Brian Bell, Anna Bindler and Stephen Machin

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Crime Scars:
Recessions and the Making of Career Criminals

Brian Bell*, Anna Bindler** and Stephen Machin***

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*   *School of Management, King's College London and Centre for Economic Performance, London School of Economics*

**   *Department of Economics, University of Gothenburg*

***   *Department of Economics and Centre for Economic Performance, London School of Economics*

Corresponding Author:

Professor Stephen Machin
Department of Economics and Centre for Economic Performance
London School of Economics
WC2A 2AE
UK

s.j.machin@lse.ac.uk

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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 labor market during economic downturns suffer from poor job matches that can 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 labor market. Thus crime scars resulting from higher entry level unemployment rates prove to be both long lasting and substantial.

Keywords: Crime; Recessions; Unemployment.

JEL Classifications: J64; K42.

1. Introduction

Do the labor market conditions the young encounter when first leaving 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, 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 who would otherwise have avoided such a result in a more buoyant labor market.

But what might happen as these same youngsters age? Two obvious mechanisms 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 subsequent crime participation. Second, a previous criminal record (and less on-the-job human capital accumulation) may reduce the expected wage in the legal labor market. Both can be expected to increase the likelihood that the individual eventually ends up becoming 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 with age quite

rapidly.[1] Unsurprisingly in our data, the same patterns emerge.[2] The peak is at age 17 or 18 and then declines reasonably smoothly. However, it is still the case that the offender rate at age 29 is a lot higher than at age 39 – showing criminality to be not just a feature of teenage years.

Existing evidence also points to strong links between criminality in teenage years and subsequent criminal behavior.[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 behavior for juveniles have scope to raise the lifetime criminal participation rate. The focus of this paper is whether the state of the labor 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, studying whether crime rates, and in particular property crime rates, are countercyclical. The place where one can identify effects of unemployment on 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 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

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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] Figure A1 in the Appendix plots the average male offender rate by age for the US and UK from 2000-2010. Full details on the data used in the chart are provided in Section 4 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 labor market effects on crime (for example, see Mustard, 2010, and Buonnano et al., 2011).

Grönqvist (2013) uses Swedish register data to uncover 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 labor 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 labor 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

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

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 terms of present value.

Outside of the labor market literature, labor 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 better labor 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 labor 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.

Our results uncover a more disturbing long-run effect of recessions. Based on a variety of individual and cohort level data for men aged up to 39 from the US and the UK, we report evidence of a systematic empirical link between crime and entry-level unemployment that very clearly shows 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 labor market. 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 labor 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 labor 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. We present the cohort panel results and individual-level evidence in Sections 4 and 5, respectively. Section 6 concludes.

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 labor 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 have received substantial empirical support (see the reviews of Draca and Machin, 2015, Freeman, 1999, and Chalfin and McCrary, 2014). 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 with income coming from 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 both sectors. In each period, the individual solves a dynamic stochastic optimization problem. First, they decide how much time to allocate to legal and criminal work and second, they decide on the optimal level of consumption.

Crime is risky since a criminal faces a certain probability of being caught and sent to prison (or being punished otherwise). 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. 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 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

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[6] Other dynamic crime participation models include Flinn (1986) and Lochner (2004).

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 behavior 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 labor market have developed less legal human capital and are less attached to the legal labor market. In our empirical analysis, we study cohorts entering the labor market in different economic conditions and estimate the effect of entering the labor 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 characterized by various stages: onset, persistence, escalation/specialization and desistance. 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 labor market entry conditions matter for crime fits naturally into this framework. Unemployment at labor market entry (a local life event) can contribute to the onset of criminal behavior and/or can encourage the persistence of those youths that have already begun a criminal career. The long-run effect of unemployment at labor 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 labor 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 behavior. 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 labor market entry unemployment effects on crime.

3. Empirical Strategy

Our empirical analysis exploits both panel data on year-of-birth cohorts over space and time and individual data for the US and the UK. The data are discussed in more detail in the following sections as well as in the Data Appendix.

For the panel data, we observe age/birth cohorts as they enter the labor market and follow them through their (potential) 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 refs to states in the US and to standard regions in the UK. We can estimate the long-run effect of initial labor market conditions by exploiting the regional variation in entry unemployment rates across cohorts using the following equation:

$$\log(C)_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \beta U_{cr,0} + \gamma X_{crt} + \varepsilon_{crt} \qquad (1)$$

In (1), the dependent variable is the log crime rate for the cohort, region and time cell and we include fixed effects α_c, α_r, α_t and α_a for cohort, region, time and age. X is a set of control variables (defined below) and ε is an error term. Labor market entry occurs at date 0, so $U_{cr,0}$ 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.[7] 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 baseline model in (1) is restrictive in that it assumes that subsequent unemployment rates experienced by the cohort have no effect on their criminal behavior. In effect, the model allows us to estimate the average effect of entering the labor market in a recession on crime over the life-cycle, 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 labor market conditions. To isolate this

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[7] We could alternatively have required the cohort-effects to sum to zero (Deaton, 1997), and our results prove to be robust to this alternative.

[8] The specific control variables included are to account for demographic correlates of crime and for 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).

effect, we can include regional unemployment rates experienced by the cohort in the years after labor market entry. We measures these as $U_{cr,i}$, where $i > 0$ is the number of years since entry. This gives us a second, more general model to estimate:

$$\log(C)_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \beta U_{cr,0} + \sum_{i=1}^{I} \delta_i U_{cr,i} + \gamma X_{crt} + \varepsilon_{crt} \qquad (2)$$

where i can theoretically take any value up to the latest year observed since labor 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 labor 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 labor market is restricted to be zero until the cohort actually reaches five years of experience. This ensures that future unemployment rates cannot affect current crime, which is intuitively sensible.

