of results based on the fixed-effect estimation using real benefit expenditure per claimant as an instrument for the UC rate. Notwithstanding the fact that this is a more appropriate measure for welfare spending, further endogeneity tests using the instrument have indeed revealed that, except for violent crime, it is a relatively more exogenous regressor.¹⁰ Having accounted for the endogeneity issue, we find that both the estimated α_1 and α_2 are statistically significant for robbery and property crimes (vehicle crime and arson). These findings suggest that these criminal activities exhibit the characteristics of criminal human capital-dependent, and are therefore most-likely a ‘beneficiary’ of any mis-targeting of welfare payments. The observation on property crimes also appears to be consistent with the American evidence of Burek (2005) and Liebertz and Bunch (2018), but contradicts the earlier British finding of Farrall (2006), who concentrates solely on the period of economic crisis in early-2000s.

On the opposite end of the spectrum, the crimes of public disorder and weapons possession, which generally cover the infamous knife crime (caught

before a violent crime is committed), as well as the ‘catch-all’ category of other miscellaneous crime, register statistically significant negative estimates for both and . In relation to the three propositions, these indicate that these crime types do not contain any criminal human capital element, and can therefore be reduced effectively through the income effect brought about by welfare spending. Non-conclusive evidence is observed for the remaining crime types, although the crime-UC positive association remains robust for anti-social behaviour. Given that these are likely to be basic crimes, by virtue of Proposition 2, this may suggest that on average, anti-social behaviour in England and Wales may reflect the incidence of individuals who experience a shortfall in consumption that cannot be met by the expected welfare payment received.

Next, we assess the annual data-based, fixed-effect estimation results. The corresponding estimation for Tables 2 and 3 are summarised in Tables 6 and 7. Similar to the monthly results, without accounting for the endogeneity of the UC rate, we observe that most crime types correlate positively with the gradual rise in the UC rate. Likewise, except for robbery and theft, none of these positive associations can be attributed directly to the roll-out of the UC welfare scheme. Interestingly, the robbery rate appears to respond positively to the introduction of UC, whereas the theft rate registered negative estimates for α_2 . Given what is commonly known about these two types of crime, interpreting these against Proposition 3 lends some supports to the validity of our crime heterogeneity theory.

Finally, Tables 8 and 9 summarise the results based on IV estimation of the full specification of (11), i.e. Specification 3, using three different instrumenting strategies. As mentioned, to improve the matching of the empirical evaluation with the theory, two additional instrumental variables (based on the growth and the level of RHC_{jt}) are employed in the alternative strategies presented. In these instances, the consideration is therefore not as much of an econometric one, but one driven by the desire to bring in relative human capital/educational attainment information into the estimation. In other words, any statistically significant policy effect observed is comparatively more attributable to variation in relative human capital, and therefore more relatable to the theory.

This being said, all but one of the Kleibergen-Paap instrument identification tests did return a P-value that is less than 0.1, which means the estimation of α_1 , α_2 , and α_3 remain adequately identified. In both Tables 8 and 9, we see that the positive α_1 and α_2 for criminal damage and arson remain statistically significant, which reaffirms their criminal human capital-driven nature in England and Wales. Likewise, the joint-negative α_1 and α_2 for public disorder and weapons possession remains robust, which implies that welfare spending is likely to have a direct effect in tacking such criminal activities.

Although evidence remains largely inconclusive for the other crime types, the introduction of UC appears to result in a statistically significant upward shift in the crime rates of robbery and vehicle crime. These perhaps reflect a growing opportunity hypothesis as documented by Wright *et al* (2017) for the

Table 2: Monthly-based, Fixed-Effect Estimation of the Benchmark models

	<i>Anti-social behaviour</i>			<i>Burglary</i>		
	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>
Logarithm of UC rate	0.0399*** (0.0116)	0.0369*** (0.0116)	0.0372*** (0.0114)	0.0210*** (0.0053)	0.0197*** (0.0053)	0.0197*** (0.0053)
<i>Introductory</i> effect	0.0003 (0.0170)	-0.0007 (0.0168)	0.0001 (0.0167)	0.0363* (0.0198)	0.0287 (0.0194)	0.0288 (0.0194)
Post-UC time trend	-0.0254*** (0.0083)	-0.0191*** (0.0040)	-0.0193*** (0.0037)	0.0122** (0.0051)	0.0118** (0.0056)	0.0094* (0.0053)
Logarithm, real gross weekly wage		0.4018 (0.3502)	0.3904 (0.3464)		0.2672 (0.2599)	0.2662 (0.2616)
Population density		0.0401*** (0.0106)	0.0402*** (0.0106)		0.0115 (0.0077)	0.0115 (0.0077)
Unemployment rate		2.1058 (1.6205)	2.0842 (1.6239)		-0.3548 (1.0772)	-0.3566 (1.0780)
Police per capita		0.1516 (0.3218)	0.1681 (0.3432)		-0.8167*** (0.1816)	-0.8153*** (0.1830)
CPI growth rate (national-level)			1.4878 (1.2001)			6.2224*** (1.9793)
Conviction rate (county-level)			0.3847 (0.7368)			0.0326 (0.3656)
Year-by-month FE	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES
Counties/Observations	173/9975	171/9807	171/9807	173/9949	171/9795	171/9795
Observation per group (min/avg/max)	26/57.7/79	23/57.4/79	23/57.4/79	12/57.5/77	23/57.3/77	23/57.3/77
Overall R2	0.0821	0.0287	0.0288	0.0535	0.0103	0.0112
Endogeneity test (P-value)	0.0935	0.1790	0.2377	0.5999	0.8196	0.8306
Hausman test (P-value)	0.0000	0.0000	0.0000	0.0307	0.0000	0.0000

Notes: *** Statistical significant at 1% level; ** significant at 5% level; * significant at 10% level.

Parentheses denote heteroskedasticity-consistent, robust standard errors.

Model Specification 3 uses an *Introductory* effect dummy with a different definition. Specifically, it equals 1 not just for the month when first UC entry appears, but also the previous 11 months to capture pre-expectations up to 12 months ahead.

	CD & Arson			Drugs			Other Crime		
	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3
	0.0295*** (0.0076)	0.0317*** (0.0081)	0.0308*** (0.0079)	0.0143** (0.0064)	0.0134** (0.0065)	0.0130** (0.0064)	-0.0151* (0.0081)	-0.0100 (0.0079)	-0.0103 (0.0078)
	0.0348* (0.0198)	0.0371* (0.0193)	0.0345* (0.0186)	0.0182 (0.0242)	0.0239 (0.0244)	0.0227 (0.0242)	-0.027 (0.0342)	-0.0129 (0.0327)	-0.0137 (0.0328)
	0.0017 (0.0041)	0.0009 (0.0046)	-0.0012 (0.0045)	-0.0452*** (0.0065)	-0.0457*** (0.0069)	-0.0473*** (0.0065)	-0.003 (0.0079)	-0.0026 (0.0081)	-0.0013 (0.0081)
		-0.2413 (0.4157)	-0.2053 (0.4173)		-0.2098 (0.2665)	-0.1951 (0.2669)		-0.1524 (0.3257)	-0.1420 (0.3286)
		-0.0348*** (0.0100)	-0.0349*** (0.0099)		0.0119 (0.0108)	0.0118 (0.0108)		-0.0635*** (0.0181)	-0.0635*** (0.0181)
		-1.4435 (1.4022)	-1.3750 (1.3878)		-0.1156 (1.6321)	-0.0864 (1.6299)		-1.7506 (1.4530)	-1.7312 (1.4511)
		-0.7722** (0.3813)	-0.8245** (0.3798)		0.3525 (0.2734)	0.3301 (0.2734)		0.7077*** (0.2460)	0.6927*** (0.2493)
			0.6124 (1.5338)			-16.6534*** (2.3389)			-7.8640** (3.0400)
			-1.2156*** (0.4613)			-0.5150 (0.3957)			-0.3494 (0.6643)
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	173/9978	171/9817	171/9817	173/9944	171/9795	171/9795	173/9943	171/9799	171/9799
	19/57.7/77	23/57.4/77	23/57.4/77	9/57.5/77	14/57.3/77	14/57.3/77	2/57.5/77	20/57.3/77	20/57.3/77
	0.0821	0.0259	0.0252	0.0077	0.2386	0.2354	0.1077	0.0024	0.0028
	0.0000	0.0000	0.0000	0.7389	0.5887	0.4253	0.0000	0.0000	0.0000
	0.0258	0.0000	0.0000	0.6917	0.0000	0.0000	0.0002	0.0000	0.0000

P-values in bold for the endogeneity tests mean the null of UC rate being exogenous is rejected. This means there is an endogeneity issue.

