CPB Netherlands Bureau for Economic Policy Analysis

CPB Discussion Paper | 241

The effects of a special program for multi-problem school dropouts on educational enrolment, employment and criminal behaviour

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Evidence from a field experiment

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Abstract

This paper evaluates the effects of a special program designed to increase school enrolment and employment among multi-problem youths. Treated youths are guided by personal coaches and receive a comprehensive treatment of educational, work, and health services. We investigate the impact of the program by implementing a specific assignment rule such that treatment status depends in a deterministic way on an individual's application date. We find evidence that assignment to the program increases criminal activity compared to standard intervention, especially among the subpopulation of youths who were suspected of a crime at the time of entry. Peer effects caused by grouping at-risk youths together may explain the adverse impact on criminal behaviour. We find statistically insignificant effects on school enrolment and employment.

JEL Codes: I21, I28

Keywords: disadvantaged youths, school dropout, criminal behaviour, program evaluation

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1. Introduction

School dropout among disadvantaged youths is a major concern. School dropouts often have poor prospects for a financially self-sufficient future and are more likely to become criminals (e.g., Lochner and Moretti, 2004; Machin et al., 2011). The high social costs associated with both joblessness and rising crime rates legitimize publicly subsidized interventions aimed at improving prospects for disadvantaged youths. An important question is which interventions are most effective for this group. Compared to training programs for adults, programs for young school dropouts have been less well-studied.

This paper investigates the effects of a special program for multi-problem school dropouts aged 16 to 23 by means of a field experiment. The Neighbourhood School Program (NSP) in Rotterdam was launched by the Dutch government in 2009 with the goal of helping vulnerable youths return to school or enter the labour force. Individuals in the program receive an integrated set of educational, work and health services and counselling by a personal coach. To assess the effects of the program we make use of capacity constraints at the NSP and implement a specific assignment rule such that treatment status is determined only by an individual's application date. We investigate the impact on school enrolment, employment, and criminal behaviour three years after the start of the intervention. We estimate various regression models, which essentially compare outcomes between youths assigned to the NSP and youths assigned to standard intervention activities, conditional on the time of application. To address noncompliance with the assignment rule, we use an instrumental variables approach (Angrist et al., 1996). We find evidence that assignment to the NSP increases criminal activity compared to standard intervention, especially among the youths who were suspected of a crime at the time of entry. Additional analyses suggest that peer effects caused by grouping at-risk youths together can explain this effect. We find statistically insignificant effects of assignment to the NSP on school enrolment or employment.

Our paper relates to the literature on training programs for disadvantaged youths. Previous studies show that mentoring programs can be effective in preventing school dropout. Examples of such programs in the United States that improved educational outcomes are Big Brothers Big Sisters (Tierney et al., 1995) and the Philadelphia Futures' Sponsor-A-Scholar (Johnson, 1999). The Quantum Opportunities Program, which combines counselling minority students with financial incentives, increases educational attainment in the short run (Taggart, 1995; Maxfield et al., 2003). A recent 10-year follow-up evaluation by Rodriguez-Planas (2012), however, suggests that most of the beneficial effects disappear in the long run. In addition, the author finds that the program increased the likelihood of committing a crime for male participants. Walker and Vilella-Velez (1992) evaluate the Summer Training and Employment Program, which offers remedial education and summer jobs to disadvantaged young people, and find short-run gains in test scores but no long-term impact on school completion rates or employment. Two programs that provide financial incentives to teenage parents to stay in school – Ohio's Learning, Earning, and Parenting Program and the Teenage Parent Demonstration – increased future earnings and employment among students who were still in school when they began the program, but not among participants who had already dropped out before the start of the program (Granger and Cytron, 1998). Carneiro and Heckman (2003) more generally conclude from an overview of the evaluation literature on school- and training-based policies for adolescents that intervention targeted at dropouts is much less successful than that targeted at youths still enrolled in school.

LaLonde (2003) describes the development of publicly subsidized training programs in the United States and concludes that, while modest positive effects are found among adults, these programs generally fail to produce employment gains among disadvantaged youths. Experimental evaluations of federal programs such as JobStart, the Job Training Partnership Act, and the Work Experience Programs do not provide evidence of beneficial effects on employment rates or earnings (Cave and Doolittle, 1991; Couch, 1992; Orr et al., 1994). The Job Corps program seems to be the only exception, having shown to positively affect outcomes for the target population. Job Corps is a relatively intensive and expensive program, with average costs of around US$16,500 per participant. It provides a comprehensive set of training services – including vocational education, counselling, work experience, social skills training, and health education – to disadvantaged youths aged 16 to 24 in a residential setting. Schochet et al. (2001) find that the program increased educational attainment and earnings, while it reduced arrest rates during the treatment period. A follow-up study, however, suggests that the earnings gains and reduction in criminal activity did not persist in the longer term (Schochet et al., 2008).

More recently, an evaluation of the National Guard Youth ChalleNGe Program, an intensive military structured treatment that successively offers residential-based education and mentoring to young dropouts, showed promising results after three years (Millenky et al., 2011). The program increased college enrolment, employment, and earnings, but did not decrease criminal behaviour among participants. In addition, participants appeared to do worse on some health and lifestyle outcomes. Roder and Elliot (2011) examine the effects of the Year-Up program, which offers six months of full-time classes followed by six months of internships with U.S. companies to minority youths aged 18 to 24. Treated individuals had higher earnings one year after the end of the program, although employment rates did not significantly differ between the program and control groups.

The overall intervention record for out-of-school youth is ambiguous. Some programs do not succeed in improving outcomes, while others yield encouraging results. Most studies evaluate relatively short-term impacts, while longer-term follow-up studies provide evidence that early gains can fade away over time. The empirical evidence indicates that designing effective programs to serve the problematic group of young dropouts is a serious challenge.

Our analysis contributes to the existing literature in the following ways. We evaluate the effect of a program in a European country that is largely designed based on lessons from previous studies. Carneiro and Heckman (2003) summarize guidelines regarding the design of special youth measures based on the evaluation literature and argue that effective programs need to combine an integrated mix of education, occupational skills, work-based learning, and supportive services and that adult mentors may help to handle inappropriate attitudes. Compared to most programs discussed, such as Job Corps, the NSP is targeted to a more specific subpopulation of vulnerable dropouts who face complex behavioural, financial, or health related problems. Around 50% of the target population in our sample was suspected of a crime when they entered the program and around 70% had only completed primary education. The program's content is relatively intensive and adds assistance by professional health specialists to work-based and educational elements to help youths solve their problems.

Our findings with respect to school enrolment and employment are in line with a large body of the literature that shows no impact of training programs on the labour market outcomes of at-risk youths (e.g., Carneiro and Heckman, 2003; LaLonde, 2003). Our results regarding crime are consistent with studies that document the adverse effects of group-based interventions on delinquent behaviour (Dishion et al., 1999) and studies that find evidence that peer effects due to grouping at-risk adolescents together can explain the reinforcement of criminal activity (Dodge et al., 2007).

The rest of this paper is organized as follows. Section 2 describes the institutional background, the content of the intervention, and assignment to the program. Section 3 presents the data and Section 4 discusses the empirical strategy. Section 5 reports the main findings and Section 6 presents additional analyses that add to the interpretation of our main findings. Section 7 concludes the study.

2. The NSP

2.1 Background

In the Netherlands, a school dropout is defined as someone aged 12 to 23 who withdraws from school without having completed a particular level of education called the start-qualification. This start-qualification is considered the minimum level of education needed to participate well in the labour market and corresponds to a degree in higher secondary education or intermediate vocational education.[2] One of the bigger cities in the Netherlands, Rotterdam typically has a large share of school dropouts and unemployed youth (Ministry of Education, 2010; Municipality of Rotterdam, 2011). Table 1 presents the percentage of school dropouts for the Netherlands, its four largest cities (Amsterdam, Rotterdam, The Hague, and Utrecht) and Rotterdam during the 2008-2009 school year. The fraction of school dropouts in Rotterdam is almost twice the nation's average and larger than that of the other large cities.

Table 1. School dropouts in 2008–2009

	The Netherlands	four large cities	Rotterdam
School dropouts (%)	3,2%	5,4%	6,1%

Notes. School dropouts are 12-to-23 year olds who drop out of school during the 2008-2009 school year without having obtained a start-qualification. Source: Ministry of Education (2010).

