

Visibility Is Not Guidance: Experimental Evidence from a Degree-Progress Map

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Abstract

Universities increasingly use low-cost information tools to help students navigate degree requirements, but clearer information need not change educational choices. We study a randomized pilot of a graphical degree-progress map at a private university in Uruguay. The map displayed an entire curriculum on one screen, showing courses completed, failed, in progress, and still pending, together with prerequisites. Early access increased logged use by 64.7 percentage points. We find no statistically significant effects on course enrollment during the modification window or, before universal access, on credits earned, courses passed, or grades. Eight years after launch, the estimated effect of the eight-month head start on graduation was not statistically distinguishable from zero. The short-run estimates exclude large academic gains, while this persistence estimate remains compatible with modest effects. The results show that making existing degree requirements easier to inspect can improve access to information without changing behavior when the tool offers no individualized recommendation, relative feedback, or verified advising response.

Keywords: higher education; course planning; learning analytics; dashboards; information; randomized experiment

JEL codes: I21, I23, D83

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1 Introduction

Course selection is a recurring educational investment decision made under information and attention constraints. Degree requirements are distributed across course catalogs, transcripts, prerequisite rules, and registration systems. Students must combine these sources to answer a practical question: what should I take next? Low-cost digital tools that organize academic information are therefore attractive to universities. Yet their behavioral effect is ambiguous. Better visibility may correct an information friction, but it may leave choices unchanged if students already know their feasible options, face constraints the tool cannot relax, or need a recommendation rather than a clearer description.

This paper evaluates a graphical degree-progress map, known within the university as the *Grilla de Avance Académico*, introduced at the School of Business and Economics of Universidad de Montevideo. The conventional academic record already showed each student’s course history, grades, examination attempts, credits earned, and credits remaining by requirement category. General degree requirements were available separately. The Grilla’s innovation was to place the student’s full curriculum and individual progress on one screen. Color-coded course boxes distinguished courses passed, failed, currently taken, and not yet attempted. Hovering over a course revealed prerequisites and related information. The display updated automatically with the university’s administrative records. It therefore joined information that otherwise remained split between the student’s record and the general study plan, while making prerequisite chains visually salient.

The university piloted the Grilla in August 2013. Students were randomly assigned to early student access, early student and advisor access, or delayed access. Because advisor behavior was not observed, the main estimates pool the two early-access arms. The control group received the tool when it became universal nearly eight months later, so the experiment identifies the effect of an early-access advantage rather than permanent access versus no access. Early-access students received an official email and an intranet banner during the period in which they could modify second-semester enrollment. Administrative records capture whether students opened the tool, the courses for which they were registered near the beginning and end of the modification period, academic performance before the control group received access, and exact graduation dates. The design therefore permits a direct test of the sequence the intervention was intended to produce: use of the information, a change in course enrollment, improved academic progress, and a persistent difference in completion.

The intervention reached students but did not change that sequence. Pooling the two early-access arms, assignment increased logged use by 64.7 percentage points. It produced no statistically significant difference in enrollment changes during the modification window. By March 2014, before universal access, there were also no statistically significant differences in credits earned, courses

passed, or grades. Eight years after launch, graduation rates did not differ statistically between early- and delayed-access students. Joint randomization-inference tests reach the same conclusion for enrollment decisions, academic progress before universal access, and graduation outcomes.

These findings contribute to the evidence on student-facing analytics in three ways. First, the intervention concerns degree-wide planning rather than performance within a single course. The object made visible was the structure of the curriculum itself: accumulated progress, outstanding requirements, and prerequisite chains. Second, the study observes the decision the tool was explicitly launched to support. Two administrative snapshots measure actual enrollment changes during the modification window, avoiding reliance on self-reported intentions. Third, the combination of use logs and long administrative follow-up separates three possible statements. The tool was used; use did not produce the targeted short-run behavior; and the early-access advantage did not produce an observable completion gain after the tool became universal.

