ECONSTOR
*Make Your Publications Visible.*

A Service of

ZBW Leibniz-Informationszentrum Wirtschaft
Leibniz Information Centre for Economics

Braga, Michela; Paccagnella, Marco; Pellizzari, Michele

**Working Paper**

# Evaluating students' evaluations of professors

IZA Discussion Papers, No. 5620

**Provided in Cooperation with:**
IZA – Institute of Labor Economics

*Suggested Citation:* Braga, Michela; Paccagnella, Marco; Pellizzari, Michele (2011) : Evaluating students' evaluations of professors, IZA Discussion Papers, No. 5620, Institute for the Study of Labor (IZA), Bonn,
https://nbn-resolving.de/urn:nbn:de:101:1-201104134190

This Version is available at:
https://hdl.handle.net/10419/51579

**Standard-Nutzungsbedingungen:**

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

**Terms of use:**

*Documents in EconStor may be saved and copied for your personal and scholarly purposes.*

*You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.*

*If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.*

WWW.ECONSTOR.EU

Mitglied der Leibniz-Gemeinschaft

DISCUSSION PAPER SERIES

IZA DP No. 5620

# Evaluating Students' Evaluations of Professors

Michela Braga
Marco Paccagnella
Michele Pellizzari

April 2011

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

# Evaluating Students' Evaluations of Professors

**Michela Braga**
*Università Statale di Milano*

**Marco Paccagnella**
*Bank of Italy*
*and Bocconi University*

**Michele Pellizzari**
*Bocconi University, IGIER,*
*C.F. Dondena Centre and IZA*

Discussion Paper No. 5620
April 2011

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

Any opinions expressed here are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but the institute itself takes no institutional policy positions.

The Institute for the Study of Labor (IZA) in Bonn is a local and virtual international research center and a place of communication between science, politics and business. IZA is an independent nonprofit organization supported by Deutsche Post Foundation. The center is associated with the University of Bonn and offers a stimulating research environment through its international network, workshops and conferences, data service, project support, research visits and doctoral program. IZA engages in (i) original and internationally competitive research in all fields of labor economics, (ii) development of policy concepts, and (iii) dissemination of research results and concepts to the interested public.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

IZA Discussion Paper No. 5620
April 2011

# ABSTRACT

# Evaluating Students' Evaluations of Professors*

This paper contrasts measures of teacher effectiveness with the students' evaluations for the same teachers using administrative data from Bocconi University (Italy). The effectiveness measures are estimated by comparing the subsequent performance in follow-on coursework of students who are randomly assigned to teachers in each of their compulsory courses. We find that, even in a setting where the syllabuses are fixed and all teachers in the same course present exactly the same material, teachers still matter substantially. The average difference in subsequent performance between students who were assigned to the best and worst teacher (on the effectiveness scale) is approximately 43% of a standard deviation in the distribution of exam grades, corresponding to about 5.6% of the average grade. Additionally, we find that our measure of teacher effectiveness is negatively correlated with the students' evaluations: in other words, teachers who are associated with better subsequent performance receive worst evaluations from their students. We rationalize these results with a simple model where teachers can either engage in real teaching or in teaching-to-the-test, the former requiring higher students' effort than the latter. Teaching-to-the-test guarantees high grades in the current course but does not improve future outcomes. Hence, if students are myopic and evaluate better teachers from which they derive higher utility in a static framework, the model is capable of predicting our empirical finding that good teachers receive bad evaluations, especially when teaching-to-the-test is very effective (for example, with multiple choice tests). Consistently with the predictions of the model, we also find that classes in which high skill students are over-represented produce evaluations that are less at odds with estimated teacher effectiveness.

JEL Classification: I20

Keywords: teacher quality, postsecondary education

Corresponding author:

Michele Pellizzari
Department of Economics
Bocconi University
via Roentgen 1
20136Milan
Italy
E-mail: michele.pellizzari@unibocconi.it

* We would like to thank Bocconi University for granting access to its administrative archives for this project. In particular, the following persons provided invaluable and generous help: Giacomo Carrai, Mariele Chirulli, Mariapia Chisari, Alessandro Ciarlo, Alessandra Gadioli, Roberto Grassi, Enrica Greggio, Gabriella Maggioni, Erika Palazzo, Giovanni Pavese, Cherubino Profeta, Alessandra Startari and Mariangela Vago. We are also indebted to Tito Boeri, Giovanni Bruno, Giacomo De Giorgi, Marco Leonardi, Tommaso Monacelli, Tommy Murphy and Tommaso Nannicini for their precious comments. We would also like to thank seminar participants at the Bank of Italy, Bocconi University, London School of Economics, UC Berkeley, Università Statale di Milano and LUISS University. Davide Malacrino provided able research assistance. The views expressed in this paper are solely those of the authors and do not involve the responsibility of the Bank of Italy. The usual disclaimer applies.

# 1 Introduction

The use of anonymous students' evaluations of professors to measure teachers' performance has become extremely popular in many universities around the world (Becker and Watts, 1999). These normally include questions about the clarity of lectures, the logistics of the course, and many others. They are either administered to the students during a teaching session toward the end of the term or, more recently, filled on-line.

From the point of view of the university administration, such evaluations are used to solve the agency problems related to the selection and motivation of teachers, in a context in which neither the types of teachers, nor their levels of effort, can be observed precisely. In fact, students' evaluations are often used to inform hiring and promotion decisions (Becker and Watts, 1999) and, in institutions that put a strong emphasis on research, to avoid strategic behavior in the allocation of time or effort between teaching and research activities (Brown and Saks, 1987).[1]

The validity of anonymous students' evaluations as indicators of teacher ability rests on the assumption that students are in a better position to observe the performance of their teachers. While this might be true for the simple fact that students attend lectures, there are also many reasons to question the appropriateness of such a measure. For example, the students' objectives might be different from those of the principal, i.e. the university administration. Students may simply care about their grades, whereas the university (or parents or society as a whole) cares about their learning and the two (grades and learning) might not be perfectly correlated, especially when the same professor is engaged both in teaching and in grading the exams. Consistently with this interpretation, Krautmann and Sander (1999) show that, conditional on learning, teachers who give higher grades also receive better evaluations, a finding that is confirmed by several other studies and that is thought to be a key cause of grade inflation (Carrell and West, 2010; Weinberg, Fleisher, and Hashimoto, 2009).

The estimation of teaching quality is complicated also becasuse it appears to be uncorrelated with the most common observable teachers' characteristics, such as their qualification or

[1] Although there is some evidence that a more research oriented faculty also improve academic and labor market outcomes of graduate students (Hogan, 1981).

experience (Hanushek and Rivkin, 2006; Krueger, 1999; Rivkin, Hanushek, and Kain, 2005). Despite such difficulties, there is also ample evidence that teachers' quality matters substantially in determining students achievement (Carrell and West, 2010; Rivkin, Hanushek, and Kain, 2005) and that teachers respond to incentives (Duflo, Hanna, and Kremer, 2010; Figlio and Kenny, 2007; Lavy, 2009). Hence, understanding how professors should (or should not) be monitored and incentivized is of primary importance.

In this paper we evaluate the content of the students evaluations by contrasting them with objective measures of teacher effectiveness. We construct such measures by comparing the performance in subsequent coursework of students who are randomly allocated to different teachers in their compulsory courses. For this exercise we use data on a cohort of students at Bocconi University - the 1998/1999 freshmen - who were required to take a fixed sequence of compulsory courses and who where randomly allocated to a set of teachers for each compulsory course. Additionally, the data are exceptionally rich in terms of observable characteristics, in particular they include measures of cognitive ability and family income.[2]

We find that, even in a setting where the syllabuses are fixed and all teachers in the same course present exactly the same material, professors still matter substantially. The average difference in subsequent performance between students who were assigned to the best and worst teacher (on the effectiveness scale) is approximately 15% of a standard deviation in the distribution of exam grades, corresponding to over 2% of the average grade. Moreover, our measure of teaching quality appears to be negatively correlated with the students' evaluations of the professors: in other words, teachers who are associated with better subsequent performance receive worst evaluations from their students. On the other hand, teachers who are associated with high grades in their own exams receive better evaluations.

We rationalize these results with a simple model where teaching is defined as the combination of two types of activities: *real teaching* and *teaching-to-the-test*, the former requiring higher students' effort than the latter. Practically, we think of real teaching as competent presentations of the course material with the aim of making students understand it and master it, and teaching-to-the-test as mere repetition of exam questions and exercises with the aim of

[2]The same data are used in De Giorgi, Pellizzari, and Redaelli (2010).

making students learn how to solve them, even without fully understanding their meaning.

Professors are heterogeneous in their teaching methodology, i.e. in the combination of real teaching and teaching-to-the-test. Grades are the outcome of teaching and are less dispersed the more the professor teaches to the test. The type of the exam defines the effectiveness of teaching-to-the-test. To the one extreme, one can think of an exam as a selection of multiple-choice questions randomly drawn from a given pool. In such a situation, teaching-to-the-test merely consists in going over all the possible questions and memorizing the correct answers. This is a setting in which teaching to the test can be very effective and lead to all students performing very well, regardless of their ability. The other extreme are essays, where there is no obvious correct answer and one needs to personally and originally elaborate on one's own understanding of the course material; in this type of exam teaching-to-the-test is unlikely to be particularly effective.[3]

Students evaluate teachers on the basis of their utility levels, at least when they are asked about their general satisfaction with the course. We assume that student's utility depends positively on expected grades and negatively on effort. Further, we also introduce heterogeneity by assuming that good students face a lower marginal disutility of effort.

This simple model is able to predict our empirical findings, namely that good teachers get bad evaluations. This is more likely to occur with exam types that are more prone to teaching-to-the-test and when low ability students are over-represented. Consistently with these predictions, we also find that the evaluations of classes in which high skill students (identified by their score in the cognitive admission test) are over-represented are more in line with the estimated real teacher quality. Furthermore, the distributions of grades in the classes of the most effective teachers are more dispersed, a piece of evidence that lends support to our specification of the learning function.

There is a large literature that investigates the role of teacher quality and teacher incentives in improving educational outcomes, although most of the existing studies focus on primary and secondary schooling (Figlio and Kenny, 2007; Jacob and Lefgren, 2008; Kane and Staiger, 2008; Rivkin, Hanushek, and Kain, 2005; Rockoff, 2004; Rockoff and Speroni, 2010; Tyler,

[3]Obviously, there are costs and benefits to each type of exam. For example, multiple-choice tests can be marked very quickly compared to essays. In this paper we abstract from these issues.

Taylor, Kane, and Wooten, 2010). The availability of standardized test scores facilitates the evaluation of teachers in primary and secondary schools and such tests are currently available in many countries and also across countries (Mullis, Martin, Robitaille, and Foy, 2009; OECD, 2010). The large degree of heterogeneity in subjects and syllabuses in universities makes it very difficult to design common tests that would allow to compare the performance of students who were exposed to different teachers, especially across subjects. At the same time, the large increase in college enrollment experienced in almost all countries around the world in the past decades (OECD, 2008) calls for a specific focus on higher education, as in this study.[4]

To the best of our knowledge, only three other papers investigate the role of students' evaluations in university, namely Carrell and West (2010), Hoffman and Oreopoulos (2009) and Weinberg, Fleisher, and Hashimoto (2009). Compared to these papers we improve in various directions. First of all, the random allocation of students to teachers in our setting differentiates our approach from that of Hoffman and Oreopoulos (2009) and Weinberg, Fleisher, and Hashimoto (2009), who cannot purge their estimates from the potential bias due to the best students selecting the courses of the best professors. Rothstein (2009) and Rothstein (2010) show that correcting such a selection bias is pivotal to producing reliable measures of teaching quality. The study of Carrell and West (2010) uses data from a U.S. Air Force Academy, while our empirical application is based on a more standard institution of higher education and it is therefore more likely to be generalizable to other settings.[5] Moreover, we also provide a theoretical framework for the interpretation of our results, which is absent in Carrell and West (2010).

More generally, this paper is also related and contributes to the wider literature on performance measurement and performance pay. For example, one concern with students' evalua-

[4]On average in the OECD countries 56% of school-leavers enrolled in tertiary education in 2006 versus 35% in 1995. The same secular trends appear in non-OECD countries. Further, the number of students enrolled in tertiary education has increased on average in the OECD countries by almost 20% between 1998 and 2006, with the US having experienced a higher than average increase from 13 to 17 millions.

[5]Bocconi is a selective college that offers majors in the wide area of economics, management, public policy and law, hence it is likely comparable to US colleges in the mid-upper part of the quality distribution. For example, faculty in the economics department hold PhDs from Harvard, MIT, NYU, Stanford, UCLA, LSE, Pompeu Fabra, Stockholm University. Recent top Bocconi PhD graduates landed jobs (either tenure track positions or post-docs) at the World Bank and the University College of London. Also, the Bocconi Business school is normally ranked in the same range as the Georgetown University McDonough School of Business or the Johnson School at Cornell University in the US and to the Manchester Business School or the Warwick Business School in the UK (see the *Financial Times Business Schools Rankings*).

tions of teachers is that they might divert professors from activities that have a higher learning content for the students (but that are more demanding in terms of students' effort) and concentrate more on classroom entertainment (popularity contests) or change their grading policies. This interpretation is consistent with the view that teaching is a multi-tasking job, which makes the agency problem more difficult to solve (Holmstrom and Milgrom, 1994). Subjective evaluations, which have become more and more popular in modern human resource practices, can be seen as a mean to address such a problem and, given the very limited extant empirical evidence (Baker, Gibbons, and Murphy, 1994; Prendergast and Topel, 1996), our results can certainly inform also this area of the literature.

