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IZA DP No. 17631

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ABSTRACT

More than Just Lunch: School-Meal Subsidies and Language Proficiency*

This paper is the first to provide, in the European context, credible causal estimates for the impact on educational achievement of a means-tested programme that subsidises school lunches. We use administrative data from the city of Barcelona for the whole universe of applications to the programme. Using a Regression Discontinuity Design, we measure the effect of receiving a partial subsidy, as opposed to none; meanwhile a Difference-in-Differences (DiD) strategy allows us to account for the effect of receiving a full subsidy, compared to a partial one. Our results indicate an overall positive effect of the subsidies on educational achievement across all the subjects analysed, with statistically significant estimates only for Catalan language. Heterogeneous results show that those who benefit most are boys who attend large schools and have peers who are, on average, more socio-economically advantaged. The opportunity for subsidy recipients to spend more time at school during lunch, and to communicate and socialise with wealthier and linguistically more competent children is the mechanism that lies behind our main findings.

JEL Classification: H42, H52, I38

Keywords: school meals, subsidies, means-tested programmes, children, educational achievement, language proficiency, standardised test scores, annual grades

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1 Introduction

Over a million primary-school pupils in Spain eat lunch daily in the school canteen for between €4 and €7, depending on the region (Ministerio de Educación, Formación Profesional y Deportes, 2023). Participation in the lunch service is biased towards children from more advantaged backgrounds, while those more economically disadvantaged may be left out because of affordability problems (Educo, 2022). This is of considerable importance in a context where three children in 10 live below the poverty line, according to the latest wave of the European Union — Statistics on Income and Living Conditions (Eurostat, 2022); 5.9% of children under the age of 16 cannot afford a meal with meat, chicken or fish (or a vegetarian equivalent) at least every second day; and nearly 8% depend on a non-governmental organisation (NGO) for food, clothes or basic goods (Eurostat, 2020).[1]

School-meal programmes have the potential to play a vital role in multiple domains of child well-being. By ensuring the nutritional intake of children and reducing their risk of malnutrition, school meals can have important effects both on health outcomes and on educational performance and personal behaviour. School-meal programmes can also enable students to develop social and interpersonal skills, as they allow pupils more time in school, sharing a meal with their peers from various socio-economic and cultural backgrounds. Appropriately, such programmes are more and more regarded as an integral component of the broader educational curriculum (Guio, 2023). Furthermore, one should not forget the potential effect of school-meal programmes on parents, as they facilitate a better work-family balance and can promote local employment. Addressing gaps in school-meal participation is therefore of vital importance for both children and parents, particularly in the context of high poverty rates.

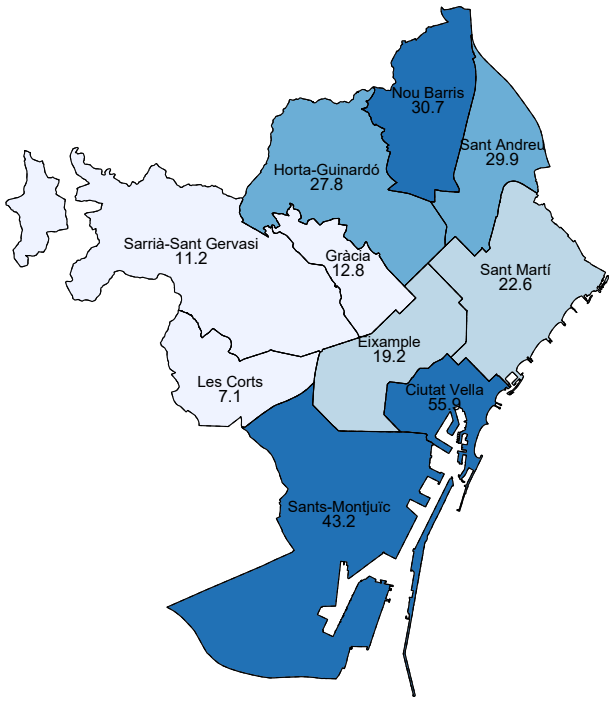
Despite the potentially large gains that can be derived from school-meal programmes — and even though they have been in place for decades in many countries — there is a dearth of studies that provide credibly causal estimates for the efficiency and efficacy of these programmes, particularly in Europe. Previous literature has encountered two main hurdles in attempting to evaluate the causal impact of school-meal programmes on child outcomes. First, the design of these programmes has often lacked the necessary quasi-experimental variation required to estimate their causal impact credibly. Second, data that links school-meal participation and child outcomes (including other socio-demographic variables) has rarely been available to researchers (Schwartz and Rothbart, 2020). That said, recent methodological advances and greater data availability are currently contributing to the growth of a body of literature that aims to measure the impact of school-meal programmes on child outcomes and then to provide recommendations for better policy design. This paper enriches that strand of literature by undertaking the first rigorous evaluation of a European means-tested school-meal subsidies programme — in the city of Barcelona.

As a case study, Barcelona is important and interesting. There are 238,000 children below the age of 18 in the city, living in 10 districts with very uneven socio-economic development. A quick glance at the data on the percentage of pupils receiving school-meal subsidies highlights stark differences between districts — see Figure 1. For example, while only 7.1% of pupils enrolled in the Les Corts district receive the school-meal subsidy, that figure rises to 55.9% in Ciutat Vella. These differences may hide important

[1] Spain ranks as the country with the third-highest child poverty rate in Europe, after Romania and Bulgaria (Eurostat, 2022).

educational inequalities and underscore the potential of a school-meal programme to address them. A combination of factors — the access we have to unique administrative records on programme applications, merged with data on educational outcomes and a survey on well-being at school; and the design of the programme, which allows the use of a Regression Discontinuity Design (RDD) and a Difference-in-Differences strategy — renders Barcelona an excellent setting in which to evaluate the effectiveness of means-tested school-meal subsidies in the European context.

Figure 1: Ratio of school-meal recipients to the total number of pupils enrolled, districts of Barcelona, academic year 2021/2022



Source: Authors' computation, using data from the Barcelona Education Consortium and the Barcelona City Council.

Our main results document positive effects for students near the eligibility cutoff who receive a partial subsidy (70% of the cost of the daily menu), compared to their peers who have family income near the eligibility threshold but who do not benefit from the subsidy. However, we only find statistically significant effects in the standardised test scores for Catalan (the language of instruction at school) — not for the rest of the subjects (Spanish, English, Maths and Science). Subgroup analyses indicate that those that benefit the most from the partial subsidy are boys who attend large schools and have peers who are, on average, more socio-economically advantaged. We also find better annual grades in the Catalan language in those academic years when a child benefits from a full subsidy (100% of the cost of the daily menu), as opposed to those years when the child receives only the partial subsidy. Our section on 'Mechanisms' shows that the school-meal subsidies not only provide low-income children with a meal, but also offer them the opportunity to spend additional, joyful free time at school, where they can communicate, socialise and interact with wealthier and linguistically more competent children, which helps them

improve their Catalan language proficiency.

We contribute to two main strands of literature. First, we expand the body of research into the impact of school-meal programmes on academic achievement by adding to the relatively sparse literature on the impact of means-tested programmes (an aspect that has been overshadowed by the more extensive research into universal free meals). We also employ an RDD approach, a methodology that has seldom been used in this particular field of research; and we broaden the geographical scope by providing the first evaluation of a means-tested school-meal subsidies programme in Europe (most prior studies on developed economies have focused on the US). Second, this is the first study to show that school-meal programmes have an effect on educational outcomes by allowing additional time to be spent in school — something which, in itself, can help improve educational outcomes (Dominguez and Ruffini, 2023; Lavy, 2019; Andersen et al., 2016); by enhancing subjective well-being during the school day (Agüero et al., 2021); and most importantly, by providing an opportunity to communicate and socialise with peers in the language of instruction at school (Coleman, 1968; Manski, 1993). And not only that: for children from less affluent backgrounds, being given the opportunity to have lunch together with their wealthier peers also forges friendships that can enhance long-term life outcomes. Economic connectedness among individuals from different socio-economic backgrounds has been shown to be one of the strongest predictors of upward income mobility (Chetty et al., 2022a, 2022b).

This paper is organised as follows. After this introduction, Section 2 offers a comprehensive literature review of experimental and quasi-experimental studies that examine the effects of school meals on children's educational outcomes. Section 3 presents an overview of the institutional setting and provides details of the subsidies programme. Information on the data is supplied in Section 4, and the empirical strategy is outlined in Section 5. Section 6 details the results and presents various robustness checks. The mechanisms that help understand our findings are discussed in Section 7. Finally, Section 8 offers a conclusion and provides a back-of-the-envelope cost-benefit analysis of the subsidies programme.

2 Literature review

In what follows, we review studies that provide reliable estimates for the causal impact of school-meal programmes on educational outcomes, including school attendance (absenteeism) and academic achievement.[2] In doing so, we consider articles that use methods which focus explicitly on causal inference — either experimental or quasi-experimental. We also limit this literature review to developed economies, in order to be in a better position to compare our results with those of previous studies.

[2]The two other outcomes that have been the primary focus of research in this strand of literature are student behaviour and children's health. Regarding behaviour, the findings are mixed: while several studies show an improvement (Domina et al., 2024; Cuadros-Meñaca et al., 2023; Gordon and Ruffini, 2021; Altindag et al., 2020; Norwood, 2020; Kho, 2018), others find no link between programme participation and better behaviour (Schanzenbach and Zaki, 2014; Dunifon and Kowaleski-Jones, 2003). Similarly, studies on health — particularly those focused on obesity — present conflicting findings: some document no effect (Abouk and Adams, 2022; Schwartz and Rothbart, 2020; Corcoran et al., 2016); others suggest that school meals increase obesity (Millimet et al., 2010; Schanzenbach, 2009); and a further group of studies reports reduced obesity (Davis et al., 2024; Holford and Rabe, 2024, 2022; Gundersen et al., 2012). See Ayllón and Lado (2024) for a meta-analysis covering all this literature.

There is a lack of consensus among studies regarding the causal relationship between participation in school-meal programmes and school attendance. Part of the literature has documented positive effects: for instance, Meyers et al. (1989), when examining the implementation of the School Breakfast Program (SBP); Anzman-Frasca et al. (2015) and Kirksey and Gottfried (2021), when studying the Breakfast In the Classroom (BIC) and the Breakfast After the Bell (BAB) initiatives; and Gordanier et al. (2020), when focusing on the Community Eligibility Provision (CEP) programme.[3] However, there is also a substantial body of research suggesting that school meals have no effect on school attendance — least of all in developed economies, where attendance rates are already high. For example, in the US, Leos-Urbel et al. (2013), Corcoran et al. (2016), Imberman and Kugler (2014) and Cuadros-Meñaca et al. (2022) find no link between school-meal participation and attendance. Outside the US, neither Bütikofer et al. (2018) in Norway nor McEwan (2013) in Chile documents any significant impact.

Regarding educational achievement, the largest body of research has found that participation in school breakfast and lunch programmes can help pupils improve their scores. Meyers et al. (1989) and Frisvold (2015) find this to be the case when they analyse the SBP; Imberman and Kugler (2014) and Dotter (2013) concur in their studies of the introduction of the BIC and BAB programmes; and the same goes for Ruffini (2022), Norwood (2020), Gordanier et al. (2020) and Schwartz and Rothbart (2020), when they focus on extending a given school-meal programme to all pupils. Moreover, a number of studies have found that disadvantaged or low-performing students are those that benefit the most (Ruffini, 2022; Imberman and Kugler, 2014; Dotter, 2013). Additionally, not all the impacts have been found to be short term: both Hinrichs (2010) and Lundborg et al. (2022) show that school-meal programmes increased the number of years of completed education in the US and Sweden, respectively. Positive effects have also been documented when, instead of changing the coverage of a programme or the mode of delivery, an intervention improves the quality of the meals provided. In this respect, Anderson et al. (2018) and Belot and James (2011) show that healthier meals resulted in improved test scores.

While one can find more studies that document a positive causal link between school-meal programmes and educational achievement, there are also analyses that find no statistically significant effects. Leos-Urbel et al. (2013) and Corcoran et al. (2016) report null findings when they examine the impact of universal free breakfasts on test scores. In the case of Abouk and Adams (2022), their analysis of the BAB programme yields imprecisely estimated coefficients. Dunifon and Kowaleski-Jones (2003) report no significant effect for the National School Lunch Program (NSLP), which provides reduced-price or free lunches following the same criteria as the SBP. Similarly, McEwan (2013) finds that higher-calorie meals in Chilean public and rural schools did not affect test scores.

All in all, the existing studies have reached no consensus regarding the causal impact of school-meal programmes on children's educational outcomes. The results seem to depend greatly on the type of programme analysed and the context in which it is implemented. Differences in the school systems being studied, in the specifics of each programme (e.g.

