Does Class Attendance Affect Academic Performance? Evidence from "D'Annunzio" University

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Abstract

We analyze data from students enrolled in an Introductory Macroeconomics course at "D'Annunzio" University (Italy) in the 2004-2005 academic year to assess the impact of attendance on academic performance. Using "proxy variables" regressions to capture the effect of unobservable factors possibly correlated with attendance, we still find a positive and significant effect of attendance. However, when using panel data fixed effect estimators to eliminate time-invariant individual specific unobservables, the effect disappears. This suggests that the positive effect of attendance commonly reported in the literature may still be capturing the impact of unobservables on academic performance.

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1 Introduction

The economics of education literature has long sought to identify the determinants of student performance in university economics courses.[1] A relatively recent strand of this literature has focused on the role of class attendance in determining student learning outcomes. Since Romer's (1993) seminal article, a number of studies have found positive effects of attendance on performance, leading some authors to call for policies to increase or even mandate class attendance. The extent to which these results are robust and generalizable is not, however, entirely clear, since most studies leave the two main problems usually affecting the attendance rate variable unresolved. First, self-reported attendance rates are likely to contain measurement errors, inducing attenuation bias into estimated coefficients. Second, attendance rates are potentially endogenous, given that students' choice of whether or not to attend lectures is positively affected by unobservable individual characteristics, such as ability, effort, and motivation, which are also likely to have a positive effect on performance. If the latter were the case, the estimated coefficients would suffer from endogeneity (upwards) bias.

This paper addresses both of these issues through the collection of a novel data set that matches survey data with administrative student records. First, careful attendance monitoring in each class session is used to ensure accurate measurement of attendance rates. Second, proxy regressions and panel data estimators are used to account for attendance rate endogeneity. Our findings are consistent with the hypothesis that the inclusion of proxy variables is not sufficient to capture all the correlation between the regressor of interest and unobservable ability, effort, and motivation. The bias correction obtained using "OLS proxy regressions" goes in the expected direction, although the effect of attendance remains positive and significant. However, when we account for time-invariant student unobservables possibly correlated with attendance by means of panel data estimators, we find that class attendance does not have an impact on performance. This finding seems to confirm what most instructors recognize: better students attend lectures more frequently

[1] See Becker et al. (1990), for an overview of this literature.

on average, and because of this inherent motivation, they also receive higher grades. In this context, the implementation of incentive schemes at universities aimed at fostering student attendance may have undesirable effects on student learning outcomes.

The remainder of this work is organized as follows. Section 2 reviews the literature. Section 3 describes the data. Section 4 illustrates the empirical strategy. Section 5 presents and discusses the results. Section 6 concludes.

2 Literature

In his widely cited paper "Do students go to class? Should they?", Romer (1993) provided the first analysis of the relationship between lecture attendance and exam performance.[2] Using attendance records in six sessions of his large ($n = 195$) Intermediate Macroeconomics course, he found that attendance had a positive and significant impact on academic performance. On the basis of these findings, Romer recommended experimenting with mandatory attendance policies to enhance student performance.

Following on Romer's (1993) seminal paper, several studies have attempted to measure the impact of attendance on learning outcomes. Durden and Ellis (1995) used students' self-reported number of absences to explore the relationship between absenteeism and academic achievement in several sections ($n = 346$) of a Principles of Economics course. Controlling for student differences in background, ability, and motivation, they found a nonlinear effect of attendance: while a few absences do not lead to lower grades, excessive absenteeism does. Using data on a sample of about 400 Agricultural Economics students at four large US universities, Devadoss and Foltz (1996) found that, after taking into account motivational and aptitude differences across students, the difference in exam performance between a student with perfect attendance and a student attending only half of the classes was, on

[2] Earlier studies, including McConnell and Lamphear (1969), Paden and Moyer (1969), Buckles and McMahon (1971), Schmidt (1983), Park and Kerr (1990), and Browne et al. (1991), provided conflicting evidence on the effect of attendance among the determinants of academic performance.

average, a full letter grade. A positive and significant relationship between class attendance and academic performance was also found by Chan, Shum, and Wright (1997) and, more recently, by Rodgers (2002).