These findings contribute to the economics of education evidence on scalable information interventions in higher education. Existing experiments show that the content of information and the support for acting on it can matter. Scalable online exercises and text messages had no academic effect in one study, while one-on-one coaching did ([Oreopoulos and Petronijevic, 2018](#)). A more intensive package combining enhanced advising, progress information, and a modest grant did not raise completion on average ([Hemelt et al., 2025](#)). A simple encouragement email increased some subsequent economics course-taking but had limited effects on major choice ([Edwards and Meer, 2025](#)). Relative-performance feedback lowered short-run performance without changing longer-run outcomes in another university experiment ([Azmat et al., 2019](#)). In a subsequent experiment at Universidad de Montevideo, comparative rankings sharply corrected students' beliefs but were followed by lower satisfaction and weaker persistence and academic progress among women, with no comparable adverse pattern among men ([Cabrera and Cid, 2026](#)). Repeated feedback on accumulated credits, by contrast, accelerated graduation in another setting ([Brade et al., 2026](#)).

The related evidence on student-facing analytics is also mixed. A recent systematic review finds limited controlled evidence that learning-analytics dashboards improve achievement ([Kaliisa et al., 2024](#)). A dashboard paired with weekly email changed online study behavior but not a final examination ([Hellings and Haelermans, 2022](#)); an advisor-facing early-warning system did not reduce dropout or improve performance ([Plak et al., 2022](#)); and a data-rich course-planning platform reduced grade point average in two experiments ([Chaturapruek et al., 2018](#)). Cross-study comparisons do not identify which feature causes these differences. They do, however, clarify what this experiment adds: a clean test of a widely scalable, largely descriptive intervention with high take-up, observed planning decisions, and unusually long follow-up.

We use guidance as shorthand for action-oriented support that goes beyond reorganizing

information already available to the student. Examples include an individualized recommendation, relative feedback, or a verified advising response. With that distinction, the portable interpretation is that visibility is not guidance. Organizing degree requirements can make the path easier to inspect without making the next action sufficiently clear or valuable to change. The experiment speaks to nearly eight months of early access among already-enrolled students at one private Uruguayan university. It does not identify the effect of permanent access versus no access or the effect of adding any of these forms of guidance.

2 Institutional Setting and Intervention

2.1 The planning problem

The School of Business and Economics offered four-year programs with semester-based study plans, credit requirements, prerequisites, and opportunities to retake courses. Before the intervention, students could consult a detailed academic record with course-level grades and examination attempts, plus a summary of credits earned and pending by requirement category. They could also consult the general study plan elsewhere. These sources did not produce a personalized representation of where the student stood within the full curriculum or show prerequisite relationships at a glance. The Grilla was designed to fill that presentational gap.

The interface arranged required courses by recommended semester. Green boxes denoted approved courses, blue boxes courses being taken or registered for the next period, and gray boxes courses never taken. Red and yellow distinguished unsuccessful attempts according to whether an examination period remained available. Symbols indicated prerequisites and repeated enrollment, while the number in parentheses gave course credits. Hovering over a box displayed status, prerequisites, and equivalencies and visually highlighted prerequisite courses. Because the display was linked to the student information system, passing or failing a course updated the map together with the conventional academic record.

This was a degree-progress map, not an optimization or recommendation engine. It did not predict the student's chance of passing, compare the student with peers, recommend a course portfolio, or remove registration and prerequisite constraints. Its intended value was to make existing curricular information easier to integrate and to draw attention to the consequences of current course choices.

2.2 Pilot and rollout

The pilot launched on August 8, 2013. Contemporaneous administrative communications describe it as a beta test and tell selected students that it could help them plan their careers and adjust

second-semester course registrations. Students could access the Grilla from the same intranet page as their academic record. An email announced the tool, and an intranet banner provided a direct prompt for students who did not regularly read email. The course-modification period ran from August 12 through August 25. Control students also received an email from the registrar’s office on August 19. A contemporaneous project note describes its purpose as ensuring that the control group also received an institutional message during the modification period; the exact text of that email has not been recovered.

The university created two early-access arms. In the first, only the student was given access. In the second, both the student and the student’s advisor or coordinator could view the map. The institution informed advisors which advisees had early access and explained how to find the display. The available records do not establish whether advisors opened the Grilla, contacted students, or changed their advice. We therefore pool both arms for the main analysis and treat their difference only as a nominal assignment contrast, not as evidence on an advising mechanism.

