Spillover effects of full-day schools: Evidence from São Paulo state

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Abstract

Lengthening school time is an attractive policy that middle-income countries have been adopting. However, regardless of an increasing number of works analyzing the impact of increasing instructional time on students' achievement, little is known about the spillover that this kind of policy generates, despite its high cost for poor students from developing countries. Therefore, we evaluate the impact of full-day schools on regular public schools (part-time). To do so, we analyze the Programa de Ensino Integral (PEI) of São Paulo state for secondary education in a dataset with schools geolocations. We use a dynamic difference-in-differences strategy to show that full-day schools change students and teachers composition of nearby regular schools and negatively affect their achievement and drop-out. Furthermore, we estimate PEI' impact on treated schools removing spillover bias and confirm that the spillovers are relatively small compared with the program's gains.

JEL Codes: A20, I24, I28

Most middle-income countries have accomplished the challenge of achieving universal primary education. Now their effort turns into raising educational quality. Alfaro et al. (2015) observes that lengthening the instructional time has been a central policy agenda in recent years for many Latin American countries. Generally, the public sector plays a crucial role in providing basic education. In Brazil, for example, 87% of enrollment in high

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school were from state public schools.[1] Even though full-time enrollment in Brazilian public education is still low (14.2%),[2] it is expanding. The sixth goal of the National Educational Plan (Plano Nacional da Educação - PNE) establishes that until 2014, 50% of public schools (corresponding to 25% of enrollment) must offer their instructional time in full-day.

Although some previous works analyze the impact o full-day school on students learning, there is limited knowledge about whose students demand such type of school and how this may affect traditional public schools' quality. Furthermore, full-day school spillover raises political debate against this kind of policy. For example, the state deputy of São Paulo, Carlos Giannazi, comments: "It is a school for few students. It will form some islands of excellence and marginalize all other students". Thus, understanding if full-day schools affect regular public schools and to which extent relative to their gains is essential to full-day school programs' continuity. Economics models do not present a conclusive answer to the direction and magnitude of full-day school spillover.[3] Ultimately, the net effect of the spillover is an empirical question.

Therefore, this paper estimates the spillover impact of full-day schools on regular (part-time) secondary public schools in the same school market. After confirming the existence of spillover, we re-estimate the impact of full-day schools on treated schools removing the spillover bias. For this, we investigate the Programa de Ensino Integral (PEI) of São Paulo' state in Brazil. The program substantially extends the school time of select regular state schools (about 72%), with new schools adhering to the program every year in a staggered adoption. With this framework, we estimate both the impact on full-day schools and the spillover on neighbours' regular schools using a dynamic difference-in-differences estimation (Callaway and Sant'Anna, 2020). We define full-day schools' neighbours as public schools inside PEI schools' local market, calculated from an administrative panel of students' enrollment over the years.

The results reveal that full-day school substantially increases the learning outcomes of high school students (more than 0.4 standard deviations both in math and language) and decreases overall school enrollment. However, looking at a student-level dataset, we confirm that PEI schools select, on average, the best students in the public educational system, having a "cream-skimming" effect, probably overestimating the achievement point estimate.

The results from the spillover analysis confirm that neighbours regular schools absorb most of the students that can not manage to study full-time. In addition, there is a com-

[1]Basic Education Census, 2019

[2]Observatório do Plano Nacional de Educação

[3]Despite the scarcity of models related to full-day school, the vast literature on school competition provides a close relation to the possible spillover of full-day school, in which changes in school productivity, student composition and peer effect may occur in traditional schools (Urquiola, 2016).

position change in students and teachers: students who stay at regular schools are older and come from underprivileged families, and teachers have a worse formation and different hiring contracts. These changes led to a negative impact of full-day schools on neighbours' achievement in standardized tests. Specifically, point estimates show a negative effect of up to 0.03 standard deviation in math. Also, students' dropout at regular school increases by 1.8 per school.

We then use Butts (2021) to estimate PEI impact removing the spillover bias, showing that occurs a small attenuation of the previous result, with the achievement gain in those schools continuing to be of the extent of 0.4sd. Taking all results together, the improvement of full-day schools is at least 15-fold higher than the loss caused in neighbours' achievement.

Our work advances the literature in three ways. First, we show evidence that full-day schools generate a "cream-skimming" effect, retaining and attracting already higher achievers students from a better background in São Paulo's public educational system. Secondly, we estimate the spillover from full-day PEI program schools, carefully defining the relevant set of schools from which this policy may affect, advancing previous studies that poorly define neighbours schools to estimate the spillover (Kawahara, 2019). Lastly, our work is the first to estimate the direct impact of full-day school removing the spillover bias that it generates in regular schools.

Some robustness checks are made considering other neighbours groups' definitions and re-doing estimations using the traditional Two-Way Fixed Effects (TWFE) estimators. The robustness checks endorse our main results and highlight the superiority of Callaway and Sant'Anna (2020) in the context of dynamic treatment effect. Withal, our results are aligned with other studies that analyze full-time school policies (lengthening school hours combined with other modifications in the school environment), showing its positive impact on student learning. Nevertheless, it also emphasizes that full-day school can reinforce education inequality, highlighting that access to students who do not fit this program needs to be considered.

1 Literature Review

Our paper is related to two literature: school competition and full-day schools. The literature about school competition is extensive for countries that adopted school choice programs. Schools' reform that implemented vouchers and charter schools across USA states have produced numerous papers. According to Urquiola (2016), these school choice reforms raise relevant questions, like what is the effect of those reforms on traditional public schools. School choice advocates argue that those programs can improve neighboring public schools through increased competition. Belfield and Levin (2002) and Jabbar et al. (2019) make a systematic review of papers that have estimated the impact of school competition. The conclusion of both studies is almost the same: although competition and education quality are regularly positively associated, these effects appear to be very modest.

Charter school studies have produced various works addressing the spillover (indirect) effect on traditional public schools. Theoretical models highlight the general equilibrium in the educational market with a closer charter entry, shaping traditional school inputs (Mehta, 2017) along the non-random demand side of parents for charters (Walters, 2018). However, empirical estimates are heterogeneous, with some negative spillover (Ni, 2009; Han and Keefe, 2020), null (Bettinger, 2005; Zimmer and Buddin, 2009) or positive (Hoxby, 2003; Booker et al., 2008; Cordes, 2018a) for students achievement in public schools. Harris and Chen (2021) make a system-level empirical analysis considering the direct and indirect effect of charter schools, getting a net effect of the policy, and finding positive results.

However, much of the studies of school competition are primarily interested in the interaction between the public and private sectors in education. An ignored source of competition is public schools with themselves. Not to mention, public schools are the major player in educational markets in almost all underdeveloped countries. Indeed, Hanushek and Rivkin (2003) argue that "the most important element of competition comes from other public schools". Some influential works of Caroline Hoxby look for competitive effects of public schools in the USA (Hoxby, 2000, 2003). The main results are that more competition between public schools positively affects their productivity. Hanushek and Rivkin (2003) look for a potential impact of public schools competition on the teachers' market.

Full-day schools are a prominent source of public school competition ignored by the literature. The indirect effect that this policy may have on regular schools is very similar to charter schools. For example, it can induce student sorting because parents of different backgrounds evaluate school quality in different ways (Burgess et al., 2015) or because schools make a concealed selection of students even though there is no formal mechanism to do so (Alves et al., 2015). Public school teachers may also be differently attracted to this kind of policy. Likewise, there is no clear idea of how regular public schools may react to it.

According to Alfaro et al. (2015), full-day school studies are inside the broader literature of extended school days, which has focused on the impact of extending school days on student achievement and labor market outcomes. Alfaro et al. (2015) make a comprehensive review of impact evaluations regarding extending the school day in Latin America, showing that the studies present mixed results, but with positive effects being more common. The paper concludes that lengthening school time is insufficient to improve educational quality, being necessary a combination with other changes in the school environment. Full-day school programs are this kind of policy: reforms that jointly extend the school time with other initiatives.

Different Brazilian extended school day policies have been analyzed. Most of them relate to increased instructional time policies without complementary initiatives. Results on students' achievement are mixed, presenting positive results (Xerxenevsky, 2012; Cruz et al., 2017), null (Aquino, 2011) and even negative (Almeida et al., 2016; Xerxenevsky, 2012). However, for studies focused only on full-day school policies, the results are consistently positive (Rosa et al., 2022; Kawahara, 2019). Besides, Rosa et al. (2022) methodologically improves the previous analyzes by controlling for students' past scores and attenuating sorting concerns. They find a program impact in the full-day school policy of Pernambuco of 0.22 standard deviations in student test scores.

Regarding the spillover effect of the policy, the literature is much more limited. Rosa (2019) investigated consequences to local private school market due to full-day schools implementation in Pernambuco. The author finds that private high schools decrease their enrollment after competing with a public full-day school and increase their likelihood of closing. Surprisingly, Kawahara (2019) is the only work that tries to evaluate spillover effects on traditional public schools. The author finds a negative impact on students' test-score and failure-rate and an increment in age/grade distortion. However, the study has the drawback of poorly defining the relevant market for the spillover. The author considered that every public school in the same city as a full-day school suffered a spillover. In large cities, for example, the installation of a full-day school in one extreme of the city may have little impact on a regular school on the other extreme.

The state of São Paulo has a regulation for the selection of students based on the proximity of student residence and school location. The general criteria for enrollment are the proximity between public school and kids' residence, in which the distance between the two can not surpass 2 kilometers (Alves et al., 2015). Selection based on proximity gives options for parental choice. However, with the implementation of full-day schools, parents can choose between a regular public school or a full-day school. Nevertheless, because the supply of full-day schools is limited, this translates into a greater likelihood of those schools screening students (OECD, 2019).