Out-of-school youths aged 18 to 27 without a job can apply for help and income services at their municipality of residence. Municipalities are obliged to offer a suitable reintegration program to applicants. Acceptance of this offer is compulsory for eligibility for social benefits. Youths aged under 18 without a start-qualification are not served because of the compulsory education requirement. In Rotterdam, the Young People's Office (YPO) is responsible for the assignment of youths to reintegration programs. Each applicant is invited for a consultation, after which he or she is referred to the best-suited program. If youths refuse to participate in the proposed program, they do not receive a second offer and lose their rights to social benefits.

[2] Unemployment among youths without a start-qualification is more than twice as high as unemployment among youths with a start-qualification and amounted to around 13% in 2009 (Statistics Netherlands, 2011).

2.2 The Intervention

The NSP was launched at two sites in Rotterdam in 2009 as an alternative to regular reintegration programs. The purpose of the NSP is to help multi-problem school dropouts return to formal education or work. Initially, the Ministries of Education, Social Affairs and Employment, Justice, and Health contributed 5.6 million euros to finance a two-year pilot. After two years, the pilot was extended.

The program's target population consists of school dropouts aged 16 to 23 who are unemployed and face complex problems in several areas of their lives. More specifically, youths must satisfy all of the following criteria for NSP eligibility:

- Is aged 16 to 23,
- Dropped out of school without having obtained a degree in intermediate vocational education or higher secondary education,
- Does not work in a structured job for more than 12 hours a week,
- Faces problems in at least two areas of their lives, including work, finances, health, housing, justice, and social environment, and
- Has an IQ above 70.

Participants in the program are formally registered as being enrolled in education and receive student grants. The program provides an integrated treatment of educational, work and care services. A special team of adult coaches and health specialists support the youths to solve their problems, increase their skills, and help them return to formal education or work. Each participant is counselled by a personal coach and is expected to be present five days a week, from 9:00 to 15:30.

The program's educational component consists of courses in specific subjects in which youths can obtain certificates. The educational level of these courses is comparable to the lowest level of intermediate vocational education. Besides increasing learning skills and knowledge, successful participation in these courses can contribute to self-esteem. With respect to work, neighbourhood schools use their regional networks to arrange small jobs or internships with local firms. These internships teach youths to participate in the labour market under the supervision of their coaches. In addition, group trainings are organized to improve general worker skills. Next to the educational and work services, youths are professionally supported by health specialists, including social workers, psychiatric nurses, a behavioural specialist, and a (part-time) psychiatrist to help solve their personal problems.

The NSP is characterized by its small-scale and personal approach. Neighbourhood schools offer customized treatment with respect to both program content and duration. The precise content is adjusted to the specific needs of the individuals and the program length is flexible, with an average duration of around 10 months. Each of the two neighbourhood schools, one located in the north and the other in the south of Rotterdam, has a capacity of 100 places. Youths accepted into the program are usually sent to one of these locations, based on their place of residence. The total costs of the program equal around 14,000 euros per place per year.

Compared to the regular programs, the NSP differs in four main ways. First, it is a small-scale program that groups youths together and treats them in a personal, customized manner for a flexible duration. Second, it offers a more comprehensive mix of educational, job training, and health services. Third, it is a more intensive and expensive program. Fourth, participants are formally enrolled in education and receive student grants rather than social benefits.

2.3 Assignment

To assess the impact of the NSP we used the capacity constraints of neighbourhood schools and implemented a specific assignment rule based on the consultation date at the YPO. We agreed upon the assignment procedure with the YPO. In the first period, between 1 August 2009 and 1 March 2010, all youths who satisfied the eligibility criteria were referred to the NSP.[3] As of 1 March 2010, the neighbourhood schools had reached their maximum capacity and hence could not accept more participants. Subsequently, all youths who satisfied the eligibility criteria and had their consultation at the YPO in the second period, between 1 March 2010 and 15 April 2010, were referred to a regular reintegration program.[4] These youths were treated normally by the YPO and serve as the control group. Individuals in the treatment group would have received the same treatment had there been no neighbourhood schools. To increase our number of observations, we repeated this procedure. In the third period, between 15 April 2010 and 1 July 2010, all youths who satisfied the eligibility criteria were sent to the NSP and in the fourth period, between 1 July 2010 and 15 September 2010, all youths who satisfied the eligibility criteria were referred to a regular program.

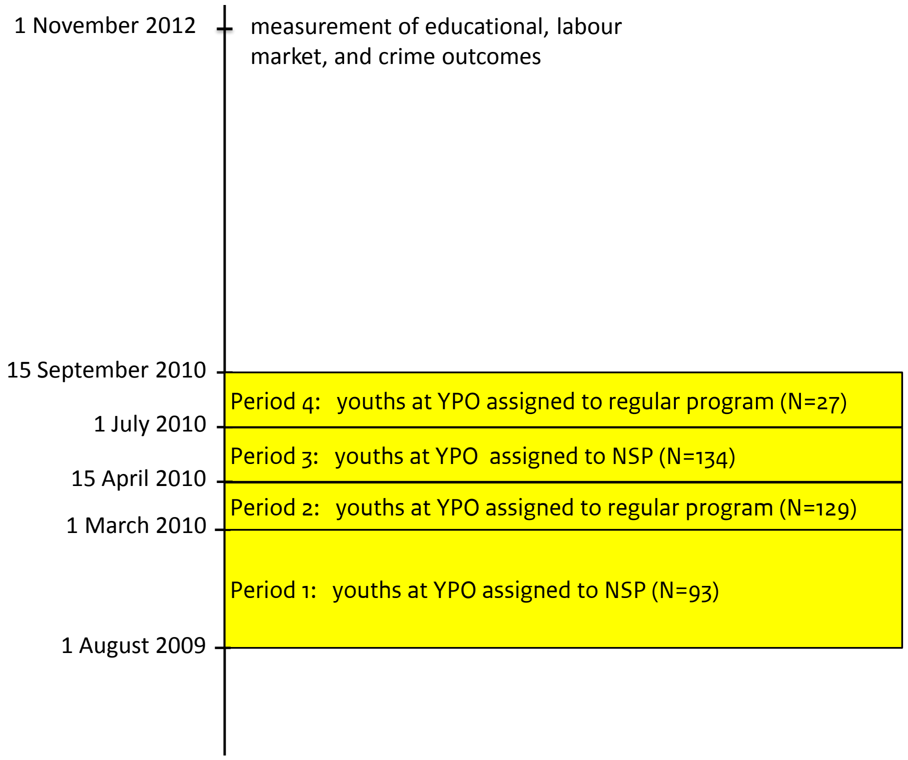
Figure 1 presents our research design. The assignment rule ensures that treatment status depends deterministically on an individual's consultation date at the YPO. Youths who enter the YPO in the

[3] When youths apply, the YPO ensures that they meet the eligibility criteria. We chose to start collecting data from 1 August 2009 and thereby excluded youths that entered the YPO in the beginning of the NSP, between March and August 2009. This reduces the likelihood of the results being affected by potential start-up problems at the NSP.

[4] We agreed that all youths would be sent to a regular program during this entire predetermined period. Hence, even when new places became available in the neighbourhood schools during this period (because of the outflow of participants), none of the applicants were sent to the NSP.

first or third period are assigned to the NSP, while similarly eligible pupils who enter the YPO in the second or fourth period are assigned to one of the regularly available programs.

Figure 1. Research design.



This approach allows us to identify the effects of assignment to the NSP by comparing relevant schooling, labour market, and crime outcomes for both groups, conditional on the time of consultation. We measure these outcome variables on 1 November 2012, two to three years after assignment to a program (see Section 3).

Our analysis includes only youths aged 18 and older, since the regular reintegration programs do not accept 16- and 17-year-olds because of compulsory education.[5] A total of 383 individuals who satisfied the profile had a consultation at the YPO between 1 August 2009 and 15 September 2010, 93 in the first period, 129 in the second period, 134 in the third period, and 27 in the fourth. Variation in the number of consultations by period is mainly caused by typical school dropout patterns during the school year. Most dropouts leave school in the last months of the school year. This explains the peak in consultations during March till July (periods 2 and 3) and the much lower number of consultations in the first half of the school year and during the summer holidays (periods 1 and 4).

[5] Since 16- and 17-year-olds can differ with respect to observable or unobservable characteristics, such as the nature of their problems, educational experience, and legal rights and obligations, the inclusion of these youths would likely cause imbalances between the treatment and control groups.

Noncompliance

According to the assignment rule, 227 youths should be assigned to the NSP and 156 youths should be assigned to a regular program. We received information on each individual's consultation date and corresponding assignment from the YPO administration. These youths were assigned to one of the two neighbourhood schools or one of the 27 different reintegration programs.

