Crime Scars:
Recessions and the Making of Career Criminals

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Abstract

Recessions lead to short-term job loss, lower levels of happiness and decreasing income levels. There is growing evidence that workers who first join the labour market during economic downturns suffer from poor job matches that have a sustained detrimental effect on their wages and career progression. This paper uses a range of US and UK data to document a more disturbing long-run effect of recessions: young people who leave school in the midst of recessions are significantly more likely to lead a life of crime than those entering a buoyant labour market. Thus crime scars from higher entry level unemployment rates are both long lasting and substantial.

Keywords: Crime; Recessions; Unemployment.

JEL Classifications: J64; K42.

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1. Introduction

Do the labour market conditions the young encounter when they first leave school play a role in initiating and forming criminal careers? Think of two otherwise identical school leavers who left high school in 2010 – one in North Dakota and the other in Michigan. Both have completed education and try to get a job. But the North Dakota school leaver faced a state unemployment rate of only 3.8 percent, while it was 12.7 percent in Michigan. At the margin, the Michigan youngster is more likely to proceed down the wrong path – no luck getting a job, no welfare to fall back on, hanging out with similarly unfortunate juveniles, trouble with the police, some petty larceny and so on – than the North Dakota youngster. Indeed this is just the standard Becker (1968) model in action. As youths leave school, they face a trade-off between legal and illegal activities. At higher unemployment rates, the expected returns to legal activity (i.e. work) are lower. All else equal, this encourages some youths to commit crime that would otherwise have successfully avoided such a result in a more buoyant labour market.

But what might happen as these same youngsters age? Two obvious mechanisms now link their experience straight out of high school with later ones. First, earlier experiences of crime can increase their stock of criminal knowledge and potentially reduce the costs of participating in subsequent criminal activity. Second, a previous criminal record (and less on-the-job human capital accumulation) may reduce the expected wage in the legal labour market. Both effects can be expected to increase the likelihood that the individual eventually becomes a career criminal.

There is a substantial body of criminological evidence that points to the importance of the experience of youths for understanding crime patterns. Almost two hundred years ago, Adolphe Quetelet showed that crime in early nineteenth-century France peaked when individuals were in their late teens (Quetelet, 1831). Subsequent research has confirmed the strong age-crime pattern, with crime peaking in the late teens and declining quite rapidly.[1] Unsurprisingly in our data, the same patterns emerge. Figure 1 plots the average male offender rate by age for the US and UK from 2000-2010.[2] The peak occurs at age 17 or 18 and declines reasonably smoothly from then on. Note however that the offender rate at age 29 is still a lot higher than at age 39 – showing that criminality is not just a feature of teenage years.

Existing evidence also points to strong links between criminality in teenage years and subsequent criminal behaviour.[3] In our data for example, 72 percent of males aged over 25 in the UK who were convicted of a crime in 2002 had a criminal record that went back to their teenage years. Thus factors that increase criminal behaviour for juveniles have scope to raise the lifetime criminal participation rate. The focus of this paper is whether the state of the labour market at entry is such a factor.

In pursuing this research question, our analysis contributes to two distinct strands of literature. First, there has been an extensive, though partly unresolved, debate over the link between recessions and crime. This literature has primarily focused on the issue of whether crime rates, and in particular property crime rates, are countercyclical. The evidence tends to suggest that the place where one can identify effects from unemployment to crime is for young adults.[4] Thus, Gould, Weinberg and Mustard (2002) examine the impact of contemporaneous unemployment and wages on the criminal behavior of less educated young males. Exploiting a

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[1] See Hirschi and Gottfredson (1983) for the development of the formulation that crime-age profiles are invariant over time and space, and the subsequent body of research trying to refute this claim that followed (for example, Greenberg, 1985, Hansen, 2003, and the meta-study of Pratt and Cullen, 2000).

[2] Full details on the data used in the chart are provided in Section 3 and the Data Appendix. The chart shows the average offender rate (arrested in US and convicted in UK), defined as the number of offenders in each age group divided by population in each age group. The data is averaged over the period 2000-2010.

[3] See the many papers cited in the review of Nagin and Paternoster (2000) which frames the positive link at the individual level between past and future criminality in terms of individual heterogeneity and state dependence.

[4] Indeed, Freeman's (1999) survey notes the relationship across the whole population to be 'fragile, at best'. More recent reviews confirm this and therefore more focus can be placed on youth crime and unemployment to see labour market effects on crime (for example, see Mustard, 2010, and Buonnano et al., 2011).

panel of US counties, they find significant effects for both wages and unemployment on property and violent crime. Fougère et al. (2009) find strong effects from youth unemployment (but not overall unemployment) on crime in France, while Grönqvist (2013) uses Swedish register data to show a strong and precisely estimated link between youth unemployment and crime, both for property and violent crimes.

Second, there is a growing literature on the effects of first entering the labour market during recessions on outcomes later in life. That literature so far has focused on whether such workers experience sustained long-run negative consequences. Early contributions by Ellwood (1982) and Gardecki and Neumark (1998) found somewhat contrasting evidence on whether initial labour market experience affected subsequent outcomes, with Ellwood finding significant effects on wages but not on future spells of unemployment, while Gardecki and Neumark found little evidence of a sustained negative effect. More recently, Hershbein (2012) finds that a recession reduces starting wages of high-school graduates by about 6 percent, but that this penalty fades away within six years. Oreopoulos, von Wachter and Heisz (2012) exploit a large Canadian longitudinal dataset to show that the cost of a recession for new graduates is substantial and long lasting. A typical recession – a 5 percentage point rise in the unemployment rate – is associated with an initial loss of earnings of about 9 percent that halves within 5 years, and finally fades to zero by 10 years. The economic mechanism operates via initial placements with lower paying employers and succeeding recoveries through gradual job mobility to better firms. Graduates in the lower quintile of the ability distribution suffer permanently lower wages, while the more able graduates quickly bounce back. Similar results are reported by Kahn (2010) who uses longitudinal data on US college graduates, though some of her results suggest that the wage penalty is longer lasting. By contrast, Benedetto, Gathright and Stinson (2010) find no evidence of a persistent impact of graduation-year unemployment on earnings using US social security earnings data.[5]

Taking a somewhat different approach, Oyer (2006, 2008) has examined the career paths of particular occupations, namely economists and investment bankers, to assess the importance of initial conditions. He shows that stock market conditions at the time of graduation have a strong effect on whether MBA students go directly to Wall Street, or instead pursue alternatives such as jobs in consulting firms. Further, he shows that starting a career in investment banking directly after graduation causes a person to be more likely to stay in the job and earn significantly more. These effects are substantial in size, amounting to several million dollars in present value.

Outside of the labour market literature, labour market entry conditions have been shown to impact other outcomes. MacLean (2013), for example, finds that males who graduate from high school during a recession show worse health outcomes at age forty than those graduating in a more auspicious labour market. This is true for both self-reported health measures and objective measures of physical and mental health. Giuliano and Spilimbergo (2014) show that those who enter the labour market in recession are more likely to believe that success in life depends more on luck than effort and support more government redistribution. Again, these effects are seen to be long lasting. The protective effect of education for cohorts who graduate in recessions is studied by Cutler et al. (2014) in their analysis of Eurobarometer data. They report evidence of lower wages and life satisfaction together with higher obesity and a greater propensity to smoke and drink later in life for individuals who graduate in recession years, with higher education levels significantly moderating these negative outcomes.

[5] See also the international comparison of unemployment entry effects on labour market outcomes in the US and Japan by Genda et al. (2010).

The results we report uncover a more disturbing long-run effect of recessions. In the US and UK, based on a variety of individual and cohort level data sources, we report evidence of a systematic empirical link between crime and entry-level unemployment that very clearly show that young people who leave school in the midst of recessions are significantly more likely to lead a life of crime than those entering a buoyant labour market. Moreover, these effects are seen to be long lasting and substantial. Thus, as other economic and social outcomes are significantly affected by the state of the business cycle at the time when individuals potentially enter the labour market, so is criminal activity. We conclude that recessions do play a role in the making of career criminals as crime scars from higher entry level unemployment rates are both long lasting and substantial.

The rest of the paper is structured as follows. In the next section we discuss possible links between initial conditions at labour market entry and the future path of criminal behavior as well as the underlying dynamics to motivate our empirical research. In Section 3 we discuss the empirical strategy and data for the US and UK. We present the cohort panel results and individual-level evidence in Sections 4 and 5, respectively. Section 6 concludes by summarising the key findings of the paper.

2. Theoretical Background

In the standard Becker (1968) economics of crime model, individuals act as rational decision makers and choose between legal and illegal activity. Their choice is based on the expected returns to both options. In this simple yet powerful framework, returns to legal activity are solely determined by the market earnings from employment whereas returns to illegal activity take into account the potential crime payoff, the probability of getting caught and the expected sanction if caught. If the expected return to illegal activity outweighs the expected return to legal activity, the individual chooses to commit crime.

In the Becker model, higher unemployment reduces the returns to legal activity. Thus, individuals facing unemployment or higher risk of unemployment may become more likely to commit crime than they would have been otherwise. That effect is expected to be higher for young people who typically are less attached to the legal labour market than older individuals further on in their careers.

The model has proved valuable in highlighting the economic incentives associated with criminal activity and its basic predictions on incentive and deterrence effects on crime has received substantial empirical support (see the reviews of Freeman, 1999, and Chalfin and McCrary, 2014, together with the introduction of Cook at al., 2013). Its weakness and limitation for our purposes is that it is explicitly static. Individuals make a one-off decision to commit crime or work in the legal sector. There is no process through which decisions made in the current period have implications both for future decisions and for the choices available to the individual in later periods.

Mocan et al. (2005) develop a dynamic model that links recessions, human capital and crime.[6] Individuals are lifetime utility maximizers where the source of utility from consumption and income comes from both the legal and the criminal sector. Individuals have endowments of legal and criminal human capital, which depreciate over time. Both types of human capital rise with experience in the sector and are increased by investment in the respective sectors. The individual's income is a function of human capital and rates of return in the both sectors. In each period, the individual solves a dynamic stochastic optimization problem. First, they decide how

[6] For other dynamic models of crime participation see Flinn (1986), Lee and McCrary (2009) and Lochner (2004).

much time to allocate to legal and criminal work and second, they decide on the optimal level of consumption.

Crime is risky in the sense that a criminal faces a certain probability of being caught and sent to prison. The probability of prison depends on the skill of the criminal as measured by criminal human capital and the amount of time spent in the criminal sector as measured by experience in the sector. While legal human capital may decline in prison in addition to depreciation effects, for example due to reputation effects, criminal human capital may increase if criminals in prison learn from each other.

In this model, recessions impact on crime through the respective dynamic evolutions of both legal and criminal human capital. In that sense, the long-term impact of recessions on crime differs with the length and the depth of a recession. In a recession, the returns to legal human capital fall. Following the arguments from the standard Becker (1968) model, involvement in criminal activity rises depending on the relative and absolute returns to crime. If involvement in criminal activity increases, the criminal human capital stock is expected to grow while the legal human capital stock depreciates. Once the recession ends, returns to legal human capital increase again, and the relative returns to criminal activity decrease. In a *short* recession, the stock of legal human capital typically remains significantly higher than the stock of criminal human capital, and the individual exits the criminal sector. Basically, in such a short recession, the individual is encouraged to get involved in criminal activity, but is not exposed to these conditions for a long enough period to develop sufficient criminal capital in order to yield higher returns in the crime market than in the legal market once the recession ends.