Studies outside economics have qualitatively examined the relation between PEI schools and regular public schools.[4] Girotto and Cássio (2018) investigate the spatial distribution of PEI schools in the city of São Paulo. They exhibit that better socioeconomic regions had more full-day schools, and the policy reduced the number of students of better socioeconomic

[4]Although the central question of these works was not the effect of full-day school on traditional school, they comment on this somehow.

backgrounds in neighbouring public schools. Furthermore, TCU (2016) has monitored the program, listing diverse sorting effects that may occur in the program after analyzing a questionnaire applied to students and parents of São Paulo state schools. The data shows that students from PEI schools are generally more motivated, and their parents are worried about their education. Also, the report comments on the movements in the teacher market when a PEI school starts to operate.

Therefore, we advance the previous studies by being the first to evaluate the causal effect of São Paulo's state full-day school program and also its effect on neighbouring public schools. We better define the relevant market for spillover considering school distances. Furthermore, after verifying the presence of spillover from full-day school, the analysis of its direct effect is revised, removing the spillover bias, something that no paper in the full-day school literature has done yet.

2 Background of Programa de Ensino Integral (PEI)

In Brazil, the state government has a central role in offering secondary education, responding to 84% of all students enrolled in 2019. Still, 89% of these students stay at regular schools (part-time), with an average daily duration of the school day of 4.5 hours (Almeida et al., 2016) and associated with a historical stagnation in their quality. Also, according to the 2018 Program for International Student Assessment (PISA), Brazilian public schools are among the last countries in terms of their school hour, standing in the 65th position of 78 countries. Therefore, the sixth goal of Brazilian's National Educational Plan (PNE) promulgated in 2014 establishes that until 2024, at least 50% of public schools must offer full-time education, with a minimum of 7 hours per day. To accomplish the PNE's goal, most Brazilian state government has been adopting its full-day program.

Brazil has adopted different policies to increase instructional time. The federal program *Mais Educação* gave some schools financial support to implement extra activities during the extended time, but not joint with other changes in the school environment. However, the program ineffectively improved schooling outcomes (Almeida et al., 2016).[5] The full-day school policy of Pernambuco state was the first to be successful in high schools in the 2000s. The program had knowledge support from *Instituto de Co-responsabilidade pela Educação* (ICE), and beyond lengthening math and language instructional time, the program changed other school resources. Since then, other state governments have been using ICE consultancy to expand their school hours. São Paulo is one of them.

[5] *Mais Educação* program existed between 2007-2016. After some studies indicated that the program was ineffective, the government changed the program's conception to the *Novo Mais Educação*.

The Integral Education Program (*Programa de Ensino Integral*, PEI) is the first program to transform traditional (part-time) public high schools into full-day in São Paulo. It started in 2012 with 16 pilot high schools distributed across São Paulo's municipalities. Since then, other regular state schools have joined the program every year. In addition, although high schools were the initial focus of the program, it has been expanded to public elementary schools.

The major transformation of a PEI school is the increase in daily instructional time. Students can spend seven to nine hours per day at school. Figure A1 exhibits the mean daily hours that students spend at school per grade for PEI schools and regular ones. Already in the first year of joining the program, PEI schools increase considerable instructional time for all high school grades. Before entering the program, schools gave about 5.1 hours of instructional time, and when in the program, the time went to 8.7 hours, an increase of 70%. At the same time, regular schools daily hours stayed in the same patamar (around 5 hours) in all years. The extra time of PEIs schools is for teaching more portuguese, math and a diversified curriculum with non-mandatory subjects, like life project and study guidance. However, only mandatory subjects have exams that students need to pass.

Moreover, schools have other changes when becoming PEI. There is a change in the management, with more accountability for results. Teachers must have an exclusive dedication to the school (40h per week) while earning a gratification up to 75% of their basic salary. The change in school management permits PEI managers to substitute underperforming teachers, transferring them to a closer state school. Also, the school's infrastructure improves with the creation of thematic rooms and labs.

To become part of the program, initially, the regular school needs to show interest. The process involves a few steps. First, the director of the school needs to consult the school council.[6] After an affirmative decision, the director assembles the school staff and students' parents to communicate the decision. Then, the school needs to fill out formal documents and send them to the state education department. Finally, the state government decides which interest schools will join the program next year based on some criteria, such as correct documentation, initial adequate infrastructure, municipality and school size (Secretaria da Educação, 2014, 2019). However, Valentim (2018) verifies that no formal document of the educational secretary specifies which school the program ought to attend. Furthermore, once in the program, the school does not turn out part-time in any other year.

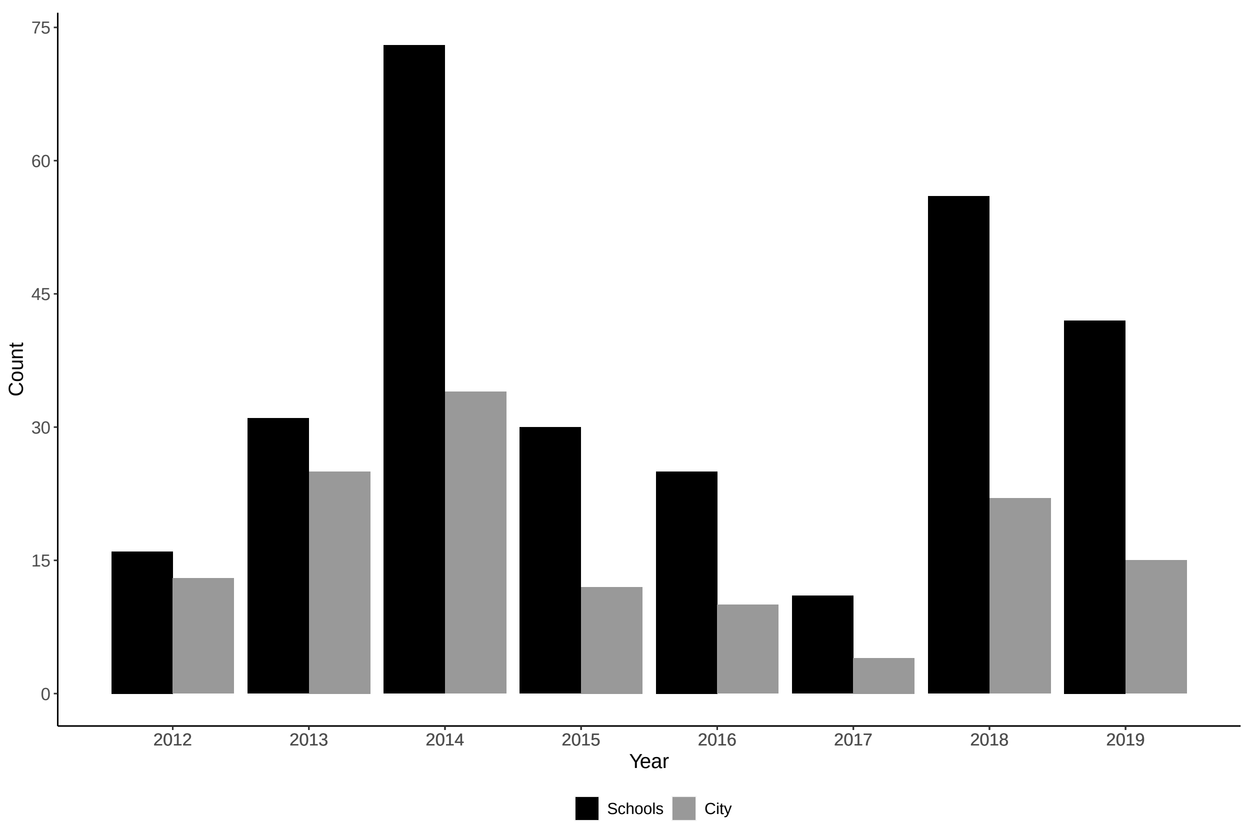
The enrollment process in the full-day school is the same as in São Paulo's regular schools. There is no selective exam (TCU, 2016), and parents need to show interest in enrolling their child in the state school system, defining its preference for the regular or full-day school. Then

[6] It is composed of 20 to 40 members of all school community, like managers, teachers and parents

the state board of education selects which school each student will attend based on previous enrollment and proximity of students' residence and the school. When a regular school converts to PEI, there is a sharp decrease in total enrollment (56%). This can happen both because students are not interested in staying more hours in school or because schools can not comport the same amount of students then when offering a part-time period. Although we can not determine which option is more relevant, our talking with PEI school secretaries indicates that the demand side is the main cause of the fall in enrollment.

PEI program still is in expansion. Each year, regular state schools adhere to the program. Figure 1 exhibits the magnitude of this expansion for secondary education. On average, 35 schools become full-day each year. Furthermore, the program deliberately spread to municipalities with no full-day school, as shown by the grey bars. At the end of 2019, 135 cities had a full-day school. Also, 272 secondary schools offered full-day time, corresponding to 60847 students enrolled, representing 7.5% of schools and 5.3% of enrollment from São Paulo's public regular education.