The original control group was scheduled to receive the tool later. Contemporaneous records show that the Grilla had become available to all students in the School of Business and Economics by April 1, 2014. The clean experimental period is consequently the interval between the August 2013 launch and that opening. Outcomes after April 2014 measure whether an eight-month head start produced persistent effects; they do not compare permanent access with permanent non-access.¹

3 Experimental Design and Data

3.1 Assignment and analysis sample

The randomization records contain 603 degree-program observations corresponding to 569 people. The design assigned 151 records to student-only access, 151 to student-and-advisor access, and 301 to delayed access. Assignment was rerandomized until 48 balance comparisons had conventional p -values of at least 0.10. The rule applied four contrasts—each early-access arm versus control, pooled early access versus control, and the two early-access arms against each other—to 12 pre-treatment measures: courses attempted, overall grade average, courses passed, grade average among passed courses, credits, gender, four degree-program indicators, attendance at PRE/U or Instituto Juan XXIII, and Montevideo residence.

¹Beginning in July 2014, the university ran a separate experiment that provided relative-performance rankings to a broader population across three schools (Cabrera and Cid, 2026). That intervention began after the Grilla’s clean experimental period and therefore cannot affect the enrollment or March 2014 outcomes. Of the 535 people in the primary Grilla sample, 433 appear in the later experiment. Within that overlap, assignment to ranking feedback was 50.7 percent among students assigned to early Grilla access and 51.8 percent among Grilla controls. The later intervention is therefore not differentially assigned in the main pooled comparison, although the long-run estimates necessarily describe an institutional environment in which other student interventions subsequently occurred.

Thirty-four people appear more than once because they held multiple degree-program records. Their records were not consistently assigned to the same arm. The main analysis therefore uses the pre-treatment rule that is independent of assigned status: retain the 535 people with exactly one baseline program record. This gives 139 student-only, 136 student-and-advisor, and 260 control students. Retaining one record for each of the 569 people using the documented arm-priority rule ($T2 > T1 > \text{control}$) yields the same qualitative pattern of estimates.² Because record selection can then depend on assignment, we retain the 535-person singleton sample as the main specification.

3.2 Outcomes and timing

The first-stage outcome is an indicator for at least one logged Grilla visit between launch and October 3, 2013. Across the full pilot file, the access log records 765 visits by 200 early-access users through that date. Within the 535-person analysis sample, 179 of 275 early-access students appear in the log; no control student does.

The immediate outcomes compare course-registration snapshots dated August 9 and August 29. The first snapshot was taken one day after launch but three days before the announced modification window opened. It is therefore not a strictly pre-treatment measure, but it records enrollment before students could use that window to adjust their registrations. The outcomes measure whether a student made any change, the number of changes, courses deleted, courses added, and the total number of courses after the window. These records match 494 of 535 students. Matching is not differential by treatment: the adjusted difference is -0.7 percentage points with a randomization-inference p -value of 0.765.

Academic records measured in March 2014 provide the primary downstream outcomes before universal access: credits accumulated since baseline, courses passed since baseline, and the grade average in passed courses. All 535 students are observed. A second record in April 2015, after universal access, covers 529 students and provides supporting persistence outcomes. Observation is not differential by treatment: the adjusted difference is 0.07 percentage points (95 percent confidence interval -1.79 to 1.93 ; randomization-inference $p = 0.871$).

Graduation records extracted in August 2026 cover all programs at the university. The primary long-run horizon is eight years after launch: twice the nominal program length and long enough to capture 431 of the 440 graduations observed during the full thirteen-year follow-up. Only nine additional students graduate between years eight and thirteen. We measure graduation from any university program within five and eight years, graduation from the baseline program within eight

²This sensitivity gives adjusted effects of 0.1 percentage points for any enrollment change (95 percent confidence interval -7.8 to 8.0), -0.91 credits by March 2014 (-3.68 to 1.87), and -1.1 percentage points for any-program graduation by year eight (-6.6 to 4.4). These are covariate-adjusted estimates with heteroskedasticity-robust intervals; because the retained record can depend on assignment, they are descriptive sensitivity estimates rather than an alternative causal specification.

years, and years to any graduation restricted at eight. The any-program outcome allows for degree switching within the university; the matching-program outcome asks whether students completed the program recorded at baseline. Thirteen-year outcomes are retained as extended-follow-up checks rather than as the paper’s headline horizon.

