Opportunity Cost of Time and Academic Performance: Evidence from Soccer Matches and University Exams

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Abstract

When students can postpone an exam, a salient leisure event can change both whether they are evaluated and how well they perform. I study this using the day-level alignment between 172,050 final-exam opportunities for 4,053 university students and salient World Cup and Copa América matches, including all Uruguay matches and selected late-stage games, from 2003 to 2014. An exam held the day after one of these matches has 3.9 percentage points higher non-attendance, grades among exam takers that are 0.14 standard deviations lower, and a 6.3 percentage-point lower probability of passing the scheduled opportunity—about 10 percent relative to the control mean. The grade effect is concentrated at this one-day horizon and remains similar with student fixed effects and flexible calendar controls. Because students can skip an exam without justification and use a later opportunity, the setting distinguishes a participation response from performance conditional on participation, two margins that compulsory-exam settings cannot separate. The results show that short-lived leisure shocks affect academic outcomes not only through performance, but also by changing when students choose to be evaluated.

Keywords: time allocation; study effort; academic performance; higher education; soccer

JEL classification: I21; I23; J22; J24

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I Introduction

How individuals allocate scarce time depends on the opportunity cost of competing activities. A short-lived increase in the value of leisure can displace productive effort, but how that shock appears in observed outcomes depends on whether the task must be completed at a fixed time or can be postponed. When timing is flexible, the same shock may affect both performance conditional on participation and the decision to participate at the scheduled time; observed performance alone may therefore capture only part of the response.

University final exams provide a setting in which these margins can be observed separately. Students decide how much time to devote to preparation and, when exams are deferrable, whether to use a scheduled opportunity or wait for a later one. I study these choices using the day-level alignment between final exams and a set of salient World Cup and Copa América matches that includes all Uruguay matches and selected late-stage games. International tournaments create a salient, short-lived increase in the value of leisure. If a match displaces study immediately before an exam, students may either postpone the exam or take it with less preparation. This interpretation is consistent with evidence that experimentally induced reductions in study time lower college grades (Stinebrickner and Stinebrickner, 2008) and that fatigue and the timing of rest within a final-exam cycle affect exam performance (Glaser and Insler, 2022).

I use administrative records for 172,050 exam opportunities for 4,053 undergraduate students at Universidad de Montevideo between 2003 and 2014. The sample spans three World Cups and three Copas América. Because students may skip an exam without justification and use a later opportunity, the data reveal both whether a student takes an exam and how the student performs conditional on taking it.

An exam scheduled the day after one of these matches has 3.9 percentage points higher non-participation, grades among exam takers that are 0.14 standard deviations lower, and a 6.3 percentage-point lower probability of passing the opportunity. In a joint event-time specification, the grade effect is concentrated one day after a match; the other four event-time coefficients are jointly indistinguishable from zero. The estimates remain similar with flexible calendar controls and student fixed effects. A sensitivity analysis limited to Uruguay matches preserves the signs and magnitudes.

An exploratory cycle-level extension follows each student–course sequence from its first observed opportunity. In a support-balanced sample with at least one year before the administrative endpoint, a treated first opportunity lowers initial passing by 5.0 percentage points and raises the probability of a later recorded opportunity by 5.1 percentage points; the estimated effect on passing by the end of the observed exam cycle is -0.6 percentage points and imprecise. This pattern is consistent with delay and subsequent recovery rather than a precisely estimated reduction in end-of-cycle passing.

Gender-specific point estimates do not yield statistically significant interactions at the 5 percent level. A 2015 student survey provides descriptive evidence on viewing and reported study choices but is not used to identify a mechanism.

Related work has examined how sports affect academic achievement at different horizons. Lindo et al. (2012) use season-to-season variation in University of Oregon football success and find lower grades among non-athletes during successful seasons. Hernández-Julián and Rotthoff (2014) examine similar variation at Clemson University over a longer period and document a different gender pattern. The closest antecedent is Metcalfe et al. (2019), who study nearly 3.5 million English secondary-school students whose compulsory GCSE examinations overlap the World Cup or European Championship. Using tournament-year comparisons and within-student differences in subject exposure, they find lower exam performance, particularly among students predicted to be more interested in football. Nguyen et al. (2025) study Vietnamese cohorts whose university entrance examination occurred near a World Cup and report gender differences in subsequent bachelor's attainment and employment. Hyman and McFarlin (2024) instead exploit a lottery for college basketball season tickets and find modest reductions in semester GPA and credits.

These studies establish that sports can affect academic achievement, but their settings do not reveal the same immediate choice over whether to use a scheduled exam opportunity. The contribution here comes from combining day-level match–exam alignment with a system in which students can postpone final exams without justification. I can therefore distinguish the decision to take the exam from performance conditional on taking it, while pass per scheduled opportunity provides an outcome observed for everyone. This decomposition shows how a precisely timed leisure shock operates through both academic participation and realized performance.

The remainder of the paper describes the data and empirical strategy, presents the main results and robustness exercises, discusses the descriptive survey evidence, and concludes.

II Data and empirical strategy

I combine administrative university records and tournament calendars. A separate 2015 survey of students and instructors provides descriptive evidence on soccer viewing, stated study behavior, and instructors' stated practices.

Administrative data

The academic performance database covers 172,050 final-exam opportunities for 4,053 undergraduate students at Universidad de Montevideo between 2003 and 2014. This private university requires students to pass both coursework and a final exam to progress. The final subject grade combines the course and exam grades with equal weights. Students must earn at least 4 out of 12 in the course and at least 6 out of 12 on the final exam. I standardize observed exam grades using their mean and standard deviation within the 2003–2014 analytical sample. Table 1 summarizes the student- and opportunity-level variables used in the analysis.

Students have three regular consecutive periods to pass the exam. They can choose not to attend any of these instances if they believe they are not prepared, or for any other reason (no justification is required). If a student fails or does not attend the first instance, they can take the exam in the second or third period. Once the exam is passed, there is no option to retake it to improve the grade. Therefore, passing the exam in one of the regular opportunities is mandatory; otherwise, they will have to repeat the year. The administrative export also contains a fourth slot used for special-period records: it accounts for 2,092 opportunities, or 1.2 percent of the sample. I retain these records and control for the administrative period code; excluding them leaves the results essentially unchanged (Appendix Table A3).

First-semester exam periods occur in June and July, with a third period in December; second-semester exams occur in December, February, and March. World Cups and Copas América therefore overlap an intensive final-exam period in this Southern Hemisphere setting. This institutional overlap supplies useful timing variation but is not, by itself, an identification assumption.

Final exams extend for more than one month, creating substantial within-period variation in match alignment. Lectures have ended by this stage, so match timing cannot change classroom instruction for these exams. It could still affect student participation, preparation, and other behaviors, which the outcome decomposition and falsification exercises address.

Final-exam dates follow a stable institutional calendar. The academic office schedules exams well in advance using recurring sequencing rules designed to space assessments within degree programs; tournament matches are not a scheduling criterion. As one documented example, the printed FCEE Academic Catalog distributed to students at the beginning of the 2012 academic year, in the first days of March, already listed exam dates through March 8, 2013. The tournament schedule is determined outside the University. Consistent with this institutional practice, the administrative data show no evidence that academic units systematically avoided the days following matches in the treatment calendar: after controlling for year, week of the examination season, and day of the week, those dates are no less likely to host an exam and have no fewer scheduled courses or exam opportunities. Comparisons with the same weekday one and two weeks before and after each match yield the same conclusion (Table A1). Together, the institutional record and the calendar audit support treating match–exam alignment as arising from independently organized calendars, while allowing for isolated operational changes.