If an individual is exposed to an unexpectedly *long* recession, the decision between legal and illegal activity changes in the same way as in a short recession. However, the individual's criminal human capital stock grows over a longer time period whereas the legal human capital stock is expected to decline even more than in a shorter recession. These two effects may result in higher returns to criminal activity than to legal activity even after the recession ends. We expect more permanent effects of a recession on criminal behaviour in that case. In addition, with higher involvement in criminal activity, the chances of being caught and imprisoned will rise. As explained above, if imprisoned, an individual's criminal human capital stock may rise further in absolute terms, and certainly rises further relative to legal human capital. In that situation hysteresis can occur and trigger criminal careers.

The mechanisms explained above are likely to be stronger for these individuals with initially low levels of legal human capital. New entrants to the labour market have developed less legal human capital and thus are less attached to the legal labour market. In our empirical analysis, we thus look at cohorts entering the labour market in different economic conditions and estimate the effect of entering the labour market in a recession on subsequent crime outcomes.

In the criminology literature there has been extensive focus on the concept of a criminal career and how it develops with age (see Piquero et al., 2003). A criminal career is often characterised by various stages: onset, persistence, escalation/specialization and desistance.[7] Sampson and Laub (1993, 2005) characterize crime as a product of persistent individual differences and local life events. They find that incarceration in later life is strongly related to the difficulty in securing stable work as individuals entered young adulthood.

Our research question of whether labour market entry conditions matter for crime fits naturally into this framework. Unemployment at labour market entry (a local life event) can

[7] Criminological research that place a focus on particular stages of these crime dynamics includes Eggleston and Laub (2002), Elliott (1994) and McGee and Farrington (2010).

contribute to the onset of criminal behaviour and/or can encourage the persistence of those youths that have already begun a criminal career. The long-run effect of unemployment at labour market entry then depends on the persistence and desistance effects. There has been less research on the duration of criminal careers. One study (Piquero et al., 2003) finds that, for offenders with two or more offences, the average duration of criminal careers was 10.4 years.

In the discussion thus far we have implicitly assumed that unemployment at labour market entry causes the criminal career to begin at that point (or to intensify for those youths already active in crime). A complementary alternative would be that entry unemployment could have delayed effects on criminal behaviour. Zara and Farrington (2010) study a group of late-onset offenders (those who commit their first crime aged 21 or over). They find a significant effect of high unemployment at age 16-18 as a predictor of subsequent offending (relative to a non-offending control group). To address this in our empirical analysis, we consider an approach that is flexible enough to permit differential timing of the effects of labour market entry unemployment effects on crime.

3. Empirical Strategy and Data

Modelling Approach

Our empirical analysis exploits both individual micro-level data and panel data on year-of-birth cohorts over space and time. The data are discussed in more detail below and in the Data Appendix. For the microdata we observe cross-sections of individuals and can identify those who are incarcerated (in the US data) and those who report having ever been arrested (in the UK data). For each individual we can match in the unemployment rate at their time of labour market entry in the area they live and estimate probability models to explore whether this has an effect on criminal outcomes in later life.

For the panel data, we observe age/birth cohorts as they enter the labour market and follow them through their (potentially) working lives up to age 39. Our unit of analysis is defined at the year-of-birth cohort (c), region (r), and calendar year (t) level where region refers to states in the US and to standard regions in the UK. We can estimate the long-run effect of initial labour market conditions by exploiting the regional variation in entry unemployment rates across cohorts using the following equation:

$$\ln(\text{crime})_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \beta \text{Urate}_{cr,0} + \gamma X_{crt} + \varepsilon_{crt} \qquad (1)$$

In (1) the dependent variable is the log(crime rate) for the cohort, region and time cells and we can include fixed effects for cohort, region, time and age that are respectively denoted by α_c, α_r, α_t and α_a. X is a set of control variables (defined below) and ε is an error term. Labour market entry occurs at date 0, so $\text{Urate}_{cr,0}$ denotes the cohort-region specific unemployment rate at that date.

The first pertinent feature of equation (1) is that, in common with a number of other applications when cohort data of different ages is followed over time, it is well known that one cannot separately identify age, cohort and time effects. We follow the standard approach of including a full-set of age, cohort and time fixed effects and arbitrarily dropping one additional cohort effect. We could alternatively have required the cohort-effects to sum to zero (Deaton, 1997), and our results are robust to this alternative. Secondly, in order to adjust for cohort compositional differences, we include the X set of covariates at the level of our unit of analysis.

In particular, we adjust for the average share of immigrants, male graduates, black males, married males and females per cohort in the region over the sample period.[8]

The model in (1) is restrictive in that it assumes subsequent unemployment rates experienced by the cohort have no effect on their criminal behaviour. In effect the model allows us to estimate the average effect on crime of entering the labour market in a recession, given the usual pattern of regional unemployment that cohorts experience after entry. For the focus of this paper, we are arguably more interested in the effect of entry unemployment *net* of subsequent labour market conditions. To isolate this effect, we can include regional unemployment rates experienced by the cohort in the years after labour market entry. We measures these as $\mathrm{Urate}_{cr,i}$, where i > 0 is the number of years since entry. This gives us a second, more general, model to estimate:

$$\ln(\mathrm{crime})_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \beta \mathrm{Urate}_{cr,0} + \delta_i \mathrm{Urate}_{cr,i} + \gamma X_{crt} + \varepsilon_{crt} \quad (2)$$

where i can theoretically take any value up to the latest year observed since labour market entry (for example, when t = 0 corresponds to age 16, it could run from 1 to 23 years subsequent to entry up to our maximum age of 39). A fully saturated unemployment rate model would allow the unemployment rate the cohort experienced in every year of their labour market experience to affect their crime rate. However, we restrict the coefficients on the i-dated unemployment rates to affect the cohort crime rate only when the cohort reaches that point in the life-cycle. For example, the coefficient on regional unemployment five years after the cohort enters the labour market is restricted to be zero until the cohort actually reaches five years of experience. This

[8] The specific control variables included are to account for demographic correlates of crime and changing patterns of immigration (for examples of papers studying directly the connections between crime and immigration see Bell et al., 2013, Bianchi et al., 2012, and Mastrobuoni and Pinotti, 2015).

ensures that future unemployment rates cannot affect current crime, which is intuitively sensible.

Next, we introduce dynamics by further generalising equations (1) and (2) to permit the main coefficient of interest β on the initial unemployment rate to vary with labour market experience/years since assumed labour market entry.[9] This enables us to see to what extent the average effect of entry unemployment on a cohort occurs because of early scarring effects that erode as time since labour market entry increases or because of more persistent effects across a cohort's life-cycle:

$$\ln(\text{crime})_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \sum_{e=1}^{E} \beta_e \left[I(\text{Exp}=e)*\text{Urate}_{cr,0} \right] + \gamma X_{crt} + \varepsilon_{crt} \tag{3}$$

This specification allows β to vary with potential labour market experience (Exp, for experience groups e = 1,…,E) and measures the extent to which any effect of initial unemployment on criminal behavior persists as length of time since labour market entry increases.

Our final, and most general estimating equation, then allows for the unemployment experienced after labour market entry to also have permanent or transitory effects:

$$\begin{aligned} \ln(\text{crime})_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \sum_{e=1}^{E} \beta_e \left[I(\text{Exp}=e)*\text{Urate}_{cr,0} \right] + \\ \sum_{e=i}^{E} \delta_{ie} \left[I(\text{Exp}=e)*\text{Urate}_{cr,i} \right] + \gamma X_{crt} + \varepsilon_{crt} \end{aligned} \tag{4}$$

In (4) both the β's and the δ's are allowed to depend on the length of time that passes since the entry and subsequent unemployment rate were experienced by the cohort. Again, the effects of subsequent unemployment are restricted to be zero until the cohort reaches the relevant age.

[9] Potential experience here is years since labour market entry (i.e. age - [age at year t = 0], with t = 0 being the assumed labour market entry age as defined below) so the notation of the age/experience fixed effect in the estimating equations can be interchangeably used as either α_a or α_e.

For all the models we estimate, it is important to be clear that identification comes from *within*-cohort variations in entry unemployment rates across states/regions. We view this as the most convincing approach that can be taken to produce evidence with the available data and this therefore forms the basis of most of our results. However it could be argued that removing the aggregate national unemployment rate at entry (which follows from including cohort fixed effects) removes much of the variation over time. To address this, we also report specifications using the national unemployment rate at labour market entry and including a quadratic cohort trend to account for changing cohort quality.

Details of US Panel Data

For the US panel analysis, our measure of criminality is arrests. Use of arrests data is driven by two considerations. First, consistent annual incarceration data at the state and cohort level simply do not exist in the United States (see Pfaff, 2011). Second, it is of interest to measure criminality in a broad way and check that the results are robust. We therefore use arrest data from the FBI Uniform Crime Reports (UCR). The UCR reports the number of arrests by year, state, age, gender and type of crime. Our sample runs for all years from 1980 to 2010.

We obtain the number of arrests for property and violent crimes by respectively aggregating arrests over crime types. Our measure for property crime includes arrests for burglary, larceny, vehicle theft and arson, while our measure for violent crime includes arrests for murder, rape, robbery and assault. We produce arrest rates by dividing the number of arrests by the annual population in the observational unit, and scale by 1,000. Population data is retrieved from the US Census population estimates.

We restrict the sample to males aged between 16 and 39, as this is the group of individuals with the highest crime propensity. The original data are grouped by age. Up to the age of 24 the data are reported by single age year, while for ages 25 and above the data are grouped in five-year age brackets (25-29, 30-34, 35-39). As our empirical strategy exploits year-of-birth cohorts, we assume that year-of-birth cohorts within these older age groups of 25-29, 30-34 and 35-39 are homogeneous in terms of arrest rates. We then construct the number of arrests for single-year-of-birth cohorts within these age groups by dividing the number of arrests by five.

Since data for some states are systematically missing, we exclude these states from our analysis.[10] States with missing data for a limited number of years only are included for the non-missing years, leading to an unbalanced panel.[11] There is however no evidence to suggest that the states that do not report data differ significantly in terms of entry unemployment rates. We also exclude state-year observations that cover arrests for less than 95 percent of the state population in that year.

For both the Census and UCR data, our samples comprise year-of-birth cohorts that run from 1941 to 1994.[12] Assuming that individuals enter the labour market at age 16, labour market entry would therefore occur from 1957 to 2010. We have data on state annual insured unemployment rates from 1957 until 2010.[13]

Two issues arise with these data. First, since we link the current arrest rate for a particular cohort in a given state to the initial entry unemployment rate of that cohort in the *same* state, we assume that cohorts do not substantially move across states over time. So for example, we

[10] As described in the Data Appendix in more detail, excluded states are: Indiana, Louisiana, Mississippi, Montana, Nebraska, New Hampshire, New Mexico, New York, Ohio, South Dakota, and Washington. As an example, New York is excluded since New York City (specifically the NYPD) systematically does not report arrests, and thus arrest data at state level would be heavily undercounted.

