# Acing the Test: Educational Effects of the *SaberEs* Test Preparation Program in Colombia

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# Contents

# Introduction

- Dramatic expansion in post-primary education in low- and middle-income countries (Ferreyra, Avitabile, Paz, Botero, & Urzúa, 2017; World Bank, 2018).
- There are important gaps in achievement and quality of education as measured by test scores (Angrist & Lavy, 2009; Gneezy et al., 2019).
- Standardized tests affect the transition to higher education and labor market outcomes (Bond, Bulman, Li, & Smith, 2018; Brunello & Kiss, 2022).
- Scores determine eligibility for financial aid (Bernal & Penney, 2019; Bruce & Carruthers, 2014; Gurantz & Odle, 2020; Londoño-Vélez, Rodríguez, & Sánchez, 2020; Melguizo, Sanchez, & Velasco, 2016).

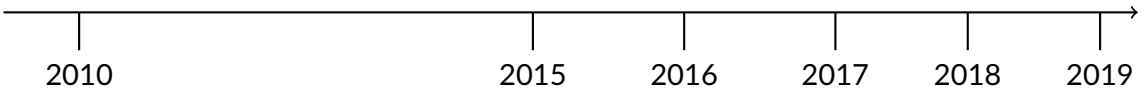
# SaberEs program

- Secretariat of Education of Medellin $\longrightarrow$ 2016.
- 2016-2019 Development Plan.
- Develop skills that strengthen preparation for standardized tests like Saber 11.
- Additional installed capacity and vocational guidance components.
- 2 companies hired:
  - Separate set of schools.
  - Grades 8 to 11.
  - Teacher training $\longrightarrow$ Trained schools' principals and coordinators $\longrightarrow$ Teachers train students during school hours $\longrightarrow$ Simulation tests $\longrightarrow$ Feedback sessions.

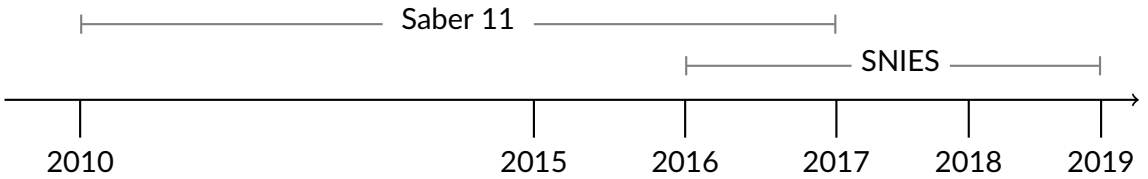
# Our paper

- Provide new evidence on the effectiveness of standardized test preparation programs.
- Especially important since most of this evidence suffers from self-selection bias.
- Three main questions:
  1. Does the *SaberEs* program affect student learning gains measured by *Saber 11* scores?
  2. Does the program affect access to tertiary education?
  3. What mechanisms made *SaberEs* successful in increasing access to tertiary education programs?

# Timeline

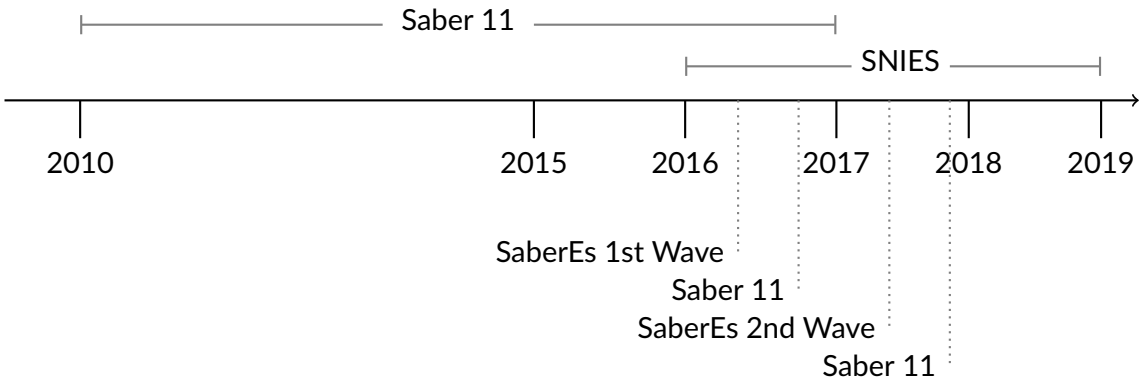


# Timeline




Other datasets: ICETEX (2018-2019), Sapiencia (2016-2019), Saber TyT (2016-2019), Ser Pilo Paga (2015-2016), Olimpiadas del conocimiento (2015-2016).

