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Heterogeneous Effects of School Resources on Child Mental Health Development: Evidence from the Netherlands[1]

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Abstract

This study investigates the effect of a Dutch policy measure (student-bound funding) that provided regular primary and secondary schools additional budgets for special needs students. Schools were responsible in deciding how to allocate the additional money ($\sim$ 14,400 euros per eligible student). We use a unique cohort study tracking pupils from primary school to adulthood between 2000 and 2012. Following norms for eligibility of the Dutch Ministry of Education, Culture and Science, 43 of the 1,218 were classified as special needs students. We use a Differences-In-Differences (DID) design to estimate the causal effect on the mental health of special needs students. The results suggest that student-bound funding improved their mental health with 0.41 standard deviation. Ten-percent quantile DID estimates show that the policy was the most (least) effective at the higher (lower) end of the mental health distribution. Estimates and statistics are robust to propensity score reweighting. Heterogeneity analyses present a cautionary note, as disadvantaged students from an ethnic minority or a lower socio-economic environment benefited less from the policy.

Keywords: Mental Health, Education Policy, Effect Heterogeneity
JEL Codes: I14, I24, I26, I28

[1]This paper uses data from the TRacking Adolescents' Individual Lives Survey (TRAILS). Our special thanks go out to the TRAILS Data Group for managing and providing the data. Furthermore, this paper was presented at the Tinbergen Institute Seminar in Rotterdam, the Royal Economic Society Annual Conference 2019 in Coventry and the International Association of Applied Econometrics Annual Conference 2019 in Nicosia. We want to thank everyone present for their comments and feedback. Additionally, many thanks for invaluable comments from Gabriella Conti, Hans van Kippersluis, Martin Salm, Beatriz Eugster, Bram Wouterse, and Sarah See. This work was supported by the Dutch Child and Adolescent Psychiatry Centre Accare. Finally, the findings and views reported in this paper are those of the authors and should not be attributed to individuals or organisations mentioned here.

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1 Introduction

A large series of studies show that if children perform better in education this significantly increases their chances in life, as education relates to a variety of outcomes throughout the life cycle. The literature demonstrates that more education leads to a higher probability of work (Oreopoulos, 2006b, 2006a, 2007), a higher income (Bhuller, Mogstad, & Salvanes, 2017; Devereux & Hart, 2010; Leigh & Ryan, 2008), healthier behaviour (Brunello, Fort, Schneeweis, & Winter-Ebmer, 2016; Grimard & Parent, 2007; Jürges, Reinhold, & Salm, 2011), a smaller chance of becoming ill (Kemptner, Jürges, & Reinhold, 2011; Oreopoulos, 2006b, 2007), fewer teenage pregnancies (Cygan-Rehm & Maeder, 2013; Silles, 2011) and less crime (Cullen, Jacob, & Levitt, 2006; Deming, 2011; Amin, Flores, Flores-Lagunes, & Parisian, 2016). Consequently, students disadvantaged by low-ability or worse health (hereafter special needs students) have potentially a lower opportunity profile through educational setbacks early in life.

One of the most important and controversial questions in the reduction of educational disparities is the impact of school resources on student outcomes, as it is a policy measure directly available to governments. Previous research conclude that there is little consistent relationship between school resources and student achievement (Hanushek, 2003, 2006). Ignoring endogeneity problems could explain the lack of a consistent relationship, as variation to evaluate policy measures is commonly correlated with (un)observed school and student characteristics.

There are several recent studies that use a quasi-experimental design or bounded analysis to tackle endogeneity problems. These studies obtain contradictory findings of the causal impact of additional budgets on student outcomes. Leuven, Lindahl, Oosterbeek, and Webbink (2007) and Van der Klaauw (2008) find negative effects on students' achievement, while Papke (2005), Holmlund, McNally, and Viarengo (2010), Machin, McNally, and Meghir (2010), De Haan (2015) and Gibbons, McNally, and Viarengo (2017) all find positive effects. Bénabou, Kramarz, and Prost (2009) find ambiguous effects of additional school resources.

These studies focus mainly on policy measures that are implemented nationwide for each student and are evaluated in achievement outcomes only. By contrast, this article studies the impact of school resources on special needs students' mental health by estimating the effect of a Dutch policy measure (student-bound funding) that provided additional budgets

to regular primary and secondary schools to support progression of special needs students in regular education. Schools were responsible in deciding how to allocate the additional money ($\sim$ 14,400 euro per eligible student). Unlike most studied resource policies, student-bound funding is implemented conditionally on eligibility terms set by the Dutch Ministry of Education, Culture and Science. This exogenous variation allows us to identify the counterfactual change over time of the mental health of special needs student if the policy had not occured. As such, we estimate the causal impact of student-bound funding on the mental health of special needs students. To do so, we use a rich cohort study tracking children from primary school to adulthood between 2000 and 2012.

The policy measure was abolished in July 2014, as budget expenditures increased over half a billion euros per year. We highlight the potential benefits of student-bound funding, as the policy measure increased the mental health of special needs students with 0.41 Standard Deviation (SD). This has an huge impact on their opportunity profile, taking into account the life cycle returns to mental health.

