Why Are Criminals Less Educated than Non-Criminals? Evidence from a Cohort of Young Australian Twins

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Many studies find a strong negative association between crime and education. This raises the question whether crime reduces investment in human capital or whether education reduces criminal activity. This article investigates posed question by using fixed-effect estimation on data of Australian twins. We find early arrests (before the age of 18) both to have a strong effect on human capital accumulation, as well as strong detrimental effects on adult crime. Schooling does not have an effect on adult crime if there is variation in early arrests. However, schooling reduces crime if there is little variation in early crime. (*JEL code: I2, K42*).

1. Introduction

Many studies document a strong negative association between education and crime. For instance, in the US, two-thirds of all incarcerated men in 1993 had not graduated from high school (Freeman 1996). Studies that use self-reported and (administrative) arrest data find large differences in property and violent crime across education groups (Tauchen et al. 1994; Lochner 2004). However, the relationship between crime and education is not straightforward. Does crime reduce investment in human capital or does education reduce criminal activity?

This article studies the relationship between human capital and crime using data of a sample of young Australian twins. We exploit two aspects of the

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Australian survey data on education and crime. First, we are able to control for many unobserved characteristics affecting both criminal behavior and schooling decisions as the data are obtained from fraternal and identical twins. Second, as criminal behavior is measured over different periods of time—prior to and after senior high school completion—we can address the causality between crime and education as well. As early criminal behavior may affect human capital formation, and human capital may influence criminal behavior in later stages of life, we follow a two-step analysis.

First, we study the relationship between early crime and the accumulation of human capital. In particular, we estimate the effect of arrests before the age of 18 on educational attainment by using within-twin estimation. To strengthen our results, we also investigate whether the timing of the arrest matters for educational attainment. Second, we estimate the effect of educational attainment on three measures of crime: incarceration, arrests since the age of 18 and a number of arrests. As early criminal behavior might be an important confounder in the estimation, we control for early arrests and measures of conduct disorder within pairs of twins.

Our article contributes to the economic literature on the relationship between education and crime in several aspects. First, the empirical economic literature on human capital and crime that takes unobserved factors into account is limited. Two previous studies arguably use exogenous variation in human capital to investigate the effect of education on crime (Lochner and Moretti 2004; Machin et al. 2011). Both studies use changes in compulsory schooling laws as an instrument for educational attainment and find that education reduces crime. We add to this literature by using an identification strategy that has not been applied before—that is, we exploit the longitudinal nature of our data in order to estimate the relationship between human capital and crime in both directions. Second, we investigate the effect of early criminal behavior on investment in human capital while controlling for fixed effects within pairs of twins. Except for Hjalmarsson (2008) study, we are not aware of other studies in the economic literature that estimate the causal effect of early criminal activity on educational attainment. Third, there is growing interest in the economic literature about the effects of early conditions in life on adult outcomes (Currie and Stabile 2006, 2007; Borghans et al. 2008). Our article addresses similar issues.

Our main finding is that the causality between education and crime runs in both directions. First, we find that early arrests (i.e., arrests before the age of 18) have a strong effect on human capital accumulation. In particular, early arrests reduce educational attainment with 0.7 to 0.9 years of education and lower the probability of completing senior high school with 20 to 23 percentage points. These effects are largely driven by the timing of the early arrest; arrests at age 13, 14, or 15 are most detrimental for educational attainment. The estimates are found after controlling for conduct disorder and early school performance. These findings are to a large extent based on the sample of fraternal twins. The estimates of the effect of early crime for the sample of identical

twins are less informative because of the small number of twin pairs that differ in early arrests.

Second, we find that human capital reduces crime. As early criminal activity might be an important confounder, we control for early arrests. For the sample of fraternal twins, we find no effect of human capital on adult crime in models that take early arrests into account. For the sample of identical twins, we find that human capital has a negative effect on crime. In addition, the size of these estimates might be downward biased because of measurement error in schooling. Instrumental variables (IV) estimates using a second independent measure of schooling suggest that the effect of human capital might be larger.

When combining these findings, it seems that both relationships between human capital and crime are important. For fraternal twins, the impact of early criminal behavior on human capital formation dominates the impact of human capital formation on future crime behavior. Controlling for early arrests and early behavior problems strongly reduces the estimated effect of human capital. For identical twins, which hardly differ in early criminal behavior, we find that human capital reduces crime. The strong detrimental effects of early criminal behavior become also transparent if we consider the estimated effects of early arrests on all three measures of crime. We then find large effects of early criminal behavior on participation in crime later on. These effects are much larger than the (isolated) impact of human capital on crime.

We conclude that the strong association between human capital and crime is to a large extent driven by the effect of early criminal behavior on educational attainment. Programs that succeed in preventing early criminal behavior might therefore yield high social and private returns.

2. Previous Studies

The major difficulty in studying the relationship between human capital and crime is that both variables are driven by a multitude of unobserved factors. For instance, a person's level of schooling is typically not randomly determined but the result of individual choices and ability. These individuals might also have unobserved factors that prevent them from committing crimes. Unobserved factors that are both correlated with the decision to invest in human capital and the decision to participate in crime will confound the empirical relationship between education and crime. As such, ordinary least squares (OLSs) estimates of the effects of human capital on crime or OLS estimates of the effects of crime on human capital are likely to be biased.

The first part of this article focuses on the effect of early criminal behavior on human capital formation. To our knowledge, there are not many previous economic studies that empirically estimate the effect of early crime on investment in human capital while taking unobserved factors into account. For example, Hjalmarsson (2008) studies the impact of being arrested and incarcerated before finishing school on probability of graduating high school. Her results suggest that the more times you are caught committing crime and the amount of time spent in prison both greatly increase the likelihood of becoming a high

school dropout. Some recent studies in health economics investigate the effect of childhood mental health problems such as ADHD, aggression, antisocial behavior, and depression on human capital accumulation later in life (Le et al. 2005; Currie and Stabile 2006, 2007; Slade and Wissow 2007; Fletcher and Wolfe 2007). These studies typically find large negative effects of childhood mental health problems on educational attainment. Other related literature focuses on the importance of cognitive and noncognitive skills for labor market outcomes and social behavior (Carneiro and Heckman 2003; Heckman et al. 2006; Heckman and Masterov 2007; Borghans et al. 2008). These studies stress the importance of skills development early in life for human capital accumulation and success later in life. Early schooling programmes, such as the Perry Preschool Programme (PPP), the Syracuse Programme (SP), or the Head Start Programme (HSP) have proven to be highly effective in reducing criminal activity, promoting socioeconomic skills, and integrating disadvantaged children into mainstream society (see for instance Schweinhart et al. 1993; Donohue and Siegelman 1998; Lally et al. 1988; and Garces et al. 2002). These social, motivational, and emotional skills affect performance in school and in the workplace. Programmes that aim at intervening in the lives of children in their teenage years only attempt to redress the damage of bad childhoods (Carneiro and Heckman 2003).

The second part of this article studies the causal effect of human capital on crime. So far, only two articles in the economic literature try to establish a causal relationship between education and crime (Lochner and Moretti 2004; Machin et al. 2011). Both studies use changes in compulsory school leaving age laws in order to account for the endogeneity of schooling decisions. Using the US Census data, Lochner and Moretti (2004) show that one more year of schooling reduces the probability of incarceration by 0.37 percentage points for blacks and 0.10 percentage points for whites. They corroborate these results using FBI Uniform Crime Reports (UCR) data for different types of offenses and conclude that the greatest impacts of graduation are associated with murder, assault, and motor vehicle theft. The authors also calibrate the social savings from crime reduction associated with completing secondary education. They show that a 1% increase in male high school graduation rates would yield $1.4 billion dollars in social benefits in 2004 dollars. Machin et al. (2011) study the crime reducing potential of education, presenting causal statistical estimates based upon a law that changed the compulsory school leaving age in England and Wales in 1973.The authors frame the analysis in a regression-discontinuity setting and uncover significant decreases in property crime from reductions in the proportion of people with no educational qualifications and increases in the age of leaving school that resulted from the change in the law.

As mentioned before, these two studies use an IV approach and typically estimate a local average treatment effect for the particular subgroup of the population that is affected by the instrument (a change in compulsory schooling). We expect that this subgroup consists of those at the lower end of the education distribution. Our approach (see next section) uses variation over the whole

distribution of education that may bring the advantage that our estimates are applicable to a broader population.

