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RESEARCH IN ECONOMIC EDUCATION

Lecture Attendance, Study Time, and Academic Performance: A Panel Data Study

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The authors analyze matched administrative survey data on economics students enrolled in two econometrics courses offered in consecutive terms at a major public university in Spain to assess the impact of lecture attendance and study time on academic performance. Using proxy variables in a cross-sectional regression setting, they find a positive and significant effect of attendance and study time, with a substantially higher return on each additional hour of attendance. However, when panel data first-difference estimators are used to eliminate time-invariant individual-specific unobservables possibly correlated with regressors of interest, the attendance effect disappears, while study time substantially increases its economic impact. These results suggest that study time may be much more important than attendance as a causal determinant of academic performance.

Keywords *academic performance, lecture attendance, panel data, study time*

JEL codes *A22, I21*

Estimating the impact of instructional and study time on student performance in higher education has potentially important policy implications. Educational institutions could allocate their scarce resources more efficiently among different possible modes of instruction and/or courses requiring different amounts of instructional time. Furthermore, students could use this knowledge to improve the efficiency of their time allocation to maximize academic performance. While administrators and faculty of academic institutions intuitively believe that student attendance in the classroom and hours of self-study matter for student achievement, in most institutions this belief is supported only by casual observation or common sense. Even at higher institutional levels, it is often the case that education reforms designed by policy makers to improve university quality by creating better incentives for class attendance and self-study are not supported by strong empirical evidence. As an example of these practices, under the Bologna Process to create

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a European Higher Education Area (EHEA), Spanish universities were required in the late 2000s to introduce an educational model based on a new concept of teaching and evaluation. This has had important implications for the quality and quantity of instructional time (i.e., emphasis on small interactive classes rather than on traditional large lectures) and for the amount of study time required of students (i.e., a substantial increase in weekly hours of self-study because of continuous student assessment). Despite the highly controversial implementation of the EHEA in Spain, little empirical evidence supporting the reform was available at the time of this study.[1]

While the literature analyzing the empirical relationship between educational inputs and learning outcomes has traditionally focused on the extent to which school inputs affect student performance,[2] the focus of attention in more recent studies has shifted to evaluating the role of student inputs in the education production function. In particular, and reflecting the increasing policy relevance of the issue, two branches of this most recent literature have studied the impact of attendance or study time on student performance.

On the one hand, since Romer's (1993) seminal article, a number of cross-sectional studies (see, among others, Durden and Ellis (1995) and Devadoss and Foltz (1996)) have found a significant positive relationship between lecture attendance and academic performance, leading some authors to call for policies to increase or even mandate attendance.[3]

The main threat to the internal validity of these early studies (i.e., the potential endogeneity of attendance) is addressed in more recent studies by research designs (e.g., instrumental variable, experimental, and panel data) that exploit other plausible sources of exogenous variation. Among the studies that use an instrumental variable approach, Krohn and O'Connor (2005) found no relation between attendance and grades. By contrast, other studies found a positive and significant impact of lecture attendance when exploiting an experimental setting. Chen and Lin (2008) randomly assigned some of the course material covered to only one of two sections of the same course, while Dobkin, Gil, and Marion (2010) exploited the discontinuity generated by a mandatory attendance policy for lower scoring students in a midterm exam. Similar findings are also provided by panel data studies (e.g., Marburger 2001 and Lin and Chen 2006) that link students' absence records to teachers' records of the material covered in each class session, and estimate the relationship between the probability of a correct response in multiple-choice question exams and student absence during the corresponding class period. By contrast, panel data studies that exploit within-student between-midterm/subject variation provide mixed results. Whereas Cohn and Johnson (2006) and Stanca (2006), among others, found a positive and significant effect of attendance, Arulampalam, Naylor, and Smith (2012) found such an effect only for high-performing students, and Martins and Walker (2006) and Andrietti (2014) found no effect.

While the mixed nature of this evidence suggests that a causal link between lecture attendance and academic performance has not yet been established, the relationship between study time and performance has received relatively little attention in the literature. In fact (with the exception of Krohn and O'Connor 2005), study time is typically omitted from the education production function specifications adopted by the aforementioned studies. Bratti and Staffolani (2013) argued, based on a simple theoretical model of a student's time allocation, that under plausible assumptions there is an optimal ratio between attendance and study time chosen by students, suggesting that studies that omit study time tend to overestimate the effect of attendance. Two recent studies, however, focused on estimating the causal effects of study time on performance, also taking into account the role of attendance. Stinebrickner and Stinebrickner (2008) exploited the exogenous variation in study time offered by a random roommate assignment policy. Their IV-estimates (almost ten times higher than the OLS-estimates) indicated that the returns to an additional hour of self-study are very large and suggested that endogeneity may be quite severe. Bonesrønning and Opstad (2012) estimated value added models that exploited within-student between-midterm variation. Their findings, although of lesser magnitude, were consistent with those of Stinebrickner and Stinebrickner (2008).

This variety of empirical results, most of which are based on specific course samples and thus are not necessarily generalizable to other higher education institutions, calls for further research aiming on the one hand to identify causality between policy-relevant student inputs and academic performance, and on the other hand, to assess the robustness of the literature findings using data from a wider range of higher education institutions. Our study contributes to both strands of literature by investigating the impact of lecture attendance and study time on academic performance using a a novel panel data set that matches survey data with administrative records on a cohort of students enrolled in two consecutive econometrics courses at a major Spanish public university. The choice of econometrics as the subject to conduct a study like this deserves further explanation. First, to the best of our knowledge, this is among the first studies carried out on a sample of econometrics students. Second, over the last 30 years, econometrics has become a required course in most economics programs, not only in the United States (as acknowledged by Siegfried and Walstad 2014), but also in many EU countries including Spain. Third, and most importantly, the two consecutive econometrics courses considered here were non-elective (i.e., compulsory) and tested very similar types of skills. This allowed us to treat the course-specific student grades as a series of grades drawn from their overall performance distribution and hence to exploit the panel nature of the data. Finally, the similar mathematically oriented nature of the courses, the common mathematical and statistical training shared by the enrolled students in the first two years of their BA programs, and the relatively short time between courses offer plausible support for the crucial assumptions of our panel data analysis that the effects of student-specific unobservables (e.g., ability and motivation) are common across the two econometrics courses, while allowing for some exogenous time variation in academic and personal circumstances that may lead to a reallocation of attendance and study time across courses.

Our empirical strategy has four key components. First, we provide a thorough analysis and discussion of the sample selection issues that may threaten the internal validity of our research design. Second, we use proxy regressions and first-difference (FD) estimators to account for the potential endogeneity of attendance and study time. The latter exploit within-student between-period (i.e., between-subject) variation in lecture attendance and study time, and an assumption on the error term structure, to eliminate time-invariant unobservable heterogeneity potentially correlated with the levels of attendance and study time. Third, we discuss dynamic selection issues and assess the validity of the crucial FD assumption of strict exogeneity of within-student between-period changes in attendance and study time by running a simple regression-based test on our main specification. Moreover, the value-added model we employed to run this test allows us to assess the robustness of our results to a different and economically meaningful specification of the education production function. Finally, we discuss further identification issues that our FD estimator may face, based on the amount of within-student between-period (-subject) variation found in our data.

