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Juvenile Justice
on Future Educational Outcomes**

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The Effect of Restorative Juvenile Justice on Future Educational Outcomes*

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Abstract

In this article, we study the effects of a Dutch restorative justice program for adolescent first-time offenders on early school leaving and years of education attained. Causal statistical estimates are presented using data from a randomized experiment, in which 944 adolescent offenders are randomly assigned to the experimental condition, and by linking these data to registration data that track the educational careers of all adolescents in the Netherlands. We find that the program reduces early school leaving by 6 percentage points and increases years of education attained by 0.29 years. The findings show that restorative juvenile justice programs have significant educational benefits and should be considered as a tool to reduce early school leaving and increase educational attainment.

JEL Codes: I2, K4, C93

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1 Introduction

For most types of crime, there is a negative association between education and criminal behavior (see Lochner, 2010). The impact of education on criminal behavior has been well

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documented for adults (see Becker, 1968; Lochner and Moretti, 2004; Machin et al., 2011*a*) and for the effect of educational interventions on criminal behavior (see Grossman and Tierney, 1998; Rodriguez-Planas, 2012; Heller et al., 2013). The empirical results generally indicate that more schooling or specific educational interventions can lead to a significant decrease in criminal activity. Hence, it is argued that educational interventions can be used to fight crime (see, for example, Meghir et al., 2011; Hjalmarsson et al., 2011).

Part of the observed negative correlation between educational and criminal outcomes is likely to be produced by unobserved individual characteristics. This has been the reason why many empirical studies have exploited exogenous variation in educational outcomes to obtain a better estimate of the effect of education on criminal activity. Remarkably, little attention has been paid to the reverse relation: whether reducing criminal involvement at a young age can improve educational outcomes or whether criminal prevention policies can be used to improve educational outcomes (some positive exceptions are McGarvey et al., 2007; Hjalmarsson, 2008; Webbink et al., 2013). This lack of attention may have been caused by the lack of exogenous variation with respect to criminal juvenile activity to identify the causal impact of crime prevention on educational outcomes. This study exploits exogenous variation from a large randomized field experiment in the Netherlands to estimate the effects of a Dutch restorative justice program for juvenile offenders on early school leaving and educational attainment.

The Dutch restorative justice program Halt is targeted at juvenile first-time offenders, aged between 12 and 18, who committed a non-violent crime. Halt is incorporated in the Dutch juvenile justice system. The program aims to change the behavior and attitudes of juvenile first-offenders by addressing their behavioral and development problems. The program explicitly emphasizes that it cooperates with schools to create a more safe living- and learning environment in which students can achieve better educational outcomes.

Many countries, including the US, Canada, Australia and the UK, have integrated very similar restorative justice programs in their juvenile justice system. Restorative justice programs are sometimes also referred to as intermediate punishment (Morris and Tonry, 1990), community justice (Bazemore and Schiff, 1996) and alternative sanctions (Kahan, 1996). Similarly to Halt, these programs rely on sociological theories to prevent juveniles from re-offending by improving their social behavior, and attitudes. Improving behavior and attitudes may reduce the chance of re-offending directly, but also indirectly through its effects on educational achievements. It is the latter relation that is the topic of this paper.

A few previous studies have examined the effects of criminal outcomes on schooling out-

comes. Webbink et al. (2013) use twin-information from the Australian Twin Register and find that being arrested before the age of 18 results in 0.7 to 0.9 fewer years of education and lowers the probability of finishing high school by 20 to 23 percentage points. They furthermore show that the impact of being arrested is largest for children between the age of 13 and 15. McGarvey et al. (2007) use school-level data from a large urban district in the U.S. and apply an instrumental variable analysis to examine how schooling outcomes are affected by school and neighborhood crime. They use instruments, such as the total number of adults in the school and distance from school to the nearest public housing, and argue that these factors affect schooling outcomes only through their correlation with crime and socioeconomic status. Their study suggests that school violence and neighborhood violence each have a negative effect on schooling outcomes. More precisely, they find that an additional violent incident in a school is associated with a 4 percentage point decline in academic pass rate. Hjalmarsson (2008) examines whether previous criminal involvement affects high school graduation using U.S. data. Following Altonji et al. (2005), she uses a linear multi-level probability model to examine the potential effects of selection on unobservables. The results suggest that arrest (incarceration) before the age of 16 reduces the probability of graduating from high school by 11 (26) percentage points.

The Big Brothers Big Sisters program (BBBS), the Quantum Opportunity Project (QOP) and the ‘Becoming a Man’ program (BAM) are particularly interesting for this study. These programs are not targeted at adolescents who are first-offenders, but similarly to most restorative justice programs they aim to improve schooling and criminal outcomes by changing the behavior of adolescents at-risk (i.e. adolescents who live in low-income, racially segregated and criminal neighborhoods). These programs were moreover evaluated through large-scale randomized field experiments which enabled estimation of the program’s causal impact on educational and criminal outcomes. Grossman and Tierney (1998) evaluate BBBS, a mentoring program for juveniles aged between 10 and 16. The empirical results indicate that the program improved the academic performance of girls and reduces violence in school and substance abuse. Rodriguez-Planas (2012) evaluated the QOP, which also provides mentoring, but additionally provides educational services and financial rewards to juveniles at-risk at the age of 14 or 15. The financial rewards implied that adolescents received a financial reward if they obtain a high school diploma and if they continue their education at the postsecondary level. The empirical results indicate that the program has weak positive effects on educational outcomes and a reducing effect on criminal activity for youths in the top-half of the risk distribution. The program, however, was not effective for youths in

the bottom-half of the risk distribution. Heller et al. (2013) evaluated BAM which exposes adolescents to regular interactions with pro-social adults, keeps adolescents busy during the high-risk after-school hours, and implements aspects of cognitive behavioral therapy. The empirical outcomes indicate that educational outcomes are positively influenced and that the amount of violent crime-arrests has reduced. The results of these studies therefore suggest that that educational programs may significantly influence educational and criminal outcomes, and hence, it may be that restorative justice programs establish a similar effect.