# Timeline


Saber 11
SNIES
2010
2015
2016
2017
2018
2019
SaberEs 1st Wave
Saber 11
SaberEs 2nd Wave
Saber 11

# Contents

# Data

- Administrative data from eight main sources.

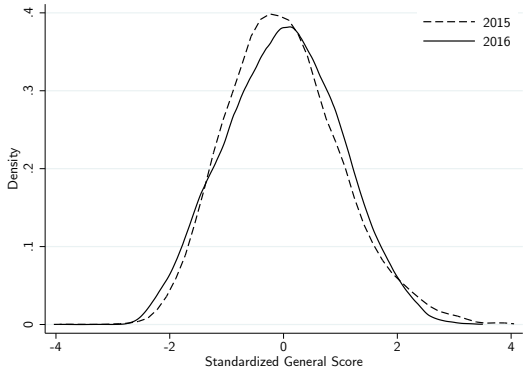
- Saber 11 had a structural change in 2014 $\longrightarrow$ Student's rank as variable of interest (0-100) to ensure comparability, following Laajaj, Moya, and Sánchez (2022).

$$Rank_i = \frac{x_i - min(x)}{max(x) - min(x)} * 100$$

- 2x2 (2015-2016) and dynamic difference-in-differences specifications (2010-2017).

# Data

- Distribution of the standardized scores in 2015-2016.
- Right shift concentrated along the median students.



# Summary Statistics

| | Mean | SD | Min | Max |
|---|---|---|---|---|
| *Panel A: Test Scores* | | | | |
| General | 258.69 | 42.14 | 13 | 450 |
| Reading | 52.85 | 8.93 | 0 | 100 |
| Math | 51.13 | 10.52 | 0 | 100 |
| Science | 51.48 | 9.11 | 0 | 100 |
| Social Studies | 51.52 | 10.06 | 0 | 93 |
| English | 51.66 | 10.37 | 0 | 100 |
| *Panel B: Higher Education and Financial Aid* | | | | |
| Access to higher education | 0.60 | 0.49 | 0 | 1 |
| Access to short-cycle | 0.31 | 0.46 | 0 | 1 |
| Access to university | 0.33 | 0.47 | 0 | 1 |
| Access to STEM | 0.26 | 0.44 | 0 | 1 |
| Access to professional STEM | 0.16 | 0.37 | 0 | 1 |
| Access to short-cycle STEM | 0.13 | 0.34 | 0 | 1 |
| Received financial aid | 0.05 | 0.22 | 0 | 1 |
| Received *Ser Pilo Paga* | 0.03 | 0.16 | 0 | 1 |
| *Panel C: Treatment* | | | | |
| Treated | 0.66 | 0.47 | 0 | 1 |
| Treated Tres Editores | 0.46 | 0.50 | 0 | 1 |
| Treated Avancemos | 0.20 | 0.40 | 0 | 1 |

