Essays in Economics of Education: Lessons from the 2010 Earthquake in Chile

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Abstract

This thesis comprises two research papers that use the 8.8 earthquake that struck Chile in 2010 as a source of exogenous variation in: peer composition for the first paper and in school year length in the second one. My first paper, chapter 2 of this thesis, studies the causal effect of classmates on students' academic performance, known as peer effects. I use the earthquake as a source of variation in peer composition, using the fact that the earthquake hit a random area of the country and forced some students to move into new schools for non-academic reasons. I use OLS and instrumental variables econometric specifications, with data from students observed in 2010, in affected or non-affected areas, to answer this question. The regressions are performed only on students who do not move, the stayers. My results show that the peer effects are positive for students in both fourth and tenth grade, but statistical significance is sensitive to the specification chosen when using instrumental variables. An increase of one standard deviation in the average score of the peers has an effect between 0.15 and 0.22 standard deviations in the student's own score. In addition, using IV quantile regressions, I find some evidence of nonlinearities in the effect. The nonlinearities are not strong enough to allow me to find a pareto improving allocation of students,

but it potentially allows for a certain allocation of students within school that might reduce educational inequality between students and marginally increase average performance according to simulations. The second paper, chapter 3 of this thesis, studies the causal effect of school year length on students' academic performance. I use a difference-in-differences econometric specification, with data from students in fourth grade of primary education observed in 2009 or 2010, to answer this question. I rely on the earthquake that happened in Chile in 2010 as the source of variation. More than 50% of the students were studying in schools located in the area affected by the earthquake. The main results show that students attending schools that closed for up to eight weeks decreased their academic performance compared to the control group of students in non-affected areas. The effect is statistically significant only in mathematics. On average students exposed to up to 5% less of time at school have a performance of nearly 0.08 standard deviations lower than the counterfactual group of students in non-affected areas in mathematics. Of this effect, only one half can be attributed to having fewer days of schooling.

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Chapter 1

Introduction

Research in education using economic and econometric tools has received wide attention since Coleman et al. (1966). However, in the past the literature relied mostly on multivariate models that were not able to provide evidence on causal relationships (Cordero et al., 2018). This fact, combined with the importance of specific schooling systems (Woessmann, 2016), may partially explain the mixed evidence in the field of economics of education about a variety of inputs, peer effects and school year length among them.

My thesis consists of two independent research papers that use a similar source of exogenous variation, the earthquake that hit Chile in 2010, in the context of the Chilean school system. Therefore, the sections that explain those features present a large overlap, but I preferred to keep the chapters independent from each other so they can be read separately. On the other hand, I include the bibliography of the whole thesis only once at the end, to be environmentally friendly.

Chapter 2 investigates the effect of classmates on student's academic performance, commonly known in the literature as "peer effects". It is a topic that has been widely studied (for literature reviews see Sacerdote (2014) or Epple and Romano (2011)), with the relevant peer group at different levels. For instance, Gibbons et al. (2013) and Gibbons et al. (2017) take the neighbourhood as the level of peer interaction. On the other hand, a large proportion of studies, including Imberman et al. (2012) which I use as the base of the methodology for my paper, consider all students at the same grade within a school as the relevant peer group. On the contrary, only a small proportion of studies rely on data at the class level, for example Hanushek et al. (2003), McEwan (2003), Ammermueller and Pischke (2009), and more recently Tincani (2017). Furthermore, from those studies, only Tincani (2017) uses a natural experiment as the basis of the empirical strategy. In the Chilean context, most of the academic peer interactions occur between classmates, which makes it more desirable to use within-school variation. This comes at the cost of possible biases generated by within-school non-random allocation of students into classes, which I acknowledge as a potential threat to my identification strategy, but I argue that are unlikely to drive my results. On the positive side, my approach allows me to test some policy recommendations that might improve within-school efficiency.

My estimation strategy involves OLS and instrumental variables, to tackle the "reflection" problem (Manski, 1993). I study only the peer effects on students who didn't change school between 2009 and 2010, because a change is school is likely to be disruptive for the moving student even in normal conditions (Hanushek et al., 2004), getting a cleaner sample. This strategy goes in the same line as Angrist and Lang (2004), Imberman et al. (2012), Gibbons et al. (2013) among others, who focus on the peer effects of students who do not move.

The results of the second chapter show that having better peers increases own performance, measured by national standardised tests in mathematics and Spanish, in primary and secondary education. An increase of 1 standard deviation in the achievement of peers may carry the median student approximately 5% up in the national distribution of scores in primary education, and nearly 8% in secondary education in mathematics. However, the results from the instrumental variables estimations are sensitive to the econometric specification used for statistical significance of the relevant coefficients. In addition, there are some nonlinearities in the effects, stronger in mathematics in primary education. However, given the structure of the nonlinearities I cannot find a within-school allocation that is pareto improving. Nevertheless, I show that it may be possible to allocate students in a way that reduces educational inequality and marginally increases the average scores of the school, or to use tracking to increase the performance of the top achievers in a school, at the cost of a worse performance of low ability students.

The third chapter of the thesis examines, through the effects of the earthquake, if a shorter school year has an impact on academic achievement, measured by the national standardised exam called SIMCE, in mathematics and Spanish.

The lack of conclusive evidence about the causal effects of instruction time on student performance are seen, for example, in the different rules about compulsory instruction time in OECD countries. For instance, in Finland the compulsory instruction time per year in primary school amounts 632 hours, in Spain it is 794 hours and in Chile it is 1039 hours, the latter being the OECD country with the most compulsory hours of schooling per year but performing below the OECD average (OECD (2015) and OECD (2016)). Not surprisingly, when studying different countries there is no clear relationship between the amount of schooling time and academic performance (Lavy, 2015).

I use a difference-in-differences identification strategy, in the same line of Pischke (2007), to study the effects of school closures for up to eight weeks, on students' performance. There are two main differences though. The first one is that I rely on a natural event as the source of exogenous variation rather than a change in policy (for other studies using some sort of natural experiment see Marcotte (2007), Marcotte and Hemelt (2008), Spencer et al. (2016)). Secondly, I use multiple treatments, to distinguish between students affected by the earthquake but not exposed to school closures, students in schools that suffered minor or moderate damage, and students in schools severely damaged by the earthquake, who may have lost up to eight weeks of schooling. Using multiple treatments may improve the reliability of the estimates compared to the standard difference-in-differences empirical strategy (Meyer, 1995).

My results show that the temporal school closures for up to eight weeks had a negative and significant effects on mathematics of -0.043 standard deviations. The total effect of the earthquake is -0.081 standard deviations in mathematics for the most affected students. On the other hand, I do not find any statistically significant effects of the earthquake on Spanish. For the students analysed in fourth grade, eight weeks represent approximately 5%

of their cumulative schooling weeks since first grade, and approximately 25% of the school year length prior to the exam. The variation in time is similar to that observed by Bellei (2009), who obtains similar results but studying an increase rather than a reduction in instruction time.

To verify that the difference-in-differences method is suitable, I check that the parallel trend assumption holds for both subjects and all the sub-groups of students. In addition, I run placebo regressions that show no effect of the earthquake before it happened.

The results of the third chapter show that even though Chilean students have the highest amount of compulsory instruction time per year, they can still benefit from spending more time at school, at least for mathematics. The same is not true for Spanish. It might be worth increasing the hours of mathematics, at least for the low achievers as in Cortes et al. (2014), perhaps reducing marginally the time devoted to Spanish, and maybe in this way Chile could improve its performance at international examinations.

Chapter 2

Peer Effects in the Classroom

2.1 Introduction

The causal effect of classmates on a student's academic performance is a question still open to debate in the field of economics of education, and for policy makers. There is no consensus in the literature about the size of peer effects (for reviews see Sacerdote (2011), Epple and Romano (2011) and Sacerdote (2014)). The main contribution of the paper is: to identify the existence and size of peer effects in primary and upper secondary education in the class, taking into account selection into schools and possible non-random assignment into classes, to investigate the nonlinearities in these effects and to provide a policy recommendation on a possible way to improve efficiency within schools.

The problem of studying changes in peer quality is that the move of new students into a peer group is not always exogenous. A source of exogenous variation in peer composition is provided by natural disasters. One of the first studies to use an earthquake to obtain exogenous variation in the peer group is Cipollone and Rosolia (2007), for students in high school. Imberman et al. (2012) use hurricane Katrina as the source of variation and I use the earthquake that happened in Chile in 2010. Because of the earthquake, some students had to move to other schools either because their school was destroyed or because their parents had to move. Therefore, some classes received a share of displaced students for exogenous reasons. In my paper I focus on the effects on the stayers, students that did not change school after the earthquake. In this aspect, I follow a similar strategy to Angrist and Lang (2004), Imberman et al. (2012), Gibbons et al. (2013) among others, who focus on the peer effects on immobile students, at different group levels. Similar to Imberman et al. (2012), I use an instrumental variables strategy, with the proportion of displaced students in a class as an instrument for the average mean performance of peers (the average score of the students in the class discarding the score of the student). My first stage resembles Gibbons and Telhaj (2011), who look at the effect of incoming students on children who do not change school. The intuition is the following: given the magnitude and the timing of the earthquake, students had to be relocated into schools in a different way compared to children who move into another school because of academic reasons. The instrument is used to address what Manski (1993) called the "reflection" problem. To address selection into schools, which is one of the main econometric issues when estimating peer effects (Epple and Romano, 2011), I include school fixed effects, hence using within-school variation. Another potential source of bias is within school non-random allocation, when studying peer effects at the class level (Ammermueller and Pischke, 2009). One of the few published articles in the economics literature studying peer effects using data from Chile is McEwan (2003). Using a sample of students from eighth grade he shows that the results do not appear to be driven by within-school sorting. I follow his strategy to show that, based on parents' education, there is no evidence of a systematic allocation of students within schools. In addition to this information, as a further robustness check I remove from the sample schools in which there might be non-random allocation, based in the exam results. For other quasi-experimental settings see Sacerdote (2001) or Zimmerman (2003), who study peer effects in higher education.

My results show that peer effects are positive both in fourth and tenth grade. The statistical significance is somewhat sensitive to the specification used. In fourth grade, an increase of one standard deviation in the average score of the peers has an effect of approximately 0.16 standard deviations in the student's own score in mathematics and 0.15 standard deviations in Spanish in my baseline specification. For the median student it means moving approximately 5% up in the national distribution of scores. In tenth grade the effect in mathematics is such that the same improvement in the quality of peers would move the median student approximately 8% up in the distribution of scores.

There is some evidence for nonlinearities in the peer effect. In primary education, students near the median of the distribution of scores are on average less affected by their peers than other students who are either near the top or near the bottom of the class. On the other hand, the nonlinearities are less clear in the case of students in tenth grade, especially when analysing mathematics. One important result is that in all the groups analysed the peer effect is either positive or not statistically significantly different from zero.

The findings in the size of peer effects when using a linear-in-means model are in line with the current literature in primary education which study peer effects at the class level, in which the magnitudes range between modest (Burke and Sass, 2013) and no effects (Vigdor and Nechyba, 2007). In particular, my findings in Spanish are similar to the results in Ammermueller and Pischke (2009), who also relies on within-school variation, using a sample of European students in primary education. Furthermore, the slightly higher size of the effects and higher significance in tenth grade compared to fourth grade in mathematics is consistent to what Imberman et al. (2012) points out of the effects being larger for students in further stages in education. Moreover, results from Kang (2007) with students in seventh and eighth grade in mathematics show similar magnitudes to mine. In addition, coherent with the findings of Imberman et al. (2012) and my own, the results in Lavy and Schlosser (2011) show an increase in the magnitude of the effect in eighth grade compared to fifth grade. Contrary to the findings of Hanushek et al. (2003), I do find a statistically significant negative effect of the standard deviation of scores on achievement, although the magnitude is rather small. It is important to bear in mind that the variation induced by the earthquake, or another natural disaster, is different to the normal changes in peer composition (Imberman et al., 2012). I focus on the effect on the students who do not change school, most likely the least affected by the earthquake. Furthermore, the variation that I observe in peer composition even though is larger than in a normal year, is not as large as in Imberman et al. (2012), which may be positive when thinking about the external validity of my results.

The assessment used is a national standardised exam in mathematics and Spanish, taken every year around October by students in fourth grade (mostly aged between nine and ten years), including 2010 and every other year by students in tenth grade, also around October (fifteen to sixteen years), available for 2010 as well.

Related to my work is Tincani (2017) because the natural experiment used is the same, the earthquake that happened in Chile in 2010, although both the research question and the identification strategy differ. Tincani (2017) studies the effort choices in a class and shows that at least partly the peer effects seen on scores are due to rank concerns. In her study, the variation comes from the assumption that the intensity of the earthquake at a student's hometown increases their cost of effort to study. Not all the children in the same class live in the same town. Therefore, in some classes the dispersion in the cost of effort increases more than in other classes because students were exposed to the earthquake with different intensities. It is important to notice that in Tincani (2017) the classes studied are those which did not receive displaced students, so the variation comes from the different costs that children face for studying because of the earthquake. This is an important difference to my study, because the variation that I use comes from the difference in the share of displaced students in the class, closer to the strategy used by Imberman et al. (2012). Another important contrast is that while I focus exclusively on peer quality, Tincani (2017) also incorporates rank concerns in their model. On the other hand, while I study peer effects in 4th and 10th grade and report some differences between both groups, Tincani

(2017) covers students in 8th grade only.