The paper is organized as follows. Section 2 describes our data and the institutional details of Bocconi University. Section 3 presents our strategy to estimate teacher effectiveness and shows the results. In Section 4 we correlate teacher effectiveness with the students' evaluations of professors. Robustness checks are reported in Section 5. In Section 6 we present a simple theoretical framework that rationalizes our results, while Section 7 discusses some additional evidence that corroborates our model. Finally, Section 8 concludes.

# 2 Data and institutional details

The empirical analysis in this paper is based on data for one enrollment cohort of undergraduate students at Bocconi university, an Italian private institution of tertiary education offering degree programs in economics, management, public policy and law. We select the cohort of the 1998/1999 freshmen for technical reasons, being the only one available in our data where students were randomly allocated to teaching classes for each of their compulsory courses.[6]

In later cohorts, the random allocation was repeated at the beginning of each academic year, so that students would take all the compulsory courses of each academic year with the same group of classmates, which only permits to identify the joint effectiveness of the entire set of

[6]The terms *class* and *lecture* often have different meanings in different countries and sometimes also in different schools within the same country. In most British universities, for example, *lecture* indicates a teaching session where an instructor - typically a full faculty member - presents the main material of the course; *classes* are instead practical sessions where a teacher assistant solves problem sets and applied exercises with the students. At Bocconi there was no such distinction, meaning that the same randomly allocated groups were kept for both regular lectures and applied classes. Hence, in the remainder of the paper we use the two terms interchangeably.

teachers in each academic year.[7] For earlier cohorts the class identifiers, which are the crucial piece of information for our study, were not recorded in the university archives.

The students entering Bocconi in the 1998/1999 academic year were offered 7 different degree programs, although only three of them attracted a sufficient number of students to require the splitting of lectures into more than one class: Management, Economics and Law&Management[8]. Students in these programs were required to take a fixed sequence of compulsory courses for the entire duration of their first two years, for a good part of their third year and, in a few cases, also in their last year. Table 1 lists the exact sequence for each of the three programs that we consider, breaking down courses by the term (or semester) in which they were taught and by subject areas (classified with different colors: red for management, black for economics, green for quantitative subjects, blue for law).[9] In Section 3 we construct measures of teacher effectiveness for the professors of these compulsory courses. We do not consider elective subjects, as the endogenous self-selection of students would complicate the identification.

[INSERT TABLE 1 ABOUT HERE]

Most (but not all) of the courses listed in Table 1 were taught in multiple classes (see Section 3 for details). The number of such classes varied across both degree programs and specific courses. For example, Management was the degree program that attracted the most students (over 85% in our cohort), who were normally divided into 8 to 10 classes for their compulsory courses. Economics and Law&Management students were much fewer and were rarely allocated to more than just two classes. Moreover, the number of classes also varied within degree programs depending on the number of available teachers. For instance, in 1998/99 Bocconi did not have a law department and all law professors were contracted from other nearby universities. Hence, the number of classes in law courses were normally fewer than in other subjects

[7] De Giorgi, Pellizzari, and Woolston (2011) use data for these later cohorts for a study of class size.

[8] The other degree programs were Economics and Social Disciplines, Economics and Finance, Economics and Public Administration.

[9] Notice that Economics and Management share exactly the same sequence of compulsory courses in the first three terms. Indeed, students in these two programs did attend these courses together and made a final decision about their major at the end of the third term. De Giorgi, Pellizzari, and Redaelli (2010) study precisely this choice. In the rest of the paper we abstract from this issue and we treat the two degree programs as entirely separated. In the Appendix we present some robustness checks to justify this approach (see Figure A-2).

(e.g. 4 in Management). Similarly, since the management department was (and still is) much larger than the economics or the mathematics department, courses in the management areas were normally split in more classes than courses in other subjects.

Regardless of the specific class to which students were allocated, they were all taught the same material. In other words, all professors of the same course were required to follow exactly the same syllabus, although some variations across degree programs were allowed (i.e. mathematics was taught slightly more formally to Economics students than Law&Management ones). Additionally, the exam questions were also the same for all students, regardless of their classes. Specifically, one of the teachers in each course (normally a senior person) acted as a coordinator for all the others, making sure that all classes progressed similarly during the term, defining changes in the syllabus and addressing specific problems that might have arisen. The coordinator also prepared the exam paper, which was administered to all classes. Grading was usually delegated to the individual teachers, each of them marking the papers of the students in his/her own class, typically with the help of one or more teaching assistants. Before communicating the marks to the students, the coordinator would check that there were no large discrepancies in the distributions across teachers.

[INSERT TABLE 2 ABOUT HERE]

Table 2 reports some descriptive statistics that summarize the distributions of (compulsory) courses and their classes across terms and degree programs. For example, in the first term Management students took 3 courses, divided into a total of 24 different classes: management I, which was split into 10 classes; private law, 6 classes; mathematics, 8 classes. The table also reports basic statistics (means and standard deviations) for the size of these classes.

Our data cover in details the entire academic history of the students in these programs, including their basic demographics (gender, place of residence and place of birth), high school leaving grades as well as the type of high school (academic or technical/vocational), the grades in each single exam they sat at Bocconi together with the date when the exams were sat. Graduation marks are observed for all non-dropout students.[10] Additionally, all students took a

[10]The dropout rate, defined as the number of students who, according to our data, do not appear to have completed their programs at Bocconi over the total size of the cohort, is just above 10%. Notice that some of these

cognitive admission test as part of their application to the university and such test scores are available in our data for all the students. Moreover, since tuition fees depend on family income, this variable is also recorded in our dataset. Importantly, we also have access to the random class identifiers that allow us to identify in which class each students attended each of their courses.

[INSERT TABLE 3 ABOUT HERE]

Table 3 reports some descriptive statistics for the students in our data by degree program. The vast majority of them were enrolled in the Management program (74%), while Economics and Law&Management attracted 11% and 14%. Female students were generally under-represented in the student body (43% overall), apart from the degree program in Law&Management. About two thirds of the students came from outside the province of Milan, which is where Bocconi is located, and such a share increased to 75% in the Economics program. Family income was recorded in brackets and one quarter of the students were in the top bracket, whose lower threshold was in the order of approximately 110,000 euros at current prices. Students from such a wealthy background were under-represented in the Economics program and over-represented in Law&Management. High school grades and entry test scores (both normalized on the scale 0-100) provide a measure of ability and suggest that Economics attracted the best students, a fact that is confirmed by looking at university grades, graduation marks and entry wages in the labor market.

Data on wages come from graduate surveys that we were able to match with the administrative records. Bocconi runs regular surveys of all alumni approximately one to one and a half years since graduation. These surveys contain a detailed set of questions on labor market experience, including employment status, occupation, and (for the employed) entry wages. As it is common with survey data, not all contacts were successful but we were still able to match almost 60% of the students in our cohort, a relatively good response rate for surveys.[11]

students might have transfered to another university or still be working towards the completion of their program, whose formal duration was 4 years. In Section 5 we perform a robustness check to show that excluding the dropouts from our calculations is irrelevant for our results.

[11]The response rates are highly correlated with gender, because of compulsory military service, and with the graduation year, given that Bocconi has improved substantially over time in its ability to track its graduates. Until the 1985 birth cohort, all Italian males were required to serve in the army for 10-12 months but were allowed to

Finally, we complement our dataset with students' evaluations of teachers. Towards the end of each term (typically in the last week), students in all classes were asked to fill an evaluation questionnaire during one lecture. The questions gathered students' opinions about and satisfaction with various aspects of the teaching experience, including the clarity of the lectures, the logistics of the course, the handiness of the professor and so on. For each item in the questionnaire, students answered on a scale from 0 (very negative) to 10 (very positive) or 1 to 5.

In order to allow students to evaluate their experience without fear of retaliation from the teachers at the exam, such questionnaires are anonymous and it is impossible to match the individual student with a specific evaluation of the teacher.[12] However, each questionnaire reports the name of the course and the class identifier, so that we can attach average evaluations to each class in each course. Figure A-1 in the Appendix shows, as an example, the first page of the evaluation questionnaire used in the academic year 1998-1999.[13]

In Table 4 we present some descriptive statistics of the answers to the evaluation questionnaires. We concentrate on a limited set of items, which we consider to be the most informative and interesting, namely overall teaching quality, lecturing clarity, the teacher's ability to generate interest in the subject, the logistic of the course and workload. These are the same items that we analyze in more details in Section 4. The exact wording and scaling of the questions are reported in Table A-4 in the Appendix.

The average valuation of overall teaching quality is around 7, with a relatively large standard deviation of 0.9 and minor variation across degree programs. Although differences are not statistically significant, professors in the Economics program seem to receive slightly better students' evaluations than their colleagues in Management and, even more, in Law&Management. The same ranking holds for the other measures of teaching quality, namely the clarity of lecturing and the ability to generate interest in the subject. Economics compares slightly worse to the other programs in terms of course logistics

postpone the service if enrolled in full time education. For college students, it was customary to enroll right after graduation.

[12]We are not aware of any university in the world where the students evaluations of their teachers are not anonymized.

[13]The questionnaires were changed slightly over time as new items were added and questions were slightly rephrased. We focus on a subset of questions that are consistent over the period under consideration.

[INSERT TABLE 4 ABOUT HERE]

Some of the evaluation items are, understandably, highly correlated. For example, the correlation coefficient between overall teaching quality and lecturing clarity is 0.89. The course logistics and the ability of the teacher in generating interest for the subject are slightly less strongly correlated with the core measures of teacher quality (around 0.5-0.6). Workload is the least correlated with any other item (all correlation coefficients are below 0.2). The full correlation matrix is reported in Table A-5 in the Appendix.

## 2.1 The random allocation

In this section we present evidence that the random allocation of students into classes was successful. De Giorgi, Pellizzari, and Redaelli (2010) use data for the same cohort (although for a smaller set of courses and programs) and provide similar evidence.

The randomization was (and still is) performed via a simple random algorithm that assigned a class identifier to each student, who were then instructed to attend the lectures for the specific course in the class labeled with the same identifier. The university administration adopted the policy of repeating the randomization for each course with the explicit purpose of encouraging wide interactions among the students.

[INSERT TABLE 5 ABOUT HERE]

Table 5 reports test statistics derived from regressions of the observable students' characteristics (by column) on class dummies and the full interaction of indicators for the degree program and the course (with standard errors clustered at the level of the individual student). The null hypothesis under consideration is the joint significance of the coefficients on the class dummies, which amounts to testing for the equality of the means of the observable variables across classes. Notice that these are very restrictive tests, as it is sufficient to have one unbalanced class to make the test fail. Results show that the F statistics are never particularly high, the highest value being 3.5. In most cases the null cannot be rejected at conventional significance levels. The only exception is residence from outside Milan, which is abnormally low in two Management groups. Four outlier groups in the Economics program (out of the 72

classes that we considered) also seem to have a particularly low presence of female students, while high school grades appear slightly lower than average in 3 classes of the same program. Overall, Table 5 suggests that the randomization was rather successful.

[INSERT FIGURE 1 ABOUT HERE]

Testing the equality of means is not a sufficient test of randomization for continuous variables. Hence, in Figure 1 we compare the distributions of our measures of ability (high school grades and entry test scores) for the entire student body and for a randomly selected class in each program. The figure evidently shows that the distributions are extremely similar and formal Kolmogorov-Smirnov tests confirm the visual impression.

Finally, in Table 6 we provide further evidence that also teachers were randomly allocated to classes, by presenting results of regressions of teachers' observable characteristics on classes' observable characteristics. For this purpose, we estimate a system of 9 seemingly unrelated simultaneous equations, where each observation is a class in a compulsory course. The dependent variables are 9 teachers' characteristics (age, gender, h-index, average citations per year and 4 dummies for academic positions) and the regressors are the class characteristics listed in the rows of the table.[14] The reported statistics test the null hypothesis that the coefficients on each class characteristic are all jointly equal to zero in all the equations of the system. The last row tests the hypothesis that the coefficients on all regressors are all jointly zero in all equations.[15]

[INSERT TABLE 6 ABOUT HERE]

Results show that only for a few class characteristics the correlation with the teachers' observables is significant at conventional statistical levels, such as the share of students who come from an academic high school (*lyceums*) or indicators for classes taught primarily in the early morning or late evening. Overall, the results in Table 6 are broadly consistent with random allocation.

[14]The h-index is a quality-adjusted measure of individual citations based on search results on Google Scholar. It was proposed by Hirsch (2005) and it is defined as follows: *A scientist has index $h$ if $h$ of his/her $N_p$ papers have at least $h$ citations each, and the other $(N_p - h)$ papers have no more than $h$ citations each.*

[15]To construct the tests we use the small sample estimate of the variance-covariance matrix of the system.

# 3 Estimating teacher effectiveness

We use performance data for our students to estimate measures of teacher effectiveness. Namely, for each of the compulsory courses listed in Table 1 we compare the future outcomes of students that attended those courses in different classes, under the assumption that students who were taught by better professors will enjoy better outcomes later on. This approach is similar to the *value-added* methodology that is more commonly used in primary and secondary schools (Goldhaber and Hansen, 2010; Hanushek, 1979; Hanushek and Rivkin, 2006, 2010; Rivkin, Hanushek, and Kain, 2005; Rothstein, 2009) but it departs from its standard version, that uses contemporaneous outcomes and conditions on past performance, since we use future performance to infer current teaching quality.[16]

One most obvious concern with the estimation of teacher quality is the non-random assignment of students to professors. For example, if the best students self-select themselves into the classes of the best teachers, then estimates of teacher quality would be biased upward. Rothstein (2009) shows that such a bias can be substantial even in well-specified models and especially when selection is mostly driven by unobservabes.

We avoid these complications by exploiting the random allocation of students in our cohort to different classes for each of their compulsory courses. For this same reason, we focus exclusively on compulsory courses, as self-selection is an obvious concern for elective subjects. Moreover, elective courses were usually taken by fewer students than compulsory ones, hence they were usually taught in a single class.