The remainder of the article is structured as follows. Section 2 describes the Dutch education system and the details of the policy measure. Section 3 provides a detailed description of the data. Section 4 explains the identification strategy. Results from this strategy are provided in Section 5. Supplementary analyses of the results are given in Section 6. Finally, Section 7 summarises and concludes.

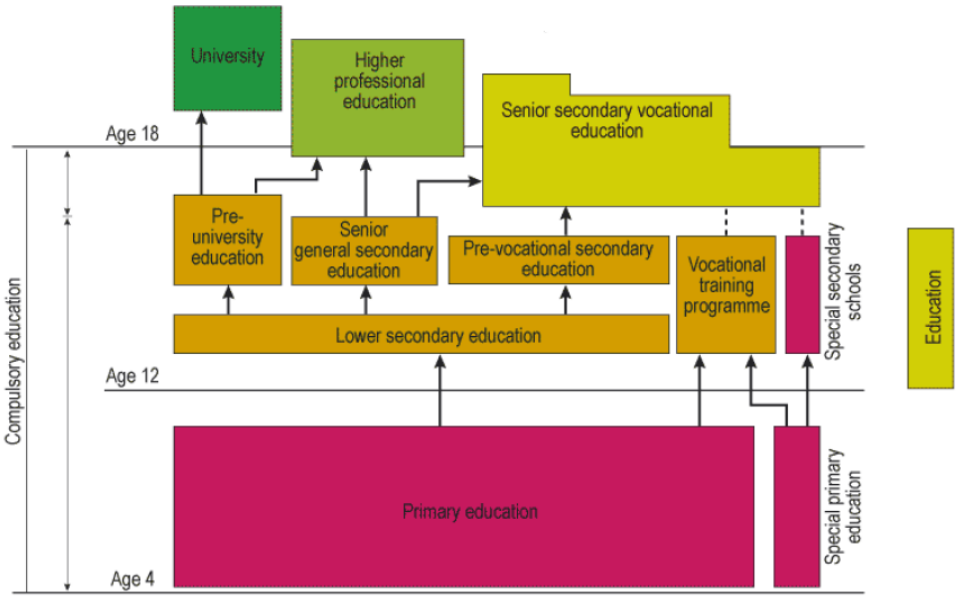
2 Background

2.1 Dutch Education System

A flowchart of the Dutch education system is presented in Figure 1. Most children start primary school at the age of four, although they are not required by law to attend school until the age of five.[1] At the end of primary school (at around 12 years) pupils receive an advice which type of educational programme would be the most appropriate, given the school results and capacities of the pupil. The advice is based on the judgment of the primary school

[1] In this study we focus on the primary and secondary school period. However, children have the opportunity to follow early childhood education (Dutch: Voor- en Vroegschoolse Educatie (VVE)). Early childhood education is a form of education designed to optimize the development opportunities of children aged two to five. Early childhood education for pre-schoolers (two to four-year-olds) is provided by playgroups and day nurseries. Primary schools offer VVE programs for four and five-year-olds.

Figure 1: Flowchart of the Dutch education system



teachers about the capacity of the pupil and on the results of an objective test. About 95% of the schools use the national test provided by Cito[2], the Dutch Testing and Assessment Company. Based on this advice parents and pupils can apply for the school they prefer and that provides the appropriate educational program.

After finishing primary education children can apply for secondary education. Secondary education consists of pre-vocational secondary education (Dutch: Voorbereidend Middelbaar BeroepsOnderwijs (VMBO)), general secondary education (Dutch: Hoger Algemeen Voortgezet Onderwijs (HAVO)) and pre-university education (Dutch: Voorbereidend Wetenschappelijk Onderwijs (VWO)). Schools are free in the composition of the classroom (homo- or heterogeneous) and in the manner they design education towards the attainment standards. Most schools in secondary education are combined schools, which makes it possible to make the first two years of secondary education as common as possible in so called bridge classes. This makes it easy during these years to switch from one type of secondary education as possible. Combined schools also give better opportunities for switches in the upper levels of

[2]We refer the reader to http://cito.com for more information.

secondary education. After finishing the first years of lower secondary education, students make their final decision on one of the three types of secondary education.

Pre-vocational secondary education (four years) is meant as a preparation for senior secondary vocational education (Dutch: Middelbaar BeroepsOnderwijs (MBO)). General secondary education (five years) is designed to prepare students for higher professional education (Dutch: Hoger BeroepsOnderwijs (HBO)). In practice, however, general secondary education school-leavers also go on to the upper years of pre-university education or to senior secondary vocational education. Finally, pre-university education (six years) is designed to prepare students for university (Dutch: Wetenschappelijk Onderwijs (WO)) and in most cases pre-university education certificate-holders go on to university. Some of them though enter higher professional education.

For children with disabilities, or such special needs that they cannnot attend regular education, special education is provided. Children can be placed into a special school based on an indication given by an indication committee. It is up to the parents to decide whether their child attends a special school or a regular school, given the indication. Is the latter the case the regular school could get extra funding through an additional budget which is provided to the parents, as further discussed in Section 2.2. There are special schools for the visually handicapped children, for the deaf or the ones with speaking problems, for the physically handicapped and for the children with severe behaviour problems. There are special primary schools for children of primary education age (Dutch: Speciaal Onderwijs (SO)) and special secondary schools for children from twelve age onwards (Dutch: Voorgezet Speciaal Onderwijs (VSO)). For children in special secondary schools a new provision is developed, the so-called on the job training schools. Special secondary schools students are stimulated to combine school with work from age sixteen on. This provision is aimed for an adequate labour market perspective of these students.