[3]In the US, the School Breakfast Program (SBP) provides reduced-price breakfasts for pupils from families with an income of between 130% and 185% of the federal poverty line, and free breakfasts for those from a family with income of below 130% of that benchmark. Breakfast After the Bell (BAB) initiatives, including 'Breakfast In the Classroom', 'grab-and-go' and 'second chance', offer free breakfasts after the school day begins. The Community Eligibility Provision (CEP) extends free meals to all students in a given school or district where at least 25% of students — 40% before October 2023 — receive income-based assistance, such as under the Supplemental Nutrition Assistance Program (SNAP).

the dietary content of the meals) and, most importantly, in the levels of socio-economic development may explain the diversity of findings in this strand of the literature. By focusing on the case study of the city of Barcelona, we provide fresh evidence on (the barely studied) means-tested programmes and broaden the geographical scope to Europe.

3 Institutional setting: the subsidised school-meal programme in Barcelona

In Barcelona, as in the rest of the Catalan region, a typical school day starts around 8:30–9:00 and ends at about 16:30–17:00. Shortly after noon, children take a 2–2.5 hour break for lunch, which they can either have at home or in school (for a charge). Normally, lunch takes about an hour, while the rest of the time is free for children to play on the school premises. Each academic year, the Department of Education of the Catalan government sets a maximum price for the daily menu: for example, in the final year of our analysis (academic year 2021/2022) it cost €6.33. This amounts to about €1,100 per academic year — or nearly 3% of the total average household income for a family with one child.

In order to help low-income families, means-tested programmes that subsidise school lunches run throughout the territory. In the particular case of Barcelona, the programme is designed and run by the Barcelona Education Consortium. Figure 2 shows a significant increase in recent years in the proportion of students applying for and receiving subsidised school meals, relative to the total number of pupils enrolled. Despite this increase, the programme's coverage — the ratio of recipients to applicants — has remained relatively stable, at over 80%. In the 2021/2022 academic year, of 48,259 applicants, 40,251 received the subsidy.[4] In the last year of our analysis, the total budget for the programme was €35 million.

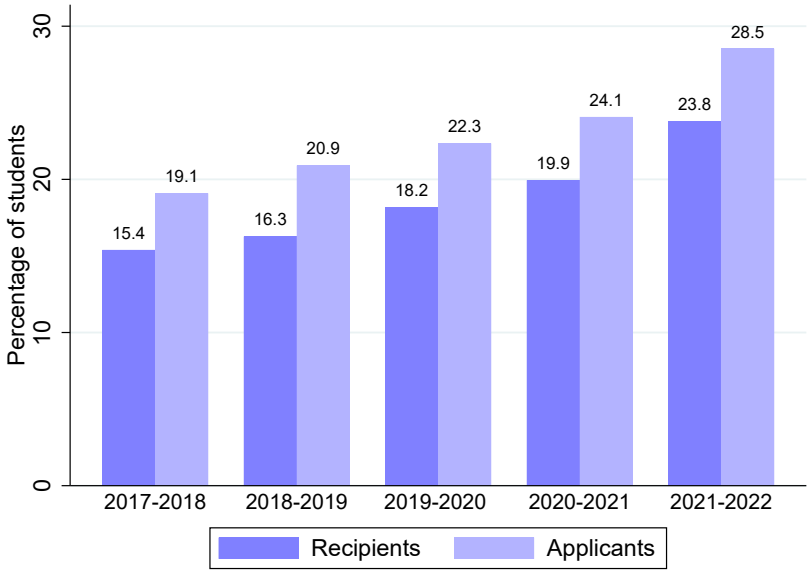
In Barcelona, programme eligibility is decided mainly on the basis of household income. There are other requirements (such as not exceeding specified amounts for business turnover, capital gains or property value), but according to the data at our disposal, 96.2% of applications are decided solely on the basis of household income, since applicant families rarely have sources of wealth other than current income. Each academic year, the income eligibility threshold is defined, as are the amounts of the subsidy. To compute the threshold, family structure is taken into account. Thus, for example in academic year 2021/2022, the first adult in the household accounted for €10,981.40; the second adult for €5,490.80; any other adult member for €2,745.35 each; and each child in the family unit for €3,294.45.[5] For example, a family with two parents and two school-aged children would have a reference threshold of €23,061.10.

Regarding subsidy amounts, families with household income below the eligibility threshold receive a *partial* subsidy: 50% of the cost of the school meal up to academic year 2019/2020 and 70% from academic year 2020/2021 onwards. Consequently, children from those families either need to go home for lunch several times a month, or else the family has to cover the rest of the cost itself. In order to get a *full* subsidy (100% of the cost

[4]We estimate that in 2022, approximately 87% of children living below the poverty line in Barcelona received the subsidy. The remainder were probably left out because of problems with uptake. For example, a survey by the Spanish NGO Educo concludes that about 11% of eligible children do not apply because of a lack of awareness or because their families miss the application deadline (Educo, 2022).

[5]Table A.1 in the Appendix provides full details of the eligibility criteria in academic years 2017/2018 to 2021/2022. Figure A.1 describes both the application procedure and the different decision stages leading up to a decision on whether or not to grant the subsidy.

Figure 2: Ratio of school-meal recipients and applicants to the total number of pupils enrolled, Barcelona, academic years 2017/2018 — 2021/2022



Source: Authors' computation, using data from the Barcelona Education Consortium and the Barcelona City Council.

of the school meal), a family needs to meet two criteria: (1) have total family income below 60% of the reference threshold; and (2) have received a minimum of 10 points (out of a possible 15) in a so-called *family circumstances and social needs assessment* by the Barcelona Municipal Institute of Social Services. The criteria used for such an assessment include: the family being a large one, the family having only a single parent, the child being in foster care, the child being at social risk, etc. In our example, a family with two adults and two children would receive a partial subsidy if annual household income is below €23,061.10, and the full subsidy if household income is below €13,836.66 and if the family has obtained 10 points in the evaluation made by Social Services.[6]

There are two additional features that need to be taken into consideration in our analysis. First, any student whose family receives direct cash benefits from the state (specifically, the Minimum Income or the Guaranteed Citizenship Income) or is enrolled in some other programme (such as the Social Emergency Fund or the Shock Plan Against School Segregation), or who requires specific educational support for socio-economic reasons, is automatically entitled to the full subsidy.[7] Second, although it is the family that

[6]In the case of children with a disability, the eligibility threshold is multiplied by 2.5 and they are entitled to receive the full subsidy even if they do not have 10 points in the *family circumstances and social needs assessment*.

[7]Minimum Income was a non-contributory benefit aimed at preventing the risk of poverty and social exclusion among people who lacked the basic economic resources to cover their needs. In 2017, it was replaced by the so-called Guaranteed Citizenship Income. The Social Emergency Fund provides financial assistance to families with dependent children aged 0–16 through an electronic benefits transfer card for essential expenses. The Shock Plan Against School Segregation aims to ensure equitable student distribution and to prevent the concentration of socio-economically disadvantaged students in specific schools. It also provides eligible students with free access to educational activities and services.

applies for the benefit, it does not actually receive the money itself: the subsidy is paid each month direct to the schools, which then do not bill parents for the meals.[8] Note that, under this system, stigma is virtually eliminated. Families that do not receive the subsidy but still use the lunch service pay via monthly transfers, ensuring a discreet payment process. In the day-to-day lives of the children, there is no discernible difference between a child who receives the subsidy and one who does not.

4 Data

Our main dataset contains the whole universe of applications to the school-meal subsidies programme in Barcelona from academic years 2017/2018 to 2021/2022. We focus on pupils in primary school. The data is organised longitudinally and is at the student level. It contains information on pupils' gender, age, date of birth, country of origin, postcode of residence, household composition (number of adults and children), whether the child has special educational needs, course level, school attended, the school's level of complexity, distance (in euros) to the income eligibility threshold and whether the child has been granted the subsidy or not.[9] Importantly, the school's 'level of complexity' is a classification undertaken by the Department of Education, via Principal Component Analysis (PCA), that considers several factors: the educational level and occupation of parents and their immigrant origin, as well as the students' immigrant background and special educational needs. Schools are categorised into five groups: low, medium-low, medium-high, high and very high complexity.[10] Thus, while the low-complexity category covers schools whose children are drawn from more advantaged backgrounds, highly complex schools are faced with multiple sources of disadvantage.

In our analysis, we use three additional sources of data; these were merged with our main database on subsidy applications.[11] First, given that our dataset contains information on the school attended by a student, we can include additional information at the school level from publicly available databases from the Department of Education: in particular, the number of male and female students enrolled at each course level, the school address, and whether the school is public or semi-private.[12] Second, and in order to evaluate the impact of the school-meal subsidies on educational achievement, our records were merged with the results of exams that were uniformly administered to all students at level 6 of primary education (before the move to high school) for all years except 2019/2020 (when the exam was cancelled due to the coronavirus pandemic). These test scores range from 0 to 100 and serve as valuable indicators of academic proficiency in the Catalan language, Spanish, English, Maths and Science. While analysis of these scores is interesting (since all students take the exact same exam on the same day), it does have

[8] Schools have the autonomy either to manage the provision of meals themselves or to contract an outside company. Additionally, at the end of the academic year, schools are required to account to the Barcelona Education Consortium for the funds allocated for school-meal subsidies. If the funds are not administered correctly, the schools must return them to the Barcelona Education Consortium.

[9] Children with special educational needs are defined as children whose learning difficulties impair their ability to benefit from the general educational system. This would include (among other things) physical and intellectual disabilities and behavioural disorders.

[10] We use the school classification undertaken in the first year of our analysis (2017).

[11] The process of merging multiple databases was conducted by the Barcelona Education Consortium. The data provided to us has been completely anonymised, ensuring the privacy and confidentiality of the individuals concerned.

[12] Semi-private schools are partially subsidised by the state.

the disadvantage of providing a relatively small sample size, as each student only takes the exam once during primary school. For this reason, we also use student-level data on annual grades for the same subjects: researchers have access to this information only for public schools and only for academic years 2019/2020 through 2021/2022. In this case, student achievement is graded using the following scale: 1 indicates 'fail', 2 'satisfactory', 3 'good' and 4 'excellent'. Although in this instance, pupils do not take the same exam, the scores proxy student achievement within a school and provide us with a much larger sample. Third, we merge our data on school-meal subsidy applications with information from the 2021 Survey of Subjective Well-being of Children in the city of Barcelona, conducted by the Barcelona City Council and the *Institut Infància i Adolescència de Barcelona*. This survey asks children between the ages of 10 and 12 across 52 schools in Barcelona about their daily routine and activities; how they allocate their time; and to what extent they agree with certain specific statements. It also gathers information about their socio-demographic and economic background. Of particular interest in our context is the level of satisfaction expressed by children (on a scale of 0 to 10) with their life, friends, peers, student life and their learning at school. This survey data will be useful in our section on 'Mechanisms'.

Table 1: Summary statistics for all applicants to the school-meal programme, Barcelona, academic years 2017/2018 — 2021/2022

	Mean	Std. Dev.	Min.	Max.
Panel A: Student characteristics				
Age	8.48	1.73	6	12
Month of birth	6.71	3.44	1	12
Female	0.49	0.50	0	1
Immigrant origin	0.19	0.39	0	1
Special educational needs	0.38	0.48	0	1
Single-parent household	0.12	0.32	0	1
Large family (+3 children)	0.22	0.41	0	1
Household size	3.64	1.19	1	13
1st grade	0.17	0.38	0	1
2nd grade	0.17	0.38	0	1
3rd grade	0.17	0.38	0	1
4th grade	0.17	0.37	0	1
5th grade	0.16	0.37	0	1
6th grade	0.15	0.36	0	1
70% subsidy	0.51	0.50	0	1
100% subsidy	0.32	0.47	0	1
No subsidy	0.17	0.37	0	1
Panel B: School characteristics				
Public provision	0.52	0.50	0	1
School size	245.91	130.30	50	962
School complexity: Low	0.31	0.46	0	1
School complexity: Medium-low	0.29	0.45	0	1
School complexity: Medium-high	0.19	0.39	0	1
School complexity: High	0.12	0.33	0	1
School complexity: Very high	0.10	0.30	0	1

Note: The unit of analysis in Panel A is each student, while in Panel B it is each school. The total number of observations is 87,221, covering 41,566 pupils in 349 schools.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

In total we have 87,221 observations for all applications whose eligibility can be determined by household income. Therefore, we exclude applications that are deemed ineligible because of the other requirements (about 3.8% of the total sample) and those from disabled children, as their eligibility threshold is totally different from the rest (about 0.35% of the total sample). Regarding the student characteristics of all applicants to the programme shown in Table 1 (Panel A), average age is 8.48 years, 49% of applicants are female and 19% have an immigrant background. About 38% of the students have special educational needs. Concerning family structure, 12% of the children live in a single-parent household and 22% in a large family. Mean household size is 3.64. Observations are equally distributed across course grades. Around 51% of the applicant children receive the partial subsidy, while 32% are awarded the full subsidy. Approximately 17% of applications are not granted. In terms of school characteristics (Panel B), half of the schools are public, with an average school size of about 246 pupils. Additionally, approximately 31% of schools are classified as having a low level of complexity; meanwhile 29% have a medium-low complexity, 19% — medium-high, 12% — high and 10% — very high.

