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Abstract

This paper evaluates the effects of two subsidy schemes targeted at disadvantaged students in the Netherlands. The first scheme gives schools with at least 70 percent minority students extra funding for personnel. The second scheme gives schools with at least 70 percent students from different disadvantaged groups extra funding for computers and for language materials. The cutoffs at 70 percent provide a regression discontinuity design which we exploit in an instrumental variables framework. Estimates of the effects of the personnel subsidy on achievement of 8th graders in language, math and information processing are positive but never significantly different from zero. Estimates of the effects of the computer subsidy on the same outcome variables are negative and in some cases significantly different from zero.

JEL Codes: I21, I28, J24

Keywords: disadvantaged students, computers, teachers, regression discontinuity

1. Introduction

Children with unfavorable social backgrounds are on average less successful in school than their non-disadvantaged peers. Since educational attainment is believed to have important long-term effects, such differences are considered undesirable in most countries. The Dutch government spends substantial amounts of resources to combat unequal educational outcomes. In the Netherlands the main funding scheme for primary schools weighs students based on their social background. This scheme distinguishes two main groups of disadvantaged students: students with lower educated parents and students with an ethnic minority background. Students not belonging to a disadvantaged group enter the funding scheme with a weight equal to unity. Students with lower educated parents have a weight factor of 1.25 and students with an ethnic minority background have a weight factor of 1.9. A school with all of its students with an ethnic minority background receives therefore almost twice as much funding as a school with all its students being non-disadvantaged. In the total population of primary school students 18 percent have lower educated parents and 13 percent have an ethnic minority background.[1] In 2000 the total amount spent on this compensatory program was 233 million Euro for 450,000 disadvantaged students.

Ever since the implementation of this weighted funding scheme in 1985, policy-makers and researchers have been interested in its effects. A large number of studies of this scheme has been conducted, many of these commissioned by the Dutch government. In a review of 35 of these studies the Dutch General Accounting Office concludes that they do not allow to relate changes in the achievement levels of disadvantaged students to the funding scheme. It is not difficult to pinpoint down the reason behind this failure to determine the program's effects: the funding scheme treats all students with the same social background equally. As a result there is no natural control group, nor is there a possibility to construct a suitable comparison group.

[1] In addition to these two groups, the funding scheme also distinguishes students living in a boarding school or a foster home and whose parents are master of a ship, and students whose parents are transients. These groups enter the funding scheme with weights of 1.4 and 1.7 respectively. The shares of these groups in the population are, however, negligible.

In the years 2000 and 2001, however, the Dutch ministry of education launched two additional programs targeted at the same groups of disadvantaged pupils. One program provides extra funds for personnel to schools where at least 70 percent of the pupils have an ethnic minority background. The other program provides extra funding for computers, software and language materials to schools where at least 70 percent of the pupils are from any of the disadvantaged groups.

In both cases the 70 percent threshold was maintained rather strictly but not perfectly. The two subsidies thus provide fuzzy regression discontinuities, which we exploit in an instrumental variables framework to evaluate the effect of the two programs on students' achievement. To this end we combine administrative data with data from the Dutch educational testing service on achievement of 8th graders in nationwide exams.

For the personnel subsidy we find positive effects which are, however, never significantly different from zero. For the computer subsidy we find negative effects which are in some cases significantly different from zero. We discuss several explanations for our findings. One explanation is that the targeted schools already possess sufficient resources. Funding is not a problem at these schools. While we cannot formally prove that this explanation is the correct one, it is supported by information we obtained during visits at and interviews with targeted schools. This suggests low or even zero marginal benefits from extra resources for schools with around 70 percent of disadvantaged students. Given positive marginal costs, this in turn suggests that the current compensation for these schools is inefficiently high.

The remainder of this paper is organized as follows. The next section provides details of the two programs and describes the data. Section 3 outlines the estimation strategy. Section 4 presents and discusses the empirical findings. The final section summarizes and concludes.

2. Programs and data

2.1 The two programs

In February 2000 the Dutch ministry of Education announced the provision of an additional subsidy for schools with at least 70 percent minority students.[2] Eligibility was based on the percentage of minority students of a school on October 1 1998 as counted in administrative data. The extra funding amounted to 5170 Dutch guilders *per teacher* in the school year 1999-2000 and 5670 in 2000-2001. These sums were paid in May 2000 and March 2001. In November 2000 it turned out that the available budget for the year 2000 was not exhausted, and in December 2000 the eligible schools received an additional 1365 Dutch guilders per teacher. The total payment equaled therefore 6,102.50 Dutch guilders per teacher per year over a two-year period. This annual amount is roughly equal to 9 percent of the average annual gross salary of Dutch primary school teachers, and 11 percent of the annual gross salary of young teachers.