We compute our measures of teacher effectiveness in two steps. First, we estimate the conditional mean of future grades (in compulsory courses) of students in each class according to the following procedure. Consider a set of students enrolled in degree program $d$ and indexed by $i = 1, \ldots, N_d$, where $N_d$ is the total number of students in the program. In our application there are three degree programs ($d = \{1, 2, 3\}$): Management, Economics and Law&Management. Each student $i$ attends a fixed sequence of compulsory courses indexed by $c = 1, \ldots, C_d$, where $C_d$ is the total number of such compulsory courses in degree program $d$. In each course $c$ the

[16] For this reason we prefer to use the label *teacher effectiveness* for our estimates. Carrell and West (2010) use our same approach but stick to naming it *value-added*.

student is randomly allocated to a class $s = 1, \ldots, S_c$, where $S_c$ is the total number of classes in course $c$. Denote by $\zeta \in Z_c$ a generic (compulsory) course, different from $c$, which student $i$ attends in semester $t \geq t_c$, where $t_c$ denotes the semester in which course $c$ is taught.

Let $y_{ids\zeta}$ denote the grade obtained by student $i$ in course $\zeta$. To control for differences in the distribution of grades across courses, $y_{ids\zeta}$ is standardized at the course level. Then, for each course $c$ in each program $d$ we run the following regression:

$$y_{ids\zeta} = \alpha_{dcs} + \beta X_i + \epsilon_{ids\zeta} \tag{1}$$

where $X_i$ is a vector of student-level characteristics including a gender dummy, a dummy for whether the student is in the top income bracket, the entry test score and the high school leaving grade. The $\alpha$'s are our parameters of interest and they measure the conditional means of the future grades of students in class $s$: high values of $\alpha$ indicate that, on average, students attending course $c$ in class $s$ performed better (in subsequent courses) than students taking course $c$ in a different class. The random allocation procedure guarantees that the class fixed effects $\alpha_{dcs}$ in equation 1 are purely exogenous and identification is straightforward.[17]

Notice that, since in general there are several subsequent courses $\zeta$ for each course $c$, each student is observed multiple times and the error terms $\epsilon_{ids\zeta}$ are serially correlated within $i$ and across $\zeta$. We address this issue by adopting a standard random effect model to estimate all the equations 1 (we estimate one such equation for each course $c$). Moreover, we further allow cross-sectional correlation among the error terms of students in the same class by clustering the standard errors at the class level. More formally, we assume that the error term is composed of three additive components (all with mean equal zero):

$$\epsilon_{ids\zeta} = \upsilon_i + \omega_s + \nu_{ids\zeta} \tag{2}$$

where $\upsilon_i$ and $\omega_s$ are, respectively, an individual and a class component, and $\nu_{ids\zeta}$ is a purely random term. Operatively, we first apply the standard random effect transformation to the

[17]Notice that in few cases more than one teacher taught in the same class, so that our class effects capture the overall effectiveness of teaching and cannot be attached to a specific person. Since the students' evaluations are also available at the class level and not for specific teachers, we cannot disaggregate further.

original model of equation 1.[18]

In the absence of other sources of serial correlation (i.e if the variance of $\omega_s$ were zero), such a transformation would lead to a serially uncorrelated and homoskedastic variance-covariance matrix of the error terms, so that the standard random effect estimator could be produced by running simple OLS on the transformed model. In our specific case, we further cluster the transformed errors at the class level to account for the additional serial correlation induced by the term $\omega_s$.

Overall, we are able to estimate 230 such fixed effects, the large majority of which are for Management courses.[19]. Descriptive statistics of the estimated $\alpha$'s are reported in Table A-1 in the Appendix.

The second step of our approach is meant to purge the estimated $\alpha$'s from the effect of other class characteristics that might affect the performance of students in later courses but are not attributable to teachers. By definition, the class fixed effects capture all those features, both observable and unobservable, that are fixed for all students in the class. These certainly include teaching quality but also other factors that are documented to be important ingredients of the education production function, such as class size and class composition (De Giorgi, Pellizzari, and Woolston, 2011). A key advantage of our data is that most of these other factors are observable. In particular, based on our academic records we can construct measures of both class size and class composition (in terms of students' characteristics). Additionally, we also have access to the identifiers of the teachers in each class and we can recover a large set of variables like gender, tenure status, and measures of research output. We also know which of the several teachers in each course acted as coordinator. These are the same teacher characteristics that we used in Table 6. Once we condition on all these observable controls,

[18]The standard random effect transformation subtracts from each variable in the model (both the dependent and each of the regressors) its within-mean scaled by the factor $\theta = 1 - \sqrt{\frac{\sigma_v^2}{|Z_c|(\sigma_\omega^2+\sigma_\nu^2)+\sigma_v^2}}$, where $|Z_c|$ is the cardinality of $Z_c$. For example, the random-effects transformed dependent variable is $y_{ids\zeta} - \theta\overline{y}_{ids}$, where $\overline{y}_{ids} = |Z_c|^{-1}\sum_{h=1}^{|Z_c|} y_{idh\zeta}$. Similarly for all the regressors. The estimates of $\sigma_v^2$ and $(\sigma_\omega^2 + \sigma_\nu^2)$ that we use for this transformation are the usual Swamy-Arora, also used by the command *xtreg* in Stata (Swamy and Arora, 1972).

[19]We cannot run equation 1 for courses that have no contemporaneous nor subsequent courses, such as Corporate Strategy for Management, Banking for Economics and Business Law for Law&Management (see Table 1). For such courses, the set $Z_c$ is empty. Additionally, some courses in Economics and in Law&Management are taught in one single class, for example Econometrics (for Economics students) or Statistics (for Law&Management). For such courses, we have $S_c = 1$. The evidence that we reported in Tables 5 and 6 also refer to the same set of 230 classes.

unobservable teaching quality is likely to be the only remaining factor that generates variation in the estimated $\alpha$'s. At a minimum, it should be uncontroversial that teaching quality is by far the single most important unobservable that generates variation in the estimated class effects.

[INSERT TABLE 7 ABOUT HERE]

Thus, in Table 7 we regress the estimated $\alpha$'s on all observable class and teacher characteristics. In column 1 we condition only on class size and class composition, in column 2 only on information about the teachers and in column 3 we combine the two sets of controls. In all cases we weight observations by the inverse of the standard error of the estimated $\alpha$'s to take into account differences in the precision of such estimates. Consistently with previous studies on the same data (De Giorgi, Pellizzari, and Woolston, 2011), we find that larger classes tend to be associated with worse learning outcomes, that classes with more able students, measured with either high school grades or the entry test score, also perform better and that a high concentration of high income students appears to be detrimental for learning. Overall, observable class characteristics explain about 42% of the variation in the estimated $\alpha$'s within degree program, term and subject cells.[20]

The results in column 2 show a non linear relationship between teachers' age and teaching outcomes, which might be rationalized with increasing returns to experience. Also, professors who are more productive in research seem to be less effective as teachers, when output is measured with the h-index. The effect is reversed using yearly citations but it never reaches acceptable levels of statistical significance. Finally, and consistently with the age effect, also the professor's academic position matters, with a ranking that gradually improves from assistant to associate to full professors (other academic positions, such as external or non tenured-track teachers, are the excluded group). However, as in Hanushek and Rivkin (2006); Krueger (1999), we find that the individual traits of the teachers explain slightly more than one third of the variation within degree program, term and subject cells. Overall, the complete set of observable class and teachers' variables explains approximately 57% of the (residual) variation.

Our final measures of teacher effectiveness are the residuals of the regression of the esti-

[20]The Partial R-squared reported at the bottom of the table refer to the R-squared of a partitioned regression where the dummies for the degree program, the term and the subject are partialled out.

mated $\alpha$'s on all the observable variables, i.e the regression reported in column 3 of Table 7. In Table 8 we present descriptive statistics of such measures.

[INSERT TABLE 8 ABOUT HERE]

The overall standard deviation of teacher effectiveness is 0.174. This average is the composition of a larger variation among the courses of the program in Law&Management (0.220) and a slightly more limited variation in Management (0.160) and Economics (0.144). Recall that grades are normalized so that the distributions of the class effects are comparable across courses. Hence, these results can be directly interpreted in terms of changes in outcomes. In other words, the overall effect of increasing teacher effectiveness by one standard deviation is an increase in the average grade of subsequent courses by 0.174 standard deviations, roughly 0.6 of a grade point or 2.3% over the average grade of approximately 26.[21] Given an estimated conditional elasticity of entry wages to GPA of 0.45, such an effect would cost students slightly more than 1% of their average entry monthly wage of 967 euros, almost 10 euros per month or 120-130 euros per year.[22] Since in our data we only observe entry wages, it might well be that the long term effects of teaching quality are even larger.

In Table 8 we also report the standard deviations of teacher effectiveness of the courses with the least and the most variation to show that there is substantial heterogeneity across courses. Overall, we find that in the course with the highest variation (management I in the Economics program) the standard deviation of our measure of effectiveness is approximately one third of a standard deviation in grades. This compares to a standard deviation of essentially zero (0.003) in the course with the lowest variation (accounting in the Law&Management program).

In the lower panel of Table 8 we report the mean (across courses) of the difference between the largest and the smallest indicators of teacher effectiveness, which allows us to compute the effect of attending a course in the class of the best versus the worst teacher. On average,

[21] In Italy, university exams are graded on a scale 0 to 30, with pass equal to 18. Such a peculiar grading scale comes from historical legacy: while in primary, middle and high school students were graded by one teacher per subject on a scale 0 to 10 (pass equal to 6), at university each exam was supposed to be evaluated by a commission of three professors, each grading on the same 0-10 scale, the final mark being the sum of these three. Hence, 18 is pass and 30 is full marks. Apart from the scaling, the actual grading at Bocconi is performed as in the average US or UK university.

[22] In Italy wages are normally paid either 13 or 14 times over the year, once every month plus one additional payment around mid December (*tredicesima*) and around mid June (*quattordicesima*).

this effect amounts to 0.427 of a standard deviation, that is almost 1.5 grade points or 5.6% over the average grade. As already noted above, this average effect masks a large degree of heterogeneity across subjects ranging from almost 1 full standard deviation to a mere 0.4% of a standard deviation.

To further understand the importance of these effects, we can also compare particularly lucky students, who are assigned to good teachers (defined as those in the top 5% of the distribution of effectiveness) for all their compulsory courses, to particularly unlucky students, who are always assigned to bad teachers (defined as those in the bottom 5% of the distribution of effectiveness). The average grades of these two groups of students are 1.6 standard deviations apart, corresponding to over 5.5 grade points. Based on our estimate of the wage elasticity, this difference translates into a sizable 1,100-1,200 euros per year (93.2 euros/month) or 9.6% over the average.

For robustness and comparison, we estimate the class effects in two alternative ways. First, we restrict the set $Z_c$ to courses belonging to the same subject area of course $c$, under the assumption that good teaching in one course is likely to have a stronger effect on learning in courses of the same subject areas (e.g. a good basic mathematics teacher is more effective in improving students performance in financial mathematics than in business law). The subject areas are defined by the colors in Table 1 and correspond to the department that was responsible for the organization and teaching of the course. We label these estimates *subject* effects. Given the more restrictive definition of $Z_c$ we can only produce these estimates for a smaller set of courses and using fewer observation, which is the reason why we do not take them as our benchmark.

Next, rather than using performance in subsequent courses, we run equation 1 with the grade in the same course $c$ as the dependent variable. We label these estimates *contemporaneous* effects. We do not consider these contemporaneous effects as alternative and equivalent measures of teacher effectiveness, but we will use them to show that they correlate very differently with the students' evaluations. Descriptive statistics for the subject and contemporaneous effects are reported in Tables A-2 and A-3 in the Appendix.

[INSERT TABLE 9 ABOUT HERE]

In Table 9 we investigate the correlation between these alternative estimates of teacher effectiveness. Specifically, we report results from weighted OLS regressions with our benchmark estimates as the dependent variable and, in turn, the subject and the contemporaneous effects on the right hand side, together with dummies for degree program, term and subject area.[23]

Reassuringly, the subject effects are positively and significantly correlated with our benchmark, while the contemporaneous effects are negatively and significantly correlated with our benchmark, a result that is consistent with previous findings (Carrell and West, 2010; Krautmann and Sander, 1999; Weinberg, Fleisher, and Hashimoto, 2009) and to which we will return in Section 4.

# 4 Correlating teacher effectiveness and student evaluations

In this section we investigate the relationship between our measures of teaching effectiveness from Section 3 and the evaluations received by the same teachers from their students. We concentrate on two core items from the evaluation questionnaires, namely overall teaching quality and the overall clarity of the lectures. Additionally, we also look at other items: the teacher's ability in generating interest for the subject, the logistics of the course (schedule of classes, combinations of practical sessions and traditional lectures) and the total workload compared to other courses.

Formally, we estimate the following equation:

$$q^k_{dtcs} = \lambda_0 + \lambda_1 \widehat{\alpha}_{dtcs} + \lambda_2 C_{dtcs} + \lambda_3 T_{dtcs} + \gamma_d + \delta_t + \upsilon_c + \epsilon_{dtcs} \tag{3}$$

where $q^k_{dtcs}$ is the average answer to question $k$ in class $s$ of course $c$ in the degree program $d$ (which is taught in term $t$), $\widehat{\alpha}_{dtcs}$ is the estimated class fixed effect, $C_{dtcs}$ is the set of class characteristics, $T_{dtcs}$ is the set of teacher characteristics. $\gamma_d$, $\delta_t$ and $\upsilon_c$ are fixed effects for degree program, term and subject areas, respectively. $\epsilon_{dtcs}$ is a residual error term.