Among the cross-sectional studies that have reached less robust conclusions about the positive effect of attendance on performance are Douglas and Sulock (1995), Bratti and Staffolani (2002), Dolton, Marcenaro, and Navarro (2003), and Kirby and McElroy (2003). In particular, using a sample of ($n = 371$) first-year Economics students at an Italian university, Bratti and Staffolani (2002) found that the positive and significant effect of class attendance on performance is not robust to the inclusion of self-study hours.

Two recent strands of the literature exploit the availability of richer data sets including repeated observations of the same students' responses to different questions, as well as different students' responses to different questions. The use of panel data models makes it possible to control for time-invariant characteristics of both students and exam questions. In the first of these two strands, Marburger (2001, 2006), Lin and Chen (2006) and Chen and Lin (2008) built original data sets, matching students' absence records with teachers' records of the class sessions when the material corresponding to midterm exam questions was covered. Marburger (2001, 2006) estimated a probit model in which the probability of a student responding incorrectly to each question in a set of multiple-choice questions was related to the student's attendance at the lecture when the relevant material was covered. He found that absenteeism increases the probability of an incorrect response by 7 to 14 percent. Lin and Chen (2006) and Chen and Lin (2008) took a different approach, measuring performance as a dummy variable taking the value one for correct answers and attendance as a dummy variable taking the value one if the student attended the lecture where the question material was covered. Results from probit regressions including fixed effects for students and questions indicate that attendance on a specific day significantly increases the likelihood of responding correctly to a question based on the material covered that day, thus suggesting a positive relationship between attendance and academic performance. In the second strand of the literature, Rodgers (2001, 2003), Cohn and Johnson (2006) and Stanca (2006) simply exploited variation in attendance and academic performance (both measured in percent) over different midterm exams, using panel data estimators to account for time-invariant individual heterogeneity. Their findings indicate that fixed effect estimators are preferable and that attendance has positive and significant effects on performance, ranging from 0.04 to 0.15 percentage points of performance for each additional percentage point of attendance. Following this latter strand of the literature, we estimate the effect of attendance on academic performance using panel data on Introductory Macroeconomics students from an Italian university.

3 Data

The data used in this study were collected by conducting a survey among undergraduate students in Economics attending an Introductory Macroeconomics course at "G. D'Annunzio" University of Chieti and Pescara (Italy) in the fall semester of the 2004-2005 academic year. The course was delivered in three two-hour lectures per week over a twelve-week period to second-year students enrolled in the Economics and Commerce (CLEC) and Environmental Economics (CLEAM) degree programs.[3] Survey data were collected through an initial and a follow-up questionnaire, and later matched with administrative student records. At the beginning of the course, students were told by their instructor that attendance rates would not affect their final grade. Attendance was monitored at the beginning and at the end of each class session. This allows us to construct attendance rates that do not suffer from measurement error, as is the case in most of the previous literature, where attendance rates are either self-reported[4] or taken during a sample period.[5] Enrolled students were offered the opportunity to have their final grade calculated as the average of a first and a second midterm exam, each covering an equal fraction of the course and each carrying the same weight in

[3]The course was also taken by a few students enrolled in other degrees, as well as by third- and fourth-year students.

[4]Durden and Ellis (1995), Stanca (2006)

[5]Romer (1993), Rodgers (2001), Rodgers (2003).

the final grade. The first midterm was held in the seventh week of the course and covered materials taught in weeks 1-6. The second midterm was held in week 14 and covered materials taught in weeks 8-13. Alternatively, students could take a single comprehensive final exam.