Theoretical work on the relationship between human capital and crime has been done by Lochner (2004). He developed a model of crime in which human capital increases the opportunity costs of crime. The model predicts that older, more intelligent, and more educated adults should commit fewer street (unskilled) crimes. It is also expected that white-collar crime should decline less with age and education than unskilled crime. These predictions receive broad empirical support in self-reported data for the United States.

3. Empirical Strategy

In this article, we use variation within pairs of twins in order to study the relationship between education and crime. Obviously, the advantage of twin data is that many (unobserved) characteristics that twins share—like socioeconomic background and family factors—can be controlled for. Within-twin estimation has been used in several studies on the returns to schooling (see for instance, Ashenfelter and Krueger 1994; Miller et al. 1995), on the effect of women's education on the education of their children (Behrman and Rosenzweig 2002) and on the effect of spousal education on earnings (Huang et al. 2009).

In order to get a full picture of the relationship between human capital formation and criminal behavior, our estimation strategy consists of two separate analyses. First, we focus on the relationship between early criminal behavior and educational attainment. Early criminal behavior is measured as the event of being arrested before the age of 18. It is likely that these early criminal activities occur during the time that the accumulation of human capital is still in progress because compulsory schooling laws force individuals in Australia to attend schooling until the age of 15 to 17, depending on the State of residence. In order to estimate the effect of early arrests on educational attainment, we use the usual linear (probability) model for within-family estimation:

$$S_{ij} = \alpha_0 + \alpha_1 A_{ij}^{17} + \alpha_2 X_{ij} + f_j + \epsilon_{ij} \quad (1)$$

where S_{ij} is the educational attainment of individual i in family j, A_{ij}^{17} is a dummy for being arrested before the age of 18, X_{ij} a vector of covariates, f_j is an unobserved family effect common to all twins in family j, and ϵ_{ij} is a random error term. In this model, the family-fixed effects, which consist of all shared socioeconomic and genetic factors, are removed by differencing between twins. In equation (1), we expect that the causality primarily runs from early arrests to educational attainment, as early arrests occur before the completion of schooling. We argue that we can largely control for reverse effects—that is, bad school performance driving kids to start criminal activities—by including several measures of early school performance as additional controls. Moreover, we control for differences in early behavior within pairs of twins by including an indicator of conduct disorder (see next section).

The second part of our analysis addresses the effect of human capital on crime since the age of 18, which is usually the perspective that is taken in the literature. The model we estimate is very similar to equation (1):

$$C_{ij}^{18} = \beta_0 + \beta_1 S_{ij} + \beta_2 X_{ij} + \beta_3 A_{ij}^{17} + g_j + \eta_{ij} \quad (2)$$

where C_{ij}^{18} is the criminal activity since the age of 18. As early criminal activity is likely to be an important confounder for the estimated effect of human capital on crime, we include early arrests as an additional control. We argue that these lagged arrests can be treated as exogenous variables. Equation (2) can also be seen as a value-added model or a growth model. By including early arrest in the model, we are able to control for unobserved differences within pairs of twins that are related with early criminal behavior. Note that we do not use early arrest as an instrumental variable because it does not seem plausible that the exclusion restrictions will hold. That is, we do not think likely that the effect of early arrest on adult crime will only be channeled through the effect of early arrest on education.

Obviously, the twin setup—together with the use of lagged information—helps us to cancel out many possible sources of endogeneity. Still, there are two important concerns in the use of within-twin estimation (Bound and Solon 1999) which need to be addressed in order to check the robustness of our results. First, measurement error in (self-) reported schooling (or crime) may bias the estimates toward zero (attenuation bias). A solution for this problem has been introduced by Ashenfelter and Krueger (1994). They obtained two measures of schooling of twins by asking the twins to report both on their own schooling as well as on the schooling of their twin sibling. The second measure of schooling can then be used as an instrument to correct for measurement error. This approach has been used in several studies (for instance Miller et al. 1995; Behrman and Rosenzweig 2002). In these studies, the size of the estimated effects increases after instrumenting for measurement error. This article follows the same approach in order to address any attenuation biases.

The second concern in the within-twin models is with respect to endogeneity bias within twin pairs. Although (identical) twins share many genes and are raised in the same social environment, they are not exactly identical. Bound and Solon (1999) show that the within-family estimator bias is not necessarily smaller than the cross-sectional estimator bias. This depends on the importance of the fixed family component in the unobservables. We address this possible bias by using additional controls in the within-twin models, such as conduct disorder and early arrests.

4. Description of Data

We use data from the so-called younger cohort of twins of the Australian Twin Register (ATR). This cohort consists of a sample of 4262 twin pairs born between 1964 and 1971. The twins were registered with the ATR as children by their parents in response to media appeals and systematic appeals through

the school system in the period 1980–1982. The ATR data that we use in this article were gathered in two surveys. In 1989–1990, when the twins were 18–25 years old, the first survey by mailed questionnaire was conducted, called *Alcohol Cohort 2*. The response rate of this questionnaire survey was 63%. In 1996–2000, the second survey was launched, called *TWIN89*. For this survey, telephone interviews were completed with 6267 individuals, 2805 men (889 complete and 1027 incomplete pairs) and 3462 women (1215 complete and 1032 incomplete pairs). At the time of the interview, the twins were 24–36 years old (on average 30 years). The individual response rate for this telephone interview was 86%.

The surveys gathered information on the respondent's family background (parents, siblings, marital status, and children), socioeconomic status (education, employment status, and income), health behavior (body size, smoking, and drinking habits), conduct disorder, personality, feelings, and attitudes. Zygosity was determined by a combination of diagnostic questions plus blood grouping and genotyping.

The measures of crime used in the analysis are self-reported data on arrests and incarceration. The survey contains questions on the age of first and last arrest, the number of arrests and incarceration. The questions explicitly exclude arrests for traffic violations, drunken behavior, or drunk driving.[1] The question on incarceration excludes time spent in jail for using drugs or alcohol.

The reliability of these self-reported data is an important issue. In criminology, the use of self-reported data is well established. Self-reported data collection has been the dominant technique used for measuring criminal behavior since its introduction in the 1950s by Short and Nye (1957). A large literature shows that self-reported data have consistently acceptable reliability and validity. Many studies find high correlations of self-reported data with other criterion-related measures of criminal frequency and arrest histories (Farrington 1973; Maddux and Desmond 1975; Hardt and Peterson-Hardt 1977; Huizinga and Elliott 1986; Mieczkowski 1990; Horney and Marshall 1992a, 1992b; Weiss et al. 1998). Thornberry and Krohn (2000) conclude that "self-reported measures of delinquency are as reliable as, if not more reliable than, most social science measures." A recent study among street-drug users recruited in 11 cities throughout the United States revealed that lifetime arrest and incarceration items demonstrated good-to-excellent reliability (Fisher et al. 2004). In addition, it has been shown that substance abuse factors and mental illness factors did not affect the quality and accuracy of self-reported arrest history (Nieves et al. 2000).

Educational attainment was measured in the first survey using a seven-point scale and translated into years of education (Miller et al. 1995). The second survey of the younger cohort uses an eight-point scale which we also translate

1. According to the Australian Institute of Criminology (1998), in 1995/1996 of all non-Aboriginal juveniles, 9.5% appeared in court for drug offenses and 8.2% for driving offenses. The vast majority of offenses, however, like offenses against property (40.7%) and those against good order (18.9%), are covered by the ATR sample.

into years of education (Miller et al. 2006). We prefer to use this more recent measure, as it contains less missing values for our main estimation sample.

As covariates we use mother's and father's education and age. In addition, we control for conduct disorder and early school performance. Our data contains self-reported information on 21 statements that reflect behavioral problems before the age of 18 (see Table A.1). In Webbink et al. (2011), we constructed a measure of conduct disorder by summing occurrences of these 21 statements. In this article, we excluded the items most related to committing crimes from the conduct disorder variable because the implicit inclusion of crime indicators in conduct disorder might generate a bias that could attenuate the effect of interest. We used these crime-related items (see Table A.1 in Appendix A: questions L8; L8a; L8b; L9; L10; L11; L12; L15; L16; L17; L20) in order to construct a separate variable "early crime score" which we used in the sensitivity analysis. The survey contains four questions on early school performance. Grades in primary and secondary education were measured using a three-point scale: better than average, average, and below average. Respondents were also asked about the teacher's view on their school achievements: did as well as could, could have done much better, don't know. Finally, grade repetition was measured.

