



# The effects of a restorative justice programme (Halt) on educational outcomes and recidivism of young people

Melvin Vooren<sup>1,2</sup>  · Iryna Rud<sup>3</sup> · Ilja Cornelisz<sup>1</sup> · Chris Van Klaveren<sup>1</sup> · Wim Groot<sup>3</sup> · Henriëtte Maassen van den Brink<sup>2</sup>

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## Abstract

**Objectives** This study examines the effects of a restorative justice programme in the Netherlands on educational outcomes and repeated delinquent behaviour of young people who have committed a criminal offence.

**Methods** We use data from a field experiment, in which the participants are randomly assigned to a restorative justice programme. We link the data from the field experiment to longitudinal administrative data on education and criminal records and correct for selective attrition by implementing an instrumental variable approach.

**Results** The results show that participation in the restorative justice programme increases the probability of recidivism one year after the programme by 39.3 percent from a base rate of 17.1 percent and decreases tertiary educational attainment by 29.1 percent from a base rate of 30.9 percent.

**Conclusions** Altogether, the results of this study suggest that despite the design backed by criminological and sociological theory, a restorative punishment from the Halt programme does not succeed in reducing criminal involvement and improving the educational outcomes.

**Keywords** Restorative justice · Education · Juvenile crime · Field experiment

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✉ Melvin Vooren  
m.vooren@vu.nl

<sup>1</sup> Amsterdam Center for Learning Analytics, Faculty of Behavioral and Movement Sciences, Vrije Universiteit Amsterdam, Amsterdam, Netherlands

<sup>2</sup> Top Institute for Evidence Based Education Research, Faculty of Economics and Business, University of Amsterdam, Amsterdam, Netherlands

<sup>3</sup> Top Institute for Evidence Based Education Research, Faculty of Humanities and Sciences, Maastricht University, Maastricht, Netherlands

## Introduction

The mainstream theory of law and economics predicts that humans act rationally (Bush, 2003; Ehrlich, 1996; Ehrlich and Becker, 1972; Nagin and Paternoster, 1991, 1993). For economic agents, criminal behaviour is dependent on the personal trade-off between the costs and benefits of committing a crime. The mechanism behind traditional punishment is that enforcing penalties increases the individual costs of committing a crime. This in turn results in a reduction of criminal behaviour. However, enforcing penalties may also come with additional costs on society. In addition to the costs of the judicial system, traditional punishment also comes with other societal costs, such as negative effects on educational outcomes (see Hjalmarsson, 2008; Webbink et al., 2013; Aizer and Doyle, 2015). This is especially relevant considering the crime-reducing effect of educational attainment (Machin et al., 2011). Driven by the intention of healing rather than hurting, restorative justice programmes are used to diminish the societal costs of enforcing penalties. To assess whether restorative justice is effective in decreasing these societal costs, this paper assesses the effects of a restorative justice programme on both recidivism and tertiary education attainment.

Restorative justice is becoming more prevalent in many developed countries for both adults and juveniles. Restorative justice is a broad approach in criminal justice that focuses on the reintegration of the participants after they have taken responsibility for their actions, through their reconciliation with victims and the community. Despite the growing interest in restorative justice programmes among policymakers, empirical evidence on the effect of such programmes on recidivism is rather ambiguous (e.g., Miers et al., 2001; Bradshaw and Roseborough, 2005; Sherman and Strang, 2007). Evidence with respect to other outcomes, such as education, is scarce. In this paper, we re-examine experimental data from the Netherlands to answer the question whether restorative justice is less expensive to society than simply apprehending a youthful offender and processing them through the initial steps of the criminal justice system without further punishment or formal mechanism through which to make amends.

In the Netherlands, restorative juvenile justice is arranged by *Halt*, which is an acronym for the Dutch word *Het Alternatief* (*The Alternative*). *Halt* is an institution integrated in the national justice system that conducts short-term interventions to prevent and reduce juvenile crime. For several decades, this institution has been involved in organising restorative justice programmes for juvenile delinquents. Juveniles aged between 12 and 18 years, apprehended by police for a delinquent act, are directed to a local *Halt* bureau. These local *Halt* bureaus first confront juvenile delinquents with the consequences of their behaviour and subsequently offer them an opportunity to repair the harm they have caused. This is achieved by conducting a working or an educational assignment or a combination of both. For the juvenile delinquents, the main benefit of completing these assignments is that they avoid a criminal record.

To assess the effects of *Halt* assignment on recidivism, the Dutch Ministry of Justice conducted a randomised field experiment between the years 2003 and

2004. In this experiment, 944 juveniles were randomly assigned to the juvenile restorative justice arranged by *Halt*. Individuals who were assigned to the placebo group did not participate in the restorative programme and did not get any other punishment. Ferwerda et al. (2006) evaluated this experiment using administrative police data up to 1 year since the first contact with *Halt*. They concluded that the recidivism rate was not statistically significant between the two groups, even though the rate of vandalism was statistically higher for juveniles in the treatment group. However, the authors did not control for potential selective attrition.

In this study, we improve on the analysis in the Ferwerda et al. (2006) study in two ways. First, we first estimate the effect of *Halt* on recidivism by controlling for selective dropout of participants from the treatment group by implementing an instrumental variables (IV) approach. Second, we examine if the *Halt* programme also affects the educational careers of juveniles, measured by tertiary educational attainment. For this purpose, the experimental data is linked to annual longitudinal administrative data from Statistics Netherlands. Our results suggest that participation in the *Halt* programme increases the probability of recidivism 1 year after the experiment by 39.3 percent from a base rate of 17.1 percent. This effect is driven by males and participants who have committed solo offences. In addition, *Halt* assignment statistically decreases the probability of tertiary educational attainment of the participants by 29.1 percent from a base rate of 30.9 percent. The results from this study contribute to the literature on the effectiveness of restorative justice programmes.

