The Effect of Restorative Juvenile Justice on Early School Leaving and Years of Education*

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Abstract

We study the effects of a Dutch restorative juvenile justice program on early school leaving and education years. Causal 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 5.9 percentage points and increases attained education years by 0.29 years. The findings show that restorative juvenile justice programs can have significant educational benefits.

JEL Codes: I2, K4, C93

Keywords: Restorative Justice, Education, Juvenile Crime, Field Experiment

*We thank Bill Evans, Randi Hjalmarsson, Mathew Lindquist, Hessel Oosterbeek, Erik Plug, and Dinand Webbink for valuable comments and discussions. Furthermore, we thank participants of the SOLE, EEA-ESAM and ESPE conference for providing valuable comments and the Dutch Ministry of Security and Justice for providing us the experimental data.

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1 Introduction

There is growing evidence on the relationship between education and criminal behavior through the life-cycle. The negative effect of educational attainment on criminal involvement has been well documented for adults (see Lochner, 2010; Machin et al., 2011) and quasi-experimental evidence suggests that early criminal involvement negatively influences educational outcomes of young people (Hjalmarsson, 2008; Webbink et al., 2013; Aizer and Doyle, 2013). Several evaluation studies moreover show that interventions targeted at adolescents with low socio-economic status can effectively improve their school performance and reduce delinquent behavior (e.g. Grossman and Tierney (1998); Heckman et al. (2010); Rodriguez-Planas (2012); Heller et al. (2013)). Even though the mechanisms through which these interventions work are not always clear, there is evidence suggesting that particular non-cognitive skills (e.g. conscientiousness) are strongly related to criminal and educational outcomes (John et al., 1994; Almlund et al., 2011). This study examines the effects of a Dutch restorative juvenile justice program targeted at juvenile offenders on early school leaving and years of education.

Restorative justice is frequently referred to as intermediate punishment (Morris and Tonry, 1990), community justice (Bazemore and Schiff, 1996) and alternative sanctions (Kahan, 1996), and it is becoming more prevalent in many developed countries (e.g. the US, Canada, Australia, the UK and the Netherlands). It refers to a process of resolving crime by focusing on redressing the harm done to the victims or to the community, holding offenders responsible for their actions and engaging the community in the conflict resolution (Dandurand and Griffiths, 2006). Restorative juvenile justice programs rely on sociological theories of criminal behavior and aim to reduce re-offending behavior among delinquent youth. Despite the growing popularity of restorative juvenile justice programs, the effect on re-offending is measured mainly for short-term outcomes and the evidence is ambiguous (see, for example, Bradshaw and Roseborough, 2005; Hayes, 2005; Sherman and Strang, 2007). To our knowledge, there are no (quasi)-experimental studies that examine the effects of restorative justice programs on educational outcomes, even though the literature indicates that behavioral interventions may positively affect educational outcomes.

This study examines the effects of the Dutch restorative justice program *Halt* on early school leaving and education years. This program targets juvenile first-time offenders aged between 12 and 18 who committed a non-violent crime and is incorporated in the Dutch juvenile justice system. Similar to most restorative justice programs it relies on sociological theories of criminal behavior and aims to change the behavior and attitudes of juvenile first-offenders by addressing their 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.

Data is used from a unique field experiment that was conducted in 2003. 1,064 juvenile first-offenders, 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 who were assigned to the control group did not receive any treatment. This includes not being sent to the public prosecutor and not receiving a criminal record.

The program effects on early school leaving and educational attainment can be determined by linking the experimental data to education data of Statistics Netherlands, such that the educational careers of juvenile first-offenders who were invited to participate in the experiment is observed. 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 and instrument actual program participation by how juveniles were randomly assigned to the treatment.

The empirical results show that the restorative justice program Halt reduces early school leaving by 5.9 percentage points and increases years of education attained by 0.29 years. 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. These interaction effects were, however, never statistically significant. The empirical findings show that restorative juvenile justice programs can significantly improve educational outcomes and thereby confirms the literature suggesting that behavioral interventions can positively affect educational outcomes.