For the Hausman tests, a P-value <10% indicates a FE-estimator is the preferred option.

Table 3: Monthly-based, Fixed-Effect Estimation of the Benchmark models, continued

	<i>P. disorder & weapon possessions</i>			<i>Robbery</i>		
	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>
Logarithm of UC rate	-0.0180 (0.0150)	-0.0112 (0.0142)	-0.0108 (0.0141)	0.0008 (0.0081)	0.0019 (0.0082)	0.0023 (0.0081)
<i>Introductory</i> effect	0.0026 (0.0339)	0.0183 (0.0330)	0.0194 (0.0330)	0.0371 (0.0410)	0.0428 (0.0399)	0.0437 (0.0399)
Post-UC time trend	0.0709*** (0.0069)	0.0745*** (0.0071)	0.0726*** (0.0060)	0.0396*** (0.0074)	0.0409*** (0.0076)	0.0370*** (0.0080)
Logarithm of real gross weekly wage		0.2760 (0.5873)	0.2607 (0.5900)		0.1410 (0.3219)	0.1286 (0.3195)
Population density		-0.0711*** (0.0201)	-0.0711*** (0.0201)		-0.0029 (0.0137)	-0.0029 (0.0137)
Unemployment rate		-1.6322 (2.2947)	-1.6614 (2.2968)		1.5589 (1.4238)	1.5337 (1.4215)
Police per capita		1.8489*** (0.3991)	1.8713*** (0.4067)		-0.1963 (0.2714)	-0.1759 (0.2664)
CPI growth rate (national-level)			19.2675*** (2.2709)			12.3650*** (3.1142)
Conviction rate (county-level)			0.5196 (0.7373)			0.4438 (0.5221)
Year-by-month FE	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES
Counties/Observations	173/9974	171/9830	171/9830	172/9657	171/9538	171/9538
Observation per group (min/avg/max)	3/57.7/79	20/57.5/79	20/57.5/79	8/56.1/77	2/55.8/77	2/55.8/77
Overall R2	0.0833	0.0004	0.0003	0.0211	0.1108	0.1105
Endogeneity test (P-value)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Hausman test (P-value)	0.0688	0.0000	0.0000	0.4273	0.0000	0.0000

Notes: *** Statistical significant at 1% level; ** significant at 5% level; * significant at 10% level.

Parentheses denote heteroskedasticity-consistent, robust standard errors.

Model Specification 3 uses an *Introductory* effect dummy with a different definition. Specifically, it equals 1 not just for the month when first UC entry appears, but also the previous 11 months to capture pre-expectations up to 12 months ahead.

	Theft			Vehicle crime			Violent crime		
	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3
	0.0361*** (0.0105)	0.0358*** (0.0106)	0.0354*** (0.0105)	0.0366*** (0.0082)	0.0371*** (0.0083)	0.0373*** (0.0084)	0.0291** (0.0132)	0.0345*** (0.0129)	0.0338*** (0.0127)
	0.0057 (0.0137)	0.0039 (0.0131)	0.0028 (0.0130)	0.0099 (0.0215)	0.0089 (0.0209)	0.0094 (0.0209)	0.0120 (0.0199)	0.0205 (0.0192)	0.0184 (0.0191)
	0.0196*** (0.0036)	0.0189*** (0.0040)	0.0190*** (0.0040)	0.0336*** (0.0054)	0.0337*** (0.0056)	0.0327*** (0.0054)	0.0003 (0.0055)	0.0013 (0.0054)	0.0003 (0.0052)
		-0.0680 (0.3956)	-0.0528 (0.3969)		-0.1944 (0.3343)	-0.2011 (0.3350)		0.1004 (0.5013)	0.1281 (0.5010)
		0.0112 (0.0096)	0.0111 (0.0096)		-0.0114* (0.0066)	-0.0114* (0.0067)		-0.0598*** (0.0142)	-0.0598*** (0.0141)
		-0.4957 (1.3203)	-0.4666 (1.3154)		-0.4712 (1.3150)	-0.4839 (1.3154)		-1.7417 (1.8189)	-1.6888 (1.8056)
		0.0842 (0.1672)	0.0619 (0.1638)		-0.9692*** (0.1874)	-0.9602*** (0.1900)		0.5261** (0.2595)	0.4854* (0.2608)
			11.4688*** (1.3957)			15.4961*** (2.0249)			-1.3726 (1.8175)
			-0.5174* (0.2817)			(0.2261) (0.3812)			-0.9446** (0.4576)
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	173/9998	171/9835	171/9835	173/9967	171/9815	171/9815	173/10015	171/9845	171/9845
	21/57.8/78	23/57.5/78	23/57.5/78	10/57.6/79	23/57.4/79	23/57.4/79	28/57.9/80	23/57.6/80	23/57.6/80
	0.0021	0.1876	0.1903	0.0091	0.1963	0.1978	0.1450	0.0145	0.0148
	0.0704	0.2123	0.2959	0.0036	0.0503	0.0606	0.4177	0.1431	0.2462
	0.4607	0.0000	0.0000	0.0806	0.0000	0.0000	0.0000	0.0000	0.0000

P-values in bold for the endogeneity tests mean the null of UC rate being exogenous is rejected. This means there is an endogeneity issue.

For the Hausman tests, a P-value <10% indicates a FE-estimator is the preferred option.

Table 4: Monthly-based, IV estimation with fixed-effects (real benefit expenditure per claimant as instrument for UC rate)

	<i>Anti-social behaviour</i>			<i>Burglary</i>		
	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>
Logarithm of UC rate (instrumented)	0.0840*** (0.0230)	0.0728*** (0.0224)	0.0675*** (0.0218)	0.0337 (0.0224)	0.0254 (0.0228)	0.0249 (0.0219)
<i>Introductory</i> effect	0.0837 (0.0466)	0.0675 (0.0459)	0.0576 (0.0447)	0.0605 (0.0450)	0.0395 (0.0458)	0.0385 (0.0443)
Post-UC time trend	-0.0226*** (0.0085)	-0.0166*** (0.0040)	-0.022*** (0.0042)	0.0132*** (0.0051)	0.0122** (0.0053)	0.0090* (0.0054)
Logarithm, real gross weekly wage		0.3986** (0.1621)	0.3847** (0.1624)		0.2667* (0.1553)	0.2653* (0.1557)
Population density		0.0334*** (0.0052)	0.0345*** (0.0051)		0.0104** (0.0050)	0.0105** (0.0048)
Unemployment rate		1.8361*** (0.6562)	1.8520*** (0.6541)		-0.3973 (0.6126)	-0.3957 (0.6116)
Police per capita		0.1441* (0.0867)	0.1662* (0.0892)		-0.8179*** (0.0886)	-0.8156*** (0.0895)
CPI growth rate (national-level)			0.5761*** (0.3657)			0.4097 (0.4261)
Conviction rate (county-level)			0.4843*** (0.1884)			0.0494 (0.1739)
First stage: (Log UC rate)						
Log (real benefit expenditure per capita)	-1.0661*** (0.0744)	-1.0631*** (0.0761)	-1.1136*** (0.0763)	-1.0749*** (0.0747)	-1.0621*** (0.0761)	-1.1127*** (0.0763)
F-statistics for excluded instruments	205.47***	195.33***	213.01***	206.83***	194.92***	212.54***
Year-by-month FE	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES
Counties/Observations	173/9974	171/9807	171/9807	173/9948	171/9795	171/9796
Observation per group (min/avg/max)	25/57.7/79	23/57.4/79	23/57.4/79	11/57.5/77	23/57.3/77	23/57.3/78
Centered R2	0.3438	0.3602	0.3621	0.0649	0.0750	0.0751
Instrument identification test, Chi-sq(1) P-val	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Hausman test (P-value)	0.0000	0.0000	0.0000	0.0307	0.0000	0.0000

Notes: *** Statistical significant at 1% level; ** significant at 5% level; * significant at 10% level.

Parentheses denote heteroskedasticity-consistent, robust standard errors.

Model Specification 3 uses an *Introductory* effect dummy with a different definition. Specifically, it equals 1 not just for the month when first UC entry appears, but also the previous 11 months to capture pre-expectations up to 12 months ahead.

