New evidence on the effect of time in school on early achievement

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New evidence on the effect of time in school on early achievement[1]

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[1]This version: June 2004. An earlier version of this paper circulated under the title "The effect of potential time in school on early test scores". Leuven and Oosterbeek are affiliated with Department of Economics, University of Amsterdam and NWO Priority Program 'Scholar', and the Tinbergen Institute. Lindahl is affiliated with Swedish Institute for Social Research (SOFI), Stockholm University. Webbink is affiliated with the CPB Netherlands Bureau for Economic Policy Analysis, We acknowledge comments from seminar participants in Amsterdam, Padova, Paris and Stockholm.

Abstract

This study estimates the effect of expanding enrollment possibilities in early eduction on the achievement of young children. To do so it exploits two features of the Dutch schooling system. First, children are allowed to enroll in school on their fourth birthday. Second, children having their birthday before, during and after the summer holiday are placed in the same class. Together these features generate sufficient exogenous variation in children's potential time in school to identify its effects on test scores. We find that allowing disadvantaged pupils to start school one month earlier increases their test scores on average by 0.06 of a standard deviation. This effect is of the same magnitude for pupils with lower educated parents and for minority pupils. For non-disadvantaged pupils we find no effect. Results are similar for language and math scores.

JEL-codes: I21, I28, J24 Key words: Early childhood intervention, early test scores

1 Introduction

From a theoretical point of view the returns to early human capital investments are higher than investments later in life for a least two reasons: the payoff period is longer, and human capital has dynamic complementarities; early learning makes subsequent learning easier (Heckman, 1999). Given the importance of early human capital investment it is important to understand when to best start making these investments and the extent to which public interventions affect child learning.

Several studies find substantial positive effects of early childhood education programs. 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 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.

An alternative to these targeted early childhood programs can be found in regular education. Starting primary school at a younger age comprises an interesting early childhood intervention in the sense that it provides arguably the smoothest and most natural transition into regular education. However, regulations with respect to the age at which children start school vary across countries. For most OECD countries, including Canada, Belgium, France, Germany, Italy, Spain and a majority of states in the United States, education is compulsory at age 6. In Sweden, Norway, Finland and Denmark education is compulsory from age 7 onwards, whereas for the United Kingdom it is at age 5. Typically all children from the same cohort (typically the calendar year) start compulsory schooling at the first day of the school year.

Most countries have some option for children to start school one year before the compulsory schooling age. In France for example, where schooling is compulsory from age 6 onwards, parents have the possibility to send their children to school starting from the age of 3. If parents can enroll their child earlier, the starting day is usually the first day of the school year. The United Kingdom is a bit more flexible with regard to the moment of entry, where some schools have two or three intakes during the school year, determined by the birth date. Germany is currently discussing to have multiple intakes during the school year. In contrast, the Netherlands and New Zealand have very flexible entry rules. In the Netherlands, children are permitted to attend primary school the first day after their fourth birthday, and are obliged to attend school at age 5. In New Zealand, both thresholds are set one year later. Hence, in both these countries children can enroll during the whole school year.

Governments have therefore a wide choice of institutional arrangements with regard to school starting age, and different countries exploit this freedom at varying degree. Moreover, to the extent that publicly provided education and targetted early childhood interventions are substitutes governments have to decide on the mix of such targetted interventions and regular education. In this paper we address the question whether pupils benefit from being allowed to spend more time in school at young ages. We do this by estimating the effect of (expanding) enrollment possibilities in regular education on early achievement.

Most studies focus on the effect of an additional year of schooling conditional on starting age, and surprisingly few studies have looked at the effect of expanding enrollment at early ages. Related to the analysis presented in this paper is the study by Cahan and Cohen (1989) who estimate the effect of extra time in school on (early) test scores. They collected test score data for over 12,000 pupils in grades 4, 5 and 6 in Israel. Israeli children born in the same calender year start school at the same day. In each grade level children can therefore vary in age by at most one year, and children of very similar ages are placed in different grade levels. Test score differences between children in the same grade identify the effect of age on achievement. Test score differences among children in different grades but of nearly the same age identify the effect of extra time in school. Overall, 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.