[11] For example, Florida does report arrests until 1995, but not afterwards. Thus, we include Florida in our sample until 1995.

[12] Our first year of data on arrests/incarceration is 1980 and the oldest age we consider is 39, so this cohort was born in 1941. Similarly our data ends in 2010 and the youngest age is 16 (i.e. the 1994 birth-cohort).

[13] The downside of using that kind of data is that it does not allow us to distinguish between total and youth unemployment rates at labour market entry, nor provide measures of the duration of unemployment.

assume that the criminal behavior of the 30 year-old cohort in California in the year 2000 is affected by the unemployment rate in California in the year 1986, when that cohort entered the labour market. The empirical validity of this is subject to no inter-state mobility since school-leaving age. If there is mobility but it is random since school exit, the estimates will merely be noisy. However if mobility is driven by self-selection, the coefficient of interest may be biased. Following Dahl (2002) we present robustness tests based on mobility data from the US Census.

Second, in our empirical work for the US we use the average unemployment rate the cohort experienced at ages 16 to 18 as our measure of entry unemployment. This is motivated by the observation that the majority of arrested criminals have low educational attainment and generally do leave school at or around the compulsory school leaving age. In the US Census data that we use in our microdata analysis, 86 percent of those incarcerated over the 1980-2010 sample had high school or less (<=12 years of education) as their highest level of education. Since school-leaving ages differ slightly across time and states and unemployment within a cohort/state observation is autocorrelated, we use the 16 to 18 average unemployment rate to characterise the state of the labour market that the cohort first experiences. An alternative would be to use the age 16 (or indeed age 17 or 18) unemployment rate only. We show below that our results are robust to these alternative approaches to defining entry unemployment.

Details of US Micro Data

The micro data on US incarceration of individuals comes from US decennial Census and American Community Survey (ACS) data. We study all males aged 18-39 from the 5 percent samples of the 1980, 1990 and 2000 Census and the 2008-2012 ACS from IPUMS-USA (the Integrated Public Use Microdata Series).

We identify the institutionalized population using the Group Quarters variable. However, only in the 1980 sample is the Group Quarters variable available at a detailed enough level to uniquely identify those in correctional facilities. In subsequent Censuses (and the ACS), the institutionalized population includes the following categories: correctional facilities, nursing homes and mental hospitals, and juvenile institutions. Fortunately for our purposes, the share of the total institutionalized population accounted for by those in correctional facilities is very high in our sample (see Data Appendix for more discussion on the validity of this). The additional covariates from the Census data include race, marital status, veteran status and education.

Details of UK Panel Data

Crime data for the UK panel come from the Offenders Index Database (OID) and the Police National Computer (PNC). The measure of crime is convictions. This has the advantage of capturing actual offenders (subject of course to wrongful conviction) rather than the proportion of a particular cohort that come into contact with the police. The OID/PNC provides data on gender, date of birth, region of conviction and offence category. This data sample again runs from 1980 to 2010.

We obtain the number of convictions for property and violent crimes by aggregating convictions over crime types. As such, our measure for property crime includes burglary, theft and handling stolen goods and criminal damage, while our measure for violent crime includes violence against the person, sexual offences and robbery. We produce conviction rates by dividing the number of convictions by the annual population in the observational unit, and scale by 1,000. Population data is taken from the ONS population estimates. As with the US data, the sample covers convictions from 1980 to 2010 for 16-39 year-old males. Individual year-of-birth cohorts again therefore run from 1941 until 1994. Assuming that individuals enter the labour market at age 16, we consider unemployment rates at labour market entry from 1957 until 2010. The unemployment rate data from 1975 onwards comes from the Labour Force Survey. Prior to 1975 the unemployment rate is derived from the claimant count data.

In contrast to the US, there is a standard national school-leaving age in the UK. For those leaving school by 1972, the compulsory school leaving-age was 15, and 16 from 1973 onward. We use this compulsory age to date labour market entry for each cohort, rather than taking the 16 to 18 average. However results are reported that again show that our conclusions are robust to this alternative measure of labour market entry.

Details of UK Micro Data

Our micro-level data for the UK comes from the British Crime Survey (BCS). The BCS is a large (45,000 individuals) annual cross-section survey used to construct measures of crime victimization. Each year, a sub-sample of respondents is asked whether they have ever been arrested by the police. There is no information on the type of crime for which they were arrested, or on the eventual outcome. However as we will use conviction data in the UK panel analysis, it is useful to have an alternative measure of criminal behaviour (as in Lochner and Moretti, 2004) to evaluate robustness. We have a broad array of personal characteristics including educational attainment, ethnicity, marital status, housing status and employment and income measures.

4. Cohort Panel Evidence

(a) United States

We begin our analysis of the panel data for the US by presenting evidence on the average effect of initial labour market conditions on criminal activity. In terms of the equations above, this specification is equation (1) that restricts the coefficient β to be the same across experience groups. Table 1 shows the results. The dependent variable is the log of the crime rate, where the crime rate is defined using arrest rates as explained above. Columns (1), (3) and (5) consider the national unemployment rate at labour market entry while columns (2), (4) and (6) use the state unemployment rate at labour market entry (our preferred specification). All regressions include year, state and age fixed effects and cohort composition variables. The national results control for a quadratic cohort trend while the state results include a full set of cohort fixed effects. The regressions are weighted by cell-population and robust standard errors are clustered at the state-cohort level.

Columns (1) and (2) of Table 1 show a strong positive estimated coefficient on the entry unemployment rate, whether we use the national or state-level variation in entry unemployment. For the state-level entry unemployment rate specification in column (2), the average arrest rate for a cohort entering the labour market in a recession is estimated to be around 10.2 percent higher than for a similar cohort entering into a normal labour market (using a 5 percentage point increase in unemployment as a measure of recession relative to normal). The estimate is statistically significant at the 1 percent level.

This amounts to a substantial estimate of labour market entry effects on crime, but in some respects the average effect of recessions may not be the most relevant parameter of interest. Indeed, within a cohort, there will be a substantial share for which the marginal effect is zero, since their optimal decision will be unaffected – i.e. they are at an interior solution that results in no illegal behaviour and the recession does not move them across the threshold. Thus the average effect that we estimate is a combination of a zero effect for potentially a large share of the cohort and a substantial effect for those close to the legal/illegal threshold in the absence of a recession. Indeed, the results from the analysis of the individual-level Census data presented in Section 5 below will suggest that this is the case, as the estimated entry-level unemployment effects are seen to be much larger for the less educated. The remaining columns show results for property crime and violent crime, using both national and state unemployment variation. The results suggest very similar and statistically significant effects in all cases. In all subsequent results we report only those that use state-level unemployment rates as we view this as providing the most convincing identification.[14]

The specification used in Table 1 implies that subsequent unemployment rates do not matter, or are at least are orthogonal to the entry unemployment rate. There is no reason for this to be the case and so we follow the earnings study of Oreopoulos et al. (2012) in allowing for subsequent unemployment rates to affect our outcome of interest, crime, in addition to the entry rate. We do so by including the average unemployment rates for ages 19-21, 22-24 and 25-27. In essence this means that for a particular cohort we allow for their crime path to be explained by both the unemployment experienced when entering the labour market and the unemployment rates they experience over the next 10 years.[15]

Table 2 presents the results of this exercise. It is perhaps most useful to focus on column (3) where we allow for two changes, breaking the age 16-18 unemployment rate into its component parts and allowing for subsequent unemployment rates. On the first of these, when we allow for separate estimated effects for any individual year of unemployment, the estimates are imprecise. However the p-value from a hypothesis test of the joint significance of the three

[14] We have also broken down property and violent crime into more disaggregated measures of crime types (breaking violent crime separately into murder, rape, assault and robbery and property crime separately into burglary, larceny theft, vehicle theft and arson) and find there to be significant positive estimates of entry level unemployment rates for all crimes with the exception of murder. These results are available from the authors on request.

[15] We have also experimented with including unemployment rates prior to school-leaving age. Their additional inclusion leaves the estimated impact of entry unemployment intact, remaining positive and statistically significant.

individual year effects is significant at the 1 percent level. The reason is that there is a high degree of autocorrelation in the within-cohort unemployment rate.[16] We therefore prefer to either use the age 16 effect alone (recognizing that it is picking up effects for age 17 and 18 as well) or use the three-year average. As columns (1) and (2) show, it matters little which we choose. The second key result of column (3) is that none of the subsequent three-year average unemployment rates that the cohort experiences have an individually significant effect on arrests – though they are all negative. This helps us to better understand a puzzle in the literature that we referred to in the introduction. The overall link between crime and unemployment appears quite weak in many studies. Our results show that the key effect from unemployment on a cohort's crime trajectory is the early experience of unemployment rather than the average unemployment experienced over the life-cycle.

Tables 1 and 2 demonstrated a statistically significant and economically substantial effect of initial unemployment conditions on the arrest rates of cohorts over their entire lifetime. But we are also interested in examining the persistence of this effect: is the entry unemployment effect primarily driven by a very large impact on crime in the early years after labour market entry that subsides as the young age and go on to establish a stable legal career? Or is the effect persistent, with some of those affected by harsh labour market conditions at labour market entry pushed into a criminal career that becomes self-perpetuating for the reasons discussed in Section 2? In order to examine this, we allow the coefficient on initial unemployment to vary by years since labour market entry as outlined in equation (3) of section 3.

We group experience into four categories (0-5, 6-11, 12-17 and 18-21 years) and use an identical regression specification as in the previous table. Experience set to 0 for ages 16 to 18.

[16] Appendix Figures A1 and A2 show this strong persistence in the autocovariances of unemployment rates within a cohort/state group for the US and UK respectively.

The results are shown in Table 3 with columns (1) and (2) showing results for all crimes, and columns (3) and (4) for property and violent crime respectively. Column (1) is estimated without controlling for subsequent unemployment whereas columns (2) to (4) allow for these effects interacted with experience dummies (i.e. equation (4)). There are strong positive effects of entry unemployment on arrests in the early years in the labour market, that fall as experience increases. However, even a decade after leaving school there remain significant positive effects from entry unemployment on arrests, particularly for property crime. Juveniles who leave school in a recession have higher arrest rates during their first few years in the labour market *and* higher arrests rates over a decade later than juveniles who leave school in a buoyant economy.

An alternative specification to examine the persistence of entry unemployment is to allow for the interaction term with experience to vary with individual years of experience (rather than group experience as done in Table 3). Figure 2a plots the estimated coefficients (together with 95 percent confidence intervals) for every year of labour market experience without allowing for subsequent unemployment rates. Figure 2b shows the respective results when one allows unemployment rates later in life to enter the regression. Again, the drop in the effect after the first few years of labour market entry is clear but the individual year estimates suggest a consistent longer lasting scarring effect.