Table 1: Summary statistics of the exams database

Variable	Mean	S.D.	Min.	Max.	N
Students					
Female	0.52	0.50	0	1	4,053
Cohort	2007.78	4.16	1995	2014	4,053
Grade in courses	7.11	1.50	2	12	4,017
Number of exams taken	32.89	20.12	0	115	4,053
Exams during tournament	3.16	2.45	0	12	4,053
Did not sit for the exam	0.23	0.21	0	1	4,053
Grade in exams	7.57	1.55	1	12	4,000
Exam opportunities					
Did not sit for the exam	0.24	0.42	0	1	172,050
Grade in exams	7.59	2.36	1	12	131,615
Grade in exams (std.)	0.00	1.00	-3	2	131,615
Credits attempted	6.37	2.24	0	29	172,050
Credits gained	5.22	3.16	0	29	172,050
Passed the exam	0.81	0.39	0	1	131,615
Year	2008.81	3.29	2003	2014	172,050
Semester	1.46	0.50	1	2	172,050
Period	1.46	0.74	1	4	172,050
During tournament	0.07	0.26	0	1	172,050
Exam takers per date	53.96	75.98	1	474	2,439

Notes. The table reports descriptive statistics for 4,053 students and 172,050 exam opportunities from 2003 to 2014. Student-level counts vary with data availability; 131,615 opportunities have an observed grade. The final row reports the number of exam takers on each of the 2,439 dates with an observed exam grade.

Surveys

During the 2015 Copa América, I conducted separate surveys of students and instructors at the School of Business. The student analysis uses 712 questionnaire records after removing all confirmed duplicates within and across collection modes. Because some records cannot be linked to an individual identifier, 712 is a count of cleaned questionnaire records rather than a verified number of unique respondents. The instructor survey contains 21 responses. The project and survey instruments received approval from the University's Research Ethics Committee.

Student responses were collected on paper and online during the final two weeks of the first semester. The available records do not support a defensible response rate, and collection mode may be selective. The survey was conducted after the administrative sample ends, so it cannot affect the main estimates. Its role is to characterize exposure and stated time-use choices, not to identify why the administrative outcomes changed.

Empirical strategy

The empirical strategy uses variation in the timing of exams and matches from June through July between 2003 and 2014. The sample includes the 2004, 2007, and 2011 Copas América and the 2006, 2010, and 2014 World Cups. Tournament starting dates vary across years, and exam dates vary within the June-July period. Figure 1 displays this overlap.

The preferred specification defines an opportunity as treated when one of the matches in the treatment calendar is played on the preceding day; nearby days provide event-time falsifications. Student fixed-effects models use within-student variation. Because matches were not assigned by the researcher, the estimates are reduced-form evidence conditional on calendar comparability, supported by controls and falsifications.

The treatment calendar was constructed to capture games likely to command broad attention in Uruguay, not only appearances by the national team. It includes all Uruguay matches in the five tournaments for which Uruguay participated. In the 2004 and 2007 Copas América it also includes the other semifinal and the final; in the 2010 World Cup, the other semifinal and the final; and in the 2014 World Cup, both semifinals and the final. In 2011 it includes only Uruguay matches. Because Uruguay did not qualify for the 2006 World Cup, that tournament contributes all quarterfinal dates, both semifinals, the third-place match, and the final. This is the historical calendar used to construct the analytical data; its selection of non-Uruguay games is transparent but is not a uniform round-based rule across tournaments.

The resulting calendar contains 44 potential post-match dates: 29 follow Uruguay matches and 15 follow other selected games. Twenty-seven of these dates coincide with scheduled exams, generating 4,083 treated opportunities; 15 treated exam dates (1,919 opportunities) follow Uruguay and 12 (2,164 opportunities) follow other selected games. Public audience data are incomplete for the sample period. A Kantar IBOPE Media retrospective reports a 45.23 household rating in Montevideo for the 2014 World Cup final between Germany and Argentina, illustrating that a late-stage match without Uruguay could command a large domestic audience, but I do not observe comparable match-level ratings for all tournaments (Kantar IBOPE Media, 2022). I therefore report a sensitivity that isolates Uruguay matches rather than using audience to redefine exposure.

Figure 1: Treatment: overlap of exams and major sport events

Panel A: Zoom in June–July, 2003–2014

june

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
2003	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2	2	38	0	11
2004	0	0	2	51	0	0	2	0	5	4	2	0	0	11	0	0	2	4	0	0	0	0	18	30	0	0	0	35	69	40
2005	0	5	1	0	0	14	0	2	12	0	0	0	8	1	0	2	0	0	0	1	3	0	6	1	0	0	28	47	2	118
2006	9	0	0	0	2	5	1	1	2	0	0	4	0	0	5	17	6	0	0	2	0	0	1	0	1	5	27	12	5	34
																								World Cup – Germany 2006						
2007	1	0	0	18	0	10	15	0	0	0	8	1	0	2	2	0	0	2	0	4	30	18	0	0	143	111	4	67	104	0
2008	0	0	0	0	2	4	0	0	1	3	0	0	0	0	0	0	0	0	0	50	0	0	93	50	1	40	174	0	0	147
2009	1	0	0	14	42	0	0	0	0	1	0	1	0	0	1	1	19	0	0	0	0	5	21	27	1	19	0	0	3	123
2010	0	16	0	1	0	0	5	2	12	2	0	0	0	5	2	0	12	0	0	0	15	0	0	22	43	0	0	99	123	120
																										World Cup – South Africa 2010				
2011	0	2	6	0	0	0	1	0	0	2	0	0	8	0	0	0	5	0	0	0	0	0	0	15	1	0	2	0	5	6
2012	36	0	0	0	0	0	3	1	0	0	0	1	0	0	38	0	0	0	0	8	13	30	83	0	24	92	163	217	243	0
2013	0	0	3	0	0	0	24	0	0	9	0	39	0	6	21	0	52	14	0	70	3	0	0	12	0	0	1	36	0	0
2014	0	0	3	1	6	5	49	0	0	2	2	0	2	0	0	0	0	0	0	0	0	0	0	0	1	0	30	0	0	5
																												World Cup – Brazil 2014		

july

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31
2003	46	1	24	73	20	0	79	57	180	30	47	46	0	98	118	44	173	0	4	0	69	170	27	114	79	125	0	52	152	23	1
2004	38	161	13	0	134	76	12	126	138	3	0	149	20	23	131	209	0	0	155	75	62	48	184	22	0	172	144	32	143	193	0
																Copa América – Perú 2004															
2005	5	0	0	141	22	158	176	78	0	0	220	93	150	74	193	18	0	0	269	33	180	142	0	0	224	32	31	197	155	19	0
2006	0	0	203	51	20	28	279	0	0	252	53	36	78	217	7	0	165	0	156	113	77	31	0	321	194	42	52	173	9	0	319
2007	0	179	113	197	67	130	0	0	316	146	81	68	120	1	0	370	118	0	103	258	0	37	122	132	40	72	309	0	0	61	137
					Copa América – Venezuela 2007																										
2008	226	27	44	376	1	0	247	102	125	86	223	0	0	299	129	39	268	0	4	8	283	140	57	151	88	0	25	135	171	98	96
2009	159	167	237	0	0	174	124	112	113	299	3	0	148	153	128	172	211	0	0	310	123	237	126	248	11	0	211	99	105	205	125
2010	215	304	1	0	128	121	113	167	300	0	0	87	121	207	196	308	0	0	333	164	176	118	226	0	0	237	124	78	240	170	0
2011	22	0	0	150	144	183	196	259	0	0	149	83	163	244	269	1	0	0	296	163	159	411	0	0	216	176	167	104	382	0	0
												Copa América – Argentina 2011																			
2012	0	125	104	229	136	274	22	0	151	85	215	236	314	0	0	180	247	0	277	453	0	0	169	115	99	132	207	0	0	178	160
2013	166	195	220	304	192	0	0	93	85	210	263	209	21	0	130	120	197	0	459	0	0	76	150	136	354	179	0	0	143	85	121
2014	72	140	127	230	0	0	300	180	190	167	360	0	0	140	158	144	465	0	0	0	277	213	17	142	474	0	0	209	137	68	210

Each cell contains the number of grade observations in each date.