Notice that the class and teacher characteristics are exactly the same as in Table 7, so that

[23] To take into account the additional noise due to the presence of generated regressors on the right hand side of these models, the standard errors are bootstrapped. Further, each observation is weighted by the inverse of the standard error of the dependent variable, which is also a generated variable.

equation 3 is equivalent to a partitioned regression model of the evaluations $q_{dtcs}$ on our measures of teacher effectiveness, i.e. the residuals of the regressions in Table 7, where all the observables and the fixed effects are partialled out.

Since the dependent variable in equation 3 is an average, we estimate it using weighted OLS, where each observation is weighted by the square root of the number of collected questionnaires in the class, which corresponds to the size of the sample over which the average answers are taken. Additionally, we also bootstrap the standard errors to take into account the presence of generated regressors (the $\widehat{\alpha}$s).

[INSERT TABLE 10 ABOUT HERE]

The first four columns of Table 10 reports the estimates of equation 3 for a first set of core evaluation items, namely overall teaching quality and lecturing clarity. For each of these items we show results obtained using our benchmark estimates of teacher effectiveness and those obtained using the contemporaneous class effects.

Results show that our benchmark class effects are negatively associated with all the items that we consider. In other words, teachers who are more effective in promoting future performance receive worst evaluations from their students. This relationship is statistically significant for all items (but logistics), and are of sizable magnitude. For example, one standard deviation increase in teacher effectiveness reduces the students evaluations of overall teaching quality by about 40% of a standard deviation. Such an effect could move a teacher who would otherwise receive a median evaluation down to the 29th percentile of the distribution. Effects of very similar magnitude can be computed for lecturing clarity.

Consistently with the findings of other studies (Carrell and West, 2010; Krautmann and Sander, 1999; Weinberg, Fleisher, and Hashimoto, 2009), when we use the contemporaneous effects (even columns) the estimated coefficients turn positive and highly statistically significant for all items (but workload). In other words, the teachers of classes that are associated with higher grades in their own exam receive better evaluations from their students. The magnitude of these effects is only marginally smaller than those estimated for our benchmark measures: one standard deviation change in the contemporaneous teacher effect increases the evaluation

of overall teaching quality by 33% of a standard deviation and the evaluation of lecturing clarity by 32%.

The results in Table 10 clearly challenge the validity of students' evaluations of professors as a measure of teaching quality. Even abstracting from the possibility that professors strategically adjust their grades to please the students (a practice that is made difficult by the timing of the evaluations, that are always collected before the exam takes place), it might still be possible that professors who make the classroom experience more enjoyable do that at the expense of true learning or fail to encourage students to exert effort. Alternatively, students might reward teachers who prepare them for the exam, that is teachers who teach to the test, even if this is done at the expenses of true learning. This interpretation is consistent with the results in Weinberg, Fleisher, and Hashimoto (2009), who provide evidence that students are generally unaware of the value of the material they have learned in a course, and it is the interpretation that we adopt to develop the theoretical framework of Section 6.

Of course, one may also argue that students' satisfaction is important *per se* and, even, that universities should aim at maximizing satisfaction rather than learning, especially private institutions like Bocconi. We doubt that this is the most common understanding of higher education policy.

# 5 Robustness checks

In this section we present robustness checks for our main results in Sections 3 and 4.

First, we investigate the role of students' dropout in the estimation of our measures of teacher effectiveness. In our main empirical analysis students who do not have a complete academic record are excluded. These are students who either dropped out of higher education or have transfered to another university or are still working towards the completion of their programs, whose formal duration was 4 years. They total about 10% of all the students who enrolled in their first year in 1998-1999. In order to check that excluding them does not affect our main results, in Figure 2 we compare our benchmark measure of teacher effectiveness estimated in Section 3 with similar estimates that include such dropout students, conditioning

on degree program effects. As it is evident, the two sets of estimates are very similar and regressing one over the other (controlling for degree program, term and subject fixed effects) yields an $R^2$ of over 88%. Importantly, there does not seem to be larger discrepancies between the two versions of the class effects for the best or the worst teachers.

[INSERT FIGURE 2 ABOUT HERE]

Second, one might be worried that students might not comply with the random assignment to the classes. For various reasons they may decide to attend one or more courses in a different class from the one to which they were formally allocated. For example, they may desire to stay with their friends, who might have been assigned to a different class, or they may like a specific teacher, who is known to present the subject particularly clearly. Unfortunately, such changes would not be recorded in our data, unless the student formally asked to be allocated to a different class, a request that needed to be adequately motivated.[24] Hence, we cannot exclude a priori that some students switch classes.

If the process of class switching is unrelated to teaching quality, then it merely affects the precision of our estimated class effects, but it is very well possible that students switch in search for good or lenient lecturers. We can get some indication of the extent of this problem from the students' answers to an item of the evaluation questionnaires that asks about the congestion in the classroom. Specifically, the question asks whether the number of students in the class was detrimental to one's learning. We can, thus, identify the most congested classes from the average answer to such question in each course.

Courses in which students concentrate in the class of one or few professors should be characterized by a very skewed distribution of such a measure of congestion, with one (or a few) classes being very congested and the others being pretty empty. Thus, for each course we compute the difference in the congestion indicator between the most and the least congested classes (over the standard deviation). Courses in which such difference is very large should be the ones that are more affected by switching behaviors.

[24] Possible motivations for such requests could be health reasons. For example, due to a broken leg a student might not be able to reach classrooms in the upper floors of the university buildings and could ask to be assigned to a class taught on the ground floor.

[INSERT TABLE 11 ABOUT HERE]

In Table 11 we replicate our benchmark estimates for the two core evaluation items (overall teaching quality and lecturing clarity) by excluding the most switched course (Panel B), i.e. the course with the largest difference between the most and the least congested classes (which is marketing). For comparison we also report the original estimates from Table 10 in Panel A and we find that results change only marginally. Next, in Panel C and D we exclude from the estimates also the second most switched course (human resource management) and the five most switched courses, respectively.[25] Again, the estimated coefficients are only mildly affected, although the significance levels are reduced according with the smaller sample sizes. Overall, this exercise suggests that course switching should not affect our estimates in any major direction.

Finally, one might be worried that our results may be generated by some endogenous reaction of students to the quality of their past teachers. For example, as one meets a bad teacher in one course one might be induced to exert higher effort in the future to catch up, especially if bad teaching resulted in a lower (contemporaneous) grade. Hence, the students evaluations may reflect real teaching quality and our measure of teacher effectiveness would be biased by such a process of mean reversion, leading to a negative correlation with real teaching quality and, consequently, also with the evaluations of the students.

[INSERT FIGURE 3 ABOUT HERE]

To control for this potential feedback effect on students' effort, we recompute our benchmark measures of teacher effectiveness adding the student average grade in all previous courses to the set of controls. Figure 3 compares our benchmark teacher effectiveness with this augmented version, conditioning on the usual fixed effects for degree program, term and subject area and shows that the two are strongly correlated (even accounting for the outliers).

[25] The five most switched courses are marketing, human resource management, mathematics for Economics and Management, financial mathematics and managerial accounting.

# 6 Interpreting the results: a simple theoretical framework

We think of teaching as the combination of two types of activities: *real teaching* and *teaching-to-the-test*. The first consists of presentations and discussions of the course material and leads to actual learning, conditional on the students exerting effort; the latter is aimed at maximizing performance in the exam, it requires lower effort by the students and it is not necessarily related to actual learning.

Practically, we think of real teaching as competent presentations of the course material with the aim of making students understand and master it and of teaching-to-the-test as mere repetition of exam tests and exercises with the aim of making students learn how to solve them, even without fully understanding their meaning.

Consider a setting in which teachers are heterogenous in their preference (or ability) to do real teaching. We measure such heterogeneity with a parameter $\mu_j \in [0, 1]$, such that a teacher $j$ with $\mu_j = 0$ exclusively teaches to the test and a teacher with $\mu_j = 1$ exclusively engages in real teaching.

The grade $x_i$ of a generic student $i$ in the course taught by teacher (or in class) $j$ is defined by the following production function:

$$x_i = \mu_j h(e_i) + (1 - \mu_j)\overline{x} \tag{4}$$

which is a linear combination of a function $h(\cdot)$ of student's effort $e_i$ and a constant $\overline{x}$, weighted by the teacher's type $\mu_j$. We assume $h(\cdot)$ to be a continuous and twice differentiable concave function. Under full real teaching ($\mu_j = 1$) grades vary with students' effort; on the other hand, if the teacher exclusively teaches to the test ($\mu_j = 0$), everyone gets the same grade $\overline{x}$, regardless of effort. This strong assumption can obviously be relaxed and all our implications will be maintained as long as the gradient of grades to effort increases with $\mu_j$, the extent of real teaching.

The parameter $\overline{x}$ measures the extent to which the exam material and the exam format lend themselves to teaching-to-the-test. To the one extreme, one can think of the exam as a selection of multiple-choice questions randomly drawn from a large pool. In such a situation,

teaching-to-the-test merely consists in going over all the possible questions, memorizing the correct answer. This is a setting which would feature a large $\overline{x}$. The other extreme are essays, where there is no obvious correct answers and one needs to personally and originally elaborate on one's own understanding of the course material. Of course, there are costs and benefits to each type of exam and multiple-choice tests are often adopted because they can be marked quickly, easily and uncontroversially. For the sake of simplicity, however, we abstract from these considerations.

For simplicity, equation 4 assumes that teaching-to-the-test does not require students to exert effort. All our results would be qualitatively unchanged under the weaker assumption that teaching-to-the-test requires less effort by the students. We also assume that $\mu_j$ is a fixed characteristic of teacher $j$, so that the model effectively describes the conditions for identifying teachers of different types, a key piece of information for hiring and promotion decisions. Alternatively, $\mu_j$ could be treated as an endogenous variable under the control of the individual teacher, in which case the model would feature a rather standard agency problem where the university tries to provide incentives to the teachers to choose a $\mu_j$ close to 1. Although, such a model would be considerably more complicated than what we present in this section, its qualitative results would be unchanged and the limited information on teachers in our data would make its additional empirical content redundant in our setting.

More specifically, one could model $\mu_j$ as an endogenous choice of the teacher and generate heterogeneity by assuming that different activities (real teaching or teaching-to-the-test) require different efforts from the professors, who face heterogeneous marginal disutilities. Such an alternative model would feature both adverse selection and moral hazard and proper measurement of teaching quality could help addressing both issues, by facilitating the identification of low quality agents (high disutility of effort) and by incentivizing effort. In our simplified framework, only adverse selection of professors takes place, but the general intuition holds also in a more complicated setting.

In all cases, a key assumption is that $\mu_j$ is unobservable by the university administrators (the principal) and, although it might be observable to the students, cannot be credibly communicated to third parties.

Assume now that students care about their grades but dislike exerting effort, so that the utility function of a generic student $i$ can be written as follows:

$$U_i = x_i - \frac{1}{2}\frac{e_i^2}{\eta_i} \tag{5}$$

where $\eta_i$ is a measure of student's ability. For simplicity, we assume that students are perfectly informed about the production function of grades, i.e. they know the type of their teacher, they know the return to their effort and there is no additional stochastic component to equation 4. This assumption can be easily relaxed by introducing either imperfect information about the teacher's type or the exact specification of the production function and, consequently, by rewriting the utility function 5 in expected terms. The main intuition of our results would be unchanged. Although the perfect information assumption is obviously a modeling device and does not correspond to reality, we do believe that students know a lot about their professors, either through conversations with older students or by observation through the duration of the course.

The quasi-linearity of equation 5 simplifies the algebra of the model. Alternatively, we could have introduced some curvature in the utility function and assumed a linear production process without affecting the results. With non-linearities both in the production and in the utility functions one would have to make explicit a number of additional assumptions to guarantee existence and uniqueness of the equilibrium.

Students choose their optimal level of effort $e_i^*$ according to the following first order conditions:

$$\mu_j \frac{\partial h(e)}{\partial e_i}(e_i^*) = \frac{e_i^*}{\eta_i} \tag{6}$$

Using equation 6 it is easy to derive the following results:

$$\frac{de_i^*}{d\eta_i} > 0 \tag{7}$$

$$\frac{de_i^*}{d\mu_j} > 0 \tag{8}$$

$$\frac{de_i^*}{d\mu_j d\eta_i} > 0 \tag{9}$$

Equation 7 shows that more able students exert higher effort. Equation 8 shows that more real teaching induces higher effort from the students and equation 9 indicates that such an effect is larger for the more able students [26]

Additionally, using the envelope theorem it is easy to show that:

$$\frac{\partial U_i(e_i^*)}{\partial \mu_j} = h(e_i^*) - \overline{x} \tag{10}$$

Define $\overline{e}$ the level of effort such that $h(\overline{e}) = \overline{x}$. Moreover, since for a given $\mu_j$ there is a unique correspondence between effort and ability, $\overline{e}$ uniquely identifies a $\overline{\eta}$. Hence:

$$\frac{\partial U_i(e_i^*)}{\partial \mu_j} > 0 \quad \text{if} \quad \eta_i > \overline{\eta} \tag{11}$$

$$\frac{\partial U_i(e_i^*)}{\partial \mu_j} < 0 \quad \text{if} \quad \eta_i < \overline{\eta} \tag{12}$$

Equations 11 and 12 are particularly important under the assumption that, especially when answering questions about the overall quality of a course, students give a better evaluation to teachers (or classes) that are associated with a higher level of utility. Equations 11 and 12 suggest that high ability students evaluate better teachers or classes that are more focused on real learning while low ability students prefer teachers that teach to the test. Hence, if the (benchmark) teacher effects estimated in Section 3 indeed measure the real learning value of a class ($\mu_j$, in the terminology of our model), we expect to see a more positive (or less negative) correlation between such class effects and the students' evaluations in those classes where the concentration of high ability students is higher.

# 7 Further evidence

In this section we present two additional pieces of evidence that are consistent with the implications of the model of Section 6.