As previously discussed, one may have potential concerns about inter-state mobility in the data. More precisely, the presence of mobility raises the question as to what is the correct (best measured) entry unemployment rate for cohort c at time t in state s? Thus far we have assumed it was the unemployment rate in state s at the time that cohort c left high-school. But this ignores mobility and if potential criminals are likely to move across state boundaries, this could be of concern. Some of those in cohort c at time t in state s will have completed high-school in state k and entered the labour market there. For this part of the cohort, the correct entry unemployment rate is of course the unemployment rate in state k at the time cohort c left high-school. Dahl (2002) makes the same point with respect to estimates of state-specific earnings returns to education, which he shows differ substantially across states. His proposed solution to this mobility problem is to use reported migration flows across states to correct the estimated returns. We follow broadly the same procedure here. We use the 5 percent US Census for 1980, 1990 and 2000 and the 2010 ACS to calculate for each cohort c in state s the distribution of states-of-birth, and use the unemployment rates at age 16 only. Assuming that state-of-birth and state-at-16 are highly correlated, we generate a mobility-adjusted entry unemployment rate for cohort c in state s as:

$$\text{Urate}_{cs} = \sum_{k=1}^{K} p_{csk}\,\text{Urate}_{ck} \qquad (5)$$

where p is the proportion of cohort c in state s that were born in state k.

Table 4 reports estimates using this mobility-adjusted entry unemployment rate. The results are robust to the new specification, in that a positive and substantial entry-level unemployment rate effect on crime remains. The result reported in the upper panel of the Table for all crimes is similar, and a little bigger in magnitude (at 2.470 compared to 2.039 from Table 1), but very much corroborates the earlier results. In fact, the estimated coefficients on property and violent crime also go up a little, as shown in the Table. Moreover, if we apply the mobility adjustment to the age 16, rather than age 16-18, entry unemployment rate, the results (shown in the lower panel of the Table) remain robust. Hence, this robustness check offers a useful corroboration of our main results as, if anything, we appear to marginally underestimate the effect of initial unemployment at labour market entry on crime when we do not adjust for interregional mobility (although all the differences between mobility adjusted and non-adjusted estimates are not distinguishable from one another in terms of statistical significance).

(b) United Kingdom

We begin in Table 5 with the same specification reported in Table 1 for the US. Recall that this estimates the average effect of initial labour market conditions on criminal activity, in this case convictions rather than arrests. As with Table 1, we report estimates for total crime and for property and violent crime separately and using either the national or region-specific entry unemployment rate. The only specification difference is that we allow cohort composition effects to have different coefficients in London compared to the rest of the UK. The differences in these estimated coefficients are statistically significant, suggesting that over time cohort composition and their effects on crime have differed substantially between London and the rest of the UK.[17]

As with the US results, we find a statistically significant effect of entry unemployment on overall lifetime crime. Taking the estimated coefficient in Column (2), a recession that raises the unemployment rate by 5 percentage points would raise the lifetime conviction rate by 4 percent. We are somewhat skeptical about the magnitude of the effect when using national entry unemployment as the source of identification. The difficulty arises because we have to assume a specific functional form for the cohort effect whereas when regional entry unemployment is used we can non-parametrically control for the cohort effect since identification comes from *within*-cohort variation across regions. To see the sensitivity of the results to this, note that the coefficient on national entry unemployment in column (1) of Table 5 is 2.664 (0.189) when we

[17] An alternative would be to estimate the models using the regional dimension outside of London only. Results available on request show that this generates the same qualitative results as reported in the main text, though the precision tends to be somewhat higher. We prefer to include London and control directly for differences in the effect of cohort composition. Note that we do not allow for separate cohort effects for London, since this would remove any variation in entry unemployment for London.

allow a quadratic cohort trend. If instead we allow a quartic cohort trend this coefficient drops to 1.007 (0.189). We prefer to focus on the results that exploit within-cohort variation.

Next we consider unemployment rates other than that at age 16. These results are shown in Table 6. Columns (1) and (2) show that it matters little whether we use the age 16 unemployment rate or the 16-18 average to capture entry effects, whilst columns (3) and (4) show two key results. First, subsequent unemployment experiences seem to have little effect on overall crime rates. This suggests that if youths can get through their initial experience of the labour market without turning to crime, they will be largely unaffected by subsequent unemployment experiences. This is consistent with the theoretical model under which the rising level of legal versus criminal human capital increasingly reduces the chances of resorting to crime. Second, controlling for subsequent unemployment has no effect on the size of the entry unemployment effect.

Next we examine the persistence of the entry unemployment effect. As in Table 3 for the US, we split the data into four experience groups (0-5, 6-11, 12-17 and 18-23 years) and allow the entry effect to differ across the experience groups. Column (1) of Table 7 reports the estimates without controlling for subsequent unemployment whilst Columns (2)-(4) control for these effects interacted with experience (i.e. equation (4)). The estimates show there to be a strongly persistent effect of entry unemployment on subsequent criminal convictions. Once again, the key message is that high entry unemployment contributes to significantly higher crime rates among affected cohorts that are long-lasting. Over a decade after entry, conviction rates remain significantly higher. For property crime, the influence eventually dies out after 15-20 years post-school experience while it remains (and indeed becomes quantitatively more significant) for violent crime.

Figures 3a and b shows the year-by-year effect of entry unemployment as a cohort spends more time in the labour market. One key difference between the time-profile of the experience effects for the UK and the US is that the entry unemployment effect for 16 and 17 year olds is substantially higher than the average entry unemployment effect in the US, but not in the UK. An obvious explanation for this rests with our measure of criminality in the two countries. In the US we use arrests while we use convictions in the UK. It seems likely that the detrimental effects of entry unemployment will take substantially more time to feed through to convictions than to arrests – youths may be frequently arrested but avoid the courts until a tipping point has been reached. In any event, the effects are very similar across the two countries from age 18 onwards.

Table 8 focuses on whether all recessions are alike. A feature of the labour market common to European countries over the last forty years, but almost completely absent for the US (until the Great Recession) has been the incidence of long-term unemployment. We might expect, and the model of Mocan et al. (2005) predicts, that recessions characterized by rising rates of long-term unemployment would be much worse for potential scaring. Of course initially, a rising duration for the stock of currently unemployed is positive for new entrants since the stock of unemployed provide less competition for available vacancies, but we might expect this effect to be fleeting before the negative effects of unemployment duration on new entrants takes its toll. To examine this we divide the entry unemployment rate into the short-term and long-term unemployment rate. Short-term unemployment covers all those with a current unemployment spell of less than twelve months. For our entire sample, the average unemployment rate of 7.4 percent is made up of a short-term rate of 4.6 percent and a long-term rate of 2.8 percent. The results of Table 8 show strongly that it is deep and long recessions characterized by high long-term unemployment that are particularly problematic.

5. Individual-Level Evidence

(a) United States

We now turn to individual-level data to add to the picture developed in the previous section. We begin the analysis with the US incarceration data. We use state-at-birth to identify the state in which the individual went to school (Dahl, 2002) and so restrict the data to those born in the United States. Table 9, Panel A reports the key regression results on the Census micro data using a linear probability model. Column (1) reports the results for the full sample of males aged 18-39 whilst the subsequent three columns focus on samples defined by educational attainment. All regressions include a full set of year, state of residence, state of birth and cohort effects, a quartic in age and controls for race, education, marital status and veteran status.

The estimated coefficient on entry unemployment in column (1) is 0.031. The mean of the dependent variable is 0.028 (i.e. 2.8 percent of males aged 18-39 are incarcerated). Thus, entering the labour market in a time of recession (defined as the unemployment rate being 5 percentage points higher than normal) results in a 5.5 percent increase in the probability of being incarcerated at the time of subsequent census survey dates.

However we can see from the subsequent columns that this effect is almost entirely due to the high-school dropouts. A recession increases this group's probability of incarceration by 7 percent, from an already high mean of 8.4 percent. These are sizeable effects when we realize that this is averaged over more than twenty years of the individual's post-school experience.

Finally, in columns (3) and (4) we see only weak effects for those who successfully graduate from high school and no effect at all for those with 4-years of college – who should not of course be affected by the unemployment rate at the compulsory school-leaving age. The results in Panel B show that redefining the 1980 measure of incarceration by explicitly excluding those not in correctional facilities (see the Data Appendix for discussion) does not alter our conclusions. This suggests that policy focused explicitly on the least educated during periods of high unemployment would likely produce substantially more benefit on crime reduction within that group than the average estimate from the previously reported panel regressions would imply.

(b) United Kingdom

For the UK, we look at the individual-level data on self-reported arrests. The data provide information on the age at which the respondent left full-time education and so allow us to precisely date the year of labour market entry. The data also provide an extensive set of personal characteristics, which we would expect to be correlated with criminal activity. There are two key disadvantages in using this micro data. First, the usual concern associated with the self-reporting of arrests. In the context of this study however, this would only bias our estimates if the self-reporting probability varied *within* a cohort depending on the initial entry unemployment rate. It seems to us hard to make such a case. Second, we have no information of when the arrest occurred – the question is simply whether the individual has *ever* been arrested. So this micro data allows us to estimate the *average* impact of initial entry unemployment on the probability of being arrested in adulthood, but does not allow us to investigate the time pattern of the persistence of such effects.

We estimate probit models with the dependent variable taking the value one if the respondent reports having *ever* been arrested by the police. We include survey year dummies and an extensive set of personal controls. Table 10 reports the results. Column (1) shows an estimated significant positive coefficient on the entry unemployment rate – a recession (again defined as a 5 percentage points higher than normal unemployment rate) is thus associated with a 5.7 percent increase in the probability of ever being arrested.

In the second column we restrict attention to those whose highest educational qualification was achieved at age 16 and therefore definitely left education at age 16. Here we can more closely link exit from education and the initial unemployment rate and this is likely a sample that contains a larger fraction of individuals at risk of criminal behaviour. As expected, we find a substantially larger and more strongly significant impact of entry unemployment for this group – a recession raises the probability of ever being arrested by 8 percent for this group.

In the final column we conduct a placebo-type experiment. We examine the arrest record of individuals who report educational qualifications that required school attendance at least to age 18. This group should not have been directly affected by the unemployment rate when they were 16. Sure enough we no longer find a positive effect for these individuals – indeed the estimated coefficient on the entry unemployment rate is indistinguishable from zero, though the standard error is large.

Overall, then, the individual-level analysis of the relationship between crime and entry-level unemployment produces results that are very similar to the cohort panel analysis of Section 4. This is true for both countries, despite some differences in the nature of the data that is available. The individual data also permits us to study variations across individuals with different levels of education in more detail than the more macro cohort analysis which does not permit such differentiation, It is highly reassuring that the overall pattern of results are very consistent across the two.

6. Conclusions

We have presented the first evidence that recessions can lead to substantial and persistently higher rates of criminal behavior among those likely to be most impacted by such conditions – those newly entering the labour market. In contrast to much of the evidence on the long-run effect of initial unemployment on wages and career trajectories, we find that the effect on criminal behaviour remains substantial, though attenuated, a number of years after labour market entry. These sizable and persistent entry level unemployment effects thus show that recessions can produce career criminals. One might argue that our results are also consistent with a one-time criminal event for individuals in a particular cohort that happens at different times since leaving school and that the probability of such a subsequent event is higher if entry level unemployment were higher. Such a view would however be in conflict with two key empirical findings in the criminology literature: late-onset offending is extremely rare and prolific offenders account for a disproportionate share of total crime. Both are consistent with our interpretation of the results.