Although restorative justice programs are implemented on a large scale, there is no consistent evidence that these programmes reduce reoffending or improve other societal outcomes. The use of experimental data is vital for the current debate about the effectiveness of restorative justice programmes. However, experiments in the justice system like the *Halt* experiment in the Netherlands are very rare. In addition, this study contributes to the empirical literature by examining the medium- and long-term programme effects instead of only the short-term effects, as is the case in many RCT studies.

This paper consists of five sections. The next section discusses the restorative justice programme *Halt*. Section 5 describes the experimental design of the study. Section 6 describes the data and descriptive statistics. Section 9 discusses the estimation strategy and empirical findings. Finally, Section 16 concludes and discusses our findings.

## Institutional background

The restorative justice programme *Halt* was initiated in 1981 to combat and prevent vandalism among juveniles in the Netherlands. In 1995, it was integrated into the Dutch juvenile justice system. According to the Dutch Ministry of Justice, it is important to address minor offences such as vandalism at an early stage to prevent juveniles from committing more serious offences. In order to address these problems at an early stage, juveniles who were apprehended by the police for committing a minor first-time offence are directed to the *Halt* programme. The programme aims

to change the behaviour and attitudes of these juveniles by addressing their behavioural and development problems. The offences for which juveniles can be directed to the Halt programme include vandalism, graffiti, arson, and abuse of fireworks. In Appendix Table 6, we present the complete list of offences for which juveniles can be sent to Halt (see also the website of Halt at <https://www.halt.nl/en/>).

### The Dutch secondary educational system

When children in Dutch education finish primary education, which is generally at the age of twelve, they are tracked into different secondary education levels. This is also the moment from which children can enrol in Halt. The main tracks are *pre-vocational education*, *general secondary education*, and *pre-university secondary education*. Pre-vocational education takes 4 years to prepare children for another 4 years of vocational education. General secondary education has a duration of 5 years and prepares children for vocational higher education, which has a duration of 4 years. Finally, pre-university secondary education gives access to university education and has the longest duration: 6 years. In addition to the main three tracks, there is a track for children with learning problems as well. This *special needs secondary education* refers to secondary education for children with learning problems. Tertiary educational attainment can be directly inferred from the educational registration data from Statistics Netherlands. In this data, tertiary educational attainment is measured as the enrolment into a university or a vocational higher educational degree at some point of time, within 10 years after the experiment. It is not mandatory to enrol in tertiary education after having completed the general or the pre-university secondary educational track. However, it is mandatory for students from the pre-vocational track to enrol in vocational education until they have reached a basic vocational degree or until they have reached the age of 18.

### The design of the Halt programme

Juveniles who are apprehended by the police for a Halt-worthy offence are directly referred to the nearest local Halt bureau. At the local Halt bureau, they are screened by social workers. The juveniles are questioned for their motives and confronted with the consequences of their criminal behaviour. After this, the juveniles can choose to either participate in the Halt programme or to be prosecuted in the traditional juvenile justice, which entails being sent to the public prosecutor and the possibility of getting a criminal record. In the experiment, however, the juveniles in the placebo group were enrolled in Halt, but did not receive the restorative treatment and no other punishment.

Taking the committed offence into account, Halt professionals develop a personalised programme that includes sessions with the juvenile participant and their parents. Taking all of the aspects of the offence into account, this results in a punishment programme that consists of the following components: (i) community work, such as cleaning, repairing, or administrative work in the neighbourhood; (ii) learning assignments, for instance writing an essay or apology letter, discussion sessions

with a Halt professional, and Aggression Replacement Training; (iii) financially compensating the damage that was done; and (iv) meeting with the victims. A crucial component of the programme is that juveniles have to apologize to their victims. Victims explain how they were affected by the criminal offence, such that juveniles are directly confronted with the consequences of their behaviour.

In the Halt programme, the total maximum duration of a restorative punishment is 20 h, to be completed outside of school hours. Community work assignments vary between 1 and 20 h, and learning assignments vary between 1 and 8 h. The variation in time intensity depends on the committed criminal offence and on the emotional or behavioural problems of the young person diagnosed by the Halt professionals. For example, juveniles who committed property crime, shoplifting, arson, and demolition receive the longest working assignment, including, for instance, cleaning and working at the victim's store. This can vary between 18 and 20 h. The longest learning assignment (6 to 8 h) is mainly given for offences such as demolition, shoplifting, property crime, handling of stolen goods, and reckless behaviour. The average time spent on community work and learning assignments is 8 and 4 h, respectively.

## Experimental design

The experiment was conducted between 2003 and 2004 by research bureau 'Beke', as an initiative of the Dutch Ministry of Justice. In total, 1,064 juvenile delinquents who committed a Halt-worthy offence were apprehended by the police and received an invitation to participate in the experiment. The first meeting took place within 1 or 2 months after the offence. A total of 944 participants together with their parents or caretakers approved to participate in the Halt experiment. The other 120 adolescents decided to participate in the restorative juvenile justice programme Halt, but not in the experiment. Juveniles in the latter group were, on average, more frequently non-Dutch and were more often participants of the Halt programme earlier. The reasons for not participating in the experiment were lack of motivation; a negative attitude towards the experiment; poor language skills; or dyslexia problems (see Ferwerda et al., 2006).

