

# Expanding schooling opportunities for 4-year-olds\*

Edwin Leuven<sup>†</sup>      Mikael Lindahl      Hessel Oosterbeek  
Dinand Webbink

## Abstract

We use a novel quasi-experimental strategy to estimate the effect of expanding early schooling enrollment possibilities on early achievement. It exploits two features of the school system in Holland. The first is rolling admissions; children are allowed start school immediately after their 4th birthday instead of at the beginning of the school year. The second is that children having their birthday before, during and after the summer holiday are placed in the same class. These features generate sufficient exogenous variation in children's enrollment opportunities to identify its effects on test scores. Making available one additional month of time in school increases language scores of disadvantaged pupils by 0.06 of a standard deviation and their math scores by 0.05 of a standard deviation. For non-disadvantaged pupils we find no effect.

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\*An earlier version of this paper circulated as Leuven et al. (2004). Leuven and Oosterbeek are both affiliated with the School of Economics of the University of Amsterdam and the Tinbergen Institute. Leuven is also affiliated with CEPR, CREST and IZA. Lindahl is affiliated with Uppsala University, IFAU and IZA. Webbink is affiliated with the CPB Netherlands Bureau for Economic Policy Analysis, We acknowledge comments from seminar participants in Amsterdam, Padova, Paris and Stockholm.

<sup>†</sup>Corresponding author: e.leuven@uva.nl

# 1 Introduction

Impact estimates of early childhood interventions are of particular interest as it has been argued that early interventions have dynamic complementarities in human capital production; early learning makes subsequent learning easier (Heckman, 1999). Most evidence on the effectiveness of early interventions comes from targeted early childhood programs in the US.<sup>1</sup> This paper investigates whether these results carry over to the more general setting of increasing the amount of regular education for young children.

Many developed countries provide free high-quality education for 4-year-olds.<sup>2</sup> This is also the case for the Netherlands where children can start school when they are 4 years old, and where compulsory schooling begins when children turn five. The program consists of 24 hours per week during 41 weeks per year. The staff in front of the classroom are certified primary school teachers and are paid according to standard primary school wage scales. The curriculum consists of structured learning activities, and typically children will have started to read and write by the age of six. Over the period 1995-2001 the annual expenditure per pupil was about 3,500 Euro. To compare, in the United States state spending on public prekindergarten programs was \$3,551 per child in 2004/2005 (Barnett et al., 2005).

We estimate the impact of expanding enrollment opportunities (i.e. the intention to treat) around age 4 on subsequent achievement at age 6. An important contribution of our study is that we are able to separate this effect from age effects. For identification we exploit institutional features of the Dutch early schooling system that generate arguably exogenous variation in the time young children can spend in school. The first feature is that children are allowed to start school immediately

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<sup>1</sup>Currie (2001) summarizes the beneficial short-term and long-term effects of small-scale intensive interventions such as the Perry Preschool project, the Chicago Child-Parents Centers and the Carolina Abecedarian Project. Large-scale targeted programs with lower per pupil expenditures also appear to have positive short-term and long-term effects. Garces et al. (2002) report beneficial effects on various later outcomes from participation in Head Start.

<sup>2</sup>Some countries, like France for example, provide state funded preschooling for 3-year-olds.

after their fourth birthday as opposed to the beginning of the school year as most other countries. The second feature is that children having their birthday before, during and after the summer holiday are placed in the same class. Conditional on age these features generate a difference of up to 11 weeks in the time children can spend in school, which constitutes about 15 percent of the amount of schooling they may have had at the moment of the tests. The key identifying assumption is that birth patterns are independent of other factors that affect test scores. We present evidence that birth patterns are unrelated to observed background characteristics, suggesting that the identifying assumption is satisfied.

Our main finding is that making available one additional month of time in school increases language scores of disadvantaged pupils by 0.06 of a standard deviation and their math scores by 0.05 of a standard deviation. For non-disadvantaged pupils we find no effect.

In the next section we briefly review the related literature and discuss how these analyses relate to our analysis. Section 3 documents the details of the Dutch regulations regarding school enrollment age and the scheduling of holiday periods and describes how we use this in our estimation framework. Section 4 describes the data and presents descriptive statistics. Section 5 presents the estimated effects of increasing early school availability on achievement. Section 6 concludes.

## 2 Related literature

This study is most closely related to the literature that studies the impact of school entry, and schooling at young ages. To put our contribution into perspective it is useful to assume that a person's amount of human capital is an additive function of two components. The first component is solely determined by calendar age ( $age$ ). The second component relates to human capital accumulation while in school, which is assumed to depend on the amount of time spent in school ( $s$ ) and the age at school entry ( $age_e$ ). A general specification capturing this is the following:

$$Y(\text{age}, s, \text{age}_e) = Y^0(\text{age}) + Y^1(s, \text{age}_e)$$

where  $Y^0$  captures both absolute and relative age effects. For young people it seems reasonable to assume this function to be increasing and possibly concave (after a certain age this function may however be decreasing). The amount of human capital that has been accumulated in school,  $Y^1$ , obviously depends on the amount of time spent in school,  $s$ . In addition it may depend on the age at which pupils enroll in school, measured by their age at school entry,  $\text{age}_e$ . Various papers argue that the effectiveness of time in school depends on age at school entry, for instance because a pupil's span of attention varies with age or teachers' instruction is aimed at mean age in the class.