Next, we introduce dynamics by further generalizing equations (1) and (2) to permit the main coefficient of interest on the initial unemployment rate, β, to vary with labor market experience/years since assumed labor 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 labor market entry increases or because of more persistent effects:

$$\log(C)_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \sum_{e=1}^{E} \beta_e \left[I(Exp=e)*U_{cr,0} \right] + \gamma X_{crt} + \varepsilon_{crt} \qquad (3)$$

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[9] Potential experience here is years since labor market entry (i.e. age - [age at year t = 0], with t = 0 being the assumed labor 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.

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

Our final and most general estimating equation further allows for the unemployment experienced after labor market entry to have permanent or transitory effects:

$$\log(C)_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \sum_{e=1}^{E} \beta_e \left[I(Exp=e)*U_{cr,0}\right] + \qquad (4)$$

$$\sum_{i=1}^{I} \sum_{e=1}^{E} \delta_{ie} \left[I(Exp=e)*U_{cr,i}\right] + \gamma X_{crt} + \varepsilon_{crt}$$

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.

It is important to be clear that identification comes from *within*-cohort variation in entry unemployment rates across states/regions. We view this as the most convincing approach that can be taken 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 labor market entry, including a quadratic cohort trend to account for changing cohort quality.

For the micro-level data 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 the unemployment rate at the time of labor market entry by area of residence and estimate probability models to explore the effect on criminal outcomes in later life.

4. Cohort Panel Evidence

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 to 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 crime type. Our sample runs 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.

The sample covers males aged between 16 and 39, 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 bands (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. Our sample comprises year-of-birth cohorts that run from 1941 to 1994.[10] Assuming that individuals enter the labor market at age 16, labor market entry would therefore occur from

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[10] Our first year of data on arrests 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).

1957 to 2010. We have data on state annual insured unemployment rates from 1957 until 2010.[11]

Since data for some states are systematically missing, we exclude these states from our analysis.[12] States with missing data for a limited number of years only are included for the non-missing years, leading to an unbalanced panel.[13] 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.

Two conceptual issues arise with these data. First, since we link the current arrest rate for a particular cohort in a given state to the entry unemployment rate of that cohort in the *same* state, we assume cohorts do not substantially move across states over time. So for example, we 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 labor market. The empirical validity of this relies on 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. But, if mobility is driven by self-selection, the coefficient of interest may be biased. Following Dahl (2002), below we present robustness tests based on mobility corrections from the US Census.

Second, in our empirical work for the US we use the average unemployment rate that 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 has low educational

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

[12] 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.

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

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 characterize the state of the labor 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 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. Our variable construction matches that of the US panel. However, 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 labor 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 labor market entry.

US Results

We begin our analysis of the panel data by presenting evidence on the average effect of initial labor market conditions on criminal activity. In terms of the equations above, this corresponds to equation (1) that restricts the coefficient β to be the same across experience

groups. Panel A of Table 1 shows the results for the US. The dependent variable is the log of the crime rate. Columns (1), (3) and (5) consider the national unemployment rate at labor market entry while columns (2), (4) and (6) use the state unemployment rate at labor 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 labor market in a recession is estimated to be around 10.2 percent higher than for a similar cohort entering into a normal labor market (using a 5 percentage point increase in unemployment as a measure of recession relative to normal).[14] The estimate is statistically significant at the 1 percent level.

This amounts to a substantial estimate of labor market entry effects on crime, but in some respects the average effect of recessions may not be the most relevant parameter of interest. 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 behavior 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

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[14] We have used a five percentage points increase to measure a recession. This is in line with the scale of the unemployment rate increase in the Great Recession in the US, and also with the recessions seen in the UK over the whole period we analyse. However, this may be a little high for the US for pre-2000 recessions, where perhaps a 3 percentage point increase is a more appropriate magnitude of increase. Of course, the impact on the arrest rate would need to be scaled down by 3/5 for this recession size, so in the example here it would drop to 6.1 percent.

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.[15]

As previously discussed, one may have potential concerns about inter-state mobility in the US data. More precisely, the presence of mobility raises the question of what is the correct (best measured) entry unemployment rate for cohort c at time t in state s? So far we have used the unemployment rate in state s at the time that cohort c left high-school. This ignores any mobility and if potential criminals 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 labor 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 regarding estimates of state-specific earnings returns to education, which he shows differ substantially across states. His solution is to use reported migration flows across states to correct the estimated returns. We can follow broadly the same procedure here for the US, though such data is not available for the UK. 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. 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:

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[15] 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 in Table A1 of the Appendix.

$$U_{cs} = \sum_{k=1}^{K} p_{csk} U_{ck} \tag{5}$$

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

Panel B of Table 1 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 effect tends to be slightly larger in magnitude (at 2.470 compared to 2.039 in Column (2)), but very much validates the earlier results. 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 labor market entry on crime when we do not adjust for inter-regional mobility (although all the differences between mobility adjusted and non-adjusted estimates are not distinguishable from one another in terms of statistical significance).

For the 1980-2000 time period, we also explored what happened to the entry-level unemployment rate on inclusion of the state-level "crack index" constructed by Fryer et al. (2013). The main results fully survived its inclusion and the crack index itself displayed a positive and significant relation with the arrest rate.[16] Thus the results remain robust to controlling for the crack prevalence index that has been shown to be a driver of US crime rates in the period that we study.