This study uses data from a unique field experiment that was conducted in 2003. In this experiment 1,064 juvenile first-offenders who were who are apprehended by the police for a non-violent offense were invited to participate in the Halt-experiment. Juveniles had an incentive to participate in the experiment because by participation they avoided criminal charges and a criminal record. 944 juveniles agreed to participate in the experiment and were randomly assigned to the Halt program and a control group. Juvenile first-offenders assigned to the control group were sent home without a criminal record and criminal charges, but with the obligation to come back after 6 months to complete a questionnaire.

To determine the program effects on early school leaving and educational attainment, we linked the experimental data to education data of Statistics Netherlands, such that we could track the educational careers of juvenile first-offenders who were invited to participate in the experiment. 19 percent of the juveniles in the Halt group did not complete the program and to control for the potential bias that is imposed by the (un)observed selective dropout we apply an instrumental variable approach in which we instrument actual participation by the assignment status.

The empirical results show that the restorative justice program Halt reduces early school leaving by 6 percentage points and increases years of education attained by 0.29 years. Tests for heterogeneous treatment effects indicate that there are insignificant differences for subgroups in the sense that the program effects are (insignificantly) smaller for boys, adolescents whose parents are born in the Netherlands and juveniles in single parent households. Our findings therefore show that criminal intervention programs targeted at juveniles can significantly improve educational outcomes and, hence, that criminal policies can be used to improve educational outcomes.

This study contributes in several ways to the literature on criminal and educational outcomes. First of all, our study is the first that evaluates the causal impact of a restorative justice program on educational outcomes. Hence, it is also the first study that empirically shows that criminal intervention programs significantly reduces early school leaving and

increases years of education. Because the Dutch program appears to be representative for restorative justice programs in many other countries it is informative about the potential effects that restorative justice may have in these countries.

The second contribution is that this study not only focuses on the short-term program effects, as is often the case in randomized field experiments, but also examines the medium- and long-term program effects (e.g. see Reynolds et al., 1997).

Finally, the empirical findings of this study contribute to the current debate on the value and cost-effectiveness of restorative justice programs, which takes place in many countries, such as the U.K., Canada and the U.S. (Carreira Da Cruz, 2010). Restorative justice programs are rather controversial because they are implemented on a large scale, while there is no consistent evidence that it reduces criminal outcomes (see Miers et al., 2001; Sherman and Strang, 2007) and (until now) no evidence that it improves educational outcomes. Nevertheless, the costs of restorative justice programs are often assumed to be lower than the costs of the traditional justice system (Sherman et al., 2010; Murphy, 2008). The program costs of Halt are €485 per person (KPMG, 2011) and this allows us to translate the estimated effect sizes into the costs per early school leaver less.

This study proceeds as follows. Section 2 describes the restorative justice program Halt. Section 3 discusses the experimental design of the study. Section 4 describes the data and descriptive statistics. Section 5 shows and discusses the estimation strategy and empirical findings. Finally, Section 6 concludes.

2 Restorative Justice Program Halt

The restorative justice program Halt¹ was initiated in 1981 and is aimed to combat and prevent vandalism among juveniles. In 1995 the program was integrated in the Dutch juvenile justice system. It is targeted towards adolescent first-offenders who have been apprehended by the police, among others, for vandalism, theft or firework nuisance. Appendix A presents the full list of offenses for which juveniles are sent to the Halt program. An important argument for integrating Halt in the Dutch juveniles justice system was that criminal offenses often result from behavioral problems or result from problems at home or at school. According to the Dutch Ministry of Security and Justice it is important to address these problems at an early stage to prevent that juveniles will continue to commit more, and

¹Halt is the Dutch acronym for Het Alternatief, which means The Alternative, and it refers to the fact that the Halt arrangement is an alternative to traditional juvenile justice.

more serious, offenses. As a consequence, Halt aims to change the behavior and attitudes of juvenile first-offenders by addressing their behavioral and development problems. The program explicitly emphasizes that it cooperates with schools to create a more safe living- and learning environment in which students can perform better (see also the Halt website at [http://www.halt.nl/index.cfm/site/Halt English](http://www.halt.nl/index.cfm/site/Halt%20English)).

Adolescent first-time offenders who are apprehended by the police for a Halt-worthy offense are directly referred to the Halt bureau where they are ‘screened’ by professionals. First juveniles are confronted with the reasons and consequences of their criminal behavior. They are then presented with a choice between participating in the Halt program or being sent to the Public Prosecutor. Clearly juveniles have an incentive to participate in and complete the Halt program because it means that criminal charges and a criminal record is avoided.

Halt professionals develop individual programs that include sessions with juveniles and their parents, taking the committed offense into account. This results in a tailor-made punishment program that consists of the following components: more sessions with the Halt professional, community work, learning assignments and compensating (financially) the damage that was done. A crucial component of the program is that juveniles *must* apologize to their victims, if possible. Victims then explain how they were affected by the criminal offense, such that juveniles are directly confronted with how their behavior has affected others. The program duration is on average one year and the time spent on community work and learning assignments varies between 2 and 20 hours per week after-school time. The variation in time-intensity depends on the committed criminal offense and on the diagnosed emotional or behavioral disorder of the offender by the Halt professional.

The program is built on several sociological theories of criminal behavior (see Ferwerda et al., 2006). We shortly elaborate on the three main sociological theories that underlie the Halt program: (I) social learning theory (Bandura, 1969), (II) reintegrative shaming theory (Braithwaite, 1989), and (III) strain theory (Merton, 1957; Cloward and Ohlin, 1960). According to social learning theory, criminal behavior is acquired through observational learning and therefore it depends largely on social and environmental factors. Halt therefore confronts juveniles with their antisocial and criminal behavior through learning assignments. These learning assignments teach first offenders how to reflect on their own behavior and show them behavioral ‘role models’ to improve their antisocial behavior. Juvenile first offenders moreover receive training, if necessary, such that they are better able to handle their behavioral disturbances. Juveniles who, for example, tend to behave aggressively follow an

Aggression Replacement Training (better known as ART), which is an evidence based cognitive behavioral intervention program that aims to improve social skill competences and moral reasoning, improve anger management, and reduce aggressive behavior (see, among others, Goldstein and Glick, 1994, 1999).