In our total sample of 6267 individuals, 70 twins reported having spent time in jail and 340 twins reported having been arrested, which is 1.1% and 5.4% of our sample, respectively. Approximately, 10% of male twins and 2% of female twins reported having been arrested. A direct comparison with population statistics is complicated because of differences in reporting measures. Statistics on alleged offenders in Australia in the period from 1995 to 2005 show that among males aged 15–19 approximately 9% to 13% get arrested and among females 2 to 3% (Australian Institute of Criminology 2007). For individuals aged 20–24, the rates drop to 6% to 9% for men and 2% for women, and for individuals older than 24, the rates drop further to approximately 1%. It should be noted that the number of alleged offenders does not equal the number of distinct offenders during a year because police may take action against the same individual for several offenses or the individual may be processed on more than one occasion for the same offense type. In addition, we might expect that many of those arrested since the age of 20 will be recidivists. As such, a direct comparison of the arrest rates found in our data with population statistics is difficult. However, the difference between males and females seem in line with the population statistics. In addition, the total arrest rates in our sample do not seem implausibly high or low.

The samples we use in the main estimations consist of pairs of twins with information on educational attainment and criminal participation. If this information is missing for one or both of the twins, we dropped the complete pair. In these samples, 47 twins reported having spent time in jail and 224 twins reported having been arrested. This includes 6 twin pairs (12 twins) who both reported having spent time in jail and 28 twin pairs (56 individuals) who both reported having been arrested. Our data contain information on the zygosity of the twins, enabling us to distinguish fraternal and identical twins.

We estimate our models for separate samples of identical twins and fraternal twins only. However, it should be noted that only 14 pairs in the sample of identical twins differ in early arrest. The sample of fraternal twins contains 56 twins that differ in being arrested before the age of 18. Hence, a separate analysis on the sample of identical twins strongly reduces the sample size and especially the variation within pairs of twins on the main variables of criminal behavior. The intraclass correlation for being arrested (incarcerated) is 0.31 (0.41) for identical twins and 0.07 (0.13) for fraternal twins. Unfortunately, due to the routing of the questionnaire[2] twins with a conduct disorder score of zero did not answer questions on criminal behavior. As this may bias the estimates, we did robustness analysis with imputations for missing values on these outcomes for twins with no childhood conduct disorder (see section 7).

Table 1 shows sample means and proportions for educational attainment and background characteristics by criminal participation. The first two columns compare twins that spent time in jail with twins that have not been incarcerated. The last two columns compare twins that have ever been arrested with twins that have never been arrested. The sample size slightly differs between the first two columns and the last two columns because of missing values on "incarceration" or "having been arrested." Clearly, the sample statistics show a strong association between educational attainment and participation in crime. Twins that have been incarcerated attain on average 1.6 years less education than twins that have not been incarcerated. The difference in educational attainment between those that have ever been arrested and those that have never been arrested is on average 1 year. Most strikingly, two-thirds of individuals who have been incarcerated did not graduate from senior high school, compared with only one quarter of the remaining group of those who have not been incarcerated. Twins who participated in crime have lower educated parents, and the difference between the columns is larger for those who spent time in jail. Male twins are more likely to be involved in criminal activity.

The bottom panel shows the statistics on conduct disorder and early school performance. The difference in conduct disorder is striking: twins that were incarcerated score approximately 5 points (2 standard deviations) higher on the indicator of conduct disorder. For twins that have been arrested this difference is more than two points. We also observe that twins that have been incarcerated or arrested have a higher grade repetition rate. The differences in self-reported grades in primary and secondary school seem quite modest. Moreover, the first arrest occurs much earlier for twins that have been incarcerated than for other twins who have been arrested.

We further explore the association between human capital and crime by looking at the relationship between education and arrests.

2. *Routing* is the term for instructing the interviewer or respondent to skip questions depending on the answers to previous questions. In our data, due to the *routing of the questionnaire*, twins with a conduct disorder score of zero, which means that they reported negative on all 21 statements on conduct disorder before the age of 18, did not answer questions about arrests and incarceration.

Table 1. Summary Statistics for the Main Estimation Samples

	Spent time in jail		Ever arrested	
	No	Yes	No	Yes
Education	11.9	10.3	12.0	11.0
	(2.4)	(2.3)	(2.4)	(2.4)
Senior high school	75.0	36.2	76.7	52.2
	(43.3)	(48.6)	(42.3)	(50.0)
Education (twin report)	11.7	10.2	11.7	10.9
	(2.3)	(2.1)	(2.3)	(2.3)
Education father	10.4	9.5	10.4	9.9
	(2.7)	(2.5)	(2.7)	(2.6)
Education mother	10.4	9.7	10.4	10.1
	(3.1)	(2.9)	(3.1)	(2.8)
Male	53.1	85.1	51.1	78.1
	(50)	(36)	(50.0)	(41.4)
Age in 1996	29.8	29.8	29.8	29.9
	(2.5)	(2.7)	(2.5)	(2.5)
Conduct disorder	3.3	8.1	3.3	5.8
	(2.5)	(3.4)	(2.4)	(3.4)
Grades primary school (1–3)	2.3	2.1	2.3	2.2
	(0.6)	(0.7)	(0.6)	(0.6)
Grades secondary school (1–3)	2.2	2.0	2.2	2.0
	(0.6)	(0.7)	(0.6)	(0.6)
Underachiever (%)	71.1	76.0	70.8	77.0
	(45.3)	(43.1)	(45.5)	(43.1)
Grade repetition (%)	18.1	31.9	18.1	24.6
	(38.5)	(47.1)	(38.2)	(0.43)
Age of first arrest	20.3	18.6	NA	19.9
	(4.6)	(4.7)	NA	(4.7)
Identical twin	40.7	40.4	40.7	40.6
	(49.1)	(49.6)	(49.1)	(49.2)
Estimation sample	2199	47	2028	224
Total sample	6197	70	5927	340

NA, not applicable.

For each schooling level, Table 2 shows the proportions for several measures of criminal participation. We see that criminal participation is concentrated at the two lowest schooling levels. Twins that did not complete 11 years of education are more likely to be arrested and to be incarcerated. In addition, the number of arrests is higher for those with less than 11 years of education. We also observe that many arrests of those with less than 11 years of education already take place at an early age. Moreover, the arrest rates since the age of 18 of this group are much higher than those for twins with at least 11 or 12 years of education. Table 2 also makes apparent that criminal participation is fairly stable for those with at least 11 or 12 years of education. This suggests a nonlinear relationship between human capital and crime. Completion of senior high school (11–12 years of education) seems to be a critical boundary in this respect. Lochner and Moretti (2004) report a similar nonlinear

Table 2. Arrests by Schooling Level (%)

	Years of schooling					
	≤7	8–10	11–12	13	15	17
Ever arrested (%)	40	18.3	7.1	6.8	7.2	6.1
First arrest						
≤15 years	20	4	0.6	0.5	0.7	1.1
16 years	0	1.1	0.4	0	0.3	0.6
17 years	0	2.9	0.8	1.0	0	0
18 years	0	2.3	1.1	1.9	0.7	0.5
19 years	0	0.9	0.8	1.0	0.7	0.6
Ever arrested since 18 years	40	14.3	6.0	5.3	6.2	4.5
Number of arrests						
0	60	81.9	93.2	93.2	92.8	93.9
1	0	10.5	4.5	5.3	5.8	5.0
2	0	3.7	1.3	1.5	1.4	0.6
≤3	40	4.0	1.0	0	0	0.6
Spent time in jail (%)	20	5.1	1.1	0.5	1.4	0.6
Sample size	5	574	995	207	292	179

relation between education and crime for the US. In particular, they find a steep drop in criminal participation at the level of high school graduation.

For many countries and time periods, it has well been established that crime rates increase during the teenage years, peak around the age of 20 and decrease afterward (Lochner 2004). This age-crime profile is well documented in literature (Blumstein et al. 1986; Farrington 1986; Gottfredson and Hirschi 1986; Grogger 1998). This relationship between age and crime is robust over time, across countries, demographic subgroups, types of crime, and holds irrespective of the way the crime is measured (Hirschi and Gottfredson 1983). Figure 1 shows age-crime profiles from our data based on the self-reported age of first arrest. The left figure shows an age-crime profile for individuals with less than 11 years of education. The right figure shows an age-crime profile for individuals who completed at least 11 years of education (senior high school). The patterns in Figure 1 confirm the typical features of age-crime profiles found in the literature. That is, participation in crime increases until the age of 20 and drops afterward. A comparison of the left and right figures suggests that individuals with less than 11 years of education start earlier with criminal activities.