The extra funding was earmarked to improve working conditions. How schools were to spend the budget was not specified as long as they were aiming to improve working conditions. The explanatory memorandum to the ministry's decision lists as examples: a plain financial premium, a bonus to stimulate teachers to work extra hours, compensations for housing costs, traveling costs or childcare facilities, and hiring teaching assistants. The memorandum is ambiguous about a possible continuation of this measure. Although at one place it states that this measure is a pilot and that after August 2001 it will be judged whether continuation is necessary or that reconsideration of the allocation is desirable, at another place it emphasizes that the extra funding is provided for a limited period and that obligations pertaining after this period have to be paid from the regular budget.[3] We

[2] The formal description of this group is students with parents born in Surinam, the Netherlands Antilles or non-English speaking countries outside Europe or whose parents are refugees, and whose father or mother has at most completed lower vocational education or whose primary earnings parent has a job involving physical labor or has no income from labor.

[3] It was recently decided to continue the subsidy. This was based on an evaluation

refer to this subsidy as the Personnel Subsidy.

In November 2000 the ministry announced a second measure, which stipulates that schools with at least 70 percent of their students belonging to any disadvantaged group (that is having a weight factor exceeding 1) receive extra funding in the amount of 209.70 Dutch guilders *per student*.[4] For this scheme the percentage of disadvantaged students of a school was based on administrative data counted on October 1 1999. 36 guilders were earmarked to spend on renewal of language materials, and 173.70 guilders were earmarked to spend on computers and (education) software. This amount was paid only once in December 2000. We refer to this subsidy as the Computer Subsidy.

A common feature of the two subsidies is that they specify a minimum percentage of disadvantaged students schools need to have to qualify for the extra compensation. The Personnel Subsidy requires at least 70 percent of 1.9-students counted at October 1 1998, the Computer Subsidy requires at least 70 percent students with a weight factor exceeding 1 counted October 1 1999. When the percentages of disadvantaged students are below this cutoff level, a school should receive no extra funding at all.

2.2 Data construction

From the ministry of Education we obtained data on the numbers of students in the various weight factor categories for all primary schools in the Netherlands counted at October 1 1998 and October 1 1999. The data also contain information about which schools actually received extra funding and how much these schools received. These administrative data were merged with information about pupils' results in nationwide test. At over 80 percent of Dutch primary schools all students who are in the highest (8th) grade par-

study commissioned by the ministry of Education [Beerends and van der Ploeg, 2001]. In this study the effectiveness of the scheme was measured by targeted schools' responses to the question whether they had the opinion that the funding had an effect. 80 percent of the responding schools answered that the effect was clearly positive. It was, however, not asked to specify the nature of these positive effects. Moreover, this evaluation took place before schools obtained the last tranche.

[4]Formally a student's parents are lower educated if one parent has at most an education at the level of lower vocational education.

ticipate in an annual nationwide test known as the CITO-test. Where CITO is the name of the testing agency that develops the test. The test covers four areas:

- Language: spelling, writing, reading and vocabulary;
- Arithmetic: understanding of numbers, mental arithmetic, percentages, fractions, dealing with measures, weights, money and time;
- Information processing: use of texts and other information sources, reading and understanding of tables, graphs and maps;
- World orientation (optional): applying knowledge in the fields of geography, history, biology, science and form of government.

Testing takes place during three days in February. In 2002 the complete test consists of 240 multiple-choice questions (90 for language, 60 for arithmetic, 30 for information processing, and 60 for world orientation). Based on the number of correct answers relative to others, students receive a score. These scores are used for the assignment of students to different levels of secondary schools. Many secondary schools apply strict thresholds to admit students to the more advantaged types of secondary education. This gives students an incentive to perform well on this test. Furthermore, the average scores of schools' students are currently used as information to judge the quality of primary schools. These average scores are public information and parents use it in their choice of primary school. This gives schools an incentive to prepare their students well for the test. To illustrate the importance of the test, each year all national newspapers as well as national television pay special attention to it. The impression is often that preparing and making this test is the main activity of students in their last two years in primary school (7th and 8th grade).