We report three major results. First, our findings are consistent with the hypothesis that the inclusion of proxy variables in a regression setting is not sufficient to capture all the correlation

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between the regressors of interest and unobservable student traits. Next, failing to control for unobserved heterogeneity leads to overestimation of the causal effect of lecture attendance and underestimation of the causal effect of study time. Once time-invariant unobservable heterogeneity is controlled for by first differencing, lecture attendance does not have a significant impact on student performance. This finding (at odds with most of the findings in the attendance literature,[4] although similar to the results provided by Martins and Walker 2006 and Andrietti 2014) is consistent with a view shared by most instructors: Better students attend lectures more frequently on average, and because of their inherent ability/motivation, they also receive higher grades. By contrast, our finding that once endogeneity is accounted for, study time has a substantially higher positive impact on performance, is consistent with the findings of Stinebrickner and Stinebrickner (2008) and Bonesrønning and Opstad (2012), suggesting that study time may be considered an important causal determinant of academic performance whose impact may be seriously underestimated in OLS-proxy regressions. Finally, the fact that we cannot reject the hypothesis of strict exogeneity of attendance and study time, together with the substantial within-student between-period (-subject) variation that both our regressors of interest display in the data, lend further support to a causal interpretation of our findings.

For one thing, our results suggest that in traditional lecture settings (like those prevalent in Spanish universities before the EHEA reform and still typical in other EU countries such as Italy), the implementation of incentive schemes aimed simply at increasing attendance may have undesirable effects on student learning outcomes. Furthermore, they are consistent with the view[5] that the shift from a traditional lecture setting (like the one analyzed in this study) to an instructional setting requiring higher student effort and active classroom participation (as introduced by the Spanish EHEA reform) may be a more effective policy for improving student learning.

DATA

We collected data on undergraduate economics students enrolled in two econometrics courses offered in the BA programs (*Licenciaturas*) in economics and economics and journalism at Universidad Carlos III de Madrid (UC3M), a major public university in Spain. The courses were scheduled in the spring and fall terms of two consecutive academic years and offered to the same cohort of students. Introduction to Econometrics (IE) was scheduled in the spring term of the 2006–7 academic year as a second-year course. Econometrics I (E1) was scheduled in the fall term of the following academic year (2007–8) as a third-year course. Both courses were non-elective (i.e., compulsory), worth the same number of ECTS credits (7), and structured into parallel sections: There were three sections in the BA program in economics (sections 61–63) and one section in the BA program in economics and journalism (section 66). Students from the BA program in economics were assigned to their section by surname, and they were not typically allowed to switch sections, either within or between academic years.[6] The courses were taught by four different instructors. Although three of the IE sections and two of the E1 sections shared the same subject instructor, none of the sections shared the same instructor across subjects.[7]

What is most relevant for the purpose of our study is that although taught by different instructors, the courses offered to the different sections were centrally coordinated and therefore shared the same organizational structure, lecture content (syllabus and textbook), and final examination.

Each course was delivered in two two-hour lectures per week over 14 weeks, for a total of 56 instructional hours.[8] The lectures were typically scheduled in morning sessions, except for the courses offered to section 63 and the E1 course offered to section 62, which were scheduled in the afternoon.

The final exam offered at the end of each course had a similar format, consisting of a two-hour written test with three problem sets. There were two exam sessions in each academic year: an "ordinary" session at the end of the term (i.e., February for the courses taught in the fall term and June for those taught in the spring term) and an "extraordinary" session in September. To obtain credits for a course offered in a given academic program year, students must be enrolled in that course.[9] Under normal circumstances, enrollment could only be repeated once. Each student therefore had a maximum of four exam sessions potentially available to obtain the corresponding credits for each of these courses.[10]

We measure academic performance, our dependent variable, as the grade obtained in the first examination (either ordinary or extraordinary) taken by the student during the academic year of first-time course attendance. Attendance was monitored in each lecture session. Enrolled students were informed at the start of the course by their instructor that attendance would be recorded throughout the term for research purposes and that absences would not affect their final grade.

A short questionnaire was distributed before the start of each exam session to collect subject-specific data on weekly study hours, subject interest, and teaching evaluation. However, due to coordination failures among survey data collectors, some of the students who took the IE exam and/or the E1 exam were not asked to complete the questionnaire.[11] It is important to note here that the missing survey data is therefore not due to student refusal to complete the questionnaires, but due to mistakes by the survey data collectors. Given the randomness of these errors, we would expect the missing survey data to be unrelated to student observed (and unobserved) traits. Survey data were later matched with administrative student records upon IE enrollment.

We describe the data as having panel properties because these two consecutive econometrics courses tested essentially similar types of skills. This allowed us to consider the grades obtained by each student as a series of grades drawn from their overall performance distribution. The assumption we maintain in this context is that the effects of student-specific unobservables are common across the two econometrics courses. This would be less reasonable if students were taking academically disparate courses or if a considerable length of time separated the courses. Instead, the courses we consider in our study shared a similar mathematical orientation, and the students enrolled in these courses received essentially the same mathematical and statistical training during the first two years of their BA degree. Moreover, the courses were taught across two consecutive semesters of the same calendar year (2007).

Descriptive statistics on administrative and survey variables for the samples used in our empirical analysis are displayed inTables 1, 2, and 3. Besides administrative variables representing time-invariant student characteristics (age, gender, campus area residence, enrollment year, and section), the set of control factors includes time-invariant variables taken from the students' academic records just before IE enrollment (admission score [SAT] and grade point average [GPA] [both measured in percentage], credits earned per year, and average exam sessions attended in previous courses), which we used as proxies for unobservable student ability. Students who completed the survey questionnaires distributed before each exam session reported subject-specific study time (measured by average weekly study hours) as well as teaching evaluation and subject interest (both expressed in a percentage scale). We use these two latter variables in

