Computers and students' achievement. An analysis of the One Laptop per Child program in Catalonia

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Abstract

This paper analyses the impact of a One Laptop per Child (OLPC) program introduced by the Catalan government (Spain) on student achievement. We exploit longitudinal population data for students in secondary education during the period 2009–2016 and adopt matching estimation procedures to evaluate the program, given that participation was not randomly assigned. As the blocking criteria are quite restrictive, we are able to use a considerable number of matches. Additionally, we combine administrative data with information on school time-varying characteristics obtained from our own survey. Results indicate that this OLPC program had a negative impact on students' competence acquisition in reading (in Catalan, Spanish and English) and in mathematics, their grades falling by 0.20-0.22 standardized units (depending on the subject), which represents 3.8-6.2% of their average grades. This negative effect was stronger among boys than it was among girls (with differences ranging from 10 to 42%).

Key words: competence acquisition, education policy, OLPC, secondary education

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1. Introduction

Education has moved into the digital era and laptops and other technologies have been progressively introduced into classrooms around the world. This diffusion has been partly fostered by public authorities, often as part of the One Laptop per Child (OLPC) program, whereby an education authority provides laptops to children (free or partially free of charge) for use instead of traditional textbooks. The value of OLPC programs is rarely questioned as they appear to be modernizing schools and boosting pupils' acquisition of information and communication technology (ICT) skills. However, in the economics literature, no consensus has yet been reached about the impact of OLPC programs, in particular, and the use of computers, in general, on student skills and academic performance.

In this study, we analyse a program implemented by the Catalan government (known in the first place as eduCAT1x1 and, later, as eduCAT2.0), aimed at providing laptops, wireless connectivity and digital boards to participating schools. Specifically, we study the impact that this program had on student acquisition of reading competencies (in Catalan, Spanish, and English) as well as on student achievement in mathematics.

Our main contribution to this research field is our use of longitudinal data for a student population in a developed country. Although school participation in the program was not random, we are able to control for a number of observed school characteristics that drove selection into the program. Additionally, we would highlight the following contributions. First, to the best of our knowledge, this is the first study to be undertaken of the impact of an OLPC program on students' achievement in Europe. Second, our analysis is one of only a few that considers a whole educational administration (the region of Catalonia in our case) and not just a local experience limited to a few schools. Finally, administrative data are combined with information on school time-varying characteristics derived from our own survey conducted with almost 70% of all schools.

Our results show that the eduCAT program had a small but statistically significant negative effect on student performance. This negative effect was greater among boys than among girls. In order to confirm the robustness of these findings, we conducted a survey of the schools in an attempt to identify the presence of possible confounding characteristics, such as, specific problems encountered in the implementation of the program or differences in teacher training in relation to laptop use. We obtained responses from 693 schools from a total of 1,008 (a response rate of 69%). Results are robust to the inclusion of this additional information.

The remainder of this paper is organized as follows. Section 2 provides an overview of the related literature. In Section 3, we outline the program and provide a description of the data used. Section 4 describes the econometric methodology and presents the empirical results. Section 5 concludes.

2. Literature review

Many studies have examined the effects of computer use at school on student achievement: some have analysed pioneer experiences, such as the program run in Maine (United States), although it involved only a few schools (Penuel, 2006), others have attempted to evaluate the OLPC program, the aim of which is to distribute low-cost, low-power laptop computers to children in less developed countries. According to Beuermann *et al*. (2015), in 2015, there were around 50 OLPC projects in operation around the world.

De Melo, Machado and Miranda (2014) analyse the impact of one of the largest deployments of an OLPC program. Known as the Plan Ceibal, it provided a laptop to every student attending primary and secondary schools in Uruguay. The variation in the date of laptop delivery across students within the same school is used to identify the program's impact. Difference-in-differences estimates indicate that the Plan Ceibal had no effect on maths and reading scores two years after its implementation. The authors argue that the lack of training programs for teachers may account for this outcome, plus the fact the laptops might have been used primarily to conduct internet searches. Similarly, Cristia *et al*. (2012) investigate the effect of an OLPC program that provided laptops to 319 public schools in Peruvian rural communities. Employing a randomized controlled trial at the school level, they find that this program had no effect on maths and language scores. This result may reflect the fact the treated schools had no access to the internet and teachers received no training at all. Mo *et al*. (2013) examine the effectiveness of an OLPC program that targeted 13 migrant schools in suburban areas of

Beijing (China). Notwithstanding the small sample size (i.e. 300 third-grade students) and the participants' lack of internet access at home, the authors conclude that this program enhanced student computer skills and self-esteem and led to a reduction in the number of hours spent watching TV.

Various studies examine the effect of other programs (i.e. other than that of OLCP) involving the distribution of computers. For instance, Carrillo, Onofa and Ponce (2011) evaluate a program providing computer-aid assistance in maths and reading to primary school students in the Guayaquil municipality (Ecuador). Their findings show that while this program had a positive impact on maths test scores, no statistically significant effect was found on language test scores. Barrera-Osorio and Linden (2009) conduct a two-year experiment involving the random provision of free computers to 97 Colombian schools (5,201 students). These computers were donated by the private sector in an attempt to improve the academic performance of public school students. Overall, the program appears to have had little impact on students' test scores. The main reason for this may lie in the teachers' failure to incorporate the computers into the curriculum.

Most of the aforementioned studies conclude that computers have little or no effect on student performance. The main reason for this outcome seems to be the failure to incorporate the use of computers in day-to-day classroom activities due to the teachers' lack of training. This argument is further supported by Alonso-Cano *et al*. (2014), who claim that, in Catalonia, teachers appear to be reluctant to use technology in the classroom. Given the crucial role played by teachers in the educational process, Slater, Davies and Burgess (2012) argue that future research on the effect of computer use at school needs to account for the heterogeneity of teachers. According to Bietenbeck (2014), a further reason why computer use appears not to have any impact on student academic performance is that while modern teaching practices involving computers may boost reasoning skills, these are often not tested on standardized tests.

Additional research examines the impact of specific software programs on student achievement. Banerjee *et al*. (2007) show that a computer assistance-learning program focusing on maths improved the maths scores of fourth-grade students in Mumbai and Vadodara (India). Similarly, Barrow, Markman and Rouse (2009) run an experiment involving the random assignment of a specific algebra software program in three US urban districts. They find that treated students had higher scores on pre-algebra/algebra tests than those who did not use this software program. In contrast, Campuzano *et al*. (2009) conclude that the provision of specific software products did not affect student test scores after one school year. This result is consistent with that of Rouse and Krueger (2004) who study the impact of a well-known software program (i.e. *Fast ForWord*) designed to improve language and reading skills.

