

# Youth Crime and Education Expansion

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## Abstract

We present new evidence on the causal impact of education on crime, by considering a large expansion of the UK post-compulsory education system that occurred in the late 1980s and early 1990s. The education expansion raised education levels across the whole education distribution and, in particular for our analysis, at the bottom end. At the same time, youth crime fell, revealing a significant cross-cohort relationship between crime and education. The causal effect of education on crime is estimated to be negative and significant, and considerably bigger in (absolute) magnitude than that implied by least squares estimates. The education boost also significantly impacted other productivity related economic variables (qualification attainment, wages, unemployment), demonstrating that the incapacitation effect of time spent in school is not the sole driver of the results, in that crime was also reduced via productivity gains for individuals affected by the education expansion.

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## 1. Introduction

A small, but growing, economic literature has presented empirical evidence showing a negative causal impact of education on crime. For the US, Lochner and Moretti (2004) use variations in compulsory school leaving age laws across states to identify the impact of education on crime, and Machin, Marie and Vujić (2011) exploit the raising of the school leaving age across cohorts in England and Wales to do the same. Both studies uncover a robust and sizable crime reducing effect of education.<sup>1</sup>

In this paper, we also consider the causal impact of education on crime, but adopt a different route to identification. We present new evidence of the causal relationship, by studying what happened to crime in a period when the UK post-compulsory education system was very rapidly expanded. This large expansion occurred in the late 1980s and early 1990s and significantly raised education levels across the whole education distribution, thereby significantly reducing the number of individuals with low education levels in birth cohorts exposed to the expansion. Our approach is to think of these cohorts as a ‘treated’ set of individuals whose education was raised and we can compare their education and crime outcomes with a ‘control’ set of cohorts who did not benefit from the expansion.

The education expansion we consider has been studied in other areas. Blanden and Machin’s (2004) study focuses on the expansion as a key driver of falling intergenerational mobility in Britain. Devereux and Fan (2011) have also looked at wage effects associated with the education expansion, showing that on average it caused men and women to gain respectively a year or slightly more than a year of education and that this significantly raised wages. By considering the education expansion as an exogenous increase in educational attainment, they present causal

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<sup>1</sup> See also Lochner’s (2011) review of empirical work on the non-wage benefits of education where part of his focus is to review evidence on the crime-education relationship.

estimates of the wage return to an extra year of schooling of about six percent for men and women.

To preview our results, we also report significant improvements in education levels for cohorts affected by the education expansion. Given our focus on crime, it is important that the education improvements occurred at the bottom end of the education distribution, as this is where its impact is most likely to affect offending behaviour. We spend some time in our empirical analysis showing this to be the case. We then consider what happened to crime for the treated cohorts relative to the controls and we show reduced form evidence that crime fell significantly as educational attainment rose.

We present causal estimates in an instrumental variable setting, where a 1 percent increase in the proportion of male students reduces male crime by around 1.6 percent and a 1 percent increase in the proportion of men staying on at school after the compulsory school leaving age reduces male crime by around 1.5 percent. Whilst going in the same direction, results for women are much smaller and rather imprecisely determined. For young men, we also find that education causally reduces both property and violent crimes.

Finally, we look at mechanisms underpinning the crime-education relation and present evidence that the education boost from expansion also significantly impacted on other productivity related economic variables (qualification attainment, wages, and unemployment). We interpret this as showing that keeping people in the education system (incapacitation) is not the sole driver of the results, because the education boost is also associated with better, productivity raising, economic outcomes.

The rest of the paper is organised as follows. Section 2 offers a brief discussion of the relevant literature, with an emphasis on the mechanisms behind the education-

crime relationship and their implications for our empirical analysis. Section 3 describes the nature of the education expansion and the data sources we use. Section 4 first shows the impact of the education expansion on the education distribution, before moving on to report results from a causal analysis of the crime and education relationship. In Section 5, we explore potential mechanisms underpinning the causal crime-education relation by also looking at other economic effects of the education expansion. Section 6 concludes.

## 2. Education and Crime: Mechanisms and Implications

### *Mechanisms*

There are number of theoretical reasons why education can impact on crime. Existing literature highlights (at least) three main channels through which schooling might affect criminal participation: income effects, patience or risk aversion, and time availability. For most crimes, one would expect that these factors induce a negative effect of schooling on crime, although ultimately this ends up being an empirical question.

We briefly consider each of these three mechanisms in turn:

(i) Income effects operate through education increasing the returns to legitimate work and/or raising the opportunity costs of illegal behaviour (Lochner, 2004; Lochner and Moretti, 2004; Hjalmarsson, 2008). A body of empirical work connecting crime to wages broadly supports this notion (Gould et al., 2002; Grogger, 1998; Machin and Meghir, 2004), although there is some counter evidence that education can also increase the earnings from crime as certain skills acquired in school may be inappropriately used for criminal activities. For example, Levitt and Lochner (2001) find that males with higher scores on mechanical information tests had increased

offence rates and Lochner (2004) reports some cross-cohort evidence that increases in average education are associated with higher white-collar arrest rates (although this is not statistically significant).

(ii) Patience and risk aversion may also offer a channel through which education may impact on crime. The idea here is that individuals with a lot of patience have low discount rates and value future earnings more highly as compared to those with high discount rates. Oreopoulos (2007) concludes that young people who drop out of school tend to be myopic and more focussed on immediate costs from schooling (stress from taking tests, uninteresting curricula, foregone earnings, etc.), rather than on future gains from an additional year of schooling. Thus adolescents lack abstract reasoning skills and are more predisposed to risky behaviour. Education can increase patience, which reduces the discount rate of future earnings, and hence reduces the propensity to commit crimes. Education may also increase risk aversion that, in turn, increases the weight given by individuals to a possible punishment and consequently reduces the likelihood of committing crimes.

(iii) Time spent in education may also be important for teenagers in terms of limiting the time available for participating in criminal activity. This 'self-incapacitation' effect was documented by Tauchen et al. (1994) who found that time spent at school (and work) during a year is negatively correlated with the probability of arrest that year. Hjalmarsson (2008) looked at the opposite relationship, studying the impact of being arrested and incarcerated before finishing school on the probability of graduating high school. Her results suggest that the more times you are caught committing crime and the amount of time spent in prison both greatly increase the likelihood of becoming a high school dropout.

As these still may be endogenous decisions, Jacob and Lefgren (2003) instrument days off school with exogenous teacher training days and Luallen (2006) uses unexpected school closings driven by teacher strikes as an instrument for student absence from school. Both papers find important incapacitation effects of education on criminal participation. However, they also report that violent offences increase while school is in session, a finding that is attributed to a concentration effect.<sup>2</sup> Anderson (2009) also reports evidence for United States (US), based on minimum high school dropout ages that vary across states, in line with the notion that keeping youth in school decreases arrest rates.

### *Implications*

After obtaining estimates of the impact of education expansion on crime, an issue we will face is to interpret them in view of these potential mechanisms that underpin the crime-education relation. This is important here as the outcome variable we will focus on measures convictions of individuals aged 16 to 21 and the education expansion we study substantially increased the educational participation of this age group.<sup>3</sup> Our estimates will therefore have a component that could be attributed to the self-incapacitation effect due to certain cohorts of youths spending more time at school. It is hard to totally rule out an incapacitation effect, but we will present evidence that try to establish that this may not be the sole explanation of any crime reductions that result from education expansion.