TABLE 1
Summary Statistics: Students Enrolled in IE, by IE Exam and IE Survey

	IE exam		No IE exam		IE survey		No IE survey	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
IE grade (%)	52.93	17.89			52.82	16.94	53.47	22.14
IE attendance (%)	36.83	31.16	30.40	21.20	38.11	31.94	30.79	26.83
Administrative variables								
Age	21.13**	1.28	21.83	1.68	21.13	1.24	21.14	1.51
Female	0.53**	0.50	0.29	0.46	0.55	0.50	0.45	0.51
Campus area resident	0.12	0.33	0.17	0.38	0.13	0.34	0.07	0.26
Enrollment year	2.32**	0.47	2.03	0.18	2.33	0.47	2.28	0.45
Economics: section 61	0.25	0.43	0.17	0.38	0.26	0.44	0.21	0.41
Economics: section 62	0.27	0.45	0.38	0.49	0.25	0.43	0.38	0.49
Economics: section 63	0.22	0.41	0.29	0.46	0.23	0.42	0.17	0.38
Economics & J.: section 66	0.27*	0.44	0.16	0.37	0.27	0.45	0.24	0.44
Instructor 1	0.78	0.41	0.71	0.46	0.77	0.42	0.83	0.38
Instructor 2	0.22	0.41	0.29	0.46	0.23	0.42	0.17	0.38
Administrative academic records								
SAT (%)	66.23**	11.44	60.85	8.05	66.70	11.94	63.97	8.47
GPA (%)	45.36**	10.52	34.24	8.01	46.61**	10.30	39.48	9.61
Avg. exam sessions attended	1.05	0.42	1.06	0.48	1.06	0.41	0.98	0.46
Credits earned per year	38.98**	6.44	34.58	7.62	39.77**	6.40	35.23	5.26
Survey variables								
IE study time (avg. weekly hours)					2.82	2.07		
IE subject interest (%)					61.53	26.30		
IE teaching evaluation (%)					64.24	23.87		
Observations	166	58	137	29				

Note: Own calculations on IE samples. Statistical significance from a *t*-test of the difference in means among comparison groups reported in columns 1 and 5, respectively.
*Mean difference significant at the 10 percent level. **Mean difference significant at the 5 percent level.

a last robustness check of our main specification as proxies to capture information on academic motivation and on teaching quality as perceived by students, respectively. Moreover, the variation in instructor-specific teaching quality across subjects and sections is controlled for in our empirical analysis through instructor-subject interaction dummies.

Our initial sample includes 224 students enrolled for the first time in the IE course in the spring semester of the 2006–7 academic year. Of these, 166 took the IE exam, and 137 participated in the IE survey administered at the exam. Table 1 compares students who took the IE exam (columns 1 and 2) with those who did not (columns 3 and 4), as well as (among the former) students who did participate in the IE survey (columns 5 and 6) with those who did not (columns 7 and 8). Students who took the IE exam differ significantly from those who did not by age, gender, and enrollment year. As expected, they also have significantly higher SAT, GPA, and credits earned per year. However, the two groups do not differ significantly in terms of attendance or average exam sessions attended. Moreover, the fact that students who did and did not participate in the IE survey do not significantly differ (beyond average GPA and credits earned per year) suggests that survey data is missing at random. Although such a limited evidence of “selection on observables”

TABLE 2
Summary Statistics: Students Enrolled in E1, by E1 Exam and E1 Survey

	E1 exam		No E1 exam		E1 survey		No E1 survey	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
E1 grade (%)	39.60	18.51			37.28**	18.31	45.59	17.97
E1 attendance (%)	22.46**	25.86	8.72	17.64	21.74	26.34	24.31	24.94
IE grade (%)	56.40**	16.60	47.10	18.57	56.03	17.81	57.38	13.17
IE attendance (%)	39.53	32.06	32.29	29.27	36.77	30.94	46.67	34.32
Administrative variables								
Age	22.09	1.27	22.21	1.31	22.04	1.25	22.21	1.35
Female	0.55	0.50	0.50	0.50	0.53	0.50	0.59	0.50
Campus area resident	0.12	0.33	0.11	0.32	0.11	0.31	0.17	0.38
Enrollment year	3.42**	0.50	3.15	0.36	3.40	0.49	3.48	0.51
Economics: section 61	0.28	0.45	0.19	0.40	0.28	0.45	0.28	0.45
Economics: section 62	0.26	0.44	0.29	0.46	0.23	0.42	0.34	0.48
Economics: section 63	0.21	0.41	0.23	0.42	0.19	0.39	0.28	0.45
Economics & J.: section 66	0.25	0.44	0.29	0.46	0.31**	0.46	0.10	0.31
Instructor 2	0.53	0.50	0.48	0.50	0.59*	0.50	0.38	0.49
Instructor 3	0.26	0.44	0.29	0.46	0.23	0.42	0.34	0.48
Instructor 4	0.21	0.41	0.23	0.42	0.19	0.39	0.28	0.45
Administrative academic records								
SAT (%)	67.29	12.35	64.43	9.54	68.10	9.67	65.20	17.54
GPA (%)	48.89**	9.75	39.44	9.04	48.19	9.54	50.72	10.22
Avg. exam sessions attended	1.08	0.42	0.98	0.42	1.05	0.39	1.16	0.47
Credits earned per year	27.77**	3.15	25.58	5.27	27.92	3.22	27.38	2.98
Survey variables								
IE study time (avg. weekly hours)					3.05	2.28		
IE subject interest (%)					60.84	22.12		
IE teaching evaluation (%)					48.04	26.00		
Observations	104	62	75	29				

Note: Own calculations on E1 samples. Statistical significance from a *t*-test of the difference in means among comparison groups reported in columns 1 and 5, respectively.
*Mean difference significant at the 10 percent level. **Mean difference significant at the 5 percent level.

can be addressed through regression methods, it might also signal a "selection on unobservables" issue that could threaten the internal validity of our research design. To investigate this possibility, we estimate two-step selection models of (1) the student decision to take the IE and (2) the student participation to the IE survey. Both models confirm that selection on unobservables should not be an issue in our sample.[12]

Of the 166 students who took the IE exam, 104 also took the E1 exam. As a further step in our sample selection analysis, we therefore compare (in Table 2) students who did take the E1 exam (columns 1 and 2) with those who did not (columns 3 and 4), and among the former, students who did participate in the E1 survey (columns 5 and 6) with those who did not (columns 7 and 8). Students who also took the E1 exam have significantly higher E1 attendance rates, IE grades, GPA, and credits earned per year, but do not seem to differ in terms of IE attendance, SAT, average exam sessions attended, or demographic characteristics from students who did not take the E1 exam. Furthermore, the fact that students who did and did not participate in the E1 survey do not