Summary statistics for student achievement and for indicators of subjective well-being are presented in Table 2. In terms of educational achievement (Panel A), test scores vary across subjects, with Spanish scoring highest (69.59) and Science scoring lowest (66.19). Conversely, for annual grades, while Catalan has the lowest average score (2.31), Science achieves the highest average score (2.60). As for subjective well-being, students generally report high levels of satisfaction with the different aspects of their lives. Overall life satisfaction stands at 8.31, while satisfaction with friends is notably higher, at 9.03. Satisfaction with peers, student life and learning also indicates contentment, with mean scores of above 8.

5 Identification strategy

The particular features of the school-meal benefits programme in Barcelona presented above allow us to employ quasi-experimental techniques that can identify the causal effect of the subsidies on academic achievement. Our first approach involves the use of a sharp Regression Discontinuity Design (RDD) to examine the causal effects of the partial subsidy, as its eligibility criterion relies on family income. In the second approach — and given that the eligibility criteria of the full subsidy take account not only of family income, but also of specific family circumstances and the evaluation of social needs — we use Difference-in-Differences (DiD). In this case, we leverage the longitudinal component of our database to assess the within-student effect of receiving the full subsidy, as opposed to the partial one.

Regression Discontinuity Design

Regarding the RDD, our identifying assumption is that, provided families do not manipulate their earnings in order to become eligible, household incomes around the cutoff point are as good as random, as is eligibility near the threshold for a partial subsidy. Our running variable is therefore household income — in particular, the distance in euros to the eligibility threshold defined by the programme. We obtain intention-to-treat (ITT) effects, as we do not observe whether children actually use the lunch service —

Table 2: Summary statistics for student achievement and indicators of subjective well-being, Barcelona, academic years 2017/2018 — 2021/2022

	Mean	Std. Dev.	Min.	Max.	Observations
Panel A: Educational achievement					
Test scores					
Catalan	68.16	16.31	0	100	8094
Spanish	69.59	15.37	5	100	8379
English	69.18	20.06	0	100	8304
Maths	69.33	18.33	0	100	8290
Science	66.19	18.08	0	100	8163
Annual grades					
Catalan	2.31	0.77	1	4	45151
Spanish	2.46	0.76	1	4	45144
English	2.43	0.82	1	4	45139
Maths	2.42	0.84	1	4	45154
Science	2.60	0.76	1	4	45148
Panel B: Subjective well-being					
Satisfaction: life	8.31	1.93	0	10	990
Satisfaction: friends	9.03	1.55	0	10	992
Satisfaction: peers	8.27	1.74	0	10	992
Satisfaction: student life	8.54	1.66	0	10	991
Satisfaction: learning	8.40	2.32	0	10	983

Note: Data for test scores is available for academic years 2017/2018 to 2021/2022, except for 2019/2020 (when the exam was cancelled due to the coronavirus pandemic). As for annual grades, data is available for academic years 2019/2020 to 2021/2022 and only covers public schools. Information regarding subjective well-being is only available for the year 2021.
Source: Authors' computation, using data from the Barcelona Education Consortium, the Department of Education and the 2021 Survey of Subjective Well-being of Children in the city of Barcelona.

only whether or not they receive the subsidy.[13]

We estimate the following equation:

$$Y_{it} = \alpha + (-1)\beta T_{it} + \gamma_1 f(d) + \gamma_2 (f(d) \times T_{it}) + \delta X_{it} + \epsilon_{it} \quad (1)$$

where Y_{it} is the outcome of interest for child i in academic year t (for example, the standardised test score for Maths in academic year 2018/2019); T_{it} is an indicator for whether the child is granted the subsidy or not, and f is a first-order polynomial in the running variable (d), which is the equivalent income distance to the threshold in euros normalised to zero at the eligibility cutoff.[14] Students with family income below the threshold (and therefore with a negative income distance to the threshold) are eligible for the partial subsidy, while those with income above the eligibility threshold (i.e. with a positive income distance to the cutoff) are not. To ease interpretation, we multiply β by -1 so that a positive value directly reflects the positive impact of the subsidy. X_{it} is a vector of control variables including age, month of birth, gender, immigrant origin,

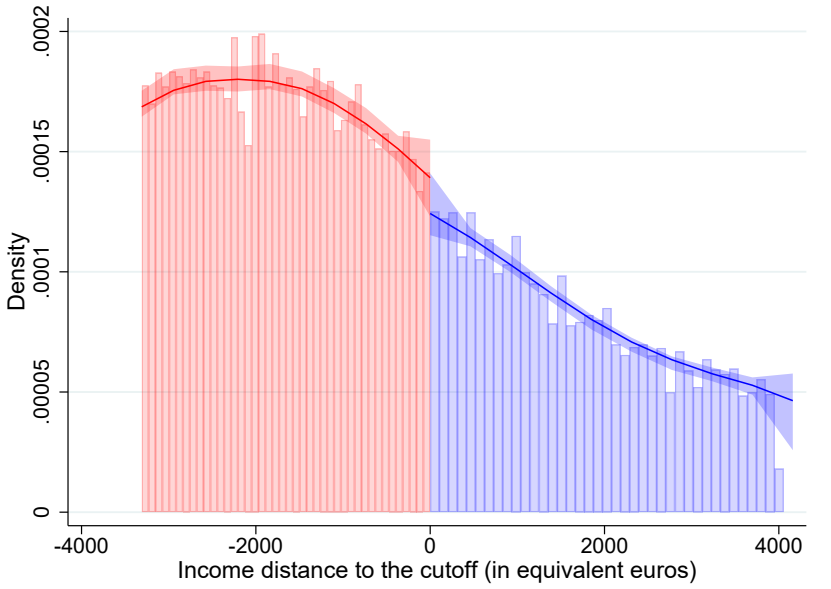
[13]The rules of the programme stipulate that if a child who is granted the subsidy does not use the canteen service for more than 15 days without justification, the subsidy is revoked; thus our ITT effect must be close to the true effect.

[14]To compute the equivalent income distance to the threshold we use the modified OECD equivalence scale, assigning a weight of 1 to the first adult, 0.5 to any other adults in the household and 0.3 to children below the age of 14.

household size, special educational needs, school provision, school size, whether the school is classified as highly complex or not, and school district.[15] Test scores are standardised at the academic year level and annual grades at the course-academic year level. Standard errors are clustered at the school level to account for the fact that test scores and annual grades within each school are not independent of one another. Additionally, we employ triangular kernel weights to assign greater importance to observations nearer the income threshold — though our results do not depend on such choice.

A key assumption in our analysis is that families should not be able to manipulate their household income in order to become eligible. This is — by virtue of the design of the programme — necessarily true: to assess eligibility, information on family income is taken from the previous year's tax declaration and is requested by the programme administrators directly from the Ministry of Finance (provided a family consents). Thus, the subsidy eligibility rules for the next academic year are not known to families when they do their tax returns, which implies that they have no incentive to declare a figure that differs from the true amount. In any case, we confirm that there is no bunching around the eligibility threshold in Figure 3. The results of the manipulation test using a local polynomial density estimation for the partial subsidy threshold indicate that we cannot reject the null hypothesis of continuity around the threshold (p-value = 0.11).

Figure 3: Manipulation test at the partial subsidy eligibility threshold, Barcelona, academic years 2017/2018 — 2021/2022

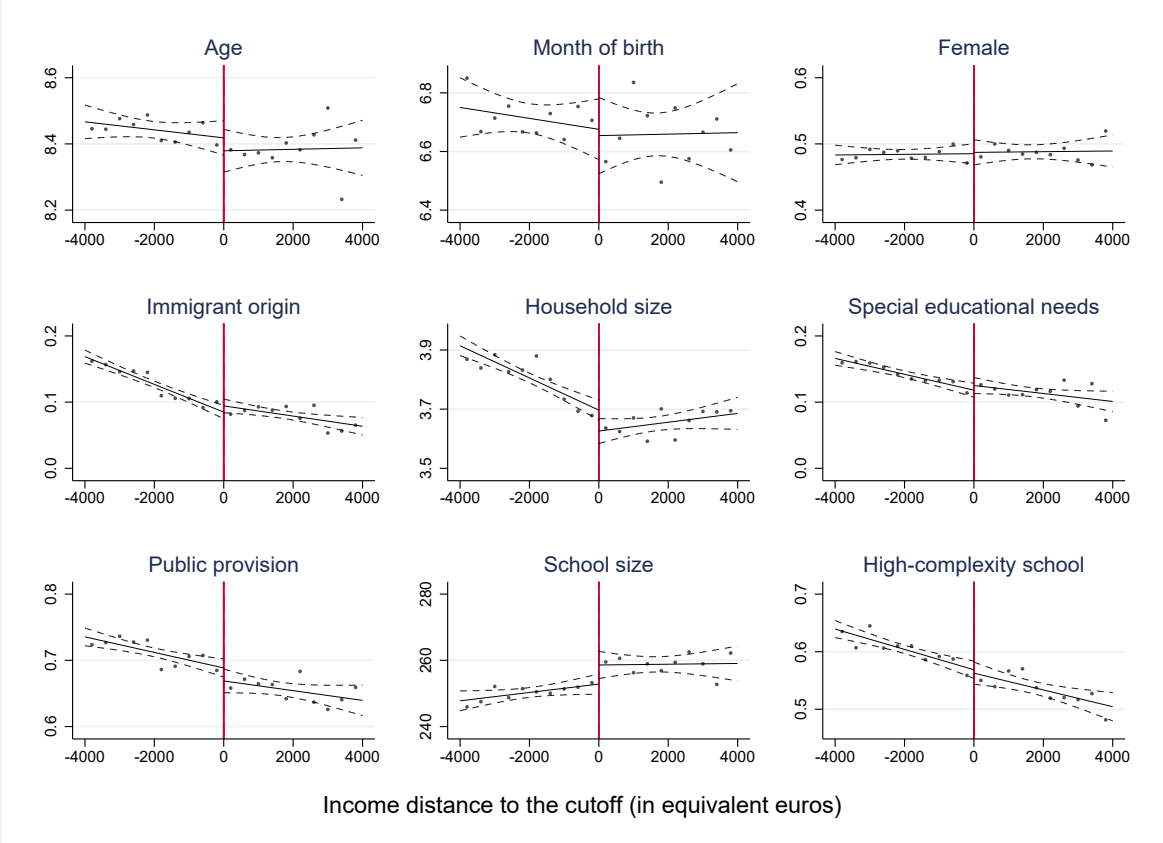


Note: Equivalent income in euros is normalised to zero at the eligibility threshold. Students on the left side of the cutoff receive the partial subsidy, while those on the right side do not qualify for any subsidy. Shaded regions around the curve are confidence intervals at 95%.
Source: Authors' computation, using data from the Barcelona Education Consortium.

[15] To ensure a sufficient sample size on which to conduct analyses of heterogeneity, we merge schools classified as having low complexity with those categorised as having medium-low complexity. Likewise, we combine highly complex schools with those labelled as medium-high, high and very high. Our results, though, are not dependent on such aggregation.

As well as the absence of manipulation, our analysis relies on another assumption: the distribution of predetermined characteristics among students should be the same on both sides of the eligibility cutoff. In Figure 4 we plot whether students just below and just above the eligibility cutoff are balanced in their observable characteristics, including age, month of birth, gender, immigrant origin, household size, special educational needs, school provision, school size and high level of complexity. None of the variables shows any significant change at the income threshold. Further confirmation of these findings can be found in Table A.2 in the Appendix, where we estimate Equation (1) using each of these characteristics as the dependent variable.

Figure 4: Balance in covariates around the partial subsidy eligibility threshold, Barcelona, academic years 2017/2018 — 2021/2022



Note: Equivalent income in euros is normalised to zero at the eligibility threshold. Students on the left side of the cutoff receive the partial subsidy, while those on the right side do not qualify for any subsidy. Ten bins on either side of the cutoff are used. Short dashes represent confidence intervals at 95%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

Difference-in-Differences

In order to measure the effect of the full subsidy, we use a DiD strategy while exploiting the longitudinal dimension of our database.[16] We evaluate the within-student impact on academic achievement of receiving the full subsidy (treated year), as opposed to the partial subsidy (control year). We limit our sample to those students whom we observe on at least three occasions during the whole period of analysis. Furthermore, we focus exclusively on students who receive a 100%-subsidy at some point in the estimation period, ensuring a more homogeneous and comparable sample. Given that we track the same pupils over time, our analysis is limited to evaluating the impact of the full subsidy on annual grades, as the standardised test is only taken on one occasion during primary education.