Among students observed graduating during the full follow-up, the 90th percentile of time from pilot launch to graduation is 4.95 years. The administrative records also contain a cohort-year variable: the 90th percentile of graduation year minus recorded cohort year is seven calendar years. These are descriptive timing diagnostics, not treatment comparisons among graduates; conditioning on graduation would select on a post-treatment outcome.

3.3 Estimand and inference

The main estimand is the intention-to-treat effect of assignment to either early-access arm relative to delayed access in the 535-person sample. For outcome Y_i , we estimate

$$Y_i = \alpha + \tau T_i + X_i' \beta + \varepsilon_i, \tag{1}$$

where T_i indicates either early-access arm and X_i includes baseline academic performance and pre-treatment demographic and program indicators. We report heteroskedasticity-robust confidence intervals. Unadjusted estimates lead to the same substantive conclusions.

Because assignment was rerandomized, conventional regression inference does not fully describe the design. We reconstruct the original balance restriction and report two-sided randomization-inference p -values from 10,000 accepted assignments. A joint statistic tests each outcome family. The realized assignment satisfies the original rule, and a Monte Carlo reconstruction accepts approximately 9.2 percent of proposed assignments.

The experiment was not formally preregistered. Contemporaneous correspondence shows that the registration snapshots and access logs were requested as project outcomes before results were available. We treat this as evidence of prospective outcome choice, but recognize that it is weaker than a public analysis plan.

4 Results

4.1 Treatment delivery

Table 1 follows the intended causal sequence. Early access increased the probability of a logged visit by 64.7 percentage points (95 percent confidence interval 59.1 to 70.4; randomization-inference $p < 0.001$). The student-only and student-and-advisor arms generated similar student access rates;

their adjusted difference is 1.4 percentage points ($p = 0.807$). The null effects below are therefore not explained by a failure to expose most treated students to the tool during the early pilot.

Table 1: Intent-to-treat effects of early access to the degree-progress map

Outcome	Control mean	ITT	95% CI	RI p	N
<i>A. Treatment delivery</i>					
Opened the degree-progress map	0.000	0.647	[0.591, 0.704]	< 0.001	535
<i>B. Enrollment decisions</i>					
Any enrollment change	0.651	-0.011	[-0.092, 0.071]	0.793	494
Number of enrollment changes	1.751	-0.107	[-0.441, 0.227]	0.526	494
Courses added	0.768	-0.015	[-0.217, 0.186]	0.880	494
Courses after modification window	5.336	-0.044	[-0.291, 0.202]	0.729	494
<i>C. Academic progress before universal access</i>					
Credits earned by March 2014	36.675	-1.662	[-4.515, 1.190]	0.253	535
Courses passed by March 2014	6.050	-0.343	[-0.801, 0.115]	0.139	535
GPA in passed courses, March 2014	8.052	0.007	[-0.066, 0.080]	0.841	535
<i>D. Persistence after universal access</i>					
Any UM graduation by year 5	0.742	0.001	[-0.060, 0.062]	0.970	535
Any UM graduation by year 8	0.812	-0.016	[-0.074, 0.042]	0.594	535
Baseline-program graduation by year 8	0.785	-0.032	[-0.093, 0.029]	0.306	535
Years to graduation, restricted at 8	3.861	0.068	[-0.216, 0.352]	0.641	535

Notes: The main sample contains students with one baseline degree-program record. The ITT compares the two early-access arms jointly with delayed access. Estimates adjust for baseline academic performance and pre-treatment characteristics; confidence intervals use heteroskedasticity-robust standard errors. RI p -values use 10,000 assignments accepted under the original balance rule. Enrollment outcomes are available for 494 students. Courses deleted are omitted because total changes equal courses added plus courses deleted; all three enter the enrollment-family test. March 2014 outcomes precede universal access in FCEE. The final row is a restricted-mean outcome that assigns eight years to students not observed graduating by the cutoff. Familywise randomization-inference p -values are 0.892 for enrollment decisions, 0.298 for pre-crossover progress, and 0.546 for graduation outcomes through year eight.

4.2 Enrollment decisions

Despite high use, the Grilla did not change the decisions it was launched to support. In the control group, 65.1 percent of matched students changed at least one course registration. Early access produced no statistically significant difference in that probability.