The 944 participants who participated in the Halt experiment were randomly assigned to the treatment group ( $N = 465$ ) or to the placebo group ( $N = 479$ ). The participants in the treatment group were apprehended and received the restorative (Halt) programme. The participants in the placebo group were apprehended and released after their first visit to Halt, without receiving the restorative programme. Out of the 62 local Halt bureaus in the Netherlands, 12 participated in the experiment. These local Halt bureaus are located in different geographical locations (see Ferwerda et al. (2006) for a list of geographical locations). Juveniles were assigned to the treatment within each of these local Halt bureaus, depending on where we're living. The local Halt bureaus were chosen for participation in the experiment in such a way that geographically, both local bureaus in both the larger Dutch cities and local bureaus in less urbanised areas were included, spread out over the country. The number of assigned juveniles within each Halt bureau varies between 20 and 166 juveniles (see Appendix, Table 7 for the exact numbers).

The randomisation was performed in five subsequent steps. First, representative Halt staff members were appointed by the 12 Halt bureaus, and these representatives and the juveniles had to fill out 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 offence was committed, and type of offence). In the third step, stratified block randomisation based on Halt bureau and group level was applied to assign juveniles who had committed a first-time offence to the Halt programme and to the placebo group. Juveniles who had committed a group offence were randomly assigned to the Halt treatment on the group level in order to avoid unnecessary interactions between the peers. More detailed information on group sizes assigned to the treatment and placebo groups is given in Section 6. In the empirical analysis, it is therefore crucial to cluster the standard errors at the level of the group in which the offence was committed. In step four, researchers informed Halt representatives on who was assigned to the Halt programme and who was assigned to the placebo group. In the final step, juveniles received information on whether they had to participate in the restorative Halt programme.

Juveniles assigned to the placebo group received no treatment besides being apprehended by the police, visiting the local Halt bureau with their parents or caretakers, and completing the questionnaire. In reality, non-enrolment in Halt can result in prosecution by the public prosecutor. In the experiment however, the juveniles in the placebo group were enrolled in Halt, but did not receive the restorative treatment. Therefore, the estimated Halt effect in this paper represents the impact on recidivism and educational outcomes given that prosecution effects are excluded. These prosecution effects are, however, limited because the programme targets adolescents who only have committed minor first-time offences. As a consequence, juveniles are never incapacitated during trial (punishment), and they can only receive a criminal record if they are aged 16 years or older. Adolescents who are 16 years or older can receive a criminal record in the Dutch jurisdiction, but this record is only preserved for 2 to 4 years. All things considered, it is unlikely that the fact that the placebo excludes the prosecution effects will impose a large bias on the estimated programme effects.

Six months after the Halt intervention started, all 944 juveniles from the treatment, and the placebo group had to return to the Halt bureau together with their parents to complete a second-round questionnaire. Juveniles had an incentive to fill out the second questionnaire as it was a condition to complete the experiment successfully and thus to be released from prosecution. For juveniles assigned to Halt, successful completion also meant completion of the entire programme. Juveniles who did not complete the Halt programme were considered as programme dropouts. Even though there were strong incentives to complete the programme successfully, 91 juveniles (19.6%) from the total treatment group dropped out of the programme.

## Data and descriptive statistics

The experimental data contain background characteristics of the 944 juveniles who participated in the experiment. Table 1 compares family- and education-type characteristics of the 479 juveniles assigned to Halt and the 465 to the placebo group. This

**Table 1** Characteristics of participants in the treatment and the placebo group

	Placebo ( <i>N</i> =479)		Halt ( <i>N</i> =465)		<i>p</i> val.
	Mean	Std. dev.	Mean	Std. dev.	
Education type					
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

information comes from the Halt questionnaire. 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 12, 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 juveniles 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).

In most of the local Halt bureaus, the number of observations in the treatment and placebo group is not the same (see Appendix A-2).<sup>1</sup> If some of the individual Halt bureaus differ in terms of characteristics of juveniles than other Halt bureaus, large imbalances in the number of observations between the treatment and placebo groups can create differences in the overall characteristics even when characteristics are balanced within each Halt bureau. These imbalances can be accounted for by using weights equal to the inverse of the relative size of the treatment and placebo group within each bureau. However, using weights does not alter the results, which

<sup>1</sup> This is mainly caused by assigning juveniles to the Halt or the placebo group together with others.

**Table 2** Characteristics of participants with RIN and those without RIN (only statistically significant differences)

	With RIN (N=815)		Without RIN (N=129)		p val.
	Mean	Std. dev.	Mean	Std. dev.	
Sibling(s)	0.95	0.23	0.91	0.29	0.07
Mother alive	0.99	0.09	0.98	0.15	0.08
Education					
Currently in education	0.95	0.23	0.90	0.30	0.04
Registered truancy	0.31	0.46	0.40	0.49	0.06
Offence type					
Group offence	0.75	0.43	0.67	0.47	0.06

can be explained by the fact that the actual sample sizes in treatment and placebo in each bureau do not vary that much. Therefore, we do not use weights and show unweighted estimates in Section 9 of this paper.