UK Results

The UK results are shown in Table 2. The only specification difference compared to the US results 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

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[16] In fact, when the impact of the crack index was broken down into the sub-periods that Fryer et al. (2013) use – 1980-84, 1985-89, 1990-94 and 1995-2000 – we obtain similar estimates with the most important positive crime association being in 1985-89. But the estimated coefficient on the entry-level unemployment rate was barely affected. For the 1980-2000 time period the estimated coefficient (standard error) on specifications comparable to column (2) of Table 1 were 1.156 (0.393) for the entry-level unemployment rate and 1.480 (0.557) for the mobility adjusted rate. On inclusion of the time varying crack index these respectively became 1.155 (0.383) and 1.549 (0.548).

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]

We again find a statistically significant effect of entry unemployment on overall crime. Taking the estimated coefficient in Column (2), a recession that raises the (regional) unemployment rate by 5 percentage points would raise the lifetime conviction rate by 4 percent. We are somewhat skeptical about magnitudes 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 2 is 2.664 (0.189) when we 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.

Panel B of Table 2 focuses on whether all recessions are alike. A feature of the labor 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 scarring. Of course, initially a rising duration for the stock of currently unemployed is positive for new entrants since the stock of unemployed provides 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

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[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.

rate in the UK 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 reported in Panel B show strongly that it is deep and long recessions characterized by high long-term unemployment that are particularly problematic.

Dynamic Effects

The specification used in Tables 1 and 2 implies that subsequent unemployment rates do not matter, or are at least 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 labor market and the unemployment rates they experience over the next 10 years.[18]

Tables A2 and A3 in the Appendix present 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 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.[19] We therefore prefer to either use the age 16 effect alone (recognizing

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

[19] Appendix Figures A2 and A3 show this strong persistence in the autocovariances of unemployment rates within a cohort/state group for the US and UK, respectively.

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 has an individually significant effect on arrests. 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 the persistence of this. Is the entry unemployment effect primarily driven by a very large impact on crime in the early years after labor 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 labor market conditions at labor market entry pushed into a criminal career that becomes self-perpetuating for the reasons discussed in Section 2? To study this, we allow the coefficient on initial unemployment to vary by years since labor 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 Table 1. Experience is set to 0 for ages 16 to 18. The results are shown in Tables 3 and 4 for the US and UK respectively, 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 labor market, that fall as experience increases. However, even a decade after

leaving school there remain significant positive effects from entry unemployment on crime rates. Juveniles who leave school in a recession have higher crime rates during their first few years in the labor market *and* higher crime rates over a decade later than juveniles who leave school in a buoyant economy.

An alternative specification to examine the persistence of entry unemployment allows the interaction term with experience to vary with individual years of experience (rather than group experience as in Tables 3 and 4). Figures 1 and 2 plot the estimated coefficients (together with the 95 percent confidence intervals) for every year of labor market experience for the US and UK, respectively.[20] Again, the drop in the effect after the first few years of labor market entry is clear, but the individual year estimates suggest a consistent longer lasting scarring effect for both countries.

5. Individual-Level Evidence

Details of US Micro Data

The micro-level 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 contained in these data sources. 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 in the ACS), the institutionalized population includes the following additional categories: correctional facilities, nursing homes and mental hospitals, and juvenile institutions. Fortunately for our purposes, among the younger ages we focus upon, the share of the total institutionalized

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[20] Figures A4 and A5 in the Appendix show the respective results when one allows unemployment rates later in life to enter the regression. The figures show patterns that are substantively the same.

population accounted for by those in correctional facilities is very high in our sample (see Appendix Table A4 and its surrounding discussion in the Data Appendix on the validity of this). The additional covariates from the Census data include race, marital status, veteran status and education.

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 used conviction data in the UK panel analysis, it is useful to have an alternative counterpoint measure of criminal behavior (as in Lochner and Moretti, 2004) to evaluate robustness. We have a broad array of personal characteristics including education, ethnicity, marital status, housing status and employment and income.

US Results

Panel A of Table 5 reports marginal effects from a probit regression on the Census 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. 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 0.038. The mean of the dependent variable is 0.030 (i.e. 3 percent of males aged 18-39 are incarcerated). Thus, entering the labor market in a time of recession (defined as the unemployment rate being 5

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percentage points higher than normal) results in a 6.3 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 6.6 percent, from an already high mean of 8.9 percent. These are sizeable effects taking into account that this is averaged over more than twenty years of the individual's post-school experience.

Finally, columns (3) and (4) show 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 of course not be affected by the unemployment rate at the compulsory school-leaving age.[21] 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 the conclusions. This suggests that policy focused explicitly on the least educated during periods of high unemployment would likely reduce crime substantially more within that group than the average estimate from the previously reported panel regressions would imply.

UK Results

For the UK, we look at 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 labor 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 these micro data. First, there is 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 hard to us to make such a case. Second, we have

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[21] To further examine the sensitivity of our results to mobility, we restricted the Census sample in Table 5 to include only those observations where the state of residence was the same as the state of birth. Re-estimating Table 5 with this restricted sample produces estimated coefficients (and associated standard errors) of 0.047 (0.024) and 0.161 (0.074) for the first two columns. The coefficients in the final two columns remain small in magnitude and statistically insignificant.

no information of when the arrest occurred – the question is simply whether the individual has *ever* been arrested. That means that this 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 6 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 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 the exit from education and the initial unemployment rate, which results in a sample that is likely to contain a larger fraction of individuals at risk of criminal behavior. As expected, we find a substantially larger and more significant impact of entry unemployment for this group – a recession raises the probability of ever being arrested by 8 percent.

