

CPB Document

No 177

December, 2008

Did the 2006 covenant program reduce school dropout in the Netherlands?

Marc van der Steeg, Roel van Elk and Dinand Webbink

CPB Netherlands Bureau for Economic Policy Analysis
Van Stolkweg 14
P.O. Box 80510
2508 GM The Hague, the Netherlands

Telephone +31 70 338 33 80
Telefax +31 70 338 33 50
Internet www.cpb.nl

ISBN 978-90-5833-388-9

Abstract in English

Early school-leaving is considered to be one of the major problems in Dutch education. In order to reduce the number of dropouts in the school year 2006-2007 the Dutch government has offered a financial incentive scheme to 14 out of 39 regions. This scheme provides a reward of 2000 euro per school dropout less in 2006-07. The target of the scheme was a reduction of the total number of school dropouts by at least 10 percent in one year. This paper evaluates the effectiveness of this school dropout policy by comparing the change in school dropout in these 14 regions with the change in the remaining 25 regions before and after the introduction of the policy. We observe a modest decline in the probability of dropping out in the 14 covenant regions. However, the decline in the non-covenant regions was equally large. We therefore find no significant effect on the probability of dropping out in the post-covenant year. In both regions, the number of dropouts has fallen by 3 percent in the year after the covenants. This nationwide decline can be largely assigned to changes in the student populations among the pre- and post-covenant year.

The covenants also gave a reward to regions for a successful reintegration of dropouts in order to reduce school dropout in that way. However, estimates for the effect on the re-enrolment of previous dropouts are statistically insignificant as well. We conclude that 2006 covenant policy has not been effective in reducing early school-leaving.

Key words: school dropout, financial incentives, policy evaluation

Abstract in Dutch

Voortijdig schoolverlaten wordt beschouwd als een van de grootste problemen in het huidige Nederlandse onderwijs. Om voortijdig schoolverlaten tegen te gaan, heeft het ministerie van OCW in 2006 convenanten afgesloten met 14 van de 39 RMC-regio's die verantwoordelijk zijn voor de registratie en regionale bestrijding van voortijdig schoolverlaten. De convenanten geven aan de regio's een financiële prikkel van 2000 euro per voortijdig schoolverlater minder in het schooljaar 2006-07. Doelstelling van de convenanten uit 2006 was een afname van het totale aantal voortijdig schoolverlaters met ten minste 10 procent in 1 jaar. Deze studie evalueert de effectiviteit van dit beleid door de verandering in voortijdig schoolverlaten in de geselecteerde convenantsregio's te vergelijken met de verandering in de andere regio's voor en na invoering van het beleid. Er is weliswaar sprake van een afname in de kans op voortijdig schoolverlaten in de 14 convenantsregio's in 2006-07, maar de afname in de niet-convenantsregio's was even groot. Het convenantenbeleid van 2006 heeft daarom geen significant effect op de kans op voortijdig schoolverlaten in het jaar waarop de afspraken betrekking hadden (2006-2007). In beide typen regio's is het aantal nieuwe voortijdig schoolverlaters gedaald met drie procent. De

afname in voortijdig schoolverlaten kan grotendeels worden verklaard door veranderingen in de leerlingenpopulaties.

De convenanten uit 2006 boden ook een beloning voor het succesvol 're-integreren' van eerdere voortijdig schoolverlaters om ook via die weg het aantal drop-outs te verminderen. De convenanten hebben echter niet geleid tot een significant grotere kans op terugkeer van eerdere voortijdig schoolverlaters in het onderwijs.

Op basis van deze analyses concluderen we dat de eerste ronde van het convenantenbeleid van 2006 niet effectief is geweest in het bestrijden van voortijdig schoolverlaten.

Steekwoorden: voortijdig schoolverlaten, financiële prikkels, beleidsevaluatie

Een uitgebreide Nederlandse samenvatting is beschikbaar via www.cpb.nl.

Contents

Preface	7
Summary	9
1 Introduction	13
2 The school dropout covenant scheme	19
3 Concerns about previous findings	23
4 Empirical strategy	27
5 Data	31
5.1 Summary statistics full sample	32
5.2 Summary statistics discontinuity and matched sub-samples	33
6 Main estimation results	35
6.1 Effect on probability of dropping out in post-covenant year 2006-2007	35
6.2 Effect on return of past dropouts in education	39
7 Conclusions and discussion	43
Literature	45
Appendix A: Selection of covenant regions	46
Appendix B: Descriptive statistics by RMC region	47
Appendix C: regression outcomes	48
Appendix D: sensitivity analyses	50

Preface

School dropout is generally considered to be one of the major problems in Dutch education. Concerns about school dropout are based on potential adverse consequences with respect to perspectives on the labour market and participation in society. This study aims to evaluate one of the important pillars of recent Dutch dropout reduction policy, notably regional covenant arrangements. These covenants provide financial incentives to regions to reduce school dropout. The Dutch Ministry of Education started the covenant policy in 2006 with a subset of regions which had the largest number of dropouts. New covenants with a somewhat different setup have been signed with all regions in late 2007 and early 2008. This study evaluates the 2006 covenants.

The document has been written by Marc van der Steeg, Roel van Elk and Dinand Webbink. First of all, the authors are grateful for the help of Erik Smits and Erik Fleur from CFI for delivering the BRON data on school dropout and explaining the details of the dataset. Further, we thank Cees-Jan van Overveld and Coen de Jong from the Ministry of Education for their help with delivering information on the setup of the covenants and the evolution of school dropout. The study also gained from the valuable discussions we had with Fred Voncken (project director on school dropout), Kasper Weekenborg and Sipke Boorsma from the Ministry of Education. Finally, we would like to thank Pierre Koning, Debby Lanser, Free Huizinga and George Gelauff for their valuable comments on earlier versions of this document.

Coen Teulings

Director of CPB Netherlands Bureau for Economic Policy Analysis

Summary

The problem of school dropout is currently high on the policy agenda in the Netherlands. In the Netherlands dropouts are defined as pupils aged below 23 that leave school before having reached a certain level of education, called the 'start qualification'.¹ This start qualification is considered to be the minimum level of education needed to participate well in the labour market. The Dutch government has committed itself to the Lisbon goal of a halving of school dropout between 2000 and 2010. To attain this goal, various new measures have been introduced and budgets for existing measures have been raised over the last couple of years.

Covenants with regions considered important new dropout policy tool

An important component of the recent Dutch dropout reduction policy is the introduction of dropout covenants in the summer of 2006. These covenants were offered to 14 out of 39 so-called RMC regions.² These 14 regions had the highest number of dropouts and were responsible for nearly two-thirds of all dropouts in the Netherlands.

The covenants consist of a financial incentive to these regions to reduce the number of dropouts. The Ministry of Education offered the contact municipalities of these regions 2000 euros for each early school-leaver less in 2006-2007, compared to 2004-2005. This monetary reward scheme stops if a reduction of 10 per cent is realized; any reduction above 10 per cent is not rewarded. The goal of the covenants, as formulated by the Ministry, is to reduce the total (i.e. old and new) number of dropouts in these regions by at least 10 percent in one year. The total government budget for the covenants was 16 million euros.

RMC figures: fewer dropouts after introduction of covenants

RMC figures show that the number of dropouts declined in the year after the covenants were signed. For instance, considering new dropouts, RMC figures show that the covenant regions registered 10 per cent less *new* dropouts in 2006/07 relative to the chosen reference year 2004/05, whereas the 25 remaining non-covenant regions have witnessed an increase of 18 per cent over the same period (cf. Ministry of Education, 2008b). These figures are based on registrations of dropouts which are produced by the regions themselves. These figures suggest substantial effects of the covenants. Based on these figures, the Ministry of Education has concluded in several publications that the dropout covenants have been effective (see e.g. Ministry of Education, 2008b, 2008c).

¹ A start qualification is defined as being graduated from havo, vwo or at least level two of mbo. Both havo and vwo are general secondary education and last five and six years, respectively. Mbo is secondary vocational education and consists of four levels.

² RMC stands for 'Regionale Meld- en Coördinatiefunctie'.

In response to these figures, the Ministry of Education has decided to scale up the intervention and sign new 4-year covenants with all 39 RMC regions in the Netherlands.³ It should be noted that these new covenants differ from the first round of covenants. The most important differences are: (1) the financial incentive is now provided directly to the schools rather than to the RMC regions; (2) the focus of the new covenants lies on prevention of school dropout (3) the dataset used to determine the results is more reliable.⁴ The aim of this new round of covenants is to reduce the total number of yearly dropouts by 40 percent in 2011 (relative to 2006). Total costs of this new round of covenants may amount to 117 million euros (cf. Ministry of Education, 2008d).

Research question: have the 2006 covenants been effective?

Although the RMC figures on the development of the number of dropouts suggest a positive effect of the new policy, there are some serious concerns with these findings. First, and most important, the registrations of the number of dropouts carried out by the regions appear to suffer from serious reliability and consistency problems (Deloitte, 2006; Ministry of Education, 2008b). Second, looking at changes in the total number of school dropouts can be misleading as total numbers might change due to changes in the size or composition of school populations among regions. For instance, a reduction of the size of the population might lead to a lower total number of dropouts. Third, the choice of the reference year, that is 2004-2005 instead of the pre-treatment year 2005-06, seems to give an upward bias to the results.

This paper aims to evaluate the effectiveness of the covenant policy by addressing these issues. In our analysis we focus on changes in the *probability* of dropping out rather than changes in the absolute *number* of dropouts. Further, we use a different data source that is expected to suffer substantially less from measurement error. These data, that recently have become available, include all students of the relevant school populations under consideration. Moreover, they contain information on the education position and several background characteristics of these students. Finally, we use the pre-treatment year 2005-06 as the reference year rather than 2004-05.

Difference-in-differences and regression discontinuity estimation approaches

The introduction of the covenants in 14 out of 39 regions offers a special opportunity for evaluation. We exploit this opportunity by using a difference-in-differences (DD) approach and a regression discontinuity approach.

The difference-in-differences approach compares the dropout probability before and after the introduction of the covenant policy. The first difference is the change in the drop out probability in the covenant regions. This difference might be the result of the covenant policy

³ These covenants have been signed at the end of 2007 and in the first half of 2008.

⁴ For a complete description of the setup of the new covenants, see Ministry of Education (2008d)

but might also be the result of other factors that have changed in the same period. To control for these other factors we use a second difference, which is the change in performance in the other (non-covenant) regions.

The second approach aims to improve the first analysis by focusing on covenant and non-covenant regions that are more similar. We construct samples of comparable regions by exploiting the selection rule of the covenant regions. The covenant regions have been selected on the basis of presence in the top 10 of either the number of new dropouts or the total number of dropouts. We construct discontinuity samples of covenant regions that have just been selected for treatment, and non-covenant regions that have just-not been selected. In addition, we match covenant regions with non-covenant regions based on the dropout probability in the year before the introduction of the new policy.

The same discontinuity and matched samples are used to estimate the effects of covenants on the probability of return of previous dropouts into education.

Main findings: 2006 covenants did not reduce school dropout

Our main findings are twofold. First, we find no evidence for a significant effect of the 2006 covenants on the probability of dropping out in the target year 2006-2007. Where the covenant regions witnessed a modest decline in the probability of dropping out, the non-covenant regions witnessed a similar decline. The decline in the number of dropouts in 2006-07 amounted to three percent in both type of regions. This nationwide decline in school dropout is statistically insignificant and can be largely assigned to changes in the characteristics of the student population.