A threat to this identification strategy is that especially around the cutoff dates, substantial fractions of pupils repeat or skip a grade. This biases the estimates if retention and skipping are correlated with the outcome of interest - achievement. In an attempt to fix this problem, Cahan and Cohen drop all pupils from the analysis who were born in the months close to the cutoffs. To be still able to identify the difference in test scores around the cutoffs (the effect of an extra year in school), they specify a linear relation between age and test scores within a grade level and extrapolate this relation to the cutoffs.[1]

Mayer and Knutson (1999) study the effect of being exposed to school at an earlier age (while holding the amount of schooling constant). Also in the United States all children born before a certain date are required to enroll in school at the day the school year starts. Within a grade children therefore have the same amount of schooling but differ in the age at which they started school. Children born in the

[1] Moreover, this linear relation (intercept and slope) is restricted to be identical across grades.

third quarter of the year have an estimated enrollment age of 6.20 while children born in the third quarter of the year have an estimated enrollment age of 6.51. For the second and fourth quarters the respective ages are 6.33 and 6.40. Controlling for a linear age effect and family background variables, Mayer and Knutson find that children born in the third quarter have higher reading scores than children born in the other quarters, and have higher math scores than children born in the first quarter. Starting school a year younger (but having the same amount of schooling!) results in a reading score increase of 0.403 of a standard deviation and a math score increase of 0.261 of a standard deviation (p.92). A potential problem with this analysis is that the quarter of birth dummies could easily pick up non-linear age effects.[2]

Recently Strøm (2004) conducted a fairly similar analysis using Norwegian data. In Norway too, all children born in the same calender year enrol in school at the same day. Moreover, grade retention and skipping a grade are extremely uncommon. As a result all 15-16 year old Norwegian students participating in the OECD-PISA achievement tests have had the same exposure to schooling but differ up to one year in age. Differences in age are completely determined by birth dates and not by choices of parents or schools. Regressing achievement on age (or quarter of birth) then shows that older students perform better. Strom interprets this as the effect of starting school at an older age. Contrary to the results Mayer and Knutson obtained with data from the United States, Norwegian students seem to suffer from starting school at a younger age. This interpretation ignores, however, the direct effect of age on achievement. With the available data

[2] Angrist and Krueger (1991) use birth quarters as instruments to estimate the effect of years of schooling on earnings. A key finding is that the endogeneity-corrected return to schooling is at least as large as the conventional (uncorrected) OLS estimate.

it is not possible to disentangle the effects of age at school entry and age at date of test.

The existing studies pertain to countries where all children of the same cohort start school at the same day. In this paper we analyze data from the Netherlands. As mentioned above, Dutch children are allowed to start school the day after they turned 4 years old. Together with the incidence of school holidays and the fact that a school year cohort runs from October 1 to September 30 of the next year, this generates variation in potential time in school conditional on age. To see this, consider the effect of the 6-week summer holiday. Conditional on their age, the children who turn 4 after the summer holiday have six more weeks of potential time in school than those who turn 4 before the summer. The reason for this is that the 6 weeks of holiday are subtracted from the potential enrollment of the pupils who turn 4 before the summer. We exploit this variation to estimate the average effect of increasing potential enrollment by one month on test scores. This requires that relative birth patterns are independent of other factors that affect test scores.

We want to emphasize that, where American children of age 4 are in kindergarten, in the Netherlands children start elementary education at the age of 4 to 5. It is (early) schooling; not only are they in the same school building as, say, 8th graders, but the people in front of the classroom are certified teachers, and there is a curriculum that consists of structured learning activities. These activities are of course adapted to the maturity of the individual pupils, but typically children will have started to read and write by the end of second grade.

The remainder of this paper is organized as follows. Section 2 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 3 describes the data and presents descriptive statistics. Section 4 presents the estimated effects of potential time in school on achievement. These are reduced form estimates. To disentangle the reduced form effects into first stage and second stage effects requires information about actual school enrollment. Unfortunately the information on this key variable is very limited, Section 5 presents the available evidence. Section 6 concludes.