One study that looks for some sort of heterogeneity in peer effects is Patacchini et al. (2017). They distinguish the length of a friendship, either short or long, finding that only friendships that last more than one year have a persistent effect on academic outcomes. Another paper that investigates the heterogeneity in the peer effects is by Imberman et al. (2012), which looks for nonlinearities. They find monotonicity of peer effects and they reject the linear-in-means model.

A possible policy implication is that it might be feasible to reallocate students in a way that both improves average results and reduces inequality using the same resources, therefore improving efficiency.

However, my simulations show that the scope for improvement is rather limited for average performance, with an increase of 0.01 standard deviation in scores, and more promising for reducing inequality. However, this result should be taken with caution. For example, in the experiment made by Carrell et al. (2013) trying to group freshmen entering at the United States Air Force Academy in an optimal way, the result is that the treatment group performed worse than the group of peers allocated randomly. Similarly, Burke and Sass (2013) and Feld and Zolitz (2017) findings suggest that it may be detrimental for low-achieving students to have classmates of high ability. On the other hand, Carrell et al. (2013) and Feld and Zolitz (2017) studies focus on students in Higher Education, whereas in primary education it may be possible for the teacher to have a larger influence in the group formation via coursework for example.

The rest of the study is organised as follows: section 2 gives an overview of the Chilean school system and the Earthquake, section 3 covers the data, its sources and some descriptive statistics, section 4 is about the empirical strategy, in section 5 I go through the results while in section 6 I conclude.

2.2 The Chilean school system and the Earthquake

2.2.1 Chilean school system

The Chilean school system is organised in three tiers: primary school from first to fourth grade, lower secondary school from fifth to eighth grade, and upper secondary school from ninth to twelfth grade. Children usually start primary school at the age of six-seven years and leave upper secondary school at the age of seventeen-eighteen years.

There are three types of schools: public, private subsidised, and private schools (Valenzuela et al., 2014). The public, or municipal, are run by the municipalities, cannot charge any fees to the students and receive funding from the state. The private subsidised also receive funding from the state but in addition they were allowed to charge some tuition fees to the students at the time of the exams analysed in this study. Finally, private schools do not receive any funding from the state and are allowed to charge tuition fees. Around 90% of the students attend either public or subsidised schools, while less than 10% are enrolled in private schools in fourth grade. Public schools cannot select students before seventh grade. On the other hand, private subsidised and private schools may interview parents, hold some playing sessions for the children or do other type of selection processes (Anand et al., 2009).

In order for a student to progress to the next academic year, the following rules apply. From first to second grade and from third to fourth grade, every student with an attendance rate of at least 85% will be automatically promoted to the next year. The minimum attendance of 85% is a necessary but not sufficient condition in other grades. From second to third grade and from fourth to twelfth grade, students must either have passed all their subjects (minimum of 4.0 in a scale from 1 to 7 in teachers' assessment), have an average of 4.5 if they have failed one subject, or an average of 5.0 if they have failed 2 subjects. The averages are calculated considering the compulsory subjects in the national curricula, including Mathematics, Spanish, History among others. I refer to this average as GPA or teacher assessment in the rest of the paper.

To measure the quality of education, there are exams called "SIMCE", that are sat by students in fourth, eighth and tenth grade. In the case of students in fourth grade, this is every year, and for the other two cases only every other year.[1] The exams are centralised and the same for every child in the country. The results are publicly available at the school level. If a school shows a poor performance, then some sort of support is given to it to improve. On the other hand, if a school shows a good performance in the SIMCE, a bonus is paid to their teachers (Carnoy et al., 2007). According to Mizala and Urquiola (2009), the existing descriptive evidence does not

[1] In odd years students in eighth grade must sit the exam and in even years students in tenth grade must do the same.

suggest that the incentives have changed average testing performance.

In the case of primary school, the same teacher will teach most of the subjects in a given class. From fifth grade onwards, in most schools, students are taught each subject by a different teacher.

2.2.2 Earthquake

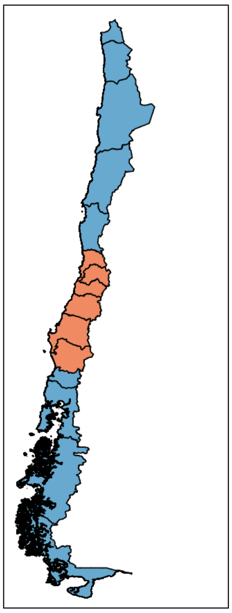
Chile was struck by an earthquake on 27th February 2010, at 3:34 am local time. Its magnitude was 8.8 Richter in the epicentre, approximately 400 km south of Santiago, the capital, and it was the fifth-largest ever instrumentally recorded in the world (Astroza et al., 2010).[2] It affected six of the fifteen regions of the country. Approximately 80% of the population lived in that area. The earthquake occurred just 2 days before the official start of the school year, set to be the 1st of March 2010 and to include 38 weeks of teaching.[3] According to the information retrieved from the Chilean Ministry of Education (MINEDUC), more than half of all schools (4635 out of nearly 9000) were damaged. The damage was classified as: minor, moderate, or severe.

Figure 3.1 shows a map of the country divided by regions. The area affected by the earthquake includes 6 of the 15 regions, and is shown in red.

[2]Chile is a seismic country. According to the information from the National Seismological Centre (*Centro Sismologico Nacional*), before 2010 there were 109 earthquakes of magnitude at least 7.0, from Arica (northernmost part of the country) to Tierra del Fuego (southernmost part of the country). This, considering all the earthquakes from 1570. More accurate measures are from the beginning of the 20th century, and in that case the number of earthquakes before 2010 is 77. This represents on average 0.7 earthquakes per year, compared to 14.6 in the whole world since 1990 (or 0.07 per country per year, statistics from the United States Geological Survey). Another way to look at this is to say that Chile's surface is 0.56% of the world's, and its share of earthquakes is 5%.

[3]Information retrieved from MINEDUC.

Figure 2.1: Map of Chile.



In red, the six regions affected by the earthquake. In blue, the areas not affected by it.

The regions not affected by it are in blue.[4] It covers the three most populous cities in the country: Santiago, Valparaiso and Concepcion. The epicentre was on the coast, and after the earthquake there was a tsunami which was responsible of most of the deaths due to the earthquake. The official number of deaths related to the earthquake and the subsequent tsunami was 525, according to the National Office for Emergencies ONEMI (*Oficina Nacional de Emergencias del Ministerio del Interior*).

[4]The definition of affected area comes from the Chilean Government, is at the regional level, and includes regions V-VI, VII, VIII, IX and the Metropolitan Region.

2.3 Data

I use two sources of data. One is an administrative data from the Ministry of Education, which includes the following information at the student level: yearly academic performance measured by the teacher, a full record of schools attended every academic year, and town in which the school is located.[5] It covers from 2002 onwards. The second main source comes from the national standardised exam called SIMCE (System to Measure the Quality of Education), organised by the Agency for the Quality of Education. Its main purpose is to provide information about students' learning in different areas of the national curricula, and to contribute to the improvement of the quality and equity of education. The exam is sat every year by students in fourth grade (nine to ten years old), and every other year by students in eighth grade (thirteen to fourteen years old) and tenth grade (fifteen to sixteen years old). There are identifiers for the student, the classroom and the school. Therefore, it is possible to identify different classes within a school.

I use two datasets, one with information about students in fourth grade and the other one which has the same information about students in tenth grade, both in 2010, the year of the earthquake. I study those groups because they are highly comparable, they sat the exam at the same time and therefore one can expect that they were exposed in a similar way to the earthquake. Information about the students in both grades include the results in both mathematics and Spanish, gender, parents' education, household income, number of books available at home, class size, and a full record of the schools

[5] A small proportion of students change school at the end of the academic year.

attended before.

From the information that comes from the dataset I create a dummy variable called *displaced*. The following criteria must be met for a student to be considered *displaced*: the school attended by the student in 2010 is different to the school attended in 2009, the year before the earthquake; the student was attending a school in 2009 in the affected area;[6] the student did not move to another school for academic reasons.[7] If all the previous conditions are met the variable *displaced* takes value 1, and value 0 otherwise. Therefore, children who were studying at schools outside the affected area in 2009 are not considered as displaced even if they did change school afterwards. Data about the school attended before and after the earthquake are in Table 2.1 for students in fourth grade and Table 2.2 for students in tenth grade. In both grades approximately 60% of the changes are between schools of the same type. From the rest, the majority of the changes are between Municipal and Private Subsidised schools.

Summary statistics of the main variables used in the regression are shown in Table 2.3 and Table 2.4. The first two variables are the raw score of the students in mathematics and the class standard deviation in mathematics. The next two variables are the same but for Spanish. GPA is the raw score for teacher assessment, which can go from 1 to 7. Father education and Mother education indicate how many years of education received their parents, and Female is a dummy which takes value 1 if the student is female and

[6] There is information about the town in which the student resides but it is self-reported and in some cases inconsistent with the location of the school attended, therefore I use the location of the school instead.

[7] There is a survey for parents in which they are asked about the main reason of choosing the current school at which the student is.

Table 2.1: Displaced students 4th grade

Type of school before and after the earthquake	**Frequency**	**%**
Municipal to Municipal	2,829	24.17
Municipal to Subsidised	1,950	16.66
Municipal to Private	16	0.14
Subsidised to Subsidised	3,929	33.56
Subsidised to Municipal	2,091	17.86
Subsidised to Private	230	1.96
Private to Private	368	3.14
Private to Municipal	28	0.24
Private to Subsidised	265	2.26
Total	**11,706**	**100**

Table 2.2: Displaced students 10th grade

Type of school before and after the earthquake	**Frequency**	**%**
Municipal to Municipal	428	10.67
Municipal to Subsidised	523	13.04
Municipal to Private	23	0.57
Subsidised to Subsidised	1,720	42.87
Subsidised to Municipal	588	14.66
Subsidised to Private	139	3.46
Private to Private	389	9.70
Private to Municipal	26	0.65
Private to Subsidised	176	4.39
Total	**4,012**	**100**

0 if the student is male. Finally, I include a dummy variable which indicates if there are more than 10 books at the student's home, and I also include a measure of household monthly income, which includes 13 categories, each category separated by approximately £150. In my regressions I use standardised scores for both the SIMCE exam and teacher's assessment, with mean 0 and standard deviation of 1.

The separation between a change of school caused by academic vs non-academic reasons is relevant. In the cases where parents say that the reason of the change was because they found a better school, average score in the SIMCE exam in mathematics of the new school is on average 20 points higher, nearly a third of one standard deviation, compared to the previous school attended by the student. On the other hand, for the subgroup of students who are considered displaced, the current school's score is on average 2 points lower (approximately 0.04 standard deviations) than in the previous school.

Table 2.3: Summary statistics for students in fourth grade, 2010

Variable	**Mean**	**Std. Dev.**
Maths raw score	262.932	52.374
Class SD in maths scores	43.784	6.908
Spanish raw score	278.674	48.364
Class SD in Spanish scores	43.083	8.000
Female	0.511	0.5
GPA 1st grade	6.357	0.465
GPA 2nd grade	6.187	0.492
GPA 3rd grade	6.005	0.517
Class size	33.054	7.285
More than 10 books at home	0.687	0.464
Father education	12.01	3.439
Mother education	12.108	3.555
Household monthly income	4.714	3.404
Observations	107095	

Table 2.4: Summary statistics for students in tenth grade, 2010

Variable	Mean	Std. Dev.
Maths raw score	274.794	63.247
Class SD in maths scores	42.754	7.681
Spanish raw score	273.167	51.251
Class SD in Spanish scores	38.915	6.015
Female	0.529	0.499
GPA 7th grade	5.767	0.551
GPA 8th grade	5.788	0.545
GPA 9th grade	5.534	0.548
Class size	33.978	6.964
More than 10 books at home	0.803	0.398
Father education	11.897	3.697
Mother education	11.993	3.79
Household monthly income	5.188	3.56
Observations	89788	

2.3.1 Subsample analysed

From the total of more than 200,000 students observed in each exam, approximately 50% are in the main analysis. First, in order to include controls as parents' education, I restrict the sample to observations in which the parents' survey was answered, which reduces the sample size by approximately 10%. Also, I only considered students who didn't repeat any grade, reducing my sample. However, in the peer group I do consider all the students who have a valid score. Hence, if there is a classroom with 30 students, including one who repeated some level before and another one for whom I do not have additional information, the final sample will consist of 28 students, but for the peer group I will consider all the 30 students. That is, when calculating the average peer score, I include all the students who have a valid score in the exam, even if they are not included in the final sample. In addition, there are some students, especially in rural areas, who have less than 10 classmates, so I restricted my sample to students in a class with at least 10 students.[8] Furthermore, to use school fixed effects I had to restrict the sample to students in schools with at least two classes per grade, otherwise there would be no variation left within schools with only one class. Therefore, my final sample I observe 107,095 students in fourth grade and 89,788 students in tenth grade.

2.4 Empirical Strategy

The aim of the paper is to identify the causal effect of peers on students' academic achievement, measured with national standardised exam scores in mathematics an Spanish. I start with a standard linear-in-means OLS specification for peer effects in the class:

$$Y_{ics} = \beta_0 + \beta_1 P_{-ics} + \beta_2 X_{ics} + \beta_3 M_{cs} + k_s + \epsilon_{ics} \tag{2.1}$$

where Y_{ics} is the Spanish or mathematics score in SIMCE for student i in class c, attending school s, P_{-ics} is the average peer performance of all the students excluding student i in class c, attending school s, X_{ics} are observable characteristics of the student, M_{cs} are observable characteristics of the class, k_s are school fixed effects and ϵ_{ics} is the error term that captures unobserved factors that determine academic achievement.