We show the distribution of the group sizes for both the number of participants as well as the number of individual groups per group size in Table 8 in the Appendix. The table shows that 77.9 percent of the offences are committed alone or with one other person. This represents 91.0 percent of the total number of groups. For the analysis, it is important to recognize that the randomization occurred at the group level and the standard errors must therefore be clustered at the level of the 645 groups (Abadie et al., 2017).

### Linking the experimental data to register data

We link the experimental data to administrative data on criminal involvement and educational attainment available from Statistics Netherlands, using a Record Identification Number (RIN). For 14 percent of juveniles who participated in the experiment, the RIN could not be determined. Therefore, these 14 percent could not be linked to the administrative data and could not be used in the analysis.

Table 2 compares the characteristics presented in Table 1 between participants that could and could not be linked to the administrative data (based on a unique person identifier, referred to as RIN). Only mean differences that are statistically significant at the 95 percent confidence level are shown. The table shows that only the characteristic *currently in education* significantly is different at 5 percent confidence level. Given the small difference in mean, it seems unlikely that the omission of these 14 percent would bias our results.

### Dropout from the Halt programme

Out of the 408 first-timers assigned to the treatment, 78 did not complete the Halt programme. In order to assess whether this dropout is related to observable characteristics, we report the characteristics of the group that completed the Halt programme and the group that dropped out from the Halt programme in Table 3. The selective programme dropout is mainly characterised by differences in family background characteristics rather than by differences in committed offences or education levels. Juveniles



**Table 3** Characteristics of juveniles who completed the Halt programme and juveniles who terminated their participation

	Assigned to placebo		Assigned to Halt treatment				
	Placebo ( <i>N</i> =407)	Mean	Treatment ( <i>N</i> =330)		Dropout ( <i>N</i> =78)		
			Mean	Diff	Mean	Diff	
Age in 2003	14.43	14.51	-0.09		14.81	-0.38	**
Girl	0.31	0.24	0.07	**	0.32	-0.01	
Dutch ethnicity	0.64	0.73	-0.08	**	0.61	0.03	
Two-parent household	0.65	0.66	-0.01		0.50	0.15	**
Group offence	0.72	0.79	-0.07	**	0.74	-0.02	
Delinquent offence in 2003	0.71	0.8	-0.09	***	0.59	0.12	**
Registered truancy	0.32	0.31	0.01		0.32	-0.00	
Offence type: theft	0.01	0.02	-0.00		0.06	-0.05	***

\* $p < 0.10$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

who dropped out from Halt are somewhat more often enrolled in vocational education. Juveniles who dropped out of the Halt programme are also more often suspended at some point in secondary education. Juveniles who completed the Halt programme are more often from two-parent households and more often have the Dutch ethnicity.

## Estimation strategy and empirical findings

### Estimation strategy

To take into account the potential effect of selection bias introduced by the selective dropout of 78 participants, we adopt an instrumental variable (IV) approach when estimating the programme effect. We estimate an IV model to control for selective dropout from the Halt treatment. The treatment effect is estimated using a two-stage least squares model (Angrist and Pischke, 2009). The IV estimate gives the local average treatment effect (LATE). This gives the impact of the Halt programme for the group that experience the entire programme, accounting for non-compliance. Ferwerda et al. (2006) estimate the intent to treat (ITT). This differs from the LATE estimated here, because the ITT does not account for non-compliance. In the first stage, we estimate the probability of Halt participation by regressing the participation status  $H_i$  on a set of covariates  $X_i$  and on a variable that indicates whether juveniles were assigned to Halt ( $Z_i = 1$ ) or to the placebo group ( $Z_i = 0$ ):<sup>2</sup>

<sup>2</sup> 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 programme, if juveniles finished school before the programme started, and Halt bureau dummies.

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

Subscript  $i$  is a student indicator and  $\varepsilon_i$  is a random error term that has mean zero and variance  $\sigma_\varepsilon^2$ . In the second stage, we regress criminal involvement and the two educational outcome variables considered in this paper ( $Y_{ij}$ ) 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:

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

Subscript  $j$  refers to the two second stage models that we estimate, one for each of the two outcome variables: recidivism and tertiary educational attainment. The random errors  $\mu_{ij}$  and  $\varepsilon_i$  are correlated, so we estimate the first and second stage equations simultaneously (Wooldridge, 2009). The estimated treatment effect is consistent and unbiased in large samples because instrument  $Z_i$  is assumed to be uncorrelated with the error terms  $\varepsilon_i$  and  $\mu_{ij}$  due to the randomisation and thus can only influence the considered educational outcomes through  $H_i$ . As shown in Angrist and Pischke (2009), the two-stage least squares estimator has a finite sample bias. This bias shrinks when the F-statistic is very large. Although we have a limited sample of 815 observations, the F-statistic is very large ( $>80$ ), which erases our concern for this finite sample bias.

### 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 first-stage equations is equal to the number of subgroups considered in the regression model. If we take into account, for example, that Halt may affect boys differently than girls, we estimate the following system of first stage equations:

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

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

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 for the model that includes subgroup effects with respect to gender then equals:

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

Again, we estimate the first and second stage equations simultaneously, since the error terms  $\theta_{i1}$  and  $\theta_{i2}$  are positively correlated with  $\mu_{ij}$ . The term  $\beta_{1j}$  measures the treatment effect for girls on educational outcome  $j$  and the term  $\beta_{2j}$  measures whether the treatment effect for boys differs from the estimated effect for girls.