These data reveal that in practice not all youths were assigned according to their time of entry. The YPO employees did not always act in line with the assignment rule and sometimes over-ruled our research design by referring individuals who were not intended to be treated to the NSP, and vice versa. It turns out that 39 of the 227 individuals who should have been assigned to the NSP were referred to a regular program. Similarly, 12 of the 156 individuals who should have been referred to a regular program were assigned to the NSP.

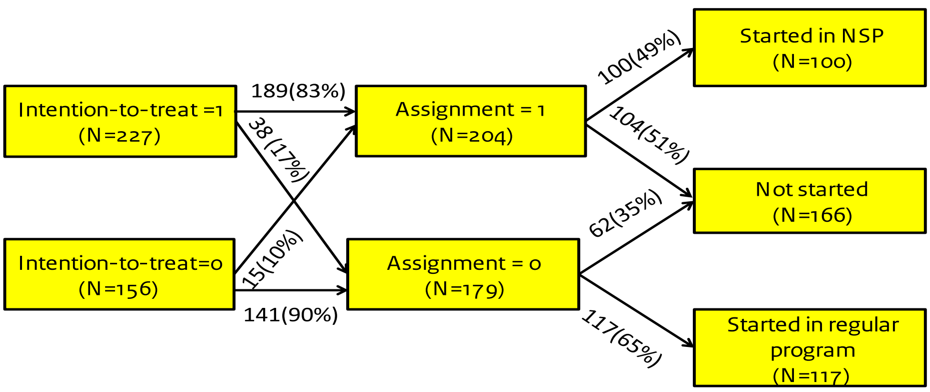
Because of this observed noncompliance, we distinguish between an 'intention-to-treat' status and an 'assignment' status. Youths who entered the YPO in the first or third period were intended to be treated and youths who entered the YPO in the second or fourth period were not intended to be treated. The intention-to-treat variable is thus only determined by the consultation date. The assignment variable depends on the actual assignment and takes the value of one if an individual is assigned to the NSP and zero if not. The assignment differs from the intention-to-treat when the referral is not in accordance with the assignment rule. A second – and less important – reason why assignment does not always coincide with the intended treatment is that a few individuals had a second consultation at the YPO after 15 September 2010. These four individuals had initially been referred to a regular program and were reassigned to the NSP after their second consultation.[6] Of the 227 individuals who were intended to be treated, 38 (17%) were referred to a regular program. Similarly, 15 of the 156 individuals (10%) not intended to be treated were assigned to the NSP.

Furthermore, not all youths who were assigned to the NSP decided to participate. Of the 204 individuals assigned to the NSP, 104 refused to participate in the program. Hence, 51% of the assigned youths decided not to start at the neighbourhood school after the consultation at the YPO. These youths sometimes communicated that they did had no intention of starting, but in most cases they simply did not show up and were unreachable. This large fraction of non-starters already indicates that it is difficult to hold on to the target group and place them into treatment. For a comparison, the fraction of non-starters among the remaining 179 youths assigned to one of the regular reintegration programs was also relatively high, at 35%. There are two potential explanations for the larger fraction of non-starters among youths assigned to the NSP. First, they may be less

[6] One of the individuals with a later assignment to the NSP, was intended to be treated, but initially referred to the control group. The other three individuals all had an intention-to-treat status equal to zero.

inclined to participate in a more intensive program that requires more effort and time. Second, youths in the NSP receive student grants instead of social benefits, which is financially less attractive. Figure 2 summarizes the assignment flow by intention-to-treat status and starting behaviour by assignment status for all 383 youths in our estimation sample.

Figure 2. Intention-to-treat, assignment, and starting behaviour.



3. Data

Our data come from three sources. First, we use data on educational and labour market position from the YPO's administrative records.[7] This information is collected in the status forms completed by the YPO on four different reference dates: 1 April 2011, 1 October 2011, 1 May 2012, and 1 November 2012. In addition to individual information on consultation dates and assigned programs, the status forms contain four main dummy variables for each youth, indicating (i) whether the individual has ever participated in the assigned program, (ii) whether the individual has moved out of the program on the corresponding reference date, (iii) whether the individual is enrolled in regular education on the corresponding reference date[8] and (iv) whether the individual had a job on the corresponding reference date. Second, individual background characteristics are collected from registration forms, which are completed by the YPO during the consultations. Third, we use the

[7] The YPO uses administrative systems called MensCentraal and Key2Onderwijs, which derives its information from the administrative enrolment figures of the Ministry of Education (the so-called BRON data).
[8] Obviously, an individual who still participates in the NSP is not considered to have a regular educational position.

administrative BVH data from the police of Rotterdam–Rijnmond.[9] These data contain information on the number of crimes an individual is suspected of having committed during the period between 1 August 2001 and 1 November 2012.

Outcome variables

As a first outcome, we use a dummy variable that indicates a successful labour market position. This variable takes the value one if an individual is in education and/or has a job and zero otherwise. We use this as an outcome variable to investigate the effects of the NSP on school enrolment and/or employment. Our main analyses use the data of the status form on reference date 1 November 2012, since this provides the most recent information on educational enrolment and employment and hence is most informative of the longer-term effects of the NSP. Moreover, this status form is best suited for the analyses, since only by the latest reference date did all participating youths move out of the NSP. In addition we construct a variable that indicates the yearly average number of crimes an individual is suspected of during the period between the consultation date and 1 November 2012. Although suspected individuals are not proven to have committed a crime, it seems plausible that this variable is a credible indicator of criminal activity.[10] We label this variable as the crime rate after the consultation date and use it to assess the effects of the NSP on criminal behaviour. [11] Although a reduction in criminal activity is not an explicit goal of the program, it seems to be a relevant outcome regarding the multi-problem target population and the program's health services focused on solving personal and behavioural problems.

Covariates

For the covariates we use a set of individual background characteristics, including gender, age, country of birth and educational attainment. In addition, we construct three variables indicating the average yearly number of crimes an individual is suspected of during the eight-, four- or two-year periods before the consultation date at the YPO. We denote these as the eight-, four- and two-year pre-treatment crime rates and add these objective measures of problem intensity as covariates to our analyses.

Table 2 presents the descriptive statistics for all individuals in our estimation sample by intention-to-treat status.

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[9] The acronym BVH stands for *Basis Voorziening Handhaving*.

[10] A potential concern might be that differences in the number of suspected crimes between treatment and control groups do not only reflect differences in criminal behaviour but also in the reporting or arrest probabilities. In our case, comparing similar youths in Rotterdam, there is no reason to expect that the two groups differ with respect to reporting or arrest probabilities.

[11] Hereafter in the paper, we use the term '*crime rate*' as short-hand for the average yearly number of crimes of which an individual is suspected.

Table 2. Descriptive statistics: Comparison by intention-to-treat status

	Intention-to-treat = 1	Intention-to-treat = 0	*p*-Value
Covariates			
Gender (male = 1)	0.58	0.65	0.16
Age	20.59	20.63	0.80
Country of birth			0.73
The Netherlands	0.77	0.71	
Morocco	0.05	0.04	
The Antilles	0.08	0.10	
Surinam	0.04	0.06	
Other	0.06	0.08	
Latest educational position			0.31
Primary education	0.01	0.00	
Practical education	0.00	0.01	
Special education	0.04	0.05	
Pre-vocational secondary education	0.04	0.07	
Intermediate vocational education (level 1)	0.26	0.19	
Intermediate vocational education (level 2)	0.42	0.44	
Intermediate vocational education (level 3)	0.08	0.11	
Intermediate vocational education (level 4)	0.08	0.08	
Higher secondary education	0.00	0.01	
Unknown	0.03	0.05	
Highest completed education level			0.25
Primary education	0.74	0.63	
Pre-vocational secondary education	0.12	0.20	
Intermediate vocational education (level 1)	0.07	0.06	
Intermediate vocational education (level 2)	0.04	0.04	
Intermediate vocational education (level 3)	0.00	0.00	
Intermediate vocational education (level 4)	0.00	0.01	
Higher secondary education	0.00	0.01	
Unknown	0.03	0.05	
Pre-treatment crime rate (2-year period)*	0.39	0.44	0.55
Pre-treatment crime rate (4-year period)*	0.44	0.41	0.78
Pre-treatment crime rate (8-year period)*	0.35	0.31	0.49
Outcome variables			
Educational and/or labour market position	0.30	0.28	0.71
Crime rate (after consultation)*	0.33	0.29	0.55
Total number of observations	227	156	

* Crime rates represent the average yearly number of crimes an individual is suspected of during the relevant period.