When estimating the impact of these components on outcomes, one important complication arises from the fact that age, age at entry and length of schooling are linearly dependent:<sup>3</sup>

$$\text{age} = \text{age}_e + s$$

The second complication is that age at entry and length of schooling are choice variables, and this endogeneity needs to be accounted for in the analysis. The key challenge is thus to identify sources of exogenous variation that affect one of the determinants of  $Y$  without affecting at the same time one of the other determinants. How do previous papers deal with this?

A series of recent papers have attempted to estimate the effect of age on test scores. Bedard and Dhuey (2006); Datar (2006); Strøm (2004) use data from school systems characterized by one annual school entry date.<sup>4</sup> Moreover these papers use data from pupils placed in the same grade level and tested at the same date. By implication, pupils will all have identical amounts of time in school, meaning that

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<sup>3</sup>For another discussion of the consequences of this identity for identification of schooling effects, see Hansen et al. (2004).

<sup>4</sup>For a survey of older papers, see Stipek (2002).

there is no variation in  $s$ . Consequently, these studies estimate functions of the following form:

$$Y(\text{age}, \text{age}_e | s) = Y^0(\text{age}) + Y^1(\text{age}_e | s)$$

and find that older children perform better. The fundamental problem faced by these studies is that with the data at hand,  $\text{age}$  and  $\text{age}_e$  are perfectly correlated. Hence, it is impossible to disentangle the effects from these two variables. Datar (2006) formulates this explicitly where she states that "the entrance age effect [ $\text{age}_e$ ] is not identified separately from the age effect [ $\text{age}$ ] when we are interested in outcomes of school-age children". Datar and Strøm interpret their results in terms of differences in school entry age, whereas Bedard and Dhuey frame their interpretation in terms of age. Note that testing pupils with different birth dates at different dates in order to keep age at test date constant, does not help identification because this results in a perfect (negative) correlation between age at school entry ( $\text{age}_e$ ) and amount of time in school ( $s$ ).<sup>5</sup>

The estimates in Bedard and Dhuey (2006) are for 4th and 8th graders in 19 OECD countries. The mean estimate across countries indicates about 0.2 unit of a standard deviation (S.D.) higher test score for the oldest in a class. These estimates capture the combined effects of maturity, relative age and starting school at an older age. Mayer and Knutson (1999) and Elder and Lubotsky (2006) use data from the US, which also has one school entry date per year, and thus face the same problem as the previous papers. However, since school cutoffs differ across U.S. states, the variation in age across pupils is due to difference in birth dates and school-cohort cutoffs. Hence, they break the perfect correlation between entry age and age at test date and can therefore include controls for age. Mayer and Knutson include a linear age

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<sup>5</sup>Angrist and Krueger (1992); Dobkin and Ferreira (2006); Fredriksson and Öckert (2006) look at long-term effects of school-start age. If cohort-effects in completed education and earnings are negligible and relative age effects are small, these estimates can more likely be interpreted as true school-age entry effects on long-term outcomes.

term and quarter of birth dummies. The quarter of birth dummies are interpreted as the effect of age at school entry. Starting school a year younger (but having the same amount of schooling and age at the test date) results in an average test score increase of 0.2-0.4 S.D. Elder and Lubotsky instrument for actual kindergarten entrance age with a constructed measure based on (interactions between) birth date and state cutoff laws. They estimate entrance-age effects for children affected by the state entry cutoffs, also controlling for quarter-of-birth dummies. They find entering kindergarten at a later age to lower test scores.

A similar strategy is used by Bedard and Dhuey (2006) when they perform pooled cross-country estimations utilizing that school-entry age regulations varies across countries. Controlling for month-of-birth, they get estimates of similar sizes as for within countries (about 0.2 S.D.). A potential complication with these approaches is that the quarter of birth dummies may pick up non-linear age effects.

Another strand of papers has attempted to estimate the effect of time in school on test scores.<sup>6</sup> An early example of such a study is Cahan and Cohen (1989). These authors collected test score data for over 12,000 pupils in grades 4, 5 and 6 in Israel, where children born in the same calendar year also start school at the same day. Since they have data from adjacent grades and pupils are tested at the same time using the same test, they can estimate effects of both age and time in school. Pupils placed in the same grade level have the same amount of schooling but differ in age, whereas pupils born in adjacent months but placed in different grade levels have (almost) the same age but differ in their amounts of schooling (and in their school starting age). The findings indicate that the effect of an additional year of schooling on test scores is about twice the effect of being one year older (about 0.30 versus 0.15 S.D. on average over the 12 tests). Note that in this design, the effect of age captures both the effect of chronological age and the effect of age at school entry. Cahan and Cohen discuss their age effects in terms of the effects of chronological age,

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<sup>6</sup>For surveys of the development psychology literature on the estimation of age and schooling effects, see Ceci (1991) and Stipek (2002).

not in terms of effects of age at school entry. Since the effect of schooling captures both effects from time in school, school entry age, as well as relative age effects, the 0.3 S.D.-estimate is likely an overestimate of spending an additional year in school as a consequence of starting school one year earlier.