2 Background and identification strategy

This section provides more details about the regulations concerning primary school enrollment in the Netherlands, and describes how we use these rules in our 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 its 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

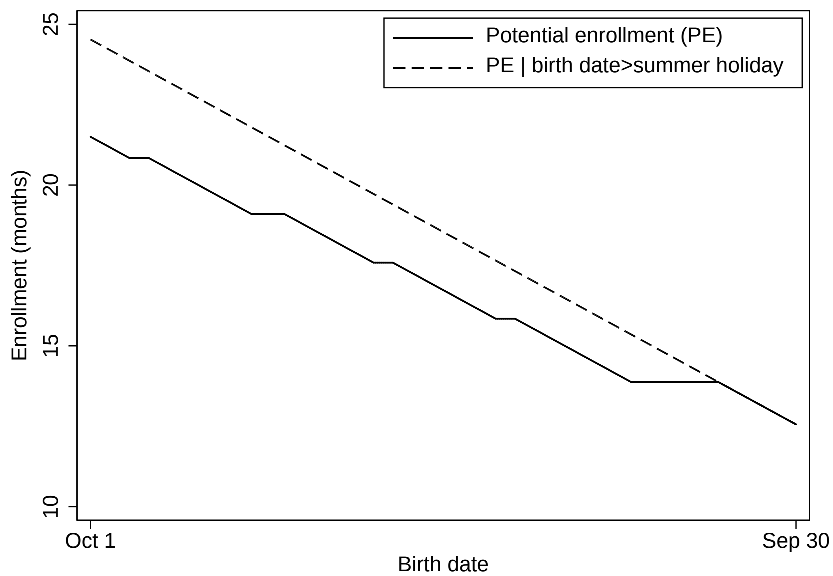

Figure 1: Potential enrollment

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.[3] At any given day in grade 2 - the day of the test - these regulations lead to a relationship between a child's birthday and its potential time in school shown in Figure 1 for one cohort. This figure abstracts from weekends.[4]

Figure 1 has flat and downward sloping segments. The flat segments corre-

[3]A child may repeat grade 1 but this rarely happens.

[4]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.

spond to holiday periods and the downward sloping segments represent school periods. For children having their fourth birthday on the same downward sloping segment, potential enrollment varies one-to-one with day of birth; being one day older adds one day to potential time in school.[5] 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 potential time in school. Potential time in school does not vary with birth date for 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 their difference in age.

Conditional on age, the incidence of the holidays therefore creates variation in potential enrollment. Children in the after-summer-group have, conditional on their age, at most eleven weeks more potential 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) with the solid line segments for the before-summer-group. The vertical distance between these two lines is then the difference in potential time in school given age. From the figure it is clear that we use cross-sectional variation within the whole 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 potential time in school.

[5] We keep on abstracting from the effects of weekends.

In the analysis we will estimate the following (reduced form) equation

$$t_i = \alpha + \beta_{RF} \cdot penroll_i + \delta \cdot age_i + \lambda \cdot age_i^2 + x_i'\gamma + \varepsilon_i \tag{1}$$

where t_i is the 2nd grade test score (language or math), $penroll_i$ is the potential months enrolled in school, x_i are year and regions indicators and individual characteristics. The identifying assumption is

$$E[penroll \cdot \varepsilon | age, age^2, x] = 0$$

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). Even though the evidence that such systematic differences actually exist is mixed, we think that it is not very likely to cause a serious bias in our application since we are not comparing on the basis of different quarters of birth but on the basis birth date relative to the different holidays. Moreover, the exact timing of holidays differs across regions and changes from year to year.

3 Data and descriptive statistics

3.1 Data

This paper uses data from four waves of the PRIMA survey. This biannual survey contains information on Dutch pupils who were enrolled in grades 2, 4, 6 and 8 in the school year 1994/1995, 1996/1997, 1998/1999, 2000/2001. 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 800 primary schools and around 16,000 pupils which is approximately 10 percent of the 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. 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 two achievement scores; one for arithmetic and one for 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.[6] As 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.

