Erasmus University

Master's Thesis

Effects of additional funding on performance of disadvantaged primary school students

Abstract

In this paper, the effect of additional funding for primary schools through the Gewichtenregeling and Impulse subsidy in the Netherlands is analysed. The strict requirements that come with the impulse subsidy, allow the appliance of RDD. The RDD was altered to fit the unique case of not one but two thresholds. Data provided by governmental institutions from the schoolyears 2014-2015 up and until 2018-2019 is used. Doing so, we find significant negative effects of the additional funding on student performance. Although caution is needed in interpreting the results.

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1. Introduction and context

"Education is at its best when it emancipates. [...] The promotion of equal opportunities within the educational system is not only an important responsibility for us as ministers, but also of the educational system itself. [...] It is what we owe to the new generation and our society as a whole."

– Minister Van Engelshoven (Ministerie van Onderwijs, Cultuur en Wetenschap, 2019)

Within the Dutch field of education, there is a general consensus on whether equality of opportunity within the educational system should be obtained. A more debated topic is how to achieve this goal. During the 1980s, the Dutch government began to introduce policies to reduce the gap in educational opportunities at primary schools caused by differences in socioeconomic status of the pupils. Not only ability played a role in the performance of the pupil, but also environmental factors, such as the students' home-situation. The leverage of the latter was argued to be unethical as children should be able to all grow up with the same opportunities. Hence, the Dutch 'Weighted Student Funding scheme', or in Dutch the *Gewichtenregeling*, was brought into place. Through this measurement schools received additional funding for students with higher probabilities of experiencing educational disadvantage. Each student is assigned a 'weight' based on certain indicators that would affect their probability of learning difficulties. Between the schoolyears 2006-2007 and 2018-2019, there was a single indicator, which was the parents' educational attainment (Claassen & Mulder, 2011). There are two weights the students can be assigned:

- A weight of 0.3 when both parents did not attain an education higher than pre-vocational secondary education (Dutch: *vmbo-kader*). For these pupils, the schools received additional funding with a value of 30% of a 'regular' student.
- A weight of 1.2 when only one of the parents attained primary education, and the other did not attain an education higher than pre-vocational secondary education. For these pupils, the schools received additional funding with a value of 120% of students without a weight.

Other students are not assigned a weight. In practice, these numbers are only used on a school-level (from now on referred to as school weight), meaning that all individual 'weights' are summed for each school, and additional funding is provided accordingly. Schools are free to spend these subsidies as they prefer. Hence, these subsidies are not necessarily spent to target the ‘weighted’ students and could have affected outcomes for all students and not just the students with a weight of 0.3 or 1.2.

In a report from 2016 (Cebeon) the spending of these additional funding was researched by questioning the schoolboards. According to this report, schools with a higher average student weight are more likely to decrease their class seizes by investing in more Full-Time Equivalents (FTEs).

A policy called the impulse subsidy scheme, in Dutch the *Impulsgebiedentoeslag* or the *Impulsregeling,* is an extension to this measurement implemented during the schoolyear 2009-2010. Through this policy schools receive additional funding for each weighted student on top of the *Gewichtenregeling* if they are located within a certain postal code area. The subsidy is approximately €1700 per student, with some fluctuations over time. This amounts to approximately €150 million annually. The postal code areas are selected by the Dutch Central Bureau of Statistics (*Centraal Bureau voor de Statistiek*) based on data from the Poverty-monitor (Dutch: *Armoedemonitor*) (2005). The areas are selected if one or both of the following requirements are met:

- More than 11.3% of the households received unemployment benefits in 2004.
- More than 11.5% of the households had a low income in 2004.

These strict requirements allow the appliance of a Regression Discontinuity Design (RDD) as is also done in previous research (Leuven et al., 2007; Matsudaira et al.,2012; Verspaandonk, 2016; Van Eijk, 2017). While in this case there are not one, but two requirements to be met, the usual RDD method needs to be adjusted. Verspaandonk (2016) and CPB (2017) chose to only look around one of the thresholds and Van Eijk (2017) looked at both of them separately. This research will look at the two thresholds separately as well, but only up and until the intersection of the two requirements as will be further discussed in section 5. Using this method, the following question will be answered:

What is the effect of the school weight subsidy in primary education on student performance?

In section 2, I will go further into debt on what is used as an indicator for student performance, section 3 will provide an overview of existing literature regarding this subject, section 4 will summarize the data used in this research, in section 5 the methodology will be explained, in section 6 the results will be stated, and lastly section 7 will state the conclusion and a discussion.

2. Student performance

Both the impulse subsidy scheme as well as the weighted subsidy student funding scheme were implemented in order to decrease inequality in early education. In other words, the goal was to increase student performance of the so-called disadvantaged students. In order to draw conclusions, boundaries must be set to what we define as 'student performance' as it can be widely interpreted. Not only knowledge, but also emotional development or the level of social capacity can be seen as 'student performance'.