Recently Gormley, Jr. and Gayer (2005); Gormley, Jr. et al. (2005) used the same design as Cahan and Cohen and estimate the impact of Oklahoma’s pre-K program for 4-year-olds in Tulsa on cognitive/knowledge test scores, motor skills and language scores. Attendance increases test scores by approximately 0.4 S.D. Because there is no full compliance (as in Cahan and Cohen’s analysis for Israel) the analysis recovers impact estimates for the treated.

Berlinski et al. (2006a) estimate the impact of pre-primary education on 3rd grade test scores in Argentina. They exploit variation in treatment intensity across regions and cohorts stemming from a large-scale program aimed at increasing pre-school attendance for children, and find that one year of pre-primary school increases test scores by 0.23 S.D.<sup>7</sup>

Hansen et al. (2004) and Cascio and Lewis (2006) estimate the impact of time in school on scores at military qualification tests (AFQT) in the US. The test is administrated at the same date for one birth cohort, even though students attend different grades. Cascio and Lewis extends the approach in Cahan and Cohen by also using variation in the states’ school entry cutoffs to estimate the effect of time in school. Hence, they estimate the schooling effect that is due to variation in school entry age across states. This makes it possible to allow for independent effects of age by including quarter-of-birth dummies. They find that an additional year of formal education raise scores for minority groups with about 0.3 S.D.

Taken together, the results are remarkably similar across samples in different

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<sup>7</sup>Berlinski et al. (2006b) finds long-term effects of pre-school attendance (retrospectively collected) for Uruguay, exploiting variation in pre-school attendance between siblings. Dhuey (2007) uses U.S. state-variation in adoption of a policy to publicly subsidizing kindergarten to estimate the effects of enrollment in kindergarten on later outcomes. She finds positive effects on academic and labor market outcomes, especially for black individuals and those from low SES backgrounds.

countries. An additional year of education raises test scores with about 0.2-0.4 S.D.

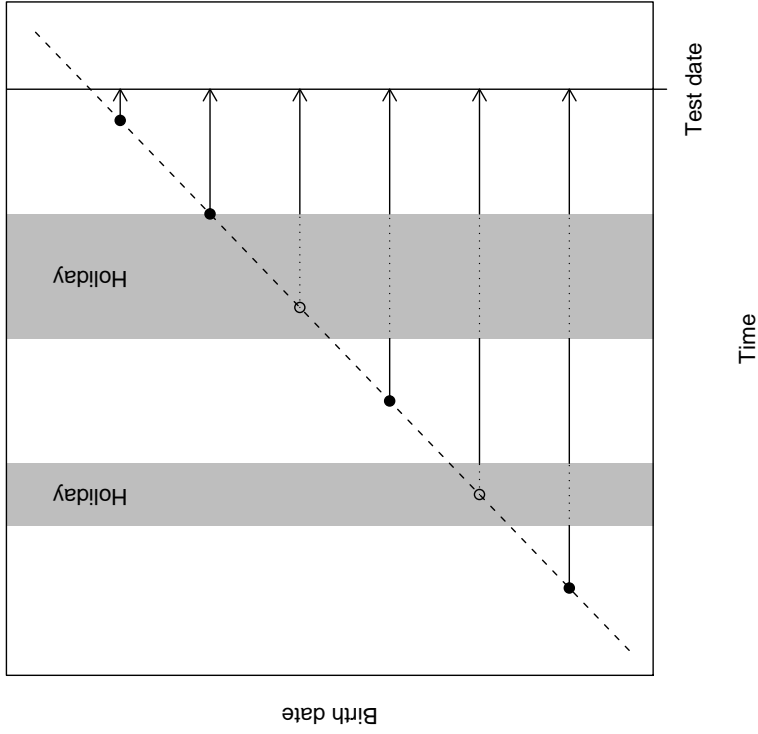
The next section explains how the timing of school holidays in Dutch primary schools causes variation in the amount of time in school independent of age and allows us to estimate the effect of making (pre)school available at age 4.