In the final column we conduct a placebo-type experiment. We examine the arrest record of individuals who report educational qualifications that require 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 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 labor 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 behavior remains substantial, though attenuated, a number of years after labor market entry. These sizable and persistent entry level unemployment effects 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 could be higher if entry level unemployment were higher. Such a view would however be in conflict with two key empirical findings in the criminology literature, both of which are consistent with our interpretation of the results: late-onset offending is extremely rare and prolific offenders account for a disproportionate share of total crime.

This evidence of a crime scarring effect from unemployment at the time of labor 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. Whilst the reported effects are likely to be one factor amongst several that can plausibly explain the very sizable swings in youth criminality that have occurred in both countries in

the past few decades, the evidence of crime scars does demonstrate a rather more disturbing long-run effect of recessions. In doing so, it also adds to the research picture that the state of the business cycle when people leave school and enter the labor market can have profound and sizable impacts on economic and social outcomes across their life.

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Figure 1: Entry Unemployment Effects By Experience, US

Crime and Entry Unemployment Effects By Experience, US
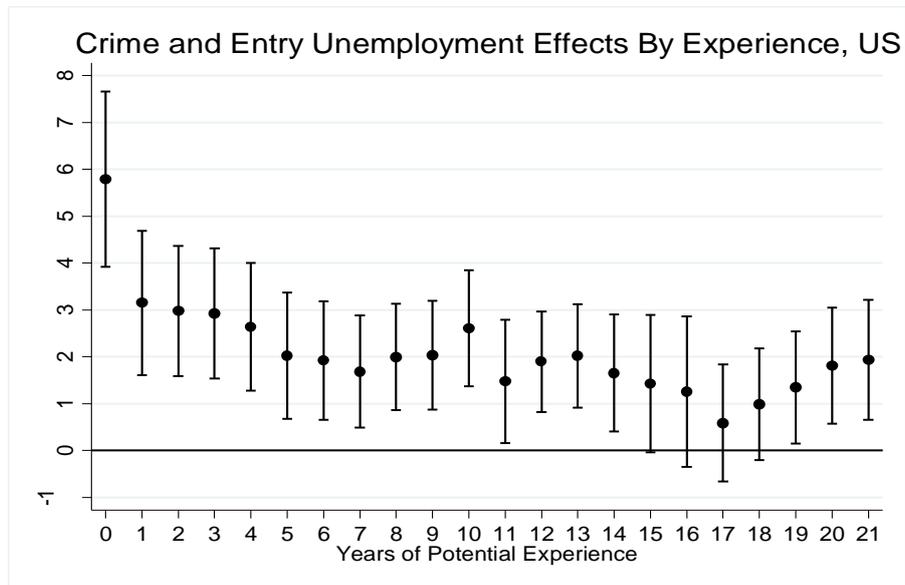
Notes: Derived from specification comparable to that in column (1) of Table 3, with separate estimates for each year of experience.

Figure 2: Entry Unemployment Effects By Experience, UK

Crime and Entry Unemployment Effects By Experience, UK
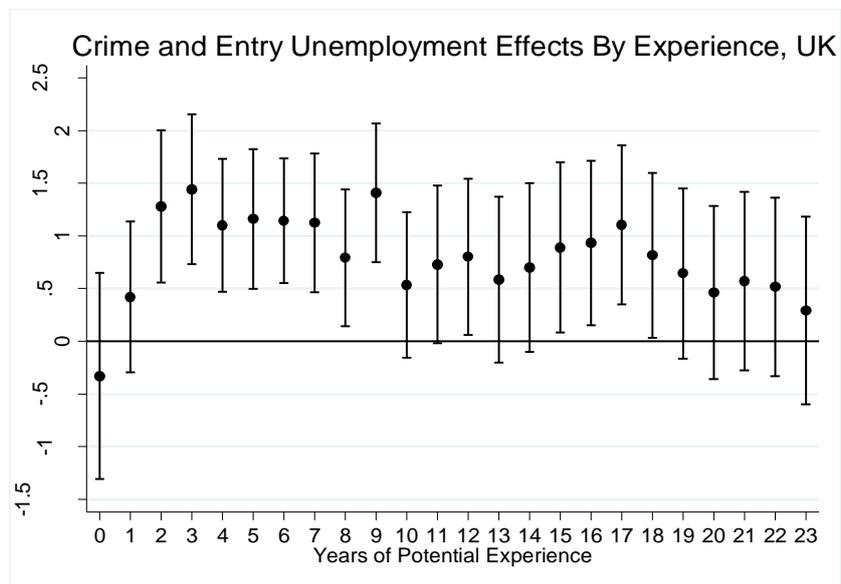
Notes: Derived from specification comparable to that in column (1) of Table 4, with separate estimates for each year of experience.