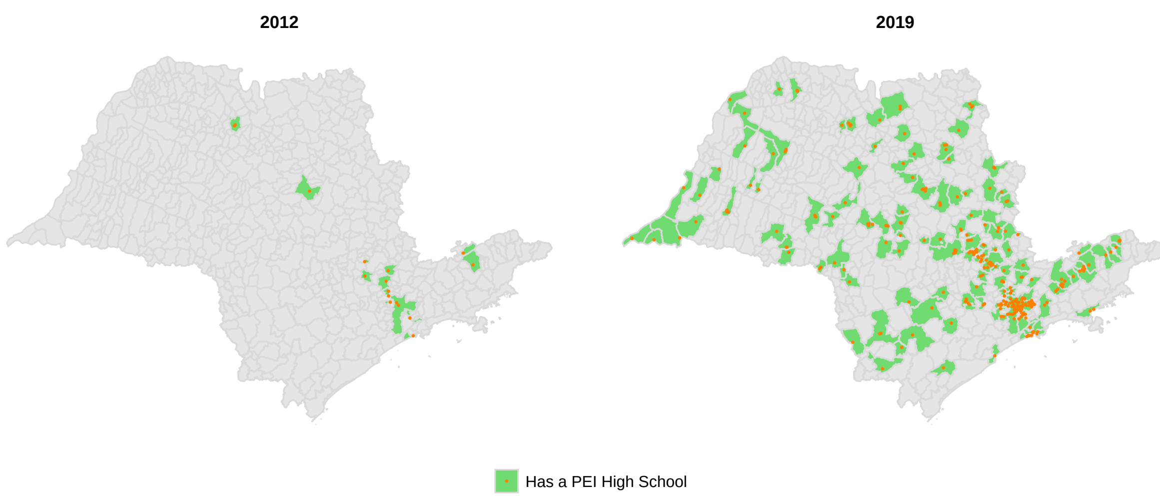
Figure 1: Evolution of High Schools PEIs



Notes: School bars represent newly schools that adhered to the program; city bar is the number of cities that had a full-day school for the first time.

Figure 2 exhibits the spatial location of PEI schools over the years. There is a higher concentration of full-day schools in São Paulo's metropolitan region, where population density is higher and wealthier. However, more countryside cities have had at least one school participating in the program over the years. Girotto and Cássio (2018) comment on PEI schools' locations in São Paulo city and verify that they are regularly in more privileged areas.

Figure 2: Geographical location of PEIs



Notes: Municipalities paint green have at least one PEI high school. The orange points represent the exact location of PEI schools.

3 Data and Descriptive Statistics

3.1 Data

Different datasets form our final panel at the school level from 2010 to 2019. The primary dataset used is the Brazilian Basic Educational Census, an administrative data of all

Brazilian schools made by the National Institute of Educational Studies and Research Anísio Teixeira (INEP). It informs annually schools' characteristics like infrastructure, enrollment by grade and staff traits. It also contains data at the student level and gives their demographic characteristics (gender, skin color, and age). Also, the students' level dataset has a unique identifier for students until 2017. We use this information to construct some flow variables (student dropout and failure) and to analyze student migration between public schools to define the relevant distance for the local market of each PEI school.

Dados Abertos da Educação de São Paulo informs which school joined PEI program each year. However, it does not specify the educational level at which schools offer full-day education. Thus, we look at the daily hour duration of these schools in the Educational Census and consider those having at least 7 hours of daily class in secondary education as a PEI school.[7]

São Paulo State School Performance Assessment System (SARESP) gives students' standardized test scores in language and math in the last year of high school for all state school students. We also use the National High School Exam (Enem) dataset to characterize students' socioeconomic characteristics in the last year of high school. Unlike SARESP, which is mandatory for all state schools, ENEM is done for only those students who want to enter college. Thus, we use SARESP to have schools' learning achievement (normalizing test scores at the students' level and then aggregating at the school level) and ENEM to characterize schools' average family income and parents' education.

To define the relevant school market for public schools, we use INEP data containing the geolocation of Brazilian schools. The dataset contains latitudes and school identifiers of almost every São Paulo state school. When the latitude is missing, we fill it manually.

After merging the datasets using schools' identifiers, we filter the data to keep only São Paulo's state regular high schools. Furthermore, because there is PEI school only in urban areas, we drop rural schools. The final dataset is a school panel covering 2010 to 2019, with 2998 schools per year. Also, to analyze PEIs' impact on treated schools, we add observations of schools that have converted their regular secondary education into the PEI program (271 schools).[8]

[7]After an information request to the São Paulo Educational Secretary, the high schools that adhered to PEI was available. However, the daily hour duration of a few of these high schools was less than 7 hours. So, we have maintained our definition of full-day school. Nonetheless, the difference between our construction and the information request was only 11 schools

[8]We removed 13 PEI schools that, when they started to offer secondary education, had already done into the PEI program. We exclude these schools from the analyses because they do not have historical observation and generate a different type of spillover than other PEIs, only attracting students from regular schools, not expelling

3.2 Treatment Definition

To evaluate the spillover effect of full-day school, the treatment group must encompass all schools indirectly affected by full-day creation. Recent studies have highlighted the importance of considering geographical distance when analyzing competition in the school market (Rosa, 2019; Misra et al., 2012; Cordes, 2018b). The relevance of this dimension comes from the perception that public school interacts more with closer schools.

Furthermore, distance is also a priority in São Paulo public schools' enrollment, where there is an "enrollment sectorization". The general criteria for enrollment are the proximity between students' residence and the school (Alves et al., 2015). Also, São Paulo municipalities are diverse and have different characteristics in terms of population and number of schools. Therefore, the local market for each school has its relevant distance. So, we define the neighbours' groups as regular schools in the same local market as a full-day school, in which each full-day school has its own local market extension. Formally, consider PEI_g the set of all regular schools converted into full-time in year g, then neighbours' groups in year g of our local market definition are:

$$G_{ig} = \begin{cases} 1 \text{ , if } |\theta_i - \theta_j| \leq h_j \text{ for any } j \in PEI_g \\ 0 \text{ , Otherwise} \end{cases} \tag{1}$$

Where G_{ig} is a dummy for the regular school i being neighbour of full-day schools converted in year g, θ_i and θ_j are the locations of regular and full-day school and h_j is a distance parameter in kilometers specific for each full-day school. The h_j parameter is defined using Census enrollment dataset (student level). We analyze the migration (leave and enter) of students in the PEI schools to other public schools in the 2010-2017 window and define h_j as the distance between schools of the 70th percentile migrated student. Schools within more than one PEI local market in different years were considered only part of the neighbours' group of the first closer PEI created. Also, if PEI's neighbours exceed 15 schools, we keep the 15 closer ones (this correction occurs in only 18% of PEI schools).

The local market definition is flexible enough to consider different particularities of the public education market. For example, in high-density municipalities like São Paulo, some PEI schools have a h_j of less than 2 kilometers and have more than five regular state high schools in this area. On the other hand, in more countryside municipalities, like Cândito Mota, PEI school have a h_j equal to 10km, and only three high schools are within this area. Likewise, this definition permits local markets to vary within cities, considering each school's local specificity, like PEI schools in São José dos Campos, with schools with a local market of 2km in the center of the city and one of 5km in the periphery area.

Other neighbours' definitions are considered for robustness. We considered neighbours' definition with a constant h (2 and 3 kilometers) and the three schools that received more students migrating from a PEI school at the year of conversion.[9] Table 1 display the numbers of neighbours schools for each definition. Each PEI created has about five neighbours schools suffering spillover in the local market definition.

Table 1: High schools neighbours of PEIs

	2012	2013	2014	2015	2016	2017	2018	2019	Total
PEIs	10	30	69	30	24	10	56	42	271
Local Market	95	192	335	121	111	52	265	187	1358
2km	33	95	254	123	79	42	230	118	974
3km	57	177	397	190	140	54	311	167	1493
3 main migration	28	84	171	69	59	21	155	95	682

Notes: PEIs are the number of full-day high schools considered. The other rows are the number of neighbours for each definition.

3.3 Descriptive Statistics

Because of the non-random selection of schools that adhered to PEI, the most capable schools were the first to be selected for the program. The non-random selection of PEI influences our treatment groups. Traditional schools inside PEI local market are located in privileged areas within cities and differ from other state schools. Table 2 confirms this reasoning by showing the mean and standard deviation for some variables of our treatment group (neighbour schools) and other regular schools for the pre-treatment period (2010 and 2011)..

[9] Because we can follow students until 2017, this definition encompasses only PEI schools created until that year.

Table 2: Balance Table of Local Market Neighbours

	Neighbors before PEI (N=2491)		Others (N=3920)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
School Characteristic						
Daily school duration	4.76	0.41	4.69	0.42	-0.07	0.00
Enrollment	503.12	321.26	392.34	253.22	-110.78	0.00
Class Size	35.32	4.61	33.61	5.49	-1.71	0.00
Students Characteristic						
Girls' Share	0.51	0.04	0.51	0.04	-0.00	0.00
Special Students	2.63	3.52	2.17	3.15	-0.46	0.00
Students Age	16.72	0.33	16.73	0.41	0.01	0.31
Family Income	2293.06	542.18	2057.72	487.41	-235.34	0.00
Father schooling (stages)	2.13	0.34	1.94	0.33	-0.19	0.00
Mother schooling (stages)	2.28	0.33	2.11	0.33	-0.16	0.00
Teacher Characteristic						
Graduate teachers Share	0.25	0.16	0.25	0.17	-0.00	0.80
Statutory teachers Share	0.31	0.34	0.29	0.32	-0.03	0.00
Quality						
Math Test Score - SARESP	269.47	13.04	268.94	13.34	-0.53	0.12
Language Test Score - SARESP	266.08	14.26	264.18	13.61	-1.90	0.00
IDESP	1.79	0.59	1.80	0.61	0.01	0.42
Infrastructure						
Computer Lab	0.97	0.16	0.97	0.17	-0.00	0.70
Library	0.07	0.26	0.07	0.25	-0.01	0.44
Internet	0.99	0.11	0.99	0.11	-0.00	0.93

Notes: This table shows summary statistics describing our school panel. The Neighbour group covers the pre-treatment group (before a nearby PEI school enters the local school market). The "Others" group are all São Paulo state high schools outside PEIs local market. The statistics consider only the years 2010—2011.

The statistics indicate that treated schools are, on average, in more developed and populated locations. Enrollment is higher in the neighbours' group, with students from a wealthier and more educated families. These schools also admitted more teachers from a public contest, and neighbour schools scored more in the state standardized test. All of those variables' mean differences are statistically significant. However, the magnitude of these differences is not high, and there is no difference in school infrastructure. The difference is considerable only for variables related to school size (number of enrollment). The difference in school size confirms that neighbouring schools are, on average, in the most populated location across São Paulo state.