For our analysis we use pupil-school data of the CITO scores for the years 2000 and 2002. In the empirical analysis, the average scores of a school's pupils on the language, arithmetic and information processing parts serve as the outcome variables. To standardize the estimated effects, the scores are

Table 1: Timing of events

October 1 1998	Reference date for personnel subsidy
October 1 1999	Reference date for ICT subsidy
February 2000	Nationwide test 2000
February 2000	Decision and announcement personnel subsidy
May 2000	Payment of 5,170 NLG per teacher as personnel subsidy
November 2000	Decision and announcement ICT subsidy
November 2000	Decision and announcement of extra tranche personnel subsidy
December 2000	Payment of 209.70 NLG per pupil as ICT subsidy
December 2000	Payment of 1,365 NLG per teacher as extra tranche subsidy
March 2001	Payment of 5,670 NLG per teacher as personnel subsidy
February 2002	Nationwide test 2002

divided by their standard deviations and normalized to mean zero relative to the whole population. Per school we also obtained an index of the average social background of the students who participated in the test. This index was developed by the testing agency. It runs from 1 to 9, with a higher number indicating a poorer background. For individual pupils we know, besides their scores on the four (or three) fields, also their gender.

Table 1 gives an overview of the timing of the relevant events. It is clear from the table that the test of February 2000 took place before schools received extra funding. We use information from this test as a school quality control variable. The 2002 test took place almost two years after the first tranche of the Personnel Subsidy, more than one year after the payments of the extra tranche of the Personnel Subsidy and the Computer subsidy, and almost a year after the payment of the last tranche of the Personnel Subsidy. We use the 2002 test scores therefore as the relevant outcome measure for all of the subsidies.

2.3 Data description

In 1998 there were 7,045 primary schools in the Netherlands. In total 270 (4%) of these schools had at least 70 percent of their students belonging to the 1.9-group thereby qualifying for the Personnel Subsidy. Out of these 270 there were 267 schools that actually received the Personnel Subsidy.[5] Seven schools with less than 70 percent of their students belonging to the 1.9-group (mistakingly) received this subsidy.[6] Considering the eligible schools for the Computer Subsidy there were 7,028 primary schools in the Netherlands of which 564 (8%) had at least 70 percent of their students belonging to one of the disadvantaged groups (weight>1) in 1999. Of these 564 schools 551 received the Computer Subsidy.[7] Sixteen schools with less than 70 percent of their students belonging to a disadvantaged group (mistakingly) received this subsidy.[8] We do not know the reasons for the misclassifications. The empirical analysis compares schools just above the 70%-threshold with schools just below it. Section 3 describes how we deal with schools that received the extra funding although they were not eligible and vice versa.

In our identification setup one might be concerned that schools anticipated the subsidies and accordingly manipulated their relevant shares of disadvantaged students to become eligible. This seems unlikely since they would have needed to anticipate the Personnel Subsidy by one-and-a-half year and the Computer Subsidy by one year. Nevertheless, one check of such manipulation is to compare the distribution of schools around the cutoff level. Manipulation would lead to a drop below the 70 percent cutoff and a rise just after. Figure 1 shows the frequency distributions of schools in the range of 10 percent around the cutoff levels of 70 percent. These distributions give no indication of such manipulation thereby confirming that schools did not

[5] The 3 schools not receiving the personnel subsidy had shares of 1.9 students equal to 1, 0.84 and 0.73.

[6] The shares of 1.9-students at these 7 schools are: 0.69, 0.69, 0.68, 0.67, 0.64, 0.58, and 0.34.

[7] The 13 schools not receiving the personnel subsidy had shares of disadvantaged students equal to: 0.71, 0.75, 0.76, 0.89, 0.92, 0.93, 0.93, 0.94, 0.95, 0.96, 0.97 and 1 (twice).

[8] The shares of disadvantaged students at these 16 schools are: 0.39, 0.43, 0.48, 0.56, 0.56, 0.57, 0.58, 0.61, 0.65, 0.67, 0.67, 0.67, 0.68, 0.69, 0.69 and 0.69.

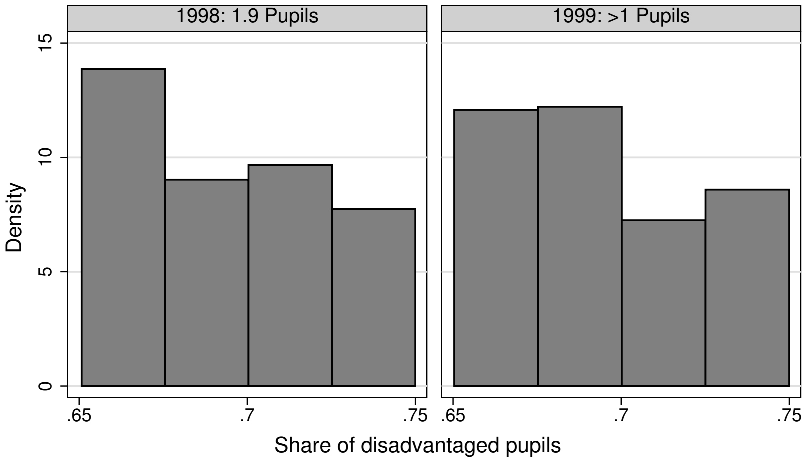

Figure 1: Distribution of schools

anticipate the implementation of the two programs.