We estimate the following equation for each subject under analysis:

$$Y_{it} = \sigma + \theta T_{it} + \delta X_{it} + \varphi_i + \mu_t + \upsilon_{it} \tag{2}$$

where Y_{it} is the outcome of interest for child i in academic year t. T_{it} is an indicator for whether the child is granted the full subsidy or not. The parameter of interest, θ, captures the difference between a student's annual grades in an academic year when that student receives the full subsidy and those achieved in a year when he or she benefits from only the partial subsidy. For this parameter to have a causal interpretation in our context, the assignment of the treatment (the full subsidy) should be random. However, this is unlikely, as students receive the full subsidy based on their individual characteristics. To address this concern, in addition to selecting those students who have received the full subsidy at some point, Equation (2) includes individual fixed effects, φ_i, and year fixed effects, μ_t. This allows us to control for unobservable time-invariant factors at both the individual and the year level. Consequently, we leverage within-individual variation to discern the impact of the full subsidy on student outcomes. X_{it} includes a range of school characteristics, including school size, the school's level of complexity and school district. Furthermore, we include key time-varying controls that influence eligibility — specifically family income, the family circumstances and social needs assessment score, and whether or not the child is in receipt of state benefits. As in the context of the RDD analysis, annual grades are standardised at the course-academic year level, and standard errors are clustered at the school level.

6 Results

In this section we present our main findings. First, we document the results relative to the partial subsidy, using the RDD strategy. We round off that analysis by looking at heterogeneous effects by individual and school characteristics. Then we focus on the results for the full subsidy, using the DiD approach. The final part of the section is devoted to robustness checks.

The results for the causal impact of the partial subsidy on educational achievement are shown in Table 3. Panel A details the coefficients corresponding to standardised test scores, while Panel B shows those on annual grades for all five subjects (Catalan language, Spanish, English, Maths and Science). Column (1) presents first the results

[16] We considered the use of an RDD with multiple cutoffs to account for the two eligibility criteria of the full subsidy (family income and family circumstances and social needs assessment points), but we discarded the idea, as the balancing tests for such a strategy did not hold.

for a bandwidth of ±500 equivalent euros around the eligibility cutoff. The subsequent columns expand the bandwidth by ±500 equivalent euros at a time up until Column (10), when the bandwidth used is ±5,000 equivalent euros. Column (11) reports the results using the optimal bandwidth (OB) that minimises the mean squared error (MSE) (Calonico et al., 2014). Each coefficient comes from a different regression.

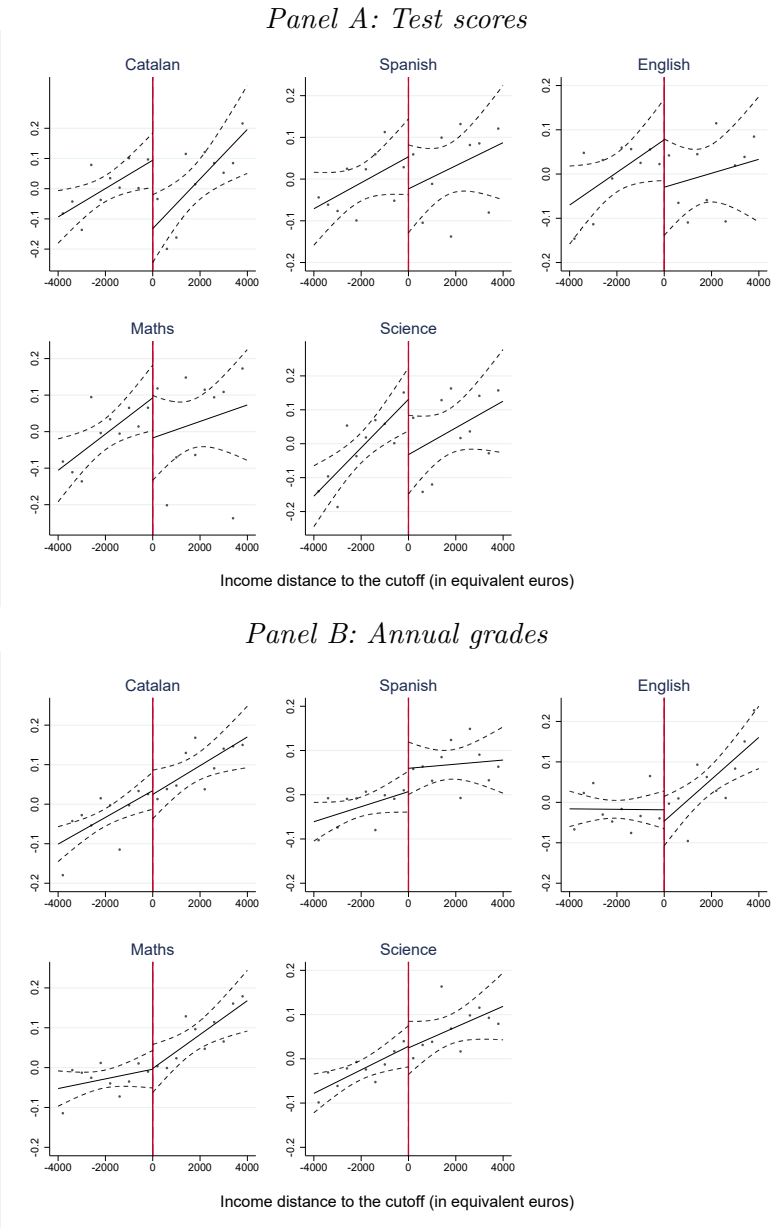
Our results document two main findings. First, the great majority of coefficients in the table are positive, indicating a certain advantage for those students near the income threshold who receive the partial subsidy over their peers who do not. However, the great majority of coefficients do not attain statistical significance. Second, we find statistically significant effects for Catalan language test scores when using bandwidths above ±2,500 equivalent euros. Pupils who receive the subsidy have a score that is 0.22–0.24 standard deviation units higher than pupils who do not. This effect size is similar to those reported by Frisvold (2015) with coefficients in the range of 0.29–0.39 standard deviations when also using an RDD. Interestingly, the results for annual grades do not confirm those for the standardised tests scores; this suggests that when pupils are not evaluated using the same standardised test, but by their own teacher, the potential advantage of the subsidy disappears. Figure 5 shows the RDD plots for test scores and annual grades for each subject and confirms our previous findings: a significant jump around the cutoff is observed only in the case of standardised test scores for Catalan language.

In Table 4 we extend our analysis to examine whether the effect of the partial subsidy varies across different student and school characteristics. Columns (1) to (4) detail results by gender and immigrant origin, while Columns (5) to (10) consider school provision, school size (determined by whether the student population of a school is above or below the median) and school level of complexity. In this table, we use only the results for regressions that consider the optimal bandwidth (OB), but the findings remain qualitatively consistent when various bandwidths are used.[17] In terms of test scores (Panel A), boys near the income threshold who receive the partial subsidy have a test score about 0.28 deviations higher (at 95%) in Catalan language than their counterparts who do not receive any subsidy. We observe positive effects among male pupils for the other subjects considered, too, but these effects only reach conventional statistical significance for Science. The estimates for girls are substantially lower than those for boys, and none is statistically different from zero. When we consider pupils' immigrant background, most coefficients tend to be larger among immigrant children, compared to native Spanish children; however, the estimates are not statistically significant, possibly because of smaller sample sizes. In the case of Catalan language, non-immigrant students derive a certain advantage from the partial subsidy over their peers who do not receive it. Small sample sizes prevent us from going any further with this analysis.

When we split the sample on the basis of school characteristics, we do not find that attendance at a semi-private or a public school has any distinct effect on the impact of the partial subsidy on children's standardised test scores. Rather than school provision, it is school size that matters. The results indicate that being a beneficiary of the partial subsidy (as opposed to being left out of the subsidies programme) has a more substantial impact among pupils who attend a large school. This is the case for all subjects analysed, with the coefficients for Catalan, English, Maths and Science statistically significant at 95%. No such impact is observed when comparing beneficiaries and non-beneficiaries at a small school. Interestingly, the results also indicate that beneficiaries profit the most when studying at an advantaged school of low complexity. Such a gradient is found for all

[17] Results available from the authors upon request.

Figure 5: RDD plots for the partial subsidy, standardised test scores and annual grades, Barcelona, academic years 2017/2018 — 2021/2022



Note: Data for test scores is available for academic years 2017/2018 to 2021/2022, except for 2019/2020 (when the exam was cancelled due to the coronavirus pandemic). As for annual grades, data is available for academic years 2019/2020 to 2021/2022 and only covers public schools. Equivalent income in euros is normalised to zero at the eligibility threshold. Students on the left side of the cutoff receive the partial subsidy, while those on the right side do not qualify for any subsidy. Controls include age, month of birth, gender, immigrant origin, household size, special educational needs, school provision, school size, whether the school is classified as highly complex, and school district. Ten bins on either side of the cutoff are used. Short dashes represent confidence intervals at 95%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

Table 3: RDD results for the partial subsidy, standardised test scores and annual grades, Barcelona, academic years 2017/2018 — 2021/2022

	Bandwidth										
	±500	±1000	±1500	±2000	±2500	±3000	±3500	±4000	±4500	±5000	OB
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Test scores											
Catalan	0.28	0.17	0.18	0.21*	0.23**	0.24**	0.23**	0.23***	0.23***	0.22***	0.22***
	(0.20)	(0.15)	(0.13)	(0.11)	(0.10)	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.08)
Observations	330	664	969	1294	1612	1913	2207	2457	2656	2857	2718
Spanish	0.13	0.05	0.04	0.03	0.05	0.07	0.07	0.07	0.07	0.07	0.07
	(0.21)	(0.16)	(0.13)	(0.12)	(0.11)	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.07)
Observations	332	671	982	1312	1637	1943	2243	2495	2702	2911	3042
English	0.01	-0.01	0.01	0.03	0.05	0.06	0.07	0.08	0.09	0.09	0.09
	(0.20)	(0.14)	(0.12)	(0.11)	(0.10)	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.08)
Observations	332	665	974	1304	1622	1924	2224	2481	2685	2888	2987
Maths	0.04	-0.05	0.02	0.05	0.07	0.08	0.09	0.10	0.09	0.09	0.08
	(0.18)	(0.14)	(0.13)	(0.11)	(0.10)	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.08)
Observations	329	666	979	1309	1633	1937	2235	2487	2694	2898	3057
Science	-0.02	0.03	0.08	0.11	0.14	0.15	0.16*	0.16**	0.15*	0.15*	0.16*
	(0.20)	(0.15)	(0.12)	(0.11)	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.08)	(0.08)
Observations	332	670	976	1300	1621	1919	2215	2463	2666	2873	2578
Panel B: Annual grades											
Catalan	0.16	0.06	0.07	0.07	0.04	0.02	0.01	0.01	0.01	0.01	0.00
	(0.12)	(0.09)	(0.08)	(0.07)	(0.07)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)
Observations	1296	2582	3890	5246	6605	7946	9164	10284	11279	12154	14069
Spanish	0.20*	0.03	-0.01	-0.02	-0.04	-0.05	-0.05	-0.05	-0.05	-0.05	-0.04
	(0.12)	(0.09)	(0.08)	(0.07)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.04)
Observations	1296	2582	3890	5246	6604	7945	9163	10283	11278	12152	15601
English	-0.05	-0.07	-0.02	0.01	0.03	0.02	0.01	0.01	0.02	0.02	0.03
	(0.12)	(0.09)	(0.07)	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)
Observations	1295	2581	3888	5244	6602	7942	9159	10279	11274	12148	14430
Maths	0.05	0.03	0.04	0.04	0.01	-0.00	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.12)	(0.09)	(0.07)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)
Observations	1296	2582	3890	5246	6605	7946	9164	10284	11279	12154	13310
Science	0.06	0.08	0.08	0.07	0.05	0.03	0.02	0.02	0.02	0.02	0.02
	(0.11)	(0.09)	(0.08)	(0.07)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)
Observations	1296	2582	3889	5245	6604	7945	9163	10283	11278	12152	13253

Note: Data for test scores is available from academic years 2017/2018 to 2021/2022, except for 2019/2020 (when the exam was cancelled due to the coronavirus pandemic). As for annual grades, data is available from academic years 2019/2020 to 2021/2022 and only covers public schools. Each coefficient comes from a different regression. Equivalent income distance in euros is normalised to zero at the eligibility threshold. Controls include age, month of birth, gender, immigrant origin, household size, special educational needs, school provision, school size, whether the school is classified as highly complex, and school district. OB refers to optimal bandwidth. Standard errors are clustered at the school level in parentheses. *** significant at 1%, ** at 5% and * at 10%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

courses, but it is again for Catalan that the coefficients reach statistical significance. In other words, the effects are concentrated among subsidy recipients who attend a school where their peers are more socio-economically advantaged — and, as we shall see in the

next section, more proficient in Catalan, the language of instruction in the region.

In Panel B, we show the results for annual grades. Since we were not provided with data on such grades for students at semi-private schools, we only split the sample by gender, immigrant origin, school size and school disadvantage. Confirming previous findings, we do not observe any significant difference between those students who receive the partial subsidy and those who do not, regardless of their individual or school characteristics.

Next, we present the results for the 100% subsidy. Table 5 displays the DiD estimates, in which we examine, for any given student, the impact on annual grades of transitioning from the partial to the full subsidy. The odd-numbered columns present the results without any controls, while the even-numbered columns incorporate controls for time-varying individual and school characteristics. Our findings suggest that students who transition from the partial to the full subsidy achieve better annual grades across all the subjects considered. When we include controls, the estimates increase in magnitude, which is why the estimate for Catalan language reaches 95% statistical significance. Specifically, students who transition from a partial to a full subsidy improve their annual grades for Catalan by 0.10 standard deviation units. While caution is warranted in interpreting these results due to the non-random assignment of the treatment, as well as to the impossibility of accounting for all the time-varying factors that influence both eligibility for the 100% subsidy and annual grades, we still regard our findings as confirmation that the subsidies programme does help children improve their proficiency in Catalan.