Reintegrative Shaming Theory (RST) emphasizes the important role of guilt and shame feelings for conscience formation and for the observed behavior of first offenders. The neuropsychological literature shows that the part of the brain which controls reasoning and impulses (Prefrontal Cortex) does not fully mature until the age of 25, and consequently adolescents are not able to oversee what the consequences are of their actions for others and for themselves (see, for example, Bogin, 1999; Paus, 2005). This explains for example why adolescents have a strong preference for high excitement and low effort activities (Steinberg, 2005). Because feelings of guilt and shame during adolescence play an important role in conscience formation it is believed that a program that focuses on these feelings instead of confronting adolescents only with the (long-term) consequences of their actions is more effective to improve behavior. This is the reason why juvenile first offenders must apologize to (and sometimes work for) their victims, if possible, so that they feel how victims were affected (in terms of shame and guilt). This in turn should positively impact on their behavior.

According to the strain theory individuals can be driven into criminal activities because of social pressure or because it is not possible to achieve the desired status and goals in a legal way. It is well known that social deprivation highly correlates with juvenile delinquency (see, among others, Utting et al., 1993) and in socially deprived neighborhoods the investments of parents in their children's education are relatively low (Moon, 2010). This is one of the reasons why many early childhood and adolescent interventions aim at improving simultaneously crime prevention and educational attainment (e.g. Perry Preschool program, the Chicago Child-Parent Center, Head Start, the Seattle Social Development Project). Halt therefore cooperates intensively with school and involves parents in the program in order to release the social pressure and to improve the educational perspectives.

3 Experimental Design

The Halt experiment was an initiative of the Dutch Ministry of Security and Justice and Beke Consultancy, a specialized research bureau in crime-related research, which conducted

the experiment. 1064 adolescent first-time offenders who were caught by the police and committed a Halt-worthy offense (see Appendix A) were invited in 2003 to participate in the Halt experiment. At the police station they were informed about the nature of the Halt program and were told that participation in the experiment implies that they would not get a criminal record and would not be prosecuted by the public prosecutor. Participation in the Halt experiment was possible if first-time offenders were willing to participate in the program and if their parents gave their consent. This resulted in 944 participants in the Halt experiment and 120 adolescents who participated in the Halt program but not in the Halt experiment.

The 944 participating juveniles were randomly assigned to a treatment group (465) and a control group (479) at the 12 participating Halt bureaus. There are 62 Halt bureaus in the Netherlands and the 12 participating Halt bureaus were selectively chosen to ensure that the experimental sample contained bureaus located in the largest Dutch cities and bureaus located outside the high urbanized Randstad area. The number of juveniles assigned per Halt bureau is limited and varies between 20 and 166 juveniles (see Appendix B for the exact numbers). Schulz and Grimes (2002) and *Use of randomisation in clinical trials: a survey of UK practice* (2012) showed that unconditional randomization can lead to imbalances on baseline characteristics between the treatment and control group and therefore stratified block randomization was used to assign juveniles to the Halt treatment conditionally on age, gender, ethnicity and type of offense.

The randomization was performed in five subsequent steps. First, representative Halt staff members were appointed by the 12 Halt bureaus and these representatives and the first-time offenders had to fill in a first-round questionnaire. In the second step, Halt representatives provided the researchers of Beke Consultancy with the background characteristics needed for the randomization (i.e. age, gender, ethnicity, whether a group offense was committed and type of offense). In the third step, stratified block randomization was applied to assign juvenile first-offenders to the Halt program and to a control group. To avoid contamination, group offenders were assigned to the treatment or the control group as one group. In the empirical analysis it is therefore crucial to cluster the standard errors at the level of the group in which the offense was committed. In step four, researchers informed Halt representatives on who was assigned to the Halt program and who was assigned to the control group. In the final step, juveniles received information on whether they had to participate in the Halt program.

All 944 juveniles and their parents had to return to the Halt bureau six months after the

first Halt-meeting to complete a second-round questionnaire. To encourage participation, they received 15 euros during this visit. Still, the main incentive of juveniles to participate in the experiment and to complete the experiment successfully was that it released them from any juridical charges. For juveniles in the treatment group, completion also meant completing the entire Halt program. Juveniles from the Halt group who did not appear at the second-round questionnaire meeting, even after sending reminders, were considered as program dropouts. Even though there were strong incentives to complete the program, 91 juveniles dropped out of the Halt program, and as a consequence were prosecuted by the public prosecutor and obtained a criminal record.

4 Data and Descriptive Statistics

This study links the Halt experimental data to registration data of Statistics Netherlands that tracks the educational careers of all Dutch adolescents in secondary and vocational education between 2004 and 2010. The Halt experimental data contain a wide range of background and assignment characteristics for the 944 juveniles who participated in the Halt experiment and their parents, and contain background characteristics for the 120 juveniles who refused to participate in the Halt experiment.

The educational tracking system for students in secondary and vocational education was initiated by the Dutch Ministry of Education in 2003 to determine the number of early school leavers. All Dutch students received a personal identification number and their enrollment status in secondary and vocational education was registered. The registration data furthermore contain information on ethnicity, family structure, secondary education type, grade and living area. The educational tracking data allow us to follow juveniles in their educational careers six years after enrolment in the Halt program.

The number of years of education attained can be directly derived from the educational tracking data and based on the information on education type and grade for each student each year. Information on early school leaving is derived from the educational tracking data using the definition of the Dutch Ministry of Education (2012). This definition states that students are not considered as early school leavers if they are (1) registered in secondary or vocational education, or (2) finished senior general secondary, pre-university, or a level 2 post-secondary vocational education with a diploma.

Table 1 presents the characteristics of the participants in the experimental group and compares the assignment characteristics of juveniles assigned to Halt and the control group.