5. The Effect of Early Arrests on Educational Attainment

The strong association between education and criminal activity might be the result of early participation in crime. Early criminal involvement might be detrimental for human capital investment because of various reasons such as "meeting the wrong friends" (building criminal capital), "getting stigmatized," and changes in motivation or aspirations. In this section, we investigate

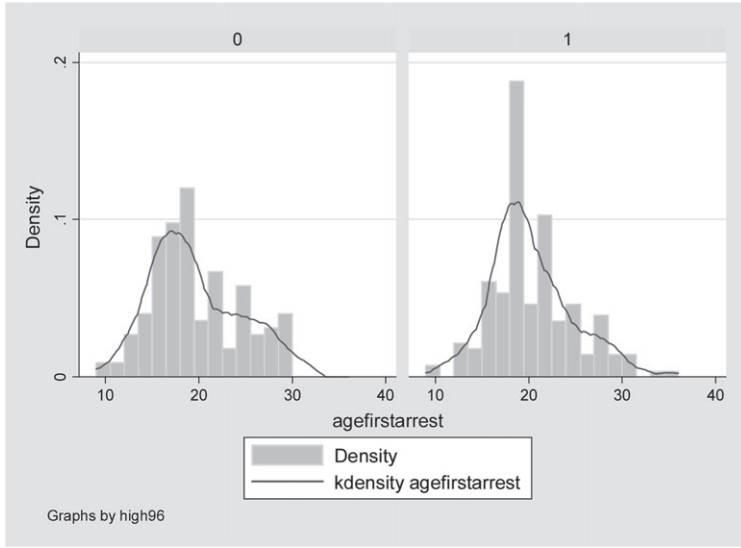

Figure 1. Age of First Arrest by Schooling Level. Source: Author's calculations. Note: Figure shows age-crime profiles based on the self-reported age of first arrest. The left figure shows an age-crime profiles for individuals with less than 11 years of education. The right figure shows an age-crime profile for individuals with less than 11 years of education (senior high school).

the effect of early arrest on human capital accumulation by estimating linear (probability) models of early arrests on education. Table 3 shows estimates of the effect of early criminal participation on human capital. We used the information on the age of first arrest as an indicator for early criminal participation and constructed a dummy for early arrests, which equals 1 (0) if someone had (not) been arrested before the age of 18. Column (1) shows the OLS estimates of the effect of early arrests on educational attainment controlling for gender, age, age squared, and education of parents. Column (2) includes as additional controls conduct disorder and early school performance (grades in primary school [1–3], grades in secondary school [1–3], grade repetition, and teacher's view on under-achievement). Column (3) shows the fixed-effects estimates which control for gender, whereas column (4) also controls for conduct disorder and early school performance. Finally, columns (5) and (6) show the estimates for separate samples of fraternal and identical twins. The top panel of Table 3 shows the effect of early arrests on years of education, whereas the effect of early arrests on completing senior high school are shown at the bottom of Table 3.

All estimates in Table 3 based on the total sample of twins (columns (1) to (4)) suggest that early arrests have a substantial impact on human capital accumulation. The cross-sectional estimates show that those who are arrested before the age of 18 attain 0.9 to 1.5 less years of education and their probability of completing senior high school is 28 to 38 percentage points lower.

Table 3. Estimates of the Effect of Early Arrests on Educational Attainment

	OLS All (1)	OLS All (2)	FE All (3)	FE All (4)	FE Fraternal (5)	FE Identical (6)
Years of education						
Early arrests (before 18)	−1.534 (0.235)***	−0.915 (0.225)***	−0.856 (0.329)***	−0.787 (0.316)**	−0.989 (0.382)***	−0.025 (0.583)
Conduct disorder		−0.127 (0.028)***		−0.100 (0.039)**	−0.099 (0.049)**	−0.069 (0.066)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Early school performance	No	Yes	No	Yes	Yes	Yes
Sample size	2252	2252	2252	2252	1336	916
Twin pairs			1126	1126	668	458
Senior high school						
Early arrests (before 18)	−0.380 (0.055)***	−0.277 (0.054)***	−0.230 (0.064)***	−0.215 (0.063)***	−0.238 (0.076)***	−0.122 (0.115)
Conduct disorder		−0.026 (0.006)***		−0.021 (0.008)***	−0.024 (0.010)**	−0.008 (0.013)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Early school performance	No	Yes	No	Yes	Yes	Yes
Sample size	2252	2252	2252	2252	1336	916
Twin pairs			1126	1126	668	458

OLS, ordinary least squares; FE, fixed effects. Standard errors in brackets.
*Significant at 10%; **Significant at 5%; ***Significant at 1%. In the top panel, dependent variable is *Years of education*. In the bottom panel, dependent variable is *Senior high school*. All specifications control for gender, age, age squared, education of parents, columns 2, 4, 5, and 6 also control for early school performance.

The within-twin effects are smaller but remain large. Early arrests reduce educational attainment with 0.7 to 0.9 years and lower the probability of completing senior high school with 22 to 23 percentage points. Including controls for conduct disorder and early school performance reduces the effect of early arrests.[3] A separate analysis shows that the estimates are not affected after including early school performance.[4] This indicates that, conditional on conduct disorder, the findings are determined by early criminal behavior rather than differences in early school performance. Columns (5) and (6) in Table 3 show the estimates for separate samples of fraternal and identical twins. The most remarkable finding is that the effects of early arrest on human capital are primarily driven by differences between fraternal twins. The estimates become statistically insignificant after controlling for all genetic

3. In case of missing values on conduct disorder, we included the value of the other twin. If both values were missing, we included the mean of the sample. In total, we imputed values for 39 twins. We find similar results for the smaller sample without imputation. In column (4), the estimates for years of education are –0.801 (0.321) and for high school –0.221 (0.063). In column (6), these estimates are –0.012 (0.589) and –0.117 (0.114).

4. We imputed missing values on early school performance for 5 individuals. The results for the smaller sample without imputation are similar.

differences within pairs of twins (column [6]). This suggests that genetic factors are important determinants of early crime. However, as mentioned in the previous section, our sample of identical twins contains only 14 pairs that differ in being arrested before the age of 18. Hence, the findings in column (6) are based on a sample with very limited variation in early arrests. In section 7, we will do a sensitivity test on a much larger sample which is constructed through imputation of missing values based on weak assumptions.

We further investigated the effect of the timing of the first arrest on education by constructing a second variable for early arrests. This variable measures the number of years before the age of 18 when the arrest took place (18 minus age first arrest). Table 4 shows the fixed-effect estimates for models that include this arrest-years variable and the square of this variable, and use all controls as in Table 3. Columns (1), (2), and (3) show the estimates of the effect on years of education for separate samples of all twins, fraternal twins, and identical twins, whereas columns (4), (5), and (6) show the effect on completing senior high school for these samples.

The estimates in Table 4 corroborate the previous findings. The estimates show that the effect of early arrests also depends on the timing of the arrest, with earlier arrests being more detrimental for educational attainment. These effects are only found in the sample of fraternal twins, again suggesting an important role for genetic differences within pairs of twins. Arrests at the age of 13, 14, or 15 are the most detrimental and reduce the probability of high school completion with more than 25 percentage points. Considering the fact that arrests at the age of 13, 14, or 15 took place during compulsory education, these findings seem well in line with our expectations that the causality runs from early arrests to human capital and not vice versa.

