Equal Opportunities in Access to Higher Education: Evidence from the Randomization of a National Policy

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Introduction

The goal of the analysis described here is to estimate the effects of the PACE programme on disadvantaged Chilean high-school students. The PACE programme is a percent plan policy whose main feature is to grant access to university to students in targeted disadvantaged schools whose grades put them in the top 15 percent of their school, regardless of their results on the national university admission test (PSU). In this respect, it can be thought of as an SES-based affirmative action policy for access to higher education.

To evaluate the PACE policy, we will use data that we are collecting within the policy's nationally randomised expansion, which created a group of treated and a group of control schools. This study's main objective is to examine the policy effects on students' pre-university study effort and achievement, and social capital measures. Furthermore, the study analyses potential channels using the comprehensive student, teacher, and head teacher (*Jefe de Unidad Tecnico-Pedagogica*) questionnaires that we developed, as well as matched administrative data collected by the Ministry of Education. Specifically, we evaluate impacts on students' beliefs, on teachers' effort and focus of instruction, on student mobility across schools, on parental involvement, and on school-level policy response. Moreover, our analysis explores effect heterogeneity by student, teacher, classroom and school characteristics (such as peer composition).

This analysis plan describes the analysis before data on the outcomes of treated and control students is available, and it performs balancing tests based on pre-intervention characteristics. The goal is to reduce issues of data mining once the data are available. This plan was constructed after viewing the findings from a data collection performed by the Chilean Ministry of Education (MinEduc) in March/April 2017, which contains a few survey items that are related to our data collection. The primary new outcome measures that will be available in October 2017 are measures of student achievement, developed by two professional agencies (*Puntaje Nacional* and *AptusChile*), and a number of additional measures of student effort. Moreover, the October 2017 data will include outcome measures used to explore the impact channels: various teacher behaviours (effort, focus of instruction, curriculum coverage), school organisation (grading discretion, tuition, student allocation to classrooms), parental involvement in child education, and student beliefs about peer behaviour and outcomes and about returns to effort.

The PACE programme and the PACE randomized experiment

Intervention

University enrolment in Chile is largely determined by parental income: while 82 percent of students from households in the highest income quintile enrol in university, only 20 percent from the lowest income quantile do so (data: CASEN 2009). To increase the chances to enter university education of children from disadvantaged families, in 2014, the Ministry of Education of Chile (MinEduc) introduced PACE*, Programa de Acompañamiento y Acceso Efectivo*. PACE grants university admission to students from disadvantaged schools whose marks place them in the top 15% of their school. [5] Moreover, starting in the 2016 school year, MinEduc implemented a national fellowship programme known as *gratuidad*, which waives university tuition fees for students belonging to households in the bottom 50% of the income distribution. Administrative data indicate that since its inception in 2014, roughly 90% of students admitted to university through PACE have qualified for a *gratuidad* fellowship. Finally, PACE provides additional college preparation classes to students and teachers in the last two years of high-school (*tercero medio* and *cuarto medio*). These include orientation and soft skills classes, information interventions on the application process for university, and discussions with teachers regarding university preparedness.

Randomization

The expansion phase of PACE relevant for this study started in 2016, when MinEduc identified 221 schools in the entire country considered eligible to enter PACE based on the SES of the students. Among these, 64

[5] There is a guaranteed access if they score greater than 705 points in Puntaje Ranking de Notas.

schools were chosen at random to enter the programme.[6] Specifically, in these schools only the cohort of *tercero medio* (11th grade) students in 2016 entered the PACE programme. This cohort, in these 64 schools, constitutes our Treatment Group. Of the remaining 153 eligible schools, 64 were selected at random to serve as control schools (both in the April 2017 MinEduc data collection and in our October 2017 data collection). The randomisation was not stratified. Overall, roughly 9,000 students are part of the experiment. These students are in *cuarto medio* (12th grade) in 2017. We refer to the MinEduc April 2017 data collection as wave I, and to our data collection planned for August-October 2017 as wave II.