Table 1: US Cohort Panel Estimates, Basic Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Crime Type:	All	All	Property	Property	Violent	Violent

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)
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)

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)
Mobility Adjusted State Entry U Rate at Age 16-18		2.470***		2.016**		3.288***
		(0.609)		(0.771)		(0.776)

	(1)	(2)	(3)	(4)	(5)	(6)
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 labor 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 labor 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 labor market entry and include a cohort quadratic trend. Columns (2), (4) and (6) include cohort-level state unemployment rates and include cohort fixed effects. Standard errors in parentheses 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: UK Cohort Panel Estimates, Basic Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Crime Type:	All	All	Property	Property	Violent	Violent

Panel A

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		0.811***		0.712**		1.531***
		(0.277)		(0.350)		(0.365)

Panel B

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	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 labor market at age 15/16. All unemployment rates are measured in year of labor 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 labor market entry and include a cohort quadratic trend. Columns (2), (4) and (6) include cohort-level region unemployment rates and include cohort fixed effects. Standard errors in parentheses 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 3: US Cohort Panel Estimates, Effects by Labor 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 Panel A, 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: UK Cohort Panel Estimates, Effects by Labor Market Experience Groups

	(1)	(2)	(3)	(4)
	All	All	Property	Violent
Region Entry U Rate at Age 16*Exp(0-5)	0.861***	0.967***	0.972***	1.034*
	(0.305)	(0.316)	(0.362)	(0.532)
Region Entry U Rate at Age 16*Exp(6-11)	0.913***	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**	0.809**	0.733	1.369***
	(0.343)	(0.358)	(0.448)	(0.435)
Region Entry U Rate at Age 16*Exp(18-23)	0.582	0.529	0.124	2.701***
	(0.369)	(0.405)	(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 Panel A, Table 2.
* indicates significance at the 10 percent level, ** indicates significance at the 5 percent level, *** indicates significance at the 1 percent level.

Table 5: 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	0.038**	0.117*	0.023	-0.002
	(0.018)	(0.063)	(0.030)	(0.008)
B. Aged 18 And Over, 1980 Redefined				
State Entry U Rate at Age 16-18	0.035*	0.130**	0.016	-0.007
	(0.018)	(0.062)	(0.030)	(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	5,759,537	798,546	2,552,973	1,164,030

Notes: Table reports estimated marginal effects from a probit where 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 in parentheses 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 6: UK Self Report Arrest Regressions, 2001/2 to 2010/11

	(1)	(2)	(3)
	Ever Arrested	Ever Arrested, Age 16 Qualification	Ever Arrested, Age 18+ Qualification
Region Entry U Rate at Age 16	0.259**	0.543***	0.055
	(0.125)	(0.166)	(0.207)
Year Dummies	x	x	x
Personal Controls	x	x	x
Mean of Dependent Variable	0.216	0.288	0.153
Sample Size	22,281	7,849	9,006

Notes: Table reports estimated marginal effects from a probit where the dependent variable is a dummy equal to 1 if the individual reports having ever been arrested and 0 otherwise. 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.

Appendix 1: Additional Figures and Tables

Figure A1a: Male Offender Rates by Age, US

Male Crime Age Profiles, US, 2000-2010
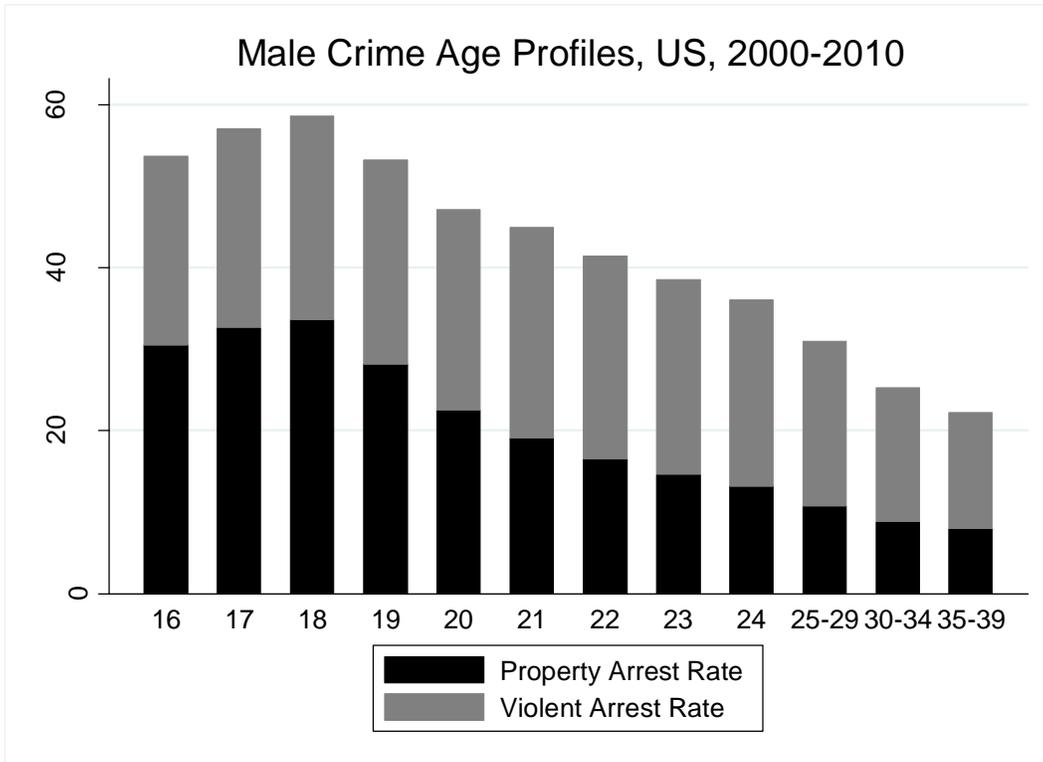
Notes: Male arrest rates by age, calculated from UCR data (see Data Appendix for more detail).