The instrument test is based on the Underidentification Test (LM test statistics) reported in `xtivreg2`. A P-value of zero indicates a strong instrument.

	CD & Arson			Drugs			Other Crime		
	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3
	0.1586*** (0.0264)	0.1538*** (0.0264)	0.1624*** (0.0260)	0.0222 (0.0232)	0.0266 (0.0239)	0.0317 (0.0229)	-0.1997*** (0.0321)	-0.2002*** (0.0330)	-0.1898*** (0.0313)
	0.2801*** (0.0546)	0.2688*** (0.0546)	0.2847*** (0.0538)	0.0333 (0.0496)	0.0491 (0.0507)	0.0583 (0.0491)	-0.3770*** (0.0719)	-0.3733*** (0.0730)	-0.3541*** (0.0703)
	0.0108 (0.0069)	0.0097 (0.0070)	-0.0129* (0.0074)	-0.0446*** (0.0061)	-0.0447*** (0.0060)	-0.0489*** (0.0064)	-0.0165 (0.0097)	-0.0161 (0.0099)	0.0147 (0.0098)
		-0.2486 (0.1859)	-0.2260 (0.1874)		-0.2099 (0.1510)	-0.1970 (0.1512)		-0.1408 (0.1859)	-0.1135 (0.1849)
		-0.0577*** (0.0065)	-0.0595*** (0.0066)		0.0094 (0.0062)	0.0083 (0.0061)		-0.0279*** (0.0085)	-0.0301*** (0.0083)
		-2.3424*** (0.7402)	-2.3665*** (0.7458)		-0.2116 (0.6564)	-0.2252 (0.6552)		-0.3439 (0.8971)	-0.3745 (0.8852)
		-0.7972*** (0.1192)	-0.8328*** (0.1204)		0.3498*** (0.0999)	0.3290*** (0.1001)		0.7480*** (0.1277)	0.7053*** (0.1262)
									0.4552 (0.3606)
									-0.9387*** (0.3354)
	-1.0730*** (0.0745)	-1.0650*** (0.0760)	-1.1157*** (0.0763)	-1.0752*** (0.0747)	-1.0603*** (0.0760)	-1.1112*** (0.0763)	-1.0755*** (0.0750)	-1.0586*** (0.0761)	-1.1109*** (0.0763)
	207.51***	196.3***	214.03***	207.21***	194.53***	212.29***	205.37***	193.58***	211.12***
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	173/9977	171/9817	171/9817	173/9944	171/9795	171/9795	173/9943	171/9799	171/9799
	18/57.7/77	23/57.4/77	23/57.4/77	9/57.5/77	14/57.3/77	14/57.3/77	2/57.5/77	20/57.3/77	20/57.3/77
	0.0257	0.0053	0.0168	0.1304	0.1309	0.1301	0.1211	0.1327	0.1491
	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	0.0258	0.0000	0.0000	0.6917	0.0000	0.0000	0.0002	0.0000	0.0000

As F-test statistics for excluded instruments are significant for all regressions, first-stage regression output is abbreviated to save space: only the estimated coefficient of instrument(s) is reported.

In theory, the first-stage estimated coefficients ought to be the same for the same specification, despite difference in crime types. Here, they differ due to the difference in missing observations occurring in different months and counties for the different crime types, hence creating a different sample for estimation of the first-stage equation.

For the Hausman tests, a P-value <10% indicates a FE-estimator is the preferred option.

Table 5: Monthly-based, IV estimation with fixed-effects (real benefit expenditure per claimant as instrument for UC rate), continued

	<i>P. disorder & weapon possessions</i>			<i>Robbery</i>		
	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>
Log UC rate (instrumented)	-0.2262*** (0.0322)	-0.2287*** (0.0327)	-0.2265*** (0.0315)	0.2164*** (0.0432)	0.2045*** (0.0428)	0.1915*** (0.0405)
<i>Introductory</i> effect	-0.3944*** (0.0700)	-0.3961*** (0.0712)	-0.3921*** (0.0691)	0.4502*** (0.0917)	0.4300*** (0.0895)	0.4059*** (0.0855)
Post-UC time trend	0.0557 (0.0058)	0.0587*** (0.0090)	0.0917*** (0.0092)	0.0552 (0.0126)	0.0552*** (0.0121)	0.0198 (0.0121)
Logarithm, real gross weekly wage		0.3029 (0.2267)	0.3085 (0.2276)		0.1678 (0.2244)	0.1362 (0.2221)
Population density		-0.0301*** (0.0077)	-0.0306*** (0.0075)		-0.0411*** (0.0099)	-0.0384*** (0.0095)
Unemployment rate		0.0043 (1.0274)	-0.0021 (1.0241)		-0.0642 (0.9939)	-0.0143 (0.9785)
Police per capita		1.8939*** (0.1258)	1.8848*** (0.1263)		-0.2841* (0.1662)	-0.2289 (0.1634)
CPI growth rate (national-level)			0.6326 (0.5510)			0.2482 (0.3088)
Conviction rate (county-level)			-0.1981 (0.3160)			1.0687*** (0.3740)
First stage (Log UC rate)						
Log (real benefit expenditure per capita)	-1.0870*** (0.0750)	-1.0702*** (0.0761)	-1.1212*** (0.0763)	-1.0293*** (0.0781)	-1.0254*** (0.0788)	-1.0788*** (0.0791)
F-statistics for excluded instruments	209.87***	197.92***	215.88***	173.61***	169.22***	185.92***
Year-by-month FE	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES
Counties/Observations	173/9974	171/9830	171/9831	172/9657	171/9538	171/9538
Observation per group (min/avg/max)	3/57.7/79	20/57.5/79	20/57.5/80	8/56.1/77	2/55.8/77	2/55.8/77
Centered R2	0.2776	0.2892	0.2914	0.0522	0.0670	0.0850
Instrument identification test, Chi-sq(1) P-val	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Hausman test (P-value)	0.0688	0.0000	0.0000	0.4273	0.0000	0.0000

Notes: *** Statistical significant at 1% level; ** significant at 5% level; * significant at 10% level.

Parentheses denote heteroscedasticity-consistent, robust standard errors.

Model Specification 3 uses an *Introductory* effect dummy with a different definition. Specifically, it equals 1 not just for the month when first UC entry appears, but also the previous 11 months to capture pre-expectations up to 12 months ahead.

The instrument test is based on the Underidentification test (LM test statistics) reported in `xtivreg2`. A P-value of zero indicating a strong instrument.

	Theft			Vehicle crime			Violent crime		
	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3
	-0.0148 (0.0198)	-0.0005 (0.0198)	0.0062 (0.0192)	0.1184*** (0.0264)	0.0937*** (0.0263)	0.0893*** (0.0252)	0.0013 (0.0231)	-0.0175 (0.0231)	-0.0057 (0.0223)
	-0.0905** (0.0368)	-0.0653* (0.0375)	-0.0529 (0.0361)	0.1643*** (0.0533)	0.1165** (0.0532)	0.1084** (0.0511)	-0.041 (0.0458)	-0.0787* (0.0463)	-0.0569 (0.0446)
	0.0158*** (0.0031)	0.0163*** (0.0031)	0.0216*** (0.0034)	0.0392*** (0.0058)	0.0377*** (0.0057)	0.0281*** (0.0058)	-0.0014 (0.0048)	-0.0025 (0.0045)	0.0038 (0.0047)
		-0.0640 (0.1695)	-0.0467 (0.1705)		-0.1991 (0.1729)	-0.2105 (0.1732)		0.1069 (0.2014)	0.1369 (0.2021)
		0.0180*** (0.0045)	0.0166*** (0.0044)		-0.0221*** (0.0056)	-0.0212*** (0.0054)		-0.0500*** (0.0048)	-0.0524*** (0.0047)
		-0.2242 (0.6908)	-0.2437 (0.6885)		-0.8904 (0.6948)	-0.8781 (0.6916)		-1.3473 (0.8727)	-1.3829 (0.8674)
		0.0915 (0.0577)	0.0636 (0.0570)		-0.9815*** (0.0878)	-0.9634*** (0.0874)		0.5366*** (0.0722)	0.4877*** (0.0710)
			0.1923 (0.5409)			0.4554 (0.4976)			0.0559 (0.6045)
			-0.6143*** (0.1398)			0.3978* (0.2206)			-1.0765*** (0.1834)
	-1.0811*** (0.0745)	-1.0730*** (0.0760)	-1.1241*** (0.0763)	-1.0810*** (0.0749)	-1.0557*** (0.0760)	-1.1175*** (0.0763)	-1.0801*** (0.0745)	-1.0731*** (0.0761)	-1.1243*** (0.0763)
	210.37***	199.12***	217.12***	208.07***	196.76***	214.54***	210.28***	198.89***	216.97***
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	173/9998	171/9835	171/9836	173/9967	171/9815	171/9816	173/10014	171/9845	171/9845
	21/57.8/78	23/57.5/78	23/57.5/79	10/57.6/79	23/57.4/79	23/57.4/80	27/57.9/80	23/57.6/80	23/57.6/80
	0.0257	0.0967	0.0995	0.0401	0.0656	0.0684	0.1986	0.1996	0.2038
	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	0.4607	0.0000	0.0000	0.0806	0.0000	0.0000	0.0000	0.0000	0.0000

As F-test statistics for excluded instruments are significant for all regressions, first-stage regression output is abbreviated to save space: only estimated coefficient of instrument(s) is reported.

