Where did the subsidy go?
The effect of extra funding for schools with disadvantaged students on teacher retention and hiring rates

Fouad El Allaoui
Master of Science (Msc.) Thesis
Erasmus School of Economics
Supervisor: Prof. Dr. H.D. Webbink

October 10, 2013

Abstract

This study examines the effect of extra funding for schools with disadvantaged minority students on teacher turnover and teachers' experience. In this study we aim to find out whether a personnel subsidy targeted at elementary schools with large proportions of disadvantaged minority pupils affected teacher retention and hiring rates, and as a result average teacher experience. The subsidy we evaluate gives elementary schools with at least 70 percent disadvantaged minority pupils extra funding for personnel. This 70 percent cutoff provides a regression discontinuity design that we exploit in a local difference-in-differences framework. The (temporary) personnel subsidy did not have the desired impact on several aspects of teacher mobility. The point estimates of the effect on retention rates are not significantly different from zero. We do find a significant effect on hiring rates in the first 8 months after the announcement. In subsequent years the point estimates are insignificant. Our results with respect to the effects on experience show that, except for experience in education in 2001, all point estimates have a negative sign. For the year 2004 it is even statistically significant at the 10% significance level. We conclude that the (temporary) extra funding does not have a positive impact on the average teacher experience/quality. Finally, we also checked whether the extra resources are allocated to higher salaries and whether more teachers are hired in terms of full-time jobs (FTEs). We did not find any significant effects on teacher remuneration nor on the total number of full-time teacher jobs that a school employs.

Contents

1 Introduction

In many countries, including the Netherlands, there is an increasing awareness and concern about teacher shortage. In the Netherlands the shortages increase, and the authorities expect to have an overall teacher shortage in 2016 of 2.200 fte (3.5 % of employment) in secondary education and a shortage of 1.400 fte (1.4 % of employment) in elementary education (Dutch Ministry of Education, Culture and Science, 2013). For schools with a large proportion of disadvantaged pupils it might be even more difficult to retain and hire teachers as teachers tend to leave schools with a large share of minority students and large share of students with special needs (Falch and Strøm, 2005). Hanushek et al. (2004) found that schools in urban areas serving economically disadvantaged and minority students appear to have large difficulties in retaining and hiring teachers. The findings of Hanushek et al. (2004) suggest that characteristics of the students, particularly race and achievement, rather than salary, are strongly related to teacher mobility. This is one of the reasons why many developed countries have adopted compensatory education programs that provide extra resources for schools with high shares of disadvantaged pupils.[1] However, other studies indicate that in addition to student characteristics, high levels of teacher mobility are strongly related to poor working conditions (Berry and Hirsch, 2005, Hanushek et al., 2004, Ingersoll and Smith, 2003, Loeb et al., 2005, Tabs, 2004).

In this study we aim to find out whether a personnel subsidy targeted at schools with large proportions of disadvantaged minority pupils affected teacher turnover (retention and hiring) rates and the corresponding years of teacher experience. Moreover, we examine whether schools used the extra funding to expand teachers' appointment size (by increasing the number of fte) and whether they used it to improve teachers' remuneration. The aim of the evaluated subsidy was purely to improve the working conditions of teachers. By providing extra resources to improve the working conditions on these schools, schools might be more successful in retaining their teachers and/or in hiring novel teachers.

In an earlier study by Leuven et al. (2007) the effects on achievement of two subsidies targeted at schools with large proportions of disadvantaged pupils were evaluated. The authors evaluated the effect of this extra funding on eight-graders' achievement by using a regression discontinuity design that was exploited in a local difference-in-difference framework. The first program (personnel subsidy) provided primary schools with at least 70% disadvantaged minority pupils extra funding to improve the working conditions of personnel. The second program (computer subsidy) provides primary schools with at least 70% pupils from any disadvantaged with extra funding for computer hardware and software. The authors found mainly negative point estimates of the effect of both subsidies (and in some cases statistically significant). A possible explanation mentioned by Leuven et al. (2007) is related to teacher mobility. The authors mentioned that new young (temporary) teachers might have replaced regular teachers and that these replaced regular teachers were assigned managerial tasks. This suggests that actual teaching was carried out by the new, less experienced teachers. As a consequence, the average experience on the 'subsidy schools' might have decreased. As teacher experience has been found to be of major importance in determining student achievement (Hanushek, 1997, 2003, Hanushek and Rivkin, 2006), this might explain the negative effects on pupils' achievement. The possible explanation mentioned by Leuven et al. (2007) is not tested though. In this thesis we attempt to gain more insight into the so called black box by investigating the effects of the subsidy

[1] Some examples of compensatory programs in developed countries are the Title I program in the United States, Education Action zones in Great Britain, and Zones d'éducation Prioriatire in France. In the Netherlands, primary schools already receive extra funding from the main funding scheme based on the proportion of disadvantaged pupils.

on several teacher mobility variables. In this manner we can gain more insight into the mechanisms behind the lack of any desirable effect of the subsidy on student achievement.

In this study we will only examine the effects of the personnel subsidy as this subsidy was intended to improve working conditions. Hence, this subsidy might affect the extent to which schools are able to retain existing teachers, hire novel teachers, and thereby also affecting the average experience of their teachers.

The 70% treshold of the personnel subsidy was maintained almost perfectly, thereby providing a regression discontinuity design. In this study we will follow a similar approach as Leuven et al. (2007) by exploiting the regression discontinuity design in a local diference-in-differences framework. The identifying assumption in this analysis -in order to obtain unbiased estimates- is that there are no confounding discontinuities at the treshold. This means for instance that there were no other interventions based on the same 70% treshold that could possibly influence teacher mobility during the years that we examine. We will use this approach to examine the effects of the personnel subsidy on teacher turnover (retention and hiring rates) and the corresponding teachers' experience. We will also investigate whether the subsidy affected the number of full-time jobs (FTEs) that schools employ, and teacher remuneration. We attempt to link our findings on teacher mobility to the negative findings on pupils' achievement.

We find that the personnel subsidy has no impact on schools' ability to retain teachers. We do find a significant effect on hiring rates in the first 8 months after the announcement. In the following years there is however no significant effect on hiring rates. Our results with respect to the effects on experience show that, except for experience in education in 2001, all point estimates have a negative sign. The point estimate of the effect on 'experience in education' in 2004 is even significantly negative at the 10% significance level. Hence, it seems that additional (temporary) funding for schools with large proportions of disadvantaged minority pupils has no positive impact on hiring and retaining the (better) more experienced teachers. These findings are in line with the findings of Leuven et al. (2007). Recall that these authors find negative point estimates of the effect of the subsidy on student achievement (and in some cases even significantly). They also presumed that this might be due to the failure of eligible schools to hire (better) more experienced teachers. This presumption is confirmed by our study. Moreover, the subsidy seems to have no effect on teacher remuneration nor on the total number of full-time teacher jobs that a school employs. So, there is no evidence that the eligible schools allocated the extra funding to the hiring and recruitment of extra (temporary) personnel.

In line with Leuven et al. (2007), we argue that the already generous main funding scheme for Dutch primary schools can explain our results. At the time this subsidy was provided, disadvantaged minority pupils had a 90% higher weight than nondisadvantaged pupils. Due to this extra resources, schools with high shares of disadvantaged minority pupils already had a lower pupil-teacher ratio at the time (14 respectively 22). It seems unlikely that these schools would hire more teachers to lower the pupil-teacher ratio (also because of restrictions with respect to available space to expand the number of classes). It seems that schools targeted by the subsidy already have sufficient resources to hire sufficient personnel. As a result, the marginal value of extra resources is lower, and therefore less effective.

The remainder of this thesis continues as follows. In the next session we review recent literature regarding teacher turnover and teacher quality. Subsequently, section 3 elaborates on the details of the programme, and discusses the data and methodology of our research. Section 4 discusses the results of our analyses. Finally, Section 5 gives the conclusion and derives policy recommendations.

2 Literature overview

Conventional wisdom among policy makers is generally that more investments in education will automatically improve teachers' quality and student achievement. Hanushek (1996) found already two decades ago that there is no strong relation between resources that schools have at their disposal and student achievement. More recent research confirmed these outcomes (Hanushek, 2003). The question is, in this case, how effective additional investments are in affecting teacher mobility and what the effects of teacher turnover and experience are on pupils' achievement.

2.1 Additional Resources and Teacher Mobility

Characteristics that determine teacher retention and attrition rates have been examined extensively. These studies generally indicate that poor working conditions are an important factor in determining teacher mobility (see Berry and Hirsch, 2005, Hanushek et al., 2004, Ingersoll and Smith, 2003, Loeb et al., 2005, Tabs, 2004). This will motivate policy makers to provide additional resources to schools with high teacher turnover rates. The evidence on the effect of extra resources on teacher mobility is however very limited.

The problem of high teacher turnover rates among schools with disadvantaged students was already faced in the 1980's by several school districts in the United States. Bruno and Negrete (1983) investigate the effects of a compensatory funding scheme in a large urban school district. This so called 'wage incentive program' was purely aimed at paying a salary differential (of 11% of base salary) to teachers serving at schools with a large share of disadvantaged (minority) students. The authors did not find the desired effects on teacher turnover. The program failed in hiring and retaining the 'high quality' teachers. Although the hiring of new teachers improved, these new teachers were mainly young, inexperienced teachers or teachers from other low performing schools rather than older more experienced teachers.

Another more recent study on the effect of extra funding on teacher mobility is carried out by Bénabou et al. (2009). The results are consistent with the findings of Bruno and Negrete (1983). Teachers who taught on schools that received the subsidy were offered bonuses and additional career perspectives. This study takes into account the endogeneity issue by using both difference-in-difference and instrumental variable methods. The extra resources merely lead to a small increase of newly hired teachers. As a consequence, the fraction of young teachers (less than age 30) increases slightly and the average experience of the teachers decreased. This indicates that schools who received the extra resources did not improve their teacher retention rates (as average teacher experience decreases). Moreover, no improvement was found on teacher qualifications (as measured on the fraction of teachers with tenure and the fraction of teachers with a regular teaching certificate). Summarized, the results show no improvement in teacher qualifications, years of experience and turnover rates.

2.2 Teacher Mobility and Student Achievement

Previous studies found that teachers tend to leave schools with large shares of disadvantaged (minority) students and a large shares of students with special needs(Bonesronning et al., 2005, Falch and Strøm, 2005, Feng and Sass, 2012, Hanushek et al., 2004). However, evidence on the effects of teacher mobility on student achievement is less clear. Several studies find a positive relation between teacher experience and teacher quality, indicating detrimental effects of high turnover rates among teachers on student achievement.

The general consensus among economists is that experience is an important factor in determining teacher quality. Using matched panel data on teachers and students - which contained a wide range of individual student characteristics- Hanushek et al. (2005) find that, unlike advanced degrees or cerifications, experience is an important factor in determining teacher quality. This is especially the case in the first few years of teaching. Rivkin et al. (2005) take into consideration the possibility of a nonlinear relationship between experience and teacher quality (student achievement). His results confirm the finding that experience effects are concentrated in the first few years of teaching. They find that teachers perform significantly worse in their first and second year of teaching. These experience effects indicate that the negative effects of high turnover rates among teachers on student achievement can be explained by the hiring of new (unexperienced) teachers.

