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Education and criminal behavior: Insights from an expansion of upper secondary school

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**Education and criminal behavior:
insights from an expansion of
upper secondary school**

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Hans Grönqvist
Caroline Hall
Jonas Vlachos

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Education and criminal behavior: insights from an expansion of upper secondary school^a

by

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Abstract

We study the impact on criminal activity from a large scale Swedish reform of vocational upper secondary education, extending programs from two to three years and adding more general theoretical content. The reform directly concerns age groups where criminal activity is high and students who are highly overrepresented among criminal offenders. The nature of the reform and the rich administrative data allow us to shed light on several behavioral mechanisms. Our results show that the prolonged and more general education lead to a reduction in property crime, but no significant decrease in violent crime. The effect is mainly concentrated to the third year after enrollment, which suggests that being in school reduces the opportunities and/or inclinations to commit crime.

Keywords: Education, delinquency

JEL-codes: K42

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1 Introduction

Education can generate large private and social gains in many dimensions. The literature not only documents strong associations with earnings, employment and other indicators of economic well-being, but also with health, family formation and crime (Oreopoulos and Salvanes 2011). Having secured basic education for essentially all residents, many developed societies now focus reforms on how to extend and improve secondary education for broad groups of students. A commonly held view is that students are not sufficiently prepared for higher education and lack the type of general knowledge typically required by the labor markets of today. Consequently, it is a common trend in most developed countries that secondary education curricula are becoming more like primary school curricula with a broader range of subjects, less specialization and more integrated themes in order to provide all students with opportunities for tertiary education (Sahlberg 2007). This development is particularly profound when it comes to vocational education. Reforming secondary education in a way that satisfies modern standards may however lead to externalities that have not been fully appreciated. In particular, such policies run the risk of also increasing dropout rates.¹ This potential tradeoff is made particularly stark by the literature on the impact of education on criminal behavior.² Since crime typically has significant negative externalities, the social returns to education may be substantially higher than the private returns if education has crime reducing effects. On the other hand, dropouts from secondary education are highly overrepresented among those involved in criminal activities.³

This paper studies the impact on crime by a major reform of Swedish upper-secondary vocational education. The reform extended vocational programs from two to three years and added more general theoretical content to the curriculum. The intervention took place for age groups where crime is common, and primarily affected segments of the youth cohorts with particularly high crime risks. Our analysis is made possible by access to rich longitudinal population-wide micro data including details on all criminal convictions in Swedish courts. We take advantage of the fact that the reform

¹ Bishop and Mane (2001) and Goodman (2012) find that higher requirements can improve labor market outcomes while other studies find that they increase dropout rates (Dee and Jacob 2007; Ou 2011). Note that Clark and See (2011) do not find that tougher education standards have an impact, either on dropout rates or on earnings.

² Hjalmarsson and Lochner (2012) provide a recent literature review.

³ According to Harlow (2003), 75 percent of US inmates did not complete high school. Our own calculations reveal that about 50 percent of the individuals who were sentenced to prison in 2005 had not completed high school.

was implemented gradually over time in different municipalities. According to Hall (2012), the reform increased average educational attainment, but also led to a substantial increase in dropout rates among low-performing students.

We complement and add to the existing literature in several ways. Most importantly, the nature of the reform and the richness of the data allow us to provide one of the first pieces of evidence on the causal effect on criminal activity of making secondary education more general. As related policies are being discussed and implemented throughout the developed world, our findings are likely to be relevant outside the specific setting that we analyze. Moreover, we can study both long- and short-run effects on criminal behavior, as well as separate between crimes committed on school operating days and when the school is in recess. Also, we study asymmetric effects of the reform across different student characteristics, among them the predicted dropout risk and previous school performance. Taken together, these sources of variation enable us to shed light on the mechanisms through which education can affect crime.

There are several theoretical reasons as to why increased educational attainment could affect crime. First of all, education typically raises expected earnings from legitimate work, thereby making crime relatively less appealing (Lochner 2004).⁴ Second, one can hypothesize that education directly affects preferences, potentially making individuals more socialized, future-oriented, and risk-averse.⁵ These factors could then decrease the probability of crime. Third, merely being in school may in itself affect the risk of crime by incapacitation; i.e. by participating in education individuals have less time to engage in criminal activities. If there is habit formation in crime, then such decreases in criminal involvement may lead to even larger crime reductions in the long run. On the other hand, schools also offer arenas potentially increasing contacts and thereby frictions between youth, thus triggering certain types of crime (a so-called concentration effect). Educational reform can also affect the peer composition for a given individual, meaning exposure to contexts with varying degrees of crime

⁴ Several studies have shown that better labor market opportunities reduce crime (e.g. Grogger 1998; Gould, Mustard and Weinberg 2002; Machin and Meghir 2004; Grönqvist 2013). This effect is potentially counteracted if education raises the opportunities to commit crime and the returns to certain types of crime.

⁵ In an influential study, Becker and Mulligan (1997) posit that people could learn to be more future-oriented. Perez-Arce (2011) demonstrates empirically that college students in Mexico who were randomly admitted from a pool of applicants were more patient than individuals in the control group, which indicates that education has an impact on time preferences.

propensity. The incapacitation and human capital mechanisms suggest negative effects of education on crime, while peer influences are ambiguous in sign.

We find robust evidence that increased access to prolonged and more general education for male vocational students lead to a persistent reduction in property crime by about 20 percent, but no significant decrease in violent crime.⁶ These effects are concentrated among students in the lower half of the compulsory school GPA distribution. We also show that these effects are present in the age interval 16–20 but not at higher ages, and are mainly concentrated during the added third year in school.⁷ These findings suggest that incapacitation is the main mechanism behind the reduction in criminal behavior. However, since Hall (2012) does not find an impact on future earnings by the reform, the results should not be read as a refutation of the hypothesis that the alternative cost of crime is also relevant.

Having a degree from upper secondary school is negatively correlated with criminal behavior. It is therefore interesting that the increase in dropout rates induced by the reform does not appear to have increased crime. One possibility is that a decrease in crime by increased schooling is counteracted by an increase in crime due to the higher likelihood of dropping out of school. In line with this we find that among the students predicted to be most likely to drop out, the estimated reform impact is zero. The results further show that students who enrolled in the longer and more academic tracks interacted more with students from more favorable backgrounds. There is however no indication that changes in the social environment constitute an important mechanism for the reduction in crime.

The quasi-experimental set-up of the reform allows us to deal with the challenge of disentangling the causal component of the association between crime and education. Obviously, a negative correlation may arise for several reasons, e.g. family background or innate abilities making education more (less) attractive/likely and working in the opposite direction for the probability of engaging in crime. Despite a voluminous multi-disciplinary literature (see Lochner 2010 for an overview), relatively few studies have been able to achieve credible causal identification. Lochner and Moretti (2004) is one exception, finding that schooling decreases the probability of incarceration using

⁶ We follow the convention in the literature and focus on males because they are substantially overrepresented among criminal offenders but note we also present some results also for females.

⁷ We do however not find any clear differences in the effect of the reform on crime committed on weekdays versus weekends or for crime on schools days compared to school breaks.

variation in compulsory schooling laws across US states. Machin, Marie and Vujic (2011) is another example, concluding that education can be an important measure for reducing crime, based on their UK study. Recent Swedish studies exploiting a compulsory school reform also find a negative impact of compulsory education on crime (Meghir, Palme and Schnabel 2011; Hjalmarsson, Holmlund and Lindquist 2011). These quasi-experimental studies have focused on compulsory schooling reforms or policies that affected the lower part of the educational distribution, while we study a different population at higher risk of criminal involvement. Further, these studies link educational expansion to adult crime while our study directly concerns age groups where criminal activity is relatively high, as well as the future criminal behavior of these groups. Our study bears similarities with the recent work on Norway by Brugård and Falch (2013). Using high school structure and geographic information in an IV setting they show that high school achievement decreases crime. Compared to our study, the data does not allow Brugård and Falch to study the time profile of criminal activity, hence making it difficult to draw conclusions concerning the mechanisms behind the decline.

A second strand of the quasi-experimental literature studies the contemporaneous link between education and crime among youths (Jacob and Lefgren 2003; Luallen 2006; Anderson 2013; Berthelon and Kruger 2011).⁸ These papers conclude that education reduces crime by incapacitating individuals. This squares well with our analysis, which expands on this research by using more detailed data and studying the long-run consequences by following individuals for a period stretching almost 20 years.

The rest of the paper is outlined as follows. Section 2 gives some institutional background on crime and the criminal justice system in Sweden, and on the educational reform under study. Section 3 describes the data and the research design. Section 4 presents the empirical analysis, starting with the baseline results, then turning to investigate effects by background characteristics, and finally trying to disentangle the mechanisms at work. Section 5 concludes.

⁸ Related is also Deming (2011) who finds that winners of school choice lotteries in middle or high school are less likely to be arrested and incarcerated seven years after the school assignment.

2 Institutional background

2.1 Crime in Sweden and the Swedish criminal justice system⁹

The Swedish crime rate is high in comparison to many other countries. In 2006, the total number of assaults reported to the police per 100,000 inhabitants amounted to 845. The same year, official crime statistics from the US police revealed 787 recorded cases of assaults per 100,000 inhabitants, and the corresponding number for Canada was 738 (Harrendorf et al. 2010). Even though these figures partly reflect differences in the propensity to report crime, they are similar across many types of crime. For instance, in 2006 the number of reported burglaries per 100,000 persons was in Sweden 1,094. In the US and in Canada the equivalent numbers were 714 and 680, respectively. Despite the many problems inherited in making these type of cross-country comparisons, these numbers at least suggest that crime does not seem trivial the context we study.