| | Mean | SD | Min | Max |
|---|---|---|---|---|
| *Panel D: Covariates* | | | | |
| Female | 0.57 | 0.50 | 0 | 1 |
| TV | 0.80 | 0.40 | 0 | 1 |
| Oven | 0.60 | 0.49 | 0 | 1 |
| Landline | 0.85 | 0.36 | 0 | 1 |
| Microwave | 0.50 | 0.50 | 0 | 1 |
| PC | 0.78 | 0.42 | 0 | 1 |
| Car | 0.16 | 0.37 | 0 | 1 |
| Internet | 0.77 | 0.42 | 0 | 1 |
| Washing machine | 0.81 | 0.39 | 0 | 1 |
| DVD | 0.61 | 0.49 | 0 | 1 |
| NSE 1 | 0.03 | 0.18 | 0 | 1 |
| NSE 2 | 0.33 | 0.47 | 0 | 1 |
| NSE 3 | 0.62 | 0.48 | 0 | 1 |
| NSE 4 | 0.02 | 0.13 | 0 | 1 |
| Employed | 0.06 | 0.23 | 0 | 1 |
| Parent's education | 0.10 | 0.30 | 0 | 1 |
| High income | 0.07 | 0.26 | 0 | 1 |
| High stratum | 0.04 | 0.20 | 0 | 1 |
| Household floor | 0.42 | 0.49 | 0 | 1 |
| > 6 People in household | 0.20 | 0.40 | 0 | 1 |
| > 3 Rooms in household | 0.61 | 0.49 | 0 | 1 |

# Contents

# Empirical Strategy - 2x2

We estimate a simple difference-in-differences regression as:

$$Y_{ict} = \alpha + \beta_0 \text{Treated}_c + \beta_1 \text{Post}_t + \beta_2 \text{Treated*Post}_{ct} + X'_{ict}\delta + \varepsilon_{ict}$$

where:

- $Y_{ict}$ is the general rank of student *i* from school *c* in period *t*.
- $Treated_c$ is a dummy variable indicating whether school *c* is treated.
- $Post_t$ takes a value of 1 if the student's test application year is 2016.
- $Treated * Post_{ct}$ is their interaction.
- $X'_{ict}$ is a vector of controls.
- $\varepsilon_{ict}$ is the error term.

# Empirical Strategy - 2x2

We also estimate a two-way fixed effects regression as:

$$Y_{ict} = \alpha + \theta_1 \text{Treated}^* \text{Post}_{ct} + \psi_c + \gamma_t + \mu_{ict}$$

where $\psi_c$ and $\gamma_t$ are the school and year fixed effects, respectively. $\mu_{ict}$ is the error term.

# Empirical Strategy - 2x2

We use alternatives that better handle the inclusion of covariates:

- Outcome regression (Heckman, Ichimura, & Todd, 1997).
- Hájek (1971) type inverse probability weighting (IPW) with normalized weights.
- Sant'Anna and Zhao (2020) doubly robust improved difference-in-differences estimator for repeated cross sections.
- RIF regressions (Firpo, Fortin, & Lemieux, 2009) $\longrightarrow$ Effects on the unconditional quantiles.

# Empirical Strategy - Dynamic

- Observations from 2010 to 2016 (2017) $\longrightarrow$ Multiple time periods and one (two) year (years) of treatment.

- When treatment is staggered TWFE would potentially be biased due to the presence of heterogeneous effects (Borusyak & Jaravel, 2017; De Chaisemartin & d'Haultfoeuille, 2020).

- Estimator is a weighted average of all 2x2 comparisons and includes "forbidden comparisons" (Goodman-Bacon, 2021).

# Empirical Strategy - Dynamic (non-staggered)

We estimate an event studies regression as:

$$Y_{ict} = \alpha_0 + \sum_{h=2010}^{2016} \beta_h [t * \text{Treated}_{ch} = h] + \psi_c + \gamma_t + u_{ict} \quad \forall \quad h \neq 2015$$

where $Y_{ict}$ is the outcome of student *i* from school *c* at time *t*, and $t * Treated_{ct}$ are the interactions of year and treatment status for each of the leads or lags (h). $\psi_c$ and $\gamma_t$ are the school and year fixed effects respectively, and $u_{ict}$ is the error term.

# Empirical Strategy - Dynamic (staggered)

- We use the Callaway and Sant'Anna (2021) estimator.
  - Simple aggregation.
  - Event study aggregation.

- Alternative specification proposed by Borusyak, Jaravel, and Spiess (2021).
  - Stronger assumption about parallel trends could lead to a larger bias (Roth, Sant'Anna, Bilinski, & Poe, 2022).