Summarizing, we find a large effect of early criminal behavior on educational attainment, even when family-fixed effects are taken into account. This effect largely disappears when genetic factors are also controlled

Table 4. Estimates of the Effect of the Timing of the Early Arrest on Educational Attainment

	Years of education			Senior high school		
	FE All (1)	FE Fraternal (2)	FE Identical (3)	FE All (4)	FE Fraternal (5)	FE Identical (6)
(18 minus age first arrest)	−0.648	−1.463	−0.045	−0.134	−0.281	−0.060
	(0.242)***	(0.399)***	(0.422)	(0.048)***	(0.080)***	(0.083)
(18 minus age first arrest)^2	0.103	0.297	0.013	0.018	0.054	0.006
	(0.039)***	(0.086)***	(0.057)	(0.008)**	(0.017)***	(0.011)
Conduct disorder	−0.105	−0.100	−0.071	−0.022	−0.025	−0.008
	(0.039)***	(0.049)**	(0.066)	(0.008)***	(0.010)**	(0.013)
Sample size	2252	1336	916	2252	1336	916
Twin pairs	1126	668	458	1126	668	458

OLS, ordinary least squares; FE, fixed effects. Standard errors in brackets.
*Significant at 10%; **Significant at 5%; ***Significant at 1%. All specifications control for gender, conduct disorder, and early school performance.

for. This suggests that genetic factors are important determinants of early crime, which might also be related to the limited variation within the sample of identical twins. In addition, the timing of the early arrests matters, such that arrests at the age of 13, 14, or 15 are most detrimental for human capital accumulation.

6. The Effect of Human Capital on Crime

The second aspect of the strong association between education and criminal activity might be the effect of education on crime. Investments in human capital raise the opportunity costs of crime and may also alter preferences and discount rates. In this section, we therefore analyze the effect of human capital on crime. The previous section showed that reverse causality cannot be ignored, as we found substantial effects of early criminal behavior on educational attainment. We therefore include various controls in our model that are informative of criminal behavior before the age of 18. First, the "early arrests" variable (arrests before the age of 18) can be used as an obvious control. Second, we can also include the "conduct disorder" variable, which is likely to precede investments in human capital.

We use the senior high school completion variable as our main measure of human capital. Senior high school can be completed at the age of 17 or 18. This brings the advantage that we can estimate the effect of completing senior high school on criminal activities since this age. The distinction between the investment in human capital and the timing of criminal activity would be less clear if we would use years of education as a measure of human capital instead. A second argument for using senior high school completion as a measure of human capital is that the effect of human capital on crime seems to be nonlinear (see Table 2).

We investigate the effect of human capital on three self-reported measures of crime: incarceration, arrests since the age of 18 and number of arrests. Unfortunately, our data do not contain information on the age of incarceration. However, statistics on incarceration in Australia show that the probability of being incarcerated before the age of 18 is only very small.[5] Arrests since the age of 18 are derived from the age of the last arrest. For the number of arrests, we constructed a variable which has 4 categories (0; 1; 2; 3). All individuals who reported more than three arrests were included in the last category (52 individuals reported at least three arrests, out of which 22 reported exactly three arrest). The data only contain information on the age of the first and the age of the last arrest. Hence, for the other arrests it is not clear whether they took place after the completion of high school. Considering the evidence on reverse causality from the previous section, we expect that this will produce a downward bias to the estimates (more negative estimates).

5. The rate of non-indigenous persons aged 10-17 in juvenile detention between 1994 and 2003 was between 16 and 26 per 100,000 of relevant population (Charlton and McCall 2004). This is on average approximately 0.02% of the population.

Table 5 shows the estimates of the effect of completing senior high school on the three measures of crime, using linear probability models. The first two columns present OLS estimates, whereas the next two columns present estimates of fixed effects models for the sample of all twins using different controls. Columns (5) and (6) show the estimates for separate samples of fraternal and identical twins. The top panel shows the effects on the probability of incarceration, the middle panel shows the effect on the probability of being arrested since the age of 18, and the bottom panel shows the effect on the number of arrests (0–3).

From the OLS estimates, it appears that education has a negative association with all three measures of crime. This association reduces substantially when including arrest(s) before 18 and conduct disorder. All fixed-effects estimates in column (3) are statistically significant. Controlling for early arrests and conduct disorder substantially reduces the size of the estimates. This confirms the earlier findings on reverse causality. Only the estimates for the effects on incarceration remain statistically significant. For the separate samples in

Table 5. Estimates of the Effect of High School Completion on Crime

	OLS (1)	OLS (2)	FE All (3)	FE All (4)	FE Fraternal (5)	FE Identical (6)
Incarceration						
Senior high school	−0.041	−0.012	−0.038	−0.021	−0.026	−0.008
	(0.010)***	(0.008)	(0.011)***	(0.010)**	(0.013)*	(0.017)
Early arrests		0.292		0.206	0.247	0.076
(before 18)		(0.057)***		(0.023)***	(0.028)***	(0.043)*
Conduct disorder		0.011		0.015	0.013	0.015
		(0.003)***		(0.003)***	(0.003)***	(0.005)***
Sample size	2246	2246	2246	2246	1332	914
Twin pairs			1123	1123	666	457
Arrested since the age of 18						
Senior high school	−0.085	−0.046	−0.037	−0.018	0.008	−0.060
	(0.016)***	(0.015)***	(0.022)*	(0.022)	(0.028)	(0.037)
Early arrests		0.260		0.112	0.150	−0.009
(before 18)		(0.057)***		(0.048)**	(0.057)***	(0.089)
Conduct disorder		0.028		0.028	0.037	0.004
		(0.004)***		(0.006)***	(0.007)***	(0.010)
Sample size	2252	2252	2252	2252	1336	916
Twin pairs			1126	1126	668	458
Number of arrests						
Senior high school	−0.214	−0.071	−0.108	−0.025	0.012	−0.082
	(0.034)***	(0.023)***	(0.037)***	(0.033)	(0.043)	(0.052)
Early arrests		1.568		1.200	1.241	1.060
(before 18)		(0.107)***		(0.072)***	(0.088)***	(0.127)***
Conduct disorder		0.049		0.050	0.068	0.007
		(0.008)***		(0.009)***	(0.011)***	(0.014)
Sample size	2250	2250	2250	2250	1334	916
Twin pairs			1125	1125	667	458

OLS, ordinary least squares; FE, fixed effects. Standard errors in brackets.
*Significant at 10%; **Significant at 5%; ***Significant at 1%. All columns control for gender, columns (2), (4), (5), and (6) control for age, age squared, and education of parents.

columns (5) and (6), we only find a statistical effect of human capital on incarceration in the sample of fraternal twins. Completing senior high school reduces the probability of incarceration with 2.6 percentage points. Hence, the fixed effects estimates in Table 5 show that the effect of human capital on crime strongly reduces after controlling for early crime.[6]

The estimates for the effect of early arrests on the three measures of crime in columns (1) to (5) in Table 5 are striking. The fixed-effects estimates suggest that an early arrest increases the probability of incarceration with more than 20 percentage points and increases the probability of getting arrested since the age of 18 with 11 to 15 percentage points. In addition, the average number of arrests increases with approximately 0.2. The size of these effects is much larger than the estimated effect of completing senior high school. Similar to the previous section, controlling for all genetic differences within pairs of twins (column [6]) strongly reduces the coefficient estimates, suggesting that genetic factors might be important drivers of differences in criminal behavior within pairs of twins. We also estimated the same models as in Table 5, with years of education instead of completing senior high school. The findings are quite similar to those in Table 5 and suggest a small effect of human capital on crime after controlling for early arrests and conduct disorder (see Table B.1 in Appendix B).

We conclude that the effect of human capital on adult crime strongly reduces when early criminal behavior is taken into account. This confirms that reverse causality is an important issue. The most remarkable findings are the large effects of early arrests on all three measures of crime. These effects are substantially larger than the estimated effects of human capital on crime. Reverse causality is less important in the sample of identical twins because of the limited variation in early crime in this sample.

7. Robustness

In this section, we investigate the robustness of the findings by addressing two issues. First, we test the sensitivity of the results by imputing missing values on criminal outcomes, which are due to the routing of the questionnaire that instructed the interviewer or respondent to skip questions depending on the answers to previous questions. Second, we address the issue of measurement error which is likely to bias the estimates downward.

7.1 Missing Values due to the Routing of the Questionnaire

Due to the routing of the questionnaire, twins with a conduct disorder score of zero, which means that they reported negative on all 21 statements on conduct disorder before the age of 18, did not answer questions about arrests and incarceration. This may bias the estimates because it involves a large fraction of our sample (approximately 3000 observations). It is

6. As in the previous tables, we imputed values for 39 twins with missing data on conduct disorder. The estimation results on the smaller sample without the imputed values are similar.

likely that individuals who report no conduct disorder behavior will be less involved in crime than those who have a positive conduct disorder score. For instance, the arrest (incarceration) rate of those with a conduct disorder score of 3 is 7.4 (0.6) against 2.5 (0.3) for those with a conduct disorder score of 1. We therefore checked the sensitivity of the results by imputing zeros for twins with missing values on being arrested and being incarcerated. Tables 6 and 7 show the estimation results for the main models of the previous sections. Table 6 shows the results for the effect of early crime on educational attainment.