The table shows that 479 juveniles were assigned to the control group and that 465 juveniles were assigned to the Halt program. This difference in group size is because group offenders were assigned to the treatment as one group to avoid contamination. This also explains why there is a marginally significant difference in the proportion of group offenders between the Halt group and the control group. The table further shows that the differences in the means of the assignment characteristics of juveniles in the Halt and the control group are generally not statistically significant. This is, however, not the case for shoplifting: juveniles in the control group committed shoplifting significantly more frequently than those assigned to the Halt group.

TABLE I
Assignment Characteristics for Juveniles in the Halt and the Control Group

	Control (N=479)		Halt (N=465)		p-value
	Mean	Std. dev.	Mean	Std. dev.	
Age	14.477	1.530	14.578	1.469	0.298
Female	0.307	0.462	0.265	0.442	0.150
Parents born in the Netherlands	0.653	0.477	0.708	0.453	0.071
Group offense	0.702	0.454	0.780	0.413	0.007
Offense Type:					
Demolition	0.166	0.375	0.195	0.399	0.245
Graffiti	0.025	0.156	0.032	0.177	0.497
Shoplifting	0.380	0.486	0.313	0.464	0.030
Property crime	0.141	0.352	0.156	0.362	0.535
Handling stolen goods	0.021	0.143	0.024	0.152	0.767
Reckless behavior	0.141	0.347	0.127	0.333	0.543
Arson	0.026	0.163	0.034	0.182	0.464
Light abuse	0.008	0.091	0.011	0.103	0.702

Table I shows that participants of the Halt experiment were on average 14.5 years old. In Appendix C we show the exact age distribution of the participants in the Halt experiment to illustrate that the early school leaver status of all participants can be determined (i.e. if

they finished a senior general secondary, pre-university, or a level 2 post-secondary vocational education with a diploma). Around 30 percent of the offenders were women and 70 percent had parents who were born in the Netherlands. Approximately 70 percent of the offenses were group offenses and the most frequently committed offenses were demolition, shoplifting, reckless behavior and property crime.

Juveniles who committed a group offense were assigned to the treatment together with their fellow-offenders. The unit of assignment is therefore the group in which the offense is committed and because this influences the precision of our estimates in the empirical analysis we show the group size distribution in Table II. The first column indicates the group size and the second column indicates how many offenders were in this group size category. The last column is the most important column of Table II, as it indicates how many groups were assigned to the treatment (i.e. $\frac{\text{Freq.}}{\text{Group size}}$) which determines the power and precision in the empirical analysis in Section 5. The table shows that 78.3 percent of the offenses are committed alone or with one fellow offender. In total 648 groups were assigned to the treatment category which is more than sufficient to obtain an internally valid and robust estimate if standard errors are clustered on a variable that identifies the offense group.

TABLE II
Frequency Table of Group Size

Group size	Freq.	Percent	Cum.	$\frac{\text{Freq.}}{\text{Group size}}$
1	441	46.72	46.72	441
2	298	31.57	78.28	149
3	117	12.39	90.68	39
4	36	3.81	94.49	9
5	40	4.24	98.73	8
6	12	1.27	100.00	2
Total	944	100.00		648

Table III compares family and education type characteristics of juveniles assigned to Halt and the control group. These characteristics were measured in the first-round questionnaire (see Section 3) and the table shows that none of the differences in the means of these characteristics of juveniles in the Halt and the control group are statistically significant.

Before the descriptive statistics are described we briefly elaborate on the Dutch education system. At the age of twelve, after finishing primary school, children are tracked into different

TABLE III

Family and Education Type Characteristics for Juveniles in the Halt and the Control Group.

Education Type	Control (N=479)		Halt (N=465)		p-value
	Mean	Std. dev.	Mean	Std. dev.	
Primary education	0.054	0.227	0.034	0.182	0.143
Secondary special needs education	0.092	0.289	0.081	0.274	0.530
Pre-vocational education: theoretical path	0.300	0.459	0.300	0.458	0.987
Pre-vocational education: mixed path	0.307	0.462	0.323	0.468	0.592
Senior general secondary education	0.127	0.334	0.148	0.356	0.340
Pre-university secondary education	0.098	0.298	0.078	0.268	0.268
Vocational education	0.049	0.214	0.053	0.226	0.750
Family Characteristics:					
Single parent household	0.392	0.489	0.380	0.486	0.704
Household size	3.656	1.662	3.496	1.581	0.129

secondary education levels. Pre-vocational education (4 years) prepares children for vocational education (4 years). Within pre-vocational education there are two paths, of which the theoretical path is more difficult than the mixed path. Senior general secondary education (5 years) prepares children for higher professional education (4 years) and pre-university education (6 years) prepares children for an academic study (4 or 5 years). Secondary special needs education is secondary education for children with learning problems.

Approximately 75 percent of the juvenile first-time offenders are enrolled in pre-vocational education or a lower education type. Based on a report published by the Ministry of Education and Science (2010) we conclude that the proportion of juveniles enrolled in pre-vocational education is relatively large, which is consistent with the extensive literature that finds a negative correlation between education levels and criminal involvement (see Ellis et al., 2009, and references therein).

Even there are strong incentives for juveniles to complete the Halt program, 91 of the 465 first offenders did not do so. Table IV characterizes the selective nature of this dropout. We only show the mean differences that are significantly different when the characteristics in Tables I and III are considered. Table IV shows that selective program dropout is mainly characterized by differences in family background characteristics rather than by differences in the type of committed offense or differences in education levels. Juveniles who complete Halt are somewhat more often enrolled in pre-vocational education, but this difference is small compared to the other observed significant differences. The juveniles who drop out of the

Halt program are, on average, older, are less likely to have parents born in the Netherlands and live more frequently in single-parent families. The latter two characteristics are often associated with lower educational outcomes. To control for the bias that is imposed by the observed (and unobserved) selective dropout we apply an IV-approach in Section 4.