This study contributes to the literature on crime and education in several ways. First of all, our study is the first that evaluates the causal impact of a restorative juvenile justice program on educational outcomes. The Dutch program appears to be representative for restorative juvenile justice programs in other countries and hence the results of this study are informative for the potential effects that these restorative programs may have in other 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 (see, for example, 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 developed 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; Bradshaw and Roseborough, 2005; 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 prevented early school leaver.

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 Halt[1] 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 juvenile 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 are frequently caused by 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 juveniles from committing more, and 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).

[1]Halt 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 junvile justice.

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. Then, they are 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: sessions with a professional (e.g. a child psychologist), community work (e.g. cleaning, repairing, administrative work), learning assignments (e.g. writing an essay or apology letter, group trainings), compensating (i.e. financially) the damage that was done, and meeting with the victimes. 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 average time spent on community work and learning assignments are respectively 8 and 4 hours per week outside the school hours. Community work assignments vary between 1 and 20 hours and learning assignments vary beween 1 and 8 hours. 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. For example, juvenile offenders who committed property crime, shoplifting, arson and demolition receive on the longest working assignment (e.g. cleaning, working in the shop) that varies between 18 and 20 hours per week. The longest learning assignment of 6-8 hours per week is mainly given for offenses such as demolition, shoplifting, property crime, handling stolen goods and reckless behavior.

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 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 of their actions are 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 several social outcomes of at-risk individuals, among them, criminal behavior 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 in order to evaluate the effect of Halt program on recidivism.[2] 1,064 adolescent first-time offenders who committed a Halt-worthy offense (see Appendix A) and were caught by the police 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 located across the country. Group offenders were randomly assigned to the treatment as one group to avoid contamination, and it follows that standard errors should be clustured at the level of the group in which the offense was committed in the empirical analysis. We note that information on group sizes is given in Table II. In total 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).

The randomization was performed in six subsequent steps. First, representative Halt staff members were appointed by the 12 Halt bureaus. In the second step, Halt presentatives and first-time offenders who agreed to participate in the Halt experiment had to fill in a first-round questionnaire. In the third step, Halt representatives provided researchers of Beke Consultancy with information on the first-offenders. These first-offenders were randomly assigned to the Halt treatment and the unit of randomization was the group in which the criminal offense was committed. In step four, Halt representatives received information about who is and is not allowed to participate in the Halt program. Finally, juveniles

[2]Ferwerda et al. (2006) showed that Halt led to fewer and less serious offenses after one year for juveniles who committed criminal offenses under peer pressure and for juveniles who had to apologize to victims in the Halt program. This study does not control for the selective program dropout that occurs and therefore the estimated Halt effects may be biased. Unfortunately, we could not obtain obtain information on recidivism.

received information about if they had to participate in the Halt program or if they were exempt from participation. The control group therefore did not receive the Halt treatment, were not sent to the public prosecutor and did not receive a criminal record.

All 944 juveniles and their parents were obligated to return to the Halt bureau six months after the first Halt-meeting to complete a second-round questionnaire. To encourage participation juveniles were informed that they would receive 15 euros during this visit. Still, the main incentive for 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 directed to 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 compares background characteristics of juveniles who were 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 within each Halt bureau to avoid contamination. 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. In Appendix B we show the balancing table for each Halt bureau.