TABLE 3
Summary Statistics: Balanced Pooled Samples

	Panel A (admin)		Panel B (admin-survey)		Panel C (admin-no survey)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Grade (%)	48.00	19.46	46.90	20.10	49.91	18.26
IE grade	56.40	16.60	55.86	17.54	57.34	15.01
E1 grade	39.60	18.51	37.94	18.54	42.47	18.36
Attendance (%)	30.99	30.29	31.02	30.47	30.95	30.18
IE attendance	39.53	32.06	38.82	31.51	40.76	33.40
E1 attendance	22.46	25.86	23.22	27.47	21.14	23.09
Administrative variables						
Age	21.59	1.36	21.42**	1.27	21.87	1.48
Female	0.55	0.50	0.58	0.50	0.50	0.50
Campus area resident	0.12	0.33	0.12	0.33	0.13	0.34
Enrollment year	2.92	0.70	2.86	0.70	3.03	0.71
Economics: section 61	0.28	0.45	0.29	0.45	0.26	0.43
Economics: section 62	0.26	0.44	0.26	0.44	0.26	0.44
Economics: section 63	0.21	0.41	0.18	0.39	0.27	0.44
Economics & J.: section 66	0.25	0.43	0.27	0.45	0.21	0.41
Instructor 1	.39	.49	.41	.49	.37	.49
Instructor 2	.37	.48	.37	.48	.37	.49
Instructor 3	.13	.34	.13	.34	.13	.34
Instructor 4	.11	.31	.09	.29	.13	.34
Administrative academic records						
SAT (%)	67.30	12.30	68.80**	9.62	64.72	15.7
GPA (%)	48.90	9.73	48.30	9.75	49.87	9.67
Avg. exam sessions attended	1.08	0.41	1.02**	0.37	1.19	0.46
Credits earned per year	33.70	7.36	34.20	7.63	32.90	6.84
Survey variables						
Study time (avg. weekly hours)			2.98	2.15		
IE study time			3.15	2.44		
E1 study time			2.80	1.81		
Subject interest (%)			62.50	24.20		
IE subject interest			64.03	26.09		
E1 subject interest			60.95	22.23		
Teaching evaluation (%)			57.10	25.40		
IE teaching evaluation			66.52	21.49		
E1 teaching evaluation			47.77	25.61		
Observations	208	132	76			

Note: Own calculations on balanced pooled samples. Panel A includes students who took both exams. Panel B includes students who took both exams and participated in both surveys. Panel C includes students who took both exams but did not participate in the IE and/or E1 survey. Statistical significance from a *t*-test of the difference in means between Panel B and Panel C reported in column 3.

*Mean difference significant at the 10 percent level. **Mean difference significant at the 5 percent level.

significantly differ (beyond E1 grades) is also consistent with the missing at random hypothesis for survey data. Again, we find no evidence of selection on unobservables in the selection models estimated for these groups.[13]

Finally, Table 3 reports summary statistics for our balanced pooled samples, comprising students who took both exams. Panel A (columns 1 and 2) includes the full balanced sample with valid information on administrative student records. Panels B (columns 3 and 4) and C (columns 5 and 6) include the subsets of students participating and not participating in the two surveys, respectively. Statistical significance of the difference in means between panels B and C is reported in column 3. Like the previous evidence, this further comparison suggests that overall, sample selection should not be a major issue in our data. Furthermore, the empirical strategy (first-differencing) described in the next section allows us to eliminate any potential selection problem that operates exclusively through time-invariant unobservable characteristics.

A quick look at the figures displayed in Table 3 for panels A and B indicates that both average exam grades and average attendance rates dropped by about 17 percentage points across subjects. While the decline in attendance might be explained by the partial overlapping of some of the materials presented in the two courses, the fact that subject-specific attendance rates in our samples are substantially lower than those reported in the literature may be due to the higher quantitative content of the courses analyzed here. Furthermore, the low attendance rates might be explained by a relatively common behavior among students attending Spanish higher education institutions: Low-ability students enrolled in a university course tend to contemporaneously enroll in "academias," private institutions typically located around the campus area that offer course-specific tutorials with the particular aim of helping students obtain a passing grade. Although not directly captured in our data, this practice might be especially prevalent among students in mathematically oriented subjects and might be reflected in the low attendance rates we observe for both courses. It also suggests that the (potential) issues related to endogeneity of lecture attendance might be even more relevant in this context. Finally, while study time and interest in the subject decrease only slightly from the first period to the second (from 3.15 to 2.80 hours per week, and from 64 percent to 61 percent, respectively), teaching evaluation shows a more substantial drop, from 66 percent to 48 percent. This decline seems to suggest that students found the E1 subject more difficult (as compared to IE) and/or that they benefited less from attending E1 lectures (consistent with the decline in E1 attendance).

EMPIRICAL STRATEGY

We specify and estimate an education production function (EPF)[14]:

$$y_{it} = \beta_0 + \beta_1 x_{i1t} + \beta_2 x_{i2t} + \gamma \mathbf{x}_{i3t} + v_{it}, \quad i = 1, 2, \ldots, nt \quad = 1, 2 \tag{1}$$

where y_{it} is the learning outcome for individual i, measured by the grade obtained in $t = 1$ (IE) or $t = 2$ (E1); x_{i1t} is an academic input, measured by subject-specific lecture attendance; x_{i2t} is a measure of subject-specific student effort, represented by self-reported weekly study hours (study time); $\mathbf{x}_{i3t}$ is a vector of other student inputs including a dummy indicating the second period (E1 subject), section and instructor-subject interaction dummies, demographic characteristics (gender, age, campus area residence, enrollment year), and unobservable student inputs that are potentially correlated with attendance and study time. The error term in equation (1) can therefore be given the following structure:

$$v_{it} = c_i + u_{it}, \tag{2}$$

including a time-invariant student-specific component (c_i) and an idiosyncratic component (u_{it}). In the presence of unobserved confounding factors (c_i), the explanatory variables in equation (1) will be correlated with the error term v_{it}, and the OLS estimator of our parameters of interest will be biased and inconsistent.

Our empirical strategy exploits two different econometric approaches to address the endogeneity issue. The first approach consists in estimating equation (1) by pooled OLS, using appropriate proxy variables for unobservable student inputs. If those proxy variables are closely related to the student specific unobservable traits, as well as ignorable in the educational production equation (i.e., irrelevant, in a conditional mean sense, for explaining grades), then we can hope to mitigate or even eliminate the endogeneity bias. In our analysis, ability is proxied by time-invariant administrative variables measured before the start of the IE course: SAT, GPA, credits earned, and average number of exam sessions attended. Instructor-subject interaction terms are included in all of our specifications to control for instructor-specific teaching quality heterogeneity across subjects and sections. Moreover, as a further robustness check, in our last specification we also control for time-variant survey variables (i.e., subject interest and teaching evaluation), which are meant to measure academic motivation and teaching quality as perceived by students.[15]

Adequate proxy variables may, however, be difficult to obtain in practice, and/or the ones available may not capture all the correlation between unobservable student inputs and our regressors of interest (lecture attendance and study time). A second approach to address the endogeneity issue relies on panel data methods that exploit within-unit overtime variation and an assumption on the decomposition of the error term in equation (2). In our research design, attendance, study time, and grades are monitored across two consecutive semesters for students who took both the IE and the E1 exams. We therefore exploit their within-student between-period (i.e., between-subject) variation to identify the causal effect of lecture attendance and study time on academic performance. In particular, we use balanced panels A (including all students with complete administrative records) and B (including all students with complete matched administrative-survey data) to estimate different specifications of an FD equation:

$$\Delta y_{i2} = \gamma_1 + \beta_1 \Delta x_{i12} + \beta_2 \Delta x_{i22} + \gamma_2 \Delta \mathbf{x}_{i3t} + \Delta u_{i2}, \tag{3}$$

where Δy_{i2} is the within-student between-period (-subject) grade; and Δx_{i12}, Δx_{i22}, and $\Delta \mathbf{x}_{i32}$ represent within-student between-period (-subject) differences in attendance rate, study time, and other time-variant variables taken in the second period, respectively. Besides β_1 and β_2 (our parameters of interest), the parametrization of the FD model in equation (3) includes a constant term γ_1 for the second period (E1) dummy and a vector of coefficients (γ_2) for additional time-variant variables included in our specifications.