Regarding the measurable indicators of student performance, research often falls back on test scores as is done by Leuven et al. (2007), Matsudaira et al. (2012), CPB (2017), Verspaandonk (2016), and Van Eijk (2017). This can, however, be problematic as not all test scores are comparable if the tests are not identical. This research will, therefore, make use of the test scores from students that are taken in the last year of primary school. The goal of these tests is to assign students to the level of secondary school, and to measure the schools' educational quality. Primary schools are legally required to let their students take such an end test since the school year 2014-2015. The schools are free to decide which test to use for their students as there are a number of approved options. For the schoolyear 2019-2020, there were five options. Up till now, the by far largest share of the schools have chosen for the Central End Test (in Dutch *de Centrale Eindtoets*, previously known as the *CITO-toets*). Almost half of the schools (47,7%) chose for this test for the schoolyear 2018-2019 (Ministerie van Onderwijs, Cultuur en Wetenschap, 2021). Not all tests make use of the same grading system. In this research, all tests will be standardized, which makes it possible to include all available data and thereby solving the issue of the possibility of certain schools selecting a certain testing method.

The tests have to meet a number of requirements: it must assess Dutch language skills and mathematics. Additional subjects are optional. Students have an incentive to perform as good as possible on this test, while it assigns them to their level of secondary school. The average score of each school is made publicly available, higher scores are usually preferred by parents. Hence, schools believe high scores to lead to a higher number of enrolments. Schools are therefore incentivised to optimally prepare their students for this test. This makes the test score a solid instrument to measure the performance of the students. Other factors such as the emotional development or the level of social capacity are difficult to define in numbers and will therefore not be taken into account in this paper.

3. Literature

Evaluating the impact of additional funding in the educational system, such as the *Gewichtenregeling* is challenging as these subsidies are often targeted at certain schools. This makes a comparison between treated schools (i.e. the schools receiving the subsidy) and untreated schools (i.e. the schools not receiving the subsidy) challenging. It is likely that the students from the targeted schools score lower on average than from the untreated schools would the subsidy not have been into place. In other words, we cannot observe both potential outcomes. Therefore, often other methods than Ordinary Least Squares (OLS) are applied.

Lafortune et al. (2018) made use of the apparent randomness of post-1990 school finance reforms in the US and found that reforms lead to increased spending on low-income school districts and that within these districts the performance of students increased. According to their findings, the impact of increased resources for the schools had a large impact on the performance of the students. The findings of Lafortune et al. could suggest that policies which target children from low-income families would be successful in increasing the achievements of the pupils. However, this is contradictory to other research findings.

Matsudaira et al. (2012) examined the effect of Title 1 in the US, the largest federal aid package targeted at schools with at least 40% of the students coming from low-income families, on student achievements. They applied RDD to examine this and found no effect on school-level test scores, and neither on the achievements of the students from low-income families, on which the aid is targeted. A mentioned, possible explanation for the absence of an observable effect could be the relatively small contribution Title 1 has on total fundings of the schools. On average revenues of schools increased with 3-4% relative to not being eligible. Van der Klaauw (2008) has done similar research applying the RDD method and zoomed in on the effects in New York City. Evidence was found of a negative effect of the additional funding on student performance in earlier years of Title 1, 1993 and 1997. Less evidence was found, however, for the negative impact of Title 1 for the year 2001. Van der Klaauw discusses the way the additional funding was spend as a possible explanation for the differences in outcomes between the years.

Research done by Jackson et al. (2015) applies an event-study and instrumental variable models for which the timing of the policy reforms is used as an exogenous shock. They used data from the US. Doing so, they found a positive relationship between increased per-pupil spending on the amount of completed years of education (0.27 more completed years of education), wage-level (7.25% higher wages), and a reduction in the annual incidence of adult poverty (3.67 percentage-point reduction of adult poverty incidences per year). This holds especially for disadvantaged students. Additionally, the authors found that the increased spending on education led to a higher quality of the schools, e.g. lower student-to-teacher ratios and higher teacher salaries.

Leuven et al. (2007) apply a RDD to identify the causal effect of extra funding for disadvantaged pupils on their achievements. The authors analyse two subsidies for primary schools with large proportions of disadvantaged students in the Netherlands. One of these was a subsidy for personnel for schools with at least 70% disadvantaged minority pupils. The other scheme was for computers and software for schools with at least 70% pupils from any disadvantaged group. They found that for both subsidies the effect on student performance was negative.