This evidence of a crime scarring effect from unemployment at the time of labour market entry emerges from empirical analysis of a range of different US and UK data sources, both at the level of the individual and from longitudinal analysis of age/birth cohorts over time. The evidence of crime scars demonstrates a rather more disturbing long-run effect of recessions, and adds to the research picture that the state of the business cycle when people leave school and enter the labour market can have profound and sizable impacts on economic and social outcomes across their life.

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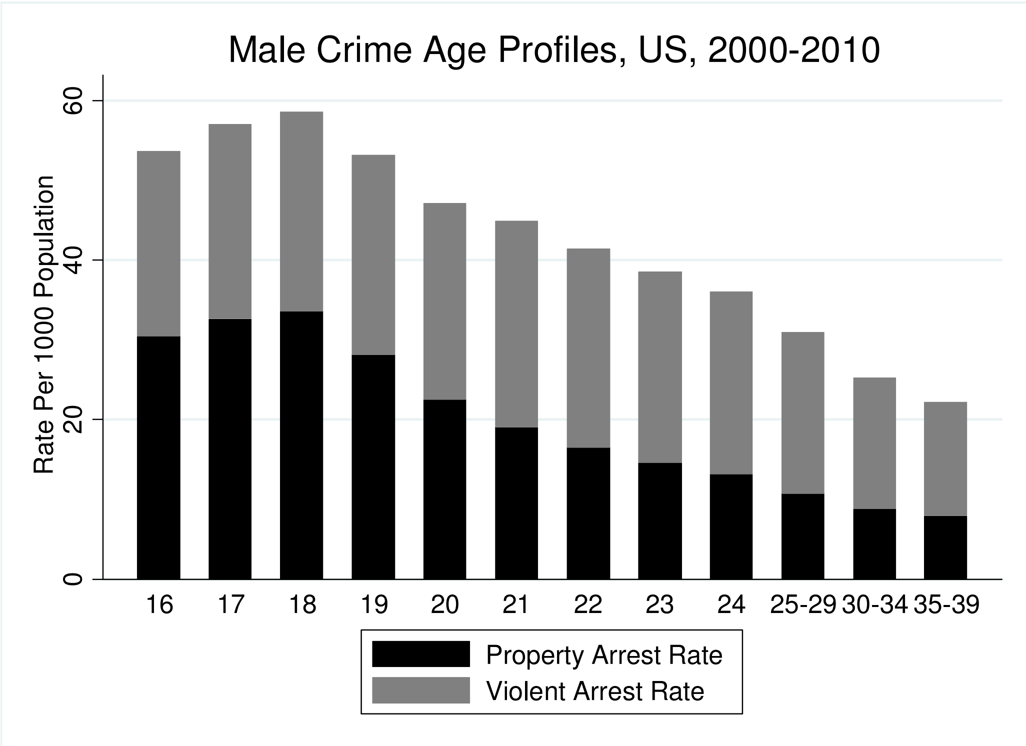
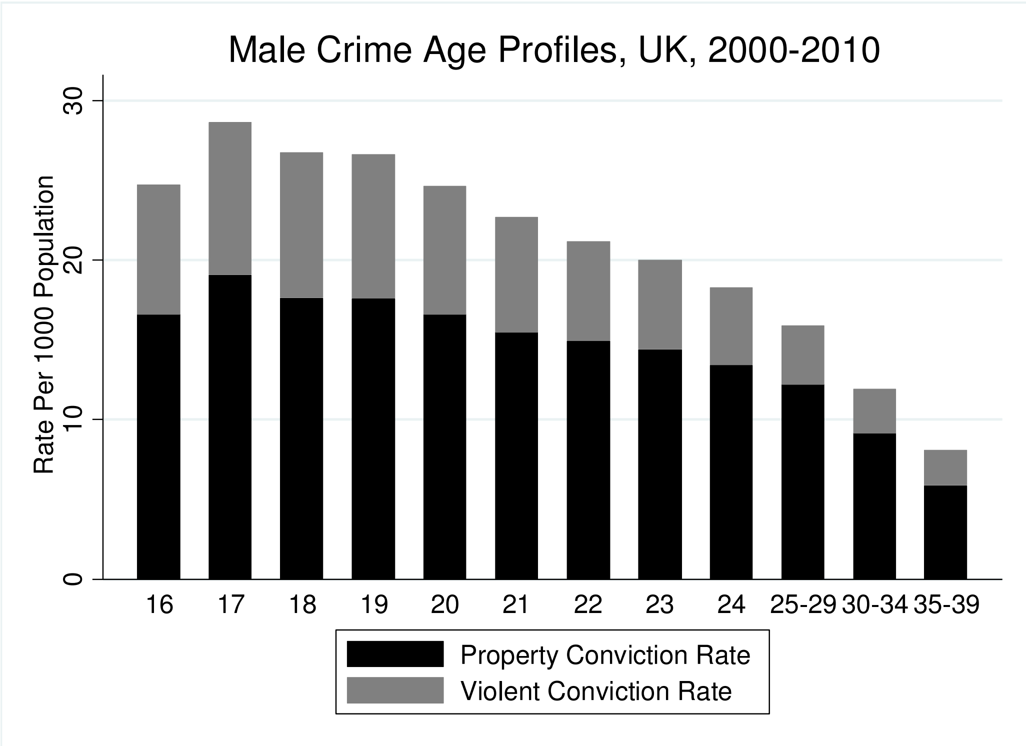
Figure 1a: Male Offender Rates by Age, US



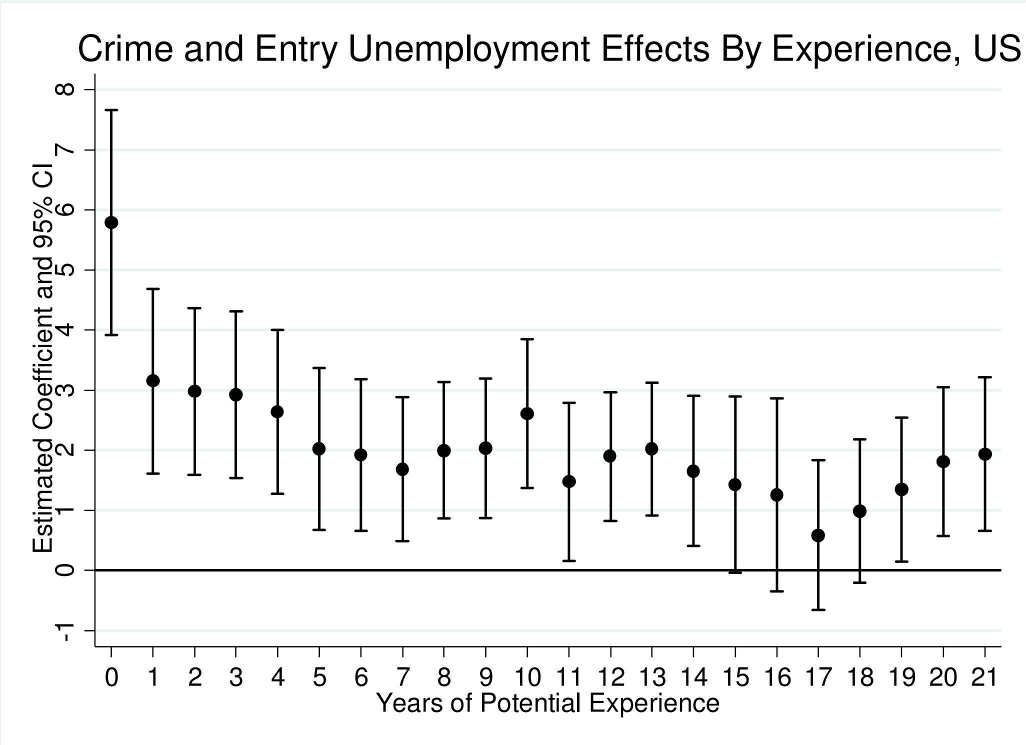
Notes: Male arrest rates by age, calculated from UCR data (see Data Appendix for more detail).

Figure 1b: Male Offender Rates by Age, UK



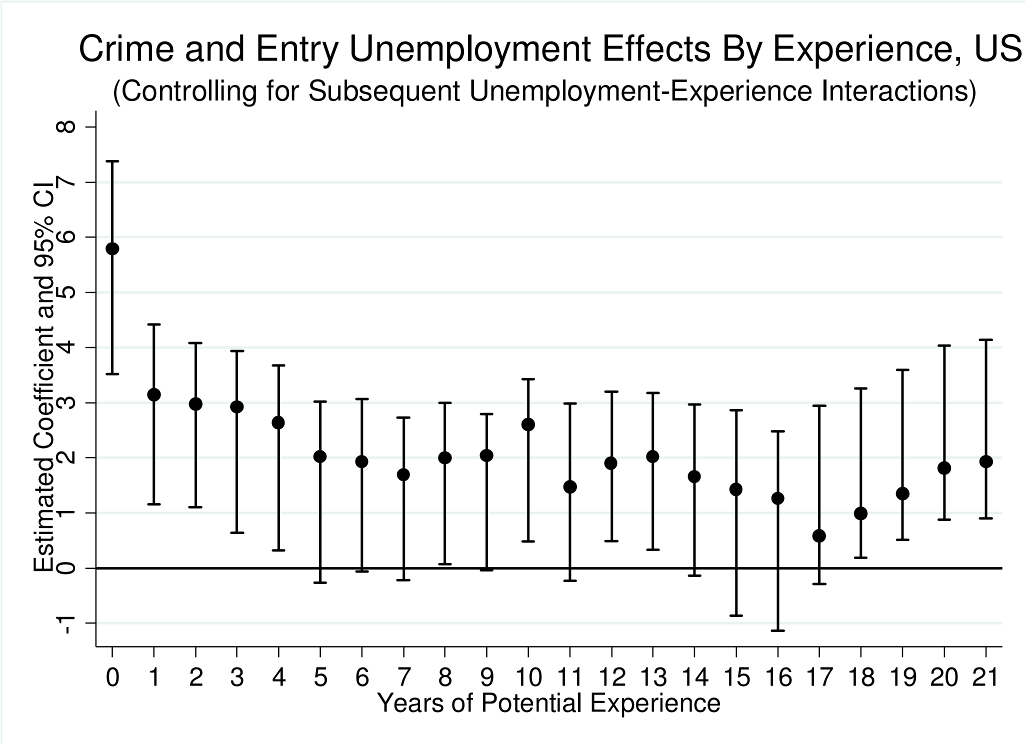
Notes: Male convction rates by age, calculated from OID/PNC data (see Data Appendix for more detail).