Likewise, PEI schools differ from regular ones before converting into full-time. Table A1 exhibits the balancing statistics for PEI schools and confirms that these schools were already of better quality, with better initial infrastructure and attending students from a better socioeconomic background. Nonetheless, again, the magnitude of the differences is small.

4 Empirical Strategy

Since PEI has been implemented in different years and locations in a staggered structure, it allows the adoption of an event-study framework to identify the impact of full-day schools on their public school market. However, competition shock caused by full-day schools may have heterogeneous effects on regular schools across cohorts or time. So, we left aside the traditional event-study approach (Two-Way Fixed Effect - TWFE) to use the dynamic difference-in-difference procedure based on Callaway and Sant'Anna (2020). This estimator overcomes the "forbidden comparison" between already-treated groups, making TWFE estimators biased.[10]

The key identification assumption in our setup is that the timing of neighbours schools receiving a competitive shock of a full-day school is as good as random. The PEI program has not assigned schools randomly. However, São Paulo's educational secretary considered only schools' initial pre-conditions to become full-time. Therefore, local public education market was not considered for school selection. Table A2 presents evidence corroborating with that by showing that public district-level market enrollment characteristics do not predict schools that entered into the program. However, a bunch of school characteristics does predict treatment status.

The estimation procedure in Callaway and Sant'Anna (2020) relies fundamentally in identify group-specific average treatment effects on the treated, $ATT(g,t)$. It reflects average treatment effects on the treated in period t for the group treated at time g. In our setup, $g \in \{2012, 2013, ..., 2019\}$, in which group $g = 2012$ includes all public high schools that are within the local market of PEIs implemented in 2012. This procedure was the advantage of focusing on the family of $ATT(g,t)$'s, which do not impose restrictions on treatment heterogeneity. The potential outcome for this group-specific average treatment effect is:

$$ATT(g,t) = \mathbb{E}\left[Y_t(g) - Y_t(0) \mid G_g = 1\right], \text{ for } t \geq g \tag{2}$$

Where $Y_t(g)$ denote the potential outcome at time t for the group first treated in period g, and $Y_t(0)$ is the potential outcome for those units had they not been treated. G_g is a binary variable equal to one for all units that have received treatment in time g, as defined in

[10]Goodman-Bacon (2021) proves the bias that the usual 2x2 Difference-in-Differences and TWFE can have in the context of multiple time treatment periods. Sun and Abraham (2021) do the same job for event-study approaches. de Chaisemartin and D'Haultfoeuille (2022) and Roth et al. (2022) summarise this recent econometric literature.

equation 1. Because observing $Y_t(0)$ is impossible for treated units after g, the never-treated and not-yet-treated schools ($D_{it} = 0$, where D_{it} is a binary variable equal to one if unit i is treated in period t) form a proxy for what would have occurred if some schools had not been treated. Also, Callaway and Sant'Anna (2020) weaken the idea of parallel trends after controlling for some baseline (time-invariant) covariates (X):

$$ATT(g,t) = \mathbb{E}\left[Y_t - Y_{g-1} \mid X, G_g = 1\right] - \mathbb{E}\left[Y_t - Y_{g-1} \mid X, D_t = 0\right] \tag{3}$$

The first term of the expression is the evolution of the outcome for the treatment group conditional on covariates and the second term is the equivalent for the control group. Estimating $ATT(g,t)$ with pre-treatment covariates relies on a propensity-score-based approach to generate a weighted differences in means between treated and untreated units.. If the parallel trend assumption only holds conditionally on covariates, this adjustment is necessary. Conditional parallel trends only require that schools with the same value of time-invariant covariates have equal evolution of their never-treated outcome. Table 2 revealed that the treatment group is different in level from the never-treated. Although this difference is uninformative about the evolution of the interest variables, we conjecture that similar schools are more likely to exhibit the same evolution of educational variables throughout time. So, our main specification will calculate $ATT(g,t)$ using pre-treatment values of school infrastructure, parents' education, school quality, and teachers' characteristics as covariates (X) for weighting. Also, we include the not-yet-treated schools to the control group. Then, the estimation using the Doubly Robust method (recommended by the authors) becomes:

$$ATT_{dr}^{ny}(g,t) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}\left[G_g\right]} - \frac{\frac{p_{g,t}(X)(1-D_t)(1-G_g)}{1-p_{g,t}(X)}}{\mathbb{E}\left[\frac{p_{g,t}(X)(1-D_t)(1-G_g)}{1-p_{g,t}(X)}\right]}\right)\left(Y_t - Y_{g-1} - m_{g,t}^{ny}(X)\right)\right] \tag{4}$$

Where $m_{g,t}^{ny}(X)$ is the evolution of interest variable for the control group and $p_{g,t}(X)$ indicates the probability of being first treated at time g.[11] Therefore, our estimates using Callaway and Sant'Anna (2020) consider the never-treated and not-yet-treated schools as the control group. We evaluate different outcomes (Y) at the school level, such as students' achievement, flows (drop-out and failure rate), and teachers' and students' characteristics. With $ATT(g,t)$ calculated from Equation 4 using the plug-in principle, all other aggregations become straightforward by different weightings of the $ATT(g,t)$'s. We present summary results from the Simple Aggregation, a weighted average of all group-time average treatment

[11]Formally, $m_{g,t}^{ny}(X) = \mathbb{E}\left[Y_t - Y_{g-1} \mid X, D_t = 0, G_g = 0\right]$ and $p_{g,t}(X) = P\left(G_g = 1 | X, G_g + (1 - D_t)(1 - G_g) = 1\right)$

effects with weights proportional to the group size, and the Overall Event-Study. For the event-study plots, $ATT(g,t)$'s are averaged into average treatment effects at different lengths of exposure to the treatment (e). Consequentially, the Overall Event-Study averages the average treatment effects across all lengths of exposure to the treatment ($e \geq 0$). For inference, we use Callaway and Sant'Anna (2020) recommendation of bootstrapping procedure. All errors are clustered at the school level.

5 Results

5.1 PEI direct effect

Before presenting the main result regarding the spillover effects of full-day schools, we show PEIs' direct impact on schools that adhered to the program using Callaway and Sant'Anna (2020) as described in the previous section. We increment our dataset of regular schools with PEI schools for this estimation. The estimations use baseline covariates and observations of never-treated (regular schools) and not-yet-treated (school before converting to PEI) as the control group. The results are in Table 3.

The general result verifies that the program changed school inputs, leading to better school achievement and student flow. Firstly, we confirm the program's object of increasing daily hours. PEI schools have more than 3.7 daily hours than regular schools, a substantial increase considering the initial 5 hours. Also, teachers' composition changed, with more educated teachers and from the state contest. Changes in school inputs markedly impact students' achievement scores. The annual normalized portuguese and math score in SARESP rose by more than 0.4 standard deviations. Furthermore, the flow indicators of students in these schools improve with the share of drop-out and failure of students declining.

Nevertheless, the improvement in the quality of PEI schools comes with the sorting of students. High schools' enrollment decreased by around 100 students, a considerable amount considering that these schools had 408 students enrolled before entering the program. Also, students who stay in full-day school are younger (0.3 years old) and from wealthier families. Surprisingly, the full-day program also affects students' gender composition, with more girls staying in those schools than boys. Every estimation is statistically significant at a 0.01 level, and the Simple Aggregation and Event Study aggregation of Callaway and Sant'Anna (2020) point in the same direction with a closer point estimation.

We investigate further students' sorting due to full-day schools in SARESP microdata. SARESP is done annually by the São Paulo state to evaluate the quality of their schools. Students of 3th, 5th, 7th, 9th, and 12th grades of all state schools take the test. The dataset

Table 3: PEI impact at school level

Variable	Simple Aggregation	Event-Study	Pre-Treatment Mean
School			
Daily hour	3.761***	3.833***	5.01
	(0.038)	(0.038)	
Math Score - Saresp (Z)	0.476***	0.579***	0.04
	(0.027)	(0.036)	
Language Score - Saresp (Z)	0.407***	0.486***	0.06
	(0.024)	(0.027)	
Failure Rate	-0.03***	-0.034***	0.10
	(0.004)	(0.006)	
Drop Rate	-0.055***	-0.066***	0.10
	(0.004)	(0.004)	
Students			
Enrollment	-115.844***	-96.66***	408.47
	(9.57)	(13.491)	
Age	-0.318***	-0.305***	16.63
	(0.018)	(0.017)	
Girls' Share	0.039***	0.047***	0.50
	(0.004)	(0.005)	
Family Income	123.273***	187.513***	2303.11
	(29.149)	(58.142)	
Teachers			
Teachers' Education	0.017***	0.011***	5.51
	(0.003)	(0.004)	
Statutory teachers %	0.187***	0.209***	0.61
	(0.013)	(0.015)	

Notes: The first two columns represent $ATT(g,t)$'s summaries. The first is the simple weight aggregation, and the second is a dynamic aggregation (event-study). All estimations use baseline covariates, and the control group includes never-treated and not-yet-treated schools. The last column is the variables mean for PEI schools before converting to full-day. The variables Failure Rate and Drop Rate have observations until 2017. Standard errors are clustered at the school level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

has a unique identifier of students, so we observe students' grades and characteristics over the years when they have done all primary education in public school. The dataset also has students' socioeconomic characteristics from a questionary filled out by parents until 2013.[12] With this dataset at the student level, we run the following regression:

$$Y_{ict} = \alpha_t + \eta_c + Score\_9th_{ict} + X_{ict} + \epsilon_{ict} \tag{5}$$

Where Y_{ict} is a dummy related to student enrollment in a PEI school, α_t and η_c are time and city fixed effects. $Score\_9th_{ict}$ is the normalized test score of the student in the 9th grade in Saresp, and X_{ict} is a vector of controls. This regression investigates whether students'