Figure A1b: Male Offender Rates by Age, UK
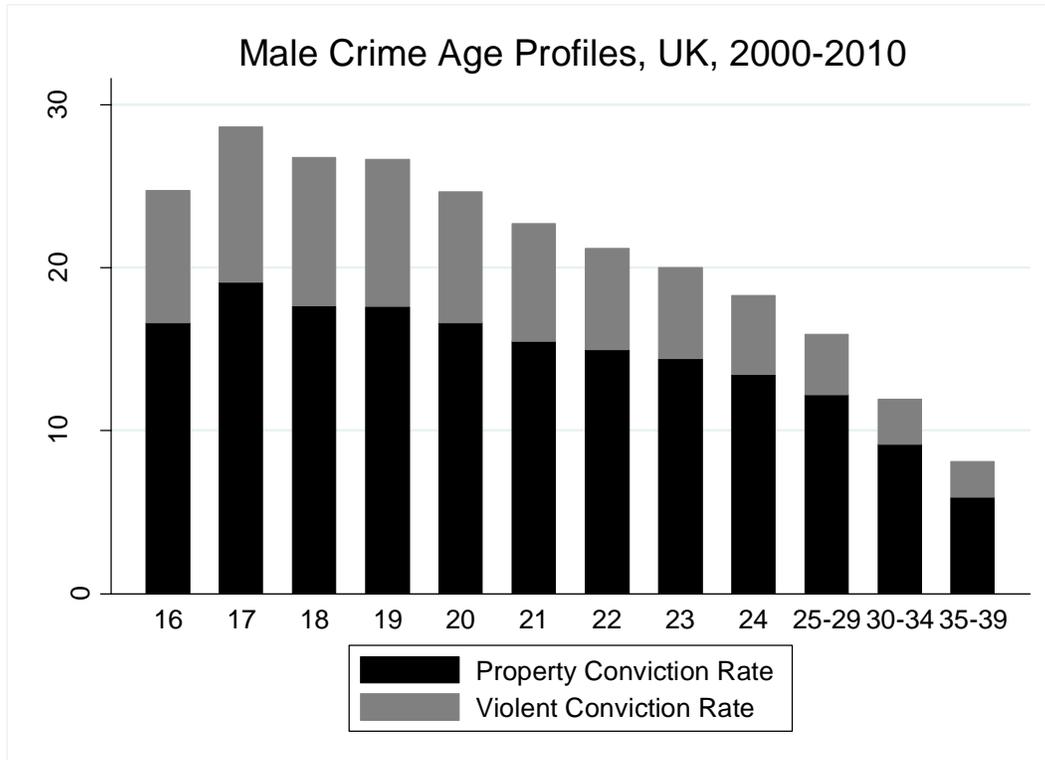
Notes: Male conviction rates by age, calculated from OID/PNC data (see Data Appendix for more detail).

Figure A2: Autocovariance Structure of Unemployment Rates, US

Autocovariances of Unemployment Rates, US
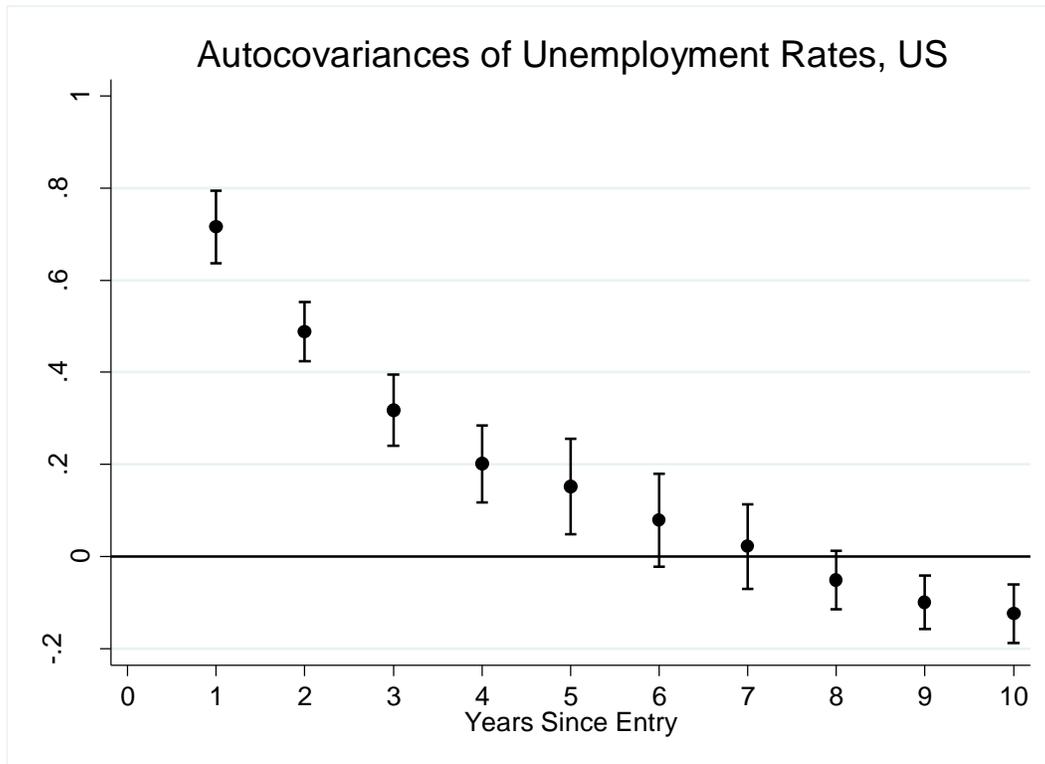
Figure A3: Autocovariance Structure of Unemployment Rates, UK