Panel B: Zoom in July, 2003–2006

july

2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	3
1	24	73	20	0	79	57	180	30	47	46	0	98	118	44	173	0	4	0	69	170	27	114	79	125	0	52	152	[illegible]
61	13	0	134	76	12	126	138	3	0	149	20	23	131	209	0	0	155	75	62	48	184	22	0	172	144	32	143	1
										Copa América – Perú 2004																		
0	0	141	22	158	176	78	0	0	220	93	150	74	193	18	0	0	269	33	180	142	0	0	224	32	31	197	155	1
0	203	51	20	28	279	0	0	252	53	36	78	217	7	0	165	0	156	113	77	31	0	321	194	42	52	173	9	

Notes. These panels show the overlap between exam periods and tournament periods, and the number of exam-level observations in each day, for a selected time frame.

Figure 1 summarizes the between-year overlap of tournament and examination periods. Figure 2 illustrates the within-year variation for 2004, when exam opportunities occur both inside and outside the Copa América period.

The principal exam periods begin in late June or July and in late November or December. The preferred day-level analysis further distinguishes dates inside and outside the shaded period according to their exact proximity to a match in the treatment calendar. Figure A1 reports all sample years.

I organize the empirical analysis around three specifications. The tournament-period model provides a broad benchmark for average differences over the multiweek event. The preferred exact-day model asks whether an exam falls immediately after a match in the treatment calendar, which is the timing most directly tied to last-minute preparation. A third specification exploits repeated observations for each student to absorb time-invariant individual heterogeneity.

The broad tournament-period model is

$$Y_{ijt} = \alpha_0 + \alpha_1 Tournament_t + X_j' \theta_1 + Z_i' \delta_1 + \varepsilon_{ijt}. \tag{1}$$

In equation (1), Y_{ijt} is alternatively non-participation, standardized grade conditional on attendance, or pass per scheduled opportunity for student i, exam opportunity j, and date t. $Tournament_t$ indicates that date t falls within a World Cup or Copa América period. The baseline controls include year, semester, exam period, gender, major, and high-school location. Preferred standard errors are clustered two ways by exam date and subject-by-year, allowing for a common date shock and dependence within course offerings.

The complete opportunity sample contains 2,463 exam-date clusters and 5,403 subject-by-year clusters; the conditional-grade sample contains 2,439 and 5,347, respectively.

Equation (1) captures the average difference between tournament and non-tournament periods. Because exposure and student responses may vary over a multiweek event, this broad comparison can average over short-lived effects.

Equation (2) therefore defines the main treatment at the day level. A match on the day before an exam leaves little time to replace displaced study; a match after the exam cannot affect preparation for that exam. I also estimate a joint event-time model with indicators for matches two days before through two days after an exam.

$$Y_{ijt} = \beta_0 + \beta_1 Match_{t-1} + X_j' \theta_2 + Z_i' \delta_2 + \mu_{ijt}. \tag{2}$$

The coefficient of interest, β_1, is attached to an indicator equal to one when a match in the treatment calendar was played on date $t-1$. The same outcomes and controls as in equation (1) are used.

Finally, I exploit the panel structure of the database by adding student fixed effects, following the strategy used by Lindo et al. (2012) to remove persistent individual heterogeneity from estimates of the relationship between sports exposure and academic performance:

$$Y_{ijt} = \gamma_0 + \gamma_1 Treatment_t + X_j' \theta_3 + f_i + \omega_{ijt}. \tag{3}$$

Here, $Treatment_t$ is alternatively the tournament-period indicator from equation (1) or the prior-day match indicator from equation (2). Student fixed effects, f_i, absorb time-invariant individual characteristics such as persistent ability, interest in soccer, or propensity to procrastinate. Inference in these models is clustered two ways by exam date and student; clustering only by student would not account for the common shock faced by everyone examined on the same date.

The coefficients in equations (1)–(3) capture reduced-form effects of match timing on administrative academic outcomes.

I report estimates by gender together with formal interaction tests. Because the data do not reject equal effects at the 5 percent level, gender is treated as a secondary, suggestive dimension rather than as the paper's main contribution.

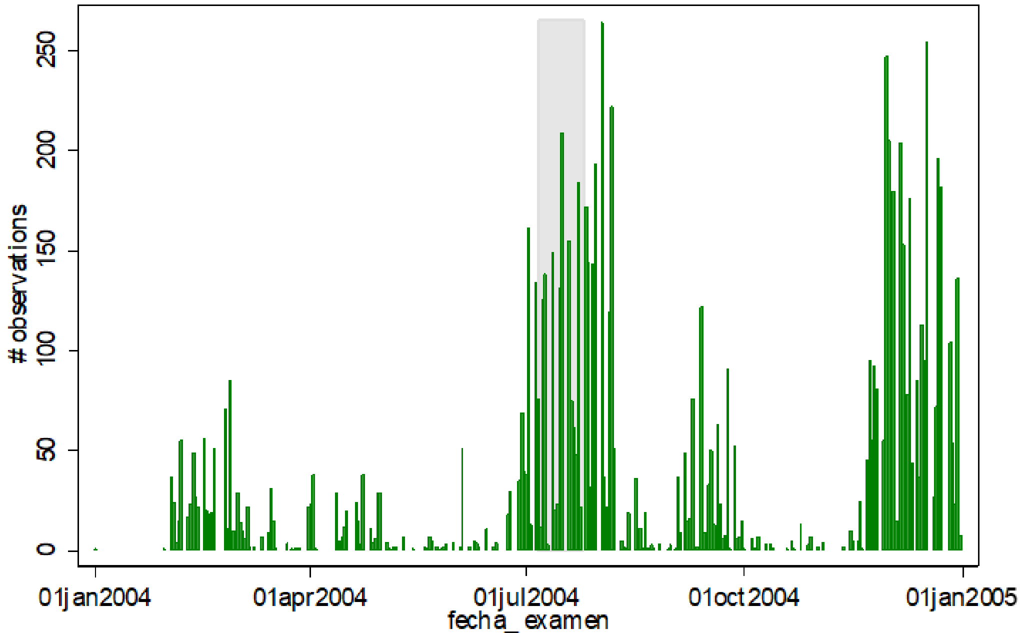
III Main estimation results

The first row of Table 2 reports the broad tournament-period benchmark in equation (1). The estimates are small and statistically indistinguishable from zero for all three outcomes. Appendix Table A2 shows the same null pattern with flexible calendar controls and student fixed effects. This pattern is consistent with a multiweek comparison averaging over shorter-lived responses and motivates the exact day-level specification.