In 2017, the Netherlands Bureau for Economic Policy Analysis (CPB) applied the RDD method to analyse the effectiveness of the *Gewichtenregeling*. They made use of the strict threshold in the *Impulsgebiedentoeslag*. However, they solely compared the schools around the threshold for the percentage of households receiving unemployment benefits, and not the threshold for households with a low income. In interpreting the results, they assumed no difference between the resources provided through the *Gewichtenregeling* or the *Impulsgebiedentoeslag*. Hence, their conclusion is applicable to both policies. They justify this as both subsidies share the same purpose and are paid-out at the same time. They found that primary schools just above the threshold employ 0.8-1.0 FTE more teaching personnel for each 225 students, which is a direct result of the resources provided through the subsidies. In line with the findings of Leuven et al. (2007), however, they found no improvements in educational achievements as a result.

Other research by Verspaandonk (2016) and Van Eijk (2017) applied a similar method to analyse the effects of the impulse subsidy. Verspaandonk (2016) used the same thresholds as the CPB (2017) in finding the effect of the impulse-area subsidy on student performance, namely the percentage of households receiving unemployment benefits. He used data from a single schoolyear, namely 2014-2015. Van Eijk (2017) expanded on this by including more data on different school years (2013 up till 2016) and applying both eligibility criteria. Thereby, splitting the group into four subgroups of which three received the treatment (i.e. schools that were eligible for the impulse-area subsidy based on either only the first criterium, the second criterium, or both), and one represented the control group (i.e. schools that were not eligible for the impulse-area subsidy). Both Verspaandonk (2016) and Van Eijk (2017) found no significant evidence of a positive relationship between the subsidy and student performance. They did, however, find significant evidence of a reduction in class size as a result of the additional funding.

4. Data

The data used for this research is obtained through different channels. Most of the data is publicly available and is retrieved from the website of the Dutch Ministry of Education, Culture and Science (2021). This data contains annual data from the schoolyears 2014-2015 up and until 2018-2019. The data is on two levels: the level of the school institution (one school per schoolboard) and on school establishment level (some schoolboards have different buildings in different postal code areas). The data on school establishment level includes information on the number of end-tests that were taken, the average score of the pupils of that school, the postal code, whether that postal code is an impulse area, the school weight (accumulated weight of all students), and the total number of students. The data on school level contains information on the student-teacher ratio, the average age of the teachers at the schools, and the amount of money the schools received and through which policies this money is assigned to the school. On average, 2% of all the money received by the schools came from the impulse-area policy. For all schools that received money through the impulse-area policy, this was around 6.8% of their annual funding.

The non-public data was made available through Marijn Verspaandonk and Joris van Eijk, both Erasmus University alumni. This data provides information on the level of the four-digit postal areas in 2004. It contains information on the percentage of low-income households in this area and the percentage of households receiving unemployment benefits in this year. This is the information on which the Armoedemonitor of 2005 was based; the report used to label the postal code areas as impulse-areas or not.

As there are not one but two requirements for a postal code to be labelled an impulse-area, there exist four groups of schools. These groups are visualised in figure 1. The impulse-areas were determent based upon data from 2004. If either one of the following requirements is met, the postal code area in 2004 the area was labelled an impulse area:

- Within the area over 11.3% of the households receive unemployment benefits.

- More than 11.5% of the households have a low income. Low income is defined in the *Armoedemonitor* as an annual income between €10,200 and €19,300 depending on the family situation (amount of children, age of the parents, amount of parents).

