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Larrikin Youth: New Evidence on Crime and Schooling

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Abstract

This paper reports new evidence on the causal link between education and male youth crime using individual level state-wide administrative data for Queensland, Australia. Enactment of the Earning or Learning education reform of 2006, with a mandatory increase in minimum school leaving age, is used to identify a causal impact of schooling on male youth crime. The richness of the matched (across agency) individual level panel data enables the analysis to shed significant light on the extent to which the causal impact reflects incapacitation, or whether more schooling acts to reduce crime after youths have left compulsory schooling. The empirical analysis uncovers a significant incapacitation effect, as remaining in school for longer reduces crime whilst in school, but also a sizeable crime reducing impact of education for young men in their late teens and early twenties. We also carry out analysis by major crime type and differentiate between single and multiple offending behaviour. Crime reduction effects are concentrated in property crime and single crime incidence, rather than altering the behaviour of the recalcitrant persistent offender.

Keywords: youth crime, schooling

JEL codes: I2; K42

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Larrikin: “A boisterous, often badly behaved young man”

[Noun, Australian, Oxford Dictionary]

1. Introduction

A small, but growing, body of economic research examines the causal link between education and crime. In this research, causality has been established using instrumental variable methods, with three key alternative approaches thus far adopted. Two of the approaches – from Jacob and Lefgren (2003) and Luallen (2006) use teacher training days and teacher strikes respectively to identify an exogenous shock in the number of days pupils spend in school. Given the small size and infrequency of such shocks, these evidently identify very local effects on crime. The alternative approach is both more general and more frequently adopted, and exploits changes or differences in compulsory school leaving age either spatially or temporally (or both) to generate exogenous increases in schooling level. For example, Lochner and Moretti (2004), Anderson (2014) and Bell, Costa and Machin (2016a, 2016b) exploit state level school dropout age reforms in the US, whereas Machin, Marie and Vujic (2011) and Hjalmarsson, Holmlund and Lindquist (2015) respectively analyse changes in the compulsory school leaving age in England and Wales and Sweden respectively.

The empirical literature based on exogenous variation generated from schooling reforms consistently reports a significant, often sizeable, causal relationship between increases in education and crime reduction. There are two credible explanations. First, additional schooling increases human capital which in turn increases the return from traditional labour market pursuits relative to a life of crime. Alternatively, staying in school

for longer may generate crime reduction via incapacitation which minimises opportunities for criminal activity.

The implications of the two alternate explanations differ. In the former more positive view, additional schooling equips students with higher valued labour market skills and anticipates a lifelong impact of reduced criminal activity. In the strongest form of the latter explanation, the incapacitation effect limits the opportunity for criminal activity only during the time period of extended schooling. However, if a more dynamic response is permitted, then incapacitation effects may spill over into future time periods post school, and thus also generate longer run crime reductions.¹ This possibility arises if the hazard function of initiating a life of crime spikes over the very early ages of 15 to 18, which in many countries correspond to the final years of compulsory schooling. In this case, incapacitation during this critical period may also have a long run impact. Finally, if age of crime onset shifts due to incapacitation then the shape of the crime-age profile may also alter (Costa, Bell and Machin, 2016b).

Given data limitations it has proven rather difficult to cast light on the relative importance of the above two major underlying explanations of the causal impact of schooling on crime. Jacob and Lefgren (2003), Luallen (2006) and Anderson (2014) all examine the contemporaneous relationship between crime and education. They find evidence to support the role of an incapacitation effect that reduces crime. The former two studies suggest that although property crime decreases due to an exogenous increase in school attendance, violent crime increases. Anderson (2014) on the other hand concludes that additional schooling decreases both property and violent crime.

¹ For theoretical approaches looking at crime dynamics and education see, for example, Mocan, Billups and Overland (2005) or Fella and Gallipoli (2014).

In this paper, we revisit the causal impact of education on crime, as well as studying the importance of incapacitation effects. To do so, we utilise a rich administrative data set incorporating matched individual level panel data on both schooling and criminal offences prior to and post compulsory school for the entire Queensland state school student population from 2002 to 2013. The longitudinal dimension at the level of the individual proves to be extremely valuable for pinning down evidence of short run incapacitation versus longer run deterrence effects of education on crime.

To ensure causality runs from schooling to crime, we exploit the exogenous variation induced by the introduction of the Earning and Learning reform which raised the school leaving age in Queensland in the 2006 school year.² Prior to the reform the minimum school leaving age was either 15 or completion of grade 10 (i.e. whichever occurred first). The reform defined a two year compulsory participation phase where individuals must engage in either learning or earning broadly defined, incorporating the options of staying on at school, enrolling in vocational education, an apprenticeship, or working full-time. The compulsory participation phase in turn lasts two years post-compulsory school age or until the individual turns age 17 (ACARA, 2009).

The rich micro data set also enables us to examine the issue of incapacitation at the level of individual offenders. We do this in two ways. First, we look at the interaction between exposure to the treatment - the Earning and Learning reform - and age and alleged criminal activity. Thus we examine the persistence of crime reduction after leaving school. Second, we study whether the policy reform altered the shape of the crime-age profile

² In Queensland, the school year runs from January to December. However, the enrolment window is different. A child can enter the current year grade 1 class if they are 6 years of age in January of the year of enrolment or turning 6 by 30 June of the year of enrolment (DET, 2016). This effectively means the enrolment window for grade 1 is from 1 July of the previous year to 30 June of the year of enrolment. As we have exact date of birth in the data – unlike in much of the work so far – we can match students exactly to school years. Access to micro data with exact date of birth means we can identify the birth cohorts affected by the reform accurately.

characterising young people in Queensland and whether patterns of offending behaviour altered.

To preview our findings, we uncover a sizeable causal impact of education on youth crime. This is the case for all crime, as well as by major crime type (that is, violent, property and drug offences). Moreover, part of the crime reduction works through an incapacitation effect as the impact of the schooling reform on crime is larger for individuals aged 17 and lower relative to those older than 17 (the boundary for the post-reform dropout age). Interestingly, the school reform effects in the crime reduced form for those age 17 and lower are a lot bigger for property crime, and not so different for violent and drug crime. So the in school incapacitation effect plays a pivotal role in deterring property crime.

