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The Crime Reducing Effect of Education

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Abstract

In this paper, we present evidence on empirical connections between crime and education, using various data sources from Britain. A robust finding is that criminal activity is negatively associated with higher levels of education. However, it is essential to ensure that the direction of causation flows from education to crime. Therefore, we identify the effect of education on participation in criminal activity using changes in compulsory school leaving age laws over time to account for the endogeneity of education. In this causal approach, for property crimes, the negative crime-education relationship remains strong and significant. The implications of these findings are unambiguous and clear. They show that improving education can yield significant social benefits and can be a key policy tool in the drive to reduce crime.

Keywords: Crime; education; offenders
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1. Introduction

Crime reduction is high on the public policy agenda, not least because of the large economic and social benefits it brings. Indeed, research on the determinants of crime points in several directions as to how crime reduction can be facilitated. For example, a relatively large body of research undertaken by social scientists considers the potential for expenditures on crime fighting resources (like increased police presence, or new crime fighting technologies), or on particular policies, to combat crime. Other work focuses more on the characteristics of criminals and considers what characteristics are more connected to higher criminal participation. In this latter case, policies that affect these characteristics can, if implemented successfully, be used to counter crime.

In this paper, we focus on one such characteristic that has received some attention in the quantitative literature on the determinants of crime, namely education. In this literature, there are a number of studies that relate crime participation to the education of individuals, typically reporting that less educated individuals are more likely to engage in crime.[1] A drawback associated with almost all of this work is that it is difficult to guarantee that the direction of causation flows from education to crime (and not the other way round). This, of course, matters if one wishes to consider appropriate policy responses to empirical findings.

In this paper, we try to carefully isolate the causal empirical connection between crime and education in the UK context. We do so using several different modelling approaches, based on different measures of crime and education from several different data sources. Our results show sizeable effects of education on crime that appear robust to methodological approaches and data sources. The implications of these findings are clear,

[1] Examples from the criminology literature include Farrington (1986, 2001) and from the education literature include Sabates (2008, 2009) and Sabates and Feinstein (2008). There is much less work by economists. Lochner and Moretti (2004) is a highly notable exception.

showing that improving educational attainment of the marginal individuals can act as a key policy tool in the drive to reduce crime.

The rest of the paper is organised as follows. Section 2 gives some theoretical background on the relationship between education and crime. Section 3 describes available crime data sources in Britain, their quality and, where relevant, how they can be matched to data on education. Section 4 discusses the empirical strategies that we are able to implement and the results, together with a calculation of the social benefits that follow from the crime reducing effect of education. Concluding remarks are given in the last section of the paper.

2. How Education Can Impact on Crime

There are number of theoretical reasons why education may have an effect on crime. From the existing socio-economic literature there are (at least) three main channels through which schooling might affect criminal participation: income effects, time availability, and patience or risk aversion. For most crimes, one would expect that these factors induce a negative effect of schooling on crime. In what follows, we discuss each of these channels in more detail.

For the case of *income effects*, education increases the returns to legitimate work, raising the opportunity costs of illegal behaviour. Consequently, subsidies that encourage investments in human capital reduce crime indirectly by raising future wage rates (Lochner, 2004). Additionally, punishment for criminal behaviour may entail imprisonment. By raising wage rates, schooling makes any time spent out of the labour market more costly (Lochner and Moretti, 2004; Hjalmarsson, 2008). Therefore, those who can earn more are less likely to engage in crime.

The idea that education raises skill levels and wage rates, which then lowers crime, is not a new one. Ehrlich (1975) empirically examined a number of predictions from an intuitive model relating education to crime. Grogger (1998) investigated the relationship between wage rates and criminal participation. The author shows that graduating from high school reduces criminal productivity and that criminals have on average less education than non-criminals. Linking crime to wages, Grogger (1998) concludes that youth offending behaviour is responsive to price incentives and that falling real wages may have been an important factor in rising youth crime during the 1970s and 1980s. Machin and Meghir (2004) look at cross-area changes in crime and the low wage labour market in England and Wales. They find that crime fell in areas where wage growth in the bottom 25th percentile of the distribution was faster and conclude that "improvements in human capital accumulation through the education system or other means… enhancing individual labour market productivity… would be important ingredients in reducing crime."

However, there is also some evidence that education can also increase the earnings from crime and the tools learnt in school may be inappropriately used for criminal activities. In this sense, education may have a positive effect on crime. Levitt and Lochner (2001) find that males with higher scores on mechanical information tests had increased offence rates. Lochner (2004) also estimates that across cohorts, increases in average education are associated with 11% increase in white collar arrest rates (although this estimated effect is not statistically significant).

Time spent in education may also be important for teenagers in terms of limiting the *time available* for participating in criminal activity. This can be thought of as "the cynical explanation is that whilst youngsters are at school they are being kept off the streets," (Hansen, 2003). 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 to the probability of arrest that year. Hjalmarsson (2008) looked at the opposite relationship of the impact of being arrested and incarcerated before finishing school on probability to graduate. Her results suggest that the more times you are caught committing crime and the amount of time spent in prison both greatly increases the likelihood of becoming a high school dropout.

As these still may be endogenous decisions, Jacobs and Lefgren (2003) instrument days off school with exogenous teacher training days. They find that property crime increases significantly in areas where youths have days off school validating the idea of the self-incapacitation effect of education on criminal participation. However, they also report that violent offences arrests increase while school is in session and attribute this to a concentration effect.[2] This, as Jacobs and Lefgren (2003) point out, only measures potential short-term impacts of education on crime. However, we can easily argue that criminal participation as a youth has longer run effects on future offending behaviour. Moreover, it is important when considering the immediate impact of policies that incentivise youths to stay on at school.

Education may also influence crime through its effect on *patience* and *risk aversion* (Lochner and Moretti, 2004). Here, future returns from any activity are discounted according to one's patience in waiting for them. Thus, 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) summarizes a sample of studies from the from psychological and neurological literatures, concluding 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. This line of literature also suggests that adolescents lack abstract reasoning skills and are predisposed to risky behaviour. Education can increase patience, which reduces the discount rate of future earnings and hence reduces the propensity to

[2] This is the geographical proximity of a large number of youths – in the educational establishment – which may result in increasing the probability of violent encounters.