First, our specification of the production function for exam grades in equation 4 implies a

[26] In the more complicated setting in which $\mu_j$ is an endogenous choice of the teacher, equation 9 shows that teachers' and students' effort are complement.

positive relationship between grade dispersion and the professor's propensity to engage in real teaching ($\mu_j$). In our empirical exercise our measures of teacher effectiveness can be interpreted as measures of the $\mu_j$'s in the terminology of the model. Hence, if grades were more dispersed in the classes of the worst teachers one would have to question our specification of equation 4.

[INSERT FIGURE 4 ABOUT HERE]

In Figure 4 we plot the coefficient of variation of grades in each class (on the vertical axis) against our measure of teacher effectiveness (on the horizontal axis). To take proper account of differences across degree programs, the variables on both axes are the residuals of weighted OLS regressions that condition on degree program, term and subject area fixed effects, as in standard partitioned regressions (the weight is the squared root of classes' size). Consistently with equation 4 in our model, the two variables are positively correlated and such a correlation is statistically significant at conventional levels: a simple univariate OLS regression of the variable on the vertical axis on the variable on the horizontal axis yields a coefficient of 0.007 with a standard error of 0.003.

[INSERT TABLE 12 ABOUT HERE]

Next, according to equations 11 and 12, we expect the correlation between our measures of teacher effectiveness and the average student evaluations to be less negative in classes where the share of high ability students is higher. This is the hypothesis that we investigate in Table 12. We define as high ability those students who score in the upper quartile of the distribution of the entry test score and, for each class in our data, we compute the share of such students. Then, we investigate the relationship between the students' evaluations and teacher effectiveness by restricting the sample to classes in which high-ability students are over-represented. Results seem to suggest the presence of non linearities or threshold effects, as the estimated coefficient remains relatively stable until the fraction of high ability students in the class goes above 25%. At that point, the estimated effect of teacher effectiveness on students' evaluations is about a third of the one estimated on the entire sample, and it is not statistically different from zero (although this is also due to the smaller sample size). The results, thus, suggest that the negative

correlations reported in Table 10 are mostly due to classes with a particularly low incidence of high ability students.

# 8 Conclusions

Using administrative archives from Bocconi University and exploiting random variation in students' allocation to teachers within courses we find that, on average, students evaluate positively classes that give high grades and negatively classes that are associated with high grades in subsequent courses. These empirical findings can be rationalized with a simple model featuring heterogeneity in the preferences (or ability) of teachers to engage in real teaching rather than teaching-to-the-test, with the former requiring higher effort from students than the latter. Overall, our results cast serious doubts on the validity of students' evaluations of professors as measures of teaching quality or effort.

At the same time, the strong effects of teaching quality on students' outcomes, as documented in Section 3, suggest that improving the quantity or the quality of professors' inputs in the education production function can lead to large gains. Under the interpretation offered by our model in Section 6, this could be achieved through various types of interventions. For example, one may think of adopting exam formats that reduce the returns to teaching-to-the-test, although this may come at larger costs due to the additional time needed to grade less standardized tests.

Alternatively, one may stick to the use of students' evaluations to measure teachers' performance but limit the extent of grade leniency that may be induced in such a system, for example by making sure that teaching and grading are done by different persons. Anecdotically, we know that at Bocconi it is common practice among the teachers of the core statistics course to randomize the grading, i.e. at the end of the course the teachers of the different classes are randomly assigned the papers of another class for marking. In the only year in which this practice was abandoned, average grades increased substantially.

Another variation to the current most common use of the students' evaluations consists in postponing the collection of students' opinions, so as to give them time to appreciate the value

of real teaching in subsequent learning (or even in the market). Obviously, this would also pose problems in terms of recall bias and possible retaliation for low grading.

Alternatively, one may also think of alternative forms of performance measurement that are more in line with the peer-review approach adopted in the evaluation of research output. It is already common practice in several departments to have colleagues sitting in some classes and observing teacher performance, especially of assistant professors. This is often done primarily with the aim of offering advise, but in principle it could also be used to measure outcomes. An obvious concern is that one could change behavior due to the presence of the observer. A slightly more sophisticated version of the same method could be based on the use of cameras to record a few teaching sessions during the course without the teacher knowing exactly which ones. The video recordings could then be viewed and evaluated by an external professor in the same field.

Obviously, these, as well as other potential alternative measurement methods, are costly but they should be compared with the costs of the current systems of collecting students' opinions about teachers, which are often non trivial.

# References


BAKER, G., R. GIBBONS, AND K. J. MURPHY (1994): "Subjective performance measures in optimal incentive contracts," *Quarterly Journal of Economics*, 109(4), 1125–1156.

BECKER, W. E., AND M. WATTS (1999): "How departments of economics should evaluate teaching," *American Economic Review (Papers and Proceedings)*, 89(2), 344–349.

BROWN, B. W., AND D. H. SAKS (1987): "The microeconomics of the allocation of teachers' time and student learning," *Economics of Education Review*, 6(4), 319–332.

CARRELL, S. E., AND J. E. WEST (2010): "Does Professor Quality Matter? Evidence from Random Assignment of Students to Professors," *Journal of Political Economy*, 118(3), 409–32.

DE GIORGI, G., M. PELLIZZARI, AND S. REDAELLI (2010): "Identification of Social Interactions through Partially Overlapping Peer Groups," *American Economic Journal: Applied Economics*, 2(2), 241–275.

DE GIORGI, G., M. PELLIZZARI, AND W. G. WOOLSTON (2011): "Class Size and Class Heterogeneity," *Journal of the European Economic Association*, forthcoming.

DUFLO, E., R. HANNA, AND M. KREMER (2010): "Incentives Work: Getting Teachers to Come to School," mimeo, MIT.

FIGLIO, D. N., AND L. KENNY (2007): "Individual teacher incentives and student performance," *Journal of Public Economics*, 91, 901–914.

GOLDHABER, D., AND M. HANSEN (2010): "Using performance on the job to inform teacher tenure decisions," *American Economic Review (Papers and Proceedings)*, 100(2), 250–255.

HANUSHEK, E. A. (1979): "Conceptual and empirical issues in the estimation of educational production functions," *Journal of Human Resources*, 14, 351–388.

HANUSHEK, E. A., AND S. G. RIVKIN (2006): "Teacher quality," in *Handbook of the Economics of Education*, ed. by E. A. Hanushek, and F. Welch, vol. 1, pp. 1050–1078. North Holland, Amsterdam.

——— (2010): "Generalizations about using value-added measures of teacher quality," *American Economic Review (Papers and Proceedings)*, 100(2), 267–271.

HIRSCH, J. E. (2005): "An index to quantify an individual's scientific research output," *Proceedings of the National Academy of Sciences of the United States of America*, 102(46), 16569–16572.

HOFFMAN, F., AND P. OREOPOULOS (2009): "Professor Qualities and Student Achievement," *The Review of Economics and Statistics*, 91(1), 83–92.

HOGAN, T. D. (1981): "Faculty research activity and the quality of graduate training," *Journal of Human Resources*, 16(3), 400–415.

HOLMSTROM, B., AND P. MILGROM (1994): "The firm as an incentive system," *American Economic Review*, 84(4), 972–991.

JACOB, B. A., AND L. LEFGREN (2008): "Can principals identify effective teachers? Evidence on subjective performance evaluation in education," *Journal of Labor Economics*, 26, 101–136.

KANE, T. J., AND D. O. STAIGER (2008): "Estimating teacher impacts on student achievement: an experimental evaluation," Discussion Paper 14607, NBER Working Paper Series.

KRAUTMANN, A. C., AND W. SANDER (1999): "Grades and student evaluations of teachers," *Economics of Education Review*, 18, 59–63.

KRUEGER, A. B. (1999): "Experimental estimates of education production functions," *Quarterly Journal of Economics*, 114, 497–532.

LAVY, V. (2009): "Performance Pay and Teachers' Effort, Productivity and Grading Ethics," *The American Economic Review*, 95(5), 1979–2011.

MULLIS, I. V., M. O. MARTIN, D. F. ROBITAILLE, AND P. FOY (2009): *TIMSS Advanced 2008 International Report*. TIMSS & PIRLS International Study Center, Lynch School of Education, Boston College.

OECD (2008): *Education at a Glance*. Organization of Economic Cooperation and Development, Paris.

——— (2010): *PISA 2009 at a Glance*. OECD Publishing.

PRENDERGAST, C., AND R. H. TOPEL (1996): "Favoritism in organizations," *Journal of Political Economy*, 104(5), 958–978.

RIVKIN, S. G., E. A. HANUSHEK, AND J. F. KAIN (2005): "Teachers, Schools and Acedemic Achievement," *Econometrica*, 73(2), 417–458.

ROCKOFF, J. E. (2004): "The impact of individual teachers on student achievement: evidence from panel data," *American Economic Review (Papers and Proceedings)*, 94(2), 247–252.

ROCKOFF, J. E., AND C. SPERONI (2010): "Subjective and Objective Evaluations of Teacher Effectiveness," *American Economic Review (Papers and Proceedings)*, 100(2), 261–266.

ROTHSTEIN, J. (2009): "Student sorting and bias in value added estimation: selection on observables and unobservables," *Education Finance and Policy*, 4(4), 537–571.

——— (2010): "Teacher Quality in Educational Production: Tracking, Decay, and Student Achievement," *Quarterly Journal of Economics*, 125(1), 175–214.

SWAMY, P. A. V. B., AND S. S. ARORA (1972): "The Exact Finite Sample Properties of the Estimators of Coefficients in the Error Components Regression Models," *Econometrica*, 40(2), pp. 261–275.

TYLER, J. H., E. S. TAYLOR, T. J. KANE, AND A. L. WOOTEN (2010): "Using student performance data to identify effective classroom practices," *American Economic Review (Papers and Proceedings)*, 100(2), 256–260.

WEINBERG, B. A., B. M. FLEISHER, AND M. HASHIMOTO (2009): "Evaluating Teaching in Higher Education," *Journal of Economic Education*, 40(3), 227–261.