TABLE I

Comparing Juveniles in the Halt and the Control Group

	Control (N=479)		Halt (N=465)		
	Mean	Std. dev.	Mean	Std. dev.	p-value
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
Test of joint significance			Prob>F=0.2929		

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

TABLE III

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

	Control (N=479)		Halt (N=465)		
Education Type	Mean	Std. dev.	Mean	Std. dev.	p-value
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
Test of joint significance			Prob>F=0.5356		

(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 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 though 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)		
	Mean	Std. dev.	Mean	Std. dev.	p-value
Age	14.495	1.493	14.912	1.322	0.015
Parents born in the Netherlands	0.730	0.444	0.618	0.489	0.032
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
Test of joint significance			Prob>F=0.0308		

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 and, as a consequence, we had to exclude these 118 juveniles from the empirical analysis. Statistics Netherlands could not provide us with information on why these juveniles could not be linked to the education data, because providing these data would be a violation of the Dutch data protection and privacy laws.

Juveniles were randomly assigned to the treatment and therefore juveniles who could not be linked to the education data are randomly assigned to the treatment. Table V.1 reports characteristics of juveniles who could not be linked to the educational tracking data separately for the Halt and the control group. The table shows that the mean differences are sometimes sizable but never statistically significant. These differences may however be statistically insignificant only because the standard errors are large.

TABLE V.1

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

	Control (N=65)		Halt (N=53)		
	Mean	Std. dev.	Mean	Std. dev.	p-value
Age	14.631	1.409	14.774	1.354	0.578
Female	0.215	0.414	0.340	0.478	0.133
Parents born in the Netherlands	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	0.837837
Test of joint significance			Prob>F=0.9533		

Therefore Table V.2 shows background characteristics of the full sample and of the juvenile sample that could be linked to the education data separately for the control and the Halt group. The table shows that the mean differences are never statistically significant. The observed sizable (but non-significant) mean differences between the linkable control and

Halt group are consequently very comparable to those shown in Table I. To examine if the estimated Halt effect changes when dropout due to non-linkability is taken into account we perform a bounding exercise in Section 5.2. In this bounding exercise non-linkable juveniles in the intervention (control) group receive the average outcome value of the control (intervention) group. By assuming 'unfavorable' outcomes for juveniles who could not be linked to the education data we can include them in the empirical analysis and test if the inclusion of these juveniles alters the estimated Halt effect.

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 treatment effect 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):[3]

$$H_i = \alpha_0 + \alpha_1 Z_i + X_i' \alpha_2 + \varepsilon_i. \tag{1}$$

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:

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

Subscript j refers to the fact we consider the educational outcomes early school leaving and After-Program Education Years 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

[3]The 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 V.2

Comparing Linkable Juveniles to the Full Sample Separately for the Halt and the Control Group

	Control Group					Halt Group				
	Full Sample (N=479)		Linkable Juveniles (414)			Halt total (N=465)		Halt linkable (412)		p-value
	Mean	Std. dev.	Mean	Std. dev.	p-value	Mean	Std. dev.	Mean	Std. dev.	p-value
Age	14.477	1.530	14.449	1.549	0.812	14.578	1.469	14.551	1.483	0.800
Female	0.307	0.462	0.321	0.468	0.645	0.265	0.442	0.255	0.436	0.745
Parents born in the Netherlands	0.653	0.477	0.646	0.478	0.896	0.708	0.453	0.704	0.454	0.933
Single parent household	0.392	0.489	0.377	0.485	0.632	0.380	0.486	0.362	0.481	0.562
Household size	3.656	1.662	3.684	1.573	0.812	3.496	1.581	3.476	2.642	0.856
Primary education	0.054	0.227	0.056	0.229	0.934	0.034	0.182	0.036	0.188	0.873
Secondary special needs education	0.092	0.289	0.075	0.264	0.362	0.081	0.274	0.078	0.268	0.825
Pre-vocational education: theoretical path	0.300	0.459	0.292	0.455	0.786	0.300	0.458	0.291	0.455	0.804
Pre-vocational education: mixed path	0.307	0.462	0.316	0.466	0.759	0.323	0.468	0.333	0.472	0.754
Senior general secondary education	0.127	0.334	0.130	0.337	0.891	0.148	0.356	0.148	0.356	0.989
Pre-university secondary education	0.098	0.298	0.106	0.309	0.688	0.078	0.268	0.075	0.264	0.904
Vocational education	0.049	0.214	0.053	0.225	0.727	0.053	0.226	0.053	0.225	0.981
Group offense	0.702	0.454	0.717	0.451	0.803	0.780	0.413	0.789	0.409	0.828
Demolition	0.166	0.375	0.171	0.377	0.924	0.195	0.399	0.194	0.396	0.891
Graffiti	0.025	0.156	0.022	0.146	0.745	0.032	0.177	0.034	0.181	0.887
Shoplifting	0.380	0.486	0.382	0.486	0.959	0.313	0.464	0.296	0.457	0.614
Property crime	0.141	0.352	0.143	0.350	0.948	0.156	0.362	0.160	0.367	0.828
Handling stolen goods	0.021	0.143	0.019	0.138	0.869	0.024	0.152	0.024	0.154	0.953
Reckless behavior	0.141	0.347	0.133	0.340	0.761	0.127	0.333	0.129	0.335	0.938
Light abuse	0.008	0.091	0.010	0.098	0.836	0.011	0.103	0.012	0.110	0.848
Arson	0.026	0.163	0.027	0.161	0.958	0.034	0.182	0.034	0.181	0.972

