The effect of six year subsidizing primary schools in poor neighborhoods

-Using the neighborhood characteristics as running variables instead of bias-

Marijn J. Verspaandonk

Abstract

This paper investigates the effect of 6 years of subsidy provided to primary schools located in so-called impulse areas. The strict criteria for impulse areas created an ideal setting for a regression discontinuity design. The paper finds evidence that the subsidy increased the funding per student and reduced the class size of schools located in impulse areas. However, the empirical analysis provides no evidence that the subsidy improved the performance of students in their final year of primary school on a nationwide exam.

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Introduction

Education policy in the Netherlands and elsewhere in the world is often regarded as the most important policy to combat inequality within a country. For education to be an effective tool to fight inequality it should at least be characterized by a level playing field. This means that although the chances of pupils to succeed might differ, the rules that apply to all pupils should be the same. To increase the equality stemming from education even further, Dutch policy effectively aims at increasing the chances of students with a disadvantaged background. The most recent report of the Dutch Ministry of Education, Culture and Science evaluating the state of Dutch education raises concerns (Inspectorate of Education, 2016). Firstly it shows that primary schools tend to send a pupil with wealthy parents to a higher form of secondary education than they would send a pupil from poor parents to, even though these pupils have the same score on a centralized exam. This violates the level playing field condition in which the same rules should apply to all pupils. Secondly, the report shows that over time the performance gap in primary education between pupils from low- and high-educated parents, is still present and even slightly increased in 2014. As mentioned earlier, the Dutch Government has a history of trying to improve the educational performances of students with a less favored background. The main primary education funding scheme for this purpose assigns different weights to students based on the educational attainment of their parents. Students whose parents at most finished primary education are assigned a weight of 1.2. While students whose parents followed at most two years of secondary education are assigned a weight of 0.3. Effectively this means that a school with just students of weight 1.2 will be assigned 120% more funding compared to a school with only students with weight 0, students with relatively well-educated parents. The total amount of funding based on these weights in 2016 amounts to approximately €170 million, while the total spending on primary education is almost €6 billion in the Netherlands.

On top of this main funding scheme, the Dutch Ministry of Education introduced a funding scheme in 2009 with the purpose to stimulate students from areas characterized by social economic problems; these areas are referred to as impulse areas. This Impulse Area Subsidy was introduced on the belief that besides the influence of parents, the area where a student grows up influences the educational results of students. More precisely; a student from a poor neighborhood was expected to underperform relative to his peers from better neighborhoods. As a proxy of how bad a neighborhood was they used two indicators deducted from Regional Income Research (RIO, 2005), a yearly research performed by Statistics Netherlands. This research aims at giving an impression of the income distribution across areas in the Netherlands. By doing so it reported the percentage of low income households and the percentage of households whose main source of income is welfare. The percentages were calculated on a four-digit ZIP Code level, there are 4766 unique four-digit ZIP Codes in the Netherlands. The outcome of RIO was eventually used by the assignment of the Impulse Area Subsidy, by doing so it created a quasi-experimental setting which will be exploited in this paper with a Regression Discontinuity Design (RDD).

The paper is organized as follows; section I gives an overview of the related literature, section II gives background information about the data used and the Impulse Area Subsidy, section III provides explains the research design. In section IV there are performed the necessary validity checks of a RDD design, section V presents and discusses the estimates of the RDD. At last section VI concludes.

I. Literature

This paper evaluates the effect of extra educational funding based on district level characteristics. In particular, the extra funding is used to boost the performance of primary school students living in a bad neighborhood. A study of Guryan (2000) looks at a similar setting, the equalization law in Massachusetts. This reform redistributed funds across districts using information about spending levels and per-capita income (i.e. redistributed funds to bad districts). The study used idiosyncratic variation in state education caused by discontinuities and non-linearities in the state aid formula to find the causal effect of the extra spending. The results of the regression discontinuity design show that the extra funding has a positive significant effect on the test scores of 4th graders in primary education. However, there is no significant effect found for 8th graders. The underlying mechanism for these confounding results might be the cumulative process of education. The students in 8th grade have spent a smaller portion of their education in the well-funded schools.

A study by Rivkin, Hanushek and Kain (2005) disentangles the impact of schools and teachers in influencing achievement with the use of unique panel data from UTD Texas Schools Project. The results suggest that the effect of a costly class-size reduction of ten students is smaller than the benefits of moving one standard deviation up the teacher quality distribution. Moreover, it is mentioned that in particular schools serving largely disadvantaged students, an expansion of the teacher staff to reduce class size might actually harm the quality of the teacher staff. Teachers tend to prefer working at schools with students with a more favored background, therefore the bad schools might have to resort to hiring noncertified teachers. This implies a trade-off between class size reduction and teacher quality.

Back in 1990 Card and Krueger (Card & Krueger, 1990) conducted a broad study of the relationship of school quality, measured by the pupil-teacher ratio, the average term length and the relative pay of teachers, and the rate of return to education. They find that a decrease in the pupil-teacher ratio from 30 to 25 is associated with a 0.4 percentage point increase in the rate of return to education. The relationship is similar for white and black students. Improvements in school quality for black students were mainly driven by political policy, hence they argue that the evidence for blacks reinforce a causal relationship. In 1999 Card reviewed the recent scientific literature on the causal relationship between education and earning (Card, The Causal Effect of Education on Earnings, 1999). One of his main conclusions is that instrumental variable estimates of the return to education based on school interventions tend to be 20% or more above the corresponding OLS estimates. He argues that this is caused by the difference in the marginal return to schooling for different groups. The subgroups that are most affected by the school (policy) interventions used in the IV are mostly disadvantaged students. These groups are assumed to have a higher marginal return to education than the population as a whole.

This international literature suggests that extra funding might indeed be an effective tool to reduce class sizes and therefore boost the performance of students. However, it also suggests that particular poor schools might have difficulties attracting good teachers when they are eligible for extra funding. This is considered important because the quality of the teacher also plays a big role in the performance of students. The subsidy evaluated in this paper is implemented in the Dutch educational system. Therefore besides the international literature, there will be special attention for the Dutch related literature.

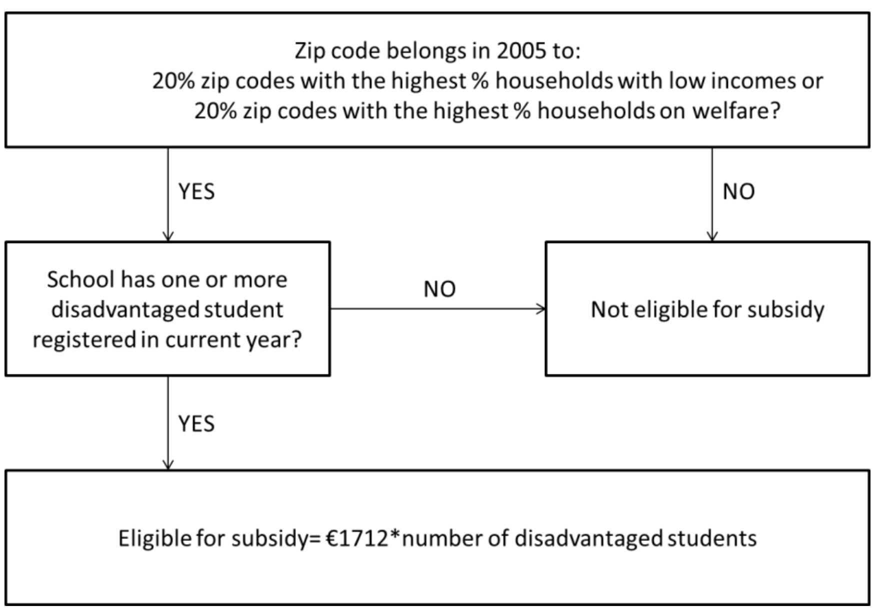
In 2007 Leuven et al. published a paper that evaluated the effect of two subsidies targeted at schools with large proportions of disadvantaged students in the Netherlands (Leuven, Lindahl, Oosterbeek, & Webbink, 2007). The subsidy provided schools above the threshold of 70% disadvantaged students with a subsidy, this cutoff was used in a regression discontinuity design. Surprisingly the study finds negative point estimates of the subsidy. This is evidence that just providing extra funding for disadvantaged schools does not necessarily lead to increased test scores.