[12] Although the questionary was applied until 2013, we can determine students' socioeconomics characteristics until 2016 because of the students' identifier. For example, 9th-grade students that made SARESP in 2013 have their information filled when they remake the test in 2016 in the 12th grade. With that construction, our 2011-2016 dataset comprises 66% of all regular state high schools students

previous test scores help predict enrollment in full-day school. The results are in Table 4

Table 4: Student sorting in PEI school

	PEI	Migrated to a PEI	PEI pre-treatment
Math Score 9th Grade	0.0005**	0.0005***	-0.0003
	(0.0002)	(0.0001)	(0.0004)
Portuguese Score 9th Grade	0.0007***	0.0006***	0.0002
	(0.0002)	(0.0001)	(0.0005)
Female	0.0010***	0.0011***	0.0005
	(0.0003)	(0.0002)	(0.0004)
Age	-0.0011***	-0.0008***	0.0004
	(0.0002)	(0.0002)	(0.0006)
Did Kindergarten	0.0001	-0.0002	0.0029**
	(0.0003)	(0.0003)	(0.0011)
Father's education	0.0007***	0.0005***	0.0015***
	(0.0001)	(0.0001)	(0.0004)
Mother's education	0.0008***	0.0007***	0.0013**
	(0.0002)	(0.0001)	(0.0005)
Num.Obs.	1490153	1490153	1490153
R2	0.058	0.056	0.109
R2 Adj.	0.058	0.056	0.108
FE: Year	X	X	X
FE: City	X	X	X

Notes: Each column represents a Linear probability model. PEI is a dummy equal to one for students enrolled in a PEI school. Migrated to a PEI is a dummy for students who migrated from a regular middle school to a PEI in high school. PEI pre-treatment is a binary variable indicating students enrolled in PEI schools in the period before converting into the program. All regression includes year and city fixed effect. Standard errors are clustered at the school level. Regressions use SARESP's students level database on years 2011-2016.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The first column exhibit students' characteristics that explain enrollment in PEI schools. Younger students with better scores in 9th-grade and from more educated families are more likely to enroll in the full-day program. The analysis stays the same when looking only at students who enroll in PEI but have studied in a regular middle state school. However, when considering the enrollment of students at PEI in the pre-treated period (when schools were regular), students' achievement in 9th grade has no predictive power to explain enrollment. These results confirm a "cream-skimming"" effect of full-day schools.

Thereby, results presented so far indicated an impressive improvement in schools that adhered to the program. However, fewer students afford to stay in full-day school, and those who stay in full-day school are, on average, students from a better socioeconomic background and already with better grades. All of this raises the concern about what is happening in the other schools in the public school system.

5.2 Spillover Effect

The spillover analyses consider the dataset with only regular schools, using Callaway and Sant'Anna (2020) with observations never-treated (in this context, regular schools outside PEI local market) and not-yet-treated (regular schools that will become a PEI neighbour) as the control group, as described in the econometric section. Table 5 exhibits different aggregations for the impact of the full-day schools on their public educational market. The overall results reveal that regular schools have their human inputs changed because of the full-day school, worsening its educational outcomes.

As expected, regular public schools absorb students that left schools converted to the full-day program. More students are studying in regular schools after a closer PEI emerges. Furthermore, the composition of students enrolled in traditional schools also changed: students are older and from underprivileged families. These effects impact the class size and age/grade distortion in those schools. Teachers' composition also alters, worsening the average formation' quality of those who stay in regular schools and the share of them hired by the state contest.

These changes in inputs have an impact on regular school quality. The achievement in language and math worsens (range of 0.01 to 0.03 sd), with a deterioration sharper in math. Dropout also rises in regular schools, but no effect is noticed in the number of failures. Despite the statistically significant results, considering variables mean at the baseline, the spillover impact seems to be modest for almost all educational variables. More prominent effects are seen in enrollment, age/grade distortion and share of teachers with public degree (proxy of formation quality), increasing by more than 6% of its baseline level. For test scores, the negative impact in a range of 0.03sd seems modest compared to other full-day school programs' direct effect. For example, the full-day school program in Pernambuco increased students' achievement by 0.22sd in math, according to Rosa et al. (2022). Furthermore, our more accurate definition of schools that may suffer spillover (neighbour) detects more statistically significant and higher spillover effects of full-day schools compared to Kawahara

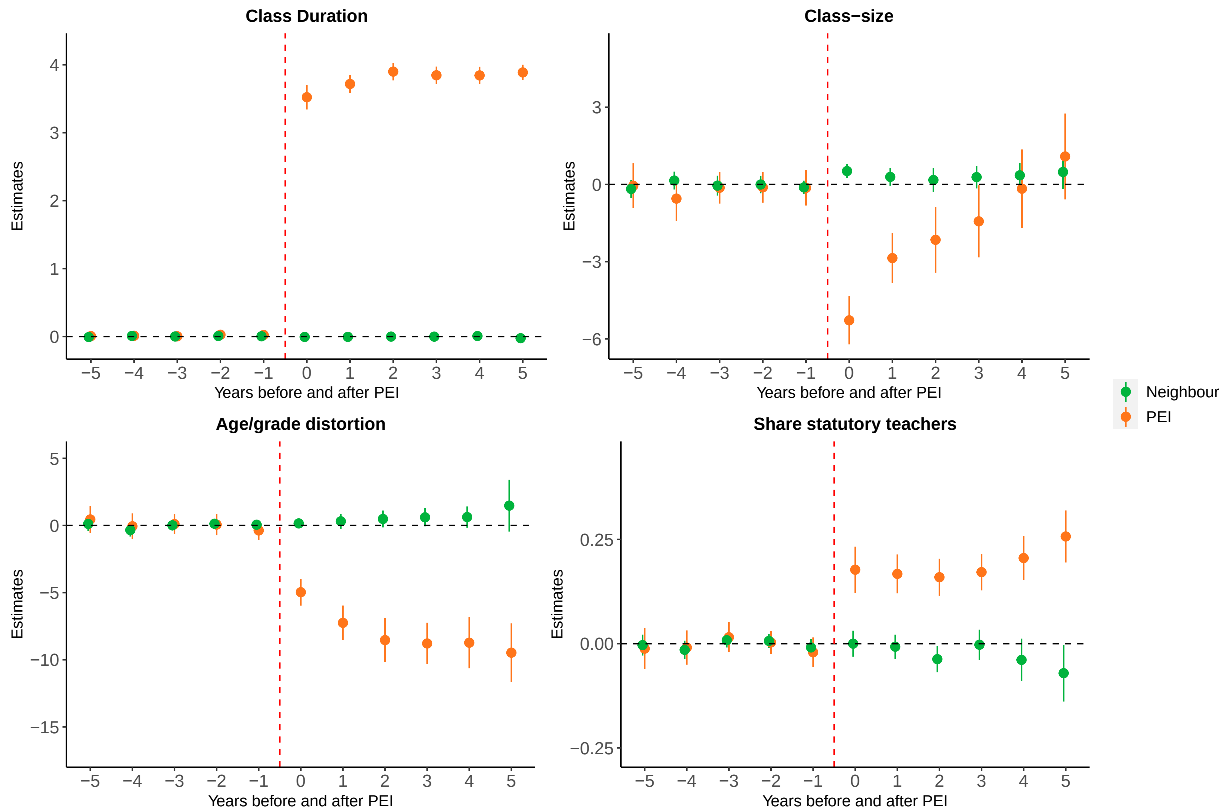
(2019).

5.3 Event-Study

We combine the previous results of the direct and spillover effect of PEI schools (Table 3 and 5) into a event-study aggregation. The results highlight the different magnitudes of benefits and costs of the full-day policy. Although full-day schools harm traditional schools, the program's benefits on treated schools are much larger. The event studies for -5 to 5 elapse time window from different outcomes are presented in Figure 3 to 5.

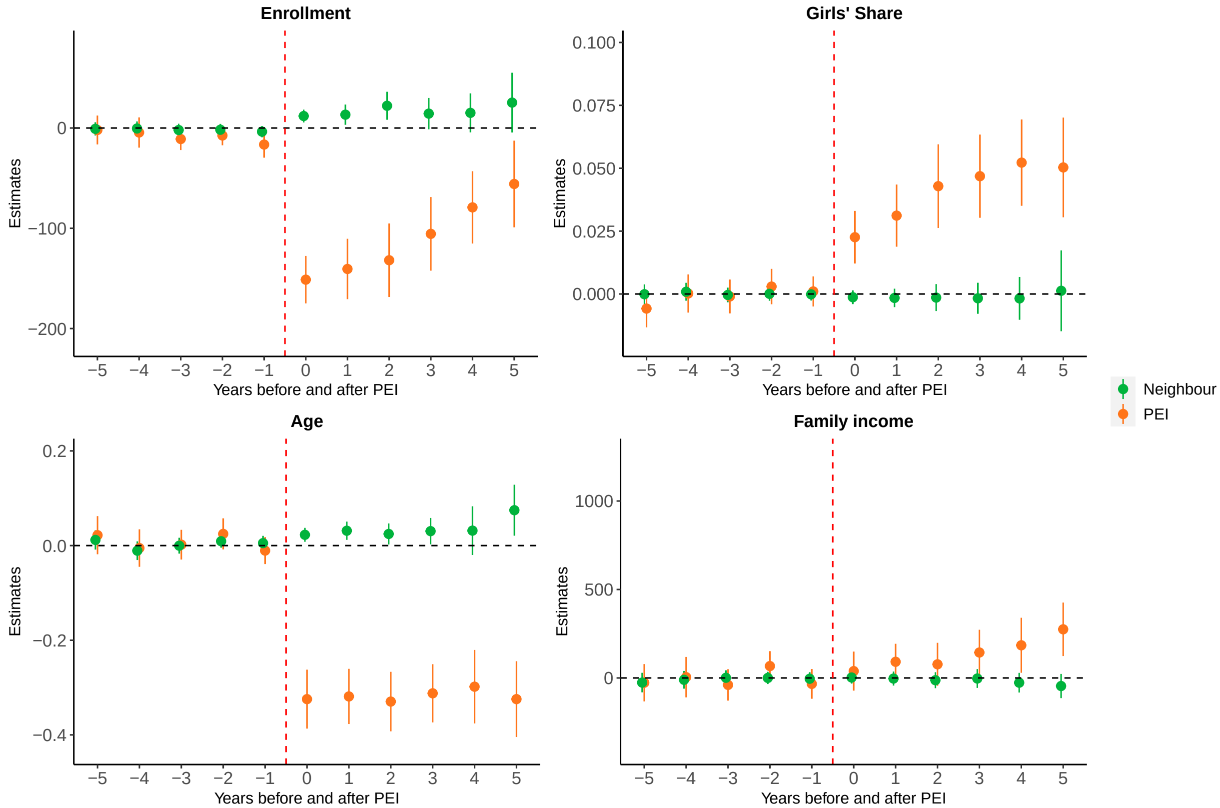
PEI schools have a considerable increase in daily school duration just in the first year of treatment, staying at the same level in subsequent years, while no change is seen in regular schools in the same school market. The enrollment at PEI schools decreases sharply at the beginning of the program, transferring students to regular schools, affecting the class size of both types of schools. Also, the composition of students changes, with younger students going to the full-day schools and girls demanding more full-day schools than boys. The share of statutory teachers in full-day school increases because this is one of the program requirements for teachers. These input changes affect PEI schools' math and portuguese achievement, which presents a growing pattern across program duration.