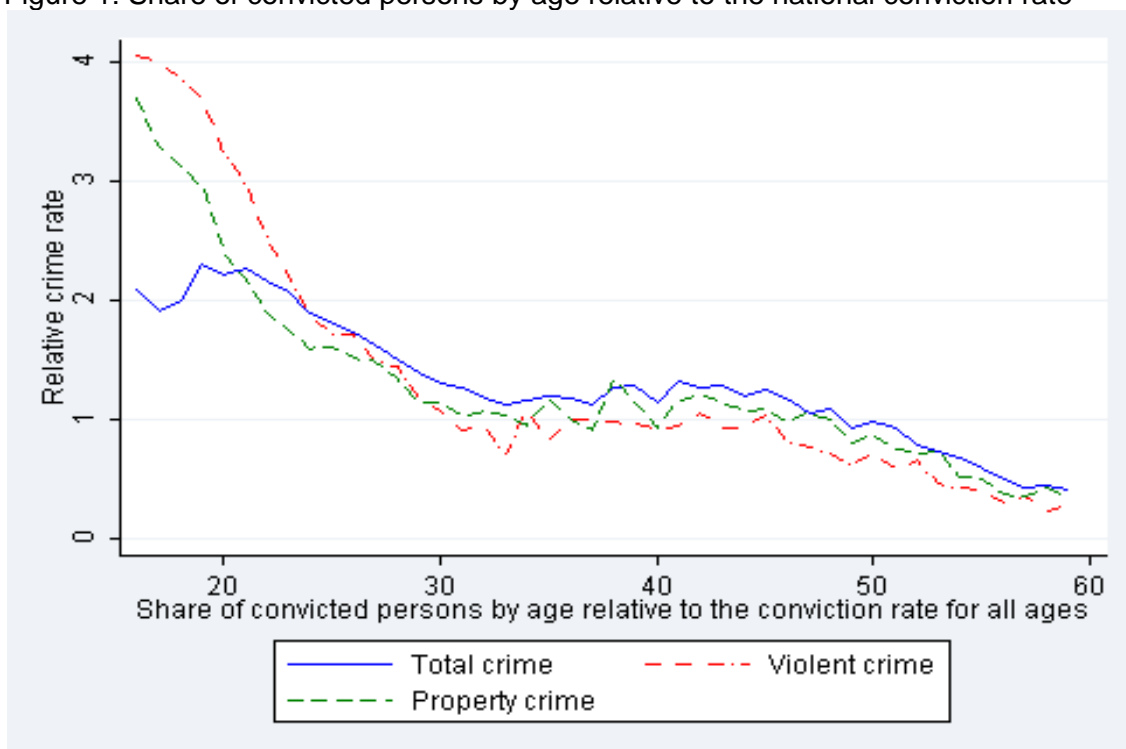
As in most other countries, youths represent the most criminally active age group. Figure 1 plots the share of convicted males in 2005 by age relative to the national conviction rate. A number above (below) one indicates that the share of convicted males for that age group is higher (lower) than the average for all age groups. It is clear that the conviction rate peaks already before age 20, and then falls sharply. This is especially true for violent and property crime.

In Sweden, the general courts deal with both criminal and civil cases. The general courts are organised in a three-tier system: district courts, courts of appeal and the Supreme Court. The district court is the court of first instance. Criminal cases are normally instituted when a public prosecutor initiates prosecution proceedings against a suspect by submitting an application to a district court. The court rules on cases after a main hearing attended by both parties who state their claims and other circumstances relating to the case. Criminal cases are normally tried by one judge and three lay judges. Those who lack the economic means to take advantage of their rights are entitled to public legal aid.

The age of criminal liability is 15. All individuals above this age are treated in the same judicial system. Some special rules do however apply for juveniles. The main difference is that cases involving youths are to be dealt with promptly.

⁹ This section closely follows Grönqvist and Niknami (2012).

Figure 1: Share of convicted persons by age relative to the national conviction rate



Notes: The sample includes all men aged 16 and above. The year of observation is 2005.

2.2 The Swedish education system and the reform under study¹⁰

After completing nine years of compulsory schooling, students in Sweden are entitled to upper secondary education. Schooling at the upper secondary level is voluntary but the vast majority enrolls. In the end of the 1980s, almost 90 percent continued directly to upper secondary school. In recent years, the enrollment rate has been as high as 98 percent (National Agency for Education 2008). Upper secondary school comprises several different educational tracks to which individuals apply based on their compulsory school GPA.¹¹

The Swedish upper secondary school went through a major reform in the beginning of the 1990s. Before the reform, upper secondary education consisted of a few academic and several vocational tracks. The vocational tracks were two years long and consisted mainly of vocational training. The academic tracks typically lasted three years¹² and prepared the students for higher education. In the years leading up to the reform, around

¹⁰ This section draws heavily on Hall (2012).

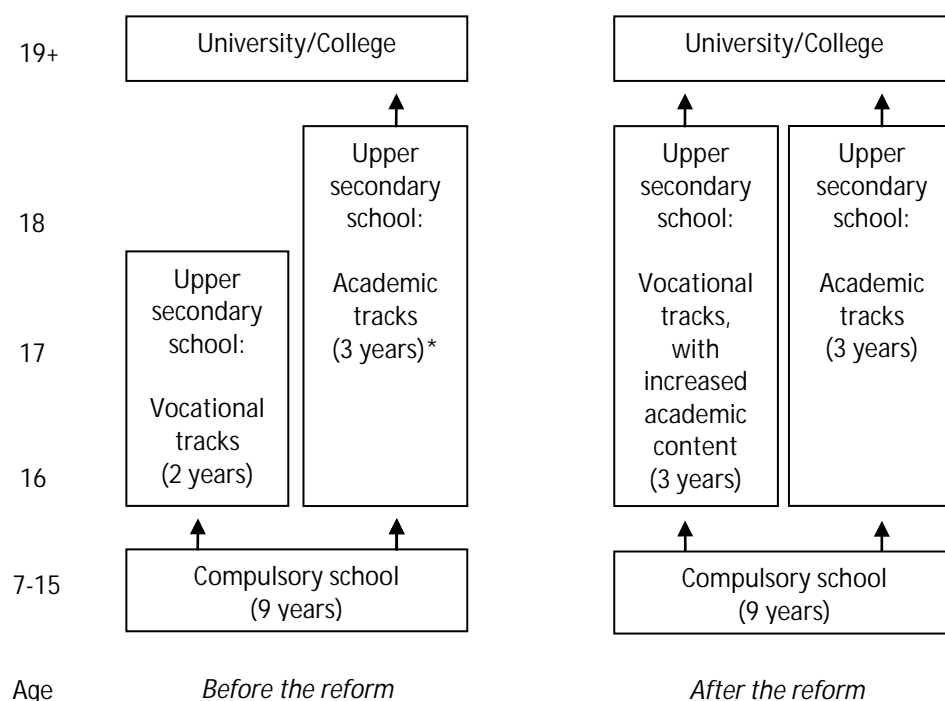
¹¹ Individuals who are older than 20 when they begin upper secondary education are not entitled to attend a general upper secondary school, but instead enter the adult education system. Within this system, both those who lack any upper secondary education and those who dropped out before graduating can finalize a degree. It is also possible to supplement e.g. a two-year upper secondary degree in order to obtain a three-year degree. For more information on the adult education system, see Stenberg (2009).

¹² There also existed a few two-year academic options as well as one four-year option.

45 percent of the upper secondary school students were enrolled in vocational tracks. Construction, electrical engineering and caring services represented some of the most common tracks. (Table A 1 lists the vocational tracks that existed at the time.)

The general aim of the reform was to enhance the quality of education and increase the flexibility of the upper secondary school system. The largest changes concerned the vocational tracks: They were prolonged from two- to three-year tracks, and received a considerably larger academic content. These changes were motivated by the view that today's working life requires more general knowledge, as well as by the desire to enable all students to enroll in university studies. As a result of the reform, all students graduating from vocational tracks attained basic eligibility¹³ for university studies. Figure 2 illustrates the Swedish school system before and after the reform.

Figure 2: The Swedish school system before and after the reform



Note: *) There was also a small proportion of 2-year academic tracks as well as one 4-year academic track. Regardless of track length, all academic tracks gave eligibility to university studies.

¹³ Note that 'basic eligibility' does not mean eligibility to all university studies as some programs have special requirements.

The reform was preceded by a nation-wide pilot period during 1988–1993 in which new three-year vocational tracks were tried out in several municipalities.¹⁴ The vocational tracks in the pilot had increased academic content compared to the regular two-year tracks. While Swedish was the only general theoretical subject included in all two-year tracks, the pilot tracks also contained English, Social Studies and an elective course. Math appears to be by far the most common choice of elective.¹⁵ Another difference between the two- and three-year tracks is that the latter located a larger share of the vocational training to actual workplaces.¹⁶

The pilot comprised around 6,000 educational slots in 1988, 10,000 in 1989, and 11,200 in 1990. On top of this, there was a very limited ‘pre-pilot’ in 1987 which only contained 500 slots.¹⁷ The number of three-year slots thereby corresponded to between 1 and 20 percent of the total number of slots in vocational tracks. For the current study it is important to point out that the total number of slots in vocational tracks did not expand due to the pilot; rather a class in the pilot always replaced a class in a corresponding two-year track.

The National Board of Education was responsible for allocating the pilot slots among the different vocational tracks as well among the municipalities. The allocation of slots among the different tracks was done proportionally; the goal was that each track would receive the same share of three-year vocational slots as they received of two-year slots. There were however some deviations from this principle, e.g. tracks with a smaller number of slots were somewhat overrepresented. The allocation decision was further restricted by the fact that in the beginning of the pilot period no curricula had yet been prepared for some of the three-year tracks. This meant that all of the 18 three-year tracks available could not be included in the pilot the first years. Table A 2 lists the tracks that were included each year as well as their number of available slots.¹⁸

¹⁴ This extensive pilot scheme was the outcome of a thorough evaluation of the vocational upper secondary education conducted by a government appointed committee (ÖGY). See e.g. Prop. 1987/88:102 for a description of the pilot.

¹⁵ The National Board of Education (1990a) reports that 86 percent of the students in 1988 chose to study Math.

¹⁶ About 40 percent of the extended time seems to have consisted of general theoretical courses for most tracks (own calculations based on information provided in Government Bill 1987/88: 102, p. 35–39). Compared to the pilot tracks, the three-year tracks that were implemented after the 1991 reform contained even more academic subjects and somewhat less training in workplaces.

¹⁷ The 1987 tracks were somewhat different as they did not contain more extensive workplace training. The description of the implementation process below is based on SOU 1989:106 and refers to the actual pilot. There is no available documentation of the implementation of the pre-pilot scheme.

¹⁸ The share vacant slots in the pilot varied from 0.03 in 1987 to 0.07 in 1990. The share vacant slots was in general somewhat lower for the pilot tracks than for the regular two-year vocational tracks.