The remaining rows report the preferred exact-day estimates from equation (2). An exam scheduled after a match in the treatment calendar has a 3.92 percentage-point higher no-show probability (SE 1.66 pp; column 1), a 0.144-standard-deviation lower grade among exam takers (SE 0.054; column 2), and a 6.34 percentage-point lower probability of passing the opportunity (SE 2.21 pp; column 3). Flexible calendar controls produce similar performance estimates and a 3.28 percentage-point increase in non-participation. Relative to the 61.9 percent control mean, the baseline decline in passing the scheduled opportunity is 10.2 percent.

Appendix Table A3 shows that the conditional-grade and pass-per-opportunity estimates remain similar across leave-one-tournament-out exclusions. The non-participation estimate remains positive but attenuates when the 2011 or 2014 tournament is excluded. The same table separates Uruguay matches from the other selected games in a joint regression. The conditional-grade estimates are nearly identical (-0.141 and -0.147 standard deviations), while the no-show point

Figure 2: Daily exams and Copa América period in 2004



Notes. The figure shows the number of exam opportunities on each date in 2004. Grey shading denotes the Copa América period. The main treatment used below is narrower: a match in the treatment calendar on the day before the exam.

Table 2: Match timing, exam participation, and performance

Specification	(1) No-show	(2) Grade | attended (std. dev.)	(3) Pass per opportunity
Tournament period (broad benchmark)	0.0053 (0.0091)	−0.0137 (0.0340)	−0.0127 (0.0122)
Match on prior day (baseline)	0.0392^{**} (0.0166)	-0.1441^{***} (0.0539)	-0.0634^{***} (0.0221)
Match on prior day (flexible calendar controls)	0.0328^{**} (0.0156)	-0.1463^{***} (0.0547)	-0.0601^{***} (0.0217)
Observations	172,050	131,615	172,050

Notes. Each coefficient is from a separate regression. The first row estimates equation (1); the remaining rows estimate equation (2). Baseline models include year, semester, exam-period, gender, major, and high-school-location controls. Flexible calendar models replace the separate year and exam-period indicators with year-by-exam-period effects and add day-of-week controls. Standard errors in parentheses are clustered two ways by exam date and subject-by-year. The no-show and pass-per-opportunity samples contain 2,463 date clusters and 5,403 subject-by-year clusters; the grade sample contains 2,439 and 5,347. Pass per opportunity equals one only when the student attends and passes. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

estimate is larger after Uruguay matches (6.24 versus 1.90 percentage points); the differences between the two coefficients are imprecise for all three outcomes.

The conditional-grade estimate need not bound the effect that would arise under compulsory attendance because match timing also changes who sits the exam. Among 162,790 opportunities with an observed pre-exam course grade, the interaction between the prior-day match indicator and standardized course grade in the no-show regression is -0.0146 (SE 0.0073). Thus, the treatment-induced increase in non-participation is larger among students with lower prior performance. This pattern could attenuate the observed conditional-grade decline if course grade captured preparation fully, but it does not identify latent grades for marginal no-shows or establish a formal lower bound.

Pass per opportunity combines participation and performance in an outcome observed for the full opportunity sample. It equals one when a student attends and passes, and zero when the student either does not attend or attends and fails. The 6.34 percentage-point decline therefore measures lower success at the scheduled opportunity without imputing grades for no-shows. Whether this initial loss persists when later opportunities are considered is examined below.

Event time and calendar placebos

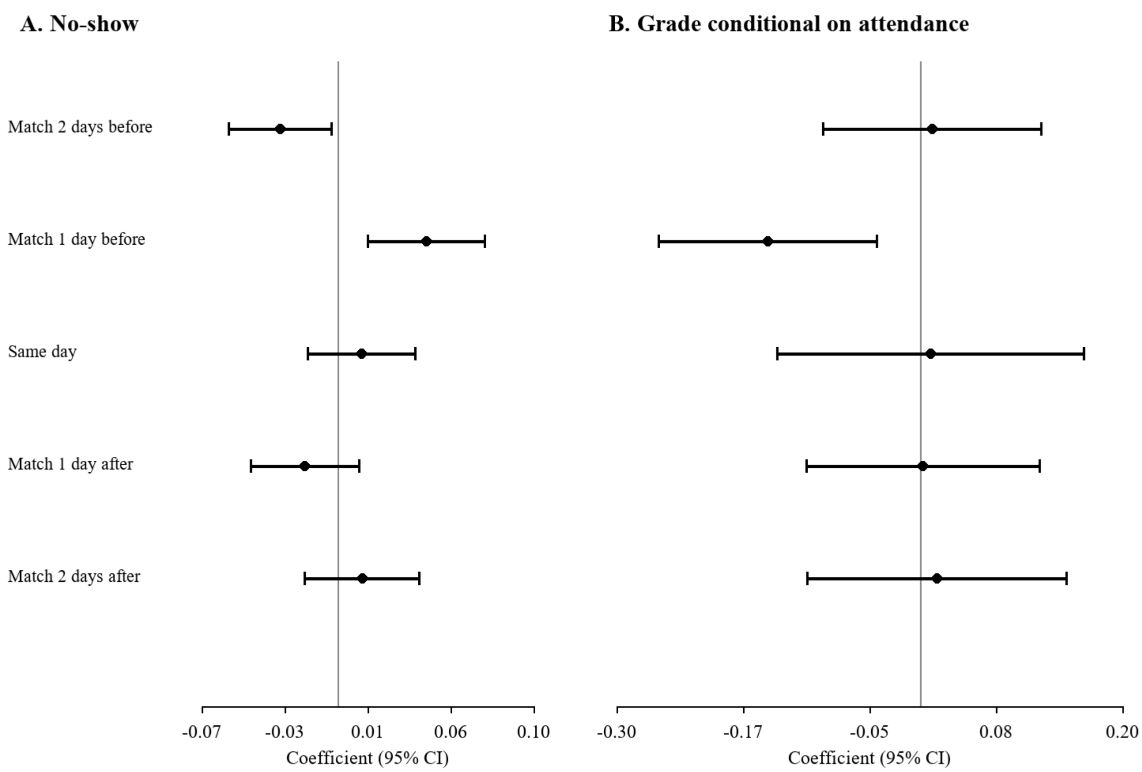
I use two complementary timing checks. The event-time model asks whether the response is concentrated at the horizon implied by last-minute preparation. The seasonal permutation exercises ask how unusual the observed prior-day estimate is relative to alternative match calendars that preserve the examination season.

Figure 3 estimates five relative-day indicators jointly. For conditional grades, the coefficient is -0.150 standard deviations when the match occurs one day before the exam; the other four coefficients are jointly zero (p=0.966). For non-participation, the preceding-day coefficient is positive, but a negative coefficient two days before the exam makes the surrounding pattern less clean (joint p=0.087 for the four placebo days).

Figure 4 compares the observed coefficient with seasonal placebo assignments. The independent-shift design moves each tournament calendar separately within June-July and uses 10,000 assignments. The synchronized design shifts all six tournament calendars together and has 61 possible positions. Both preserve seasonality but encode different exchangeability assumptions, so I report both rather than selecting the more favorable rule. Across synchronized shifts, treated opportunities range from 675 to 7,609 for non-participation and from 550 to 6,199 for grades because exams are unevenly distributed within June–July. Because the synchronized design has only 61 distinct assignments, its empirical p-values also have coarse finite-sample support, with minimum nonzero resolution $1/61$.