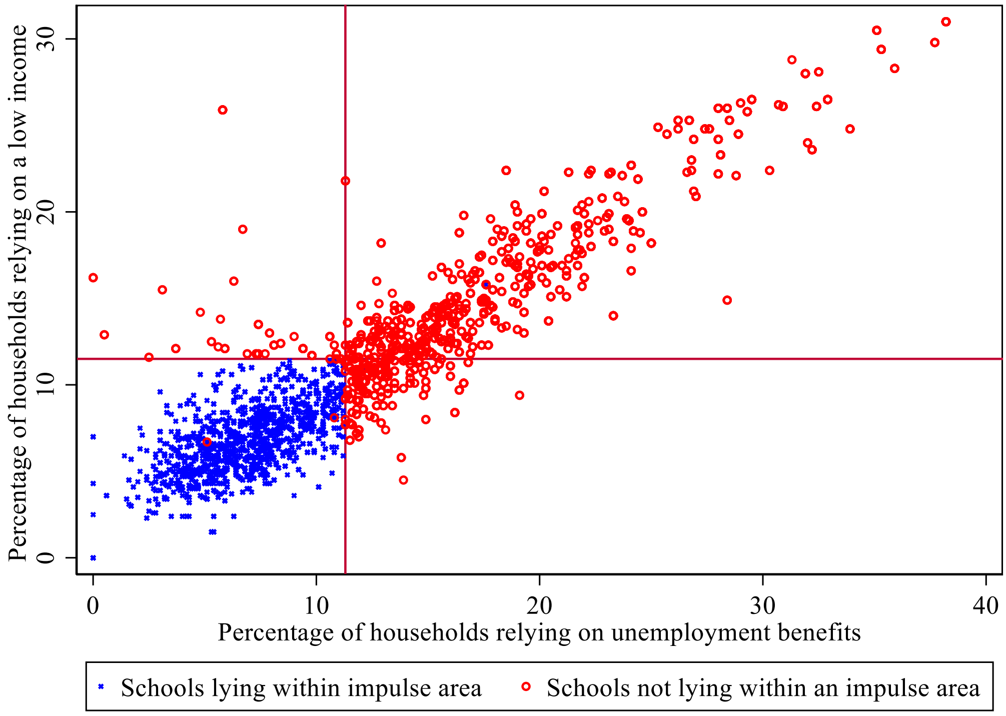

Figure 4.1: All schools in the different schoolyears (2014 up and until 2019). The schools within an impulse area are depicted by a red circle (i.e. the treatment group), the schools outside the impulse area are depicted by blue X's (i.e. the control group). The red lines represent the two requirements that label a postal code area as an impulse area or not. Two red circles are found in the bottom-left quadrant, these schools were not included in the regression as it is unclear why they received the funding but do not meet one of the requirements.

Table 4.1: Number of observations per quadrant per schoolyear

	2014-2015	2015-2016	2016-2017	2017-2018	2018-2019
Bottom-left (regular area)	4,190	4,556	4,47	4,310	4,157
Top-left (impulse area)	124	141	137	123	125
Top-right (impulse area)	876	978	949	932	872
Bottom-right (impulse area)	329	366	359	346	333

Figure 4.1 shows all schools in the different schoolyears. The x-axis represents the percentage of households in the postal area of the given school receiving unemployment benefits. The y-axis showcases this feature for the percentage of households relying on a low income. The

vertical line represents the threshold point at 11.3% of households that receive unemployment benefits, and the horizontal line represents the threshold point at the 11.5% of households that receive a low income. If the school lies either above the horizontal line or on the right side of the vertical line, the school is located in an impulse area and depicted by a red circle. Whether the schools actually receive the subsidy depends on whether they have a non-zero school weight. The group of schools for which this hold, will be referred to as the treatment group.

The schools in the bottom-left square are the schools that are not located within an impulse area. These are depicted by a blue dot. This group will be the control group. As can be seen in figure 4.1 schools are located in an area that do not meet one or both of the requirements, but still received the impulse subsidy. It is unclear why this is the case, and these observations were not included in the analysis.

The crosses and circles clearly form a diagonal line from the bottom-left part of the figure to the top-right part. This can be explained by households depending on unemployment benefits and households depending on a low income often clump together in deprived neighbourhoods.

As a measurement for student performance, the scores of three different tests are used. Schools are free to choose which of these tests they hand out to their students. The three tests all apply a different scale for grading. The scores are therefore standardized to have a mean of zero and a standard deviation of one in order to use the outcomes from the three tests together as this increases our dataset and annihilates the possibility of schools with similar characteristics selecting into certain tests. Special cases were not included, such as schools with very small classes (less than 5 pupils). This data is not made available for privacy reasons. Moreover, very small classes are not informative for the research, as they would experience different treatment than a group of pupils in an average class size, approximately 25 pupils per class in the Netherlands.

Table 4.2: Descriptive statistics on all four quadrants and the complete data set.

	Normalized test score	School weight	Number of students	Student-teacher ratio	Impulse funding	Percentage funding from impulse
Bottom-left						
Mean	.1426067	2.327814	226.0396	19.23056	255.9487	.0001255
Standard deviation	.917988	8.345317	138.8172	3.3122	6566.686	.0026432
Minimum	-5.112792	0	9	.6	0	0
Maximum	3.403528	236	1747	76.2	402320	.12
Top-left						
Mean	.1239952	2.261538	152.9246	17.36108	20580.73	.0254
Standard deviation	1.054208	7.382311	132.2978	3.93795	28252.13	.0251451
Minimum	-3.591305	0	27	7.1	0	0
Maximum	3.053483	54	726	39.6	186608	.12
Top-right						
Mean	-.5816956	31.86303	248.4845	16.93127	105008.4	.077404
Standard deviation	1.115252	42.54271	133.4523	3.468008	95192.74	.0453001
Minimum	-5.467812	0	13	1.4	0	0
Maximum	2.652984	405	1258	40.2	769000	.19
Bottom-right						
Mean	-.2528381	11.31564	216.7046	18.19667	57483.35	.0528679
Standard deviation	1.012505	20.92189	123.3236	3.314937	60234.66	.0373304
Minimum	-4.859211	0	13	6.8	0	0
Maximum	3.032272	223	802	39.7	583792	.17
Complete data set						
Mean	.0011928	7.613267	227.1602	18.74696	21122.07	.0164832
Standard deviation	.999193	22.04727	137.5462	3.469623	57052.23	.0363159
Minimum	-5.467812	0	9	.6	0	0
Maximum	3.403528	405	1747	76.2	769000	.19

Note: These descriptive statistics are derived from data of all schools from the schoolyears 2014-2015 up and util the schoolyear 2018-2019. The student-teacher ratio is the total amount of students at a particular school divided by the total amount of full-time teachers (which is the total amount of hours worked at a school by the teachers divided by 40). Impulse funding is the total amount of funding the schools received that year through the impulse subsidy in euros.