### 3 Background and identification strategy

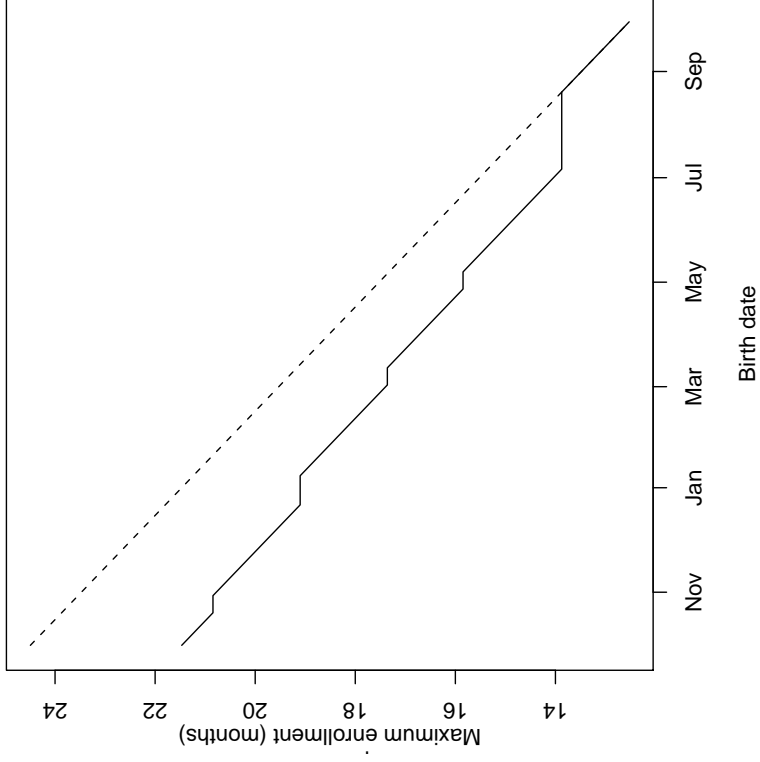
Dutch primary schools consist of 8 grades covering the age groups of 4 to 12-year-old children. While in most countries children typically enter primary school at the same date, in the Netherlands the rule is that children are *allowed* to enroll in primary school the first school day after their 4th birthday, while enrollment is *compulsory* from the first school day of the month after the child reached the age of 5 onwards. About 98 percent of the children start school before their 5th birthday. When exactly between their 4th and 5th birthday a child actually enrolls is up to the parents. The total number of schooldays a child has attended at a given date is therefore to some degree a choice variable. The rule that enrollment is permitted at age 4 and compulsory at age 5 determines the maximum and minimum amounts of time a child can spend in primary school.

The second important feature where our identification builds on, is that a school year cohort in the Netherlands consists of everyone born between October 1 of a given year and September 30 of the next year. At the same time a school year runs from summer holiday to summer holiday. The formal rule is that a child who enrolls in school on the first school day after its 4th birthday spends the period until October 1 in grade 1. Then it spends the period from October 1 until the (next) summer holiday again in grade 1. After the summer holiday the child continues in grade 2. Together these features produce variation in the amount of time a child can spend (maximum length of schooling) in school conditional on age.

Panel (a) of Figure 1 provides a stylized illustration of how these institutions



(a) Timing of enrollment and max. length of schooling



(b) Relation between birth date and max. length of schooling for a given cohort

Figure 1: Illustration of identification strategy

generate maximum length of schooling.<sup>8</sup> Length of schooling at the time of testing is at its maximum when enrollment takes place at the earliest possible moment which is at age four, i.e.  $age_e = 4$ . For any given birth date this is then the length of the segment between entry at age four and the moment of the test. However, children are out of school during holidays and we therefore need to subtract the holidays from this schooling spell. This amounts to adding up the solid segments in panel (a). Doing this with the actual school holiday data for every birth date (for a given cohort) leads to a relationship between a child's birthday and its maximum length of enrollment in school as shown in panel (b) in Figure 1.<sup>9</sup> The flat segments correspond to holiday periods and the downward sloping segments represent school periods. For children having their fourth birthday on the same downward sloping segment, maximum length of schooling in school varies one-to-one with age; being one day older adds one day to the maximum length of schooling. Differences in test scores between two otherwise identical pupils within a segment are attributable to their difference in age as well as to their difference in maximum length of schooling. Maximum length of schooling does not vary across children having their fourth birthday on the same flat segment. Consequently, differences in test scores between two otherwise identical pupils from these segments are solely attributable to differences in their age.

Children in the after-summer-group can have, *conditional on their age*, at most eleven weeks more time in school than children born in other periods. This is most easily seen when we compare the extrapolation of the line segment for the after-summer-group (the dashed line in Figure 1 which ignores holidays) with the solid line segments for the before-summer-group. The vertical distance between these two lines is the difference in maximum length of schooling given age.

Maximum length of schooling at the test date is by definition equal to the dif-

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<sup>8</sup>This figure abstracts from weekends.

<sup>9</sup>In practice the exact timing of the summer holidays varies somewhat from year to year and is different for three different regions (North, Middle and South). In all cases, however, the summer holiday ends well before October 1, and hence there are always children at a grade level who have their 4th birthday between the end of the summer holiday and October 1.

ference between age at the test date, age at school entry and the length of holiday between entry and test:  $s^{max} = age - 4 - h$ . Conditional on age, the amount of holidays between age 4 and the moment of the test ( $h$ ), breaks the perfect correlation between maximum length of schooling and chronological age.

Since the identification in our analysis comes from a non-linearity there are limits to the extent to which we can control for age. Given that we only consider children within the same cohort this might not be an important problem. We show below that moving from a linear to a quadratic age specification does not affect our results, higher order terms however capture the non-linearity we are exploiting and our identification breaks down.