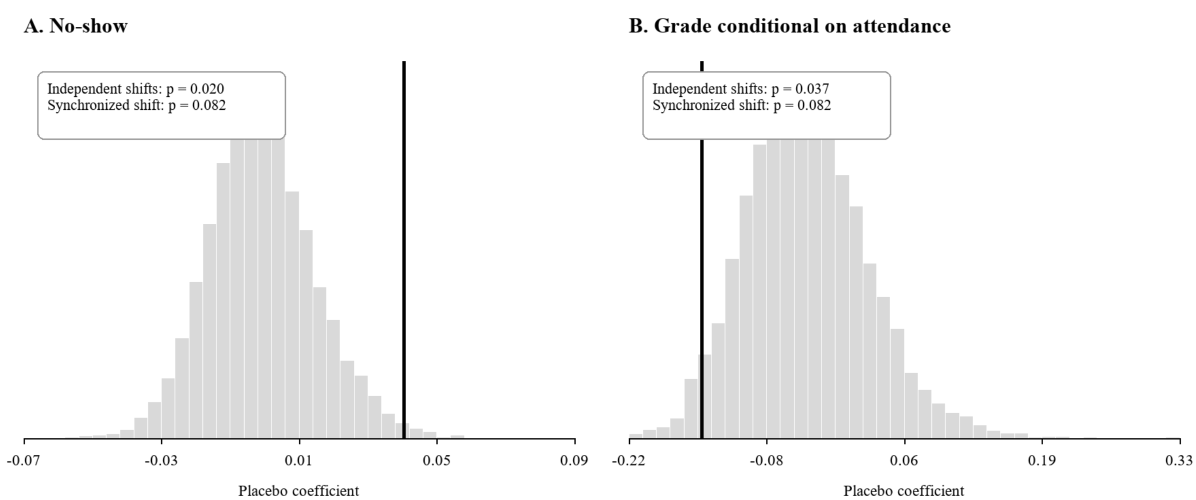
The independent-shift p-values are 0.020 for non-participation and 0.037 for conditional

Figure 3: Joint event-time estimates for match timing and exam outcomes



Notes. The figure reports coefficients and 95 percent confidence intervals from a single regression for each outcome with all five relative-day indicators included jointly. Standard errors are clustered by exam date. The omitted condition is having no match from the treatment calendar in the five-day event window.

Figure 4: Seasonal permutation distributions



Notes. Histograms show coefficients from 10,000 independent within-season shifts; the vertical line is the observed estimate. Text boxes report two-sided empirical p-values for the independent and synchronized reassignment rules. The number of treated opportunities varies across assignments because exams are unevenly distributed within June-July.

grades. Under synchronized shifts, both two-sided p-values are 0.082 (one-sided p=0.049). The observed estimates lie in the expected tails, but the strength of the placebo evidence is sensitive to the reassignment rule.

Student fixed effects

Table 3 reports the student fixed-effects estimates from equation (3) as a within-student robustness check. The specification absorbs persistent individual differences while retaining variation from students whose exam opportunities fall both on treated and untreated dates.

The fixed-effects estimates are close to the cross-sectional results: a 3.55 percentage-point increase in non-participation and a 0.141-standard-deviation decline in conditional grades. The identifying variation comes from 2,292 students, representing 71.8 percent of no-show opportunities, and 1,989 students, representing 64.5 percent of conditional-grade opportunities, who have both treated and untreated observations. The no-show interaction is essentially zero; the grade interaction is -0.068 standard deviations (p=0.053).

Later exam opportunities

Because students who skip an exam can use a later opportunity, the immediate estimates do not show whether the shock merely delays passing or changes the outcome of the observed exam cycle. I therefore reorganize the records into student–course–academic-year–session cycles and define treatment according to whether a match in the treatment calendar was played on the day before the first observed opportunity. This extension changes both the unit of observation and the estimand relative to the opportunity-level analysis.

The full cycle sample reproduces the immediate effects: non-participation at the first opportunity rises by 4.53 percentage points (SE 1.68) and passing falls by 7.02 percentage points (SE 2.27). Later outcomes require additional temporal support because the administrative records end on December 31, 2014. I therefore require the first opportunity to occur at least 365 days before that endpoint, leaving 106,480 cycles, including 3,334 treated first opportunities.

In this support-balanced sample, passing at the first opportunity falls by 5.01 percentage points (SE 2.40) and the probability of a later recorded opportunity rises by 5.12 percentage points (SE 2.39). The passing deficit narrows to 0.49 percentage points at 180 days (SE 1.00) and 0.61 percentage points by the end of the observed cycle (SE 0.96). The sequence is consistent with short-run disruption and delay followed by recovery, but the later-horizon estimates are imprecise. The exact 30-, 90-, and 180-day horizons and the restricted-time outcome were developed during this extension and are reported as exploratory in Appendix Table A4.

Gender heterogeneity

Table 4 reports gender-specific estimates and the corresponding interaction tests. The point estimate for conditional grades is more negative for women, but neither the no-show nor the grade interaction is statistically distinguishable from zero at the 5 percent level.

For non-participation, the estimates are 4.26 percentage points for men and 3.63 for women; the difference is -0.63 percentage points (p=0.679). Conditional grades fall by 0.116 standard deviations for men and 0.169 for women; the difference is -0.052 standard deviations (p=0.192). The ordering is suggestive but does not establish differential effects.

IV Survey evidence on soccer viewing and study behavior

To provide descriptive evidence on students' exposure to soccer and stated study behavior, I conducted a survey during the 2015 Copa América. The questionnaire asked about regular soccer viewing, viewing of Uruguay's recent matches, planned viewing during the tournament, and intended behavior if a national-team match were played the day before an important exam. The survey was fielded in the same School of Business that supplies the administrative records, but one year after the administrative sample ends.

Table 5 shows substantial soccer engagement among both men and women, together with sizeable gender differences in regular viewing. Men report greater engagement with club leagues and planned Copa América viewing, but the national team's recent matches also attract many women who report less regular soccer viewing.

Table 3: Student fixed-effects estimates

Sample / contrast	(1) No-show	(2) Grade ∣ attended (std. dev.)
All students	0.0355**	−0.1414***
	(0.0154)	(0.0543)
Men	0.0362*	−0.1053*
	(0.0204)	(0.0553)
Women	0.0349**	−0.1730***
	(0.0139)	(0.0588)
Women − men	−0.0013	−0.0678*
	[$p = 0.932$]	[$p = 0.053$]
Observations	172,050	131,615

Notes. Models include student fixed effects and year, semester, and exam-period indicators. Standard errors in parentheses are clustered two ways by exam date and student. The no-show sample contains 2,463 date clusters and 4,053 students; the grade sample contains 2,439 and 4,000. Brackets report two-sided p-values for women-minus-men interactions. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 4: Match effects by gender

Outcome	(1) Men	(2) Women	(3) Women − men	(4) p-value
No-show	0.0426**	0.0363**	−0.0063	0.679
	(0.0216)	(0.0147)		
Grade ∣ attended (std. dev.)	−0.1161**	−0.1685***	−0.0524	0.192
	(0.0567)	(0.0581)		

Notes. The table reports gender-specific effects of a match in the treatment calendar on the prior day. Standard errors for the gender-specific estimates are clustered by exam date. The women-minus-men contrasts come from joint regressions, with p-values based on standard errors clustered two ways by exam date and subject-by-year. The no-show sample contains 2,463 date clusters and 5,403 subject-by-year clusters; the grade sample contains 2,439 and 5,347. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 5: Gender differences in soccer viewing and stated study behavior