II. Subsidy and Data

A. The impulse area subsidy

As outlined earlier, the impulse area subsidy provides additional funding for primary schools located in a poor neighborhood, impulse area, to boost the performance of students that attend these schools. The subsidy was first introduced in the school year 2009-2010. To be eligible for the subsidy there were two criteria. The first one is based on the zip code characteristics of the school. If the zip code belongs to the 20% zip codes with the highest percentage households that have a low income or to the 20% zip codes with the highest percentage households on welfare, a zip code is considered as an impulse area. If a school is located in an impulse area, the amount of the subsidy depends on the number of disadvantaged students registered at the beginning of each year. A student is considered a disadvantaged student if his or her parents are low educated. For each disadvantaged student registered at the beginning of a school year at a school in an impulse area, the school receives a subsidy of €1712. This is on top of the main funding scheme that already compensates schools for the number of disadvantaged students. In 2009 the Ministry of Education first selected the impulse area zip codes. These impulse areas are deducted from Regional Income Research (RIO, 2005), a yearly research performed by Statistics Netherlands. Although the subsidy was first provided in 2009, the assignment of impulse areas was done with data about income levels of 2005. The impulse areas were selected for four years in 2009. After these four years, in 2013, the ministry decided to extend the subsidy for another four years. This was done without reconsidering the impulse areas, meaning that the zip codes that were assigned the status of impulse areas in 2009 remained fixed until 2017. Even though the zip codes are fixed, the height of the subsidy depends on the number of disadvantaged students registered at the school of the beginning of each year.

Figure 1: Assignment criteria for the Impulse Area Subsidy



B. Data

For the sake of this research, there are four main data sets used. The first data reports neighborhood characteristics that are used for the assignment of the subsidy, provided by Statistics Netherlands. The second data set reveals precise information about the yearly funding of each primary school in Netherlands, provided by DUO. The third data set is also provided by DUO and reports the average test results of four different end tests used by the primary schools in the Netherlands. The fourth data set describes the characteristics of each primary school. The data sets are referred to as neighborhood data, funding data, test data and school characteristics data respectively.

The neighborhood data reports neighborhood characteristics on a four-digit zip code level for 2005 since the percentages of 2005 are used for the assignment of the impulse area subsidy. For each zip code in the Netherlands, it reports the percentage of households with a low income, the percentage of households whose main source of income is welfare and the percentage of non-western immigrants living in the zip code. Additionally, it provides accumulation codes; these define if a zip code belongs to the top 20% of one of the percentages (i.e. to the 20% zip codes with the highest percentage low income households).

The funding data reports for each primary school in the Netherlands the amount of money they receive for their personnel. It reports detailed information about where the funding is based upon, such as the number of disadvantaged students that are attached a different weight when it comes to funding. Moreover, it contains the number of disadvantaged students registered at a school in an impulse area, which is highly relevant since the height of the impulse area subsidy depends upon this number.

The test data reports the average score of primary schools on one of the four available end tests. Schools are allowed to choose between the four different end tests for their students in 8^{th} grade, this is the last grade of primary school in the Netherlands. However, more than 80% of the schools uses the Cito test. To compare the scores of the schools that use the Cito test with the schools that use another end test, all of the scores will be normalized. The tests are normalized to a mean value of 0 and a standard deviation of 1. To control for possible self-selection into the different end test there will be added a dummy indicating the type of test a school used. The result of these test play an important role in the transition to high school, therefore they are considered as high-stake tests.

The school characteristics data provides detailed information about the denomination of each primary school. It reports the number of students enrolled at each school as well as information about the type of school. This information will be used as control variables in the analysis. Moreover, it reports the number of full-time employees (teachers) at each school. This data will be used to calculate class sizes.

III. Research Design

A. Regression discontinuity

The strict criteria for impulse areas created an ideal setting for a regression discontinuity design to investigate the effect of the subsidy. In a typical RD with a treatment D, an individual is assigned to the treatment or control group, depending on a single running variable S crossing a cutoff or not. There are, however, many cases where multiple running variables are involved in determining a single treatment (Choi & Lee, 2014). The current paper is an example of such a case. The two running variables, the percentage of households with a low income and the percentage of households on welfare, determine a single treatment, the impulse area subsidy. The basic intuition behind the regression discontinuity design is that the schools just below the cutoff are similar to the schools just above the cutoff. The cutoff, therefore, creates a natural control and treatment group.

B. Assignment variable

From the eligibility rule outlined in the subsidy section, there can be deducted two assignment criteria. The first assignment criterium is the percentage of low incomes and households on welfare in the zip code of the school. This implies that for schools below the cut-off values for low incomes and welfare, 11.5% and 11.3% respectively, the probability of receiving treatment is zero. Above these cutoff values the probability increases. However, not all schools that are above one of these cutoffs received a subsidy. This is the results of the second assignment criterium; the number of disadvantaged students registered. The height of the subsidy depends on the number of disadvantaged students. Therefore there are schools that are above the cutoff values and didn't receive a subsidy, these schools are referred to as 'no shows'. Shadish, Cook, and Campbell (2002) came up with two solutions to such overrides of the cutoff. The first solution is to retain the no show observations and classify them according to their eligibility status. Their second solution is to delete no show observations from the analysis. In the current analysis both solutions of Shadish et al. (2002) are used. This means that in the first strategy all schools that are above the cutoffs are treated as they received treatment. While in the second strategy the no shows are deleted from the sample. The first strategy gives an unbiased estimate of the effect of assignment to treatment rather than of the treatment itself.

For both strategies, the dummy indicating treatment is defined in the following way. Denote the percentage of low income households in zip code z of school j in 2005 by i_{zj}^{05}. Likewise the percentage of welfare households is denoted by w_{zj}^{05}. This leads to the following specification of the treatment dummy, d_{zj}^{05}.

$$d_z^{05} = \begin{cases} 1 \; if \; i_z^{05} \geq 11.5 \\ 1 \; if \; w_z^{05} \geq 11.3 \\ 0 \; if \; otherwise \end{cases}$$

In figure 2 this assignment criteria corresponds to all points above the y-line of 11.5 and or right to the x-line of 11.3. As outcome variable the average test score of a school is used. Such that the outcome can be written as:

$$E[y_j] = \alpha + \delta d_z^{05}$$

Where $\alpha \equiv E[y_{0j}]$ is the average test score without the impulse area subsidy and $\delta \equiv E[y_{1j}] - E[y_{0j}]$ is the change in test scores due to the subsidy. However not all schools with $d_z^{05} = 1$ actually received the treatment. Therefore it is more accurate to define δ as the change in test score due to the eligibility of receiving treatment.

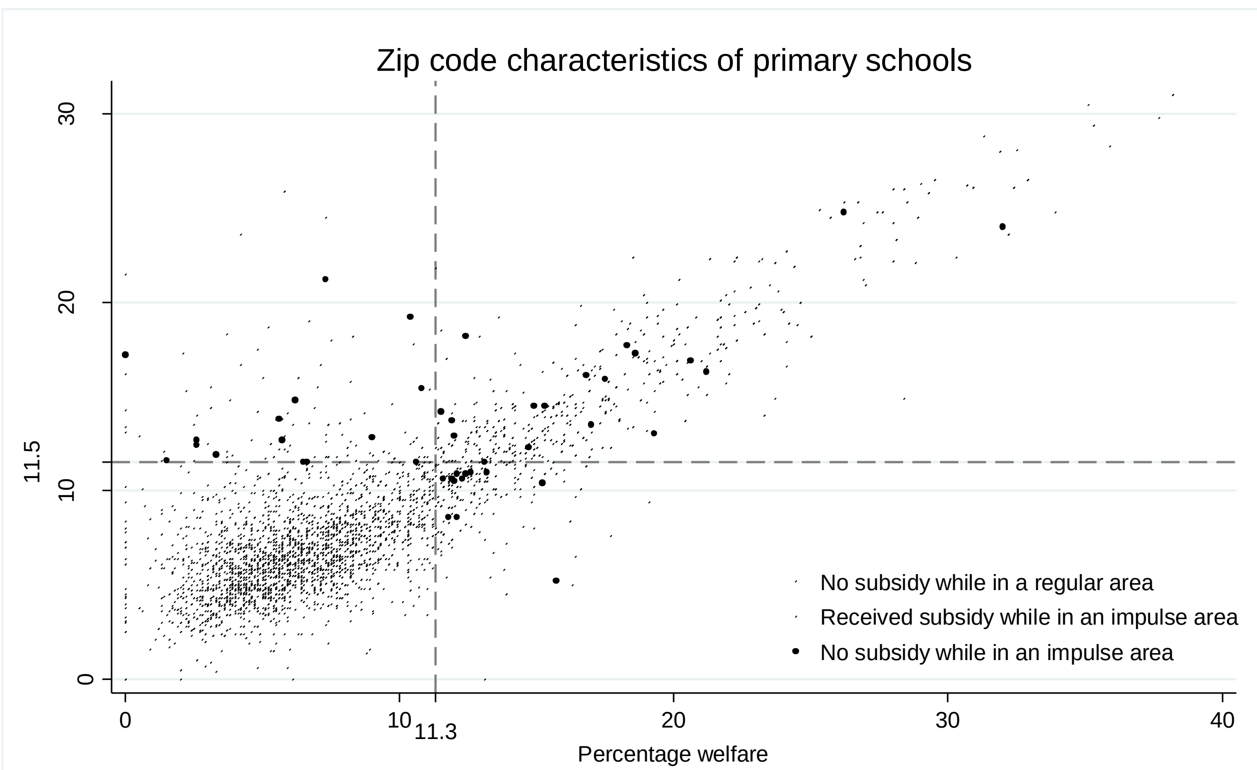
The second strategy uses the same treatment dummy as the first strategy. However the key difference between the two strategies is that in this strategy schools that don't have any disadvantaged students registered are restricted from the sample. This naturally deletes all the no show observations as well as the schools that are below the cutoff and don't have any disadvantaged student. This keeps the control and treatment group balanced, which is the most important feature of the RDD design. The suggested strategy of Shadish et al. (2002) would solely delete the no show observations from the treatment group and leave the control group untouched. In the used strategy the outcome can be written the same as in the first strategy:

$$E[y_j] = \alpha + \delta d_z^{05}$$

Where $\alpha \equiv E[y_{0j}]$ is the average test score without the impulse area subsidy and $\delta \equiv E[y_{1j}] - E[y_{0j}]$ is the change in test scores due to the subsidy. In this strategy all schools with $d_z^{05} = 1$ actual received treatment. Such that δ can be defined as the treatment effect for schools that have one or more disadvantaged students.

The difference between the first and second strategy can be best displayed graphically. In the first strategy all schools that meet the criteria of an impulse area are treated as they received treatment. In figure 2[1] this corresponds to all observations above the y-line of 11.5 and or right to the x-line of 11.3. In the second strategy all bold points that indicate no show observations are deleted as well as their counterparts[2] under the y-line of 11.5 and left to the x-line of 11.3. As such there are in the treatment and control group only schools that have one or more disadvantaged students registered.

Figure 2: Treatment status of schools plotted against the two running variables; bold points indicate 'no shows'



[1] See Appendix A for a larger format of figure 2
[2] Schools with zero disadvantaged students

In order to implement the regression discontinuity design, assignment to treatment must vary discontinuously at the cutoff point. Figure 3 presents the probability of receiving the subsidy conditional on the zip code characteristics of the school. There are plotted 30 equally sized bins, with a quadratic regression line to fit the underlying data of the bins. The x-axis represents the distance from the cutoff value of the neighborhood characteristic that is most likely to influence treatment, r_z^{05} (i.e. the running variable). This value is calculated in the following way:

$$r_z^{05} = \max[(i_z^{05} - 11.5)(w_z^{05} - 11.3)]$$

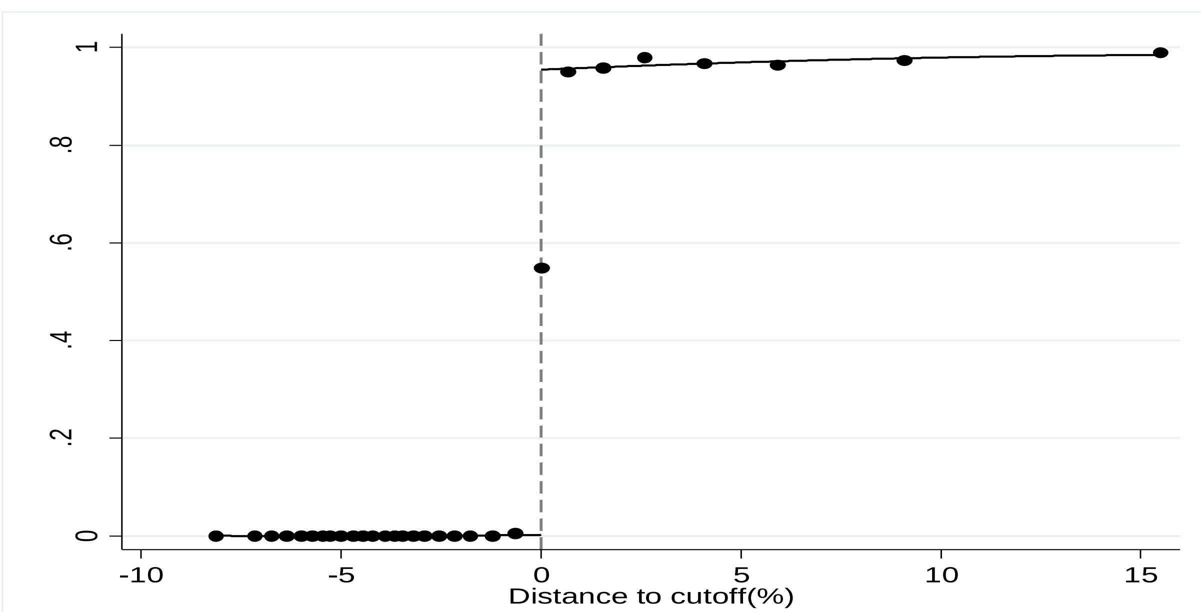
Where i_z^{05} and w_z^{05} represent the percentage of low income households and welfare households respectively. The values 11.5 and 11.3 represent the percentages that define the cutoff values of i_z^{05} and w_z^{05} . This combination of the two running variables will be referred to as the 'Distance to cutoff' throughout the paper.

Figure 3 clearly demonstrates that the probability of receiving funding increases sharply at the cutoff value of treatment. For schools that are located in a regular area, zip code that has less than 11.5% of households with a low income and less than 11.3% of households on welfare, the probability of receiving a subsidy is zero. While for schools located in an impulse area the probability is close to 1. Table 2 reports the estimate of the discontinuity that is calculated by the following equation:

$$(1)\ Treatment = \alpha + \delta d_z^{05} + f(i_z^{05}) + f(w_z^{05}) + \varepsilon$$

The impulse area dummy, d_z^{05}, indicates the discontinuity of the probability of receiving the subsidy at the cutoff value. The variables $f(i_z^{05})$ and $f(w_z^{05})$ are polynomial expansions of the running variables, low income households and welfare households percentages respectively. The results indicate that the running variable increases the probability of receiving the subsidy with more than 95%. This finding is robust to different polynomial expansion of the running variable.

Figure 3: Probability of receiving funding relative to the distance to cutoff



Note: There are 30 equal sized bins containing the underlying 5576 school observations. The density of school with respect to the distance to cutoff is normally distributed , the cutoff is in the right tail of the normal distribution, see figure 4-6. Therefore there are fewer bins right to the cutoff. Bins do not strictly contain data point on one side of the cutoff.

Table 1: Estimated discontinuity in probability of receiving the Impulse Area Subsidy

	(1) Linear	(2) Quadratic	(3) Cubic
Impulse area	0.952***	0.952***	0.949***
(i_{zj}^{05}>11.5 or w_{zj}^{05}>11.3)	(0.004)	(0.005)	(0.005)
Low Income	-0.002***	-0.001	0.007**
	(0.001)	(0.001)	(0.003)
Welfare	0.003***	0.002**	-0.005**
	(0.000)	(0.001)	(0.002)
Low Income^2		-0.000	-0.001***
		(0.000)	(0.000)
Welfare^2		0.000	0.001***
		(0.000)	(0.000)
Low Income^3			0.000***
			(0.000)
Welfare^3			-0.000***
			(0.000)
Observations	5,576	5,576	5,576
R-squared	0.957	0.957	0.957

Note: Discontinuity in the probability of receiving the impulse area subsidy estimated with equation (1). Standard errors in parentheses. Significance indicated by; *** p<0.01, ** p<0.05, * p<0.1

C. Estimation

To investigate the effect of the extra funding due to the assignment to the subsidy, a RD design will be used. The following equation will be used in the regression discontinuity design to assess the effect of the subsidy:

$$(2)\quad y_j^{14} = \alpha + \delta d_z^{05} + f(i_z^{05}) + f(w_z^{05}) + \beta X + \varepsilon$$

In this equation, y_j^{14} is the outcome variable of interest; test score, funding per student and class size, for school j in 2014. The three different outcome variables used will give a clear picture of how the subsidy worked. First it will reveal how much extra funding per student was provided to the schools in impulse areas. Secondly it shows if the extra funding is effectively used to reduce class size. As third and most important step it will show if the subsidy increased the test results of schools located in impulse areas. The variables $f(i_z^{05})$ and $f(w_z^{05})$ are polynomial expansions of the running variables, low income households and welfare households percentages respectively. The variable X is a vector of the control variables such as the percentage of disadvantage students. In the first strategy the coefficient δ in front of the dummy variable indicates the effect of assignment to the subsidy. In the second strategy this coefficient indicates the effect of the subsidy on schools with one or more disadvantaged students.