The first column reports the sample means of the 227 youths who were intended to be treated. The second column reports the sample means of the 156 youths who were not intended to be treated. The third column reports the *p*-value of the difference, calculated using a two-tailed *t*-test or a chi-squared test. The variable gender takes the value one in the case of a male and zero in the case of a female. The latest educational position (10 categories) indicates the most recent education type an individual was enrolled in at the time of consultation.[12] The highest completed education level (eight categories) refers to the highest educational degree an individual has obtained.

A large fraction, 69%, of the individuals in our estimation sample only completed primary education. The pre-treatment crime rates report the yearly average number of crimes an individual is suspected of in the period two, four or eight years before the consultation date. Individuals in our estimation sample are, on average, suspected of around 0.3–0.4 crimes a year, depending on the period in question. A total of 52% of all youths in our sample were suspected of a crime at least once at the time of consultation, 53% in the intention-to-treat group and 50% in the other group (not shown in the table). The sample statistics reveal that both groups are comparable in observable characteristics. We observe no significant differences with respect to gender, age, educational attainment, or number of suspected crimes.

Since the intention-to-treat status is only determined by the time of entry at the YPO, we would not expect to observe systematic unbalances between the two groups. The descriptive statistics suggest that the period of entry is not (largely) correlated with background characteristics.

The bottom panel of Table 2 gives a first impression of the potential effects on outcomes. We observe no significant differences between the groups with respect to either educational and/or labour market position and crime rates after the consultation at the YPO. The fraction of individuals in our sample who are either enrolled in school or employed on 1 November 2012 equals 29%. The average crime rate after the consultation date equals 0.31, which is slightly below the average pre-treatment crime rate.

As discussed, the selective assignment of youths to either the NSP or the control group is a concern. To provide insight into this issue, we also compare observable characteristics by assignment status. Table 3 displays the sample statistics for both the group of youths assigned to the NSP and those assigned to a regular reintegration program.

[12] One of these categories, practical education, is an education type that provides special care for pupils with a low IQ (below 85) or learning arrears, while special education targets pupils who have some kind of handicap. Intermediate vocational education is offered at four levels, which prepare students for specific professions in the labour market.

Table 3. Descriptive statistics: Comparison by assignment

	Assigned to the NSP	Assigned to regular program	*p*-Value
Covariates			
Gender (male = 1)	0.61	0.60	0.77
Age	20.53	20.68	0.36
Country of birth			0.40
The Netherlands	0.77	0.72	
Morocco	0.05	0.04	
The Antilles	0.08	0.10	
Surinam	0.04	0.05	
Other	0.05	0.10	
Latest educational position			0.38
Primary education	0.01	0.01	
Practical education	0.02	0.00	
Special education	0.05	0.04	
Pre-vocational secondary education	0.05	0.06	
Intermediate vocational education (level 1)	0.27	0.19	
Intermediate vocational education (level 2)	0.42	0.44	
Intermediate vocational education (level 3)	0.08	0.11	
Intermediate vocational education (level 4)	0.07	0.10	
Higher secondary education	0.00	0.01	
Unknown	0.03	0.05	
Highest completed education level			0.25
Primary education	0.75	0.63	
Pre-vocational secondary education	0.12	0.19	
Intermediate vocational education (level 1)	0.06	0.07	
Intermediate vocational education (level 2)	0.04	0.05	
Intermediate vocational education (level 3)	0.00	0.00	
Intermediate vocational education (level 4)	0.00	0.01	
Higher secondary education	0.00	0.01	
Unknown	0.03	0.05	
Pre-treatment crime rate (2-year period)*	0.44	0.38	0.51
Pre-treatment crime rate (4-year period)*	0.48	0.36	0.17
Pre-treatment crime rate (8-year period)*	0.39	0.27	0.04
Outcome variables			
Educational and/or labour market position	0.27	0.32	0.30
Crime rate (after consultation)*	0.36	0.26	0.13
Total number of observations	204	179	

* Crime rates represent the average yearly number of crimes an individual is suspected of during the relevant period.

The observable characteristics are quite similar. Both groups do not differ significantly with respect to gender, age, country of birth, or educational attainment. With respect to criminal activity, however, we observe that youths with a higher eight-year period pre-treatment crime rate are more frequently assigned to the NSP. This may suggest selective assignment.

In our analyses (see Section 5) we control for all covariates. With respect to criminal behaviour, our main analyses include the eight-year pre-treatment crime rate in. Although including a shorter period might yield a better indicator of an individual's behaviour at the time of entry at the YPO, we believe that including a longer period is most informative. In addition, this is the only covariate that differs significantly between those assigned to the NSP and those assigned to the control group.

Table A.1 in the Appendix presents the correlations between some of the most important variables in our analyses. Being male is positively correlated with both pre- and post-treatment crime rates. The pre-treatment crime rate is strongly correlated with the post-treatment crime rate and negatively correlated with an educational or labour market position. The other variables are less strongly correlated.

4. Empirical strategy

Because of noncompliance with the intended treatment, potential selection is the most important concern we must address to identify causal treatment effects.

We first assess the impact of assignment to the NSP by estimating the equation

$$Y_i = \beta_o + \beta_1 D_i + \beta_2 X_i + S(T_i) + \varepsilon_i , \quad (1)$$

where Y_i is the outcome variable of interest of individual i; X_i is a vector of individual background characteristics, including gender, age, age squared, country of birth, most recent education level, highest completed education level, and eight-year pre-treatment crime rate; D_i is a dummy variable that indicates whether individual i was assigned to the NSP; $S(T_i)$ is a smooth polynomial function of the month of entry, and ε_i is the error term. The coefficient of interest is β_1. Since our counterfactual is assignment to a regular reintegration program, the estimated coefficient β_1 should be interpreted as the effect of assignment to the NSP compared to assignment to a regularly available program. We use two main outcome variables: a dummy variable indicating whether the individual is enrolled in education and/or has a job on 1 November 2012 and the crime rate in the period between the consultation date and 1 November 2012.

Since estimation by ordinary-least squares (OLS) can yield inconsistent results because of selective assignment, we use an instrumental variables (IV) approach. To address the endogeneity problem, we instrument assignment by intended treatment and estimate equation (1) with two-stage-least squares (2SLS). The first stage, in which the assignment is regressed on the intended treatment and covariates, is

$$D_i = \gamma_o + \gamma_1 Z_i + \gamma_2 X_i + S(T_i) + u_i, \tag{2}$$

where Z_i is a dummy variable indicating the intention-to-treat status. This variable takes the value one if individual *i* had a consultation at the YPO between 1 August 2009 and 1 March 2010 or between 15 April 2010 and 1 July 2010 and zero if individual *i* had a consultation in the remaining periods, between 1 March 2010 and 15 April 2010 or between 1 July 2010 and 15 September 2010.

We identify the treatment effect on the assumption that the intended treatment (Z_i) is not correlated with the outcome variable (Y_i), conditional on covariates. Our model controls for smooth polynomials of the month of entry, which pick up potential continuous time effects on outcomes. Since variation in our instrument is due only to differences in the time of entry, it seems plausible that the instrument is not correlated with unobservables in the error term, conditional on the continuous function of the month of entry. Strictly speaking, the identifying assumption rules out other discontinuous effects of the time of entry on the outcomes.

A potential threat to the validity of our instrument might be the manipulation of consultation dates. Since we are using the administrative data of the YPO, an independent agency in Rotterdam with no particular interest in the relative performance of the NSP compared to other reintegration programs, the manipulation of consultation dates does not seem very realistic. An inspection of the number of intakes around the cut-off dates does not reveal any suspicious patterns. We observe no anomalous increase or decrease in the number of consultations during these periods. Moreover, the fact that we observe noncompliance with the assignment rule contributes to the credibility of non-manipulation. After all, in the case of manipulation, we should not still be observing assignments that are out of line with the consultation date.