Outcome	Men	Women	Men − women	p-value	N
Panel A. Soccer viewing and planned study					
Likes to watch soccer	0.957	0.897	0.060	0.002	700
Watched Uruguay vs. Jamaica (index)	0.829	0.678	0.151	< 0.001	701
Watched Uruguay vs. Argentina (index)	0.901	0.791	0.111	< 0.001	701
Watches domestic/international club leagues (index)	0.735	0.365	0.370	< 0.001	700
Knows Uruguay's next opponent	0.945	0.815	0.130	< 0.001	674
Copa América viewing-intensity index	0.841	0.667	0.175	< 0.001	700
Expects to study less because of tournament (index)	0.351	0.234	0.117	< 0.001	697
Panel B. Stated response if Uruguay plays the day before an important exam					
Would watch the entire match for sure	0.591	0.364	0.228	< 0.001	699
Would watch part and continue studying	0.151	0.323	−0.171	< 0.001	700
Would study in advance to watch	0.491	0.454	0.037	0.326	700
Might postpone exam to next period	0.017	0.014	0.003	0.762	700
Would not watch in order to study	0.071	0.166	−0.094	< 0.001	700
Would not watch for another reason	0.037	0.057	−0.020	0.212	700

Notes. The table reports means by gender and two-sided tests of equal means. The analysis uses 712 cleaned questionnaire records after removing confirmed duplicates; outcome-specific sample sizes are smaller because of missing answers or gender. The Jamaica and Argentina indices equal 1 for watching the entire match, 0.5 for part, and 0 otherwise. The club-league index equals 1 for weekly viewing, 0.5 for once or twice monthly, 0.2 for a few times yearly, and 0 for never. The Copa América index equals 1, 0.8, 0.5, 0.2, or 0 according to planned viewing intensity. The expected-study index equals 1 for substantially less study, 0.5 for somewhat less, and 0 otherwise. Panel B entries are binary and respondents could select multiple options.

The viewing indices should not be read as literal viewing rates. For example, the women's mean of 0.791 for the Argentina match combines complete, partial, and no viewing. In the hypothetical exam-day question, 59.1 percent of men and 36.4 percent of women say they would certainly watch the entire match; women more often say they would watch part and continue studying or would not watch in order to study.

These patterns are consistent with national-team matches being a salient leisure activity that can compete with study time. They also show why regular club-soccer interest is not a complete measure of exposure to Uruguay matches. The survey directly documents the national-team component of the administrative treatment, but it does not measure viewing of every additional late-stage game in the treatment calendar.

The survey does not identify the mechanism behind the administrative estimates or establish a planning fallacy. Retrospective viewing and hypothetical exam-day questions refer to different contingencies, and participation and collection mode may be selective. The evidence should therefore be read as descriptive rather than as a causal explanation for gender heterogeneity.

A separate anonymous survey of 21 instructors provides limited institutional evidence. All respondents state that they do not change exam difficulty or grading because Uruguay played the previous day. Eleven believe students study less during a tournament, and fourteen believe students study less when a match occurs the day before the exam. These stated practices and perceptions are informative, but the small survey cannot independently verify unchanged teaching or grading behavior.

V Conclusion

This paper shows that a salient, short-lived increase in the value of leisure can change both whether students use a scheduled exam opportunity and how they perform when they do. An exam held the day after one of the salient tournament matches in the treatment calendar has 3.9 percentage points higher non-participation, grades among exam takers that are 0.14 standard deviations lower, and a 6.3 percentage-point lower probability of passing the scheduled opportunity. The concentration of the grade response at the one-day horizon indicates that the disruption is immediate rather than a general tournament-period effect. Restricting exposure to Uruguay matches preserves the signs and magnitudes.

The ability to postpone the exam is central to interpreting this response. It allows a temporary shock to appear not only in performance among participants, but also in the timing of evaluation—a margin that compulsory-exam settings cannot reveal. The cycle-level extension shows more later recorded opportunities and a substantially smaller estimated passing deficit within 180 days and by the end of the observed cycle. Because these later-horizon estimates are imprecise, they are consistent with delay and recovery but do not establish either full recovery or a persistent loss.

The broader lesson is that institutional flexibility shapes the margin on which a time-use shock becomes visible. When individuals can choose when to be evaluated, performance among participants provides an incomplete account: participation at the scheduled time is itself an outcome. The relative size of these margins will depend on event salience and the cost of delay, but separating them is essential for understanding how short-lived changes in the opportunity cost of time affect observed performance.

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Web Appendix

Opportunity Cost of Time and Academic Performance:

Evidence from Soccer Matches and University Exams

Table A1: Exam scheduling around matches in the treatment calendar

Outcome	(1) Core-window calendar controls	(2) Same-weekday comparison
Any exam scheduled	−0.0105	0.0130
	(0.0448)	(0.0586)
Exam opportunities	0.52	10.66
	(13.43)	(12.27)
Courses scheduled	0.01	0.67
	(0.71)	(0.60)
Students scheduled	−0.40	10.11
	(13.13)	(11.94)
Composition on dates with exams		
Female share	—	−0.0155
		(0.0135)
Course grade	—	−0.013
		(0.156)

Notes. Each entry reports the coefficient on an indicator for the day after a match in the treatment calendar; standard errors are in parentheses. The core-window regressions use daily data from June 20 through August 5 and control for year, week of the examination season, and day of the week ($N = 564$ dates). The same-weekday design compares each candidate date with the same weekday one and two weeks before and after, excluding treated controls ($N = 187$ date comparisons). Composition outcomes are available only for dates with exams ($N = 116$ for female share; $N = 101$ for course grade). The tests assess systematic scheduling responses and cannot rule out isolated calendar changes.

Table A2: Tournament-period estimates across specifications

Specification	(1) No-show	(2) Grade | attended (std. dev.)	(3) Pass per opportunity
Baseline controls	0.0053	−0.0137	−0.0127
	(0.0091)	(0.0340)	(0.0122)
Flexible calendar controls	0.0030	−0.0097	−0.0104
	(0.0089)	(0.0347)	(0.0126)
Student fixed effects	0.0056	−0.0129	−0.0148
	(0.0087)	(0.0365)	(0.0132)

Notes. Each entry reports the coefficient on the tournament-period indicator from equation (1). Baseline models include year, semester, exam-period, gender, major, and high-school-location controls. Flexible calendar models replace the separate year and exam-period indicators with year-by-exam-period effects and add day-of-week controls. Student fixed-effects models include year, semester, and exam-period indicators. Standard errors in parentheses are clustered two ways by exam date and subject-by-year in the first two rows and by exam date and student in the final row.

Table A3: Robustness of prior-day match estimates

Specification	(1) No-show	(2) Grade | attended (std. dev.)	(3) Pass per opportunity
Baseline controls	0.0392^{**}	-0.1441^{***}	-0.0634^{***}
	(0.0166)	(0.0539)	(0.0221)
Flexible calendar controls	0.0328^{**}	-0.1463^{***}	-0.0601^{***}
	(0.0156)	(0.0547)	(0.0217)
Dates with at least 10 opportunities	0.0315^{**}	-0.1379^{**}	-0.0575^{***}
	(0.0156)	(0.0545)	(0.0217)
Exclude special-period code 4	0.0330^{**}	-0.1450^{***}	-0.0597^{***}
	(0.0157)	(0.0546)	(0.0217)
First-semester first period	0.0417^{***}	-0.1653^{***}	-0.0726^{***}
	(0.0146)	(0.0619)	(0.0220)
Exclude 2004 Copa América	0.0395^{**}	-0.1980^{***}	-0.0784^{***}
	(0.0171)	(0.0569)	(0.0224)
Exclude 2006 World Cup	0.0383^{**}	-0.1283^{**}	-0.0572^{**}
	(0.0165)	(0.0551)	(0.0231)
Exclude 2007 Copa América	0.0429^{**}	-0.1296^{**}	-0.0572^{**}
	(0.0170)	(0.0621)	(0.0226)
Exclude 2010 World Cup	0.0338^{*}	-0.1670^{***}	-0.0664^{***}
	(0.0175)	(0.0603)	(0.0241)
Exclude 2011 Copa América	0.0239	-0.1432^{**}	-0.0605^{**}
	(0.0172)	(0.0630)	(0.0260)
Exclude 2014 World Cup	0.0159	-0.1044^{*}	-0.0392^{*}
	(0.0155)	(0.0570)	(0.0229)
Decomposition of the prior-day calendar			
Uruguay match	0.0624^{***}	-0.1406^{*}	-0.0827^{***}
	(0.0163)	(0.0726)	(0.0263)
Other selected match	0.0190	-0.1470^{*}	-0.0465
	(0.0244)	(0.0763)	(0.0329)