We also find a crime reducing deterrence effect of more schooling for all criminal activity post age 17, so there is evidence of a longer run crime reduction induced by the reform. These patterns of incapacitation and later deterrence shift the crime age profile, so the peak occurs at a later age following the reform. Our longitudinal data which follows individuals aged 15 to 21 (both in and out of school) lets us examine the dynamics of the relationship in more detail. Analysing the observed crime age profile pre and post school reform reveals it is deterrence rather than age of onset due to incapacitation that alters the shape of the profile. Finally, we see that the crime reducing effects are concentrated in property crime and single crime incidence, rather than altering the behaviour of persistent offenders.

The remainder of the paper is structured as follows. In Section 2, we outline the empirical tests of the causal impact of schooling on crime, and extension of the framework to look at incapacitation and shifts in crime-age profiles. Section 3 describes the data and the process of matching administrative data on crime and education for the population of

young people in Queensland. Section 4 reports the estimates of the causal impact of schooling on crime. Section 5 reports the results from studying incapacitation effects, shifts in the crime age profile and changing patterns of offender behaviour. Section 6 offers some concluding remarks.

2. Empirical Models of Crime and Schooling

Crime and Schooling

Consider a basic relationship between crime (C) and schooling (S) for individual i at time period t :

$$C_{it} = \beta S_{it} + \gamma X_{it} + f(a, c, t) + \varepsilon_{it} \quad (1)$$

In (1), X is a set of control variables, $f(a, c, t)$ is a function (to be discussed below) of the individual's age (a), birth cohort (c) and time and ε_{it} an error term. The first empirical challenge, as well documented in prior work, is to obtain an unbiased estimate of β , the empirical association between crime and schooling. The main econometric issue is that schooling is unlikely to be exogenous in (1), as a likely correlation with the error term renders ordinary least squares biased. To circumvent this issue, we implement Instrumental Variable (IV) estimation of (1) where we use the Earning and Learning school leaving age reform (SL) as an instrumental variable.

In this set up, the two reduced form equations for crime and schooling (the first stage) are given respectively as:

$$C_{it} = \theta_1 SL_{it} + \pi_1 X_{it} + f(a, c, t) + \varepsilon_{1it} \quad (2)$$

$$S_{it} = \theta_2 SL_{it} + \pi_2 X_{it} + f(a, c, t) + \varepsilon_{2it} \quad (3)$$

An IV estimate of the impact of schooling on crime is given by the ratio of the reduced form coefficients in (2) and (3), θ_1/θ_2 . In general, if this IV estimate is negative then crime

reduction from schooling works through the combination of the two reduced form relationships with: θ_2 being positive if schooling laws increase education and $\theta_1 < 0$ if the schooling laws lower crime.

Lochner and Moretti (2004), together with subsequent authors in the field, note that θ_1 in the crime reduced form is likely to be negative for at least two distinct reasons:

- i) In the presence of a causal link between education and crime and if the raising of the school dropout age raises education levels, then the coefficient on SL in the reduced form for crime will pick up this effect.
- ii) The reform itself may directly reduce crime, over and above any effect on schooling.

Whilst we are interested in the causal impact of education on crime, and begin by presenting estimates of this, we also frame some of our empirical work around the crime reduced form in (2). In fact, as we next discuss, we examine incapacitation effects and shifting crime-age profiles utilising this reduced form relationship.

Incapacitation and Deterrence Effects

If there is a direct impact of school leaving laws on crime, how might it operate? One clear way is if the law change forces youth to be in a supervised environment rather than roaming the streets, so there is an incapacitation effect that reduces crime. There is a body of evidence to support this which uses plausibly exogenous changes in the length of the school day or exploits random days in which schools do not open to identify incapacitation effects (Jacob and Lefgren, 2003, and Luallen, 2006).³ Also, Anderson

³ Prior to this research that placed the question of establishing causality its forefront, there were earlier (non-causal) estimates of the impact of time spent in school on crime (see, for example, Gottfredson, 1985, Farrington et al., 1986 or Witte and Tauchen, 1994). Hjalmarsson (2008) studied the impact of being arrested and incarcerated before finishing school on the probability of graduating high school, reporting there to be a strong negative association.

(2014) examines whether students affected by a dropout reform show different responses by age.

In a similar vein to Anderson (2014), we examine whether the treatment effect from school reform varies by age. More specifically, we estimate separate specifications of the crime reduced form for individuals younger and older than the post-reform dropout age of 17. This corresponds to estimating the following crime reduced forms:

$$C_{it} = \theta_1^{a \leq 17} S_{L_{it}} + \pi_1^{a \leq 17} X_{it} + f^{a \leq 17}(a, c, t) + \varepsilon_{1it}^{a \leq 17} \quad (4a)$$

$$C_{it} = \theta_1^{a > 17} S_{L_{it}} + \pi_1^{a > 17} X_{it} + f^{a > 17}(a, c, t) + \varepsilon_{1it}^{a > 17} \quad (4b)$$

Estimating separate treatment effects (the θ_1 's) therefore enables us to explore whether incapacitation effects are operating in the $a \leq 17$ sub-sample.

The second way in which we study incapacitation versus deterrence builds upon the insight that crime onset often begins in the teenage years and that criminal behaviour peaks in the late teenage years (as in the life course approaches to crime of Sampson, and Laub, 1993, 1995). Thus one sees an inverse U-shape in the crime-age profile, which has often been modelled by imposing a quadratic functional form.⁴ If the incapacitation effect reduces criminal activity at these crucial ages, it may in addition generate a persistently lower crime rate as the cohort ages, since some of the cohort members will have avoided going down the wrong path at a crucial age.

⁴ Almost two hundred years ago, Quetelet (1831) showed a peak in the late teens for crime in early nineteenth-century France. A mass of subsequent research has confirmed the strong age-crime pattern, with crime peaking in the late teens and declining with age quite rapidly. Hirschi and Gottfredson (1983) proposed that crime-age profiles are broadly invariant over time and across demographic groups. Such a view has been challenged empirically by a number of authors. Examples are Greenberg (1985) who presents evidence that both the peak crime age and the rate of subsequent decline differs across crime types, localities, race and gender and Hansen (2003) who shows that the age-crime profile differs for those who leave school at the compulsory school leaving age and those who remain in education..