The estimates in Table 6 are somewhat smaller but quite similar to those in Table 3. After the imputation of the missing values for being arrested, we still find a large effect of early arrests on educational attainment. In addition, the estimates for the sample of identical twins only now indicate a negative effect of early crime on human capital, although the standard errors remain quite large. The estimate for the effect on high school graduation for the sample of identical twins is statistically not different from the estimate for the sample of fraternal twins. This suggests that the findings in column (6) of Table 3 are partly driven by the issue of a small sample size.

Table 6. Estimates of the Effect of Early Arrests on Educational Attainment after Imputations for Missing Values on Early Arrests

	OLS	OLS	FE All	FE All	FE Fraternal	FE Identical
	(1)	(2)	(3)	(4)	(5)	(6)
Years of education						
Early arrests (before 18)	−1.597 (0.215)***	−0.752 (0.200)***	−0.803 (0.289)***	−0.668 (0.277)**	−0.772 (0.334)**	−0.322 (0.506)
Conduct disorder		−0.147 (0.020)***		−0.086 (0.028)***	−0.082 (0.034)**	−0.052 (0.048)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Early school performance	No	Yes	No	Yes	Yes	Yes
Sample size	5332	5332	5332	5332	3080	2252
Twin pairs			2666	2666	1540	1126
Senior high school						
Early arrests (before 18)	−0.363 (0.052)***	−0.229 (0.049)***	−0.189 (0.053)***	−0.159 (0.052)***	−0.165 (0.063)***	−0.140 (0.097)
Conduct disorder		−0.027 (0.004)***		−0.015 (0.005)***	−0.015 (0.006)**	−0.012 (0.009)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Early school performance	No	Yes	No	Yes	Yes	Yes
Sample size	5332	5332	5332	5332	3080	2252
Twin pairs			2666	2666	1540	1126

OLS, ordinary least squares; FE, fixed effects. Standard errors in brackets.
*Significant at 10%; **Significant at 5%; ***Significant at 1%. In the top panel, dependent variable is *Years of education*. In the bottom panel, dependent variable is *Senior high school*. All specifications control for gender, age, age squared, education of parents, columns 2, 4, 5, and 6 also control for early school performance.

Table 7 shows the estimates for the effect of high school completion on crime.

The pattern of findings in Table 7 is similar to that in Table 5. The effect of high school completion on crime becomes statistically insignificant when controlling for early criminal behavior. Early crime seems a much more important determinant of adult crime than schooling. We even find some statistical significant effects in the sample of identical twins only.

We conclude that the main estimates in the previous sections are robust to the imputation of missing values for individuals with a conduct disorder score of zero. The estimated effects of high school completion on crime decrease in these larger samples. In addition, the estimates in the sample of identical twins indicate a negative effect of early crime on schooling.

Table 7. Estimates of the Effect of High School Completion on Crime after Imputations for Missing Values on the Crime Variables

	OLS	OLS	FE All	FE All	FE Fraternal	FE Identical
	(1)	(2)	(3)	(4)	(5)	(6)
Incarceration						
Senior high school	−0.021	−0.004	−0.016	−0.006	−0.005	−0.007
	(0.005)***	(0.004)	(0.006)***	(0.006)	(0.008)	(0.008)
Early arrests		0.267		0.190	0.218	0.107
(before 18)		(0.051)***		(0.016)***	(0.020)***	(0.027)***
Conduct disorder		0.010		0.014	0.014	0.013
		(0.002)***		(0.002)***	(0.002)***	(0.002)***
Sample size	5326	5326	5326	5326	3076	2250
Twin pairs				2663	1538	1125
Arrested since the age of 18						
Senior high school	−0.054	−0.024	−0.023	−0.008	0.002	−0.024
	(0.009)***	(0.008)***	(0.012)*	(0.012)	(0.016)	(0.017)
Early arrests		0.247		0.110	0.132	0.043
(before 18)		(0.052)***		(0.032)***	(0.040)***	(0.056)
Conduct disorder		0.030		0.031	0.038	0.012
		(0.003)***		(0.003)***	(0.004)***	(0.005)**
Sample size	5332	5332	5332	5332	3080	2252
Twin pairs			2666	2666	1540	1126
Number of arrests						
Senior high school	−0.127	−0.035	−0.058	−0.008	0.006	−0.026
	(0.020)***	(0.013)***	(0.020)***	(0.018)	(0.024)	(0.025)
Early arrests		1.537		1.210	1.227	1.149
(before 18)		(0.097)***		(0.049)***	(0.062)***	(0.080)***
Conduct disorder		0.050		0.054	0.067	0.019
		(0.006)***		(0.005)***	(0.006)***	(0.008)**
Sample size	5330	5330	5330	5330	3078	2252
Twin pairs			2665	2665	1539	1126

OLS, ordinary least squares; FE, fixed effects. Standard errors in brackets.
*Significant at 10%; **Significant at 5%; ***Significant at 1%. All columns control for gender, columns (2), (4), (5), and (6) control for age, age squared, and education of parents.

7.2 Measurement Issues

7.2.1 Measurement Error in Education. A well-known concern in the literature using within-family models is a measurement error (Griliches 1979). By taking a within-family perspective, measurement error may exacerbate, which in turn is likely to bias the estimates toward zero. A solution for this problem has been proposed by Ashenfelter and Krueger (1994) in their study on the returns to schooling using data on twins. They suggested using a second independent measure of education as an instrument for educational attainment. In their study, they asked each sibling to report on both their own and their twin's schooling and used this information as independent measure of schooling. They constructed two instruments for the difference in education within twins depending on the assumptions about measurement error. Let S_1^1 refer to the self-reported education level of the first twin, S_1^2 to the sibling-reported education level of the first twin, S_2^2 to the self-reported education level of the second twin, and S_2^1 to the sibling-reported education level of the second twin. The first instrument uses the difference in the twin's report on the schooling of their sibling as an instrument for the difference in the report on the own schooling. Hence, $S_1^1 - S_2^2$ is instrumented with $S_1^2 - S_2^1$. The second instrument assumes that the measurement error of respondent's report on the own schooling and the schooling of their sibling are correlated. In the estimation, the difference in the reports of twin A about the own schooling and the sibling's schooling is instrumented with the difference in the reports of twin B on the sibling's schooling and the own schooling. Hence, $S_1^1 - S_2^2$ is instrumented with $S_1^2 - S_2^1$. Ashenfelter and Krueger (1994) preferred the use of the second instrument.

In our study, we can follow this approach in the models that estimate the effect of education on crime because our data include the same questions on the sibling's schooling. The correlation between the self-reported level of education and the sibling-reported education level, which indicates the reliability ratio, is 0.80. For high school completion, this correlation is 0.63. It should be noted that this approach produces consistent estimates when the measurement error is classical. However, since our main variable (senior high school completion) is a binary indicator, this assumption does not hold. It has been shown that the IV estimate will then be upward biased (Aigner 1973; Kane et al. 1999). The within-family estimate from the previous analyses will then provide a lower bound and the IV estimate an upper bound of the true (negative) effect.

Table 8 shows the IV estimates for the effect of high school completion on the three measures of crime using the second instrument. Columns (1), (3), and (5) show the estimation results for the sample of all twins. Columns (2), (4), and (6) show the results for the sample of identical twins only. The bottom panel shows the estimation results after imputing the missing values for individuals with a conduct disorder score of zero. All specifications use early arrest, conduct disorder, and gender as controls.