Additionally, Table 6 shows whether the impact of the 100% subsidy varies across student and school characteristics, including gender, immigrant origin, school size and school disadvantage. No noticeable differences emerge among students transitioning from the partial subsidy to the full subsidy, regardless of gender or immigrant background. Notably, when the sample is split on the basis of school attributes, a distinct pattern emerges. Specifically, students in smaller schools who move from the partial to the full subsidy improve their Catalan annual grades by 0.15 standard deviation units. Furthermore, in terms of school complexity, students in low-complexity schools who transition to the full subsidy obtain higher annual grades in Science (at 95%) and Maths (at 95%).

Robustness checks. We confirm our main findings concerning the partial subsidy using four empirical validation tests. First, in Table A.3 in the Appendix, we employ second-order polynomials instead of the first-order polynomials presented in Table 3. As the bandwidth narrows, we observe that the coefficients exhibit higher values compared to those in the linear specification. Coefficients for higher bandwidths remain similar to those of the linear specification, although with lower statistical significance. Second, in Figure A.2 we plot, for each subject, the RDD estimates from 16 regressions, using placebo treatment cutoffs that range from -4,000 to +4,000 equivalent euros, in 500 equivalent-euro increments. We find that, across all the artificially introduced cutoff points and for every subject, the RDD point estimations for both test scores (Panel A) and annual grades (Panel B) do not differ from zero. This implies that our results are not due to chance. Third, although in Section 5 we show that families do not manipulate their household income in order to receive the partial subsidy, if any systematic manipulation were to occur, it is more likely that families closest to the cutoff would be those involved in such manipulative activities. Therefore, in Figure A.3 in the Appendix we illustrate, for each subject, the RDD estimates when we exclude students with family income falling within the $\pm$100, $\pm$200, $\pm$300, $\pm$400 and $\pm$500 equivalent euro range, respectively. Our results remain largely unchanged, as we only observe positive and statistically significant effects

Table 4: RDD results for the partial subsidy by student and school characteristics (optimal bandwidth), standardised test scores and annual grades, Barcelona, academic years 2017/2018 — 2021/2022

	Gender		Immigrant origin		School provision		School size		School complexity	
	Male	Female	No	Yes	Semi-private	Public	Small	Large	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Test scores										
Catalan	0.28**	0.19	0.18**	0.27	0.27**	0.25**	-0.02	0.42***	0.39***	0.12
	(0.12)	(0.12)	(0.08)	(0.33)	(0.13)	(0.11)	(0.12)	(0.12)	(0.13)	(0.12)
Observations	1208	1229	2571	174	923	1563	1193	1331	930	1441
OB	3905.52	4009.30	5037.70	2722.01	5043.34	3708.82	4536.54	3875.23	3417.96	4120.43
Spanish	0.14	0.03	0.06	-0.14	0.19	-0.00	-0.08	0.15	0.18*	-0.04
	(0.12)	(0.12)	(0.07)	(0.32)	(0.13)	(0.09)	(0.12)	(0.11)	(0.11)	(0.12)
Observations	1334	1236	2843	185	829	2144	1158	1498	1123	1525
OB	4385.58	3976.24	5859.51	2674.71	4305.67	5648.09	4147.77	4578.86	4409.17	4333.16
English	0.17	0.03	0.04	0.42	0.17	0.04	-0.14	0.26**	0.17	0.00
	(0.11)	(0.11)	(0.08)	(0.35)	(0.13)	(0.11)	(0.13)	(0.12)	(0.12)	(0.13)
Observations	1314	1508	2862	182	793	1945	942	1413	1161	1353
OB	4368.87	5319.65	5954.62	2685.83	4025.30	4876.62	3329.52	4204.54	4599.85	3767.44
Maths	0.15	0.04	0.03	0.62	0.04	0.14	-0.11	0.28**	0.21*	-0.00
	(0.12)	(0.11)	(0.08)	(0.38)	(0.13)	(0.10)	(0.13)	(0.11)	(0.12)	(0.12)
Observations	1300	1424	2894	186	825	1986	1106	1453	1093	1408
OB	4239.58	4953.15	6002.57	2717.96	4272.97	5023.77	3954.50	4383.99	4222.56	3932.61
Science	0.28**	0.08	0.11	0.00	0.13	0.20*	-0.02	0.27**	0.27**	0.07
	(0.12)	(0.11)	(0.08)	(0.36)	(0.12)	(0.10)	(0.12)	(0.12)	(0.12)	(0.11)
Observations	1084	1394	2485	163	897	1707	1118	1366	972	1493
OB	3382.47	4828.11	4747.99	2416.46	4775.60	4120.90	4070.16	4027.40	3674.40	4281.78
Panel B: Annual grades										
Catalan	-0.05	0.05	0.00	0.05			-0.05	0.03	0.05	-0.03
	(0.06)	(0.06)	(0.05)	(0.12)			(0.07)	(0.06)	(0.07)	(0.07)
Observations	7755	6932	12496	1424			6458	8040	4399	8277
OB	6898.47	6324.98	6668.31	4189.33			7065.10	6044.40	5489.32	5258.55
Spanish	-0.06	-0.02	-0.03	-0.04			-0.07	-0.02	-0.01	-0.07
	(0.06)	(0.06)	(0.04)	(0.13)			(0.07)	(0.06)	(0.07)	(0.07)
Observations	7981	7110	13704	1869			6237	8999	4331	8720
OB	7190.27	6556.16	7617.81	5509.74			6757.32	7153.61	5366.64	5660.82
English	0.04	0.02	0.02	0.07			-0.03	0.06	0.06	-0.00
	(0.06)	(0.06)	(0.05)	(0.12)			(0.07)	(0.06)	(0.07)	(0.08)
Observations	7310	6941	12324	1655			6141	8178	4229	7188
OB	6354.95	6333.28	6520	4909.98			6625.54	6225.39	5105.41	4337.76
Maths	-0.06	0.05	0.00	-0.03			-0.06	0.02	0.11	-0.08
	(0.05)	(0.05)	(0.04)	(0.12)			(0.07)	(0.05)	(0.07)	(0.06)
Observations	7873	7145	12042	1467			6143	7450	3811	7784
OB	7056.30	6601.13	6262.07	4327.72			6625.54	5394.99	4385.10	4835.59
Science	-0.01	0.05	0.01	0.06			-0.03	0.03	0.09	-0.03
	(0.06)	(0.06)	(0.04)	(0.11)			(0.06)	(0.06)	(0.07)	(0.06)
Observations	7228	6702	12817	1828			6146	7557	4256	8074
OB	6239.14	5979.25	6915.64	5439.64			6636.24	5496.52	5173.14	5091.69

Note: Data for test scores is available for academic years 2017/2018 to 2021/2022, except for 2019/2020 (when the exam was cancelled due to the coronavirus pandemic). As for annual grades, data is available for academic years 2019/2020 to 2021/2022 and only covers public schools. Each coefficient comes from a different regression. Equivalent income distance in euros is normalised to zero at the eligibility threshold. Controls include age, month of birth, gender, immigrant origin, household size, special educational needs, school provision, school size, whether the school is classified as highly complex, and school district. OB refers to optimal bandwidth. Standard errors are clustered at the school level in parentheses. *** significant at 1%, ** at 5% and * at 10%.

Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

Table 5: DiD results for the 100% subsidy, standardised annual grades, Barcelona, academic years 2019/2020 — 2021/2022

	Catalan		Spanish		English		Maths		Science	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
100% subsidy	0.05	0.10**	0.03	0.07	0.02	0.04	0.03	0.05	0.06	0.06
	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.06)	(0.04)	(0.05)	(0.04)	(0.05)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Student FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9909	9116	9908	9115	9907	9114	9909	9116	9909	9116
R-squared	0.73	0.73	0.71	0.71	0.73	0.73	0.73	0.73	0.67	0.68

Note: Each coefficient comes from a different regression. Controls include family income, family circumstances and social needs assessment score, whether the child receives state benefits, school size, whether the school is classified as highly complex, and school district. Data only covers public schools. Standard errors clustered at the school level in parentheses. *** significant at 1%, ** at 5% and * at 10%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

for Catalan language test scores.[18]

Furthermore, we assess the robustness of our findings with respect to bandwidth choice. We estimate Equation (1) with bandwidths ranging from ±500 to ±10,000 equivalent euros, in 100 equivalent-euro increments, and plot the RDD estimates in Figure A.4 in the Appendix. When we consider test scores (Panel A), as we increase the bandwidth, then — for all subjects except Catalan language — we observe a convergence of point estimates towards zero, accompanied by a reduction in the width of the confidence intervals. In the case of Catalan test scores, while the point estimates do decrease, they do so at a different rate than for the rest of the subjects, ultimately achieving statistical significance. As for annual grades (Panel B), regardless of the bandwidth employed, we find no statistically significant RDD estimates across any of the subjects.

Turning to the analysis for the full subsidy, in Table 5 we employ the canonical DiD estimator. This estimator is a weighted average of all potential two-group/two-period DiD estimators (Goodman-Bacon, 2021). In this case, it is essential to acknowledge that, as highlighted by Goodman-Bacon (2021) and de Chaisemartin and D'Haultfœuille (2022), these estimates may be biased when there is heterogeneity in treatment timing, which is the case in our study. In Table A.4 in the Appendix, we estimate smaller DiD effects for all possible combinations of treatment (receiving the full subsidy) and control (receiving the partial subsidy) groups. The results are positive and identical to those presented in Table 5. Additionally, we find that only about 3% of the observed treatment effects come from variations in treatment timing. The main impact (around 57%) comes from comparing the treated year with the control year. Adding time-varying controls contributes about 40% to the DiD estimates. These results are therefore reassuring.

[18]Even in the absence of manipulation, this test, often referred to as the 'doughnut-hole' approach, is also useful for evaluating the sensitivity of the results (Cattaneo et al., 2020).

Table 6: DiD results for the 100% subsidy by student and school characteristics, standardised annual grades, Barcelona, academic years 2019/2020 — 2021/2022

	Gender		Immigrant origin		School size		School complexity	
	Male	Female	No	Yes	Small	Large	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Catalan	0.07	0.12	0.08	0.13	0.15**	0.04	0.12	0.08
	(0.06)	(0.07)	(0.05)	(0.09)	(0.07)	(0.07)	(0.10)	(0.05)
Observations	4616	4498	7164	1949	4366	4175	1900	7165
R-squared	0.70	0.75	0.74	0.68	0.74	0.75	0.71	0.74
Spanish	0.07	0.07	0.07	0.09	0.08	0.08	-0.02	0.09
	(0.07)	(0.07)	(0.06)	(0.11)	(0.08)	(0.07)	(0.10)	(0.06)
Observations	4615	4498	7164	1948	4365	4175	1899	7165
R-squared	0.69	0.73	0.72	0.68	0.74	0.71	0.69	0.72
English	0.08	0.00	0.01	0.04	0.04	0.07	0.08	0.04
	(0.07)	(0.08)	(0.07)	(0.10)	(0.08)	(0.09)	(0.10)	(0.07)
Observations	4614	4498	7163	1948	4364	4175	1899	7164
R-squared	0.72	0.74	0.74	0.70	0.76	0.74	0.72	0.74
Maths	0.05	0.04	0.02	0.10	0.05	0.11	0.22**	0.00
	(0.07)	(0.06)	(0.07)	(0.08)	(0.07)	(0.08)	(0.11)	(0.05)
Observations	4616	4498	7164	1949	4366	4175	1900	7165
R-squared	0.73	0.73	0.73	0.72	0.74	0.74	0.71	0.74
Science	0.09	0.03	0.08	-0.00	0.09	0.05	0.30**	-0.01
	(0.08)	(0.07)	(0.06)	(0.11)	(0.07)	(0.09)	(0.13)	(0.06)
Observations	4616	4498	7164	1949	4366	4175	1900	7165
R-squared	0.67	0.68	0.69	0.65	0.68	0.69	0.63	0.69

Note: Each coefficient comes from a different regression. Controls include family income, family circumstances and social needs assessment score, whether the child receives state benefits, school size, whether the school is classified as highly complex, and school district. We also incorporate student and year fixed effects. Data only covers public schools. Standard errors clustered at the school level in parentheses. *** significant at 1%, ** at 5% and * at 10%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

7 Mechanisms

So far, we have shown that both the partial and the full subsidy imply an improvement in children's educational achievement. However, conventional statistical significance is consistently reached only in the case of Catalan — the language of instruction in the region. Moreover, we have learnt that the effects of the partial subsidy — the majority of subsidies granted — are strongest in large, low-complex schools, where the peers of subsidy recipients have a more advantaged background. What explains such results?