- Calculate possible bias from pre-testing (Roth, Forthcoming) and conduct a sensitivity analysis (Rambachan & Roth, 2022).

# Contents

# Results - 2x2

- Statistically significant and positive effect on the average student's rank.
- Holds to more robust specifications like the doubly robust one.
- Using the standardized test scores further proves the robustness of the results.

▸ See Results

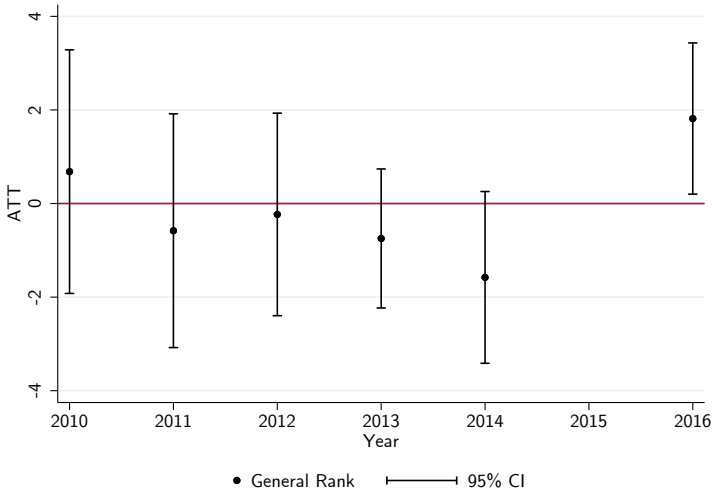
| | (1) DiD | (2) DiD | (3) TWFE | (4) OR | (5) IPW | (6) DR |
|---|---|---|---|---|---|---|
| *SaberEs* effect ($\beta$) | 2.965*** | 2.559*** | 1.511** | 2.222** | 2.693*** | 2.233** |
| | (0.976) | (0.886) | (0.741) | (0.917) | (1.032) | (0.916) |
| Observations | 35,495 | 35,484 | 35,484 | 35,484 | 35,484 | 35,484 |
| Controls | NO | YES | NO | YES | YES | YES |

# Results - 2x2

- Statistically significant and positive effect on the average student's rank.
- Holds to more robust specifications like the doubly robust one.
- Using the standardized test scores further proves the robustness of the results.
  ▸ See Results
- 22.9% reduction in the rank's gap between treated and untreated students.
- Mainly driven by math improvements
  ▸ See Results

| | (1) DiD | (2) DiD | (3) TWFE | (4) OR | (5) IPW | (6) DR |
|---|---|---|---|---|---|---|
| *SaberEs* effect ($\beta$) | 2.965*** | 2.559*** | 1.511** | 2.222** | 2.693*** | 2.233** |
| | (0.976) | (0.886) | (0.741) | (0.917) | (1.032) | (0.916) |
| Gap reduction | 30.6% | 26.4% | 15.6% | 22.9% | 27.8% | 22.9% |
| Observations | 35,495 | 35,484 | 35,484 | 35,484 | 35,484 | 35,484 |
| Controls | NO | YES | NO | YES | YES | YES |

# Results - Dynamic (non-staggered)



# Results - Dynamic (staggered)

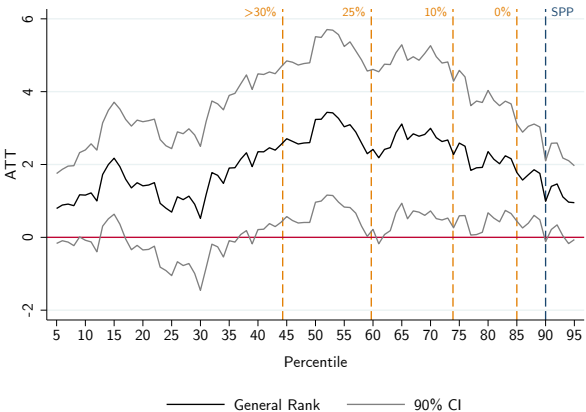
- Extending the sample from 2010 to 2017 yields better results.
- Effects are robust to both specifications.
- The program reduced the gap by around 30-40%.
- Results are robust when using the standardized test scores. ▸ See Results
- The event study aggregations with balanced groups show a similar pattern. ▸ See Results
- Power and sensitivity analyses on the pre-trends further show the robustness of the results.
  ▸ Power analysis ▸ Sensitivity analysis