IV. Validity checks

The regression discontinuity design depends on the assumption that the schools just below the cutoff and the schools just above the cutoff are comparable (i.e. have the same school characteristics) after controlling for the functional form of the running variable. This creates a natural control and treatment group. This assumption might be violated if schools are able to manipulate the running variable, in such a way that there will occur bunching just above the cutoff. In the current framework, this would mean that schools are able to manipulate the relevant zip code characteristics (percentage low income and welfare household) in their zip code. The subsidy was first introduced in 2009 while the government used zip code characteristics of 2005. So besides the difficulty of influencing zip code characteristics, schools also needed to anticipate the subsidy four years before it was actually introduced. There are two strategies to test if schools were able to manipulate the running variable. The first strategy tests if there occurs bunching just above the cutoff. The second strategy tests if the characteristics of schools just below and just above the cutoff do not significantly differ.

A. Density at the cutoff

The most applicable way to test if there occurs bunching just above the cutoff is by using the density test proposed by McCrary (McCrary, 2008). The null hypothesis of this test is that there is no jump in density at the cutoff value. For this test it is necessary to first select the optimal bandwidth. There are a set of calculations possible to do so, appendix D shows the result of the different calculation. The calculations are performed following Calonico et al. (2014). Most estimates suggest a bandwidth of circa 3. Therefore this is used as the preferred bandwidth in the density analysis as well as throughout the rest of the paper. Figure 3-5 plots the different running variables against the density of schools with a bandwidth of 3 and the recommended bin size of McCrary that are reported in table 2. The figures show a clear normal distribution with the cutoff of the three different running variables in the right tail of the distribution. The figures reveal a rather small jump in density just after the cutoffs. While the jump is observed for all three running variables it does not seem like a jump to worry too much about.

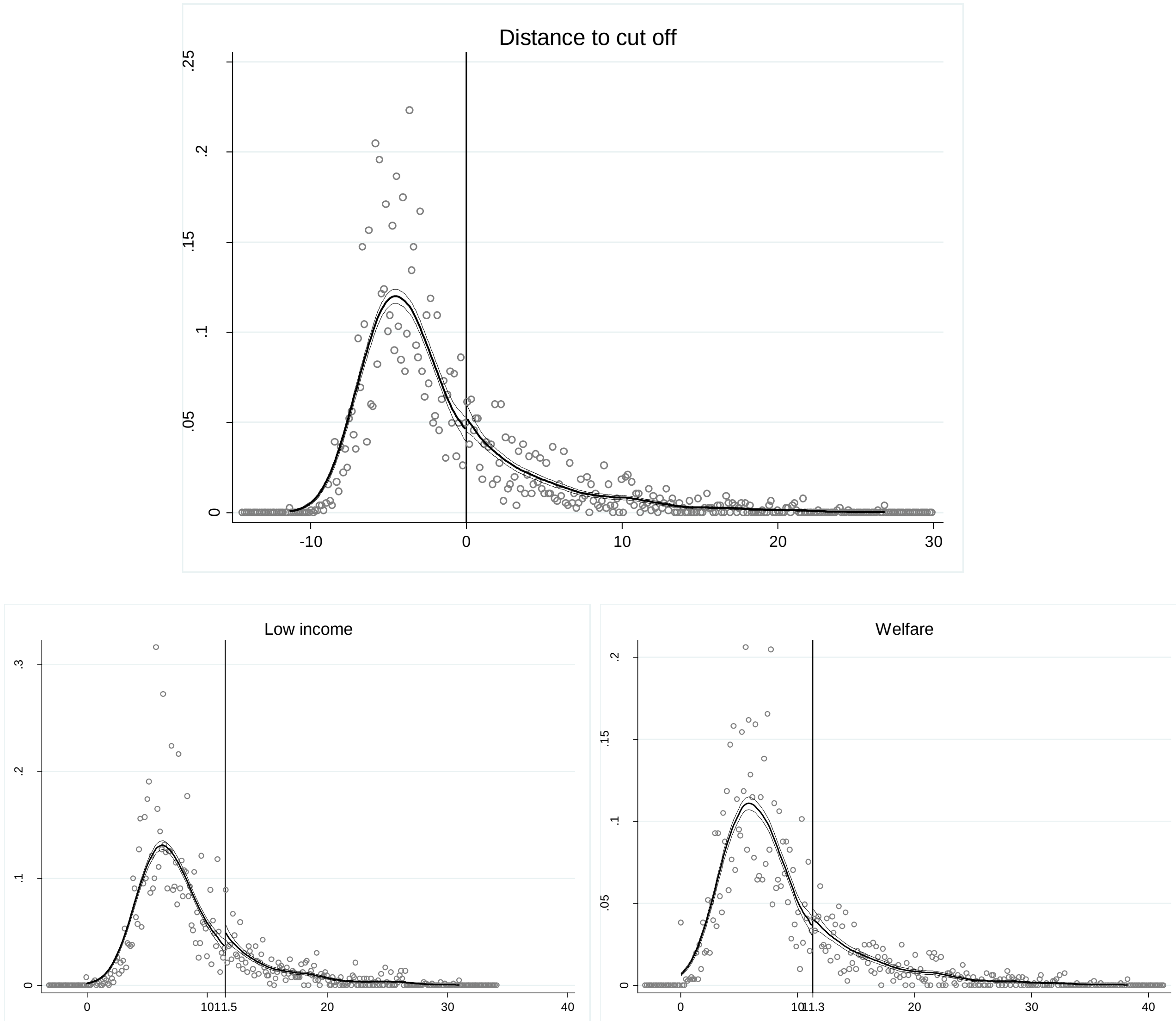
Besides the visual inspection of the density plots, McCrary proposes a formal test to estimate the jump in density at the cutoff. The results of these formal tests are presented in table 2. For these test there is used the recommended bandwidth calculated following Calonico et al. (2014) and the bin size following McCrary (2008). The formal test of McCrary finds evidence of a jump in the density of schools at one of the three running variables; the percentage of welfare households. This means that in 2014 there are significantly more schools located in a zip code that had in 2005 slightly more than 11.3% low income households than there are schools located in a zip code that had slightly less than 11.3% low income households. This might be the result of the fact that the schools just after the cutoff have been receiving more funding due to the subsidy since 2009. Therefore one could argue that these schools had a greater potential to stay in business during the period 2009-2014. Since there is no data at hand to formally test this, it remains speculation.

Table 2: Formal McCrary test estimating the log point estimate of the jump in density at the cutoff value.

Running variable	Bandwidth	Bin Size	Point estimate	St. Error	T-value
Distance to cut off	3	0.137	0.139	0.108	1.290
Income	3	0.119	**0.331**	0.117	2.822
Welfare	3	0.145	0.118	0.123	0.961

Bold point estimates indicate a T-Test value higher than 1.96. These are associated with a significance level of 5%, indicating that there is a significant jump at the cutoff value.

Figure 4-6: McCrary density plot; Distance to Cutoff, Low Income, and Welfare



B. Continuity of school characteristics

For the internal validity of the regression discontinuity design it is crucial that there are no discontinuities in school characteristics. The RDD design depends on the assumption that the characteristics of the control group ($r_z^{05} < 0$) are similar to that of the treatment group ($r_z^{05} > 0$) around the cutoff value. If certain schools would be able to self-select them into the treatment group, this assumption won't hold. Therefor table 3 reports the discontinuities of school characteristics around the cutoff value. These estimates are calculated with equation 3, 4 and 5 for the three running variables.

$$(3)\ \ y_j = \alpha + \delta d_{iz}^{05} + f(i_z^{05}) + \beta_2 f(i_z^{05}) * d_{iz}^{05} + \varepsilon$$
$$(4)\ \ y_j = \alpha + \delta d_{wz}^{05} + f(w_z^{05}) + \beta_2 f(w_z^{05}) * d_{wz}^{05} + \varepsilon$$
$$(5)\ \ y_j = \alpha + \delta d_{rz}^{05} + f(r_z^{05}) + \beta_2 f(r_z^{05}) * d_{rz}^{05} + \varepsilon$$

Where the outcome variable y_j are the school characteristics, such as the percentage of disadvantaged students and the size of the school. All equations control for the functional form of the running variable. Moreover all equation allows the functional form of the running variable to vary around the cutoff by adding an interaction term between treatment status and the running variable. The coefficient that determines if the outcome variable varies discontinuously at the cutoff value is δ. We use three different specifications of the regression. The first exploits the continuity of the school characteristics around the cutoff value of low income households, d_{iz}^{05}. The second does this for the cut off value of households on welfare, d_{wz}^{05}. The third combines these two by using the distance to cutoff d_{rz}^{05}.