Figure 3: Event-Study of School characteristics



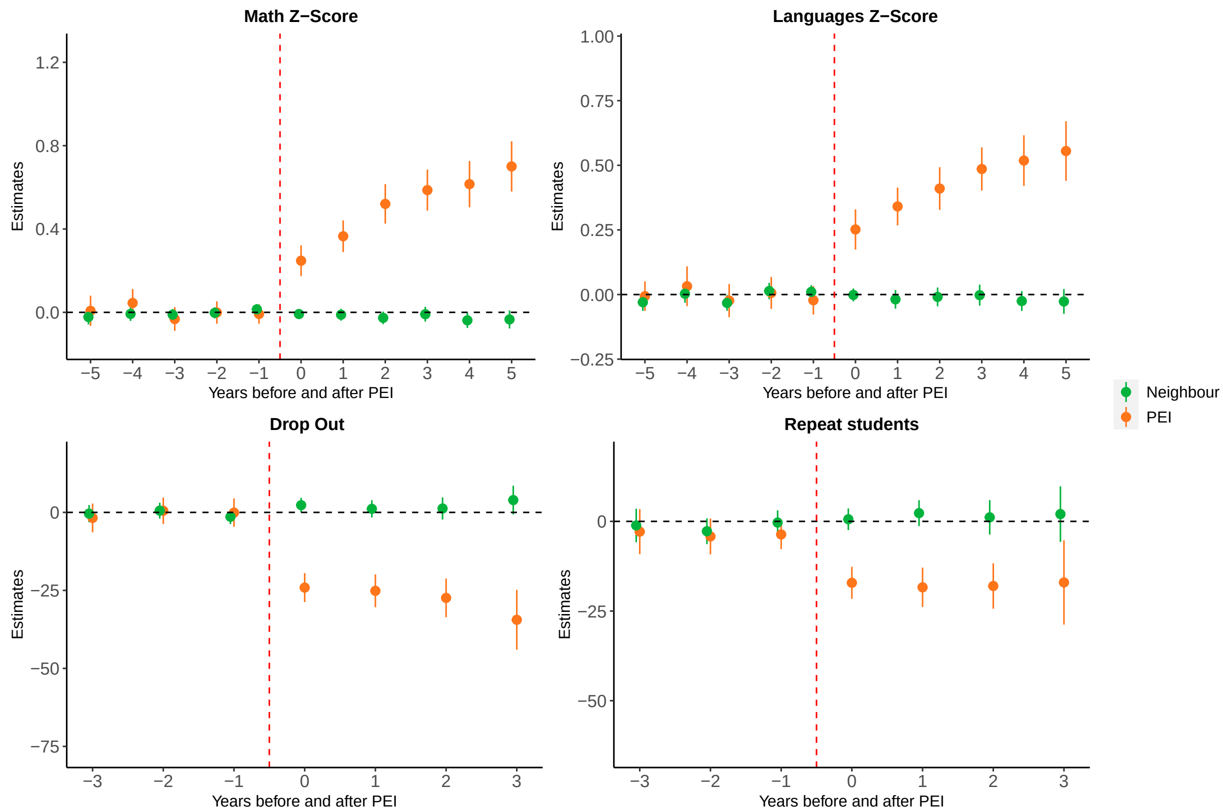
Notes: Event-study aggregation of different outcomes from Table 3 and 5

Figure 4: Event-Study of Students' characteristics



Notes: Event-study aggregation of different outcomes from Table 3 and 5

Figure 5: Event-Study of School Quality



Notes: Event-study aggregation of different outcomes from Table 3 and 5

The event-study framework also supports some assumptions made so far. First, many variables seem to have a dynamic effect across time, which compromises the analyses in the traditional TWFE approach. Likewise, the pre-treatment period gives a grinch of our parallel-trend assumption. Neither point estimate is statistically different from zero, so there is no evidence that PEI or neighbours schools evolved differently from regular schools (control group) in the pre-treatment period.

5.4 Direct effect controlling for spillover

The presence of a spillover of full-day schools compromises the estimation of PEI's direct effect (impact on schools that adhered to the program) because of the violation of the Stable Unit Treatment Value Assumption (SUTVA). There is a spillover bias in which the treatment effect of schools becoming full-day also affects regular schools (control group) outcomes. Therefore, we remake the PEI direct analyses to remove spillover bias using the estimation procedure proposed by Butts (2021).

This imputation estimator considers all units that have suffered some effect from the intervention using Gardner (2021) Two-stage difference-in-differences estimator that also avoids TWFE problems. The first stage of this estimation uses observations never-treated (regular high schools outside PEIs' local market) and not-yet-treated (PEI and neighbours schools before full-day implementation) to residualize outcomes using the estimated unit and time fixed effects. The second stage regresses the residualized outcomes on treated dummies and covariates. [13] Thus, this estimator recovers the ATT, removing spillover bias. Explicitly, the two stages are:

1. Estimate $Y_{it} = \alpha_i + \lambda_t + \epsilon_{it}$ employing only never-treated and not-yet-treated observations. Residualize $\tilde{Y}_{it} = Y_{it} - \hat{\alpha}_i - \hat{\lambda}_t$ for all observations.

2. Regress $\tilde{Y}_{it} = \beta_0 + \beta_1 PEI_{it} + \beta_2 Neighbour_{it} + \beta_3 X_i + \epsilon_{it}$ using all observations. PEI and Neighbours are dummies indicating PEI and their neighbours in year t

We use the same baseline controls variables of the previous sections in all regression and clustered standard errors at the school level. Table 6. exhibits the results both for Gardner (2021) and Butts (2021). The Gardner (2021) column is the Two-stage difference-in-differences estimation without controlling for the spillover (no $Neighbour_{it}$ dummy in the second stage). It can be seen that the results are close to the Callaway and Sant'Anna (2020) estimators, with all results significant, going in the same direction and similar point estimates. Butts (2021) column controls for the spillover bias and gives very similar results presented so far. Generally, removing the spillover bias only slight attenuates the point estimates, but PEI's direct effect continues to be significant and high even when controlling for its spillover. The results certify the previous findings that PEI benefits are much more substantial than its cost.

PEIs' learning impact continues to be larger than 0.4sd in math and becomes 0.36sd in portuguese after controlling for the spillover. Also, the change in school inputs continues

[13] To have a similar estimation as Callaway and Sant'Anna (2020), we use baseline covariates (time-invariant)

to be significant, however, with slightly smaller intensity. Butts (2021) also estimates the spillover into neighbours schools in the same regression framework. The results are in Table A3. Most results align with Callaway and Sant'Anna (2020), with estimates going in the same direction. The spillover continues to be statistically significant for learning outcomes, but with point estimates closer to -0.01sd for both portuguese and math. However, Butts (2021) spillover estimator does not capture the enrollment migration of students to regular school as good as Callaway and Sant'Anna (2020).

6 Robustness

6.1 Different Neighbors definition

To endorse spillover results in the previous section, we present results from different definitions of the neighbours' groups. First, the h parameter in Equation 1 is changed to a constant of 2, 3 and 5 kilometers. Likewise, another group of neighbours is considered by taking for each school that adhered to PEI, the three state high schools that had received more students from it.[14] The results are exhibited in Table 7 for the simple aggregation of Callaway and Sant'Anna (2020).

The results for the 2, 3 and 5 kilometers definitions confirm the local market neighbor estimates. All the estimations go in the same direction and generally with the same statistical significance. As we increase the radius of the neighbour group, the impact of student enrollment decreases monotonically, showing the importance of the proximity when considering the spillover effect. Nevertheless, not all estimates are statistically significant for all h. For example, the 2km neighbours show no relevant effect on math test scores, unlike the 3 and 5km neighbours. This heterogeneity highlights the importance of the adequate definition of neighbours group. When spillovers are statistically significant, they were already detected in the local market definition. Only for the number of students' failure, there is a statistically significant effect in the 3km and 5km neighbours definition that was not caught in our main definition.

The three main target schools' neighbour definition also presents results in the same direction but with a considerable increment in the point estimate for the variables related to student enrollment. These schools have 36 more enrollment because of full-day creation, a double relative to the radius neighbours specification. The sorting of students is the same: older students from underprivileged families (however, with no significance in family income).