Second, our estimates suggest that the effects on the probability of return of past dropouts into education are not significantly different from zero either. These results are robust for a variety of specifications and robustness checks.

The overall conclusion is that the 2006 dropout covenant scheme in the 14 regions has not been effective in reducing school dropout.

Regions could also obtain positive results under the covenant scheme by guiding dropouts towards work or care. The effect of the covenants on these outcomes could not be investigated because these outcomes can not be observed in our data. Other data on these outcomes which are produced by the regions themselves suffer from serious reliability and consistency problems.

It should be noted, moreover, that placing someone outside education does not lead to a start qualification (i.e. to less school dropout), at least not in the short term.⁵

Effectiveness of new 2007-2011 covenants

As mentioned earlier, the Ministry of Education has negotiated new covenants with all 39 regions between the end of 2007 and half of 2008. It is difficult to predict whether these new covenants do succeed in reducing school dropout. However, both the new and the old covenants carry elements that seem to provide sub-optimal incentives to reduce dropout and that can be improved with the use of the BRON data.

First, the choice of the reference year against which results are measured is chosen at least one year before the moment of signing the covenants. This leads to a situation in which some regions (or schools) already have reached the covenant target at the moment the covenant is signed, whereas other regions (schools) start with a (strong) negative result. For example, there is one school that already looked forward to a reward of 1.5 million euro at the moment of signing the new covenant, because they witnessed a decline in the number of dropouts in the period before the covenants were signed.⁶

Second, the reward depends on the reduction in the *number* of dropouts rather than the *probability* of dropping out. Exogenous changes in the size or composition of student populations of a region (or school) may therefore affect the size of the reward, irrespective of whether that particular region (or school) has become more successful in reducing dropout.⁷

⁵ It is possible that a dropout that is being guided towards work or care through an intervention by the RMC region returns to school after some time (or follows a so-called EVC-traject) and eventually obtains a start qualification. However, it is unclear if this actually happens in a lot of cases, and if so, at what term. Note that a dropout who starts to work as a stockboy in a grocery store for example, may already be considered a dropout guided to work.

⁶ This is under the assumption that this school manages to keep the number of dropouts at their 2006-07 level. On the other hand, there are also schools that miss out on a reward because they witnessed an increase in the number of dropouts in the year(s) before they signed the covenants. The amount a school may miss out on may go up to 1.1 million euro.

⁷ The new covenants only apply a correction factor if the (percentage) change in the size of the student population level relative to the reference year exceeds a certain level. This percentage is set at 30 percent for the first year of the covenants. This implies that the expected growth in the number of school dropouts for a school whose population grows by say 25 per cent is not taken into account when determining the size of the reward. On the other hand, a school whose population shrinks may benefit from this setup. The same applies for a school that has less students in the school types in which school dropout is more prominent (e.g. mbo 1). Note that the total number of students in mbo 1 education (with an average dropout probability of nearly 40 per cent) has declined by 7.5 per cent between 2005/06 and 2006/07.

1 Introduction

School dropout considered major problem in Dutch education

School dropout (or early school-leaving) is generally considered one of the major problems of current Dutch education. This concern is based on potential adverse consequences of dropout for chances on the labour market or participation in society. Dropout may be a source of various kinds of exclusion from society, for example in terms of unemployment, social exclusion or participation in criminal activity. Therefore, apart from private costs in terms of lower expected income, social costs associated with school dropout can be large.⁸

Dutch dropout policy intensified

The problem of school dropout is currently high on the policy agenda in the Netherlands. In the Netherlands dropouts are defined as pupils aged below 23 that leave school before having reached a certain level of education, called the ‘start qualification’.⁹ This start qualification is considered to be the minimum level of education needed to participate well in the labour market. The Dutch government has committed itself to the Lisbon goal of a halving of school dropout between 2000 and 2010. To attain this goal, various new measures have been introduced and budgets for existing measures have been raised over the last couple of years.

Examples of new measures included in the recently introduced national dropout reduction policy agenda named “Aanval op de uitval” are, among others, compulsory participation in education for youth aged between 16-18 without a start qualification, certification courses for 18-23 year olds in which competences gained through work experience are acknowledged, and investments in a better registration system of dropouts for policy evaluation and adjustment (see Ministry of Education, 2008a).

Covenants with regions considered important new dropout policy tool

An important component of the recent Dutch dropout reduction policy is the introduction of dropout covenants in the summer of 2006. These covenants were offered to 14 out of 39 so-called RMC regions. These 14 regions had the highest number of dropouts and were responsible for nearly two-thirds of all dropouts in the Netherlands.

The covenants consist of a financial incentive to these regions to reduce the number of dropouts. The Ministry of Education offered the contact municipalities of these regions 2000 euros for each reduced early school-leaver in 2006-2007, compared to 2004-2005. This monetary reward scheme stops if a reduction of 10 per cent is realized; any reduction above 10 per cent is not rewarded. The goal of the covenants, as formulated by the Ministry, is to reduce

⁸ See for example Lochner and Moretti (2004) who provide some evidence for the causal relationship between schooling and crime.

⁹ A start qualification is defined as being graduated from havo, vwo or at least level two of mbo. Both havo and vwo are general secondary education and last five and six years, respectively. Mbo is secondary vocational education and consists of four levels.

the total (i.e. old and new) number of dropouts in these regions by at least 10 percent in one year. The total government budget for the 2006 covenants was 16 million euros.

RMC figures: fewer dropouts after introduction of dropout covenants

RMC figures show that the number of dropouts declined in the year after the covenants were signed. For instance, considering new dropouts, RMC figures show that the covenant regions registered 10 per cent less *new* dropouts in 2006/07 relative to the chosen reference year 2004/05, whereas the 25 remaining non-covenant regions witnessed an increase of 18 per cent over the same period (cf. Ministry of Education, 2008b). These figures are based on registrations of dropouts which are produced by the regions themselves. These figures suggest substantial effects of the covenants. Based on these figures, the Ministry of Education has concluded in several publications that the dropout covenants have been effective (see e.g. Ministry of Education, 2008b, 2008c).

Covenant policy scaled up to all 39 regions in 2008

In response to these figures, the Ministry of Education has decided to scale up the covenant policy and sign new 4-year covenants with all 39 RMC regions in the Netherlands.¹⁰ It should be noted that these new covenants differ from the first round of covenants. The most important differences are: (1) the financial incentive is now provided directly to the schools rather than to the RMC regions; (2) the focus of the new covenants lies on preventive policies (3) the dataset used to determine the results is more reliable; (4) vmbo schools started to participate as well in the new covenants.¹¹ The aim of this new round of covenants is to reduce the total number of yearly dropouts by 40 percent in 2011, relative to the reference year 2005-06. Total costs of this new round of covenants may amount to 117 million euros (cf. Ministry of Education, 2008d).

Research question: have the covenants been effective?

Although the RMC figures on the development of the number of dropouts suggest a positive effect of the new policy, there are some serious concerns with these findings. First, and most important, the registrations of the number of dropouts carried out by the regions appear to suffer from serious reliability and consistency problems (Deloitte, 2006; Ministry of Education, 2008b). Second, looking at changes in the total number of school dropouts can be misleading as total numbers might change due to changes in the size or composition of school populations among regions. For instance, a reduction of the size of the population might lead to a lower total number of dropouts. Third, the choice of the reference year, that is 2004-2005 instead of the pre-treatment year 2005-06, seems to give an upward bias to the results.

¹⁰ These covenants have been signed between the end of 2007 and mid 2008.

¹¹ For a complete description of the setup of the new covenants, see Ministry of Education (2008d).

This paper aims to evaluate the effectiveness of the covenant policy by addressing these issues. In our analysis we focus on changes in the *probability* of dropping out rather than changes in the absolute *number* of dropouts. Further, we use a different data source that is expected to suffer substantially less from measurement error. These data, that recently have become available, include all students of the relevant school populations under consideration. Moreover, they contain information on the education position and several background characteristics of these students. Finally, we use the pre-treatment year 2005-06 as the reference year rather than 2004-05.

Difference-in-differences and regression discontinuity estimation approaches

The introduction of the covenants in 14 out of 39 regions offers a special opportunity for evaluation. We exploit this opportunity by using a difference-in-differences (DD) approach and a regression discontinuity approach.

The difference-in-differences approach compares the dropout probability before and after the introduction of the covenant policy. The first difference is the change in the drop out probability in the covenant regions. This difference might be the result of the covenant policy but might also be the result of other factors that have changed in the same period. To control for these other factors we use a second difference, which is the change in performance after the introduction of the covenant policies in the other (non-covenant) regions.

The second approach aims to improve the first analysis by focusing on covenant and non-covenant regions that are more similar. We construct samples of comparable regions by exploiting the selection rule of the covenant regions. The covenant regions have been selected on the basis of presence in the top 10 of either the number of new dropouts or the total number of dropouts. We construct discontinuity samples of covenant regions that have just been selected for treatment, and non-covenant regions that have just-not been selected. In addition, we match covenant regions with non-covenant regions based on the dropout probability in the year before the introduction of the new policy.

The same discontinuity and matched samples are used to estimate the effects of covenants on the probability of return of previous dropouts into education.

Main findings: 2006 covenants not effective with respect to school dropout

Our main findings are twofold. First of all, we find no evidence for a significant effect of the 2006 covenants on the probability of dropping out in the target year 2006-2007. Whereas the covenant regions witnessed a modest decline in the probability of dropping out, the non-covenant regions witnessed a similar decline. The decrease in the number of dropouts amounted to three percent in both types of regions. This nationwide decline in school dropout is statistically insignificant and can be largely assigned to changes in the characteristics of the student population.

Second, our estimates suggest that the effects on the probability of return of past dropouts into education are statistically insignificant as well. These results are robust for a variety of specifications and robustness checks.

The overall conclusion is that the dropout covenant scheme in the 14 regions has not been effective in reducing school dropout.

Regions could also obtain positive results under the covenant scheme by replacing dropouts towards work or care. The effect of the covenants on these outcomes could not be investigated because these outcomes can not be observed in our data. Other data on these outcomes which are produced by the regions themselves suffer from serious reliability and consistency problems. It should be noted, moreover, that these replacements outside education do not lead to a start qualification (i.e. to less school dropout), at least not in the short term.¹²

Effectiveness of new 2007-2011 covenants

As mentioned earlier, the Ministry of Education has negotiated new covenants with all 39 regions between the end of 2007 and half of 2008. It is difficult to predict whether these new covenants do succeed in reducing school dropout. However, both the new and the old covenants carry elements that seem to provide sub-optimal incentives to reduce dropout and that can be improved with the use of the BRON data.

First, the choice of the reference year against which results are measured is chosen at least one year before the moment of signing the covenants. This leads to a situation in which some regions (or schools) already have reached the covenant target at the moment the covenant is signed, whereas other regions (schools) start with a (strong) negative result. For example, there is one school that already looked forward to a reward of 1.5 million euro at the moment of signing the new covenant, because they witnessed a decline in the number of dropouts in the period before the covenants were signed.¹³

Second, the reward depends on the reduction in the *number* of dropouts rather than the *probability* of dropping out.