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.

In summary, if education increases the marginal returns of earnings from legal more than illegal activities, schooling reduces the time available to commit crimes and positively affects patience levels. We therefore expect crime to be decreasing in the number of years of schooling and higher qualification attainment. It is also very likely that, everything else equal, individuals with higher wage rates, those who spend more time in school, and those with lower discount factors, will commit less crime.

3. Data

In analysing crime and education, a number of data related issues arise. First, there is the issue of crime measurement that is different across data sources. Second, whilst some micro-data on crime does contain information on the characteristics of criminals, the majority does not. In the latter case, we need some means of matching crime data to education data. We consider each of these in turn.

Crime Data

Probably the most commonly used source of crime data in quantitative research is information on criminal offences recorded by the police. As not all of these offences are solved, this type of data does not contain information on characteristics of the individuals committing these recorded offences. Unless these data are aggregated to some geographical level (like Police Force Areas) and matched to education data at this level, then it is not possible to use these data to study the empirical relationship between crime and education. Being realistic, only spatial aggregation is feasible as the offence data cannot be broken down by individual demographic characteristics. This does not offer much hope to credibly study the research question of interest in this paper.

The other main form of crime data available from the criminal justice system is on individuals who enter the criminal justice system after having been apprehended or charged for a crime. The Offenders Index Database (OID) contains information of the characteristics of individual offenders, holding criminal history data for offenders convicted of standard list offences from 1963 onwards.[3] The data is derived from the Court Appearances system and is updated quarterly. The Index was created purely for research and statistical analysis. Its main purpose is to provide full criminal history data on a randomly selected sample of offenders.

The OID dataset we have access to holds anonymous samples for offenders sentenced during four weeks each year from the 1960s onwards.[4] We also have the entire pre and post court appearance history of these individuals after this period. However, there is no information on a defendant's education level in the OID and the data needs to be aggregated in some way to connect to education data. A big advantage (certainly relative to the recorded offences data) is that some demographic characteristics are available in the OID, notably age and gender, and so these data are more suited to a study of crime and education to be undertaken at a level aggregated to the demographic breakdown of crimes that is available.

Micro data that simultaneously contains information on an individual's education level *and* criminal activity is only occasionally available. In the UK context there are, however, two large scale datasets with such information available that we can consider:

[3] Standard list offences are all indictable or triable offences plus a few of the more serious summary offences. Standard list class codes are set out in the Offenders Index codebook (see Offenders Index "Codebook" and Offenders Index "A User's Guide," Research Development and Statistics Directorate, Home Office.)

[4] Offenders were chosen 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. The first week in any calendar month is the week where the Monday is the first Monday in that month.

i) Census data containing information on incarceration and on individual education levels. The Samples of Anonymised Records (SARs) are samples of individual records from the 1991 and 2001 UK Censuses. They are micro-data files with a separate record for each individual, covering large sample sizes (between 1-5 percent of the population). The key advantage of the Census data is that we are able to identify individuals who are in prison service establishments (see the Communal Establishment Breakdown in Table A1 in Appendix A). However, only the 2001 Census has good enough data on individual education and so we are constrained to looking at links between imprisonment and education in the 2001 cross-section only.[5]

ii) British Crime Survey (BCS) data which asks a large sample of the British population about, among other things, their contacts with the criminal justice system and also contains information on the respondent's education level and rudimentary self report information on criminal histories. We report results using the 2001/2 through 2007/8 surveys, the period since the survey went annual.[6] Using this alternative data source is an important complementary part of our study since it should be relatively free of any biases in arrest, prosecution, and imprisonment probability due to levels of education (which may be a worry when using non self-reported crime information).

Amalgamating Data on Crime and Education

To carry out the cohort analysis, we aggregated the number of OID court appearances by age and gender from 1984 to 2002.[7] We calculated offending rates (per 1000 population) using

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[5] Specifically we use the Controlled Access Microdata Samples (CAMS) in the 2001 Census.

[6] The British Crime Survey was first carried out in 1982, collecting information about people's experiences of crime in 1981. The BCS was then carried out in 1984, 1988, 1992, 1994, 1996, 1998, 2000 and 2001. Since 2001/02, the survey has run continuously on an annual basis, containing consistent questions and sampling methods, and now covers around 45000 households each year in England and Wales.

[7] Although the OID is available from 1963 onwards, consistent age-cohort level data on education from the LFS database is only available from 1984 onwards. See further data description in this section.

the ONS population data by age-gender cohort and year.[8] For the estimation results, criminal offences have been broadly categorised as property crimes (burglary, theft and handling stolen goods, and criminal damage) and violent crimes (violence against the person, sexual offences, and robbery).

To this cohort panel, we matched Labour Force Survey (LFS) data on education, and data on wages from the New Earnings Survey.[9] Several explanatory variables were extracted from the LFS data for the period 1984 to 2002. In particular, we focused on age gender, date of birth (in order to construct school leaving age dummies), age when completed continuous full-time education, and highest level of qualification obtained. Other characteristics extracted are ethnicity, whether employed or unemployed, and whether living in London or not. These variables were first aggregated into cell means by age cohort and year and then matched with the OID in order to form a quasi-panel for age cohorts from 16 to 59 in the period 1984 to 2002. This was done overall and then separately for men and women, and for property and violent crimes. We also carried out the same matching exercise with data on wages from the New Earnings Survey.

4. Results

There are two main empirical approaches we adopt, the first using micro-data from the 2001 Census cross-section and BCS data, and the second looking at age cohorts from OID data matched to the LFS and NES data sources. We begin by considering basic empirical

[8] The population data were kindly made available by the UK Office for National Statistics (ONS).
[9] The LFS is a large-scale household survey which was carried out in 1975, 1977, 1979, 1981 and then annually from 1983 through 1992, after which it became a quarterly survey. The NES is a 1% employer reported annual survey on individual wages, on which we have access to micro-data from 1975 onwards.

correlations from both, and then turn to the causal estimates that can be obtained from the cohort data.