Hanushek (1997, 2003) conducted a comprehensive meta-analysis on the effect of teacher education and experience on teacher quality. In these reviews of the empirical literature they show that the vast majority of the estimated parameters of teacher education are either negative or significantly insignificant. If the review is limited to high quality estimates, none of the estimated parameters are positive.[2] The same results indicate that a master's degree has no systematic effect on teacher quality as measured by student outcomes. In contrast, there seems to be a positive relation between teacher experience and teacher quality as measured by achievement of the students. A vast majority of the value-added estimated parameters find a positive effect, although only 41 percent of the estimates are statistically significant. However, it seems likely that a number of these studies lack the statistical power necessary to identify the positive results as statistically significant. Nevertheless, the results from the review of the literature are a strong indication that experience has a substantial impact on teacher quality/student achievement.

Studies that directly examine the relationship between teacher mobility and teacher quality are less abundant. The studies that are done on this subject report in general a negative effect of teacher turnover on teacher quality and student performance.

Using extensive data from Texas public schools Hanushek and Rivkin (2007) examined what the consequences are of teacher mobility. The authors measure teacher quality by teacher value added to achievement. The researchers compared teachers who stay in their urban schools with those who move to another school or leave the teaching profession altogether. The results show that teachers who move to another school or leave the profession are on average the less effective teachers. This indicates that teacher mobility is not that detrimental to student achievement. However, their results also show that schools in urban districts -serving higher shares of disadvantaged students- face higher turnover rates. As a consequence, these schools have higher shares of teachers with little or no experience —which tend to be less effective. Therefore, these study shows little evidence on the relation between teacher mobility and pupils' achievement and development.

The study by Dolton and Newson (2003) shows a negative relation between teacher mobility and student achievement. Using data on 316 primary schools they explore the relationship between teacher turnover and school performance. Although the authors use merely a OLS regression model, they corrected for a wide range of school characteristics. The results show a significant negative relation between levels of teacher turnover and both standardized reading and math tests. This indicates that teacher mobility has detrimental effects on the progress and achievement of pupils.

A similar study is performed by Feng and Sass (2012) who use student-teacher panel

[2]High quality estimates are estimates from empirical studies in which they use a value added model to estimate student achievement. These value added models use prior achievement to mitigate problems of omitted variables bias and are therefore much more reliable.

data from the state of Florida to estimate the effect of teacher mobility on the distribution of teacher quality. This study uses another model in which it measures teacher quality by a teacher's contribution to student achievement, net of educational inputs and concurrent student, peer and school impacts. There results show that the fraction of top quartile movers hired by schools whose teachers are in the top quartile of the quality distribution is higher than that of schools whose teachers are in the bottom quartile of the quality distribution. This means that better schools (with better teachers) are more able to hire the good quality movers. This indicates that especially the effective (or 'better quality') teachers tend to move to schools with more advantaged students and with smaller shares of minority and low income students, thereby exacerbating the achievement gap between schools with a small and a high share of disadvantaged (minority) pupils.

3 Data and Methodology

3.1 The program

In February 2000 the Duth Ministry of Education announced a compensatory funding scheme for schools with at least 70% disadvantaged minority pupils.[3] This 70% eligibility criterium was based on the percentage of these pupils that were enrolled in the school on October 1, 1998, as registered in administrative data. The extra funding amounted to € 2654.50 per teacher over a two-year period and all schools that got the treatment received the same amount per teacher.[4] These amounts were paid between May 2000 and March 2001 (see Table 1). This personnel subsidy can be perceived as a substantial intervention since the amount is equal to about 9% of the annual gross salary of Dutch primary school teachers, and even 11% of the annual gross salary of young teachers. Moreover, personnel costs account for about 80% of schools' overall budget.[5]

This subsidy was motivated by an increasingly tight labour market in the educational sector. Especially elementary schools with high shares of minority pupils appeared to face more and more difficulties in hiring new teachers, and some of these schools reported that they were not able to hire new teachers at all. Moreover, these schools faced increasing problems in retaining their teachers. Main reasons were: high shares of minority pupils, location of the schools, and the (perceived) lack of facilities with respect to working conditions.[6]

The subsidy was intended to alleviate these labour market bottlenecks. Schools were free to spend the subsidy corresponding to the schools' needs, as long as it was spent on staff policy or aimed to improve working conditions. The memorandum of the ministry's decision listed the following examples: a plain financial premium, a bonus to stimulate teachers to work more hours, stimulation of expanding the contract hours of teachers, compensation for housing costs, traveling costs, childcare facilities, and hiring teaching assistants. The memorandum emphasized that the extra resources were only provided for the specified period. Hence, if the school committed itself to obligations after the specified period, this had to paid from the regular scheme.

In November 2000 the Ministry announced another temporary compensatory scheme for schools with at least 70% disadvantaged pupils belonging to any disadvantaged group. This scheme amounted to € 34 per pupil, which is about € 450 per class in the eligible schools and equal to about 20% of the schools' nonpersonnel budget. This so called "ICT subsidy" was a one-time financial boost to schools aimed at investing in ICT applications to improve the quality of education. Although this subsidy is not subject to our analysis,

[3]The description of the personnel subsidy program that we elaborate on in this section is based on the explanatory memorandum following the ministry's decision but to a large extent also on the description by Leuven et al. (2007).

[4]The personnel subsidy is a fixed amount per teacher. However, the pupil-teacher ratio decreases with the share of minority pupils(as a result of a higher compensation from the main funding scheme). As a consequence, schools with a higher share of minority pupils receive more subsidy per pupil. There is no natural way to exploit this, since the payment per pupil variation within the treatment group varies one-to-one with the share of minority pupils.

[5]Note that, during the periode that we analyse (1999/2000 - 2004), the government's main funding scheme for primary schools assisgns extra funding to schools with higher shares of minority (and other disadvantaged) pupils. Relative to the funding for nondisadvantaged pupils, the extra funding is 90% for minority pupils. So, a school with all of its pupils from the minority group receives almost twice as much public funding as schools without any disadvantaged pupil.

[6]The increased tightness of the labour market and the rising problems with respect to teacher mobility was found by several studies of different research companies commissioned by the Dutch Ministry of Education.

it is worth to note that we evaluate the impact of the personnel subsidy conditional on schools (treatment and control) also receiving the computer subsidy since only five (4%) of the schools in the 60-80% range were not eligible for the computer subsidy.

3.2 Data

Data on the number of pupils of different social backgrounds for all primary schools in the Netherlands counted at October 1, 1998, was provided by the Dutch Ministry of Education. This dataset also contains information about which schools actually received the extra funding. Furthermore, the dataset contained information at the school level about the socioeconomic index, urbanization of the school area, school denomination, share of female teachers, average age of the teachers, and school size in 1998 and 1999. This administrative dataset is merged with another administrative dataset that allows to follow each individual teacher over time from 1994 to 2004.[7] For each teacher the dataset gives information about when the teacher first started in the profession (allows to generate variable 'teacher experience') and for each year between 1994 and 2004 it shows at which school the teacher is employed.[8] Furthermore, this dataset contains the following individual background characteristics: gender, date of birth (allows to generate variable age), and year started at the school were the teacher works in year t (allows to generate the variable 'years of experience at the school'). The merged dataset allows to define the following outcome variables:

- Teacher Retention: this is defined as the probability that teacher i, who is employed at a school in 1999, is still employed at the same school in the subsequent years after the treatment.
- Teacher Hiring: this is the probability that teacher i is newly employed in one of the subsequent years after the treatment.
- Teacher Experience: this is defined as the years of experience of teacher i. We make a distinction between experience in the educational sector and years of experience at the school.
- Size of teaching staff: this is the total number of teachers that school j employes in terms of full time equities (FTEs).
- Remuneration (salary): this is teacher i's gross salary based on 1 FTE.

Note that all outcome variables are on the individual teacher level, except the 'size of teaching staff' variable which is at the school level. This is to examine whether, if schools did not improve their ability to hire or retain teachers, schools were able to stimulate their teachers to work more hours, thereby expanding the number of hours that they are employed. For our analysis we use data of the outcome variables from preintervention

[7] This dataset is provided by the executive agency of the Ministry of Education DUO (Dienst uitvoering Onderwijs). DUO composed this dataset through the information about individual teachers. All schools in the Netherlands are obliged to provide specific (administrative) information about all their teachers. Therefore, the mobility of each individual teacher can be determined for each year between 1994 and 2004.

[8] Two schools in the 60-80% range that existed in 1998 did not exist anymore in 2004 (because they merged or closed). Therefore, our dataset consists of 123 schools in the 60-80% range instead of 125 as in Leuven et al. (2007).

year 1999 and from postintervention years 2000 to 2004.[9] For the difference-in-differences analyses with respect to teacher retention we also need to use data from preintervention years 1994 to 1998.[10] Recall that retention of teacher i in year 2000 is defined as the probability that teacher i, who is employed at a school in 1999, is still employed at the same school in 2000 (one year later). With the diff-in-diff analysis, this outcome is compared with the probability that teacher i, who is employed at a school in 1998, is still employed at the same school in 1999 (one year later). In the same manner, for the year 2001 (two years after), this is defined as the probability that teacher i, who is employed at a school in 1999, is still employed at the same school in 2001. Hence, for this year we need to use data from preintervention year 1997, to measure probability that teacher i, who is employed at a school in 1997, is still employed at the same school in 1999 (two years after). And so on for the subsequent years.

Table 1: Timing of Events

October 1 1998	Reference date for personnel subsidy
October 1 1999	Reference date for ICT subsidy
February 2000	Decision and announcement of personnel subsidy
May 2000	Payment of €2,346 per teacher as personnel subsidy
November 2000	Decision and announcement of ICT subsidy
November 2000	Decision and announcement of extra payment of personnel subsidy
December 2000	Extra payment of €617 per teacher as personnel subsidy
March 2001	Payment (second) of €2,346 per teacher as personnel subsidy.
December 2001	Payment of €34 per pupil as ICT subsidy
October 1, 1999 - 2004	Reference dates for teacher turnover rates (retention and attrition), size of teaching sttaff (in FTEs), and salary.

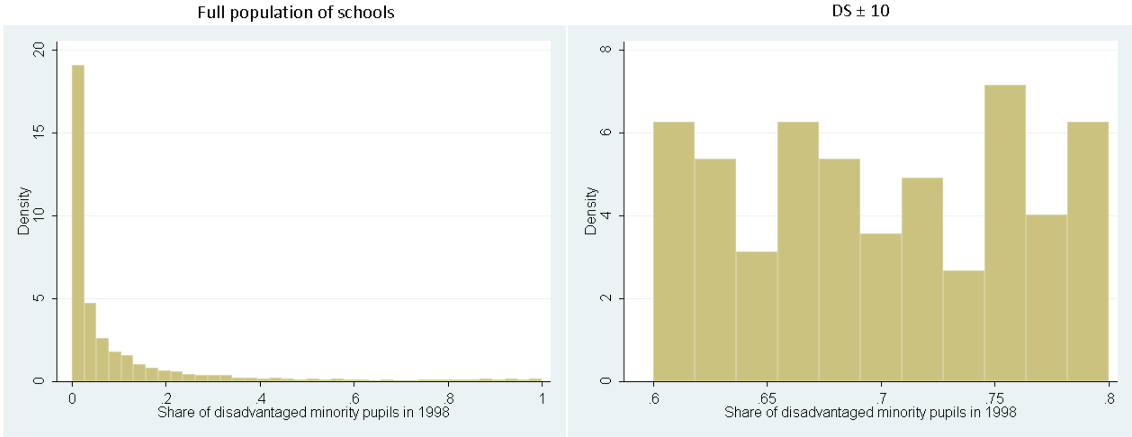
Source: Leuven, E., Lindahl, M., Oosterbeek, H., & Webbink, D. (2007). The effect of extra funding for disadvantaged pupils on achievement. The Review of Economics and Statistics, 89(4), 721-736.