Of the 64 treated schools, one school refused to participate in wave I. Of the original 64 control schools, 4 refused to participate in wave I, and one did not have any students in *cuarto medio* in 2017. These 4 control schools were replaced by 4 schools randomly chosen from the remaining 93 eligible non-PACE schools. The resulting data collection carried out by MinEduc successfully collected information in 63 treated schools, and 64 control schools. Our wave II data collection, which started August 21st 2017, aims to collect data from all 64 PACE schools in the treatment group, and from the same 64 control schools in wave I. Finally, we will use administrative data from 2015 as baseline. We will be able to link the baseline, wave I and wave II follow-ups longitudinally by student, creating a student level panel dataset.

To the best of our knowledge, this is the only randomisation in the world of a national percent plan policy for access to higher education. The PACE policy shares some features with the Texas Top 10 policy in the United States which, however, has not been randomized.

Methods and data

MinEduc data collection (wave I)

In April 2017, MinEduc administered a student, teacher, head teacher and school principal questionnaire to 63 treated schools[7] and 64 control schools. The student questionnaire took approximately 30 minutes to complete, while the other questionnaires took approximately 10 minutes. The goal of this data collection was to obtain expectations about future educational attainment and about life in general. We added some items to the student questionnaire.[8] Specifically, we added one item on study effort, two items on returns to study effort, two items on the outcomes of other students in the school, four items on expected labor market outcomes at age 30 with or without a university degree, two items on the perceived probability of enrolling in higher education, and nine items on attitudes/personality traits.[9] To elicit expectations in a probabilistic way, we illustrated probabilities with an example and visualised them with a ruler, as in Attanasio and Kaufmann (2012).

Our data collection (wave II)

Our data collection started on August 21st 2017 and is planned to last through to mid-October 2017. We administer a student achievement test in Mathematics, with questions developed by the testing department of *AptusChile*, a consulting company specialising in vulnerable schools in Chile, and *PuntajeNacional*, a consulting company specialised in developing standardised tests. We also administer questionnaires for students, their Mathematics and Spanish teachers, and for the *Jefe de Unidad Técnico Pedagógica*, a head teacher who manages and supervises all Technical-Pedagogical aspects in the school. We aim to survey all 128 schools in the experimental randomization.[10]

The student questionnaire collects various measures for the following outcome domains: study effort, social capital, parental involvement in student education, beliefs over own future educational attainment and over

[6] The rationing of the programme to 64 out of 221 eligible schools was due to Government budget limitations. The randomisation was performed by PNUD - Chile (United Nations Development Programme). The decision to randomise was taken jointly by various teams within the Ministry, including the research group (*Centro de Estudios*), and with inputs from some members of our team (Orazio Attanasio, Ranjita Rajan, and Michela Tincani).

[7] One school refused participation in wave I but we plan to include this school in the wave II data collection.

[8] We thank Stephanie Finnel, Laia Mayol, Andrea Puig and Ximena Saez for help translating the questions into Spanish.

[9] We thank Teodora Boneva for invaluable help in developing some of these items.

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returns to effort, beliefs about outcomes and effort of others in the school, perceptions about teaching.[11] It takes around 20-25 minutes to complete. The achievement test is administered before the survey and it lasts 15 minutes.

Administrative data

We will be able to merge waves I and II to administrative data which constitute our baseline. In particular, we will merge with the SIMCE *segundo medio* dataset, which contains standardised SIMCE test scores for our cohort of students when they were in 10th grade in 2015. This dataset contains also information on the household such as parental education and various measures of SES.[12] This dataset will constitute our baseline data, because it was collected in November 2015, that is, four months before the start of the programme, in March 2016.

Additionally, we are able to merge with administrative data on school enrolment in every year prior to and during the experiment. This allows us to trace student location over time, which allows us to verify whether movement of students across schools is one margin of policy response. A preliminary analysis of movement across schools is available in this analysis plan. Another advantage of administrative enrolment data is that it allows us to identify students classified as vulnerable by MinEduc (*prioritarios*), giving us an additional student baseline characteristic. After the end of the school year (December 2017), we will also be able to merge to administrative data containing students' GPA, which will allow us to explore whether the mapping between a student outcome on the standardised achievement test that we administer and his/her grade is affected by the programme.