5. Methodology

In order to analyse the effect of the impulse subsidy on the student performance a sharp RDD will be applied. This method was firstly introduced by Angrist and Pischke in 2008. It is a local estimator and compares the schools just above and just below a certain threshold. This specific case is called a sharp RDD considering the reliance on a deterministic and discontinuous jump in treatment that occurs when an observable variable crosses one of the two thresholds.

When applying this method, we assume that the schools around the two thresholds are identical in all characteristics, except for the additional funding they do or do not receive. The research performed by the Netherlands Bureau of Economic Policy Analysis (2017) indicates this to be the case. The research shows that schools located in areas around the unemployment benefit threshold are not significantly different for a bandwidth of 3 percentage point on both sides. For the unemployment threshold this would mean a bandwidth between 8.3% and 14.3%.

Another key identification assumption is that schools have imperfect control over whether their school receives or does not receive the impulse subsidy. This could occur if schools would decide to move from a non-impulse area to an impulse area or for new schools to settle in an impulse area postal code. This is unlikely to be true, however, because the list of impulse area postal codes could be changed every four years. Although in practice a change in postal code selection never occurred, the possibility was present, repressing the incentive for schools to move to an impulse area. Nonetheless, a McCrary Test (McCrary, 2008) is performed in order to discard such occurrences. Figure 5.1 and figure 5.2 suggest that the number of schools varies smoothly across the two thresholds. Table 5.1 describes the formal outcomes of these figures. The null hypothesis that there is a significant change in density around the threshold can be rejected for the unemployment benefit threshold. There is, however, a significant difference in density around the low-income threshold at a 5% significance level. Meaning that the number of schools just below the low-income threshold is higher than just above this threshold. This could be the result of schools being more financially stable from receiving the subsidy, thus being more likely to survive financial setbacks. This, however, remains speculation. Nonetheless, these results would suggest that no problematic self-selection bias occurred legitimizing the appliance of RDD.

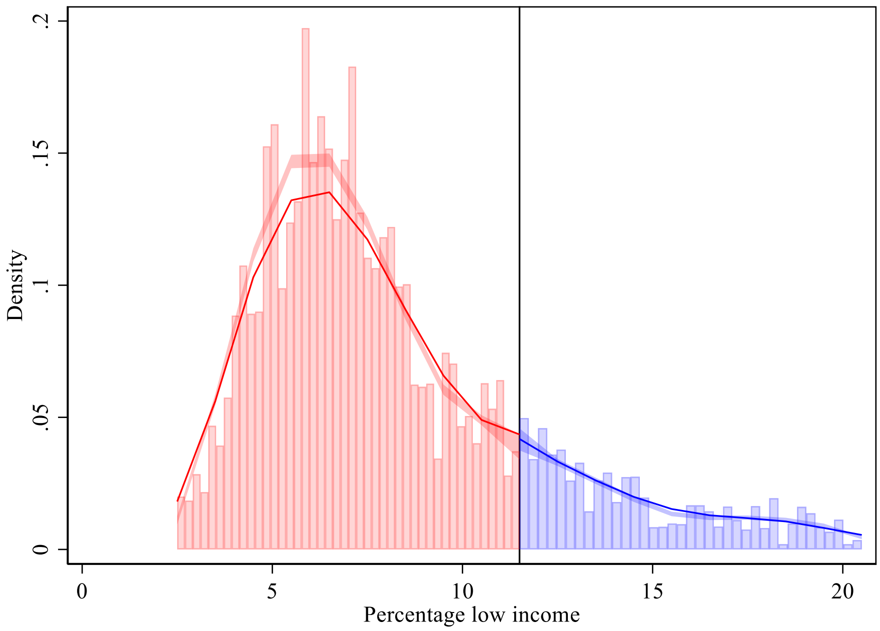

Figure 5.1: McCrary density test for manipulation around the threshold of 11.5% of households receiving a low income.