The government stipulated that the pilot be distributed between regions with different industry and population structures. It also stated that different regions should participate to different extents: in some regions all or a large share of the vocational tracks should be prolonged to three-year tracks; in others only a few of the tracks should be prolonged. The motive behind these requirements was to get an idea of how the more extensive workplace training worked in different types of labor markets, as well as of the strain on the local labor market if it was implemented on a large scale. On top of these criteria, the National Board of Education tried to assess whether the local labor market would be able to arrange the extended workplace training in a relatively short time. To judge this they relied upon recommendations from employer and union representatives in different sectors. This concern seems to have meant that some priority was given to municipalities with a tradition of involving workplace training in the vocational education. The initiative to participate always came from the municipalities themselves as they had to apply in order to be considered. The interest was large; each year the demand for pilot slots by far exceeded the number of available places.

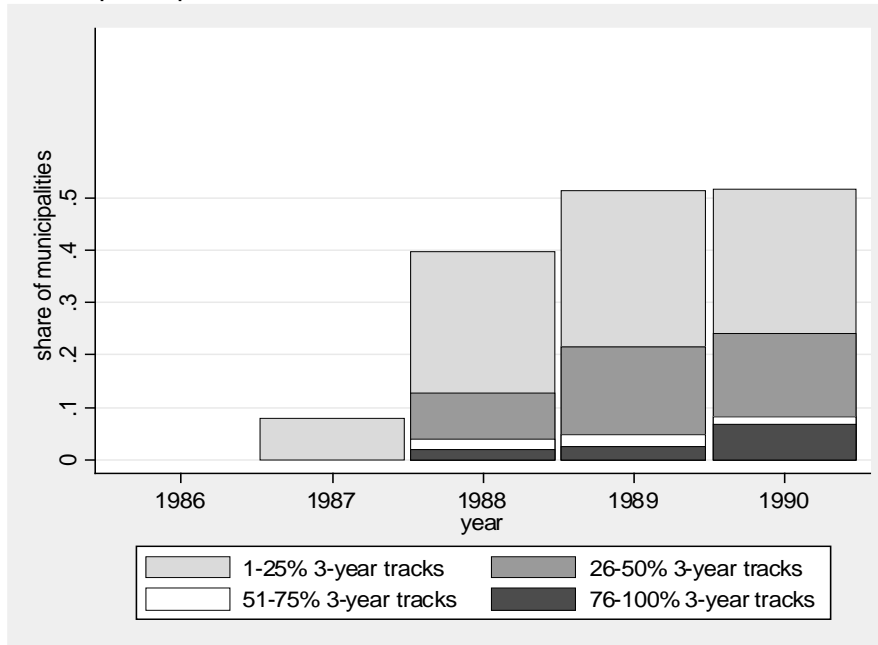
Around 70 percent of Sweden's 284 municipalities offered vocational tracks at the time. Students living in the other municipalities had to attend school in a nearby municipality if they wanted to obtain a vocational degree.¹⁹ When the pilot was launched in 1988, about 40 percent of the municipalities were granted participation. In 1990, the share had increased to about 52 percent. The extent to which the municipalities participated also increased each year as more tracks were included in municipalities that already participated. Figure 3 shows the share of municipalities that participated each year as well as how the extent to which they participated varied over time.

All through the pilot period most participating municipalities offered both two- and three-year vocational tracks. Sometimes a municipality would offer the same track both as a two- and as a three-year option. Also in municipalities that only offered either two- or three-year tracks, students could in some cases have a choice of program length if a nearby municipality offered tracks of a different length. Hence, the pilot generates a setting where some students were given the choice of enrolling in a more academic three-year, rather than a regular two-year, vocational track. The degree to which an

¹⁹ Students generally attended a school in their municipality of residence, but if the track they desired to follow was not offered they could instead choose to attend in a nearby municipality.

individual had this choice depended on where he/she lived as well as on which year he/she began upper secondary school.

Figure 3: Share of municipalities that participated in the pilot each year, and the extent of their participation



Note: '% 3-year tracks' is the percent of all vocational tracks available in a municipality which were part of the pilot. Source: Figure 1 in Hall (2012). The calculations are based on the Upper Secondary School Application record.

3 Research design and data

3.1 Exploiting the pilot scheme as a policy experiment

For the baseline results, we wish to estimate the following regression model

$$Convicted_{ijs}^{\leq 15 \text{ years}} = \alpha + \beta 3\text{yearprogram}_{ijs} + \gamma X_i + \delta_j + \theta_s + \varepsilon_{ijs} \quad (1)$$

where i indexes individual, j municipality of residence, and s upper secondary school starting year; $Convicted_{ijs}$ is an indicator equal to one if the individual has been convicted for a crime committed within 15 years of starting upper secondary school and zero otherwise; $3\text{yearprogram}_{ijs}$ is a dummy which takes the value one if the individual chose to attend a three-year (or longer) track, and zero if he/she attended a two-year track; X_i is a vector of individual and family characteristics (compulsory school GPA, sex, age at enrolment, immigrant background, each parent's highest education level, each parent's age, each parent's wage earnings, father convicted, and

whether both of the parents have immigrant background); δ_j and θ_s represent municipality of residence and upper secondary school starting year fixed effects; ε_{ijs} is an error term. The parameter of interest is β which ideally gives the causal effect of attending a longer track.

Even though equation (1) includes a rich set of covariates as well as controls for ability, as measured by compulsory school GPA, one could still be concerned that standard OLS estimates may be biased due to non-random selection into education. There is potentially a large number of unobserved factors included in ε_{ijs} which could be correlated with an individual's choice of educational track. For instance, individuals with high career aspirations or low discount rates may be more likely to invest in education and may also have a lower risk of committing crime. It is also possible that investments in education and crime are determined jointly, making the analysis susceptible to reverse causality (see Hjalmarsson 2008).

To account for endogenous schooling choices we exploit variation across regions over time in the implementation of the pilot which preceded the reform. As previously mentioned, the pilot gave some students the choice of attending a three-year rather than a regular two-year vocational track. The extent to which a person had this choice depended jointly on: (i) which year the student finished compulsory school, and (ii) the student's municipality of residence. We argue that this plausibly exogenous variation, conditional on upper secondary school starting year and municipality of residence, is a valid instrument for the length of the chosen track.²⁰

More specifically, our instrument is the degree to which the individual's municipality of residence participated in the pilot by the time he/she began upper secondary school, as measured by the share of the available vocational tracks which constituted three-year tracks, i.e. $N^{-1} \sum_i^N 1\{Track_i > 2year\}$.²¹ Ideally, the instrument would be measured as the share of available *slots* in vocational tracks which represented three-year tracks, but such data are not available at the municipality level. Assuming that the instrument is uncorrelated with any unobserved variables affecting the conditional outcomes of

²⁰ This identification strategy has previously been used by Hall (2012) to investigate the effect of the reform on educational attainment and earnings, and by Grönqvist and Hall (2013) to investigate the effect on teenage childbearing. Similar strategies have also been used in other studies; see e.g. Duflo (2001).

²¹ The instrument is zero for municipalities not offering any vocational tracks. In section 4.3 we discuss some alternative definitions of pilot intensity.

interest, and that it had no direct effect on the outcomes other than through influencing whether the person attended a two- or a three-year track, an instrumental variables (IV) estimator of β is consistent. In the empirical analysis we provide several pieces of evidence in support of the validity of these assumptions. If the effect of attending the three-year track varies across individuals, and if there is also sorting on gains, the IV estimate should be interpreted as the effect for individuals who on the margin are induced to select a three-year track because of the pilot and the margin that varies with the instrument (e.g. Björklund and Moffitt 1987; Heckman and Vytlacil 2005).

It is important to note that the design of the pilot generates a setting where some students were given *the choice* of attending a three-year rather than an ordinary two-year vocational track. Because individuals are allowed to drop out of school, the parameter we estimate is thus not necessarily the same as in studies on compulsory schooling reforms where individuals are forced to stay in school. However, as long as countries decide to keep their higher education voluntary, we believe that this parameter is relevant for public policy. We discuss effects of the reform on the risk of dropping out in section 4.3. It is also worth mentioning that just as for studies exploring compulsory schooling reforms our estimates reflect the combined effect of prolonging education and changes in the curriculum.

3.2 Data

Our data originate from several administrative registers collected and maintained by Statistics Sweden. The registers contain information on the entire Swedish population aged 16 and above each year from 1985 to 2007. These data have been linked to the Swedish Conviction Register kept by the National Council for Crime Prevention (BRÅ).²² We obtained complete records of all criminal convictions during the period. The data include information on crime type, date of the crime, as well as the sentence ruled by the court, and covers convictions in Swedish district courts (the court of first instance). One conviction may include several crimes and we observe all crimes within a single conviction. The conviction data exclude some offenses such as speeding tickets, but include e.g. driving without a license and driving under the influence. In some cases, individuals may be found guilty of a crime without being prosecuted or sentenced

²² Only a few previous studies that analyzes crime have used Swedish individual conviction data merged to population registers; see Grönqvist (2013); Hällsten, Sarnecki and Szulkin (2011); Hjalmarsson, Holmlund and Lindquist (2011); Meghir, Palme and Schnabel (2011).

in court. This happens if the offender is very young or if he/she confesses to a less severe crime. Although these cases are handled by the district attorney they are still included in our data.

We use the Upper Secondary School Application Record to obtain information on when and where an individual began upper secondary school as well as what track (type and length) he/she started. This register is used to construct the sample of individuals, but also to acquire information on which educational tracks each municipality offered each year. Based on this information we then determined which municipalities participated in the pilot each year and the share of the available vocational tracks which constituted three-year tracks.

Our sample consists of individuals who began upper secondary school during 1986–1990. In the baseline analysis we include all upper secondary school enrollees, including those in academic tracks. As will be discussed below, the results are qualitatively very similar if we instead focus on those who enrolled in vocational tracks.²³ A restriction imposed is that only pilot tracks which corresponded to tracks in the regular system are included (see Table A 1).²⁴ We have also excluded some individuals who were younger than 15 or older than 20 when they enrolled in upper secondary school. Furthermore, and importantly, we limit the main analysis to males. Results for females will however also be discussed. Crime rates are substantially lower among women compared to men. In our sample, the fraction convicted for any type of crime up to 15 years post the start of upper secondary school is four times higher among men (27.9 compared to 7.0 percent). Our sample of male students consists of 116,787 vocational and 107,654 academic students, which makes a total of 224,441 individuals.²⁵

We augmented the dataset with information on each parent's education (measured in 1990), age, and whether both parents have foreign background (defined as born outside Sweden). For the fathers, we also added information on employment status and wage earnings (measured in 1990), as well as criminal convictions (measured in the year the child enrolled in upper secondary school). We also have data on the students' final

²³ In section 3.4 we investigate whether the availability of pilot programs affected selection into vocational tracks.