We thus study whether, and if so how, the crime-age profile changes before and after the school reform (see Bell, Costa and Machin, 2016b, for a framework that links changing crime age profiles to changes in dropout age). To explain how we do this we need to place some structure upon the age-cohort-year $f(a, c, t)$ function. As is well known, separating age, cohort and time effects offers an empirical challenge because of the identity $t \equiv c + a$. A fully saturated specification would include a full set of additive age, cohort and time effects (respectively denoted by α_a , α_c and α_t) such that $f(a, c, t) = \alpha_a + \alpha_c + \alpha_t$. This fully saturated model is not identified as at least one age, cohort or year dummy needs to be excluded as the reference group. Deaton (1997) carefully discusses this, and also states that an alternative is to require the cohort effects to sum to zero.

The school reform SL is a cohort level policy reform, with the first enrolment cohort affected being the 1990/91 birth cohort who are 16 during the 2006 school year. Thus, we study this with a regression discontinuity research design that looks at what happens before and after the reform for school cohorts born near to the first treated 1990/91 cohort.⁵ We use birth cohort as a running variable in this design. We therefore can include a full set of age and year effects in our empirical specifications (see below).

We also estimate a more restricted specification that imposes a quadratic structure on age to pick up the well-established pattern of crime-age profiles commonly found in existing research. We present estimates of possible changes in the crime-age profile before and after the school reform using both the unrestricted age dummies and the quadratic functional form.

⁵ For discussion of the standard regression discontinuity (RD) framework, and associated applications, see Imbens and Lemieux (2008) or Lee and Lemieux (2010)

The final set of issues we study are enabled by the nature of our rich longitudinal data following the same individuals from different birth cohorts over time. Specifically we examine whether there are pre and post reform changes in the offending behaviour of individuals aged between 15 and 21. We consider two specific dimensions. First, we study whether the reform has an effect on engaging in any form of offending, or whether it affects multiple offending. This lets us consider whether crime reductions are likely to generate desistence from those on the margins of crime participation in their teenage years, or whether it can act to reduce criminality amongst more persistent offenders. Second, and closely related to our study of possible shifts in crime age profiles, we consider whether age of onset – that is, the first time an individual commits a crime – differs before and after reform.

3. Data and Descriptive Statistics

Data

Much of the empirical literature examining the relationship between education and crime is forced to match across criminal offences data and individual level data that records education. The reason is that most data sets recording criminal offences at the individual level do not contain information on education levels. In general the demographic data recorded along with criminal offences is limited to age and gender (and sometimes race). The existing literature then utilises these characteristics to derive average education level by birth cohort (age) and gender from the alternative data source. This matching then requires aggregation of individual level data into birth cohorts by gender to match the information on education and thus the entire focus is on the relationship between education and crime at the cohort level.

We are in the fortunate position of obtaining Queensland administrative data matched at the individual level across State agencies, Department of Education and Training (DET) and the Queensland Police Service (QPS). Thus, we have individual record data for the entire population of attendees at all Queensland Government funded schools, together with matched individual criminal offence data for the period 2002 to 2013. Our focus in this paper is on males aged 15-21. For most of the analysis, we work with a balanced panel that covers approximately 138,000 15-21 year old males, each of whom is observed for seven years. The precise sample structure is shown in Table 1, where the treatment group cells (those affected by the school reform, discussed in more detail below) are in bold.

The crime data refers to alleged criminal offences in a given year, and so our focus is on whether an individual in a given year is an alleged offender. An alleged offender is a person who has allegedly committed a crime and has been processed for that offence by arrest, caution or warrant of apprehension. We have matched these data at individual level to the Queensland schooling data. In the latter we observe individuals until they complete their compulsory education.

We consider two measures of schooling. The first is the number of years of compulsory education, which can reach a maximum of 12 years. Prior to the education reform we assume that the final observed year of grade school completed the individual's education. For years post completion we assume schooling is fixed. Before the school reform students could leave school at age 15 with 10 years of completed schooling. After the reform, students had to stay on until they turn 17 years of age, which translates to completing 12 years of grade/high school education. A second measure, which is useful to show the nature of the school reform is whether or not the individual completed the equivalent of 11 years of schooling (broadly defined) or not.

Thus a major advantage of our data relative to that employed by previous literature is that we can match education and criminal offence data at the individual level. We follow the same individual through the education system and beyond, simultaneously tracking criminal offences. As Table 1 shows, we are able to follow individuals who are not affected by the school reform and complete their compulsory schooling under the old regime (e.g. the 1987/88 enrolment cohort); those who are directly affected (the first cohort being the 1990/91 enrolment cohort who were turning age 16 in the first year of enactment of the reform); and those who are always in the new regime (e.g. the 1992/93 cohort). We thus study six birth cohorts tightly banded around the Earning and Learning reform, enabling a difference in difference approach within a regression discontinuity setting. This allows us to identify both the causal effect of education on crime, and to study the changing nature of the crime reduced form in detail.

Descriptive Analysis

Figure 1 shows what happens to the two schooling measures for the six cohorts in the window around the school reform (denoted by the vertical line between the 1989/90 and 1990/91 cohorts). There is a very clear – and statistically significant – jump after the reform. The proportion of 17 year olds with at least 11 years of schooling jumps from around 75 to over 90 percent for the first post-reform cohort (as the discontinuity estimate, expressed in proportions as 0.156, shown in the subtitle of the left hand Figure demonstrates). Two years post-reform the proportion almost reaches the ceiling at 98 percent, as one would expect if the policy is binding.⁶ The right hand chart of Figure 1 shows the same cohort pattern for

⁶ For example, of the 43,475 16 years olds enrolled in grade 11 in 2007, 43,172 (i.e. 99 percent) continued on to grade 12 in 2008.

the average years of schooling, with a significant jump up by 0.33 of a year, which again rises a little further two years post reform.