We will estimate the following model

$$Y = Y(s^{max}, age)$$

From the figure it is clear that we use cross-sectional variation within the entire school year cohort. Since the variation we exploit is conditional on age, we need to control sufficiently flexible for the effect of differences in age on test scores. Once we do this any remaining differences in test scores between children is attributable to differences in maximum length of schooling. In the analysis we will estimate the following specification

$$y_i = \alpha + \beta_{RF} \cdot s_i^{max} + \delta \cdot age_i + \lambda \cdot age_i^2 + x_i' \gamma + \epsilon_i \quad (1)$$

where  $y_i$  is the 2nd grade test score,  $s_i^{max}$  is the maximum length of schooling for pupil  $i$ ,  $x_i$  are year and region indicators and their interactions, and individual characteristics. The identifying assumption is

$$E[s^{max} \cdot \epsilon | age, age^2, x] = 0$$

Some have argued that the timing of births during the year may depend on unobserved characteristics of the parents which have an effect of children's achievement. This point was raised by Bound et al. (1995) in their comment on the use of quarter of birth as an instrumental variable for years of schooling by Angrist and Krueger (1991). For the 18 countries in their dataset Bedard and Dhuey (2006) find no evidence that more educated mothers target birth so that their children are the oldest in the class. In Section 5 we will present similar evidence for our dataset which indicates that there is no systematic relation between parents' levels of education and children's maximum length of schooling.

One might be interested in the effect of making enrollment compulsory, and estimate the following equation using 2SLS

$$y = \alpha + \beta_{IV} \cdot s_i + \delta \cdot age_i + \lambda \cdot age_i^2 + x_i' \gamma + \epsilon_i \quad (2)$$

where  $s_i$ , actual length of schooling, is instrumented with  $s_i^{max}$ . Unfortunately our dataset contains only limited and unreliable information on actual enrollment. For this reason we will report estimates of the reduced form model,  $\hat{\beta}_{RF}$ , which as we argued are interesting from a policy point of view in their own right.

We can, however, infer something about  $\beta_{IV}$  even without information on actual enrollment since the IV estimate is given by the following expression

$$\hat{\beta}_{IV} = \frac{\hat{\beta}_{RF}}{\hat{\pi}_{FS}} \quad (3)$$

The question then is what value the first-stage estimate associated with equation (2),  $\hat{\pi}_{FS}$ , has. Suppose we increase enrollment opportunities by 1 month. Those who enroll immediately when they turn 4 before the expansion takes place - the constrained group  $c$  -, can increase their enrollment by at most 1 month. For this group we can thus infer that  $0 \leq \hat{\pi}_{FS,c} \leq 1$ . Those who delay enrollment before the expansion - the unconstrained group  $u$  -, can increase their enrollment by at

most 1 month plus the delay. While possible in principle, this seems an unlikely response because it requires that people respond to loosening a constraint that was not binding for them. A more likely scenario is that those who enter at, say, age 4 years and 3 months when they are allowed to start at age 4 years and 0 months, will also enter at age 4 years and 3 months when they are allowed to start at the age of 3 years and 11 months. For this group we then would have  $\hat{\pi}_{FS,u} = 0$ .

Under the behavioral assumptions implicit in the above discussion, the overall estimate of  $\hat{\pi}_{FS}$  is thus a weighted average of 0 and a value between 0 and 1, so that we have that  $0 \leq \hat{\pi}_{FS} \leq 1$ . It then follows from (4) that

$$\hat{\beta}_{IV} \geq \hat{\beta}_{RF}$$

and our reduced form estimate is a lower bound on the effect of making schooling compulsory at a lower age.

## 4 Data and descriptive statistics

### 4.1 Data

This paper uses data from five waves of the PRIMA survey. This bi-annual survey contains information on Dutch pupils who were enrolled in grades 2, 4, 6 and 8 in the school years 1994/1995, 1996/1997, 1998/1999, 2000/2001 and 2002/2003. Several survey instruments have been used for collection of the data: administrative sources, tests, and questionnaires for teachers, parents and school headmasters. Each wave contains information of about 600 primary schools and around 55,000 pupils which is approximately 10 percent of the relevant age population. The survey design is such that it samples pupils from grades and not from cohorts. This is unfortunate because grade repeating is a fairly common phenomenon in Dutch primary schools and thus introduces substantial selection issues. Only for advancement from grade

1 to grade 2, grade repeating is not an issue.<sup>10</sup> We therefore restrict the analysis to 2nd graders who are for the first time in grade 2 and, as a consequence, estimate the short term effect.

The outcome measures we use in the analysis are scores on two cognitive tests; one related to counting, classification and order and one related to the understanding of words, sentences and placement of words in sentences.<sup>11</sup> These tests were developed for the Dutch government by testing agency CITO in order to measure pupils' readiness for arithmetic and reading, respectively. We will therefore refer to these tests as "arithmetic" and "language". The raw scores on these measures are based on tests which are especially designed for this data collection. From year to year the tests for the same grade levels are identical. The purpose of this is to compare achievement levels over time. Because the scales of the raw scores have no clear meaning, we transformed these scores for each test into wave specific standardized scores, having mean zero and standard deviation one.

In the estimations we control for gender, education levels of father and mother, year and region. We report results separately for disadvantaged pupils and for non-disadvantaged pupils. The reason for this is that the counterfactual of not going to school is likely to be quite different across these groups because the situation at home is very different. Children in non-disadvantaged families may face a home environment providing them with ample opportunities to develop their cognitive and language skills whereas children in disadvantaged families are more likely to face a home environment providing few learning opportunities. The Dutch funding scheme for primary schools distinguishes two main groups of disadvantaged pupils: native Dutch pupils with low educated parents and pupils with an ethnic minority background with low educated parents. Schools receive extra funding for pupils from

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<sup>10</sup>Less than 3 percent of the second graders in the survey are older than second graders should be, indicating that they repeated a grade. The survey contains no information, however, that identifies whether they repeated first or second grade. Three percent is therefore an upper bound on grade repetition from grade 1 to grade 2.