In order to work with a more homogeneous sample and – most importantly – to exploit the panel nature of the data, we focused on the students who chose to take midterm exams.[6] The sample used in the empirical analysis is an unbalanced panel of 144 observations, with valid information on midterms scores and individual characteristics.[7] Descriptive statistics are presented in Table 2. Academic performance, our dependent variable, was measured by midterm exam scores. Actual test scores ranged from 0 to 30, with 18 as the passing grade, and were rescaled to the 0-100 range for ease of interpretation and comparability with the results reported in the literature. The overall average rescaled score in the full sample was 78.9 percent, with a significant increase from 77.8 in the first midterm to 85.2 in the second midterm. A typical student attended, on average, 79.9 percent of the classes, a figure that is well above the figures reported in Romer and Stanca (67 and 71 percent, respectively). Average class attendance decreased slightly – from 80.1 to 78.1 percent – over the two midterm periods. Ability was proxied by two indicators of past performance – *high school grade (HSG)* and *grade point average (GPA)* – both measured on a 60-100 points scale, and by the average number of credits completed per year since registration – *CFU per year* – as an indicator of the speed in course work completion. The average *GPA* and *HSG* values – 84.98 and 85.79, respectively – were also considerably higher

[6] If the decision to take midterms rather than a final exam was based on unobservables correlated with the error term in the performance equation, a selection problem would arise. Table 1 reports descriptive statistics for midterms versus final exam takers. Despite the observed differences (notably related to attendance rates and exam scores), we found no evidence of selection on unobservables based on the estimation of a two-step Heckman sample selection model. Estimation results are available upon request.

[7] From a potential balanced panel of 162 observations ($N = 81, T = 2$) based on students taking the first midterm and with nonmissing values in observable characteristics relevant for the empirical analysis, 18 observations were dropped for students that did not take the second midterm. Again, we estimated a Heckman sample selection model for the academic performance observed in the second midterm, and could not reject the hypothesis of no correlation among errors in the performance and selection equations. Estimation results are available upon request.

than those reported by Stanca (76.86 and 77.24, respectively). Effort was proxied by two variables: *hours of self-study during the lecture term* and *hours of self-study during the lecture-free week prior to midterms*, both measured as average weekly hours. As we would expect, the average time that students dedicated to self-study rose from 10.67 hours per week during the lecture period – a similar figure to the one reported by Stanca (10.85) – to 19.92 hours in the lecture-free week prior to midterms. Motivation was measured, on a 0-100 percent scale, by four student self-reported variables – *subject difficulty*, *attendance benefits*, *subject interest*, and *teaching evaluation* – aiming to capture information on the match between academic and student inputs, and therefore the suitability of the student for the subject.[8] On average, students reported a high interest in their evaluations of the subject and the teaching (82 and 89.7 percent, respectively). The set of control factors used in the empirical analysis also includes demographic variables (*age, female, siblings, living away from family*), family background variables (dummies for *parents' education*), and student characteristics (*second year, CLEAM, other courses*) as well as a dummy variable indicating the second midterm.

4 Empirical Strategy

Our goal is to specify and estimate an appropriate education production function (EPF) explaining academic performance in terms of class attendance rate, all other things being equal. According to the EPF approach,[9] a basic learning model can take the following form:

$$y_i = \beta_0 + \beta_1 x_{i1} + \beta_2 \mathbf{x}_{i2} + u_i \qquad i = 1, 2, \ldots, n \tag{1}$$

where y_i is the learning outcome for individual i, measured by academic performance (exam score), x_{i1} is academic input, measured by class attendance, $\mathbf{x}_{i2}$ is a vector of student inputs, and u_i is an error term containing all other factors that affect learning.

[8] See Stanca (2006).

[9] See, among others, Lazear (2001) and Todd and Wolpin (2003).

Student inputs include observable student characteristics, such as study habits and family background, as well as variables that are not directly observable, such as ability, effort, and motivation. Since the latter are all likely to be positively correlated with the students' propensity to attend class and with academic performance, excluding them from the model because of their unobservability would make the OLS estimator of β_1 upwardly biased and inconsistent.