Autocovariances of Unemployment Rates, UK
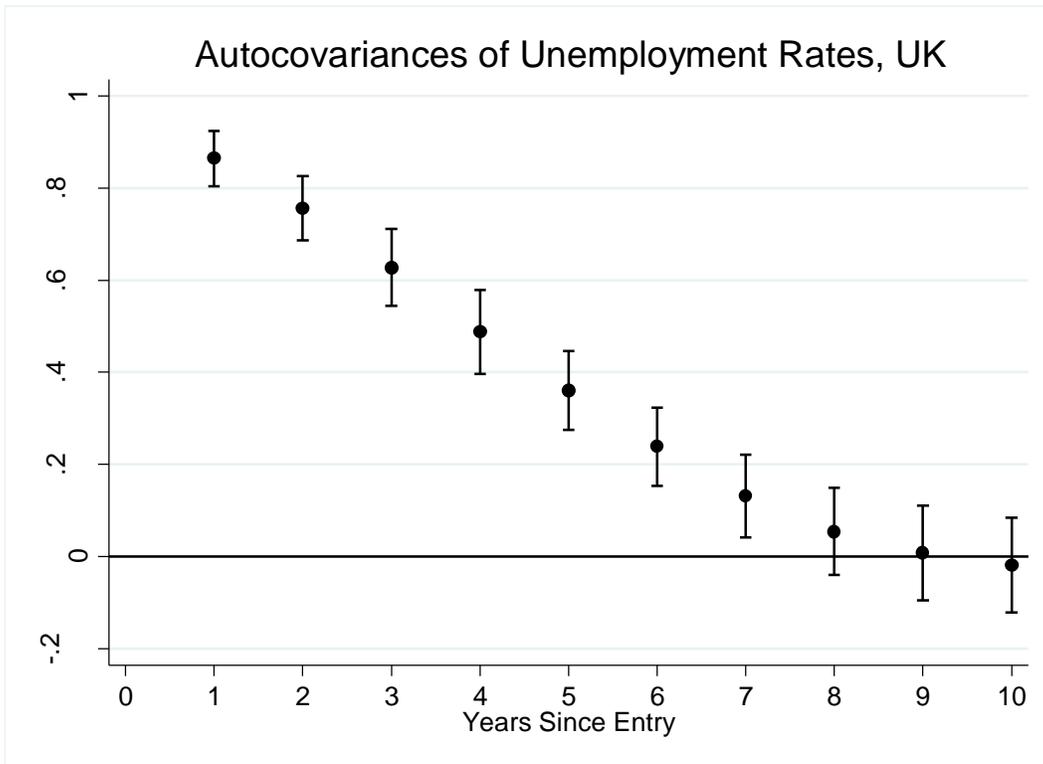
Figure A4: Entry Unemployment Effects By Experience Controlling for Subsequent Unemployment-Experience Interactions, US

Crime and Entry Unemployment Effects By Experience, US
(Controlling for Subsequent Unemployment-Experience Interactions)
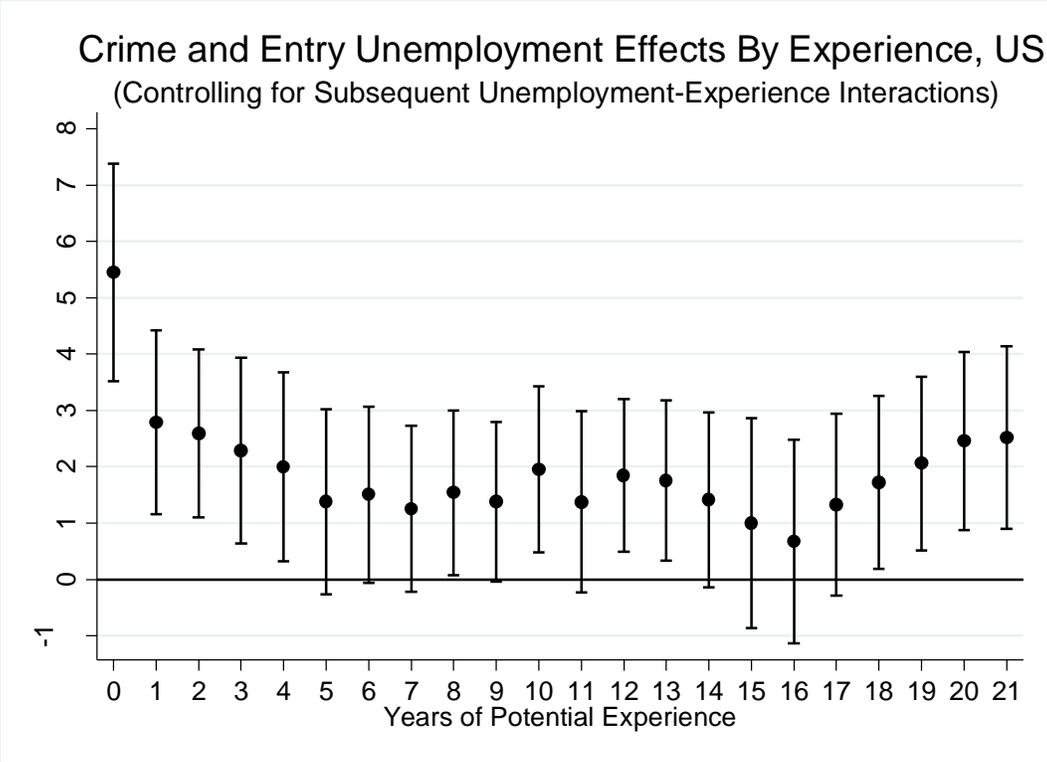
Notes: Derived from specification comparable to that in column (2) of Table 3, with separate estimates for each year of experience.