Figure 2 shows the pattern for youth crime. There is a clear drop in the offending measure to the right of the discontinuity. The proportion drops from just under 0.08 to just under 0.07, or just under a one percentage point change (the strongly significant discontinuity estimate is -0.009, as shown in the Figure). Thus as more schooling was induced by the reform, male youth crime fell.

Table 2 shows cohort by cohort means of the schooling and crime measures for both the pooled sample of 15-21 year olds (thus mixing up young men in and of school) as well as the sample restricted to 17 year olds only. It is very clear that the schooling measures rise significantly after the enactment of the Earning and Learning reform, and also that the proportion offending falls. These are the basic facts that underpin the empirical investigation of crime and schooling, which we turn to in the next two sections of the paper.

4. The Causal Impact of Schooling on Youth Crime

Baseline Estimates

The first main set of statistical results are presented in Table 3. The Table shows four sets of estimates of various crime and education specifications for two samples of male youth in Queensland. The upper panel A shows estimates for the discontinuity sample of six cohorts, three before and three after the Earning and Learning reform. The lower panel B shows an even tighter window around the discontinuity for the 1989/90 through 1991/92 cohorts.

In each panel, four specifications are shown. Specification (1) is the OLS estimate of a regression of crime on years of schooling, with a full set of age and year dummies, plus a linear cohort variable. These same controls are included in the other specifications, namely

the two reduced forms, for years of schooling in specification (2) and for crime in specification (3). The final column (4) shows the causal estimates of education on crime, where the estimates are the ratio of those in (3) to (2).⁷

The first thing to note is that results from the +/- 3 cohort window in panel A are very similar to the narrower +/- 2 window in panel B and so we can discuss results in general. The OLS specification shows that crime and education (column (1)) are negatively related to one another, with a statistically significant coefficient of -0.018 in both panels. Thus, as in prior work in the area, male crime and education are negatively correlated.

Turning to the years of schooling first stage, and perhaps not at all surprisingly given the patterns shown in Figure 1, specification (2) uncovers a strong and binding impact of the Earning and Learning reform on years of schooling. In the upper panel 0.26 more years of schooling accrue on average to the male youth affected by the reform. This is slightly lower at 0.17 in the lower panel due to dropping of the 1992/93 cohort. Thus the age 16 youths in 1990/91 (the first cohort affected by the reform) who could only gain 1 rather than 2 extra years receive a bigger weight than in panel A.

The crime reduced form in specification (3) confirms the earlier graphical representation of the data, as crime amongst male youth is seen to be significantly lower in the post-reform treated cohorts.⁸ Finally, specification (4) shows a strong and significant causal crime reducing effect of education. The instrumental variable estimate in panel A is -0.030 and strongly significant. This amounts to a substantive reduction in crime induced

⁷ Table 3 shows results for the completed years of schooling measure. Qualitatively very similar results emerged if we modelled education as a dummy variable for ≥ 11 years or for ≥ 12 years, the results of which are available on request.

⁸ Marginal effects from probit models of crime incidence revealed almost identical estimates (that only differ after the third decimal place) for the crime reduced form: for the specification (3) in panels A and B, the probit marginal effects (and associated standard errors) were the same as Table 3, respectively being -0.008 (0.002) and -0.006 (0.002).

by increased schooling. To place the result in context the pre-reform mean offending rate was around 0.080.

Estimates By Crime Type

In Table 4 we show estimates for violent crime, property crime and drug crime as separate components of all crime. Schooling is seen to exert a significant causal impact on crime for all three types. Thus the causal effects are not manifesting themselves differentially, by moderating only some kinds of broad crime categories. In terms of the education reducing impact of crime on average, this applies to violent, property and drug offences.

Robustness

We probe these causal estimates in a number of directions that the data permit. In particular, we consider a number of issues of robustness with our causal results, as follows:

- 1). To date our analysis has focussed on a balanced panel (7 waves per individual) of 15 to 21 year olds in birth cohorts three years before and after the Earning and Learning reform. We do have data for a larger unbalanced panel, covering all young people in school for at least one year between 2002 and 2013. Appendix Table A1 shows results for this unbalanced panel for male youth crime participation, in a comparable way to the main all crime results reported in Table 3. The results show a reassuring pattern, with identification of a strong causal impact of schooling on crime. The only differences that occurs reflects the nature of the sample, which is now unbalanced around the schooling reform, and hence the first stage education impact is a little higher than in Table 3 at 0.358 additional years of schooling. The crime reduced form is also more negative, at -0.009. The structural form results show a strongly significant crime reducing causal effect of schooling of -0.028 (which is very similar to the -0.030 estimate from the balanced panel reported in panel A of Table 3).

2). Appendix Tables A2 and A3 show results comparable to Tables 3 and 4, but now using the number of offences per year rather than the yearly incidence measure we considered to date. In terms of interpretation and qualitative results the overall story remains the same. . The only slight difference is that effects seem a bit more pronounced for property crimes, which is not surprising as, in terms of numbers of offences, they are the major offence category amongst young people.

3). If an older age cut-off is utilised to define the study sample, extending from ages 15 to 25 inclusive, the same overall pattern of results persists. For this wider sample, the estimate (standard error) of a causal impact of schooling on crime comparable to specification (4) in panel A of Table 3 is -0.028 (0.005). However, adding in older age observations makes the sample unbalanced and skewed to the post-reform period. This is not so useful for the empirical analysis on incapacitation effects, crime-age profiles and individual offending patterns that we move to in the next section of the paper. So, for that, we stick with the balanced panel structure we have used for our main causal results.

5. Incapacitation and Crime-Age Profiles

Incapacitation Effects

To consider possible incapacitation effects due to students mandatorily staying in school to a later age, Table 5 shows separate estimates of the crime reduced form for young men aged 17 and below and above 17 . Estimates are shown for all crime (in specification (1)) and separately for violent, property and drug offences (in specifications (2) through (4)).

The all crime results from specification (1) show a significantly lower crime incidence for both age groups for those treated by the Earning and Learning reform. However, there is a suggestion of a more negative effect – at -0.9 of a percentage point

compared to -0.7 for the 17 and under age group. This more negative estimate is in line with an incapacitation effect operating amongst those kept in the classroom up to a later age.