Schools that have at least 70 percent minority students in 1998 are also very likely to have at least 70 percent disadvantaged students in 1999. In other words, schools that qualify for the Personnel Subsidy are also very likely to qualify for the Computer Subsidy. In the empirical analysis we focus on schools with their shares of minority students or disadvantaged students at most 5 percentage points away from the 70 percent thresholds. Within these subsamples nearly all schools that qualify for the Personnel Subsidy also qualify for the Computer Subsidy and almost no school that qualifies for the Computer Subsidy qualifies for the Personnel Subsidy.

3. Empirical strategy

3.1 Regression Discontinuity Design

This section discusses the empirical strategy used to identify the effect of the two subsidies. The discussion is phrased in terms of the Personnel Subsidy. The approach for identification of the effect of the Computer Subsidy is identical.

The eligibility rule of the Personnel Subsidy specifies that all schools with at least 70 percent minority students receive the subsidy and all school with less than 70 percent minority students do not receive the subsidy. If there were no exceptions to this rule we would have a so-called sharp regression discontinuity design in which treatment depends in a deterministic way on the share of minority students.[9] Assuming that there are no confounding discontinuities at 70 percent, we can compare the average outcome of the group just above the threshold with the average outcome of the group just below the threshold. This gives an unbiased effect of the average treatment effect for schools with 70 percent of disadvantaged students.

As mentioned in the previous section, a number of schools did receive the Personnel Subsidy although they have less than 70 percent minority students, and some schools did not receive the subsidy although they had more than 70 percent minority students. Because the rule behind these exceptions is unknown (at least to us), this breaks down the sharp regression discontinuity design. There is no longer a deterministic relation between assignment to treatment and the share of minority students.

Shadish, Cook, and Campbell [2002] propose two solutions to such overrides of the cutoff. The first solution is to retain the misclassified cases in the analysis and classify them according to their eligibility status rather than by the their treatment status. According to Shadish et al. this "yields an unbiased estimate ... of the effects of assignment to treatment rather than of treatment itself" (p.227). Their second solution is to eliminate misassigned observations from the analysis. Shadish et al. argue that this solution works well if the deleted cases are in a narrow range around the cutoff and if no more than 5% of the participants are misassigned (p.229). Because the first solution does not give an estimate of the treatment effect of interest, and because the second solution appears to be somewhat ad hoc, we adopt a third approach and use the fuzzy regression discontinuity design in an instrumental variables framework as in Angrist and Lavy [1999] and Van der Klaauw [2002].[10]

[9]Leuven and Oosterbeek [2004] provide a recent application of the sharp regression discontinuity design.

[10]The two methods proposed by Shadish et al. [2002] produce virtually identical results

In a fuzzy regression discontinuity design, assignment to treatment is not deterministic but probabilistic because it may depend on unobserved factors as well. First, denote the share of minority students in school j in 1998 by s_j^{98}. Then the variable denoting treatment eligibility d_j^{98} is defined as follows

$$d_j^{98} = \begin{cases} 1 & \text{if } s_j^{98} \geq 0.7 \\ 0 & \text{if } s_j^{98} < 0.7 \end{cases}$$

The probability of treatment t_j of school j is a function of s_j^{98} with a discontinuity at $s_j^{98} = 0.7$.

$$\Pr(t_j) = f(s_j^{98}, 1\{s_j^{98} \geq 0.7\}) = f(s_j^{98}, d_j^{98})$$

The outcome can be written as follows.

$$E[y_j] = \alpha + \delta t_j$$

where $\alpha \equiv E[y_{0j}]$ is the (average) test score without the subsidy under consideration, and $\delta \equiv E[y_{1j}] - E[y_{0j}]$ is the change in test scores due to the subsidy. Under the assumption of a common treatment effect, it can be shown that δ can be identified by (cf. Hahn, Todd, and Van der Klaauw, 2001):

$$\delta = \frac{y^+ - y^-}{t^+ - t^-}$$

where $y^+ \equiv \lim_{s\downarrow 0.7} E[y|s]$, $y^- \equiv \lim_{s\uparrow 0.7} E[y|s]$, $t^+ \equiv \lim_{s\downarrow 0.7} E[t|s]$ and $t^- \equiv \lim_{s\uparrow 0.7} E[t|s]$. Because of the discontinuity at 0.7 the denominator does not equal zero. This formula is a local version of the Wald estimator and is an IV estimator. The major identifying assumption is that there are no other discontinuities around 0.7. This is in fact an exclusion restriction with respect to the discontinuity.

to those obtained using IV.