**Table 4** The effects of Halt on criminal involvement

	OLS Results		IV Results			
	OLS	Red Frm	1st stage	2nd stage		
Halt assignment (Z)		0.054* (0.028)	0.807*** (0.020)			
Halt participation (X)	0.044 (0.029)			0.067** (0.034)	0.087** (0.041)	0.082 (0.076)
Halt*girl					-0.074 (0.069)	
Halt*group						-0.019 (0.085)
Age	-0.012 (0.011)	-0.013 (0.011)	-0.006 (0.007)	-0.012 (0.011)	-0.013 (0.011)	-0.012 (0.011)
Girl	-0.107*** (0.036)	-0.108*** (0.036)	-0.052* (0.027)	-0.104*** (0.035)	-0.076* (0.043)	-0.104*** (0.035)
Parents born in NL	-0.049 (0.036)	-0.049 (0.036)	0.025 (0.027)	-0.051 (0.035)	-0.051 (0.035)	-0.051 (0.035)
Halt bureau dummies	Included	Included	Included	Included	Included	Included
Other controls	Included	Included	Included	Included	Included	Included
R-sq.	0.0948	0.0964	0.7034	0.0941	0.0959	0.0934
F-statistic	2.26	2.29	82.44	2.29	2.25	2.24
Mean non-participants	0.1794	0.1788	0.1201	0.1707	0.1707	0.1707
No. obs.	815	815	815	815	815	815

Other controls are dummies for single-parent household, school graduation before Halt, offence type, group offence, working status of parents. SEs are clustered at the group offence level and printed in parenthesis. \* $\rho \leq 0.1$ , \*\* $\rho \leq 0.05$ , \*\*\* $\rho \leq 0.01$

## Empirical findings

### Criminal involvement

Table 4 shows the results for the effects of Halt on criminal involvement. The first two columns show the OLS and reduced form results. The OLS results show no statistically significant difference in criminal involvement between the treatment and control groups. These results are in line with the results in the original report (Ferberda, 2006, p. 67). The reduced form results suggest that assignment to Halt is marginally related to criminal involvement. However, given that this is a field experiment, it is unlikely that this is problematic for the validity of the IV results. The third column shows the first stage regression results and the other columns show the second stage estimation results. The Kleibergen and Paap (2006) F-statistic, which provides an under-identification test, together with the coefficient of the Halt assignment variable, clearly shows that the Halt assignment variable is a very strong predictor of Halt participation. The second stage estimation results indicate that Halt participation increases criminal involvement by 6.7 percentage points, which is statistically significant at the five

percent level. In order to get an indication of the magnitude of this effect, we divide this coefficient by the mean of the group of non-participants. This shows that Halt participation results in an increase in criminal involvement of 39.3 percent.

When we add the interaction term of Halt participation and a dummy variable for girls to the specification, the coefficient of Halt participation increases to 8.7 percentage points. Also, the coefficient is statistically significant at the five percent level. This suggests that there are heterogeneous effects between males and females. However, hypothesis testing fails to reject the null that the coefficients are not statistically different from each other, suggesting that there is no evidence of heterogeneous effects on criminal involvement between males and females. In the last column of Table 4, we control for the interaction of Halt participation with a dummy variable for group offences. This dummy variable equals one when the group size is larger than one (see Table 8). Controlling for the interaction of Halt participation with this group offence dummy increases the effect of Halt participation on criminal involvement to 8.2 percentage points.

The coefficients of the interaction terms themselves are not statistically significant. This can be explained by the fact that when we estimate multiple first-stage equations, more noise is included in the second stage regressions, which in turn results in less precise estimates. The signs of the interaction terms suggest that male, non-group offences mainly drive the positive effect of Halt participation on criminal involvement. Given the sample size, statistical power might be an issue when adding the interaction effects. So in short, the evidence on heterogeneous effects is inconclusive.

### Tertiary educational attainment

The effects of Halt participation on tertiary educational attainment are shown in Table 5. From the results of the second stage regression, it seems that Halt participation reduces tertiary educational attainment by 9.0 percentage points. This corresponds to a decrease of 29.1 percent, measured as the percentage change in the probability of tertiary educational attainment with respect to the placebo group. This effect is significant at the one percent level. This negative effect on tertiary educational attainment can be explained by the positive effect of Halt participation on criminal involvement shown in Table 4. When we include the interaction of Halt participation with a dummy variable for girls to the specification, the coefficient becomes statistically insignificant. The coefficient of the interaction term is negative and statistically significant at the one per cent level. This suggests that the negative effect of Halt participation on tertiary educational attainment is mainly driven by females.

### Non-linkage

As described in Section 7, 129 juveniles could not be linked to the register data from CBS. Tahamont et al. (2020) demonstrate that this non-linkage can potentially lower the statistical power of the experiment. Given that the results of this paper show statistically significant effects of Halt participation on both tertiary educational attainment and criminal involvement, we do not have to be concerned about this reduction of power due to non-linkage.