[8] I tried different thresholds, also having a minimum of 15 or 20 students, without any significant variation in the main results.

This specification follows what McEwan (2003) does with Chilean data as I do but with a different cohort and grade. He argues that including school-specific fixed effects can help to at least partially address the bias in the estimates of β_1 caused by families with similar unobserved preferences choosing similar schools, commonly referred as selection into schools (see Manski (1993) and Sacerdote (2014)). One may think that students are not randomly allocated into schools. It may be more likely that if parents can choose, they will try to put their children in a "good" school instead of a "bad" one, leading to self-selection of students into peer groups.[9] Therefore, it is important to address sorting into schools. This selection problem is particularly important in the Chilean context, which has a high socioeconomic segregation (Valenzuela et al., 2014). A possible improvement with respect to McEwan (2003) is that I include information of previous ability for each student, measured by teacher's assessment. Furthermore, equation 2.1 is similar to the linear-in-means specification used in Imberman et al. (2012). The main difference is that I use the class as the relevant peer group and their study uses the grade-school as the relevant peer group (for other examples using instruments at the grade-school level see Lavy and Schlosser (2011) and Lavy et al. (2011)). In the Chilean education system, most of the interactions, especially in mathematics and Spanish, occur at the class level rather than at the school level. Moreover, in primary education the head teacher usually covers both subjects for a given class. Hence, I opt to group at the class level as in McEwan (2003) and Tincani (2017), both using

[9] What is good or bad will vary from case to case but one possible measure could be the performance of students in that school in a national exam.

Chilean data. Nevertheless, even when including school fixed effects and a measure of previous ability the estimates of β_1 may be biased because of the "reflection" problem (Manski, 1993). The reflection problem arises because the score of student i has an impact on the average score of her peers, which may cause a bias in the β_1 coefficient. Therefore, the next step is to find an instrument that generates exogenous variation into the peer composition.

To add this exogenous variation, the main specification to look for peer effects uses the fraction of displaced students in the class as an instrument for average peer performance, in a similar way as Imberman et al. (2012). The identification strategy relies on the following assumptions: the random allocation of students into schools and within schools because of the earthquake.[10] Under this assumption, the fraction of displaced students is an exogenous source of variation for the peer composition, addressing the reflection problem when identifying peer effects. Therefore, the first stage regression can be written as:

$$P_{-ics} = \alpha_0 + \alpha_1 \frac{D_{cs}}{N_{cs}} + \alpha_2 X_{ics} + \alpha_3 M_{cs} + k_s + e_{ics} \tag{2.2}$$

where D_{cs} is the number of displaced students and N_{cs} is the number of students in class c, attending school s. Consequently, the fraction of displaced students will not be necessarily be the same for two different classes within a school.

[10]For the students considered as displaced, the quality of the school attended in 2009 is on average similar to the quality of the school attended in 2010. This is not the case for students who move for academic reasons, which I exclude from the sample.

The second stage regression is:

$$Y_{ics} = \beta_0 + \beta_1 \hat{P}_{-ics} + \beta_2 X_{ics} + \beta_3 M_{cs} + k_s + \epsilon_{ics} \tag{2.3}$$

where $\hat{P}_{-ics}$ is the predicted average peer score for all the students excluding student i in class c, attending school s.

2.4.1 Selection into classes

The identification strategy relies on the assumption that within schools, children are allocated randomly into different classes. If it is not the case, then my results may be biased. A preliminary check that I perform is to compare the average peer score when using either the class or the school as the aggregation group. For all the grades and subjects the correlation is above 0.9, suggesting no large differences within a school. A more formal test is done by McEwan (2003). In his study he compares a measure of average mothers' education at the classroom level with a simulated random class allocation. His findings suggest that the differences are not large enough to suggest a systematic assignment into classes by ability. I follow his approach and I reach the same conclusions for both cohorts studied. My results are in Table 2.5, and includes the same procedure using fathers' education as well. The observed and the simulated dispersion are virtually the same across grades and subjects. As an additional way to attempt to rule out the problem of non-random allocation within schools, I compare outcomes of different classes within a school. The method I used to test this claim is by looking at a subsample of students which are in schools that do not appear to set their classes by ability. The criteria that I impose is that the difference in scores between classes must be not statistically significant either in mathematics or Spanish. If a school does not meet it, then I remove it from the subsample. Therefore, I keep in my sample only schools in which their classes have a similar score in both mathematics and Spanish. Finally, I perform the regressions for the main sample, including all the students as described in the data section, and a subsample of students in schools that meet the previous criteria of no differences that are statistically significant. The latter approach has some limitations. First, it uses the current outcome in mathematics or Spanish as an indicator of an allocation into different groups that happened in the past. Second, by construction it reduces the within-school variation, which is the source of my identification strategy. [11]

Table 2.5: Selection into classes according to family characteristics

Variable	Observed Std. Dev.	Simulated Std. Dev.
Class Father Ed. 10th grade	2.194	2.193
Class Mother Ed. 10th grade	2.025	2.025
Class Father Ed. 4th grade	2.043	2.179
Class Mother Ed. 4th grade	1.930	2.065

2.4.2 Nonlinearities

There is a growing literature looking beyond linear-in-means peer effects (Tincani, 2017). In this study I use IV quantile regressions to show how the peer effect vary depending on the position of student i in the distribution

[11] This subsample is approximately 80% of the students used in the main regressions.

of scores. The focus of this section is to estimate an equivalent version of equation 2.3, the specification with instrumental variables, for 10 different quantiles, using the estimator developed by Powell (2017). The importance of nonlinearities in peer effects lies in the fact that without them, it is not possible to find any allocation of students that increases average performance (see Carrell et al. (2013) for a previous attempt) based on peer quality, because the total amount of ability is fixed. On the other hand, other targets such as reducing educational inequality or increasing the performance of certain students are possible to achieve even in the absence of nonlinearities in the importance of peers.

2.5 Results

2.5.1 Main results

The main results of the effect of peers on students' performance are summarised in four tables, one for each combination of cohort-outcome. The combinations are: mathematics in fourth grade (M4), Spanish in fourth grade (S4), mathematics in tenth grade (M10) and Spanish in tenth grade (S10). The sample analysed comprises students who did not move to another school between 2009 and 2010, attending schools with at least two different classes and at least ten students per classroom. In columns 1-3 are the results of estimating equation 2.1 and column 4 shows the results when estimating equation 2.3.

The first column of Table 2.6 for M4 shows that the coefficient of peer effects is positive and significant when including only individual controls. The magnitude is 0.420 and it is significant at the 1% level. In line with the literature, the coefficient of -0.223 for the dummy variable Female shows that male students perform better than their female counterparts in mathematics. In column two, when including class level controls and school fixed effects, the coefficient of peer effects nearly halves to 0.220 and remains significant at the 1% level, showing that selection into schools is an important driver of upward biases in the results when not taken into account. In column 3, when adding controls from the family background of the student, the results are virtually unchanged. Column 4 has the same variables as column three but shows the results when estimating equation 2.3, the instrumental variables specification. The main difference compared to column three is that the coefficient of peer effect decreases to 0.150 and is significant only at the 5% level.

Table 2.7 shows the results for the group M10. In line with the results in mathematics for students in primary education, the coefficient of peer effects decreases when including school fixed effects, from 0.584 to 0.266, always significant at the 1% level. Furthermore, when using instrumental variables, the coefficient for peer effects further declines to 0.219, with the same statistical significance as before. On the other hand, the gender gap marginally increases.

The results of Spanish for students in fourth grade are in Table 2.8. One substantial difference that the coefficient for the variable Female, which now is positive and significant at the 1% level. The coefficient for peer effects in column 1 is 0.326, significant at the 1% level. It goes down to 0.126 in column two when controlling for selection into schools but remains significant at the 1% up to column 3. In column four, when estimated using instrumental variables it increases to 0.141, but due to the larger standard error, it is not statistically significantly different from the coefficient in column three estimated with OLS. Overall, results in Table 2.8 tell a similar story as those for mathematics in fourth grade.

Finally, the results of Spanish in tenth grade, which I include in Table 2.9, do not change substantially when compared with Spanish in fourth grade. The peer effect coefficient is 0.487 and significant at the 1% level when using OLS and individual controls, decreases to 0.181 when accounting for selection into schools and further decreases to 0.170 after adding some family background characteristics. In column 4, when using instrumental variables, the coefficient for peer effects decreases to 0.124, and it is significant only at the 10% level. One noticeable difference when comparing the results with those for students in fourth grade in Spanish is that the gender gap reverses, with male students now performing slightly better than their female counterparts, after controlling for observables.

The results are consistent with the problems for estimating peer effects mentioned in Manski (1993). I show that the main issue is selection into schools, and the reduction in the coefficient of the peer effect when estimated with instrumental variables may arise because it deals with the reflection problem. My results are consistent with the findings of Imberman et al. (2012), who show lower coefficients when using IV compared to OLS, even though in my case they remain significant, at least at the 10%. This in turn may be explained by the fact that I look for peer effects at the class level, as in Gibbons et al. (2013), and it is plausible that the influence of classmates may be higher than the influence of other students in the same grade but not in the same class.

The goodness of fit, reported at the end of each table for the results estimated with OLS, increases when including information about schools and class level controls but remains virtually the same when adding the family controls in column 3.

2.5.2 Robustness checks

One important issue when trying to get causal estimates of peer effects is the selection into the peer group under study. Even though I argue that selection does not seem likely to be the driver of my results, I attempt further checks. To start, I perform the same regressions for a subsample of students who are in schools where the different classes within a grade do not show differences that are statistically significantly differently from zero neither in mathematics nor in Spanish. The results change for students in fourth grade, especially when using instrumental variables. In the case of M4 reported in Table 2.10, the coefficient of peer effects remains positive and significant at the 1% level in columns 1-3, with a similar order of magnitude to the baseline results in Table 2.6. However, when using instrumental variables, the coefficient of peer effect is no longer statistically significantly different from zero. For the case of S4 reported in Table 2.12, columns 2 and 3 remain positive and significant at the 1% level, although with a lower magnitude compared to the results in the main sample. Similarly to the case of mathematics, when

Table 2.6: Peer effects on stayers, OLS and IV (4th grade, Mathematics)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Maths	0.420***	0.220***	0.217***	0.150**
	(0.004)	(0.009)	(0.009)	(0.074)
Female	-0.223***	-0.267***	-0.264***	-0.263***
	(0.005)	(0.005)	(0.005)	(0.005)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	107095	107095	107095	107095
R^2	0.519	0.580	0.582	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 49

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Peer effects on stayers, OLS and IV (10th grade, Mathematics)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Maths	0.584***	0.266***	0.258***	0.219***
	(0.003)	(0.009)	(0.009)	(0.050)
Female	-0.249***	-0.278***	-0.275***	-0.275***
	(0.005)	(0.005)	(0.005)	(0.005)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	89788	89788	89788	89788
R^2	0.638	0.672	0.674	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 66

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8: Peer effects on stayers, OLS and IV (4th grade, Spanish)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Spanish	0.326***	0.126***	0.124***	0.141**
	(0.004)	(0.008)	(0.008)	(0.061)
Female	0.088***	0.070***	0.073***	0.073***
	(0.005)	(0.005)	(0.005)	(0.005)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	107095	107095	107095	107095
R^2	0.386	0.438	0.440	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 51

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.9: Peer effects on stayers, OLS and IV (10th grade, Spanish)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Spanish	0.487***	0.181***	0.170***	0.124*
	(0.003)	(0.008)	(0.008)	(0.065)
Female	-0.036***	-0.026***	-0.022***	-0.022***
	(0.005)	(0.005)	(0.005)	(0.005)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	89788	89788	89788	89788
R^2	0.511	0.544	0.548	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 43

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

using instrumental variables the coefficient is not significant any more. It is important to take these results with caution, because these results may arise from the loss in variation between classes within a school rather than showing a systematic selection of students into classes in primary school, under the current rules.

The differences are smaller when looking at tenth grade, group M10 in Table 2.11 and group S10 in Table 2.13. The magnitudes and significance levels are virtually the same as those presented in the main sample. One potential explanation to the difference between grades is that in primary education each class is usually taught most of the subjects by their head teacher, including mathematics and Spanish. This potentially means a larger variation of scores within a school, showing a spurious larger selection of students within schools. It is documented in the literature that there are differences in teacher effectiveness, even when controlling for certification status and experience as in Kane et al. (2008). Moreover, in some cases teacher credentials are not positively reflected in student's academic achievement (see Carrell and West (2010)). In a recent review Coenen et al. (2018) find that teacher experience does matter for students' test scores but not general teacher certifications. On the contrary, in secondary education usually every class in the same grade within a school will have the same teacher for mathematics and another one for Spanish. This fact may explain the lower variation within schools for secondary schools.

Overall the results suggest that the issue of selection within schools might exist, but that it is not the main driver for the results in peer effects, which is also what McEwan (2003) finds when using a cohort of eighth graders.