Our empirical strategy exploits two different econometric approaches to account for possible endogeneity of class attendance. The first approach consists in finding appropriate proxy variables for unobservable student inputs. Consider a population model:

$$y_i = \beta_0 + \beta_1 x_{i1} + \beta_2 x_{i2}^* + u_i, \tag{2}$$

in which (x_{i2}^*) is unobserved, and suppose a proxy variable (x_{i2}) is available for x_{i2}^*, where:

$$x_{i2}^* = \delta_0 + \delta_2 x_{i2} + \eta_i. \tag{3}$$

If η_i is uncorrelated with x_{i1}, a plug-in regression of y_i on x_{i1} and x_{i2} would result in unbiased and consistent estimates of the class attendance parameter β_1. In our analysis, ability is proxied by *HSG* and *GPA*, effort by *hours of self-study*, motivation by *subject difficulty level* and *interest, attendance benefits*, and *teaching evaluation*.

Proxy variables may be difficult to find in practice, and/or the ones available may not capture all of the correlation between the unobserved factors (student inputs) and the regressor of interest (class attendance). An alternative possibility to address the potential endogeneity of class attendance is to exploit the variability of attendance and performance within observational units over time. The availability of panel data, where the same n cross-sectional units are observed at two or more time periods, makes it possible, under certain assumptions, to eliminate the effect of unobservable variables that differ across units but are constant over time.

Consider a linear population model of the form:

$$y_{it} = \beta_0 + \beta_1 x_{i1t} + \beta_2 x_{i2} + c_i + u_{it}, \qquad t = 1, 2, \ldots, T \tag{4}$$

where y_{it} denotes the time-varying dependent variable (academic performance); x_{i1t} is the time-varying explanatory variable of interest (class attendance), x_{i2} is a time-invariant regressor, c_i is a time-constant unobserved heterogeneity potentially correlated with the regressors, and u_{it} is the idiosyncratic error component, uncorrelated with (x_{i1t}, x_{i2}, c_i). The fixed effect (FE) estimator is based on the assumption that c_i represents time-invariant effects, potentially correlated with regressors, that can be eliminated by subtraction of the corresponding model for individual means:

$$(y_{it} - \bar{y}_i) = \beta_1(x_{i1t} - \bar{x}_i) + (u_{it} - \bar{u}_i). \tag{5}$$

The FE estimator is unbiased and consistent, as long as the explanatory variables are strictly exogenous. The drawbacks are the lack of efficiency and the loss of all time-constant regressors, either observed and unobserved.

To address these issues, we estimate a random effects (RE) model based on the assumption that the time-invariant unobserved heterogeneity term – c_i – is uncorrelated with the regressors.[10] The RE estimator can be obtained as the OLS estimator from data transformed in quasi-deviations from individual means:

$$y_{it} - \lambda\bar{y}_i = \beta_0(1 - \lambda) + \beta_1(x_{i1t} - \lambda\bar{x}_{i1}) + \beta_2(x_{i2} - \lambda\bar{x}_{i2}) + (\varepsilon_{it} - \lambda\varepsilon_i) \tag{6}$$

where:

$$\lambda = 1 - \left(\frac{\sigma_u^2}{(\sigma_u^2 + T\sigma_c^2)}\right)^{1/2}.$$

Under the assumption of orthogonality between unobserved individual hetero-

[10]Under this assumption, a pooled OLS regression could also be used to obtain a consistent estimator of β_1. However, the pooled estimator would not be efficient, since the composite errors $\varepsilon_{it} = c_i + u_{it}$ are serially correlated due to the presence of c_i in each time period.

geneity and explanatory variables, the RE estimator is consistent and efficient and makes it possible to estimate the effect of observed time-invariant individual characteristics. However, the restriction imposed by the orthogonality assumption also represents the main limitation of the RE estimator. In particular, the choice between FE and RE hinges on the validity of the assumption about the relationship between the unobserved individual heterogeneity and the regressors included in the model.