Survey weights

We will check for baseline balance and selective attrition across Treatment and Control groups. Selective attrition will be addressed with propensity score weighting as well as Lee bounds (Lee 2009). To build the propensity score weights, we will estimate the probability of being in the 2017 PACE data as a function of baseline administrative variables.[13] In this pre-analysis plan, in Tables 1a and 1b below we present balance checks among treatment and control groups for the wave I data collection. The results indicate baseline balance across treatment and control groups.

[11] In treated schools, the student questionnaire includes also a few short questions to measure the understanding of the PACE programme.

[12] There are three caveats: first, two schools that will be in our sample were not in the SIMCE 2015 sample. These two schools are treated schools, and preliminary enrolment data indicate that their total enrolment in *cuarto medio* is below 100 students. However, to avoid having no information at all on baseline variables, the August student questionnaire measures a few time-fixed household characteristics. This will allow us to obtain a measure of baseline characteristics for the students in the schools that were not in 2015 SIMCE. To do so, we will adjust for bias in reporting using data on students for whom both administrative and reported characteristics are available. We will also present separate reports on the sample of students for whom baseline administrative data is available. Overall, only around 100 students are in the PACE schools that do not have baseline SIMCE information. The second caveat is that students who repeated one or more years will have taken the standardised SIMCE test in years prior to 2015. For these students, we will search for their *segundo medio* SIMCE data by searching through all the yearly SIMCE datasets. Students who repeated one year will be in the SIMCE *segundo medio* dataset in 2014, those who repeated two years in 2013, and so forth. Regressions will include controls that indicate whether a student has repeated one or more years. The last caveat is non-random missing data on household characteristics (collected in SIMCE 2015). Household variables where collected within the SIMCE data collection through a parental questionnaire. Some parents did not fill out the questionnaires, and sometimes even when they did, their child failed to bring it back to school for collection. Therefore, we will present results separately with and without household information. Moreover, when using household information we will present results adjusted and unadjusted for attrition bias. To do so, we will build weights to adjust for sample selection.

[13] Attrition will be defined in two ways: the likelihood that an observation is in wave II, and the likelihood that an observation is in wave I and wave II. Selective attrition according to both definitions will be studied and adjusted for accordingly.

Statistical analysis

We will group measures into domains that measure the same construct. For example, we have a number of variables which all measure student effort. In this case we consider student effort as a single domain. Within each domain, individual measures will be signed so as to have a common direction (for example, a more positive value of each measure indicates a larger value for the construct of interest). This allows us to test whether there is any effect of PACE not only on each single measure, but also on each domain, adjusting the standard errors for multiple hypothesis within domain. We will report the family-wise p-values within each domain.

Below, we describe our main outcomes (section 1), further outcomes that will inform us on the mechanisms of PACE (section 2), and heterogeneity of the treatment effects (section 3):

1. Main outcomes:
We will evaluate the overall impact of the intervention focusing on the domains of: student achievement, student effort, and personality measures.

- Achievement: we designed and extensively pre-tested a 9-item multiple choice achievement test of Mathematics.

- Effort: We measure self-reported student effort, considering both effort in class and effort outside of school.

- Social capital: We elicit social capital using the items on trust, altruism as well as positive and negative reciprocity by Falk et al. (2016).

2. Outcomes that help us understand impact channels:

2.1 Students:
Student beliefs about returns to effort and about the study effort and outcomes of other students in the school.

2.2 Teachers:
Teacher effort, teacher focus of instruction, teaching and evaluation methods (including how much discretion teachers use in grading).

2.3 Parents:
Parental involvement in their child education.

2.4 Head teachers:
Assignment of students to classrooms, how much arbitrariness there is in the marks that teachers give in the school, additional tutoring classes offered by the school.

2.5 Administrative school GPAs
Together with the teacher and head teacher questionnaires, studying the mapping between standardised achievement (as measured by the test we administer in wave II) and the marks that students obtain (linkable administrative data) will inform us on whether teachers adjusted their grading strategically to influence university admission.