The estimates in Table 8 suggest that measurement error in education might be important. All estimates increase and several estimates become statistically

Table 8. Fixed Effects IVs Estimates of the Effect of Senior High School Completion on Crime

	Incarceration		Arrested since 18		Number of arrests	
	All (1)	Identical (2)	All (3)	Identical (4)	All (5)	Identical (6)
Senior high school	−0.070	−0.070	−0.097	−0.239	−0.119	−0.197
	(0.024)***	(0.043)	(0.052)*	(0.096)**	(0.077)	(0.134)
Sample size	2240	912	2246	914	2244	914
Twin pairs	1120	456	1123	457	1123	457
Imputed missing values						
Senior high school	−0.025	−0.039	−0.042	−0.087	−0.056	−0.069
	(0.013)**	(0.017)**	(0.026)	(0.038)**	(0.040)	(0.054)
Sample size	5318	2248	5324	2250	5322	2250
Twin pairs	2659	1124	2662	1125	2661	1125

OLS, ordinary least squares; FE, fixed effects. Standard errors in brackets. Dependent variable in columns (1) and (2) is *Incarceration*. Dependent variable in columns (3) and (4) is *Arrested since 18*. Dependent variable in columns (5) and (6) is *Number of arrests*. Bottom panel shows the estimation results after imputing the missing values for individuals with a conduct disorder score of zero. All columns control for gender, early arrest, conduct disorder, and a fixed twin-pair effect.

significant. For the sample of identical twins, only some estimates become quite large but also have large standard errors. The bottom panel of Table 8 shows that the size of the estimates is smaller after the imputation of missing values but the pattern of findings is quite similar. Remarkably, for the sample of identical twins, we find that completing high school significantly reduces incarceration and the probability of arrest since 18. We find a similar pattern when using years of education instead of completion of senior high school. However, the estimates are smaller (see Table B.2 in Appendix B). These results suggest that the findings in Table 5 might underestimate the true effect of human capital on crime.

7.2.2 Measurement Error in Crime. Measurement error in self-reported crime might also be important and even more important than measurement error in self-reported schooling. Unfortunately, our data do not contain sibling reports on criminal behavior. As such we cannot use the approach from the previous section for the models that investigate the effect of early crime on education. However, we can make a tentative assessment using external information on the reliability of self-reported crime and the intra-class correlation in early crime measured in our sample of twins. Assuming classical measurement error, Griliches (1979) shows that within-family estimation increases the bias from measurement with $1/(1-\rho_c)$, where ρ_c is the intra-class correlation in early crime within families. Thornberry and Krohn (2000) report that many studies have found a reliability ratio of self-reported crime to be well above 0.8. The intraclass correlation in early crime in our data is 0.22. This means that the bias in the OLS estimator is $-2 \times \beta$ and the

bias in the fixed effects estimator is $-\frac{0.22}{1-0.22} \times \beta = -0.26 \times \beta$. This calculation suggests that the additional downward bias in the within estimator is quite modest.

7.2.3 Do Clever Criminals Stay out of Jail? In the previous sections, we used early arrest as a proxy for early crime. This proxy might be a source of measurement error since many criminal activities happen without arrests. It could even be possible that unobserved factors correlated with crime are also important for not being caught committing a crime and for being successful in school. For instance, bright criminals might both be successful in staying out of a jail and doing well in school. Hence, there might be a relationship between undetected crime and educational attainment. We investigated these issues by using the variable early crime score based on the crime-related items from the conduct disorder statements (see section 4). First, we investigated whether a higher score on this early crime variable is related with a higher probability of being arrested

Table 9. Estimates of the Effect of Early Crime on Early Arrests (Before 18)

	OLS	OLS	FE All	FE All	FE Fraternal	FE Identical
	(1)	(2)	(3)	(4)	(5)	(6)
Early arrests (before 18)						
Early crime score	0.029 (0.005)***	0.023 (0.004)***	0.015 (0.004)***	0.014 (0.004)***	0.022 (0.006)***	−0.002 (0.006)
Conduct disorder		0.013 (0.003)***		0.006 (0.004)	0.006 (0.005)	0.002 (0.005)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Early school performance	No	Yes	No	Yes	Yes	Yes
Sample size	2182	2182	2182	2182	1294	888
Twin pairs			1091	1091	647	444
Imputed missing values						
Early arrests (before 18)						
Early crime score	0.026 (0.004)***	0.020 (0.003)***	0.017 (0.002)***	0.015 (0.002)***	0.021 (0.003)***	0.002 (0.004)
Conduct disorder		0.009 (0.002)***		0.005 (0.002)**	0.004 (0.003)	0.006 (0.003)**
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Early school performance	No	Yes	No	Yes	Yes	Yes
Sample size	5246	5246	5246	5246	3030	2216
Twin pairs			2623	2623	1515	1108

OLS, ordinary least squares; FE, fixed effects. Standard errors in brackets.
In both panels, dependent variable is *Early arrests (before 18)*. Bottom panel shows the estimation results after imputing the missing values for individuals with a conduct disorder score of zero. All specifications control for gender. Column (1) and (2) control for age, age squared, education of parents, columns (2), (4), (5), and (6) also control for conduct disorder, and early school performance.

before the age of 18. The estimation results are shown in Table 9. The bottom panel shows the results after imputing missing values for individuals with a conduct disorder score of zero.

The estimation results in the top panel of Table 9 demonstrate that a stronger involvement in early crime is positively related with the probability of being arrested before the age of 18. Each additionally reported crime increases the probability of being arrested before the age of 18 with approximately 2 percentage points. This also holds in the models that control for twin-fixed effects (columns [3] and [4]). However, for the sample of identical twins only we do not find a significant effect of early crime. As in the previous sections, this might suggest that genetic factors are important for early criminal behavior. The results in the bottom panel of Table 9 are very similar. These findings suggest that being arrested before the age of 18 proxies the level of involvement in early crime.

Second, we investigated the relationship between undetected crime and educational attainment. We estimated the effect of the early crime score on educational attainment in models that control for early arrests. These estimates might give insight into the effect of undetected crime on educational attainment since, after controlling for early arrests, the remaining variation in early crime can be viewed as undetected early crime. Table 10 shows the estimates for the main model. The left panel shows the effects on years of education, the right panel shows the effects on high school graduation.

The estimates in Table 10 suggest that undetected crime is not related with educational attainment. After controlling for twin-fixed effects, all the estimates become statistically insignificant. Hence, these estimates do not provide support for the hypothesis that criminals who remain undetected have a higher educational attainment. Therefore, the sensitivity analyses in this section does not provide evidence that measurement error in education due to undetected crime seriously biases our findings.

Table 10. Estimates of the Effect of Early Crime on Educational Attainment Controlling for Early Arrests

	Years of education			Senior high school		
	OLS	FE Fraternal	FE Identical	OLS	FE Fraternal	FE Identical
	(1)	(2)	(3)	(4)	(5)	(6)
Early crime score	0.104	0.068	−0.048	0.003	0.006	−0.008
	(0.037)***	(0.060)	(0.077)	(0.007)	(0.012)	(0.015)
Early arrests	−1.080	−1.084	−0.019	−0.284	−0.249	−0.118
(before 18)	(0.247)***	(0.392)***	(0.590)	(0.056)***	(0.078)***	(0.115)
Sample size	2180	1294	886	2180	1294	886
Twin pairs	1090	647	443	1090	647	443

OLS, ordinary least squares; FE, Fixed effects. Standard errors in brackets. Dependent variable in columns (1), (2), and (3) is *Years of education*. Dependent variable in columns (4), (5), and (6) is *Senior high school*. All specifications control for gender, age, age squared, education of parents, conduct disorder, and early school performance.

8. Conclusions and Discussion

This article aims to disentangle the strong association between human capital and crime by investigating whether crime reduces investment in human capital or whether education reduces criminal activity. Heretofore, we exploit two aspects of the utilized Australian survey data on education and crime. First, as the data are obtained from twins, we are able to control for many unobserved characteristics affecting both criminal behavior and the schooling decisions. Second, as criminal behavior is measured over different periods of time—prior to and after senior high school completion—we can address the causality between crime and education as well. As early criminal behavior may affect human capital formation, and human capital may influence criminal behavior in later stages of life, we follow a two-step analysis.

First, we address the effects of early criminal behavior on educational attainment. The estimates suggest that early criminal behavior is detrimental to investment in human capital. Within pairs of twins, we find that early arrests (before the age of 18) reduce educational attainment with 0.7 to 0.9 years and lower the probability of completing senior high school with 20 to 23 percentage points. In addition, the timing of the early arrest matters, such that arrests at age 13, 14, or 15 are most detrimental for educational attainment. These effects are estimated after controlling for conduct disorder and early school performance and are, to a large extent, based on the sample of fraternal twins. The estimates for the sample of identical twins are less informative because of the small number of twin pairs who differ in early arrests.

Second, we focus on the effect of human capital on crime. As early criminal activity might be an important confounder, we control for early arrests. For the sample of fraternal twins, we find no effect of human capital on adult crime in models that take early arrests into account. For the sample of identical twins, we find that human capital has a negative effect on crime. In addition, the size of these estimates might be downward biased because of measurement error in schooling. IVs estimates, produced using a second independent measure of schooling, suggest that the effect of human capital might be larger.