Estimates of Crime-Education Associations

a) Census Data on Imprisonment

Table 1 presents summary statistics for 2001 Census imprisonment rates, for all individuals and broken down by gender and age. The first column of the Table shows that, overall, 0.13 percent of 16-64 year olds in the British population were in prison on the Census date in April 2001. Imprisonment rates for young men aged 16 to 20 are higher than average at 0.34 percent, and are highest, at 0.57 percent, among the age 21 to 25 males. The imprisonment rates then declines for older age men.[10] Far fewer women are in prison and even amongst the highest sub-group (again aged 21-25) imprisonment rates remain low.

Columns (2), (3) and (4) show there to be stark differences by education level. The percent in prison is massively higher amongst those with no educational qualifications. For example, 2.57 percent of men aged 21-25 with no educational qualifications were in prison in 2001. This compares to 0.30 percent of the same age-gender group with at least some qualifications.

Column (4) shows imprisonment gaps between the no qualification and some qualification groups. The gaps are reported in two ways, as percentage gaps and as relative risk ratios (RRR). It is evident that there are large gaps in imprisonment rates that are related to the possession of educational qualifications. Moreover, the gaps are at their largest for the age groups where more people are in prison: see the largest relative risk ratios in the final column for the age 21-25 group, for both men (8.57) and women (8.50).

[10] This is in line with the postulations of the well documented "crime-age curve" which peaks in the late teens and early twenties (Gottfredson and Hirshi, 1986).

Table 2 presents logit estimates that condition upon an additional range of individual characteristics from the Census (listed in the notes to the Table). The results are reported for the whole sample, men and women separately, and for the different age groups by gender. The logit regression model is based on the log odds ratio (log[p/(1-p)]) where p is a 0/1 variable indicating whether a person is in prison or not), which represents the probability of a success compared with the probability of failure. Hence, an interpretation of estimated coefficients in the logit regression which is usually more intuitive is the 'odds ratio' or the relative risk ratio (RRR), reported in the third column of the Table. The marginal effects, reported in the second column show the change in the probability of imprisonment due to a unit change of an education variable in question.

The results in Table 2 very much confirm the descriptive analysis. Even after conditioning on a range of factors, there is a sizeable gap in imprisonment rates between those with no qualifications and those with some educational qualifications. For the full sample, the RRR of around 4 shows that people with no qualifications are four times more likely to be in prison than those with some qualifications. For young men these odds rise even more, to around 9.1 for 16-20 year olds, and to 14.8 for women in the same age group.[11]

b) Self-Report Data on Criminal Histories

This section considers crime-education associations from self-report data in the British Crime Surveys. Table 3 shows descriptive statistics on two self-report measures on whether individuals have ever been arrested (in Panel A) or whether they have ever been in court as the person accused of committing a crime (Panel B). Column (1) shows summary statistics

[11] The Census education variable is more detailed than the no/some educational qualifications split we consider. There is information on five qualification levels, ranging from Level 0 (No Qualifications) through to Level 4 (Degree or higher). We look at the no/some distinction so we can include the young people in our sample since some may not have completed their education, and these are an important group to consider in studies of criminal activity. Specifications estimated for older samples that enter in four dummy variables for No Qualifications, Level 1, Level 2 and Level 3 (omitting Level 4 as the reference category) show a monotonic relationship between the probability of imprisonment and qualification attainment. For example, for men aged 26-30 the relative risk ratios were estimated as 13.46 (Level 0), 6.32 (Level 1), 5.56 (Level 2), 2.27 (Level 3).

for all sample respondents, then broken down by age and gender. Columns (2), and (3) consider breakdowns by level of education, and column (4) the gaps between them.

The upper panel of the Table shows that 12.9 percent of people report ever being arrested. Not surprisingly, this is a lot higher for men than for women (at 21.3 percent compared to 4.1 percent). Considering breakdowns by whether or not BCS sample members have some or no educational qualifications, sizable gaps emerge. Almost 17 percent of those with no educational qualifications report having ever been arrested, whilst the comparable number for people with some qualifications is 11.8 percent (column (4) shows the 4.9 percentage point gap to be strongly significant).

The breakdown across demographic (age by gender) groups is also interesting and follows a very similar pattern to the imprisonment rates from the Census data. The biggest percentage point gap in being arrested is the 13.6 points difference we observe for men aged 16-24. Gaps are much lower amongst women of all ages. The bottom Panel of Table 3 shows similar patterns for the other self-report measure, whether an individual reports having been in court as the accused. Overall, 9 percent of the sample report this to be the case, with the percentage being significantly higher for those with no qualifications (at 11.9 percent), and being consistently higher for men.

Table 4 reports estimates of coefficients (and associated marginal effects and relative risk ratios) on a no qualifications variable entered into logit regressions of the probability of being arrested or in court as the accused. The strong patterns seen in the descriptive Table hold up. Individuals with no educational qualifications have significantly higher models of self-reported crime incidence, with relative risks being higher for men, especially younger men. Overall, these results are very similar, in qualitative terms, to the Census imprisonment equations reported in Table 2.

c) Cross-Cohort Data on Offending Rates

The third piece of observational evidence we consider comes from the cohort panel data we have assembled from OID and LFS/NES data. A first set of results is reported in Table 5. Unlike with the Census or BCS analysis we are now able to consider different types of crimes. The upper panel of the Table thus reports results from models of property crimes, whilst the lower panel considers violent crimes. These are useful distinctions to draw if we think education may have less of an impact on violent rather than property offending, given the potential importance of labour market opportunities in explaining the relationship between education and property crime. Because of the availability of more detailed education data in the LFS, we can also consider models where education is measured, as with the analysis to date, in terms of no educational qualifications, but also in terms of years of education. Results from both are considered in the Table.

The results in Table 5 provide more evidence of a significant association between crime and education. There are several results of interest. First, the effects seem to be most important for property crimes, and there is little systematic relation with violent crime. This is in line with the use of the standard economic model of crime which predicts that the likely effects of education investments are more likely to be of relevance for property, rather than violent, crime. Second, the significant negative association between property crime and education is revealed in the Table for both measures of education considered.