Table 1 gives a chronological overview of the relevant events. This shows that reference dates with respect to the outcome variables in the years 2000 to 2004 are dated after schools received the first payment of the subsidy. For the year 2000 the reference date for the dependent variables is about eight months after the decision and announcement of the subsidy, and about five months after the payment of it. Hence, we can already analyse whether the extra funding received during the second half of school year 1999/2000 affected teacher composition at the begin of school year 2000/2001, which would be the case if the treatment schools were able to retain and/or hire more teachers. For the year 2001, the reference date for the dependent variables is about 10 months after the extra payment of the subsidy and about 7 months after the final payment of the subsidy. For the year 2002 (2003, 2004), the reference date is about one-(two, three)-and-a-half years after the last payment.

[9]Although the extra payment in December 2000 and the final payment in March 2001 take place after the reference date of October 2000, we will treat the year 2000 as a postintervention year aswell, since the Ministry already committed itself to pay the subsidy and because the schools already received the first payment. Moreover, we analyse each year separately and therefore it will not affect other outcome variables.

[10]The difference-in-differences approach will be discussed in the next subsection (3.3 Methodology).

Figure 1: Distribution of Schools



The research design that we use in this study would be unreliable if schools were able to anticipate the subsidy. If the latter would be the case, schools might manipulate their shares of disadvantaged minority pupils to become eligible. Leuven et al. (2007) showed that the distribution of schools is properly symmetric around the cutoff by comparing the distribution of schools around the cutoff level. We repeated this for the schools in our dataset which consist of the same schools, but where two schools were excluded (see footnote 8 in the Data subsection). Figure 1 shows the frequency distribution of schools in the range of 10 percentage points arount the 70% cutoff level. The figure confirms that the distribution of schools is properly symmetric around the cutoffs. Hence, there is no evidence that schools anticipated the implementaion of the subsidy.

Another important requirement of our identification setup is that actual assignment is consistent with eligibility. On the school level the full population consists of 6,851 schools.[11] Of these schools 255 (4%) had at least 70% disadvantaged minority pupils, thereby qualifying for the subsidy. 254 of these 255 schools (99.6%) actually received the subsidy. By mistake, seven schools (0.1%) that fell (just) below the 70% treshold received the subsidy. Of the 123 schools in the 60-80% range, five schools (4%) received the subsidy while they were not qualified and one school (0.8%) did not receive the subsidy while they were eligible. The reason for these misspecifications is unknown. However, to deal with these few non compliance schools, in the empirical analysis we will use eligibility status as instrument for actual treatment.[12]

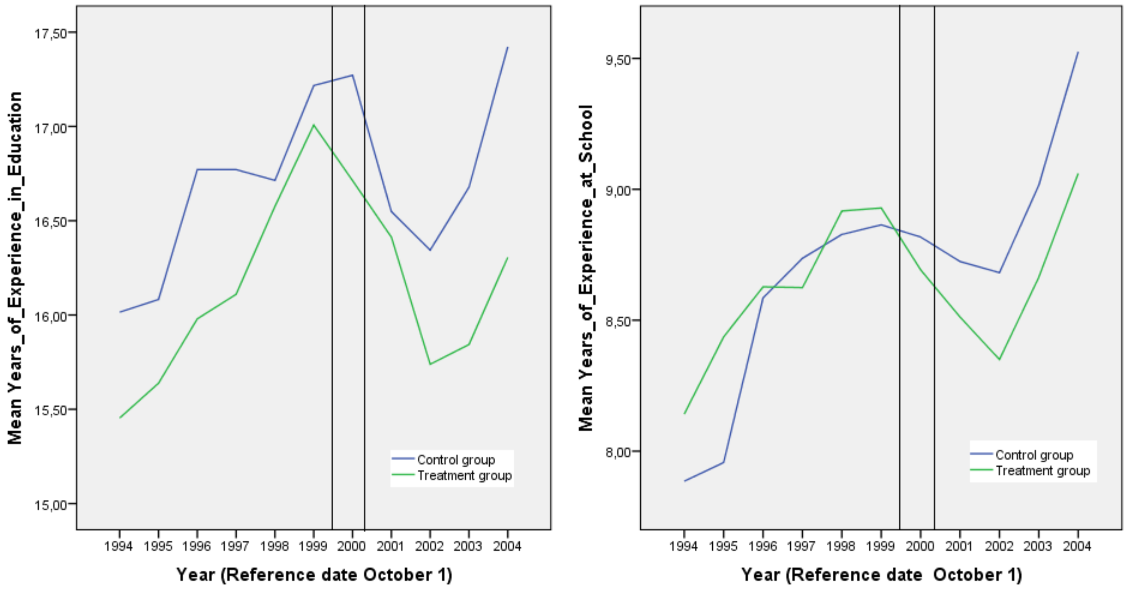
Finally, our difference-in-differences model requires that the treatment and control group follow the same long-term trend in the years prior to implementation of the treatment (common trend assumption). Because there is a one-to-one relationship between experience and the net value of teacher retention and hiring, we need to examine whether the experimental and control group follow the same preintervention trend in experience. Figure

[11] In 1998 there were actually 7,045 elementary schools in the Netherlands. As we mentioned earlier, we used the 2004 dataset with information about teachers' employment, which contains for each year information about the school(s) were each teacher was employed. However, if a school has been merged before 2004, the dataset adopts the new name of the (merged) school for all preceding years (otherwise teachers of merged schools would be detected as leavers). During 1998 and 2004 194 schools merged. Therefore, these schools are still part of our dataset but under their new name (after the merger). None of these schools are noncompliance schools.

[12] Regressing actual assignment on eligibility status (controlling for a third-order polynomial in the share of disadvantaged minority pupils) gives a coefficient of 0.918 (s.e. 0.006).

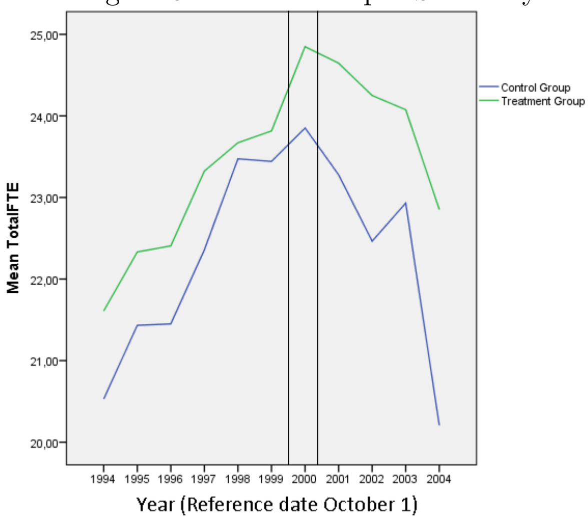
2 shows the relationship between year and average years of experience for both the treatment and control group.[13] This figure shows that the common trend assumption seems plausible.

Figure 2: Average Years of Education by Year



In the same way we can check whether the common trend assumption seems plausible for the teaching capacity (size of teaching staff) variable. Figure 3 shows the relationship between year and the average number of FTEs that a school employs for both the experimental and control group. We see that also for this variable both groups follow the same long-term trend during the period prior to the treatment.

Figure 3: Total FTEs per School by Year



[13]The graphs in figure 2 and 3 are based on the 60-80% range.

3.3 Methodology

In this section we will discuss the empirical strategy to identify the effects of the personnel subsidy.[14] The eligibility rule of the subsidy allows to use a regression discontinuity design. To fully exploit the available data on pre- and postintervention years, our strategy is to exploit the discontinuity in a difference-in-differences approach which increases the power and precision of the estimates. We first describe how to use the standard (sharp) regression discontinuity design. Subsequently, we describe how to exploit this in a difference-in-differences approach.

The Ministry specified that all primary schools with at least 70% disadvantaged minority pupils qualify for additional personnel funding and schools with less than 70% of such pupils did not qualify for this extra funding. If there would be no exception to this eligibility rule, treatment would depend in a deterministic way on the share of disadvantaged minority pupils and we would have a sharp regression discontinuity design. This design estimates the effect of the treatment by comparing the average outcome just above the treshold with the average outcome just below the treshold. An important condition, which is fulfilled in our situation, is that there are no confounding discontinuities at the treshold. Therefore, we are able to obtain an unbiased estimate of the average treatment effect for schools around 70% disadvantaged minority pupils.

The share of disadvantaged minority pupils in school j in 1998 is denoted by s_j^{98}. In case of a sharp regression discontinuity design treatment, which is denoted by d_j^{98}, is defined as follows:

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

Then, we can write the outcome as follows:

$$E[y_j] = \alpha + \delta d_j^{98},$$

where $\alpha \equiv E[y_{0j}]$ is the (average) outcome without the extra funding (counterfactual). Then, $\delta \equiv E[y_{1j}] - E[y_{0j}]$ stands for the (average) change in the outcome variable due to the personnel subsidy. We can now (assuming a common treatment effect) identify δ as follows:

$$\delta = y^+ - y^-,$$

where $y^+ \equiv \lim_{s \downarrow 0.7} E[y|s]$ and $y^- \equiv \lim_{s \uparrow 0.7} E[y|s]$.

As mentioned earlier, our strategy is to exploit the discontinuity in a difference-in-differences design to take full advantage of the available data on pre- and postintervention years. This also gives more accurate estimates and increases the power of the statistical tests. To obtain these estimates we will use the following fixed-effect regression:

$$y_{ijt} = \alpha + \delta \cdot (D_{ij}^{98} \times m_t) + \beta \cdot D_{ij}^{98} + \gamma \cdot \chi_i + \tau \cdot m_t + \eta_j + \varepsilon_{ijt}, \quad (1)$$

where y_{ijt} is the outcome variable with respect to teacher i in school j in year t (e.g. whether teacher i in school j in year t is retained or whether this teacher is newly hired), D_{ij}^{98} is a dummy variable that indicates whether school j received the subsidy (equals 1 if schools received subsidy), χ_i captures individual teacher characteristics, m_t is a time

[14] We follow the empirical strategy that is used in Leuven et al. (2007). They provide an application of analysing this subsidy by exploiting the regression discontinuity design in a difference-in-differences framework.

dummy variable to indicate whether it is a pre- or postintervention year (equals 1 if postintervention year), η_j capture school fixed effects, and ε_{ijt} represents the error term.[15] Note that the point estimate δ of the interaction term $D_{ij}^{98} \times m_t$ in equation (1) captures the standard difference-in-differences effect.

In a standard regression discontinuity model the outcomes from observations just above the cutoff are compared to those from the observations just below the cutoff. By exploiting the discontinuity in the difference-in-differences model, we estimate the point estimates locally, and the necessary common trend assumption becomes more credible. To this end, we construct a 10% discontinuity sample. This 10% discontinuity sample (DS $\pm$ 10%) consists of the group of schools that qualify for the subsidy with their percentage of disadvantaged minority pupils not more than 10 percentage points above the 70% cutoff, and the group of non eligible schools with their percentage of disadvantaged minority pupils not more than 10 percentage points below the 70% cutoff. Applying a wide discontinuity sample will increase the number of observations but at the same time it increases the risk that the common trend assumption is not fulfilled. The 10% discontinuity sample is relatively close to the discontinuity and includes sufficient schools and teachers to obtain meaningful results.