3.2 Implementation

To apply the regression discontinuity design one needs to compare observations just below the cutoff to observations just above it. For this purpose we construct so-called discontinuity samples. The p percent discontinuity sample (abbreviated DS$\pm p$) consists of the eligible group of schools with their percentage of minority students at most p percent above the cutoff of 70 percent, and the non-eligible group of schools with their percentage of minority students at most p percent below the cutoff of 70 percent. So, for example, DS$\pm$5 consists of the schools whose percentage of minority students lies at most 5 percent above the cutoff of 70 percent, and schools whose percentage of minority students is at most 5 percent below 70 percent.

If sample size was not a consideration one could take a DS arbitrarily close to the threshold to calculate the effect of the subsidy. Unfortunately sample size is a limitation in most applications in which case, it may be important to control for characteristics that affect outcomes as the discontinuity sample widens.

To evaluate the effects of the Computer Subsidy we constructed three discontinuity samples: DS$\pm$1, DS$\pm$2.5 and DS$\pm$5. For the Personnel Subsidy we will work with two discontinuity samples: DS$\pm$2.5 and DS$\pm$5. For this subsidy DS$\pm$1 includes too few observation to produce meaningful results.

Table 2 reports the numbers of schools in each of the samples broken down by eligibility status. It also reports in parentheses the numbers of misclassified schools. This reveals that while in the entire sample of schools misclassifications may appear a minor issue, this is not true for some of the discontinuity samples. For instance, of the 13 schools in DS$\pm$2.5 that are not entitled for the Personnel Subsidy, 3 schools actually did receive the subsidy. This indicates that the regression discontinuity design is fuzzy and not sharp.

We will calculate the effect by simply comparing outcomes above and below the threshold for the various discontinuity samples by estimating 2SLS regressions of the form

$$y_s = \alpha + \delta t_s + x_s'\beta + \varepsilon_s$$

Table 2: Number of schools by treatment status

	Treated (1)	Non-treated (2)	Total (3)
Personnel Subsidy			
DS±2.5	19 (3)	10 (0)	29
DS±5	32 (4)	31 (1)	63
Computer Subsidy			
DS±1	11 (1)	21 (1)	32
DS±2.5	31 (3)	44 (2)	75
DS±5	64 (4)	86 (2)	150

Note: In parentheses the number of non-eligible schools are reported in column (1) and the number of eligible schools in column (2).

where t_s is instrumented by d_s. Adding school characteristics x_s allows us to control for differences between schools above and below the threshold as the discontinuity sample widens. All regressions are weighted by the number of students that took the test, and the reported standard errors are heteroscedasticity robust (White-Huber type standard errors).[11]

4. Results

4.1 Effects of Personnel Subsidy

Figure 2 shows the cross-sectional relation between the share of 1.9-type students and test scores. The figure shows mean scores in the population for 1 percent share bands. The relationship is negative and approximately linear until 0.6. After this point the relationship flattens somewhat for language and information and after 0.8 even becomes positive on the arithmetic test. The difference between schools without minority students and schools with

[11] Per school we have separate observations with averages for boys and girls. Note that this is equivalent to estimating individual level regressions and allowing for an unrestricted correlation structure as in the Generalized Estimating Equation framework (see Liang and Zeger 1986).

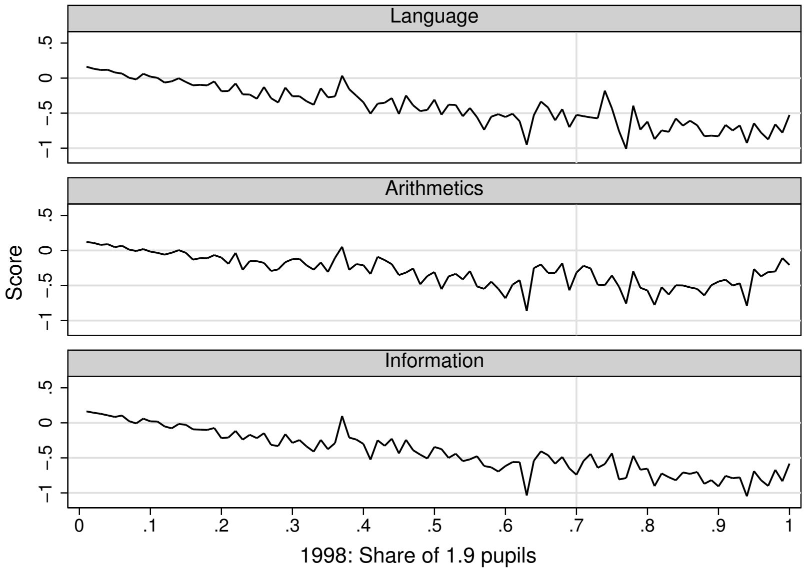

Figure 2: Relation between 2002 test scores and the share of minority students in 1998

only minority students is roughly one standard deviation.