3. Heterogeneity of treatment effects

We will further analyse heterogeneous treatment effects. We will consider heterogeneities by:

- **Student ability and household background**. Use SIMCE test scores in mathematics, Spanish and social and natural sciences, separately and averaged, to measure initial ability. We will explore if there is a different response to treatment depending on the student's location in the ability distribution, both nationally (to explore the importance of ability in absolute terms) and in the school (to explore the importance of ability relative to other students in the school). In particular, students closer to the 15% cutoff in the school may respond differently to PACE than students at the top or bottom of the school distribution. Separately, we will also explore heterogeneity by ability in absolute level because complementarities between PACE classes and ability may imply a different marginal product of effort for students of different ability level (in absolute terms). Finally, we will use both SIMCE data on household characteristics as well as data that we collect in wave II on (time-fixed) household characteristics to explore heterogeneity by SES.
- (Potential) variation in the school-specific implementation of the PACE activities.[14]
- **School characteristics** such as average class size, average teacher experience, average measured teacher ability (thought various governmental assessment programmes), percentage of vulnerable students in the school (publicly available data linkable through school identifiers).[15]
- **Personality traits, expectations and beliefs**. First, identify the personality, expectation and belief measures which were not affected by treatment. To establish this, verify if ATE are zero and if the distributions of the variable in the treatment and in the control group are statistically indistinguishable. Second, explore treatment heterogeneity by this variable.
- **School composition**, in particular, dispersion of baseline ability as measured by past SIMCE test scores, as well as average.

Preliminary analysis of school transitions (wave I data)

One margin of policy response could be the movement of students across schools, that is, some students may decide to attend a school that is in the PACE programme. Alternatively, students in schools that are selected to be in PACE may decide to move to a different PACE schools, for example, to a PACE school with a lower quality student body, in order to increase his/her chances of being admitted to university through the 15 percent cutoff rule.[16] If this margin of response was present in our data, we would have to take this into account when evaluating policy impacts.

We use data from the wave I data collection to examine whether there were any such movements across experimental schools. We find that, in the context of the experimental expansion of PACE that we are examining, movement of students across schools are not a margin of policy response. This is easy to explain: the inclusion into the PACE programme was not announced to treated schools until January 2016, less than two months before the start of the new school year. Therefore, January 2016 is the earliest that parents could have heard about the policy (many of them would have learned about it later). Moreover, to be eligible for the PACE cutoff rule, a student must be in a PACE school for all of the 11^{th} and 12^{th} grade, which means that any movements across schools that may have occurred after March 2016 cannot have happened in response to the PACE policy. As a result, parents had only at most 2 months to strategically try to enrol their child in a PACE school.

The relevant school transitions that must be analysed are those that occurred between the 2015 and 2016 school years. To verify whether students moved across schools as a response to the policy, we use

[14] Using a survey designed by the PACE team at MinEduc with inputs from Michela Tincani, we will examine the reasons behind any potential variation in PACE activities across schools.

[15] Heterogeneity will be explored only with respect to the variables that have not endogenously reacted to treatment. For example, if we find that schools decided to reshuffle teacher assignment as a reaction to the PACE policy, we will not examine heterogeneity by teacher characteristics.

[16] See Estevan et al. (2017) for evidence that this happened in Texas, as a response to the Texas top ten policy.

administrative enrolment data for the 2015 and 2016 school years. To be assigned as a treated student in the randomisation, a student must be in *tercero medio* (11th grade) in a school that is selected to be treated in 2016. Therefore, we examine the cohort of students who in 2015 were either in *segundo medio* (10th grade) or in *tercero medio* (11th grade), and who in 2016 are in *tercero medio* (11th grade).[17] Table 2 shows the transition matrix. Very few students move across treatment groups, and slightly more students move from schools who will enter treatment in 2016 to schools who do not enter treatment in 2016. This is evidence against strategic transitions across schools as a policy response.

Table 3 examines the ability (as measured by past SIMCE test scores) of students who move across treatment groups. As can be seen, ability is not related to the likelihood of moving across treatment groups. This evidence suggests lack of strategic movements across schools.

Together, Tables 2 and 3 suggest that moving from non-treated to treated schools does not appear to be a margin of reaction to the randomised policy expansion.