A first attempt to consider this comes about because of differences between our approach and that of the other causal studies. Lochner and Moretti (2004) for the US

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<sup>2</sup> The term concentration effect refers to the presence of a large number of youths in an educational establishment, which may result in an increased probability of violent encounters.

<sup>3</sup> The chosen age group is partly due to data limitations. Namely, the Offenders Index Database (described below) is only available until 2002 and we therefore chose to limit our analysis to the age group 16 to 21 in order to have a relatively balanced panel and enough cohorts around the ones treated by the policy. See the Data Appendix for details on the structure of the OID.

and Machin, Marie, and Vujić (2011) – MMV (2011) henceforth – for England and Wales exploit changes in compulsory school leaving age laws to identify the causal impact of an extra year of schooling on criminal participation. The cohort level approaches of these papers is methodologically akin to the one we will adopt, but both studies are able to mostly rule out any direct self-incapacitation effect of education on crime. They do so by excluding the age group affected by the extra school year brought about by the policies studied from the arrest or conviction data used. The findings from these papers are very similar and point to a 1 percent drop in male property crime for every 1 percent of a male cohort obtaining some educational qualification as a result of the reforms. They both find no effect on female offending rates. The impact on male violent crime is less conclusive and while Lochner and Moretti (2004) obtain relatively large but imprecise estimates, MMV (2011) do not find a significant effect of education on this outcome.

Thus, a first simple way to attempt and disentangle which mechanism is driving our results will be to benchmark our findings to those of MMV (2011). As their results are ‘net’ of self-incapacitation effects, we could expect a stronger effect on male property convictions in our context and tentatively attribute some of the difference directly to the extra time spent at school. Perhaps more relevant will be the interpretation of a potential effect on male violent offences of education. Economic theory does not give much support to the existence of an income effect mechanism for this type of criminal behaviour and this is why MMV (2011) argued that it was not surprising they found no impact of education on violent offending. There is, however, no reason to assume that violent offending is not affected via the self-incapacitation

channel.<sup>4</sup> We could therefore also tentatively interpret a connection between young male violent convictions and education as stemming from this mechanism.

A second possible route to investigate the underlying mechanisms of an education impact on crime is to consider whether the education boost from expansion impacted other productivity related outcomes. If we uncover evidence of this, then we can plausibly rule out that the incapacitation effect is the sole explanation of a crime reducing effect of education. For example, if part of the changes in youth crime can be attributed to an income impact of education expansion, this would be reflected in differences in productivity related measures such as higher qualification attainment, wages, and unemployment across treated and non-treated cohorts.

We will therefore also estimate the causal impact of education increases induced by the expansion on these economic outcomes. This has similarities to the analysis of wage returns to education expansion carried out by Devereux and Fan (2011), but we will extend this to consider a wider set of outcomes than just wages. Devereux and Fan (2011) report significant wage returns associated with education expansion, though they focus on older individuals (aged 25 to 50) than we will do and use a smaller number of years of Labour Force Survey data (see the discussion below and in the Data Appendix).

### 3. Expansion of the UK Post-Compulsory Education System

The post-compulsory education system in the UK has hugely expanded since the 1960s. Figure 1 shows two post-compulsory education participation series between 1960 and 2000: (a) the rate of staying on beyond the compulsory school leaving age,

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<sup>4</sup> For example, Sabates (2010) and Sabates and Feinstein (2008) find evidence of reduced convictions for both ‘antisocial behaviour’ and burglary when studying a localised post-compulsory schooling incentivization scheme in a British context. These papers do not however investigate which potential mechanisms are driving these results.



and (b) the higher education age participation index (API).<sup>5</sup> The Figure shows sharp increases in both series from 1960 onwards.

A close look at Figure 1 reveals that the staying on series appears to have been on a steadily increasing path (although is subject to cyclical variations) from the start of the series through to the mid-1980s. However, from the late 1980s/early 1990s there appears to be a step-change as staying on rates rise much faster, before plateauing out in the mid 1990s.

The increase in university participation is also very rapid, as shown by the API. There was a sharp expansion in the 1960s, when the age participation index doubled from 6 to 14 percent. It then rose marginally from this level through until the late 1980s, after which it grew even more rapidly than the 1960s change. By 1998 it had reached 34 percent, rising up from under 20 percent at the start of the 1990s.

Figure 1 makes it clear that a very rapid education expansion took place in the late 1980s/early 1990s in the UK, and that it occurred all through the education distribution. The fact that the staying on rate rose, and hence there were fewer people leaving school at the compulsory school leaving age of 16, is important and we will probe the nature and scale of this change in more detail with available micro-data.

It is also worth reflecting on why the expansion of post-compulsory schooling took place. Blanden and Machin (2004) emphasise two main factors. For the expansion of the post-compulsory sector through increased staying on rates, they highlight the change in the school leaving examination system that took place in 1988, with the introduction of the General Certificate of Secondary Education (GCSE) and

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<sup>5</sup> The API measures the proportion of young people entering full-time higher education.

the consequent improvement in examination results.<sup>6</sup> For HE participation, they discuss increased university enrolment in the light of changes in admissions and in financing, together with the perception of increased wage returns to HE facilitated by very fast rises in the wage returns to a degree that occurred through the 1980s, as a key part of a general rise in wage inequality (see, inter alia, Machin and Vignoles, 2005).

We must finally note that there is no evidence, to our knowledge, that the political decision to expand educational attainment was motivated as a response to particular trends in youth crime rates. It was also not specifically combined with other crime fighting policies aimed at certain age groups or cohorts, thus making it a good potential instrument to study the causal impact of education on offending.

#### *Data Description*

To study the impact of this education expansion on educational attainment and crime there are a number of pertinent data issues that need to be discussed. The key issue we face is that no single data source exists which contains crime and education data such that we can follow cohorts of individuals over time. We thus need some means of matching crime and education data from different sources.

To do so we match convictions data from England and Wales in the Offenders Index Database (OID) to education data from the Labour Force Survey (LFS). The OID contains criminal history data for offenders convicted of standard list offences from 1963 and up to 2002.<sup>7</sup> The data (which are described in more detail in the Data Appendix) are derived from the court appearances system and are updated quarterly. The Index was created purely for research and statistical analysis. Its main purpose is

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<sup>6</sup> The GCSE examinations are taken at the end of the last year of compulsory schooling (at age 16). Therefore the first affected cohort by this change was the cohort of students born in 1972. This will be important to keep in mind when defining treated cohorts for our identification below.

<sup>7</sup> Standard list offences are all indictable or triable offences, plus a few of the more serious summary offences.

to provide full criminal history data on a randomly selected sample of offenders. We have access to OID data on anonymous samples for offenders sentenced during four weeks each year. We also have the entire pre- and post-court appearance history of these individuals. However, there is no information on a defendant's education level in the OID and so the data needs to be aggregated and matched to education data from other sources. A big advantage (certainly relative to recorded offences data) is that some demographic characteristics are available in the OID, notably age and gender.

We therefore calculated offending rates (per 10,000 population) using Office for National Statistics (ONS) population data by birth cohort and year, separately for men and women. In doing so, criminal offences were also broadly categorised as property crimes (burglary and theft and handling stolen goods) and violent crimes (violence against the person and robbery). These offending rates can be matched to education data from other micro-data sources where education measures can be collapsed into birth cohort by year and gender cells.