In theory, the first-stage estimated coefficients ought to be the same for the same specification despite difference in crime types. Here, they differ due to the difference in missing observations occurring in different months and counties for the different crime types, hence creating a different sample for estimation of the first-stage equation.

For the Hausman tests, a P-value <10% indicate FE-estimator is the preferred option.

Table 6: Annual-based, Fixed-Effect Estimation of the Benchmark Models

	<i>Anti-social behaviour</i>			<i>Burglary</i>		
	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>
Logarithm of UC rate	0.0277*** (0.0081)	0.0266*** (0.0081)	0.0266*** (0.0082)	0.0195*** (0.0053)	0.0187*** (0.0052)	0.0187*** (0.0052)
<i>Introductory</i> effect	-0.0540 (0.0379)	-0.0355 (0.0382)	-0.0359 (0.0384)	-0.0008 (0.0265)	0.0030 (0.0273)	0.0028 (0.0274)
Post-UC time trend	-0.1416*** (0.0177)	-0.1465*** (0.0203)	-0.0585 (0.0389)	-0.1440*** (0.0183)	-0.1337*** (0.0175)	-0.2230*** (0.0405)
Logarithm, real gross weekly wage		0.2401 (0.2867)	0.2372 (0.2854)		0.1135 (0.2143)	0.1127 (0.2151)
Population density		0.0423*** (0.0096)	0.0423*** (0.0097)		0.0146** (0.0069)	0.0146 (0.0069)
Unemployment rate		2.8794 (1.9395)	2.8732 (1.9440)		0.6041 (1.3144)	0.6022 (1.3171)
Police per capita		0.2484 (0.3039)	0.2580 (0.3254)		-0.7869*** (0.1801)	-0.7840*** (0.1813)
CPI growth rate (national-level)			2.1132*** (0.5253)			-2.1428*** (0.5926)
Conviction rate (county-level)			0.1969 (0.6590)			0.0593 (0.3568)
Year FE	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES
Counties/Observations	173/923	171/907	171/907	173/923	171/907	171/907
Observation per group (min/avg/max)	4/5.3/7	2/5.3/7	2/5.3/7	4/5.3/7	2/5.3/7	2/5.3/7
Overall R2	0.1083	0.0236	0.0257	0.0021	0.0108	0.0137
Endogeneity test (P-value)	0.1850	0.2916	0.3121	0.3654	0.2003	0.2043
Hausman test (P-value)	0.0829	0.0000	0.0000	0.0293	0.0000	0.0000

Notes: *** Statistical significant at 1% level; ** significant at 5% level; * significant at 10% level.

Parentheses denote heteroskedasticity-consistent, robust standard errors.

Model Specification 3 uses an *Introductory* effect dummy with a different definition. Specifically, it equals 1 not just for the year when first UC entry appears, but also the previous year to capture pre-expectations up to 12 months ahead.

	CD & Arson			Drugs			Other Crime		
	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3
	0.0175*** (0.0063)	0.0197*** (0.0065)	0.0196*** (0.0064)	0.0210*** (0.0077)	0.0201*** (0.0077)	0.0200*** (0.0076)	-0.0191* (0.0108)	-0.0149 (0.0107)	-0.0149 (0.0108)
	0.0340 (0.0271)	0.0393 (0.0270)	0.0419 (0.0273)	-0.0015 (0.0430)	0.0058 (0.0449)	0.0072 (0.0446)	-0.0405 (0.0434)	-0.0460 (0.0442)	-0.0446 (0.0440)
	-0.0928*** (0.0169)	-0.0902*** (0.0176)	-0.1465*** (0.0389)	0.1194*** (0.0184)	0.1117*** (0.0186)	0.1865*** (0.0466)	0.0056 (0.0195)	-0.0022 (0.0199)	-0.1696*** (0.0463)
		-0.1791 (0.3219)	-0.1620 (0.3192)		-0.1485 (0.2791)	-0.1390 0.2786		-0.1600 (0.3101)	-0.1510 (0.3138)
		-0.0269*** (0.0073)	-0.0272*** (0.0072)		0.0184* (0.0094)	0.0182* (0.0094)		-0.0734*** (0.0168)	-0.0736*** (0.0168)
		-0.4545 (1.6670)	-0.4167 (1.6412)		0.6493 (2.1700)	0.6702 (2.1539)		-2.9309 (1.9693)	-2.9111 (1.9639)
		-0.6097* (0.3220)	-0.6678** (0.3245)		0.4778 (0.2910)	0.4456 (0.2931)		0.3217 (0.2813)	0.2913 (0.2805)
			-1.3653** (0.5499)			1.7876** (0.7217)			-4.0256*** (0.7090)
			-1.1912*** (0.4581)			-0.6599* (0.3882)			-0.6234 (0.6795)
	YES YES	YES YES	YES YES	YES YES	YES YES	YES YES	YES YES	YES YES	YES YES
	173/923	171/907	171/907	173/923	171/907	171/907	173/921	171/907	171/907
	4/5.3/7	2/5.3/7	2/5.3/7	4/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7
	0.0003	0.0400	0.0378	0.0155	0.3054	0.3015	0.1633	0.0027	0.0028
	0.0002	0.0006	0.0001	0.1354	0.1587	0.1065	0.0070	0.0048	0.0070
	0.0171	0.0000	0.0000	0.7969	0.0014	0.0000	0.0018	0.0000	0.0000

P-values in bold for the endogeneity tests mean the null of UC rate being exogenous is rejected. This means there is an endogeneity issue.

For the Hausman tests, a P-value <10% indicates a FE-estimator is the preferred option.

Table 7: Annual-based, Fixed-Effect Estimation of the Benchmark Models, continued

	<i>P. disorder & weapon possessions</i>			<i>Robbery</i>		
	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>	<i>Spec 1</i>	<i>Spec 2</i>	<i>Spec 3</i>
Logarithm of UC rate	-0.0373*** (0.0131)	-0.0353*** (0.0124)	-0.0353*** (0.0124)	0.0053 (0.0079)	0.0065 (0.0079)	0.0065 (0.0079)
<i>Introductory</i> effect	0.1096 (0.0778)	0.0619 (0.0665)	0.0622 (0.0663)	0.0688** (0.0282)	0.0747*** (0.0284)	0.0734*** (0.0282)
Post-UC time trend	0.0845*** (0.0241)	0.0694*** (0.0253)	-0.1562*** (0.0531)	0.0264 (0.0224)	0.0282 (0.0235)	-0.0993* (0.0589)
Logarithm, real gross weekly wage		0.1450 (0.3959)	0.1471 (0.3965)		-0.0361 (0.2915)	-0.0443 (0.2910)
Population density		-0.0799*** (0.0208)	-0.0799*** (0.0209)		0.0006 (0.0114)	0.0008 (0.0114)
Unemployment rate		-2.7263 (2.7407)	-2.7218 (2.7391)		1.9050 (1.5736)	1.8872 (1.5760)
Police per capita		1.4739*** (0.4133)	1.4669*** (0.4229)		-0.0081 (0.2712)	0.0211 (0.2652)
CPI growth rate (national-level)			-5.4159*** (0.7816)			-3.0524*** (0.9068)
Conviction rate (county-level)			-0.1432 (0.6905)			0.5998 (0.5444)
Year FE	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES
Counties/Observations	173/923	171/907	171/907	172/918	171/906	171/906
Observation per group (min/avg/max)	4/5.3/7	2/5.3/7	2/5.3/7	4/5.3/7	2/5.3/7	2/5.3/7
Overall R2	0.2526	0.0103	1.0107	0.0319	0.0680	0.0683
Endogeneity test (P-value)	0.0003	0.0006	0.0006	0.0068	0.0091	0.0133
Hausman test (P-value)	0.0000	0.0000	0.0000	0.1178	0.0000	0.0000

Notes: *** Statistical significant at 1% level; ** significant at 5% level; * significant at 10% level.