TABLE IV
Characteristics of Juveniles who Completed Halt and Halt Dropouts

	Completed (N=374)		Dropouts (N=91)		p-value
	Mean	Std. dev.	Mean	Std. dev.	
Age	14.495	1.493	14.912	1.322	0.015
Parents born in the Netherlands	0.730	0.444	0.618	0.489	0.036
Single parent household	0.353	0.479	0.495	0.503	0.013
Pre-vocational education: theoretical path	0.318	0.468	0.286	0.401	0.019

The Halt experimental data are linked to the educational tracking data based on the student’s family name, address, living place, date of birth and gender. Because it is not allowed to disclose personal information, such as name and address, the Ministry of Security and Justice delivered the experimental data to Statistics Netherlands where the experimental data were linked to the educational tracking data. Unfortunately there were 118 cases in which the experimental data could not be linked to the educational tracking data due to non-uniqueness of the observed personal identification numbers. This implies that we had to exclude these 118 juveniles from the empirical analysis.

Juveniles were randomly assigned to the treatment and therefore the juveniles who could not be linked to the educational tracking data must also be randomly assigned to the treatment. Table 5 reports characteristics for those juveniles who could not be linked to the educational tracking data separately for the Halt and the control group. The table shows that none of the reported means of the control and the Halt group are significantly different. It follows that the problem of non-linkability does not impose a systematic bias on our Halt estimate.

TABLE V
 Characteristics for the Non-linkable First Offenders by Assignment Groups

	Control (N=65)		Halt (N=53)		p-value
	Mean	Std. dev.	Mean	Std. dev.	
Age	14.631	1.409	14.774	1.354	0.578
Female	0.215	0.414	0.340	0.478	0.133
Ethnicity: Dutch	0.677	0.471	0.726	0.445	0.562
Single parent household	0.492	0.504	0.528	0.504	0.700
Household size	3.492	2.151	3.642	2.158	0.709
Primary education	0.046	0.211	0.019	0.137	0.420
Secondary special needs education	0.200	0.403	0.113	0.320	0.205
Pre-vocational education: theoretical path	0.354	0.482	0.358	0.484	0.959
Pre-vocational education: mixed path	0.246	0.434	0.245	0.434	0.991
Senior general secondary education	0.108	0.312	0.151	0.361	0.487
Pre-university secondary education	0.046	0.211	0.094	0.295	0.304
Regional training center education	0.015	0.124	0.057	0.233	0.222
Group offense	0.662	0.477	0.736	0.445	0.388
Demolition	0.154	0.364	0.226	0.423	0.318
Graffiti	0.046	0.211	0.019	0.137	0.420
Shoplifting	0.354	0.482	0.434	0.500	0.379
Property crime	0.154	0.364	0.113	0.320	0.525
Handling stolen goods	0.031	0.174	0.019	0.137	0.686
Reckless behavior	0.185	0.391	0.113	0.320	0.287
Arson	0.031	0.174	0.038	0.192	0.837

5 Estimation Strategy and Empirical Findings

5.1 Estimation Strategy

An instrumental variable (IV) approach is adopted to control for selective dropout from the Halt treatment and to obtain unbiased Halt estimates. The local average treatment effect on the treated is estimated using a two-stage least squares model (Angrist and Pischke, 2009). In the first stage we estimate the probability of participating in the Halt program

by regressing the participation status, H_i on a set of covariates, X_i and on a variable that indicates if a juvenile was assigned to the Halt treatment ($Z_i=1$) or to the control group ($Z_i=0$):²

$$H_i = \alpha_0 + \alpha_1 Z_i + X_i' \alpha_2 + \varepsilon_i. \quad (1)$$

Subscript i is a student indicator and we assume that the error term, ε_i , is normally distributed with mean zero and variance σ_ε^2 . In the second stage we regress the two educational outcome variables considered in this study (Y_{ij}) on the predicted probability of participating in Halt (i.e. \hat{H}_i) and on the set of covariates (X_i) included the first stage regression:

$$Y_{ij} = \beta_{0j} + \beta_{1j} \hat{H}_i + X_i' \beta_{2j} + \eta_{ij}. \quad (2)$$

Subscript j refers to the fact we consider the educational outcomes early school leaving (*ESL*) and After-Program Education Years (*APEY*) and therefore estimate two second stage models. The error term η_{ij} is assumed to be normally distributed with mean zero and variance $\sigma_{\eta_j}^2$ and the correlation between η_{ij} and ε_i are assumed to be nonzero. The estimated local average treatment effect is unbiased because instrument Z_i is by construction uncorrelated with the error terms η_{ij} and ε_i due to the randomization and can only influence the considered educational outcomes through H_i . We note that the first and second stage equations are estimated simultaneously such that the standard errors are correctly estimated (Wooldridge, 2009).

5.2 Empirical Findings

Table VI shows the descriptive statistics of the educational outcome variables, early school leaving and after-program years of education, and shows whether there are significant differences between the Halt group and the control group. The Table shows differences in educational outcomes that are favorable for juveniles in the Halt group, but these differences are not statistically significant. Selective dropout is however not taken into account in Table VI, and observed differences in the educational outcome variables hence represent differences between juveniles conform their assignment status, not conform their treatment status. The presented mean differences can therefore be viewed as intend-to-treat estimates,

²The covariates included in the regression analysis are age, gender, ethnicity, living in a single-parent household, working status of both parents, household size, group offense indicator, offense type, educational level at the start of the program, if juveniles finished school before the program started, Halt bureau dummies.

TABLE VI
Comparing Educational Outcomes between Treatment Groups

	Halt group (N=414)		Control group (N=412)		Mean Difference	
	Coeff.	St.Dev.	Coeff.	St.Dev.	Coeff.	St.Dev.
Early School Leaving	0.150	(0.358)	0.181	(0.386)	-0.031	(0.026)
After-Program Education Years (in years)	3.146	(2.192)	3.019	(2.198)	0.126	(0.153)

Note: * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

without controlling for any covariates. Given the selective nature of the program dropout these estimates underestimate the true program effects.