We constructed education measures for the same birth cohort by year and gender cells in the LFS. The Data Appendix describes more fully how we matched the OID and LFS data for the main analysis in this paper. Our focus is on youth crime and so our sample consists of year of birth cohorts aged 16 to 21 born between 1962 and 1982 from OID and LFS data across the 1978 to 2002 time period.<sup>8</sup> This ensures the panel of cohorts is relatively well balanced, especially around the years of the policy intervention (see the Data Appendix for more details).

We focus upon two education measures:

i) the proportion of 16 to 21 year olds in full time education;

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<sup>8</sup> In the early years of the LFS, the survey was biannual (in 1975, 1977, 1979, 1981 and 1983), after which it became annual. Therefore, we imputed values for missing years by cohort (see the Data Appendix).

ii) the proportion of 16 to 21 year olds who stayed on after the compulsory school leaving age (of 16).

We look at the relationship between youth crime and these education measures, using the education expansion as an exogenous shock that we can exploit in order to identify the causal impact of education on youth crime. For this to be a legitimate exercise, we need to show that the expansion which, as we argued above, was driven by the reform of the school leaving exams (the move to GCSE) and a subsequent rise in higher education participation, raised the education measures that we look at.

Figure 2 shows the cross-cohort evolution of the two education measures defined above. The upper Figure shows the proportion of the cohort still in full-time education and the lower Figure shows the proportion staying on. In each of these, the dotted line is for women and the solid line for men. A very clear pattern is present for both measures. For the 1962 to 1971 birth cohorts the education measures slowly rise (more so for women than men). The next four birth cohorts (1972 to 1975) show very rapid increases. The 1972 birth cohort was the first to take the GCSE exams in 1988. Thereafter, for cohorts born in 1976 and after, the education measures plateau out at a higher level.

Thus, it seems that the education expansion occurred very strongly for the 1972 to 1975 birth cohorts. Indeed, Blanden and Machin (2004) and Devereux and Fan (2011) show that this increase also extended to higher education participation once these cohorts became old enough. Our interest is in whether the expansion impacted on youth crime. This is considered in Figure 3, which shows OID conviction rates by gender (measured as convictions per 10,000 men or women) for the 1962 to 1982 birth cohorts. The solid line shows that male convictions rose for the 1962 to 1971 cohorts, but fell very sharply for the education expansion cohorts, and fell more slowly after

that. The faster fall for the 1972 to 1975 cohorts is suggestive of a crime fall happening because of the expansion. For women, as shown by the dotted line, the conviction rates are much noisier (owing to there being far fewer female convictions), but a relatively similar pattern emerges.

For men, we have big enough sample sizes to also break down convictions in those for property and violent crimes. This is done in Figure 4. The sharp reduction in conviction rates is seen for both crime types. Their evolutions are slightly different in that violent convictions seem to rise again after the education expansion period but, based on the descriptive analysis, the notion that education increased rapidly and that convictions fell at the same time seems to be the case. In the next Section of the paper, we model this more formally, and use the variation in education induced by the expansion to present estimates of the causal impact of education on youth crime.

#### 4. Statistical Estimates of the Crime-Education Relationship

##### *Reduced and Structural Forms*

We begin our statistical analysis by considering education and crime reduced form models. Formally, these can be represented as follows:

$$\begin{aligned} ED_{at} &= \alpha_0 + \alpha_1 EE_{at} + \sum_{j=1}^J (\varphi_{1j} COH_{at}^j + \varphi_{2j} AGE_{at}^j) + v_{at} \\ C_{at} &= \beta_0 + \beta_1 EE_{at} + \sum_{j=1}^J (\lambda_{1j} COH_{at}^j + \lambda_{2j} AGE_{at}^j) + \omega_{at} \end{aligned} \quad (1)$$

where ED is education, C is crime, EE denotes the education expansion cohorts, where each reduced form equation contains a  $J^{\text{th}}$  order polynomial in birth cohort (COH) and age (AGE) - in most models below we include a quadratic ( $J=2$ ) effect in cohort and a cubic ( $J=3$ ) effect in age - and  $v$  and  $\omega$  are equation error terms.

The reduced form parameters in (1) are related to the parameters of the following crime structural form that models the causal crime-education relation:

$$C_{at} = \theta_0 + \theta_1 ED_{at} + \sum_{j=1}^J (\gamma_{1j} COH_{at}^j + \gamma_{2j} AGE_{at}^j) + \varepsilon_{at} \quad (2)$$

where the instrumental variable/two stage least squares (IV/2SLS) estimate of the coefficient on the education variable in (2) is the ratio of the reduced form coefficients in (1),  $\theta_1 = \beta_1/\alpha_1$ .

### *Reduced Form Estimates*

Table 1 shows the reduced form estimates (equation (1)) for the two education variables and for the total conviction rate for male and female cohorts. The education expansion cohorts (EE in (1)) are modelled through the inclusion of four cohort dummies, for birth cohorts 1972 through 1975. Each specification also includes a post-EE cohort dummy to pick up the plateauing out, shown in the earlier education Figures. The cohort effects are therefore estimated relative to cohorts born in 1971 or before (the pre-GCSE examination period).

There are several interesting features of the results. Consider those for men first. The estimated coefficients on the cohort dummies in the education equations show significant step changes upwards, going from 0.049 to 0.145 from the 1972 to 1975 cohort, for the proportion of students in the cohort, and then plateauing out at 0.157. The same kind of pattern, going from 0.041 to 0.173 and plateauing out at 0.200 is seen for the staying on at age 16 variable. Moreover, as the  $F$ -test in the Table shows, these cohort dummies are strongly significant with  $F$ -statistics above 10 (and associated  $p$ -values of 0.00). This supports use of the cohort dummy variables as instruments in the causal estimations that we will discuss in due course.

The conviction rate reduced form also shows a step change pattern, but this time the estimated coefficients on the cohort dummies are negative, going from -0.045 to -0.229, and again showing the plateauing out feature with an estimated coefficient of -0.214 on the post-EE cohort dummy variable. Similarly, the  $F$ -statistic testing for the exclusion of the 1972 through 1975 cohort dummies is strongly significant, with an  $F$ -statistic of 12.06 (and associated  $p$ -value of 0.00). We can therefore conclude that for men education significantly rose amongst the EE cohorts, whilst their conviction rate fell at the same time.

For women, the pattern is qualitatively similar. There is evidence of strong educational improvements, again with significant  $F$ -statistics testing the joint significance of the EE cohort dummies. However, the crime reduced form is much weaker than the male one and the hypothesis of joint insignificance of the estimated treated cohort effects can no longer be rejected. As discussed earlier, this probably reflects the more infrequent occurrence of female convictions and thus the noisier data.

#### *Structural Form Estimates*

Table 2 shows ordinary least squares (OLS) estimates of the crime-education association and the causal 2SLS structural form estimates (of equation (2)) for male and female cohorts. The OLS estimates reveal a negative association between the conviction rate and both education measures for men and women, though the estimated coefficients are not significant for women. The causal 2SLS estimates (using the EE cohort dummies as instruments for the education variables) are greater in (absolute) magnitude in all cases, but are imprecisely determined and statistically insignificant for women. Thus, we are able to identify a large and strongly significant causal crime reducing effect of education on young males working through the education expansion induced by the reform of the examination system.