Figure 2a: Entry Unemployment Effects By Experience, US



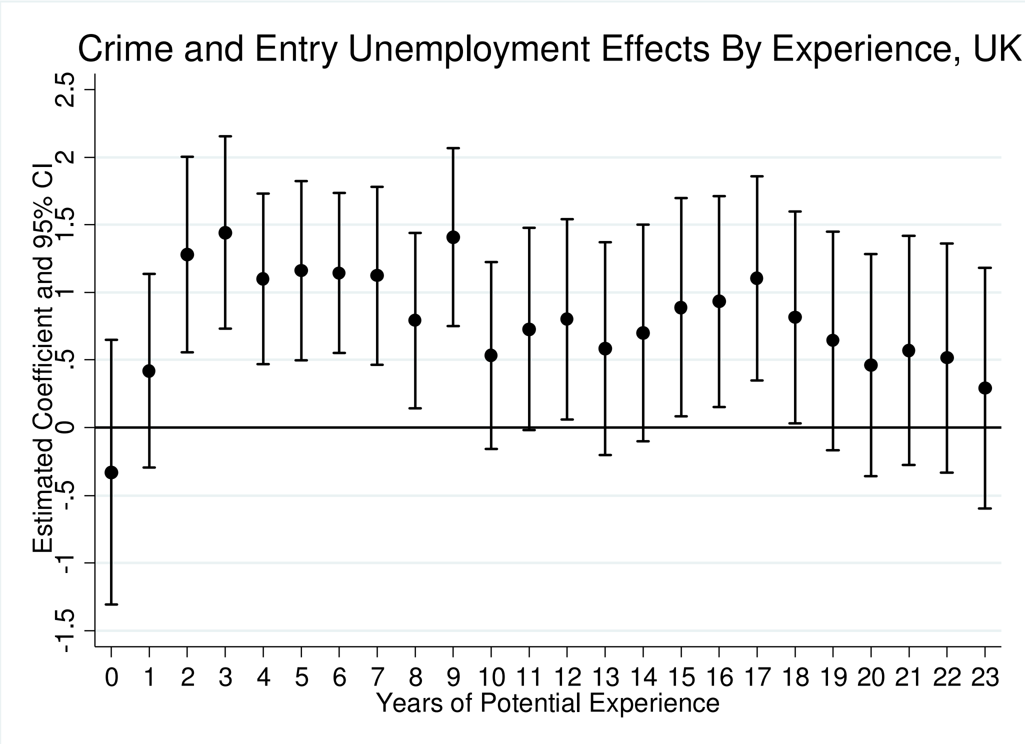
Notes: Derived from specification comparable to that in column (1) of Table 3, with separate estimates for each year of experience.

Figure 2b: Entry Unemployment Effects By Experience Controlling for Subsequent Unemployment-Experience Interactions, US



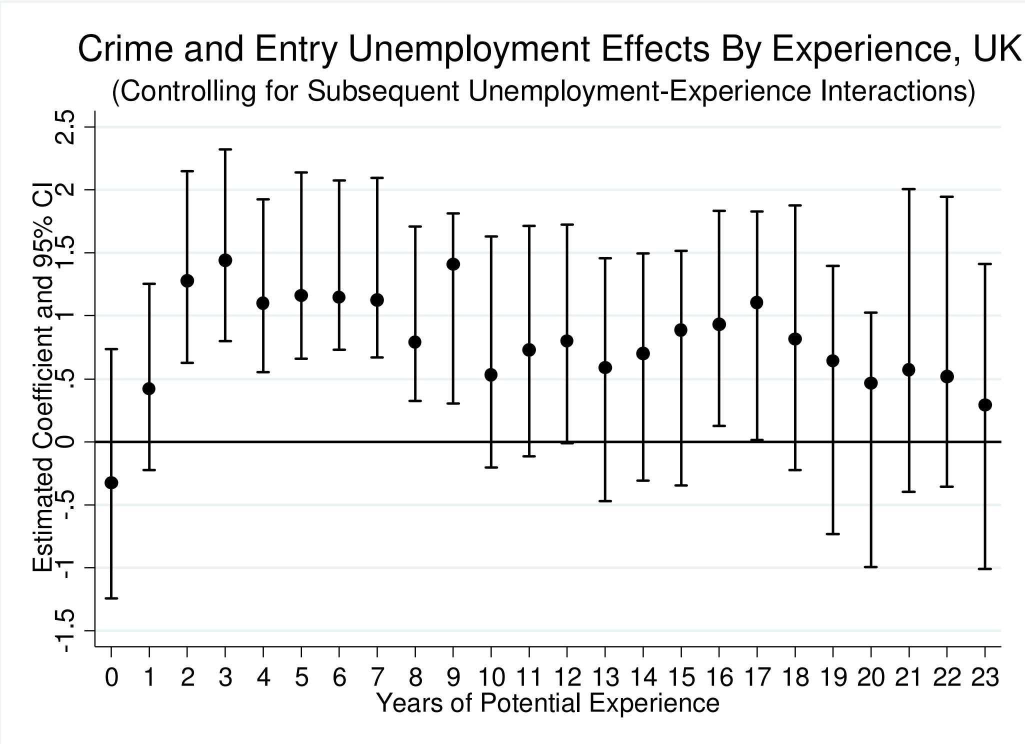
Notes: Derived from specification comparable to that in column (2) of Table 3, with separate estimates for each year of experience.

Figure 3a: Entry Unemployment Effects By Experience, UK



Notes: Derived from specification comparable to that in column (1) of Table 7, with separate estimates for each year of experience.

Figure 3b: Entry Unemployment Effects By Experience Controlling for Subsequent Unemployment-Experience Interactions, UK



Notes: Derived from specification comparable to that in column (2) of Table 7, with separate estimates for each year of experience.

Table 1: US Cohort Panel Estimates, Basic Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Crime Type:	All	All	Property	Property	Violent	Violent
National Entry U Rate at Age 16-18	1.550***		1.419*		1.871***	
	(0.506)		(0.732)		(0.519)	
State Entry U Rate at Age 16-18		2.039***		2.115***		2.156***
		(0.443)		(0.598)		(0.524)
State Fixed Effects	x	x	x	x	x	x
Year Fixed Effects	x	x	x	x	x	x
Quadratic Cohort Trend	x		x		x	
Cohort Fixed Effects		x		x		x
Age Fixed Effects	x	x	x	x	x	x
Compositional Adjustment	x	x	x	x	x	x
Sample Size	19,429	19,429	19,429	19,429	19,429	19,429

Notes: Dependent variable is the log male arrest rate from the UCR. Sample runs from 1980-2010. Individual year-of-birth cohorts run from 1941-1994. We assume that cohorts enter the labour market between the age of 16 and 18. All insured unemployment rates are measured as the average unemployment rate at the three potential years of labour market entry. All regressions include year, age and state fixed effects. We include control variables for cohort compositional adjustments (average share of immigrants, male graduates, black men, married men and females per cohort in that state 1980-2010). Columns (1), (3) and (5) include the cohort-level national unemployment rate at labour market entry and include a cohort linear trend. Columns (2), (4) and (6) include cohort-level state unemployment rates and include cohort fixed effects. Standard errors are clustered at the state-cohort level and regressions are weighted by the male cell-population.

* indicates significance at the 10 percent level.

** indicates significance at the 5 percent level.

*** indicates significance at the 1 percent level.

Table 2: US Cohort Panel Estimates, Allowing for Subsequent Unemployment Rates

	(1)	(2)	(3)	(4)
Crime Type:	All	All	All	All
State Entry U Rate at Age 16	1.525***		0.797	
	(0.381)		(0.570)	
State Entry U Rate at Age 17			0.236	
			(0.744)	
State Entry U Rate at Age 18			1.481**	
			(0.678)	
State Entry U Rate at Age 16-18		2.039***		1.382***
		(0.443)		(0.483)
State Entry U Rate at Age 19-21			-0.517	-0.384
			(0.536)	(0.541)
State Entry U Rate at Age 22-24			-0.882	-0.904*
			(0.538)	(0.538)
State Entry U Rate at Age 25-27			-0.783	-0.790
			(0.525)	(0.524)
p(sum of 16, 17, 18 effects = 0)			0.007***	
State Fixed Effects	x	x	x	x
Year Fixed Effects	x	x	x	x
Cohort Fixed Effects	x	x	x	x
Age Fixed Effects	x	x	x	x
Compositional adjustment	x	x	x	x
Sample Size	19,487	19,429	19,429	19,429

Notes: As for column (2) specification of Table 1.
* indicates significance at the 10 percent level.
** indicates significance at the 5 percent level.
*** indicates significance at the 1 percent level.

Table 3: US Cohort Panel Estimates, Effects by Labour Market Experience Groups

	(1)	(2)	(3)	(4)
Crime Type:	All	All	Property	Violent
State Entry U Rate at Age 16-18*Exp(0-5)	3.609***	3.290***	1.481**	5.151***
	(0.626)	(0.702)	(0.717)	(1.124)
State Entry U Rate at Age 16-18*Exp(6-11)	1.926***	1.705***	0.965	2.615***
	(0.535)	(0.617)	(0.737)	(0.821)
State Entry U Rate at Age 16-18*Exp(12-17)	1.475***	1.558**	2.151**	0.883
	(0.556)	(0.643)	(0.911)	(0.752)
State Entry U Rate at Age 16-18*Exp(18-21)	1.515***	2.421***	3.345***	2.032**
	(0.566)	(0.707)	(0.959)	(0.859)
State Fixed Effects	x	x	x	x
Year Fixed Effects	x	x	x	x
Cohort Fixed Effects	x	x	x	x
Age Fixed Effects	x	x	x	x
Compositional adjustment	x	x	x	x
Allowing for subsequent U rates	-	x	x	x
Sample Size	19,429	19,429	19,429	19,429

Notes: As for columns (2), (4) and (6) specifications of Table 1.
* indicates significance at the 10 percent level.
** indicates significance at the 5 percent level.
*** indicates significance at the 1 percent level.

Table 4: US Cohort Panel Estimates, Robustness Checks for Mobility and Age of Entry Unemployment

	(1)	(2)	(3)
Crime Type:	All	Property	Violent
A: Age 16-18 Entry U Rate			
Mobility Adjusted State Entry U Rate at Age 16-18	2.470***	2.016**	3.288***
	(0.609)	(0.771)	(0.776)
B: Age 16 Entry U Rate			
Mobility Adjusted State Entry U Rate at Age 16	1.857***	1.483**	2.621***
	(0.527)	(0.662)	(0.668)
State Fixed Effects	x	x	x
Year Fixed Effects	x	x	x
Cohort Fixed Effects	x	x	x
Age Fixed Effects	x	x	x
Compositional Adjustment	x	x	x
Sample Size, Panel A	19,429	19,429	19,429
Sample Size, Panel B	19,487	19,487	19,487

Notes: As for columns (2), (4) and (6) specifications of Table 1.
* indicates significance at the 10 percent level.
** indicates significance at the 5 percent level.
*** indicates significance at the 1 percent level.