¹² It is possible that a dropout that is being replaced towards work or care through an intervention by the RMC region returns to school after some time (or follows a so-called EVC-traject) and eventually obtains a start qualification. However, it is unclear if this actually happens in a lot of cases, and if so, at what term. Note that a dropout who starts to work as a pizza delivery boy may already be considered a replaced dropout towards work, whereas his or her long term labour market perspectives may be relatively weak.

¹³ This is under the assumption that this school manages to keep the number of dropouts at their 2006-07 level. On the other hand, there are also schools that miss out on a reward because they witnessed an increase in the number of dropouts in the year(s) before they signed the covenants. The amount a school may miss out on may go up to 1.1 million euro.

Exogenous changes in the size or composition of student populations of a region (or school) may therefore affect the size of the reward, irrespective of whether that particular region (or school) has become more successful in reducing dropout.¹⁴

Outline of the paper

The remainder of this paper is organized as follows. Section 2 provides more details about the 2006 covenants. Section 3 discusses the concerns with previous findings. Section 4 discusses the empirical strategy for the identification of causal effects. In section 5, the data are described. Section 6 presents the main estimation outcomes. Finally, section 7 concludes.

¹⁴ The new covenants only apply a correction factor if the (percentage) change in the size of the student population level relative to the reference year exceeds a certain level. This percentage is set at 30 percent for the first year of the covenants. This implies that the expected growth in the number of school dropouts for a school whose population grows by say 25 per cent is not taken into account when determining the size of the reward. On the other hand, a school whose population shrinks may benefit from this setup. The same applies for a school that has less students in the school types in which school dropout is more prominent (e.g. mbo 1). Note that the total number of students in mbo 1 education (with an average dropout probability of nearly 40 per cent) has declined by 7.5 per cent between 2005/06 and 2006/07.

2 The school dropout covenant scheme

In the summer of 2006, the Dutch Ministry of Education has signed covenants with the contact municipalities of 14 out of 39 RMC Regions (see box for more information on RMC regions). Directors of the involved schools signed the covenants as well. Selection of the regions was based on presence in either the top 10 ranking of the number of new dropouts or the top 10 ranking of total (i.e. new + old) dropouts in the reference school year 2004-2005.¹⁵

Goal

The goal of the covenants as formulated by the ministry is to reduce the total number of dropouts in those regions with at least 10 percent by the end of school year 2006-2007 (cf. OCW, 2008a).

Type of measures

Effects could be attained both through preventive actions (leading to fewer new dropouts) or through actions targeted at those who had already dropped out from school (lowering the stock of previous dropouts). RMC regions - in cooperation with schools - were free to choose which instruments to use to reduce the total number of dropouts. The Ministry presented a menu of promising options, but it did not impose any specific actions the regions should undertake.

The projects which RMC's and schools were planning to carry out in the covenant year are explicitly mentioned in the covenants.¹⁶ The majority of the measures agreed upon under the covenant scheme have a preventive rather than a curative character.¹⁷ It is however unclear whether most of the actual efforts and investments have gone to preventive or to curative measures as the regular tasks of the RMC regions are of a curative nature.

¹⁵ This resulted in a list of 12 RMC regions. Two other regions, which wanted to join the covenant program and were just outside the top ten lists, were added later. These regions are Centraal en Westelijk Groningen and Zuidoost-Brabant.

¹⁶ It is not clear whether all of these projects are really new or additional projects, that would not have been carried out (on the same scale) in the absence of these covenants. The frequently mentioned vmbo-mbo project, for instance, which aims at promoting a good transition from vmbo to mbo, has been carried out in practically all 39 RMC regions in the covenant year. However, it turns out that all 39 RMC regions have received earmarked extra money in 2006 to carry out this project (source: <http://www.minocw.nl/beroepskolom/553/Maatregelen-aansluiting-vmbo.html#A1600>).

¹⁷ Examples of frequently undertaken preventive measures are projects to promote a good transition from vmbo to mbo, Zorgadviesteams (teams of different actors offering care and guidance at school to students at risk of dropping out), and projects promoting the number and choice process of so-called 'BPV-stageplaatsen' (apprenticeships/internships). We refer to Ministry of Education (2008b) for a complete list of measures undertaken under the covenants in the different covenant regions.

What is an RMC?

In 1994, the government has divided the Netherlands into 39 RMC regions. Each RMC has its own contact municipality.

Tasks

The main legal tasks of an RMC are twofold:

- Registration of all school dropouts in their region, that is, of all youth aged 12-22 having left education without a start-qualification for more than a month;
- Guidance or counselling of school dropouts back to school or to work;
- Participation in and coordination of networks of schools, school attendance officers, and various local and regional youth assistance bodies.

Funding

The contact municipality of each RMC receives funding from the central government to carry out its main tasks. In school year 2006-2007, the direct central government funding amounted to 17.5 mln euros. On average, this corresponds to a little less than half a million euros per RMC region, or 330 euros per (new) school dropout, though these figures vary to a large extent per region. Apart from (direct) central government funding, RMC regions also use other municipal funds to carry out their tasks, for instance so-called “GSB-means” and “GOA-means”. These ‘own’ municipal financial means amounted to 21.5 million euro in 2006-2007 (Research voor Beleid, 2008). A rough estimate of the average yearly amount of money available to RMC’s per new school dropout is a little less than 750 euros. The offered 2000 euro per school dropout less under the covenant incentive scheme is more than 2.5 times this amount.

Financial reward

The covenants are based on a “no-cure no-pay”-principle. For each reduced dropout in 2006-2007, relative to 2004-2005, the contact municipality of the RMC region receives 2000 euros¹⁸. The Ministry has set an upper limit to the financial reward, in the sense that any reduction in the total number of school dropouts above 10 percent was not rewarded.

Formally, the reward scheme can be defined as follows:

$$P_j = D_{2004_j} * 0,10 * 2000 \quad \text{if } (\Delta D_j > 10\%)$$
$$P_j = (D_{2004_j} - D_{2006_j}) * 2000 \quad \text{if } (\Delta D_j \leq 10\%)$$

with:

- P_j = reward from the Ministry to RMC region j in euros.
 D_{2004} = total number of dropouts in region j in 2004-2005.
 D_{2006} = total number of dropouts in region j in 2006-2007 (post-covenant year).

¹⁸ This reference year was chosen because the figures for the pre-covenant year 2005-2006 were not available at the time the covenants were signed. A drawback is that fluctuations in the dropout figures in 2005-2006 relative to 2004-2005 have affected the outcomes of the covenants, whereas these fluctuations in the pre-covenant year had nothing to do with the covenants. This means that some regions have been lucky (i.e. the ones that witnessed a decline in 2005-2006), and others were unlucky (i.e. the regions with an increase in the pre-covenant year).

ΔD_j = percentage change in total number of dropouts in region j between 2004-2005 and 2006-2007

A particular detail of the program is that payments were made in advance to the covenant regions, notably 60 percent in 2006, 30 percent in 2007 and 10 percent in 2008. If the region did not succeed in reaching the goal of a 10 per cent reduction in the total number of school dropouts, it could be proportionately cut back on these payments, or - in case of a zero or negative result - the payments should be returned to the government.

This implies that the advance payments are completely risk-sensitive, which may make the RMC regions reserved to invest this money. In response, some RMC regions have shifted part of the financial risk to the participating schools through no-cure no-pay arrangements with these schools. Other regions have invested financial reserves, or did not invest the advance covenant payments while just hoping to score a positive result without extra expenditures (Ministry of Education, 2008b).¹⁹

The total budget of the covenant program amounted to 16 million euros. This is roughly twice the amount of the direct contribution from the central government (or 'RMC Rijksbijdrage') to the covenant regions for the school year 2006-2007.

¹⁹ This reward should be spent on school dropout policies in any case.

3 Concerns about previous findings

As already mentioned in the introduction, previous analysis reported by the Ministry of Education suggested positive effects of the covenant policy. However, there are some serious concerns with these findings, which mainly originate from the data that have been used. Other concerns are the reference year that has been chosen and the comparison of changes in the absolute number of dropouts. In this section we will discuss the concerns and the arguments for using a different data source in this study.

RMC data weak in terms of consistency, reliability and uniformity

The previous findings are based on the so-called RMC-data which, until recently, were the main source for the monitoring of student drop-out in Dutch education. As these data suffer from some serious shortcomings, the Ministry is currently using a new source, the so-called BRON-data, covering the whole population of students in secondary education. These data were not yet available at the time of the first analyses of the covenant policies by the Ministry.

An extensive analysis of the pros and cons of different datasets on school dropout by Deloitte (2006) indicates serious weaknesses in terms of the quality of the RMC-data: “RMC dropout data show red scores on the criteria reliability, uniformity and consistence. It turns out that the delivery of these figures by schools does not occur completely, not consistently and not always in time, due to which the quality of these data is insufficient. There is no guarantee for the objectivity of the figures”.²⁰

An inspection of the yearly changes in the number of new and old school dropouts at the regional level shows several very large changes, even up to 2000 percent. The average absolute change in the number of old (new) dropouts per region in 2006-07 is 114 (43) percent.²¹ It is likely that large increases (or declines) in dropout numbers do not reflect real changes in school dropout but are the result of improvements in reporting and registration practices (see also Ministry of Education, 2006). For the analysis, it is difficult to disentangle real developments in dropout from changes in registration practices. If improvements in registration practices are on average biased towards either covenant or non-covenant regions, then using RMC figures will bias the effect estimates of the covenants as well.

An additional argument against using RMC data for an evaluation of the covenants is that the size of the monetary reward for the covenant regions depends on the figures which are produced by the RMC regions themselves. Given the lack of uniformity and consistency of measuring dropout, this may provide an opportunity for strategic behaviour of the covenant

²⁰ This picture is confirmed in a recent report (Sardes, 2005): “data are available too late, registrations are incomplete, no checks are carried out on the correctness of the figures, there is a polluted registration system, etcetera.”

²¹ The Ministry of Education (2006) reports that in particular the registration of dropouts who dropped out longer than a year ago (i.e. 'old' dropouts) is incomplete. This may be an explanation for the relatively large fluctuations observed in RMC data on old dropouts by region.

regions. They might report more favourable developments than actually happened in order to gain a larger reward.

A comparison of RMC data with the BRON data

The main advantage of BRON-data is that these data are more reliable, as they are based on yearly enrolment figures of schools which are checked by accountants.²² Moreover, unlike RMC data, dropout is measured consistently and uniformly in BRON. An additional advantage of BRON data over RMC is the availability of a rich set of individual variables for the whole relevant student population in BRON. In the evaluation these variables can be used to control for differences in the composition of the student populations across covenant and non-covenant regions.