Finally, we examine transitions within PACE and non-PACE groups. First, we note that only 0.83 percent (37 out of 4,441) of students who are in non-PACE schools in 2015 move to another non-PACE school in 2016, and only 1.57 percent (73 out of 4,656) of students who are in schools in 2015 that will enter PACE in 2016 move to another school that will enter PACE in 2016. Therefore, there have not been much movement of students within treatment groups. Movements across schools indicate that, in general, students tend to move to better performing schools. This is true regardless of whether students are moving within PACE or within non-PACE schools (see Tables 4 and 5). In fact, students who in 2015 are in schools that enter PACE in 2016 move to, on average, better schools than their counterparts who move across non-PACE schools. If these students were moving to exploit the 15 percent plan, we would be observing the opposite, that is, we would observe students in PACE schools moving to lower-performing PACE schools in order to improve their chances of entering the top 15 percent in the school. This is precisely what Estevan et al. (2017) argue has happened in Texas.

This suggests that moving to lower performing PACE schools to increase the likelihood of obtaining a *cupo* PACE (i.e. university admission through PACE) does not appear to be a margin of reaction to the randomized policy experiment.

[17] We consider also those who were in 11th grade in 2015 because some of them have to repeat the year and therefore are in 11th grade also in 2016.

Tables

Table 1a: Characteristics of the study population

	Mean for controls	**Difference between treatment and controls**
	(1)	**(2)**
Female	0.471	-0.0120 (0.0490) [0.806]
Spanish standardised test score in 10th grade	221.03	5.6256 (4.7954) [0.243]
Mathematics standardised test score in 10th grade	219.21	6.6693 (6.0220) [0.270]
Social Sciences standardised test score in 10th grade	220.94	5.5123 (3.9656) [0.167]
Alumno prioritario (i.e., beneficiary of Ley SEP because identified as low SES by MinEduc)	0.655	0.0296 (0.0667) [0.658]

Notes: The first column reports the mean for the control students. The second column reports the difference between the average outcome for all treated individuals and the average outcome for all control individuals, as calculated by OLS. The dependent variable is given in the left hand column. All regressions cluster the standard errors at the school level. We report the coefficient on the treatment dummy, the standard error (in parenthesis), and the per comparison p-value [in square brackets]. These regressions are based on 2015 *segundo medio* enrollment data, that is, in these regressions students are considered treated if in 2015 they were in *segundo medio* in a school whose *tercero medio* students in 2016 entered the PACE experimental treated group. Students are considered control if in 2015 they were in *segundo medio* in a school whose *tercero medio* students in 2016 entered the PACE experimental control group. The baseline characteristics (left hand column) were collected in November 2015 within the SIMCE *segundo medio* 2015 evaluation. Finally, notice that 2 treated schools were not matched with baseline SIMCE data because they did not participate in the 2015 *segundo medio* SIMCE evaluation. For the students in these schools (fewer than 100 total), no administrative baseline data is available.

The sample consists of all students who took the SIMCE test in 2015 and who in 2015 were in 10th grade in schools that in 2016 entered the PACE experimental study. N=9,134.

Table 1b: Characteristics of the study population

	Mean for controls	**Difference between treatment and controls**
	(1)	**(2)**
Household size (including student)	3.545	-0.0737 (0.0767) [0.339]
Father's education in years	9.287	0.0842 (0.1701) [0.622]
Mather's education in years	9.532	-0.0616 (0.1642) [0.708]
Monthly household income in Chilean pesos	283,538	9,331 (12,301) [0.450]

Notes: The characteristics on the left hand column were collected through a parental survey. There are many missing observations (40%), due to the fact that some parents did not fill in the survey, and some students did not bring back to school the completed parental questionnaire. These missing observations are non-random, for example, students who do not bring a parental survey score on average 11.41 points lower on the Mathematics SIMCE test. However, attrition appears to be balanced across treatment and control. In a probit regression where the dependent variable is whether the parental survey is available and the independent variable is treatment, the marginal effect of treatment is estimated to be 0.0079647, and insignificant (p-value 0.353). In a probit regression with the same dependent variable, and with math score and math score interacted with treatment status as regressors, the interaction has an estimated insignificant marginal effect of 0 .0000515 (p-value 0.165).