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 intention-to-treat (ITT) estimates of Halt on early school leaving (ESL) and after-program years of education ($APEY$). Juveniles first-time offenders cannot be forced to complete the program and the ITT effect is measuring the effect of offering Halt to juvenile first-offenders, building in the fact that many offers will be declined (Angrist and Pischke, 2009). It naturally follows that the estimated ITT effect is smaller than the average treatment effect on the treated. The estimation results in Table VI show receiving a Halt offer reduces early school leaving with 5.2 percentage points and increases the number of after-program years with .258. From a policy perspective these estimates are interesting, because it means that offering Halt vouchers to juveniles first-offenders would effectively improve the educational outcomes considered in this study.

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 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.

TABLE VI

Intention-to-treat Estimates of Halt on Early School Leaving (ESL) and After-Program Education Years (APEY) without controls

	ESL	APEY
Halt Assignment	-0.053**	0.258*
	(0.023)	(0.131)
Age	-0.007	-0.727***
	(0.010)	(0.053)
Girl	-0.056*	-0.09
	(0.033)	(0.171)
Parents born in the Netherlands	0.057*	-0.384**
	(0.032)	(0.162)
Household size	0.007	0.029
	(0.009)	(0.046)
Constant	0.422**	12.480***
	(0.184)	(0.957)
Other controls	Yes	Yes
R^2	0.1317	0.3578
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.

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**	0.287**
		(0.026)	(0.143)
Age	-0.006*	-0.006	-0.724***
	(0.004)	(0.009)	(0.052)
Girl	0.002	-0.056*	-0.09
	(0.015)	(0.032)	(0.166)
Parents born in the Netherlands	0.016	0.057*	-0.384**
	(0.014)	(0.031)	(0.158)
Household size	-0.010***	0.007	0.031
	(0.004)	(0.009)	(0.044)
Constant	0.152**	0.430**	12.436***
	(0.068)	(0.18)	(0.937)
Other controls	Yes	Yes	Yes
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.

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:

$$H_i = \alpha_{01} + \alpha_{11} Z_i + \alpha_{21} Z_i Girl_i + X_i' \alpha_{31} + \theta_{i1}. \tag{3}$$

$$H_i Girl_i = \alpha_{02} + \alpha_{12} Z_i + \alpha_{22} Z_i Girl_i + X_i' \alpha_{32} + \theta_{i2}. \tag{4}$$

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:

$$Y_{ij} = \beta_{0j} + \beta_{1j} \hat{H}_i + \beta_{2j} H_i \hat{G}irl_i + X_i' \beta_{3j} + \eta_{ij}. \tag{5}$$

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.[4]