### *Property and Violent Crimes*

In the existing literature, when crimes are broken down into property and violent crimes, there is some disagreement about whether one can identify a causal effect for both (for example, Lochner and Moretti's, 2004, US work reports evidence of both, whereas the England and Wales analysis of MMV, 2011, only reports a significant effect for property crimes). In Table 3 we therefore consider property crime convictions and violent crime convictions separately. These are considered for young men only, because of the low frequency of female violent crime convictions and the insignificance of the results for women reported in Table 2.

Consider first the crime reduced forms in the left panel of Table 3. For both property and violent crime convictions we see the same kind of pattern as before, with there being a step change and a plateauing out. The  $F$ -statistics of joint significance are stronger for property than violent crimes.

Given these observations, it is not surprising that we identify a significant causal effect of education on property and violent crime convictions amongst young men. The 2SLS estimates in the right panel of Table 3 are again large in absolute magnitude, that is, about 3 to 4 times larger than the corresponding OLS estimates. Thus, for young men, our estimates uncover a causal crime reducing effect of education for both property and violent crime convictions.

### 5. Effects on Other Economic Outcomes

In Section 2 of the paper, we argued that looking at the impact of education on other productivity related outcomes gives potential to rule out the notion that the only mechanism at work in explaining the causal impact of education on crime is an incapacitation effect. The stronger impacts on male property and violent crimes than



found by MMV (2011) do suggest that increased time spent at school had a direct impact on criminal behaviour. However, in order to argue that it was the only channel at play, we need to investigate if education expansion had an impact on other outcomes (principally working through what we labelled the income effect earlier in the paper). To do so, we consider the causal impact of education on productivity related outcomes for our treated individuals later in life (when aged 25 to 30) once the vast majority have completed their education. The three outcomes we consider are: qualification attainment, wages and unemployment.

In this Section, we are not forced to carry out a cohort level analysis since we observe both the education variables and the productivity related outcomes at individual level in the Labour Force Survey. Thus we can carry out individual analysis of the relationship between these outcomes and the education variables affected by the education expansion reform. We consider two education variables we can measure at the individual level: a) whether the individual stayed on at school after the compulsory school leaving age of 16; and b) age left full-time education.

The upper panel of Table 4 shows reduced form estimates for the two education variables and for qualification attainment, wages (weekly and hourly), and unemployment. As with the earlier, cohort based analysis, the education variables are strongly related to the 1972 to 1975 cohort dummies, with the same step-up pattern across cohorts, and then a plateauing out, being observed. Again the *F*-tests of joint significance of the cohort dummies are sizable.

There are also strong, similarly structured, reduced forms for qualification attainment (here measured as whether an individual only achieves a low qualification - see the Data Appendix for precise details) and wages (both weekly and hourly). The probability of achieving a low qualification falls across the cohorts and then plateaus

out, whilst wages significantly improve. The unemployment reduced form is suggestive of modest unemployment reductions, but statistically the results are much weaker.

The lower panel of Table 4 shows OLS and 2SLS estimates of the relationship between the productivity related outcomes of interest and the education variables where (in the case of 2SLS) the education expansion cohort dummies are used as instruments. We identify a causal impact of increased education due to the expansion that reduces the probability of only getting a low qualification and boosts weekly and hourly wages. The unemployment 2SLS effects, not surprisingly in the light of the weaker reduced form, are not statistically significant.

It is interesting that the 2SLS estimates for qualification attainment and wages, as with the earlier crime results, are larger in (absolute) magnitude than the least squares results. We read these as showing a productivity enhancing effect of the extra time spent in education because of the education expansion. Thus, we conclude that this productivity increasing aspect of the education expansion means that we can rule out the notion that the only mechanism underpinning the significant crime reducing impact of education is incapacitation.

## 6. Conclusions

In this paper, we revisit the small, but growing, economic literature on the causal crime reducing effect of education. We adopt a different route to identification compared to the papers using raisings of the compulsory school leaving age. Instead, we study an expansion of the post-compulsory education system that occurred in the UK for cohorts born between 1972 and 1975 who faced a change in the school leaving examination system in 1988 when they reached the compulsory school leaving age.

This reform significantly expanded the number of individuals who participated in post-compulsory education as full-time students who stayed on after the compulsory school leaving age.

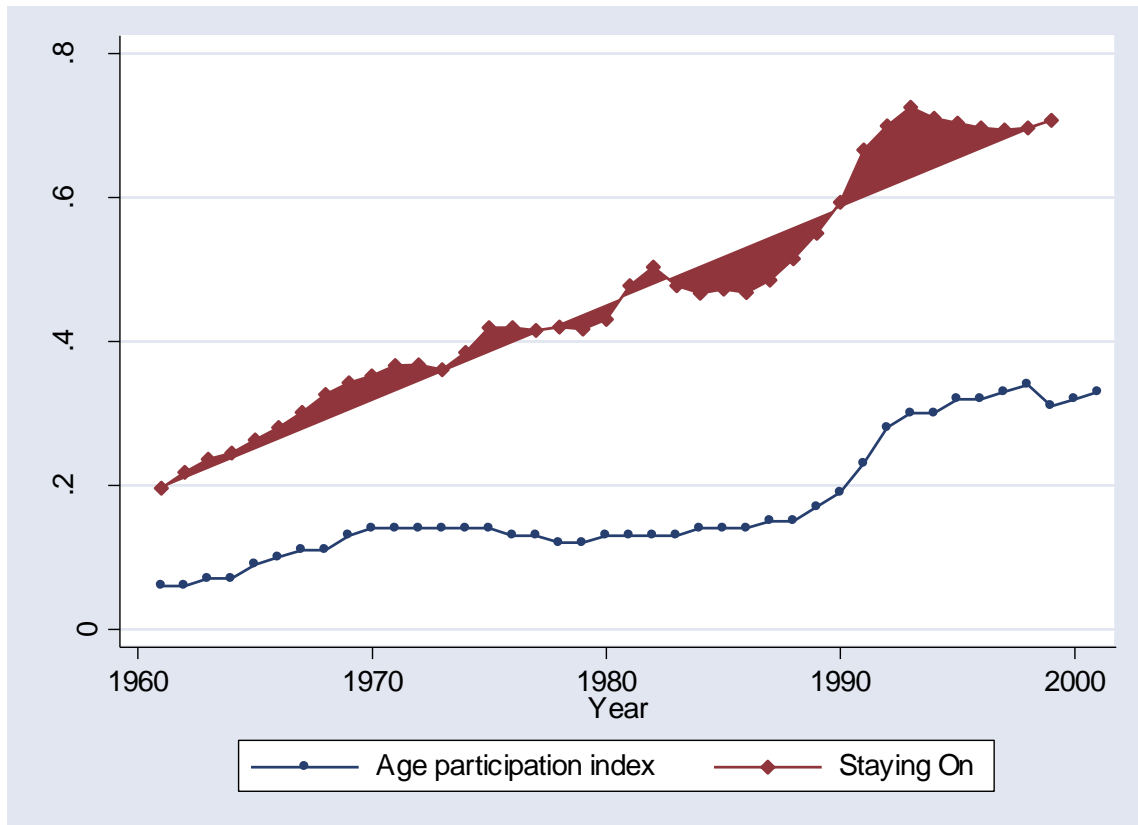
We use this variation in post-compulsory education participation to identify the causal impact of education on crime. For young men, we report a strong crime reducing effect of education, which is considerably bigger in (absolute) magnitude than that implied by least squares regressions. We also find that the education boost from expansion significantly affected other productivity related economic variables (notably qualification attainment and wages), which we interpret as saying that keeping people in the education system (incapacitation) is not the sole driver of the results. Rather there is also a productivity enhancing aspect of the increased time spent in post-compulsory education that has a direct crime reducing impact.