There are at least three potential mechanisms, all pointing in the same direction. First, school-meal subsidies allow recipients to spend more time at school than children who do not receive the subsidy and are therefore more likely to go home for lunch. This additional time at school can enhance a child's subjective well-being, which in turn has been linked to better educational outcomes (Dominguez and Ruffini, 2023; Lavy, 2019; Andersen et al., 2016; Agüero et al., 2021; Kutsyuruba et al., 2015; Hristova and Tosheva, 2021). This is precisely what is shown in Table 7, which uses data from the 2021 Survey of Subjective Well-being of Children in the city of Barcelona linked to our main dataset: compared to applicants not granted the subsidy, subsidy recipients have higher levels

of subjective well-being overall, but particularly for satisfaction with school peers and student life. In other words, school-meal subsidies provide children with the opportunity to spend additional free time at school, surrounded by peers that they enjoy being with, which in turn potentially helps their educational development.

Table 7: Ordered probit results, subjective well-being, Barcelona, 2021

	Life	Friends	Peers	Student life	Learning
	(1)	(2)	(3)	(4)	(5)
Ref. Applicant, non-recipient					
School-meal recipient	0.28*	0.28*	0.29**	0.37**	0.01
	(0.15)	(0.16)	(0.15)	(0.15)	(0.18)
Observations	990	992	992	991	983
Mean	8.24	9.04	8.30	8.56	8.38

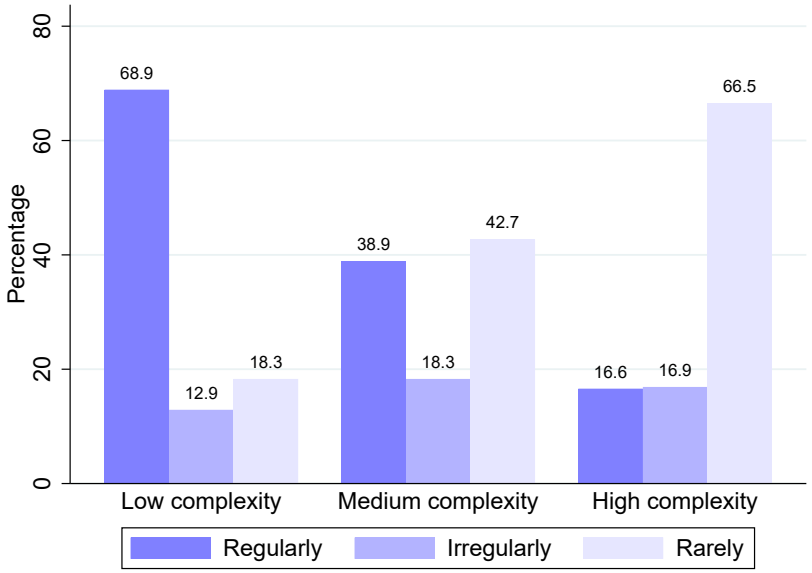
Note: Each coefficient comes from a different regression. Controls include age, gender, month of birth, family income, whether the child receives state benefits, household size, grade, special educational needs and academic course level. We also incorporate school fixed effects. Robust standard errors in parentheses. *** significant at 1%, ** at 5% and * at 10%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the 2021 Survey of Subjective Well-being of Children in the city of Barcelona.

Second, we have shown that the positive effect on Catalan language test scores is particularly strong in low-complexity schools. By the way the school complexity indicator is built, these are schools that Catalan-native children are more likely to attend.[19] As a result, subsidy recipients can improve their proficiency in Catalan more successfully precisely because the use of Catalan is much more prevalent in this environment. Figure 6, using external data from the Higher Council for Evaluation of the Educational System, confirms this. The histogram illustrates the level of Catalan language use at school among students in peer interactions outside the classroom, categorised by school complexity. In low-complexity schools, nearly seven pupils in 10 regularly use Catalan during their free time at school, whereas in highly complex schools, the figure is less than two in 10. In fact, the vast majority of pupils in high-complexity schools use Catalan rarely (66.5%) or irregularly (16.9%). In other words, in low-complexity schools, school-meal subsidies provide an opportunity for less-advantaged students to spend free time with peers who regularly speak Catalan. The same effect could not be found in highly complex schools, where children do not socialise in Catalan.

Third, there is the possibility that not only do children in low-complexity schools spend more time with peers who speak Catalan, but also that such peers are more proficient in Catalan — and therefore serve as *better teachers* for subsidy recipients. Two pieces of evidence support this mechanism. On the one hand, data for all the schools in Barcelona indicates that for the period of analysis, from academic years 2017/2018 to 2021/2022, the average test score for Catalan language in low-complexity schools was 8.2 points (0.55 standard deviations) higher than in high-complexity schools — confirming the greater proficiency of students in low-complexity schools. On the other hand, data from PISA 2022 for Catalonia shows that children from more advantaged backgrounds — precisely those

[19]Recall that the immigrant background of both parents and children is used to define a school's level of complexity. For example, the proportion of students who do not have Spanish nationality is approximately 49% in very high-complexity schools, 25% in high-complexity schools and only 4.5% in low-complexity schools (Consell Superior d'Avaluació del Sistema Educatiu, 2021).

Figure 6: Prevalence of use of Catalan language at school among students in peer interactions outside the classroom, by school complexity, Catalonia, academic year 2021/2022



Note: School complexity is determined through Principal Component Analysis (PCA), which considers various factors, including the educational level and occupation of parents, their immigrant origin, as well as the students' immigrant background and special educational needs.
Source: Authors' computation, using data from the Higher Council for Evaluation of the Educational System (Consell Superior d'Avaluació del Sistema Educatiu, 2022).

more likely to be able to afford and to participate in the lunch service (Educo, 2022) — outperform children from more disadvantaged backgrounds by 0.5 standard deviations.[20] That is, school-meal subsidies not only provide lunch to disadvantaged children, but they give those children the opportunity to interact with children from more advantaged backgrounds who not only speak Catalan more often, but are also more proficient in the language of instruction at school.

Our hypothesis that the difference between subsidy recipients and non-recipients in terms of their test scores for Catalan is one of communication and socialisation is further confirmed in Table 8 where we can disaggregate the result for the Catalan test scores into 'Reading' and 'Writing'. As is shown, the better results for Catalan language are entirely driven by improvements in reading — which, of the two aspects, is the one more closely associated with the oral type of communication that children undertake during lunch at school (Hjetland et al., 2019; Storch and Whitehurst, 2002).[21]

[20]For this analysis, we categorise children as having an advantaged background based on their index of economic, social and cultural status (variable *escs* in PISA).

[21]The literature on reading development describes reading comprehension as the product of two components: decoding and language comprehension. Decoding involves translating written text into spoken words through phoneme awareness and letter knowledge, while language comprehension encompasses broader language skills, including vocabulary and grammar. Therefore, effective reading comprehension requires both proficient decoding and strong language comprehension, with oral language abilities influencing both components (Hjetland et al., 2019; Gough and Tunmer, 1986).

Table 8: Catalan language RDD results by domain for the partial subsidy, standardised test scores, Barcelona, academic years 2017/2018 — 2021/2022

	Bandwidth										
	±500	±1000	±1500	±2000	±2500	±3000	±3500	±4000	±4500	±5000	OB
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Reading	0.28	0.20	0.29**	0.32**	0.35***	0.34***	0.32***	0.31***	0.29***	0.27***	0.34***
	(0.21)	(0.17)	(0.14)	(0.13)	(0.12)	(0.11)	(0.11)	(0.10)	(0.10)	(0.09)	(0.11)
Observations	258	516	763	1024	1287	1532	1768	1972	2118	2276	1646
Writing	0.22	0.15	0.14	0.16	0.18	0.15	0.15	0.15	0.14	0.13	0.13
	(0.22)	(0.17)	(0.14)	(0.13)	(0.12)	(0.11)	(0.11)	(0.10)	(0.10)	(0.09)	(0.09)
Observations	258	516	763	1024	1287	1532	1768	1972	2118	2276	2274

Note: Data is available from academic years 2017/2018 to 2021/2022, except for 2019/2020 (when the exam was cancelled due to the coronavirus pandemic). Each coefficient comes from a different regression. Equivalent income distance in euros is normalised to zero at the eligibility threshold. Controls include age, month of birth, gender, immigrant origin, household size, special educational needs, school provision, school size, whether the school is classified as highly complex, and school district. OB refers to optimal bandwidth. Standard errors clustered at the school level in parentheses. *** significant at 1%, ** at 5% and * at 10%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

8 Concluding remarks

This paper explores the causal impact of a means-tested programme that subsidises school lunches in the city of Barcelona. Using administrative data and a Regression Discontinuity Design, we document the extent to which those near the eligibility cutoff who receive a partial subsidy obtain better results in primary school than those with similar characteristics who do not receive it. In a Difference-in-Differences setting, we also study the impact for each individual student of transitioning from a partial to a full subsidy. Our period of analysis extends from academic years 2017/2018 to 2021/2022, and our outcomes of interest are the test scores for the same exam taken by all pupils in the region in sixth grade (just before leaving for high school) and annual grades. In both cases, we have data on five courses: Catalan language, Spanish, English, Maths and Science.

In most of our specifications and for all courses, we find a positive impact on the educational attainment of recipients of the partial subsidy; however, only in the case of test scores for Catalan language do our coefficients attain conventional statistical significance. Neither for the other courses nor for annual grades are the coefficients estimated precisely in the RDD setting. Our DiD results indicate that receipt of the full subsidy (as opposed to the partial subsidy) provides an advantage in annual grades, but again only in the case of Catalan language. Heterogeneous results have helped us sketch the profile of the child who benefits most from the subsidies programme: a boy who goes to a large school of low complexity with Catalan-native peers from a more affluent background. For such a pupil, the subsidy not only provides a meal: above all, it offers him an opportunity to spend additional, joyful time at school, where he can communicate, socialise and interact with wealthier peers who are proficient in the Catalan language. These 'learning interactions' help him improve in the school's language of instruction, with potential spillovers into

other courses and into other life domains (Manski, 1993; Glaeser and Scheinkman, 1999).[22] The reduction in *friending bias* — the tendency for people to be more prepared to befriend people from a similar socio-economic background — that the subsidies programme entails can have many future implications in the life-time of these children (Chetty et al., 2022b). Without doubt, this is an interesting avenue for future research.

Unfortunately, the results are not so rosy for pupils who attend a highly-complex school, as they do not benefit from the subsidies programme in the same way. Catalan is rarely used in such schools outside the classroom, and as a result, the mechanism by which disadvantaged children can improve their language skills through their peers is lacking. This does not exclude the possibility that these children still benefit from the programme in dimensions other than those considered here. On that note, it is important to take account of the fact that, on average, the cost of the meals is 11.2% of recipient households' annual income.[23] Being unable to count on the subsidies programme would lead to a huge risk across multiple domains for these vulnerable children. Yet our results indicate that if the objective is to improve the educational outcomes for children in highly complex schools, policy designers should consider other means over and above a school-meal programme.

[22] A back-of-the-envelope cost-benefit analysis indicates that the benefits of the partial subsidy amount to nine times the cost of the programme — see Appendix B for full details.

[23] Annual mean household income for school-meal subsidy recipients was €9,859.26 in the last year of our analysis. The total annual cost of eating in the school canteen amounted to €1,100 (175 school days at €6.33 — the price of the menu), which equates to 11.2% of total family income.

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A Appendix

Table A.1: School-meal subsidies eligibility criteria, Barcelona, academic years 2017/2018 — 2021/2022

		2017-2018	2018-2019	2019-2020	2020-2021	2021-2022
Economic thresholds (in €)						
Family income	First adult	9,667.30	9,667.30	10,096.50	10,981.40	10,981.40
	Second adult	4,833.60	4,833.60	5,048.30	5,490.80	5,490.80
	Any other adults	2,416.80	2,416.80	2,524.15	2,745.35	2,745.35
	Each child	2,900.20	2,900.20	3,028.95	3,294.45	3,294.45
Maximum business turnover		155,000	155,000	155,000	155,000	155,000
Maximum capital gains		1,700	1,700	1,700	1,700	1,700
Maximum value of urban properties (not primary residence)		42,900	42,900	42,900	42,900	42,900
Maximum value of rural properties (not primary residence)		13,130	13,130	13,130	13,130	13,130
Family circumstances (number of points)						
Large family (up to 4 children)		1.5	1.5	1.5	1.5	1.5
Especially large family (5 or more children)		3	3	3	3	3
Single-parent family		1.5	1.5	1.5	1.5	1.5
Special single-parent family (2 or more children or disabled member)		3	3	3	3	3
Child in foster care		3	3	3	3	3
Child's own disability or sibling's disability up to 33%		1.5	1.5	1.5	1.5	1.5
Child's own disability or sibling's disability more than 33%		3	3	3	3	3
Balanced redistribution of students with specific educational needs		-	3	3	3	3
Social needs assessment (number of points)						
Social risk		10	10	10	10	10
Severe social risk		15	15	15	15	15
State benefits and other programmes						
Specific educational support for socio-economic reasons		-	-	-	-	100% subsidy
Minimum Insertion Income		-	100% subsidy	-	-	-
Guaranteed Citizenship Income		-	100% subsidy	100% subsidy	-	-
Social Emergency Fund		100% subsidy	100% subsidy	100% subsidy	-	-
Anti-Segregation Shock Plan		-	-	100% subsidy	100% subsidy	100% subsidy

Source: Authors' computation, using data from the Barcelona Education Consortium.