Table VII presents the estimation results of the instrumental variable analysis. Column 2 presents the first-stage estimation results for early school leaving and after-program education years. The high R^2 of .913, the Kleibergen-Paap F-statistic (2006), which provides an under-identification test, and the coefficient of the Halt assignment variable clearly show that the Halt assignment variable is a strong predictor for Halt participation. The second stage estimation results for early school leaving (ESL) indicate that participation in the Halt program reduces early school leaving by 5.9 percentage points. This estimate is relative to the early school leaving mean of the control group. This mean is presented in Table VI and is .181 and hence the interpretation of the estimate is that participation in Halt reduced early school leaving from .181 to .122, which is a substantial effect. The second stage regression for after-program years of education (APYE) shows a positive and significant effect of Halt. The estimated effect of .287 is relative to the control group mean of 3.56 in Table VI. Participation in the Halt program thus increases the number of after-program years of education from 3.56 to 3.85 years, which again is a substantial program effect.

Heterogeneous Treatment Effects

The direction and magnitude of the treatment effect may vary for different subgroups. Taking into account subgroup effects alters the empirical strategy, because the number of equations that have to be estimated in the first stage is equal to the number of subgroups considered in the regression model (see Angrist and Pischke, 2009). If we take into account, for example, that Halt may affect boys differently than girls we can rewrite equation 3 and estimate the following system of first stage equations:

$$H_i = \alpha_{01} + \alpha_{11}Z_i + \alpha_{21}Z_i\text{Girl}_i + X_i'\alpha_{31} + \theta_{i1}. \quad (3)$$

TABLE VII
The Halt Effects on Early School Leaving (ESL) and After-Program Education Years (APEY)

		ESL	APEY
	1 st Stage	2 nd Stage	2 nd Stage
Halt assignment	0.897*** (0.014)		
Instrumented Halt participation		-0.059** (0.026)	0.287** (0.143)
Age	-0.006* (0.004)	-0.006 (0.009)	-0.724*** (0.052)
Girl	0.002 (0.015)	-0.056* (0.032)	-0.09 (0.166)
Parents born in the Netherlands	0.016 (0.014)	0.057* (0.031)	-0.384** (0.158)
Household size	-0.010*** (0.004)	0.007 (0.009)	0.031 (0.044)
Other controls	Yes	Yes	Yes
Constant	0.152** (0.068)	0.430** (0.18)	12.436*** (0.937)
R^2	0.9126	0.1318	0.3567
Kleibergen-Paap F-statistic	3876.56		
Observations		826	

Note: Other controls are dummies for single-parent household, school graduation before Halt, offense type, group offender, working status of parents. SEs are clustered at the group offense level and printed in parenthesis. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

$$H_i \text{Girl}_i = \alpha_{02} + \alpha_{12} Z_i + \alpha_{22} Z_i \text{Girl}_i + X_i' \alpha_{32} + \theta_{i2}. \quad (4)$$

In the second stage, the educational outcome variables (Y_{ij}) are regressed on the set of covariates (X_i) and on the predicted participation probabilities resulting from the first stage regressions. The second stage regression when we consider the subgroup effects with respect to gender is then:

$$Y_{ij} = \beta_{0j} + \beta_{1j} \hat{H}_i + \beta_{2j} H_i \hat{\text{Girl}}_i + X_i' \beta_{3j} + \eta_{ij}. \quad (5)$$

The error terms θ_{i1} and θ_{i2} are assumed to be normally distributed and to be positively correlated with η_{ij} . Intuitively, β_{1j} measures the Halt effect for girls on educational outcome j and β_{2j} measures if the Halt effect for boys on educational outcome j differs from the estimated effect for girls (i.e. differs from β_{1j}).

Table VIII shows whether the estimated Halt effects on early school leaving and after-program education years differ by gender, ethnicity, group offense and single-parent family. We only show the second-stage estimation results, because the system of first stage regressions differs for each subgroup. Based on the estimated coefficients the estimation results suggest that smaller program effects are observed for boys, Dutch adolescents and juveniles in single parents household. The interaction effects are, however, generally statistically non-significant such that we cannot reject that there are constant program effects for the subgroups considered. Table VIII however shows that the standard errors are larger than those reported in Table VII, especially for years of education. The standard errors are larger because two first-stage equations are estimated such that more noise is included in the second stage regression, and this makes the estimated coefficients less precise (see also Angrist and Pischke, 2009). The interaction effect for group offense is statistically significant for after-program years of education, which suggests that the program is less effective for group offenders. This result may possibly be explained by a negative peer-effect, in the sense that the program was less effective for those adolescents who were surrounded by peer-offenders.

3

³We also tested whether the program effect depends on the type of offense. For this purpose we estimated 5 and 9 first-stage equations (depending on the definition of offense categories) and this resulted in second stage estimates that are not so precise. The results are available on request.

TABLE VIII

The Halt Effects on Early School Leaving (ESL) and After-Program Education Years (APEY)

		ESL	APEY
Girl	Halt	-0.063** (0.031)	0.246 (0.172)
	Halt*Girl	0.014 (0.064)	0.157 (0.356)
Parents born in the Netherlands (PBN)	Halt	-0.115** (0.049)	0.651** (0.300)
	Halt*PBN	0.080 (0.062)	-0.517 (0.367)
Single Parent	Halt	-0.076** (0.031)	0.385** (0.174)
	Halt*Single	0.051 (0.066)	-0.277 (0.333)
Group offense (G)	Halt	-0.215*** (0.064)	0.956*** (0.314)
	Halt*G	0.197 (0.073)	-0.876** (0.369)
Observations			826

Note: Control variables are similar to those in Table VII. SEs are clustered at the group offense level and printed in parenthesis. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

6 Discussion

This is the first article that documents that there are statistically significant positive effects of a restorative justice program on the educational outcomes early school leaving and years of education attained. Previous studies have estimated the causal influence of educational effects on criminal outcomes (see, among others, Lochner and Moretti, 2004; Cullen et al., 2006; Machin et al., 2011*b*), but the possibility that criminal intervention programs can positively affect the educational outcomes of juveniles tends to be ignored. In many countries there is, however, a consensus that criminal behavior is determined by (behavioral) problems at home and school and that criminal behavior of adolescents may lead to more criminal

behavior and lower educational outcomes in the future. It is therefore not surprising that many countries, such as the US, Canada, Australia, the UK and the Netherlands, currently have very similar restorative punishment programs that are part of the juvenile justice system. Because of the similarity in restorative punishment programs between countries, the results of this study are informative for the potential effects that restorative justice can have on educational outcomes in other countries.