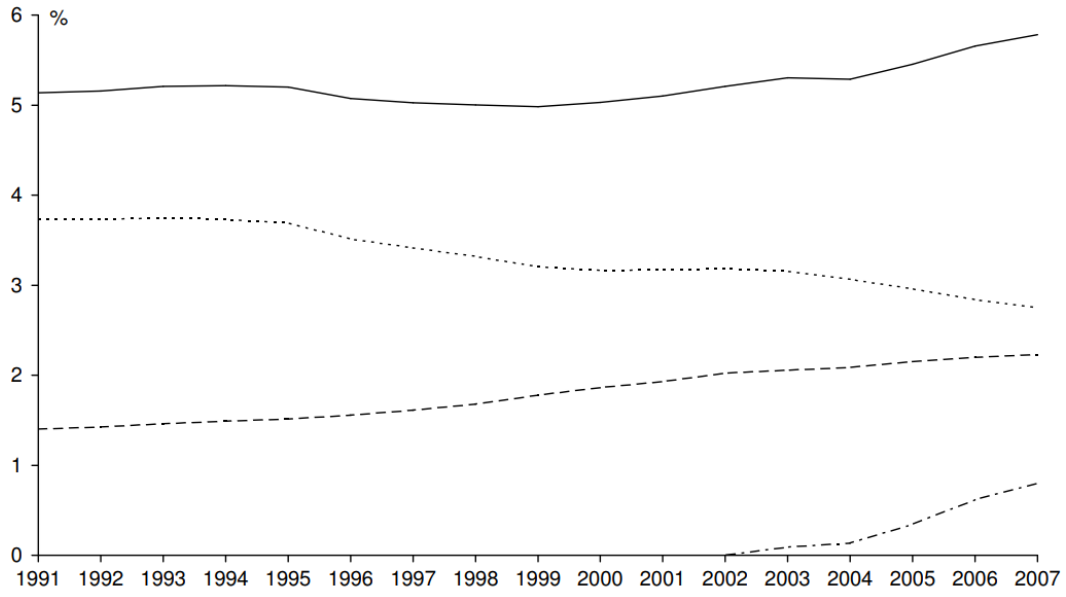
2.2 Student-bound Funding

In the Netherlands Regional Expertise Centra (REC) are aimed at the indication of special needs students. Each school for special education participates in one of these 34 RECs. In the August of 2003 the Dutch Ministry of Education, Culture and Science introduced a policy measure that provided additional budgets to regular primary and secondary schools to

support progression of special needs students in regular education (student-bound funding). Student-bound funding was implemented conditionally on eligibility terms set by the Dutch Ministry of Education, Culture and Science. The Committee of Indication of Assessment became responsible for the assignment and assessment of referred students. Once a student was assigned to the policy measure a budget was allocated to the corresponding school. Schools were responsible in deciding how to allocate the additional money of $\sim$ 14,400 euro per eligible student.[3] The money was mainly used for additional school materials or teaching assistance to support the assigned students to catch up with their regular peers.

The proportion of special needs students remained virtually constant between 1991 and 2002 and increased since 2003 to 5.7% in 2007 (see the continuous line in Figure 2). The dotted and dashed lines present the percentage of special needs students in special and regular education, respectively. The subsitution of special needs students from special to regular education is illustrated by the dashed dotted line – the percentage of students with student-bound funding from 2003 (Minne et al., 2009).

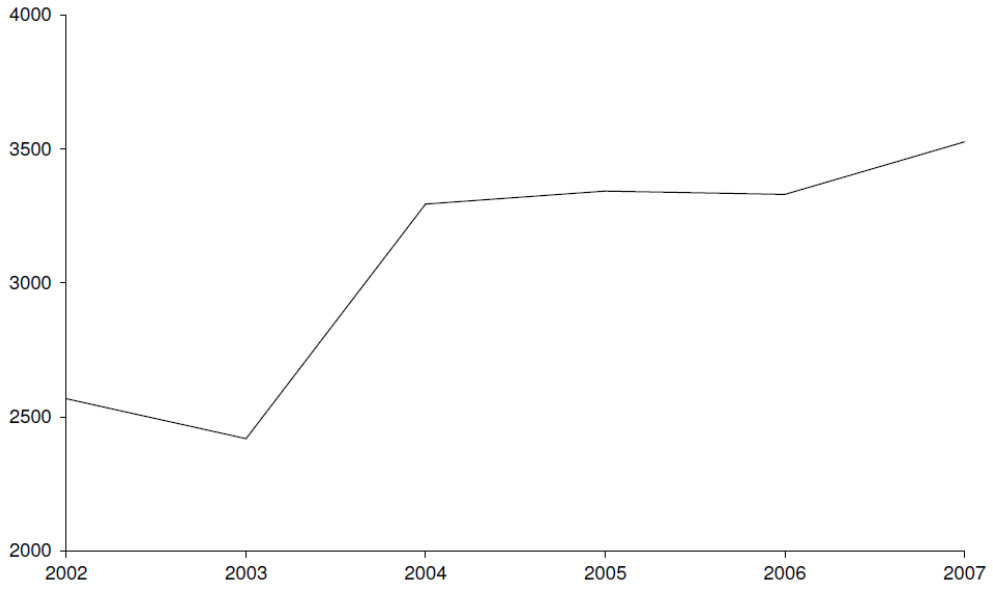
Figure 2: Percentage of special needs students



[3]The direct educational costs differ substantially between students. Special needs students cost on average 18,400 euros per year if the student is assigned to student-bound funding and about 8,900 euros per year if the student is still in special education. The educational costs of a regular student are on average 4,000 euros per year (Minne, Webbink, & Wiel, 2009).