Notes. Each entry reports the coefficient on a match on the prior day from equation (2). The first two specifications correspond to Table 2. The third drops dates with fewer than 10 exam opportunities; the fourth excludes the 2,092 records assigned to the administrative special-period slot; the fifth restricts the sample to the first examination period following the first semester. The next rows exclude one tournament year at a time. The final two rows come from a joint regression with mutually exclusive indicators for Uruguay matches and the other selected matches; tests of equality between those coefficients have p-values 0.131, 0.951, and 0.387 for no-show, grade, and pass per opportunity, respectively. All models use the baseline controls unless otherwise indicated. Standard errors in parentheses are clustered two ways by exam date and subject-by-year. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A4: Follow-up after the first observed exam opportunity

Outcome	(1) Effect	(2) Multiway SE	(3) Control mean
Panel A. Full cycle sample: immediate outcomes			
No-show at first opportunity	0.0453^{***}	0.0168	0.1979
Pass at first opportunity	-0.0702^{***}	0.0227	0.6853
Panel B. Complete one-year support			
No-show at first opportunity	0.0296^{*}	0.0163	0.2021
Pass at first opportunity	-0.0501^{**}	0.0240	0.6813
Pass within 30 days	-0.0220	0.0221	0.7193
Pass within 90 days	-0.0199	0.0167	0.8390
Pass within 180 days	-0.0049	0.0100	0.8964
Pass by end of observed cycle	-0.0061	0.0096	0.9132
Later opportunity recorded	0.0512^{**}	0.0239	0.3154
Number of observed opportunities	0.0694^{*}	0.0389	1.4865
Restricted days not passed within 180 days	4.132	2.927	34.430

Notes. Each coefficient is from a separate OLS regression. Panel A contains 117,530 student–course–academic-year–session cycles and 3,975 treated first opportunities. Panel B requires the first opportunity to occur at least 365 days before the December 31, 2014 administrative endpoint; it contains 106,480 cycles and 3,334 treated first opportunities. Treatment equals one when a match in the treatment calendar was played on the day before the first observed opportunity. Opportunities are defined by distinct dates; 182 repeated same-date rows with identical outcomes are collapsed and 11 cycles with contradictory same-date outcomes are excluded. Models include year, semester, exam-period, gender, major, and high-school-location controls. Standard errors are clustered two ways by first exam date and subject-by-year. Restricted days not passed equals days to pass within 180 days and is set to 180 when no pass is observed within that horizon. A sensitivity excluding 321 cycles whose recorded period slots span more than 365 days is nearly identical. The exact horizon sequence and restricted-time outcome are exploratory. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Figure A1: Daily exams and tournament periods for each year, 2003–2014

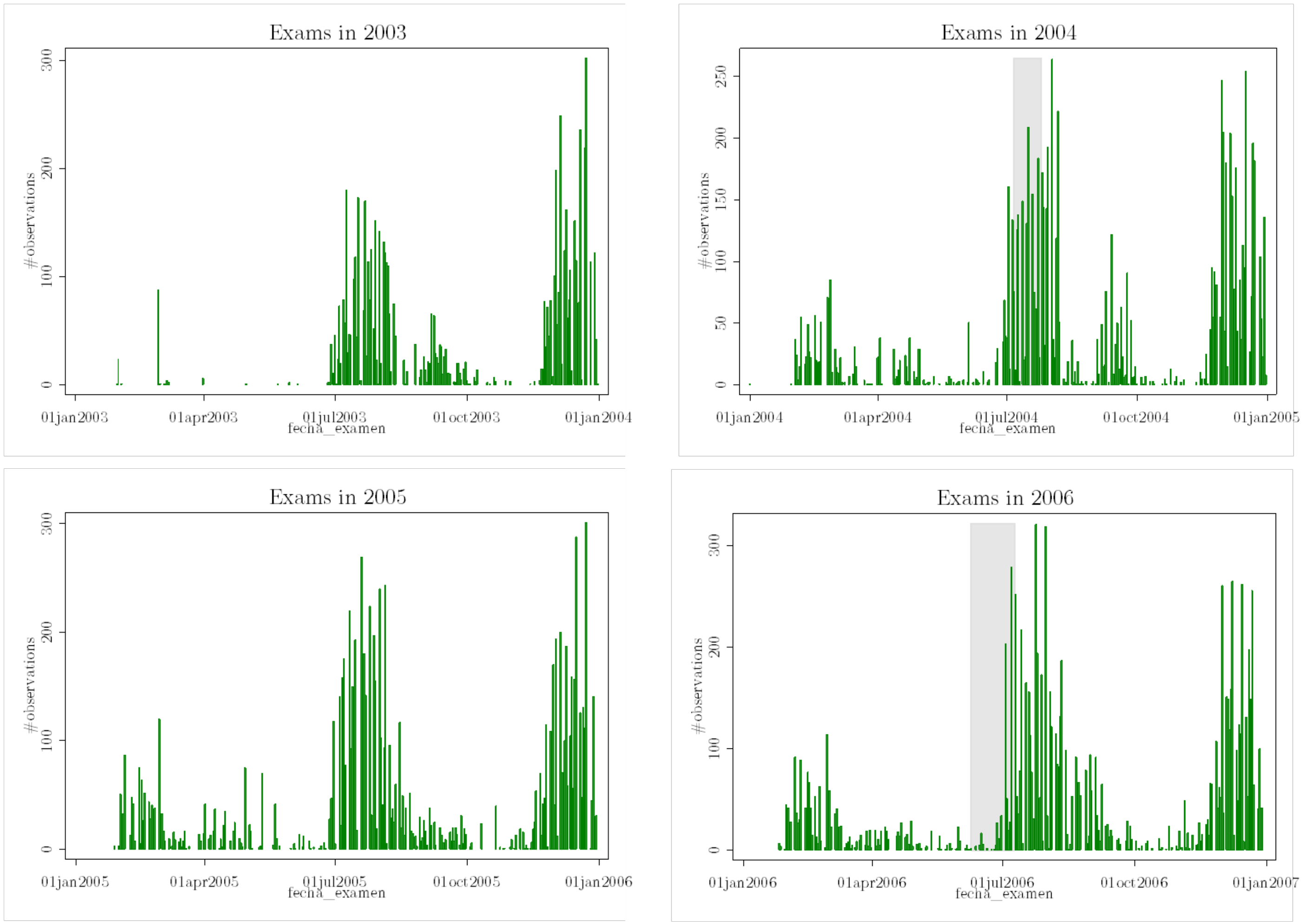

Figure A1 (continued)