Table 3 reports the findings for the Personnel Subsidy on the three outcome variables language, arithmetic and information processing for DS±2.5 and DS±5. Columns (1) and (2) give the average scores (measured in standard deviations) for the groups just below and just above the threshold. Column (3) presents the difference in these raw scores between the two groups and columns (4) to (6) present the 2SLS estimates of the effects of the Personnel Subsidy for three different specifications. Column (4) includes no covariates. Column (5) includes controls for gender, school size, whether the school is a public school, whether the school is located in one of the four big cities, whether the school was eligible for the other subsidy, and an index for the social background of the school's pupils doing the test.[12] Column (6)

[12]We prefer this index to the share of disadvantaged students in the school in the reference year since this index measures the social background of the 8th grade students that actually took the test. The share on the other hand, measures (more roughly) the social background of all students in the school rather than of the 8th graders, and refers to the situation a number of years prior to the date of the test.

includes in addition to the previous set of covariates also the school's lagged average achievement score.

Independent of the discontinuity sample used, the outcome variable and of the set controls, all point estimates in columns (4)-(6) are positive. In all cases, however, the standard errors on our estimates are large and none of the effects is significantly different from zero. The point estimates vary substantially across discontinuity samples and sets of controls. We consider results for DS$\pm$5 where all controls are included the most credible. In that case the standard errors are smallest and it takes possible differences between schools into account. The point estimates for this combination of discontinuity sample and controls are small. For all three scores the effects are less than 5 percent of a standard deviation, for arithmetic the effect is smallest and 3.3 percent of a standard deviation. But as mentioned, the standard errors on the estimates are large implying that we cannot rule out more substantial positive or negative effects.

A relevant comparison for the estimated effects of the Personnel Subsidy are the results reported in Lavy [2002]. Lavy evaluates the effects of financial incentives for teachers on scholastic outcomes of students in secondary education in Israel. Incentives are given in the form of a rank-order tournament in which teachers can earn cash bonuses on the basis of the performance of their students on high-school exams. Performance of students was measured in terms of passing rate and mean score and purged for various background characteristics. The teachers could earn awards varying from \$1,750 to \$7,500, and teachers could win multiple awards. The amount up for division under this program roughly equaled 7 percent of teachers' gross annual income, which is fairly similar to the amount available under the Personnel Subsidy in the Netherlands. But where we find no significant effects of the program, Lavy finds substantial positive effects that are significantly different from zero. This suggests that the structure of pay may be at least as important as the level of pay.

Table 3: Effects of the Personnel Subsidy

	<0.7	>0.7	Δ	2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
LANGUAGE						
DS±2.5	-0.574	-0.559	0.015	0.022	0.138	0.122
			(0.162)	(0.242)	(0.212)	(0.195)
DS±5	-0.557	-0.506	0.050	0.060	0.096	0.044
			(0.090)	(0.108)	(0.118)	(0.102)
ARITHMETIC						
DS±2.5	-0.450	-0.254	0.196	0.292	0.341	0.236
			(0.166)	(0.263)	(0.222)	(0.170)
DS±5	-0.358	-0.317	0.041	0.049	0.109	0.033
			(0.098)	(0.118)	(0.128)	(0.101)
INFORMATION						
DS±2.5	-0.621	-0.493	0.129	0.192	0.161	0.030
			(0.170)	(0.266)	(0.210)	(0.161)
DS±5	-0.578	-0.501	0.077	0.092	0.130	0.048
			(0.098)	(0.118)	(0.137)	(0.105)
Controls						
Characteristics			No	No	Yes	Yes
Lagged Dependent			No	No	No	Yes

Note: Characteristics include gender, average school size in 1998 and 1999, public school dummy, big city dummy, eligible for computer subsidy dummy, and an index for the social background of the pupils that took the test. Standard errors are reported in parentheses.