Table 2.10: Peer effects on stayers, no large differences between classes (4th grade, Mathematics)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Maths	0.416***	0.141***	0.139***	-0.187
	(0.004)	(0.013)	(0.013)	(0.220)
Female	-0.223***	-0.267***	-0.264***	-0.262***
	(0.005)	(0.005)	(0.005)	(0.005)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	88693	88693	88693	88693
R^2	0.513	0.579	0.581	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 13

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.11: Peer effects on stayers, no large differences between classes (10th grade, Mathematics)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Maths	0.584***	0.263***	0.255***	0.221***
	(0.003)	(0.010)	(0.009)	(0.056)
Female	-0.249***	-0.279***	-0.276***	-0.277***
	(0.005)	(0.005)	(0.005)	(0.005)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	83577	83577	83577	83577
R^2	0.641	0.675	0.676	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 47

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.12: Peer effects on stayers, no large differences between classes (4th grade, Spanish)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Spanish	0.320***	0.040***	0.039***	-0.069
	(0.004)	(0.012)	(0.012)	(0.155)
Female	0.085***	0.067***	0.069***	0.070***
	(0.006)	(0.006)	(0.006)	(0.006)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	88693	88693	88693	88693
R^2	0.378	0.435	0.436	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 14

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.13: Peer effects on stayers, no large differences between classes (10th grade, Spanish)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Spanish	0.485***	0.165***	0.155***	0.131*
	(0.003)	(0.009)	(0.009)	(0.073)
Female	-0.037***	-0.026***	-0.022***	-0.022***
	(0.005)	(0.006)	(0.006)	(0.006)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	83577	83577	83577	83577
R^2	0.513	0.547	0.551	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 32

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Another concern is that mean ability may not be the only aspect that matters when quantifying peer effects. It is plausible that dispersion in ability could be relevant as well. In fact, the evidence about the effects of heterogeneous class composition is mixed (Cortes et al., 2014). Therefore, I add the standard deviation of class scores in my regressions, as a further robustness check. The results are in Tables 2.14-2.17 in the appendix section. The standard deviation of scores within a class has a negative and statistically significant association with students' performance in both grades and both subjects. Increasing the dispersion in one standard deviation has an effect of reducing students' scores by less than 0.02 standard deviations, everything else equal. Besides this effect, it does not affect largely the results in mathematics, with the peer effects remaining with similar magnitudes and statistical significance compared to the baseline results. However, it does change some results for students in Spanish for students in tenth grade, as I show in Table 2.17. The peer effect is no longer significant at the 10% level (the p-value, not reported in the table, is approximately 0.11).

Furthermore, given the characteristics of the earthquake, students who received a large influx of displaced new classmates may have been affected not only by the changes in peer composition but also by a radical change in the number of students in the class. I test my results by adding growth in class size (as percentage) to the controls, in Tables 2.18-2.21. The coefficients of peer effects estimated with instrumental variables are lower in magnitude and, with the exception of mathematics in tenth grade, are not statistically significantly different from zero anymore. On the other hand, the coefficients of peer effects remain stable when I use OLS estimation. The results show that an increase in class size of 10% reduces the score of a student between 0 and 0.02 standard deviations.

2.5.3 Nonlinearities

My results using quantile regressions suggest some non-linearity in the peer effects, especially in mathematics with different patterns when comparing fourth grade with tenth grade. I show these features graphically in Figure 2.2. For the group M4, it shows that students in the middle of the distribution of scores are less affected by their peers than students towards either the bottom or the top of the distribution of scores. The differences are statistically significant. For the group M10 the nonlinearities are of a smaller magnitude, except for students in the bottom of the distribution of scores being clearly less affected by their peers than the rest. For Spanish, the opposite is true for the group S4, in which students at the bottom of the distribution seem more affected by their peers than the rest. Finally, for the group S10 the most interesting pattern is that one quantile towards the top achievers is affected negatively by their peers, even though the coefficient is not statistically significantly different from zero.

One interesting implication from the results of the group M4 is that it may be possible to reallocate the students within a school in a way that marginally improves the average score of the school and reduces the inequality between students' outcomes. The alternative allocation is to put the bottom 20% of students with the top 30% in one group, and have a second group with the rest. To test this claim I run some simulations. The typical school in my sample has 2 classes and on average 30 students in each class, grouped in a way that suggests a random allocation within the school. I run the following simulation: first I generate randomly 60 observations drawing scores from a normal distribution with mean 250 and standard deviation of 50, which are approximately the parameters of the SIMCE exam. Then I standardise their scores to mean zero and standard deviation of 1. I calculate the peer score for student i considering all the students in the school except the student's own score. This would be an equivalent to a perfectly mixed peer group, similar to what I observe in the data. Then I rank the observations according to the scores and I put in one group the lowest 12 observations with the top 18 and in the other group the other 30 observations. I recalculate the peer score for each observation and, using the results from the quantile regressions, I recalculate the score for each observation with the simulated new peer score. On average this new allocation improves the score of the group by approximately 0.01 standard deviations. The magnitude is similar to an increase of one year in father's education for example.

One study that tries to allocate groups in an optimal way is Carrell et al. (2013). They group freshmen entering at the United States Air Force Academy in an way supposed to benefit those who performed worst in exams. However, the result is that the treatment group performed worse than the group of peers allocated randomly. The explanation is that freshmen formed subgroups of more homogeneous peers, for example among students who performed badly in previous exams. One difference between their experiment and the context studied in this paper is that in primary education it is possible for the teacher to allocate students in sub-groups either for working in the classroom or at home. If done in an earlier stage of the academic year, it may be possible form heterogeneous groups of students which are beneficial for the low achievers. I am aware of the difficulties of identifying in an early stage the ability of children. Another difference is that according to the information they had before the experiment, it should have helped the low achievers to improve without negatively affecting the high achievers. My proposal is less ambitious in that sense because it may decrease the score of the high achievers. However, this effect is more than offset in the simulations by the improvement of the low achievers, improving average performance and reducing inequality.

2.6 Conclusion

The first conclusion is that apparently the largest source of bias of the peer effects comes from the self-selection into schools. Across all the four specifications the coefficient of peer effect decreases by nearly 50% or more once controlling for school fixed effects and class characteristics There is a further reduction in the coefficient when using instrumental variables, but the difference is, on average, smaller in magnitude. When taking into account a possible sorting within schools, some coefficients go further down but not as much as when controlling for school fixed effects. Secondly, the main difference between students in fourth grade compared to those in tenth grade is that the peer effect in mathematics is consistently higher for older students. The same is not true for Spanish, in which case it depends in the specification. Another relevant conclusion is that the results derived from

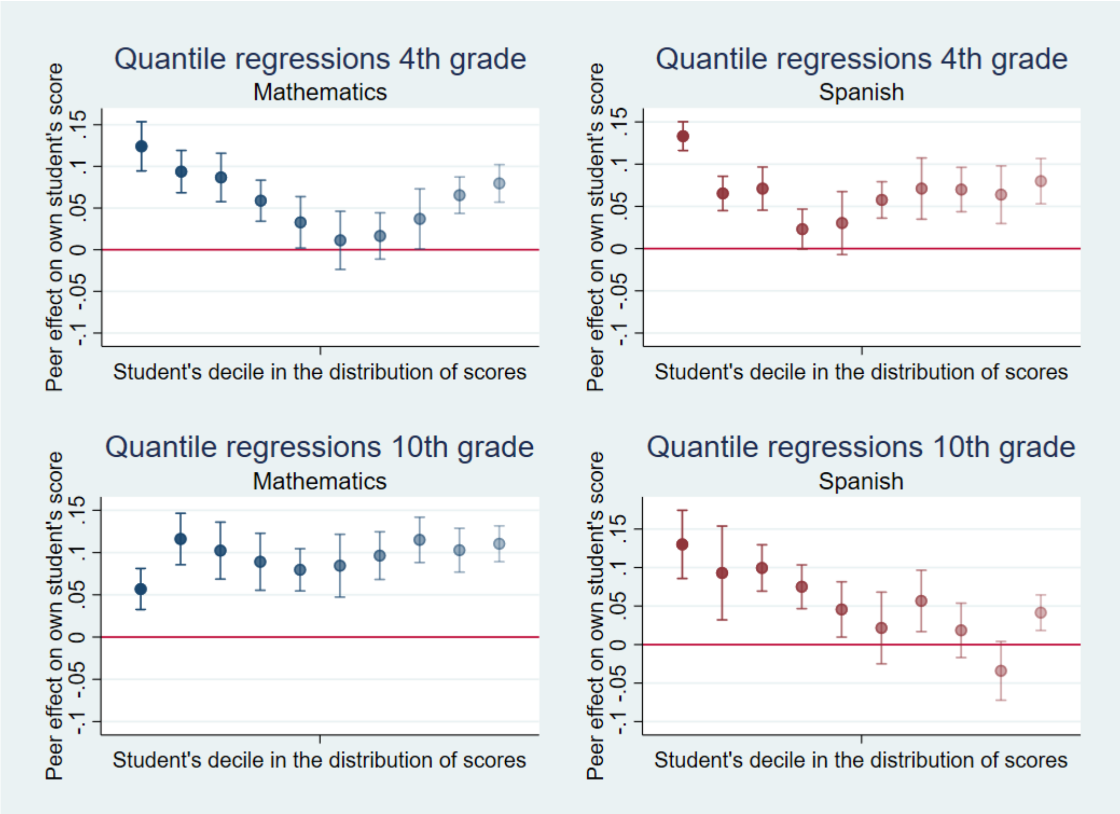


Figure 2.2: Quantile regressions using instrumental variables and school fixed effects.

the estimation using instrumental variables are more sensitive to changes in the sample analysed or the controls included.

Regarding nonlinearities, the results suggest that peer effects are not linear. For both subjects in fourth grade, students in the middle of the distribution of scores are less affected by their peers than students in the bottom of the same distribution. Furthermore, only one of the quantile groups have a negative point estimate for the peer effect, even though this is not statistically significantly different from zero. I can conclude that on average it is better to have better peers, or at least it is not harmful. However, the nonlinearities found are not strong enough to allow me to find a pareto improving allocation of students within schools. I do show some results derived from simulations that give some hope to reduce educational inequality without negatively affecting average achievement, and even improving it marginally.

An important caveat of this study is that displaced students may have affected the stayers through channels not controlled by my empirical strategy. For example, either student behaviour or class size might have been affected in a different way across classes within a school, which may in turn introduce some sort of bias to the main results. Another possible limitation of the study may come from the issue of selection into classes, which may not be entirely ruled out. Another limitation may be the external validity of my results, given that I use a natural disaster as a source of variation.

Patacchini et al. (2017) finds that only long-lasting friendships have an important effect. Even though the data does not allow me to identify groups of friends within the same class, it may be an interesting extension of this study to consider the length of the student in the same peer group.

2.A Appendix to Chapter 2

2.A.1 Tables

Table 2.14: Peer mean and dispersion, OLS and IV (4th grade, Mathematics)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Maths	0.420***	0.218***	0.216***	0.160**
	(0.004)	(0.009)	(0.009)	(0.070)
Class SD in maths scores		-0.010***	-0.010***	-0.013**
		(0.004)	(0.004)	(0.006)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	107095	107095	107095	107095
R^2	0.519	0.580	0.582	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 55

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.15: Peer mean and dispersion, OLS and IV (10th grade, Mathematics)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Maths	0.584***	0.263***	0.254***	0.206***
	(0.003)	(0.009)	(0.009)	(0.051)
Class SD in maths scores		-0.017***	-0.018***	-0.019***
		(0.003)	(0.003)	(0.003)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	89788	89788	89788	89788
R^2	0.638	0.672	0.674	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 64

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.16: Peer mean and dispersion, OLS and IV (4th grade, Spanish)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Spanish	0.326***	0.075***	0.073***	0.145**
	(0.004)	(0.010)	(0.010)	(0.058)
Class SD in Spanish scores		-0.081***	-0.081***	-0.062***
		(0.005)	(0.005)	(0.016)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	107095	107095	107095	107095
R^2	0.386	0.438	0.440	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 65

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.17: Peer mean and dispersion, OLS and IV (10th grade, Spanish)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Spanish	0.485***	0.179***	0.168***	0.109
	(0.003)	(0.008)	(0.008)	(0.069)
Class SD in Spanish scores		-0.011***	-0.012***	-0.014***
		(0.003)	(0.003)	(0.004)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	89788	89788	89788	89788
R^2	0.511	0.544	0.548	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 41

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.18: Peer effects and growth in class size, OLS and IV (4th grade, Mathematics)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Maths	0.421***	0.219***	0.217***	0.067
	(0.004)	(0.009)	(0.009)	(0.093)
Growth class size 2009- 2010		-0.001***	-0.001***	-0.002***
		(0.000)	(0.000)	(0.001)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	105674	105674	105674	105674
R^2	0.519	0.580	0.582	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 37

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.19: Peer effects and growth in class size, OLS and IV (10th grade, Mathematics)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Maths	0.585***	0.265***	0.256***	0.208***
	(0.003)	(0.009)	(0.009)	(0.062)
Growth class size 2009- 2010		-0.000	-0.000	-0.001
		(0.000)	(0.000)	(0.000)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	87390	87390	87390	87390
R^2	0.638	0.672	0.674	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 43

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.20: Peer effects and growth in class size, OLS and IV (4th grade, Spanish)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Spanish	0.326***	0.125***	0.124***	0.062
	(0.004)	(0.008)	(0.008)	(0.078)
Growth class size 2009- 2010		-0.002***	-0.002***	-0.002***
		(0.000)	(0.000)	(0.001)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	105674	105674	105674	105674
R^2	0.386	0.438	0.440	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 37

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.21: Peer effects and growth in class size, OLS and IV (10th grade, Spanish)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Peer score Spanish	0.487***	0.175***	0.165***	0.039
	(0.003)	(0.008)	(0.008)	(0.095)
Growth class size 2009- 2010		-0.002***	-0.001***	-0.002***
		(0.000)	(0.000)	(0.001)
Individual controls	Yes	Yes	Yes	Yes
Class level controls	No	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes
Family background controls	No	No	Yes	Yes
Observations	87390	87390	87390	87390
R^2	0.511	0.544	0.548	N/A