Table A.2: Balance in covariates around the partial subsidy eligibility threshold, Barcelona, academic years 2017/2018 — 2021/2022

	Bandwidth										
	±500	±1000	±1500	±2000	±2500	±3000	±3500	±4000	±4500	±5000	OB
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Age	-0.11	-0.02	0.01	0.03	0.04	0.04	0.04	0.04	0.04	0.04	0.03
	(0.13)	(0.09)	(0.07)	(0.07)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)
Observations	3539	7137	10679	14246	17545	20775	23808	26591	29020	31225	37191
Month of birth	0.28	0.24	0.24	0.17	0.14	0.10	0.09	0.07	0.06	0.06	0.04
	(0.27)	(0.19)	(0.16)	(0.14)	(0.13)	(0.11)	(0.11)	(0.10)	(0.10)	(0.09)	(0.08)
Observations	3539	7137	10679	14246	17545	20775	23808	26591	29020	31225	40240
Female	0.03	0.01	0.01	0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Observations	3539	7137	10679	14246	17545	20775	23808	26591	29020	31225	39395
Immigrant origin	0.01	0.02	0.01	0.01	0.01	0.00	-0.00	-0.00	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	3539	7137	10679	14246	17545	20775	23808	26591	29020	31225	30916
Household size	-0.05	-0.02	-0.00	-0.01	0.00	0.01	0.02	0.04	0.05	0.07	0.00
	(0.11)	(0.08)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
Observations	3539	7137	10679	14246	17545	20775	23808	26591	29020	31225	18261
Special educational needs	-0.02	-0.02	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	3539	7137	10679	14246	17545	20775	23808	26591	29020	31225	33635
Public provision	0.04	0.03	0.03	0.03	0.03	0.02	0.02	0.02	0.02	0.02	0.03
	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Observations	3537	7130	10664	14227	17517	20738	23764	26539	28960	31161	40248
School size	-23.39*	-10.45	-9.29	-7.91	-7.63	-6.81	-6.70	-6.58	-6.20	-5.86	-4.45
	(12.83)	(10.68)	(10.03)	(9.61)	(9.38)	(9.29)	(9.29)	(9.30)	(9.33)	(9.36)	(9.35)
Observations	3537	7130	10664	14227	17517	20738	23764	26539	28960	31161	39167
High-complexity school	0.00	0.00	0.02	0.02	0.01	0.01	0.01	0.01	0.01	0.01	0.02
	(0.07)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Observations	3402	6862	10253	13689	16844	19941	22861	25502	27828	29948	41526

Note: Each coefficient comes from a different regression. Equivalent income distance in euros is normalised to zero at the eligibility threshold. OB refers to optimal bandwidth. Standard errors clustered at the school level in parentheses. *** significant at 1%, ** at 5% and * at 10%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

Table A.3: RDD results for the partial subsidy, second-order polynomials, educational achievement, Barcelona, academic years 2017/2018 — 2021/2022

	Bandwidth										
	±500	±1000	±1500	±2000	±2500	±3000	±3500	±4000	±4500	±5000	OB
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Test scores											
Catalan	0.57**	0.24	0.17	0.15	0.16	0.19	0.21	0.22*	0.23*	0.24**	0.25**
	(0.28)	(0.22)	(0.18)	(0.16)	(0.15)	(0.14)	(0.13)	(0.12)	(0.12)	(0.11)	(0.10)
Observations	330	664	969	1294	1612	1913	2207	2457	2656	2857	3460
Spanish	0.44	0.14	0.09	0.06	0.01	0.01	0.02	0.04	0.05	0.06	0.08
	(0.31)	(0.23)	(0.19)	(0.16)	(0.15)	(0.14)	(0.13)	(0.12)	(0.12)	(0.11)	(0.09)
Observations	332	671	982	1312	1637	1943	2243	2495	2702	2911	3838
English	0.27	-0.05	-0.02	-0.02	-0.03	-0.01	-0.00	0.01	0.02	0.03	0.06
	(0.30)	(0.22)	(0.17)	(0.15)	(0.14)	(0.13)	(0.12)	(0.12)	(0.11)	(0.11)	(0.10)
Observations	332	665	974	1304	1622	1924	2224	2481	2685	2888	3684
Maths	0.10	-0.04	-0.07	-0.02	-0.01	0.01	0.02	0.04	0.06	0.07	0.10
	(0.25)	(0.20)	(0.17)	(0.15)	(0.14)	(0.13)	(0.13)	(0.12)	(0.11)	(0.11)	(0.09)
Observations	329	666	979	1309	1633	1937	2235	2487	2694	2898	4175
Science	0.10	-0.01	-0.01	0.02	0.05	0.08	0.10	0.12	0.14	0.15	0.17*
	(0.25)	(0.22)	(0.18)	(0.15)	(0.14)	(0.13)	(0.12)	(0.11)	(0.11)	(0.11)	(0.09)
Observations	332	670	976	1300	1621	1919	2215	2463	2666	2873	3757
Panel B: Annual grades											
Catalan	0.15	0.14	0.06	0.07	0.10	0.09	0.07	0.06	0.04	0.03	0.02
	(0.17)	(0.13)	(0.11)	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.07)	(0.07)	(0.05)
Observations	1296	2582	3890	5246	6605	7946	9164	10284	11279	12154	19176
Spanish	0.27*	0.20*	0.06	0.02	0.02	-0.00	-0.02	-0.02	-0.04	-0.04	-0.04
	(0.15)	(0.12)	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.07)	(0.07)	(0.07)	(0.05)
Observations	1296	2582	3890	5246	6604	7945	9163	10283	11278	12152	18811
English	-0.12	-0.06	-0.10	-0.07	-0.03	0.00	0.02	0.02	0.01	0.01	0.02
	(0.17)	(0.12)	(0.10)	(0.09)	(0.08)	(0.08)	(0.08)	(0.07)	(0.07)	(0.07)	(0.06)
Observations	1295	2581	3888	5244	6602	7942	9159	10279	11274	12148	17182
Maths	-0.06	0.03	0.01	0.03	0.06	0.05	0.04	0.02	0.00	-0.00	0.01
	(0.18)	(0.13)	(0.10)	(0.09)	(0.08)	(0.08)	(0.07)	(0.07)	(0.07)	(0.06)	(0.05)
Observations	1296	2582	3890	5246	6605	7946	9164	10284	11279	12154	17869
Science	-0.02	0.04	0.06	0.09	0.11	0.09	0.08	0.07	0.05	0.04	0.04
	(0.16)	(0.12)	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.07)	(0.07)	(0.07)	(0.05)
Observations	1296	2582	3889	5245	6604	7945	9163	10283	11278	12152	18656

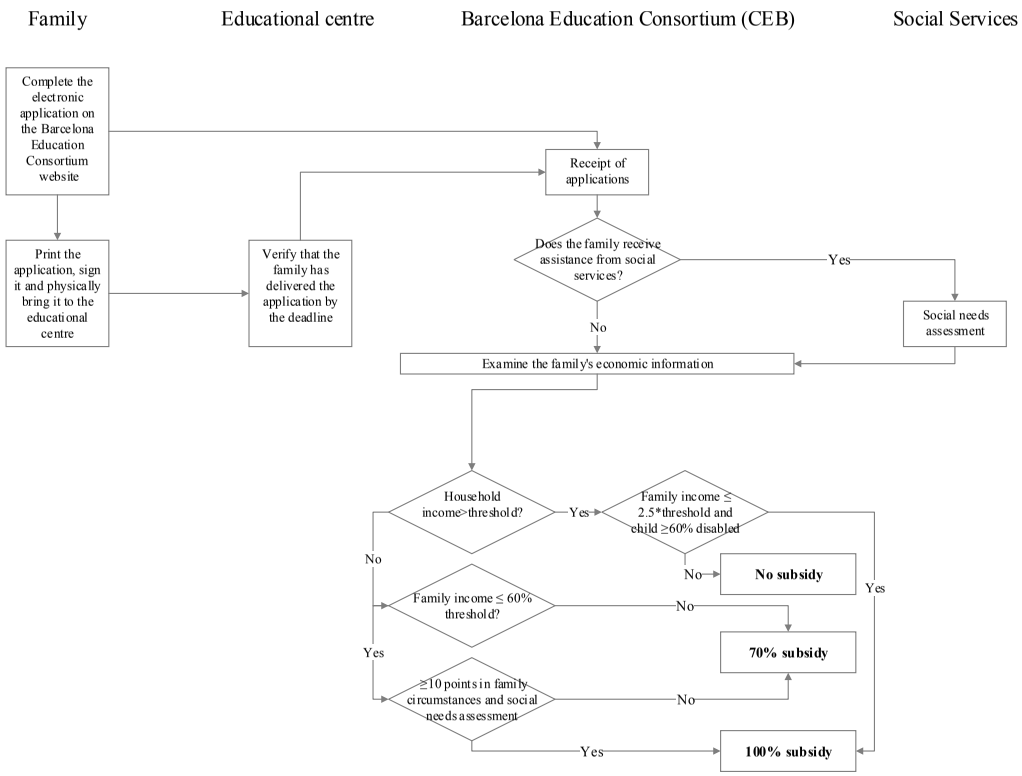
Note: Data for test scores is available for academic years 2017/2018 to 2021/2022, except for 2019/2020 (when the exam was cancelled due to the coronavirus pandemic). As for annual grades, data is available for academic years 2019/2020 to 2021/2022. Each coefficient comes from a different regression. Equivalent income distance in euros is normalised to zero at the eligibility threshold. Controls include age, month of birth, gender, immigrant origin, household size, special educational needs, school provision, school size, whether the school is classified as highly complex, and school district. OB refers to optimal bandwidth. Standard errors clustered at the school level in parentheses. *** significant at 1%, ** at 5% and * at 10%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

Table A.4: DiD decomposition for the 100% subsidy, educational achievement (annual grades), Barcelona, academic years 2019/2020 — 2021/2022

	Estimate	Group	Beta	Weight
Catalan	0.10** (0.04)	Timing groups	0.08	0.03
		Always vs. timing	0.14	0.57
		Never vs. timing	-0.20	≈ 0
		Always vs. never	0.55	≈ 0
		Within	0.03	0.40
Spanish	0.07* (0.04)	Timing groups	0.07	0.03
		Always vs. timing	0.07	0.57
		Never vs. timing	-1.56	≈ 0
		Always vs. never	1.45	≈ 0
		Within	0.06	0.40
English	0.04 (0.04)	Timing groups	0.15	0.03
		Always vs. timing	0.03	0.57
		Never vs. timing	-2.75	≈ 0
		Always vs. never	2.70	≈ 0
		Within	0.03	0.40
Maths	0.05 (0.04)	Timing groups	0.17	0.03
		Always vs. timing	0.05	0.57
		Never vs. timing	-1.38	≈ 0
		Always vs. never	1.57	≈ 0
		Within	0.04	0.40
Science	0.06 (0.04)	Timing groups	-0.02	0.03
		Always vs. timing	0.08	0.57
		Never vs. timing	-0.38	≈ 0
		Always vs. never	0.53	≈ 0
		Within	0.04	0.40