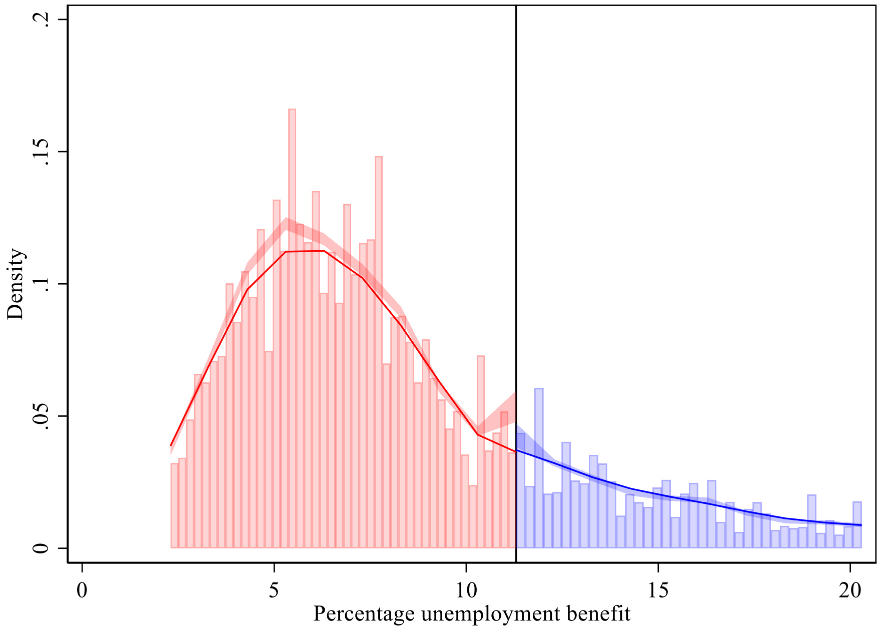

Figure 5.2: McCrary density test for manipulation around the threshold of 11.3% of households receiving unemployment benefits.

Table 5.1: Formal manipulation test.

VARIABLES	T	P > |T|
Percentage low income	1.1836	0.2366
Percentage unemployment benefits	-2.8604	0.0042

When applying RDD, choosing the optimal bandwidth is crucial. A larger bandwidth increases the variance but decreases the bias, while a lower bandwidth decreases the variance and increases the bias. On the one hand, data as close to the thresholds as possible is preferred, as these observations are each other's best comparisons. On the other hand, a smaller bandwidth means less data, hence, less accuracy. In other words, choosing the bandwidth comes with a bias-variance trade-off. To find the optimal bandwidth to apply in the RDD, cross-validation is used. Cross-validation is a technique that assesses how the results of a statistical analysis will generalize to an independent data set. It generates the best possible bandwidth regarding both sides of the threshold.

What is unique in this particular RD design is that there is not one but two thresholds that separate the treatment group from the control group. These two covariates are the percentage of households within the postal code receiving unemployment benefits, *U*, and the percentage of households within the postal code with a low income, *I*. Looking back at figure 4.1 it is important that we only compare the treatment and control group around the thresholds up and until the point where the thresholds intersect. Solely the schools around the vertical threshold that lie beneath the horizontal threshold, and the schools around the horizontal threshold to the left of the vertical threshold will be included into the analysis (see figure 5.3). If the schools lying in the top-right part of figure 5.3 would be included, the treatment group would be compared with a control group that consists of schools partly receiving (top-left or bottom-right quadrant) and partly not receiving the subsidy (bottom-left quadrant). This would bias the results. Moreover, we want to compare the schools that actually received the impulse subsidy to the schools that would have received the impulse subsidy if they would be located within an impulse area. This means that another requirement for the schools to be included into the analysis is for them to have a non-zero school weight. The dummy variable that separates the treatment group from the control group can be defined as:

$$D_i = \begin{cases} 1, & if\ U \geq 11.3\%\ and\ I < 11.5\% \\ 1, & if\ I \geq 11.5\%\ and\ U < 11.3\% \\ 0, & if\ U < 11.3\ and\ I < 11.5 \end{cases} \tag{5.1}$$

where D_i = 1 for a school *i* not lying in an impulse are, and D_i = 0 for a school *i* lying in an impulse area. Schools with U>11.3% and I>11.5% are excluded from the regression. The following equation will be applied in order to assess the effect of the impulse subsidy:

$$Y_i = \beta_0 + \beta_{rd} D_i + \beta_1 X_{i1} + \beta_2 D_i X_{i1} + e_i$$

The outcome variable, Y_i, is the outcome variable of interest. Firstly, the outcome variable will be the amount of funding received. This is done to define the extent of the impact of the *Gewichtenregeling* on the funding of schools. Secondly, the student-teacher ratio will be used for Y_i. . Lastly, the standardized average test score will be the outcome variable. X_i is a vector of the control variables. The control variables consist of the school weight and the denomination of the school.