Several studies investigate the impact of home computers on educational outcomes. For instance, Malamud and Pop-Eleches (2011) look at the effect of a program in Romania that allocated vouchers for the purchase of a home computer to children of more disadvantaged families. This program was found to have a negative impact on educational performance, but it improved computer skills. This finding is in line with that of Vigdor, Ladd and Martinez (2014) who analyse the effect of a similar program in North Carolina public schools. Similarly, Fairlie and Robinson (2013) do not find any evidence of a significant impact of home computers on maths and reading test scores in several schools in California. Again, no impact on student achievement is found in a study by Beuermann *et al*. (2015) in which about 1,000 laptops were provided for home use to students randomly selected from those attending primary school in Lima, Peru.

Finally, a few papers analyse the impact of investment in ICT on school performance. Machin, McNally and Silva (2007) study the effect of ICT expenditure on pupil outcomes in the UK exploiting exogenous variations in ICT funding across different school districts. Their estimates identify a positive impact on science and English grades, but not on maths grades. Leuven *et al*. (2007) examine the effect of extra funding for computers and software on the academic performance of low-income students in several Dutch primary schools. They find evidence of negative effects. Finally, Goolsbee and Guryan (2006) show that while a major subsidy for ICT investment in schools in California increased internet access in classrooms, it did not improve student achievement.

3. The eduCAT program

The Catalan education system, which is part of the Spanish state system, comprises a) six years of primary school, b) four years of compulsory secondary education (known as ESO, *Educación Secundaria Obligatoria*), and c) two years of non-compulsory education. As regards the latter, students may choose a vocational track (*ciclos formativos de grado medio*) or an academic track (*bachillerato*).

In academic year 2009-10, the Education Department of the Catalan regional government initiated a program (i.e. eduCAT1x1) aimed at promoting the use of technology in secondary schools. This program, which was financed by the Spanish Ministry of Education, was managed by the Catalan Department of Education that had autonomy in its implementation. EduCAT1x1 consisted of three main actions: i) providing laptops as a learning device, ii) providing interactive digital boards and wireless connectivity (local and Internet), and, iii) freely and progressively replacing traditional hard copies of textbooks with e-books. Half the cost of each laptop was covered by the regional government and the other half by the student's parents. However, financial assistance was given to those parents who could not afford to pay for this. Students were, of course, able to take their laptops home with them. All secondary schools in Catalonia were invited to participate in the program. EduCAT1x1 was addressed only at students in the first year of compulsory secondary education. Thus, laptops were only given to these students enrolled at the participating schools.

One year after its implementation, i.e. in the academic year 2010-11, eduCAT1x1 was replaced by eduCAT2.0. This new version of the program was identical to the earlier version with the exception that schools could choose whether to provide one laptop per student (the only option in the first version) or have two students share a laptop (which would be owned by the school). Students entering compulsory secondary education in the academic year 2011-12 were potentially exposed to eduCAT2.0. However, this program was abandoned in the academic year 2012/13. Figure 1 shows how the program developed across academic years and the cohorts of students potentially exposed to the program.

Table 1a shows student participation rates in the program across the three academic years. In academic year 2009-10, 25 percent of first-year secondary students took part in eduCAT1x1, while in academic years 2010-11 and 2011-12, this figure increased to a 36 percent participation rate in the second version of the program. A large majority of students participating in the program received their own laptop, as opposed to sharing it with another student. Thus, in practice, almost all students in eduCAT2.0 adhered to the same format as that applied to eduCAT1x1.

[Insert Figure 1 and Table 1a here]

3.1. Academic performance database

In Catalonia, all students take a standardized test at the end of primary education (grade 6) and at the end of compulsory secondary education (grade 10). In both tests, four skills are tested: proficiency in Catalan language, proficiency in Spanish language, foreign language skills (mainly English, although some students choose French) and mathematical skills. These tests are designed and implemented by the Catalan government.

Information on student performance is contained in two different datasets: one includes the results of the tests taken at the end of primary education, while the other records the outcomes of the tests taken at the end of compulsory secondary education. The first dataset is merged with the second so that for each student completing primary education we have data about his/her academic performance at grade 10 (matching is possible as both datasets contain the student's name, surname, gender and date of birth). However, we were only able to access information on the results of the tests taken at grade 10 for around 78% of the students that completed primary education between academic years 2008-09 and 2010-11. This is attributable to consistently high repetition rates and incipient waves of immigration leaving Catalonia and even Spain. Thus, our final sample contains information for 175,493 students.

Table 1b provides descriptive statistics for student performance on both tests. For each student cohort, the average student performance on the tests at the end of primary education is compared with the average student performance on the tests at the end of compulsory secondary education. A score between 0 and 100 is used for each of the four skills being tested. It can be seen that the difference between the average primary and secondary school test scores is smaller in the case of Catalan and Spanish language proficiency than it is in that of mathematics and English. In the case of the latter two skills, the average test scores are much higher at the end of primary education than at the end of compulsory secondary education.

[Insert Table 1b here]

Table 1c indicates that the average number of students in their last year of compulsory secondary education per school and per cohort is about 43 (note that there is a legal threshold of 35 students per class in Catalonia). Unfortunately, we do not have any information about how many classes of these students there were within each school. Students were aged about 16 when they took the test at the end of compulsory secondary education and 50% of them were female. Students in public schools accounted for 60% of the total number of students. Additionally, schools are classified as either ‘less disadvantaged’, ‘disadvantaged’ or ‘more disadvantaged’. The following criteria are used for this classification: first, the socio-economic conditions of the area in which the school is located; second, the students’ socio-economic background; and, third, the number of students with special educational needs as well as the number of foreign students. Some schools fall quite clearly into the category of ‘more disadvantaged’. These are, for instance, public schools for special education, rural schools and adult education schools located in prisons. In Catalonia, 13% of all schools are classified as ‘more disadvantaged’, 16% as ‘disadvantaged’ and the rest (i.e. 71%) can be considered as ‘less disadvantaged’.