In order to obtain unbiased estimates of the effects of the treatment, the main identifying assumption, that there are no other discontinuities around the 70% cutoff, needs to be fulfilled. This assumption is the so-called "exclusion restriction with respect to the discontinuity". Since the variable that determines eligibility (share of disadvantaged minority pupils $[s_j^{98}]$) is a continuous variable, applying the regression discontinuity design allows to control for smooth functions of this variable. In the difference-in-differences framework school specific characteristics (such as s_j^{98}) enter in the form of fixed effects, and teacher specific characteristics are added as control variables.

As mentioned in the previous subsection, in a small number of cases there was non-compliance. That is, a few schools did (not) receive the subsidy although their share of disadvantaged minority pupils was above (below) the 70% eligibility cutoff. The rule behind this misclassification is unknown. Therefore, there is no longer a deterministic relation between the share of disadvantaged minority pupils and receiving the treatment. This breaks down the sharp regression discontinuity design. To address this issue we conduct two-stage-least-squares (2SLS) where eligibility is used as an instrument for treatment.

Because our outcome variables are either binary (retention and hiring) or only take positive values (experience, number of FTEs and salary), one might be concerned about the use of OLS regressions. Therefore, one might argue to use nonlinear models such as Probit and Tobit instead. For the discrete outcome variables, our linear probability model may suffer from the problem that the estimated probabilities could be outside the [0,1] interval. However, we follow Angrist and Pischke (2009) who show that using OLS regression when the outcome variable is limited does not affect empirical practice. The outcome of interest is eventually the unconditional average treatment effect, which is given by the difference in means between the group of schools with treatment ($D_{ij}^{98} = 1$) and those without treatment ($D_{ij}^{98} = 0$). Recall that in this study we denoted this as follows: $\delta \equiv E[y_{1j}] - E[y_{0j}]$. When we have binary outcome variables we are intested in: $P[y_{1j}] - P[y_{0j}]$ which is equal to $E[y_{1j}] - E[y_{0j}]$. Hence, the estimation of causal effects can be estimated with OLS irrespective of whether the dependent variable is binary, only positive, or continuously distributed.

[15]for the RD results we report heteroskedasticity robust standard errors and allow for clustering at the school level. For the DD results we allow for clustering at the 'school $\times$ year' level instead of school level.

3.4 Descriptive statistics

Since we estimate the effects of the treatment locally, it is important to know how the estimation sample compares to the whole population of teachers/schools. Table 2 shows the sample means for the estimation sample (DS ± 10%) in the postintervention years 2000 to 2004, and how these compare to the whole population. As we can see from the first four rows of Table 2, teachers employed at schools in the discontinuity sample have, compared to the full population, lower retention rates. Note that the difference between the full population and the estimation sample increase after the year 2000. So, the difference between the two groups of teachers with respect to retention rates increases for longer-term retention. The differences in hiring rates between the full population and estimation sample are limited. Although schools around the cutoff have lower retention rates, this group also has slightly lower hiring rates, except for the year 2000 where it is of equal amount. Apparently, the schools around the cutoff do not hire more teachers to replace the ones who left. The lower retention rates for the discontinuity sample are also reflected in the years of experience. Teachers from the estimation sample have on average less experience than the full group of teachers. This is true for both experience in education and experience at the school. Teachers in the estimation sample tend to leave the schools were they are employed more often. As a consequence it appears that schools in the estimation sample employ more young, newly hired teachers with less experience. This explains the differences with respect to the average years of experience between these two groups.

The total number of full-time teachers (FTEs) that a school employs is much larger for group of schools in the estimation sample. In the first place this is because schools around the cutoff are larger in terms of the number of pupils. Another reason is that these schools receive more funding from the main funding scheme of the government, and (as a consequence) have a lower pupil-teacher ratio.

The first two school characteristics, share of minority pupils in 1998 and the share of pupils from any disadvantaged group in 1999, differ obviously among the two groups. The full population of schools have on average about 11% disadvantaged minority pupils, whereas for the schools in the 60-80% range this is 70%. For the share of pupils from any disadvantaged group this is 28% and 85% respectively. The socioeconomic index differs aswell among the two groups. Schools in the discontinuity sample are practically only in the two most disadvantaged groups of the socioeconomic index classification of the school population. Compare this with the whole population of schools, where the vast majority of the schools having their pupil population from the three least disadvantaged categories.

Table 2 further shows that schools with higher shares of disadvantaged minority pupils are more likely to be located in one of the major cities. Over 60% of the schools in the 60-80% range is situated in one of the major cities of the country, and 87% is situated in either a high or a very high urbanization area. This is not suprising as most immigrants live in one of the large cities. The whole population of schools is more equally distributed over the urbanization groups. Half of all schools are located in the low/none or modest urbanization areas.

Finally, almost half of the schools in the discontinuity range have a public denomination, whereas this is 33% for all elementary schools. This might be due to the fact that public schools are more located in urban areas and Protestant and Catholic more in rural areas. The table also shows that the share of female teachers and the average age of teachers does not differ between the groups, and that schools in the estimation sample are on average somewhat larger.

Table 2: Sample Means for Population and Estimation Samples

	Population (1)	DS ± 10 (2)
Retention		
2000 (s.d.)	0.879 (0.326)	0.858 (0.349)
2001 (s.d.)	0.763 (0.425)	0.702 (0.457)
2002 (s.d.)	0.700 (0.458)	0.650 (0.477)
2003 (s.d.)	0.651 (0.477)	0.581 (0.493)
2004 (s.d.)	0.603 (0.489)	0.484 (0.500)
Hiring		
2000 (s.d.)	0.149 (0.356)	0.149 (0.356)
2001 (s.d.)	0.153 (0.360)	0.147 (0.354)
2002 (s.d.)	0.150 (0.357)	0.136 (0.343)
2003 (s.d.)	0.100 (0.300)	0.096 (0.295)
2004 (s.d.)	0.087 (0.282)	0.080 (0.271)
Experience		
In Education		
2000 (s.d.)	17.76 (11.367)	16.97 (11.907)
2001 (s.d.)	17.08 (11.958)	16.48 (12.344)
2002 (s.d.)	16.48 (12.339)	16.02 (12.594)
2003 (s.d.)	16.63 (12.442)	16.22 (12.696)
2004 (s.d.)	16.85 (12.550)	16.81 (12.889)
At the school		
2000 (s.d.)	9.45 (9.098)	8.75 (8.602)
2001 (s.d.)	9.08 (9.240)	8.61 (8.758)
2002 (s.d.)	8.77 (9.266)	8.50 (8.795)
2003 (s.d.)	9.03 (9.280)	8.82 (8.858)
2004 (s.d.)	9.32 (9.311)	9.27 (9.128)
Size of teaching staff (Number of FTEs)		
2000 (s.d.)	11.14 (6.9019)	20.14 (9.5251)
2001 (s.d.)	11.54 (7.0316)	20.18 (9.0052)
2002 (s.d.)	12.08 (7.2439)	19.77 (8.7495)
2003 (s.d.)	12.16 (7.2848)	19.52 (9.2631)
2004 (s.d.)	12.06 (7.0485)	17.89 (8.5618)
Remuneration (Gross salary based on 1 FTE)		
2002 (s.d.)	2695.65 (366.18)	2705.35 (383.80)
2003 (s.d.)	2753.60 (375.87)	2758.00 (393.28)
2004 (s.d.)	2756.30 (372.13)	2763.96 (390.36)
School characteristics		
Share minority pupils 1998	0.107	0.700
Share disadvantaged pupils 1999	0.284	0.854
Socioeconomic Index		
1 (least disadvantaged)	0.103	0.000
2	0.254	0.000
3	0.288	0.000
4	0.098	0.008
5	0.068	0.033
6	0.029	0.553
7 (most disadvantaged)	0.035	0.382
Urbanization School Area		
1 (Very high)	0.108	0.602
2 (High)	0.189	0.268
3 (Median)	0.193	0.065
4 (Modest)	0.260	0.041
5 (Low/none)	0.246	0.024
School Denomination		
1 (Public)	0.329	0.480
2 (Catholic)	0.299	0.276
3 (Protestant)	0.298	0.211
4 (Montessori/Daltonian)	0.058	0.024
5 (Other)	0.011	0.0081
Share female teachers	0.767	0.750
Average age teachers	41.49	41.53
School size 1998	218.39	265.81
School size 1999	220.54	263.02
Number of teachers (in 1999)	99,081	3,067
Number of schools	6,851	123

We next investigate whether the samples just above and just below the cutoff differ in terms of outcome variables and school characteristics. Table 3 reports this information for the subsamples in the 60-70% range of disadvantaged minority pupils and in the 70-80% range of these pupils. The outcome variables are reported on the individual/teacher level and the school characteristics and the teaching capacity (size of teaching staff) variables are on the school level. Except for the retention rates, the samples just below and just above the treshold are very similar in terms of pretreatment outcomes. We observe that before the treatment the average retention rates are higher for the just below sample than for the just above sample. This is even significantly so for each of the reported years. After the treatment the average retention rates are still higher among the teachers that are in the 60-70% range, and in most years significantly so as well.

Before the treatment, the sample above the cutoff reports higher hiring rates, although this difference is not significant. In each of the posttreatment years (except 2003), teachers in the above sample become even more likely to be newly employed. The differences in hiring rates become even significant in the first three years after the treatment. The experience means show a similar pattern. For both experience in education and at the school the pretreatment means are higher for the just below samples than for the just above samples, although these difference are not significant. The posttreatment means are higher for the just below samples as well, but now the differences become significant in all posttreatment years (except for one year). The descriptive statistics regarding hiring rates and experience levels are not surprising since schools above the cutoff have higher shares of disadvantaged minority pupils. As the share of these pupils is a continuous variable, it allows to control for smooth functions of this variable in our models.

Before the treatment, but also thereafter, schools in the just below sample have on average a larger total teaching staff (number of full-time teacher jobs). None of these differences are however significantly different. This corresponds to the number of teachers per school and the fact that these schools are larger in terms of number of pupils. Data on remuneration is only availabe from 2002 onwards. Hence, only the posttreatment averages are reported in the table. Teachers who taught at schools that were almost eligible for the subsidy earn higher salary on average in each of the three reported postintervention years. The differences are not large but statistically significant. A possible explanation is that teachers in the just below sample have on average more experience in both education and at the school.

The remainder of Table 3 reports average values of several schoolcharacteristics. We observe significant differences between the 60-70% range and the 70-80% range for the share of disadvantaged minority pupils, share of pupils from any disadvantaged group, and socioeconomic index. This is not unexpected since the just below and the just above groups are determined by the proportion of disadvantaged minority pupils, and schools with a higher share of minority pupils are more likely to have a higher share of pupils from any disadvantaged group. The same is true for the schools' socioeconomic index, which is closely related to the share of (disadvantaged) minority pupils. We observe a higher proportion of schools from the most disdadvantaged group in the just above subsample. There also appears to be a significant difference in terms of denomination of the school. The proportion of public schools is higher for the group of schools in the just below subsample than in the just above subsample. For all other variables we do not observe significant differences between the two subgroups of the discontinuity sample.