Under the assumption of monotonicity, estimation with 2SLS yields a local average treatment effect that can be interpreted as the average treatment effect of assignment to the NSP for the subpopulation of compliers (Imbens and Angrist, 1994). Monotonicity implies that an increase in an individual's value of the instrument (i.e., change in the intention-to-treat status from zero to one) does not decrease the assignment variable (i.e., the probability of being assigned to the NSP), which seems plausible. This assumption partitions the estimation sample into three subgroups: compliers, never-takers, and always-takers (Angrist et al., 1996). Compliers are individuals whose treatment status is affected by the instrument. In our case, these are youths whose assignment to the NSP is completely determined by the intended treatment. Both never-takers and always-takers are individuals whose treatment status is not affected by the instrument. These are the youths who are never (or always) assigned to the NSP, independent of their intended treatment. Since the assignment is invariant to our instrument for never- and always-takers, the IV estimates are uninformative on the treatment effects for these subgroups.[13]

We focus our analyses on identifying the causal impact of assignment to the NSP, which seems to be most relevant from a policy perspective. We cannot use similar IV models to estimate the causal effects of participation in the NSP, because the intended treatment is not a valid instrument for participation. Apart from its impact via participation, the instrument can affect outcomes among the subsample of individuals who refuse to participate when assigned to the NSP. Since the YPO offers only one program, youths assigned to the NSP and who refuse to participate, do not start in any program at all. Similar youths who are assigned to the control group (but would have refused to participate in case of assignment to the NSP) can either start in a regular program or not start in any program at all. A comparison of the 51% non-starters among youths assigned to the NSP and the 35% non-starters among youths assigned to the regular programs indicates that part of the youths in the control group who are not willing to participate in the NSP accept to start in a regular program. Hence, since the intention-to-treat status affects assignment status, the instrument can affect outcomes through differences in starting behaviour among youths who are not willing to participate in the NSP. We thus only estimate the effect of assignment to the NSP, which is driven by both the effect of participation in the NSP and the effect of a different probability of starting in a program.

5. Estimated effects on schooling, labour market and crime outcomes

Table 4 reports the OLS and IV estimates of the effect of assignment to the NSP on both educational and/or labour market position and criminal activity for six model specifications. The first model regresses the outcome variable on a constant, a dummy variable for assignment to the NSP, and a linear function of the month of entry at the YPO. Model (2) additionally controls for socio-economic background characteristics such as gender, age, age squared, and country of birth. Model (3) adds the

[13] We can infer the population share of compliers in our sample. Since we observe (see Section 2) that the population shares of never-takers and always-takers equal 16.7% (38/227) and 9.6% (15/156), respectively, it follows from monotonicity that the population share of compliers equals 73.7%.

most recent educational position and the highest completed education level, while model (4) also includes the crime rate during the eight-year period before the consultation date at the YPO.[14] In models (5) and (6) the linear function of the month of entry is extended to quadratic and cubic polynomials, respectively.[15] The top panel of Table 4 presents the estimated effects on a dummy variable for school enrolment and/or employment on 1 November 2012, while the middle panel reports the estimated effects on the number of crimes an individual is suspected of during the period between the consultation date and 1 November 2012. In addition, the bottom panel of Table 4 provides information on the first-stage regression, in which assignment to the NSP is regressed on the intended treatment. Each estimate is based on a separate regression and robust standard errors corrected for clustering at the program level are shown in parentheses.[16]

The OLS regressions show that assignment to the NSP is not associated with an increase in educational or labour market position. All estimates are statistically insignificant and point estimates are slightly below zero. Since the OLS estimates may suffer from endogeneity, we proceed with IV estimates. The IV estimates confirm the OLS findings. In all specifications the effect of assignment to the NSP does not significantly differ from zero. Hence, we find no evidence that assignment to the NSP affects school enrolment or employment probabilities.

With respect to criminal activity, the OLS regressions yield positive but statistically insignificant effects. The IV regressions also show positive point estimates. In the full model, including higher-order polynomials of the time of entry, we find marginally significant positive effects of assignment to the NSP. This suggests that assignment to the NSP leads to an increase in criminal activity. To test the sensitivity of this result to outliers, we estimate the full-model IV specification on a restricted sample in which we remove the top 5% with the largest number of pre-treatment crimes (i.e., all individuals suspected of 15 crimes or more in the eight-year period before the consultation date). This yields a statistically significant effect at the 10% level of 0.14 (not shown in the table), which suggests that the result is not caused by a small subgroup of highly criminally active youths. Inclusion of a higher-order term of the month of entry substantially affects the estimated effect on the crime rate. This indicates there are unobserved differences in inflow over time, that are picked up by the smooth function of the month of entry.[17]

[14] Including the number of crimes during the shorter two- or four-year period instead of the eight-year period before the consultation date hardly affects the estimation results.

[15] Alternative specifications in which we include continuous functions of the day or week of entry rather than the month of entry yield similar estimation results.

[16] The total number of clusters equals 29, which are the programs youths are referred to after their consultations. We distinguish 27 different regular reintegration programs and two locations within the NSP, in the north and south of Rotterdam.

[17] Table A.2 in the Appendix presents additional estimation results for the specifications (1)-(4), including a third order polynomial of the month of entry. Adding covariates does not largely affect the estimated effects.

The first-stage regression determines the effect of the intention-to-treat on assignment. The estimates are highly significant, with *F*-values above 93 in all our models. This implies that the problem of weak instruments is of no concern in our analyses (Staiger and Stock, 1997). Point estimates are around 0.70. An interpretation of this estimate is the percentage of compliers in our sample: If the intention-to-treat variable increases from zero to one, the probability of being assigned to the NSP increases by around 70%.

Table 4. Impact of assignment to the NSP on schooling, labour market and crime outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Effect on educational/labour market position					
OLS						
Assignment to the NSP	-0.040	-0.051	-0.039	-0.029	-0.023	-0.023
	(0.067)	(0.049)	(0.053)	(0.051)	(0.041)	(0.041)
IV (second stage)						
Assignment to the NSP	0.032	0.010	0.024	0.032	0.052	0.052
	(0.082)	(0.073)	(0.088)	(0.083)	(0.076)	(0.073)
	Effect on crime rate					
OLS						
Assignment to the NSP	0.069	0.083	0.081	0.041	0.101	0.101
	(0.077)	(0.060)	(0.055)	(0.044)	(0.060)	(0.061)
IV (second stage)						
Assignment to the NSP	0.031	0.083	0.099	0.068	0.165*	0.163*
	(0.108)	(0.088)	(0.075)	(0.053)	(0.084)	(0.090)
	Effect on assignment to the NSP					
First-stage						
Intended assignment	0.723***	0.721***	0.721***	0.719***	0.686***	0.692***
	(0.067)	(0.064)	(0.065)	(0.066)	(0.071)	(0.070)
F-test for instrument	116.45	126.91	123.04	118.68	93.35	97.73
Controls						
SES	no	yes	yes	yes	yes	yes
Educational level	no	no	yes	yes	yes	yes
Pre-treatment crime	no	no	no	yes	yes	yes
Polynomial control for month of entry	linear	linear	linear	linear	quadratic	cubic
Observations	383	383	383	383	383	383

Notes. Robust standard errors are in parentheses. Asterisks indicate that the estimates are statistically significant at the *** 1% level and *10% level. The SES control variables include gender, age, age squared, and country of birth. The educational level controls include the latest educational position and the highest completed education level. Pre-treatment crime refers to a variable indicating the yearly average number of crimes an individual is suspected of during the eight-year period before the consultation date.

Heterogeneity

We now investigate the impact of assignment to the NSP for subgroups. We examine whether the effects differ between suspected and non-suspected youths, men and women, older and younger individuals, and relatively lower- and higher-educated youths. A suspected individual is defined as one who was suspected of a crime at least once during the eight-year period before entry at the YPO. A non-suspected individual was never suspected of a crime during this period. We define individuals aged 20.6 years (the average age in the estimation sample) or older as old and individuals aged below 20.6 years as young. Lower-educated youths are those who have only completed primary education and higher-educated youths are the remaining ones who obtained a secondary or higher educational degree.[18]

Table 5 presents the full-model IV estimation results of the effects of assignment to the NSP for these subpopulations (compare model (6) in Table 4). The second-stage results show that assignment to the NSP has no significant effects on educational or labour market position for all subpopulations. With respect to the number of crimes, we find a statistically significant positive effect, 0.36, for suspected individuals, while the point estimate for non-suspected individuals is close to zero. This finding implies that assignment to the NSP increases crime rates during the period between the consultation date and 1 November 2012, with 0.36 for the (compliant subpopulation of the) sample of youths suspected of a crime when they entered the program. We also find positive effects for the male subpopulation, while the effects for females are close to zero. Being suspected of a crime is strongly correlated with gender: 75% of the suspected subpopulation is male. In addition, statistically significant effects for the younger subpopulation are found (but the difference between estimated effects for young and old individuals is not very large).

These findings indicate that the marginally significant effects of assignment to the NSP on criminal activity are driven by the effects on the subpopulation of suspected individuals. Table A.3 in the Appendix presents all OLS and IV estimates for the subsample of suspected youths. Positive effects on crime rates are also found in the IV models that do not include a higher-order polynomial of the month of entry and in the OLS models. We conclude that assignment to the NSP increases criminal activity among youths who are already criminally active.