TABLE VIII

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

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

Bounding Exercise: Accounting for Dropout because of Non-linkability

We mentioned in Section 4 that 118 juveniles first-offenders could not be linked to the educational data and showed that this did not lead to imbalances in background characteristics

[4]We 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.

between the Halt sample and the control sample. Below we perform a bounding exercise and examine if the estimated Halt effect is influenced by the fact that 118 juveniles could not be linked to the education data. First we calculated average educational outcomes separately for juveniles in the Halt and the control group who could be linked to the educational data. Then non-linkable juveniles in the Halt group received the average educational outcome of juveniles in the control group who could be linked to the educational data. This imputed outcome is an unfavorable outcome for the non-linkable juveniles in the Halt group, since estimated Halt effect was positive. Similarly, we assign the average educational outcome of juveniles in the Halt group who could be linked to the educational data to the non-linkable juveniles in the control group. This imputed outcome is a favorable outcome for the non-linkable juveniles in the control group because the average educational outcome partly contains the Halt effect. Because we have assigned unfavorable outcomes to juveniles in the Halt group and favorable outcomes to juveniles in the control group it is possible to consider juveniles who could not be linked to the education data in the empirical analysis and estimate a lower bound estimate of the Halt effect.

The results of this bounding exercise are shown in Table IX. The estimates in Table IX are lower bound estimates, and therefore somewhat lower than the estimates presented in Table VII. The estimation results indicate that participation in the Halt program reduces early school leaving by 5.2 percentage points and increased the after program education years with .26 years relatively to the control group mean. These estimated effects are statistically significant, sizable and moreover remarkably similar to the estimated effects in Table VII.

TABLE IX

Lower Bound Estimates of 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	0.901***		
	(0.014)		
Instrumented Halt participation		-0.052**	0.263**
		(0.024)	(0.129)
Age	-0.007**	-0.010	-0.632***
	(0.004)	(0.009)	(0.046)
Girl	-0.007	-0.043	-0.147
	(0.015)	(0.029)	(0.145)
Parents born in the Netherlands	-0.007	0.049*	-0.352**
	(0.013)	(0.028)	(0.143)
Household size	-0.008***	0.005	0.039
	(0.003)	(0.008)	(0.035)
Constant	0.154**	0.476***	11.039***
	(0.068)	(0.163)	(0.827)
Other controls	Yes	Yes	Yes
R^2	0.9154	0.1091	0.3255
Kleibergen-Paap F-statistic	4420.82		
Observations		944	

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.

6 Discussion

This is the first article that documents that there are statistically significant positive effects of a restorative justice program on 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), 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 5.9 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 suggest that early school leaving is reduced by 27 juveniles if 465 juveniles are treated. The treatment costs for each juvenile are €485, such that the total treatment costs for 465 juveniles are €225,525. The costs per early school leaver less are therefore about €8,352. 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.

An important feature of the Halt experiment was that the control youth were released with no punishment, while in practice non-enrollment in the program results in criminal prosecution and a record. It implies that the estimated Halt effect represents the program impact separate from the lack of prosecution on educational outcomes. We can not exclude the possibility that prosecution harm educational outcomes (via changing labor market prospects) or improve them (by scaring youth into better behavior). Unfortunately, we were not able to test if the control youths' outcomes were different than the outcomes of youth who experienced prosecution as-usual, which makes it difficult to generalize the empirical results of this study to the real-world counterfactual. It is however important to recall that the control youth did experience a police encounter (as the Halt youth did) and because they committed minor offences they never experienced incapacitation during trial/punishment.

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)
Test of Joint Sign.	Prob>F=0.1962		Prob>F=0.0175		Prob>F=0.2010		Prob>F=0.5556		Prob>F=0.6240		Prob>F=0.0392	

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)
Test of Joint Sign.	Prob>F=0.1107		Prob>F=0.7952		Prob>F=0.3783		Prob>F=0.3766		Prob>F=0.2249		Prob>F=0.2637	

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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