Altogether, the descriptive results in table 3 show that, except for the retention rates, schools in the two subgroups of the discontinuity sample are very similar in terms of pretreatment outcomes. The schools are also very similar in terms of most background characteristics. As one would expect, the schools differ in background characteristics that determine eligibility status. Furthermore, there appears to be some differences between the two subsamples in terms of school denomination. We control for these differences in our empirical strategy by means of covariates or fixed effects.

Table 3: Sample means just below and just above samples

	DS - 10 (1)	DS + 10 (2)	P-value (3)
Retention			
2000 (s.d.) Pretreatment	0.883 (0.322)	0.856 (0.351)	0.032 **
2000 (s.d.) Posttreatment	0.870 (0.336)	0.845 (0.363)	0.043 **
2001 (s.d.) Pretreatment	0.810 (0.392)	0.764 (0.425)	0.002 ***
2001 (s.d.) Posttreatment	0.732 (0.443)	0.668 (0.471)	0.000 ***
2002 (s.d.) Pretreatment	0.751 (0.432)	0.703 (0.457)	0.003 ***
2002 (s.d.) Posttreatment	0.663 (0.473)	0.635 (0.482)	0.106
2003 (s.d.) Pretreatment	0.696 (0.460)	0.646 (0.478)	0.004 ***
2003 (s.d.) Posttreatment	0.592 (0.492)	0.569 (0.495)	0.208
2004 (s.d.) Pretreatment	0.666 (0.472)	0.612 (0.488)	0.003 ***
2004 (s.d.) Posttreatment	0.499 (0.500)	0.469 (0.499)	0.099 *
Hiring			
1999 (s.d.) Pretreatment	0.118 (0.323)	0.130 (0.337)	0.307
2000 (s.d.) Posttreatment	0.130 (0.336)	0.169 (0.356)	0.002 ***
2001 (s.d.) Posttreatment	0.133 (0.340)	0.163 (0.369)	0.027 **
2002 (s.d.) Posttreatment	0.124 (0.330)	0.150 (0.357)	0.042 **
2003 (s.d.) Posttreatment	0.098 (0.298)	0.095 (0.293)	0.732
2004 (s.d.) Posttreatment	0.074 (0.262)	0.086 (0.280)	0.244
Experience			
In Education			
1999 (s.d.) Pretreatment	17.25 (11.403)	16.94 (11.590)	0.462
2000 (s.d.) Posttreatment	17.33 (11.760)	16.57 (12.055)	0.076 *
2001 (s.d.) Posttreatment	16.72 (12.251)	16.19 (12.448)	0.248
2002 (s.d.) Posttreatment	16.53 (12.510)	15.46 (12.664)	0.018 **
2003 (s.d.) Posttreatment	16.82 (12.613)	15.59 (12.696)	0.008 ***
2004 (s.d.) Posttreatment	17.46 (12.773)	16.12 (12.975)	0.006 ***
At the school			
1999 (s.d.) Pretreatment	8.98 (8.468)	8.80 (8.314)	0.559
2000 (s.d.) Posttreatment	8.95 (8.644)	8.53 (8.554)	0.178
2001 (s.d.) Posttreatment	8.96 (8.835)	8.21 (8.654)	0.020 **
2002 (s.d.) Posttreatment	8.92 (8.897)	8.056 (8.665)	0.007 ***
2003 (s.d.) Posttreatment	9.23 (9.001)	8.40 (8.685)	0.010 ***
2004 (s.d.) Posttreatment	9.74 (9.262)	8.77 (8.963)	0.005 ***
Size of teaching staff (Number of FTEs)			
1999 (s.d.) Pretreatment	20.77 (9.6636)	18.81 (8.1734)	0.228
2000 (s.d.) Posttreatment	20.83 (10.2444)	19.44 (8.7626)	0.419
2001 (s.d.) Posttreatment	20.93 (9.4075)	19.39 (8.5833)	0.366
2002 (s.d.) Posttreatment	20.33 (8.6179)	19.21 (8.9147)	0.479
2003 (s.d.) Posttreatment	20.01(9.7257)	19.03 (8.8294)	0.560
2004 (s.d.) Posttreatment	18.31 (8.4045)	17.47 (8.7656)	0.591
Remuneration (Gross salary based on 1 FTE)			
2002 (s.d.) Posttreatment	2723.37 (370.06)	2685.52 (397.57)	0.007 ***
2003 (s.d.) Posttreatment	2775.03 (384.61)	2739.27 (401.90)	0.014 **
2004 (s.d.) Posttreatment	2782.91 (386.35)	2743.53 (393.76)	0.009 ***
School characteristics			
Share minority pupils 1998	0.648	0.753	0.000 ***
Share disadvantaged pupils 1999	0.841	0.868	0.047 **
Socioeconomic Index			0.014 **
1 (least disadvantaged)	0.000	0.000	
2	0.000	0.000	
3	0.000	0.000	
4	0.000	0.016	
5	0.032	0.033	
6	0.710	0.393	
7 (most disadvantaged)	0.242	0.525	
Urbanization School Area			0.284
1 (Very high)	0.565	0.639	
2 (High)	0.290	0.246	
3 (Median)	0.065	0.066	
4 (Modest)	0.032	0.049	
5 (Low/none)	0.048	0.000	
School Denomination			0.05 **
1 (Public)	0.613	0.344	
2 (Catholic)	0.177	0.377	
3 (Protestant)	0.177	0.246	
4 (Montessori/Daltonian)	0.016	0.033	
5 (Other)	0.016	0.000	
Share female teachers	0.755	0.745	0.500
Average age teachers	41.75	41.31	0.445
School size 1998	280.23	251.16	0.159
School size 1999	278.05	247.75	0.145
Number of teachers (in 1999)	1633	1434	
Number of schools	62	61	

Note: * significance at the 10% level, ** significance at the 5% level and *** significance at the 1% level.

4 Results

In this section we describe the results of our empirical analyses. We start with describing the results of the regression discontinuity design. Subsequently, we discuss the results based on the difference-in-differences approach to obtain more accurate and more reliable estimates. The latter approach can be performed for all outcome variables, except for remuneration as the data on this variable were only registered from 2002 onwards.

4.1 Regression Discontinuity Results

Table 4 presents the estimates on teacher turnover rates and teachers' experience derived from the regression discontinuity analyses. The table presents estimates for the whole sample of schools and for the 10% discontinuity sample. We obtained these results by regressing the different outcome variables on a dummy variable that specifies whether or not a teacher is employed at a school that receives the personnel subsidy (treatment). The dummy equals 1 for teachers who taught at a school that receives the treatment, and 0 otherwise. To deal with the fact that a few schools did not comply to the eligibility rule, we conduct two-stage-least-squares (2SLS) analyses where eligibility is used as an instrument for actual receipt of the treatment. We control for polynomials in the fractions of disadvantaged minority pupils and share of pupils from any disadvantaged group. Since we report the outcome variables for each year seperately, we do not need to include a dummy variable to identify the relevant year. The regressions for retention in each year include dummy variables for teacher's gender and covariates for teacher's age and experience (both in education and at the school). Furthermore, the regressions include controls for the schoolcharacteristics by means of dummies for socioeconomic index, school denomination, degree of urbanization and covariates for school size in 1998 and 1999. For the hiring and experience outcome variables the individual control variables are dropped as they become irrelevant.

In table 4 the results from three different specifications are presented. The first specification only controls for first degree polynomials in the share of disadvantaged minority pupils in 1998 and the proportion of pupils from any disadvantaged group in 1999. In the second and third specification respectively quadratic and cubic controls are added to the regression. We observe that for the full population of schools the point estimates change substantially when we add quadratic controls (and in some cases the signs change). This is especially the case for the point estimates with respect to the experience outcome variables. The point estimates of the regressions based on the 10% discontinuity sample, however, hardly change when we add quadratic controls. If we add third-order controls the point estimates from the regressions based on the whole population hardly change any further. For the regressions based on the discontinuity samples it is the other way around. The point estimates from the regressions based on the 10% discontinuity samples do change substantially and also change sign in many cases when we add third-order terms.

Table 4: Regression Discontinuity IV Estimates on Teacher Turnover and Teachers' Experience

	(1)		(2)		(3)	
	All schools	DS ± 10	All schools	DS ± 10	All schools	DS ± 10
Retention						
2000	-0.0013 (0.0156) [N=99,018]	0.0508 (0.0422) [N=3,067]	-0.0268 (0.0203) [N=99,018]	0.0486 (0.0420) [N=3,067]	-0.0268 (0.0203) [N=99,018]	0.0662 (0.0540) [N=3,067]
2001	-0.0670 (0.0306) ** [N=99,018]	0.0323 (0.0991) [N=3,067]	-0.1036 (0.0445) ** [N=99,018]	0.0316 (0.0973) [N=3,067]	-0.1031 (0.0445) * [N=99,018]	-0.0579 (0.1580) [N=3,067]
2002	-0.0127 (0.0245) [N=99,018]	0.0595 (0.0671) [N=3,067]	-0.0151 (0.0342) [N=99,018]	0.0608 (0.0663) [N=3,067]	-0.0152 (0.0343) [N=99,018]	-0.0016 (0.1057) [N=3,067]
2003	-0.0096 (0.0263) [N=99,018]	0.0773 (0.0777) [N=3,067]	-0.0096 (0.0351) [N=99,018]	0.0807 (0.0766) [N=3,067]	-0.0098 (0.0351) [N=99,018]	-0.0369 (0.1147) [N=3,067]
2004	-0.0067 (0.0267) [N=99,018]	0.0645 (0.0800) [N=3,067]	0.0015 (0.0351) [N=99,018]	0.0676 (0.0785) [N=3,067]	0.0012 (0.0351) [N=99,018]	-0.0909 (0.1206) [N=3,067]
Hiring						
2000	0.0245 (0.0149) * [N=103,380]	-0.0102 (0.0402) [N=3,131]	0.0324 (0.0196) * [N=103,380]	-0.0116 (0.0396) [N=3,131]	0.0328 (0.0196) * [N=103,380]	-0.0366 (0.0529) [N=3,131]
2001	0.0013 (0.0182) [N=104,928]	-0.0266 (0.0410) [N=2,946]	0.0322 (0.0224) [N=104,928]	-0.0242 (0.0409) [N=2,946]	0.0323 (0.0224) [N=104,928]	-0.0619 (0.0519) [N=2,946]
2002	0.0118 (0.0148) [N=110,781]	0.0157 (0.0415) [N=3,093]	0.0331 (0.0194) * [N=110,781]	0.0145 (0.0407) [N=3,093]	0.0333 (0.0194) * [N=110,781]	-0.0078 (0.0517) [N=3,093]
2003	0.0015 (0.0137) [N=111,569]	-0.0267 (0.0404) [N=3,016]	-0.0042 (0.0171) [N=111,569]	-0.0325 (0.0407) [N=3,016]	-0.0040 (0.0172) [N=111,569]	-0.0178 (0.0476) [N=3,016]
2004	-0.0067 (0.0123) [N=111,124]	-0.0127 (0.0369) [N=2,748]	-0.0077 (0.0165) [N=111,124]	-0.0147 (0.0367) [N=2,748]	-0.0079 (0.0167) [N=111,124]	-0.0365 (0.0529) [N=2,748]
Experience						
In Education						
2000	-1.3308 (0.6139) ** [N=103,380]	2.4537 (2.0409) [N=3,131]	0.5979 (0.8378) [N=103,380]	2.4256 (2.0439) [N=3,131]	0.5678 (0.8346) [N=103,380]	4.0796 (2.7042) [N=3,131]
2001	-0.8972 (0.6534) [N=104,928]	3.2017 (2.0351) [N=2,946]	0.7844 (0.8583) [N=104,928]	3.1560 (2.0484) [N=2,946]	0.7502 (0.8583) [N=104,928]	4.5690 (2.6332) * [N=2,946]
2002	-1.0433 (0.6390) [N=110,781]	2.1519 (1.8041) [N=3,093]	0.1738 (0.8244) [N=110,781]	2.1287 (1.8141) [N=3,093]	0.1417 (0.8254) [N=110,781]	4.1535 (2.3166) * [N=3,093]
2003	-1.2306 (0.6123) ** [N=111,565]	2.6440 (1.7026) [N=3,016]	0.2187 (0.7703) [N=111,565]	2.6750 (1.6733) [N=3,016]	0.1770 (0.7695) [N=111,565]	4.1247 (2.0415) ** [N=3,016]
2004	-1.4130 (0.6970) ** [N=111,124]	2.9505 (1.9702) [N=2,748]	0.0370 (0.8686) [N=111,124]	2.9301 (1.9311) [N=2,748]	-0.0094 (0.8650) [N=111,124]	3.9363 (2.5903) [N=2,748]
At the school						
2000	-0.9142 (0.4935) * [N=103,380]	0.4919 (1.3610) [N=3,131]	0.3070 (0.6750) [N=103,380]	0.4494 (1.3402) [N=3,131]	0.3004 (0.6661) [N=103,380]	0.2115 (1.8504) [N=3,131]
2001	-0.8184 (0.5295) [N=104,928]	0.1982 (1.4803) [N=2,946]	-0.0158 (0.7140) [N=104,928]	0.1847 (1.5296) [N=2,946]	-0.0416 (0.7073) [N=104,928]	0.3193 (2.1192) [N=2,946]
2002	-0.8003 (0.5289) [N=110,781]	0.3052 (1.4306) [N=3,093]	-0.1429 (0.6940) [N=110,781]	0.2611 (1.4393) [N=3,093]	-0.1599 (0.6892) [N=110,781]	1.1225 (2.0580) [N=3,093]
2003	-0.8055 (0.5340) [N=111,569]	0.2943 (1.4437) [N=3,016]	-0.0987 (0.6855) [N=111,569]	0.3153 (1.4485) [N=3,016]	-0.1248 (0.6804) [N=111,569]	0.2212 (2.0685) [N=3,016]
2004	-0.8515 (0.5723) [N=111,124]	0.6355 (1.6487) [N=2,748]	-0.1550 (0.7373) [N=111,124]	0.6512 (1.6523) [N=2,748]	-0.1957 (0.7316) [N=111,124]	0.5614 (2.3613) [N=2,748]
Degree polynomial in fractions of disadvantaged pupils	1st		2nd		3rd	