Standard errors in parentheses clustered at the class level. Column 1 includes individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared and school fixed effects. Column 3 adds characteristics from the family background including parents' education, books available at home and household monthly income. Column 4 includes the same controls as column 3 but shows the results of the IV estimation, using the shared of displaced students in the class as an instrument for mean ability. F statistic of weak identification for IV: 25

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.22: First stages for all grades and subjects from baseline results

	(1) M4	(2) S4	(3) M10	(4) S10
Share of displaced students in the class	-0.923***	-1.098***	-1.059***	-0.992***
	(0.131)	(0.153)	(0.131)	(0.151)
Female	0.007***	0.005*	-0.004	0.003
	(0.002)	(0.002)	(0.003)	(0.003)
GPA 1st grade	0.013***	0.013***		
	(0.003)	(0.003)		
GPA 2nd grade	-0.006	-0.007		
	(0.004)	(0.005)		
GPA 3rd grade	-0.002	-0.003		
	(0.004)	(0.004)		
GPA 7th grade			0.013***	0.012***
			(0.002)	(0.003)
GPA 8th grade			0.037***	0.049***
			(0.004)	(0.004)
GPA 9th grade			0.042***	0.043***
			(0.003)	(0.004)
Class size	0.095***	0.085***	0.007	0.010
	(0.014)	(0.016)	(0.010)	(0.010)
Class size squared	-0.001***	-0.001***	0.000**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
More than 10 books at home	0.003	0.005**	0.014***	0.016***
	(0.002)	(0.002)	(0.003)	(0.003)
Father education	0.001	0.001*	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Mother education	0.002***	0.001***	0.003***	0.003***
	(0.000)	(0.000)	(0.001)	(0.001)
Household monthly income	0.002***	0.002***	0.004***	0.004***
	(0.000)	(0.001)	(0.001)	(0.001)
Observations	107095	107095	89788	89788

Standard errors in parentheses clustered at the class level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.23: First stages for school selection robustness

	(1) M4	(2) S4	(3) M10	(4) S10
Share of displaced students in the class	-0.463***	-0.590***	-0.988***	-0.954***
	(0.133)	(0.157)	(0.139)	(0.162)
Female	0.006***	0.004	-0.006*	0.002
	(0.002)	(0.003)	(0.003)	(0.003)
GPA 1st grade	0.006**	0.006*		
	(0.003)	(0.003)		
GPA 2nd grade	-0.005	-0.010**		
	(0.004)	(0.005)		
GPA 3rd grade	-0.004	-0.004		
	(0.004)	(0.004)		
GPA 7th grade			0.014***	0.012***
			(0.002)	(0.003)
GPA 8th grade			0.039***	0.051***
			(0.004)	(0.004)
GPA 9th grade			0.043***	0.045***
			(0.004)	(0.004)
Class size	0.048***	0.024	0.007	0.008
	(0.015)	(0.016)	(0.010)	(0.011)
Class size squared	-0.000*	-0.000	0.000**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
More than 10 books at home	0.000	0.002	0.014***	0.017***
	(0.002)	(0.002)	(0.003)	(0.004)
Father education	0.001	0.000	0.002***	0.003***
	(0.000)	(0.001)	(0.001)	(0.001)
Mother education	0.002***	0.001**	0.004***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
Household monthly income	0.001**	0.001**	0.003***	0.003***
	(0.000)	(0.001)	(0.001)	(0.001)
Observations	87028	87028	81030	81030
R^2	0.908	0.877	0.910	0.888

Standard errors in parentheses clustered at the class level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.24: First stages for all grades and subjects from baseline results with mean and SD of ability.

	(1) M4	(2) S4	(3) M10	(4) S10
Share of displaced students in the class	-0.966***	-1.138***	-1.033***	-0.959***
	(0.130)	(0.152)	(0.130)	(0.150)
Class SD in maths scores	-0.058***	-0.055***		
	(0.009)	(0.010)		
Class SD in Spanish scores			-0.028***	-0.036***
			(0.007)	(0.008)
Female	0.006***	0.004*	-0.004	0.002
	(0.002)	(0.002)	(0.003)	(0.003)
GPA 1st grade	0.013***	0.013***		
	(0.003)	(0.003)		
GPA 2nd grade	-0.005	-0.006		
	(0.004)	(0.005)		
GPA 3rd grade	-0.002	-0.003		
	(0.004)	(0.004)		
GPA 7th grade			0.013***	0.012***
			(0.002)	(0.002)
GPA 8th grade			0.037***	0.049***
			(0.004)	(0.004)
GPA 9th grade			0.043***	0.043***
			(0.003)	(0.004)
Class size	0.098***	0.088***	0.007	0.011
	(0.015)	(0.016)	(0.010)	(0.010)
Class size squared	-0.001***	-0.001***	0.000**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
More than 10 books at home	0.004*	0.006**	0.014***	0.017***
	(0.002)	(0.002)	(0.003)	(0.003)
Father education	0.001	0.001*	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Mother education	0.001***	0.001***	0.003***	0.003***
	(0.000)	(0.000)	(0.001)	(0.001)
Household monthly income	0.002***	0.002***	0.004***	0.004***
	(0.000)	(0.001)	(0.001)	(0.001)
Observations	107095	107095	89788	89788
R^2	0.884	0.849	0.911	0.888

Standard errors in parentheses clustered at the class level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.25: First stages for all grades and subjects from baseline results and growth of class size.

	(1) M4	(2) S4	(3) M10	(4) S10
Share of displaced students in the class	-0.853***	-1.004***	-0.889***	-0.785***
	(0.141)	(0.164)	(0.135)	(0.156)
Growth class size 2009-2010	-0.003***	-0.004***	-0.005***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
Female	0.007***	0.005*	-0.004	0.003
	(0.002)	(0.003)	(0.003)	(0.003)
GPA 1st grade	0.013***	0.012***		
	(0.003)	(0.003)		
GPA 2nd grade	-0.007	-0.008		
	(0.004)	(0.005)		
GPA 3rd grade	-0.002	-0.003		
	(0.004)	(0.004)		
GPA 7th grade			0.013***	0.011***
			(0.002)	(0.003)
GPA 8th grade			0.037***	0.048***
			(0.004)	(0.004)
GPA 9th grade			0.042***	0.042***
			(0.003)	(0.004)
Class size	0.088***	0.078***	0.018*	0.025**
	(0.014)	(0.015)	(0.010)	(0.011)
Class size squared	-0.001***	-0.001***	0.000*	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
More than 10 books at home	0.002	0.004*	0.013***	0.016***
	(0.002)	(0.002)	(0.003)	(0.003)
Father education	0.001	0.001*	0.002***	0.002***
	(0.000)	(0.001)	(0.000)	(0.001)
Mother education	0.002***	0.002***	0.004***	0.004***
	(0.000)	(0.001)	(0.001)	(0.001)
Household monthly income	0.002***	0.002***	0.003***	0.003***
	(0.000)	(0.001)	(0.001)	(0.001)
Observations	105833	105833	87396	87396
R^2	0.884	0.848	0.911	0.889

Standard errors in parentheses clustered at the class level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 3

School Year Length and Student Performance

3.1 Introduction

Spending more time at school would appear to be an obvious thing to do to improve children's achievement. However, to find a causal link it is necessary to find an exogenous source of variation in instruction time (Rivkin and Schiman, 2015). In practice, the length of study measured by the average hours per year of instruction time varies substantially across OECD countries. For instance, in Finland the compulsory instruction time per year in primary school is 632 hours, in Italy it is 891 hours while in Chile it is 1039 hours, and the OECD average is 804 hours (OECD, 2015). At the country level, a relationship between length of study and achievement is not clear cut (Lavy, 2015). In international comparisons such as the Programme for International Student Assessment, PISA, Chilean students perform worse than most of the other OECD countries in all the three subjects measured, Science, Reading and Mathematics (OECD, 2016). Chile is the country with more instructional hours in its curriculum. However, is still not performing well. Therefore, one natural question arises: Does it pay to increase the amount of time children spend at school?

To answer this question, I use the earthquake that hit Chile in February 2010, two days before the start of the academic year, officially the first of March. Because of the earthquake more than 50% of the schools were damaged in varying degrees, and some of them were not able to reopen by the start of the school year for up to eight weeks. Every year, all students in fourth grade must sit a national standardised exam, called SIMCE, in October. As a result of the earthquake, by the time of the exam some students had received up to eight weeks less of instruction at school depending on how long it took for their school to reopen. Therefore, the earthquake provides an exogenous source of variation in schooling time. To identify the causal effect of instruction time I use a difference-in-differences framework. I compare the change in exam scores before and after the earthquake. The treatment group consists of students who were attending schools located in the affected area. Furthermore, I divide this group in several treatments depending on how much they were affected by the earthquake. The control group consists of students who were in schools in areas not affected by the earthquake.

My main results show that students attending schools severely damaged by the earthquake, who may have lost up to eight weeks of schooling, perform 0.081 standard deviations lower in mathematics than in the counterfactual scenario of not having been hit by the natural disaster. This effect is robust to the different controls. Approximately half of the effect may be related to school closures, and the other half to other effects of the earthquake. The results of students in affected areas attending schools not damaged are 0.038 standard deviation lower than in the case of no earthquake. Therefore, the potential effect of a shortened schooling time nearly 5% may be approximately -0.043 standard deviations in mathematics.[1] On the contrary, for Spanish there is no evidence of a statistically significant effect.

A negative effect of the 2010 earthquake over student's achievement was found in the study by Tincani (2017) as well, although the empirical strategy and the data used are not entirely comparable. In that study there are two cohorts with exam scores observed twice, in fourth and eighth grade. The first cohort is observed entirely before the earthquake, and the second cohort sat the fourth-grade exam before the earthquake and the eighth-grade exam after it.[2] To define the treatment and control groups, Tincani (2017) splits students between affected and non-affected areas, which I use as a starting point. Afterwards, I further split the treatment group in two: students affected by the earthquake in schools not damaged, and students in schools damaged by the earthquake, who may have faced a shortened school year prior to the exams. Afterwards, I split the latter group by the level of damage suffered by the school, which was recorded in three levels by the Ministry of Education. The main difference between my paper and Tincani (2017) is

[1] I am considering cumulative schooling time from first grade in primary education when stating 5%.

[2] I do not use that data in my study for two reasons. The first one is that schools may have been able to add some weeks at the end of 2010 to compensate for the time lost at the beginning. My second reason is that in 2011 there were some student strikes which may add noise to my estimation.

that in my study I try to address the question of instruction time lost due to school closures, which I assume depends on the damage suffered because of the earthquake, and on the other hand, Tincani (2017) uses the earthquake's intensity as a source of variation in student's study effort to address questions related to peer effects.

The answer to the question about instruction time is important for many reasons. One of them is about the costs involved. Increasing instruction time is a costly policy and sometimes more space is required, for instance in the case of a change of regime from two shifts (morning and afternoon) to a system in which all students spend the whole day at the school as was the case of the reform implemented in Chile in the early 2000s (Bellei, 2009). If the policy is not effective, then it might be worth exploring other options on how to spend that money. Another reason is the opportunity cost of time. It might be worth to consider reducing the number of hours of children exposed to instruction and allow the teachers to use that time in preparing themselves better, for example. This may be useful in the context of educational systems similar to the Chilean one where teachers have a low share of non-teaching hours (Avalos and Valenzuela, 2016).

So far one of the few published studies about the effects of instruction time on student performance using Chilean data is Bellei (2009), who looks for the effect of the full school day reform on academic achievement, using data for students in secondary education. According to the literature, the effects of instruction time seem to differ across ages, as well as across countries (see Lavy (2015) and Woessmann (2016) for cross-country comparisons). Hence, my study is not directly comparable to Bellei (2009). Neither it is comparable to Tincani (2017). The main focus of that study is on peer effects, using the earthquake as a source of variation in the cost of effort for students affected by it, measured by the intensity of the earthquake at the student's home.

One problem when studying this topic is to find an exogenous source of variation in instruction time to overcome endogeneity problems (Rivkin and Schiman, 2015), because attendance rates may be correlated with some unobserved characteristics of the student that are likely to be correlated with their outcomes in standardised exams, such as motivation. For example, the study by Gottfried (2009) compares excused and unexcused absences finding that students who miss classes with a valid excuse (for instance because of sickness), perform better than students having a similar number of unexcused absences (truancy for instance). The results hold for both reading and math scores, and shows that attendance rates may not be the best way to investigate this question.[3]

Some studies have used different kind of quasi-experimental designs to study this research question. Among the papers that use a policy change to explore this question one of the most cited ones is Pischke (2007) who uses a reform in Germany in the late 1960s as a source of exogenous variation in the time that students spent at school. It is one first studies to use that kind of approach at a national level (see Patall et al. (2010) for a review of the literature). While he finds a negative effect of having a shorter school year on the pass rate of students (his measure of student performance), he doesn't find any significant effect on future earnings. A similar approach is used in Aguero and Beleche (2013), using a quasi-experimental design to

[3] I only observe attendance rates so I cannot use a similar approach.

study the effect of a shorter school year on student achievement in Mexico, finding that having more school days before an examination does improve the results, although with diminishing marginal returns. Also looking at policy changes, Meroni and Abbiati (2016) and Battistin and Meroni (2016) use data from consecutive cohorts in 2009 and 2010, with a difference-in-differences estimation strategy very similar to my approach. The results of Meroni and Abbiati (2016) are significant only for mathematics and not for Italian language. On the other hand, Huebener et al. (2016) also uses a policy as the source of variation but in the number of weekly hours rather than the number of days, finding a positive significant effect of the increase in the hours on student's achievement. The main difference between this paper and the studies that use a policy change as the source of variation is that policy endogeneity is not an issue in my case (Besley and Case, 2000).