| | (1) C&S | (2) BJS | (3) C&S | (4) BJS |
|---|---|---|---|---|
| *SaberEs* effect ($\beta$) | 3.715*** | 2.711*** | 3.598*** | 2.688*** |
| | (0.785) | (0.497) | (0.820) | (0.462) |
| Gap reduction | 38.3% | 28.0% | 37.1% | 27.7% |
| Observations | 147,656 | 147,554 | 147,656 | 70,859 |
| Controls | NO | NO | YES | YES |

*Notes:* Standard errors clustered at the school level. C&S relates to the "simple" aggregation from Callaway and Sant'Anna (2021). BJS relates to the estimator proposed by Borusyak et al. (2021). Controls include gender, household goods and services (computer, car, internet and washing machine), parents education, and stratum. *p<.05; **p<.01; ***p<.001

# Results - Heterogeneous effects on the outcome distribution

- Effects are evidenced above the 40th percentile of the students' rank distribution.
- They are similar when looking at the effects on the distribution of standardized test scores. ▸ See Figure
- There are positive effects on students above the SPP cut-off.



# Results - Higher education

| | Higher Education | | | Short-cycle | | | Professional | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) 1 year | (2) 2 years | (3) 3 years | (4) 1 year | (5) 2 years | (6) 3 years | (7) 1 year | (8) 2 years | (9) 3 years |
| *SaberEs* effect ($\beta$) | 0.033** | 0.039*** | 0.024** | 0.026** | 0.023** | 0.010 | 0.006 | 0.015 | 0.014 |
| | (0.015) | (0.015) | (0.012) | (0.013) | (0.011) | (0.011) | (0.011) | (0.012) | (0.010) |
| Observations | 35,484 | 35,484 | 35,484 | 35,484 | 35,484 | 35,484 | 35,484 | 35,484 | 35,484 |
| Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Mean Control 2015 | 0.511 | 0.559 | 0.587 | 0.219 | 0.230 | 0.224 | 0.292 | 0.329 | 0.361 |

*Notes:* Standard errors clustered at the school level. Results come from a doubly robust estimation as in Sant'Anna and Zhao (2020). The columns indicate access to each outcome 1, 2 and 3 years after students graduate from high school. *p<.05; **p<.01; ***p<.001

# Results - Higher Education

| | (1) STEM | (2) Professional STEM | (3) Short-cycle STEM |
|---|---|---|---|
| *SaberEs* effect ($\beta$) | 0.021* | 0.011 | 0.019** |
| | (0.011) | (0.009) | (0.008) |
| Observations | 35,484 | 35,484 | 35,484 |
| Controls | YES | YES | YES |
| Mean Control 2015 | 0.287 | 0.173 | 0.152 |

*Notes:* Standard errors clustered at the school level. Results come from a doubly robust estimation as in Sant'Anna and Zhao (2020). *p<.05; **p<.01; ***p<.001

# Results - Graduation from short-cycle education

Graduation from short-cycle education increases for all programs, and even for STEM programs.

| | (1)<br>All short-cycle programs | (2)<br>Short-cycle STEM programs |
|---|---|---|
| *SaberEs* effect ($\beta$) | 0.023** | 0.010* |
| | (0.010) | (0.006) |
| Observations | 35,484 | 35,484 |
| Controls | YES | YES |
| Mean Control 2015 | 0.152 | 0.062 |

*Notes:* Standard errors clustered at the school level. Results come from a doubly robust estimation as in Sant'Anna and Zhao (2020). *p<.05; **p<.01; ***p<.001

# Potential Mechanisms

For the majority of the population:

- Accumulation of specific human capital $\longrightarrow$ Access to universities that require admission exams (UdeA, Nacional & SENA). ✓ See results
- Motivational effect (effects on *Olimpiadas del conocimiento*). × See results
- Access to financial aid (ICETEX & Sapiencia). × See results

For elite students:

- Access to Ser Pilo Paga. ✓ See results

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# Conclusion

- One of the few papers to analyze these types of policies for socioeconomically disadvantaged students in Latin America aside from Gómez, Bernal, and Herrera (2020).