This sudden increase in special needs students is also reflected in Figure 3, as the number of students on the waiting lists of the Committee of Indication of Assessment jumped upwards after 2003.

Figure 3: Number of students on the waiting list for assignment



The policy for special needs students came under serious change due to heavy criticism, because the additional budget which was necessary to support Student-bound funding was exploding and credible estimates on the effects were lacking. The policy measure was therefore abolished in July 2014, as budget expenditures increased over half a billion euros per year. In the new policy the Dutch Ministry of Education, Culture and Science aimed to reduce the impact of the indication system. This new policy involved a budget cut of 300 million euros. In this study we evaluate the value of additional budgets to support progression of special needs students in regular education by estimating the causal impact of student-bound funding on special needs students' mental health development.

3 Data

3.1 TRacking Adolescents' Individual Lives Survey

We use data of the TRacking Adolescents' Individual Lives Survey (TRAILS). TRAILS sampled participants born between 1 October 1989 and 30 September 1991, who lived in one of the five municipalities in the North of the Netherlands at the time of the baseline assessment in 2000, including both urban and rural areas to construct a representative sample. In total 2,230 children were included, corresponding to a response rate of 76%. The mean age was 11.1 and 51% of the sample were girls (Huisman et al., 2008). Subsequent data collection waves took place bi- or triennially, and all five waves had good retention rates with 71% of the original cohort participating in all data collections (Oldehinkel et al., 2015). The sample for this study counts 1,218, as for 365 participants scores on mental health were missing.

Scores on the Youth Self Report (YSR) for the years 2000, 2003 and 2006 and on the Adult Self Report (ASR) for the years 2009 and 2012 were used to measure mental health. YSR and ASR are questionnaires of 113 items, validated by the Achenbach System of Empirically Based Assessment (ASEBA) (Achenbach & Rescorla, 2013). All items are scored by the participant with a 0 ('not applicable at all'), 1 ('a little or sometimes applicable') or 2 ('clear or often applicable'). We convert the scores for the analysis into average scores between 0 and 100 in which (0) 100 represents the worst (best) mental health state. The questions about behaviour are the problem scales (withdrawn/depressed, physical complaints, anxious/depressed, social problems, thinking problems, attention problems, norm-deviant behaviour and aggressive behaviour). The first three problem scales together form the broad-band syndrome 'internalizing problems', the latter two the broad-band syndrome 'externalizing problems', and all problem scales together form the scale 'total problems' (Achenbach & Rescorla, 2013). For the main results of this study we used the scale total problems. We split the broad-band syndromes in the supplementary analyses.

We add a set of observed characteristics as control variables in the empirical strategy. We use a dummy variable for ethnicity with non-Caucasian background as reference category for at least one foreign-born parent. To define the family compostion, we use the number of children in the family. Educational attainment of the parents is similarly specified as for the children. Family income is a categorical variable with nine monthly household income

classes, ranging from less than €680.67 (score 1), between €680.67 and €1,134.45 (score 2), ..., between €3,403.35 and €3,857.13 (score 8), to more than €3,857.13 per month (score 9). Futhermore, in the Netherlands, each municipality receives money for general purposes from the national "municipalities fund". Many municipalities (especially the larger ones) use this money for educational purposes. Children in the sample live in one of the following cities: Groningen, Leeuwarden, Assen, Winschoten, Dantumadeel, and Grijpskerk. We discerned a dummy variable with large municipality as the reference category for individuals from the cities Groningen, Leeuwarden and Assen.

3.2 Indication Process

Eligibility for additional budgets was assessed by the Committee of Indication of Assessment of the Dutch Ministry of Education, Culture and Science. We identified special needs students accordingly. Baseline mental health scores were discerned into normal-borderline-clinical cut-offs based on the validated Dutch norms of the ASEBA (Achenbach & Rescorla, 2013). We multiplied the original baseline score with 119. Subsequently, we followed the validated cut-offs for boys younger than twelve years old of scores below 39 ('normal'), between 39 and 48 (' borderline') and above 48 ('clinical'). The validated cutoffs for boys of twelve years or older are scores below 40 ('normal'), between 40 and 52 (' borderline') and above 52 ('clinical'). Similarly, we followed the validated cutoffs for girls younger than twelve years old of scores below 36 ('normal'), between 36 and 48 (' borderline') and above 48 ('clinical'). Finally, the validated cutoffs for girls of twelve years or older are scores below 36 ('normal'), between 36 and 44 (' borderline') and above 44 ('clinical'). Students with a score above the clinical cutoff ($N = 43$) were assumed eligible, which corresponds to 3.53 percent of the sample. This percentage is representative for national averages, as is illusttrated by Figure 2. Other students are classified as regular students, not eligible for student-bound funding. Baseline characteristics for both groups are provided in Table 1.