Father's and mother's education make the following assumptions: completing the 4^{th} or 5^{th} year of "educacion media" in a vocational high-school corresponds to 12 years of education; dropping out of a vocational university corresponds to 13 years of education; completing vocational education correspond to 14.5 years of education (because most vocational universities offer courses of the duration of 2 or 3 years); dropping out of a non-vocational university corresponds to 14 years of education; a university degree corresponds to 16 years of education; a Master's degree corresponds to 17 years of education; a doctoral degree corresponds to 21 years of education.

The sample consists of all students who took the SIMCE test and brought back the parental questionnaire in 2015 and who in 2015 were in 10^{th} grade in schools that in 2016 entered the PACE experimental study. N=7,625.

Table 2: Movement across schools between 2015 and 2016 school years

	Treatment status 2016		Total
Treatment status 2015	**0**	**1**	
0	4,441 99.28	32 0.72	4,473 100.00
1	48 1.02	4,656 98.98	4,704 100.00
Total	4,489 48.92	4,688 51.08	9,177 100.00

Population of *segundo* and *tercero medio* students in 2015 who in 2016 are in *tercero medio*. Population of 127 wave I experimental schools. Treatment status 2015 is equal to 1 when in 2015, a student is in a school that enters the Treatment group in 2016, 0 otherwise. Treatment status 2016 is equal to 1 when I n2016, a student is in a Treatment group school, 0 otherwise.

Table 3: Movement across schools by student ability between the 2015 and 2016 school years

	(1) T 2016	(2) T 2016
SIMCE Spanish score	-0.001 (0.602)	-0.003 (0.181)
SIMCE Mathematics score	-0.005** (0.029)	0.003 (0.105)
SIMCE Social Science score	0.001 (0.664)	0.004 (0.153)
Observations	3,359	3,448

p-values in parentheses. *p<0.10, ** p<0.05, ***, p<0.01

Probit regressions, marginal effects reported. Test scores are standardised to have mean 0 and standard deviation 1. A number of students are dropped from the analysis because of measurement error on the student identifiers when matching administrative enrolment data with SIMCE test score data.

Column 1 conditions on being n 2015 in a school that does not enter the Treatment group in 2016. Column 2 conditions on being in 2015 in a school that enters the Treatment group in 2016. The dependent variable is a dummy equal to 1 if the student is in a Treatment group school in 2016. Population of *segundo* and *tercero medio* students in 2015 who in 2016 are in *tercero medio* (in the population of wave I experimental schools). The coefficient on the Mathematics score in column 1 is statistically significant, but extremely close to 0, therefore, we interpret it as a precisely estimated zero.

Table 4: Movement across PACE schools between the 2015 and 2016 school years

	(1) Avg Math	(2) Avg Spanish	(3) Avg Social Science
Change PACE school	-11.263*** (0.002)	-4.256 (0.118)	-6.689*** (0.007)
Constant	225.866*** (0.000)	225.982*** (0.000)	226.621*** (0.000)
Observations	4,579	4,579	4,579

p-values in parentheses.

The dependent variables are average scores in the school in 2015.

The independent variable is a dummy equal to 1 if the student moves to a different PACE school between 2015 and 2016. A negative coefficient indicates movement from lower-performing to better-performing schools. Population of *segundo* and *tercero medio* students in 2015 who are in schools that in 2016 enter the PACE group (from wave I), and who in 2016 are in *tercero medio* and still in experimental PACE schools.

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

Table 5: Movement across PACE schools between the 2015 and 2016 school years

	(1) Avg Math	(2) Avg Spanish	(3) Avg Social Science
Change non-PACE school	-5.129* (0.095)	-4.297 (0.159)	-1.248 (0.534)
Constant	218.913*** (0.000)	221.000*** (0.000)	220.519*** (0.000)
Observations	4,441	4,441	4,441

p-values in parentheses.

The dependent variables are average scores in the school in 2015.

The independent variable is a dummy equal to 1 if the student moves to a different non-PACE school between 2015 and 2016. A negative coefficient indicates movement from lower-performing to better-performing schools. Population of *segundo* and *tercero medio* students in 2015 who are in schools that in 2016 do not enter the PACE group (from wave I), and who in 2016 are in *tercero medio* and still in experimental non-PACE schools.

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

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