# Figures

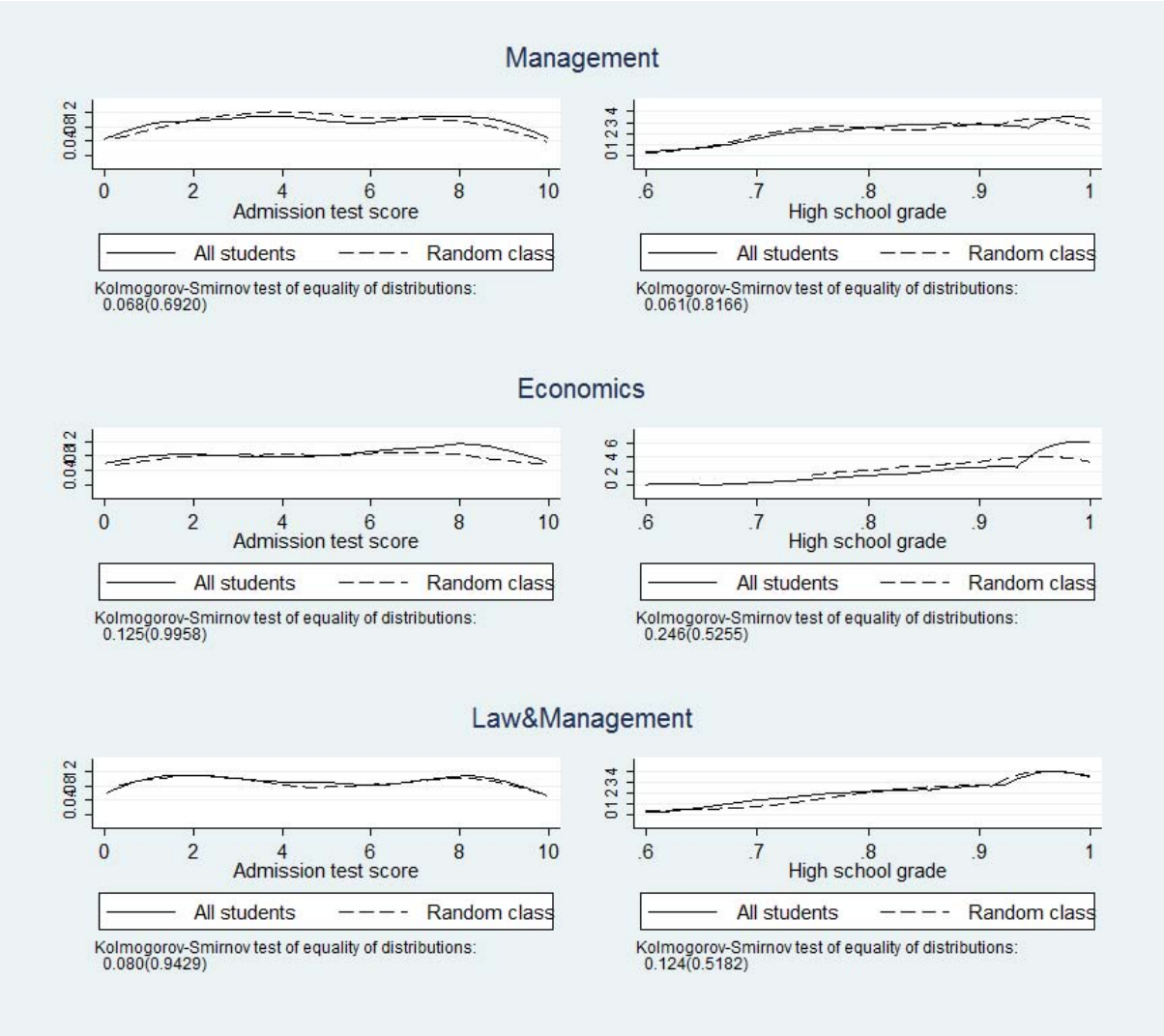



Figure 1: Evidence of random allocation - Ability variables

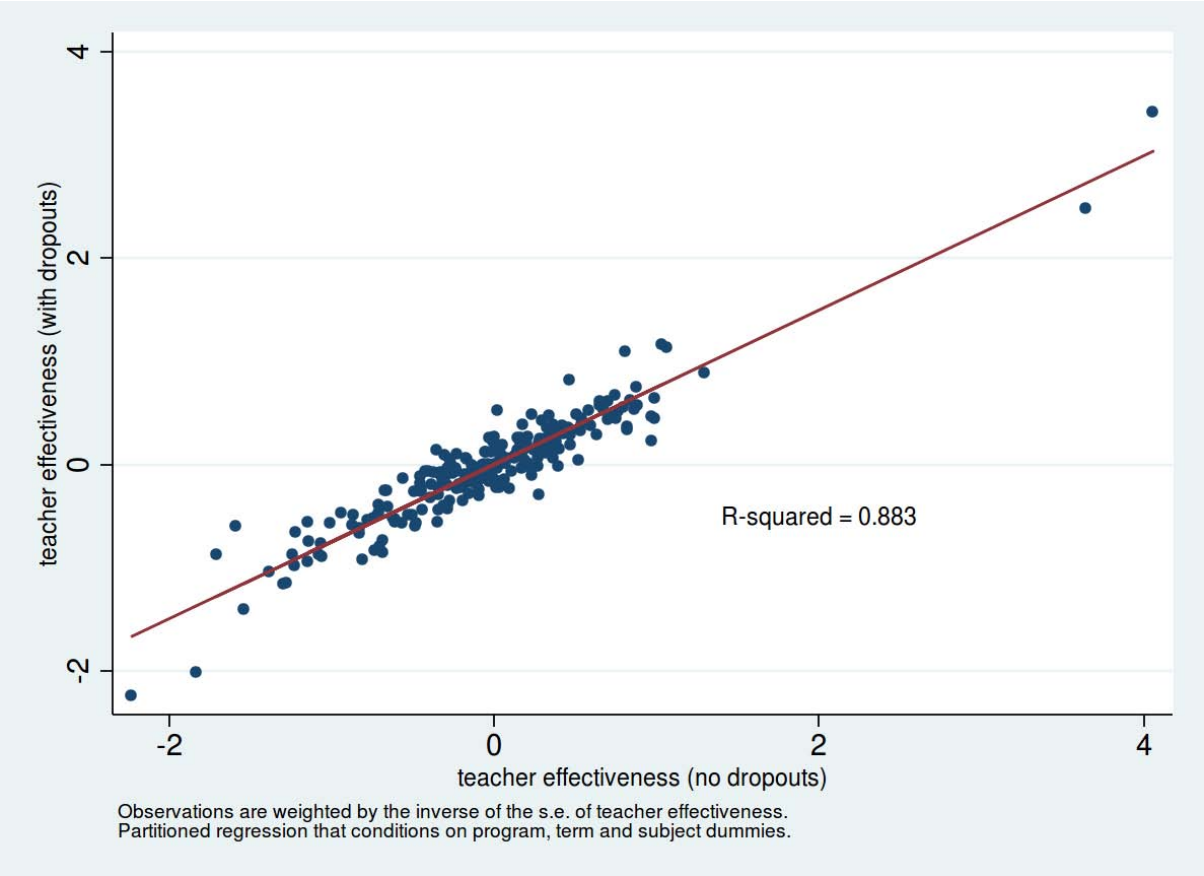



Figure 2: Robustness check for dropouts

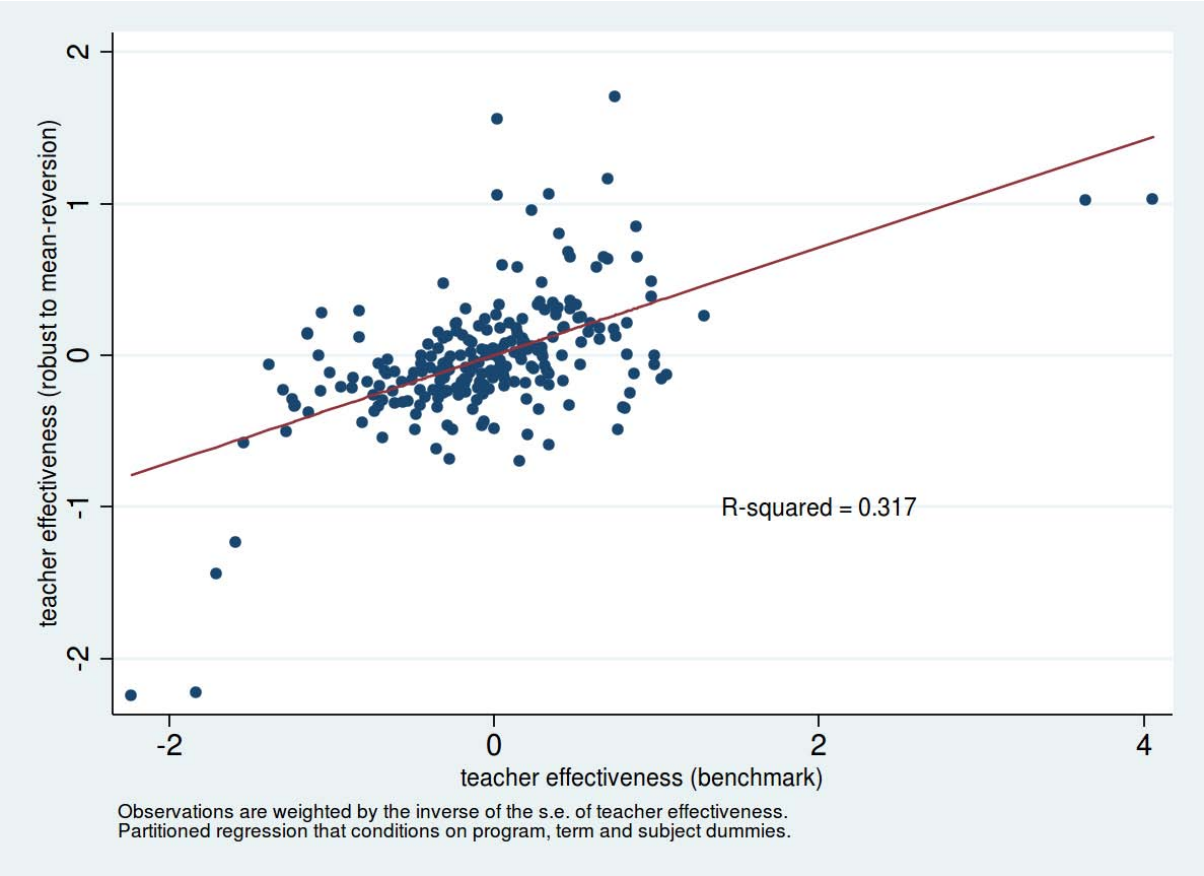



Figure 3: Robustness check for mean reversion in grades

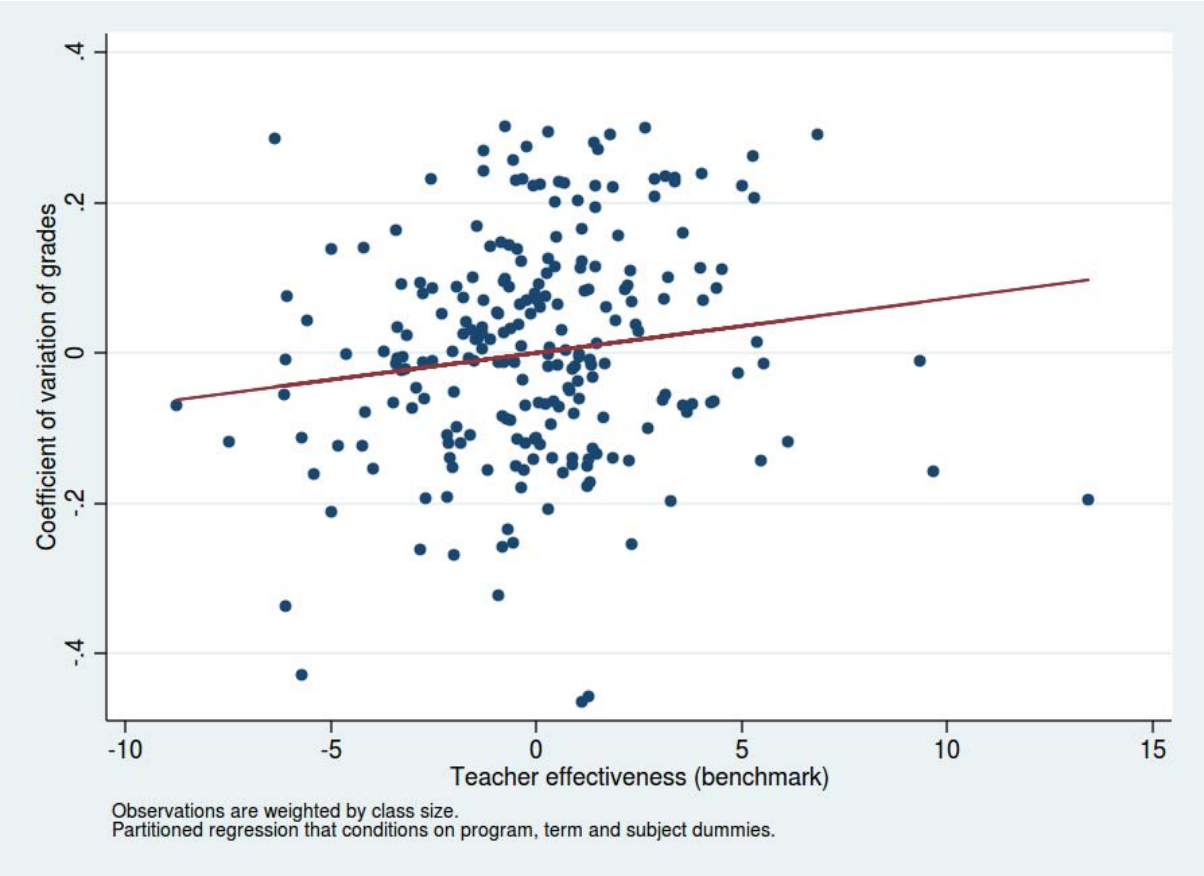



Figure 4: Teacher effectiveness and grade dispersion

# Tables

Table 1: Structure of degree programs

| | MANAGEMENT | ECONOMICS | LAW&MANAG. |
|---|---|---|---|
| Term I | Management I<br>Private law<br>Mathematics | Management I<br>Private law<br>Mathematics | Management I<br>Mathematics |
| Term II | Microeconomics<br>Public law<br>Accounting | Microeconomics<br>Public law<br>Accounting | Accounting |
| Term III | Management II<br>Macroeconomics<br>Statistics | Management II<br>Macroeconomics<br>Statistics | Management II<br>Statistics |
| Term IV | Business law<br>Manag. of Public Administrations<br>Financial mathematics<br>Human resources management | Financial mathematics<br>Public economics<br>Business law | Accounting II<br>Fiscal law<br>Financial mathematics |
| Term V | Banking<br>Corporate finance<br>Management of industrial firms | Econometrics<br>Economic policy | Corporate finance |
| Term VI | Marketing<br>Management III<br>Economic policy<br>Managerial accounting | Banking | |
| Term VII | Corporate strategy | | |
| Term VIII | | | Business law II |

The colors indicate the subject area the courses belong to: red=management, black=economics, green=quantitative, blue=law. Only compulsory courses are displayed.

Table 2: Descriptive statistics of degree programs

| | Term | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Variable | I | II | III | IV | V | VI | VII | VIII |
| | | | | Management | | | | |
| No. Courses | 3 | 3 | 3 | 4 | 3 | 4 | 1 | - |
| No. Classes | 24 | 21 | 23 | 26 | 23 | 27 | 12 | - |
| Avg. Class Size | 129.00 | 147.42 | 134.61 | 138.6 | 117.5 | 133.5 | 75.1 | - |
| SD Class Size | 73.13 | 80.57 | 57.46 | 100.06 | 16.64 | 46.20 | 11.89 | - |
| | | | | Economics | | | | |
| No. Courses | 3 | 3 | 3 | 3 | 2 | 1 | - | - |
| No. Classes | 24 | 21 | 23 | 4 | 2 | 2 | - | - |
| Avg. Class Size | 129.00 | 147.42 | 134.61 | 98.3 | 131.0 | 65.5 | - | - |
| SD Class Size | 73.13 | 80.57 | 57.46 | 37.81 | 0 | 37.81 | - | - |
| | | | | Law & Management | | | | |
| No. Courses | 3 | 4 | 4 | 4 | 2 | - | - | 1 |
| No. Classes | 5 | 5 | 5 | 6 | 3 | - | - | 1 |
| Avg. Class Size | 104.4 | 139.2 | 139.2 | 116 | 116 | - | - | 174 |
| SD Class Size | 39.11 | 47.65 | 47.67 | 44.96 | 50.47 | - | - | 0 |

Table 3: Descriptive statistics of students

| Variable | Management | Economics | Law &Management | Total |
|---|---|---|---|---|
| 1=female | 0.408 | 0.427 | 0.523 | 0.427 |
| 1=outside Milan[a] | 0.620 | 0.748 | 0.621 | 0.634 |
| 1=top Income Bracket[b] | 0.239 | 0.153 | 0.368 | 0.248 |
| 1=academic high school[c] | 0.779 | 0.794 | 0.684 | 0.767 |
| 1=late enrollee[d] | 0.014 | 0.015 | 0.011 | 0.014 |
| High-school grade (0-100) | 86.152 | 93.053 | 88.084 | 87.181 |
| | (10.905) | (8.878) | (10.852) | (10.904) |
| Entry Test Score (0-100) | 50.615 | 52.415 | 48.772 | 50.544 |
| | (28.530) | (31.752) | (29.902) | (29.084) |
| University Grades (0-30) | 25.684 | 27.032 | 25.618 | 25.799 |
| | (3.382) | (2.938) | (3.473) | (3.379) |
| Wage (Euro)[e] | 966.191 | 1,012.241 | 958.381 | 967.964 |
| | (260.145) | (265.089) | (198.437) | (250.367) |
| Number of students | 901 | 131 | 174 | 1,206 |

[a] Dummy equal to one if the student's place of residence at the time of first enrollment is outside the province of Milan (which is where Bocconi university is located).
[b] Family income is recorded in brackets and the dummy is equal to one for students who report incomes in the top bracket, whose lower threshold is in the order of approximately 110,000 euros at current prices.
[c] Dummy equal to one if the student attended a academic high school, such as a lyceum, rather than professional or vocational schools.
[d] Dummy equal to one if the student enrolled at Bocconi after age 19.
[e] Nominal value at current (2010) prices. Based on 391 observations for Management, 36 observations for Economics, 94 observations for Law&Management, i.e. 521 observations overall.