Parentheses denote heteroskedasticity-consistent, robust standard errors.

Model Specification 3 uses an *Introductory* effect dummy with a different definition. Specifically, it equals 1 not just for the year when first UC entry appears, but also the previous year to capture pre-expectations up to 12 months ahead.

	Theft			Vehicle crime			Violent crime		
	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3
	0.0092** (0.0038)	0.0070** (0.0034)	0.0070** (0.0034)	0.0256*** (0.0065)	0.0237*** (0.0059)	0.0237*** (0.0059)	-0.0120* (0.0071)	-0.0076 (0.0070)	-0.0077 (0.0069)
	-0.0281 (0.0279)	-0.0489*** (0.0159)	-0.0480*** (0.0157)	0.0153 (0.0369)	-0.0135 (0.0261)	-0.0140 (0.0261)	-0.0237 (0.0309)	-0.0280 (0.0333)	-0.0260 (0.0331)
	-0.0985*** (0.0152)	-0.0937*** (0.0156)	-0.1828*** (0.0364)	-0.0791*** (0.0191)	-0.0812*** (0.0189)	-0.1625*** (0.0442)	0.0478*** (0.0181)	0.0412** (0.0189)	-0.1403*** (0.0391)
		-0.1523 (0.1759)	-0.1461 (0.1748)		-0.1644 (0.2352)	-0.1675 (0.2355)		-0.0420 (0.2362)	-0.0286 (0.2352)
		0.0101 (0.0078)	0.0100 (0.0077)		-0.0061 (0.0052)	-0.0060 (0.0052)		-0.0659*** (0.0119)	-0.0662*** (0.0119)
		-0.2912 (0.8365)	-0.2774 (0.8306)		1.1425 (1.4540)	1.1355 (1.4560)		-2.8381* (1.5286)	-2.8087* (1.5159)
		0.0589 (0.1315)	0.0377 (0.1293)		-0.9771*** (0.1922)	-0.9664*** (0.1944)		0.3178 (0.2082)	0.2726 (0.2093)
			-2.1449*** (0.5155)			-1.9498*** (0.6575)			-4.3670*** (0.5194)
			-0.4347** (0.2111)			0.2202 (0.3645)			-0.9262** (0.4043)
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	173/923	171/907	171/907	173/923	171/907	171/907	173/923	171/907	171/907
	4/5.3/7	2/5.3/7	2/5.3/7	4/5.3/7	2/5.3/7	2/5.3/7	4/5.3/7	2/5.3/7	2/5.3/7
	0.005	0.3875	0.3724	0.0033	0.2508	0.2533	0.3238	0.0226	0.0208
	0.1391	0.4444	0.5739	0.0006	0.0059	0.0069	0.1264	0.1286	0.2141
	0.9033	0.0000	0.0000	0.4277	0.0000	0.0000	0.0044	0.0000	0.0000

P-values in bold for the endogeneity tests mean the null of UC rate being exogenous is rejected. This means there is an endogeneity issue.

For the Hausman tests, a P-value <10% indicates a FE-estimator is the preferred option.

Table 8: Annual-based, IV estimation with fixed-effects, involving different instrumenting strategies

	<i>Anti-social behaviour</i>			<i>Burglary</i>		
	<i>IS 1</i>	<i>IS 2</i>	<i>IS 3</i>	<i>IS 1</i>	<i>IS 2</i>	<i>IS 3</i>
Logarithm of UC rate instrumented for all 3	0.1343 (0.1251)	0.0091 (0.1046)	0.0954 (0.0953)	0.1227 (0.1023)	0.0830 (0.0858)	0.1250 (0.0852)
<i>Introductory</i> effect instrumented for IS2 and IS3	0.2042 (0.2775)	0.2108 (0.2535)	0.3217 (0.2554)	0.2349 (0.2301)	0.2370 (0.2076)	0.2911 (0.2315)
Post-UC time trend instrumented for IS3 only	-0.2138 (0.1855)	-0.0276 (0.1621)	-0.1537 (0.1495)	-0.3731** (0.1604)	-0.3141** (0.1353)	-0.3756*** (0.1357)
Logarithm, real gross weekly wage	0.2679 (0.2953)	0.0162 (0.3119)	0.1024 (0.3128)	0.1423 (0.2716)	0.0625 (0.2700)	0.1045 (0.3026)
Population density	0.0245 (0.0235)	0.0549** (0.0228)	0.0379* (0.0208)	-0.0026 (0.0194)	0.0071 (0.0181)	-0.0012 (0.0191)
Unemployment rate	2.1100 (2.0031)	3.1114* (1.8781)	2.4673 (1.8986)	-0.1354 (1.7436)	0.1824 (1.5684)	-0.1319 (1.7233)
Police per capita	0.2335 (0.2953)	0.3697 (0.2577)	0.3193 (0.2723)	-0.8076*** (0.2092)	-0.7644*** (0.1983)	-0.7890*** (0.2226)
CPI growth rate (national-level)	0.8249 (1.6802)	2.3932 (1.5368)	1.3408 (1.4373)	-3.3880** (1.5145)	-2.8903** (1.2970)	-3.4038*** (1.3232)
Conviction rate (county-level)	0.2385 (0.6222)	0.0355 (0.5305)	0.1130 (0.5696)	0.0996 (0.5225)	0.0352 (0.4157)	0.0730 (0.5168)
First stage (Log UC rate)						
Log (real benefit expenditure per capita)	-0.5817* (0.3217)	-0.0411 (0.3622)	-0.0163 (0.3633)	-0.5817* (0.3217)	-0.0411 (0.3622)	-0.0163 (0.3633)
RHC - Growth		-0.3239** (0.1550)	-0.4119** (0.1835)		-0.3239** (0.1550)	-0.4119** (0.1835)
RHC - Level			0.8386** (0.4367)			0.8386** (0.4367)
F-statistics for excluded instruments	7.27**	3.13*	4.99*	7.27**	3.13*	4.99*
Year FE	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES
Counties/Observations	171/907	171/907	171/907	171/907	171/907	171/907
Observation per group (min/avg/max)	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7
Centered R2	0.4255	0.5021	0.4775	0.1698	0.0493	0.1926
Instrument identification test, Chi-sq (P-val)	0.0762	0.0573	0.0975	0.0762	0.0573	0.0975
Hausman test (P-value)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Notes: *** Statistical significant at 1% level; ** significant at 5% level; * significant at 10% level; Parantheses denote heteroskedasticity-consistent, robust standard errors.
 Instrumenting Strategy 1 (IS 1) is the direct counterpart to Tables 4-5, where only the logarithm of real benefit expenditure per claimant is used as an instrument for the UC rate.
 Instrumenting Strategy 2 (IS 2) uses both logarithm of real benefit expenditure per claimant and the change of the relative human capital ratio as instruments for both the UC rate and the *Introductory* effect measure.
 Instrumenting Strategy 3 (IS 3) uses logarithm of real benefit expenditure per claimant, the change of the relative human capital proxy measure, and the level measure of this ratio as instruments for the UC rate, and