A potential criticism on the use of BRON-data is that these data do not show the incidence of dropouts that returned to school within the same school year. A student dropping out during the school year 2006-07 and returning in education before the first of October 2007 (possibly due to actions of the RMC region) is not considered as a 2006-07 dropout in BRON, since he is again registered in education at the count date. In contrast, a student dropping out during 2006-07, which does not return in education before the first of October 2007 (possibly due to lacking actions by the RMC region) is considered a dropout in BRON. The measurement of dropout over school year t in BRON starts with the sample of students who were enrolled on the first of October of school year t . Dropout for a region R is measured as the difference (D_R) between the total dropout after the first of October (T_R) and the number of re-enrolled students between the count dates of school year t and school year $t+1$ (R_R). Hence, for the covenant regions (C) and non covenant regions (NC) we can only compare the difference, and do not observe the two specific components:

$$D_C = T_C - R_C \text{ and } D_{NC} = T_{NC} - R_{NC}$$

However, the fact that we do not directly observe the two underlying components is not a problem for the evaluation. For instance, if the total number of dropouts during the year is equal in the covenant and non covenant regions (i.e. $T_C = T_{NC}$) but the covenant regions are more successful in re-enrolling dropouts (i.e. $R_C > R_{NC}$), we would observe a lower number of dropouts in the covenant regions in BRON (i.e. $D_C < D_{NC}$). If we observe in BRON that dropout in the covenant and non-covenant regions is equal then the underlying components can all be equal. It is also possible that the covenant regions have much higher re-enrolment. However, this also means that they have a much higher total dropout. This would suggest that they are performing very well with respect to curative actions but not good with respect to preventive actions. As such, the evaluation using BRON sheds light on the total performance of the regions

²² This is because the central government yearly contributions to schools depend on the number of enrolled students to a large extent.

but not on the performance on the underlying components with regard to preventive and curative measures.

Given the arguments above there seem to be much fewer concerns in using the data from BRON than the RMC-data for the evaluation of the covenant policy. We therefore use the BRON data in this study. Table 3.1 summarizes the main differences between the two datasets.

Table 3.1 Differences between RMC and BRON data

	BRON	RMC
Definition of school dropout in school year t (e.g. 2006/07)	A student who is registered in education ^a at October first of a year (e.g. 2006) but not one year later (e.g. 2007), and not having reached a start qualification in the meantime.	A student aged 12-22 who leaves education in year t for at least a month without having reached a start qualification.
Source of data	School enrolment registrations	Registrations by (contact municipalities of) RMC regions. These are based on reports of dropouts by schools
Information on dropout	- New dropouts - Old (i.e. previous) dropouts ^b - Return of old dropouts in education	- New dropouts - Old (previous) dropouts - Replacements of dropouts by RMC regions ^c
Available years	2005/06 and 2006/07	several years
Individual data available for analysis	yes	no ^d
Reliability	good	weak
Consistency over years	good	weak
Uniformity over regions	good	weak

^a Secondary education ('voortgezet onderwijs') or senior secondary vocational education ('mbo').

^b Only old dropouts that dropped out in 2005-06 (pre-treatment year).

^c The reported number of replacements include replacements towards education, work or relief programs.

^d RMC reports only include aggregated figures.

Other concerns

Another concern with the previous findings by the Ministry of Education is the choice of the reference year. In the previous analysis the year 2004-05 was chosen as the base line year. However, the covenants were signed in the summer of 2006. This implies that the pre-treatment year is 2005-06 instead of 2004-05. With this base line year the change in new dropouts according to the RMC-data is somewhat smaller: -12 % in covenant regions, versus + 8% in the non-covenant regions (instead of -10 versus +18 %).

A final concern is that the analysis of the Ministry is based on absolute numbers and therefore vulnerable for changes in the size or the composition of the student populations. The next section explains the approach that is used in this study, which is less vulnerable for this concern.

4 Empirical strategy

This paper provides estimates of the causal effect of the covenants on the probability of dropout and, in addition, estimates the effect on the return of past dropouts into education.

Effect on probability of school dropout in 2006-2007

In order to estimate the effect of the covenant policy on the probability of dropping out in the post-treatment year 2006-2007, we start with a difference-in-differences (DD) estimation approach on the full sample of all 39 regions. This approach exploits the availability of data for the full population in the pre-treatment (2005-2006) as well as the post-treatment year (2006-07). This allows us to compare changes in dropout rates over time between covenant and non-covenant regions, while controlling for non-treatment related changes over time.

Formally, we estimate the following linear probability model:

$$DROP_{ijt} = \beta_0 + \beta_1 C_{jt} + \beta_2 T_t + \beta_3 C_{jt} * T_t + \beta_4 X_{ijt} + \varepsilon_{ijt} , \quad (1)$$

with:

$DROP_{ijt}$ Being a dummy variable indicating whether pupil i in region j in year t is a dropout.

C_{jt} Being a dummy variable which takes value 1 if RMC region j belongs to the 14 covenant regions in year t , and zero if it belongs to one of the remaining 25 RMC regions.

T_t Being a time dummy variable taking value 1 if $t = 2006-2007$ (post-treatment year) and value 0 if $t = 2005-2006$ (pre-treatment year).

X_{ijt} Being background characteristics of pupil i in region j in year t and

ε_{ijt} Is the error term representing all unobservables of pupil i in region j in year t .

For each region j and year t , we include all pupils aged 12-22 that are registered in education and do not have a start-qualification at the beginning of the year. The coefficient β_3 then gives the treatment effect of interest.

The identifying assumption underlying the DD estimation approach is that treatment and non-treatment regions have a common trend in the outcome variable. The common-trend assumption

rules out group specific trends and composition effects.²³ Treatment may be correlated with unobservables that affect the outcome variable, but as long as these unobservable differences between treatment and non-treatment regions are fixed over time, they cancel out. The common trend assumption is not testable. The evolution of dropout probabilities in previous years could give an indication of the validity of the common trend assumption. However, since the BRON data we use for our analysis are only available from the year 2005-2006 onwards, we are not able to investigate this further.

To further improve comparability of experimental and control group we proceed by estimating the difference-in-difference equation locally on sub-samples of more comparable treatment and non-treatment regions, instead of on the whole sample of 39 regions.

A first approach is to combine the DD approach with a so-called regression discontinuity (or RD) approach (see e.g. van der Klauw, 2008).²⁴ Construction of the discontinuity samples is done by exploiting the selection rule for treatment. This selection rule is based on two rankings. First, the ministry has ranked the 39 regions from high to low based on the absolute number of *new* dropouts in the year 2004-2005.²⁵ Second, it has ranked the regions based on the *total* (new + old) absolute number of dropouts. Each region that is in the top ten of one of these rankings, is offered a covenant by the ministry. This yields 12 regions.²⁶ The complete rankings and selected regions can be found in appendix A.

This selection rule can be formalized as follows.

Let $x(k, l)$ denote the region x that is in k -th position in the first ranking and in l -th position in the second ranking ($k=1, \dots, 39$ and $l=1, \dots, 39$). The set of treatment regions, denoted by T , is then given by:

$$T = \{ x(k, l) : k \leq 10 \text{ or } l \leq 10 \}.$$

We exploit this selection rule to construct the discontinuity samples of covenant regions which have just been selected for treatment and non-covenant regions which have just not been

²³ Composition effects are not very plausible here, These effects would arise if the composition of school populations in treatment and/or non-treatment regions would change due to the covenant scheme. It does not seem realistic that pupils that already attend a certain school before the post-covenant year 2006-2007 move to another region because of the covenants. In addition, also pupils who enter a new education level in 2006-2007 are not likely to move to another region because of the introduction of the covenant scheme.

²⁴ Some recent examples of this local diff-in-diff approach are Leuven & Oosterbeek (2004), who evaluate the impact of tax deductions on training participation, and Leuven et al. (2007), who evaluate the effect of extra funding for disadvantaged pupils on achievement.

²⁵ It should be stressed that this is not the pre-treatment year (which is school year 2005/06), but even one year earlier.

²⁶ Two other regions have self-selected themselves later on (in the end of 2006). This brings the total number of covenant regions at 14.

selected for treatment. This is done by ranking all regions according to the number of new and total (i.e. old + new) dropouts. Subsequently, we can determine the “just-selected” (treatment) regions by looking at the covenant regions that would not have been selected if for example only the top 5 (instead of the top-10) of both lists would have been selected for treatment. Similarly, we can determine the “just-not-selected” (non-treatment) regions, by looking which non-covenant regions would have been selected if say the top 20 of both lists would have been selected for treatment. Adding these regions just below and just above the threshold together yields the discontinuity sample.

The construction of our discontinuity samples is formalized as follows. Let DS (T) denote the set of treatment regions that are ‘just selected’ and let DS (NT) denote the set of non-treatment regions that is ‘just not’ selected. Then:

$$\text{DS (T)} = \{ x \in \text{T} : k > 5 \text{ and } l > 5 \} \text{ and}$$

$$\text{DS (NT)} = \{ x \notin \text{T} : k < 20 \text{ or } l < 20 \}.$$

The set of regions in our discontinuity sample, consisting of both treatment and non-treatment regions, is then defined by:

$$\text{DS} = \text{DS (T)} \cup \text{DS (NT)}.$$

The above formalization of the construction of a discontinuity sample refers to hypothetical top-5 and top-20 selection criteria for treatment. The choice of bandwidth around the cut-off is in principle arbitrary. We also construct a second, smaller discontinuity sample of just-selected treatment and just-not selected non-treatment regions by looking at a hypothetical situation in which top-7 and top-17 criteria would have been used.

In addition to the selection rule, which is based on the *number* of dropouts, we also use the (ranking of) pre-treatment-year average *probabilities* of school dropout to construct two sub-samples of more comparable regions in the pre-treatment situation.²⁷ This is done by taking only those regions remaining after having removed the 10 (or 13) regions with the highest and the 10 (or 13) regions with the lowest pre-treatment dropout probabilities.

As explained in section 3, the estimated effect on the probability of dropping out in 2006-2007 takes into account both the effect on new dropouts in 2006-2007 and the effect of re-enrolment of dropouts that dropped out from school during 2006-2007. Hence, it is a composite effect of both preventive actions and curative actions aimed at the dropouts in the year 2006-2007.

²⁷ This is another approach to add to the credibility of the common trend assumption.

It does not, however, take into account the effect of curative actions aimed at earlier dropouts, i.e. those that had already dropped out before the year 2006-2007. This effect on previous dropouts will be estimated separately.

Effect on probability of return of past dropouts in 2006-2007

In order to investigate the potential effects of curative actions aimed at earlier dropouts, we adopt a regression discontinuity approach.

The discontinuity samples are the same as the ones identified to estimate the (local) effect on the probability of school dropout in 2006-2007. A combination with a diff-in-diff approach is not possible here, due to lacking data on the return of previous dropouts in the pre-treatment year 2005-2006. However, registrations of counselling of students that have dropped out from school point at little or no difference in the average probability of return of past dropouts among covenant and non-covenant regions in the pre-covenant year 2005-2006.²⁸

The estimated equation is:

$$RET_{ij} = \beta_0 + \beta_1 C_j + \beta_2 X_{ij} + \varepsilon_{ij} \quad (2)$$

with:

RET_{ij} Being a dummy variable indicating whether a dropout i in region j , who has dropped out from school in the pre-covenant year 2005-2006, is again registered in education on the first of October of 2007.

C_j Being a dummy variable which takes value 1 if RMC region j belongs to the 14 covenant regions, and zero if it belongs to one of the remaining 25 RMC regions.

X_{ij} Being background characteristics of past dropout i in region j

ε_{ij} Being the error term representing all unobservables of past dropout i in region j .

β_1 Giving the treatment effect on the probability of return.

The estimated treatment effect is the effect on re-enrolment in education of the dropouts of the year 2005-2006.²⁹

²⁸ Actually, there is no difference at all in the percentage of counselled dropouts (relative to the total stock of dropouts) in the pre-treatment year 2005-06 among covenant and non-covenant regions.

²⁹ Because of a lack of data availability we are not able to investigate the effect of re-enrolment of those who were already dropped out before 2005-06. However, the RMC regions seem to put relatively little effort in early school-leavers that dropped out more than a year ago (Sardes, 2006).