Causal Estimates from Cross-Cohort Data

The results to date consistently show evidence from observational data of higher crime rates for less educated individuals. However, as we have already noted several times, results from the kinds of exercises considered so far may not reflect the causal impact of education on crime.

To see this for our cohort models, consider a simple least squares regression of a measure of offending for a particular age cohort *i* in year *t* (O_{it}) with an education variable (E_{it}) as an explanatory variable and X_{jit} ($j = 1,2,\cdots,J$) being a set of other control variables:

$$O_{it} = \alpha_0 + \alpha_1 E_{it} + \sum_{j=0}^{J} \delta_j X_{jit} + u_{it} \tag{1}$$

where u_{it} is an error term in the equation.

If unobserved characteristics of cohorts drive crime participation, but also education, then least squares estimates of α_1 (like those given in Table 5) will be biased. This is a key issue to the extent that unobserved characteristics affecting schooling decisions may be correlated with unobservables influencing the decision to engage in crime. For example, α_1 could be estimated to be negative, even if schooling has no causal effect on crime. This would be the case if individuals who have high criminal returns were likely to spend most of their time committing crime rather than work, regardless of their educational background. As long as education does not increase the returns to crime, these individuals are likely to drop out of further education. As a result, we might observe a negative correlation between education and crime even though there is no causal effect between the two. Therefore, the challenge is to find an appropriate instrument for education.

To credibly identify a causal impact of education on crime, we adopt a quasi-experimental approach relying on variations in education induced by changes in compulsory school leaving age laws over time to validate the direction of causation. This is akin to Lochner and Moretti's (2004) approach, which exploits changes in school leaving age laws across US states. We use here two raisings of the school leaving age that occurred in Britain in 1947 and 1973 as instrumental variables in our empirical analysis.[12] Details on the nature, and rationales, for the reform are given in Appendix B.

It needs to be acknowledged that the variation induced by these two instruments is likely to only identify a local impact, as it is much more likely to have an impact at the bottom of the education distribution and very little impact at the top of the education distribution. This is because people near the top would have stayed on after the compulsory school leaving age anyway and the change would not affect them.[13] Therefore, the effect that our empirical approach estimates is the local average treatment (LATE) effect among those who alter their treatment status because they react to the instrument. For this reason, we consider the effects separately for the continuous years of education measure, but also more appropriately for the no qualifications variable. We also show some results where those with no qualifications are compared only to those with slightly higher qualifications.

Identification is achieved through inclusion in a first stage education regression of two dummy variables that record the exogenous change in the minimum school-leaving age (SLA) that occurred in England and Wales in two particular years. In particular, the two dummy variables are defined for individuals who entered their last compulsory school year between 1947 and 1972 and hence faced a minimum SLA of 15 (variable SLA1), and for those entering their last compulsory year from 1973 onwards who therefore faced a minimum SLA of 16 (variable SLA2). The minimum SLA of 14 is our omitted category. Hence we effectively use changes over time in the number of years of compulsory education that government imposed as an instrument for years of education. Since we have more than one

[12] The education reform in Britain served as a source of exogenous variation in many papers in labour and health economics. Harmon and Walker (1995) and Oreopoulos (2006) focus on the causal impact of education and earnings. Galindo-Rueda (2003), Chevalier (2004), and Chevalier et al. (2005) look at the effect of parental income on education of their children. Oreopoulos (2006), Doyle et al. (2007), and Lindeboom et al. (2009) examine the impact of education on health. We are the first to consider this overall of the schooling system in England and Wales to study the causal impact of education on crime.

[13] Papers by Lindeboom et al. (2009) and Oreopoulos (2006) show that the first reform in Britain in 1947 only affected the schooling decisions of individuals at the lower end of the education distribution.

instrument, and only one variable to instrument, the model is over-identified, permitting us to implement a two-stage least squares (2SLS) approach.

The set of estimating equations now look as follows:

$$
\begin{aligned}
O_{it} &= \beta_0 + \beta_1 E_{it} + \sum_{j=0}^{J} \varphi_j X_{jit} + \upsilon_{it} \\
E_{it} &= \delta_0 + \delta_1 SLA1_{it} + \delta_2 SLA2_{it} + \sum_{j=0}^{J} \theta_j X_{jit} + \nu_{it}
\end{aligned}
\tag{2}
$$

In this framework, it is important whether changes in compulsory schooling laws act as valid instruments. A legitimate instrument for education in equation (1) is a variable that: (i) significantly explains part of the variation in education; and (ii) is not correlated with the unobservables that are correlated with both offending and education. Put alternatively, it is a variable that is a determinant of schooling that can legitimately be omitted from equation (1).

To answer the first criteria, let us go back to the definition of our instruments. We use changes over time in the number of years of compulsory education that government imposed as an instrument for years of education. Harmon and Walker (1995) use the same instruments to identify the causal impact of education on wages. They show that the 1947 change was particularly influential in raising participation in post-compulsory education. That is, many of those who would otherwise have left at the old minimum stayed on beyond the new minimum age. Oreopoulos (2006) even argues that his IV estimate of the returns to schooling using only the 1947 change as instrument for education is probably closer to the average treatment effect (ATE) estimator than the LATE, since the 1947 legislation affected almost half of the population. The strength of these compulsory school leaving age changes is very much confirmed in the first stage regressions we report below where there is a strong and highly significant correlation between the two policy changes and education.

Considering the second criteria for a valid instrument, we believe our instruments form a plausible identification strategy since changes in compulsory attendance laws have not historically been concerned by problems with crime. To our knowledge, legislators enacting the laws did not act in response to concerns with juvenile delinquency, youth unemployment, or other factors related to crime, thus making schooling laws an appropriate instrument.