Among the literature that uses some sort of unscheduled random school closures to see any effects on student performance, thus a similar source of variation as the one that I use, Sacerdote (2012) finds that in the short run the Katrina hurricane has a negative impact on schooling outcomes. The main difference between his study and mine is that he observes a much larger displacement of students to other cities (and schools) than I do, generating additional disruption Hanushek et al. (2004). Another study that uses hurricanes as a source of variation is the one by Spencer et al. (2016). In their paper they use the number of hurricanes and look for effects on achievement, distinguishing subjects by being related to science or not. They find a significant effect on science-related subjects when the hurricane occurs during the academic year. In subjects not science-related such as Spanish they do not find any significant effects, which is similar to my results. Two papers that attempt to answer a similar research question as mine are Marcotte (2007) and Marcotte and Hemelt (2008). In their work the exogenous source of variation in time spent at school is provided by snowfall. In winter, some schools have to close due to snow and therefore the children attending those schools are exposed to less instructional time. They find that there is a negative effect of school closures on student performance, and that the effect is small for children in 5th and 8th grade compared to children in 3rd grade. Furthermore, the effect is larger in mathematics. Also, the study by Hansen (2011) uses weather-related variation as an empirical strategy to try to answer this question, finding a negative effect of school closures on student outcomes in mathematics. Since in my study I use multiple treatments, with consistent results, this may be an advantage over the standard difference-in-differences with only one treatment group (Meyer, 1995).

On the strand of literature that looks for the relationship of student absences and student achievement, it is important to note that one has to be careful in the interpretation of possible correlations as causal effects. One of the studies, Aucejo and Romano (2014), finds that increasing the number of school days is less effective than reducing student absences by the same amount of time. In addition, the effects are larger in math compared to reading. A similar approach used the study by Goodman (2014).

The rest of the study is organised as follows: section 2 gives an overview to the Chilean school system and the Earthquake, section 3 covers the data, its sources and some descriptive statistics, section 4 is about the empirical strategy, in section 5 I go through the results and robustness checks while in section 6 there is some discussion about those results and the conclusions.

3.2 The Chilean school system and the Earthquake

3.2.1 Chilean school system

The Chilean school system is organised in three tiers: primary school from first to fourth grade, lower secondary school from fifth to eighth grade, and upper secondary school from ninth to twelfth grade. Children usually start primary school at the age of six-seven years and leave upper secondary school at the age of seventeen-eighteen years.

There are three types of schools: public, private subsidised, and private schools (Valenzuela et al., 2014). The public, or municipal, are run by the municipalities, cannot charge any fees to the students and receive funding from the state. The private subsidised also receive funding from the state but in addition they were allowed to charge some tuition fees to the students at the time of the exams analysed in this study. Finally, private schools do not receive any funding from the state and are allowed to charge tuition fees. Around 90% of the students attend either public or subsidised schools, while less than 10% are enrolled in private schools in fourth grade. Public schools cannot select students before seventh grade. On the other hand, private subsidised and private schools may interview parents, hold some playing sessions for the children or do other type of selection processes (Anand et al., 2009).

In order for a student to progress to the next academic year, the following rules apply. From first to second grade and from third to fourth grade, every student with an attendance rate of at least 85% will be automatically promoted to the next year. The minimum attendance of 85% is a necessary but not sufficient condition in other grades. From second to third grade and from fourth to twelfth grade, students must either have passed all their subjects (minimum of 4.0 in a scale from 1 to 7 in teachers' assessment), have an average of 4.5 if they have failed one subject, or an average of 5.0 if they have failed 2 subjects. The averages are calculated considering the compulsory subjects in the national curricula, including Mathematics, Spanish, History among others. I refer to this average as GPA or teacher assessment in the rest of the paper.

To measure the quality of education, there are exams known as SIMCE, that are sat by students in fourth, eighth and tenth grade. In the case of students in fourth grade, this is every year. The exams are centralised and the same for every child in the country. The results are publicly available at the school level. If a school shows a poor performance, then some sort of support is given to it to improve. On the other hand, if a school shows a good performance in the SIMCE, a bonus is paid to their teachers (Carnoy et al., 2007). According to Mizala and Urquiola (2009), the existing descriptive evidence does not suggest that the incentives have changed average testing performance.

In the case of primary school, the same teacher will teach most of the subjects in a given class. From fifth grade onwards, in most schools, students are taught each subject by a different teacher.

3.2.2 Earthquake

Chile was struck by an earthquake on 27th February 2010, at 3:34 am local time. Its magnitude was 8.8 Richter in the epicentre, approximately 400 km south of Santiago, the capital, and it was the fifth-largest ever instrumentally recorded in the world (Astroza et al., 2010).[4] It affected six of the fifteen regions of the country. Approximately 80% of the population lived in that area. The earthquake occurred just 2 days before the official start of the school year, set to be the 1st of March 2010 and to include 38 weeks of teaching. [5] According to the information retrieved from the Chilean Ministry of Education (MINEDUC), more than half of all schools (4635 out of nearly 9000) were damaged. The damage was classified as: minor, moderate, or severe.

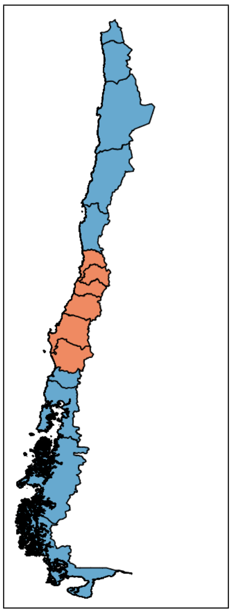
Figure 3.1 shows a map of the country divided by regions. The area affected by the earthquake includes 6 of the 15 regions, and is shown in red. The regions not affected by it are in blue.[6] It covers the three most populous cities in the country: Santiago, Valparaiso and Concepcion. The epicentre was on the coast, and after the earthquake there was a tsunami which was responsible of most of the deaths due to the earthquake. The official number

[4] Chile is a seismic country. According to the information from the National Seismological Centre (*Centro Sismologico Nacional*), before 2010 there were 109 earthquakes of magnitude at least 7.0, from Arica (northernmost part of the country) to Tierra del Fuego (southernmost part of the country). This, considering all the earthquakes from 1570. More accurate measures are from the beginning of the 20th century, and in that case the number of earthquakes before 2010 is 77. This represents on average 0.7 earthquakes per year, compared to 14.6 in the whole world since 1990 (or 0.07 per country per year, statistics from the United States Geological Survey). Another way to look at this is to say that Chile's surface is 0.56% of the world's, and its share of earthquakes is 5%.

[5] Information retrieved from MINEDUC.

[6] The definition of affected area comes from the Chilean Government, is at the regional level, and includes regions V-VI, VII, VIII, IX and the Metropolitan Region.

Figure 3.1: Map of Chile.



In red, the six regions affected by the earthquake. In blue, the areas not affected by it.

of deaths related to the earthquake and the subsequent tsunami was 525, according to the National Office for Emergencies ONEMI (*Oficina Nacional de Emergencias del Ministerio del Interior*).

Table 3.1 shows the share of students belonging to each of the categories depending on how they were affected by the earthquake. Students who were attending schools in areas not affected, the blue area in Figure 3.1, are the group not affected by it, comprises 23.2% of the sample. 25.6% of the students were "Affected" by the earthquake, studying in the red area but attending schools not damaged. 32.8% of the students attended schools which suffered Minor Damage, 9.4% of the children were studying in schools that suffered Moderate Damage, and 9.1% were in schools severely damaged by the earthquake.

Table 3.1: Sample composition

Not affected	0.232
	(0.422)
Affected	0.256
	(0.436)
Minor Damage	0.328
	(0.469)
Moderate Damage	0.094
	(0.292)
Severe Damage	0.091
	(0.287)
Observations	**290200**

3.3 Data

I use two sources of data. I use administrative data from the Ministry of Education, which includes the following information at the student level: yearly academic performance measured by the teacher, attendance rates, gender, a full record of schools attended every academic year, town in which the school is located and town where the student lives.[7] At the school level it includes type of school, which can be public, private subsidised by the state or private not subsidised by the state, the town where the school is and also if it is located in a rural or urban area. In addition, I have another dataset with information about the level of damage suffered by schools in the area affected by the earthquake, where approximately 70% of the children in fourth grade studied. It classifies the damage in three categories: minor

[7]Student's residence is self-reported and in some cases inconsistent with the location of the school attended, therefore I base my main analysis on school location.

damage, moderate damage and severe damage.[8]

The second main source comes from the national standardised exam called SIMCE (System to Measure the Quality of Education), organised by the Agency for the Quality of Education. Its main purpose is to provide information about students' learning in different areas of the national curricula, and to contribute to the improvement of the quality and equity of education. The exam is sat every year by students in fourth grade (nine to ten years old). Information about the students include the results in both mathematics and Spanish, parents' education measured in years, household monthly income in 13 categories, number of books available at home and class size.

3.3.1 Descriptive statistics

I present the summary statistics in Table 3.2, separating the data by year and by area, if it was affected or not by the earthquake. Column 1 shows information about students attending schools in areas not affected by the earthquake, the control group, in 2009. Column 2 shows the summary statistics of children studying in the area affected by the earthquake, which I call "Eregion", in 2009, before the earthquake. Column 3 includes the same summary statistics for the control group in 2010 and column 4 for students located in the earthquake region. Nearly three quarters of the students attend schools located in affected regions, which are the areas with the highest population density in the country. From the comparison of students in the different areas

[8]Furthermore, I have data about student scores in previous years, from 2005 onwards and also from 2011 to 2013. However, the information about the student's background and classroom level information is not recorded in a comparable way since the surveys were modified often. The years 2009 and 2010 are the most comparable ones.

the year before the earthquake, there are barely any differences when comparing the different groups. For example, the average score in mathematics is the same in both groups, but it decreases for the affected students in 2010. Surprisingly, the narrow gap in Spanish decreases in 2010. One exception is the share of students attending private subsidised schools 57% vs 49% in 2009, 57% vs 47% in 2010. This may be explained by the population density, because other variables such as parent's education and books available at home do not suggest any large differences in average income between the two groups. Class size is slightly higher in the region affected by the earthquake, 30.58 vs 28.40 children per class, which may be partly explained at least by the fact of having a smaller share of students in rural schools, a difference of approximately 1.5% , which tend to serve fewer children. Overall, the data do not suggest significant differences between children studying in the different areas before the earthquake occurred.

It is important to mention that although the data from scores come from the entire population, students attending private schools in the affected areas do not appear in most of the cases. This is due to the fact that the Ministry of Education only collected data on the damage for public and private subsidised schools, therefore most of the private schools are reported with no information.

3.4 Empirical Strategy

The main goal of this paper is to identify the causal effect of having less time at school on the student performance, measured by the SIMCE test.

Table 3.2: Summary statistics by year and region

	(1) Controls 2009	(2) Eregion 2009	(3) Controls 2010	(4) Eregion 2010
Math	252.1	252.1	252.1	250.3
	(53.71)	(53.97)	(52.45)	(52.39)
Spanish	263.8	261.0	270.9	269.3
	(52.04)	(52.81)	(49.88)	(49.79)
Female	0.506	0.497	0.495	0.498
	(0.500)	(0.500)	(0.500)	(0.500)
GPA 1st grade	6.383	6.308	6.361	6.287
	(0.478)	(0.501)	(0.474)	(0.499)
GPA 2nd grade	6.231	6.137	6.199	6.121
	(0.497)	(0.519)	(0.498)	(0.513)
GPA 3rd grade	6.069	5.958	6.037	5.940
	(0.525)	(0.537)	(0.521)	(0.528)
Attendance in 4th grade	93.41	93.54	93.44	93.72
	(5.312)	(5.572)	(5.329)	(5.628)
Class size	28.40	30.58	29.38	30.71
	(10.51)	(9.560)	(10.37)	(9.543)
Private Subsidised	0.490	0.568	0.469	0.570
	(0.500)	(0.495)	(0.499)	(0.495)
Rural school	0.134	0.118	0.129	0.116
	(0.341)	(0.322)	(0.336)	(0.321)
Father education	11.05	10.67	11.33	10.97
	(3.952)	(3.808)	(3.478)	(3.357)
Mother education	11.08	10.78	11.35	11.05
	(4.080)	(3.825)	(3.642)	(3.375)
10 or more books	0.588	0.582	0.606	0.614
	(0.492)	(0.493)	(0.489)	(0.487)
Observations	**31799**	**114545**	**43225**	**139464**

I do so by exploiting the earthquake as a source of exogenous variation. I use a difference-in-differences identification strategy. First, I show that the parallel trend assumption seems to hold for all the students in both subjects. Figure 3.2 shows the national average scores per year between 2005 and 2010 for the different groups analysed in mathematics. The academic achievement of students attending schools that were severely damaged by the earthquake present a sharp decline in 2010 in maths. In the case of other students attending damaged schools, their scores also decrease after the earthquake but in a moderate way. Figure 3.3 shows the same information for Spanish. However, all the groups show similar trends before and after the earthquake.