Note: Standard errors (in parentheses) take into account clustering at the school level and are heteroskedasticity robust. The control variables for the retention outcome variables are polynomials of the fractions of disadvantaged (minority) pupils, teacher's experience, age and gender, schoolsize in 1998 and 1999, and dummies for socioeconomic index, denomination and degree of urbanization of the school. For the hiring and experience outcome variables the individual covariates are dropped.
*Significance at the 10% level, ** significance at the 5% level and *** significance at the 1% level.

None of the point estimates of the first and second specifications that are based on the discontinuity samples show a significant effect of the subsidy. However, some of the point estimates with respect to experience in education become statistically significant when third-order terms are added. Let's focus on the point estimates of the discontinuity sample where cubic controls are added (column 6). In 2000 the retention point estimate is positive, while in subsequent years they are negative. This indicates that teachers who taught at schools that received the treatment were more likely to stay at the school short after the payment of the first part of the subsidy, but that they were less able in doing so in subsequent years. The point estimates with respect to hiring rates are all negative, indicating that the subsidy did not cause hiring of more new teachers. None of these retention and hiring point estimates are however significantly different from zero. Nevertheless, the results indicate that the subsidy has changed teacher composition. In 2001, 2002 and 2003 the point estimates of the effect on the experience in education outcome variables are significant (in 2001 and 2002 only at the 10% level and in 2003 even at the 5% significance level). These results indicate that the subsidy enabled schools to employ teachers who are on average about 4 years more experienced in the teaching profession. Regressing the same independent variables on "eligibility" (placebo treatment) shows no significant effects on any of the outcome variables (the results of these 'robustness ckeck regressions' are reported in table A1 in the Appendix and are based on an arbitrary cutoff level at 50% disadvantaged minority pupils). It seems like the subsidy have had the desired effect. In the next subsection we check whether these results still hold when we use a difference-in-differences approach. This difference-in-differences model corrects for initial differences (from the pre-intervention years) as well, and therefore it provides more reliable and more precise estimates.

Table 5 presents the RD estimates on the size of the teaching staff and remuneration. The table is constructed in the same way as table 4. Again we observe that for the full population of schools the point estimates change substantially when we add quadratic controls (and in some cases the sign changes). This is the case for both the point estimates of the effect on the size of the teaching staff and those of the effect on remuneration. The point estimates of the regressions based on the discontinuity samples hardly change when we add quadratic controls. If we add third-order controls the point estimates from the regressions based on the whole population hardly change any further. For the regressions based on the discontinuity samples it is again the opposite. The point estimates based on the discontinuity samples change substantially (and also change sign in some cases). Nevertheless, none of the point estimates become statistically significant. This indicates that schools who received the subsidy did not employed more FTEs (either by employing more teachers or persuade teachers to work more). Moreover, the results indicate that the subsidy is not used to offer teachers a higher remuneration. Table A2 in the appendix reports the results of the RD estimates of "eligibility" on the size of the teaching staff and remuneration as robustness check (based on an arbitrary cutoff level at 50% disadvantaged minority pupils). The RD "placebo" point estimates of the effects on remuneration show no effects at all. The effects of "eligibility" on the size of the teaching staff are statistically significant negative in 3 of the 5 reported years, although the estimates are relatively small. The reason for these outcomes might be due to the fact that the regressions of the robustness check are based on a larger number of observations (220 schools in the 40-60% range compared to 123 schools in the 60-80% range). Nevertheless, this does not counteract our findings that the subsidy had no effect on the number of full-time teacher jobs (FTEs) that are employed by a school and that the treatment had no effect on teachers' remuneration.

Table 5: Regression Discontinuity IV Estimates on the Size of the Teaching Staff and Remuneration

	(1)		(2)		(3)	
	All schools	DS ± 10	All schools	DS ± 10	All schools	DS ± 10
Size of teaching staff (Number of FTEs)						
2000	0.6134 (0.3533) * [N=6,798]	2.9280 (1.9784) [N=123]	-0.8222 (0.4545) * [N=6,798]	2.7583 (1.9775) [N=123]	-0.7489 (0.4543) * [N=6,798]	2.010 (2.7729) [N=123]
2001	0.0673 (0.3926) [N=6,622]	1.1390 (1.9574) [N=114]	-0.5764 (0.5086) [N=6,622]	1.0291 (1.9291) [N=114]	-0.4349 (0.5083) [N=6,622]	0.0457 (2.6529) [N=114]
2002	0.2455 (0.4240) [N=6,675]	2.4698 (1.7593) [N=122]	-0.3445 (0.5473) [N=6,675]	2.3124 (1.7554) [N=122]	-0.2608 (0.5471) [N=6,675]	0.2424 (2.3579) [N=122]
2003	0.7818 (0.4683) * [N=6,690]	3.0942 (1.9952) [N=122]	-0.1354 (0.6048) [N=6,690]	2.8902 (1.9752) [N=122]	-0.0602 (0.6048) [N=6,690]	1.1036 (2.6807) [N=122]
2004	0.5431 (0.4931) [N=6,688]	3.1746 (2.0262) [N=122]	0.5756 (0.6375) [N=6,688]	2.8858 (1.9741) [N=122]	0.6736 (0.6374) [N=6,688]	-0.9005 (2.6430) [N=122]
Remuneration (Gross monthly salary based on 1 FTE)						
2002	1.0033 (7.7926) [N=109,158]	-2.3354 (24.6790) [N=2,979]	-9.5786 (9.8808) [N=109,158]	-2.4679 (24.2958) [N=2,979]	-9.4730 (9.8879) [N=109,158]	-4.7248 (29.8272) [N=2,979]
2003	-4.3612 (8.2386) [N=111,520]	-16.5098 (23.0784) [N=2,956]	-9.7354 (10.6497) [N=111,520]	-15.9413 (22.9750) [N=2,956]	-9.4858 (10.6486) [N=111,520]	-17.8668 (31.3873) [N=2,956]
2004	5.4327 (7.9378) [N=111,078]	1.8634 (27.2700) [N=2,702]	-1.5470 (10.3049) [N=111,078]	1.3111 (26.0001) [N=2,702]	-1.3580 (10.2843) [N=111,078]	36.5671 (38.4582) [N=2,702]
Degree polynomial in fractions of disadvantaged pupils	1st		2nd		3rd	

Note: Standard errors (in parentheses) take into account clustering at the school level and are heteroskedasticity robust. The control variables for the teaching capacity (size of teaching staff in FTE) outcome variables are polynomials of the fractions of disadvantaged (minority) pupils, average age of the teachers, share female teachers, schoolsize in 1998 and 1999, and dummies for socioeconomic index, denomination and degree of urbanization of the school. For the Remuneration outcome variables teacher's experience, age and gender come into the place of average age of teachers and share female teachers.
* Significance at the 10% level, ** significance at the 5% level and *** significance at the 1% level.

4.2 Difference-in-Differences Results

We continue by reporting the findings from the difference-in-differences approach using the samples that are at most 10% around the discontinuity. Table 6 reports the point estimates for the personnel subsidy on the different outcome variables. As mentioned earlier, the current approach is not conducted for the remuneration outcome variables, as data on salaries were only registered from 2002 onwards. We report the outcomes for each intervention year (2000 to 2004) separately, as these are in fact different outcomes. This also allows to detect possible effects in some specific years, as the treatment might be only effective short after implementation of the subsidy. Moreover, the payment of the subsidy is spread out over a period of one year, which might only enable to detect possible effects in some of the postintervention years and not in other postintervention years.

First we observe that the results reported in table 6 are somewhat different from the RD results reported in tables 4 and 5. Although boh methodological procedures give insignificant outcomes, the point estimates of the effect on the retention rates have opposite signs in most years. For the hiring rates, a similar pattern emerges. The point estimates differ as well, and for three of the five years the estimates do not have the same sign. For the year 2000, the point estimate even becomes significant. When we analyse the estimates of the experience outcome variables, we again see a similar pattern. For the experience in education, the estimates become quite small in absolute terms, have an opposite sign, and for the year 2004 the estimate becomes significant. For the experience at the school variables, the signs of the estimates all change, but are, just like the RD results, not significantly different from zero.