In the next section, we report and discuss results from estimation of FE and RE models on an unbalanced panel of 144 observations. The assumption of orthogonality between unobserved time-invariant effects and regressors is tested using an Hausman test.[11]

5 Results

Our empirical analysis focused on two different econometric approaches. First, we estimated alternative specifications of our learning model (1) by OLS-proxy regression. Table 3 presents the point estimates for the full sample. Attendance is found to have a positive and statistically significant effect on performance in all models. In the basic univariate specification (column 1), the point estimate indicates that one additional percentage point of class attendance increases test scores by 0.21 percentage points. The addition of a set of controls for individual characteristics (column 2) slightly increases the estimated attendance coefficient to 0.22. Next, we considered how controlling for unobservable factors, such as ability, effort, and motivation, affected the estimated coefficient for attendance. The addition of the set of ability proxy (column 3) slightly reduces the estimated effect of attendance to 0.21. All the ability proxies are found to have a positive and statistically significant effect on academic performance. Moreover, there is a considerable improvement in

[11] The Hausman test is based on the difference between RE and FE estimators. Under the null hypothesis, the unobserved time-invariant effect is uncorrelated with the explanatory variables and RE is consistent as well as efficient. Under the alternative hypothesis, RE is inconsistent. Alternatively, FE is consistent under both hypotheses. A statistically significant difference between the estimators would lead to rejection of the null, suggesting FE as a better choice.

the model's capacity to explain the variation in the dependent variable (R^2 rises from 0.18 to 0.43). Alternatively, introducing only the set of proxies for effort and motivation reduces the effect of attendance on performance to 0.15. However, none of the proxies is now statistically significant at standard levels, and the model R^2 is reduced considerably. Focusing on the complete specification (column 5), where both set of proxies are added, the *attendance* coefficient is reduced to 0.19, while the R^2 rises to 0.46. Given that each lecture represents about 5.5% of total attendance during the midterm,[12] the point estimate resulting from the complete specification implies that the gross return of attending an additional two-hour lecture would on average be a percentage point in terms of midterm score.[13] This also implies that a typical student with a 100% attendance rate would obtain on average a score 9 percentage points higher[14] than a student attending only 50% of the classes, all else equal.

Among the ability proxies, *CFU per year* and *GPA* remain significant at standard levels: an additional percentage point of *GPA* increases performance by about 1 percent, while an additional *CFU per year* increases performance on average by 0.4 percent. Among the proxies for effort and motivation, only *teacher evaluation* is statistically significant at standard levels, each additional percentage evaluation point being on average associated with a 0.28 percent increase in academic performance. Among the remaining control variables, only the dummy indicating the second midterm test – which aims to capture heterogeneity among midterms – and the variable indicating the number of siblings are economically and statistically significant across all specifications.

The results in Table 3 are consistent with the findings of Romer (1993) in suggesting that ability is positively related to both attendance and performance. If this is the case, attempting to control for the effect of unobserved ability becomes crucial when estimating the effect of attendance on performance. Alternatively, the findings indicate that controlling for effort and motivation has a lower impact on the estimated coefficient for attendance. A

[12] 18 two-hours lectures were offered in each of the two midterms.

[13] Stanca (2006) obtained a similar return on attendance (0.91 percentage points) from cross-sectional OLS estimates.

[14] (0.18*0.05)=0.09

different, more plausible interpretation is that, despite the introduction of a set of control variables, the relationship still reflects the effect of omitted factors correlated with regressors. We therefore attempted an alternative means of addressing this issue, exploiting the variability of attendance and performance in the time dimension. We used an unbalanced panel of 144 observations, collected by recording grades on two midterm tests and attendance levels in the related fractions of the course for all the students taking the midterms, to perform a panel data analysis. The results of fixed effects (FE) and random effects (RE) model estimations are reported in Table 4. The RE estimator confirms a positive effect of attendance. In the complete specification, the effect of attendance is slightly reduced to 0.18, but still significant at 5%. However, the results based on the FE estimator, accounting for possible correlation between attendance and time-invariant individual-specific unobservables, show that the impact of class attendance is both economically and statistically not significant. This suggests a persisting positive correlation between unobserved effects and time-varying regressors, even after controlling for ability, effort, motivation, and other individual characteristics. The Hausman test statistic strongly rejects ($p = 0.00$) the null hypothesis of orthogonality between unobservable characteristics and regressors, therefore confirming FE as the valid estimator.