With $T = 2$, the FD estimator is numerically equivalent to the fixed-effect (FE) estimator (Wooldridge 2010, 321). Similar to the FE estimator (where time-invariant [fixed] effects are eliminated by subtracting from equation (1) the corresponding equation taken in within-student means), the FD estimator is based on the assumption that c_i in equation (2) represents time-invariant unobserved effects, which are the only ones potentially correlated with regressors, and can be eliminated by first differencing. A further advantage of the FD estimator is that it not only eliminates the problem of correlated individual heterogeneity but also any potential selection problem that operates exclusively through c_i.[16]

The FD estimator is unbiased and consistent as long as the explanatory variables in equation (3) are strictly exogenous, that is,

$$E[\Delta u_{i2}|\Delta \mathbf{x}_{i2}] = 0,$$

where $\Delta \mathbf{x}_{i2}$ is the vector of first differences of all the time-varying explanatory variables, taken in the second period. As suggested by Wooldridge (2010, 325), with $T = 2$ it is possible to test whether equation (3) satisfies the assumption of strict exogeneity by including attendance and/or study time in the second period (E1) in our FD model and then performing a F-test of their statistical significance. The extended FD model is therefore specified as

$$\Delta y_{i2} = \gamma_1 + \alpha_1 \Delta x_{i12} + \alpha_2 \Delta x_{i22} + \delta_1 x_{i12} + \delta_2 x_{i22} + \gamma_2 \Delta \mathbf{x}_{i32} + \Delta u_{i2}, \tag{4}$$

where x_{i12} is E1 attendance, and x_{i22} is E1 study time (i.e., attendance and study time interacted with E1). Failing to reject the null of δ_1 and δ_2 being equal to zero would signal that differencing has apparently solved the endogeneity concern. Incidentally, equation (4) is equivalent to a "value added" (VA) version of the EPF,[17] where the returns of attendance and/or study time are allowed to differ between periods (subjects).[18] Accordingly, this specification allows us to also test the individual significance of the parameters representing differential second period (E1) effects of attendance (δ_1) and/or study time (δ_2).

RESULTS

Our empirical analysis focuses on the two different econometric approaches described in the previous section. First, we estimate alternative specifications of our EPF (1) by pooled OLS.[19]

Table 4 reports the estimates obtained for the pooled balanced samples. All the specifications include a subject dummy (E1, corresponding to period 2) that captures subject-specific grade heterogeneity, and instructor-subject interaction terms to control for instructor-specific teaching quality heterogeneity across subjects and sections. While attendance is always found to have a positive and statistically significant effect on grades, its economic impact varies depending on the specification. Our first specification (1A) is estimated on panel A and includes only administrative controls for students' observable characteristics. The estimated coefficient on attendance indicates that increasing attendance by 10 percentage points would lead to an average grade increase of 1.15 percentage points, all else equal. This same specification as well as additional specifications ([2] to [6], including further subsets of survey and/or administrative proxy variables) are then estimated on panel B. In particular, when estimated on the smaller sample of students with complete administrative-survey data (specification [1]), the effect of attendance increases slightly and preserves its economic and statistical significance. In specification (2), which includes study time but omits attendance, study time is not found to have a statistically significant impact on performance.

The economic and statistical significance of attendance and study time is not altered when both variables are included in specification (3). By contrast, including proxy variables for unobserved ability (in specification [4]) substantially affects the results while confirming the significant impact of unobserved ability proxies such as GPA, credits earned, and average exam sessions attended. For one thing, the estimated effect of attendance is reduced to 0.098 but is still significant at the 5-percent level. Furthermore, the effect of study time increases in magnitude to 1.224 and

TABLE 4
Estimation Results: Pooled OLS

	(1A)	(1)	(2)	(3)	(4)	(5)	(6)
Attendance	0.116**	0.134**		0.136**	0.098**	0.096*	0.089*
	(0.050)	(0.057)		(0.059)	(0.046)	(0.054)	(0.047)
Study time			0.892	0.941	1.224*	1.297*	0.980*
			(0.555)	(0.587)	(0.614)	(0.754)	(0.594)
Instructor 2 × E1	−1.958	3.379	3.938	3.640	3.121	3.072	2.253
	(4.478)	(6.145)	(5.925)	(5.978)	(6.137)	(6.264)	(6.316)
Instructor 3 × E1	0.156	7.558	5.906	7.980	6.636	6.556	7.084
	(5.702)	(8.147)	(7.533)	(7.948)	(7.964)	(8.100)	(7.972)
E1	−14.508**	−22.968**	−22.162**	−22.406**	−26.224**	−26.009**	−26.520**
	(4.159)	(5.493)	(4.929)	(5.349)	(7.481)	(7.587)	(7.892)
SAT					0.154	0.205	0.183
					(0.187)	(0.374)	(0.180)
GPA					0.814**	0.811**	0.770**
					(0.349)	(0.376)	(0.358)
Avg. sessions attended					−8.656*	−8.351	−7.561
					(4.751)	(5.160)	(4.914)
Credits earned					−1.081**	−1.090**	−1.108**
					(0.511)	(0.524)	(0.519)
Attendance × SAT						0.000	
						(0.006)	
Study time × SAT						−0.019	
						(0.083)	
Subject interest							0.123*
							(0.065)
Teaching evaluation							−0.015
							(0.064)
Adj. R^2	0.29	0.36	0.33	0.36	0.43	0.41	0.43
Observations	208	132	132	132	132	132	132

Note: Dependent variable is grade. All specifications include controls for gender, age, campus area residence, enrollment year, and section. Specification (1A) is estimated on Panel A, including students who took both exams. All other specifications are estimated on Panel B, including students who took both exams and participated in both surveys. Clustered standard errors are in parentheses.
*Significant at the 10 percent significance level. **Significant at the 5 percent significance level.

becomes significant at the 5-percent level. The results from this specification suggest that our ability proxies are positively correlated with attendance and negatively correlated with study time, and that excluding them would lead to a substantial bias in the estimated impact of attendance (upward) and study time (downward).

Introducing an interaction term of attendance and study time with SAT (as a proxy for ability, in specification [5]) does not further alter the estimation results. This specification reduces the fit of the model (as measured by the adjusted R^2), and most importantly, we cannot reject the null hypothesis that the interaction terms are individually or jointly not significant.[20] Therefore, we do not consider this specification in the subsequent panel data analysis.

Finally, the inclusion of proxies for academic motivation and teaching quality as perceived by students (in specification [6]) decreases the impact of attendance slightly and the impact of study time more substantially, although both variables remain statistically significant at standard levels. The results emerging from this last specification seem to suggest that students who are more interested in the subject or who experience higher quality teaching, all else equal, attend/study more and tend also to obtain higher grades. However, while the impact of subject interest is found to be statistically significant, teaching evaluation is found to have no impact on student performance. Moreover, this specification does not improve the model fit.