When combining these findings, it seems that the causality between human capital and crime runs in both directions. For fraternal twins, the impact of early criminal behavior on human capital formation dominates the impact of human capital formation on future crime behavior. Controlling for early arrests and early behavior problems strongly reduces the estimated effect of human capital on future crime behavior. For identical twins, that hardly differ in early criminal behavior, human capital reduces crime.

The strong detrimental effects of early criminal behavior also become transparent if we consider the estimated effects of early arrests on all three measures of crime. Early arrests increase the probability of incarceration with 20 percentage points and the probability of being arrested since the age of 18 with 10

percentage points. These effects are much larger than the estimated effects of early arrests on human capital. For instance, the estimated effect of being arrested before the age of 18 on incarceration is almost ten times higher than on completing high school.

Some cautionary notes about this study are in order. First, it is possible that results from a sample of twins might not be transferable to the population at large. Various studies that have compared samples of twins with the population at large on outcomes such as educational attainment, IQ, psychiatric symptoms, or personality (Kendler et al. 1986; Baker et al. 1996; Webbink et al. 2008; Calvin et al. 2009) have found that the twins seem more or less representative of the wider population. However, it remains unclear whether twins, and especially twins that participated in the surveys used in this article, are more or less likely to be involved in crime than the overall population. Second, measurement error in crime might be important. Although we find that our results are robust to sensitivity tests on this issue, some caution seems appropriate.

In line with previous studies (Lochner and Moretti 2004; Machin et al. 2011), our findings suggest that policies that succeed in raising investment in human capital might reduce crime. However, the (direct) returns to polices that succeed in preventing early criminal behavior might even be larger. The estimated effects of early criminal behavior and conduct disorder stress the importance of preventing crime in the early stages of life. Programmes that keep children on "the right track" may not only yield high private returns but may also yield high social returns through their impact on crime reduction. Studies on the effects of early intervention programmes in the United States show that these programmes have large social returns mainly through their impact on preventing crime (Carneiro and Heckman 2003).

Our estimates show that the strong association between human capital and crime is to a large extent driven by the effect of early criminal behavior on educational attainment. This finding based on within-twin estimation confirms one of the main conclusions from a synthesis of the literature on the causes of crime: "We must rivet our attention on the earliest stages of the life cycle, for after all is said and done, the most serious offenders are boys who begin their criminal careers at a very early age' (Wilson and Herrnstein 1985, cited in DiIulio 1996).

Appendix A: Variable Definitions

Table A.1. Conduct Disorder Statements from the TWIN89 Questionnaire

Variable	Question
Misbehaved	**L3** Did you frequently get into a lot of trouble with the teacher or principal for misbehaving in school? (primary or secondary school)
Wagged school	**L4** Before age 18, did you ever wag school for an entire day at least twice in 1 year?
Suspended/expelled	**L5** Were you ever suspended or expelled from school?
Stay out late	**L6** As a child or a teenager, did you often stay out much later than you were supposed to?
Sneak out at night	**L6A** Did you often sneak out of the house at night?
Run away overnight	**L6C** Before age 18, did you ever run away from home overnight?
Lied, used false name	**L7** Before 18, did you ever tell a lot of lies or use a false name or alias?
Outsmarted, conned others	**L7B** Before age of 18, was there ever a period when you often outsmarted others and "conned" them?
Stole from home or family	**L8** Before age 18, did you steal money or things from your home or family more than once? If yes, did you only steal things of trivial value, like loose change or things like that?
Shoplifted	**L8A** Before age 18, did you steal or shoplift from shops or other people (without their knowing) more than once? If yes, did you only steal things of trivial value like comics or lollies?
Forged signature	**L8B** Before age 18, did you forge anyone's signature on a cheque or credit card more than once?
Damaged property	**L9** Have you ever damaged someone's property on purpose?
Started physical fights	**L10** Before age 18, did you start physical fights (with persons other than your brothers or sisters) 3 or more times?
Used a weapon	**L11** Before age 18, did you ever use a weapon like a bat, brick, broken bottle, gun, or a knife (other than in combat, when hunting, or as part of your job) to threaten or harm someone?
Physically injured someone	**L12** Before age 18, (other than fighting or using a weapon) did you ever physically injure anyone on purpose?
Bullied others	**L13** Before age 18, were you often a bully, deliberately hurting or being mean to others?
Mean to animals	**L14** Before age 18, were you ever mean to animals including pets or did you hurt animals on purpose?
Lighted fires	**L15** Before 18, did you ever deliberately light any fires you were not supposed to?
Broke into someone's car/house	**L16** Before 18, did you ever break into someone's car or house or anywhere else (not because you were locked out)?
Forcefully stole money or property	**L17** Before age 18, did you ever take money or property from someone else by threatening them or using force, like snatching a purse or robbing them?
Forced someone into sexual activity	**L20** Before age 18, did you ever force anyone into intercourse or any other form of sexual activity?

Appendix B: Additional Estimation Results

Table B.1. Estimates of the Effect of Years of Education on Crime

	OLS (1)	OLS (2)	FE All (3)	FE All (4)	FE Fraternal (5)	FE Identical (6)
Incarceration						
Years of education	−0.006	−0.001	−0.004	−0.001	−0.001	0.000
	(0.002)***	(0.001)	(0.002)*	(0.002)	(0.003)	(0.003)
Early arrests		0.295		0.209	0.252	0.077
(before 18)		(0.057)***		(0.023)***	(0.028)***	(0.043)*
Conduct disorder		0.012		0.015	0.014	0.015
		(0.003)***		(0.003)***	(0.003)***	(0.005)***
Sample size	2246	2246	2246	2246	1332	914
Twin pairs			1123	1123	666	457
Arrested since the age of 18						
Years of education	−0.012	−0.005	−0.006	−0.003	0.001	−0.008
	(0.003)***	(0.003)*	(0.004)	(0.004)	(0.005)	(0.007)
Early arrests		0.269		0.114	0.150	−0.001
(before 18)		(0.057)***		(0.048)**	(0.057)***	(0.089)
Conduct disorder		0.029		0.028	0.037	0.004
		(0.004)***		(0.006)***	(0.007)***	(0.010)
Sample size	2252	2252	2252	2252	1336	916
Twin pairs			1126	1126	668	458
Number of arrests						
Years of education	−0.032	−0.008	−0.019	−0.006	−0.001	−0.014
	(0.006)***	(0.004)**	(0.007)***	(0.007)	(0.008)	(0.010)
Early arrests		1.581		1.201	1.237	1.070
(before 18)		(0.106)***		(0.072)***	(0.088)***	(0.127)***
Conduct disorder		0.050		0.050	0.068	0.006
		(0.008)***		(0.009)***	(0.011)***	(0.014)
Sample size	2250	2250	2250	2250	1334	916
Twin pairs			1125	1125	667	458

OLS, ordinary least squares; FE, fixed effects. Standard errors in brackets. All columns control for gender, columns (2), (4), (5), and (6) control for age, age squared, and education of parents.

Table B.2. Fixed Effects IVs Estimates of the Effect of Years of Education on Crime

	Incarceration		Arrested since 18		Number of arrests	
	All (1)	Identical (2)	All (3)	Identical (4)	All (5)	Identical (6)
Years of education	−0.003	−0.002	−0.007	−0.023	−0.008	−0.023
	(0.004)	(0.006)	(0.008)	(0.014)*	(0.011)	(0.019)
Sample size	2240	912	2246	914	2244	914
Twin pairs	1120	456	1123	457	1123	457
Imputed missing values						
Years of education	−0.000	−0.002	−0.003	−0.009	−0.004	−0.009
	(0.002)	(0.003)	(0.004)	(0.006)	(0.006)	(0.008)
Sample size	5318	2248	5324	2250	5322	2250
Twin pairs	2659	1124	2662	1125	2661	1125

OLS, ordinary least squares; FE, fixed effects. Standard errors in brackets. Dependent variable in columns (1) and (2) is *Incarceration*. Dependent variable in columns (3) and (4) is *Arrested since 18*. Dependent variable in columns (5) and (6) is *Number of arrests*. Bottom panel shows the estimation results after imputing the missing values for individuals with a conduct disorder score of zero. All columns control for gender, early arrest, conduct disorder, and a fixed twin-pair effect.

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