²⁴ This restriction excludes students in the two smallest three-year tracks (Graphic and Handicraft); in total 176 persons.

²⁵ Around 8 percent of the original sample has been excluded due to missing information on some of the variables. For most of these we lack information on location of residence and compulsory school GPA.

grade point average (GPA) from compulsory school. Region of residence is defined as the municipality of residence in December the year before enrollment in upper secondary school. This way we avoid the possibility that municipality of upper secondary school attendance may be endogenous with respect to the location of the pilot.²⁶

The main advantage of using individual level conviction data is that we can investigate whether the potential effect on crime differs in subgroups of the population. We center on groups at higher risk of criminal involvement. We stratify individuals according to their compulsory school GPA as well as according to their predicted dropout risk.

Criminal behavior is in this paper inferred from register information on convictions. The main advantage of administrative data compared to crime self-reports is that the latter is known to be plagued by underreporting and measurement error (McDonald 2002). The large samples available in administrative registers also increase statistical precision. Still, conviction data are not flawless. A possible objection is that criminal behavior is only observed for individuals who have been convicted in court. One concern in the context of this paper is that people who perform worse in school may be more likely to get convicted conditional on actually having committed a crime. These individuals may for instance have fewer resources available for defense in a criminal trial. This is a caveat important to bear in mind when interpreting the results. Note however that this is only a problem if this kind of selection is not picked up by our extensive set of control variables.²⁷

3.3 Descriptive statistics

Table 1 presents descriptive statistics, for males attending two-year and three-year vocational tracks, and for those enrolling in academic programs. Regardless of track, the vast majority starts upper secondary education at age 16. The table clearly shows the expected pattern that academic students have more favorable background characteristics

²⁶ It is unlikely that students would move already during compulsory school in order to take advantage of the pilot tracks, especially as it was already possible to apply to upper secondary schools in municipalities other than one's own. Moreover, the decision of where to locate the new available pilot slots each year was not taken until during the following spring, i.e. after the point in time when we measure municipality of residence (see SOU 1989:106 for details on the implementation process).

²⁷ In their study of the effect of education on crime as measured by arrests, Lochner and Moretti (2005) raise a similar concern. Using data on self-reported crime they conclude that for this to be a problem, education must substantially alter the probability of being arrested conditional on criminal behavior.

and study credentials. Compulsory school GPAs are much higher, and mothers and fathers have substantially more education. Since the three-year vocational tracks were not only longer but also contained more general theoretical content, it is not surprising to find that the students taking these programs are also somewhat positively selected in these dimensions. However, the differences relative to the two-year enrollees are modest, especially compared to the academics students. In particular, there is no significant difference in the criminal background of fathers between these two groups.

Table 1: Descriptive statistics. Males only

	Vocational students		Academic students
	2-year tracks	3-year tracks	
Individual background characteristics:			
Age at enrolment in upper secondary school	16.10	16.09	16.02
GPA compulsory school ^a	25.57	26.55	64.35
Parental characteristics:			
Both parents foreign born	.070	.052	.058
Mother's age	42.16	42.21	43.13
Father's age	45.12	45.20	45.65
Mother completed upper secondary education	.591	.639	.791
Mother completed post- secondary education	.117	.137	.380
Missing data on mother's education	.020	.016	.015
Father completed upper secondary education	.505	.554	.746
Father completed post- secondary education	.094	.113	.353
Missing data on father's education	.056	.043	.043
Father's wage earnings ^{b, c}	135,764	139,144	193,354
Father employed ^{b, c}	.888	.896	.917
Father convicted of any type of crime ^b	.020	.019	.011
Outcome variables:			
Convicted of any type of crime, £ 15 years post school start	.363	.344	.190
Number of convictions, £ 15 years post school start	.894	.750	.286
Convicted of violent crime, £ 15 years post school start	.067	.059	.019
Convicted of property crime, £ 15 years post school start	.124	.110	.050
Number of observations	103,621	13,166	107,654

Notes: a) The GPA's are percentile ranked by year of graduation. b) In year 1990. c) Missing values are replaced with zeros.

The lower panel of the table exhibits the fractions with at least one crime conviction within 15 years post starting upper secondary school. The levels for “any crime” are strikingly high. One in three of those in vocational programs, and one in five of those in

academic programs have a conviction. Property crimes are roughly twice as common as violent crimes, even though also the latter are relatively frequent. A closer look (see Table 2) at the types of offenses in the data, reveals that about 30 percent of the convictions are for violations of traffic laws: driving under the influence, driving without a license etc. About 23 percent of the convictions consider theft (shoplifting, burglary, robbery).

Table 2: Types of crime in the main sample

	Examples	Percent of total crime
Crimes against the general penal code		50.28
Violent crime	Assault, murder, manslaughter	12.05
Property crime: theft	Burglary, robbery, shoplifting	46.04
Other property crimes	Fraud, forgery	15.55
Vandalism		10.35
Unlawful threat		5.87
Sex crimes	Rape, sexual abuse	0.54
Other crimes against the general penal code	Perjury, public disorder	9.4
Crimes against the special penal codes		49.72
Traffic law	DUI, driving without licence, hit and run, reckless driving, driving substandard vehicle	60.91
Narcotics law	Smuggling, possession, selling of illicit drugs	8.32
Weapons law	Unlawful possession of fire arms, possession of knives at public venues	3.77
Other crime against the special penal codes	Violations of the hunting law, moonshining, tax crimes	27.01

3.4 Issues related to the identification strategy

As described in section 3.1, our empirical approach builds on the arguably exogenous variation given by the pilot scheme for the possibility of choosing a three-year rather than a two-year vocational track. Two potential issues arise immediately: (i) pilot intensity may affect the choice of whether to enroll in upper secondary school and/or the choice between vocational and academic programs, and thus the selection of individuals; (ii) pilot intensity could be endogenous to the characteristics of the student population.

Table A 3 provides some evidence related to the first points. It displays estimates of the effect of pilot intensity on the probability of enrolling in upper secondary school and choosing a vocational as opposed to an academic track respectively (using the same set of covariates as in the main analysis). None of the estimates, with or without covariates, in the overall sample or in subgroups, are statistically significant at conventional levels. This suggests that selection is probably not a big concern. For those with the highest compulsory school GPA, there is a tendency to choose vocational tracks more often when there are more three-year vocational programs. Therefore, in the baseline analysis, we include all upper secondary school enrollees, including those in academic programs.

The second point raised above is addressed in Table A 4, which presents results from regressions of individual characteristics on pilot intensity. Overall, the results do not indicate that pilot intensity is to any substantial degree correlated with student characteristics. A few estimates are indeed statistically significant, but the association is rather small in a quantitative meaning. For example, those who were 17 instead of 16 years old when they enrolled in upper secondary school, met on average a 0.35 percentage points lower share of three-year programs, i.e. a quite marginal difference relative to the average pilot intensity of 11.2 percent.

4 Results

This section presents the results from the empirical analysis. We begin by discussing the effects of the reform on crime in general and on certain types of crimes, in the overall population under study as well as in different subgroups. This presentation is followed by some robustness checks. Then we turn to investigate some issues related to potential underlying mechanisms.

4.1 Effects of the reform

Table 3 presents the baseline results on the association between attending a three-year (or longer), instead of a two-year, upper secondary school track and the probability of

Table 3: OLS and IV estimates of the effect of enrolling in a 3-year (or longer) track on the probability of committing crime in different ages (all male students in upper secondary school)

	Crime £ 15 years post school start	# Crimes £ 15 years post school start	Age 16-20	Age 21-25	Age 26-30
	(1)	(2)	(3)	(4)	(5)
Any crime					
OLS	-.003 (.003)	-.530*** (.033)	-.009*** (.002)	.001 (.002)	.004*** (.002)
IV	-.020 (.032)	-.170 (.146)	-.048* (.028)	.001 (.020)	.007 (.015)
Reduced form: Effect of pilot intensity	-.007 (.012)	-.061 (.055)	-.017* (.010)	.000 (.007)	.003 (.005)
Mean of dependent variable	.279	.593	.183	.120	.070
Violent crime					
OLS	-.003** (.001)	-.057*** (.002)	-.003*** (.001)	.000 (.001)	-.000 (.001)
IV	-.008 (.014)	-.001 (.021)	-.005 (.009)	-.006 (.008)	-.004 (.005)
Reduced form: Effect of pilot intensity	-.003 (.005)	-.000 (.007)	-.002 (.003)	-.002 (.003)	-.002 (.002)
Mean of dependent variable	.044	.053	.020	.016	.008
Property crime					
OLS	-.001 (.002)	-.185*** (.013)	-.001 (.002)	.000 (.001)	-.000 (.000)
IV	-.046** (.019)	-.072 (.051)	-.042** (.018)	-.007 (.009)	.007 (.005)
Reduced form: Effect of pilot intensity	-.016** (.007)	-.026 (.019)	-.015** (.007)	-.003 (.003)	.002 (.002)
Mean of dependent variable	.088	.174	.072	.024	.009
Number of observations	224,441	224,441	224,441	224,441	224,441

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in a three-year (or longer) track and zero if he/she enrolled in a two-year track. The instrument (pilot intensity) is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/** denotes significance on the 10/5/1 percent level.

being convicted of a crime. Column (1) contains the total sample within 15 years after enrollment; column (2) reports results using the number of convictions rather than a binary outcome; columns (3)–(5) presents estimates for finer age intervals. The first panel investigates the probability of any type of crime, whereas the lower panels consider violent and property crimes respectively. We include three different estimators: OLS (regressing the outcome on the potentially endogenous enrollment variable); IV (using pilot intensity as an instrument for track choice);²⁸ and Reduced form (regressing the outcome on the instrument).

In column (1), the OLS estimates suggest very little correlation between crime propensity and attending a longer and more general upper secondary school program. There is a statistically significant association with violent crime, but it is very small in economic terms. However, the IV estimate for property crime suggests a sizable impact of the reform: 4.6 percentage points relative to an average of 8.8 percent means a decrease of over 50 percent. Comparing instead to the higher and arguably more relevant baseline of 12.2 percent among vocational students, the decrease is still 37 percent. Assuming a LATE interpretation of the coefficient, it seems that those induced to attend three-year programs by higher coverage in their home municipality at the time of enrollment, exhibit lower degrees of property crime due to the reform.