[Insert Table 1c here]

3.2. Selection into treatment

Table 2a shows the differences in academic ability (measured in terms of the average test scores at the end of primary education) between students enrolled at schools that participated in eduCAT and those enrolled at non-participating schools. As can be seen, these differences are statistically significant across all skills and all cohorts. Students with lower academic ability at the end of primary education are more likely to attend schools that decided to participate in the program. However, the differences are quite small, i.e., between one and three percentage points. Additionally, we performed Kolmogorov-Smirnov tests to examine the equality of distributions based on treatment assignment. Figure 2 shows this comparison for two cohorts and two specific skills. We do not report all these statistical tests given the great number of possible combinations of skills and cohorts. All the tests, however, confirm statistical significance for the difference between the treated and the control groups.

[Insert Table 2a & Figure 2 here]

Not only were students with lower academic ability more likely to have been exposed to eduCAT (as shown in Table 2a), but selection into the treatment was also driven by school characteristics. School participation was, in fact, initially promoted at a meeting attended by all school headmasters and those that signed up first were the ones selected, based on the budget available at that time. Although it is *a priori* unclear which school variables might have affected participation in the program, school status (public or private), school size and school socio-economic indicators (see above) might have played an important role in this context.

Table 3 shows estimates of the main characteristics associated with those schools opting to participate in the eduCAT program. Public schools and schools with more students are found to be more likely to take part in eduCAT. The school socio-economic indicators are also found to have an effect. In fact, compared to the 'more disadvantaged' schools, their 'less disadvantaged' counterparts show a lower statistically significant probability of participation. Average student school characteristics (e.g. test score at the end of primary education in different skill areas, age) are also included as additional regressors. It is, however, interesting to note that none of the coefficients on these variables is statistically significant at conventional levels. All these results were robust to the inclusion or otherwise of the non-matched individuals.

These results are in line with our expectations, given that the overall intention of the eduCAT was to provide laptops, wireless connectivity and digital boards to public schools, namely those depending wholly on public policy-makers' decisions. Moreover, another rationale behind the program was to provide laptops to students that could not otherwise afford them, that is, children studying at schools that might be labelled as being 'more disadvantaged'.

We also tested whether there is a geographical pattern in the distribution of untreated/treated schools. For this reason, we plotted them across the Catalan territory distinguishing between treated and untreated units, but also mapping any overlapping geographic distributions. Although, this needs to be confirmed by our econometric results by means of municipality dummies, an inspection of Figure 3 shows that there were no marked spatial differences between the two distributions (treated and untreated schools). The Appendix shows whether any spillover effects across schools were found.

[Insert Figure 3 here]

4. Econometric procedure and results

Our econometric strategy is conducted in four steps. First, we examine whether in evaluating the effect of eduCAT there is a bias attributable to the exclusion of those students whose test scores at the end of compulsory secondary education could not be matched up with their earlier test scores, i.e. at the end of primary education. Second, using the test scores at the end of compulsory secondary education, we look at the effect of eduCAT on student academic achievement, controlling for a number of student and school characteristics. Third, we take advantage of the panel data structure of our dataset and re-estimate the model including student fixed effects. Fourth, the model is estimated again accounting for school time-varying characteristics affecting student performance that might be correlated with the timing of eduCAT.

4.1. Non-matched individuals

Unfortunately, for some students (i.e. around 22% per cohort) we are unable to match their grade 10 test scores with the scores they obtained in grade 6. Although, as explained above, this can be explained in terms of repetition, dropout and the impact of immigration waves, it is important to determine if this matching rate is related in some way to eduCAT. That is, whether, conditional on school and student characteristics, a student's probability of having matched test scores differs across treated and untreated schools. In an attempt at addressing this question, we run a logit model explaining the non-matching condition as a function of treatment, individual and school characteristics. Results are shown in Table 4. The most important result is the non-statistically significant coefficient associated with participation in the program. This indicates that there is no bias in the evaluation of eduCAT stemming from the exclusion of those students for whom grade 10 test scores could not be matched with grade 6 test scores.

[Insert Table 4 here]

Estimates reported in Table 4 show that, in terms of student characteristics, being female and older increase the likelihood of our not being able to match grade 10 test scores with grade 6 test scores. Additionally, in terms of school characteristics, students attending public schools are found to be more likely to be non-matched than their private school counterparts. The opposite occurs for students enrolled in ‘less disadvantaged’ schools compared to those attending ‘more disadvantaged’ schools.

4.2. Cross-sectional results

Next, we conduct linear regressions using cross-sectional student level data. The following equation is estimated:

$$G_{ijk} = \alpha_0 + \gamma E_{ijk} + \varphi m_{ik} + x_i'\beta_1 + x_j'\beta_2 + \theta Coh_k + \varepsilon_{ijk} \quad (1)$$

where G_{ijk} represents the standardized score obtained on each of the four tests (Catalan, Spanish, English and maths) taken by student i at school j in cohort k at the end of compulsory secondary education. E is a dummy variable for student participation in eduCAT. Coh_k represents cohort fixed effects. x_i' is a vector of student characteristics (i.e. age and gender), while x_j' is a vector of school characteristics (i.e. public/private, socio-economic indicator, size) thought to influence academic performance. m_{ik} is a dummy variable representing missing cases (i.e., students whose grade 10 test scores could not be matched with their grade 6 scores). γ measures the average effect of participation in the program on student performance. Table 5a shows estimates for the Catalan test score.

Column one of Table 5a considers participation in the program as the only determinant of Catalan test scores, in addition, that is, to the constant. The corresponding coefficient is statistically significant and has a negative sign (-0.118) and this result holds even after adding the dummy representing missing cases (-0.106) in column two. This estimate is also robust to the inclusion of individual characteristics (column three). However, when school characteristics are accounted for in column four, the coefficient is no longer statistically significant and is considerably smaller in magnitude (-0.026). A similar result is obtained when grade 6 test scores are included as additional regressors (column six). Recognising that the inclusion of grade 6 test scores is problematic because of issues of endogeneity (an unobserved factor affecting grade 6 test scores may also influence grade 10 test scores), a robustness test is conducted. Thus, we interact Catalan grade 6 test scores with the eduCAT dummy in an attempt to eliminate differences in earlier test scores based on treatment assignment. This coefficient is extremely close to zero and is not statistically significant at conventional levels. This would confirm that individual and school differences picked up all the initial differences based on the treatment condition. Finally, in column seven, we account for school fixed effects (α_j). Although we recognise that an estimation of this kind might be inefficient, we need to know what happens when we include a dummy for each school to capture its idiosyncrasy, its specific tuition configuration and social environment. All these characteristics might condition a school's final decision to participate in the program. As can be seen, the relevant coefficient rises to -0.028, but it is still not statistically significant, although this could be a consequence of inefficiency.