Table 3: Continuity of school characteristics

	(1) Low income	(2) Welfare	(3) Distance to cut off
Disadvantaged	-2.078	0.821	0.196
	(1.306)	(1.258)	(0.846)
Exemption	0.089	-0.713*	-0.644**
	(0.423)	(0.429)	(0.294)
School Size	-1,882.469	1,819.060	-1,081.830
	(1,372.456)	(1,471.219)	(1,048.549)
Test: Cito	0.451	1.890	-3.016
	(3.540)	(3.562)	(2.527)
Test: IEP	-2.705	-0.253	-0.893
	(2.064)	(2.000)	(1.482)
Test: Route8	-0.061	2.101	1.861*
	(1.278)	(1.295)	(0.950)
Test: Drempel	2.978	-2.302	3.003
	(2.830)	(2.885)	(1.986)
Observations	1492	1464	1802

Note: The bandwidth used for the different running variables is 3% points left and right to the cutoff of the running variable. Standard errors in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1

For two of the three running variables there is found a significant discontinuity of the percentage of students that were exempted from the end test. The coefficient suggests that schools just above the cut off were more likely to exempt students from the end test. In the estimations to find the causal effect of the subsidy there will be controlled for this. There are no other discontinuities of school characteristics found. In appendix B there are reported estimates of the continuity of the different type of schools. There is no evidence of any discontinuities found there.

V. Results

In this section the estimates of the regression discontinuity model that investigates the effect of the impulse area subsidy will be presented and discussed in the following three stages. The first stage exploits the effect of the subsidy on the funding per student at the school level. In this stage there is expected to find an increase in the funding per student. In stage two there is tested if the increased funding is used to reduce class sizes. The third and ultimately most important stage determines the effect of the subsidy on the average end test results of schools. This is widely used as the most important performance measure of primary schools. Each stage consists of 5 models that differ in their specifications. The first three models are estimated with equation (2), the first strategy that uses the dummy indicating the treatment status of a school (i.e. school is located in an impulse area). Model (4) and (5) are estimated with the same equation, however, all schools that don't have any disadvantaged students registered are excluded from the sample in these models. Model (1) is a linear specification that doesn't use any control variables. Model (2) uses a quadratic form of the running variables, in model (3) there are added control variables to model (2). Model (4) uses a quadratic specification of the running variables as well, the only difference with model (3) is that model (4) uses solely schools that have one or more disadvantaged students registered in 2014. In model (5) there are only schools in the sample that had more than 20% disadvantaged students registered in 2014. Remember that schools receive subsidy proportional to the amount of disadvantaged students registered at their school. Therefore these schools received a substantial amount of money due to the subsidy. This model is referred to as the Poor Schools model. On top of the 5 models, there will be a bin-scatter plot of each stage that visualizes the relationship between the variable of interest and the running variable. These bin-scatter plots consist of 30 equal sized bins. As well as two quadratic lines fitting the 5576 unique observations underlying the bins at each side of the cutoff. The density of school with respect to the distance to cutoff is normally distributed, the cutoff is in the right tail of the normal distribution, see figure 3-5. Therefore there are fewer bins right to the cutoff, bins do not strictly contain data point on one side of the cutoff.

A. *Funding per Student*

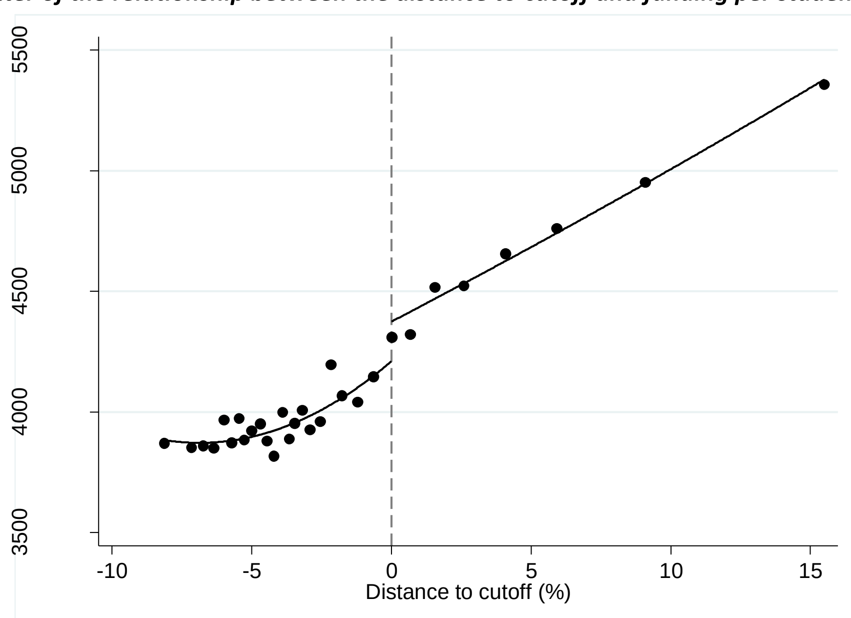
The results in table 4 provide evidence that the subsidy significantly increases the funding per student for all specifications. The point estimates suggest that schools receive additional funding per student within the range of 200 up to 482 euros depending on the specification. As expected, the funding per student increased the most due to the subsidy at schools with at least 20% disadvantaged students. These schools receive on average an additional €482 per student. Figure 7 reveals a positive relationship between the distance to cutoff and funding per student. This means that schools located in a zip code with a high percentage of low income or welfare households receive more funding per student. This can be largely explained by the relationship between the percentage of disadvantaged students and the neighborhood characteristics of schools. Schools located in bad neighborhoods have more disadvantaged students registered and therefore they receive more funding. In figure 7, there is observed a positive jump in funding per student at the cutoff. This jump confirms the findings in table 4 that the subsidy increased the funding per student.

Table 4: The effect of the Impulse Area Subsidy on funding per student

	(1) Linear	(2) Quadratic	(3) Controls	(4) Received	(5) Poor schools
Impulse Area	481***	256***	206***	227***	482***
	(56)	(64)	(47)	(47)	(107)
Low income	18*	-128**	-55	-6	-15
	(11)	(54)	(40)	(40)	(121)
Welfare	-45***	-278***	-171***	-150***	-113
	(8)	(36)	(27)	(27)	(101)
Low income²		7**	3	1	1
		(3)	(2)	(2)	(6)
Welfare²		14***	8***	7***	6
		(2)	(2)	(2)	(5)
Constant	4,294***	5,861***	5,629***	5,244***	4,212***
	(130)	(298)	(236)	(240)	(788)
Controls	X	X	✓	✓	✓
Observations	1,802	1,802	1,802	1,701	294
Average	4196.28	4196.28	4196.28	4189.72	4894.50
R-squared	0.075	0.100	0.540	0.557	0.677

Note: Funding per student measured in euros. Bandwidth = 3 %-points. Control variables: percentage disadvantaged students, province, type of school, type of end test, school size, percentage exempted from end test. Standard errors in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1

Figure 7: Bin-scatter of the relationship between the distance to cutoff and funding per student



Note: 30 equal sized bins; quadratic fit line based on the underlying 5576 school observation