- We take advantage of granular administrative data to identify the causal effect of SaberEs on students' academic performance.

- We use state of the art econometric methods in a difference-in-differences estimation.

# Conclusion

- One of the few papers to analyze these types of policies for socioeconomically disadvantaged students in Latin America aside from Gómez et al. (2020).
- We take advantage of granular administrative data to identify the causal effect of SaberEs on students' academic performance.
- We use state of the art econometric methods in a difference-in-differences estimation.
- We find a positive effect of over 2 points on the average student's rank in the test $\longrightarrow$ 22.9% reduction in the pre-existing gap.
- In terms of higher education, we find a positive effect on access to short-cycle and STEM programs. Also, the program positively affected graduation from short-cycle programs.
- A limitation of our paper is the absence of a cost-benefit analysis. However, we expect the net present value of benefits to be large and positive.

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# Contents

# Saber 11

- High school exit exam administered by ICFES.
- Compulsory test with compliance rates above 90% (Bernal & Penney, 2019).
- 500,000 students take it every year (March-August).
- Structural change in 2014 $\longrightarrow$ 5 subject areas with scores between 0-100.
- ICFES offered a familiarization test that costed $30 USD (Bernal & Penney, 2019).
- Private companies offer courses (mostly used by private schools).

# Higher education

- Public and private institutions with admission processes each semester.
- Saber 11 plays a central role in the admission processes (Londoño-Vélez et al., 2020).
- Higher education costs in Colombia are relatively high (Ferreyra, 2021).
- Biggest public universities have highly competitive admission processes $\longrightarrow$ Highest-achieving students enroll.
- Enrollment in private institutions for low-income students is mainly driven by funding.

# Results - 2x2

Table: Main results: Standardized score.

| | (1) DiD | (2) DiD | (3) TWFE | (4) OR | (5) IPW | (6) DR |
|---|---|---|---|---|---|---|
| *SaberEs* effect ($\beta$) | 0.104*** | 0.089*** | 0.053** | 0.073** | 0.092** | 0.074** |
| | (0.034) | (0.030) | (0.026) | (0.032) | (0.036) | (0.032) |
| Gap reduction | 31.1% | 26.6% | 15.8% | 22.0% | 27.4% | 22.3% |
| Observations | 35,495 | 35,484 | 35,484 | 35,484 | 35,484 | 35,484 |
| Controls | NO | YES | YES | YES | YES | YES |

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# Results - 2x2 specific

Figure: Rank.

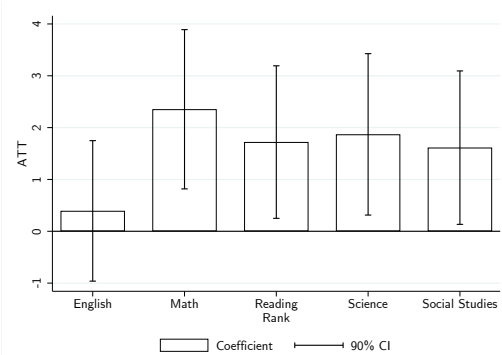
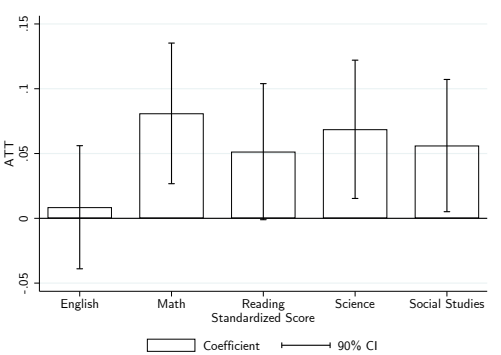



Figure: Standardized scores.