5 Data

This paper uses individual data on dropouts for both the pre-treatment year 2005-2006 and the post-treatment year 2006-2007. The dataset we use are the so-called *BRON*-data. This is a relatively new dataset containing information on the year-to-year progress of each individual student throughout education. The *BRON*-definition of a school dropout in a particular school year is someone who was present in education at the beginning of that school year (October first), but is not exactly one year later (i.e. in the beginning of next school year) AND has not reached a start qualification in the mean time.³⁰

The *BRON* dataset contains information at a student level on:

- Background characteristics: age, gender and ethnicity (7 categories).
- Education level and type. *BRON* contains very detailed information on the level, year and type of education of a student at the count date (i.e. first of October of a particular year). We have constructed an education variable containing 14 categories. These categories are shown in Appendix C.
- The neighbourhood of the student: size of the municipality (in three categories: four largest cities, medium-sized and small-sized), whether a student is inhabitant of a so-called poverty accumulation area, and the RMC region the student belongs to.
- Dropout. *BRON* identifies dropouts on the basis of a comparison of school enrolment data between two subsequent count dates. For instance, if a student was present in education at October first of 2006, but not one year later, while not having attained a start qualification in the meantime, this student is marked as a school dropout for the school year 2006-2007.

In addition to *BRON*-data, we also use figures from CFI on the central government contributions (per student) to the RMC regions in pre- and post-treatment year, and CBS information on several socio-economic characteristics of the neighbourhood of the student (based on 4-digit level postal codes). These include the share of low and high-income households, the density of addresses, the average house value, average income and the share of non-western foreigners.

³⁰ This definition slightly differs from the RMC-definition of a school dropout, which is a person aged 12-22 who has left education for more than a month without a start qualification. These RMC registration figures have been used to determine the size of the monetary reward to the covenant regions. The slightly different definition used in the *BRON* dataset does not matter when assessing the effectiveness of the covenants. We refer to section 3 for a list of arguments why we use *BRON* data instead of RMC-data.

5.1 Summary statistics full sample

Table 5.1 reports descriptive statistics for all 14 covenant and all 25 non-covenant regions in the pre-treatment year 2005-2006. These are reported for both the outcome variable (dropout probability) as well as for the various covariates. The last column gives the p-value of a standard t-test on the equality of these means.

Table 5.1 Sample means for covenant and non-covenant regions, school year 2005-2006 (pre-covenant year) ^a			
Variable	Non-covenant regions (NCR's)	Covenant regions (CR's)	P-value of t-test for equality of CR's and NCR's ^b
Dropout probability	3.95	4.71	0.000
Education level (all percentages)			
Secondary education	70.1	70.6	0.000
Vmbo (pre-vocational)	17.6	17.4	0.076
Post-secondary vocational education (mbo)	29.9	29.4	0.000
Mbo level 1 or 2 (lowest two levels)	8.4	9.0	0.000
Bol	23.9	24.3	0.000
Bbl	5.9	5.0	0.000
In exam class	14.5	14.3	0.072
Personal characteristics			
Age (years)	15.3	15.3	0.000
Of school age ^c	69.7	69.7	0.357
Male	51.0	50.8	0.029
Ethnicity			
Dutch	85.5	73.2	0.000
Foreign (non-western)	5.4	6.7	0.000
Foreign (western)	8.8	19.8	0.000
Environment of pupil (all percentages)			
Degree of urbanisation			
Inhabitant of G4 (4 largest cities)	0	17.8	0.000
Medium-sized municipality	20.6	25.5	0.000
Small municipality	79.4	56.7	0.000
Inhabitant poverty accumulation area ^d	4.8	20.0	0.000
RMC budget per student (€)	14.9	9.4	
Total number of pupils	525 019	764 806	1 289 825
Total number of schools			608

^a All numbers represent percentages of the population, unless stated otherwise.
^b A P-value of < 0.01 (<0.05 / <0.10) denotes the difference is significant at a 1 (5/10) percent significance level.
^c Of school age means 16 or younger.
^d These are postal code areas which are characterized by an accumulation of social problems. Underlying indicators are the percentage of low incomes, the share of welfare recipients and the share of non-western foreigners.

The selection of the covenant regions has been based on the total number of dropouts. This selection criterion implies that regions which contain the largest cities and/or more disadvantaged students are more likely to have been selected for treatment. Table 5.1 confirms this. For instance, the average dropout probability in covenant regions was significantly larger in the pre-treatment year (4.7 versus 4.0 per cent).

5.2 Summary statistics discontinuity and matched sub-samples

In order to carry out the difference-in-differences estimation approach locally on more comparable treatment and non-treatment regions, we construct new sub samples. The first two are discontinuity samples which exploit the selection rule of the covenants, whereas the latter two samples are matched samples that consist of covenant and non-covenant regions within a similar interval of pre-treatment year dropout probabilities. More specifically, the four sub samples have been constructed as follows:

1. The treatment regions included are the ones that would not have been selected if only the top 5 of the lists of number of new and of total school dropouts were selected (instead of the top 10). Just not selected non-treatment regions are the regions that would have been selected if the top 20 of the lists of new and of total school dropouts were selected (instead of the top 10).
2. The treatment regions included in this discontinuity sample are the ones that would not have been selected if only the top 7 of both lists were selected. The non-treatment regions are the ones that would have been selected if the top 17 of both lists were selected.
3. Sample contains all regions left over after removing 10 regions with highest and 10 regions with lowest dropout percentage in the pre-treatment year.³¹
4. Sample contains all remaining regions after removing 13 regions with highest and 13 regions with lowest dropout percentage in the pre-treatment year.³²

Table 5.2 shows that the differences in pre-treatment dropout probabilities indeed become much smaller in the various sub samples and completely disappear in matched sample 3. Differences in socio-economic characteristics (e.g. ethnic distribution, share of inhabitants of poverty accumulation area) become much smaller as well.

³¹ This means that all regions are included with an average pre-treatment year dropout probability within the range of 3.64 and 4.65 per cent.

³² The discontinuity sample contains all regions with an average pre-treatment year dropout probability within the range of 3.8 and 4.5 per cent.

Table 5.2 Difference in outcome variable and background characteristics among covenant and non-covenant student populations, various sub samples, 2005-2006 (pre-covenant year)^{a,b}

Variable	Discontinuity samples based on selection rule		Matched samples based on pre-treatment year dropout probabilities		Full sample (i.e. all regions) FS
	DS1	DS2	MS3	MS4	
Dropout percentage	- 0.1	- 0.2***	0.0	+ 0.1	+ 0.8***
Education level					
Secondary education	- 1.2***	- 1.5***	+ 0.5***	+ 1.4***	+ 0.5***
Vmbo (pre-vocational)	0.3**	0.0	0.3***	- 0.5***	- 0.1*
Post-secondary vocational education (mbo)	+ 1.2***	+ 1.5***	- 0.5***	- 1.4***	- 0.5***
Mbo level 1 or 2 (lowest two levels)	+ 0.4***	0.0	- 0.3***	- 0.3***	+ 0.6***
Bol	+ 2.2***	+ 0.2***	+ 0.2*	- 0.8***	+ 0.4***
Bbl	- 0.9***	- 0.8***	- 0.7***	- 0.6***	- 0.9***
In exam class	0.0	- 0.3**	-0.1	0.0	- 0.2*
Personal characteristics					
Age (years)	+ 0.1***	+ 0.1***	0.0	- 0.0**	+ 0.0***
Of school age	- 0.8***	- 1.0***	+ 0.1	+ 0.6***	- 0.1
Male	-0.1	0.0	0.0	0.0	- 0.2**
Ethnicity					
Native	- 3.7***	- 1.7***	- 3.0***	- 3.2***	- 12.3***
Foreign (non-western)	+ 3.1***	+ 1.3***	+ 2.6***	+ 2.8***	+ 10.9***
Foreign (western)	+ 0.6***	+ 0.5***	+ 0.4***	+ 0.5***	+ 1.3***
Environment of pupil					
Degree of urbanisation					
Inhabitant of G4 (4 largest cities)	0.0	0.0	+ 4.1***	0.0	+ 17.8***
Medium-sized municipality	+ 2.7***	+ 0.7***	- 3.5***	- 2.0***	+ 4.9***
Small municipality	- 2.7***	- 0.7***	- 0.6***	+ 2.0***	- 22.7***
Inhabitant poverty accumulation area	+ 9.7***	+ 7.0***	+7.3***	+ 7.7***	+ 15.2***
RMC budget per student (€)	- 2.1	- 2.2	- 4.4	- 4.1	- 5.6
Number of covenant regions	6	4	11	5	14
Number of non-covenant regions	8	5	8	8	25
Number of pupils	490 266	316 169	625 692	406 473	1 289 825

^a All numbers represent differences in percentage points, unless stated otherwise.

^b * = difference among covenant and non-covenant regions is significant at 10 % significance level; ** = significant difference at 5 % level ; *** = significant difference at a 1 % level.

6 Main estimation results

6.1 Effect on probability of dropping out in post-covenant year 2006-2007

6.1.1 Descriptive statistics on evolution of dropout probabilities

Table 6.1 shows the development of the average dropout probabilities between the pre- and post-covenant school year.³³ We observe a drop in the average dropout probability of 0.20 percentage point in the 14 covenant regions. The 25 non-covenant regions, however, witnessed a decline in the dropout probability as well - though somewhat smaller - of 0.16 percentage point. The average fall in the dropout probability is therefore 0.03 percentage points larger in the covenant regions. In the next section we further investigate this in a difference-in-differences estimation approach.

In terms of numbers of dropouts, the average decline amounted to 3.2 per cent in both covenant as well as non-covenant regions.³⁴ It turns out that this nationwide decline in school dropout can be assigned to differences in the characteristics of the student populations between the pre- and post-covenant year.³⁵

Table 6.1 Development of school dropout percentages between pre-covenant (2005-2006) and post-covenant year (2006-2007), covenant versus non-covenant regions, complete sample, source: BRON-data.

Region	2005-2006	2006-2007	Δ 2006/07 - 2005/06 (%-point)
Covenant regions (14)	4.71	4.51	- 0.20
Non-covenant regions (25)	3.95	3.79	- 0.16
All regions (39)	4.42	4.23	-0.18 ^a

^a The average decline in the dropout probability for all regions together turns out to be statistically insignificant (tested at a 10% significance level) after having controlled for changes in the composition/characteristics of the student population between the pre- and post-covenant year.

³³ We refer to Appendix Table B1 for a complete list of pre- and post-treatment year dropout probabilities for all 39 regions separately. It can be seen that the evolution of dropout varies widely over the regions.

³⁴ , from 36.0 thousand to 34.9 thousand people that have left education without a start qualification. The 14 covenant regions together account for more than 60 per cent of all dropouts in the Netherlands.

³⁵ For instance, in the post-covenant year, relatively more students participated in havo, vwo and the highest two levels of mbo. These are levels for which the probability of school dropout is inherently lower.

6.1.2 Difference-in-differences effect estimates

Full sample estimates

Table 6.2 shows the difference-in-differences estimates of the effect of the covenants on the probability of school dropout using four different specifications.³⁶ The first specification does not include any controls. In the next three specifications, personal characteristics, education level/type dummies and information on the environment of the student are added to the regression equation step-by-step as additional covariates. The complete regression outcomes for the fourth specification are shown in Appendix Table C1.