We think it is plausible to assume that if the pilot affected individual crime rates, it worked through the amount and type of education given to participating individuals (i.e. the IV approach is justified). Nevertheless, Table 4 also presents reduced-form estimates from regressing conviction probabilities on pilot intensity directly. This model is relevant if one is interested in the total effect of offering longer and more general education to a greater share of a given population. The point estimate for the entire sample suggests that offering only three-year vocational programs as opposed to no three-year vocational programs leads to a 1.6 percentage point drop in property crime among the students, i.e. a reduction of around 18 percent.

When using the number of convictions as the outcome, column (2) shows a larger discrepancy between OLS and IV. The OLS estimates suggest a large decrease in the number of convictions from attending a three year program. The IV results are negative and close in magnitude (to the relevant baseline), to the results in column (1). The

²⁸ As shown in Table A5, the instrument very strongly predicts participation in three-year vocational programs. This is true for the overall sample as well as in all subgroups. Weak instruments are thus not a concern.

estimates for the number of convictions are, however, quite noisy and not statistically significant. For the rest of the analysis, we focus on the extensive margin rather than the intensive.

Table 4: Compliance and crime by place in the compulsory school GPA distribution

	Fraction of the sample	First-stage coefficient	Compliance rate	Any crime (4)	Violent crime (5)	Property crime (6)
	(1)	(2)	(3)			
Q1 quartile	.261	.649	.476	.398	.085	.176
Q3-Q2	.492	.347	.480	.217	.027	.069
Q3	.248	.043	.030	.115	.008	.033
Overall	1	.356	1	.237	.037	.088

Notes: In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in a three-year (or longer) track and zero if he/she enrolled in a two-year track. The instrument (pilot intensity) is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Compliance rate is calculated as $[(1) \times (2)] / \text{overall first-stage}$.

Turning to columns (2)–(4) it is quite clear that the effects on crime are concentrated to the ages 16–20. There we find a substantial reduction in overall as well as in property crime. For the higher age intervals there are no significant negative effects. We will return to this pattern in the discussion of mechanisms below.

A relevant question for almost all IV approaches is who responds to the instrument, i.e. who the compliers are, and if this group is different than the overall population under study. To investigate this we divide the sample into quartiles of the compulsory school GPA distribution: Q1, Q3-Q2 (i.e. the interquartile range) and Q3. Column (1) of Table 4 reports the fraction of the sample and column (2) the first stage coefficient for each group. The compliance rate reported in column (3) is calculated as the fraction of the sample times the first-stage coefficient for each group, divided by the overall first stage coefficient. We see that the compliance rate is substantially higher in the lowest quartile (0.48) than in the highest (0.03), which is expected since few students in the highest quartile chose a vocational track. Columns (4)-(6) in Table 4 report the crime rates for each group.

By re-weighting the share of convicted students in each group by the compliance rates, we can compare the crime rates for the compliers with those of the full sample.

We find that the crime rates are 25 (any crime), 43 (violent crime), and 33 (property crime) percent higher among compliers than in the general population.²⁹ Thus, the LATEs that we estimate using IV are unlikely to reflect an ATE for the overall population. The possibility that reforms such as the one we study lead to heterogeneous responses/impacts across subpopulations is expected and something we investigate further below.

4.2 Effects by background characteristics

Even though crime is rather prevalent in the population under study, it is likely to vary with background characteristics, and so is potentially the impact of educational reform on crime propensities. In this section we investigate heterogeneous effects by compulsory school GPA, by estimated drop-out risk, and by gender.

Table 5a presents estimates by compulsory school GPA (below or above average). As noted in the compliers analysis above, the mean of the dependent variables sharply decreases with previous school performance. For example, while 7.3 percent of those below the average have a conviction for violent crime, the corresponding figure for those above is 1.8 percent. Limiting the analysis to vocational students yields a similar pattern (see 0).

For overall and violent crime we see no significant impact in any part of the grade distribution. For property crime, the IV point estimates are similar and substantial, although precision is poor for the better-performing students. The reduced-form estimates reveal that the reform effect is indeed concentrated to the lower half of the grade distribution.

In Table 5 b we investigate whether the impact of additional education varies with the predicted probability to drop out of upper secondary school. The regressions used to retrieve the predictions are presented in Table A 7. For property crime, we see that the impact is present throughout most of the distribution of dropout propensities, but not for the group most likely to drop out. One interpretation is that the criminal behavior of the latter group is harder to affect through educational reform. Another potential explanation is that the crime reducing effect of prolonged education is counteracted by

²⁹ For any crime, the calculation is $.398*.476+.217*.480+.115*0.030=.297$ compared to $.237$ in the full sample. For violent crime $.085*.476+.027*.480+.008*.003=.053$ ($.037$ in the full sample), and for property crime $.176*.476+.069*.480+.033*.003=.117$ ($.088$ in the full sample).

an increase in crime due to higher dropout rates, rendering the net effect to be zero. We return to the question of dropouts in Section 4.4.

Table 5a: Effects on the probability of committing crime \leq 15 years post school start (all male students in upper secondary school)

	Any type of crime (1)	Violent crime (2)	Property crime (3)
A. Entire sample (Col. (1) in Table 3)			
IV: Effect of enrolling in a 3-year (or longer) track	-.020 (.032)	-.008 (.014)	-.046** (.019)
Reduced form: Effect of pilot intensity	-.007 (.012)	-.003 (.005)	-.016** (.007)
Mean of dependent variable	.279	.044	.088
Number of observations	224,441	224,441	224,441
B. GPA: below average			
IV: Effect of enrolling in a 3-year (or longer) track	-.031 (.033)	-.004 (.016)	-.043*** (.019)
Reduced form: Effect of pilot intensity	-.018 (.020)	-.002 (.009)	-.025** (.012)
Mean of dependent variable	.385	.073	.136
Number of observations	106,482	106,482	106,482
C. GPA: at least average			
IV: Effect of enrolling in a 3-year (or longer) track	.029 (.091)	-.019 (.029)	-.048 (.051)
Reduced form: Effect of pilot intensity	.004 (.013)	-.003 (.004)	-.007 (.007)
Mean of dependent variable	.183	.018	.045
Number of observations	117,959	117,959	117,959

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in a three-year (or longer) track and zero if he/she enrolled in a two-year track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/** denotes significance on the 10/5/1 percent level. In Panel B–C, the students are divided into sub-groups based on the grade distribution among the male upper secondary school students. The first stage coefficients in Panel A–C are .357 (.008), .586 (.012), and .141 (.012), respectively.

As mentioned above, crime is much more common among males than among females. This certainly is the case also for our population: while 28 percent of the male students have a conviction within 15 years of starting upper secondary school, the corresponding figure for females is 7 percent (see Table A 6). For violent crime, the female conviction rate of 0.4 percent is one tenth of that for males. By contrast, property crimes are in a relative sense more common among females in the sample; 3 percent have a conviction compared to just under 9 percent among males.

Table 5 b: Effects on the probability of committing crime ≤ 15 years post school start (all male students in upper secondary school)

	Any type of crime (1)	Violent crime (2)	Property crime (3)
A. Entire sample (Col. (1) in Table 3)			
IV: Effect of enrolling in a 3-year (or longer) track	-.020 (.032)	-.008 (.014)	-.046** (.019)
Reduced form: Effect of pilot intensity	-.007 (.012)	-.003 (.005)	-.016** (.007)
Mean of dependent variable	.279	.044	.088
Number of observations	224,441	224,441	224,441
B. Predicted risk of dropout: $\leq 25^{\text{th}}$ percentile			
IV: Effect of enrolling in a 3-year (or longer) track	-.018 (.066)	-.018 (.018)	-.073** (.035)
Reduced form: Effect of pilot intensity	-.004 (.013)	-.004 (.004)	-.014** (.006)
Mean of dependent variable	.179	.016	.043
C. Predicted risk of dropout: $>25^{\text{th}}$-75^{th} percentile			
IV: Effect of enrolling in a 3-year (or longer) track	-.037 (.041)	-.007 (.019)	-.039* (.022)
Reduced form: Effect of pilot intensity	-.020 (.023)	-.004 (.010)	-.021* (.012)
Mean of dependent variable	.325	.051	.099
D. Predicted risk of dropout: $>75^{\text{th}}$ percentile			
IV: Effect of enrolling in a 3-year (or longer) track	.041 (.048)	.012 (.036)	-.017 (.038)
Reduced form: Effect of pilot intensity	.026 (.031)	.008 (.023)	-.011 (.025)
Mean of dependent variable	.519	.126	.220

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in a three-year (or longer) track and zero if he/she enrolled in a two-year track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/** denotes significance on the 10/5/1 percent level. In Panel B–D, the students are divided into sub-groups based on the predicted probability of dropping out. The first stage coefficients in Panel A–D are .357 (.036), .198 (.031), .541 (.059) and .650 (.063), respectively.

The estimates shown in Table A 6 suggest very little impact of the reform on female crime. The reduced-form results are small and insignificant for overall and property crime. Somewhat surprisingly, there is a statistically significant although miniscule positive association between the availability of three-year programs and violent crimes. The IV estimate is positive and of non-negligible magnitude, but also seems to be

driven by outcomes at rather high ages. However, given that violent crime is such a rare event, the IV estimate becomes sensitive to outliers among the affected population.

4.3 Robustness checks

4.3.1 Vocational students only

Studying all upper secondary school students means that we may be less concerned that the pilot may have altered the composition of the students in vocational tracks (remember also that Table 2 suggests little impact on the enrollment probabilities). But given the marked differences between vocational and academic students (Table 1) one could also argue that the most affected population are those who actually start vocational programs. Appendix B presents an analysis restricting the sample to vocational students (including specifications not yet discussed for the baseline approach). The results are overall very similar to those of the baseline analysis. The clearest effects are found for property crime at younger ages.³⁰

4.3.2 Dropping covariates

Table 6 presents another variation to investigate the potential concern of unobserved heterogeneity driving the results. Assuming that unobserved components of skills are positively correlated with observed components, we re-estimate the models dropping GPA and parental education from the controls, respectively. It is reassuring to find that this has virtually no impact on the estimated coefficients.

³⁰ A somewhat odd finding is the strong positive effect on violent crime among vocational students with above-average compulsory school GPA. Further inspection reveals that the result is driven by the highest quartile, where we also see greater participation effects for vocational tracks of the pilot. We are therefore reluctant to viewing this estimate as a causal effect.