The bottom panel of Table 5 presents the corresponding first-stage results. Since the estimated first-stage effects can be interpreted as the percentage of compliers, a comparison of the first-stage effects in the different subsamples provides information on the composition of the compliant subpopulation in the complete sample. A large first-stage estimate in a specific subsample compared to the first-

[18] We choose this definition of higher education, since around 70% of youths in our estimation sample completed only primary education.

stage estimate in the complete sample implies an overrepresentation of this subgroup in the compliant population in the complete sample. Comparable first-stage effects within the range 0.59–0.73 suggest that there are no specific subpopulations that are largely over- or underrepresented in the sample of compliers.

Table 5. Heterogeneous treatment effects: IV estimates of the impact of assignment to the NSP on labour market/educational position and crime rates

	(1) Suspected	Not suspected	(2) Male	Female	(3) Old	Young	(4) Lower-educated	Higher-educated
	Effect on educational/labour market position							
IV (second stage)								
Assignment to the NSP	-0.002	0.135	0.161	-0.099	-0.024	0.097	-0.050	0.214
	(0.094)	(0.107)	(0.134)	(0.138)	(0.093)	(0.081)	(0.070)	(0.167)
	Effect on crime rate							
IV (second stage)								
Assignment to the NSP	0.355**	0.008	0.290**	-0.005	0.124	0.252**	0.117	0.281
	(0.127)	(0.027)	(0.118)	(0.068)	(0.074)	(0.120)	(0.086)	(0.222)
	Effect on assignment to the NSP							
First-stage								
Intended treatment	0.600***	0.732***	0.721***	0.594***	0.695***	0.678***	0.698***	0.687***
	(0.077)	(0.104)	(0.054)	(0.129)	(0.084)	(0.071)	(0.056)	(0.122)
F-test for instrument	60.72	49.54	178.27	21.20	68.46	91.19	155.35	31.71
Controls								
SES	yes	yes	yes	yes	yes	yes	yes	yes
Educational level	yes	yes	yes	yes	yes	yes	yes	yes
Pretreatment crime	yes	yes	yes	yes	yes	yes	yes	yes
Polynomial control for month of entry	cubic	cubic	cubic	cubic	cubic	cubic	cubic	cubic
Observations	199	184	232	151	194	189	266	117

Notes. Robust standard errors are in parentheses. Asterisks indicate that the estimates are statistically significant at the *** 1% level and **5% level. The SES control variables include gender, age, age squared, and country of birth. The educational level controls include the latest educational position and the highest completed education level. Pre-treatment crime refers to a variable indicating the yearly average number of crimes an individual is suspected of during the eight-year period before the consultation date.

Alternative outcome measures

In addition to our two main outcomes, we construct six related outcome variables: (1) a dummy variable that indicates whether an individual is enrolled in education on 1 November 2012; (2) a dummy variable that indicates whether an individual has work on 1 November 2012; (3) a dummy variable that indicates whether an individual has an educational and/or labour market position on 1 November 2012 or obtained an educational degree between the consultation date and 1 November

2012; (4) a dummy variable that indicates whether an individual has ever been in education or worked on either of the four reference dates (1 April 2011, 1 October 2011, 1 May 2012, and 1 November 2012); (5) the crime rate during a one-year period after the consultation date; and (6) a dummy variable that indicates whether an individual is suspected of at least one crime in the period between the consultation date and 1 November 2012. Table 6 presents the full-model (second-stage) IV estimates of the effect of assignment to the NSP for these six outcome variables. Each column corresponds to one outcome variable.

Table 6. Impact of assignment to the NSP on six alternative outcome measures

	Educational and/or labour market position				Criminal behaviour	
	(1) Educational position on latest reference date	(2) Labour market position on latest reference date	(3) Educational/ labour market position or educational degree on the latest reference date	(4) Educational/ labour market position on at least one of the four reference dates	(5) Crime rate in one-year period after the consultation date	(6) Suspected of at least one crime after the consultation date
IV (second stage)						
Assignment to the NSP	0.018 (0.062)	0.016 (0.046)	0.052 (0.078)	-0.061 (0.073)	0.115 (0.156)	0.017 (0.084)
Controls						
SES	yes	yes	yes	yes	yes	yes
Educational level	yes	yes	yes	yes	yes	yes
Pre-treatment crime	yes	yes	yes	yes	yes	yes
Polynomial control for month of entry	cubic	cubic	cubic	cubic	cubic	cubic
Observations	383	383	383	383	383	383

Notes. Robust standard errors are in parentheses. The SES control variables include gender, age, age squared, and country of birth. The educational level controls include the latest educational position and the highest completed education level. Pre-treatment crime refers to a variable indicating the yearly average number of crimes an individual is suspected of during the eight-year period before the consultation date.

Models (1) to (4) are related to educational and employment outcomes. Since the program is relatively more focused on educational outflow, one might hypothesize that it affected school enrolment more than employment. We find no evidence that the NSP specifically affected any one of these outcomes; both estimated effects on school enrolment and employment are statistically insignificant. Furthermore, we investigate the effects on an extended measure of educational and/or labour market success by including youths who have obtained a formal educational degree. In addition to educational enrolment, we received data on educational completion from the YPO. Five persons completed intermediate vocational educational before the latest reference date, two of whom assigned to the NSP and three to a regular program. Including these ‘successfully treated youths’ in our outcome measure does not affect estimation results. Finally, we assess potential short-run effects by looking at educational or labour market position on at least one of the four reference dates. Previous studies on youth programs found that positive effects can fade away in the longer term (e.g., Schochet et al., 2008; Rodriguez-Planas, 2012). If assignment to the NSP initially increases school enrolment or employment, we expect to see this on the earlier reference dates. A complicating factor is that not all youths moved out of their programs on the earlier reference dates. Variation in the start dates and program durations of youths cause differences in outflow between both groups. Because of the longer program durations in the NSP, the fraction of youths who left the program is larger in the control group for each of the reference dates. Since youths still in their programs are, by definition, not enrolled in education or employed, the analysis seems to provide a lower bound on the NSP's potential short-run effects. The negative, statistically insignificant point estimate does not provide evidence of short-run gains but, given the lower bound interpretation, we cannot be conclusive.

Models (5) and (6) are related to criminal behaviour. Previous studies found that, although the effects did not persist, youth programs in a residential setting reduced crimes during the treatment period (e.g., Sochet et al., 2001; Sochet et al., 2008; Millenky et al., 2011). To shed light on potential short-run reductions in criminal activity, we estimate the effects on crime rates only during the first year after the consultation date. The positive (but statistically insignificant) point estimate of 0.11 suggests that criminal activity was not reduced during the treatment period. If anything, a comparison with the estimated effect of 0.16 on criminal activity in the whole period after the consultation date indicates that the positive impact of assignment to the NSP on crime rates increases over time. We investigate these timing issues further in Section 6. The impact on crime rates may be driven by effects on both the number of criminally active youths and the number of crimes among those already criminally active. The strong effect among the group of suspected individuals at the start of the program suggests that the latter is most prominent. We estimate the effect on a dummy variable for being suspected of a crime after the consultation date and find an insignificant point estimate close to zero. This finding implies that assignment to the NSP does not increase the number of criminally active youths and supports our finding that the overall increase in crime rates is predominantly caused by an increase in crimes among those who were already criminally active.

6. Mechanisms

Our estimation results indicate that assignment to the NSP increases criminal activity, especially for the subpopulation of suspected individuals. We now examine two mechanisms that may explain our findings. First, we try to distinguish between criminal behaviour during program participation and after leaving the program. In our main analyses, we estimate the effects on crime rates after the consultation date, which includes both the active treatment period and the period after leaving the program. Criminal activity can differ between these periods. During the treatment criminal behaviour can, for example, be hampered by the supervision of the coaches or the program's requirements. This can also lead to postponed criminal activity after leaving the program. On the other hand, if youths feel protected by their coaches when they get into trouble, they may be less deterred by potential sanctions and more inclined to risky criminal behaviour. A distinction in the crime rates provides insight into the timing of criminal activities.