The sample of special needs students consists of less boys and children live more frequently in a larger municipality. Additionally, we observe that special needs students are from a similar ethnic and socio-economic environment as their regular peers.

Table 1: Baseline characteristics of regular and special needs students

	Regular students $N = 1,175$	Special needs students $N = 43$
Student		
Mental health (0-100)	88.41	77.95***
Age (10-12)	11.08	11.09
Males	0.48	0.39***
Non-Caucasian background	0.07	0.06
Family background		
Number of children (0-8)	2.53	2.61
Educational attainment mother (1-5)	3.14	3.12
Educational attainment father (1-5)	3.33	3.32
Family income (1-9)	4.85	4.80
Demographic		
Large municipality	0.82	0.87**

Note: */**/*** indicate significant differences at the 10%/5%/1% level based on the mean differences of the two groups, assessed with a t-test.

4 Empirical Strategy

We use a Differences-In-Differences (DID) design to control for time-invariant unobserved heterogeneity, enabling direct identification of the policy effect on the mental health of special needs students. Four elements are specifc for the DID setting: the first one is the availability of a treated ('eligible') group and control ('non-eligible') group; the second is the existence of common trends ('parallel paths') in the pre-policy period; the third is the clear time cutoff when the policy starts, hence the existence of a pre- and post-policy period; and fourth is the assumption that, with-out the policy, the treated group would show a trend similar to that observed for the control group.

As mentioned above, the student-bound funding was introduced 1 August 2003. In the pre-policy period ($t = 0$), waves 1 and 2 of TRAILS, students i can not make use of the policy, since it has not been implemented yet. In the follow-up period ($t = 1$), waves 3 to 5 of TRAILS, students could have benefited from the policy. Hence, the clear time cutoff is between the second and third wave. As discussed, the policy is targeted on special needs students for whom it is difficult to proceed in regular education. Therefore, special needs students are assigned to the treated group ($Z_i = 1$) and regular students to the control group ($Z_i = 0$). Furthermore, we assume absence of any intervention in the baseline for either group

$(D_{i,t=0} = 0|Z_i = 1, 0)$ and the policy to have a positive effect on the mental health of special needs students in the follow-up $(D_{i,t=1}|Z_i = 1)$.

For the outcome variable $(Y_{i,t})$ the population DID effect is then given by the mean difference in mental health for special needs students and regular students before and after the student-bound funding. The corresponding DID setting is given by

$$\begin{aligned} \text{DID} = &\big\{E(Y_{i,t=1}|D_{i,t=1} = 1, Z_i = 1) - E(Y_{i,t=1}|D_{i,t=1} = 0, Z_i = 0)\big\} \\ &- \big\{E(Y_{i,t=0}|D_{i,t=0} = 1, Z_i = 1) - E(Y_{i,t=0}|D_{i,t=0} = 0, Z_i = 0)\big\}. \end{aligned} \tag{1}$$

In the DID extended model we add the individual-level control variables (X_i) age, gender, ethnicity, family composition, educational attainment mother, educational attainment father, family income, and municipality.[4] The corresponding extended DID setting is given by

$$\begin{aligned} \text{DID} = &\big\{E(Y_{i,t=1}|D_{i,t=1} = 1, Z_i = 1, X_i) - E(Y_{i,t=1}|D_{i,t=1} = 0, Z_i = 0, X_i)\big\} \\ &- \big\{E(Y_{i,t=0}|D_{i,t=0} = 1, Z_i = 1, X_i) - E(Y_{i,t=0}|D_{i,t=0} = 0, Z_i = 0, X_i)\big\}. \end{aligned} \tag{2}$$

The regression formulation of the population DID in (2) is given by

$$Y_{ist} = \alpha + \beta \cdot T_{st} \cdot (T = 2003) + \delta_t + \boldsymbol{X}_{ist} \cdot \boldsymbol{\Gamma} + \varepsilon_{ist}, \tag{3}$$

where Y_{ist} denotes the mental health of student i for policy s in wave t, T_{st} presents the policy indicator in 2003, δ_t the time fixed effect, $\boldsymbol{X}_{ist}$ are the individual-level control variables, and ε_{ist} denotes the error term. The formulation in (3) enables to obtain the standard error and t-statistic of β.