	CD & Arson			Drugs			Other Crime		
	IS 1	IS 2	IS 3	IS 1	IS 2	IS 3	IS 1	IS 2	IS 3
	0.3922* (0.2325)	0.2270* (0.1309)	0.3446** (0.1723)	0.1734 (0.1269)	0.0524 (0.0938)	0.1415 (0.1059)	-0.3341 (0.2295)	-0.3058 (0.1967)	-0.2370 (0.1527)
	0.8733* (0.5133)	0.8820** (0.3698)	1.0332** (0.4832)	0.3494 (0.2799)	0.3558 (0.2348)	0.4704* (0.2794)	-0.7567 (0.5053)	-0.7582 (0.4806)	-0.6697* (0.3741)
	-0.6843* (0.3508)	-0.4385* (0.2161)	-0.6104** (0.2617)	-0.0348 (0.1930)	0.1452 (0.1474)	0.0149 (0.1659)	0.2909 (0.3508)	0.2489 (0.3069)	0.1483 (0.2430)
	-0.0557 (0.6326)	-0.3880 (0.4884)	-0.2704 (0.6007)	-0.0953 (0.3602)	-0.3387 (0.3144)	-0.2495 (0.3553)	-0.2420 (0.5756)	-0.1852 (0.6191)	-0.1164 (0.4988)
	-0.0888* (0.0497)	-0.0486 (0.0338)	-0.0718 (0.0441)	-0.0071 (0.0255)	0.0223 (0.0211)	0.0047 (0.0243)	-0.0209 (0.0419)	-0.0277 (0.0405)	-0.0413 (0.0348)
	-3.0593 (4.0350)	-1.7376 (2.8152)	-2.6160 (3.6801)	-0.4173 (2.3720)	0.5508 (1.9705)	-0.1149 (2.1768)	-0.6478 (3.6021)	-0.8739 (3.2504)	-1.3878 (2.7620)
	-0.7526 (0.5385)	-0.5729 (0.4057)	-0.6415 (0.5015)	0.4107 (0.2821)	0.5423** (0.2359)	0.4903* (0.2714)	0.3639 (0.4499)	0.3331 (0.4469)	0.2930 (0.3618)
	-5.8263* (3.1363)	-3.7561* (1.9626)	-5.1915** (2.3981)	-0.0483 (1.7965)	1.4681 (1.4227)	0.3802 (1.5881)	-0.2049 (3.1384)	-0.5590 (2.8202)	-1.3988 (2.4746)
	-1.0468 (1.2364)	-1.3149* (0.7422)	-1.2092 (1.0601)	-0.6005 (0.6300)	-0.7969* (0.4378)	-0.7168 (0.5603)	-0.7470 (1.2921)	-0.7012 (1.1873)	-0.6393 (0.7489)
	-0.5817* (0.3217)	-0.0411 (0.3622)	-0.0163 (0.3633)	-0.5817* (0.3217)	-0.0411 (0.3622)	-0.0163 (0.3633)	-0.5817* (0.3217)	-0.0411 (0.3622)	-0.0163 (0.3633)
		-0.3239** (0.1550)	-0.4119** (0.1835)		-0.3239** (0.1550)	-0.4119** (0.1835)		-0.3239** (0.1550)	-0.4119** (0.1835)
			0.8386** (0.4367)			0.8386** (0.4367)			0.8386** (0.4367)
	7.27**	3.13*	4.99*	7.27**	3.13*	4.99*	7.27**	3.13*	4.99*
	YES YES	YES YES	YES YES	YES YES	YES YES	YES YES	YES YES	YES YES	YES YES
	171/907 2/5.3/7	171/907 2/5.3/7	171/907 2/5.3/7	171/907 2/5.3/7	171/907 2/5.3/7	171/907 2/5.3/7	171/907 2/5.3/7	171/907 2/5.3/7	171/907 2/5.3/7
	0.3755	0.4131	0.3946	0.1238	0.2555	0.0154	0.3050	0.1556	0.1483
	0.0762	0.0573	0.0975	0.0762	0.0573	0.0975	0.0762	0.0573	0.0975
	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

the two measures associated with the UC evaluation (the *Introductory* effect measure and the rolling variable of post-UC time trend).

The instrument test is based on the Underidentification Test (LM test statistics) reported in *xtivreg2*. A P-value of zero indicates a strong instrument.

First-stage regression output is abbreviated to save space: only the estimated coefficient of instrument(s) and F-statistics are reported. Sanderson-Windmeijer multivariate F-stats are reported for IS2 and IS3.

For the Hausman tests, a P-value <10% indicates a FE-estimator is the preferred option.

Table 9: Annual-based, IV estimation with fixed-effects, involving different instrumenting strategies, continued

	<i>P. disorder & weapon possessions</i>			<i>Robbery</i>		
	<i>IS 1</i>	<i>IS 2</i>	<i>IS 3</i>	<i>IS 1</i>	<i>IS 2</i>	<i>IS 3</i>
Logarithm of UC rate instrumented for all 3	-0.5472* (0.3269)	-0.3584* (0.2195)	-0.3483* (0.1921)	0.2770 (0.2038)	0.1931 (0.1461)	0.1878 (0.1318)
<i>Introductory</i> effect instrumented for IS2 and IS3	-1.0800 (0.7241)	-1.0899** (0.5512)	-1.0769** (0.5173)	0.6769 (0.4530)	0.6803* (0.3819)	0.6736* (0.3724)
Post-UC time trend instrumented for IS3 only	0.5826 (0.4943)	0.3016 (0.3365)	0.2868 (0.2962)	-0.4908 (0.3041)	-0.3657* (0.2202)	-0.3579* (0.2023)
Logarithm, real gross weekly wage	0.0011 (0.8136)	0.3809 (0.6635)	0.3910 (0.6499)	0.0515 (0.5036)	-0.1217 (0.4587)	-0.1273 (0.4553)
Population density	0.0046 (0.0652)	-0.0412 (0.0475)	-0.0432 (0.0441)	-0.0440 (0.0396)	-0.0236 (0.0307)	-0.0226 (0.0281)
Unemployment rate	0.9083 (5.4869)	-0.6026 (3.9641)	-0.6782 (3.9687)	0.0208 (3.2305)	0.6772 (2.5265)	0.7157 (2.4794)
Police per capita	1.5833** (0.6754)	1.3779*** (0.5235)	1.3720*** (0.5203)	-0.0438 (0.3957)	0.0480 (0.3235)	0.0511 (0.3122)
CPI growth rate (national-level)	0.7122 (4.4093)	-1.654 (3.1145)	-1.778 (2.7966)	-6.3054** (2.7069)	-5.2503*** (2.0216)	-5.1855*** (1.8982)
Conviction rate (county-level)	-0.3415 (1.8398)	-0.0350 (1.2234)	-0.0259 (1.1893)	0.7081 (1.0419)	0.5716 (0.7952)	0.5667 (0.7876)
First stage (Log UC rate)						
Log (real benefit expenditure per capita)	-0.5817* (0.3217)	-0.0411 (0.3622)	-0.0163 (0.3633)	-0.5817* (0.3217)	-0.0411 (0.3622)	-0.0163 (0.3633)
RHC - Growth		-0.3239** (0.1550)	-0.4119** (0.1835)		-0.3239** (0.1550)	-0.4119** (0.1835)
RHC - Level			0.8386** (0.4367)			0.8386** (0.4367)
F-statistics for excluded instruments	7.27**	3.13*	4.99*	7.27**	3.13*	4.99*
Year FE	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES
Counties/Observations	171/907	171/907	171/907	171/906	171/906	171/906
Observation per group (min/avg/max)	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7
Centered R2	0.2608	0.0493	0.3119	0.2018	0.1821	0.2018
Instrument identification test, Chi-sq (P-val)	0.0762	0.0573	0.0975	0.0779	0.0619	0.1054
Hausman test (P-value)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Notes: *** Statistical significant at 1% level; ** significant at 5% level; * significant at 10% level; Parantheses denote heteroskedasticity-consistent, robust standard errors.
 Instrumenting Strategy 1 (IS 1) is the direct counterpart to Tables 4-5, where only the logarithm of real benefit expenditure per claimant is used as an instrument for the UC rate.
 Instrumenting Strategy 2 (IS 2) uses both logarithm of real benefit expenditure per claimant and the change of the relative human capital ratio as instruments for both the UC rate and the *Introductory* effect measure.
 Instrumenting Strategy 3 (IS 3) uses logarithm of real benefit expenditure per claimant, the change of the relative human capital proxy measure, and the level measure of this ratio as instruments for the UC rate, and