Our estimates indicate that the Dutch restorative justice program, Halt, reduces early school leaving by 6 percentage points and increases years of education attained by .29 years. The direction and magnitude of the program effect appear to be remarkably similar for several subgroups considered. Tests for heterogeneous treatment effects indicate that the program effects are smaller for boys, adolescents whose parents are born in the Netherlands and juveniles in single parents household. The interaction effects were, however, never statistically significant. An interesting result for after-program years of education is that the program tends to be significantly less effective for group offenders. This result possibly points at a negative peer-effect, in the sense that the program was less effective for those adolescents who were surrounded with peer-offenders.

The IV estimates that early school leaving is reduced by 28 juveniles if 465 juveniles are treated. The treatment costs for each juvenile are €450. The total costs of the HALT are therefore €209,250 (i.e. $465 \cdot 450$) and the costs per early school leaver less are therefore about €7,472. This seems a favorable cost-effectiveness and worth the investment, given that these juveniles leave school with at least a level 2 post-secondary vocational education and considering that the costs of one extra year of education are roughly similar to the program costs of Halt for one early school leaver less.

A drawback of the Halt experiment is that juveniles in the control group did not receive the Halt treatment, but were also not sent to the public prosecutor or obtained a criminal record. Normally, juvenile first-offenders can choose between Halt participation or being sent to the public prosecutor and getting a criminal record. The control group thus does not adequately represent the counterfactual outcome and we, as a consequence, estimate the Halt effect relative to receiving no treatment instead of relative to receiving the counterfactual treatment (i.e. being sent to the public prosecutor and obtaining a criminal record). This does not affect the internal validity of our estimation results, but if the educational outcomes of first-offenders in the control group differ from the educational outcomes of juvenile offenders who received the counterfactual treatment then we may not estimate the effect we are interested in.

Two explanations can be given why educational outcomes of first-offenders in the control group are a good proxy for the counterfactual educational outcomes. First of all, the risk of being caught by the police and receiving a criminal record apparently did not prevent these juveniles to commit a minor offense. Juveniles who were asked to participate in the Halt experiment were moreover not aware that participation could imply receiving no treatment. Secondly, it is unlikely that a criminal record significantly impacts on educational outcomes in the long run. Juveniles decided to risk receiving a criminal record, and this risk attitude is not influenced (in the long run) by the fact that they received no treatment and no criminal record.

In this study we show that restorative justice programs have strong positive effects on educational outcomes. Hence there are positive effects from investments in crime prevention programs on educational outcomes. We therefore conclude that investments in criminal intervention programs should be considered as a policy tool to reduce early school leaving and increase the number of years of education. More generally, governments should not only consider the positive spill-over effects of educational investments on crime, but should also consider that there are positive spill-over effects of investments in crime prevention on educational outcomes for adolescents .

Appendix

Appendix A

Table A shows the offenses for which juveniles are referred to the Halt program. The first column refers to the section of the book of law, the second column describes the offense and the third column categorizes the offenses.

TABLE A
Halt-Worthy Offenses Related to Law Sections

Section of the Law	Offense	Category
141(1) Criminal Law (CL)	Public violence possessions	Demolition
157 CL	Incendiaries with danger or goods (not persons)	Public safety
310 CL	(Shop)Theft + attempt to	Offense against property
311(1) (under 4th) CL	(Shop)Theft in association with one or more persons + attempt to	Offense against property
321 CL	Fraud + attempt to	Offense against property
350 CL	Demolition	Demolition
	Graffiti	Demolition
416 CL	Deliberatly handling stolen goods	Offense against property
417 CL	Debt handling	Offense against property
326 CL	Change of price tags (fraud)	Offense against property
424 CL	Reckless behavior with danger/disadvantage goods	Reckless behavior
461 CL	Trespassing	Other
1.2.2 Fireworks Decree	Illigal/defective firework	Firework offense
1.2.4 Fireworks Decree	Possession of more than 10 kg of firework in stock	Firework offense
2.3.6 Fireworks Decree	Ignite fireworks outside permitted period	Firework offense
General Local Regulation	Firework	Firework offense
	Reckless behavior	Reckless behavior
72 Regulation passenger traffic	Behavior that disturbs (or can disturb) peace, safety and good order	Public safety
73 Regulation passenger traffic	Ignore regulation with respect of peace, safety and good order	Public safety