In all four specifications, the estimates of the effect of the covenants do not significantly differ from zero. Adding additional covariates does not change the significance of the estimated effects.³⁷ The point estimate of our preferred specification with all controls (cf column 4: -0.098), though statistically insignificant, would correspond to a fall in the number of dropouts in the post-treatment year 2006/07 of 2.1 percent.³⁸ As mentioned before, the policy target of the covenants was a reduction in school dropout of at least ten percent.

	(1)	(2)	(3)	(4)
Effect estimate ^a	- 0.032 (n.s.)	- 0.046 (n.s.)	- 0.059 (n.s.)	- 0.098 (n.s.)
standard error ^b	0.095	0.089	0.091	0.093
Control variables				
Personal characteristics ^c	no	yes	yes	yes
Education level/stage ^d	no	no	yes	yes
Environment ^e	no	no	no	yes
Total number of students	2 592 831	2 592 831	2 592 831	2 592 352

^a n.s. denotes not significant. The chosen significance level to determine significance is 10 per cent.
^b All reported standard errors are robust standard errors, correcting for clustering at the region level.
^c Age and dummies for gender, ethnicity (7), and whether a student is of school age.
^d 14 education categories (first class secondary education, vmbo basis, vmbo kader, vmbo gemengd en theoretische leerweg, havo, vwo, mbo bol 1, 2, 3 and 4, mbo bbl 1, 2, 3 and 4), dummy for being in exam class.
^e Size of the municipality (three categories: G4 (four largest cities), medium and small), whether a student is inhabitant of a poverty accumulation area, and central government contribution per student to RMC region.

³⁶ The effect estimates represent coefficient β_3 of equation (1), as discussed in the methodology section. The equation is estimated by standard OLS (i.e. linear probability model).

³⁷ This is not to say that these covariates do not matter for the probability of dropout; quite the contrary, most covariates are significantly and strongly correlated with the probability of dropout (see appendix table C1).

³⁸ This is calculated by dividing the effect estimate by the dropout probability in the covenant regions in the pre-covenant year.

Local diff-in-diff estimates on discontinuity and matched samples

A potential problem with our previous difference-in-differences estimations on the full sample may be that the covenant and non-covenant regions do not have the same pre-treatment common trend in terms of school dropout. In our second approach, we therefore attempt to improve on our earlier estimates by limiting our estimation sample to covenant and non-covenant regions which are more similar to each other.³⁹ We have identified four of those samples. The first two are discontinuity samples identified on the basis of the selection rule of the treatment regions; the latter two are matched samples which are constructed by selecting regions within a similar interval of dropout probability in the pre-treatment year.

Table 6.3 shows that the treatment and non-treatment regions indeed become more similar in all these sub-samples with respect to the pre-treatment dropout probability.

The average pre- and post-treatment dropout probabilities are presented in Table 6.3. Again, we find a (significant) decline in the dropout probability in the selected covenant regions. However, the decline is even larger in the subsets of comparable non-covenant regions this time, particularly in discontinuity samples 3 and 4.

Table 6.3 Descriptive statistics: pre- and post-covenant year dropout probabilities, covenant versus non-covenant regions, various sub samples.

Sub sample ^a	Discontinuity samples based on selection rule		Matched samples constructed on basis of comparable pre-treatment year dropout percentage	
	DS1	DS2	MS3	MS4
Covenant regions				
2005-2006	3.91	3.89	4.05	4.08
2006-2007	3.78	3.76	3.93	3.95
Δ 06/07 - 05-06	- 0.12	- 0.13	- 0.12	- 0.13
Non-covenant regions				
2005-2006	3.99	4.11	4.06	4.03
2006-2007	3.87	3.97	3.87	3.80
Δ 06/07 - 05-06	- 0.12	- 0.14	- 0.19	- 0.23

^a See Section 5.2 for the construction of these samples.

³⁹ This procedure adds to the credibility of the common trend assumption, which is an identifying assumption of a difference-in-differences estimation approach.

Table 6.4 reports the estimation results from the difference-in-differences estimation procedure on the sub-samples of more comparable treatment and non-treatment regions. We find that all estimates are insignificant. Moreover, all estimates are closer to zero or even positive, as compared to our earlier diff-in-diff estimate on the full sample.⁴⁰

These estimation results confirm previous findings that the covenant policy had no significant effect on the probability of school dropout in the post-treatment year 2006-07.

Table 6.4 Difference-in-differences estimates of effect of covenants on probability of dropping out in post-covenant year for various discontinuity and matched samples, estimates in percentage points, complete set of controls^a

Sample ^b	Construction based on selection rule		Construction based on comparable dropout probability in pre-covenant year	
	DS 1	DS2	'DS3'	'DS4'
Effect estimate ^c	- 0.002 (n.s.)	0.049 (n.s.)	- 0.022 (n.s.)	- 0.009 (n.s.)
standard error ^d	0.117	0.134	0.102	0.136
Number of students (both years)	987 196	632 269	1 258 654	817 671
Number of covenant regions	6	4	11	5
Number of non-covenant regions	8	5	8	8

^a The set of controls is similar to that of model specification 4 of table 5.2, i.e. including various personal characteristics, education level/stage dummies, and characteristics on the environment of the student.

^b See Section 5.2 for the construction of these discontinuity and matched samples.

^c n.s. denotes estimate is not significant. The chosen significance level to determine significance is 10 per cent.

^d All reported standard errors are robust standard errors, correcting for clustering at the RMC region level.

6.1.3 Heterogeneous effects

It is conceivable that the covenants had no overall effect on school dropout for the whole student population, but still had a partial effect on a certain subpopulation of students. This could be due to a particular focus on certain education levels/types in the covenant projects for example.

In order to test for the occurrence of heterogeneous effects, we have estimated the difference-in-differences effects on various sub-samples of the student population. In Appendix table D1, we report the estimates for the lowest two levels of mbo participants and for exam-candidates of vmbo (see columns 1 and 2). These are education types in which the probability of school dropout is particularly large, and which receive a lot of policy attention.⁴¹

The estimates for these two subgroups show statistically insignificant effects.⁴² Estimates on numerous other subgroups of education types, whether on the full population or on the various

⁴⁰ Including polynomials of the selection criteria (the absolute number of total and new dropouts in 2004-05) in the DD estimations does not change the main findings. It should be noted that these polynomials might suffer from measurement error as they are based on RMC-figures.

⁴¹ For instance, the average probability to leave school without a start qualification amounts to nearly 40 percent for mbo-1 students, 17 percent for mbo-2 students, and 13 per cent for vmbo exam candidates of the lowest level (vmbo-b).

⁴² Estimating the same diff-in-diff regression on the various discontinuity samples of regions - instead of on all regions - does not change the results.

discontinuity samples, confirm this picture. Therefore, we do not find evidence for heterogeneous effects of the covenants by education type or level.

6.1.4 Other sensitivity analyses

To check the robustness of our findings, we performed various sensitivity analyses. We report two of them here.

The first robustness check is to leave out the two covenant regions that signed covenants at a later stage than the other 12 covenant regions.⁴³ These regions were added at their own request, and were not in the original lists of top 10 regions with the largest number of new or total dropouts in the reference year. However, after the exclusion of these two regions we still find a statistically insignificant effect (see column 3 of Appendix table D1).

The second sensitivity analysis is to add an extra set of controls with socio-economic information on the neighbourhood of the student. These are the proportion of low-and high-income families, the share of non-western ethnic population, the average housing value and the population density.⁴⁴ Though some of these neighbourhood characteristics seem to be significantly correlated with the dropout probability, the effect estimate remains statistically insignificant (see column 4 of Appendix table D1).⁴⁵

Summarizing, we find no evidence for a significant (negative) effect of the covenants on the probability of school dropout in the post-covenant year, neither for the total population, nor for particular subsets of education types. This result is robust for various model specifications and sensitivity analyses.

6.2 Effect on return of past dropouts in education

The financial incentives were also targeted at reducing the number of past dropouts. RMC regions could establish this by mediating these past dropouts back to education, which is actually one of their legal tasks.

⁴³ These two regions are Centraal en Westelijk Groningen en Zuidoost Brabant. They signed their covenants in the end of 2006 instead of the summer.

⁴⁴ This neighborhood information is taken from CBS neighborhood data at the 4-digit zip code level. Including this information reduces the number of valid observations by about 20 per cent due to missing information.

⁴⁵ For example, a one percentage point increase in the percentage of non-western ethnic minorities in the neighbourhood is associated with a 0.6 percentage point increase in the probability of school dropout, even after controlling for the ethnicity of the pupils themselves.

We estimate this effect on the return of previous dropouts into education by a regression discontinuity approach (see section 4).⁴⁶ The subsets of comparable regions are the same four samples as the ones used for estimating the effect on the probability of dropout. The analysis is carried out on the sample of (past) dropouts from the pre-covenant year 2005-06.⁴⁷

Descriptive statistics

Table 6.5 shows the descriptive statistics for the return percentages for all treated and non-treated regions in the various discontinuity samples in the post-covenant year.⁴⁸ We observe that for all discontinuity samples, return of dropouts in covenant regions has been slightly larger in the treatment regions. The differences in return rates, however, are never significantly different from zero.

Table 6.5 Descriptive statistics: Return percentages of past dropouts into education in post-covenant year, covenant versus non-covenant regions, different discontinuity samples^a

Sample ^b	Discontinuity samples based on selection rule		Matched samples based on comparable pre-treatment year dropout probability		Full sample of all regions
	DS1	DS2	MS3	MS4	
Covenant regions	22.8	18.9	18.5	18.4	23.1
Non-covenant regions	22.4	18.6	18.1	18.0	22.4
Difference (CR - NCR)	0.4	0.3	0.4	0.4	0.6 ^{*c}
Number of dropouts	19 351	12 591	25 356	16 487	56 785

^a The past dropouts included are dropouts in the pre-covenant school year 2005-06, that is, students that were present in education at October first 2005, but not one year later (and have not acquired a start qualification in the meantime). Return is measured at the first of October of 2007, that is, just after the end of the post-covenant school year 2006-07.

^b We refer to section 5.2 for a description of the construction of the various discontinuity samples.

^c * denotes difference is significant at 10-% significance level.

Regression discontinuity estimation results

Table 6.6 shows the estimation results of the effect of the covenants on the probability of return of past dropouts for the various discontinuity samples. The complete regression outcomes are shown in Appendix Table C2 (for discontinuity sample 1).

⁴⁶ As mentioned earlier, a combination with a diff-in-diff approach is not possible here, due to lacking data on the return of old dropouts in the pre-treatment year 2005-2006. However, registrations of replacements of dropouts by RMC regions suggest that there are no (significant) differences in the average probability of return of dropouts among covenant and non-covenant regions in the pre-covenant year 2005-2006. This contributes to the credibility of the regression discontinuity estimates.

⁴⁷ The BRON dataset does not contain information on earlier cohorts of past dropouts. However, the RMC regions seem to pay little attention to dropouts that have dropped out longer time ago. For example, in school year 2004-2005, only 10 per cent of all replacements of dropouts by RMC's took place more than one year after registration (see Sardes, 2006).

⁴⁸ Appendix Table B1 shows the return percentages for all 39 RMC regions separately.

All estimates are statistically insignificant.⁴⁹ Some remarks should be added to this finding.