B. Class size

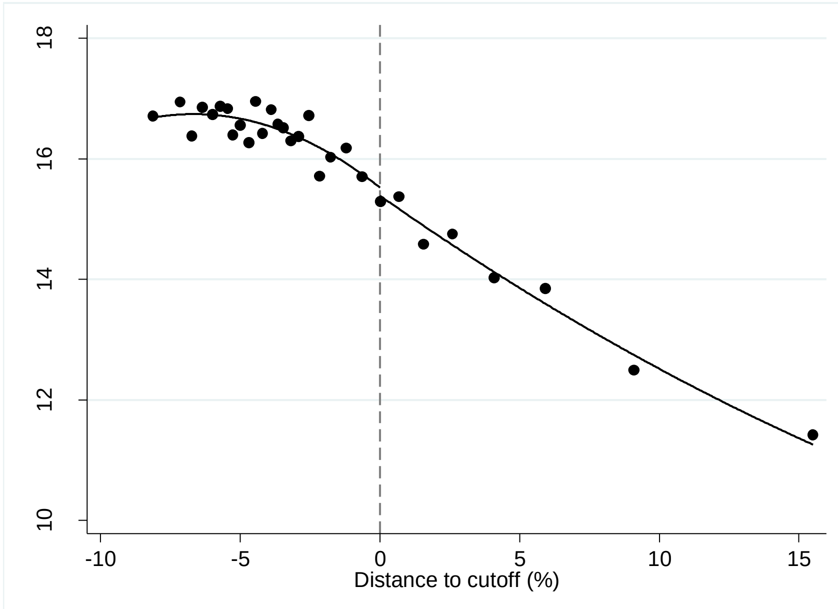
Next, we will look if the extra funding assigned to schools due to the subsidy is effectively used to reduce class sizes. All models find negative point estimates, suggesting that the funding was indeed used to reduce class sizes. The point estimates are in the range of -0.4 and -0.9, which means that schools that received the subsidy did reduce the class size with 0.4-0.9 students. This effect is significant in the first four models. In model (5) that looks just at the effect of the subsidy on poor schools fails to find a significant effect of the subsidy, while they received the highest subsidy. An explanation might be that these schools already had smaller classes, on average 13.42 students against 15.69 students in other schools. Therefore they might have found it more difficult to reduce class sizes further and hence used the money for other purposes. Figure 8 gives the relationship between the neighborhood characteristics of schools and the class size. In this figure, we observe a negative relationship, which means that schools in bad neighborhoods have smaller class sizes. This can again be explained by the relationship between the percentage of disadvantaged students and neighborhood characteristics of a school. Schools located in bad neighborhoods have more disadvantaged students registered and therefore they receive more funding which enables them to reduce class sizes. Moreover, there is observed a small negative jump in class sizes at the cutoff value, which confirms the results in table 6 that the subsidy reduced class sizes.

Table 5: The effect of the Impulse Area Subsidy on class size

	(1) Linear	(2) Quadratic	(3) Controls	(4) Received	(5) Poor schools
Impulse Area	-0.842***	-0.512*	-0.555**	-0.555**	-0.415
	(0.244)	(0.280)	(0.254)	(0.254)	(0.530)
Low income	-0.257***	-0.174	-0.279	0.058	0.583
	(0.047)	(0.238)	(0.217)	(0.220)	(0.596)
Welfare	0.113***	0.508***	0.283*	0.255*	0.169
	(0.036)	(0.159)	(0.146)	(0.150)	(0.496)
Low income²		-0.004	0.005	-0.011	-0.038
		(0.013)	(0.012)	(0.012)	(0.029)
Welfare²		-0.024**	-0.007	-0.006	-0.008
		(0.010)	(0.009)	(0.009)	(0.026)
Constant	17.308***	15.366***	15.403***	14.086***	14.126***
	(0.564)	(1.305)	(1.284)	(1.311)	(3.888)
Controls	X	X	✓	✓	✓
Observations	1,802	1,802	1,802	1,802	294
Average	15.69	15.69	15.69	15.69	13.42
R-squared	0.052	0.056	0.255	0.254	0.393

Note: Class size measured in: students per school divided by the number of FTE teachers. Bandwidth = 3 %-points. Control Variables: percentage disadvantaged students, province, type of school, type of end test, school size, percentage exempted from end test. Standard errors in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1

Figure 8: Bin-scatter of the relationship between the distance to cutoff and class size



Note: 30 equal sized bins; quadratic fit line based on the underlying 5576 school observation

C. End Test

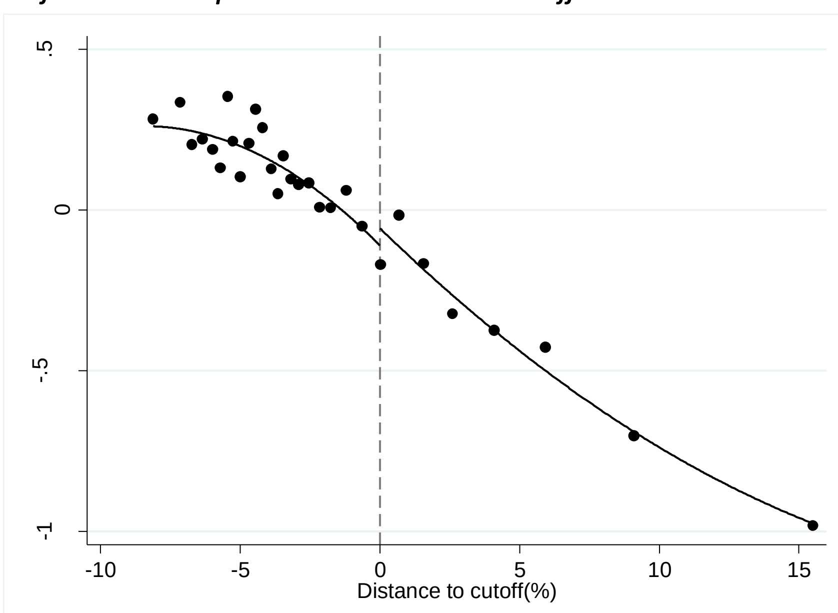
In the end, the most important question is: did the extra funding and the coinciding reduced class size lead to an increase in test scores? The outcome variable is the test scores of 2015, the subsidy was first introduced in 2009. This means that the schools that were eligible for the subsidy have been eligible for 6 years. Assuming that the students in 8th grade that took the end test did not switch schools since 2009 means that these students have been benefiting from the subsidy since 3rd grade. A Back-of-the-envelope calculation with averages taken from the previous stages shows that the subsidy provided €28.260 over 6 years for a regular class. Funding per students increased with €300, multiply this by the average class size of 15.69 and multiply this again with the 6 years that this subsidy has been provided, makes €28.260. Despite this amount of money that has been provided to the students that took the end test, there is not any significant effect of the subsidy found on their test scores. The point estimates in table 6 are in the range of -0.103 and 0.150 standard deviation; this provides little information about the direction of the effect. In appendix C the same analysis is performed with the whole sample instead of the preferred bandwidth of 3 in the main text. The results of those models find solely insignificant negative point estimates, suggesting that the subsidy had a negative effect on the test score of students. Figure 9 finds a small positive jump in test score at the cutoff value, suggesting that the subsidy increased test scores. This finding is not robust to a change of the functional form of the fitting line. The line used in figure 9 is a quadratic line fitting the underlying data of the bins. A linear line finds a negative jump in test scores at the cutoff. The only conclusion that can be drawn from all findings taken together is that the subsidy did not influence test scores in any way.

Table 6: The effect of the Impulse Area Subsidy on normalized end test score

End Test	(1) Linear	(2) Quadratic	(3) Controls	(4) Received	(5) Poor schools
Impulse Area	-0.103	0.015	-0.000	0.016	0.150
	(0.073)	(0.084)	(0.077)	(0.078)	(0.210)
Low income	-0.002	-0.036	-0.019	-0.068	-0.070
	(0.014)	(0.071)	(0.066)	(0.068)	(0.236)
Welfare	-0.033***	0.134***	0.113**	0.073	-0.212
	(0.011)	(0.048)	(0.045)	(0.046)	(0.196)
Low income²		0.002	0.002	0.005	0.003
		(0.004)	(0.004)	(0.004)	(0.012)
Welfare²		-0.010***	-0.007***	-0.005**	0.006
		(0.003)	(0.003)	(0.003)	(0.010)
Constant	0.328*	-0.190	0.074	0.352	1.147
	(0.169)	(0.391)	(0.391)	(0.403)	(1.539)
Controls	X	X	✓	✓	✓
Observations	1,802	1,802	1,802	1,701	294
Average	-0.044	-0.044	-0.044	-0.086	-0.784
R-squared	0.015	0.022	0.199	0.188	0.197

Note: Normalized end test score; mean= 0, standard error =1. Bandwidth = 3 %-points. Control Variables: percentage disadvantaged students, province, type of school, type of end test, school size, percentage exempted from end test. Standard errors in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1

Figure 9: Bin-scatter of the relationship between the distance to cutoff and the normalized test score



Note: 30 equal sized bins; quadratic fit line based on the underlying 5576 school observation

D. Results continued

The results found in the analysis are worrying. The first stage shows that the subsidy does increase the funding per student in the range of €200 and €480 depending on the specification. The average spending per student is more than €4000, which means that the subsidy increased the funding per students with approximately 7.5%. In the second stage, there is found evidence that the increase in funding has been effectively used to reduce class sizes. In the model with just poor schools this evidence is not found. The poor schools already had smaller classes, 13 students per class versus 16 students per class at regular schools. This might have been the reason these schools did not reduce the class size even further. On average the subsidy did decrease class sizes of regular schools with circa 4%. In the end, there is not any significant effect of the subsidy found on the test scores in primary education. On the one hand, this is shocking because the height of the subsidy in 2014 alone was approximately €133 million. On the other hand, this money is only 2.2% of the total spending of €5,8 billion on education, therefore the subsidy is quite marginal in comparison to the total spending on education.