I start with an econometric specification to measure the effect of the earthquake on the exam for those students studying in the affected area, in a similar way to Tincani (2017). My reference group in this equation, and the ones to follow, comprises students located in areas not affected by the earthquake. All other students are in the treatment group, and I am looking for the average treatment effect of the earthquake. The regression equation is:

$$\begin{aligned} TestScore_{icsmt} = \alpha_0 + \alpha_1 Post_{icsmt} + \alpha_2 Eregion_m \\ + \alpha_3 Post_{icsmt} * Eregion_m + \alpha_4 X_{icsmt} + u_{icsmt} \end{aligned} \tag{3.1}$$

where $TestScore_{icsmt}$ is the mathematics or Spanish score in SIMCE for student i in classroom c, attending school s located in town m in time t, $Post_{icsmt}$ is a dummy that takes value 1 if the student is observed in 2010 or 0 if observed in 2009, $Eregion_m$ is a dummy variable that takes value 1 if town m is in the area affected by the earthquake and 0 otherwise, X_{icsmt} is a vector of individual, school and family characteristics, and u_{icsmt} is the error term. The coefficient of α_3 shows the effect of the earthquake on students somehow affected by it. It includes the effect of a shortened school year for some students only.

Afterwards, the second step is to look for the differences between students attending schools not damaged by the earthquake but in affected areas, "affected" by the earthquake, and children studying in schools damaged by it and potentially exposed to a shortened school year. I will call the effect on students affected by the earthquake "child damage", because it cannot be attributed to school closures. On the other hand, if a student was attending a school damaged by the earthquake, in addition to the child damage we can also think about the effect of what I will call "school damage", which includes the reduced time spent at school by students due to the temporary school closure and maybe other effects from the damage itself. I do not observe school damage isolated for two reasons: the first one is that I rely on school location rather than student's home, because hometown is self-reported by the student, making this information less reliable than school location. Therefore, by construction I do not separate studying in the affected area with only living in the affected area.[9] Secondly, the definition of affected area was made at the regional level, and a region contains on average 23 towns. Therefore, it is unlikely that a student will study in a different region from the one where

[9] The correlation between living and studying in the affected area is 0.99 and using one or the other does not change the results.

he resides.

This econometric specification can be written as:

$$
\begin{aligned}
TestScore_{icsmt} = \delta_0 + \delta_1 Post_{icsmt} + \delta_2 Affected_{sm} + \delta_3 Damaged_{sm} \\
+ \delta_4 Post_{istm} * Affected_{sm} + \delta_5 Post_{icsmt} * Damaged_{sm} \\
+ \delta_6 X_{icsmt} + e_{icsmt}
\end{aligned}
\tag{3.2}
$$

where $Affected_{sm}$ is a dummy that takes value 1 if the student is attending a school in the area affected (therefore "affected" by the earthquake) but not damaged by it, $Post_{icsmt} * Affected_{m}$ takes value one if the student is attending a school not damaged located in the affected area, and is observed in 2010, $Damaged_{sm}$ is a dummy that takes value 1 if the score is of a student attending a damaged school or 0 otherwise, $Post_{icsmt} * Damaged_{sm}$ is an interacted term that takes value 1 if the student is attending a damaged school and is observed in 2010 and 0 otherwise, and e_{icsmt} is the error term.

Equation 3.2 does not distinguish by the damage level, and includes students attending schools in the minor damage, moderate damage and severe damage categories. Therefore, it is likely that the average closure time was well below 8 weeks and the effect of school closures alone, $\delta_5 - \delta_4$, should be considered as the lowest bound of the effect of eight weeks and is likely to suffer from downward bias.

I may be able to get a better estimate of the effect of the shortened instruction time by splitting the dummy variable *Damaged* in the three known categories, and writing the econometric specification in the following way:

$$
\begin{aligned}
TestScore_{icsmt} = {} & \beta_0 + \beta_1 Post_{icsmt} + \beta_2 Affected_{sm} + \beta_3 MinorDamage_{sm} \\
& + \beta_4 ModDamage_{sm} + \beta_5 SevDamage_{sm} \\
& + \beta_6 Post_{istm} * Affected_{sm} + \beta_7 Post_{icsmt} * MinorDamage_{sm} \\
& + \beta_8 Post_{istm} * ModDamage_{sm} + \beta_9 Post_{icsmt} * SevDamage_{sm} \\
& + \beta_{10} X_{icsmt} + \epsilon_{icsmt}
\end{aligned}
\tag{3.3}
$$

where $MinorDamage_{sm}$ is a dummy variable that takes value 1 if the damage level of school s located in town m was classified as minor damage and 0 otherwise, $ModDamage_{sm}$ a similar dummy for the case of moderate damage and $SevDamage_{sm}$ is the corresponding dummy variable for severe damage. ϵ_{icsmt} is the error term. Equation 3.3 also includes interacted terms for each of the damage categories with the same structure as explained for equation 3.2.

3.5 Results

In this section I focus on the main results after estimating equation 3.3. The intermediate results of equations 3.1 and 3.2 are in the appendix section.

For all the tables that I present in this section, column 1 includes individual level controls such as gender and previous academic performance measured by the teacher. Column 2 adds class size, class size squared and dummies for school type. Column 3 adds some family level controls including parents' education, household income, and books available at home. The reference group is made by students attending schools located in the area not affected by the earthquake.

3.5.1 Mathematics

The results for mathematics in Table 3.3 show that students in the affected area perform on average better than the control group, regardless of being only affected by the earthquake or attending damaged schools. Students in the affected area perform on average 0.13 standard deviations better than the control group, result that is significant at the 1% level. The magnitude changes slightly depending on the controls included, but for all the levels is between 0.09 and 0.13 standard deviations. Students affected by the earthquake in schools not damaged by it, perform worse than in the counterfactual case of not being hit by the earthquake, by -0.038 standard deviations. This is significant at the 10% level in column 2 and column 3, when including school and family covariates, but not significant in column 1 when I include only the individual level controls. Even though the statistical significance does not seem very robust, the magnitude remains similar across columns. This coefficient reflects what I called "child damage", and cannot be attributed to school closures.

Students attending schools that suffered either minor or moderate damage, perform between -0.044 and -0.051 standard deviations worse than they would have in a non-earthquake year. This result is statistically significant at the 5% level for the first group and at the 10% level for the second group. Similar to students only affected by the earthquake, the result for children in schools with moderate damage is not statistically significantly different from zero in column 1, but the magnitude is robust to the addition of the different controls.

In the case of students attending schools that suffered severe damage by the earthquake, their achievement is 0.081 standard deviations lower than expected after the earthquake, and the coefficient is significant at the 1% level and robust to the addition of different covariates. This combines both effects, "child damage" and "school damage", for students who were more likely to have been out of school for up to eight weeks. If we subtract the effect of being only affected by the earthquake, the "child damage" effect, this gives a loss of 0.043 standard deviations in scores. These students may have lost approximately up to 5% of schooling days considering the cumulative time between first and fourth grade.[10]

3.5.2 Spanish

The results for Spanish are in Table 3.4. Students perform on average better in the year after the earthquake. The time effect is 0.16 standard deviations and is significant at the 1% level. This is consistent with Figure 3.3 and does not seem to be related with the earthquake, because is a pattern observed in all groups, independently of their location. In line with the results observed

[10]The academic year has 38 weeks. By the end of year 3, students would have attended 114 weeks. Given that the exams are sat in October, a student outside the affected area would have received approximately 144 weeks of schooling, compared to 136 in the case of students who had to wait eight weeks.

in mathematics, children studying in the affected area have a performance between 0.057 and 0.084 standard deviations better than their peers in the areas not affected. This is significant at the 1% level when adding all the controls, but not statistically significantly different from zero for students in schools severely damaged when only adding individual controls. None of the interacted terms of being observed after the earthquake and the different damage levels are statistically significantly different from zero, robust to the addition of controls. This shows that students did not see their scores in Spanish affected by the earthquake, compared to the counterfactual case of no earthquake

3.5.3 Results in context with other studies

My results are in line with the findings of Bellei (2009) for mathematics but are different for Spanish. Interestingly, Bellei (2009) observes a change in instruction time of approximately 5%, which is similar to the upper bound of schooling weeks that I observe. Compared to other studies that use natural disasters, the effect that I find in mathematics is approximately one third of what Sacerdote (2012) found for the short run effect of the Katrina hurricane, which sounds reasonable given that in the Chilean case students were less disrupted. Furthermore, consistent with the findings of Meroni and Abbiati (2016),Marcotte (2007), Marcotte and Hemelt (2008) and Spencer et al. (2016), I also find that scores in mathematics or related to science are more sensitive to changes in instruction time than subjects which belong to humanities, such as reading.

Table 3.3: Results for Mathematics, equation 3.3

	(1) Maths score	(2) Maths score	(3) Maths score
Post EQ	0.031*	0.030*	0.018
	(0.017)	(0.016)	(0.016)
Affected	0.169***	0.113***	0.131***
	(0.017)	(0.017)	(0.016)
Minor Damage	0.102***	0.115***	0.130***
	(0.017)	(0.016)	(0.015)
Moderate Damage	0.094***	0.120***	0.134***
	(0.023)	(0.022)	(0.021)
Severe Damage	0.106***	0.126***	0.134***
	(0.024)	(0.022)	(0.021)
postXAffected	-0.036	-0.037*	-0.038*
	(0.024)	(0.023)	(0.022)
postXMinorD	-0.042*	-0.046**	-0.044**
	(0.023)	(0.021)	(0.020)
postXModerateD	-0.051	-0.053*	-0.051*
	(0.032)	(0.030)	(0.029)
postXSevereD	-0.082**	-0.081***	-0.081***
	(0.032)	(0.031)	(0.029)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	290200	290200	290200
R^2	0.343	0.368	0.382

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4: Results for Spanish, equation 3.3

	(1) Spanish score	(2) Spanish score	(3) Spanish score
Post EQ	0.169***	0.170***	0.160***
	(0.015)	(0.014)	(0.014)
Affected	0.106***	0.056***	0.069***
	(0.015)	(0.015)	(0.015)
Minor Damage	0.041***	0.053***	0.064***
	(0.015)	(0.014)	(0.014)
Moderate Damage	0.049**	0.074***	0.084***
	(0.020)	(0.019)	(0.018)
Severe Damage	0.028	0.051**	0.057***
	(0.021)	(0.020)	(0.019)
postXAffected	0.015	0.011	0.011
	(0.020)	(0.019)	(0.019)
postXMinorD	0.016	0.012	0.013
	(0.020)	(0.018)	(0.018)
postXModerateD	-0.014	-0.017	-0.016
	(0.027)	(0.025)	(0.024)
postXSevereD	-0.020	-0.020	-0.020
	(0.028)	(0.027)	(0.026)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	290200	290200	290200
R^2	0.290	0.306	0.315

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.5.4 Robustness tests

Placebo Difference-in-Differences

I run a placebo difference-in-differences regression using data from years 2008 and 2009, before the earthquake. The rationale is that before the earthquake, there should be no effect of a school being damaged or not. In results reported in the appendix, I run all the regressions using the same covariates for the 2 years prior to the earthquake. The main results for mathematics are in Table 3.13. None of the interacted coefficients of interest are significant at the 10% level. On the other hand, Table 3.14 shows the results for Spanish, which also show no evidence of any effect of the earthquake before it happened. Furthermore, the differences between students in the affected and not affected areas remain quite similar over time, which also I showed in the graphs of parallel trends assumptions. Other intermediate results do not provide any evidence of an "effect" of the earthquake before it happened either, with the coefficients of interest not statistically significantly different from zero at the 10% level. These results are in Tables 3.9-3.12 in the appendix.

Additional checks

One concern is that some municipalities may be better prepared than others to deal with the earthquake, which may ultimately have some impact on my results. I run all the equations controlling for town fixed effects, finding in some cases slightly higher significance levels but not any relevant changes in the coefficients of interest.

3.6 Discussion and conclusion

I estimated the causal effect of being exposed to less schooling time on students' academic performance, in the context of a natural experiment, using the earthquake which struck Chile in 2010 as a source of exogenous variation, with a difference-in-differences strategy. The main findings of this study are the following: first that the earthquake had a negative and significant effect on students' performance in Mathematics. It didn't have a significant effect in Spanish. Second, that this effect can be decomposed in two parts for the students that were affected the most in terms of schooling time lost, roughly half was through the "school damage" channel, and two thirds through the "child damage" channel. The former refers to the loss of time at school and the latter refers to other effects of the earthquake such as disruption at student's home. The main assumption of this paper is that the school damage is strongly correlated with the loss in instruction time, which in the case of students attending schools severely damaged by the earthquake could be up to eight weeks.

However, it is important to say that the magnitudes may look rather small, with the total effect of the earthquake being less than 0.1 standard deviations in terms of scores. But it is also worth mentioning that, considering a student that started school at first grade (i.e. only has done the compulsory school, not pre-kinder or kindergarten), then eight weeks of schooling is roughly 5% of the total time spent at school. Another factor that could explain the modest effect is that Chile is currently the country with the highest amount of compulsory instructional time at school within the OECD members. If we assume that, at least at some point there are decreasing marginal returns, then since Chilean students are already spending so many hours that having less time at school is not that detrimental to their performance in the test. These days lost were also as far as away from the exams as possible.

Even though the study is using Chilean data, it could be possible to put them in a more general context, but mainly within primary school because the relative importance of time changes as the students are exposed to more and more years of schooling. The main policy implication is that increasing the time spent at school can increase the student performance in some subjects such as mathematics, but the question of if it is worth it or not will depend on the particular costs of each place. In the specific context of Chile, I would say that more than in the quantity of time spent at school, the focus should be on the quality and productivity of this time.