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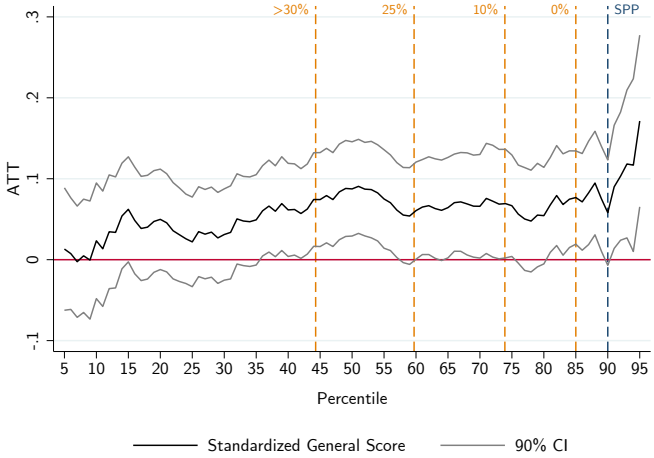
# Results - 2x2

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# Results - Dynamic

Table: Dynamic Results: Standardized Score.

| | (1) C&S | (2) BJS | (3) C&S | (4) BJS |
|---|---|---|---|---|
| *SaberEs* effect ($\beta$) | 0.131*** | 0.099*** | 0.123*** | 0.094*** |
| | (0.028) | (0.016) | (0.029) | (0.015) |
| Gap reduction | 39.2% | 29.6% | 36.8% | 28.1% |
| Observations | 147,656 | 147,554 | 147,656 | 70,859 |
| Controls | NO | NO | YES | YES |

*Notes:* Standard errors clustered at the school level. C&S relates to the "simple" aggregation from Callaway and Sant'Anna (2021). BJS relates to the estimator proposed by Borusyak et al. (2021). Controls include gender, household goods and services (computer, car, internet and washing machine), parents education, and stratum. *p<.05; **p<.01; ***p<.001

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# Results - Dynamic

Figure: Average Effect on Student's Rank by Length of Exposure.

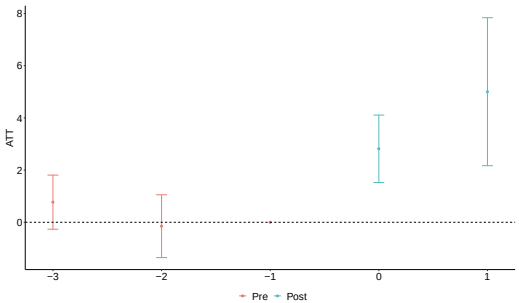
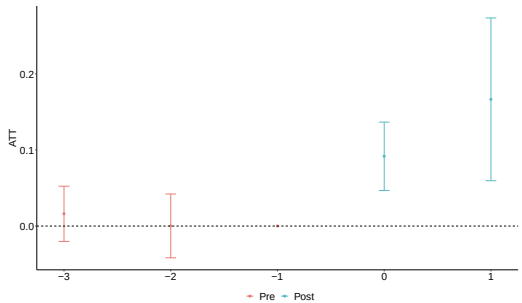



Figure: Average Effect on Student's Standardized Scores by Length of Exposure.

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# Power Analysis

Table: Power analysis: bias from hypothesized trend

| | (1) Estimate | (2) Slope | (3) Likelihood ratio |
|---|---|---|---|
| General Rank | 3.715 | 0.462 | 0.009 |
| Standardized General Score | 0.131 | 0.016 | 0.009 |

*Notes:* Column 1 displays the estimated "simple" coefficient from 21 and 2. Column 2 shows the pre-trend that has 50% power of being detected (hypothesized trend). Column 3 shows the likelihood ratio.

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# Sensitivity Analysis

Figure: Sensitivity analysis: general rank.