At the individual level we control for gender, education levels of father and mother, and two dummies indicating whether the pupil belongs to a disadvantaged group. The Dutch funding scheme for primary schools distinguishes two main groups of disadvantaged pupils: Dutch pupils with lower educated parents

[6] It should be noted that this over time comparability is hampered by relevant differences between waves. In the first wave, tests were taken early in the school year. In the second wave, tests were taken halfway during the school year. In the first two waves tests were taken under the responsibility of an external examiner, while in the third wave the teacher of the class was responsible. These differences give rise to alternative explanations for changes in achievement. Note however that we add year*region dummies in all estimations, and hence these problems are unlikely to affect our results.

and pupils with an ethnic minority background. Pupils not belonging to a disadvantaged group enter the funding scheme with a weight factor equal to unity. Dutch pupils with lower educated parents have a weight equal in to 1.25 and pupils with an ethnic minority background have a weight factor of 1.9. The two dummy variables are derived from these weight factors.

3.2 Descriptive statistics

Table 1 shows descriptive statistics for several samples. The first column shows descriptive statistics for the full sample of pupils in 2nd grade. The average age of the children in 2nd grade at the moment of the test is 70 months. On average they could have been enrolled at most almost 17 months by the time of the test. For one out of ten pupils parental education is missing. About one third of the parents reported lower secondary education and about 30 percent reports that their highest educational attainment is at the upper secondary level. One out of six parents reports only primary education and one out of six reports higher education. Slightly more than half of the sample belongs to the non-disadvantaged category, 23 percent is Dutch and has lower educated parents (the category with a weight factor of 1.25) and 25 percent belongs to an ethnic minority group (weight factor equal to 1.9). The high fractions of disadvantaged pupils are due to oversampling of these categories in the PRIMA data. In the total population of primary school pupils, 18 percent has lower educated Dutch parents and 13 percent has an ethnic minority background.

Not surprisingly, the parents of minority pupils have lower levels of education than the non-disadvantaged Dutch as can be seen in columns (2) to (4). The missing values for mother's education is much lower than for fathers, which could be

Table 1: Descriptive statistics

	All	Non-disadv.	Disadv. Dutch	Minority	Girls	Boys
	(1)	(2)	(3)	(4)	(5)	(6)
Age (months)	70.44	70.33	70.53	70.60	70.41	70.47
	(3.40)	(3.40)	(3.42)	(3.36)	(3.40)	(3.40)
Potential schooling	16.61	16.51	16.72	16.71	16.59	16.63
	(2.49)	(2.47)	(2.54)	(2.48)	(2.49)	(2.49)
Education Mother						
-Missing	0.09	0.10	0.10	0.06	0.09	0.09
-Primary	0.16	0.01	0.53	0.12	0.17	0.16
-Lower Secondary	0.31	0.17	0.23	0.73	0.31	0.31
-Upper Secondary	0.30	0.49	0.11	0.08	0.30	0.30
-Higher	0.13	0.22	0.03	0.01	0.13	0.13
Education Father						
-Missing	0.14	0.12	0.18	0.15	0.14	0.14
-Primary	0.13	0.01	0.40	0.11	0.13	0.13
-Lower Secondary	0.32	0.18	0.27	0.70	0.32	0.32
-Upper Secondary	0.25	0.41	0.10	0.04	0.25	0.25
-Higher	0.16	0.27	0.04	0.00	0.15	0.16
Dutch	0.53	1	0	0	0.52	0.53
Disadv. Dutch	0.23	0	1	0	0.23	0.22
Migrant	0.25	0	0	1	0.25	0.24
Girl	0.49	0.49	0.50	0.50	1	0
Boy	0.51	0.51	0.50	0.50	0	1

Table 2: Test scores, sample averages

		All	Non-disadv.	Disadv. Dutch	Minority	Girls	Boys
		(1)	(2)	(3)	(4)	(5)	(6)
Language	Mean	0.02	0.35	0.00	-0.66	0.13	-0.08
	s.d.	1.01	0.97	0.91	0.84	1.03	0.98
	N	43888	23142	10033	10713	21692	22196
Math	Mean	0.02	0.32	-0.11	-0.52	0.06	-0.02
	s.d.	1.02	1.00	0.93	0.87	1.02	1.02
	N	44252	23513	10038	10701	21859	22393

because mothers are perhaps more likely to have filled in the parent questionnaire.