[Insert Table 5a here]

Next, matching techniques are used in an attempt to account for selection into eduCAT. Thus, direct comparisons (matches) for each student are found. Given our large dataset and the small number of covariates, we are able to impose a large number of possible neighbours (30 matching units) and match students according to school characteristics (public/private, size and socio-economic indicator) and individual characteristics (age and gender), which influenced participation in the eduCAT program. This procedure also allows us to reduce sample variance in the treatment effect estimates.

Table 5b presents neighbouring matching results using student cross-sectional data, where the Catalan test score is once again used as our measure of student performance. The coefficient related to eduCAT participation is again statistically significant and has a negative sign, ranging from -0.056 to -0.030 (see columns 1 to 3). After accounting for school fixed effects in column 4, this coefficient remains negative and statistically significant but its magnitude increases (-0.100). Our matching estimates account for individual and school characteristics, but they also control for grade 6 test scores in Catalan, Spanish and mathematics.

[Insert Table 5b here]

4.3. Longitudinal results

The next step involves analysing the impact of eduCAT on academic achievement taking advantage of the panel data structure, which allows us to include student fixed effects. Although student fixed effects only account for time invariant characteristics, they address previous concerns about the endogeneity bias resulting from the inclusion of grade 6 test scores. Thus, the following equation is estimated:

$$G_{ijkt} = \alpha_{i0} + \varphi m_{ik} + \gamma E + \lambda d_t + \delta(E \cdot d_t) + x'_{it}\beta_1 + x'_{jt}\beta_2 + \theta Coh_k + \alpha_j + \varepsilon_{ijkt} \quad (2)$$

where G_{ijkt} represents the standardized score obtained on each of the four tests (Catalan, Spanish, English and maths) taken by student *i* at school *j* in grade *t* (6 or 10) in cohort *k*. The fixed constant term (α_{i0}) indicates the student's grade in the initial period (t=0) and d_t represents the time trend. Thus, whereas λ represents time effects and γ the initial differences in grades based on their participation in the program, the association of the eduCAT program with educational performance in t=1 would be captured by δ. Our main interest is to estimate the effect on the treated schools, that is, those participating in the eduCAT program.

Results are shown in Table 6, including and excluding the rest of the test scores (Spanish and mathematics), employing matching procedures to account for an appropriate control group. We obtained a magnitude of -0.029 when not considering alternative scores and -0.032 when we did. Finally, after considering school fixed effects, the associated effect was indeed more negative. Participating in the eduCAT program reduced students' Catalan scores by 0.22 standardized units, that is, around 4% of the average. This result is equivalent to that obtained when estimating using fixed effects panel data, indicating that selection is not as important after we account for the covariates associated with school participation in the evaluated program.

[Insert Table 6 here]

4.4. Survey information

A final concern was that the unobserved covariates might affect both treatment assignment and academic performance. Although exact matching on observables would reduce this kind of bias, we were still concerned about unobserved differences between the treated and control groups. That is, conditional on the covariates we introduced in our estimations, treated and non-treated students should be comparable with respect to the outcome in the non-treatment case as regards their unobserved characteristics.

Given that we cannot assume that all improvement or deterioration in academic performance was a consequence of the eduCAT program, we need to identify other variations at the school level during the period considered. That is, changes in the curriculum, tuition methods, staff or school composition as regards the student body. The same applies to changes in IT introduced in the schools, apart from their possible participation in the eduCAT program, including the use of laptops and digital boards. IT intensity is measured in terms of the percentage of school expenditure allocated to these items. For these reasons, we surveyed all schools in relation to their IT intensity, despite their non-participation on the eduCAT program.

The survey comprised three sets of questions related to the situation during the period under review. First, in relation to ICT we asked each school to state the percentage of school expenditure allocated to ICT; whether they had suffered incidents in specific areas, be they problems of connectivity, device reliability and quality of digital textbooks; which courses employed technological elements regularly (laptops, tablets, e-books and specific software) in the classroom; and, an open question in which they were asked to indicate the regularity with which these devices were used. The second block included questions related to teachers and teaching. Here, we asked about the average number of hours of specific training in ICT teachers in the tested subjects (languages and mathematics) had received; the average age of teachers in these subject areas, and, whether there had been any changes in the way they taught (if so, specifying what these changes involved). Finally, the last block asked schools to report their rates of absenteeism, immigrant status and dropout from compulsory secondary studies during the academic years in question.

As the number of schools is extremely large, we asked the Catalan National Institute of Statistics (IDESCAT) to define a proper sample of schools in which to conduct this survey. IDESCAT defined a sample based on two blocking definitions (public/private and treatment/control strata), while also accounting for several additional characteristics when defining the groups: school ownership (public or private), school size, eduCAT participation and degree of social complexity. Several procedures were established to minimize sample error and PISA performance information was also used. The final sample contained just 276 schools, representing around 27% of the total, and four groups were properly defined (public/treated, public/control, private/treated and private/control). However, given that we wished to provide maximum feedback to the regional government, we eventually tested the whole population but adhering to the representativeness as specified by IDESCAT. In the end, we obtained responses from 693 schools, that is, a 69% response rate.

For this reason, we include survey information about school time-varying characteristics. Specifically, we include time-varying characteristics that condition the educational production function: number of limitations to implement IT, average IT expenditure, quality of IT items, average number of training hours for teachers, the introduction of curricular changes during the period as well as various school indicators (absenteeism, immigration and dropout). This is critical in order to confirm our previous results and to identify additional school characteristics associated with the decision to participate voluntarily in the program, apart from those already identified. Results are shown in Table 7. Our results show that, after considering some previously unobserved factors collected via our research questionnaires, the impact is almost identical to that obtained for the whole population. Notwithstanding, we are aware that there might still be some confounding unobservable characteristics that we did not pick up.