Table 6: Sensitivity of the baseline IV estimates to dropping control variables (all male upper secondary school students)

	Any type of crime (1)	Violent crime (2)	Property crime (3)
Baseline (Col. (1) in Table 3)	-.020 (.032)	-.008 (.014)	-.046** (.019)
Dropping GPA	-.011 (.034)	-.004 (.015)	-.040** (.019)
Dropping parents' education	-.019 (.032)	-.008 (.014)	-.045** (.018)
Dropping GPA and parents' education	-.014 (.033)	-.005 (.014)	-.040** (.018)
Dropping all control variables	-.014 (.033)	-.004 (.014)	-.040** (.018)
Mean of dependent variable	.279	.044	.088
Number of observations	224,441	224,441	224,441

Notes: Each cell represents a separate regression. All regressions control for municipality of residence and upper secondary school starting year fixed effects. Unless otherwise noted, the regressions also control for compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in a three-year (or longer) track and zero if he/she enrolled in a two-year track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. ***/**/** denotes significance on the 10/5/1 percent level.

4.3.3 Alternative instruments

Our instrument – the extent to which a person's home municipality participated in the pilot when he/she began upper secondary school – is measured as the share of the available vocational tracks which constituted three-year tracks. As discussed in section 3.1.1, a more ideal measure would be the share of available slots in vocational tracks which represented three-year tracks, but such data are not available at a disaggregated level. To check whether our results could be sensitive to this type of measurement error in the instrument, we follow Grönqvist and Hall (2013) and repeat the analysis with an alternative definition of pilot intensity. We instead define pilot intensity as the share of students entering vocational tracks who entered three-year tracks, in the municipality of residence. These results are shown in Table A 9. As discussed by Grönqvist and Hall, this is not an ideal measure of the supply of pilot slots either, since it is likely to be affected by student demand. Still, it is reassuring to see that the results are similar. Previous analyses of the pilot project have also experimented with gender-specific instruments, taking into account that the vocational tracks are highly gender-segregated.

Hall (2013) finds that her gender specific instruments are highly correlated with the original instrument (same as ours) for men, but less so for women.

4.4 The underlying mechanisms

This section considers the potential mechanisms at work. First, we consider dropout as a potential mechanism, then turn to the time patterns of the effects. Finally, we look into the potential importance of changes in peer composition.

4.4.1 Dropping out

As mentioned above, the reform increased the dropout rate (Table A 8) and made students less likely to finish upper secondary school with grades in all subjects (Hall 2012). Even though the effects are present across most of the distribution of dropout propensities (cf Table 5 b), one could ask whether the increase in dropouts somehow hides even bigger crime effects on those who remain in school. Disentangling this is not a trivial task and to some extent we must see our results as capturing the net effect of opposing mechanisms. Nevertheless, the upper panel of Table 7 adds indicators for dropping out and finishing without complete grades to the baseline model. In the lower panel of the table we exclude individuals who have not completed upper secondary school within six years of being admitted. The results are not much affected by these exercises, and we thus conclude that not completing high school is not an important mechanism for the impact on crime from the reform. A potential explanation (which we are unfortunately not able to investigate further), is that many “added drop-outs” occurred during the third year, and therefore these students still spent more time in school than their two-year counterparts.

Table 7: Sensitivity of the baseline IV estimates to controlling for upper secondary school dropout (all male upper secondary school students)

	Any type of crime (1)	Violent crime (2)	Property crime (3)
Baseline (Col. (1) in Table 3)	-.020 (.032)	-.008 (.014)	-.046** (.019)
Controlling for dropping out of upper secondary school	-.028 (.033)	-.011 (.014)	-.052*** (.019)
Controlling for not finishing upper secondary school with grades in all subjects	-.038 (.032)	-.015 (.014)	-.059*** (.018)
Mean of dependent variable	.279	.044	.088
Number of observations	224,441	224,441	224,441
Excluding individuals who dropped out of upper secondary school	.034 (.035)	-.011 (.013)	-.056*** (.020)
Mean of dependent variable	.219	.030	.074
Number of observations	205,888	205,888	205,888

Notes: Each cell represents a separate regression. Individuals who have still not graduated six years after admittance are considered to have dropped out. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in a three-year (or longer) track and zero if he/she enrolled in a two-year track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/** denotes significance on the 10/5/1 percent level.

4.4.2 The time pattern of the effects

Table 3 above showed that the effects are driven by observations in the ages 16–20, i.e. at the time of or shortly after attending upper secondary school. This pattern speaks against an interpretation that the reform affected crime through higher education improving economic opportunities,³¹ which in turn lowers your crime risk, or that education in itself decreases the willingness to resort to crimes. Had this been the case, we would have expected also long-term effects.

The findings are however consistent with an incapacitation effect; being in school gives less time/opportunities/risk for crime. Table 8 further investigates this issue, by estimating the impact at different times relative to the first academic year (August–May) of high school enrollment. The estimates in the upper part of the table show that the impact is clearest in the third year, i.e. when those who attend three-year programs are still in school but when the two-year programs have ended. This pattern lends further

³¹ As Hall (2012) finds little impact on labor market outcomes from the same reform this should not be interpreted as labor market outcomes being irrelevant for criminal behavior.

support to the incapacitation hypothesis. For total crime, there is a significant impact also during the second year.

Table 8: The effect on the probability of committing crime by (academic) year after enrollment in upper secondary school (all male students in upper secondary school). IV-estimates

	Year t=1	t=2	t=3	t=4	t=5
Any crime	-0.003 (.016) [.047]	-.024** (.012) [.042]	-.027** (.013) [.044]	.006 (.014) [.042]	-.016 (.014) [.038]
Property crime	-0.003 (.010) [.020]	-.008 (.007) [.016]	-.019*** (.007) [.013]	-.008 (.006) [.011]	-.012* (.006) [.009]
Violent crime	.004 (.003) [.003]	-.003 (.005) [.004]	-.006 (.004) [.005]	-.000 (.004) [.004]	.004 (.004) [.004]
Weekday crime	-.004 (.013) [.038]	-.014 (.011) [.034]	-.019 (.012) [.034]	.002 (.012) [.034]	-.017 (.012) [.029]
Weekend crime	-.001 (.008) [.011]	-.008 (.007) [.011]	-.015** (.006) [.012]	.001 (.006) [.011]	-.002 (.006) [.010]
Summer break crime	-.011* (.006) [.010]	-.005 (.006) [.010]	-.002 (.006) [.010]	.008 (.007) [.009]	.005 (.006) [.007]
Number of observations	224,441	224,441	224,441	224,441	224,441

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The dependent variable is defined as crime committed during the school year (August–May). The (potentially) endogenous variable takes the value one if the individual enrolled in a three-year (or longer) track and zero if he/she enrolled in a two-year track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. Mean of dependent variable in brackets. */**/** denotes significance on the 10/5/1 percent level.

The lower part of the table exploits even finer information on the timing of the crime, classifying the observed offenses into weekdays and weekends, and adding information on summer break crimes. In the third year (when most effects are found), the only significant estimate is for weekend crimes. Even though one should be cautious given the precision of these estimates, it seems that the incapacitation at work is not only about actually physically being in school.

4.4.3 Peer composition and influences

Schools and classrooms are arenas where extensive social interactions occur. Previous studies have found a concentration effect on crime (Lualen 2006). As seen above, also in our relatively homogeneous sample, crime is more prevalent in some groups than others. To the extent that the reform altered the peer composition of the affected individuals, this is a potential channel for the observed effects.

Table 9 describes the peer composition among male students taking different tracks. Peer groups are defined based on beginning the same program (two- and three-year tracks are always considered to be different programs), in the same school, the same year. Three-year vocational students have a slightly more favorable peer composition: average GPA is higher, and mothers and father are somewhat more educated. The three-year tracks also contain fewer peers, which potentially means fewer possible frictions.³² But there are no differences in the conviction rate among fathers. The differences between the longer and shorter vocational tracks are however very small compared to the much stronger socioeconomic background seen among students in the academic tracks.

Table 9: Averages among the student's peers. Males only

	Vocational students		Academic students
	Two-year tracks	Three-year tracks	
GPA compulsory school ^a	26.64	27.99	68.39
Mother completed at least upper secondary education	.583	.633	.783
Father completed at least upper secondary education	.497	.544	.734
Father convicted of any type of crime ^b	.021	.020	.011
Number of peers	36.00	28.10	78.74
Number of observations	103,561	13,166	107,633

Notes: Students that enrolled in the same track, in the same school, the same year, are considered peers. a) GPA's are percentile ranked by year of graduation. b) During the year the child enrolled in upper secondary school.

Estimating peer influences provides several methodological challenges, which are beyond the scope of the present analysis. Table 10 does however provide an attempt to see if peers are a channel through which the effects we find operate. Departing from the baseline model, we stepwise add different sets of control variables reflecting characteristics of the peer group. The results are easily summarized: the IV estimates are not affected, suggesting that peer composition is not an important part of the story.

³² It should be acknowledged that our peer measure is not perfect; the relevant peer group may be smaller (classes) or bigger (schools) than what we measure.

Table 10: IV estimates of the effect of enrolling in a three-year (or longer) track on the probability of committing crime between age 16 and 20 controlling for peer influences (all male upper secondary school students)

Entire sample	Any type of crime (1)	Violent crime (2)	Property crime (3)
A. Baseline (Table 3, Col. 3)			
Effect of enrolling in a three-year (or longer) track	-.048* (.028)	-.005 (.009)	-.043** (.018)
B. Adding control for share of convicted fathers among peers			
Effect of enrolling in a three-year (or longer) track	-.047* (.028)	-.005 (.009)	-.042** (.019)
Share of convicted fathers	.010 (.052)	.015 (.019)	-.055 (.038)
C. + Adding controls for share of highly educated mothers and fathers among peers			
Effect of enrolling in a three-year (or longer) track	-.048 (.030)	-.005 (.010)	-.046** (.020)
Share of convicted fathers	.017 (.044)	.012 (.018)	-.033 (.032)
Share of highly educated fathers	.008 (.031)	.000 (.010)	.037* (.021)
Share of highly educated mothers	.014 (.021)	-.008 (.007)	.030** (.013)
D. + Adding control for average GPA among peers			
Effect of enrolling in a three-year (or longer) track	-.047* (.029)	-.005 (.009)	-.043** (.019)
Share of convicted fathers	.025 (.040)	.011 (.018)	-.010 (.029)
Share of highly educated fathers	-.001 (.014)	.002 (.005)	.012 (.010)
Share of highly educated mothers	.006 (.010)	-.007* (.004)	.009 (.006)
Average GPA	.000 (.000)	-.000 (.000)	.001* (.000)
Mean of dependent variable	.183	.020	.072
Number of observations	224,360	224,360	224,360

Notes: In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in the extended three-year vocational track and zero if the individual enrolled in a regular two-year vocational track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/** denotes significance on the 10/5/1 percent level.