The two-stage least squares (2SLS) results are reported in Table 6. We present results considering the causal impact of education on property crime.[14] The Table has three panels, with results for all cohorts in the upper panel, for men only in the middle panel, and for men ages 21 to 40 in the lower panel. Two sets of specifications are reported in each panel, one for the years of education variable, and one for the no educational qualifications measure comparing to some qualification. In each case the first column (column (1) for years of education and columns (4) for no educational qualification) reproduces the least squares results from Table 5, the second column (columns (2), and (5)) show the education first stages and the third columns ((3), and (6) respectively) give the 2SLS estimates.[15]

The first stage regressions are strongly significant, showing there to be no weak instrument problems, and the second stages are precisely determined. In all but one of the six specifications reported, the 2SLS estimate is (in absolute terms) larger in magnitude than the OLS estimate. The exception is for the no qualifications specification for the combined male and female cohorts, and even here one cannot reject the hypothesis that the significant 2SLS estimate is statistically different from the OLS estimate. This is suggestive that the least squares estimates are likely to be lower bounds and therefore that the causal impact of education is at least as sizable. Interestingly, this is the same pattern as the only other paper that we know identifies a causal impact of education on crime with a credible identification strategy, namely the US paper by Lochner and Moretti (2004). Overall, the pattern that emerges is of a significant causal crime reducing effect of education.

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[14] The IV strategy was clearly much less effective for the violent crime models and effects were imprecisely estimated. More detailed results are available on request from the authors.

[15] Notice that, strictly speaking, the lower panel is a just identified IV model as, due to the age restriction, only the SLA2 instrument can be considered.

In view of the issues raised in our discussion about local average treatment effects, it is interesting to consider results for sub-samples of the population that may have been proportionally more affected by the SLA changes. We do this in two ways in Table 7 where:

i) We limit the sample closer to the discontinuity that generated the abrupt education changes by looking at cohorts born 4 years before or after the second SLA change (in columns (1) to (6) for years of education and no qualification compared to some qualification).

ii) We report estimates (columns (7) to (9)) for individuals with no qualification compared to their peers who obtained a minimum qualification level (what we refer to as low qualifications in the Table).[16]

The structure of the three panels in Table 7 is the same as in the previous Table. For the around the discontinuity sample, in columns (1) through (6), the magnitudes of the causal estimates rise and are large for the sample of individuals born around the 1973 SLA change threshold. The no qualifications versus low qualifications comparison in columns (7) to (9) produces more muted effects, with strongly significant first stages and in all but one of the reported specification the 2SLS/IV estimates are larger in absolute terms than the OLS ones. A causal crime reducing effect of education is strong and significant in these 2SLS/IV estimates. Still, the causal estimates remain large and significant with, for example, for the whole sample in the top panel, the estimated 2SLS coefficient suggests that lowering the no qualifications variable by 1 percent would reduce property crime by almost 1.1 percent. We interpret this as a lower bound of the LATE estimates of the causal impact of education on crime.

[16] Using LFS variable coding we define obtaining low qualifications as any other professional/vocational qualification and O levels or equivalent.

Discussion

The analysis of the previous section identifies a robust, causal impact of education on property crime. Results on violent crime are more volatile and no clear pattern emerged, most likely because of the much noisier feature of the data. However, the vast majority of crimes that occur are property crimes (these represent more than 70 percent of offences recorded by the police and indictable offences tried in courts). Given that we have identified a sizable crime reducing impact of education, it thus seems interesting to try to say something about the economic importance of such an effect. We have therefore carried out a simple, and in our view informative, calculation of the possible social savings that could result from such crime reduction.

Table 8 shows an estimate of the social benefits from crime reduction that would follow from a 1 percent reduction in the percentage of individuals with no educational qualifications. Using cost of crime estimates from Dubourg et al (2005) we calculate that the average cost of a property offence tried in court[17] comes to £1,235.5. There were 16,319 property offences convictions in 2002. We consider 2SLS/IV estimates from Table 6 and 7 of a 1 percent reduction in the population with no educational qualification on crime compared to two reference groups: individuals with some qualification (2.117) and individuals with low qualifications (1.051). This represents between respectively 345 and 118 fewer property crime cases being brought to court. Since only 0.4 percent of property crimes recorded end up with a court conviction, this translates into an estimated net crime reduction of between 88,469 and 43,921 offences. The corresponding figure in terms of social benefits from prevented crimes ranges from £109 to £54 million.

This is a substantial amount, even for the lower bound estimate comparing no versus low qualifications, especially if one considers that the average cost to the government of a

[17] In the OID, 16 percent of property offences are 'burglaries', 77 percent 'theft and handling of stolen goods', and 7 percent 'criminal damage'.

year of education for a secondary school student is approximately £4,000 (Goodman and Sibieta, 2006). Making the assumption that an extra year of schooling at age 16 is equivalent to obtaining an educational qualification[18], we estimate that this would cost a little under £22 million to achieve a one percent change in this population. This leaves us with a net social benefit in terms of crime reduction of between £87 and 32 million.[19]

Of course, this cost-benefit calculation should be carefully interpreted, exercising some degree of caution. For example, general equilibrium effects are not factored in, and we cannot measure the exact cost of obtaining an educational qualification. However, these seem unlikely to significantly offset the large social benefit estimates we obtain from our analysis.[20] We believe these social savings to be large, reaffirming the importance of considering crime reduction as an extra indirect benefit of education policies (as highlighted by Lochner's, 2010, review).

5. Conclusions

This paper presents new evidence on the effect of education on crime, looking at different data sources from Britain, and paying attention to the causal direction and magnitude of connections between the two. We uncover evidence that crime is significantly related to

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[18] We believe this to be a reasonable assumption, especially when considering the low qualification reference group.

[19] Our net social benefit estimate is much smaller than the $1.4 billion put forward by Lochner and Moretti (2004). The main reason is that we do not identify a clear impact of education on violent crime and especially murder which account for 80 percent of crime savings. When only considering prevented property crimes, then their estimate is just above $52 million or £35 million (at the average 1.5 £/$ exchange rate from 2002) which falls very close to our lower bound estimate of the social savings of crime.