First, our estimates probably represent an upper bound of the effect on past dropouts, since we have only included the most recent wave of past dropouts in our estimation sample. The chance of success of any counselling actions taken on the stock of past dropouts is expected to diminish with the length of the time span between the moment of counselling and the moment of dropping out from school. Moreover, it has been reported that most of the (successful) replacements by RMC regions are of dropouts within one year after registration (see Sardes, 2006).

Second, apart from lacking evidence of an effect on the probability of return of the stock of *pre-treatment year* past dropouts, our earlier estimates on the probability of school dropout in 2006-07 suggest that there is no effect on the return of students that dropped out from school *during* the post-treatment year 2006-2007 either. Though we cannot directly observe dropout and return *within* the same school year with BRON-data, we have earlier shown that this return within the same school year is indirectly reflected in the 2006-07 dropout figures.⁵⁰ All else being equal, any differences in return of dropouts within the same year among regions - which could possibly be due to the covenants, would therefore have to show up in the post-treatment year dropout probabilities.

Third, the findings of insignificant effect seem in line with the fact that the majority of the projects undertaken by the covenant regions seem to have a preventive rather than a curative character.⁵¹

We conclude that there is no evidence for a significant effect of the covenants on the probability of return of past dropouts into education.

⁴⁹ The largest point estimate of 1.57 (see column 1) corresponds to an increase in the return probability of 7 percent. The negative point estimate for discontinuity sample 2, however, would correspond to a fall in the return probability of 2 percent.

⁵⁰ A student dropping out during 2006-07 and returning in education before the first of October 2007 is not considered a 2006-07 dropout in BRON, as he is present at the count date. In contrast, a student dropping out during 2006-07, which does not return before the first of October 2007, is considered a dropout in BRON, since he is not registered in education at the count date.

⁵¹ This is based on a list of all covenant projects in all regions, see Ministry of Education (2008b).

Table 6.6 Effect estimates of covenants on probability of return of past dropouts into education in post-covenant year 2006/07, estimates in percentage points^{a,b,c}

	Discontinuity samples based on selection rule		Matched samples based on comparable pre-treatment year dropout probability		Full sample (i.e. all regions)
	DS1	DS2	MS3	MS4	FS
Effect estimate ^d	1.57 (n.s.)	- 0.39 (n.s.)	0.70 (n.s.)	0.72 (n.s.)	0.10 (n.s.)
standard error ^e	1.64	1.46	1.53	1.85	1.22
Number of dropouts	19 351	12 591	25 356	16 487	56 785

^a Past dropouts are dropouts during the pre-covenant school year 2005-06. Return is measured at the first of October of 2007, that is, just after the end of the post-covenant school year 2006-07.

^b See section 5.2 for the construction of the various sub-samples.

^c All models are based on a specification with a complete set of covariates, cf. model 4 of Table 5.2. These covariates include various individual background characteristics, environment characteristics and information on the education level and type of each student in the year of dropping out (i.e. 2005-06).

^d n.s. denotes not estimate is not significant. The chosen significance level to determine significance is 10 percent.

^e All reported standard errors are robust standard errors, correcting for clustering at the RMC region level.

Effect on replacement outside education unclear

Under the covenant scheme, RMC regions were also rewarded for replacements of dropouts towards work or care. These replacement generally account for roughly half of total replacements by RMC regions (see Research voor Beleid, 2008, Sardes, 2006).

Replacement outside education is not observed in the BRON-data. As such, we cannot estimate the effect of the 2006 covenants on replacement towards work or care. The data on these outcomes produced by RMC regions themselves suffer from serious reliability and consistency problems, up and above the earlier mentioned general weaknesses in RMC dropout data. This is due to different interpretations of the concept of a replacement by RMC regions.⁵² These differences clearly become visible in the unrealistically large differences in replacement successes among RMC regions: these range from 5 to 84 per cent of all dropouts. The lack of national guidelines on the definition of a replacement may have provided an opportunity for some covenant regions to report more favourable results than actually achieved, given that this would positively affect the final reward.⁵³

⁵² For instance, one region mentions that they consider all dropouts to be replaced, in the sense that they get a destination. This destination can vary from school to work, but also to another region (with unknown destination!) or to prison. Yet another region does not consider as a replacement a dropout that is placed into a trajectory that does not lead to a start qualification, even though the dropout may be counselled to work or a combination of work and education (see Research voor beleid, 2008).

⁵³ One RMC officer actually admitted that this unclearness led them to use the most favourable definition for the post-covenant year. The officer told: "Using this indicator to determine the size of the monetary reward is asking for problems".

7 Conclusions and discussion

This study evaluates the effectiveness of a financial incentive scheme for regions to reduce school dropout in the Netherlands that was introduced in 2006. The target of this covenant scheme was to reduce the total number of dropouts by (at least) ten percent in the school year 2006-07.

Setup of 2006 covenants

Selected regions could earn 2000 euros per school dropout less in the post-treatment year 2006-07 relative to the reference year 2004-05. Positive results could be obtained by preventing new school dropout or by successful curative actions on the stock of past dropouts. The regions were free to choose their mix of instruments. Advance payments were carried out by the Ministry of Education to the selected covenant regions. If the regions did not or only partly succeed in reaching the target of 10 percent less dropouts, the advance transfers had to be paid back. The total budget of the 2006 covenants was 16 million euro.

The covenants were signed with 14 out of 39 regions. Selection of these regions was based on presence in the top 10 of either the total number of dropouts or the new number of dropouts. The other regions did not receive any financial incentive. This selection rule, and a unique dataset containing individual information on the whole student population in both the pre- and post-treatment year, offers good opportunities to evaluate the effects of this financial incentive scheme on school dropout. The effects have been estimated by a combination of a difference-in-differences approach and a regression discontinuity approach.

2006 covenants did not reduce school dropout

Though we observe a modest decline in the dropout probability in the covenant regions in the post-covenant year, a similar decline is found in the non-treatment regions. All estimates of the effects of the financial incentive scheme are statistically insignificant. The effect estimates become closer to zero when limiting the sample to treatment and non-treatment regions that are more similar to each other in terms of their pre-treatment characteristics, that is, by looking at regions just above and just below the selection cut-off.

Estimates for the effect of the covenants on the probability of return of past dropouts into education are statistically insignificant as well.

We conclude that there is no evidence that the 2006 covenants have been effective in reducing school dropout.

Effect of 2006 covenants on replacement outside education unclear

Increases in replacements of dropouts outside education (e.g. to work or to care programs) were also rewarded under the 2006 incentive scheme. However, the effect on these replacements

could not be taken into account in our analysis as our data do not contain information on these outcomes. Reports from the region themselves on replacements suffer from serious reliability and consistency problems. Moreover, it should be noted that these replacements outside education do not lead to a start qualification (i.e. to less school dropout), at least not in the short term.

Effectiveness of new 2007-2011 covenants

As mentioned earlier, the Ministry of Education has negotiated new covenants with all 39 regions in late 2007 and early 2008. These covenants differ from the first round of covenants which we evaluated in this study. The most important differences are that the money goes to the schools rather than to the regions and that the new covenants are four-year instead of one-year arrangements. These changes result in a larger emphasis on preventive policies rather than curative policies.

It is difficult to predict whether these new covenants will succeed in reducing school dropout. However, both the new and the old covenants carry elements that seem to provide sub-optimal incentives to reduce dropout and that can be improved with the use of the BRON data. First, the choice of the reference year against which results are measured is chosen at least one year before the moment of signing the covenants.⁵⁴ This leads to a situation in which some regions (or schools) already have reached the covenant target at the moment the covenant is signed, whereas other regions (schools) start with a (strong) negative result. For example, there is one school that already looked forward to a reward of 1.5 million euro at the moment of signing the new covenant, because they witnessed a decline in the number of dropouts in the period before the covenants were signed.⁵⁵

Second, the reward depends on the reduction in the *number* of dropouts rather than the *probability* of dropping out. Exogenous changes in the size or composition of student populations of a region (or school) may therefore affect the size of the reward, irrespective of whether that particular region (or school) has become more successful in reducing dropout.⁵⁶

⁵⁴ The old covenants were signed in the summer of 2006. The chosen reference year was 2004/05. The new covenants were signed between the end of 2007 and half of 2008. The chosen reference year was 2005/06.

⁵⁵ This is under the assumption that this school manages to keep the number of dropouts at their 2006-07 level. On the other hand, there are also schools that miss out on a reward because they witnessed an increase in the number of dropouts in the year(s) before they signed the covenants. The amount a school may miss out on may go up to 1.1 million euro.

⁵⁶ The new covenants only apply a correction factor if the (percentage) change in the size of the student population level relative to the reference year exceeds a certain level. This percentage is set at 30 percent for the first year of the covenants. This implies that the expected growth in the number of school dropouts for a school whose population grows by say 25 per cent is not taken into account when determining the size of the reward. On the other hand, a school whose population shrinks may benefit from this setup. The same applies for a school that has less students in the school types in which school dropout is more prominent (e.g. mbo 1). Note that the total number of students in mbo 1 education (with an average dropout probability of nearly 40 per cent) has declined by 7.5 per cent between 2005/06 and 2006/07.

Literature

Deloitte Accountants, 2006, Audit over het gebruik van de informatiebronnen voortijdig schoolverlaten.

Klauw, W. van der, JAAR, Regression-discontinuity analysis: A survey of recent developments in economics, *Labour*, vol. 22, nr. 2, pp 219-245.

Leuven, E., and H. Oosterbeek, 2004, Evaluating the effect of tax deductions on training, *Journal of Labour Economics*, vol. 22, nr. 2, pp. 461-488.

Leuven, E, M. Lindahl, H. Oosterbeek and H.D. Webbink, 2007, The effect of extra funding for disadvantaged pupils on achievement, *Review of economics and statistics*, vol. 89, nr. 4, pp. 721-736.

Lochner, L. and E. Moretti, 2004, The effect of education on crime: evidence from prison inmates, arrests, and self-reports, *American Economic Review*.

Ministry of Education, 2006, Factsheets voortijdig schoolverlaten, April 2006.

Ministry of Education, 2008a, Aanval op de uitval: Uitvoeren en doorzetten.

Ministry of Education, 2008b, Evaluatie convenantactie voortijdig schoolverlaten schooljaar 2006-2007, Directorate BVE.

Ministry of Education, 2008c, Cijfers 2006-2007 Voortijdig Schoolverlaten, Letter of Minister of Education to Second Chamber.

Ministry of Education, 2008d, Uitvoeringsregeling bestrijding voortijdig schoolverlaten en regionale meld- en coördinatiefunctie voortijdig schoolverlaten.

Research voor Beleid, 2008, RMC Analyse 2007; voortijdig schoolverlaten en RMC-functie 2006/2007.

Sardes, 2006, De uitkomsten van de RMC-analyse 2005, Utrecht.