<sup>11</sup>Documentation containing the test items can be found here: <http://www.dans.knaw.nl/nl/data/danssearch/advancedsearch?inputall=prima>

these groups.

As mentioned above, the PRIMA survey contains only limited and unreliable information on actual enrollment.<sup>12</sup> No questions with regard to actual enrollment have been asked in the 1994 and 2000 and 2002 waves. In 1996 and 1998 parents were asked how old their child was when it entered school. Here parents are supposed to report age in years (4, 5 or 6) and months (0 to 11). Only 40.1 percent of the observations in 1996 and 1998 have non-missing values on both year and month. For the disadvantaged groups, this figure is even worse; 31.2 percent for disadvantaged native pupils and 19.3 percent for disadvantaged minority pupils. Schools have also been asked to report for each pupil the year and the month in which they started to attend school. We thus have two measures of the same variable. Regressing the parents measure on the school measure and vice versa gives the reliability ratios of both measures. The reliability ratio of the parents measure equals 0.62 for all groups together, but reduces to 0.26 for minority pupils. The reliability ratio of the school measure equals 0.23 for all groups together (and 0.27 for minority pupils). The low response rates together with the low reliability ratio's among those who responded make the information on actual enrollment useless for further analysis.

#### *4.2 Descriptive statistics*

Table 1 shows descriptive statistics for the three groups of non-disadvantaged pupils, Dutch disadvantaged pupils and minority disadvantaged pupils. In column (4) we pool the two disadvantaged groups. The average age of the children in second grade at the moment of the test is almost 6 years. On average they could have spent almost 17 months in school at the day of the test. Note that there are no differences between disadvantaged and non-disadvantage children in this respect, as expected if birth patterns are comparable.

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<sup>12</sup>Unfortunately there are also no other sources with information on precise age at which young children start school. For funding purposes schools only have to administer how many pupils attend at October 1st of each year.

Table 1: Descriptive statistics

	Non-	Disadvantaged		
	Disadv. (1)	Dutch (2)	Minority (3)	All (4)
Age (months)	70.33 (3.40)	70.55 (3.40)	70.63 (3.35)	70.59 (3.38)
Max. length of Schooling	16.67 (2.58)	16.82 (2.59)	16.89 (2.56)	16.86 (2.57)
Education Mother				
-Missing	0.09	0.06	0.09	0.07
-Primary	0.01	0.12	0.53	0.34
-Lower Secondary	0.16	0.73	0.23	0.47
-Upper Secondary	0.50	0.08	0.12	0.10
-Higher	0.23	0.01	0.03	0.02
Education Father				
-Missing	0.11	0.15	0.18	0.16
-Primary	0.01	0.10	0.40	0.26
-Lower Secondary	0.19	0.70	0.27	0.47
-Upper Secondary	0.41	0.04	0.11	0.07
-Higher	0.28	<.01	0.05	0.03
Not disadvantaged	1	0	0	0
Disadv. Dutch	0	1	0	0.47
Disadv. minority	0	0	1	0.53
Girl	0.49	0.50	0.50	0.50
Boy	0.51	0.50	0.50	0.50
Language	0.32 (0.95)	-0.04 (0.89)	-0.69 (0.83)	-0.39 (0.92)
Arithmetic	0.28 (0.98)	-0.14 (0.92)	-0.52 (0.86)	-0.34 (0.91)
Number of obs	28,942	11,149	12,744	23,893

Note: mean values with standard deviations in parentheses.

Not surprisingly, the parents of disadvantaged pupils have lower levels of education than the non-disadvantaged. The missing values for mother's education is much lower than for fathers, which could be because mothers are perhaps more likely to have filled in the parent questionnaire. Moreover, mother's education is missing as often for disadvantaged as for non-disadvantaged children.

The bottom rows of Table 1 presents average test scores for the various groups. In column (1) we see that as early as in second grade, non-disadvantaged children score about  $1/3$  of a standard deviation above average. The difference between non-disadvantaged and disadvantaged minority children is 1 standard deviation on the language test, and a bit less (0.80) on the arithmetic test. Comparing non-disadvantaged to disadvantaged Dutch pupils, we observe a difference on both tests of around 0.40.

## 5 Results

### 5.1 *Exogeneity of maximum length of schooling*

Before discussing the estimates of the intention to treat effects that are the focus of this paper, we first examine the exogeneity of maximum length of schooling by regressing this on four dummies for mothers' education, four dummies for fathers' education, a gender dummy, as well as on age, age squared and dummies for years, regions and year-region interactions. We do this for each group separately. Table 2 reports results from regressions of maximum length of schooling on background characteristics controlling for other covariates. These results show that there is no systematic relation between maximum length of schooling and observed background characteristics. This holds for all groups. The F-tests for the joint significance of the background variables have p-values ranging from 0.490 to 0.834. Although our exclusion restriction only needs to hold conditionally on socio-economic status, we also checked whether our instrument is independent of observed characteristics in

the full sample. These results are reported in column (5) and the p-value on the F-test equals 0.369. This evidence therefore supports the identifying assumption that maximum length of schooling is orthogonal to unobserved characteristics related to achievement. We can therefore reject that maximum length of schooling varies systematically with these background variables which affect test scores.