Interestingly, when different crime types are considered (in (2) to (4)) this size of effect discrepancy seems to be much more pronounced for property crimes. Indeed, the violent crime effect is not as negative for the 17 and under age group and the drug crime effect is numerically the same. Thus incapacitation seems to reduce property crimes by more, a finding obtained in a different setting, but one that is qualitatively in line with the US studies of Jacob and Lefgren (2003) and Luallen (2006).⁹

Shifts in Crime-Age Profiles

The longitudinal dimension of our data allows us to follow young men over time and as they age between 15 and 21. This enables us to consider incapacitation effects from being kept in school and longer run crime deterrence effects that occur after leaving school by means of studying possible shifts in individual specific crime-age profiles. We examine crime-age profiles before and after the reform based on the crime reduced form specifications incorporating two different functional forms for age. In the upper panel A of Table 6, estimates are shown for age dummies (where the omitted reference group is age 15), and in the lower panel B for a quadratic in age.

As one would expect from the discussion of incapacitation effects for those above and below age 17, the estimated crime-age profiles in panel A show reduced crime incidence at lower ages and then less of a flattening at older ages. The same is true in panel B where

⁹ To be more precise, this study and the two cited US studies report significant property crime reductions from incapacitation. The violent crime effect for the age 17 and under group in this study is still negative and statistically significant at the 5 percent level, whereas it is actually estimated to be positive in the two US studies. Thus the common feature is that property crime reductions form the most important dimension of crime reduction from incapacitation. This contrasts with Anderson (2014) for whom property and violent crimes (and for that matter drug crimes) seem to be significantly reduced by similar amounts.

the inverse U-shape flattens, and where solving the quadratic leads to the estimated peak age rising from 18.8 to 19.1 years pre versus post reform. Thus the maximum crime incidence is shifted upwards in terms of age, resulting from more crime reduction at younger ages.

This changing nature of the crime-age profile is shown in Figure 3. The chart in the upper level quadrant is produced from the numbers in panel A of Table 6. It very clearly shows crime incidence falling, but by more at the younger ages. Thus incapacitation and deterrence are both important. The other three charts in Figure 3 show different patterns by type of crime. It seems that the key dimension of crime reduction for younger individuals arises from lower property crime. The crime falls are much more neutral by age for violent and drug offences.

Changes in Offending Behaviour

We next use the longitudinal dimension of the data to explore whether there are changes in offending behaviour pre and post the Earning and Learning reform. We study two dimensions of changing criminal behaviour, repeat offending and age of onset. Table 7 summarises various estimates of the crime reduced form.

Columns (1) to (4) of the Table shows estimates which enable us to consider whether the crime reductions due to the Earning and Learning reform reflect desistance from criminal behaviour from individuals who before the reform would have offended but after the reform did not, or whether it altered multiple offending. Column (1) shows that the likelihood of ever offending between ages 15 and 21, for our sample of 137814 young men, was significantly reduced post-reform. The mean of ever offending for the group of young men not affected by the reform was 0.276, and this falls by 0.049 (or an 18 percent reduction) for those affected. On the other hand when one considers changes amongst

offenders, there is no evidence of altered behaviour before and after the reform. Specifications (2) to (4) consider the likelihood of engaging in offending behaviour in multiple years. The results suggest there is no evidence of a shift in the probability of committing offences in two or more years, three or more years nor in all seven years.

Finally, in the last column (5) of the Table we consider whether age of offending onset is affected by the reform. The mean age of onset for those not affected by the reform was 17.56, and the estimates show that this rose by a fairly modest 0.07 for those affected. This is statistically significant at the 10 percent level and is supportive of the earlier evidence that crime age profiles are altered by the reform, owing to slightly bigger crime reduction effects operating via incapacitation for those that remaining in school longer post-reform.

6. Conclusions

In this paper, we use rich administrative data on education and crime covering several cohorts of all individuals attending state school in Queensland, Australia between the 2002/03 and 2013/14 school years. We study the causal links between male youth crime and education, using a school reform introduced in the 2006/07 school year that extended the school dropout age by two more years.

The first key finding of the paper is the uncovering of a strong causal crime reducing effect of education for young men. This confirms evidence from elsewhere in the world in which education policy reforms increasing minimum school leaving age have had the dual effect of significantly raising education participation and deterring criminal activity. The effects are sizeable, and emerge for participation in all offending behaviour, and also within the distinct domains of violent, property and drug crime.

The rich longitudinal information we have, which follows around 140,000 young men through compulsory schooling and beyond, means we can probe further to understand what underlies this causal impact. We report evidence of crime reduction being due to both incapacitation whereby young men are compelled to remain in school longer, but also of deterrence effects that go beyond the schooling years. Digging deeper reveals that this balance of larger crime reductions for those who are kept in school operates mostly through a reduction in property crime, rather than violent or drug related offences. The presence of incapacitation effects also shifts the crime age profiles such that they peak at a later age post-reform, and modestly raise the age of onset of crime. Finally, the crime reductions seem to operate through permanent deterrence for those affected by the reform as there is a large drop in the proportion of young men who ever offend between ages 15 and 21, but no change in multiple offending that occurs pre and post the reform. Overall, it seems that, as one might intuitively expect, the education reform brought about desistance in crime from male youth on the margins of offending in their teenage years rather than reductions in criminality among persistent offenders.