Table 4: Descriptive statistics of students' evaluations

| Variable | Management mean (std.dev.) | Economics mean (std.dev.) | Law&Manag. mean (std.dev.) | Total mean (std.dev.) |
|---|---|---|---|---|
| Overall teaching quality[a] | 7.103 | 7.161 | 6.999 | 7.115 |
| | (0.956) | (0.754) | (1.048) | (0.900) |
| Lecturing clarity[b] | 3.772 | 3.810 | 3.683 | 3.779 |
| | (0.476) | (0.423) | (0.599) | (0.467) |
| Teacher generates interest[a] | 6.800 | 6.981 | 6.915 | 6.864 |
| | (0.905) | (0.689) | (1.208) | (0.865) |
| Course logistic[b] | 3.683 | 3.641 | 3.617 | 3.666 |
| | (0.306) | (0.266) | (0.441) | (0.303) |
| Course workload[b] | 2.709 | 2.630 | 2.887 | 2.695 |
| | (0.461) | (0.542) | (0.518) | (0.493) |

[a] Scores range from 0 to 10.
[b] Scores range from 1 to 5.
See Table A-4 for the exact wording of the evaluation questions.

Table 5: Randomness checks - Students

| | Female | Academic High School[a] | High School Grade | Entry Test Score | Top Income Bracket[a] | Outside Milan | Late Enrollees[a] |
|---|---|---|---|---|---|---|---|
| | | | *Management* | | | | |
| F-stat | 1.110 | 1.052 | 1.074 | 1.082 | 1.248 | 1.753 | 0.132 |
| P-value | 0.208 | 0.341 | 0.286 | 0.266 | 0.043 | 0.000 | 0.999 |
| | | | *Economics* | | | | |
| F-stat | 3.592 | 0.931 | 1.684 | 1.488 | 0.649 | 1.027 | 0.109 |
| P-value | 0.000 | 0.616 | 0.007 | 0.031 | 0.969 | 0.441 | 0.999 |
| | | | *Law & Management* | | | | |
| F-stat | 1.766 | 0.664 | 0.510 | 0.382 | 0.527 | 0.765 | 0.338 |
| P-value | 0.109 | 0.679 | 0.801 | 0.890 | 0.787 | 0.598 | 0.916 |

[a] See notes to Table 4.
The reported statistics are derived from regressions of the observable students' characteristics (by column) on class dummies and the full interaction of indicators for the degree program and the course (with standard errors clustered at the level of the individual student). The reported statistics test the null hypothesis that the coefficients on all the class dummies are all jointly equal to zero. Management: 21 courses, 156 classes; Economics: 11 courses, 72 classes; Law & Management: 7 courses, 14 classes. Total: 38 courses, 230 classes.

Table 6: Randomness checks - Teachers

| | F-test | P-value |
|---|---|---|
| Class size[a] | 1.26 | 0.253 |
| Attendance[b] | 0.59 | 0.809 |
| Avg. high school grade | 0.93 | 0.496 |
| Avg. entry test score | 0.47 | 0.894 |
| Share of females | 0.70 | 0.709 |
| Share of students from outside Milan[c] | 0.36 | 0.954 |
| Share of top-income students[c] | 1.16 | 0.319 |
| Share academic high school[c] | 1.88 | 0.050 |
| Share late enrollees[c] | 0.89 | 0.530 |
| Share of high ability[d] | 0.81 | 0.607 |
| Morning lectures [e] | 2.76 | 0.003 |
| Evening lectures [f] | 1.73 | 0.079 |
| Joint significance | 1.17 | 0.119 |

The reported statistics are derived from a system of 9 seemingly unrelated simultaneous equations, where each observation is a class in a compulsory course (184 observations in total). The dependent variables are 9 teachers' characteristics (age, gender, h-index, average citations per year and 4 dummies for academic positions) and the regressors are the class characteristics listed in the table. The reported statistics test the null hypothesis that the coefficients on each class characteristic are all jointly equal to zero in all the equations of the system. The last row tests the hypothesis that the coefficients on all regressors are all jointly zero in all equations. All tests are distributed according to a F-distribution with (9,1467) degrees of freedom, apart from the joint test in the last row, which has (108,1467) degrees of freedom.

[a] Number or officially enrolled students.

[b] Attendance is monitored by random visits of university attendants to the class.

[c] See notes to Table 4.

[d] Share of students in the top 25% of the entry test score distribution.

[e] Share of lectures taught between 8.30 and 10.30 a.m.

[f] Share of lectures taught between 4.30 and 6.30 p.m.

Table 7: Determinants of class effects

| Dependent variable = $\widehat{\alpha}_s$ | [1] | [2][a] | [3] |
|---|---|---|---|
| Class size[b] | -0.001*** | - | -0.001*** |
| | (0.000) | | (0.000) |
| Avg. high school grade | 13.026*** | - | 11.692*** |
| | (1.660) | | (1.545) |
| Avg. entry test score | 0.009 | - | -0.061 |
| | (0.108) | | (0.098) |
| Share of females | 0.128 | - | -0.591 |
| | (0.417) | | (0.396) |
| Share from outside Milan[b] | -0.667* | - | -0.404 |
| | (0.356) | | (0.320) |
| Share of top income[b] | -0.894* | - | -0.628 |
| | (0.481) | | (0.443) |
| Share from academic high schools[b] | 1.056** | - | -0.159 |
| | (0.504) | | (0.503) |
| Share of late enrollees[b] | -0.315 | - | 0.820 |
| | (1.493) | | (1.377) |
| Share of high ability[b] | 1.616** | - | 1.113* |
| | (0.690) | | (0.623) |
| Morning lectures[b] | 0.019 | - | -0.023 |
| | (0.068) | | (0.065) |
| Evening lectures[b] | 0.364 | - | -0.146 |
| | (0.831) | | (0.802) |
| 1=coordinator | - | -0.013 | 0.078 |
| | | (0.074) | (0.067) |
| Male | - | -0.035 | -0.030 |
| | | (0.046) | (0.040) |
| Age | - | -0.051*** | -0.043*** |
| | | (0.007) | (0.007) |
| Age squared | - | 0.001*** | 0.000*** |
| | | (0.000) | (0.000) |
| H-index | - | -0.026** | -0.021** |
| | | (0.012) | (0.010) |
| Citations per year | - | 0.001 | 0.001 |
| | | (0.002) | (0.002) |
| Full professor[c] | - | 0.352*** | 0.174 |
| | | (0.128) | (0.123) |
| Associate professor[c] | - | 0.280** | 0.164 |
| | | (0.121) | (0.115) |
| Assistant professor[c] | - | 0.237** | 0.174 |
| | | (0.118) | (0.114) |
| Degree program dummies | yes | yes | yes |
| Subject area dummies | yes | yes | yes |
| Term dummies | yes | yes | yes |
| Partial R squared | 0.424 | 0.358 | 0.570 |
| Observations | 230 | 230 | 230 |

Observations are weighted by the inverse of the standard error of the estimated $\alpha$'s. * p<0.1, ** p<0.05,***p<0.01

[a] Weighted averages of individual characteristics if there is more than one teacher per class.

[b] See notes to Table 6.

[c] All variables regarding the academic position refer to the main teacher of the class. The excluded dummy is a residual category (visiting prof., external experts, collaborators.)

[d] R squared computed once program, term and subject fixed effects are partialled out.

Table 8: Descriptive statistics of estimated teacher effectiveness

| | Management | Economics | Law & Management | Total |
|---|---|---|---|---|
| *PANEL A: Std. dev. of estimated teacher effect* | | | | |
| mean | 0.160 | 0.144 | 0.220 | 0.174 |
| minimum | 0.067 | 0.003 | 0.040 | 0.003 |
| maximum | 0.235 | 0.244 | 0.330 | 0.330 |
| *PANEL B: Largest minus smallest class effect* | | | | |
| mean | 0.437 | 0.204 | 0.552 | 0.427 |
| minimum | 0.213 | 0.004 | 0.056 | 0.004 |
| maximum | 0.601 | 0.345 | 0.969 | 0.969 |
| No. of courses | 20 | 11 | 7 | 38 |
| No. of classes | 144 | 72 | 14 | 230 |

Teacher effectiveness is estimated by regressing the estimated class effects ($\alpha$) on observable class and teacher's characteristics (see Table 7).

Table 9: Comparison of benchmark, subject and contemporaneous teacher effects

| Dependent variable: | Benchmark teacher effectiveness | |
|---|---|---|
| Subject | 0.050** | - |
| | (0.025) | |
| Contemp. | - | -0.081*** |
| | | (0.025) |
| Program fixed effects | yes | yes |
| Term fixed effects | yes | yes |
| Subject fixed effects | yes | yes |
| Observations | 212 | 230 |

Bootstrapped standard errors in parentheses. Observations are weighted by the inverse of the standard error of the dependent variable. * p<0.1, ** p<0.05,***p<0.01

Table 10: Teacher effectiveness and students' evaluations

| | Teaching quality | | Lecturing clarity | | Teacher ability in generating interest | | Course logistics | | Course workload | |
|---|---|---|---|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] | [9] | [10] |
| *Teacher effectiveness* | | | | | | | | | | |
| Benchmark | -0.406** | - | -0.209* | - | -0.519** | - | -0.081 | - | -0.171* | - |
| | (0.200) | | (0.117) | | (0.210) | | (0.091) | | (0.095) | |
| Contemporaneous | - | 0.271*** | - | 0.135*** | - | 0.229*** | - | 0.087*** | - | 0.005 |
| | | (0.070) | | (0.030) | | (0.044) | | (0.021) | | (0.024) |
| Class characteristics | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Teacher's characteristics | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Degree program dummies | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Subject area dummies | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Term dummies | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Partial R2 | 0.0127 | 0.0874 | 0.0133 | 0.0860 | 0.0345 | 0.1045 | 0.0052 | 0.0912 | 0.0207 | 0.0003 |
| Observations | 230 | 230 | 230 | 230 | 230 | 230 | 230 | 230 | 230 | 230 |

Weighted OLS estimates. Observations are weighted by the number of collected questionnaires in each class. Bootstrapped standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 11: Robustness check for class switching

| | Overall teaching quality | | Lecturing clarity | |
|---|---|---|---|---|
| | [1] | [2] | [3] | [4] |
| *PANEL A: All courses* | | | | |
| Benchmark teacher effects | -0.410* | - | -0.210** | - |
| | (0.226) | | (0.121) | |
| Contemporaneous teacher effects | - | 0.273*** | - | 0.136*** |
| | | (0.061) | | (0.030) |
| Observations | 230 | 230 | 230 | 230 |
| *PANEL B: Excluding most switched course* | | | | |
| Benchmark teacher effects | -0.459* | - | -0.211* | - |
| | (0.259) | | (0.119) | |
| Contemporaneous teacher effects | - | 0.284*** | - | 0.137*** |
| | | (0.067) | | (0.035) |
| Observations | 222 | 222 | 222 | 222 |
| *PANEL C: Excluding most and second most switched course* | | | | |
| Benchmark teacher effects | -0.367 | - | -0.172 | - |
| | (0.291) | | (0.126) | |
| Contemporaneous teacher effects | - | 0.262*** | - | 0.128*** |
| | | (0.073) | | (0.034) |
| Observations | 214 | 214 | 214 | 214 |
| *PANEL D: Excluding five most switched courses* | | | | |
| Benchmark teacher effects | -0.474 | - | -0.186 | - |
| | (0.292) | | (0.124) | |
| Contemporaneous teacher effects | - | 0.212*** | - | 0.089** |
| | | (0.082) | | (0.041) |
| Observations | 176 | 176 | 176 | 176 |