### *5.2 Effects of expanding schooling opportunities*

Table 3 presents estimates of the effect of maximum length of schooling on pupils' language scores. Results are presented separately for the various groups of pupils and for two different specifications. The first specification includes a linear age term, the second specification also includes age squared as control variable. All specifications control for the full set of background characteristics.<sup>13</sup>

Notice first that the differences between the results from the specifications without and with the squared age term are small. We can never reject the hypothesis that the effect of this squared age term is equal to zero. This indicates that a linear specification of the effect of age on language scores is accurate for the given population. For the non-disadvantaged pupils the intervention has no impact on language scores. The point estimates are even negative and significantly so in the first specification. This suggests that an expansion of schooling opportunities will not be beneficial for this group of pupils. The results are very different for pupils from the disadvantaged groups. For disadvantaged Dutch pupils the impact of an extra month of schooling opportunities equals 5 percent of a standard deviation, but this estimate lacks precision. For disadvantaged minority pupils an extra month of schooling opportunities raises their language scores by 7 percent of a standard deviation. These estimates are significantly different from zero. Since the effects for the two disadvantaged groups are of fairly similar magnitudes (equality cannot be rejected), we pool the two groups in order to gain precision. This gives us the results

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<sup>13</sup>Excluding the background characteristics gives very similar results.

Table 2: Maximum length of schooling and background characteristics

	Non-	Disadv			Pooled
	Disadv.	Dutch	Minority	All	
	(1)	(2)	(3)	(4)	(5)
Education mother (reference category = Missing)					
- Primary	0.005 (0.014)	-0.002 (0.011)	-0.005 (0.007)	-0.005 (0.006)	-0.002 (0.005)
- Lower secondary	0.006 (0.007)	-0.003 (0.009)	0.007 (0.008)	0.002 (0.006)	0.004 (0.004)
- Upper secondary	0.006 (0.007)	0.000 (0.011)	0.002 (0.009)	0.002 (0.007)	0.004 (0.004)
- Higher	0.011 (0.007)	0.028 (0.022)	0.005 (0.014)	0.010 (0.011)	0.009* (0.005)
Education father (reference category = Missing)					
- Primary	-0.010 (0.013)	-0.004 (0.009)	0.006 (0.006)	0.003 (0.005)	0.002 (0.004)
- Lower secondary	-0.006 (0.006)	-0.004 (0.006)	-0.003 (0.006)	-0.004 (0.004)	-0.004 (0.004)
- Upper secondary	-0.007 (0.006)	-0.007 (0.011)	-0.005 (0.008)	-0.006 (0.006)	-0.005 (0.004)
- Higher	-0.007 (0.006)	-0.033 (0.030)	-0.007 (0.011)	-0.011 (0.009)	-0.006 (0.004)
Disadv. Dutch				0.0002 (0.003)	-0.003 (0.003)
Disadv. Minority					-0.003 (0.003)
Girl	0.003 (0.002)	-0.006 (0.004)	0.004 (0.003)	0.0002 (0.003)	0.002 (0.002)
N	28,942	11,149	12,744	23,893	52,835
F-test joint sign.	0.834	0.819	0.490	0.660	0.369

Note: All regressions include 4 year dummies, 2 region dummies and their interactions, age and age squared.

Table 3: Language

	Non-Disadv.				Disadvantaged			
	Disadv.		Dutch		Minority		All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Max. length of schooling	-0.037* (0.021)	-0.021 (0.027)	0.047 (0.032)	0.053 (0.039)	0.074*** (0.029)	0.066* (0.034)	0.061*** (0.021)	0.060** (0.026)
Age	0.086*** (0.016)	0.168** (0.078)	0.024 (0.024)	0.057 (0.116)	-0.003 (0.022)	-0.048 (0.102)	0.010 (0.016)	0.008 (0.075)
Age-squared/100		-0.067 (0.063)		-0.027 (0.092)		0.036 (0.080)		0.002 (0.060)
Marginal effect age		0.074*** (0.020)		0.019 (0.030)		0.003 (0.026)		0.010 (0.020)
R-squared	0.082	0.082	0.084	0.084	0.090	0.090	0.195	0.195

*Note:* All regressions include 4 year dummies, 2 region dummies and their interactions. The background variables are: 4 dummies for mother's education, 4 dummies for father's education, and 1 gender dummy variable. The final two columns also include a dummy for disadvantaged Dutch. The standard errors are corrected for clustering at the school level and are heteroscedasticity robust. The "Marginal effect age" is the derivative with respect to age of the estimated model, evaluated at the mean age. Standard errors of the marginal effect of age are calculated using the delta-method.

in final two columns. The estimate equals 0.06 and is significantly different from zero.