Crime and Entry Unemployment Effects By Experience, UK
(Controlling for Subsequent Unemployment-Experience Interactions)
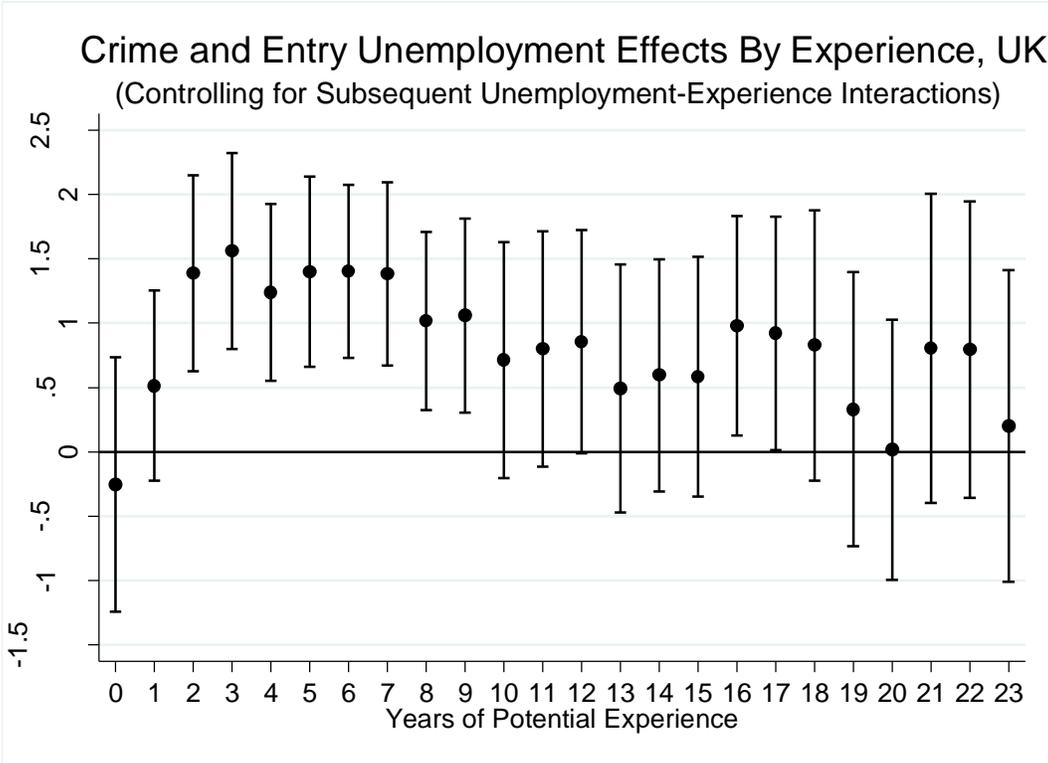
Notes: Derived from specification comparable to that in column (2) of Table 4, with separate estimates for each year of experience.

Table A1: US Baseline Results by Detailed Crime Type

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Offence:	Burglary	Theft	Arson	Murder	Rape	Robbery	Assault

A: Basic Specification

|	(1)	(2)	(3)	(4)	(5)	(6)	(7)
State Entry U Rate at Age 16-18	2.977***	3.589***	4.692***	-3.094***	3.393***	2.225***	2.106***
	(0.652)	(0.759)	(0.735)	(1.164)	(0.627)	(0.759)	(0.638)

B: By Labour Market Experience Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
State Entry U Rate at Age 16-18*Exp(0-5)	-0.639	3.809***	6.512***	-3.157*	4.093***	1.516	4.385***
	(0.838)	(0.889)	(1.085)	(1.704)	(1.042)	(1.079)	(1.329)
State Entry U Rate at Age 16-18*Exp(6-11)	0.101	2.644***	5.177***	-0.647	5.208***	1.587	2.366**
	(0.781)	(0.952)	(1.098)	(1.746)	(0.922)	(1.067)	(0.942)
State Entry U Rate at Age 16-18*Exp(12-17)	3.667***	3.858***	1.836*	-3.661***	0.845	1.305	0.945
	(0.882)	(1.110)	(0.989)	(1.410)	(0.919)	(1.135)	(0.804)
State Entry U Rate at Age 16-18*Exp(18-21)	5.392***	5.202***	5.561***	-0.851	0.063	3.541***	2.212**
	(0.961)	(1.109)	(1.078)	(1.489)	(1.076)	(1.186)	(0.899)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
State Fixed Effects	x	x	x	x	x	x	x
Year Fixed Effects	x	x	x	x	x	x	x
Cohort Fixed Effects	x	x	x	x	x	x	x
Age Fixed Effects	x	x	x	x	x	x	x
Compositional Adjustment	x	x	x	x	x	x	x
Sample Size Panel B	19,429	19,429	19,429	19,429	19,429	19,429	19,429

Notes: For Panel A as for columns (2), (4) and (6) specifications in Table 1; for Panel B as for columns (2)-(4) specifications in Table 3 (allowing for subsequent U rates).

* indicates significance at the 10 percent level.** indicates significance at the 5 percent level.*** indicates significance at the 1 percent level.

Table A2: 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***		2.385***
		(0.443)		(0.496)
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)			0.000***	
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 A3: 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	0.811***		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			-0.227	
			(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
			(0.218)	(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 2.
* indicates significance at the 10 percent level, ** indicates significance at the 5 percent level, *** indicates significance at the 1 percent level.

Table A4: 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. 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

Property crime:

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

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03 = Robbery	09 = Arson
04 = Aggravated assault	
08 = Other assaults	

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.

A2. 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 A4 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 A4. 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).

A3. Entry Unemployment Rates

We assume the age of labor 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 labor 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 Labor, 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. 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.

B2. 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.

B3. Entry Unemployment Rates

Year of labor 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.