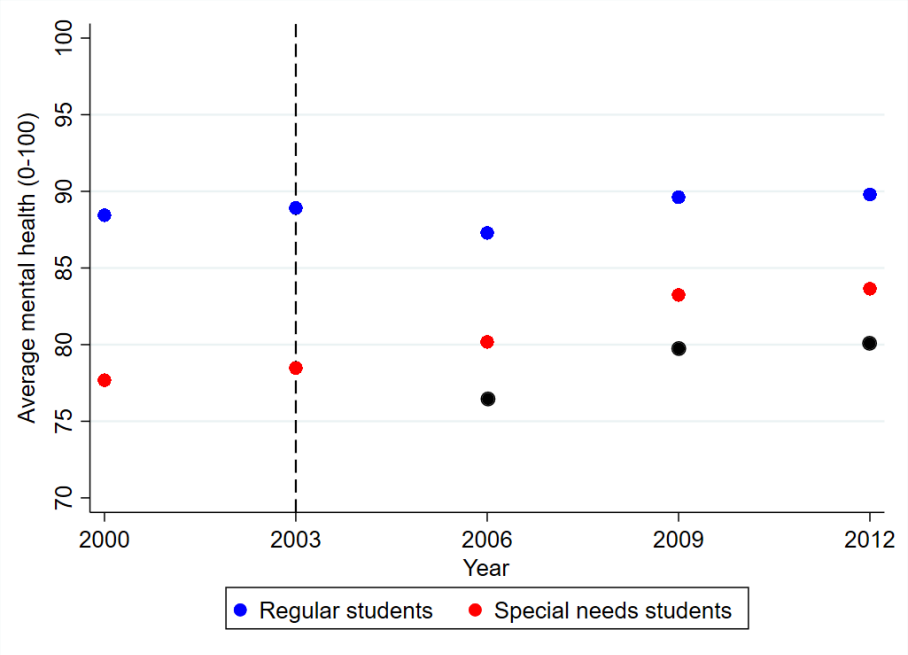
[4]Note that adding individual-level control variables only reduces the within-group variance. The within-group variation does not affect the identication of the policy effect but may reduce standard errors. Between-group variance becomes important when observed heterogeneity may confound the identification strategy. Given the features of our DID setting, observed covariates may play a role in the identification of the policy effect. Therefore, in Section 6.1 of the supplementary analyses we add non-experimental methods to the original DID setting to reduce the between-group variance. Specifically, we follow Heckman, Ichimura, and Todd (1997, 1998) and match the special needs students with their regular peers at baseline on the propensity of eligibility for the policy conditional on the control variables in (2), X_i. In the second stage we add the kernel weights obtained in the first stage to the DID setting to estimate the exempt policy effect.

5 Results

5.1 Parallel Paths

Figure 4 shows the average mental health of regular students (●) and special needs students (●) with the introduction of student-bound funding illustrated by the vertical dashed line. We observe both parallel paths before the policy introduction in 2003 and from 2006 after the policy effect ($p > 0.10$). Additionally, Ruijs (2017) demonstrated that regular students were not affected by the inclusion of special needs students. Hence, we are able to identify the counterfactual change over time of the mental health of special needs students if the policy had not occured (●).

Figure 4: Parallel paths in mental health



5.2 Policy Effect

Table 2 displays the standardized estimates of β in (3) with and without additional control variables, respectively. The results suggest that student-bound funding increased the mental health of special needs students with 0.41 SD.

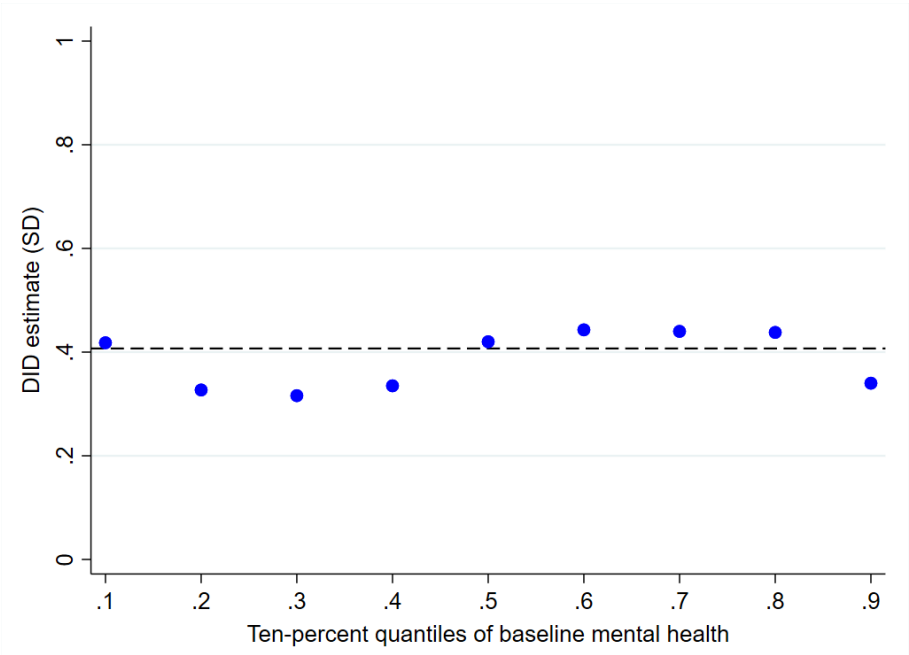
Table 2: DID estimates for mental health

	(1)	(2)
DID	0.414***	0.414***
	(0.041)	(0.028)
Covariates	NO	YES
Number of ID	1,218	1,218
R-squared	0.20	0.22

Clustered standard errors in parentheses.
Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

In Figure 5 we show the standardized estimates of β in (3) for ten-percent quantiles of baseline mental health with the main result of Table 2 as horizontal dashed line. The quantile DID estimates with t-statistic and standard error are presented in Table 8 in the Appendix. The policy yields an effect at the median of 0.42 SD. Furthermore, the policy was the most (least) effective at the sixth (third) quantile with an effect of 0.44 (0.32) SD.