On the basis of these findings, our answer to the question "does class attendance help to improve learning outcomes?" would be negative, and at odds with the results provided by most of the recent literature. Before drawing any firm conclusion, however, we should at least try to understand the possible reasons that may have led our evidence to differ from the results of other recent panel data studies. The most obvious possibility deals with the differences between instructors and testing instruments. The attendance/performance effect is less likely to be exhibited if the instructor is a relatively weak lecturer or tends to follow the book religiously during lectures. It is also less likely to appear if the testing instrument emphasizes material that can be adequately captured simply by reading the textbook (i.e., memorization or lower-level critical thinking), thereby reducing the value-added from class attendance. We address both of these possible causes by looking again at the descriptive statistics reported in Table 2. In particular, we find that the self-reported variables measuring the benefit of attendance and the teacher evaluation are very high (over 90 percent). This should indicate, on the one hand, that students have a high opinion of the instructor and, on the other hand, that students consider lectures to be beneficial in terms of the value-added provided in them.

Another possible cause of our divergent results is sample selection, given that we use a sample that includes only the subset of students that chose to take midterm exams. We addressed this issue as well by estimating sample selection models accounting first for the decision to take the first midterm and then for the decision to take the second midterm, conditional on having taken the first midterm. In both cases, we could not reject the null hypothesis of no correlation among the errors in the performance and selection equations.

Another possibility lies in the inherent difficulty in using panel data for a study of this nature. Panel data are generally used to eliminate the effect of unobservable variables that differ across individuals but are constant over time. This assumption may, however, be unreasonable in our case given that some unobserved effects (i.e., motivation and effort) may vary over time, depending on the result in the first midterm test. Moreover, even proxy variables that were meant to capture such effects – like *study hours*, *teaching evaluation*, and *subject interest*, which are time-varying by definition – were collected only once, and thus had to be treated as time-constant variables.[15] Despite these data limitations, our findings are supported by similar results in Andrietti et al. (2011), after accounting for individual time-variation in study time (and other motivation/effort proxies) in FE estimations.

The last – and perhaps most important – reason that could lead to insignificant FE results would be the lack of time-variation within units of observations:[16] in particular, the within-student variation of attendance

[15]Consistently with the students' time allocation theories posited by Becker et al (1990), Krohn and O'Connor (2005) found that self-reported study hours in intermediate macroeconomics were correlated with their performance on the midterm and that the students re-allocated their study time away from the course if they performed well on the midterm.

[16]Small sample size could be a further cause of imprecise coefficient estimation.

may simply not be large enough (i.e., most students might have similar attendance patterns in the periods before and after the first midterm exam) for identification purposes. A small within-variation of *attendance* might be highly correlated with the FE individual-specific constant terms, and would determine unstable estimates. In order to assess this possibility, we proceed to a further data check. Based on the figures reported in Table 5 on the overall and within standard deviation for our dependent variable (score) and our most relevant independent variable (attendance), it seems that there should be enough variation in the data to identify the impact of attendance on performance using panel data methods. On the basis of the above discussion, we believe that the data used in this paper contain enough information to answer our research question and that our findings – although at odds with most of the previous literature – call at least for further research on the important topic of how class attendance affects academic performance.

6 Conclusions

Although continuous evaluation of student learning is among the principles underlying the European Space of Higher Education (Bologna Process), evidence about the effect of class attendance on academic performance is lacking for most European Union countries. This is partly due to the lack of adequate data and partly due to methodological problems. This analysis represents a first step towards filling this gap. Using new data that combine different sources of information and regression proxy techniques, we find a significant effect of lecture attendance on academic performance. However, when we account for time-invariant student unobservables possibly correlated with attendance by means of panel data estimators, we find that class attendance does not have an impact on performance. This finding, despite the caveats emerging from the discussion in the previous section, confirms what most instructors recognize: better students attend lectures more frequently on average, and receive higher grades because of this inherent motivation. In this context, university policies based on incentive schemes designed to foster student attendance may have neutral or even undesirable effects on student learning. If attendance is correlated with ability and motivation, as our findings suggest, it is unlikely that instructors can improve student achievement by changing the course structure or by establishing mandatory attendance policies. Under this assumption, unmotivated students forced to attend lectures are unlikely to pay attention or participate and therefore gain minimally from such policies. In interpreting our results, it is important to recognize that the sample used here is limited to students taking an Introductory Macroeconomics course during a semester in one instructor's class at a single institution. This suggests caution, especially in view of the fact that the results are at least partially at odds with many of the findings reported in the literature. Different conclusions might be found over time and space, or when alternative statistical approaches are used. Further replication is needed.