[20] One way of thinking about general equilibrium effects would be to consider that the increase in the proportion of individuals with some qualification could *reduce* the wages of workers already with this education level. Considering the wage effects on crime with an elasticity of -1 as reported in Machin and Meghir (2004), it could be possible that it would *increase* the crime participation of the latter group. However we believe that this should be more than compensated by the *decrease* in crimes from the wage premium (estimated at around 40%) experienced by the individuals now obtaining some qualification.

education, especially in the case of property crimes. The magnitudes of the estimated effects are sizable, with causal estimates probably being larger than the non-causal least squares estimates we study. The estimated social savings from crime reduction implied by our estimates are large, being of the order of £54 to £109 million.

Other than Lochner and Moretti (2004) for the US and the results reported in this paper, evidence on the causal connection between education and crime is not available. The existence of a causal link leaves little doubt that the findings from this paper have important implications for longer term efforts aimed at reducing crime. For example, policies that subsidise schooling and human capital investment have significant potential to reduce crime in the longer run by increasing skill levels. Hence, improving education amongst offenders and potential offenders should be viewed as a key policy lever that could be used in the drive to combat crime.

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Table 1:
Imprisonment Rates (Percent), 2001 Census

	(1)		(2)		(3)		(4)	
	All		No Educational Qualifications		Some Educational Qualifications			
	Imprisonment Rate	Number of People	Imprisonment Rate	Number of People	Imprisonment Rate	Number of People	Gap in Imprisonment Rate Between No and Some Qualifications (Standard error)	Relative Risk Ratio
All	0.13	1183930	0.23	294871	0.09	804768	0.14 (0.01)	2.56
Men	0.25	587992	0.44	142373	0.17	393447	0.27 (0.02)	2.59
Men, Aged 16-20	0.34	62693	0.91	12048	0.18	45370	0.73 (0.06)	5.06
Men, Aged 21-25	0.57	57441	2.57	6176	0.30	46713	2.27 (0.10)	8.57
Men, Aged 26-30	0.42	61710	1.41	8036	0.27	50941	1.14 (0.08)	5.22
Men, Aged 31-64	0.16	406148	0.22	116113	0.13	250423	0.09 (0.01)	1.69
Women	0.01	595938	0.03	152498	0.01	411321	0.02 (0.003)	3.00
Women, Aged 16-20	0.01	60397	0.05	9856	0.01	45078	0.04 (0.01)	5.00
Women, Aged 21-25	0.04	57907	0.17	5229	0.02	48623	0.15 (0.03)	8.50
Women, Aged 26-30	0.02	62415	0.07	7153	0.01	55262	0.06 (0.02)	7.00
Women, Aged 31-64	0.01	392618	0.02	130260	0.01	262358	0.01 (0.003)	2.00

Notes: Based on 16-64 year olds in the 3% Census microdata sample.

Table 2:
Logit Estimates of Imprisonment Equations

	Coefficient (Standard Error)	Marginal Effect X 100	Relative Risk Ratio	Sample Size
All	1.417 (0.058)	0.18	4.12	1099639
Men	1.412 (0.060)	0.35	4.11	535820
Men, Aged 16-20	2.210 (0.152)	0.74	9.11	57418
Men, Aged 21-25	2.011 (0.122)	1.14	7.47	52889
Men, Aged 26-30	1.301 (0.144)	0.54	3.67	58977
Men, Aged 31-64	0.717 (0.092)	0.11	2.05	366536
Women	1.498 (0.254)	0.02	4.47	563819
Women, Aged 16-20	2.697 (0.754)	0.04	14.84	54934
Women, Aged 21-25	2.097 (0.510)	0.07	8.14	53852
Women, Aged 26-30	0.878 (0.699)	0.02	2.41	62415
Women, Aged 31-64	1.157 (0.346)	0.01	3.18	392618

Notes: As for Table 1. All specifications includes age dummies, 15 country of birth dummies, gender dummy (where applicable), non-white dummy, 5 marital status dummies, dummy for never worked, dummies for country.

Table 3:
Ever Been Arrested or Ever Been in Court as the Accused (Percentages), 2001-2007 British Crime Surveys

	(1)		(2)		(3)		(4)	
	All		No Educational Qualifications		Some Educational Qualifications			
A. Arrested	% Ever Been Arrested	N	% Ever Been Arrested	N	% Ever Been Arrested	N	Gap (Standard error)	Relative Risk Ratio
All	12.9	31349	16.6	7407	11.8	23942	4.9 (0.5)	1.41
Men	21.3	14440	29.8	3016	19.2	11424	10.6 (0.8)	1.55
Men, Aged 16-24	19.0	1847	30.8	251	17.2	1596	13.6(2.7)	1.79
Men, Aged 25-64	21.9	12593	29.6	2765	19.7	9828	9.9 (0.9)	1.50
Women	4.1	16909	5.9	4391	3.5	12518	2.3 (0.4)	1.69
Women, Aged 16-24	6.0	2173	11.6	364	5.0	1809	6.6 (1.4)	2.32
Women, Aged 25-64	3.7	14736	5.1	4027	3.1	10709	2.0 (0.3)	1.65
B. In Court	% Ever in Court as Accused	N	% Ever in Court as Accused	N	% Ever in Court as Accused	N	Gap (Standard error)	Relative Risk Ratio
All	9.0	47122	11.9	10837	8.2	36285	3.7 (0.3)	1.45
Men	15.2	21687	22.0	4460	13.5	17227	8.5 (0.6)	1.63
Men, Aged 16-25	9.6	2733	15.6	373	8.6	2360	7.0 (1.6)	1.81
Men, Aged 26-64	16.6	18954	23.0	4087	14.9	14867	8.1 (0.7)	1.54
Women	2.8	25435	3.7	6377	2.4	19058	1.3 (0.2)	1.54
Women, Aged 16-25	2.5	3272	4.7	532	2.1	2740	2.6 (0.7)	2.24
Women, Aged 26-64	2.8	22163	3.6	5845	2.5	16318	1.1 (0.2)	1.44

Notes: Based on the pooled 2001/2-2007/8 British Crime Surveys. The precise questions asked are: 'Have you ever been arrested by the police for any reason?' and 'Have you ever been in court as the person ACCUSED of committing a crime?'.