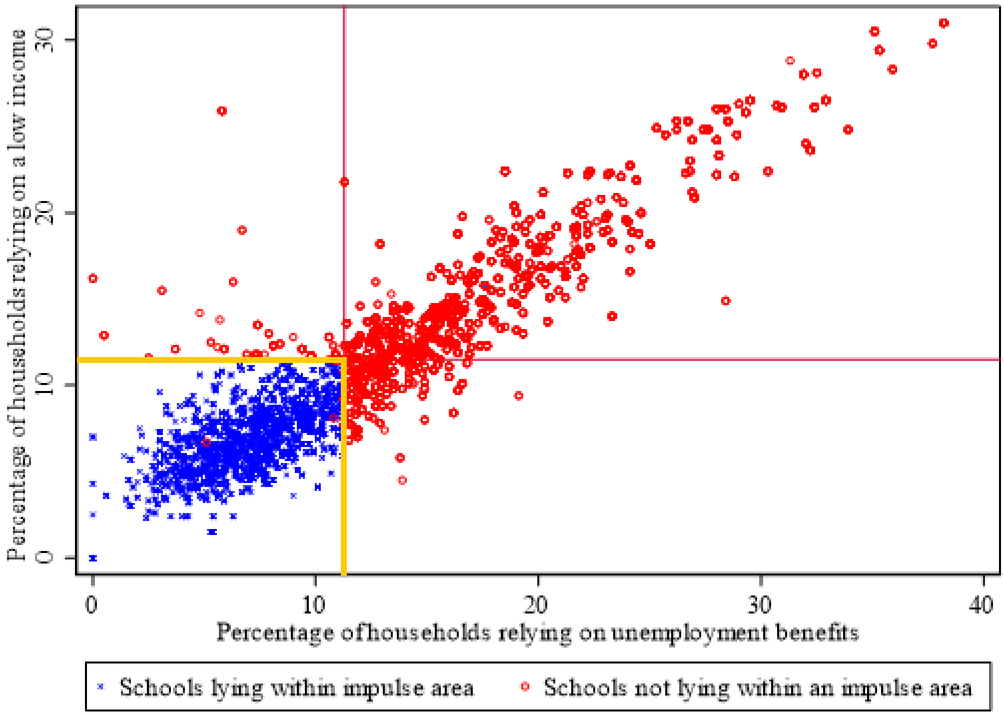

Figure 5.3: Copy of figure 4.1. Data around the yellow lines are included in the RDD analysis.

6. Results

The results are divided into three sections. In the first section the effect of the impulse policy on the funding of schools will be discussed. In the second section the effect of the impulse subsidy on the student-teacher ratio will be discussed. And lastly, the effect of the subsidy on student performance will be presented. These three models all include results from data around the low-income threshold and the unemployment benefit threshold. For all models the bandwidth is chosen by applying the cross-validation method. In table 6.1 the optimal bandwidths are presented. In column 1 the optimal bandwidth (distance to the left and right of the threshold) for the low-income threshold is presented. In column 2 the optimal bandwidth is presented for the unemployment benefit threshold. These bandwidths are applied in the models in section 6.1, 6.2, and 6.3.

Table 6.1: Optimal bandwidths according to cross-validation. Column 1 indicates the optimal bandwidths for the low-income threshold is presented, in column 2 this is presented for the unemployment benefit threshold.

	(1)	(2)
Funding per student	2.324	2.353
Student-teacher ratio	4.201	1.662
Test score	2.755	1.304

6.1 Funding

There is a direct link between a school being located within the boundaries of an impulse area to receiving additional funding from the government. The scope of this additional funding relative to a 'standard' situation, however, is crucial for our final conclusions. If the differences between the treatment and control group are not sufficient, an effect on student performance will be difficult to observe. The results are presented in table 6.2. As schools lie above the low-income threshold, they on average receive €130.48 per student more in funding. This increases the total funding with approximately 2.9%. If schools lie to the right of the unemployment benefit threshold, they receive on average €221.4 per student more in funding. This increases total funding with approximately 5.2%. All outcomes are significant at the one-percent level.

Table 6.2: In the first two columns the outcomes for the unemployment benefit threshold are presented, in the last two columns the outcomes for the low-income threshold are presented. In the uneven numbered columns, the effect of subsidy on funding per student are shown (in euro's) and in the even numbered columns the increase of funding as coming from the subsidy is presented (x100%). Robust standard errors in parentheses. *,** and *** indicates significance at 0.1, 0.05 and 0.01 respectively.

	(1)	(2)	(3)	(4)
Impulse area	130.48***	.02933***	221.4***	.05187***
	(9.0986)	(.00149)	(23.647)	(.00216)
Number of observations	22411	22411	23413	23413

6.2 Student-teacher ratio

The effects of the impulse subsidy on the student-teacher ratio are presented in table 6.3. If the schools lie above the low-income threshold, the number of students per teacher decrease with 0.6466. If the school is located to the right of the unemployment benefit threshold, the number of students per teacher decreases with 0.56137. Hence, there are more teachers available per child as a result of the impulse subsidy.

Table 6.3: Effect of impulse subsidy on student-teacher ratio. The difference is presented as the change in number of students as a fraction of amount of full-time teachers (40 FTEs). Column 1 represents data from the low-income threshold, column 2 represents the unemployment benefit threshold. Robust standard errors in parentheses. *,** and *** indicates significance at 0.1, 0.05 and 0.01 respectively.