We would actually expect that the diff-in-diff results would not differ from the RD results. Recall that with the RD model we compare the outcomes of schools just above the cutoff to the outcomes of schools just below the cutoff. By exploting the discontinuity in a diff-in-diff model we correct for possible initial differences. Because schools just around the cutoff should be very similar, we would not expect large initial differences. However, we conduct the analyses by using a 10% discontinuity sample (DS $\pm$ 10%). Applying a wide discontinuity sample will increase the number of observations but at the same time it increases the risk that schools and teachers around the cutoff differ somewhat more. Although the treatment and control group in the 10% discontinuity sample follow the same trend during the pre-intervention years, it seems that there were some initial differences between the groups. Therefore, using the diff-in-diff approach provides more reliable and more precise estimates as this approach corrects for these initial differences.

In contrast, the results of the teaching capacity (size of teaching staff in FTE) outcome variables are not very different from the RD results reported in table 5. The estimates are, just like the RD estimates, not significantly different from zero. Furthermore, except for the year 2004, they have the same sign. In 2001 the Duth Ministry of Education commissioned a research agency to investigate how schools actually spent the personnnel subsidy. This research is conducted by means of a telephone questionnaire survey among eligible schools. The findings of Beerends and Van der Ploeg (2001) indicate that over one third of the subsidy was allocated to the hiring and recruitment of (temporary) personnel. This is however only partly attested by our fndings: in 2000, we report a significant positive effect on hiring rates. However, in subsequent years the estimates are insignificant. Moreover, we did not find any significant effect on the number of FTEs that a school employs.

Next consider the results on the retention rate variables. For the years 2000 and 2001 the estimates are negative. For the subsequent years, the estimates become positive. This indicates that short after receiving the subsidy there is no effect on retaining teachers, where as it seems that schools improved their abiliy to retain teachers in the school years after the last payment (which took place in March 2001). The estimates are, however, all not significantly different from zero. Hence, there is no evidence that the personnel subsidy improved schools' ability to retain their teachers.

For the hiring rates, the estimates are positive for the years 2000, 2001 and 2002, and negative for the two years thereafter. However, only the effect on the hiring rates in 2000 is significantly different from zero (at a 5% significance level). The point estimate for this year equals 0.0289 and should be interpreted as the effect of the subsidy on the hiring rates in 2000. The probability that a teacher who taught at a school that received the subsidy was newly hired, was -compared to teachers who taught at schools who did not receive the extra funding- 2.89%-points higher. This is strong evidence that the personnel subsidy had a significant positive effect on the hiring rates in the year 2000. It seems that this effect only persisted on the short term, during the 8 months after the announcement of the subsidy and the 5 months after the first payment of it.

The results on the retention and hiring rates are also reflected in the results on the experience outcome variables. For the years of experience variables, we observe that all point estimates (except for experience in education in 2001) have a negative sign. Recall that in 2000 and 2001 the subsidy had a negative effect on teacher retention (although not significant), and until 2002 the effect on hiring novel teachers was positive (and in 2000 even significantly so). It seems that the negative point estimates of the effect on retention rates and the positive point estimates of the effect on hiring rates decreased the average years of experience of teachers who taught at schools that received extra funding. This is not surprising as newly hired teachers are in general the younger, and less experienced teachers. The point estimates are, however, in almost all cases not significantly different from zero. Only the effect on the years of experience in education in 2004 is significantly different from zero, but only at the 10% significance level.

As mentioned earlier, the Diff-in-Diff results of the effect on the teaching capacity (size of teaching staff in FTE) are very similar to those from the RD approach reported in table 5. The point estimates for all years are positive but relatively small. Furthermore, the estimates are, just like the RD estimates, not significantly different from zero. The findings therefore indicate that the extra funding had no positive impact on the number of FTEs hired by the schools that received the treatment.

In any case, the extra funding did not have the desired effect on teacher experience. The results show that the personnel subsidy fail to hire and retain (better) teachers with more experience. The results are in line with Leuven et al. (2007) who found negative (and in some cases even significantly) effects of the personnel subsidy on pupils' achievement. These findings also confirm their presumption that it might be due to the fact that the subsidy fail to hire (better) more experienced teachers.

We also conducted a robustness check, where we performed the same analyses using the arbitrary ("'fake") cutoff level at 50% disadvantaged minority pupils. The results of this exercise where we measure the effects of "eligibility" (a placebo treatment) are reported in table A3 of the appendix. Except for the retention rate in 2000, none of the point estimates are significantly different from zero. Note that the point estimate of the effect on hiring rates in 2000 (which is significantly positive in the normal analysis at the 5% significance level) is very small and not significantly different from zero in our robustness check. The only point estimate of the robustness check that appeared to be significant can be expected with a 5% significance level. Since the robustness check consists of 25 point estimates, and because we apply a 5% significance level, we can expect about one point estimate to be statistically significant different from zero. Therefore, the results of this robustness check support our findings and are further evidence that the subsidy did not have the desired effect on the retention and hiring of new teachers, and thereby not on teacher experience.

Summarizing, the findings indicate that the personnel subsidy has no impact on schools' ability to retain teachers. The extra funding, however, had a significant effect on the hiring rates at the onset of the school year after the announcement of the subsidy and the payment of the first part of the subsidy (October 2000). In the following years there is no significant effect on hiring rates. Furthermore, the extra funding had no positive impact on the number of FTEs hired by the schools that received the treatment. Nevertheless, our findings indicate that extra funding to improve teachers' working conditions does not have a positive impact on the average experience of teachers. The point estimate for the experience in education outcome variable in 2004 is even significantly negative at the 10% siginificance level. Additional (temporary) funding for schools with large proportions of disadvantaged minority pupils seems to have no positive impact on hiring and retaining the (better) more experienced teachers. These findings confirm the presumption of Leuven et al. (2007). They presumed that the effect of the personnel subsidy on pupils' achievement might be due to the fact that the subsidy fail to hire (better) more experienced teachers.

Table 6: Difference-in-Differences IV Estimates on Different Outcome Variables

	$\hat{\delta}_{2000}$ (s.e.)	$\hat{\delta}_{2001}$ (s.e.)	$\hat{\delta}_{2002}$ (s.e.)	$\hat{\delta}_{2003}$ (s.e.)	$\hat{\delta}_{2004}$ (s.e.)
1. Teacher Turnover and Experience					
Retention	-0.0099 (0.0124) [N=6,128]	-0.0309 (0.0312) [N=6,084]	0.0041 (0.0258) [N=5,5974]	0.0063 (0.0270) [N=5,979]	0.0162 (0.0272) [N=5,845]
Hiring	0.0289 (0.0130) ** [N=6,198]	0.0131 (0.0155) [N=6,013]	0.0098 (0.0139) [N=6,160]	-0.0186 (0.0152) [N=6,083]	-0.0063 (0.0143) [N=5,815]
Experience					
In Education	-0.3010 (0.1919) [N=6,198]]	0.0552 (0.2735) [N=6,013]]	-0.2634 (0.2964) [N=6,160]	-0.5262 (0.3607) [N=6,083]	-0.7903 (0.4050) * [N=5,815]
At the school	-0.2063 (0.1322) [N=6,198]	-0.2700 (0.1947) [N=6,013]	-0.2753 (0.2046) [N=6,160]	-0.3239 (0.2523) [N=6,083]	-0.3824 (0.2931) [N=5,815]
2. Size of teaching staff					
Size of teaching staff (Number of FTEs)	0.7081 (0.9114) [N=246]	0.0592 (0.9090) [N=237]	0.2108 (0.8698) [N=245]	0.2603 (0.9413) [N=245]	0.4808 (0.9776) [N=245]

Note: Standard errors (in parentheses) take into account clustering at the school x year level and are heteroskedasticity robust.
For the retention outcome variables individual teacher characteristics experience, age and gender are added. For the attrition and experience outcome variables the individual covariates are dropped. The teaching capacity (size of teaching staff) outcomes are on the school level and therefore not clustered. The control variablesfor these outcomes are fractions of disadvantaged (minority) pupils, average age of the teachers, share female teachers, schoolsize in 1998 and 1999, and dummies for socioeconomic index, denomination and degree of urbanization of the school.
* Significance at the 10% level, ** significance at the 5% level and *** significance at the 1% level.

5 Conclusion and Policy Recommendations

5.1 Conslusion

In this thesis we evaluate a subsidy targeted at schools with large proportions of disadvantaged minority pupils. This subsidy was aimed to improve the working conditions of teachers in primary education, as numbers of previous studies indicated that poor working conditions were strongly related to high teacher mobility. Moreover, it was known that schools with large proportions of disadvantaged (minority) pupils tend to have more difficulties to retain and hire more experienced teachers, as teachers tend to move from schools with large shares of disadvantaged (minority) pupils to schools with economically less disadvantaged pupils and with smaller shares of minority students. This seems a convincing justification to provide such schools extra resources for personnel. However, one may wonder how effective these extra resources are, as this subsidy comes on top of the higher funding from the main funding scheme, which is based on the proportion of disadvantaged pupils. Moreover, an earlier study by Leuven et al. (2007) on the effect of the subsidy on pupils' achievement found negative point estimates, and in some cases even significantly so.

The subsidy scheme specifies a cutoff level of 70% disadvantaged minority pupils below which schools do not receive the subsidy. Schools with at least 70% disadvantaged minority pupils were eligible for the personnel subsidy, and all schools receive the same amount per teacher independent of the exact share of minority pupils. The cutoff at the 70% level was maintained quite strictly. Moreover, schools were not able to manipulate their proportion of disadvantaged minority pupils as this proportion was based on the basis of information from almost one-and-a-half years prior to the announcement of the subsidy. Hence, the features of this subsidy provide convincing opportunities to evaluate the effects of it.

The results show that the (temporary) personnel subsidy did not have the desired impact on several aspects of teacher mobility. The point estimates of the effect on retention are slightly negative in the first two years, and slighly positive thereafter. However, none of these estimates is statistically significant, thereby indicating that extra funding has no impact on schools' ability to retain their teachers. There seems to be a significant effect on hiring rates during the period between the announcement of the extra funding and the start of the following school year (October 2000). However, in the following years this significant effect disappears. More important is to know what type of teachers are retained or hired due to the subsidy. If a school for example becomes more able to hire and retain the more experienced teachers, the average years of experience of their teachers increases. Our results show that, except for experience in education in 2001, all point estimates have a negative sign. The point estimate for the dependent variable 'experience in education' in 2004 is even significantly negative at the 10% significance level. Therefore, we can conclude that (temporary) extra funding for schools with high shares of disadvantaged minority pupils to improve teachers' working conditions, does not have a positive impact on the average teacher experience. Finally, the subsidy seems to have no effect on teacher remuneration nor on the total number of full-time teacher jobs that a school employs.

Additional (temporary) funding for schools with large proportions of disadvantaged minority pupils seems to have no positive impact on hiring and retaining the (better) more experienced teachers. Furthermore, there is no evidence that the eligible schools allocated the extra funding to the hiring and recruitment of extra (temporary) personnel, thereby not improving the pupil-teacher ratio. These findings confirm the presumption of Leuven et al. (2007) that their findings of the (lack of a) effect of the personnel subsidy on pupils' achievement might be due to the fact that the subsidy fail to improve school's ability to hire (better) more experienced teachers. There are several reasons for the absence of any effects on teacher experience, but at the end it is the net effect of two opposing processes: the retention and hiring of (more experienced) teachers. Teacher retention has usually a positive effect on the average age of teachers as a new hired teacher, who replaces the departed teacher, has usually less experience. Hiring new teachers can also lead to a higher average of teacher experience if the newly hired teacher has more experience than the replaced teacher. However, it is more likely that newly hired teachers have less experience. The reason is that teachers with more experience are less inclined to switch to another school, especially if the new school has a more disadvantaged student composition. Despite of the extra funding for personnel, which came on top of the extra funding from the main funding scheme, schools with high shares of disadvantaged minority pupils did not succeed in creating a more experienced (and better/more effective) teacher population.