Table 4:
Logit Estimates of Ever Been Arrested/Ever Been in Court as the Accused Equations, 2001-2007 British Crime Surveys

	Ever Been Arrested	Ever Been in Court as the Accused
All	0.688 (0.043) [6.6] RRR = 1.99	0.581 (0.039) [4.1] RRR = 1.79
Men	0.666 (0.050) [12.3] RRR = 1.95	0.624 (0.044) [9.3] RRR = 1.87
Men, Aged 16-24	0.829 (0.157) [15.9] RRR = 2.29	0.943 (0.154) [11.7] RRR = 2.57
Men, Aged 25-64	0.644 (0.053) [11.8] RRR = 1.90	0.595 (0.046) [9.1] RRR = 1.81
Women	0.730 (0.082) [2.8] RRR = 2.08	0.444 (0.080) [1.3] RRR = 1.56
Women, Aged 16-24	0.964 (0.174) [9.4] RRR = 2.62	0.864 (0.207) [3.2] RRR = 2.37
Women, Aged 25-64	0.665 (0.092) [2.2] RRR = 1.94	0.377 (0.086) [1.1] RRR = 1.46

Notes: Coefficients on No Qualifications dummy variable (standard error in round brackets, marginal effect X 100 in square brackets, RRR is relative risk ratio). All specifications include age dummies, gender dummy (where applicable), non-white dummy, 5 marital status dummies, dummy for in work, dummy for Wales, year dummies.

Table 5:
Offending Rates and Education - Cohort Analysis

A. Log(Property Crime Convictions Per 1000 Population), by Age and Year, 1984-2002

	(1) Age Dummies (43) + Year Dummies (19)	(2) (1) + LFS Controls,	(3) (2) + NES Hourly Wage,	(4) (2) + NES Hourly Wage,	(5) (2) + NES Hourly Wage,	(6) (2) + NES Hourly Wage,
		All		Men	Women	Men, 21-40
Years of Education	-0.053 (0.055)	-0.175 (0.049)	-0.162 (0.049)	-0.147 (0.046)	-0.342 (0.169)	-0.187 (0.046)
No Qualifications	3.113 (0.195)	2.740 (0.190)	2.350 (0.231)	1.829 (0.218)	4.451 (0.872)	2.279 (0.294)
Sample	836	836	792	792	792	360

B. Log(Violent Crime Convictions Per 1000 Population), by Age and Year, 1984-2002

	(7) Age Dummies (43) + Year Dummies (19),	(8) (7) + LFS Controls,	(9) (8) + NES Hourly Wage,	(10) (8) + NES Hourly Wage,	(11) (8) + NES Hourly Wage,	(12) (8) + NES Hourly Wage,
		All		Men	Women	Men, 21-40
Years of Education	0.036 (0.059)	0.031 (0.060)	0.035 (0.064)	0.005 (0.062)	-0.037 (0.347)	-0.095 (0.060)
No Qualifications	-0.311 (0.238)	-0.312 (0.260)	-0.601 (0.339)	-0.294 (0.305)	0.113 (1.811)	-0.798 (0.409)
Sample	836	836	792	792	792	360

Notes: Models estimated on age-year cells, including a full set of age and year dummy variables, for samples as described in Table between 1984 and 2002. Standard errors in parentheses. LFS control variables included are: proportion male (in all sample), proportion employed, proportion non-white, and proportion living in London.

Table 6:
Offending Rates and Education - Cohort Analysis, Causal Estimates

	Log(Property Crime Convictions Per 1000 Population), by Age and Year, 1984-2002					
	Years of Education			No Qualifications Versus Some Qualifications		
	(1) OLS - Crime	(2) OLS - 1st Stage Education	(3) 2SLS - Crime	(4) OLS - Crime	(5) OLS - 1st Stage Education	(6) 2SLS - Crime
A. All						
Years of Education	-0.162 (0.049)		-0.511 (0.084)			
No Qualifications				2.350 (0.231)		2.117 (0.496)
SLA1		0.530 (0.030)			-0.071 (0.006)	
SLA2		0.640 (0.038)			-0.104 (0.008)	
F-test		F(2, 724) = 165.9 [P = 0.000]			F(2, 724) = 92.5 [P = 0.000]	
Sample Size	792	792	792	792	792	792
B. Men						
Years of Education	-0.147 (0.046)		-0.317 (0.088)			
No Qualifications				1.829 (0.218)		2.571 (0.548)
SLA1		0.553 (0.035)			-0.087 (0.007)	
SLA2		0.646 (0.043)			-0.093 (0.009)	
F-test		F(2, 724) = 130.5 [P = 0.000]			F(2, 724) = 66.6 [P = 0.000]	
Sample Size	792	792	792	792	792	792
C. Men, 21-40						
Years of Education	-0.187 (0.046)		-1.166 (0.372)			
No Qualifications				2.279 (0.294)		4.147 (0.848)
SLA1		N/A			N/A	
SLA2		0.098 (0.029)			-0.027 (0.004)	
F-test		F(1, 318) = 11.44 [P = 0.008]			F(1, 318) = 45.95 [P = 0.000]	
Sample Size	360	360	360	360	360	360

Notes: As for Table 5. All models include full sets of age and year dummies, plus LFS controls and NES wage. SLA1 = 1 for those with compulsory school leaving age of 15 (raised from 14 in 1947), = 0 otherwise; SLA2 = 1 for those whose with compulsory school leaving age of 16 (raised from 15 in 1973), = 0 otherwise. Men aged 21 to 40 are not affected (N/A) by SLA1 in the sample we have available.