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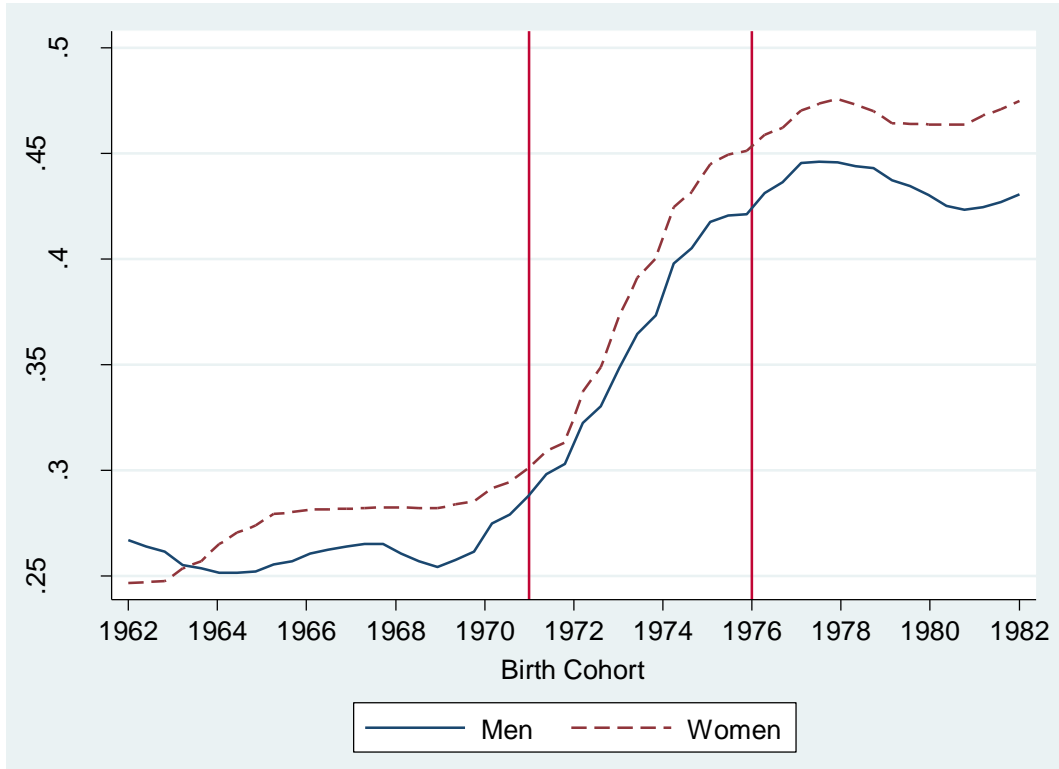
**Figure 1:**  
**Changes in Post Compulsory Education Participation**



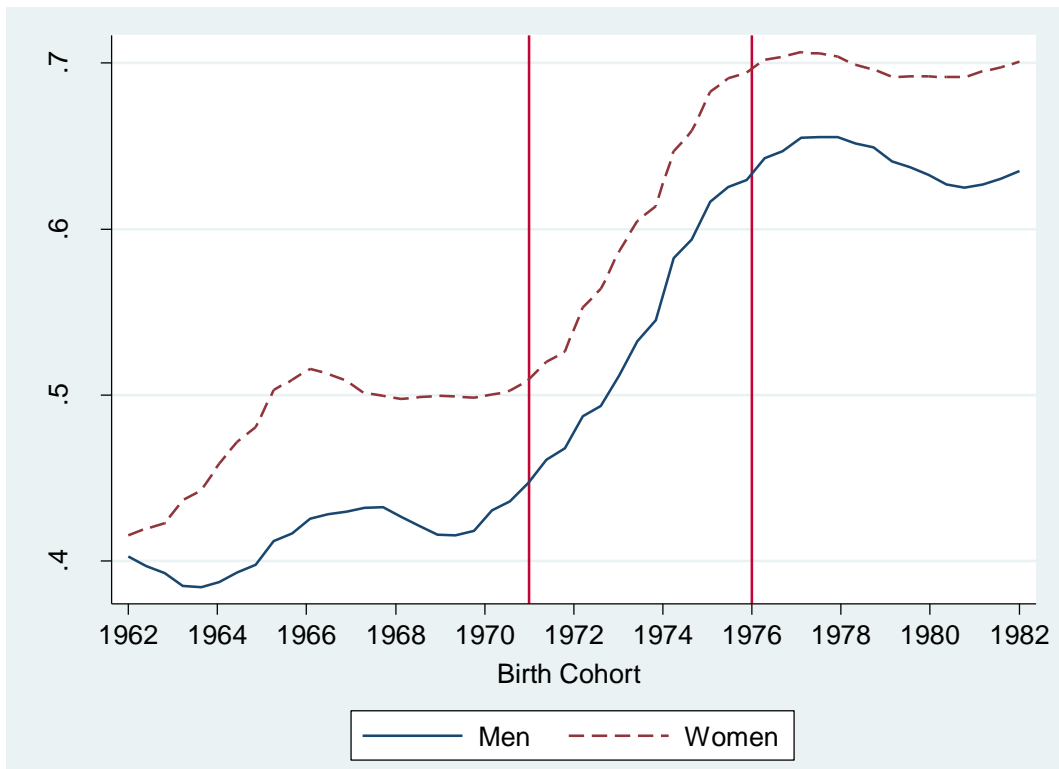
Source: Blanden and Machin (2004).