[Insert Table 7 here]

4.5. eduCAT program association with Spanish, English and mathematics grades

Given that our best approach was the one obtained using the matching estimation (being consistent with the other estimations undertaken), in Table 8 we show the association of the eduCAT program with the rest of the grades available (Spanish, English and mathematics). Our results are quite similar to those reported above. Remember that previously we found an association of standardized Catalan grades with the participation in the program of -0.22. The other competencies showed a similar but slightly lower association: mathematics (-0.21), Spanish (-0.21) and English (-0.20), when also including the rest of the grades as explanatory variables. Thus, we can affirm that the estimated correlation between participation in the eduCAT program and individual grades (reading and mathematics) was consistently negative at around a magnitude of -0.21 standardized units. In terms of test scores, eduCAT reduces the average mark by 4.1% in Catalan, 3.8% in Spanish, 6.4% in English and 6.2% in mathematics.

[Insert Table 8 here]

Next, for reasons of robustness, we also considered the cohort that started in 2012, but which was not affected by the implementation of the program. Table 9 shows these results. As can be seen, the association of the program is almost the same, albeit we obtain slightly lower results for Spanish, English and mathematics. Thus, our results were robust to the inclusion of an additional cohort.

[Insert Table 9 here]

4.6. eduCAT heterogeneous associations across groups

In this section, we tested for the presence of heterogeneous associations across students based on gender. Table 10 shows these results for all subjects considered, in estimates again based on neighbouring matching. Boys were more markedly affected than girls in all three reading competences, but the same association was found in the case of mathematics.

[Insert Table 10 here]

5. Discussion

The debate remains very much ongoing as to whether the use of ICT in the classroom promotes student learning, with empirical results being mixed (although most of the evidence for One Laptop per Child (OLPC) shows that these programs have no effect). In this paper, we contribute to this debate by analysing the effects of computer use at school on student achievement in Catalonia (Spain). Specifically, we evaluate the OLPC program organised by the Catalan government, known as eduCAT1x1.

The program, introduced in the academic year 2009/10, aimed to promote the use of computers in secondary schools. It comprised several actions, the main one being the provision of laptops to students, with half the cost of the laptop being met by the government and the other half by the students' parents (grants being provided for those unable to pay). Minor changes were introduced in the following year with the program being renamed as eduCAT2.0. The program was curtailed after the academic year 2012/13 for financial reasons.

The study evaluates the impact of the eduCAT program on the competence levels attained by high school students in Catalonia. Specifically, we used test data from assessments administered at the end of compulsory education in 2012/2013, 2013/2014 and 2014/2015. In order to test the students' individual ability, we matched these results with their scores on the tests administered in their last year of primary education (years 2008/2009, 2009/10, 2010/11, respectively). We evaluated their competencies in different languages (Catalan, Spanish and English) and mathematics. The tests are designed and implemented by the Catalan government and have no academic consequences.

We report a cross-sectional analysis and use panel data for students from Catalan schools. School participation in the OLPC program was not randomly assigned and we are aware of the reasons for participation. Thus, we use possibly random/exogenous variation, once we have controlled for the reasons resulting in the schools' participation. Given our panel data structure, it can be assumed that treatment was determined only by the school's fixed effect (that is, being the first to adopt the program) apart from the observed covariates that were associated with participation. To verify our findings, we surveyed around 700 schools (out of a population of 1,000) to identify the presence of any additional confounding characteristics affecting participation. The violation of the parallel assumption led us to discard a difference-in-differences analysis. By way of an alternative, we conducted our estimates using matching estimators and were able to impose a large number of possible neighbours given the size of the dataset.

Our results show that the eduCAT program negatively affected the acquisition of skills tested, the magnitude of this effect being very similar for each competence (results range from -0.204 to -0.220). Thus, students in schools that implemented the program scored, on average, three points less on the skills tests (c. 5.1% on average, from 3.8% to 6.2% depending on the subject). Thus, while the program appears to have had a negative effect, it is not great in quantitative terms. By gender, the impact is even lower among girls than boys. Thus, OLPC reduced male students' achievement significantly in some subjects (differences ranged from 10 to 42%).

Although most international evidence shows that OLPC programs do not enhance student achievement, the Catalan experience goes further, as our results show a negative impact of OLPC on students' competence acquisition. Given that the implementation of technological tools in the classroom is set to continue in most countries, the results suggest a need for reflection as regards the introduction and diffusion of ICT in schools.

First, when ICT is introduced, it is important to determine the relevance of a range of factors, including the type of program being promoted, whether students should be given their own computer or whether they should be shared among the students, as well as the use that is made of other ICT tools, etc. Second, the way in which the program is implemented is also important. Specifically, the initial situation (the availability of the ICT network in the school, specific training of teachers, degree of commitment of stakeholders, lack of on-line instructional materials, etc.) should be taken into careful consideration as well as the way in which the government implements the program. Finally, it cannot be taken for granted that an ICT program will be successful. Consequently, the implementation of any program should include a system of evaluation. In this way, any adverse effects that might emerge can be rectified.

The present study has several limitations. On the one hand, the data are not experimental, so it is more difficult to demonstrate the causality of the observed effects. While the methodology used allows us to infer causality, it is, nevertheless, based on certain assumptions. On the other hand, the study provides an estimate of the impact of the program on the assessed skills. However, nothing can be said regarding the consequences of the program on the skills not evaluated (other subjects), non-cognitive skills (teamwork, student engagement or student research skills, among others) or the degree of assimilation of new technologies. Yet, our study represents a first approach, establishing links between the implementation of a public education program (eduCAT) and the competence acquisition of students.

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Figure 1. Program implementation diagram