The intensive-margin estimates are similarly small relative to control behavior. Control students made 1.75 changes on average; the estimated ITT is -0.11 changes, with a confidence interval from -0.44 to 0.23. Effects on courses added and the number of courses held after the window are also close to zero. A joint randomization-inference test across enrollment outcomes yields $p = 0.892$. The evidence does not suggest that access shifted registration while leaving the number of changes unchanged.

4.3 Academic progress before universal access

The outcomes measured before universal access provide the strongest evidence on academic consequences. By March 2014, early access had produced no statistically significant differences in credits earned, courses passed, or grades. The point estimates for credits and passed courses are negative, while the estimate for grades is close to zero. A joint randomization-inference test does not reject the null for this outcome family.

These are informative nulls within the experiment’s support, not equivalence claims. The design and sample cannot rule out small effects, but they do rule out the large academic improvement that would be needed to make the Grilla a powerful stand-alone intervention over the seven-month clean period.

4.4 Persistence and graduation

After the university opened the Grilla to all students, the treatment contrast became a difference in timing of access. The adjusted April 2015 estimates are -4.02 credits (95 percent confidence interval -9.89 to 1.85), -0.78 courses passed (-1.73 to 0.18), and 0.01 grade points (-0.06 to 0.08); the joint randomization-inference p -value is 0.239 . These outcomes do not indicate a delayed academic gain from early access.

Graduation outcomes likewise show no persistent advantage. By year five, 74.2 percent of control students had graduated from some university program. The adjusted effect is 0.1 percentage points. By year eight, 81.2 percent of controls had graduated, and the effect is -1.6 points (95 percent confidence interval -7.4 to 4.2 ; randomization-inference $p = 0.594$). The estimate for completing the baseline program by year eight is -3.2 points (95 percent confidence interval -9.3 to 2.9). Restricting time to graduation at eight years produces an estimated delay of 0.07 years, with a confidence interval from a 0.22-year acceleration to a 0.35-year delay. The joint randomization-inference p -value is 0.546 .

Figure 1 shows the adjusted difference in cumulative graduation at each annual horizon through year eight. The point estimates remain close to zero throughout, but the intervals are wide enough to accommodate modest effects. The eight-year estimate rules out neither a gain of roughly four percentage points nor a loss of roughly seven.

5 Discussion

The experiment identifies a specific boundary condition for information interventions in higher education. The Grilla reduced the effort required to assemble a student’s history, remaining requirements, and prerequisite structure. Students used it at a high rate when it was introduced.