Second, we investigate whether peer group effects can explain our findings. Within each of the two neighbourhood schools, around 100 individuals are treated simultaneously. This can provide the opportunity to learn from each other or to motivate each other with respect to criminal activities. If individuals are influenced by their peer group, social interaction between youths within the neighbourhood school can affect criminal outcomes. Previous studies point out the importance of peer group effects. Glaeser (1996) finds evidence for a large amount of peer interactions in criminal behaviour. The author concludes that social interaction is an important factor in explaining the variation in crime rates across US cities. Dishion et al. (1999) conclude that interventions for at-risk adolescents delivered in peer groups can reinforce delinquency and other kinds of problem behaviour. High-risk youths seem particularly vulnerable to peer group pressure. Dodge et al. (2007) review the literature on deviant peer influences and state that group-based interventions can increase problem behaviour, since participants can learn from interaction with their deviant peers.

To investigate these issues further, we have to restrict our estimation sample to youths who actually started in a program. After all, for non-starters it is not possible to distinguish between the program and post-program period or to investigate the influence of program peers. The restricted sample of starters consists of 217 individuals, of whom 100 participated in the NSP and 117 in one of the regular reintegration programs. Table A.4 in the Appendix shows the descriptive statistics of this selective subpopulation of starters. Both groups within this sample are well comparable on most observable characteristics, such as gender, educational attainment and criminal activity. We only observe a significant difference with respect to age: Starters in the NSP are younger than starters in a regular program.[19]

[19] Table A.5 additionally reports the OLS estimates of the effect of starting in the NSP compared to starting in a regular program on educational/labour market position and crime rate. As discussed in Section 4, the estimated impact of assignment to the NSP captures both the effect of participation in the NSP and the effect of a difference in starting probabilities. We cannot causally identify the effect of participation in the NSP, but the estimated insignificant OLS effects on school enrolment or employment, which are close to zero, suggest that starting in the NSP does not increase school enrolment or employment probabilities compared to starting in a regular program.

Table 7 presents the average crime rates during and after the program for both groups of starters, with standard deviations in parentheses.[20] The crime rates are roughly equal during the program, but after the program we observe a remarkable difference: Crime rates in the NSP stay at a comparable level, whereas crime rates in the regular programs decrease.[21] This pattern does not suggest a reduction in crime rates during the program or the postponement of criminal activities.

Table 7. Crime rates during and after program participation

	starters in the NSP	starters in the control group
Crime rate during the program	0.44	0.41
	(0.12)	(0.16)
Crime rate after leaving the program	0.38	0.21
	(0.13)	(0.04)
Number of observations	100	117

Notes. The standard deviations of the average crime rates are in parentheses.

Table 8 presents the OLS estimation results of models in which the crime rate during the program (left panel) and after the program (right panel) are regressed on a dummy variable for starting in the NSP and all the covariates. Since the impact of assignment to the NSP on criminal activity is mainly driven by the group of suspected individuals, we also perform our analyses on this subpopulation. Columns (1) and (2) show the estimated effects on crime rates during the program for the full sample and the subpopulation of suspected criminals. Similarly, columns (3) and (4) present the estimated effects on the post-program crime rates for both samples.

Whereas we find no significant effects on the crime rate during the program, we find starting in the NSP has a positive and statistically significant effect at the 1% level on the crime rate after program exit. In the complete sample the estimated effect is 0.25. When restricting the sample to suspected individuals, we find an even larger effect, 0.43. These findings suggest that the positive effect of

[20] This analysis used the actual start date to construct program duration rather than the consultation date. There can be slight differences between both dates, since it takes some time before an individual actually starts the program to which he or she is referred. Our main analyses use the consultation date, since non-starters do not have a start date.
[21] The differences in crime rates presented in Table 7 are not statistically significant. The *p*-value of the difference in crime rates after leaving the program equals 0.21.

assignment to the NSP on criminal activity is mostly driven by differences in post-program criminal behaviour. Whereas we observe a drop in criminal activity after an individual leaves a regular reintegration program, crime rates for starters in the NSP stay roughly at the same level. This finding is in line with the increasing impact of the NSP on crime rates over time (see Table 6, column (5) and Table 4, column (6)).

Table 8. Impact of starting in the NSP (compared to starting in a control program) on the crime rates during and after the program

	Effects on crime rate during program		Effects on crime rate after the program	
	(1) Complete sample	(2) Subsample of suspected individuals	(3) Complete sample	(4) Subsample of suspected individuals
OLS				
Starting in the NSP	-0.065	-0.132	0.254***	0.426***
	(0.116)	(0.268)	(0.050)	(0.127)
Controls				
SES	yes	yes	yes	yes
Educational level	yes	yes	yes	yes
Pre-treatment crime	yes	yes	yes	yes
Polynomial control for month of entry	cubic	cubic	cubic	cubic
Observations	217	121	217	121

Notes. Robust standard errors are in parentheses. Asterisks indicate that the estimates are statistically significant at the *** 1% level. The SES control variables include gender, age, age squared and country of birth. The educational level controls include the latest educational position and the highest completed education level. Pre-treatment crime refers to a variable indicating the yearly average number of crimes an individual is suspected of during the eight-year period before the consultation date.

Potentially positive influences of program employees or coaches may be mitigated in the NSP by peer group effects. To investigate the existence of peer effects, we try to exploit the variation in pre-treatment criminal activity across the two locations of the NSP. Individuals assigned to the program are referred to the location in either the north or south of Rotterdam, based on place of residence. Since interaction takes place within a location, we consider the subsample of youths sent to the location we call North during our assignment period to be a good representation of the relevant peer group of each individual sent to this site. Similarly, we use the subsample of youths sent to the location we call South as the relevant peer group of each individual sent to this site. We remove the four individuals who were assigned twice from our sample, since they were referred to the NSP after the assignment period and started the program later. This leaves us with 96 starters during the assignment period. Table 9 presents the average crime rates in the eight-year period before the consultation date, with standard deviations in parentheses. For the location South, the average crime rate (0.36) is 29% higher compared to that at North (0.28).[22] Hence, an individual referred to location South is likely to face a relatively more criminally active peer group compared to an individual sent to location North.[23]

Table 9. Pre-treatment criminal activity within locations

	Location North	Location South
Crime rate (8-year period)	0.28	0.36
	(0.07)	(0.08)
Number of observations	42	54

Notes. The standard deviations of the average pre-treatment crime rates are in parentheses.

We now examine the existence of crime-related peer effects by estimating the effect of the average pre-treatment crime rate in the peer group on individual crime rate after the consultation date, conditional on individual pre-treatment crime rate and all the other covariates. The left panel of Table 10 shows the OLS estimates of the effects of both individual and average pre-treatment crime rates.[24] Model (1) includes all 96 starters in the NSP; model (2) restricts the sample to the subpopulation of suspected individuals. If peer effects related to criminal behaviour exist, we expect to find positive effects for the average crime rate, especially in the subsample of suspected individuals. Table 10 shows that the point estimates of the average pre-treatment crime rate are positive.

When we only take into account suspected individuals, we find much larger and statistically significant estimates. This confirms our conjecture and may provide evidence for the existence of crime-related peer effects. Nevertheless, since we only use variation between locations, we cannot rule out that our measure for peer effects picks up other unobservable differences between both locations. If our findings truly reveal crime-related peer effects, we expect our measure to be less clearly correlated with educational and/or labour market outcomes. If they reflect other differences, it seems more plausible that our measure is correlated to other outcomes as well.

[22] This difference is not statistically significant. The *p*-value of the difference in crime rate equals 0.45.
[23] We also compared the fraction of suspected individuals at the start of the program, which turns out to be similar in both sites. Hence, the difference in pre-treatment criminal activity is caused by higher crime rates among suspected individuals in location South.
[24] The presented models control for all covariates, including a third-order polynomial of the time of entry. Models including a linear function of the time of entry yield similar results.

Table 10. Impact of the average crime rate on individual outcomes (starters in the NSP)

	Effects on criminal activity		Effects on educational and/or labour market position	
	(1) Complete sample	(2) Subsample of suspected individuals	(3) Complete sample	(4) Subsample of suspected individuals
OLS				
Individual crime rate	0.804***	0.915***	-0.273***	-0.216*
	(0.118)	(0.176)	(0.098)	(0.125)
Average crime rate in peer group	1.563	5.089**	0.639	-0.729
	(1.457)	(2.453)	(1.205)	(1.741)
Controls				
SES	yes	yes	yes	yes
Educational level	yes	yes	yes	yes
Pre-treatment crime	yes	yes	yes	yes
Polynomial control for month of entry	cubic	cubic	cubic	cubic
Observations	96	54	96	54

Notes. Standard errors are in parentheses. Asterisks indicate that the estimates are statistically significant at the *** 1% level, **5% level and *10% level. The SES control variables include gender, age, age squared, and country of birth. The educational level controls include the latest educational position and the highest completed education level. Pre-treatment crime refers to a variable indicating the yearly average number of crimes an individual is suspected of during the eight-year period before the consultation date.