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Table 1: Descriptive Statistics. Midterms vs. Final Exam Takers

Variable	Source	Totals ($n = 116$)		Midterms ($n = 81$)		Final ($n = 35$)	
		Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Score (%)	admin	74.04	21.97	79.77	17.14	60.76	26.10
Attendance (%)	survey	59.02	37.13	77.34	23.02	16.63	27.73
Attendance Midterm 1 (%)	survey	60.83	37.32	78.70	22.98	19.46	30.76
Attendance Midterm 2 (%)	survey	56.61	39.01	75.51	26.56	12.86	26.15
Female	admin	0.55	0.50	0.60	0.49	0.43	0.50
Age	admin	23.40	4.20	22.55	2.69	25.34	6.10
Divorced Parents	survey	0.06	0.24	0.06	0.24	0.06	0.23
Siblings	survey	1.45	0.82	1.42	0.82	1.51	0.82
Living Away from Home	survey	0.43	0.50	0.49	0.50	0.28	0.46
Father Graduate	survey	0.15	0.35	0.16	0.37	0.11	0.32
Mother Graduate	survey	0.15	0.36	0.13	0.34	0.20	0.40
Second Year	admin	0.47	0.50	0.59	0.49	0.20	0.40
CLEAM	admin	0.23	0.42	0.23	0.43	0.23	0.43
Other Courses	admin	0.09	0.28	0.06	0.24	0.14	0.35
CFU per Year	admin	15.87	9.91	16.99	9.69	13.27	10.06
High School Grade	admin	83.40	13.45	84.81	13.39	80.11	13.21
Grade Point Average	admin	81.94	14.97	84.54	7.05	75.91	24.23
Hours of Study during Lectures Weeks	survey	9.71	7.46	10.33	7.60	8.26	7.01
Hours of Study in Lecture-Free Weeks	survey	20.17	14.25	20.23	13.77	20.03	15.51
Subject Difficulty Level	survey	75.61	13.92	76.25	12.30	74.14	17.21
Attendance Benefits	survey	91.08	13.19	90.84	14.12	91.63	10.89
Subject Interest	survey	80.51	16.76	81.04	17.11	79.28	16.09
Teaching Evaluation	survey	87.99	12.54	88.75	12.25	86.23	13.19

Table 2: Descriptive Statistics. Unbalanced Panel Pooled Sample

Variable	Mean	Std. Dev.	Min.	Max.	n
Avg. Score Midterms (%)	81.62	15.26	20	100	144
Score Midterm 1 (%)	77.81	18.21	23.33	100	144
Score Midterm 2 (%)	85.24	14.88	33.33	100	126
Attendance Midterms (%)	79.92	21.87	0	100	144
Attendance Midterm 1 (%)	80.12	21.17	0	100	144
Attendance Midterm 2 (%)	78.12	24.18	0	100	144
Female	0.6	0.49	0	1	144
Age	22.48	2.65	20	32	144
Divorced Parents	0.06	0.23	0	1	144
Siblings	1.41	0.8	0	4	144
Live Away from Home	0.5	0.50	0	1	144
Father Graduate	0.15	0.35	0	1	144
Mother Graduate	0.14	0.35	0	1	144
Second Year	0.61	0.49	0	1	144
CLEAM	0.21	0.408	0	1	144
Other Courses	0.06	0.24	0	1	144
CFU per Year	17.16	9.84	0	36	144
High School Grade	85.79	12.85	60	100	144
Grade Point Average	84.98	7.07	71.67	98.89	144
Hours of Study during Lecture Weeks	10.67	7.63	0	36	144
Hours of Study in Lecture-Free Weeks	19.92	13.77	0	60	144
Subject Difficulty Level	76.78	12.14	30	95	144
Attendance Benefits	91.33	14.18	10	100	144
Subject Interest	82.07	16.49	6	100	144
Teaching Evaluation	89.71	11.56	50	100	144