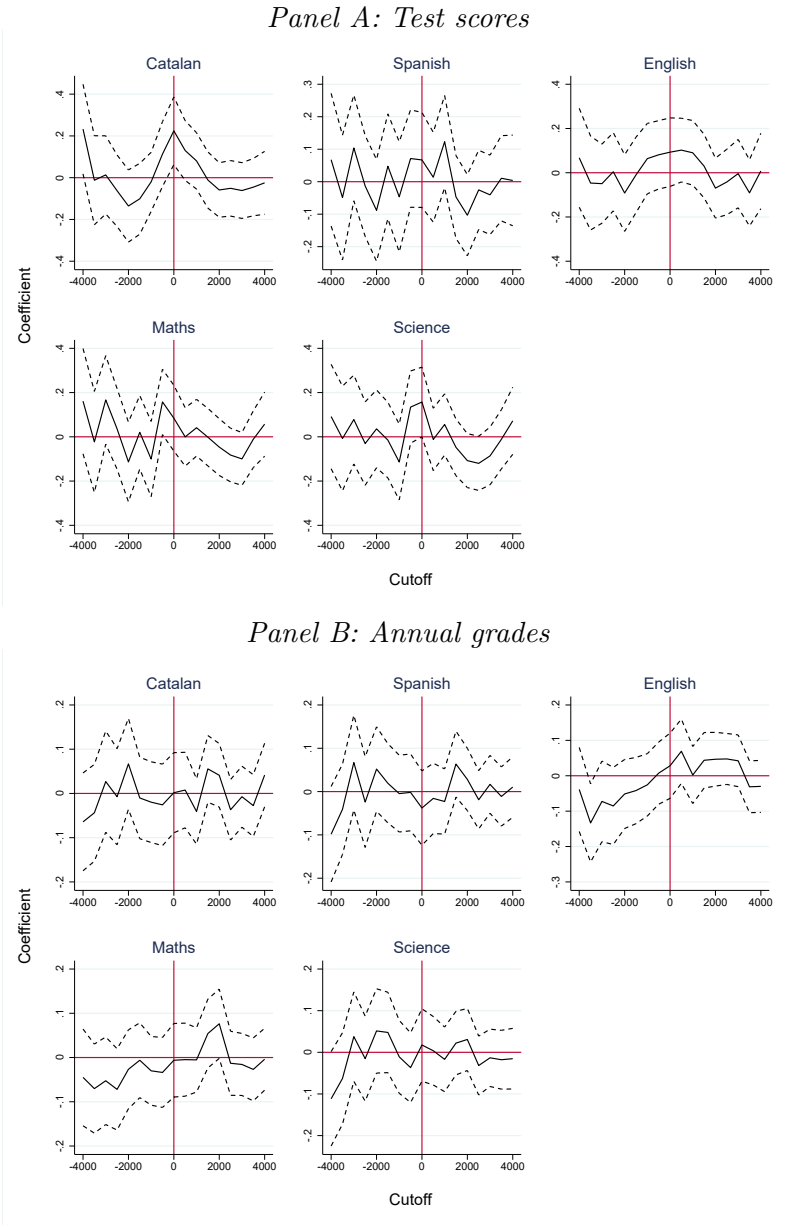
Note: Each coefficient comes from a different DiD decomposition. Controls include family income, family circumstances and social needs assessment score, whether the child receives state benefits, school size, whether the school is classified as highly complex, and school district. Data only covers public schools. Stata's command `bacondecomp` by Goodman-Bacon et al. (2019) was used for this analysis. *** significant at 1%, ** at 5% and * at 10%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

Figure A.1: School-meal subsidies application process, Barcelona, academic year 2021/2022



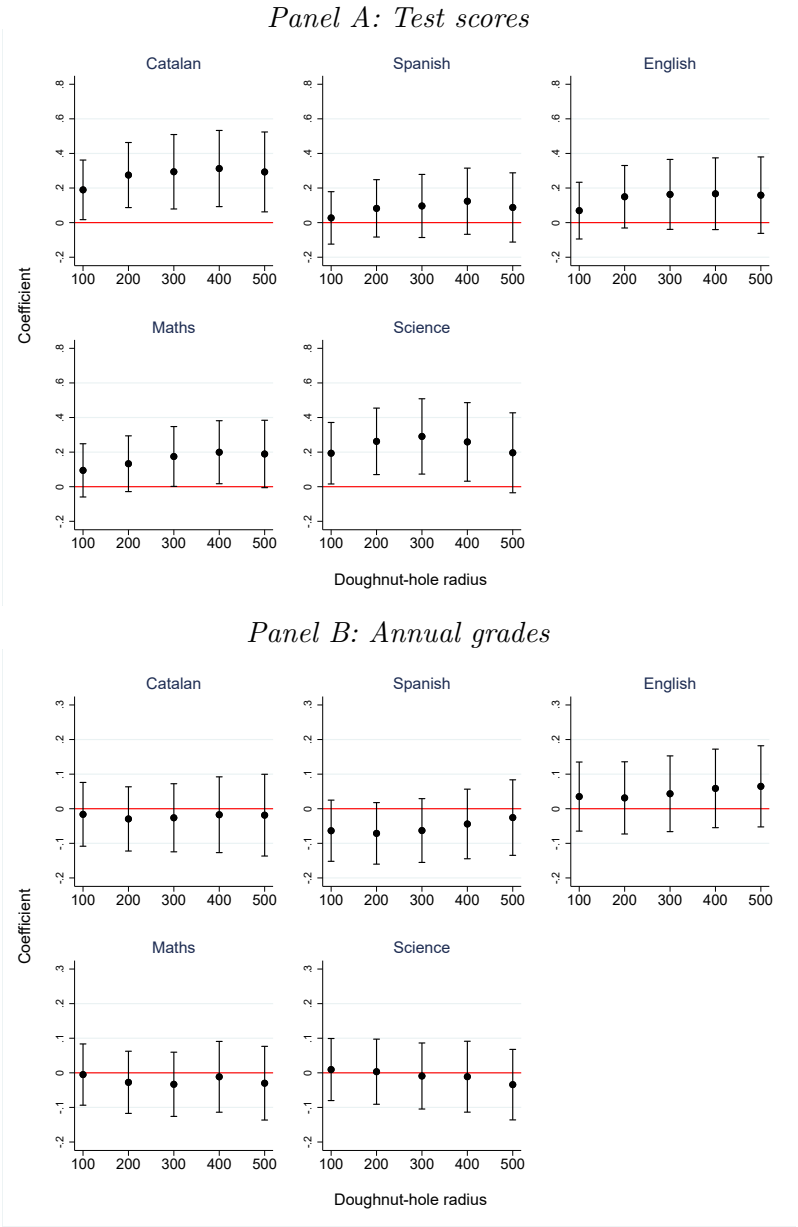
Note: Students whose families receive direct cash benefits from the state or are enrolled in other programmes automatically qualify for the 100% subsidy — see the details in Section 3.
Source: Authors' computation, using information from the Barcelona Municipal Institute of Social Services.

Figure A.2: RDD placebo tests for the partial subsidy (optimal bandwidth), educational achievement, Barcelona, academic years 2017/2018 — 2021/2022



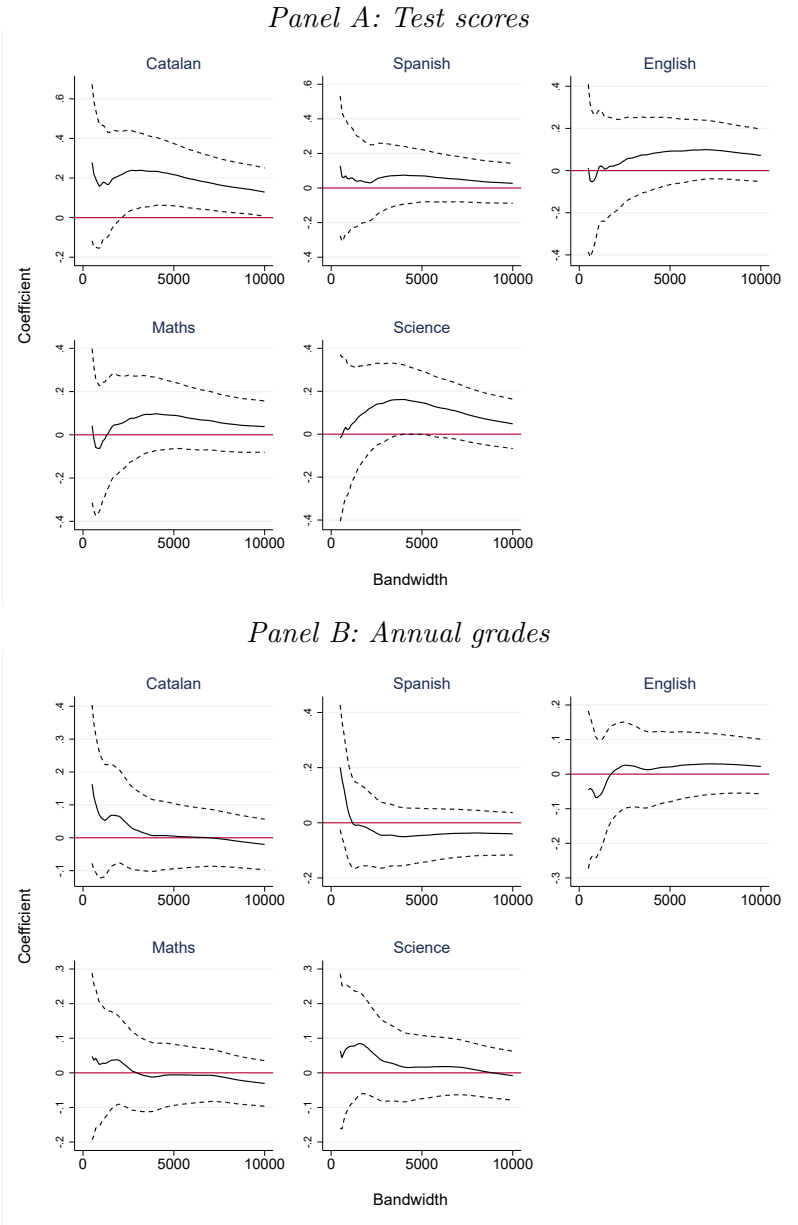
Note: Data for test scores is available for academic years 2017/2018 to 2021/2022, except for 2019/2020 (when the exam was cancelled due to the coronavirus pandemic). As for annual grades, data is available for academic years 2019/2020 to 2021/2022 and only covers public schools. Equivalent income distance in euros is normalised to zero at the eligibility threshold (vertical line). Fake cutoffs range from -4,000 to +4,000 equivalent euros, in 500 equivalent-euro increments. Controls include age, month of birth, gender, immigrant origin, household size, special educational needs, school provision, school size, whether the school is classified as highly complex, and school district. Standard errors are clustered at the school level. Short dashes represent confidence intervals at 95%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

Figure A.3: RDD 'doughnut-hole' approach for the partial subsidy (optimal bandwidth), educational achievement, Barcelona, academic years 2017/2018 — 2021/2022



Note: Data for test scores is available for academic years 2017/2018 to 2021/2022, except for 2019/2020 (when the exam was cancelled due to the coronavirus pandemic). As for annual grades, data is available for academic years 2019/2020 to 2021/2022 and only covers public schools. Equivalent income distance in euros is normalised to zero at the eligibility threshold. Controls include age, month of birth, gender, immigrant origin, household size, special educational needs, school provision, school size, whether the school is classified as highly complex, and school district. Standard errors are clustered at the school level. Vertical lines represent confidence intervals at 95%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

Figure A.4: RDD sensitivity to bandwidth choices for the partial subsidy, educational achievement, Barcelona, academic years 2017/2018 — 2021/2022



Note: Data for test scores is available for academic years 2017/2018 to 2021/2022, except for 2019/2020 (when the exam was cancelled due to the coronavirus pandemic). As for annual grades, data is available for academic years 2019/2020 to 2021/2022 and only covers public schools. Equivalent income distance in euros is normalised to zero at the eligibility threshold. Bandwidths range from ±500 to ±10,000 equivalent euros, in 100 equivalent-euro increments. Controls include age, month of birth, gender, immigrant origin, household size, special educational needs, school provision, school size, whether the school is classified as highly complex, and school district. Standard errors are clustered at the school level. Short dashes represent confidence intervals at 95%.
Source: Authors' computation, using data from the Barcelona Education Consortium and the Department of Education.

B Appendix: back-of-the-envelope cost-benefit analysis

As a policy intervention, is it an economically worthwhile investment to provide school-meal subsidies? Under normal circumstances, every school year pupils improve their learning by around 0.4 standard deviations (Hill et al., 2008). Our findings suggest that students who receive school-meal subsidies increase their Catalan language test scores by 0.22 standard deviations.[24] To account for the fact that these results are specific to only one subject, we adjust the coefficient by dividing it by the number of subjects assessed ($0.22/5 = 0.044$). Assuming that school-meal subsidies have a positive effect on actual learning (rather than test performance), the rounded estimated total impact stands at 0.04 standard deviations, which is equivalent to 10% of the learning gain of a school year. In monetary terms, an additional year of schooling in high-income economies generates about a 10% increase in annual earnings (Montenegro and Patrinos, 2014). Thus, the subsidies programme contributes to a 1% increase in annual earnings ($10\%/10\% = 1\%$) for the beneficiary children. Using data from the latest wave of the Annual Wage Structure Survey, conducted by the Spanish Statistical Office, with a 3% discount rate and counting income from age 23 to 66, school-meal subsidies increase lifetime income by €6,752.68.[25] This is almost nine times the total cost of the programme, which amounts to €775.42 per year (175 school days per academic year multiplied by the daily subsidy amount of €4.43).

Comparisons with existing literature reveal that our calculations are higher than those of Lundborg et al. (2022), who report a benefit-to-cost ratio of 7:1 for nine years of free school lunches in Sweden. However, they are lower than Dotter's (2013) findings, which estimate that providing universally free breakfasts in the US yields a discounted future earnings return of over $12 per dollar spent.

These back-of-the-envelope calculations are potentially conservative for multiple reasons. First, although our results are specific to a subject, the fact that the entire school curriculum in Catalonia is taught in Catalan suggests potential positive spillover effects into other subjects. Second, our calculations only account for the benefits of the school-meal subsidies programme in learning: other important potential benefits are not considered. Third, our results are short term, but the effects could accumulate over time.

[24] We only consider the RDD results, as in the DiD we compare students with a partial subsidy to those with a full subsidy, which is a different type of analysis and affects a smaller number of children.

[25] According to the survey, average earnings are €23,026.10 for the age group 23–34, €28,220.68 for age group 35–44, €30,941.02 for age group 45–54 and €30,995.02 for age group 55–66.