**Figure 2: Education Measures by Cohort**

**Proportion of 16 to 21 Year Olds Still in Full Time Education**

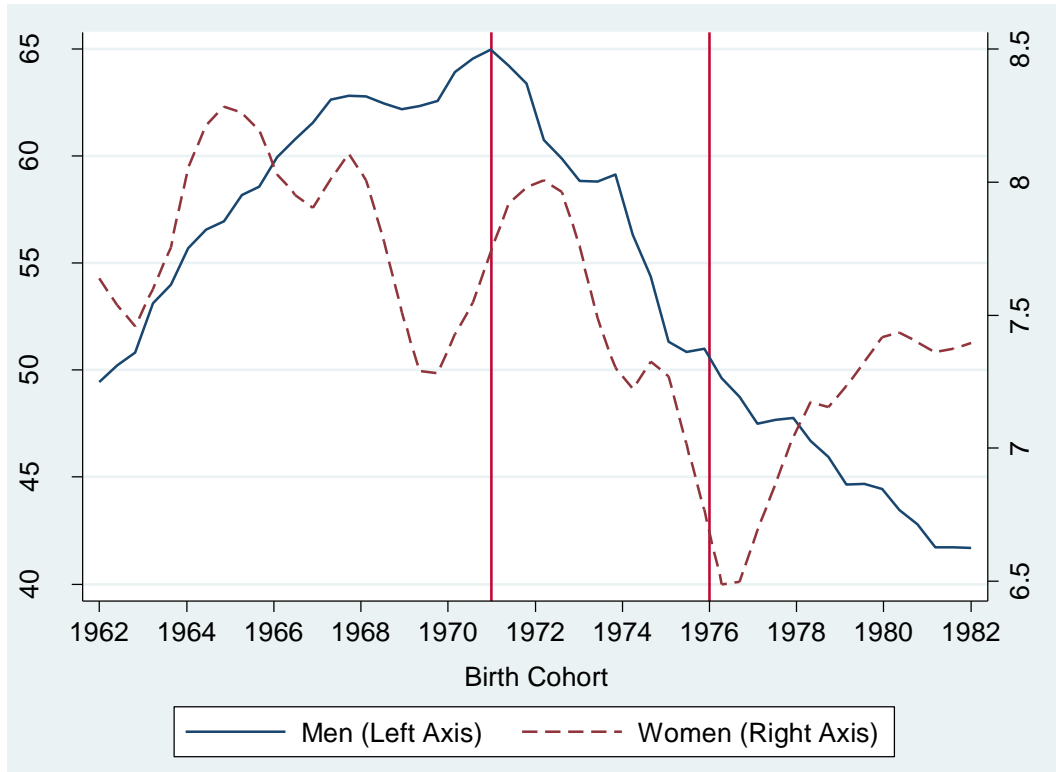


**Proportion of 16 to 21 Year Olds Staying on After 16 by Cohort**



Source: Labour Force Surveys. Authors' calculations. The cohorts between the two vertical lines (i.e. 1972 to 1975 inclusive) are the education expansion cohorts.

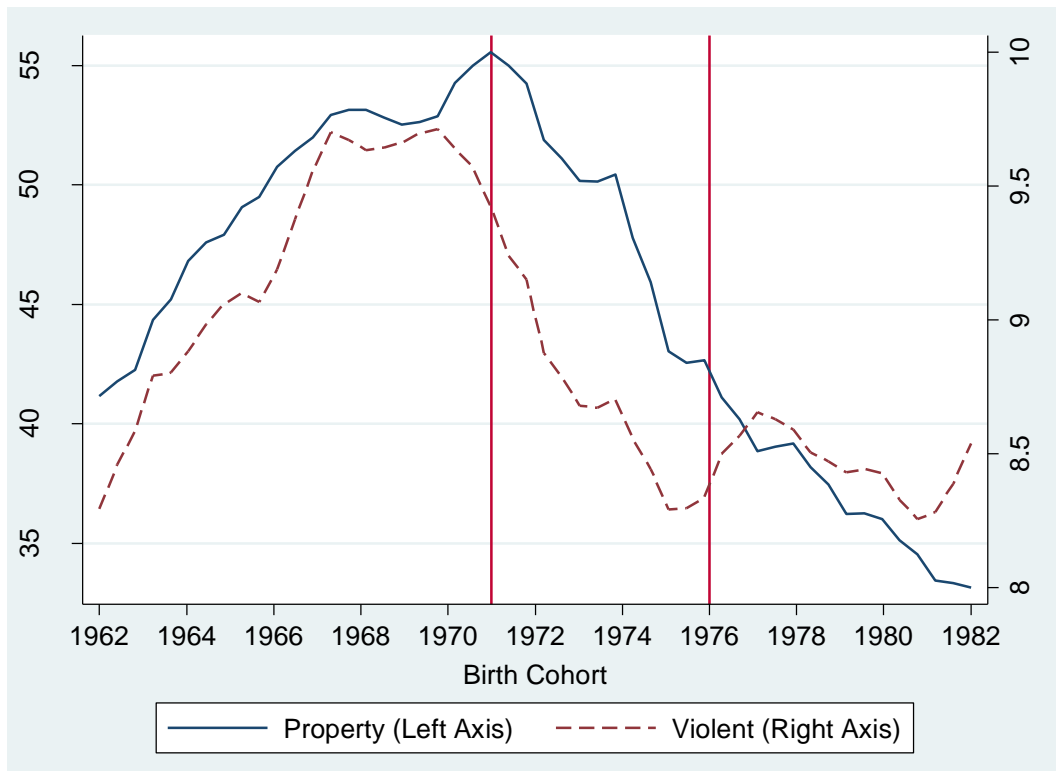
**Figure 3:**  
**Conviction Rates for Individuals Aged 16 to 21 by Cohort and Gender**



Source: Offenders Index Database. Authors' calculations. The cohorts between the two vertical lines (i.e. 1972 to 1975 inclusive) are the education expansion cohorts.



**Figure 4:**  
**Property and Violent Conviction Rates for Men Aged 16 to 21 by Cohort**



Source: Offenders Index Database. Authors' calculations. The cohorts between the two vertical lines (i.e. 1972 to 1975 inclusive) are the education expansion cohorts.

**Table 1: Education and Crime Reduced Forms – Men and Women**

	Men, Aged 16-21, Born 1962-1982			Women, Aged 16-21, Born 1962-82		
	Education Reduced Forms		Crime Reduced Form	Education Reduced Forms		Crime Reduced Form
	Proportion Students	Proportion Stay on After 16	Log(Total Conviction Rate)	Proportion Students	Proportion Stay on After 16	Log(Total Conviction Rate)
<b>Cohort 1972</b>	0.049** (0.021)	0.041* (0.024)	-0.045 (0.032)	0.031 (0.020)	0.013 (0.016)	0.018 (0.062)
<b>Cohort 1973</b>	0.082*** (0.019)	0.067*** (0.021)	-0.117*** (0.043)	0.063*** (0.018)	0.053*** (0.015)	-0.011 (0.060)
<b>Cohort 1974</b>	0.117*** (0.019)	0.128*** (0.022)	-0.081* (0.047)	0.102*** (0.016)	0.097*** (0.018)	-0.139* (0.076)
<b>Cohort 1975</b>	0.145*** (0.021)	0.173*** (0.026)	-0.229*** (0.036)	0.127*** (0.019)	0.143*** (0.020)	-0.051 (0.059)
<b>Post-EE Cohort</b>	0.157*** (0.026)	0.200*** (0.030)	-0.214*** (0.042)	0.135*** (0.027)	0.153*** (0.023)	-0.177** (0.083)
<b>F-Test of Joint Significance of Cohort 1972, 1973, 1974, 1975</b>	14.57 [p = 0.00]	16.08 [p = 0.00]	12.06 [p = 0.00]	14.73 [p = 0.00]	21.74 [p = 0.00]	1.04 [p = 0.39]
<b>Cubic in Age</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Quadratic in Cohort</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Sample Size</b>	125	125	125	125	125	125

Notes: Standard errors in round parentheses. \*, \*\* and \*\*\* respectively denote significance at the 10, 5 or 1 percent level.