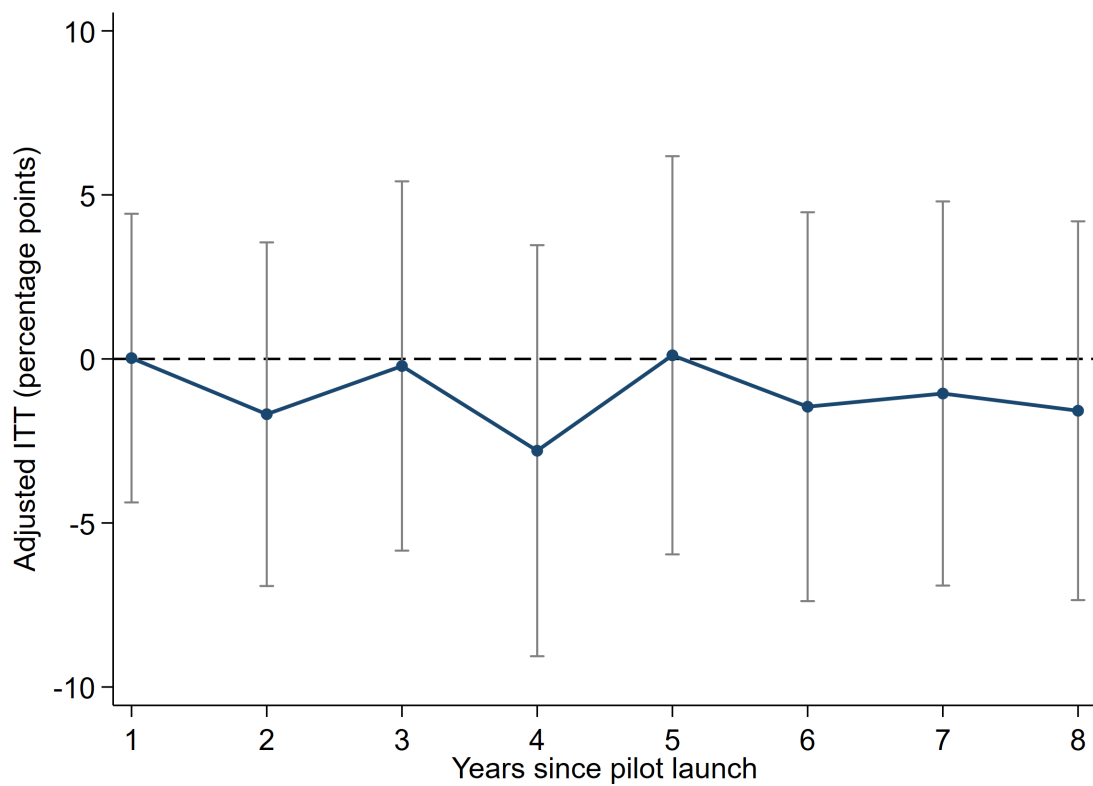


Figure 1: Adjusted effect of early access on cumulative graduation

Notes: Points are adjusted intention-to-treat estimates in percentage points for graduation from any Universidad de Montevideo program by each anniversary of the August 2013 launch. Bars are heteroskedasticity-robust 95 percent confidence intervals. The eight-year horizon is twice the nominal program length and captures 431 of the 440 graduations observed during the full follow-up.

Yet neither enrollment nor academic progress moved. Making the structure of the problem easier to see was not, in this setting, enough to change the solution students chose.

Several mechanisms are consistent with this result. Students may already have known which courses they could take, leaving the interface to confirm rather than alter plans. They may have faced binding timetables, prerequisite constraints, work commitments, or preferences that the Grilla did not change. The display may also have lacked an actionable margin: it showed which courses unlocked others but did not recommend a personalized sequence or quantify the consequences of delay. These explanations are hypotheses, not experimentally separated mechanisms.

The comparison with more directive interventions is useful but not causal. Relative or predictive feedback changes the information students receive, not merely its organization. Recommended actions and active counselor follow-up add a response technology. Experiments with these features have produced positive, negative, heterogeneous, and null effects ([Oreopoulos and Petronijevic, 2018](#); [Azmat et al., 2019](#); [Plak et al., 2022](#); [Cabrera and Cid, 2026](#); [Brade et al., 2026](#)). The present study shows that a scalable descriptive layer can achieve take-up without generating detectable academic gains. It does not establish that adding recommendations or advising would work at this university.

The study has five material limitations. First, the main sample is modest, particularly for graduation, and the long-run intervals permit effects that some institutions could consider important. Second, the treatment bundled access with an email and banner, so it is the effect of early access and communication rather than the isolated visual interface. Third, the control group later received the Grilla. Long-run estimates therefore concern persistence from earlier access, not permanent treatment. Fourth, advisor access was assigned but advisor behavior was not measured, preventing a test of advising as a mechanism. Fifth, the multiple-program assignment error requires a transparent sample rule. Excluding all multiple-record students avoids assignment-dependent precedence and preserves 535 people.

External validity is also deliberately narrow. Participants were already-enrolled students in business and economics programs at one private university in Uruguay. A similar map could matter more where curricula are less structured, information is harder to obtain, or students receive targeted guidance based on the display. Conversely, the high control-group rate of enrollment modification shows that students were active planners even without early Grilla access.

6 Conclusion

The Grilla made a student’s full degree path visible in one place. Early access caused substantial use, but did not change course registration, credits, passed courses, grades, or subsequent graduation. Evidence collected before universal access rules out large short-run academic benefits, while the long-run evidence shows no detectable persistent advantage and remains less precise.

The policy lesson is not that universities should withhold clear academic information. A visual

degree audit may be valuable for transparency, convenience, or student satisfaction even when it does not raise measured attainment. The narrower lesson is that information design and behavioral action are distinct. When the objective is to change progress toward a degree, visibility may need to be paired with actionable guidance.

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