The right panel (models (3) and (4)) presents the estimation results of equivalent OLS models, in which a dummy variable for educational and/or labour market position is used as the outcome variable. We find insignificant effects of the average pre-treatment crime rate on educational and/or labour market position. Model (3) including all starters yields a positive point estimate, whereas model (4) including only suspected individuals yields a negative point estimate. Hence, the average pre-treatment crime rate seems to be related to individual criminal activity but not to individual success with respect to education or work.

Obviously, we cannot causally claim the existence of peer effects. The youths were not assigned randomly to the sites and we still cannot fully rule out the possibility that the estimated effects of average pre-treatment crime rates reflect other differences between the two locations. Nevertheless, our results are consistent with crime-related peer effects and we interpret them as suggestive evidence that peer effects (partly) explain the positive effect of assignment to the NSP on criminal activity.

7. Conclusions

The NSP aims to increase school enrolment and employment among multi-problem school dropouts. Previous studies provide mixed evidence on the success of training programs for disadvantaged youths. It seems therefore difficult to effectively serve the vulnerable target group of at-risk adolescents and realize persistent social gains. The NSP is largely designed in line with lessons from the literature and shares several components with other promising programs. It offers an intensive and integrated approach of educational and work services combined with professional care and personal mentoring. We evaluate the effects of the program by implementing a specific assignment rule that ensures that treatment status is determined by an individual's application date. The effects of assignment to the NSP are estimated three years after the start of the program.

We find no evidence that assignment to the NSP improves school enrolment or employment probabilities compared to standard intervention. This finding is in line with a large body of the literature that shows no impact of training programs for at-risk youths on labour market outcomes (e.g., Carneiro and Heckman, 2003; LaLonde, 2003). In addition, we find evidence that assignment to the NSP increases criminal activity compared to standard intervention, especially among the subpopulation of youths suspected of a crime at the time of entry. This result is consistent with previous studies that document adverse effects of group counselling or group-based interventions on criminal activity (Dishion et al., 1999). Deviant peer effects caused by grouping at-risk adolescents together can explain the reinforcement of criminal behaviour (Dodge et al., 2007). Additional analyses provide suggestive evidence in line with this explanation. Hence, adverse peer effects due to placing at-risk youths together may have negated other promising elements of the NSP.

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Appendix

Table A.1. Correlation table

	Male	Age	Low education level	Pre-treatment crime rate (8-year period)	Post-treatment crime rate	Educational/labour market position
Male	1.000					
Age	-0.138	1.000				
Low education level	0.115	-0.073	1.000			
Pre-treatment crime rate	0.308	0.001	0.157	1.000		
Post-treatment crime rate	0.287	-0.043	0.085	0.508	1.000	
Educational/labour market position	-0.174	-0.057	-0.134	-0.221	-0.209	1.000

*Notes.*Youths with a low education level have primary education as their highest completed education level. The pre-treatment crime rate is the average number of crimes an individual is suspected of in the eight-year period before consultation at the YPO. The post-treatment crime rate is the average number of crimes an individual is suspected of during the period between consultation at the YPO and 1 November 2012.

Table A.2. OLS and IV estimates of the effects on crime rates (all models including a cubic polynomial of the month of entry)

	(1)	(2)	(3)	(4)
OLS				
Assignment to the NSP	0.145*	0.143**	0.139**	0.101
	(0.081)	(0.068)	(0.065)	(0.061)
IV (second stage)				
Assignment to the NSP	0.155	0.179	0.193*	0.163*
	(0.137)	(0.115)	(0.106)	(0.090)
Controls				
SES	no	yes	yes	yes
Educational level	no	no	yes	yes
Pre-treatment crime	no	no	no	yes
Polynomial control for month of entry	cubic	cubic	cubic	cubic
Observations	383	383	383	383

Notes. Robust standard errors are in parentheses. Asterisks indicate that the estimates are statistically significant at the ** 5% level and *10% level. The SES control variables include gender, age, age squared, and country of birth. The educational level controls include the latest educational position and the highest completed education level. Pre-treatment crime refers to a variable indicating the yearly average number of crimes an individual is suspected of during the eight-year period before the consultation date.

Table A.3. Subpopulation of suspected individuals: OLS and IV estimates of the effects on crime rates

	(1)	(2)	(3)	(4)	(5)	(6)
OLS						
Assignment to the NSP	0.113 (0.122)	0.170* (0.093)	0.185* (0.092)	0.122 (0.085)	0.202** (0.089)	0.201** (0.089)
IV (second stage)						
Assignment to the NSP	0.012 (0.179)	0.139 (0.145)	0.221* (0.114)	0.187* (0.101)	0.355** (0.128)	0.355** (0.127)
Controls						
SES	no	yes	yes	yes	yes	yes
Educational level	no	no	yes	yes	yes	yes
Pre-treatment crime	no	no	no	yes	yes	yes
Polynomial control for month of entry	linear	linear	linear	linear	quadratic	cubic
Observations	199	199	199	199	199	199

Notes. Robust standard errors are in parentheses. Asterisks indicate that the estimates are statistically significant at the ** 5% level and *10% level. The SES control variables include gender, age, age squared, and country of birth. The educational level controls include the latest educational position and the highest completed education level. Pre-treatment crime refers to a variable indicating the yearly average number of crimes an individual is suspected of during the eight-year period before the consultation date.

Table A.4. Descriptive statistics: Comparison of starters in the NSP and regular programs

	Started in the NSP	Started in a regular program	*p*-Value
<u>Covariates</u>			
Gender (male = 1)	0.65	0.56	0.20
Age	20.20	20.75	0.01
Country of birth			0.80
The Netherlands	0.74	0.68	
Morocco	0.06	0.05	
The Antilles	0.08	0.12	
Surinam	0.06	0.06	
Other	0.06	0.09	
Latest educational position			0.29
Primary education	0.00	0.00	
Practical education	0.02	0.00	
Special education	0.04	0.04	
Pre-vocational secondary education	0.04	0.07	
Intermediate vocational education (level 1)	0.30	0.19	
Intermediate vocational education (level 2)	0.45	0.44	
Intermediate vocational education (level 3)	0.07	0.13	
Intermediate vocational education (level 4)	0.05	0.09	
Higher secondary education	0.00	0.01	
Unknown	0.03	0.03	
Highest completed education level			0.45
Primary education	0.77	0.64	
Pre-vocational secondary education	0.10	0.18	
Intermediate vocational education (level 1)	0.05	0.07	
Intermediate vocational education (level 2)	0.05	0.06	
Intermediate vocational education (level 3)	0.00	0.00	
Intermediate vocational education (level 4)	0.00	0.01	
Higher secondary education	0.00	0.01	
Unknown	0.03	0.03	
Pre-treatment crime rate (2-year period)	0.44	0.42	0.92
Pre-treatment crime rate (4-year period)	0.42	0.40	0.92
Pre-treatment crime rate (8-year period)	0.35	0.29	0.47
<u>Outcome variables</u>			
Educational and/or labour market position	0.33	0.33	0.96
Crime rate (after consultation)	0.35	0.27	0.42
Total number of observations	117	100	

Table A.5. OLS estimates of the impact of starting in the NSP (compared to starting in a regular program) on educational/labour market position and the crime rate (subsample of starters)

	(1)	(2)	(3)	(4)	(5)	(6)
	Effect on educational/labour market position					
OLS						
Starting in the NSP	0.011	0.002	0.011	0.011	0.014	0.014
	(0.110)	(0.087)	(0.087)	(0.089)	(0.088)	(0.088)
	Effect on crime rate					
OLS						
Starting in the NSP	0.028	0.022	0.031	0.033	0.080	0.079
	(0.101)	(0.081)	(0.075)	(0.062)	(0.058)	(0.056)
Controls						
SES	no	yes	yes	yes	yes	yes
Educational level	no	no	yes	yes	yes	yes
Number of crimes	no	no	no	yes	yes	yes
Polynomial control for month of entry	linear	linear	linear	linear	quadratic	cubic
Observations	217	217	217	217	217	217

Notes. Robust standard errors are in parentheses. The SES control variables include gender, age, age squared ,and country of birth. The educational level controls include the latest educational position and the highest completed education level. Pretreatment crime refers to a variable indicating the yearly average number of crimes an individual is suspected of during the eight-year period before the consultation date.

Publisher:

CPB Netherlands Bureau for Economic Policy Analysis
P.O. Box 80510 | 2508 GM The Hague
T (070) 3383 380

April 2013 | ISBN 978-90-5833-595-1