Appendix A: Selection of covenant regions

Table A1 Selection of covenant regions: rankings of regions according to number of new dropouts and total (i.e. old+new) number of dropouts in 2004-05, source: RMC registration figures

Ranking	Region	Number of new dropouts	Region	Total number of dropouts (i.e. old + new)
1	agglomeratie amsterdam^a	7402	agglomeratie amsterdam^a	18047
2	rijnmond	6276	rijnmond	15379
3	west-brabant	4643	west-brabant	7803
4	haaglanden	4374	haaglanden	6360
5	utrecht	2662	gewest limburg-zuid	5631
6	arnhem/nijmegen	2260	flevoland	4845
7	twente	2072	utrecht	4185
8	gewest limburg-zuid	1841	noordoost-brabant	3310
9	noordoost-brabant	1829	zuid-holland-zuid	3111
10	gewest noord-limburg	1793	arnhem/nijmegen	2616
11	<i>zuidoost-brabant^b</i>	1773	zuid-holland-noord	2395
12	west-kennemerland	1292	<i>centraal en westelijk groningen^b</i>	2254
13	<i>centraal en westelijk groningen^b</i>	1279	twente	2162
14	zuid-holland-noord	1219	gewest noord-limburg	2059
15	flevoland	1218	<i>zuidoost-brabant^b</i>	1773
16	westfriesland	1216	noord-kennemerland	1507
17	ijssel-vecht	1075	west-kennemerland	1292
18	zuidoost-drenthe	1052	eem en vallei	1283
19	eem en vallei	1038	westfriesland	1216
20	oost-gelderland	980	zuidoost-drenthe	1171
21	rivierenland	977	oost-gelderland	1148
22	zuid-holland-zuid	935	ijssel-vecht	1138
23	stedendriehoek	748	friesland-oost	1060
24	friesland-oost	697	noord- en midden-drenthe	1046
25	noord-kennemerland	665	rivierenland	1023
26	noord-groningen en eemsmond	646	oost-groningen	984
27	noord- en midden-drenthe	581	kop van noord-holland	842
28	midden-brabant	554	stedendriehoek	809
29	gooi en vechtstreek	520	zuidwest-drenthe	740
30	zuid-holland-oost	467	friesland-noord	693
31	kop van noord-holland	417	gooi en vechtstreek	665
32	oost-groningen	391	noord-groningen en eemsmond	659
33	zuidwest-drenthe	385	midden-brabant	564
34	friesland-noord	383	noordwest-veluwe	530
35	zuidwest-friesland	380	zuidwest-friesland	517
36	walcheren	273	zuid-holland-oost	505
37	oosterschelde regio	255	walcheren	427
38	noordwest-veluwe	230	oosterschelde regio	367
39	zeeuwsch-vlaanderen	165	zeeuwsch-vlaanderen	356

^a Regions in bold are selected covenant regions. The selection principle is presence in the top 10 of at least one of the two lists of new or total number of dropouts in 2004-05.

^b Regions in italic are the two self-selected covenant regions that were interested in signing a covenant.

Appendix B: Descriptive statistics by RMC region

Table B1 Dropout probability and probability of return of previous dropouts by region, source: BRON

Region	Dropout probability 2005/06	Dropout probability 2006/07	%-change in number of dropouts 06/07- 05/06	Return probability of old dropouts in 2006/07 ^a
Average covenant regions	4.71	4.51	-3.2	23.05
Agglomeratie Amsterdam	6.35	5.65	-6.8	25.98
Rijnmond	5.78	5.76	-4.4	23.91
Haaglanden	5.55	5.28	-4.0	21.48
Zuid-Limburg	4.75	4.50	-5.1	21.76
Flevoland	4.66	4.94	+8.6	23.87
Utrecht	4.51	4.60	+2.7	22.36
West-Brabant	4.35	4.12	-3.7	21.08
Zuid-Holland-Zuid	4.28	4.10	-3.8	20.55
Arnhem/Nijmegen	4.00	4.09	+4.0	24.62
Centraal en westelijk Groningen	3.88	3.83	-1.2	26.44
Zuidoost-Brabant	3.83	3.60	-4.6	20.14
Noordoost-Brabant	3.66	3.57	-1.1	20.99
Limburg-Noord	3.65	3.23	-10.5	24.91
Twente	3.47	3.10	-5.8	22.32
Average non-covenant regions	3.95	3.79	-3.2	22.43
West-Kennemerland	5.04	4.69	-9.9	25.56
Midden-Brabant	4.97	4.99	+0.7	18.37
Kop van Noord-Holland	4.73	3.76	-16.6	27.27
Noord-Kennemerland	4.71	4.39	-6.1	28.29
Walcheren	4.70	4.62	-1.6	19.09
Gooi en Vechtstreek	4.54	4.60	+3.2	25.79
Westfriesland	4.53	4.38	+1.3	24.02
Oost-Groningen	4.11	3.63	-11.2	25.81
Friesland-noord	4.06	4.00	-2.1	25.90
Stedendriehoek	4.05	3.51	-12.7	24.51
Zuidoost-Drenthe	4.04	3.63	-7.5	20.79
Zeeuwsch-Vlaanderen	4.03	3.43	-13.4	20.77
Eem en Vallei	4.02	4.05	+3.3	18.22
Zuid-Holland-Noord	3.99	3.94	+0.7	21.81
Oosterschelde regio	3.98	3.46	-13.5	19.48
Zuid-Holland-Oost	3.74	3.59	-6.1	22.66
Friesland-oost	3.64	3.60	-1.3	24.53
Noord-Groningen en Eemsmond	3.42	2.74	-20.0	23.53
Noordwest-Veluwe	3.40	3.75	+15.0	15.86
Rivierenland	3.38	3.60	+8.1	16.94
Oost-Gelderland	3.35	3.12	-7.1	22.46
Zuidwest-Friesland	3.13	2.53	-19.4	21.16
Noord- en midden-Drenthe	3.00	3.03	+4.3	29.27
Zuidwest-Drenthe	3.00	3.04	+4.4	18.55
IJssel-Vecht	2.99	3.04	+3.1	21.23
Average all regions	4.42	4.23	-3.2	22.82

^a Previous dropouts include those dropouts that dropped out during school year 2005-06. Return is measured as per October 1 of 2007.

Appendix C: regression outcomes

Table 7.1 Complete diff-in-diff regression outcomes of effect of covenants on probability of school dropout in post-treatment year 2006-07, estimates in percentage points^a

Variable	Notes	Estimate	Significance ^b
DxT	effect estimate of covenants	- 0.09	n.s.
D	1 if treatment region ; 0 if non-treatment region	0.02	n.s.
T	1 if 2006/2007 (post-treatment year); 0 if 2005/2006 (pre-treatment year)	- 0.02	n.s.
Personal characteristics			
Age	age in years	1.02	***
Sex	1 if female; 0 if male	- 0.53	***
Of school age	1 if of school age (<=16 years); 0 if not	- 0.78	***
Cumcbs_2 ^c	From Suriname	1.11	***
Cumcbs_3	From Aruba/Nederlandse Antilles	1.18	***
Cumcbs_4	From Turkey	- 0.21	n.s.
Cumcbs_5	From Morocco	0.22	n.s.
Cumcbs_6	Other non-western	0.75	***
Cumcbs_7	Western	1.16	***
Environment			
Poverty accumulation area	1 if inhabitant of poverty accumulation area; 0 if not	0.79	***
Urb_2 ^d	Medium-sized municipality	- 0.62	**
Urb_3	Small-sized municipality	- 1.28	***
RMC budget per student	central government contribution	- 0.03	n.s.
Education level/type			
Education_2 ^e	vmbo b	3.76	***
Education_3	vmbo k	0.12	n.s.
Education_4	vmbo g+t	- 0.61	***
Education_5	havo	- 2.69	***
Education_6	vwo	- 3.38	***
Education_7	MBO-BOL1	30.80	***
Education_8	MBO-BOL2	9.28	***
Education_9	MBO-BOL3	0.84	**
Education_10	MBO-BOL4	- 1.52	***
Education_11	MBO-BBL1	36.30	***
Education_12	MBO-BBL2	10.94	***
Education_13	MBO-BBL3	- 0.05	***
Education_14	MBO-BBL4	- 1.62	***
Exam class	1 if in exam class; 0 if not	1.08	***
N		2 592 352	

^a Model specification is cf. model 4, Table 5.1, estimation sample is full sample.
^b n.s. denotes estimate is not significant; */**/*** denotes estimate is significant at 1/5/10 per cent significance level.
^c The reference category is Dutch.
^d The reference category is the G4, the four largest cities in the Netherlands (i.e. Amsterdam, Rotterdam, Utrecht and The Hague)
^e The reference category is a participant of first class of secondary education ('brugklas').

Table C2 Regression discontinuity estimates of effect of covenants on probability of return of 'old' dropouts into education, estimates in percentage points, estimates on discontinuity sample 1^a

Variable	Notes	Estimate	Significance ^b
D	1 if treatment region ; 0 if non-treatment region	1.57	n.s.
Personal characteristics			
Age	age in years	- 29.38	***
Sex	1 if female; 0 if male	- 18.74	***
Of school age	1 if of school age (<=16 years); 0 if not	1.39	n.s.
Cumcbs_2 ^c	from Suriname	8.07	***
Cumcbs_3	from Aruba/Nederlandse Antilles	9.36	***
Cumcbs_4	from Turkey	6.79	***
Cumcbs_5	from Morocco	6.97	***
Cumcbs_6	Other non-western	5.26	***
Cumcbs_7	Western	1.03	***
Environment			
Poverty accumulation area	1 if inhabitant of poverty accumulation area; 0 if not	- 0.36	n.s.
Urb_3 ^d	Small-sized municipality	- 0.59	n.s.
RMC budget per student	central government contribution	0.45	n.s.
Education level/type			
Education_2 ^e	vmbo b	0.48	n.s.
Education_3	vmbo k	8.90	***
Education_4	vmbo g+t	12.01	***
Education_5	havo	20.80	***
Education_6	vwo	28.05	***
Education_7	MBO-BOL1	- 2.39	n.s.
Education_8	MBO-BOL2	3.16	n.s.
Education_9	MBO-BOL3	7.19	**
Education_10	MBO-BOL4	13.83	***
Education_11	MBO-BBL1	- 2.63	n.s.
Education_12	MBO-BBL2	- 2.25	n.s.
Education_13	MBO-BBL3	2.36	n.s.
Education_14	MBO-BBL4	5.64	n.s.
Exam class	1 if in exam class; 0 if not	3.99	*

N

^a Included 'old' dropouts are those that dropped out during pre-treatment year 2005-06. We refer to section 5.2 for a description of the construction of the first discontinuity sample.

^b n.s. denotes estimate is not significant; ***/** denotes estimate is significant at 1/5/10 per cent significance level.

^c The reference category is Dutch.

^d The reference category is medium sized municipalities.

^e The reference category is a participant of first class of secondary education ('brugklas').

Appendix D: sensitivity analyses

Table D1 Sensitivity analyses, diff-in-diff estimates of effect of covenants on probability of school dropout in post-covenant year 2006/07, estimates in percentage points				
	S1	S2	S3	S4
Effect estimate ^a	0.187 (n.s.)	- 0.427 (n.s.)	- 0.058 (n.s.)	- 0.068 (n.s.)
Standard error ^b	0.469	0.232	0.096	0.099
Sample	mbo 1 and 2; all regions	vmbo exam year; all regions	all education levels; two self-selected covenant regions left out	complete: all regions and all education levels
Controls	standard set (education level, background, environment)	standard set (education level, background, environment)	standard set (education level, background, environment)	standard set + additional neighbourhood characteristics ^c
Number of students	227 086	220 695	2 434 703	2 100 963

^a n.s. denotes estimate is not significant. The chosen significance level to determine significance is 10 per cent.
^b All reported standard errors are robust standard errors, correcting for clustering at the RMC region level.
^c Additional neighbourhood statistics include average fiscal yearly income, share of low incomes, share of high incomes, density of addresses, average housing value and percentage of non-western inhabitants. These statistics are known at a 4-digit postal code level.