The point estimates resulting from our preferred pooled OLS model (specification [4]) suggest that all else equal, a student attending on average 4 hours per week (corresponding to a 100 percent attendance rate) would obtain on average a score about 4.9 percentage points higher than a student attending on average only 2 hours per week (corresponding to an attendance rate of 50 percent). By contrast, a student who dedicates 4 hours per week to self-study would obtain a grade about 2.45 percentage points higher than a student studying only 2 hours per week. Thus, the estimated models that impose restrictions on the correlation between time-invariant unobservables and our regressors of interest suggest that an additional hour of lecture attendance has much higher grade returns than an additional hour of self-study.

Overall, these findings are consistent with those provided by Romer (1993), among others, in suggesting that ability is positively related to attendance. At the same time, the impact of study time is also sensitive to the inclusion of ability, motivation, and teaching quality proxies. Attempting to control for the effect of unobservable student traits when estimating the effect of attendance and study time on performance then becomes crucial. However, despite the introduction of a set of proxy variables, we may suspect that the estimated relationship still reflects the effect of omitted factors correlated with regressors. We therefore attempt an alternative means of addressing this issue, exploiting the variability of grade, attendance, and study hours in the time (i.e., subject) dimension.

The estimation results based on specifications (1A), (1), (2), and (4) of the FD estimator,[21] reported in Table 5, offer a substantially different picture. In particular, the results of specification (4) show that the impact of lecture attendance is both economically and statistically nonsignificant, while the economic impact of study time increases substantially (from 1.224 [OLS] to 2.160 [FD]) and despite being estimated less precisely, preserves its statistical significance at standard levels. Though these figures might not be significantly different, the overall discrepancy between OLS and FD estimation results is confirmed by the Hausman test of endogeneity.

In specification (6), we add controls for subject-specific interest and teaching evaluation to measure possible changes in academic motivation and teaching quality, respectively. As these variables could potentially be imperfect (endogenous) proxies, they are included only to gauge the extent to which they attenuate the coefficients of interest. This provides a sense of how much of the effect attributed to attendance and study time is due to these factors. These additional controls do little to attenuate the attendance and study time estimated effects. While the effects of attendance and study time remain substantially stable, study time is estimated somewhat less precisely (p-value = .12). Moreover, the fact that these additional controls are not statistically significant suggests that there might be not much unobservable heterogeneity left to control for in the FD model that uses this specification.

Overall, these findings confirm our earlier concern that the proxy variables included in the OLS regressions may not be sufficient to account for the confounding role of time-invariant

TABLE 5
Estimation Results: FD

	(1A)	(1)	(2)	(4)	(4VA)	(6)
Attendance	0.034	0.048		0.012	−0.000	0.010
	(0.072)	(0.097)		(0.103)	(0.114)	(0.106)
Study time			2.189*	2.154*	2.160*	2.000
			(1.108)	(1.182)	(1.198)	(1.271)
Instructor 2 × E1	−1.885	3.577	4.303	4.266	4.353	4.660
	(4.381)	(5.986)	(5.519)	(5.581)	(5.691)	(6.030)
Instructor 3 × E1	−0.971	6.255	6.446	6.615	6.156	6.967
	(5.599)	(7.998)	(7.060)	(7.464)	(7.821)	(7.519)
E1	−14.980**	−20.798**	−21.234**	−21.080**	−21.361**	−20.588**
	(3.762)	(5.061)	(4.402)	(4.559)	(7.160)	(4.630)
Subject interest						0.021
						(0.093)
Teaching evaluation						0.044
						(0.085)
Attendance × E1					0.043	
					(0.096)	
Study time × E1					−0.298	
					(1.252)	
F-test for strict exogeneity: *p*-value					0.87	
Observations	208	132	132	132	132	132

Note: Dependent variable is within student between-period (i.e., -subject) difference in grade. Attendance, study time, subject interest, teaching evaluation, subject, and instructor-subject interactions are within student between-period (-subject) differences. Specification (1A) is estimated on Panel A, including students who took both exams. All other specifications are estimated on Panel B, including students who took both exams and participated in both surveys. VA indicates a value added (VA) version of the original specification. Clustered standard errors are in parentheses.
*Significant at the 10 percent significance level. **Significant at the 5 percent significance level.

unobservables, and together with the results of the Hausman test, question the validity of the OLS estimation and confirm FD as a valid robust estimator.

DISCUSSION AND ROBUSTNESS ANALYSIS

On the basis of the findings reported in the previous section, our answer to the question, "Do lecture attendance and study time improve academic performance?" would be that *only study time does*. This answer is consistent with findings from the literature on causal effects of study time, but it is partially at odds with findings from the literature on causal effects of attendance. Before drawing any firm conclusions, we should therefore discuss the possible issues that may affect our identification strategy.

The most obvious issue lies in the inherent difficulty in using panel data for a study of this nature. Panel data FD estimators are used to eliminate the effect of unobservable variables that differ across individuals but are constant over time. In particular, students who spend more time studying and/or attending lectures may differ in permanent, unobserved ways from students who spend less time studying and/or attending. While this first source of endogeneity would introduce upward bias in the attendance OLS coefficient (under the reasonable assumption that more able, more motivated students attend more), the direction of the bias introduced in the study time OLS coefficient is uncertain from a theoretical standpoint. The bias would be downward if more able students studied less at the same time as less able students systematically compensated for these disadvantages by putting more effort in than more able students, and upward otherwise. Besides being correlated with ability and motivation, attendance and study time might also be correlated with teaching quality. While the latter might be complementary to attendance and student effort, implying that the returns of attendance and study time increase with the quality of teaching, thus introducing upward bias, it also may be the case that not all students respond to a higher quality of teaching by increasing the time allocated to attendance and study. For example, students who benefit more from the quality of teaching (i.e., more able students) may decide to reallocate the time dedicated to a course, increasing their attendance rate while at the same time reducing the amount of study time. In the latter case, this additional source of endogeneity would bias the study time coefficient downward. Summarizing, while proxy pooled OLS regression would likely deliver upwardly biased estimates of the attendance coefficient, the direction of the bias that may affect the study time coefficient ultimately remains an empirical matter.

According to our FD estimates, the attendance pooled OLS coefficient (consistent with our expectation) suffers from an upward bias. By contrast, the study time pooled OLS coefficient suffers from a downward bias. These results are consistent with the idea that more able students attend more, although they also study less than less able students.

However, endogeneity could be also driven by a "dynamic selection effect" that panel data FD estimators are not able to eliminate. In particular, if some unobserved individual traits potentially correlated to the variables of interest vary over time, panel data FD estimators would still be biased because strict exogeneity fails. For example, even after removing time-invariant unobservables, changes in attendance and study time could still be correlated with the time-variant portion of the error term because students may adjust their attendance or study habits across subjects depending either on semester-specific unobservable elements of grades or in reaction to a large grade shock in the first period (i.e., IE subject).