	Theft			Vehicle crime			Violent crime		
	IS 1	IS 2	IS 3	IS 1	IS 2	IS 3	IS 1	IS 2	IS 3
	-0.0254 (0.0529)	0.0044 (0.0520)	0.0443 (0.0474)	0.2517 (0.1556)	0.1379 (0.1105)	0.1607 (0.1018)	-0.1162 (0.1054)	-0.0309 (0.0904)	-0.0569 (0.0781)
	-0.1201 (0.1161)	-0.1217 (0.1157)	-0.0704 (0.1227)	0.4948 (0.3461)	0.5008** (0.2510)	0.5300** (0.2607)	-0.2679 (0.2330)	-0.2724 (0.2053)	-0.3059 (0.1922)
	-0.1362 (0.0894)	-0.1805** (0.0848)	-0.2388*** (0.0770)	-0.4916** (0.2331)	-0.3223* (0.1683)	-0.3556** (0.1551)	0.0162 (0.1674)	-0.1107 (0.1456)	-0.0726 (0.1297)
	-0.1553 (0.1697)	-0.0953 (0.1752)	-0.0555 (0.1957)	-0.1025 (0.3989)	-0.3314 (0.3227)	-0.3086 (0.3367)	-0.0596 (0.2701)	0.1119 (0.2508)	0.0859 (0.2602)
	0.0153 (0.0116)	0.0081 (0.0116)	0.0002 (0.0115)	-0.0437 (0.0316)	-0.0161 (0.0242)	-0.0205 (0.0236)	-0.0483** (0.0192)	-0.0690*** (0.0187)	-0.0638*** (0.0168)
	-0.0481 (1.0403)	-0.2867 (1.0225)	-0.5845 (1.0630)	-0.4814 (2.6028)	0.4289 (1.9508)	0.2589 (2.0538)	-2.0396 (1.8271)	-2.7218* (1.5889)	-2.5273 (1.6313)
	0.0450 (0.1054)	0.0126 (0.1060)	-0.0107 (0.1202)	-1.0183*** (0.3214)	-0.8945*** (0.2436)	-0.9078*** (0.2560)	0.2973 (0.1974)	0.2046 (0.1803)	0.2198 (0.1851)
	-1.7579** (0.8921)	-2.1315** (0.8427)	-2.6181*** (0.7846)	-4.6795** (2.0925)	-3.2537** (1.5528)	-3.5314** (1.4512)	-3.0687* (1.5895)	-4.1373*** (1.4303)	-3.8194*** (1.3140)
	-0.4472** (0.2085)	-0.3988* (0.2045)	-0.3630 (0.2563)	0.3085 (0.9047)	0.1239 (0.5631)	0.1443 (0.6214)	-0.9682* (0.5483)	-0.8298** (0.3831)	-0.8532** (0.4148)
	-0.5817* (0.3217)	-0.0411 (0.3622)	-0.0163 (0.3633)	-0.5817* (0.3217)	-0.0411 (0.3622)	-0.0163 (0.3633)	-0.5817* (0.3217)	-0.0411 (0.3622)	-0.0163 (0.3633)
		-0.3239** (0.1550)	-0.4119** (0.1835)		-0.3239** (0.1550)	-0.4119** (0.1835)		-0.3239** (0.1550)	-0.4119** (0.1835)
			0.8386** (0.4367)			0.8386** (0.4367)			0.8386** (0.4367)
	7.27**	3.13*	4.99*	7.27**	3.13*	4.99*	7.27**	3.13*	4.99*
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	YES	YES	YES	YES	YES	YES	YES	YES	YES
	171/907	171/907	171/907	171/907	171/907	171/907	171/907	171/907	171/907
	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7	2/5.3/7
	0.1544	0.2086	0.0847	0.2377	0.2019	0.3526	0.6794	0.7575	0.7456
	0.0762	0.0573	0.0975	0.0762	0.0573	0.0975	0.0762	0.0573	0.0975
	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

the two measures associated with the UC evaluation (the *Introductory* effect measure and the rolling variable of post-UC time trend).

The instrument test is based on the Underidentification test (LM test statistics) reported in *xivreg2*. A P-value of zero indicates a strong instrument.

First-stage regression output is abbreviated to save space: only estimated coefficient of instrument(s) and F-statistics are reported. Sanderson-Windmeijer multivariate F-stats are reported for IS2 and IS3.

For the Hausman tests, a P-value <10% indicates a FE-estimator is the preferred option.

United States pre-1990s, where the increase in welfare payments, if translated to a direct increase in cash and valuables being carried around by individuals, would lead to more opportunities for robbery or vehicle break-ins. This is nonetheless not covered by our theory and is therefore speculative. However, overall our findings do suggest that, the strain theory argument made by Tiratelli *et al* (2020), in the tradition of Agnew (1992), overstates the impact of the UC welfare payment system in elevating crime rates in the UK.

5. CONCLUDING REMARKS

The main purposes of this paper are to examine the differential effects associated with different crime types in influencing the welfare spending-crime nexus. To examine whether crime heterogeneity matters in explaining the welfare spending-crime nexus, we develop a simple yet novel theoretical model of crime heterogeneity and cash transfer to explain the mechanisms linking welfare payment to crime. This shows that the effect can differ between different types of crime, given criminal human capital-induced asymmetry. Using the rollout of Universal Credit in England and Wales, we apply fixed-effect estimations at the county level using both monthly and annual data, to examine three empirically testable propositions. After implementing various specifications to improve robustness of the results documented, we find significant heterogeneity in crime in affecting the welfare spending-crime nexus, i.e. there exist differential effects across crime types in determining the impact of welfare spending on crime.

Specifically, the property crime types of criminal damage and arson, and to a weaker extent vehicle crimes and robbery, are found to exhibit both positive *level* and *introductory* effects empirically, i.e. there is a positive crime-welfare spending nexus, implying that the specific crime type is human capital dependent. This is therefore unlikely to benefit from targeted use of welfare spending. In contrast, the crime types of public disorder and weapons possession exhibit both negative *level* and *introductory* effects, i.e. a negative crime-welfare spending nexus. This implies that the specific crime type is of basic nature, which therefore can be addressed by using welfare spending as a secondary policy measure in reducing crime.

For future research, it is important to take note of some of the limitations of our study. First, the conventional measure of the UC rate is, by definition, more of a headcount measure than a monetary measure, though this is partly addressed by using non-UC real benefit expenditure at the county level as an alternative measure. Second, although we jointly evaluate both the *level* and *introductory* effects of the welfare system in question, the caveat is that this is not meant to be an evaluation of policy effectiveness, as we did not employ a regression discontinuity framework. Third, although the reverse geocoding technique allows us to build our data in a bottom-up approach, our study does not account for any migration pattern associated with criminal activities.

For example, a crime committed in Nottinghamshire can be attributed to a person who resides in another county, and this cannot be accounted for

in our study. Indeed, in the literature on the ‘flight from blight’ hypothesis (e.g., Bayoh *et al* 2006, Ellen and O’Regan 2010), not only can such crime spillover effects across regions be significant, these can materially affect the suburbanisation process of a region, therefore further exacerbating local economic conditions and consequently, altering the elasticity of the crime-UC nexus again. To model this properly would require the introduction of much more sophisticated dynamic game models, which is beyond the scope of this paper. Lastly, we acknowledge that for many crime types, we do not find conclusive empirical evidence that allows for their behavioural patterns to be explained by our criminal human capital-based theory. A future study that assesses and compares the strength of the different theories in explaining the different types of crime will be warranted.

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Appendix A refers to technical derivations. This can be made available upon request by the authors.

Appendix B

Table B1: Variables, sources, and definitions

Sample Counties/Unitary Authorities (n=173) - Balanced panel, but with data gaps*

Barking & Dagenham, Barnet, Barnsley, Bath & NE Somerset, Bedford, Bexley, Birmingham, Blackburn w. Darwen, Blackpool, Blaenau Gwent, Bolton, Bracknell Forest, Bradford, Brent, Bridgend, Brighton & Hove, Bristol, B.C. & Poole, Bromley, Buckinghamshire, Bury, Caerphilly, Calderdale, Cambridgeshire, Camden, Cardiff, Carmarthenshire, Central Bedfordshire, Ceredigion, Cheshire East, Cheshire West, City of London, Conwy, Cornwall, County Durham, Coventry, Croydon, Cumbria, Darlington, Denbighshire, Derby, Derbyshire, Devon, Doncaster, Dorset, Dudley, Ealing, East Riding of Yorkshire, East Sussex, Enfield, Essex, Flintshire, Gateshead, Gloucestershire, Greenwich, Gwynedd, Hackney, Halton, Hammersmith & Fulham, Hampshire, Haringey, Harrow, Hartlepool, Havering, Herefordshire County, Hertfordshire, Hillingdon, Hounslow, Isle of Anglesey, Isle of Scilly, Isle of Wight, Islington, Kensington & Chelsea, Kent, Kingston upon Hull, Kingston upon Thames, Kirklees, Knowsley, Lambeth, Lancashire, Leeds, Leicester, Leicestershire, Lewisham, Lincolnshire, Liverpool, Luton, Manchester, Medway, Merthyr Tydfil, Merton, Middlesbrough, M. Keynes, Monmouthshire, N.P. Talbot, Newcastle upon Tyne, Newham, Newport, Norfolk, NE Lincolnshire, North Lincolnshire, North Somerset, North Tyneside, North Yorkshire, Northamptonshire, Northumberland, Nottingham, Nottinghamshire, Oldham, Oxfordshire, Pembrokeshire, Peterborough, Plymouth, Portsmouth, Powys, Reading, Redbridge, Redcar & Cleveland, Rhondda Cynon Taf, Richmond upon Thames, Rochdale, Rotherham, Rutland, Salford, Sandwell, Sefton, Sheffield, Shropshire, Slough, Solihull, Somerset, South Gloucestershire, South Tyneside, Southampton, Southend-on-Sea, Southwark, St. Helens, Staffordshire, Stockport, Stockton-on-Tees, Stoke-on-Trent, Suffolk, Sunderland, Surrey, Sutton, Swansea, Swindon, Tameside, Telford and Wrekin, Thurrock, Torbay, Torfaen, Tower Hamlets, Trafford, Vale of Glamorgan, Wakefield, Walsall, Waltham Forest, Wandsworth, Warrington, Warwickshire, West Berkshire, West Sussex, Westminster, Wigan, Wiltshire, Windsor and Maidenhead, Wirral, Wokingham, Wolverhampton, Worcestershire, Wrexham, York.