Table 7:
Offending Rates and Education - Cohort Analysis,
Causal Estimates, Focusing on Particular Groups

Log(Property Crime Convictions Per 1000 Population), by Age and Year

	+/- 4 Birth Cohorts Around SLA2						**All Sample**		
	Years of Education			No Qualifications Versus Some Qualifications			No Qualifications Versus Low Qualifications		
	(1) OLS - Crime	(2) OLS - 1st Stage Education	(3) 2SLS – Crime	(4) OLS - Crime	(5) OLS - 1st Stage Education	(6) 2SLS - Crime	(7) OLS - Crime	(8) OLS - 1st Stage Education	(9) 2SLS - Crime
A. All									
Years of Education	-0.397 (0.124)		-0.653 (0.162)						
No Qualifications				1.732 (0.691)		3.902 (1.009)	1.237 (0.135)		1.051 (0.303)
SLA1		N/A			N/A			-0.106 (0.010)	
SLA2		0.241 (0.025)			-0.040 (0.005)			-0.162 (0.013)	
F-test		F(1, 98) = 95.1 [P = 0.000]			F(1, 98) = 68.6 [P = 0.000]			F(2, 724) = 76.8 [P = 0.000]	
Sample Size	144	144	144	144	144	144	792	792	792
B. Men									
Years of Education	-0.408 (0.124)		-0.598 (0.166)						
No Qualifications				1.175 (0.868)		6.421 (2.085)	0.728 (0.157)		0.952 (0.449)
SLA1		N/A			N/A			-0.067 (0.011)	
SLA2		0.245 (0.025)			-0.023 (0.005)			-0.135 (0.013)	
F-test		F(1, 97) = 96.19 [P = 0.000]			F(1, 97) = 23.40 [P = 0.000]			F(2, 724) = 59.99 [P = 0.000]	
Sample Size	144	144	144	144	144	144	792	792	792
C. Men, 21-40									
Years of Education	-0.349 (0.127)		-0.518 (0.175)						
No Qualifications				1.121 (0.801)		4.682 (1.811)	0.680 (0.161)		1.543 (0.329)
SLA1		N/A			N/A			N/A	
SLA2		0.226 (0.030)			-0.025 (0.006)			-0.074 0.007	
F-test		F(1, 76) = 55.6 [P = 0.000]			F(1, 76) = 18.0 [P = 0.000]			F(1, 318) = 102.05 [P = 0.000]	
Sample Size	116	116	116	116	116	116	360	360	360

Notes: As for Table 6.

Table 8:
Social Benefits from Decreasing Population with No Educational Qualification by 1 %

	No Qualifications Versus Some Qualifications	No Qualifications Versus Low Qualifications
2SLS Estimate of SLA Change of No Qualification Vs Reference Groups:	**Estimate 2.117**	**Estimate 1.051**
Cost in Anticipation of Crime	153.1	153.1
Cost as Consequence of Crime	723.4	723.4
Cost to the Criminal Justice System	359	359
Total Cost per Crime	1,235.5	1,235.5
Number of Convictions	16,319	16,319
Estimated Change in Convictions	345.5	117.5
Estimated Change in Crimes	88,469	43,921
Social Benefit from Crime Reduction	**£109,303,144**	**£54,264,338**
Cost per Student of One Year of Secondary School	4,000	4,000
Number of Pupils in Education at 16	546,729	546,729
Cost of 1% Increase or Extra Year of Education	£21,869,160	£21,869,160
Net Social Benefit from Crime Reduction	**£87,433,948**	**£32,395,178**

Notes: The cost of crime estimates are taken from Dubourg et al (2005). The estimated change in crime is adjusted by the number of crimes per conviction (i.e. 1/0.004 = 250). The cost of one year of secondary school per students is from Goodman and Sibieta (2006).

Appendix A - Census Establishments

Table A1:
Type of Communal Establishment, England, Wales and Scotland, Census 2001

Value	Label	Percentage
-9	Not Applicable	98.3
1	NHS psychiatric hospital	0.0
2	Other NHS hospital/home	0.1
3	LA Children's home	0.0
4	LA Nursing home	0.0
5	LA Residential care home	0.1
6	LA Other home	0.0
7	HA home or hostel	0.0
8	Nursing homes (not HA/LA)	0.3
9	Residential home (not HA/LA)	0.4
10	Children's home (not HA/LA)	0.0
11	Psychiatric hospital (not HA/LA)	0.0
12	Other hospital (not HA/LA)	0.0
13	Other medical and care home (not HA/LA)	0.0
14	Defence establishment (inc. ships)	0.1
15	**Prison service establishment**	**0.1**
16	Probation/bail hostel (not Scotland)	0.0
17	Educational establishment	0.5
18	Hotel/boarding house, guest home	0.1
19	Hostel (inc. youth hostel, hostels for homeless and persons sleeping rough)	0.1
20	Civilian ship, boat or barge	0.0
21	Other	0.1

Source: 2001 Individual CAMS Codebook, http://www.ccsr.ac.uk/sars

Appendix B - The Education Reforms

Background on the Schooling Reforms

The Education Act of 1944 laid the foundation for education in England and Wales as it is today (see Chan et al., 2002). The Act recognised the importance of education for economic advancement and social welfare. In terms of secondary education, its aim was to provide compulsory secondary education for all children so that every child had equal opportunity to obtain a place in a grammar school, regardless of family background. Most Local Education Authorities (LEA) interpreted the 1944 Act to mean the provision of schooling according to ability. The Act introduced a tripartite system into secondary schools, by using an 'objective' examination to test pupils' intelligence and abilities in English and arithmetic. This approach identified three groups of children: (i) Academic pupils, who went to the secondary grammar schools. These schools provided the main route to university; (ii) Practical pupils, who went to the technical schools that were vocationally based. There were very few technical schools because the cost of running them was high; and (iii) Remaining pupils, mostly working class, went to the secondary modern school where they received a more basic education.

The Education Act of 1944 also resulted in the two raisings of the minimum school-leaving age from 14 to 15 in 1947 and from 15 to 16 in 1973. The policy makers' motivation for increasing the school-leaving age was to "improve the future efficiency of the labour force, increase physical and mental adaptability, and prevent the mental and physical cramping caused by exposing children to monotonous occupations at an especially impressionable age" (Oreopoulos, 2006). Harmon and Walker (1995) show that the 1947 change was particularly influential in raising participation in post-compulsory education. That is, many of those who would otherwise have left at the old minimum stayed on beyond new minimum. Oreopoulos (2006) also shows that within two years of the 1947 policy change, the portion of 14-year-olds who left school fell from 57 to less than 10 percent.

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