	(1)	(2)
Impulse area	-.6466***	-.56137**
	(.24029)	(.27359)
Number of observations	21549	22516

6.3 Student performance

In table 6.4, the results of the impulse subsidy on standardized test scores are shown. The result for the low-income threshold is not significant. The outcome for the unemployment benefit, however, is significant at the 5-percent level. If the schools lie to the right of the unemployment benefit threshold instead of to the left, the average standardized test scores decrease with 0.18562 percentage point. Hence, the impulse subsidy does not increase student performance on the tests. The schools within impulse areas did not show an observable increase in student performance, but rather a decrease.

Table 6.4: Effect of impulse subsidy on student performance measured in standardized average test scores per school. Column 1 represents data from the low-income threshold, column 2 represents the unemployment benefit threshold. Robust standard errors in parentheses. *,** and *** indicates significance at 0.1, 0.05 and 0.01 respectively.

	(1)	(2)
Impulse area	-.024	-.18562**
	(.08564)	(.08768)
Number of observations	22411	23413

In table 6.5, a simple OLS regression is performed. The first column presents the effect of the percentage of households depending on a low-income within the postal code area on standardized test scores. The second column presents the effect of the percentage of households depending on an unemployment benefit on standardized test scores. As can be seen in the table both have a negative effect on standardized test scores with a significance at 0.01.

Table 6.5: The average effect of the impulse subsidy on student performance measured in standardized test scores. Column 1 represents data from the low-income threshold, column 2 represents the unemployment benefit threshold. Robust standard errors in parentheses. *,** and *** indicates significance at 0.1, 0.05 and 0.01 respectively.

	(1)	(2)
Impulse area	-.406049***	-.219794***
	(.0057871)	(.0047037)
Number of observations	20241	20241

7. Conclusion (and Discussion)

In this paper, the *Gewichtenregeling*, a Dutch subsidy that is targeted at disadvantaged students from primary schools, is evaluated. This is done by looking into the effects of the Impulse subsidy, which is a separate subsidy additional to the *Gewichtenregeling*. The research question was: *What is the effect of the school weight subsidy in primary education on student performance?* The specific design of the Impulse subsidy, the scope of the additional funding being dependent on the school weight and the strict requirements that come with the impulse subsidy, allowed to evaluate the effect of additional funding on student performance through an RDD. Data provided by governmental institutions from the schoolyears 2014-2015 up and until 2018-2019 was used. The method was altered to fit the unique case of not one but two thresholds. The usual RDD method compares observations close and to the left side of threshold to observations close to the threshold but to the right of it. The method applied in this research compares observations close to but on the left of threshold *a* to observations close to and on the right of threshold *a*, but does not include observations that lie above threshold *b*, and vice versa.

Doing so, we find significant evidence for an effect on the student-teacher ratio. For the low-income threshold a decrease of students per teachers of 0.6466 is found with a significance level of 1%. For the unemployment benefit threshold, a decrease of students per teachers of 0.56137 is found with a significance level of 5%. Hence, there are more teachers available per child as a result of the impulse subsidy.

A significant effect of the Impulse subsidy on student performance measured in standardized central end tests is found for the unemployment benefit threshold, but not for the low-income threshold. The measured effect of the impulse subsidy around the unemployment benefit threshold is a negative effect of 0.219794 of the standardized test scores with a significance level of 5%. This negative effect is counterintuitive, as the results would suggest that the subsidy works in opposite directions as the intentioned effect. However, the measured effect is in line with previous research on additional funding in primary schools in the Netherlands by Leuven et al. (2007), CPB (2017), Verspaandonk (2016), and Van Eijk (2017).

This negative impact is difficult to interpret as there is an overall negative effect of the number of households in a postal code area depending on unemployment benefits or a low-income on student performance as is shown in table 6.5. The assumption that schools on the left of the thresholds are similar to schools to right of them are similar would need more profound research to annihilate the possibility that the impact of additional funding is outweighed by the effect of ‘bad neighbourhoods’ on student performance.

Additionally, caution is needed when interpreting this result as effects measured by RDD do not always apply to the full extend of the schools. The effect is locally estimated, giving results on schools around the two thresholds. The effect of additional funding on schools in neighbourhoods where there are little to no households depending on a low-income or unemployment benefit can not be concluded from these locally estimated effects. The measured effects can, however, be interpreted as effect of the *Gewichtenregeling*, as is also done by the CPB (2017), up to a certain point. Again, it does not give any insights on schools with no to little households depending on a low-income or unemployment benefits in their postal code area.

Another notice must be made on the low percentage increase the impulse subsidy has on the budgets of the schools. Around the low-income threshold, the schools' budgets on average increased by 2.933% and 5.187% around the unemployment benefit threshold. Further research would be needed whether these low percentages are substantial in order to say anything about its impact. Furthermore, it would be insightful to look at other factors that could define student performance, such as incidences of adult poverty, as is done by Jackson et al. (2015), or impact on average income later on in the lives of the students.

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