Table 2 presents average test scores for the various subgroups. It also reports the relevant sample sizes, which differ somewhat between tests because for some pupils we only have a score on one test but not on the other.[7] In column (2) we see that as early as in 2nd grade, non-disadvantaged children score more than 1/3 of a standard deviation higher than the average, where the difference is more pronounced for the language scores. The difference between non-disadvantaged and minority children is 1 standard deviation on the language test, and a bit less (0.85) on the arithmetic test. Comparing non-disadvantaged to Dutch pupils with lower educated parents we see the reverse pattern; here the difference is more pronounced on the math test (0.43) than on the language test (0.35). Girls score about 0.21 standard deviation higher than boys on the language scores. They also score higher on the math test, but here the difference (0.08) is smaller.

[7]Standardization took place before some observations with missing values for relevant variables were dropped from the analysis. This explains why means and standard deviations of test scores are not exactly equal to 0 and 1.

4 Results

This section presents estimates of the reduced form equations that are the focus of this paper. These results are important from a policy point of view since they give the effect of a policy that changes the age at which children are allowed to attend school.

Table 3 presents the results for the whole sample of 2nd graders for various specifications. It is important to properly control for pupil's age, since the variation we exploit is conditional on age. We present results for both the language test and the math test. The standard errors are corrected for clustering at the school level and are heteroskedasticity robust.

We first present the estimate of the regression of language score on potential enrollment without controlling for age. One month more potential time in school is associated with 0.07 of a standard deviation higher test score, a correlation that is highly significant. Of course this estimate picks up the effect of age as well. After conditioning linearly on age in the second rows of the table the effect of one month potential enrollment drops to 0.024 of a standard deviation and is no longer significant. Adding a quadratic in age and additional background variables does not change the estimate. For the math score the results are very similar. The initial estimate is a bit higher than for the language test; 0.085 and drops to about 0.023 after controlling for age, age squared, and the background variables.

From these results we conclude that, although the point estimate is positive, we do not find statistically significant effects of potential time in school on test scores for the whole population of 2nd graders. It is important to note that adding the quadratic in age and the background variables did not change the estimate for

Table 3: Reduced form regressions for all pupils, various specifications

	coef.	s.e.	R-sq
	(1)	(2)	(3)
Language (N=43888),			
(1) None	0.070	(0.002)	0.032
(2) Age	0.024	(0.016)	0.032
(3) Age, Age^2	0.028	(0.018)	0.032
(4) Age, Age^2, Background	0.024	(0.016)	0.229
Math (N=44252),			
(1) None	0.084	(0.002)	0.051
(2) Age	0.023	(0.015)	0.051
(3) Age, Age^2	0.025	(0.017)	0.051
(4) Age, Age^2, Background	0.022	(0.016)	0.193

Note: All regressions include 3 year dummies, 2 region dummies and their interactions. The background variables are: 4 dummies for mother's education, 4 dummies for father's education, 1 disadvantaged Dutch dummy, 1 disadvantage migrant dummy and 1 gender dummy variable. The standard errors are corrected for clustering at the school level and are heteroscedasticity robust.

potential time in school, while adding these variables leads to a huge increase in the explained variance. This suggests that controlling linearly for age is sufficient.

The fact that we do not find significant results for the whole population does not imply that no group benefits from changes in the potential enrollment age. Minority children for instance would be exposed more to the Dutch language if they enroll at an earlier age. To investigate whether disadvantaged groups benefit from expanding their potential enrollment we estimate its effect for different subgroups in table 4. These estimates control for age, age-squared and background characteristics.

The first rows in the table repeat the final average estimates in table 3. The second to fourth rows report estimates for three subgroups: minority pupils, Dutch pupils with lower educated parents, and non-disadvantaged pupils. The results in table 4 show that children from the two disadvantaged groups benefit on average from expanding their enrollment opportunities. One month more potential time in school increases their performance on the language test by about 0.06 of a standard deviation. For non-disadvantaged pupils the point estimate is basically zero and not significant.

For the math scores results are similar. For non-disadvantaged pupils the point estimate is again zero and not significant. For disadvantaged pupils we find that the effect of expanding potential time in school by one month increases the score on the math test by 0.07 for minority pupils, and slightly less, 0.06, for disadvantaged Dutch pupils. This difference is not significant.