Table 4 reports comparable results from regressions in which the arithmetics score is the outcome variable. Again the results from the specifications with linear and quadratic age terms are very similar, and again we find negative point estimates for non-disadvantaged pupils and positive effects for the two groups of disadvantaged pupils. For the two disadvantaged groups, only the impact estimates from the specifications with the linear age term are significantly different from zero. The estimates from the specifications with the squared age term lack precision. When the two groups of disadvantaged pupils are pooled, the impact estimates from both specifications are significantly different from zero. Disadvantaged pupils gain around 5 percent of a standard deviation on their arithmetics score when schooling opportunities are expanded by one month. That similarity of the results for two independent outcome measures makes it less likely that the results can be attributed to randomness.

We find a significant effect for minority pupils on language and not on arithmetic. This can be explained by the fact that the two largest minority groups in the Netherlands are from Turkish and Moroccan origin. Many parents of these pupils still use Turkish or Moroccan as language at home.

As mentioned above, our identification strategy assumes that a quadratic specification of the age-achievement profiles is sufficient. While this is restrictive it is important to note that the related studies discussed in Section 2, typically include only a linear age term. To further probe the issue of the age controls, we regressed outcome measures on (1) year dummies and other controls but excluding region dummies and the region-year interactions, and on (2) region dummies and other controls but excluding year dummies and region-year interactions. The first specification also uses variation in the timing of holidays between regions to identify the effect of maximum length of schooling (but does not control for region effects), the

Table 4: Arithmetics

	Non-disadv.			Disadvantaged			All	
	(1)	(2)	(3)	(4)	(5)	(6)		(7)
Max. length of schooling	-0.027 (0.020)	-0.007 (0.025)	0.069** (0.035)	0.065 (0.042)	0.052* (0.029)	0.031 (0.037)	0.060*** (0.022)	0.047* (0.027)
Age	0.089*** (0.015)	0.192** (0.084)	0.014 (0.026)	-0.009 (0.122)	0.019 (0.022)	-0.095 (0.105)	0.017 (0.017)	-0.049 (0.078)
Age-squared/100		-0.083 (0.067)		0.018 (0.097)		0.092 (0.086)		0.054 (0.063)
Marginal effect age		0.075*** (0.019)		0.017 (0.032)		0.035 (0.029)		0.026 (0.021)
R-squared	0.093	0.093	0.087	0.087	0.086	0.086	0.123	0.123

Note: See Table 3.

second specification also exploits variation in the timing of holidays between years to estimate the effect of interest (but does not control for year effects). For all groups the estimates from these alternative specifications are very similar to the estimates reported in Tables 3 and 4, and in no single case could we reject equality of the effects from the alternative specification and from the reported specification. This is reassuring and adds to the credibility of the findings.

To put the results in perspective it is important to understand what the learning environment would have been in the absence of enrollment in school. The exogenous variation in maximum length of schooling is caused by the incidence of up to 11 weeks of school holidays. Six of these 11 weeks are in the summer, the other 5 are during the year. Most Dutch families spend 3 to 4 weeks of the summer holiday away from their home (often abroad). The other holiday weeks are typically spend at home. In most cases one of the parents will look after the child(ren) during the holiday weeks that are spend at home. In fewer cases, grandparents or other family will play a role. This implies that around 35 percent of the holiday weeks are spend away from home and the remainder at home with one of the parents or another family-member. This will often also be the situation for children whose school attendance is postponed.

## **6 Conclusions**

This study introduced a novel way to estimate the effect of expanding enrollment opportunities on test scores and identify this separately from the age effect. This was possible due to the specific feature of the Dutch schooling system that allows children to start school when they turn 4. Together with the incidence of school holidays and the fact that a school year cohort consists of children born between October 1 and September 30 of the next year, this generates exogenous variation in enrollment opportunities conditional on age.

For disadvantaged pupils we find that increasing enrollment opportunities by one month increases language scores on average by 0.06 standard deviation and

arithmetic scores on average by 0.05 standard deviation. Non-disadvantaged pupils do not benefit in test scores from expanded enrollment opportunities. This suggests that at age 4 school and home environment are close substitutes in the production of achievement for non-disadvantaged children, whereas for disadvantaged children school provides better learning opportunities than the home environment.

Although these effects are reduced form effects and as such do not estimate the causal effect of enrollment, we argue that they are lower bounds of the effects of making enrollment in primary education compulsory at a younger age.

The test scores are measured around two years later and the effects we measure are therefore relatively short-term effects. Yet, as the results of Garces et al. (2002) show, even if intervention effects on test scores fade out over time there may be long-term effects on other outcome variables.

The 0.05-0.06 standard deviation increase in test scores reported here come at a cost of (depending on the type of disadvantaged pupil) 354 to 541 euro per pupil. Our effect estimate is at the high end compared to the studies reviewed in section 2, and also compares favorably to the costs and effects of Head Start. Currie and Thomas (1995) report an effect of Head Start participation on early test scores of 0.203 of a standard deviation for disadvantaged white children. For Afro-American children they find no significant effects. Participation in Head Start costs approximately \$3,500 per child per year. Increasing opportunities to enroll into primary school at younger ages are therefore an interesting policy alternative to targeted programs such as Head Start.

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