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Figure 1: Education Before and After the Earning and Learning Education Reform

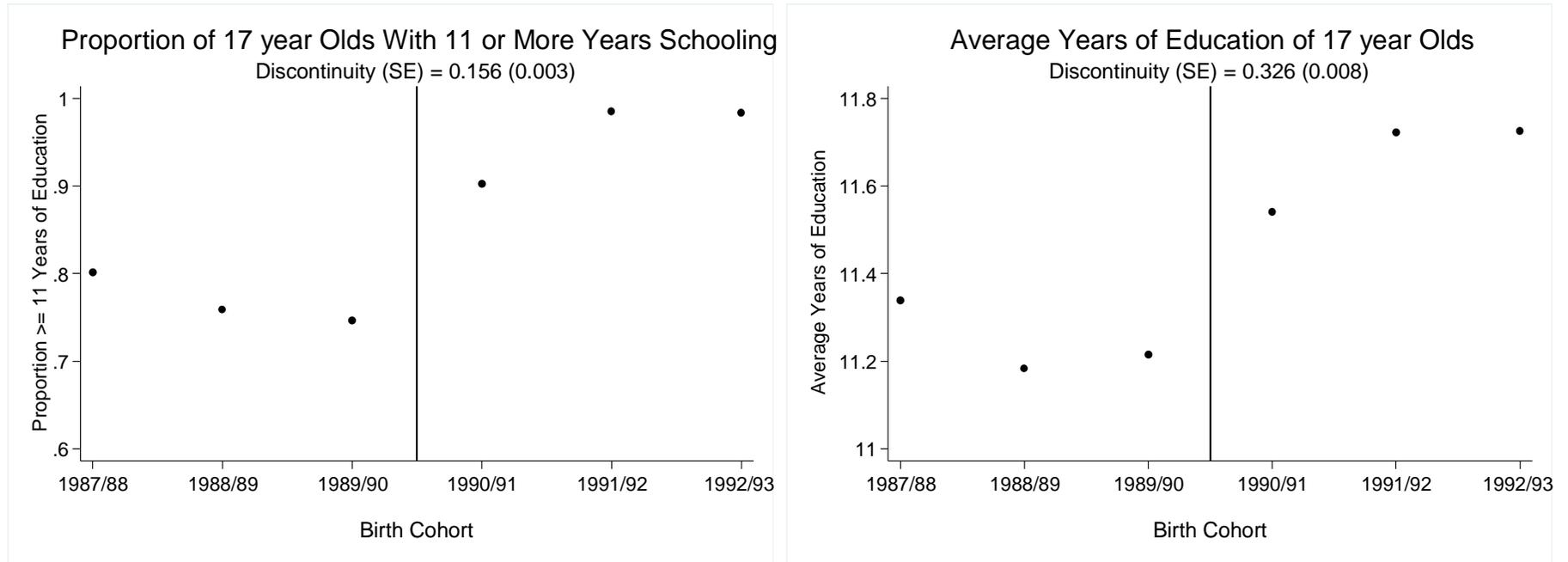


Figure 2: Youth Crime Incidence Before and After the Earning and Learning Education Reform

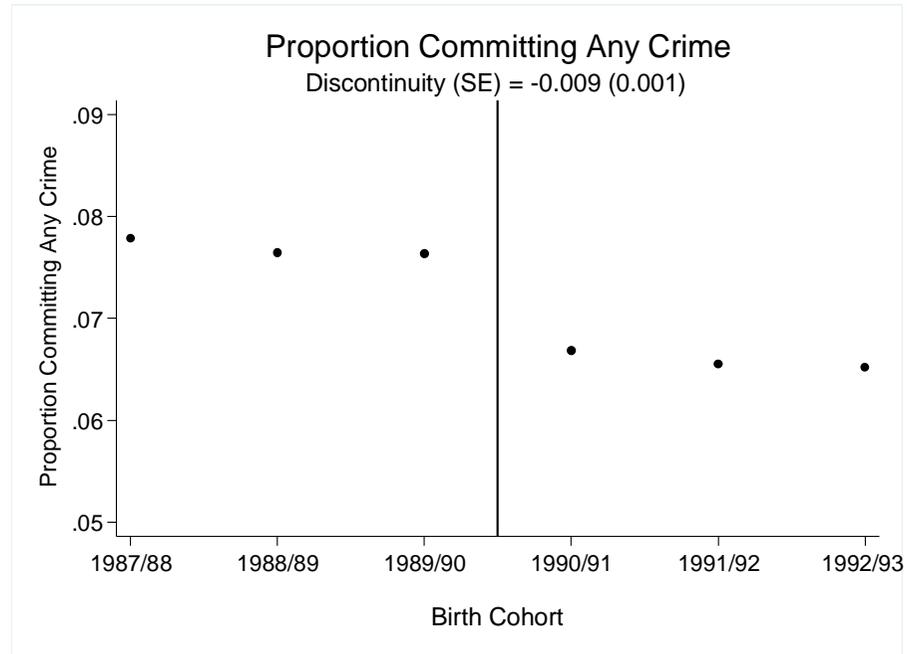


Figure 3: Youth Crime Age Profiles Before and After the Earning and Learning Education Reform