Figure 5: Ten-percent quantile DID estimates



6 Supplementary Analyses

In the supplementary analyses we first demonstrate that estimates are robust to propensity reweighting taking into account between-group variance. Second, we show that although results differ cross broad-band syndromens, conclusions remain similar. Finally, we focus on the effect heterogeneity for different classes in society – girls and children from an ethnic minority or a lower socio-economic environment benefited less from the policy.

6.1 Propensity Reweighting

In the third specification we follow Heckman et al. (1997, 1998) to reduce the between-group variance. We use the observed covariates X_i of (2) to estimate the propensity score (the likelihood of indication to Student-bound Funding) and to calculate kernel weights. Instead of only accounting for the within-group variance, this method matches the special needs students and regular students according to their propensity score. First, we obtain the propensity score (p_i) for both groups using probit estimation,

$$p_i = E(Z_i = 1|X_i). \tag{4}$$

Then according to Heckman et al. (1997), the kernel matching is given by the propensity score conditional on the covariates of (2), which leads to the calculation of the kernel weights,

$$w_i = \frac{K\left(\frac{p_i - p_k}{h_n}\right)}{\sum K\left(\frac{p_i - p_k}{h_n}\right)} \tag{5}$$

in which $K(\cdot)$ is the kernel function and h_n is the selected bandwidth, set equal to 0.05. Then we introduce the kernel weights into (1) to obtain a kernel propensity-score matching DID effect. The corresponding DID setting is given by

$$\begin{aligned} \text{DID} = &\left\{E(Y_{i,t=1}|D_{i,t=1} = 1, Z_i = 1) - w_i \times E(Y_{i,t=1}|D_{i,t=1} = 0, Z_i = 0)\right\} \\ &- \left\{E(Y_{i,t=0}|D_{i,t=0} = 1, Z_i = 1) - w_i \times E(Y_{i,t=0}|D_{i,t=0} = 0, Z_i = 0)\right\}. \end{aligned} \tag{6}$$

To increase the internal validity of the DID estimand, we restrict (6) to the common support of the propensity score for special needs students and regular students, following Rosenbaum

and Rubin (1985). The common support is the overlap region of the propensity for both groups. This sample of i individuals is then restricted to the region defined as

$$\big(i : p_i \in [max\{min(p_i|Z_i = 1), min(p_i|Z_i = 0)\}, min\{max(p_i|Z_i = 1, min(p_i|Z_i = 0)\}]\big). \quad (7)$$

Then, we show that in absence of the policy, the outcome variable is orthogonal to the policy indicator given the set of covariates X_i. That is, we test the balancing property at baseline,

$$Y_{i,t=0} \perp Z_i|X_i. \quad (8)$$

The kernel propensity-score matching DID estimate is given in column (3) in Table 3. According to this estimate, student-bound funding increased the mental health of special needs students with 0.39 SD. Although this estimate is slightly lower than the estimate of our main result of 0.41 SD, estimates are not statistically significantly different ($p > 0.10$).

Table 3: Kernel propensity DID estimates

	(1)	(2)	(3)
DID	0.414***	0.414***	0.394***
	(0.041)	(0.028)	(0.043)
Covariates	NO	YES	YES
Kernel weights	NO	NO	YES
Number of ID	1,218	1,218	1,218
R-squared	0.20	0.22	0.22

Standard errors in parentheses.
Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

In Table 9 in the Appendix we show that in absence of the policy, the outcome variable is, indeed, orthogonal to the policy indicator, as on all included covariates the difference between the treated and control group is statistically insignificant ($p > 0.10$).

6.2 Broad-band Syndromes

The scores for mental health can be split in the broad-band syndromes internalizing and externalizing socio-emotional skills (Achenbach & Rescorla, 2013; Attanasio, Blundell, Conti, Mason, et al., 2018). The former (latter) is more present in mental health problems such as mood disorders (conduct disorders). Results in Table 4 suggest a larger policy effect for special needs students' internalizing socio-emotional skills with a difference of 0.15 SD with their externalizing socio-emotional skills.

Table 4: DID estimates for broad-band syndromes

	Internalizing	Externalizing
DID	0.405***	0.253***
	(0.047)	(0.048)
Covariates	YES	YES
Number of ID	1,218	1,218
R-squared	0.22	0.12

Clustered standard errors in parentheses.
Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

6.3 Effect Heterogeneity

In this section we determine the effect heterogeneity, as the main result in Table 2 only provides the mean DID effect for the entire sample. Results are provided in Tables 5, 6 and 7 for decomposition by gender, socio-economic classes and ethnicity, respectively. We show estimates for both nested models and decomposed samples (Abadie, Chingos, & West, 2018).