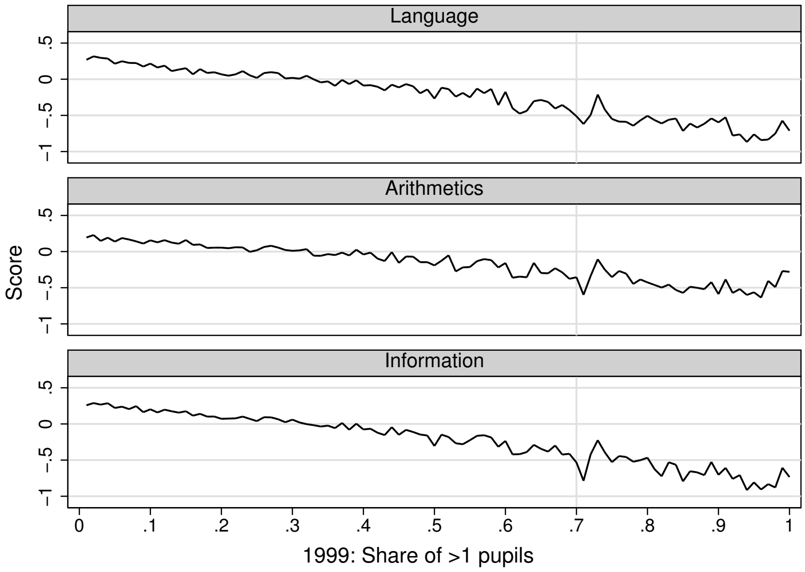

Figure 3: Relation between 2002 test scores and the total share of disadvantaged students in 1999

4.2 Effects of Computer Subsidy

Figure 3 shows the cross-sectional relation between the total share of disadvantaged students and test scores. The figure shows again The figure shows mean scores in the population for 1 percent share bands. The relationship is linear over the whole range. The difference between schools without disadvantaged students and schools with only disadvantaged students is again roughly one standard deviation.

Table 4 repeats the analysis of the previous subsection, but now for the Computer Subsidy. In the set of controls eligibility for the Computer Subsidy is replaced by eligibility for the Personnel Subsidy. The table includes three extra rows because it also reports findings for DS±1. For DS±1, the effects on arithmetic are significantly negative (at the 10% level or less) for the specifications in columns (4) and (5). For information processing this is even true for all three specifications. Going from DS±1 to DS±2.5, results in larger standard errors on the estimates and also in smaller point estimates. While the estimated effects remain negative they are now no longer significantly different from zero. Also for DS$\pm$5 all point estimates remain negative and although the standard errors are smaller than for DS$\pm$1 (and DS$\pm$2.5), the estimated effects are not significantly different from zero.

A relevant comparison for the estimated effects of the Computer Subsidy are the results reported in Angrist and Lavy [2002]. They evaluate the effects of a program in which the Israeli State Lottery funded new computers in elementary and middle schools in Israel. The authors use several estimation strategies (OLS and 2SLS) and find "a consistently negative and marginally significant relationship between the program-induced use of computers and 4th grade Maths scores" (p.760). Also for 8th graders and for scores on Hebrew, the estimated effects are mostly negative although not significantly different from zero. These findings are very similar to the results reported in the current paper.

5. Conclusion

This study has evaluated two subsidies in Dutch primary education. One subsidy provides extra resources to improve teachers' working conditions. The other gives additional funding mainly for computers and software. Both subsidy schemes specify a cutoff level of disadvantaged students (differently defined) of 70 percent below which schools receive no extra funding. All schools with at least 70 percent disadvantaged students receive the same amounts per teacher or per pupil independent of the exact share of disadvantaged students. The cutoff at 70 percent was maintained quite strictly, and manipulation of shares by schools was not possible as the shares of disadvantaged students were determined on the basis of information from years prior to the announcement of the subsidies. Due to these features the cutoffs provide very convincing instruments to identify the effects of the two subsidies.

The estimated effects of the Personnel Subsidy on achievement of 8th graders on language, math and information processing are positive but are too imprecise to be significantly different from zero. This contrasts with the