[14] We make this analysis using the Basic Educational Census at the student level in which we can follow students from 2010 to 2017. With this limitation, only neighbours until 2017 are considered for this neighbour construction

Again, there is a negative impact on students learning both portuguese and math, and 5 more students drop out in these schools (against 2 of our mains specification). This definition highlights the heterogeneity of spillover for regular schools, in which although a larger group is suffering spillover, some of them are affected more intensely.

Finally, we define an unrealistic neighbours group containing regular high schools within 20 to 40km from PEI schools. As expected, the spillover effect on this group is statistically insignificant in all regressions. Only the enrollment variable appears to have a significant result (however, having an opposite signal than other neighbours' spillovers).

6.2 TWFE estimations

We also present the traditional TWFE estimates for both PEI direct effect and spillover as robustness. The interest coefficient is β of the regression:

$$Y_{st} = \eta_s + \theta_t + \beta D_{st} + \alpha X_{st} + \epsilon_{st} \tag{6}$$

Where D_{st} is a dummy indicator of treatment status for school s in time t (1 for PEI in the direct analysis or 1 for full-day school' neighbour in the spillover investigation). Table 8 display the results for the variables analyzed so far. PEI direct effect results by the TWFE with no covariates are similar to the estimations encountered so far, with the results continuing statistically significant and going in the same direction, but with differences in the point estimates. For the spillover analyses, although most of the results keep similar to what we had already found, there is a discrepancy relative to Callaway and Sant'Anna (2020) estimates for the enrollment variable, in which the TWFE estimates actually lead to a negative spillover on the number of students enrolled in neighbours schools.

Table 8: TWFE estimator for PEI and Neighbours

	PEIs		Spillover	
	(1)	(2)	(3)	(4)
Daily hour	3.559***	3.695***	-0.008	-0.019***
	(0.041)	(0.039)	(0.007)	(0.007)
Math Score - Saresp (Z)	0.063*	0.429***	-0.007	-0.014**
	(0.036)	(0.02)	(0.006)	(0.006)
Language Score - Saresp (Z)	-0.152***	0.336***	-0.006	-0.012*
	(0.035)	(0.017)	(0.006)	(0.006)
Failure Rate	-0.039***	-0.039***	-0.003**	-0.003*
	(0.008)	(0.004)	(0.001)	(0.001)
Drop Rate	0.005	-0.042***	0	0.002**
	(0.006)	(0.003)	(0.001)	(0.001)
Enrollment	13.185**	-142.923***	2.104*	-7.633**
	(5.728)	(11.027)	(1.242)	(3.699)
Age	0.713***	-0.299***	0.046***	0.051***
	(0.048)	(0.017)	(0.007)	(0.007)
Girls' Share	-0.018***	0.036***	-0.002**	-0.003**
	(0.006)	(0.003)	(0.001)	(0.001)
Family Income	94.184**	100.886***	-30.925***	-31.696***
	(43.541)	(22.392)	(7.541)	(8.119)
Teachers' Education	0.013**	0.014***	-0.003**	-0.002*
	(0.006)	(0.003)	(0.001)	(0.001)
Statutory teachers %	0.085***	0.162***	-0.02***	-0.016**
	(0.03)	(0.01)	(0.006)	(0.006)
Covariates:	Yes	No	Yes	No

Notes: Each element of the table represents a traditional TWFE regression. We estimate a model with and without controls used in the other regressions for both PEI and neighbours (local market definition) impact. Standard errors are clustered at the school level. The variables Failure and Drop-Out have observations until 2017.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Also, for TWFE estimates using the same set of covariates used in all our regressions produce a much smaller PEIs' impact. These relevant change in point estimates happens because controls in TWFE are contemporaneous variables that are affected by the treatment and also affect outcomes, generating a bad control situation through a mediator. The lack of controls in the TWFE estimates reduces the precision of the estimates and does not permit the use of the more relaxing assumption of conditional parallel trend. In summary, the results for TWFE estimates reaffirm most of our findings and secure the relevance of Callaway and Sant'Anna (2020) in the context of the heterogeneous treatment effect of full-day school.

7 Discussion

Full-day school policy in middle-income countries has raised concerns about student sorting and school equity in the public system. There is a lack of evidence of how harmfully the spillover effect of full-day school is. Previously works had focused only on the direct impact of lengthening school time, and the only work that had looked into the spillover of full-day schools on the public schools had an inadequate definition of neighbours schools (Kawahara, 2019).

Therefore, our analysis of the Programa de Ensino Integral (PEI) of São Paulo state contributes to the literature in three ways. First, we demonstrate that full-day schools generate a "cream-skimming" effect, retaining and attracting already higher achievers students from a better background in São Paulo's public educational system. Secondly, we estimate the spillover from full-day PEI program schools, carefully defining the relevant set of schools from which this policy may indirectly affect, using schools' geolocations and administrative information of students' enrollment across years. We find that neighbours schools worsened their achievement in standardized tests up to 0.03sd, retaining more students from an underprivileged background and retaining teachers with poorer educational formation.

Lastly, our work is the first to estimate the direct impact of full-day school removing the spillover bias that it generates in regular schools. Our findings clarify that the program's gains are much larger than its cost. The achievement gains are at least 15-fold higher than the loss generated in neighbours' regular schools. This result reinforces the beneficial impact of full-day programs, that is, programs that, besides lengthening daily school duration, also combine these changes with other modifications in the school environment.

The results increment the debate about full-day school policies. The increase in educational outcomes in full-day schools comes at a relatively small cost of worsening regular schools. One explanation for the spillover is that students of underprivileged families with less educational achievement demand more regular schools, reinforcing education inequality within the public educational system.[15] Therefore, given the increasing government's interest in expanding full-day schools, access to students who do not fit this program needs to be taken into account.

Nevertheless, our analyzes have some limitations. First, we do not estimate the net effect of full-day schools. We only show the benefits and negative spillover at the school level but do not show the full-day school net effect by estimating at an aggregate level as school districts, as Harris and Chen (2021) do for charter schools. Also, the school-level analyses do not consider the students sorting into schools, biasing the results for the direct impact of full-day schools. Lastly, we do not address the financial cost of the program, which prevents us from doing a cost-effectiveness analysis. Future works can contribute in these directions.

[15] News illustrating students abandoning full-day school and going to closer regular school to conciliate work with studies reinforce that idea `https://www.agenciamural.org.br/com-ampliacao-do-ensino-integral-alunos-trocam-de-escola-para-manter-trabalho/`.

Table 5: Spillover to PEIs' Local Market

	Simple Aggregation	Event Studies	Pre-Treatment Mean
Students			
Enrollment	17.249***	20.287***	494.00
	(4.296)	(6.115)	
Age	0.038***	0.058***	16.60
	(0.007)	(0.009)	
Family Income	-14.205	-27.243*	2284.10
	(11.416)	(15.487)	
Class-size	0.375***	0.417***	34.30
	(0.106)	(0.146)	
Age/Grade Distortion	0.66***	1.133***	18.30
	(0.188)	(0.273)	
Teachers			
Statutory teachers %	-0.022***	-0.032***	0.60
	(0.007)	(0.012)	
Teachers with Public Degree %	-0.016***	-0.031***	0.20
	(0.006)	(0.01)	
School			
Math Score - Saresp (Z)	-0.022***	-0.031***	-0.00
	(0.008)	(0.011)	
Language Score - Saresp (Z)	-0.014	-0.022**	-0.00
	(0.009)	(0.011)	
Failure	1.437	1.742	61.50
	(1.302)	(1.819)	
Drop-Out	1.782**	1.551	46.10
	(0.886)	(1.079)	

Notes: The first two columns represent $ATT(g,t)$'s summaries. The first is the simple weight aggregation, and the second is a dynamic aggregation (event-study). All estimations use baseline covariates, and the control group includes never-treated and not-yet-treated schools. The last column is variables mean for neighbours schools before receiving a full-day competitive shock. The variables Failure and Drop-Out have observations until 2017. Neighbours group are those define in Equation 1. Standard errors are clustered at the school level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Direct effect of PEI considering the spillover

	Simple Aggregation	Gardner (2021)	Butts (2021)
Daily hour	3.761***	3.724***	3.728***
	(0.041)	(0.041)	(0.041)
Math Score - Saresp (Z)	0.476***	0.468***	0.463***
	(0.027)	(0.022)	(0.022)
Language Score - Saresp (Z)	0.407***	0.361***	0.361***
	(0.024)	(0.019)	(0.019)
Failure Rate	-0.03***	-0.04***	-0.04***
	(0.004)	(0.004)	(0.004)
Drop Rate	-0.055***	-0.045***	-0.045***
	(0.004)	(0.003)	(0.003)
Enrollment	-115.844***	-134.462***	-137.478***
	(10.28)	(11.493)	(10.883)
Age	-0.318***	-0.3***	-0.29***
	(0.019)	(0.019)	(0.019)
Girls' Share	0.039***	0.041***	0.04***
	(0.004)	(0.004)	(0.004)
Family Income	123.273***	118.909***	108.915***
	(30.346)	(23.971)	(24.064)
Teachers' Education	0.017***	0.017***	0.016***
	(0.004)	(0.004)	(0.004)
Statutory teachers %	0.187***	0.161***	0.157***
	(0.013)	(0.008)	(0.009)

Notes: Each column represents an estimation method. The first is Callaway and Sant'Anna (2020) simple aggregation already presented. Gardner (2021) implement its Two-stage difference-in-differences estimation that relies only on PEI school as the not-yet-treated. Additionally, Butts (2021) is the same estimation as Gardner (2021), but also controlling for the spillover of neighbours (Local market neighbours) in the not-yet-treated group. All regressions use the same set of covariates, and standard errors are clustered at the school level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Simple Aggregation Estimation for different Neighbors definitions