Table 3: Determinants of Academic Performance: OLS Estimates

Independent Variable	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)
Attendance	0.21**	0.22**	0.21**	0.15*	0.19**
	(0.07)	(0.07)	(0.06)	(0.08)	(0.06)
Midterm 2	10.67**	9.83**	8.17**	8.96**	7.74**
	(3.04)	(2.97)	(2.47)	(2.88)	(2.43)
Female		1.88	−1.25	−0.76	−2.25
		(3.18)	(2.67)	(3.30)	(2.81)
Age		−1.02	0.83	−0.28	1.46†
		(0.85)	(0.79)	(0.94)	(0.87)
Divorced Parents		9.44	12.18†	4.23	7.96
		(8.09)	(6.72)	(8.55)	(7.23)
Siblings		−3.68†	−3.37*	−4.36*	−4.01*
		(1.94)	(1.62)	(1.91)	(1.62)
Father Graduate		−4.87	−4.22	−4.15	−4.19
		(4.87)	(4.04)	(4.80)	(4.04)
Mother Graduate		4.33	5.19	−0.94	1.20
		(5.16)	(4.33)	(5.24)	(4.55)
Living Away from Home		3.59	3.11	3.41	2.82
		(3.09)	(2.65)	(3.10)	(2.68)
Second Year		2.07	1.09	0.85	0.30
		(4.70)	(3.91)	(4.64)	(3.95)
CLEAM		−6.73	−9.40*	−7.26†	−10.46**
		(4.12)	(3.60)	(4.19)	(3.66)
Other Courses		−3.65	−9.66†	−2.42	−8.42
		(6.75)	(5.80)	(7.09)	(6.18)
CFU per Year			0.32†		0.40*
			(0.18)		(0.18)
High School Grade			0.34**		0.23
			(0.12)		(0.14)
Grade Point Average			1.03**		1.04**
			(0.20)		(0.20)
Hours of Study in Lecture Weeks				−0.14	−0.25
				(0.24)	(0.21)
Hours of Study in Lecture-Free Weeks				0.00	0.10
				(0.12)	(0.11)
Subject Difficulty Level				0.23	0.17
				(0.14)	(0.12)
Attendance Benefits				0.15	0.03
				(0.14)	(0.12)
Subject Interest				0.16	0.04
				(0.12)	(0.11)
Teaching Evaluation				0.19	0.28*
				(0.15)	(0.13)
Adj. R^2	0.13	0.18	0.43	0.23	0.46

Note: $n = 144$.
Significance levels: (†) $p < 0.10$, (*) $p < 0.05$, (**) $p < 0.01$.
Standard errors of estimated coefficients are reported in brackets.

Table 4: Determinants of Academic Performance. Panel Estimates

Independent Variable	FE	RE (1)	RE (2)
Attendance	-0.02	0.19^{**}	0.18^{*}
	(0.11)	(0.07)	(0.07)
Midterm 2	2.48	6.08^{**}	5.60^{**}
	(1.63)	(2.08)	(1.98)
Hausman test		60.20	46.49
(p−value)		(0.00)	(0.00)

Note: $n = 144$.
Significance levels: (†) $p < 0.10$, (*) $p < 0.05$, (**) $p < 0.01$.
Standard errors of estimated coefficients are reported in brackets.
RE(2) specification includes the same regressors as OLS(5).

Table 5: Panel Descriptive Statistics

Variable	Mean	Std. Dev. Overall	Std. Dev. Within
Score (%)	78.90	19.34	6.08
Attendance (%)	79.92	21.87	7.00