Table 4: Effects of the Computer Subsidy

	<0.7	>0.7	Δ	2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
LANGUAGE						
DS±1	-0.513	-0.613	-0.100	-0.104	-0.128	-0.064
			(0.108)	(0.113)	(0.126)	(0.084)
DS±2.5	-0.486	-0.505	-0.019	-0.020	-0.041	-0.053
			(0.111)	(0.121)	(0.135)	(0.106)
DS±5	-0.404	-0.468	-0.065	-0.069	-0.043	-0.040
			(0.070)	(0.074)	(0.078)	(0.073)
ARITHMETIC						
DS±1	-0.360	-0.575	-0.214	-0.224	-0.206	-0.128
			(0.102)	(0.108)	(0.118)	(0.095)
DS±2.5	-0.360	-0.384	-0.024	-0.026	-0.046	-0.055
			(0.116)	(0.127)	(0.146)	(0.129)
DS±5	-0.310	-0.330	-0.020	-0.022	-0.024	-0.029
			(0.075)	(0.079)	(0.080)	(0.078)
INFORMATION						
DS±1	-0.535	-0.770	-0.235	-0.246	-0.259	-0.134
			(0.110)	(0.116)	(0.127)	(0.053)
DS±2.5	-0.496	-0.557	-0.060	-0.066	-0.104	-0.088
			(0.108)	(0.118)	(0.124)	(0.099)
DS±5	-0.422	-0.473	-0.051	-0.054	-0.049	-0.022
			(0.070)	(0.074)	(0.074)	(0.071)
Controls						
Characteristics			No	No	Yes	Yes
Lagged Dependent			No	No	No	Yes

Note: Characteristics include gender, average school size in 1998 and 1999, public school dummy, big city dummy, eligible for personnel subsidy dummy, and an index for the social background of the pupils that took the test. Standard errors are reported in parentheses.

findings reported by Lavy [2002] who evaluates a financial incentive scheme for high school teachers in Israel and who finds significantly positive effects. Both programs involve similar percentages of teachers' gross annual salary.

Due to the imprecision of our estimates, we cannot exclude the possibility that the Personnel Subsidy has substantial positive effects on achievement. Compared to some other interventions, however, the Personnel Subsidy is very unlikely to be relatively cost-effective. Angrist and Lavy [2001] find that an increase in achievement of 0.25 of a standard deviation can be realized by spending \$12.000 per class on a teacher training program. Expenses on the Dutch Personnel Subsidy amounted to 5538 Euro per class and should thus increase achievement by $0.25 * 5538/12000 \approx 0.115$ of a standard deviation. The probability that the actual effect of the Personnel Subsidy is below this equals 0.76 for language scores. For arithmetic and information processing the comparable probabilities are 0.79 en 0.74.

The estimated effects of the Computer Subsidy on the same outcome variables are all negative and in several cases the effects on math and information processing are significantly different from zero. This is consistent with the results of Angrist and Lavy [2002] who find negative effects on math scores of 4th graders of the funding of new computers in Israel.

There are several possible explanations for our findings. It may be that the Personnel Subsidy does produce significantly positive effects on achievement of 8th graders, but that our research design is not sufficiently powerful to detect this. While we cannot exclude this possibility, we think it is not very likely. We use an attractive quasi-experimental design that is likely to take away much of the confounding selectivity effects that usually trouble evaluation studies. The treatment is sizable, and the data are of high quality.

Another explanation for the Personnel Subsidy having no effect is that the program provided no incentives to improve achievement of 8th graders. This contrasts with the program evaluated by Lavy [2002] where teachers were paid for performance. The available data used in our paper give no information about the way schools used the extra resources.

Angrist and Lavy [2002, p. 760] propose two explanations for the negative effects of extra computers that are also relevant for the Dutch Computer

Subsidy. The first is that instruction methods using computers are no better and perhaps even less effective than other instruction methods. The second is that outcomes are measured too shortly after the provision of funds for new computers. A bit more than one year has elapsed between the provision of the extra funds and the measurement of the test scores. It should be noted, however, that in the Netherlands schools already owned computers so that the type of intervention was not new.

A final explanation for our results is that the schools that received the subsidies already have sufficient financial resources so that they have no shortage of teachers or computers. The schools targeted by the subsidies are schools with at least 70 percent minority students (the Personnel Subsidy) or at least 70 percent of any type of disadvantaged student (the Computer Subsidy). A school with say 150 minority students and 50 non-disadvantaged students (200 students in total), receives from the regular personnel funding scheme the same budget as a school with 317 non-disadvantaged students.[13] And a school with 75 minority students, 75 students with lower educated parents and 50 non-disadvantaged students (also 200 students in total), receives from this scheme the same budget as a school with 268 non-disadvantaged students. We visited two schools with over 70 percent minority students and talked to the (vice-)headmasters. In both instances we were impressed by the schools' prosperity for instance indicated by the state of buildings and equipment and the abundance of personnel. One school had 200 computers for 500 pupils. About the other school the Dutch Education Inspectorate reports that the school has plenty resources. In such circumstances, one does not expect extra funding to have much of an impact. While this explanation contradicts conventional wisdom in the Netherlands about schools with high proportions of disadvantaged students and is at odds with the motivation behind the extra subsidies, it is consistent with the findings of this paper.

[13] The funding scheme gives no compensation for the first 9 percent of weighted students.

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