To address this issue, in Table 5 we also consider a VA variant (4VA) of our main specification, as described in the empirical strategy section. While we find that the economic and statistical significance of lecture attendance and study time remain unaltered, the E1 inputs included in this specification (i.e., attendance and study time, referring to the second period) appear neither individually nor jointly significant (p-value = .87), hence not challenging the hypotheses of strict exogeneity and constant effects of the regressors of interest across subjects. Therefore, although in the context of our two-period FD estimation we could not discard the possibility that changes in the time-variant unobservable component may also be correlated with changes in attendance and/or study time between periods (subjects), the aforementioned evidence suggests that these dynamic selection effects should not be important in our setup.

Dynamic selection effects might be particularly relevant in studies that exploit within-student variation across midterms, where on the one hand, students are likely to adjust their study and attendance behavior in response to the results of the midterm, and on the other hand, more myopic students are likely to invest more effort in the second midterm.[22] With sufficiently long panels, more elaborate methods may be used to control for unmeasured variables whose values change over time in specific ways, for example, looking at how the rate of changes in study

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hours or attendance rates affect the rate at which student's outcomes change (obtained by twice-differencing the data) can help control for unmeasured student-level variables that change over time at a constant rate.

While the two-period panel nature of our data prevents us from following this empirical strategy, it is also likely that most of the confounding student-specific traits remain constant over the short period of our data collection (within a calendar year). Moreover, the fact that our panel data are not built from midterm tests should attenuate this potential source of endogeneity. The robustness of our results to the inclusion of subject-specific, albeit possibly imperfect, proxies for academic motivation and teaching quality is also somewhat reassuring. Finally, if some econometrics-specific ability increased relatively more for some students after taking IE, a likely consequence would be a decrease in hours of self-study of more able students on the assumption that they use their available study time more efficiently. By contrast, we would expect that students whose ability increase across subjects would attend more, as they would obtain greater returns on attendance. At the same time, students with low (or no) increments in ability would attend less, given the lower return for them on attending the usual lectures, resulting in a further contribution to the positive covariance between the changes of student ability and attendance. In the presence of this sort of dynamic selection, attendance FD estimates would most likely be biased upward. By contrast, study time FD estimates would be biased downward, thus representing a lower bound on the causal effect of study time and confirming our result interpretation.

Another issue that may affect the internal validity of our research design is sample selection. While some degree of selection on observables was detected in our exploratory descriptive analysis of the relevant samples, we did not find any significant evidence of selection on unobservables by estimating Heckman two-step selection models on the IE and E1 samples. We account for selection on observables controlling for observable heterogeneity in our regressions. Moreover, the FD estimators employed in our empirical analysis allow us to address any selection issue driven exclusively by time-invariant unobservable student traits.

A further (and potentially the most important) reason that could explain the insignificant attendance FD estimate is the possible lack of time-variation within units of observation. We acknowledge that by focusing on within-student variation through FD estimators, we are discarding the between-student variation. This yields standard errors that are considerably higher than those produced by methods that utilize both within- and between-student variation (pooled OLS, RE). However, between-student variation is very likely to be contaminated by unobservable student traits that are correlated with attendance and study time, our regressors of interest. This point is confirmed by the pooled OLS-proxy estimates reported in Table 4. By exploiting only the within-student variation, we eliminate that contamination and are much more likely to obtain unbiased estimates, although at the cost of greater sampling variability and less precise estimates. Nevertheless, under the constraints imposed by our samples and despite the nonstatistical significance of the attendance coefficient, the FD point estimates (under the strict exogeneity assumption) are still unbiased and preserve their economic interpretation, indicating that lecture attendance does not have an impact on student performance.[23]

Nonetheless, as a further attempt to investigate the nature of our results, we present in Table 6 various measures of within-student between-period (-subject) variation in grade, attendance, and study time exploited in our panel data analysis, including overall and within standard deviations of the variables in levels and standard deviations of the variables in differences. The figures in Table 6 show that the within-student between-period (-subject) variation available in both

TABLE 6
Panel Summary Statistics

	Mean	Std. Dev. overall	Std. Dev. within	Min	Max
Panel A					
Grade variables					
Grade (%)	48.00	19.46	12.59	1	92
IE grade	56.40	16.60		1	92
E1 grade	39.60	18.51		3	88
Δ grade	−16.81	18.77		−67	31
Attendance variables					
Attendance (%)	30.99	30.29	15.58	0	100
IE attendance	39.53	32.06		0	100
E1 attendance	22.46	25.86		0	88
Δ attendance	−17.07	26.10		−85.19	40.28
Observations				208	
Panel B					
Grade variables					
Grade (%)	46.90	20.10	13.56	1	92
IE grade	55.86	17.54		1	92
E1 grade	37.94	18.54		3	88
Δ grade	−17.92	20.40		−67	31
Attendance variables					
Attendance (%)	31.02	30.47	15.43	0	96
IE attendance	38.82	31.51		0	96
E1 attendance	23.22	27.47		0	88
Δ attendance	−15.60	26.70		−85.30	40.30
Study time survey variables					
Study time (hours per week)	2.98	2.15	0.98	0	15
IE study time	3.15	2.44		0.50	15
E1 study time	2.80	1.81		0	10
Δ study time	−0.35	1.95		−11	3
Observations	132				

Note: Own calculations on balanced pooled samples. Panel A includes students who took both exams. Panel B includes students who took both exams and participated in both surveys.

samples (panels A and B) should in principle be sufficient to identify with enough precision the impact of attendance on grades using panel data FD (or FE) estimators. Furthermore, the fact that the insignificant attendance FD estimates exploit a within-student variation very similar to the one exploited to obtain the significant study time FD estimates lends further support to our findings.

A final, although no less important, issue is related to the quality of our data and in particular to the measurement error that may affect the attendance and study time variables. As mentioned in the data section, we deal with potential measurement error in the attendance rate variable at the data collection stage through the careful monitoring of attendance in each lecture session. An additional strength of our attendance FD estimate is therefore that it is free from any measurement error attenuation bias. By contrast, study time is self-reported in a survey questionnaire administered before the exams and is therefore likely reported with error. While the most adequate way to reduce measurement errors of this type would be to collect time-use information at multiple points in time,[24] this information was not available in our study. However, if the students in our administrative survey data sample systematically underreported (or overreported) their study time allocation across subjects, the FD estimator would make it possible to eliminate this bias. In any case, given the attenuation bias that a measurement error in self-reported study time would introduce in its estimated coefficient, the estimation results delivered by our FD model could still be considered as lower bounds of the impact of study time on academic performance.