**Table 2: OLS and 2SLS Results – Men and Women – Total Convictions**

		Aged 16-21, Born 1962-82	
		Men - Log(Total Conviction Rate)	Women - Log(Total Conviction Rate)
<b>Proportion Students</b>	<b>OLS</b>	-0.341** (0.157)	-0.233 (0.285)
	<b>2SLS</b>	-1.649*** (0.437)	-0.650 (0.464)
<b>Proportion Stay on After 16</b>	<b>OLS</b>	-0.288** (0.119)	-0.188 (0.251)
	<b>2SLS</b>	-1.486*** (0.392)	-0.578 (0.422)
<b>Cubic in Age</b>		Yes	Yes
<b>Quadratic in Cohort</b>		Yes	Yes
<b>Sample Size</b>		125	125

Notes: Standard errors in round parentheses. \*, \*\* and \*\*\* respectively denote significance at the 10, 5 or 1 percent level.

**Table 3: Crime Reduced Forms, OLS and IV Results – Men – Property and Violent Convictions**

	Crime Reduced Forms			OLS and 2SLS Estimates		
	Log(Property Conviction Rate)	Log(Violent Conviction Rate)		Log(Property Conviction Rate)	Log(Violent Conviction Rate)	
<b>Cohort 1972</b>	-0.034 (0.040)	-0.087*** (0.034)	<b>Proportion Students</b>	<b>OLS</b>	-0.309* (0.181)	-0.527** (0.241)
<b>Cohort 1973</b>	-0.108** (0.052)	-0.141*** (0.044)		<b>2SLS</b>	-1.639*** (0.481)	-1.506*** (0.447)
<b>Cohort 1974</b>	-0.067 (0.055)	-0.132*** (0.050)	<b>Proportion Stay on After 16</b>	<b>OLS</b>	-0.244* (0.137)	-0.535*** (0.194)
<b>Cohort 1975</b>	-0.226*** (0.042)	-0.216*** (0.063)		<b>2SLS</b>	-1.488*** (0.423)	-1.315*** (0.425)
<b>Post EE Cohort</b>	-0.217*** (0.050)	-0.162** (0.082)				
<b>F-Test of Joint Significance of Cohort 1972, 1973, 1974, 1975</b>	10.55 [ $p = 0.00$ ]	3.70 [ $p = 0.01$ ]				
<b>Cubic in Age</b>	Yes	Yes		Yes	Yes	
<b>Quadratic in Cohort</b>	Yes	Yes		Yes	Yes	
<b>Sample Size</b>	125	125		125	125	

Notes: Standard errors in round parentheses. \*, \*\* and \*\*\* respectively denote significance at the 10, 5 or 1 percent level.

**Table 4: Qualification Attainment, Wages and Unemployment**

Men, Aged 25-30, Born 1962-1982 - LFS 1993 to 2010						
Panel A: Reduced Forms	Stay-On After 16	Age Left School	Low Qualification	Log(Weekly Wages)	Log(Hourly Wages)	Unemployment
Cohort 1972	0.024*** (0.006)	0.221*** (0.043)	-0.025*** (0.006)	0.022 (0.015)	0.025* (0.015)	-0.005 (0.005)
Cohort 1973	0.047*** (0.009)	0.369*** (0.054)	-0.047*** (0.008)	0.047*** (0.010)	0.056*** (0.013)	-0.012** (0.006)
Cohort 1974	0.094*** (0.009)	0.551*** (0.062)	-0.065*** (0.008)	0.056*** (0.012)	0.084*** (0.013)	-0.009 (0.006)
Cohort 1975	0.144*** (0.009)	0.743*** (0.058)	-0.078*** (0.010)	0.077*** (0.014)	0.103*** (0.016)	-0.016** (0.007)
Post EE Cohort	0.171*** (0.013)	0.740*** (0.073)	-0.073*** (0.009)	0.075*** (0.014)	0.111*** (0.016)	-0.014 (0.009)
F-Test of Joint Significance of Cohort 1972, 1973, 1974, 1975	95.20 [ $p = 0.00$ ]	48.91 [ $p = 0.00$ ]	25.56 [ $p = 0.00$ ]	10.14 [ $p = 0.00$ ]	14.79 [ $p = 0.00$ ]	1.79 [ $p = 0.14$ ]
Sample Size	310,365	310,365	303,367	57,787	56,191	293,424
Panel B: OLS and 2SLS						
Stay OnAfter 16	OLS		-0.379*** (.004)	0.223*** (0.006)	0.288*** (0.007)	-0.053*** (0.003)
	2SLS		-0.554*** (0.050)	0.471*** (0.100)	0.648*** (0.115)	-0.084 (0.054)
Age Left School	OLS		-0.106*** (.001)	0.044*** (0.001)	0.056*** (0.001)	-0.008*** (0.000)
	2SLS		-0.104*** (0.006)	0.089*** (0.017)	0.124*** (0.018)	-0.019* (0.011)

Notes: Standard errors clustered by age and cohort in round parentheses. \*, \*\* and \*\*\* respectively denote significance at the 10, 5 or 1 percent level. All specifications include a cubic in age and a quadratic in cohort.

## **Data Appendix**

### *A.1 The Offenders Index Database (OID)*

Our analysis uses Offenders Index Database (OID) data from 1978 to 2002, which we match to Labour Force Survey (LFS) data from England and Wales by age cohort and survey year. The version of the OID to which we have access holds criminal history data for offenders convicted of standard list offences between 1963 and 2002. Standard list offences are all indictable or triable either way offences, plus a few of the more serious summary offences. Standard list class codes are set out in the Offenders Index (OI) codebook. The data are derived from the Court Appearances system and are updated quarterly.

The data set holds anonymous samples (of 4 weeks) for each year. The selection of offenders is from analysis of the court appearance data using the date to select relevant offenders. Selection of offenders occurs where they appeared in court during the first week in March, the second week in June, the third week in September and the third week in November.<sup>9</sup>

Matching OID to ONS population data, we calculated offending rates (per 10,000 population) by age cohort and year, separately for men and women, using date of birth and gender variables. Criminal offences have been broadly categorised as property crimes (burglary and theft and handling stolen goods) and violent crimes (violence against the person and robbery), using categorisation in the Offence Class Code variable.<sup>10</sup> The total conviction rate we use is the sum of the two. We focus on youth convictions for individuals aged 16 to 21 years old enabling us to have a reasonably well balanced panel

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<sup>9</sup> The first week in any calendar month is the week where the Monday is the first Monday in that month.

<sup>10</sup> We do not consider sexual offences since there are very few of them and their relationship with education is contrary to that of most other crimes (as in the case of rape in Lochner and Moretti, 2004).

around the policy years (from the 1988 introduction of GCSE onwards) since the cohort studied were born between 1962 and 1982.

The data structure for men and women, with means of the total conviction rate per 10,000 population, as well as property and violent conviction rates, are presented in Table A1.