It would be interesting to know the reasons behind the difference in the results between mathematics and Spanish. One possible explanation could be that children and people in general communicate in a daily basis. Therefore, even when being absent from school it is an ability that can be developed outside school, for example reading books. On the contrary, math is not something that most of the children use in their daily activities and since it is more abstract, it may need a more structured way of being studied, at school rather than at home. Furthermore, in mathematics to learn the next step it is important to know the previous one.

The main caveat of this study is that I do not observe the actual closure of each school.[11] Hence, the assumption of a strong correlation between closure time and damage is essential for this paper. Another possible drawback is that the paper is based on repeated cross-sections rather than a proper panel. Therefore, the students observed in 2009 are not the same as the ones observed in 2010. However, there doesn't seem to be any reason to think that the two cohorts are particularly different one from each other.

A possible extension of this study could be to look how does the effect of instruction time differs across different students, either comparing them by socioeconomic level for instance or by previous academic achievement.

[11] From the Ministry of Education they told me that they did not have that information available.

3.A Appendix to Chapter 3

3.A.1 Figures

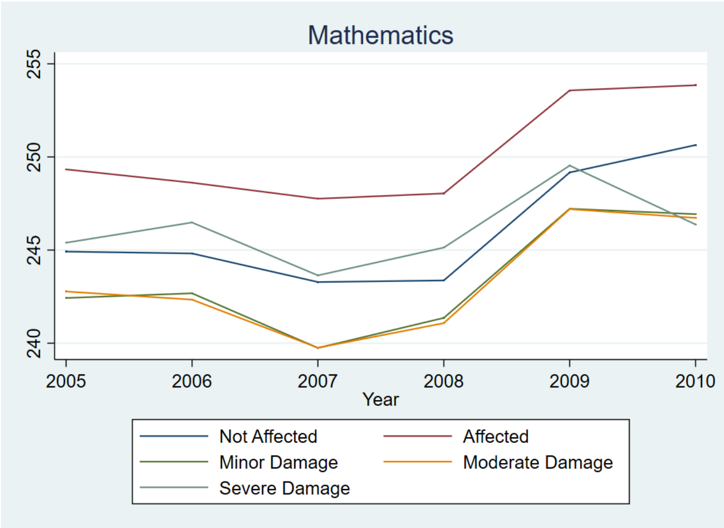


Figure 3.2: Parallel trends Mathematics
It shows national average scores of mathematics between years 2005-2010.

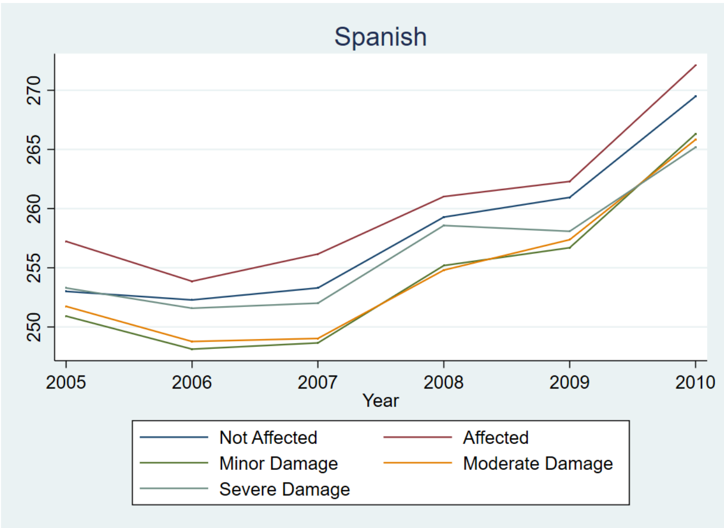


Figure 3.3: Parallel trends Spanish
It shows national average scores of Spanish between years 2005-2010.

3.A.2 Tables

Intermediate results for 2009-2010

Table 3.5: Results for Mathematics, equation 3.1

	(1) Maths score	(2) Maths score	(3) Maths score
Post EQ	0.031*	0.031*	0.018
	(0.017)	(0.016)	(0.016)
Eregion	0.124***	0.117***	0.131***
	(0.015)	(0.014)	(0.013)
postXEregion	-0.045**	-0.048***	-0.047***
	(0.020)	(0.019)	(0.018)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	290200	290200	290200
R^2	0.342	0.368	0.382

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.6: Results for Spanish, equation 3.1

	(1) Spanish score	(2) Spanish score	(3) Spanish score
Post EQ	0.169***	0.170***	0.160***
	(0.015)	(0.014)	(0.014)
Eregion	0.061***	0.055***	0.066***
	(0.013)	(0.012)	(0.012)
postXEregion	0.008	0.004	0.005
	(0.017)	(0.016)	(0.016)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	290200	290200	290200
R^2	0.289	0.305	0.315

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.7: Results for Mathematics, equation 3.2

	(1) Maths score	(2) Maths score	(3) Maths score
Post EQ	0.031*	0.030*	0.018
	(0.017)	(0.016)	(0.016)
Affected	0.170***	0.113***	0.132***
	(0.017)	(0.017)	(0.016)
School Damaged	0.102***	0.118***	0.131***
	(0.015)	(0.015)	(0.014)
postXAffected	-0.036	-0.037*	-0.038*
	(0.024)	(0.023)	(0.022)
postXdamaged	-0.051**	-0.054***	-0.052***
	(0.021)	(0.020)	(0.019)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	290200	290200	290200
R^2	0.343	0.368	0.382

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.8: Results for Spanish, equation 3.2

	(1) Spanish score	(2) Spanish score	(3) Spanish score
Post EQ	0.169***	0.170***	0.159***
	(0.015)	(0.014)	(0.014)
Affected	0.104***	0.053***	0.067***
	(0.015)	(0.015)	(0.014)
School Damaged	0.039***	0.055***	0.065***
	(0.014)	(0.013)	(0.013)
postXAffected	0.015	0.012	0.012
	(0.020)	(0.019)	(0.019)
postXdamaged	0.004	0.001	0.002
	(0.018)	(0.017)	(0.016)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	290200	290200	290200
R^2	0.290	0.305	0.315

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.A.3 Intermediate results for 2008-2009 placebo regressions

Table 3.9: Mathematics, placebo 2008-2009, equation 3.1

	(1) Maths score	(2) Maths score	(3) Maths score
Post EQ	0.095***	0.092***	0.088***
	(0.017)	(0.016)	(0.016)
Eregion	0.102***	0.093***	0.109***
	(0.014)	(0.013)	(0.013)
postXEregion	0.021	0.026	0.026
	(0.020)	(0.018)	(0.018)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	293646	293646	293646
R^2	0.354	0.380	0.396

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.10: Spanish, placebo 2008-2009, equation 3.1

	(1) Spanish score	(2) Spanish score	(3) Spanish score
Post EQ	0.020	0.015	0.012
	(0.015)	(0.014)	(0.014)
Eregion	0.054***	0.045***	0.058***
	(0.012)	(0.011)	(0.011)
postXEregion	0.011	0.018	0.017
	(0.017)	(0.016)	(0.016)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	293646	293646	293646
R^2	0.307	0.323	0.333

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.11: Mathematics, placebo 2008-09, equation 3.2

	(1) Maths score	(2) Maths score	(3) Maths score
Post EQ	0.095***	0.092***	0.088***
	(0.017)	(0.016)	(0.016)
Affected	0.146***	0.083***	0.103***
	(0.017)	(0.016)	(0.016)
School Damaged	0.082***	0.097***	0.112***
	(0.014)	(0.014)	(0.013)
postXAffected	0.023	0.027	0.027
	(0.024)	(0.023)	(0.022)
postXdamaged	0.019	0.026	0.025
	(0.021)	(0.019)	(0.019)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	293646	293646	293646
R^2	0.355	0.380	0.396

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.12: Spanish, placebo 2008-2009, equation 3.2

	(1) Spanish score	(2) Spanish score	(3) Spanish score
Post EQ	0.020	0.014	0.011
	(0.015)	(0.014)	(0.014)
Affected	0.089***	0.033**	0.048***
	(0.014)	(0.014)	(0.014)
School Damaged	0.037***	0.050***	0.062***
	(0.012)	(0.012)	(0.012)
postXAffected	0.017	0.024	0.024
	(0.021)	(0.020)	(0.019)
postXdamaged	0.008	0.015	0.014
	(0.018)	(0.017)	(0.017)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	293646	293646	293646
R^2	0.307	0.323	0.333

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.13: Mathematics, placebo 2008-2009, equation 3.3

	(1) Maths score	(2) Maths score	(3) Maths score
Post EQ	0.095***	0.092***	0.088***
	(0.017)	(0.016)	(0.016)
Affected	0.145***	0.082***	0.102***
	(0.017)	(0.016)	(0.016)
Minor Damage	0.080***	0.093***	0.109***
	(0.016)	(0.015)	(0.014)
Moderate Damage	0.073***	0.091***	0.108***
	(0.022)	(0.021)	(0.020)
Severe Damage	0.095***	0.118***	0.126***
	(0.022)	(0.021)	(0.019)
postXAffected	0.023	0.027	0.027
	(0.024)	(0.023)	(0.022)
postXMinorD	0.021	0.027	0.026
	(0.023)	(0.021)	(0.020)
postXModerateD	0.021	0.036	0.032
	(0.032)	(0.030)	(0.029)
postXSevereD	0.010	0.016	0.017
	(0.032)	(0.030)	(0.028)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	293646	293646	293646
R^2	0.355	0.380	0.396

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.14: Spanish, placebo 2008-2009, equation 3.3

	(1) Spanish score	(2) Spanish score	(3) Spanish score
Post EQ	0.020	0.013	0.011
	(0.015)	(0.014)	(0.014)
Affected	0.091***	0.035**	0.050***
	(0.014)	(0.014)	(0.014)
Minor Damage	0.038***	0.048***	0.061***
	(0.014)	(0.013)	(0.012)
Moderate Damage	0.028	0.045**	0.058***
	(0.019)	(0.018)	(0.018)
Severe Damage	0.047**	0.071***	0.076***
	(0.020)	(0.019)	(0.018)
postXAffected	0.017	0.024	0.024
	(0.021)	(0.020)	(0.019)
postXMinorD	0.008	0.015	0.014
	(0.020)	(0.019)	(0.018)
postXModerateD	0.026	0.039	0.037
	(0.027)	(0.026)	(0.025)
postXSevereD	-0.015	-0.008	-0.008
	(0.029)	(0.027)	(0.025)
Individual controls	Yes	Yes	Yes
School controls	No	Yes	Yes
Family controls	No	No	Yes
Observations	293646	293646	293646
R^2	0.307	0.323	0.333

Standard errors in parentheses clustered at the class level. Column 1 includes gender and individual measures of previous academic performance measured by the teacher. Column 2 adds class size, class size squared a dummy for rural school and for private/subsidised, with public schools as the reference group. Column 3 adds parents' education, books at home and household income.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 4

Conclusions

This thesis adds new causal evidence about two determinants of student performance that so far have been plagued with mixed evidence: the importance of peers in the classroom and the relevance of school year length. I exploit the variation generated by the earthquake that hit Chile in 2010, using a quasi-experimental design in chapters two and three. The second chapter puts the focus of effects on students who did not change school between 2009 and 2010, whereas in chapter three I consider all students.

Chapter 2 of this thesis studies the importance of average peer ability measured by academic achievement, on student's own educational outcomes. With the instrumental variables approach that I use, and relying on within-school variation, I try to deal with the main identification issues presented by Manski (1993), selection and the reflection problem. My results show that peers are more relevant in secondary education than in primary education, and that peers have a stronger effect in mathematics compared to Spanish. In addition, I show that there are some nonlinearities in the peer effects, but the structure of these nonlinearities does not allow me to reallocate students within a school in a pareto-improving way. Nevertheless, using simulations, I show that it may be possible to reduce educational inequality by grouping low ability students with high ability students, using a similar argument as Carrell et al. (2013). Furthermore, this grouping has the potential to marginally increase the average performance of the school. This is due to some nonlinear effects in the importance of peers, which are stronger in mathematics. However, for this policy to work, if it was ever to be implemented, teachers would need to play an important role to prevent low ability students to form subgroups with other low ability students, which would see the mean ability of the relevant peer group to decrease for those students. On the other hand, it may be possible to increase the performance of students at the top of the distribution of scores if that is the goal of the school, but at the cost of increasing educational inequality and marginally decreasing the average results of the school.

The third chapter studies the effect of the earthquake through school closures on academic achievement. I use a difference-in-differences strategy comparing two adjacent cohorts of students in primary education. One cohort was observed the year before the earthquake and the other one after the earthquake. The results show that students attending schools that were severely damaged by the school see a reduction in their score of 0.081 standard deviations after the earthquake. Approximately one half of the effect could be attributed to school closures of up to eight weeks at the start of the academic year, or 5% of cumulative time since the start of primary education. This is particularly interesting because Chile is the OECD country with the largest amount of compulsory instruction time per year. On the other hand, in Spanish I do not observe any effect: both groups, students affected and not affected by the earthquake continue their trends regardless of the earthquake. These results suggest that it could be possible to shift some hours of instruction time from Spanish to mathematics improving overall learning. The methodological contribution of the third chapter is to use a difference-in-differences strategy with multiple treatments, showing consistency between the different treatment groups and between the treatments and the control group.

The final conclusion of my thesis, based on the factors that I studied, is that inputs of educational achievement in isolation have a limited impact. Therefore, policies to improve academic achievement should not be designed with only one aspect in mind. Moreover, the mixed evidence across countries shows that the context is also relevant, and a policy that works in the UK may not work in Chile, or viceversa.

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