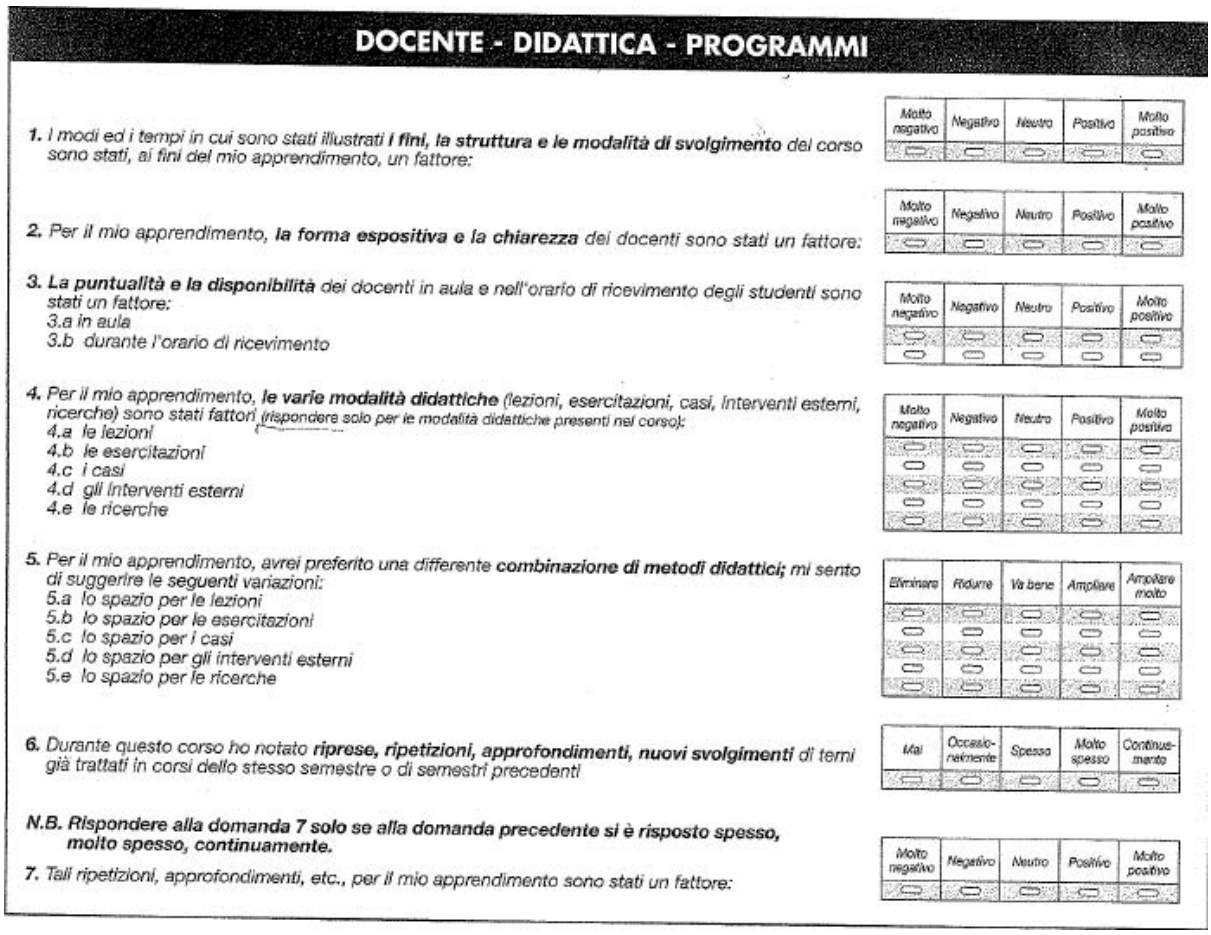
Weighted OLS estimates. Observations are weighted by the squared root of the number of collected questionnaires in each class.
Additional regressors: teacher characteristics (gender and coordinator status), class characteristics (class size, attendance, average high school grade, average entry test score, share of high ability students, share of students from outside Milan, share of top-income students), degree program dummies, term dummies, subject area dummies.
Bootstrapped standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 12: Teacher effectiveness and students evaluations by share of high ability students

| | Presence of high-ability students | | |
|---|---|---|---|
| | all | >0.22 | >0.25 |
| | [1] | [2] | [3] |
| PANEL A: Overall teaching quality | | | |
| Teaching effectiveness | -0.410* | -0.426 | -0.088 |
| | (0.227) | (0.336) | (0.329) |
| PANEL B: Lecturing clarity | | | |
| Teaching effectiveness | -0.210* | -0.210 | -0.059 |
| | (0.121) | (0.159) | (0.165) |
| Observations | 230 | 172 | 115 |

Weighted OLS estimates. Observations are weighted by the squared root of the number of collected questionnaires in each class.
Additional regressors: teacher characteristics (gender and coordinator status), class characteristics (class size, attendance, average high school grade, average entry test score, share of high ability students, share of students from outside Milan, share of top-income students), degree program dummies, term dummies, subject area dummies.
Bootstrapped standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

# Appendix



**DOCENTE - DIDATTICA - PROGRAMMI**

| | Molto negativo | Negativo | Neutro | Positivo | Molto positivo |
|---|---|---|---|---|---|
| **1.** *I modi ed i tempi in cui sono stati illustrati* ***i fini, la struttura e le modalità di svolgimento*** *del corso sono stati, ai fini del mio apprendimento, un fattore:* | | | | | |
| **2.** *Per il mio apprendimento,* ***la forma espositiva e la chiarezza*** *dei docenti sono stati un fattore:* | | | | | |
| **3.** ***La puntualità e la disponibilità*** *dei docenti in aula e nell'orario di ricevimento degli studenti sono stati un fattore:* | | | | | |
| *3.a in aula* | | | | | |
| *3.b durante l'orario di ricevimento* | | | | | |
| **4.** *Per il mio apprendimento,* ***le varie modalità didattiche*** *(lezioni, esercitazioni, casi, interventi esterni, ricerche) sono stati fattori (rispondere solo per le modalità didattiche presenti nel corso):* | | | | | |
| *4.a le lezioni* | | | | | |
| *4.b le esercitazioni* | | | | | |
| *4.c i casi* | | | | | |
| *4.d gli interventi esterni* | | | | | |
| *4.e le ricerche* | | | | | |

| | Eliminare | Ridurre | Va bene | Ampliare | Ampliare molto |
|---|---|---|---|---|---|
| **5.** *Per il mio apprendimento, avrei preferito una differente* ***combinazione di metodi didattici;*** *mi sento di suggerire le seguenti variazioni:* | | | | | |
| *5.a lo spazio per le lezioni* | | | | | |
| *5.b lo spazio per le esercitazioni* | | | | | |
| *5.c lo spazio per i casi* | | | | | |
| *5.d lo spazio per gli interventi esterni* | | | | | |
| *5.e lo spazio per le ricerche* | | | | | |

| | Mai | Occasio-nalmente | Spesso | Molto spesso | Continua-mente |
|---|---|---|---|---|---|
| **6.** *Durante questo corso ho notato* ***riprese, ripetizioni, approfondimenti, nuovi svolgimenti*** *di temi già trattati in corsi dello stesso semestre o di semestri precedenti* | | | | | |

***N.B. Rispondere alla domanda 7 solo se alla domanda precedente si è risposto spesso, molto spesso, continuamente.***

| | Molto negativo | Negativo | Neutro | Positivo | Molto positivo |
|---|---|---|---|---|---|
| **7.** *Tali ripetizioni, approfondimenti, etc., per il mio apprendimento sono stati un fattore:* | | | | | |

Figure A-1: Excerpt of student questionnaire

Table A-1: Descriptive statistics of estimated class effects

| | Management | Economics | Law & Management | Total |
|---|---|---|---|---|
| *Std. dev. of estimated class effects* | | | | |
| mean | 0.055 | 0.160 | 0.033 | 0.082 |
| minimum | 0.027 | 0.066 | 0.000 | 0.000 |
| maximum | 0.087 | 0.246 | 0.087 | 0.246 |
| *Largest minus smallest class effect* | | | | |
| mean | 0.156 | 0.433 | 0.047 | 0.216 |
| minimum | 0.043 | 0.108 | 0.000 | 0.000 |
| maximum | 0.248 | 0.748 | 0.122 | 0.748 |
| No. of courses | 20 | 11 | 7 | 38 |
| No. of classes | 144 | 72 | 14 | 230 |

Table A-2: Descriptive statistics of *subject* teacher effectiveness

| | Management | Economics | Law & Management | Total |
|---|---|---|---|---|
| *PANEL A: Std. dev. of estimated teacher effects* | | | | |
| mean | 0.087 | 0.245 | 0.108 | 0.138 |
| minimum | 0.050 | 0.000 | 0.034 | 0.000 |
| maximum | 0.146 | 0.319 | 0.156 | 0.319 |
| *PANEL B: Largest minus smallest teacher effect* | | | | |
| mean | 0.248 | 0.744 | 0.153 | 0.374 |
| minimum | 0.154 | 0.001 | 0.048 | 0.001 |
| maximum | 0.345 | 1.048 | 0.221 | 1.048 |
| No. of courses | 17 | 10 | 7 | 34 |
| No. of classes | 128 | 70 | 14 | 212 |

Table A-3: Descriptive statistics of *contemporaneous* teacher effectiveness

| | Management | Economics | Law & Management | Total |
|---|---|---|---|---|
| *PANEL A: Std. dev. of estimated teacher effects* | | | | |
| mean | 0.177 | 0.311 | 0.155 | 0.212 |
| minimum | 0.041 | 0.197 | 0.001 | 0.001 |
| maximum | 0.305 | 0.568 | 0.323 | 0.568 |
| *PANEL B: Largest minus smallest teacher effect* | | | | |
| mean | 0.493 | 0.801 | 0.220 | 0.532 |
| minimum | 0.106 | 0.278 | 0.002 | 0.002 |
| maximum | 0.989 | 1.560 | 0.456 | 1.560 |
| No. of courses | 20 | 11 | 7 | 38 |
| No. of classes | 144 | 72 | 14 | 230 |

Table A-4: Wording of the evaluation questions

| | |
|---|---|
| Overall teaching quality | *On a scale 0 to 10, provide your overall evaluation of the course you attended in terms of quality of the teaching.* |
| Clarity of the lectures | *On a scale 1 to 5, where 1 means complete disagreement and 5 complete agreement, indicate to what extent you agree with the following statement: the speech and the language of the teacher during the lectures are clear and easily understandable.* |
| Ability in generating interest for the subject | *On a scale 0 to 10, provide your overall evaluation about the teacher's ability in generating interest for the subject* |
| Logistics of the course | *On a scale 1 to 5, where 1 means complete disagreement and 5 complete agreement, indicate to what extent you agree with the following statement: the course has been carried out coherently with the objectives, the content and the schedule that were communicated to us at the beginning of the course by the teacher.* |
| Workload of the course | *On a scale 1 to 5, where 1 means complete disagreement and 5 complete agreement, indicate to what extent you agree with the following statement: the amount of study materials required for the preparation of the exam has been realistically adequate to the objective of learning and sitting the exams of all courses of the term.* |

Table A-5: Correlations between evaluations items

| | Overall teaching quality | Lecturing clarity | Teacher generates interest | Course logistics | Course workload |
|---|---|---|---|---|---|
| Overall teaching quality | 1.000 | | | | |
| Lecturing clarity | 0.888 | 1.000 | | | |
| Teacher generates interest | 0.697 | 0.536 | 1.000 | | |
| Course logistics | 0.742 | 0.698 | 0.506 | 1.000 | |
| Course workload | 0.124 | 0.122 | 0.193 | 0.094 | 1.000 |

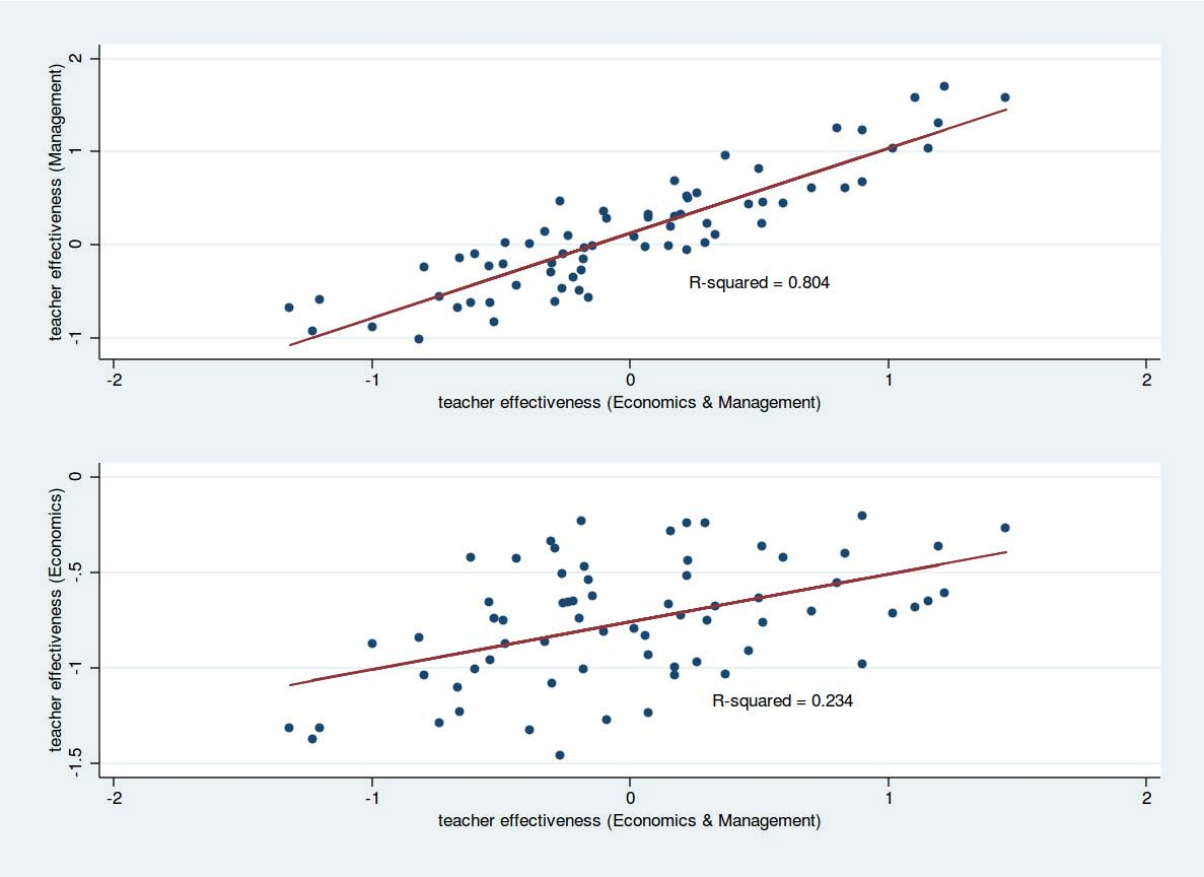



Figure A-2: Economics and Management common courses - Benchmark teacher effectiveness