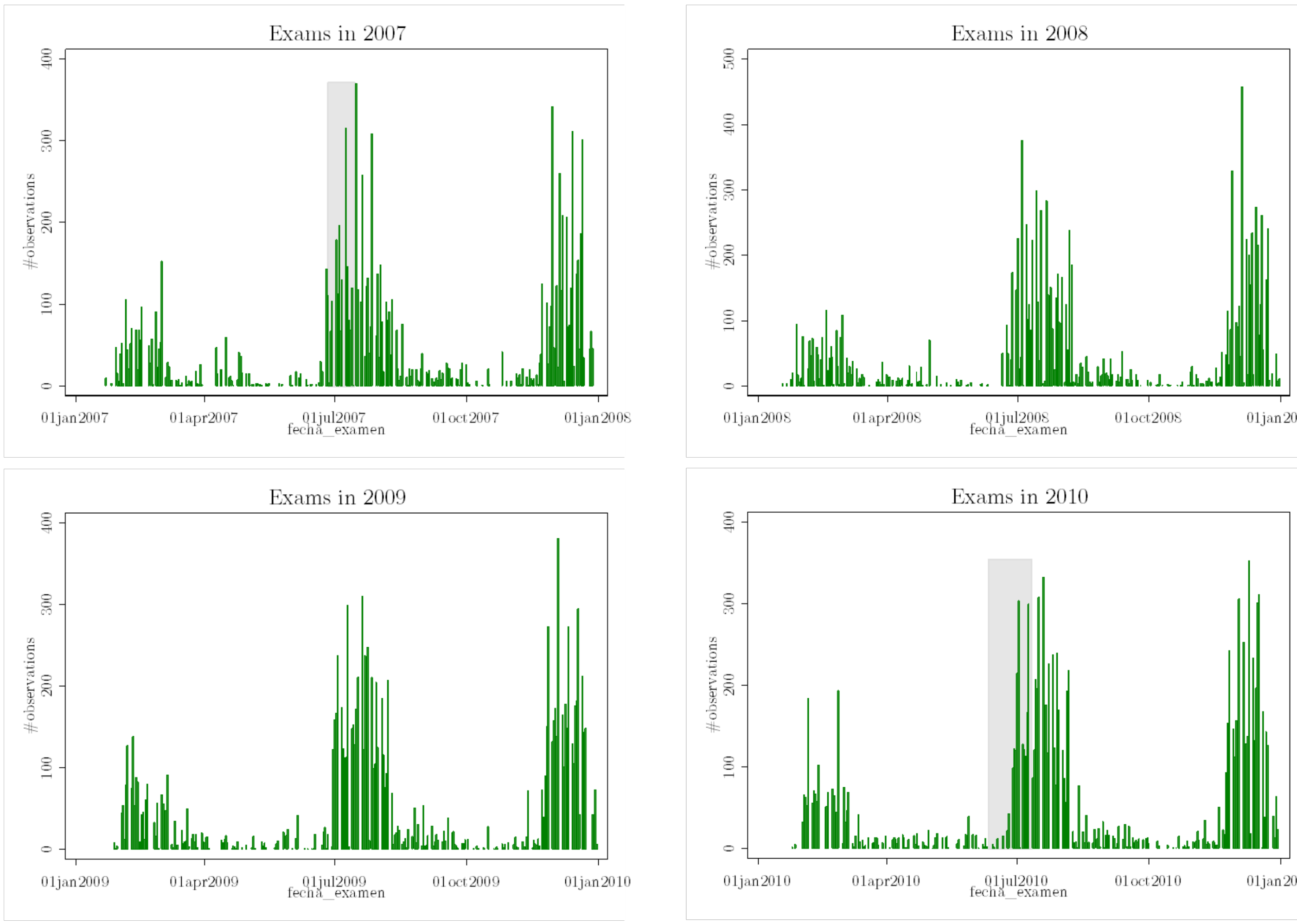

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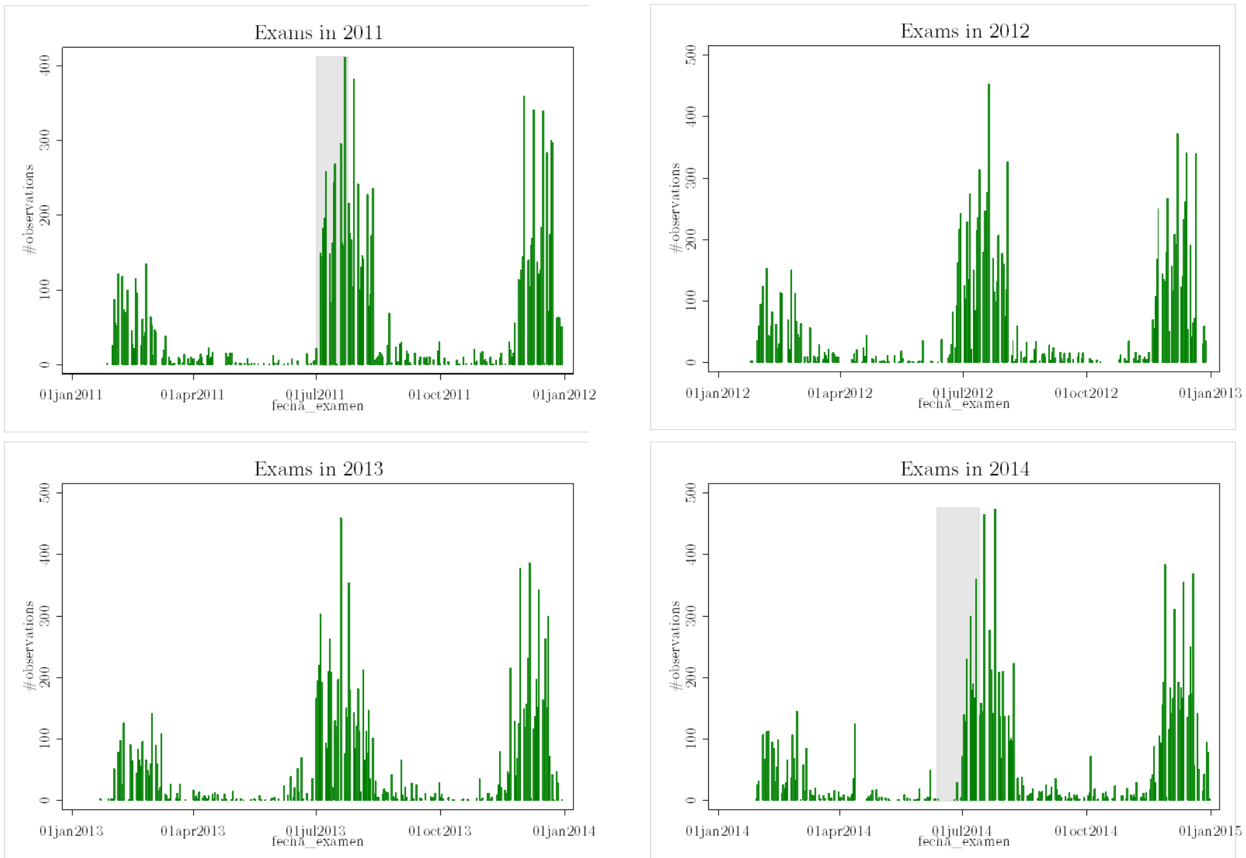
Figure A1 (continued)



Note. The figure shows the daily distribution of exam opportunities across sample years. Grey shading denotes World Cup or Copa América periods. The preferred estimates use the exact alignment between exam dates and matches in the treatment calendar rather than treating the full shaded period as uniformly exposed.