5 Concluding remarks

Many countries are in the process of implementing reforms to make secondary education more general. Such reforms may be desirable in some dimensions but also run the risk of increasing dropout rates and various social problems related to dropout behavior. We exploit a Swedish reform of upper secondary vocational education, which implied longer and more general education for a group of academically less oriented students where crime is rather prevalent. The analysis is highly policy relevant since it considers youth at risk and also concerns a type of reform much discussed and politically and economically feasible. Another contribution is that we study the impact of the reform on criminal activities both in the short and in the long run, and are able to investigate the mechanisms through which the reform worked.

We find that the reform had a robust negative impact on the probability of being convicted for a crime, in particular property crimes. The magnitude of the estimated effect is substantial. The reduced-form estimates suggest that having only as opposed no three-year programs would lead to a drop in overall crime by 1.7 percentage points, relative to a mean of 18 percent, among boys age 16–20. For property crime the corresponding estimate is 1.5 percentage points or more than 20 percent. The IV estimates suggest that for boys who were induced to take a longer education because of the reform, the probability of a conviction for property crime within 15 years of starting upper secondary school fell by 4.6 percentage points. For girls, who are much less likely to exhibit convictions, we find basically no impact of the reform. Further estimations reveal that the impact appears to be concentrated in the lower half of the GPA distribution.

The effects we find are concentrated to the age interval 16–20. We see little impact at higher ages. Zooming even closer in time reveals that most of the impact is found in the third year after starting high school; i.e. when those taking prolonged tracks are still in school but when those in the old two-year programs have already left. This indicates that incapacitation, or keeping people occupied in a structured context, is an important mechanism behind the results. The results also indicate that the increased likelihood of dropping out of school due to the reform did not lead to an increase in crime. Furthermore, we find no evidence suggesting that changed peer composition is a driving factor behind the results.

Educational reform is typically done with good intentions, but sometimes the outcome is not what policy makers hope for. This study, however, shows that also a reform associated with problems in terms of increased dropout, and little gains regarding participation in higher education, earnings and employment (Hall 2012; 2013), may still have beneficial effects in dimensions and through mechanisms not much considered but important to the individual and carrying strong externalities.

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Appendix A

Table A 1: Vocational tracks in upper secondary school

Regular two-year vocational tracks	Three-year pilot tracks
Agriculture	Use of natural resources
Forestry	
Gardening	
Business & administration	Business & services
Distribution & administration	
Caring services	Health care
Social services	
Caring services: children & youth	Caring services: children & youth
Clothing manufacturing	Textile & clothing manufacturing
Construction	Construction
	Constructional metalwork
	Heating, ventilation & sanitation
	Painting
Consumer studies*	
Electrical engineering	Electrical engineering
Food manufacturing	Food manufacturing
	Restaurant
Operation and maintenance engineering*	
Process technology	Process technology
Vehicle engineering	Transport & vehicle engineering
Wood technology	Wood technology
Workshop techniques	Industry
-	Handicraft*
-	Graphic*

Notes: *Tracks which do not directly correspond to any of the pilot tracks, but are still included in the analysis as important elements of them appear to be present on one or more of the pilot tracks. **Tracks which are not included as they do not correspond to any of the two-year tracks.

Table A 2: Number of educational slots by pilot track and year

	1987	1988	1989	1990
	No. of slots	No. of slots	No. of slots	No. of slots
Electrical engineering	48	528	656	776
Health care	46	2,182	2,918	3,072
Heating, ventilation and sanitation	64	64	72	104
Industry	352	1,608	1,952	1,968
Business and services		210	660	990
Caring services: children and youth		256	420	420
Construction		296	408	432
Textile and clothing manufacturing		136	208	224
Transport and vehicle engineering		752	992	1,056
Use of natural resources		352	640	720
Constructional metalwork			56	56
Food manufacturing			224	256
Handicraft			32	64
Painting			56	88
Process technology			176	208
Restaurant			336	416
Wood technology			144	168
Graphic				112
Total	510	6,384	9,950	11, 130

Source: National Board of Education (1988), (1989a), (1989b), and (1990b).

Table A 3: The effect of pilot intensity on the probability of enrolling in upper secondary school, and of enrolling in a vocational rather than an academic track (males only)

	Upper secondary school enrollment		Vocational track	
	(1)	(2)	(3)	(4)
A. Entire sample				
Pilot intensity in municipality of residence	.014 (.023)	.006 (.020)	.005 (.013)	.010 (.013)
Mean of dependent variable	.845	.845	.520	.520
Number of observations	163,531	163,531	224,441	224,441
B. GPA: below average				
Pilot intensity in municipality of residence	.024 (.037)	.008 (.034)	-.001 (.020)	-.001 (.019)
Mean of dependent variable	.743	.743	.841	.841
Number of observations	84,945	84,945	106,482	106,482
C. GPA: at least average				
Pilot intensity in municipality of residence	.003 (.014)	.003 (.014)	.011 (.016)	.022 (.015)
Mean of dependent variable	.961	.961	.231	.231
Number of observations	77,947	77,947	117,959	117,959
Covariates included	No	Yes	No	Yes

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, Col. 2 and 4 include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3-levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, and the father's earnings (linear). Col. 4 also controls for age at enrolment (dummies) and for whether the father has been convicted of crime. "Pilot intensity" is measured as the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */** denotes significance on the 10/5 percent level. In Panel B–C, the students are divided into sub-groups based on the grade distribution among the male upper secondary school students. The sample used in Columns (1) and (2) consists of all students who finished compulsory school during 1988–1990 (the register for compulsory school completion begins in 1988, hence this restriction).

Table A 4: Correlation between pilot intensity and student characteristics (males only)

Outcome: pilot intensity	All upper secondary school students (1)	Vocational students (2)
GPA compulsory school	-.0000 (.0000)	-.0000 (.0000)
Enrolled in upper secondary school at:		
age 15	-.0041* (.0024)	-.0041 (.0057)
age 16 (=most common age)	Ref.	Ref.
age 17	-.0029** (.0011)	-.0035*** (.0013)
age 18	.0028 (.0033)	-.0001 (.0037)
age 19	-.0049 (.0095)	-.0071 (.0106)
age 20	-.0046 (.0168)	-.0045 (.0197)
Both parents foreign born	.0007 (.0008)	.0006 (.0011)
Mother's age	-.0001 (.0001)	-.0000 (.0001)
Father's age	-.0000 (.0001)	-.0000 (.0001)
Mother completed upper secondary education	.0003 (.0005)	.0004 (.0006)
Mother completed post- secondary education	.0010** (.0005)	.0014 (.0009)
Missing data on mother's education	.0011 (.0014)	.0017 (.0019)
Father completed upper secondary education	.0006 (.0005)	.0010* (.0006)
Father completed post- secondary education	-.0004 (.0005)	-.0008 (.0011)
Missing data on father's education	-.0010 (.0010)	-.0002 (.0014)
Father employed	-.0002 (.0009)	-.0004 (.0011)
Father's wage earnings	.0000 (.0000)	.0000 (.0000)
Father convicted of crime	.0006 (.0015)	-.0005 (.0020)
Mean of dependent variable	.113	.112
Number of observations	224,441	116,787

Notes: Both regressions control for municipality of residence and upper secondary school starting year fixed effects. "Pilot intensity" is measured as the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. ***/*** denotes significance on the 10/5/1 percent level.

Table A 5: First stage regressions: The effect of pilot intensity on the probability of enrolling in a three-year (or longer) track. Male students

	All upper secondary school students		Vocational students	
	(1)	(2)	(3)	(4)
A. Entire sample				
Pilot intensity in municipality of residence	.362*** (.039)	.357*** (.036)	.660*** (.059)	.659*** (.059)
Mean of dependent variable	.488	.488	.113	.113
F-statistic on the instrument	88.02	98.97	123.07	122.76
Number of observations	224,441	224,441	116,787	116,787
B. GPA: below average				
Pilot intensity in municipality of residence	.586*** (.057)	.586*** (.056)	.679*** (.061)	.679*** (.061)
Mean of dependent variable	.185	.185	.110	.110
F-statistic on the instrument	106.76	108.10	123.72	123.24
Number of observations	106,482	106,482	89,503	89,503
C. GPA: at least average				
Pilot intensity in municipality of residence	.152*** (.024)	.141*** (.022)	.597*** (.064)	.596*** (.063)
Mean of dependent variable	.761	.761	.122	.122
F-statistic on the instrument	40.57	40.19	88.22	88.81
Number of observations	117,959	117,959	27,284	27,284
Covariates included	No	Yes	No	Yes

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, Col. 2 includes controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3-levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. "Pilot intensity" is measured as the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/** denotes significance on the 10/5/1 percent level. In Panel B–C, the students are divided into sub-groups based on the grade distribution among the male upper secondary school students.

Table A 6: OLS and IV estimates of the effect of enrolling in a three-year (or longer) track on the probability of committing crime in different ages. All female upper secondary school students

	£ 15 years post school start (1)	Age 16-20 (2)	Age 21-25 (3)	Age 26-30 (4)
Any crime				
OLS	.001 (.002)	-.000 (.001)	.002* (.001)	.001 (.001)
IV	.028 (.035)	.021 (.030)	.006 (.020)	.013 (.016)
Reduced form: Effect of pilot intensity	.005 (.006)	.004 (.005)	.001 (.004)	.002 (.003)
Mean of dependent variable	.070	.045	.021	.012
Violent crime				
OLS	-.000 (.000)	-.000 (.000)	-.000 (.000)	-.000 (.000)
IV	.016* (.009)	-.000 (.005)	.002 (.005)	.008* (.004)
Reduced form: Effect of pilot intensity	.003* (.001)	-.000 (.001)	.000 (.001)	.001** (.001)
Mean of dependent variable	.004	.002	.001	.001
Property crime				
OLS	-.000 (.001)	.000 (.001)	.000 (.000)	.000 (.000)
IV	.022 (.024)	.025 (.021)	.001 (.012)	.002 (.007)
Reduced form: Effect of pilot intensity	.004 (.004)	.004 (.004)	.000 (.002)	.000 (.001)
Mean of dependent variable	.030	.023	.006	.003
Number of observations	204,369	204,369	204,369	204,369

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in a three-year (or longer) track and zero if he/she enrolled in a two-year track. The instrument (pilot intensity) is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. ***/**/* denotes significance on the 10/5/1 percent level.