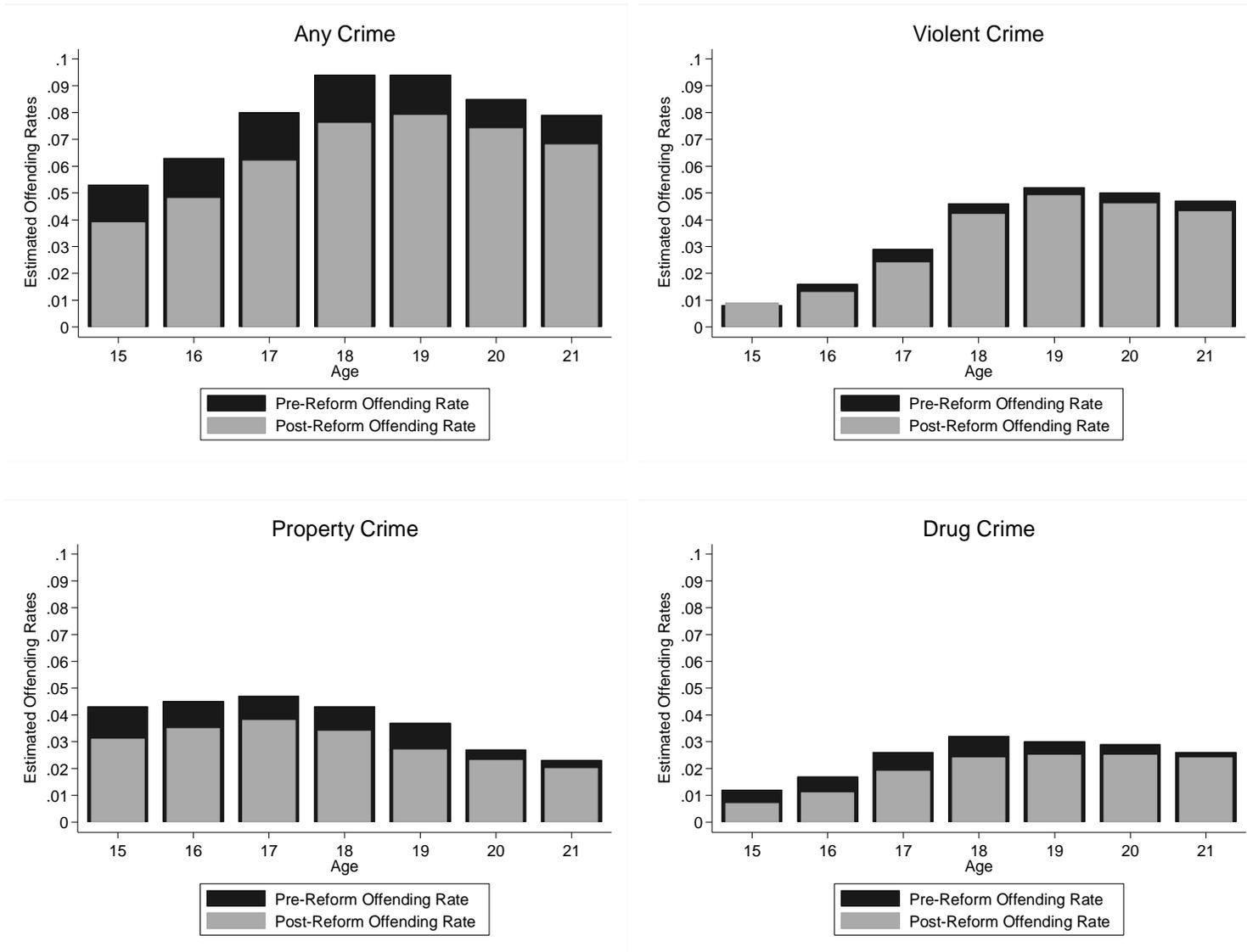


Table 1: Sample Structure of Age Group By Year and Birth Cohort

		School Year											
		2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
Birth Cohort	1987/88	15	16	17	18	19	20	21					
	1988/89		15	16	17	18	19	20	21				
	1989/90			15	16	17	18	19	20	21			
	1990/91				15	16	17	18	19	20	21		
	1991/92					15	16	17	18	19	20	21	
	1992/93						15	16	17	18	19	20	21

Notes: Birth cohorts correspond to school enrolment year, which runs from July 1 to June 30. Age is the year that an individual from a given birth cohort will turn in the school year. So, for example, an individual in birth cohort 1990/91 will turn 16 in school year 2006. The Earning and Learning reforms came into place on 1 January 2006, so for Queensland youth aged 15 or more the first affected cohort that had to stay on beyond age 15 was the 1990/91 birth cohort.

Table 2: Descriptive Statistics

Birth Cohort	Ages 15-21			Age 17		
	Proportion With 11 or More Years of Schooling	Average Years of Schooling	Proportion Offending	Proportion With 11 or More Years of Schooling	Average Years of Schooling	Proportion Offending
1987/88	0.678	11.065	0.076	0.802	11.339	0.078
1988/89	0.630	10.943	0.079	0.759	11.183	0.076
1989/90	0.620	10.961	0.077	0.747	11.215	0.076
1990/91	0.748	11.207	0.068	0.903	11.542	0.067
1991/92	0.815	11.352	0.064	0.985	11.722	0.066
1992/93	0.815	11.356	0.062	0.984	11.726	0.065
Change (1990/91 to 1992/93) – (1987/88 to 1989/90)	0.158 (0.002)	0.332 (0.004)	-0.013 (0.001)	0.196 (0.002)	0.438 (0.004)	-0.011 (0.001)
Number of individuals	137814	137814	137814	137814	137814	137814
Sample size	964698	964698	964698	137814	137814	137814

Notes: Standard errors, clustered at person level, in parentheses.

Table 3: The Causal Effect of Education on Male Youth Crime, Any Offence

	Probability of Committing An Offence, Male Youth Aged 15-21, 2002 to 2013			
	Crime	Years of Schooling	Crime	Crime
	OLS	OLS First Stage	OLS Reduced Form	IV Structural Form
	(1)	(2)	(3)	(4)
A. 1987/88 to 1992/93 Birth Cohorts				
Years of Compulsory Schooling	-0.018 (0.001)			-0.030 (0.007)
Earning or Learning Reform Dropout Age Increase		0.264 (0.008)	-0.008 (0.002)	
First Stage F-test		1080		
Age Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Number of Individuals	137814	137814	137814	137814
Sample Size	964698	964698	964698	964698
B. 1988/89 to 1991/92 Birth Cohorts				
Years of Compulsory Schooling	-0.018 (0.001)			-0.038 (0.014)
Earning or Learning Reform Dropout Age Increase		0.165 (0.011)	-0.006 (0.002)	
First Stage F-test		246		
Age Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Number of Individuals	96817	96817	96817	96817
Sample Size	677719	677719	677719	677719

Notes: Standard errors clustered by individual in parentheses. All specifications include a linear birth cohort variable.

Table 4: Male Youth Crime and Education, By Crime Type

Probability of Committing An Offence, Male Youth Aged 15-21, 2002 to 2013						
	Violent Crime		Property Crime		Drug Crime	
	OLS Reduced Form	IV Structural Form	OLS Reduced Form	IV Structural Form	OLS Reduced Form	IV Structural Form
	(1)	(2)	(3)	(4)	(5)	(6)
A. 1987/88 to 1992/93 Birth Cohorts						
Years of Compulsory Schooling		-0.010 (0.004)		-0.015 (0.004)		-0.013 (0.003)
Earning or Learning Reform Dropout Age Increase	-0.003 (0.001)		-0.004 (0.001)		-0.003 (0.001)	
Age Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of Individuals	137814	137814	137814	137814	137814	137814
Sample Size	964698	964698	964698	964698	964698	964698
B. 1988/89 to 1991/92 Birth Cohorts						
Years of Compulsory Schooling		-0.015 (0.008)		-0.014 (0.009)		-0.013 (0.006)
Earning or Learning Reform Dropout Age Increase	-0.003 (0.001)		-0.002 (0.001)		-0.002 (0.001)	
Age Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of Individuals	96817	96817	96817	96817	96817	96817
Sample Size	677719	677719	677719	677719	677719	677719

Notes: As for Table 3.