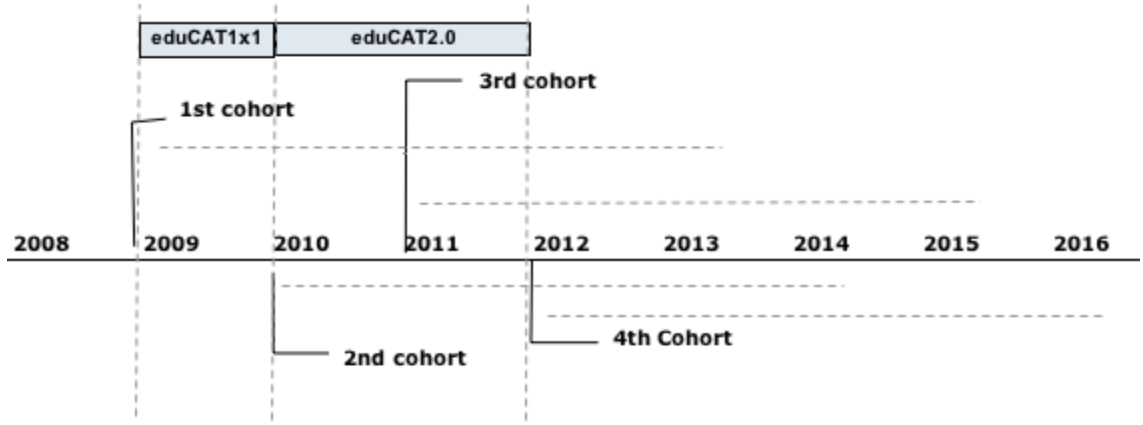
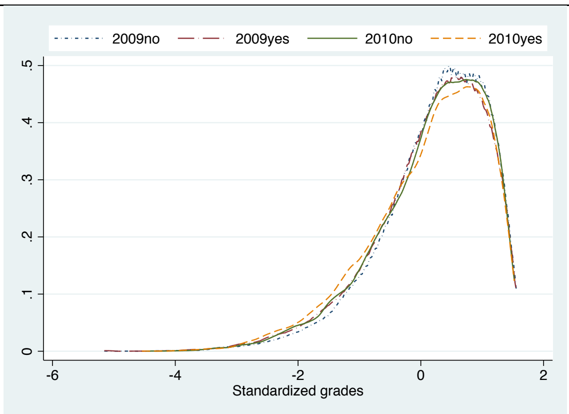
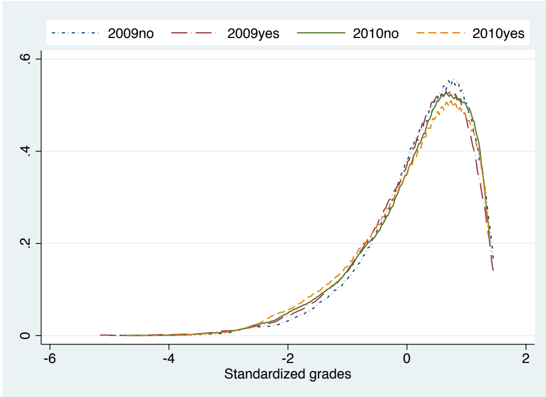


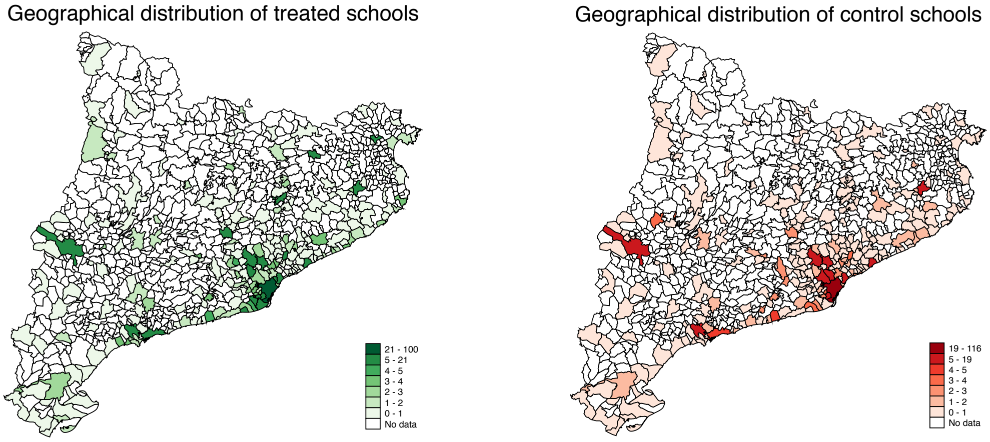
Figure 2. Population distributions: standardized grades based on treatment

Catalan	*Mathematics*
	

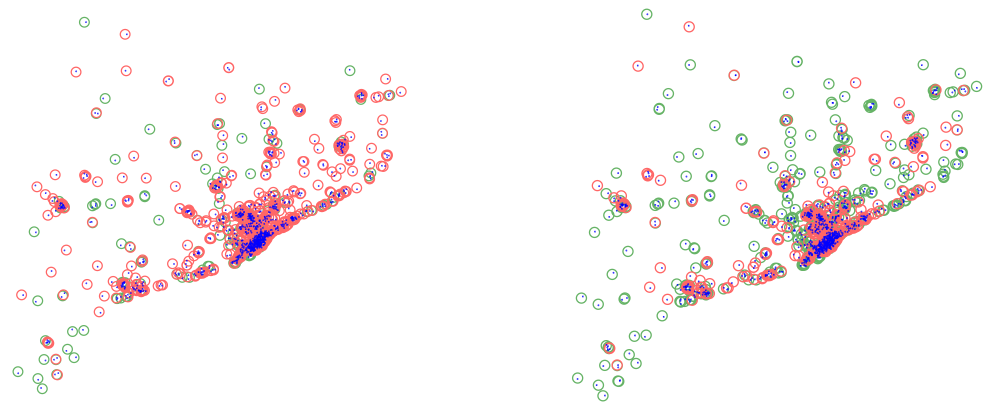
Note: yes means participation in eduCAT and no represents control schools.

Figure 3. Geographical distribution of treated/control schools

Across municipalities



Overlapping geographical distributions (cohort 2009)	*Overlapping geographical distributions (cohort 2010)*



Note: Green circles correspond to treated school centres whereas red ones represent control schools.

Table 1a. eduCAT enrolment: at student level

	Population
	Mean (sd.)
eduCAT1x1 in 2009/10	0.25 (0.44)
eduCAT2.0 in 2010/11 & 2011/12	0.36 (0.48)
eduCAT modality A (one laptop per student)	0.50 (0.50)
eduCAT modality B (one laptop per two students)	0.10 (0.30)
Dropped eduCAT	0.01 (0.14)
Matched individuals	175,493

Note: Means and standard deviations are reported into brackets.