To put the results in perspective it is important to understand the counterfactual treatment: what learning environment would children have been exposed to if they would not have been enrolled in primary school? The exogenous variation in potential enrollment 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 5 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.

Table 4: Reduced form regressions, subgroups

	coef.	s.e.	R-sq	N
	(1)	(2)	(3)	(4)
Language				
(1) All	0.024	(0.016)	0.229	43888
(2) Minority	0.060	(0.028)	0.088	10713
(3) Disadvantaged - Dutch	0.062	(0.034)	0.084	10033
(4) Non-disadvantaged	-0.008	(0.023)	0.080	23142
Math				
(1) All	0.022	(0.016)	0.193	44252
(2) Minority	0.071	(0.029)	0.083	10701
(3) Disadvantaged - Dutch	0.061	(0.034)	0.092	10038
(4) Non-disadvantaged	-0.009	(0.022)	0.094	23513

Note: All regressions include 3 year dummies, 2 region dummies and their interactions, a quadratic in age and the full set of background variables listed in the note in table 3. The standard errors are corrected for clustering at the school level and are heteroscedasticity robust.

5 The IV estimate of the effect of time in schooling on test scores

The previous section reports the effect of increasing potential enrollment on test scores. This is like a "voucher" at early ages, parents can decide to take up the enrollment possibility or they may decide to wait. We have found that expanding enrollment opportunities increases performance of disadvantaged pupils. Given this result one might be interested in the effect of making enrollment compulsory.

To address this question one would need to estimate the following outcome equation using 2SLS

$$t_i = \alpha + \beta_{IV} \cdot enroll_i + \delta \cdot age_i + \lambda \cdot age_i^2 + x_i' \gamma + \varepsilon_i \qquad (2)$$

where $enroll_i$ is actual enrollment. Since enrollment is endogenous and under the discretion of the parents it is likely to be correlated with unobserved determinants of pupil achievement. If we want a reliable estimate of the causal effect of making enrollment compulsory at an earlier age we would need to find a good instrument; something that affects enrollment yet satisfies the exclusion restriction that it must be uncorrelated with unobservables affecting test scores.

In theory we have such an instrument, namely potential enrollment. The corresponding first stage equation for $enroll_i$ would then be

$$enroll_i = \eta + \pi \cdot penroll_i + \zeta \cdot age_i + \vartheta \cdot age_i^2 + x_i' \varphi + \omega_i \qquad (3)$$

Given that potential enrollment significantly affects actual enrollment we could then estimate β_{IV}. Unfortunately the PRIMA survey contains only limited and unreliable information on actual enrollment. No questions with regard to actual enrollment have been asked in the 1994 and 2000 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 Dutch disadvantaged pupils and 19.3 percent for 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. For this reason we only reported estimates of the reduced form model and recuperated $\hat{\beta}_{RF}$. As we argued, these results are interesting from a policy point of view in their own right.

Even without information on actual enrollment, we can, however, infer something about β_{IV}. We know that the IV estimate is given by the following expression

$$\hat{\beta}_{IV} = \frac{\hat{\beta}_{RF}}{\hat{\pi}_{FS}} \tag{4}$$

The question then is, what value $\hat{\pi}_{FS}$ has. Suppose we increase potential enrollment 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 $\hat{\pi}_{FS,u} = 0$. According to the responses on actual enrollment in the parents' questionnaires (the least unreliable source), 68 percent of the pupils belong to the constrained group. For Dutch disadvantaged pupils this percentage equals 65 and for minority pupils 43.

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.

6 Conclusions

This study introduced a novel way to estimate the effect of potential time in school on test scores. 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 children born between October 1 and September 30 of the next year, this generates exogenous variation in potential schooling conditional on age.

For disadvantaged pupils we find that increasing potential enrollment by one month increases test scores on average by 0.06 standard deviation. This effect is similar for both Dutch pupils with lower educated parents and for pupils with a minority background. Non-disadvantaged Dutch pupils do not benefit in test scores from expanded enrollment opportunities. The effects are similar for both language and math tests.

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 0.06 standard deviation increase in test scores reported here come at a cost of (depending on the weight factor in the funding scheme) 354 to 541 per pupil. This 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 a interesting policy alternative to targeted programs such as Head Start.

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