CONCLUSIONS

Although continuous evaluation of student learning is among the principles of the EHEA, evidence about the effect of lecture attendance and study time on academic performance is still lacking for most EU countries. This lack of evidence, mainly due to a lack of adequate data, is a particular problem for Spain, where universities were required to introduce an educational model based on a new concept of teaching and evaluation under EHEA starting in the late 2000s.[25]

Our analysis represents a first step toward filling this gap. Using matched administrative survey data from a major Spanish public university and regression proxy techniques, we find a significant effect of lecture attendance and study time on academic performance, with much higher returns on an additional hour of attendance. However, when we account for time-invariant unobservables possibly correlated with attendance by means of panel data estimators, we find that while the returns on an additional hour of attendance fall to zero, the returns on an additional study hour substantially increase. Importantly, the fact that we cannot reject the hypotheses of strict exogeneity of attendance and study time, together with the substantial within-student between-period (-subject) variation found in the data for both of our regressors of interest, lends further support to a causal interpretation of our findings.

These findings seem to confirm what most instructors recognize: Better students attend lectures more frequently on average and receive higher grades because of their inherent high motivation. Furthermore, they suggest that students with lower unobserved ability and lower grade returns might have to study harder in order to pass exams (which also seems very plausible) and that once time-invariant endogeneity is taken into account, an increase in study time is found to have a substantially more significant impact on student performance.

In this context, the shift to an instructional approach requiring higher student effort and continuous evaluation, as introduced in the recent Spanish EHEA reform, may prove to be more effective in improving student performance. Support for this view also comes from a recent study by Artés and Rahona (2013) that exploited experimental data collected before the EHEA reform at another major public university in Spain to evaluate the potential impact of continuous evaluation (through graded problem sets)[26] on student learning. Their finding of positive and significant grading effects, particularly for weaker students, led them to conclude that the EHEA Spanish reform (which, among other things, creates incentives for the use of graded assignments in small classes as a pedagogical technique for continuous student evaluation as opposed to the final exams and lecture-based large-class formats traditionally used by Spanish universities) is expected to bring large learning gains.

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Finally, our results are based on a sample drawn from a cohort of students taking two subsequent econometrics courses offered at a single public higher education institution in Spain. This limits their external validity (a limitation shared, however, with the vast majority of the studies in this literature) and suggests caution in their interpretation, especially in view of the fact that they are at least partially at odds with many of the findings reported in the attendance literature. Nonetheless, our study, among the first to be conducted in econometrics classes and to focus on a country where a major higher education reform was recently introduced, represents an important contribution toward a better understanding of the causal relationship between student time allocation choices and student achievement. Furthermore, by questioning some of the results provided by the earlier literature, our study calls, at the very least, for further research aiming on the one hand to identify causality between policy-relevant student inputs (e.g., lecture attendance and study time) and academic achievement, and on the other hand to assess the robustness of those findings to data from a wider range of higher education institutions.

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NOTES

1. The only study of which we are aware is Artés and Rahona (2013).
2. See Hanushek (1997) for a critical review.
3. See the Brauer (1994) debate in the summer 1994 issue of the *Journal of Economic Perspectives*.
4. See, among others, Cohn and Johnson (2006) and Stanca (2006).
5. See Artés and Rahona (2013).
6. The randomness of this assignment is important in our research design as it prevents the possibility that students select their instructor and/or course timetable based on their own preferences.
7. Specifically, instructor 1 taught IE61, IE62, and IE66; instructor 2 taught IE63, E161, and E166; and instructors 3 and 4 taught E162 and E163, respectively.
8. Although 14 hours were dedicated to discussing the solutions to homework assignments, the latter were not evaluated, and a standard lecture delivery mode was used.
9. The courses had no prerequisite requirements. However, students could postpone enrollment to a later year.
10. Repeated enrollment in a course and/or use of the extraordinary exam sessions resulted in a penalty to the student's grade point average, which is calculated by the university as a weighted average of all grades obtained, with weights represented by the number of exam sessions attended and the number of credits earned in each course.

11. As a consequence, we do not have survey data for 29 of the 166 students who took the IE exam and for 29 of the 104 students who took the E1 exam. In particular, survey data are missing for 38 of the 104 students who took both exams.
12. The selection models are identified through an exclusion restriction: an S1 exam dummy retrieved from academic student records that takes the value one for students who obtained a passing grade in the Statistics I course scheduled in the first semester of their second year (a similar exclusion restriction was proposed by Becker and Walstad 1990 in their analysis of sample selection problems arising from data loss from pretest to posttest). Estimation results are included in an appendix available upon request.
13. Estimation results are included in an appendix available upon request.
14. See Todd and Wolpin (2003) for a review.
15. Students who registered for the IE and/or E1 exam were asked to report subject-specific interest and give a teaching evaluation immediately before the start of each exam session. As a consequence, these survey variables should be predetermined in the student grades equation. However, it might be the case that subject-specific interest and teaching evaluation reported by students were based on grade expectations and/or on student perceptions of their own ability so that even after controlling for them, the expected effect of teaching quality (or ability) on grades still could change with the levels of attendance and study hours. In any case, it is important to assess the robustness of our main results to an additional specification that includes further subject-specific proxy variables available in our data, despite there is no guarantee that these possibly imperfect proxies allow consistent estimation of our parameters of interest (β_1 and β_2).
16. See Wooldridge (2010).
17. See Todd and Wolpin (2003).
18. Specifically, $\alpha_1 = \beta_{11}$ is the return on attendance in period 1 (IE), $\alpha_2 = \beta_{21}$ is the return on study time in period 1 (IE), $\delta_1 = \beta_{12} - \beta_{11}$ is the differential return on attendance between period 2 (E1) and period 1 (IE), and $\delta_2 = \beta_{22} - \beta_{21}$ is the differential return on study time between period 2 (E1) and period 1 (IE). The standard FD version of the model (i.e., equation 3) is obtained imposing on equation (4) the following restrictions: $\beta_{12} = \beta_{11} = \beta_1$, and $\beta_{22} = \beta_{21} = \beta_2$.
19. Random effects (RE) estimation results are qualitatively similar to the pooled OLS ones. Therefore, they are not reported here and are included in an appendix available upon request.
20. The proxy variable for ability (SAT) used in this specification is defined in difference from its sample mean. Technically, for inferential purposes, this would require an adjustment of the standard errors. However, the adjustment is usually negligible and as suggested by Wooldridge (2010) can be safely ignored.
21. Specification (3) is not reported given that in the FD version of the model, it would be equivalent to specification (4), where all the time-invariant controls are differentiated away.
22. Krohn and O'Connor (2005) found that study time in intermediate macroeconomics was positively correlated with student performance on the midterm and that the students reallocated their study time away from the course if they performed well on the midterm.
23. Even if we were to use the lower standard errors of the most efficient RE estimates to check the statistical significance of the FD estimates, the latter would still remain nonsignificant.
24. See Stinebrickner and Stinebrickner (2004).
25. In the case of UC3M, under the typical EHEA degree program (*grado*), only 50 percent of the teaching schedule follows the traditional lecture format, while the remaining 50 percent of the teaching schedule is dedicated to small group tutorials. Moreover, students are continuously evaluated through graded assignments and frequent midterm exams.
26. Graded problem sets could provide students with a stronger incentive to work early on the material compared to nongraded problem sets, and with regular feedback (the grade) on their level of understanding.

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