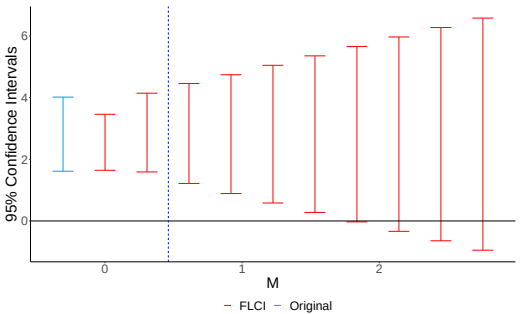
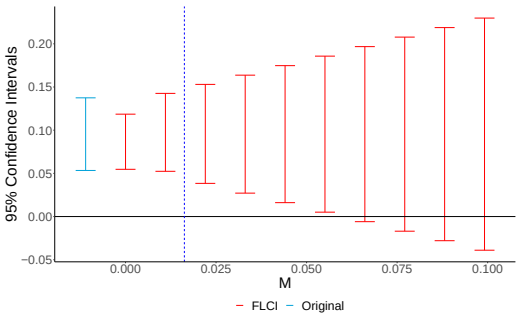



Figure: Sensitivity analysis: standardized general score.

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# Specific human capital mechanism

Table: Effects on access to short-cycle programs in institutions with *Saber 11*-like admission exams

| | Admission exam | | | No admission exam | | |
|---|---|---|---|---|---|---|
| | (1) 1 year | (2) 2 years | (3) 3 years | (4) 1 year | (5) 2 years | (6) 3 years |
| *SaberEs* effect ($\beta$) | 0.026** | 0.024** | 0.010 | 0.000 | -0.001 | 0.000 |
| | (0.012) | (0.010) | (0.010) | (0.007) | (0.006) | (0.008) |
| Observations | 35,484 | 35,484 | 35,484 | 35,484 | 35,484 | 35,484 |
| Controls | YES | YES | YES | YES | YES | YES |
| Mean Control 2015 | 0.132 | 0.141 | 0.126 | 0.0865 | 0.0896 | 0.0982 |

*Notes:* Standard errors clustered at the school level. Results come from a doubly robust estimation as in Sant'Anna and Zhao (2020). Institutions with *Saber 11*-like admission exams that offer short-cycle programs are SENA and UdeA. *p<.05; **p<.01; ***p<.001

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# Motivational effect mechanism

Table: Effects on student's rank in *Olimpiadas del Conocimiento*

| | (1) Grade 10 | (2) Grade 11 | (3) Joint |
|---|---|---|---|
| *SaberEs* effect ($\beta$) | -0.271 | -1.647 | -0.992 |
| | (1.056) | (1.213) | (0.901) |
| Observations | 35,852 | 31,592 | 67,444 |
| Controls | YES | YES | YES |

*Notes:* Standard errors clustered at the school level. Results come from a doubly robust estimation as in Sant'Anna and Zhao (2020). All specifications control for stratum, family income, parent's education, mobile phone ownership and student's working status. *p<.05; **p<.01; ***p<.001

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# Financial aid mechanism

Table: Effects on access to financial aid and *Ser Pilo Paga*

| | (1) Higher Education | (2) Financial Aid | (3) Ser Pilo Paga |
|---|---|---|---|
| *SaberEs* effect ($\beta$) | 0.037*** | 0.005 | 0.010*** |
| | (0.013) | (0.005) | (0.004) |
| Observations | 35,484 | 35,484 | 35,484 |
| Controls | YES | YES | YES |
| Mean Control 2015 | 0.677 | 0.0530 | 0.0464 |

*Notes:* Standard errors clustered at the school level. Results come from a doubly robust estimation as in Sant'Anna and Zhao (2020). *p<.05; **p<.01; ***p<.001

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# Merit-based scholarship mechanism

Table: Effects on access to SPP by type of program

| | (1) Short-cycle SPP | (2) Professional SPP |
|---|---|---|
| *SaberEs* effect ($\beta$) | 0.001* | 0.010** |
| | (0.000) | (0.004) |
| Observations | 35,484 | 35,484 |
| Controls | YES | YES |
| Mean Control 2015 | 0.0007 | 0.0457 |

*Notes:* Standard errors clustered at the school level. Results come from a doubly robust estimation as in Sant'Anna and Zhao (2020). *p<.05; **p<.01; ***p<.001

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