5.2 Policy Recommendations

From the evidence we provide in earlier sections, we conclude that extra funding for schools with large shares of minority pupils did not improve the ability of such schools to hire and retain better (more experienced teachers). Furthermore, there is no evidence that the eligible schools allocated the extra funding to the hiring and recruitment of extra (temporary) personnel. Therefore, it seems that the subsidy has not been effective in improving the pupil-teacher ratio. An earlier study already showed very convincing evidence that the subsidy did not improve pupils' achievement. Our findings are in line with this outcome.

The findings of both our study and the previous study on the effects on student performance raise questions about the effectiveness of extra funding for schools with disadvantaged (minority) pupils. Recall that the main funding scheme for Dutch primary schools, at the time this subsidy was provided, disadvantaged minority pupils had a 90% higher weight than nondisadvantaged pupils. Due to this extra resources, schools with high shares of disadvantaged minority pupils already had a lower pupil-teacher ratio at the time (14 respectively 22). It seems unlikely that these schools would hire more teachers to lower the pupil-teacher ratio (also because of restrictions with respect to available space to have more classes). It seems that schools targeted by the subsidy already have sufficient resources to hire sufficient personnel. Hence, the marginal value of extra resources will be lower, and therefore less effective. In countries like the Netherlands, the government provides schools with a disadvantaged pupil population funding from a (generous) funding scheme. However, it would be better to invest available resources on programs that have been proved to be more effective in combating educational disadvantages. More research should be conducted on how schools with disadvantaged student populations could counteract teachers' tendency to leave schools with difficult/disadvantaged student compositions. In other words, what kind of schemes make schools more able to hire and retain good quality (experienced) teachers.

References

Angrist, J. D. and Pischke, J.-S. (2009). Mostly harmless econometrics: An empiricist's companion. Princeton University Press.

Beerends, H. and Van der Ploeg, S. (2001). Onderzoek vergoeding schoolspecifieke knelpunten. Technical report, Regioplan.

Bénabou, R., Kramarz, F., and Prost, C. (2009). The french zones déducation prioritaire: Much ado about nothing? Economics of Education Review, 28(3):345–356.

Berry, B. and Hirsch, E. (2005). Recruiting and retaining teachers for hard-to-staff schools. Washington, DC: National Governors Association Center for Best Practices.

Bonesronning, H., Falch, T., and Strom, B. (2005). Teacher sorting, teacher quality, and student composition. European Economic Review, 49:457–483.

Bruno, J. E. and Negrete, E. (1983). Analysis of teacher wage incentive programs for promoting staff stability in a large urban school district. The Urban Review, 15(3):139–149.

Dolton, P. and Newson, D. (2003). The relationship between teacher turnover and school performance. London Review of Education, 1(2):132–140.

Falch, T. and Strøm, B. (2005). Teacher turnover and non-pecuniary factors. Economics of Education Review, 24(6):611–631.

Feng, L. and Sass, T. (2012). Teacher quality and teacher mobility. Andrew Young School of Policy Studies Research Paper Series, (12-08).

Hanushek, E. A. (1996). School resources and student performance. Does money matter, pages 43–73.

Hanushek, E. A. (1997). Assessing the effects of school resources on student performance: An update. Educational evaluation and policy analysis, 19(2):141–164.

Hanushek, E. A. (2003). The failure of input-based schooling policies*. The economic journal, 113(485):F64–F98.

Hanushek, E. A., Kain, J. F., O'Brien, D. M., and Rivkin, S. G. (2005). The market for teacher quality. Technical report, National Bureau of Economic Research.

Hanushek, E. A., Kain, J. F., and Rivkin, S. G. (2004). Why public schools lose teachers. Journal of human resources, 39(2):326–354.

Hanushek, E. A. and Rivkin, S. G. (2006). Teacher quality. Handbook of the Economics of Education, 2:1051–1078.

Hanushek, E. A. and Rivkin, S. G. (2007). Pay, working conditions, and teacher quality. The future of children, 17(1):69–86.

Ingersoll, R. M. and Smith, T. M. (2003). The wrong solution to the teacher shortage. Educational leadership, 60(8):30–33.

Leuven, E., Lindahl, M., Oosterbeek, H., and Webbink, D. (2007). The effect of extra funding for disadvantaged pupils on achievement. The Review of Economics and Statistics, 89(4):721–736.

Loeb, S., Darling-Hammond, L., and Luczak, J. (2005). How teaching conditions predict teacher turnover in california schools. Peabody Journal of Education, 80(3):44–70.

Rivkin, S. G., Hanushek, E. A., and Kain, J. F. (2005). Teachers, schools, and academic achievement. Econometrica, 73(2):417–458.

Tabs, E. (2004). Teacher attrition and mobility.

A Appendix

Table A1: Regression Discontinuity Estimates of "Eligibility" on Teacher Turnover and Teacher Experience (Based on Arbitrary Cutoff at 50% Disadvantaged Minority Pupils)

	(1)	(2)	(3)	N
	DS ± 10	DS ± 10	DS ± 10	
Retention				
2000	-0.0317 (0.0269)	-0.0320 (0.0268)	-0.0311 (0.0375)	4,627
2001	-0.0291 (0.0399)	-0.0321 (0.0403)	-0.0705 (0.0549)	4,627
2002	-0.0179 (0.0413)	-0.0180 (0.0413)	-0.0210 (0.0577)	4,627
2003	-0.0197 (0.0423)	-0.0186 (.0422)	-0.0173 (0.0589)	4,627
2004	-0.0081 (0.0438)	-0.0088 (0.0438)	-0.0134 (0.0624)	4,627
Hiring				
2000	0.0152 (0.0312)	0.0137 (0.0307)	-0.0334 (0.0422)	4,733
2001	0.0081 (0.0261)	0.0069 (0.0265)	0.0127 (0.0383)	4,797
2002	0.0043 (0.0246)	0.0038 (0.0246)	-0.0538 (0.0343)	4,948
2003	0.0148 (0.0232)	0.0097 (0.0222)	-0.0078 (0.0324)	4,896
2004	0.0305 (0.0207)	0.0263 (0.0210)	0.0127 (0.0285)	4,638
Experience				
In Education				
2000	-0.3019 (1.2220)	-0.2112 (1.2089)	-2.0829 (1.7555)	4,733
2001	0.1008 (1.1218)	0.1566 (1.1212)	-1.8175 (1.5980)	4,797
2002	-0.0577 (1.0322)	0.0188 (1.0280)	-1.5405 (1.4323)	4,948
2003	-0.2971 (1.0548)	-0.2258 (1.0376)	-1.5508 (1.4311)	4,895
2004	-0.6968 (1.1463)	-0.7304 (1.1396)	-1.7793 (1.5736)	4,638
At the school				
2000	-0.1544 (0.9449)	-0.1123 (0.9432)	-0.9836 (1.3243)	4,733
2001	0.0816 (0.8940)	0.1359 (0.9031)	-0.9091 (1.2688)	4,797
2002	0.0823 (0.9004)	0.0626 (0.9023)	-0.3782 (1.2659)	4,948
2003	0.0285 (0.8977)	0.0168 (0.8877)	-0.4380 (1.2447)	4,896
2004	-0.4562 (0.9459)	-0.4814 (0.9522)	-0.9462 (1.3483)	4,638
Degree polynomial in fractions of disadvantaged pupils	1st	2nd	3rd	

Note: * Significance at the 10% level, ** significance at the 5% level and *** significance at the 1% level.

Table A2: Regression Discontinuity Estimates of "Eligibility" on Size of teaching Staff and Remuneration (Based on Arbitrary Cutoff at 50% Disadvantaged Minority Pupils)

	(1)	(2)	(3)	N
	DS ± 10	DS ± 10	DS ± 10	
Size of teaching staff (Number of FTEs)				
2000	-1.7401 (1.0295) *	-1.7608 (1.0404) *	-1.6616 (1.4369)	219
2001	-2.2194 (1.0401) **	-2.3243 (1.0500) **	-2.0184 (1.4505)	215
2002	-2.3293 (1.1124) **	-2.312 (1.1246) **	-3.1300 (1.5535) **	218
2003	-2.7797 (1.2115) **	-2.7977 (1.2250) **	-3.2829 (1.6937) *	218
2004	-2.0889 (1.2074) *	-2.0971 (1.2208) *	-2.8569 (1.6886) *	218
Remuneration (Gross salary based on 1 FTE)				
2002	-6.7581 (16.7890)	-4.3127 (17.8842)	-12.8809 (26.0322)	4,791
2003	-14.4677 (18.4574)	-14.6511 (19.0032)	-4.3287 (27.4906)	4,829
2004	-16.5416 (16.1560)	-14.1315 (16.8910)	2.0860 (22.7731)	4,586
Degree polynomial in fractions of disadvantaged pupils	1st	2nd	3rd	

Note: * Significance at the 10% level, ** significance at the 5% level and *** significance at the 1% level.

Table A3: Difference-in-Difference Estimates of the Effect of "Eligibility" on Different Outcome Variables (Based on Arbitrary Cutoff at 50% Disadvantaged Minority Pupils)

	$\hat{\delta}_{2000}$ (s.e.)	$\hat{\delta}_{2001}$ (s.e.)	$\hat{\delta}_{2002}$ (s.e.)	$\hat{\delta}_{2003}$ (s.e.)	$\hat{\delta}_{2004}$ (s.e.)
1. Teacher Turnover and Experience					
Retention	-0.0195 (0.0099) ** [N=9,205]	-0.0094 (0.0167) [N=9,056]	-0.0110 (0.0193) [N=9,066]	-0.0161 (0.0200) [N=9,036]	-0.0261 (0.0197) [N=8,912]
Hiring	0.0016 (0.0109) [N=9,360]	0.0066 (0.0114) [N=9,424]	0.0008 (0.0121) [N=9,575]	0.0025 (0.0108) [N=9,523]	0.0128 (0.0116) [N=9,265]
Experience					
In Education	-0.1093 (0.1551) [N=9,360]	-0.0408 (0.2141) [N=9,424]	-0.0077 (0.2654) [N=9,575]	-0.1451 (0.2961) [N=9,523]	-0.0047 (0.3382) [N=9,265]
At the school	-0.1032 (0.1203) [N=9,360]	-0.0455 (0.1679) [N=9,424]	0.0263 (0.1969) [N=9,575]	0.0251 (0.2200) [N=9,523]]	0.1670 (0.2519) [N=9,265]
2. Size of teaching staff					
Size of teaching staff (Number of FTEs)	-0.3559 (0.6149) [N=438]	-0.2462 (0.6200) [N=434]	-0.3993 (0.6479) [N=437]	-0.6349 (0.6865) [N=437]	-1.100 (0.6910) [N=437]

Note: Standard errors (in parentheses) take into account clustering at the school x year level and are heteroskedasticity robust.
The teaching capacity (size of teaching staff) outcomes are on the school level and therefore not clustered.
* Significance at the 10% level, ** significance at the 5% level and *** significance at the 1% level.