Table 5: UK Cohort Panel Estimates, Basic Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Crime Type:	All	All	Property	Property	Violent	Violent
National Entry U Rate at Age 16	2.664***		3.443***		0.803***	
	(0.189)		(0.249)		(0.191)	
Region Entry U Rate at Age 16		0.812***		0.712**		1.531***
		(0.277)		(0.350)		(0.365)
Region Fixed Effects	x	x	x	x	x	x
Year Fixed Effects	x	x	x	x	x	x
Quadratic Cohort Effect	x		x		x	
Cohort Fixed Effects		x		x		x
Age Fixed Effects	x	x	x	x	x	x
Compositional Adjustment	x	x	x	x	x	x
Sample Size	7,440	7,440	7,440	7,440	7,440	7,440

Notes: Dependent variable is the log male conviction rate from the OID/PNC. Sample runs from 1980-2010. Individual year-of-birth cohorts run from 1941-1994. We assume that cohorts enter the labour market at age 15/16. All unemployment rates are measured in year of labour market entry. We include control variables for cohort compositional adjustments (average share of immigrants, male graduates, nonwhite men and married men in each cohort/region, 1980-2010), allowing for differential effects of composition in London. All regressions include year, age, and region fixed effects. Columns (1), (3) and (5) include the cohort-level national unemployment rate at labour market entry and include a cohort linear trend. Columns (2), (4) and (6) include cohort-level region unemployment rates and include cohort fixed effects.. Standard errors are clustered at the region-cohort level and regressions are weighted by the male cell-population.

* indicates significance at the 10 percent level.

** indicates significance at the 5 percent level.

*** indicates significance at the 1 percent level.

Table 6: UK Cohort Panel Estimates, Allowing for Subsequent Unemployment Rates

	(1)	(2)	(3)	(4)
Crime Type:	All	All	All	All
Region Entry U Rate at Age 16	0.812***		0.862*	0.815***
	(0.277)		(0.477)	(0.281)
Region Entry U Rate at Age 17			0.143	
			(0.555)	
Region Entry U Rate at Age 18			-0.228	
			(0.471)	
Region Entry U Rate at Age 16-18		0.770***		
		(0.286)		
Region Entry U Rate at Age 19-21			0.048	0.024
			(0.219)	(0.217)
Region Entry U Rate at Age 22-24			-0.102	-0.105
			(0.212)	(0.212)
Region Entry U Rate at Age 25-27			0.129	0.129
			(0.237)	(0.237)
p(sum of 16, 17, 18 effects = 0)			0.009***	
Region Fixed Effects	x	x	x	x
Year Fixed Effects	x	x	x	x
Cohort Fixed Effects	x	x	x	x
Age Fixed Effects	x	x	x	x
Compositional adjustment	x	x	x	x
Sample Size	7,440	7,410	7,410	7,410

Notes: As for column (2) specification of Table 5.
* indicates significance at the 10 percent level.
** indicates significance at the 5 percent level.
*** indicates significance at the 1 percent level.

Table 7: UK Cohort Panel Estimates, Effects by Labour Market Experience Groups

	(1)	(2)	(3)	(4)
	All	All	Property	Violent
Region Entry U Rate at Age 16*Exp(0-5)	0.861***	0.968***	0.972***	1.034*
	(0.305)	(0.316)	(0.362)	(0.532)
Region Entry U Rate at Age 16*Exp(6-11)	0.914***	0.970***	1.050***	0.996**
	(0.284)	(0.298)	(0.365)	(0.444)
Region Entry U Rate at Age 16*Exp(12-17)	0.832**	0.810**	0.733	1.369***
	(0.343)	(0.358)	(0.448)	(0.435)
Region Entry U Rate at Age 16*Exp(18-23)	0.583	0.530	0.124	2.701***
	(0.369)	(0.406)	(0.502)	(0.504)
Region Fixed Effects	x	x	x	x
Year Fixed Effects	x	x	x	x
Cohort Fixed Effects	x	x	x	x
Age Fixed Effects	x	x	x	x
Compositional Adjustment	x	x	x	x
Subsequent U-Exp Interactions		x	x	x
Sample Size	7,440	7,440	7,440	7,440

Notes: As for columns (2), (4) and (6) specifications of Table 5.
* indicates significance at the 10 percent level.
** indicates significance at the 5 percent level.
*** indicates significance at the 1 percent level.

Table 8: UK Cohort Panel Estimates, Including Short- and Long-Term Entry Unemployment Rates

	(1)	(2)	(3)
Crime Type:	All	Property	Violent
Region Entry Short-Term U Rate at Age 16	-1.188*	-1.008	-1.074
	(0.620)	(0.767)	(0.933)
Region Entry Long-Term U Rate at Age 16	1.687***	1.466***	2.673***
	(0.372)	(0.474)	(0.464)
Region Fixed Effects	x	X	x
Year Fixed Effects	x	X	x
Cohort Fixed Effects	x	X	x
Age Fixed Effects	x	X	x
Compositional adjustment	x	X	x
Sample Size	7,440	7,440	7,440

Notes: As for columns (2), (4) and (6) specifications of Table 5.
* indicates significance at the 10 percent level.
** indicates significance at the 5 percent level.
*** indicates significance at the 1 percent level.

Table 9: US Individual Level Estimates, Census/ACS Incarceration Regressions, 1980-2010

	(1)	(2)	(3)	(4)
Sample:	All Males	HS Dropouts	HS Grads	4yr College
A. Aged 18 And Over				
State Entry U Rate at Age 16-18	0.031**	0.120**	0.017	-0.004
	(0.015)	(0.053)	(0.025)	(0.009)
B. Aged 18 And Over, 1980 Redefined				
State Entry U Rate at Age 16-18	0.026*	0.137***	0.013	-0.010
	(0.015)	(0.052)	(0.024)	(0.008)
Year Effects	x	x	x	x
State Effects	x	x	x	x
State/Race Effects	x	x	x	x
Cohort Effects	x	x	x	x
State of Birth Effects	x	x	x	x
Age Quartic	x	x	x	x
Sample Size	5,760,227	798,692	2,553,430	1,169,645

Notes: The dependent variable is a dummy equal to 1 if the individual is institutionalized and 0 otherwise. Sample covers males aged 18-39 who are not in school, and born in the United States. Entry unemployment is the unemployment rate at age 16 in the state of birth. Data are from the 1980, 1990 and 2000 5 percent IPUMS US Census and the 2008-2012 IPUMS ACS. Regressions also include marital status, race, education and veteran status indicators. Standard errors are clustered at the state/cohort level and regressions are weighted with the Census person weight.
* indicates significance at the 10 percent level.
** indicates significance at the 5 percent level.
*** indicates significance at the 1 percent level.

Table 10: UK Self Reported Arrest Regressions, 2001/2 to 2010/11

	(1) Ever Arrested	(2) Ever Arrested, Age 16 Qualification	(3) Ever Arrested, Age 18+ Qualification
Region Entry U Rate at Age 16	0.246**	0.508***	0.050
	(0.123)	(0.159)	(0.208)
Year Dummies	x	x	x
Personal Controls	x	x	x
Mean of Dependent Variable	0.215	0.303	0.164
Sample Size	22,646	7,984	9,166

Notes: Table reports estimated marginal effects from a probit. Personal controls include age (10 categories), ethnic group (5 categories), education (9 categories where appropriate), student status, marital status (4 categories), income (18 categories), economic status (15 categories), number of children (10 categories), housing tenure (8 categories), years at address (9 categories), years in area (9 categories), and government office region (10 categories). The sample covers ages 16 to 65 of pooled British Crime Surveys, 2001-2002 to 2010-2011. Regressions use individual sample weights. Standard errors in parentheses are clustered at the government office region level

* indicates significance at the 10 percent level.

** indicates significance at the 5 percent level.

*** indicates significance at the 1 percent level.

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Appendix 1: Additional Figures and Tables