Table A 7: Estimated regression coefficients for the linear regression model used to calculate the probability of dropping out

	Outcome: Dropped out of upper secondary school
GPA compulsory school	-.006*** (.000)
GPA compulsory school (squared)	.000*** (.000)
Enrolled in upper secondary school at:	
age 15	.010 (.006)
age 16 (=most common age)	Ref.
age 17	.078*** (.004)
age 18	.192*** (.016)
age 19	.183*** (.040)
age 20	.236*** (.081)
Both parents foreign born	.023*** (.003)
Mother's age	-.001*** (.000)
Father's age	-.000*** (.000)
Mother completed upper secondary education	-.009*** (.001)
Mother completed post- secondary education	.005*** (.001)
Missing data on mother's education	.029*** (.006)
Father completed upper secondary education	.000 (.002)
Father completed post- secondary education	.008*** (.002)
Missing data on father's education	.014*** (.004)
Father employed	-.031*** (.003)
Father's wage earnings	-.000*** (.000)
Father convicted of crime	.042*** (.005)
Mean of dependent variable	.083
Number of observations	224,441

Table A 8: IV estimates of the effect of enrolling in a three-year (or longer) track on the probability of dropping out of upper secondary school (male students)

	All upper secondary school students (1)	Vocational students (2)
A. Entire sample		
IV: Effect of enrolling in a 3-year track	.054** (.022)	.045*** (.017)
Mean of dependent variable	.083	.107
Number of observations	224,441	116,787
B. GPA: below average		
IV: Effect of enrolling in a 3-year track	.054** (.022)	.055*** (.019)
Mean of dependent variable	.130	.129
Number of observations	106,482	89,503
C. GPA: at least average		
IV: Effect of enrolling in a 3-year track	.068 (.054)	.018 (.021)
Mean of dependent variable	.040	.035
Number of observations	117,959	27,284

Notes: Each cell represents a separate regression. Individuals who have still not graduated six years after admittance are considered to have dropped out. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in a three-year (or longer) track and zero if he/she enrolled in a two-year track. The instrument (pilot intensity) is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. ***/*** denotes significance on the 10/5/1 percent level. In Panel B–C, the students are divided into subgroups based on the grade distribution among the male upper secondary school students.

Table A 9: Sensitivity of the IV estimates to using an alternative definition of the instrument (all male upper secondary school students)

	Any type of crime (1)	Violent crime (2)	Property crime (3)
A. Baseline (Col. (1) in Table 4)	-.020 (.032)	-.008 (.014)	-.046** (.019)
B. Alternative instrument	-.034 (.032)	-.012 (.013)	-.045*** (.017)
Mean of dependent variable	.279	.044	.088
Number of observations	224,441	224,441	224,441

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in a three-year (or longer) track and zero if he/she enrolled in a two-year track. The instrument in Panel A is the share of available vocational tracks in the municipality of residence which constituted three-year tracks. The instrument in Panel B is the share of students entering vocational tracks who entered three-year tracks, in the municipality of residence. Robust standard errors in parentheses allow for clustering by municipality of residence. ***/*** denotes significance on the 10/5/1 percent level. The first stage coefficient in Panel A–B are .357 (.008) and .367 (.008), respectively.

Appendix B

Tables with only vocational students

Table B 1: OLS and IV estimates of the effect of enrolling in the three-year vocational track on the probability of committing crime in different ages. Male vocational students (corresponds to Table 3)

	£ 15 years post school start (1)	Age 16-20 (2)	Age 21-25 (3)	Age 26-30 (4)
Any crime				
OLS	-.001 (.005)	-.012** (.005)	-.001 (.004)	.002 (.003)
IV	-.008 (.028)	-.039 (.024)	.013 (.018)	.011 (.012)
Reduced form: Effect of pilot intensity	-.005 (.019)	-.026 (.016)	.009 (.012)	.007 (.008)
Mean of dependent variable	.361	.250	.161	.093
Violent crime				
OLS	-.004 (.003)	-.004** (.002)	.001 (.002)	.001 (.001)
IV	.004 (.013)	-.005 (.008)	.001 (.007)	.002 (.006)
Reduced form: Effect of pilot intensity	.003 (.008)	-.003 (.005)	.001 (.004)	.001 (.004)
Mean of dependent variable	.066	.031	.025	.013
Property crime				
OLS	-.007** (.003)	-.007** (.003)	-.003* (.002)	-.002* (.001)
IV	-.027* (.016)	-.032** (.015)	-.002 (.008)	.011** (.005)
Reduced form: Effect of pilot intensity	-.018 (.011)	-.021 (.011)**	.001 (.005)	.007** (.003)
Mean of dependent variable	.123	.101	.036	.014
Number of observations	116,787	116,787	116,787	116,787

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in the extended three-year vocational track and zero if the individual enrolled in a regular two-year vocational track. The instrument (pilot intensity) is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. ***/*** denotes significance on the 10/5/1 percent level.

Table B 2: Effects on the probability of committing crime ≤ 15 years post school start. Male vocational students (corresponds to Table 5a)

	Any type of crime (1)	Violent crime (2)	Property crime (3)
A. Entire sample			
IV: Effect of enrolling in a three-year vocational track	-.008 (.028)	.004 (.013)	-.027* (.016)
Reduced form: Effect of pilot intensity	-.005 (.019)	.003 (.008)	-.018 (.011)
Mean of dependent variable	.361	.066	.123
Number of observations	116,787	116,787	116,787
B. GPA: below average			
IV: Effect of enrolling in a three-year vocational track	-.023 (.032)	-.007 (.015)	-.036** (.018)
Reduced form: Effect of pilot intensity	-.016 (.022)	-.005 (.010)	-.024* (.013)
Mean of dependent variable	.400	.078	.143
Number of observations	89,503	89,503	89,503
C. GPA: at least average			
IV: Effect of enrolling in a three-year vocational track	.040 (.045)	.041*** (.014)	.005 (.023)
Reduced form: Effect of pilot intensity	.024 (.026)	.024*** (.008)	.003 (.024)
Mean of dependent variable	.232	.028	.056
Number of observations	27,284	27,284	27,284

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in a three-year (or longer) track and zero if he/she enrolled in a two-year track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/** denotes significance on the 10/5/1 percent level. In Panel B–C, the students are divided into sub-groups based on the grade distribution among the male upper secondary school students. The first stage coefficients in Panel A–C are .659 (.009), .679 (.010), and .596 (.018), respectively.

Table B 3: Effects on the probability of committing crime ≤ 15 years post school start. Male vocational students (corresponds to Table 5 b).

	Any type of crime (1)	Violent crime (2)	Property crime (3)
A. Entire sample			
IV: Effect of enrolling in the 3-year vocational track	-.008 (.028)	.004 (.013)	-.027* (.016)
Reduced form: Effect of pilot intensity	-.005 (.019)	.003 (.008)	-.018 (.011)
Mean of dependent variable	.361	.066	.123
Number of observations	116,787	116,787	116,787
B. Predicted risk of dropout: $\leq 25^{\text{th}}$ percentile			
IV: Effect of enrolling in the 3-year vocational track	.009 (.041)	.020 (.014)	-.033* (.019)
Reduced form: Effect of pilot intensity	.006 (.026)	.013 (.009)	-.022 (.013)*
Mean of dependent variable	.235	.026	.057
C. Predicted risk of dropout: $>25^{\text{th}}$-75^{th} percentile			
IV: Effect of enrolling in the 3-year vocational track	-.033 (.036)	-.005 (.018)	-.030 (.021)
Reduced form: Effect of pilot intensity	-.022 (.024)	-.003 (.012)	-.020 (.014)
Mean of dependent variable	.341	.055	.105
D. Predicted risk of dropout: $>75^{\text{th}}$ percentile			
IV: Effect of enrolling in the 3-year vocational track	.015 (.043)	.004 (.034)	-.013 (.038)
Reduced form: Effect of pilot intensity	.010 (.030)	.003 (.024)	-.009 (.027)
Mean of dependent variable	.526	.128	.224

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The (potentially) endogenous variable takes the value one if the individual enrolled in the extended three-year vocational track and zero if the individual enrolled in a regular two-year vocational track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/** denotes significance on the 10/5/1 percent level. In Panel B–D, the students are divided into sub-groups based on the predicted probability of dropping out. The first stage coefficients in Panel A–D are .659 (.009), .650 (.017), .657 (.012) and .695 (.017), respectively.

Table B 4: The effect on the probability of committing crime by (academic) year after enrollment in upper secondary school. IV-estimates. Male vocational students (corresponds to Table 8)

	Year t=1	t=2	t=3	t=4	t=5
Any crime	-.004 (.014) [.069]	-.016 (.010) [.063]	-.017 (.011) [.063]	.010 (.012) [.060]	-.012 (.012) [.052]
Property crime	-.006 (.008) [.029]	-.004 (.006) [.024]	-.015** (.007) [.020]	-.000 (.006) [.016]	-.008 (.005) [.013]
Violent crime	.002 (.003) [.005]	-.001 (.004) [.007]	-.007* (.004) [.007]	.001 (.003) [.007]	.002 (.003) [.006]
Weekday crime	-.005 (.012) [.056]	-.008 (.009) [.050]	-.009 (.010) [.048]	.006 (.011) [.047]	-.013 (.011) [.040]
Weekend crime	-.000 (.007) [.017]	-.003 (.007) [.017]	-.014*** (.005) [.019]	.003 (.006) [.017]	-.001 (.005) [.016]
Summer break crime	-.013*** (.005) [.015]	-.002 (.006) [.015]	-.003 (.006) [.015]	.009 (.007) [.013]	.004 (.005) [.011]
Number of observations	116,787	116,787	116,787	116,787	116,787

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions include controls for: compulsory school GPA (quadratic), age at enrolment (dummies), each parent's educational attainment (3 levels), whether both parents are foreign-born, each parent's age (linear), missing data on parents' education, the father's employment status, the father's earnings (linear), and whether the father has been convicted of crime. The dependent variable is defined as crime committed during the school year (August–May). The (potentially) endogenous variable takes the value one if the individual enrolled in the extended three-year vocational track and zero if the individual enrolled in a regular two-year vocational track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted three-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. Mean of dependent variable in brackets. */**/** denotes significance on the 10/5/1 percent level.

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