Table 5: Youth Crime Reduced Forms and Incapacitation Effects

	Probability of Committing An Offence, Male Youth Aged 15-21, 2002 to 2013			
	Any Crime	Violent Crime	Property Crime	Drug Crime
	OLS Reduced Form	OLS Reduced Form	OLS Reduced Form	OLS Reduced Form
	(1)	(2)	(3)	(4)
A. Age≤17				
Earning or Learning Reform Dropout Age Increase	-0.009 (0.002)	-0.002 (0.001)	-0.006 (0.002)	-0.003 (0.001)
Age Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Number of Individuals	137814	137814	137814	137814
Sample Size	413442	413442	413442	413442
B. Age>17				
Earning or Learning Reform Dropout Age Increase	-0.007 (0.002)	-0.004 (0.002)	-0.002 (0.001)	-0.003 (0.001)
Age Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Number of Individuals	137814	137814	137814	137814
Sample Size	551256	551256	551256	551256

Notes: As for Table 3, Panel A specifications (1), (3) and (5).

Table 6: Crime Age Profiles and Dropout Age

Probability of Committing An Offence, Male Youth Aged 15-21, 2002 to 2013		
Any Crime		
	OLS Reduced Form Before Dropout Age Increase	OLS Reduced Form After Dropout Age Increase
	(1)	(2)
A. Age Dummies		
Age = 16	0.010 (0.001)	0.009 (0.001)
Age = 17	0.027 (0.002)	0.023 (0.001)
Age = 18	0.041 (0.002)	0.037 (0.002)
Age = 19	0.041 (0.002)	0.040 (0.002)
Age = 20	0.032 (0.002)	0.035 (0.002)
Age = 21	0.026 (0.002)	0.029 (0.001)
Number of Individuals	54272	83542
Sample Size	379904	584794
Age 15 Proportion Committing An Offence	0.053	0.039
B. Age Quadratic		
Age	0.107 (0.007)	0.088 (0.005)
Age ² / 100	-0.283 (0.018)	-0.223 (0.013)
Number of Individuals	54272	83542
Sample Size	379904	584794

Notes: Standard errors clustered by individual in parentheses. All specifications include a linear birth cohort variable.

Table 7: Change in Offender Behaviour Before and After Reform

	Ever Offends	Offends in More Than One Year	Offends in More Than Two Years	Offends in All Seven Years	Onset Age
	OLS Reduced Form	OLS Reduced Form	OLS Reduced Form	OLS Reduced Form	OLS Reduced Form
	(1)	(2)	(3)	(4)	(5)
Earning or Learning Reform Dropout Age Increase	-0.049 (0.005)	-0.009 (0.011)	-0.007 (0.009)	0.001 (0.002)	0.072 (0.040)
Year Dummies	Yes	Yes	Yes	Yes	Yes
Number of Individuals	137814	34421	34421	34421	34421
Pre-Reform Mean of Dependent Variable	0.276	0.481	0.248	0.009	17.559

Notes: Standard errors in parentheses.

Appendix – Additional Results

Table A1: The Causal Effect of Education on Male Youth Crime, Any Offence, Unbalanced Sample

	Probability of Committing An Offence, Male Youth Aged 15-21, 2002 to 2013			
	Crime	Years of Schooling	Crime	Crime
	OLS	OLS First Stage	OLS Reduced Form	IV Structural Form
	(1)	(2)	(3)	(4)
Unbalanced Panel				
Years of Compulsory Schooling	-0.017 (0.001)			
Earning or Learning Reform Dropout Age Increase		0.387 (0.005)	-0.011 (0.001)	-0.028 (0.003)
First Stage F-test		6499		
Age Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Number of Individuals	395057	395057	395057	395057
Sample Size	2096123	2096123	2096123	2096123

Notes: Standard errors clustered by individual in parentheses. All specifications include a linear birth cohort variable. For different orders of cohort polynomials entered, the specification (4) IV estimates (standard errors) were as follows: quadratic, -0.034 (0.003); cubic, -0.034 (0.003).

Table A2: The Causal Effect of Education on Male Youth Crime, Number of Offences

	Number of Offences, Male Youth Aged 15-21, 2002 to 2013			
	Crime OLS (1)	Years of Schooling OLS First Stage (2)	Crime OLS Reduced Form (3)	Crime IV Structural Form (4)
1987/88 to 1992/93 Birth Cohorts				
Years of Compulsory Schooling	-0.093 (0.004)			-0.131 (0.041)
Earning or Learning Reform Dropout Age Increase		0.264 (0.008)	-0.035 (0.011)	
First Stage F-test		1080		
Age Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Number of Individuals	137814	137814	137814	137814
Sample Size	964698	964698	964698	964698

Notes: Standard errors clustered by individual in parentheses. All specifications include a linear birth cohort variable.

Table A3: Male Youth Crime and Education, Number of Offences, By Crime Type

Number of Offences, Male Youth Aged 15-21, 2002 to 2013						
	Violent Crime		Property Crime		Drug Crime	
	OLS Reduced Form	IV Structural Form	OLS Reduced Form	IV Structural Form	OLS Reduced Form	IV Structural Form
	(1)	(2)	(3)	(4)	(5)	(6)
1987/88 to 1992/93 Birth Cohorts						
Years of Compulsory Schooling		-0.023 (0.014)		-0.072 (0.029)		-0.024 (0.006)
Earning or Learning Reform Dropout Age Increase	-0.006 (0.003)		-0.019 (0.007)		-0.006 (0.002)	
Age Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of Individuals	137814	137814	137814	137814	137814	137814
Sample Size	964698	964698	964698	964698	964698	964698

Notes: As for Table A2

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