VI. Conclusion

This paper evaluated the effect of a subsidy in primary education focusing on impulse areas. Schools located in impulse areas received an additional €1702 for each disadvantaged student. The strict criteria that were used to label a zip code as an impulse area created a strict cutoff. While there is found some evidence of discontinuities at the cutoff level, it seems highly unlikely this is the result of manipulation. Besides the fact that it is unlikely that schools can manipulate zip code characteristics, the subsidy used characteristics from years prior to the announcement of the subsidy. Detailed information and data about the assignment criteria made this a great setting to assess the effect of the subsidy with a regression discontinuity design.

The effect of the subsidy is assessed in three stages. The first stage finds that the subsidy did significantly increase the funding per student within the range of €200-€500. This is approximately 7.5% of the average spending per student of €4200 in primary education. In the second stage, there is found some evidence that the subsidy reduced class sizes with 0.4-0.9 students. This is roughly a decrease of 4%. The third stage uses the score of 8th grade students on a nationwide exam as the ultimate proxy of the effectiveness of the subsidy. The students in 8th grade have been benefitting from the subsidy since 3rd grade. This paper does not find any evidence that the subsidy had an effect on test scores. This is quite worrying since the Dutch government has spent approximately €600 million on the subsidy between 2011 and 2015 alone. These are just the cost of 4 of the 8 years that the subsidy will be provided. Meaning that the total cost will be circa 1.2 billion euro's if the government decides to stop the subsidy after 8 years in 2017.

The paper of Leuven et al. (2007) evaluated a subsidy in Dutch primary education as well. Their results are in line with the current paper; the increased funding due to the subsidy evaluated in their paper did not lead to improved test results. This leads to the conclusion that just providing extra funds to schools does not directly lead to improved performance of students.

There should be made some cautionary notes relating to the previously presented findings. This paper, as well as the paper of Leuven et al. (2007), solely looks at the effect of the subsidy on test scores that measure cognitive skills. A study of Heckman, Stixrud and Urzua (2006) reveals that noncognitive skills, corrected for schooling and family background effects, raise wages through their direct effects on productivity as well as through their indirect effects on schooling and work experience. It might be that the impulse area subsidy did improve the noncognitive skills of the children at subsidized schools, which would increase their earning potential and productivity. Schweinhart et al. (2005) conducted a wide study providing evidence of the difference between short and long term effect of interventions. Disadvantaged students were randomly selected into a high-quality pre-school program at the age of 3 and got monitored until they reached the age of 40. The study reveals that the program initially didn't improve the IQ of students, however, in the long run there was a substantial positive effect on earnings found. Moreover, the current paper evaluates a subsidy that was introduced when the treatment and control group were in 3rd grade of primary education. Carneiro and Heckman (2003) found evidence that the effect of a decrease in class size mainly occurs in the earliest grades. This might as well partly explain the fact that there is no effect of the impulse area subsidy on test scores found. In two years the students that benefited from the impulse area subsidy since first grade of primary education will take the end test. It would be interesting to research the effect of the subsidy on their test results.

It is highly important that policy makers keep evaluating their policies. Eventually, this will lead to more efficient policies that might contribute to the decline of the performance gap between students from different social economic backgrounds. This paper demonstrated that six year of subsidizing primary schools in poor neighborhood hasn't been an effective tool to improve the test results of students on a nationwide exam.

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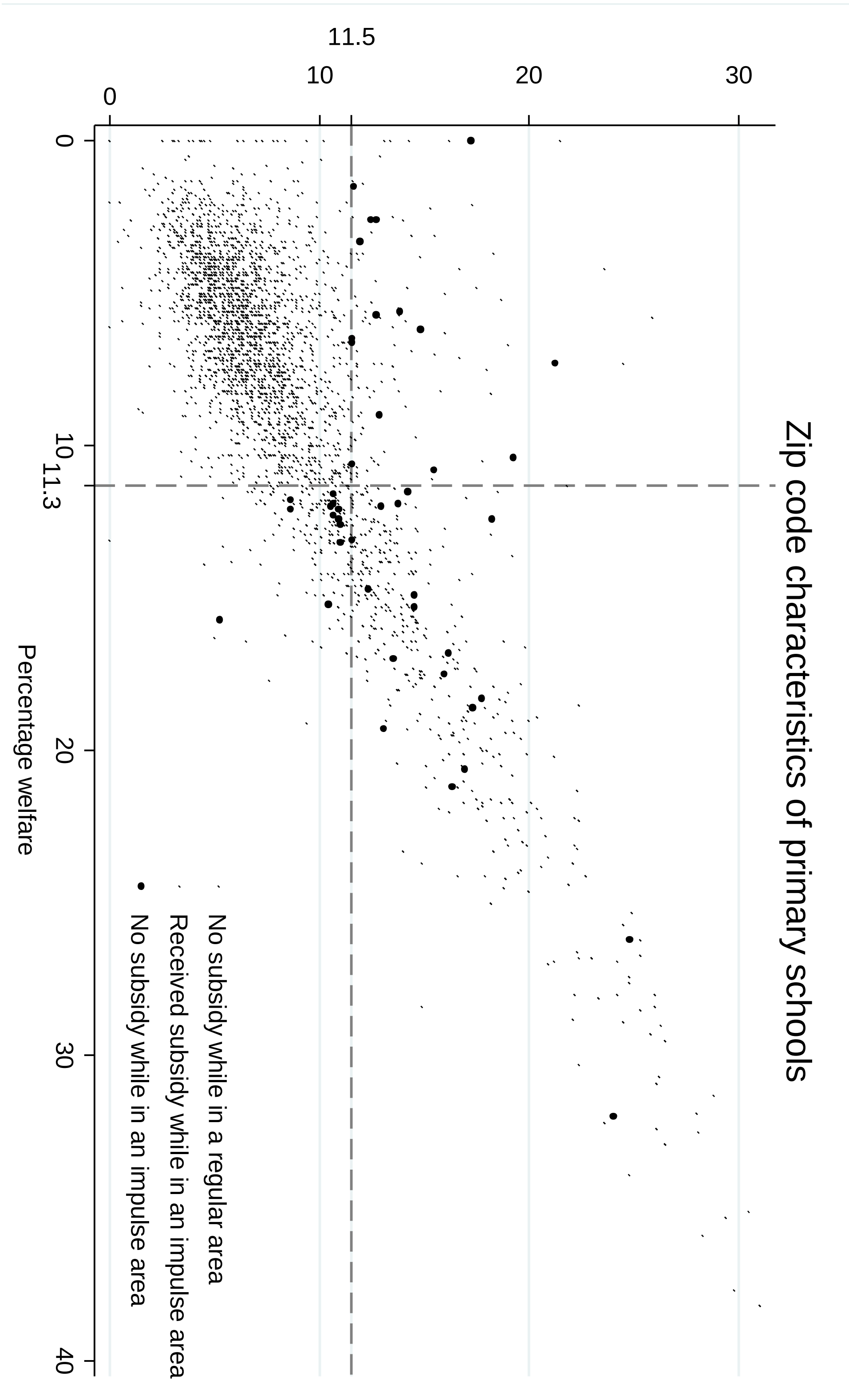
Appendix A: Figure 2 in a larger format

Figure 2: Treatment status of schools plotted against the two running variables; bold points indicate ‘no shows’



Appendix B – Discontinuities at cutoff (type of school)

Table 7: Estimated discontinuities of the type of school

Variable	Low income	Welfare	Distance to cut off
General	0.036	-0.015	0.001
	(0.105)	(0.067)	(0.057)
Reformed	0.045	0.009	0.010
	(0.067)	(0.043)	(0.037)
Public	0.129	0.071	-0.015
	(0.237)	(0.152)	(0.129)
Protestant-Christian	0.121	0.089	0.054
	(0.223)	(0.142)	(0.122)
Reformation	-0.010	-0.006	-0.023
	(0.084)	(0.054)	(0.046)
Catholic	-0.081	-0.033	0.041
	(0.237)	(0.152)	(0.130)
Observations	5576	5576	5576

Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1

Appendix C – Stage 1-3 from main text with whole sample instead of the bandwidth of 3

Table 8: The effect of the Impulse Area Subsidy on funding per student

Funding per Student	(1) Linear	(2) Quadratic	(3) Controls	(4) Received	(5) Poor schools
Impulse Area	315***	412***	311***	298***	
	(35)	(37)	(26)	(26)	
Low income	62***	37***	-5	-6	-74***
	(5)	(12)	(8)	(9)	(25)
Welfare	-5	-43***	-52***	-46***	16
	(4)	(8)	(6)	(6)	(19)
Low income²		1	1***	1***	3***
		(1)	(0)	(0)	(1)
Welfare²		2***	1***	1***	-1
		(0)	(0)	(0)	(1)
Constant	3,576***	3,885***	4,720***	4,604***	3,843***
	(24)	(46)	(61)	(63)	(191)
Controls	X	X	✓	✓	✓
Observations	5,576	5,576	5,576	5,226	881
Average	4137	4137	4137	4131	5093
R-squared	0.215	0.226	0.606	0.631	0.720

Note: Funding per student measured in euros. Control variables: percentage disadvantaged students, province, type of school, type of end test, school size, percentage exempted from end test. Standard errors in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1

Table 9: The effect of the Impulse Area Subsidy on Class size

Class size	(1) Linear	(2) Quadratic	(3) Controls	(4) Received	(5) Poor schools
Impulse Area	-0.722***	-0.977***	-0.723***	-0.779***	
	(0.160)	(0.167)	(0.152)	(0.152)	
Low income	-0.259***	-0.267***	-0.174***	-0.101**	-0.036
	(0.021)	(0.053)	(0.049)	(0.049)	(0.118)
Welfare	0.015	0.165***	0.169***	0.124***	-0.161*
	(0.016)	(0.037)	(0.035)	(0.035)	(0.088)
Low income²		0.002	0.001	-0.002	-0.000
		(0.002)	(0.002)	(0.002)	(0.004)
Welfare²		-0.007***	-0.004**	-0.002*	0.003
		(0.002)	(0.001)	(0.001)	(0.003)
Constant	18.068***	17.363***	15.615***	15.811***	18.776***
	(0.110)	(0.209)	(0.353)	(0.364)	(0.893)
Controls	X	X	✓	✓	✓
Observations	5,574	5,574	5,574	5,226	881
Average	15.83	15.83	15.83	15.83	12.61
R-squared	0.149	0.154	0.309	0.334	0.442

Note: Class size measured in: students per school divided by the number of FTE teachers.Control Variables: percentage disadvantaged students, province, type of school, type of end test, school size, percentage exempted from end test. Standard errors in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1

Table 10: The effect of the Impulse Area Subsidy on Test Scores

End Test	(1) Linear	(2) Quadratic	(3) Controls	(4) Received	(5) Poor schools
Impulse Area	-0.085*	-0.096*	-0.051	-0.022	
	(0.047)	(0.050)	(0.046)	(0.047)	
Low income	-0.013**	-0.011	0.005	0.000	-0.014
	(0.006)	(0.016)	(0.015)	(0.015)	(0.052)
Welfare	-0.041***	-0.035***	-0.026**	-0.029***	-0.076**
	(0.005)	(0.011)	(0.010)	(0.011)	(0.039)
Low income²		0.000	-0.000	-0.000	-0.001
		(0.001)	(0.001)	(0.001)	(0.002)
Welfare²		-0.000	0.000	0.001	0.002*
		(0.000)	(0.000)	(0.000)	(0.001)
Constant	0.485***	0.453***	0.623***	0.533***	0.534
	(0.033)	(0.062)	(0.107)	(0.113)	(0.390)
Controls	X	X	✓	✓	✓
Observations	5,576	5,576	5,576	5,226	881
Average	0.008	0.008	0.008	-0.032	-0.858
R-squared	0.092	0.092	0.232	0.232	0.198

Note: Normalized end test score; mean= 0, standard error =1. Bandwidth = 3 %-points. Control Variables: percentage disadvantaged students, province, type of school, type of end test, school size, percentage exempted from end test. Standard errors in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1

Appendix D- Calculation of the preferred Bandwidth, raw output following Calonico et al. (2014)

Distance to cut off bandwidth-

```
. rdbwselect z_score norm_max_lowinc_benefit, all

Bandwidth estimators for sharp RD local polynomial regression.

         Cutoff c = 0 | Left of c  Right of c            Number of obs =       5576
----------------------+-----------------------           Kernel        = Triangular
        Number of obs |      4190        1386            VCE method    =         NN
Min of norm_max_lowinc_benefit |     -2.202       0.000
Max of norm_max_lowinc_benefit |     -0.019       5.242
 Order loc. poly. (p) |         1           1
       Order bias (q) |         2           2

Outcome: z_score. Running variable: norm_max_lowinc_benefit.
-------------------------------------------------------------------------------
                      |       BW loc. poly. (h)     |         BW bias (b)
               Method |   Left of c    Right of c   |    Left of c    Right of c
----------------------+-----------------------------+--------------------------
                mserd |       2.643         2.643   |        4.243        4.243
               msetwo |       3.043         5.502   |        4.579        9.393
               msesum |       3.083         3.083   |        4.590        4.590
             msecomb1 |       2.643         2.643   |        4.243        4.243
             msecomb2 |       3.043         3.083   |        4.579        4.590
----------------------+-----------------------------+--------------------------
                cerrd |       1.717         1.717   |        4.243        4.243
               certwo |       1.977         3.574   |        4.579        9.393
               cersum |       2.003         2.003   |        4.590        4.590
             cercomb1 |       1.717         1.717   |        4.243        4.243
             cercomb2 |       1.977         2.003   |        4.579        4.590
-------------------------------------------------------------------------------
```

Percentage low income bandwidth

```
. rdbwselect z_score plink, c(11.5) all

Bandwidth estimators for sharp RD local polynomial regression.

      Cutoff c = 11.5 | Left of c  Right of c            Number of obs =       5576
----------------------+-----------------------           Kernel        = Triangular
        Number of obs |      4568        1008            VCE method    =         NN
         Min of plink |     0.000       2.621
         Max of plink |     2.599       7.005
 Order loc. poly. (p) |         1           1
       Order bias (q) |         2           2

Outcome: z_score. Running variable: plink.
-------------------------------------------------------------------------------
                      |       BW loc. poly. (h)     |         BW bias (b)
               Method |   Left of c    Right of c   |    Left of c    Right of c
----------------------+-----------------------------+--------------------------
                mserd |       2.516         2.516   |        4.227        4.227
               msetwo |       2.962         3.665   |        4.278        7.195
               msesum |       2.619         2.619   |        4.395        4.395
             msecomb1 |       2.516         2.516   |        4.227        4.227
             msecomb2 |       2.619         2.619   |        4.278        4.395
----------------------+-----------------------------+--------------------------
                cerrd |       1.634         1.634   |        4.227        4.227
               certwo |       1.924         2.381   |        4.278        7.195
               cersum |       1.702         1.702   |        4.395        4.395
             cercomb1 |       1.634         1.634   |        4.227        4.227
             cercomb2 |       1.702         1.702   |        4.278        4.395
-------------------------------------------------------------------------------
```

Percentage welfare bandwidth

Bandwidth estimators for sharp RD local polynomial regression.

Cutoff c = 11.3	Left of c	Right of c
Number of obs	4364	1212
Min of puitk	0.000	2.101
Max of puitk	2.083	7.041
Order loc. poly. (p)	1	1
Order bias (q)	2	2

Number of obs = 5576
Kernel = Triangular
VCE method = NN

Outcome: z_score. Running variable: puitk.

Method	BW loc. poly. (h) Left of c	BW loc. poly. (h) Right of c	BW bias (b) Left of c	BW bias (b) Right of c
mserd	3.570	3.570	6.090	6.090
msetwo	4.469	4.270	5.899	8.140
msesum	3.420	3.420	5.667	5.667
msecomb1	3.420	3.420	5.667	5.667
msecomb2	3.570	3.570	5.899	6.090
cerrd	2.319	2.319	6.090	6.090
certwo	2.903	2.774	5.899	8.140
cersum	2.222	2.222	5.667	5.667
cercomb1	2.222	2.222	5.667	5.667
cercomb2	2.319	2.319	5.899	6.090