Table 5: DID Estimates for females and males

	Full	Females	Males
DID	0.414***	0.390***	0.466***
	(0.041)	(0.060)	(0.069)
DID × Males	0.076***		
	(0.011)		
Covariates	YES	YES	YES
Number of ID	1,218	693	525
R-squared	0.22	0.22	0.19

Clustered standard errors in parentheses.
Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 6: DID estimates for socio-economic classes

	Full	Low SES	Middle SES	High SES
DID	0.414***	0.335***	0.410***	0.470***
	(0.042)	(0.104)	(0.060)	(0.066)
DID × Low SES	-0.135***			
	(0.029)			
DID × Middle SES	-0.060***			
	(0.040)			
Covariates	YES	YES	YES	YES
Number of ID	1,218	202	618	398
R-squared	0.22	0.21	0.21	0.21

Clustered standard errors in parentheses.
Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 7: DID estimates for caucasian and non-Caucasian students

	Full	Caucasian	Non-Caucasian
DID	0.414***	0.419***	0.323**
	(0.041)	(0.047)	(0.182)
DID × Non-Caucasian	0.096**		
	(0.072)		
Covariates	YES	YES	YES
Number of ID	1,218	1,142	76
R-squared	0.22	0.21	0.31

Clustered standard errors in parentheses.
Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

The estimates in Table 5 suggest that males benefited more from the policy with a relative difference with females of 0.076 SD. In Table 6 we use a validated categorical variable to define the student's Socio-Economic Status (SES): 25% low SES, 50% middle SES, and 25% high SES.[5] The DID estimates demonstrate that students from a high SES background capitalized to a greater extent from the policy than their lower SES peers. Specifically, the relative difference between high SES students and their low SES and middle SES peers is 0.14 and 0.06 SD, respectively. Finally, the estimates in Table 7 suggest that students with at least one parent foreign born benefited less from the policy with a relative difference with their peers from both parents born in the Netherlands of 0.10 SD.

7 Concluding Remarks

This article contributes to the discussion of the effect of school resources on student outcomes. We estimated the causal impact of a Dutch policy measure (student-bound funding) that provided regular primary and secondary schools additional budgets for special needs students. The results suggest that the policy measure improved their mental health with 0.41 SD.

To reduce the probability of placebo DID effects, we controlled for parallel paths before policy introduction. As illustrated in Figure 4, both groups have common trends before the

[5]The SES variable is derived with a factor analysis on the control variables educational attainment mother, occupation mother, educational attainment father, occupation father, and family income.

introduction of the policy measure in 2003 and from 2006 after the policy effect. Additionally, Ruijs (2017) demonstrated that regular students were not affected by the inclusion of special needs students. This allows us to identify the counterfactual change over time of the mental health of special needs students if the policy had not occured. Therefore, we can conclude that student-bound funding increased the mental health of special needs students in the post-policy phase with 0.41 SD.

Chetty, Friedman, and Rockoff (2014) suggest that students with a high SES can capitalize on the long-term effects of education to a larger extent relative to their low SES peers. Our heterogeneity analyses confirm his cautionary note, as we found larger effects for students from more privileged backgrounds than for their less privileged peers. Specifically, we observe a difference between the lowest and highest SES class of 0.14 SD. Additionally, the difference between students with Dutch-born parents with peers with a foreign-born parent is 0.10 SD.

In this study we extensively controlled for selection bias. Specifically, by using non-experimental methods as additional specification to the original DID setting in the supplementary analyses, we exempted the observed covariates from the policy effect. We relate our choice for control variables to previous findings in the literature, as socio-economic environment of living (captured by our control variables educational attainment mother, educational attainment father, family income, and municipality) affects the lifespan of children (Chetty, Hendren, & Katz, 2016). Chetty et al. (2016) found that moving to a lower-poverty neighborhood when young increases college-attendance and earnings. This difference in opportunity is endogenous in our kernel propensity-score matching DID setting. Specifically, we show that in absence of the policy, the outcome variable mental health is orthogonal to the policy indicator given the region of living. The extended specification yields a reduction of the DID estimate of 0.02 SD, which indicates that the original DID setting is robust to selection bias.

The Dutch Ministry of Education, Culture and Science decided in July 2014 to abolish the Student-bound Funding and decided to cut expenditures in the subsequent policy with 300 million euros. This study looks back on this decision and shows the potential of additional budgets to reduce mental health inequality in students, as it substantially lowered the difference in mental health of special needs students with their regular peers.

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Appendix

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B Additional Figures and Tables

Table 8: Quantile DID estimates for mental health

Quantile	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
DID	0.418***	0.327***	0.316***	0.335***	0.420***	0.443***	0.440***	0.438***	0.340***
	(0.087)	(0.072)	(0.066)	(0.063)	(0.060)	(0.062)	(0.055)	(0.056)	(0.047)
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
Number of ID	1,218	1,218	1,218	1,218	1,218	1,218	1,218	1,218	1,218
R-squared	0.16	0.16	0.16	0.15	0.13	0.12	0.10	0.08	0.06

Standard errors in parentheses.
Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 9: Results balancing test with weighted covariates at baseline

Weighted variable	$\Pr(\lvert T\rvert > \lvert t\rvert)$	Mean control	Mean treated	Diff	$\lvert t\rvert$
Mental health	0.0000***	88.772	78.369	-10.402	30.16
Age	0.8201	10.58	10.57	-0.009	0.23
Males	0.5453	0.409	0.391	-0.018	0.61
Non-caucasian background	0.9177	0.060	0.059	-0.002	0.10
Number of children	0.9508	2.605	2.608	0.004	0.06
Educational attainment mother	0.7047	3.166	3.121	-0.045	0.75
Educational attainment father	0.4522	3.326	3.321	-0.005	0.07
Family income	0.9411	5.108	4.802	-0.306	0.20
Municipality	0.9954	2.275	2.247	-0.029	0.38

Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

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