Figure A1: Autocovariance Structure of Unemployment Rates, US

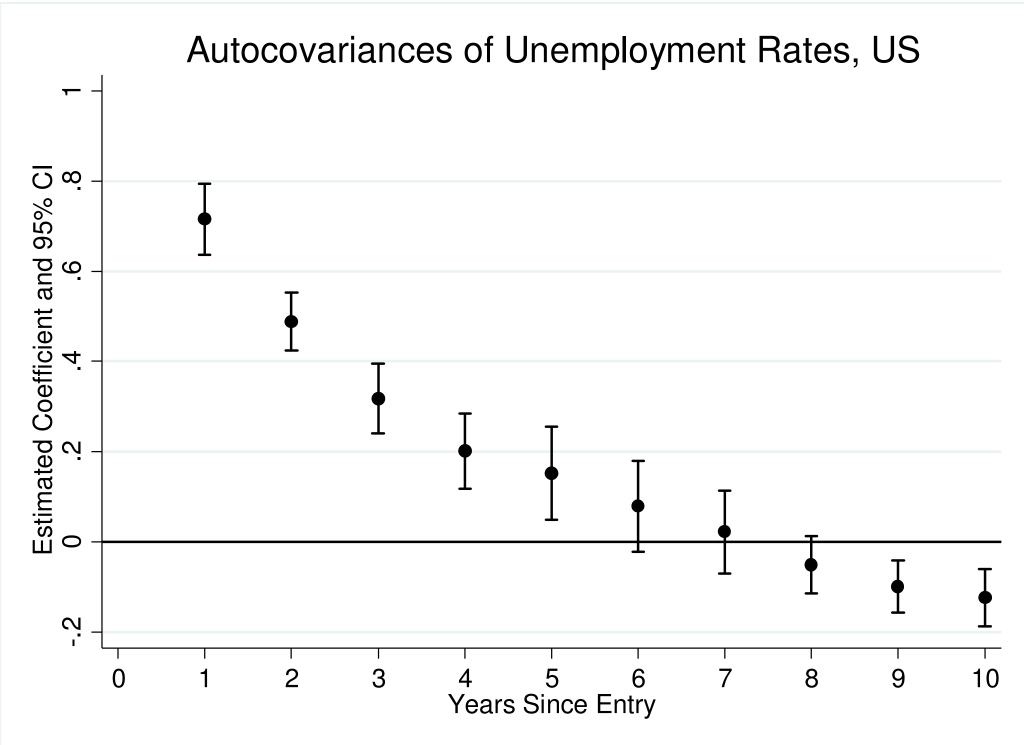

Figure A2: Autocovariance Structure of Unemployment Rates, UK

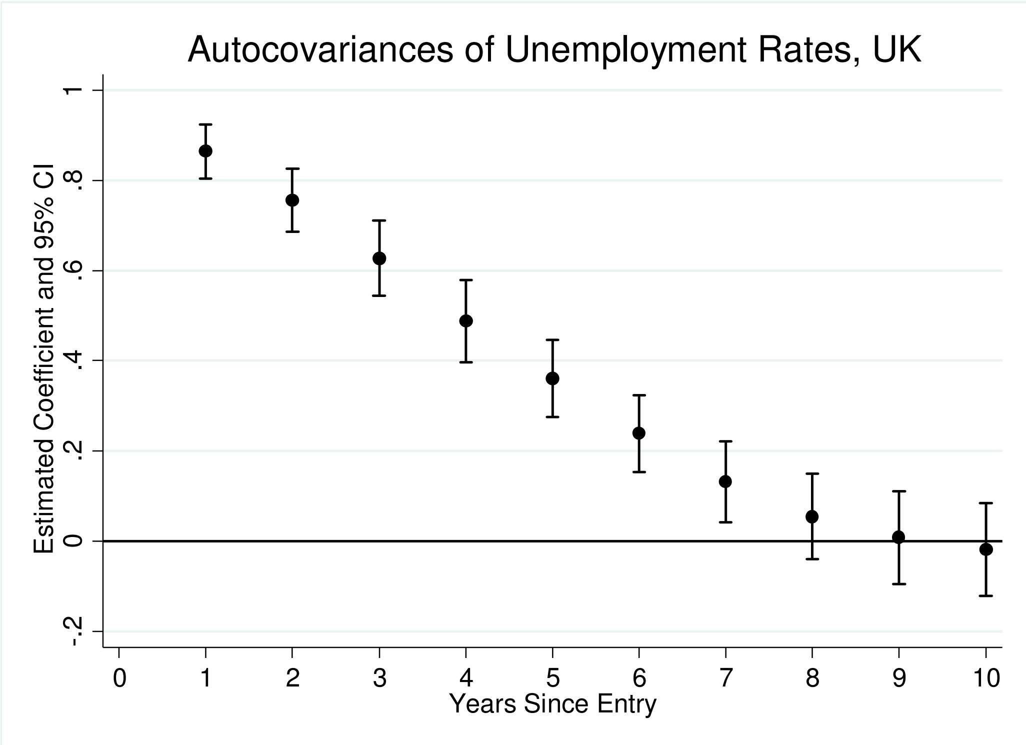

Table A1: US Male Population in Group Quarters by Type and Age, 1980-2010

	Total Institutionalized	Correctional Institutions	Correctional as Percent of Total
1980 Census			
All	1232120	439720	35.7
15-17	68300	8460	12.4
18-21	123320	89600	72.7
22-24	104060	80240	77.1
25-39	301980	205780	68.1
1990 Census			
All	1801350	1030210	57.2
15-17	68480	16490	24.1
18-21	149780	128940	86.1
22-24	143890	133490	92.8
25-39	666690	581670	87.2
2000 Census			
All	2534060	1806260	71.3
15-17	87200	18960	21.7
18-21	221660	202470	91.3
22-24	201060	195660	97.3
25-39	951660	911050	95.7
2010 Census			
All	2716877	2059020	75.8
15-19	153924	74720	48.5
20-24	327760	308926	94.3
25-39	971581	945065	97.3

Notes: Data from 1980 are calculated from IPUMS data, figures for 1990, 2000 and 2010 come from the US Census Bureau.

Appendix 2: Data Description

A. United States

A1. Micro Data on Incarceration

The micro data on US incarceration comes from the US Census. We sample all males aged 16-39 from the 5 percent IPUMS for the 1980, 1990 and 2000 Census and the 2008-2012 American Community Survey (ACS). We identify the institutionalized population using the Group Quarters (GQ) variable. The GQ variable consistently identifies the following categories:

a) Non-group quarter households;
b) Institutions (Correctional Institutions, Mental Institutions, Institutions for the elderly, handicapped and poor);
c) Non-institutional group quarters (Military, College dormitory, rooming house, other).

However only in the 1980 IPUMS is the GQ variable detailed enough to uniquely identify those in correctional facilities. In subsequent Censuses (and the ACS), the institutionalized population includes the following categories: correctional facilities, nursing homes and mental hospitals, and juvenile institutions. However, the share of the total institutionalized population accounted for by those in correctional facilities is very high in our sample.

Appendix Table A1 shows the institutionalized male population by GQ type and age. Note that this data comes from published aggregate Census reports that do break up the categories, though this is not available in the IPUMS data release. In 2000, for example, 95.3 percent of institutionalized males aged 18-39 where in correctional facilities. Two key points come from Table A1. First, incarcerated males aged less than 18 are much less well identified (since juvenile facilities are an important component for this group). We therefore restrict our analysis of the Census data to those aged 18-39. Second, the 1980 Census has a less tight correspondence between institutionalization and incarceration. Fortunately, this is the one Census that has the full GQ coding in the micro files, so as a robustness test we use only the correctional facility definition in the 1980 Census. In the main specification we prefer to use the broader institutionalized measure across all years to maintain consistency. This approach is the same as that used by Borjas, Grogger and Hanson (2010).

A2. Panel Data on Arrests

Panel data for the US come from the FBI Uniform Crime Reports (UCR). The measure of crime is arrests. The UCR reports the number of arrests by year, state, age, gender and type of crime. The original data identifies the number of arrests by law enforcement agencies within states. We construct a state-level panel on arrests by aggregating the number of arrests over law enforcement agencies within a state. Within the UCR, data for certain states are systematically missing either for the whole state or for important law enforcement agencies within that state. For example, New York City systematically does not report arrest numbers which implies that New York state level counts on arrests would be heavily undercounted if we simply aggregated the number of arrests over all reporting agencies. Hence, we exclude the following states from our sample:

Indiana, Louisiana, Mississippi, Montana, Nebraska, New Hampshire, New Mexico, New York, Ohio, South Dakota and Washington.

Data for some states are missing for a limited number of years only. For example, Florida reports arrests until 1995, but not afterwards. Since there is no evidence that would suggest that these states differ significantly in terms of unemployment rates, we exclude the respective years only and keep the non-missing years of these states as observations in the sample, leading to an unbalanced sample. In the example above that means that we include Florida in our sample until 1995. In addition, the UCR reports the total population for each law enforcement agency in the reported year. Aggregating the UCR population count to the state-year level and comparing that number to official population counts allows us to identify state-year observations that cover arrests for less than 95 percent of the state population. Since these arrest counts are likely to underreport the true number of arrests in that state and year, we exclude the respective observations from our sample. Whenever single state-year observations are missing in the resulting sample, we impute values using a linear interpolation method. Our results are robust to excluding imputed observations.

We sample males aged 16 to 39 from 1980 to 2010. The UCR data are grouped by age category. From age 16 up to the age of 24, the number of arrests is measured by single age year. For ages 25 and above, the arrests are aggregated to the number of arrests in a five-year age bracket, i.e. 25 to 29, 30 to 34, and 35 to 39. In order to be able to track the number of arrests per year-of-birth cohort, we therefore disaggregate the arrest measure to the number of arrests by single age year by dividing the arrest count by five. The underlying assumption is that year-of-birth cohorts are homogenous in terms of the number of arrests within the respective age bracket. Following the literature, we categorize arrests into property and violent crime using the UCR offense code variable as follows:

Violent crime:

01A = Murder and non-negligent manslaughter
01B = Manslaughter by negligence
02 = Forcible rape
03 = Robbery
04 = Aggravated assault
08 = Other assaults

Property crime:

05 = Burglary – breaking or entering
06 = Larceny – theft (except motor vehicle)
07 = Motor vehicle theft
09 = Arson

In order to produce arrest rates, we aggregate the number of arrests for the above categories and divide the resulting number of arrests by the annual state-age-year population. The population data for that purpose are retrieved from the US Census population estimates. We scale our arrest rates by 1,000 in order to ease the interpretation of our results.

A3. Entry Unemployment Rates

We assume the age of labour market entry to be 16 to 18 for the whole sample. The year-of-birth cohorts in both the Census and the UCR samples run from 1941 to 1994: The first year of observation is 1980 with the oldest cohort aged 39, i.e. born in 1941. The last year of observation is 2010 with the youngest cohort aged 16, i.e. born in 1994. As we want to measure unemployment rates in the year of labour market entry, we consider unemployment rates from

1957 to 2010, covering the calendar years of each cohort in the sample at age 16-18. We use data on state-level annual insured unemployment rates from 1957 until 2010. The data are available from the Unemployment Insurance Financial Data Handbook provided by the US Department of Labour, Employment & Training Administration on their website. Unfortunately, that kind of data does not allow us to disaggregate entry unemployment rates by age, nor to provide measures of the duration of unemployment. As an alternative, the Bureau of Labor Statistics (BLS) provides state level unemployment rates based on the Current Population Survey (CPS) from 1977 onwards, to which we data read from the graphs in Blanchflower and Oswald (1994) back to 1963. This robustness check yields very similar results to the use of insured unemployment rates.

B. United Kingdom

B1. Micro Data on Arrests

The micro-level data for the UK comes from the British Crime Survey (BCS). The BCS is a large (around 45,000 individuals) annual cross-section survey used to construct measures of crime victimization. It is nationally representative and contains extensive personal demographics. From 2001 onward a sub-sample of respondents complete a supplementary survey that, among other things, covers contact with law enforcement agencies. In particular, respondents are asked whether they have ever been arrested by the police. There is however no information on the type of crime for which they were arrested nor on the eventual outcome. In addition there is no information on when the arrest occurred i.e. a 65-year old may have been arrested last week or 50 years ago. We sample all males aged 16-65 and allocate the entry unemployment rate (see below) based on current region of residence and year of reaching 16 years old.

B2. Panel Data on Convictions

Crime data for the UK panel come from the Offenders Index Database (OID) and the Police National Computer (PNC). The measure of crime is convictions. The OID is a 4-week sample of all convictions in all courts across England and Wales, with the sample weeks evenly spread across the year. The data contains a unique personal identifier to allow us to remove multiple convictions for the same individual (i.e. in a given year an individual is either convicted or not) and provides data on gender, date of birth, region of conviction (10 regions) and offence category. This data sample runs from 1980 to 2002. From 2003 to 2010, the OID has been superseded by the PNC. We do not have access to the micro data of the PNC, but the Ministry of Justice have provided us with an extract of the number of individuals convicted in each year, broken down by individual year of age, gender, region of conviction and offence category. This allows us to merge the two datasets together to produce a panel covering the years 1980 to 2010. The PNC data is actually provided for the period 2000-2010 which allows us to examine the overlap between the OID and PNC in 2000-2002. Our analysis of this overlap suggests a very high concordance between the two sources. We obtain the number of convictions for property and violent crimes by aggregating convictions over crime types. As such, our measure for property crime includes burglary, theft and handling of stolen goods and criminal damage, while our measure for violent crime includes violence against the person, sexual offences and robbery. We produce conviction rates by dividing the number of convictions by the annual population in the observational unit (year-of-birth by region cell), and scale by 1,000. Population data are taken from the ONS population estimates. As with the US data, the sample covers convictions from 1980 to 2010 for 16-39 year-old males. Individual year-of-birth cohorts again therefore run from 1941 until 1994.

B3. Entry Unemployment Rates

Year of labour market entry is assumed to be 15 for those leaving school by 1972 and 16 for those leaving from 1973 onward to reflect the change in compulsory school leaving age introduced in the UK in 1973. Entry unemployment rates are measured at both the national and regional level. The male unemployment rate data from 1975 onwards comes from the Labour Force Survey (equivalent to the CPS). Prior to 1975 the unemployment rate is derived from the claimant count data. This latter measure covers only those registered as unemployed and is therefore a more narrow definition than that in the Labour Force Survey (which covers all those actively seeking employment in the previous two weeks). However our unemployment rate is only measured for males and the discrepancy between the two alternative measures was small prior to the 1980s.