Table 1b. Descriptive statistics on academic performance

Student Cohort (first year compulsory secondary education) A)	Skills B)	Average test score at the end of primary education C)	Average test score at the end of compulsory secondary education D)	Difference D)-C) E)
2009/2010	Catalan	79.54	76.59	-2.95
		(13.42)	(14.22)	
	Spanish	77.40	76.04	-1.36
		(14.48)	(14.06)	
	Mathematics	80.79	68.38	-12.41
		(13.36)	(22.73)	
	English			
2010/2011	Catalan	75.76	77.14	1.38
		(13.25)	(13.77)	
	Spanish	73.19	76.39	3.2
		(15.33)	(12.44)	
	Mathematics	80.37	69.10	-11.27
		(14.39)	(19.55)	
	English	74.35	66.86	-7.49
		(19.57)	(18.19)	
2011/2011	Catalan	79.06	76.29	-2.77
		(13.25)	(14.91)	
	Spanish	75.45	75.15	-0.3
		(14.52)	(14.40)	
	Mathematics	82.71	68.66	-14.05
		(13.45)	(18.19)	
	English	82.17	69.28	-12.89
		(14.65)	(20.65)	

Note: mean and standard deviation are reported into brackets.

Table 1c. Overall population characteristics at student level

	Population
Average number of students per school at grade 10	43.11 (22.01)
Public schools	0.60 (0.49)
More disadvantaged	0.13 (0.34)
disadvantaged	0.16 (0.37)
Less disadvantaged	0.71 (0.45)
Students' age (end secondary)	15.88 (0.57)
Female students	0.50 (0.50)
Not matched students	0,19 (0,39)
N	175,493

Note: we report mean and standard deviation into brackets.

Table 2a. Grades' differences based on treatment status

Cohort starting secondary education in academic year 2008/2009			
Subject	**Treated**	**Not treated**	**Difference in levels**
Catalan 2009	78.56 (14.03)	79.86 (13.19)	-1.31***
Spanish 2009	76.33 (15.19)	77.75 (14.22)	-1.43***
Mathematics 2009	79.63 (14.01)	81.18 (13.12)	-1.55***

Cohort starting secondary education in academic year 2009/2010			
Subject	**Treated**	**Not treated**	**Difference in levels**
Catalan 2010	75.38 (15.00)	76.38 (14.34)	-1.00***
Spanish 2010	72.71 (15.47)	73.95 (15.06)	-1.23***
English 2010	73.41 (19.75)	75.88 (19.18)	-2.47***
Mathematics 2010	80.08 (14.52)	80.84 (14.16)	-0.76***

Cohort starting secondary education in academic year 2010/2011			
Subject	**Treated**	**Not treated**	**Difference in levels**
Catalan 2011	78.79 (13.42)	79.51 (12.98)	-0.72***
Spanish 2011	74.88 (14.63)	76.37 (14.29)	-1.48***
English 2011	81.57 (14.72)	83.12 (14.49)	-1.55***
Mathematics 2011	82.46 (13.58)	83.11 (13.24)	-0.66***

Note: mean and standard deviation are reported into brackets. ***, ** and * represent statistical significance differences at 1%, 5% and 10% between the treated and the control group for each one of the cohorts.

Table 3. Treatment as function of school characteristics

Public school	0.552 (0.16)***
School size	0.002 (0.00)***
% female students	0.477 (0.43)
Average age	0.637 (0.52)
Disadvantage level	
Less	-0.762 (0.27)***
Little	-0.105 (0.20)
Average standardized grades at the end of primary education	
Catalan	0.148 (0.19)
Mathematics	0.267 (0.17)
Spanish	-0.200 (0.16)
Cohort 2010	1.719 (0.08)***
Cohort 2011	1.713 (0.08)***
Not matched individuals	0.143 (0.32)
Constant term	-9.867 (6.16)
Sample size	3,069
Wald Chi^2 (p-value)	533.04 (0.00)

Base category was more disadvantaged schools and the cohort starting in 2009. Logistic estimation computed with panel data structure. ***, ** and * represent statistical significance differences at 1%, 5% and 10%. We do not include English standardized grades because they were not evaluated for the first cohort ending primary studies.

Table 4. Not matched condition: logistic estimates

eduCAT participation	0.003 (0.02)
Student's age	7.045 (2.05)***
Age^2	-0.125 (0.06)*
Female	-0.174 (0.01)***
Public school	0.324 (0.02)***
School size	-0.000 (0.00)***
Disadvantage level	
Less	-0.465 (0.02)***
Little	-0.483 (0.03)
Cohort 2010	-0.014 (0.02)
Cohort 2011	-1.171 (0.02)***
Constant term	-81.924 (16.66)
Sample size	173,501
Wald Chi^2 (p-value)	22,634.2 (0.00)
Pseudo R2	0.357

Base category was more disadvantaged schools and the cohort starting in 2009. ***, ** and * represent statistical significance differences at 1%, 5% and 10%.

Table 5a. Cross-sectional results: standardized Catalan grades at t=1: OLS estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
eduCAT participation	-0.118 (0.02)***	-0.106 (0.02)***	-0.106 (0.02)***	-0.026 (0.01)*	-0.016 (0.01)	-0.014 (0.01)	-0.028 (0.02)
eduCAT·Catalan (t=0)					0.003 (0.01)		
Missing indicator		-0.804 (0.01)***	-0.558 (0.01)***	-0.500 (0.01)***			
Std. Catalan grade at t=0					0.497 (0.01)***	0.235 (0.00)***	0.220 (0.00)***
Std. mathematics grade at t=0						0.197 (0.00)***	0.189 (0.00)***
Std. Spanish grade at t=0						0.213 (0.00)***	0.217 (0.00)***
Student's age			1.961 (0.35)***	1.799 (0.29)***	2.127 (0.69)***	1.337 (0.51)***	1.505 (0.44)***
Age^2			-0.069 (0.01)***	-0.062 (0.01)***	-0.073 (0.02)***	-0.047 (0.02)***	-0.052 (0.01)***
Female			0.120 (0.01)***	0.127 (0.01)***	0.100 (0.00)***	0.141 (0.00)***	0.142 (0.00)***
Public school				-0.213 (0.02)***	-0.122 (0.01)***	-0.094 (0.01)***	
School size				0.000 (0.00)**	0.000 (0.00)	-0.000 (0.00)	
Disadvantage level							
Less				0.590 (0.03)***	0.269 (0.02)***	0.217 (0.02)***	
Little				0.453 (0.03)***	0.208 (0.02)***	0.176 (0.02)***	
Cohort 2010				0.013 (0.02)	0.032 (0.01)**	0.038 (0.01)***	0.041 (0.01)***
Cohort 2011				-0.005 (0.02)	0.008 (0.01)	0.015 (0.01)	0.019 (0.01)
Constant term	0.059 (0.02)***	0.206 (0.01)***	-13.727 (3.02)***	-13.086 (2.32)***	-15.551 (5.50)	-9.492 (4.09)	-10.767 (3.49)
School FE	NO	NO	NO	NO	NO	NO	YES
Sample size	168,300	168,300	168,300	168,300	134,563	133,476	133,476
F (p-value)	47.9 (0.00)	2,557.0 (0.00)	1,363.7 (0.00)	805.5 (0.00)	1,884.5 (0.00)	2,268.5 (0.00)	2,756.5 (0.00)
R2	0.004	0.101	0.121	0.169	0.333	0.394	0.437

Base category was more disadvantaged schools and the cohort starting in 2009. ***, ** and * represent statistical significance differences at 1%, 5% and 10%.