	Local Market	2km	3km	5km	3 main target school	20–40 km
Enrollment	17.249***	16.689***	13.15***	12.777***	36.002***	-8.009**
	(4.296)	(3.767)	(3.898)	(4.324)	(5.852)	(3.117)
Age	0.038***	0.039***	0.036***	0.041***	0.035***	-0.024
	(0.007)	(0.008)	(0.007)	(0.006)	(0.009)	(0.015)
Family Income	-14.205	-5.614	-16.681	-29.377***	-7.428	28.685
	(11.416)	(13.221)	(11.092)	(10.015)	(15.75)	(19.526)
Class-size	0.375***	0.611***	0.408***	0.336***	0.856***	-0.169
	(0.106)	(0.122)	(0.113)	(0.091)	(0.133)	(0.17)
Age/Grade Distortion	0.66***	0.613***	0.484***	0.405***	0.485**	0.168
	(0.188)	(0.175)	(0.161)	(0.145)	(0.202)	(0.271)
Math Score - Saresp (Z)	-0.022***	-0.006	-0.016*	-0.03***	-0.017	0.016
	(0.008)	(0.009)	(0.008)	(0.007)	(0.01)	(0.02)
Language Score - Saresp (Z)	-0.014	0.004	-0.014	-0.019**	-0.025**	0.011
	(0.009)	(0.01)	(0.009)	(0.009)	(0.011)	(0.017)
Failure	1.437	2.06	2.505**	2.187**	2.823	-0.27
	(1.302)	(1.388)	(1.163)	(0.999)	(1.922)	(0.935)
Drop-Out	1.782**	3.435***	1.574*	0.771	5.27***	0.008
	(0.886)	(0.837)	(0.816)	(0.768)	(1.132)	(0.625)
Statutory teachers %	-0.022***	-0.016**	-0.014*	-0.011	-0.023**	-0.006
	(0.007)	(0.008)	(0.008)	(0.007)	(0.01)	(0.012)
Teachers with Public Degree %	-0.016***	-0.002	-0.003	-0.005	-0.002	0.006
	(0.006)	(0.006)	(0.005)	(0.005)	(0.007)	(0.007)

Notes: Each row represents a simple aggregation estimation of Callaway and Sant'Anna (2020). Columns represent different neighbours' definitions. All estimates use the same set of covariates, and standard errors are clustered at the school level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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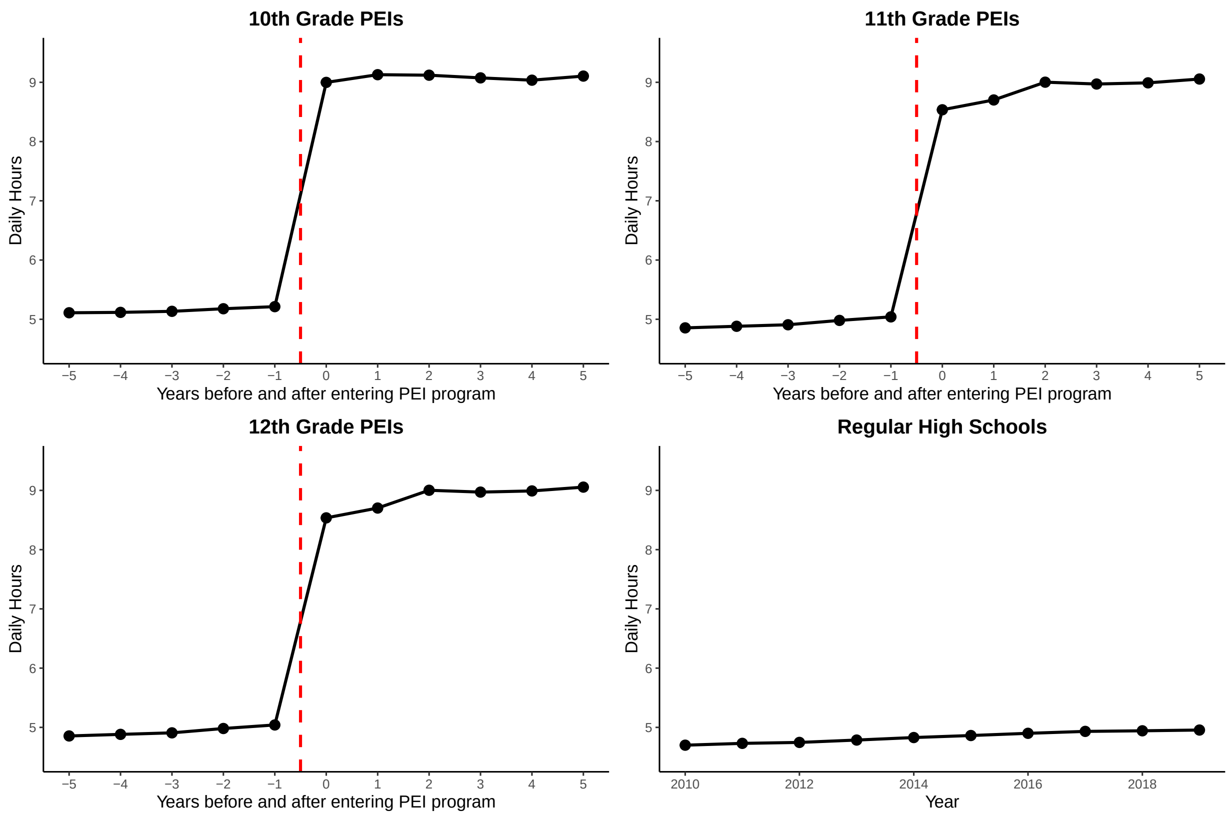
Appendix

Figure A1: Mean daily hours



Notes: Each dot point represents the mean daily hour of schools per year.

Table A1: Balance Table of PEI schools

	PEIs pre-treatment (N=502))		Regular Schools (N=6589)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
School Characteristic						
Daily school duration	4.96	0.29	4.71	0.41	-0.25	0.00
Enrollment	426.96	230.48	433.22	284.37	6.26	0.56
Class Size	33.89	3.79	34.43	5.22	0.54	0.00
Students Characteristic						
Girls' Share	0.51	0.04	0.51	0.04	-0.00	0.66
Special Students	2.81	3.73	2.29	3.29	-0.52	0.00
Students Age	16.69	0.30	16.71	0.38	0.02	0.17
Family Income	2308.05	519.79	2184.33	578.65	-123.72	0.00
Father schooling (stages)	2.14	0.29	2.03	0.36	-0.10	0.00
Mother schooling (stages)	2.29	0.31	2.20	0.36	-0.09	0.00
Teacher Characteristic						
Graduate teachers Share	0.25	0.16	0.25	0.17	0.00	0.58
Statutory teachers Share	0.31	0.34	0.30	0.33	-0.01	0.42
Quality						
Math Test Score - SARESP	271.30	10.84	269.13	13.23	-2.16	0.00
Language Test Score - SARESP	269.03	12.84	264.90	13.89	-4.14	0.00
IDESP	1.86	0.49	1.79	0.60	-0.07	0.00
Infrastructure						
Computer Lab	0.98	0.13	0.97	0.17	-0.01	0.03
Library	0.08	0.27	0.10	0.29	0.01	0.26
Internet	0.99	0.10	0.99	0.10	-0.00	0.83

Notes: This table shows summary statistics describing our school panel adding PEI schools. The PEIs group covers the pre-treatment full-day schools. Regular schools are all São Paulo state high schools that had never entered the PEI program. The statistics consider only the years 2010—2011.

Table A2: PEIs prediction

	Model 1	Model 2
PC_Classes_district	−0.0002	−0.0003
	(0.0003)	(0.0003)
PC_Enroll_district	0.0002	0.0004
	(0.0003)	(0.0003)
PC_Class_Size_district	−0.0003	−0.0004
	(0.0003)	(0.0003)
Num.Obs.	30 706	29 969
R2 Adj.	0.004	0.007
FE: Year	X	X
School Covariates		X
F-Test District Covariates		0.4052
F-Test School Covariates		0.0000

Notes: The dependent variable is a dummy equal to one for PEI schools just one year before conversion. PC_Classes_district and PC_Enroll_district are the percentual change in the number of public classes and enrollment at the educational district level. The dataset covers all regular schools and observations of PEI schools only for years before treatment.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Spillover effect of PEIs to Local Market Neighbours using Butts (2021)

	Butts (2021)	Simple Aggregation	Event Studies
Enrollment	-1.033	17.262***	20.254***
	(3.863)	(4.176)	(5.905)
Age	0.056***	0.038***	0.058***
	(0.008)	(0.007)	(0.008)
Family Income	-30.587***	-14.183	-27.171*
	(8.931)	(12.692)	(15.316)
Class-size	0.476***	0.375***	0.418***
	(0.097)	(0.113)	(0.147)
Age/Grade Distortion	0.656***	0.661***	1.134***
	(0.175)	(0.191)	(0.253)
Math Score - Saresp (Z)	-0.015**	-0.022***	-0.031***
	(0.007)	(0.008)	(0.011)
Language Score - Saresp (Z)	-0.013*	-0.014	-0.022**
	(0.007)	(0.009)	(0.011)
Failure	-2.419**	1.447	1.756
	(1.131)	(1.28)	(1.819)
Drop-Out	0.994	1.776**	1.547
	(0.66)	(0.843)	(1.164)
Statutory teachers %	-0.022***	-0.022***	-0.032***
	(0.006)	(0.008)	(0.012)
Teachers with Public Degree %	-0.006	-0.016**	-0.031***
	(0.005)	(0.007)	(0.01)

Notes: Each column represents an estimation method. Butts (2021) is the coefficient associate to neighbours (local market definition) schools. The remaining columns are spillover estimated from Callaway and Sant'Anna (2020) aggregations already presented. All regressions use the same set of covariates, and standard errors are clustered at the school level. The variables Failure and Drop-Out have observations until 2017.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$