*Table A1: Offenders Index Database (OID) – Descriptives*

OID Year	Age Range	Men, Total Convictions	Men, Property	Men, Violent	Women, Total Convictions	Women, Property	Women, Violent
1978	16-16	44.11	39.60	4.51	5.09	4.32	0.77
1979	16-17	45.22	38.27	6.95	7.03	6.24	0.79
1980	16-18	50.44	41.49	8.95	8.50	7.70	0.80
1981	16-19	54.31	45.46	8.85	7.28	6.66	0.62
1982	16-20	63.71	54.06	9.65	8.80	8.14	0.65
1983	16-21	57.97	49.01	8.96	8.03	7.17	0.87
1984	16-21	61.56	52.78	8.79	8.52	7.79	0.73
1985	16-21	60.74	51.53	9.21	8.58	7.93	0.65
1986	16-21	53.39	44.71	8.68	7.27	6.56	0.71
1987	16-21	69.41	59.97	9.43	7.43	6.70	0.72
1988	16-21	65.11	55.23	9.88	7.25	6.29	0.96
1989	16-21	57.74	47.54	10.20	7.55	6.48	1.07
1990	16-21	60.03	50.52	9.51	7.81	6.93	0.88
1991	16-21	61.58	53.23	8.35	8.14	7.31	0.83
1992	16-21	59.99	51.13	8.87	7.68	6.70	0.99
1993	16-21	53.12	43.49	9.62	6.69	5.90	0.79
1994	16-21	50.84	41.34	9.50	6.83	5.94	0.88
1995	16-21	48.87	41.14	7.73	6.40	5.79	0.62
1996	16-21	45.53	37.71	7.81	6.06	5.51	0.55
1997	16-21	46.24	37.17	9.06	6.94	6.06	0.88
1998	16-21	43.76	35.68	8.08	6.76	6.11	0.65
1999	17-21	44.11	35.66	8.45	7.85	7.13	0.72
2000	18-21	43.32	35.04	8.28	9.15	8.35	0.80
2001	19-21	41.82	33.44	8.38	8.51	7.84	0.67
2002	20-21	38.73	30.72	8.01	7.31	6.76	0.55

Note: There was an unexplained jump in the number of convictions recorded in 1999 in the OID, so that year is interpolated by cohort between 1998 and 2000. Dropping this year from our sample yielded similar results to the ones reported in the main body of the paper.

### A.2 *The Labour Force Survey (LFS)*

The LFS began in 1975 and was initially a biannual survey (up to 1983), after which it became annual (up to 1991) and has since become a quarterly (since 1991)

sample survey of households living at private addresses in the United Kingdom. Its purpose is to provide information on the UK labour market that can then be used to develop, manage, evaluate and report on labour market policies. It is conducted by the Office for National Statistics.

For the purposes of this paper, we make use of two distinct sets of LFS data:

- i) Cohort level averages (weighted by individual weights) from 1978 to 2002 to match to OID data in order to analyse the impact of education expansion on youth crime.
- ii) Individual data from 1993 to 2010 to investigate the effect of the policy on later life outcomes for the same cohorts.

The first set of LFS data is mainly used to obtain averages of educational attainment levels by cohort in order to estimate actual impact of the expansion policy. The two measures of education for our individuals aged 16 to 21 and born between 1962 and 1982 were defined as:

- ‘Student’: Individual was still enrolled at school at the time of survey.
- ‘Stay On’: Individual is still enrolled in school and is older than 16 or has left full time education after that age.

Table A2.1 below describes the education measures by year, age range, and gender:

*Table A2.1: LFS 1978-2002 - Descriptives*

LFS Year	Age Range	Men, Proportion Students	Men, Proportion Stayed On	Men, Proportion Students	Men, Proportion Stayed On
1978	16-16	0.713	0.713	0.672	0.672
1979	16-17	0.521	0.562	0.473	0.511
1980	16-18	0.413	0.471	0.417	0.481
1981	16-19	0.327	0.395	0.360	0.452
1982	16-20	0.293	0.399	0.320	0.466
1983	16-21	0.264	0.407	0.289	0.484
1984	16-21	0.267	0.412	0.279	0.494
1985	16-21	0.250	0.407	0.276	0.486
1986	16-21	0.250	0.411	0.270	0.492



1987	16-21	0.271	0.431	0.276	0.498
1988	16-21	0.267	0.426	0.284	0.505
1989	16-21	0.270	0.429	0.290	0.505
1990	16-21	0.291	0.452	0.325	0.525
1991	16-21	0.322	0.479	0.344	0.558
1992	16-21	0.371	0.542	0.395	0.611
1993	16-21	0.404	0.581	0.434	0.649
1994	16-21	0.438	0.616	0.450	0.676
1995	16-21	0.438	0.641	0.464	0.695
1996	16-21	0.434	0.636	0.458	0.692
1997	16-21	0.423	0.639	0.459	0.697
1998	16-21	0.431	0.636	0.471	0.702
1999	17-21	0.367	0.612	0.396	0.677
2000	18-21	0.316	0.605	0.344	0.668
2001	19-21	0.275	0.597	0.284	0.648
2002	20-21	0.253	0.588	0.263	0.633

Note: In the early years of the LFS, the survey was biannual (in 1977, 1979, 1981 and 1983), after which it became annual. For the early missing years (1978, 1980, and 1982), we impute values by cohort by generating averages between the previous and next year.

The second use of the LFS comes from our interest in the effect of the education expansion on productivity related outcomes later in the lives of the cohorts that experienced this education expansion. Wage information was first reported in the first quarter of 1993 and the data is available up to the last quarter of 2010. Since all the information necessary for this part of the analysis (education and outcomes) is available within the LFS, we can use individual micro information here rather than using cohort level averages. The variables we look at are:

- ‘StayOn’: Individual left school after the compulsory age of 16
- ‘Edage’: Age individual left full time education
- ‘LowQual’: Left school without obtaining an intermediate qualification
- ‘Wkearn’: Gross weekly earnings
- ‘Hrearn’: Gross hourly earnings (i.e. weekly earnings / hours worked)
- ‘Unemp’: Classified as unemployed at time of survey

The sample of individuals is restricted to males born between 1962 and 1982 aged 25 to 30, but observed in later LFS years, which corresponds to the same cohorts used in the analysis of education expansion on youth crime. Table A2.2 reports averages for these variables by year and age range.

*Table A2.2: LFS 1993-2010 - Descriptives*

LFS Year	Age Range	Stay-On After 16	Age Left School	Low Qual	Weekly Wages	Hourly Wages	Unemp
1993	25-30	0.40	17.41	0.77	307.73	7.40	0.12
1994	25-30	0.42	17.46	0.78	297.27	7.05	0.11
1995	25-30	0.42	17.43	0.77	308.45	7.38	0.11
1996	25-30	0.41	17.52	0.76	319.51	7.72	0.10
1997	25-30	0.41	17.58	0.70	325.81	7.69	0.08
1998	25-30	0.42	17.70	0.68	339.34	8.00	0.07
1999	25-30	0.43	17.83	0.65	355.99	8.46	0.06
2000	25-30	0.45	17.89	0.65	370.75	8.83	0.06
2001	25-30	0.48	18.14	0.63	394.83	9.47	0.06
2002	25-30	0.51	18.31	0.61	405.72	9.82	0.06
2003	25-30	0.53	18.47	0.58	411.34	9.99	0.06
2004	25-30	0.55	18.52	0.57	423.78	10.49	0.05
2005	25-30	0.57	18.58	0.57	430.28	10.68	0.05
2006	25-30	0.58	18.66	0.55	430.48	10.64	0.05
2007	25-30	0.58	18.72	0.54	455.11	11.11	0.05
2008	26-30	0.59	18.79	0.53	496.05	12.20	0.05
2009	27-30	0.58	18.85	0.52	477.57	11.92	0.09
2010	29-30	0.57	18.79	0.52	493.09	12.19	0.08