Table 5b. Neighbouring matching results: standardized Catalan grades at t=1

	(1)	(2)	(3)	(4)
eduCAT participation	-0.051 (0.01)***	-0.030 (0.01)***	-0.056 (0.00)***	-0.100 (0.02)***
Standardized Catalan grade at t=0	NO	YES	YES	YES
Standardized mathematics grade at t=0	NO	NO	YES	YES
Standardized Spanish grade at t=0	NO	NO	YES	YES
Individual covariates	YES	YES	YES	YES
School covariates	YES	YES	YES	YES
Cohort dummies	YES	YES	YES	YES
School FE	NO	NO	NO	YES
Sample size	168,300	134,563	133,476	133,476

***, ** and * represent statistical significance differences at 1%, 5% and 10%. Clustered standard errors at school level. Fixed covariates at individual level include: non-linearity in age, being female, attending a public school, school size, cohort and school centre disadvantage level. Base category was more disadvantaged schools and the cohort starting in 2009. Neighbouring matching at individual level considered 30 neighbouring units and exact match for public schools, school size and school centre complexity degree.

Table 6. Neighbouring matching results: standardized Catalan grades

	Not including other grades	*Including other grades (maths & Spanish)*	*Including other grades* + school FE
Neighbouring matching	-0.029 (0.00)***	-0.032 (0.00)***	-0.220 (0.00)***
N	305,691	299,604	299,604

***, ** and * represent statistical significance differences at 1%, 5% and 10%. Clustered standard errors at school level. Neighbouring matching at individual level considered 30 neighbouring units and exact match for public schools, school size and school centre disadvantage level. We included a dummy variable denoting those missing values.

Table 7. Neighbouring matching results: standardized Catalan grades for surveyed school centres. Including other grades and school FE.

	Surveyed	*Surveyed & covariates*
Neighbouring matching	-0.231 (0.00)***	-0.238 (0.01)***
N	198,778	118,421

***, ** and * represent statistical significance differences at 1%, 5% and 10%. Clustered standard errors at school level. Neighbouring matching at individual level considered 30 neighbouring units and exact match for public schools, school size and school centre disadvantage level. We included a dummy variable denoting those missing values.

Table 8. Neighbouring matching results for the rest of grades. Including other grades and school FE.

	Spanish	*English*	*Mathematics*
Not including other grades	-0.158 (0.00)***	-0.150 (0.01)***	-0.172 (0.00)***
Including other grades	-0.212 (0.00)***	-0.204 (0.00)***	-0.210 (0.00)***
N	305,853 / 299,604	259,302 / 251,576	307,507 / 299,604

***, ** and * represent statistical significance differences at 1%, 5% and 10%. Clustered standard errors at school level. Neighbouring matching at individual level considered 30 neighbouring units and exact match for public schools, school size and school centre disadvantage level. We included a dummy variable denoting those missing values and we accounted for school FE.

Table 9. Robustness check. Neighbouring matching results. Including 2012 cohort.

	Catalan	*Spanish*	*English*	*Mathematics*
Not including other grades	-0.194 (0.00)***	-0.134 (0.00)***	-0.117 (0.00)***	-0.147 (0.00)***
Including other grades	-0.202 (0.00)***	-0.188 (0.00)***	-0.173 (0.00)***	-0.182 (0.00)***
N	409,351 / 271,522	416,637 / 409,351	368,178 / 359,275	418,258 / 409,351

***, ** and * represent statistical significance differences at 1%, 5% and 10%. Clustered standard errors at school level. Neighbouring matching at individual level considered 30 neighbouring units and exact match for public schools, school size and school centre disadvantage level. We included a dummy variable denoting those missing values and we accounted for school FE.

Table 10. Heterogeneous eduCAT associations with grades across students' performance distribution: neighbouring matching results

	Catalan	*Spanish*	*English*	*Mathematics*
Boys	-0.257 (0.01)***	-0.222 (0.01)***	-0.232 (0.01)***	-0.229 (0.01)***
Girls	-0.181 (0.01)***	-0.196 (0.01)***	-0.175 (0.01)***	-0.209 (0.01)***

***, ** and * represent statistical significance differences at 1%, 5% and 10%. Clustered standard errors at school level. Neighbouring matching at individual level considered 30 neighbouring units and exact match for public schools, school size and school centre disadvantage level. We included a dummy variable denoting those missing values. Each block based on student's performance contained around 90,000 students.

Appendix. Potential spillover effects from treated schools

One assumption made when estimating potential outcomes for those schools that did not participate in the program (as if they were in the treatment group) was the non-presence of spillovers from treated to untreated regions. This can be tested using spatial econometric models and several suitable procedures are available. After testing different specifications, our preferred model was the spatial Durbin error model. In fact, this specification confirmed the statistically significant negative average treatment effect on treated schools. Notwithstanding, we focused our attention on the estimation of global spillovers, that is, on the use of the spatial lag of the covariates model and the spatial Durbin model (see LeSage, 2014, for an extensive review of these models) to test for the effect of spillovers on untreated schools. In practical terms, this means estimating the following expression:

$$G_{jk} = \alpha + \delta_1 E + \delta_2 WE + x'_{jk}\beta_1 + Wx'_{jk}\beta_2 + \rho W G_{jk} + \varepsilon \qquad (3)$$
$$\varepsilon \sim N(0, \sigma_\varepsilon^2 I_N)$$

where W accounts for the distance spatial matrix of schools across the Catalan territory. By testing the statistical significance of the parameter δ_2, we learn whether spillovers take place from treated schools to other schools located close by. We obtained a positive externality of 0.002, a value that is far from being statistically significant (p-value=0.95).