Appendix B

TABLE B.1
Assignment Characteristics of Juveniles in Each Halt Bureau

	Amsterdam		Breda		Den Bosch		The Hague		Enschede		Friesland	
	N=166		N=20		N=91		N=150		N=34		N=86	
	T=82	C=84	T=10	C=10	T=45	C=46	T=70	C=80	T=17	C=17	T=43	C=43
Age	14.72	14.02***	14.20	15.30*	15.09	15.20	14.59	14.23*	14.47	14.12	14.30	14.65
	(1.21)	(1.54)	(1.62)	(1.76)	(1.55)	(1.38)	(1.44)	(1.55)	(1.12)	(1.27)	(1.48)	(1.52)
Female	0.35	0.35	0.50	0.40	0.27	0.33	0.27	0.26	0.12	0.24	0.26	0.42*
	(0.48)	(0.48)	(0.53)	(0.52)	(0.45)	(0.47)	(0.45)	(0.44)	(0.33)	(0.44)	(0.44)	(0.50)
Dutch Parents	0.39	0.32	0.60	0.67	0.86	0.80	0.57	0.49	0.71	0.71	0.86	0.81
	(0.48)	(0.47)	(0.52)	(0.50)	(0.35)	(0.40)	(0.50)	(0.50)	(0.47)	(0.47)	(0.35)	(0.39)
Group offense	0.73	0.67	0.60	0.50	0.80	0.60**	0.83	0.75	0.65	0.47	0.79	0.74
	(0.45)	(0.47)	(0.52)	(0.53)	(0.41)	(0.50)	(0.38)	(0.43)	(0.49)	(0.51)	(0.41)	(0.45)
Demolition	0.18	0.18	0	0.20*	0.18	0.07*	0.09	0.09	0.18	0.24	0.28	0.21
	(0.39)	(0.39)	0	(0.42)	(0.39)	(0.25)	(0.28)	(0.28)	(0.39)	(0.44)	(0.45)	(0.41)
Graffiti	0.06	0.04	0	0	0	0.04*	0.04	0.05	0	0	0.07	0
	(0.24)	(0.19)	0	0	0	(0.21)	(0.20)	(0.22)	0	0	(0.26)	0
Shoplifting	0.37	0.46	0.60	0.60	0.18	0.26	0.37	0.33	0.12	0.41**	0.30	0.42
	(0.48)	(0.50)	(0.52)	(0.52)	(0.39)	(0.44)	(0.49)	(0.47)	(0.33)	(0.51)	(0.46)	(0.50)
Property crime	0.16	0.17	0.20	0	0.04	0	0.23	0.18	0.29	0.18	0.19	0.14
	(0.37)	(0.37)	(0.42)	0	(0.21)	0	(0.42)	(0.38)	(0.47)	(0.39)	(0.39)	(0.35)
Handling stolen goods	0.04	0.02	0.20	0.20	0	0.07**	0.01	0.03	0	0	0.02	0
	(0.19)	(0.15)	(0.42)	(0.42)	0	(0.25)	(0.12)	(0.16)	0	0	(0.15)	0
Reckless behavior	0.07	0.04	0.20	0.20	0.11	0.24*	0.17	0.15	0.18	0.06	0.07	0.09
	(0.26)	(0.19)	(0.42)	(0.42)	(0.32)	(0.43)	(0.38)	(0.36)	(0.39)	(0.24)	(0.26)	(0.29)

Note: T denotes treatment group and C denotes control group. Standard deviations are printed in parentheses and significant mean differences between T and C at the 10%, 5% and 1% level are indicated with *, ** and ***.

TABLE B.2 (continued)
Assignment Characteristics of Juveniles in Each Halt Bureau

	Gorinchem		Groningen		Leiden		Maastricht		Nijmegen		Zwolle	
	N=30		N=104		N=113		N=26		N=68		N=56	
	T=15	C=15	T=51	C=53	T=58	C=55	T=13	C=13	T=34	C=34	T=27	C=29
Age	13.80	14.47	14.78	14.55	14.48	14.58	14.85	14.92	13.97	14.32	14.78	14.62
	(1.47)	(1.55)	(1.50)	(1.47)	(1.67)	(1.54)	(1.34)	(1.32)	(1.44)	(1.66)	(1.50)	(0.32)
Female	0.20	0.40	0.29	0.32	0.16	0.20	0.46	0.38	0.26	0.38	0.11	0.14
	(0.41)	(0.51)	(0.46)	(0.47)	(0.37)	(0.40)	(0.52)	(0.51)	(0.45)	(0.49)	(0.32)	(0.35)
Dutch Parents	0.67	0.87	0.88	0.83	0.76	0.75	0.83	0.85	0.82	0.65*	0.96	0.83*
	(0.49)	(0.35)	(0.33)	(0.38)	(0.43)	(0.44)	(0.39)	(0.38)	(0.39)	(0.49)	(0.19)	(0.38)
Group offense	0.73	0.79	0.76	0.75	0.84	0.74*	0.75	0.38**	0.85	0.75	0.78	0.90
	(0.46)	(0.43)	(0.43)	(0.43)	(0.37)	(0.44)	(0.45)	(0.51)	(0.36)	(0.44)	(0.42)	(0.31)
Demolition	0.33	0.27	0.22	0.17	0.19	0.20	0.08	0.08	0.35	0.15**	0.26	0.31
	(0.49)	(0.46)	(0.42)	(0.38)	(0.40)	(0.40)	(0.28)	(0.28)	(0.49)	(0.36)	(0.45)	(0.47)
Graffiti	0.07	0	0	0.02	0.05	0.04	0	0	0	0	0	0
	(0.26)	0	0	(0.14)	(0.22)	(0.19)	0	0	0	0	0	0
Shoplifting	0.40	0.27	0.39	0.43	0.17	0.24	0.62	0.77	0.24	0.56***	0.30	0.17
	(0.51)	(0.46)	(0.49)	(0.50)	(0.38)	(0.43)	(0.51)	(0.44)	(0.43)	(0.50)	(0.47)	(0.38)
Property crime	0	0.40***	0.20	0.11	0.12	0.15	0.08	0	0.09	0.03	0.19	0.34*
	0	(0.51)	(0.40)	(0.32)	(0.33)	(0.35)	(0.28)	0	(0.29)	(0.17)	(0.40)	(0.48)
Handling stolen goods	0	0	0.08	0	0	0.02	0.08	0.08	0.03	0.03	0	0
	0	0	(0.27)	0	0	(0.13)	(0.29)	(0.29)	(0.17)	(0.17)	0	0
Reckless behavior	0.13	0	0.06	0.06	0.21	0.31	0.08	0.08	0.12	0.24	0.22	0.17
	(0.35)	0	(0.24)	(0.23)	(0.41)	(0.47)	(0.28)	(0.28)	(0.33)	(0.43)	(0.42)	(0.38)

Note: T denotes treatment group and C denotes control group. Standard deviations are printed in parentheses and significant mean differences between T and C at the 10%, 5% and 1% level are indicated with *, ** and ***.

Appendix C

TABLE C
Age Distribution for Participants in the Halt Experiment

Age	Freq.	Percent	Cum.
11	6	0.64	0.64
12	75	7.97	8.58
13	182	19.28	27.86
14	206	21.82	49.68
15	223	23.62	73.31
16	147	15.57	88.88
17	92	9.75	98.62
18	13	1.38	100.00
Observations	944	100.00	

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