There is however still the probability that non-linkage influences the results. To investigate this, we perform the following bounding exercise. First, we calculate the average

**Table 5** The effects of Halt on tertiary educational attainment

	IV Results			
	OLS	Rd Firm	1st stage	2nd stage
Halt assignment (Z)			0.807*** (0.020)	
Halt participation (X)	-0.052* (0.029)	0.073*** (0.028)		
Halt*girl				-0.038 (0.039)
Halt*vocational educ.				-0.191*** (0.070)
Halt*group				-0.158** (0.065)
Age	0.027** (0.011)	0.028** (0.011)	-0.006 (0.007)	0.027** (0.011)
Girl	0.036 (0.035)	0.037 (0.035)	-0.051* (0.027)	0.105** (0.047)
Parents born in NL	-0.043 (0.032)	-0.042 (0.031)	0.024 (0.027)	-0.040 (0.031)
Halt bureau dummies	Included	Included	Included	Included
Other controls	Included	Included	Included	Included
R-sq	0.3596	0.3627	0.7034	0.3578
F-statistic	16.82	16.68	82.44	16.59
Mean NP	0.2949	0.2984	0.1201	0.3091
N	815	815	815	815
				0.025** (0.011)
				0.036 (0.034)
				-0.044 (0.031)
				Included
				Included
				0.3619
				17.58
				0.3088
				815
				0.026** (0.011)
				0.035 (0.034)
				-0.040 (0.031)
				Included
				Included
				0.3610
				16.38
				0.3092
				815

Other controls are dummies for single-parent household, school graduation before Halt, offence type, group offence, working status of parents. SEs are clustered at the group offence level and printed in parenthesis. \* $p \leq 0.1$ , \*\* $p \leq 0.05$ , \*\*\* $p \leq 0.01$

educational outcomes for juveniles in the Halt and the placebo group. Then we assigned the average educational outcome of juveniles in the control (intervention) group to non-linkable juveniles in the Halt (control) group. It implies that non-linkable juveniles in the Halt (control) group receive an *unfavourable* (*favourable*) educational outcome. By estimating the IV model again, we estimate the Halt effect while assigning unfavourable educational outcomes to non-linkable juveniles with respect to the Halt estimate. Table 9 in the Appendix shows the estimation results of this bounding analysis.

As expected, the estimation results are smaller but still statistically significant. They indicate that Halt participation reduces tertiary educational attainment by 7.0 percentage points. The obtained estimates can be viewed as lower bound estimates. These estimation results are very similar to those presented in Table 5, and therefore we conclude that the estimates are unlikely to be strongly affected by non-linkage.

## Conclusion and discussion

In this study, we evaluate the effects of the Dutch restorative justice programme Halt. We apply an instrumental variables strategy to control for selective attrition from the programme. In addition to estimating the effects of programme participation on criminal involvement, we also estimate the effects of programme participation on tertiary educational attainment. There is evidence that criminal involvement at a young age is not only positively associated with future criminal involvement (see Ellis et al., 2009; Loeber et al., 2013) but that it can also lead to lower educational outcomes (see Hjalmarsson, 2008; Webbink et al., 2013; Aizer and Doyle, 2015).

From an international perspective, the Dutch Halt programme cannot be easily compared to programmes in other European countries. Instead, the Halt programme is comparable to restorative justice programmes in the USA. Early experimental research evaluating the effectiveness of restorative justice in Pennsylvania hints at lower rates of recidivism in the experimental group compared to the control group (McCold and Wachtel, 1998). However, these effects were only found for violent offences and not for minor offences. In another study, McGarrell et al. (2000) attribute these positive effects to expressing guilt and repairing the damage. Violent crimes were not included in this study. Comparatively, the Dutch Halt programme is targeted at first offenders who have committed relatively minor, non-violent offences.

The results of this study indicate that participation in the Halt programme increases criminal involvement 1 year after the programme by 39.3 percent. There is no evidence for heterogeneous effects on criminal involvement for males and females. Considering tertiary educational attainment, the results of this study suggest that participation in this restorative justice programme decreases tertiary educational attainment by 29.1 percent. This is expected given the positive effect of programme participation on criminal involvement. However, the negative effect of programme participation on tertiary educational attainment is bigger for females than for males.

An interesting result is that the positive effect of Halt participation increases when we control for the interaction of Halt participation and a dummy variable for group offences. This suggests that the Halt programme is less appropriate for solo offences, which might be

related to psychological differences in for instance the conscience between solo and group offences. The differences in the effects on tertiary educational attainment between boys and girls can also be related to differences in conscience between boys and girls but also to differences in intrinsic motivation for tertiary educational attainment between boys and girls.

Where the main mechanism of traditional punishment works by increasing the personal costs of criminal behaviour, restorative justice programmes strive to decrease the societal costs of enforcing punishments. To achieve this, the Halt programme is based on several sociological theories that aim to “heal” the delinquent *ex post*, contrasting with traditional punishments that aim to decrease criminal behaviour mainly *ex-ante* by acting as a deterrent. In the field of law and economics, the hypothesis that a higher expected punishment lowers criminal behaviour *ex ante* is known the deterrence hypothesis (see Ehrlich, 1996 for an overview). Next to providing the main economic argument for traditional punishments, the deterrence hypothesis can explain why restorative punishments such as the Halt programme do not work when we interpret the deterrence hypothesis with the insights from behavioural economics.

Using the data from an experiment in Israel, Gneezy and Rustichini (2000) show that the introduction of a small monetary fine for parents arriving late to collect their children from the day care leads to significantly more late-coming parents. Arriving late forces teachers to stay after closing time. The introduction of a fine alters an incomplete social or private contract by providing the parents with an impression of the monetary costs of the teachers staying late, justifying coming late and paying the fine. In the context of the Halt programme, the restorative Halt treatment leads to participants feeling justified in their criminal behaviour after completing the restorative programme. In addition, the life-course criminology literature (see Blokland and Nieuwebeerta, 2010) suggests that from a developmental criminological view, the impact of punishment for what might be considered a relatively trivial offence early in a person’s life be so detrimentally formative. This may also hold for restorative justice punishments. For future policy, the Halt programme should consider different eligibility rules. Currently, the programme is targeted at first offenders, who could be more affected by these adverse effects than the repeated offenders.