Table B1: Variables, sources, and definitions

<i>Variables</i>	<i>Sources</i>	<i>Remarks</i>
Crime rates:		Total recorded crimes for the specific category per 1,000 inhabitants
Anti-social behaviour	data.police.uk	Includes personal, environmental and nuisance anti-social behaviour.

Burglary	data.police.uk	Includes offences where a person enters a house or other building with the intention of stealing.
Criminal damage & arson	data.police.uk	Includes damage to buildings and vehicles and deliberate damage by fire
Drugs	data.police.uk	Includes offences related to possession, supply and production.
Other crime	data.police.uk	Includes forgery, perjury, & other white-collar crimes
Public disorder and weapons possession	data.police.uk	Includes all three categories of possession of weapons, public order, and public disorder
Robbery	data.police.uk	Includes offences where a person uses force or threat of force to steal.
Theft	data.police.uk	Includes all three categories of theft from the person, bicycle theft, shoplifting and other theft.
Vehicle crime	data.police.uk	Includes theft from or of a vehicle or interference with a vehicle
Violent crime	data.police.uk	Includes offences against the person such as common assaults, Grievous Bodily Harm and sexual offences.

Welfare spending/Cash transfer measures:

UC rate	Department for Work and Pensions
UC dummy 1	
UC dummy 2	
Real benefit expenditure	Department for Works and Pensions, Office for National Statistics
Real benefit expenditure per head	
Other variables	
Proportion of residents who are welfare claimants	Department for Work and Pensions
Proportion with NVQ4 qualification & above	Office for National Statistics Annual Population Survey
Propotion with no qualification	Office for National Statistics Annual Population Survey
Real gross weekly pay	Office for National Statistics
Population density	Office for National Statistics
Unemployment rate	Office for National Statistics
Number of policing staff	Home Office
CPI growth rate	Office for National Statistics
County-level conviction rate	Ministry of Justice

* For some of the counties, data for the second half of 2019 were not made available at the time of the analysis.

Table B2: Data treatment and frequency conversion

<i>Variables</i>	<i>Original frequency at source</i>	<i>Remarks</i>
Crime incidence data:		
Anti-social behaviour	Individual recorded cases	Cases are aggregated based on their recorded penal code categories to both monthly and annual frequency. Although anonymised, there are geographical tags (latitude/longitude information) recorded to cases and these are assigned to the corresponding Counties/Unitary Authorities using reverse geocoding.
Burglary	Individual recorded cases	
Criminal damage & arson	Individual recorded cases	
Drugs	Individual recorded cases	
Other crime	Individual recorded cases	
Public disorder and weapons possession	Individual recorded cases	
Robbery	Individual recorded cases	
Theft (includes theft from the person, bicycle theft, & other theft)	Individual recorded cases	
Vehicle crime	Individual recorded cases	
Violent crime	Individual recorded cases	
Other data:		
Universal credit enrolment data	Monthly data	Insert explanations
Benefit expenditure	Annual data	Original source data is at county level and in annual frequency. For the conversion to monthly frequency, we apply equal weighting by dividing equally by 12.
Population	Annual data	Original source data is at county level. As it is a stock measure and is mainly used as a denominator in calculating rate measures, the same population is used for all the months within a year.
Gross weekly pay	Annual data	The same measures are used as controls in both monthly- and annual-based estimations.
Unemployment		
Police headcount		
Education attainment data		
CPI		
Conviction data		
Education attainment data		

ENDNOTES

1. Reagan Pickering: Nottingham Business School, Nottingham Trent University, Email: reagan-p@outlook.com; King Yoong Lim: Corresponding author, International Business School Suzhou, Xi'an Jiaotong-Liverpool University, China, Email: KingYoong.Lim@xjtlu.edu.cn. We thank Alan Collins, Diego Morris, Yang Chen, the editor, and two anonymous reviewers, and our departmental colleagues for their invaluable feedback and suggestions. The views expressed are our own.

2. The concept of criminal human capital refers to the broad range of skills and networks that are required for one to conduct his/her illicit activities successfully. Without turning

this into a Hollywood style exposition on heist films (e.g., the *Ocean's* and *Fast & Furious* film franchises), very specific skill sets that require prior human capital investments are involved in performing some types of criminal activities. These are usually distinct from conventional human capital.

3. We could easily have introduced a capital accumulation function to legal human capital as well, in a differentiated human capital (one for legal, another for crime-specific) framework similar to Mocan *et al* (2005). This is peripheral and non-essential to the understanding of the differences in crime-welfare spending nexus, and hence abbreviated in our model.

4. In a real-life context, this can be interpreted in a number of alternative ways, yet remain analytically equivalent. We can interpret the zero income received as the individual having all of the income confiscated by the authorities, or simply being punished and put in jail for the specific period, hence resulting in zero income-earning opportunities. Nevertheless, we assume that no stigma is attached to a jailed individual in subsequent periods.

5. Of note, the UC rate is calculated by dividing total universal credit claimants by total population (per 1000 inhabitants), as in Tiratelli *et al* (2020). As they argue, this is pretty much the only available measure of the UC payment, given the general non-availability of monetary data for UC at county level, but can be justified given the 'Benefit Cap' limiting the maximum amount one could claim. This effectively compresses the distribution of the payment received per person. Nevertheless, as shown in our empirical estimation, we partly mitigate this limitation by using an instrumental variable (IV) approach.

6. The inclusion of a time trend post-UC in accompanying the *introductory* effect is a popular approach in the regression discontinuity design (RDD) literature. Although we are not interested in measuring any threshold effect associated with the role of such a rolling variable (as in most RDD studies), the inclusion of this time trend is consistent with the graphical evidence observed of a positive trend for both aggregate crime and the UC rate in recent years.

7. As discussed in studies such as Kwan *et al* (2004), Tompson *et al* (2015) and Quinn *et al* (2019), given data protection and statistical disclosure considerations, anonymised case-level data are subject to procedures such as obfuscation and geomasking. Nevertheless, this will only have a material effect on the accuracy of street-level analysis, which is not the focus of our analysis.

8. Strictly speaking, one could argue that the proportion of residents without any educational attainment does not necessarily reflect crime-specific human capital. However, given the well-documented inverse relationship between schooling and crime (Lochner and Moretti 2004; Machin *et al* 2011), a more intuitive interpretation of RHC_{jt} is that of an inverse of the formal human capital ratio. For a given level of formal human capital, an increase in RHC_{jt} would likely imply the 'human capital' at the other end of the spectrum (crime-specific) has increased.

9. It is worth pointing out that the overall R^2 in all our regression results is very small. While this suggests that there are a lot more unexplained variations in the different crime rates that our sets of explanatory variables cannot account for, the prediction of crime rates is not our main research question here.

10. Of course, one can still argue that the instrument is not fully exogenous, based on the intuition that non-UC expenditure can be a proxy for poverty, and that counties with

higher crime rates can attract more lower income citizens. Note that this is precisely the reason we use a 'financial' variable instead of a headcount-based rate measure, and indeed from the first-stage output it can clearly be seen that the instrument has a consistently negative relationship with the headcount measure of the UC rate, despite it being a statistically significant instrument. Further, we also choose to let the test statistics 'speak' by implementing a panel-data exogeneity test (see Wooldridge 2010, Section 11.2) and establish that, for the bulk of the crime types examined, the exogeneity of the instrument used is satisfied.

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