Our result that the restorative programme leads to a 39.3 percent higher chance of repeated criminal behaviour suggests that the perceived costs of criminal behaviour in the incomplete contract, *i.e.*, without punishment, are higher than the costs of criminal behaviour with the Halt punishment. This is also backed by the fact that the total duration of Halt punishments is maximised at 20 h, with an average work assignment of just 8 h combined with a learning assignment that lasts 4 h on average. Something that might play a role as well is the celerity of the sanction. There is evidence that immediate consequences play a role in the effectiveness of sanctions (Kilmer and Midgette, 2020). The first Halt meeting takes place 1 to 2 months after the offence, so there is a long time period between the offence and the sanction. Altogether, the results of this study suggest that despite the design backed by criminological theory, a seemingly well-intentioned restorative punishment in the Halt programme does not succeed in reducing criminal involvement and improving the educational outcomes of young people in the Netherlands. Policy-wise, in addition to reconsidering the eligibility rules, the program should be extended into a more punitive, labour-intensive procedure. Complementarily, this can also add a heightened focus on the costs of crime and repairing the damage for the victims.A

## Appendix

**Table 6** Halt-worthy offences related to law sections

Section of the law	Offence	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	Offence against property
311(1) (under 4th) CL	(Shop) Theft in association with one or more persons + attempt to	Offence against property
321 CL	Fraud + attempt to	Offence against property
350 CL	Demolition	Demolition
	Graffiti	Demolition
416 CL	Deliberately handling stolen goods	Offence against property
417 CL	Debt handling	Offence against property
326 CL	Change of price tags (fraud)	Offence against property
424 CL	Reckless behaviour with danger/disadvantage goods	Reckless behaviour
461 CL	Trespassing	Other
1.2.2 Fireworks decree	Illegal/defective fireworks	Firework offence
1.2.4 Fireworks decree	Possession of more than 10 kg of fireworks in stock	Firework offence
2.3.6 Fireworks decree	Ignite fireworks outside permitted period	Firework offence
General local regulation	Firework	Firework offence
Reckless behaviour	Reckless behaviour	
72 Regulation passenger traffic	Behaviour 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



**Table 7** Assignment characteristics of juveniles in each Halt bureau

	Amsterdam		Breda		Den Bosch		The Hague		Eenschede		Friesland	
	T=82	P=84	T=10	P=10	T=17	P=17	T=43	P=43	T=58	P=55	T=13	P=13
Age	14.72 (1.21)	14.02*** (1.54)	14.20 (1.62)	15.30* (1.76)	14.47 (1.12)	14.12 (1.27)	14.30 (1.48)	14.65 (1.52)	14.48 (1.67)	14.58 (1.54)	14.85 (1.34)	14.92 (1.32)
Women	0.35 (0.48)	0.32 (0.48)	0.50 (0.53)	0.40 (0.52)	0.12 (0.33)	0.24 (0.44)	0.26 (0.44)	0.42* (0.50)	0.16 (0.37)	0.20 (0.40)	0.46 (0.52)	0.38 (0.51)
Dutch parents	0.39 (0.48)	0.32 (0.47)	0.60 (0.52)	0.67 (0.50)	0.71 (0.47)	0.71 (0.47)	0.86 (0.35)	0.81 (0.39)	0.76 (0.43)	0.75 (0.44)	0.83 (0.39)	0.85 (0.38)
Group offence	0.73 (0.45)	0.67 (0.47)	0.60 (0.52)	0.50 (0.53)	0.65 (0.49)	0.47 (0.51)	0.79 (0.41)	0.74 (0.45)	0.84 (0.37)	0.74* (0.44)	0.75 (0.45)	0.38** (0.51)
Demolition	0.18 (0.39)	0.18 (0.39)	0 (0.42)	0.20* (0.39)	0.18 (0.39)	0.24 (0.44)	0.28 (0.45)	0.21 (0.41)	0.19 (0.40)	0.20 (0.40)	0.08 (0.28)	0.08 (0.28)
Graffiti	0.06 (0.24)	0.04 (0.19)	0 (0.19)	0 (0.26)	0 (0.26)	0 (0.22)	0 (0.22)	0 (0.19)	0 (0.22)	0.04 (0.19)	0 (0.19)	0 (0.22)
Shoplifting	0.37 (0.48)	0.46 (0.50)	0.60 (0.52)	0.60 (0.52)	0.12 (0.33)	0.41** (0.51)	0.30 (0.46)	0.42 (0.50)	0.17 (0.38)	0.24 (0.43)	0.62 (0.51)	0.77 (0.44)
Property crime	0.16 (0.37)	0.17 (0.37)	0.20 (0.42)	0 (0.47)	0.29 (0.47)	0.18 (0.39)	0.19 (0.39)	0.14 (0.35)	0.12 (0.33)	0.15 (0.35)	0.08 (0.28)	0 (0.28)
Handling stolen goods	0.04 (0.19)	0.02 (0.15)	0.20 (0.42)	0.20 (0.42)	0 (0.47)	0 (0.47)	0.02 (0.15)	0 (0.15)	0 (0.29)	0.02 (0.13)	0.08 (0.29)	0.08 (0.29)
Reckless behaviour	0.07 (0.26)	0.04 (0.19)	0.20 (0.42)	0.20 (0.42)	0.18 (0.39)	0.06 (0.24)	0.07 (0.26)	0.09 (0.29)	0.21 (0.41)	0.31 (0.47)	0.08 (0.28)	0.08 (0.28)
Joint sign. test	Prob>F=0.1962		Prob>F=0.0175		Prob>F=0.2010		Prob>F=0.5556		Prob>F=0.6240		Prob>F=0.0392	
	Gorinchem	Groningen	Leiden	Maastricht	Nijmegen	Zwolle						
Age	T=34 13.97	P=34 14.32	T=27 14.78	P=29 14.62	T=15 13.80	P=15 14.47	T=51 14.78	P=53 14.55	T=45 15.09	P=46 15.20	T=70 14.59	P=80 14.23*

Table 7 (continued)

Women	0.26 (0.45)	0.38 (0.49)	0.11 (0.32)	0.14 (0.35)	0.20 (0.41)	0.40 (0.51)	0.29 (0.46)	0.32 (0.47)	0.27 (0.45)	0.33 (0.47)	0.27 (0.45)	0.26 (0.44)	0.26 (0.44)
Dutch parents	0.82 (0.39)	0.65* (0.49)	0.96 (0.19)	0.83* (0.38)	0.67 (0.49)	0.87 (0.35)	0.88 (0.33)	0.83 (0.38)	0.86 (0.35)	0.80 (0.40)	0.57 (0.50)	0.57 (0.50)	0.49 (0.50)
Group offence	0.85 (0.36)	0.75 (0.44)	0.78 (0.42)	0.90 (0.31)	0.73 (0.46)	0.79 (0.43)	0.76 (0.43)	0.75 (0.43)	0.80 (0.41)	0.60** (0.50)	0.83 (0.38)	0.83 (0.38)	0.75 (0.43)
Demolition	0.35 (0.49)	0.15** (0.36)	0.26 (0.45)	0.31 (0.47)	0.33 (0.49)	0.27 (0.46)	0.22 (0.42)	0.17 (0.38)	0.18 (0.39)	0.07* (0.25)	0.09 (0.28)	0.09 (0.28)	0.09 (0.28)
Graffiti	0	0	0	0	0.07	0	0	0	0	0.04* (0.21)	0.04 (0.20)	0.04 (0.20)	0.05 (0.22)
Shoplifting	0.24 (0.43)	0.56*** (0.50)	0.30 (0.47)	0.17 (0.38)	0.40 (0.51)	0.27 (0.46)	0.39 (0.49)	0.43 (0.50)	0.18 (0.39)	0.26 (0.44)	0.37 (0.49)	0.37 (0.49)	0.33 (0.47)
Property crime	0.09 (0.29)	0.03 (0.17)	0.19 (0.40)	0.34* (0.48)	0	0.40*** (0.51)	0.20 (0.40)	0.11 (0.32)	0.04 (0.21)	0	0.23 (0.42)	0.23 (0.42)	0.18 (0.38)
Handling stolen goods	0.03 (0.17)	0.03 (0.17)	0	0	0	0	0.08 (0.27)	0	0	0.07** (0.25)	0.01 (0.12)	0.01 (0.12)	0.03 (0.16)
Reckless behaviour	0.12 (0.33)	0.24 (0.43)	0.22 (0.42)	0.17 (0.38)	0.13 (0.35)	0	0.06 (0.24)	0.06 (0.23)	0.11 (0.32)	0.24* (0.43)	0.17 (0.38)	0.17 (0.38)	0.15 (0.36)
Joint sign. test	Prob>F=0.1107	Prob>F=0.7952	Prob>F=0.3783	Prob>F=0.3766	Prob>F=0.2249	Prob>F=0.2637							

T denotes treatment group and P denotes placebo group. Standard deviations are printed in parentheses. \* $p \leq 0.1$ , \*\* $p \leq 0.05$ , \*\*\* $p \leq 0.01$

**Table 8** Distribution of group sizes

Group size	Number of participants			Number of groups		
	Frequency	Percentage	Cumulative	Frequency	Percentage	Cumulative
1	439	46.5%	46.5%	439	68.1%	68.1%
2	296	31.4%	77.9%	148	22.9%	91.0%
3	114	12.1%	89.9%	38	5.9%	96.9%
4	36	3.8%	93.8%	9	1.4%	98.3%
5	40	4.2%	98.0%	8	1.2%	99.5%
6	12	1.3%	99.3%	2	0.3%	99.8%
7	7	0.7%	100.0%	1	0.2%	100.0%
Total	944	100.0%		645	100.0%	

**Table 9** Lower bound estimates for tertiary educational attainment

	IV Results	
	1st stage	2nd stage
Halt assignment	0.801*** (0.019)	
Halt participation		-0.070** (0.031)
Age	-0.010 (0.007)	-0.013 (0.010)
Girl	-0.050** (0.024)	-0.043 (0.029)
Parents born in NL	0.018 (0.025)	-0.038* (0.030)
Halt bureau dummies	Included	Included
Other controls	Included	Included
R <sup>2</sup>		0.2966
F-statistic	105.34	
No. obs.	944	

Control variables are similar to those in Table 6. SEs are clustered at the group offence level and printed in parenthesis. \* $\rho \leq 0.1$ , \*\* $\rho \leq 0.05$ , \*\*\* $\rho \leq 0.01$

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