Gender grading bias at the university level: quasi-experimental evidence from an anonymous grading reform

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Abstract

In this paper, we first present novel evidence of female university students benefiting from being graded fairly, i.e., anonymously. This finding conflicts with previous results at the secondary education level. In contrast to the teacher gender composition at lower levels of education, teachers at the university level are predominantly male. Thus, an in-group bias mechanism could consistently explain the evidence at both the university and secondary education levels. We find that the treatment effect is driven by departments in which male teachers constitute the majority and estimate a significant in-group bias effect using randomly assigned graders. However, unexpectedly, further analysis shows that the treatment effect is essentially independent of the grader/student gender match. Hence, we document evidence of both grading bias stemming from in-group bias and other match independent factors, for example, shared culture and institutional factors.

Keywords

Grading bias; University; Discrimination; Education; Anonymous grading; in-group bias

JEL codes

I23; J16

1 Introduction

Biased grading has recently received increasing attention in economics. This literature is generally motivated by the growing gender gap in educational attainment and the sorting of males and females into specific fields.[1] One strand of the grading bias literature has focused on pre-tertiary education levels and has typically found bias against males.[2] This literature finds a bias effect independent of in-group bias, i.e., independent of the grader-student gender match. However, another strand of the literature notes that the teaching profession has been increasingly staffed by women. The majority of female teachers have been proposed as one mechanism explaining the grading bias against boys through in-group bias.[3] In contrast to the lower education levels, a large majority of university teachers are male. Therefore, a study of grading bias at the university could inform us about the role of both general and in-group bias mechanisms. Furthermore, there are very few large-scale studies based on quasi-experimental methods evaluating gender grading bias at universities.[4]

This study aims to fill this gap by making the following four main contributions: (i) using a difference-in-difference design to estimate the treatment effect on female students being graded

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[1] See, for instance, Lavy and Sand (2018), Kugler et al. (2017) and Terrier (2015).

[2] See, for instance, Lavy (2008) or Berg et al. (2019).

[3] The phenomenon whereby people tend to favor other people of their own group is usually referred to as in-group bias effects. See, for instance, Sandberg (2017) for an overview as well as Dee (2005, 2007), Lee et al. (2014), Lusher et al. (2015), Feld et al. (2016) and Lim and Meer (2017a)

[4] A pilot study on parts of the sample was undertaken by Eriksson and Nolgren (2013).

anonymously compared to male students using a large scale anonymous grading reform at Stockholm University; (ii) exploring the heterogeneous effects across departments with different male/female teacher majorities, (iii) credibly estimating the in-group bias using random grader assignments, and (iv) quantifying the extent to which the treatment effect of the grading reform is caused by a reduction of the in-group bias. To do this, we combine two unique data sets with two experimental designs. We make use of an exam reform at Stockholm University, where all standard exams had to be graded with no information about exam-takers' identity. This reform was put in place at the beginning of the fall term of 2009. To estimate the in-group bias effect, we make use of the fact that the graders were randomly assigned to students.

Using a difference-in-difference (DID) design[5], we first find a positive effect of the anonymous grading reform on the test results of female students. Thus, consistent with the findings reported by previous authors, such as Goldin and Rouse (2000), being evaluated anonymously can improve evaluations of females relative to males. We argue that this effect is likely explained by a gender bias in grading.[6] One explanation for the sign shift of the effects

[5] Since we estimate differential gender effects due to a grading reform, in fact, we estimate a difference-in-difference-in-difference model, which is discussed in section 2.4.

[6] Although we are estimating the causal effect of the anonymous grading reform, we can never be certain that the outcome is due *only* to grading bias. In fact, we can think of a situation where the behavior of the students changes; for instance, they could start to exert more effort as a consequence of the reform. We share this drawback with many prominent studies in the field based on anonymous and non-anonymous observations of this outcome. However, we are able to partly investigate the effort channel by studying the results of multiple-choice questions, which

compared to lower levels could be an in-group bias mechanism, as there are more male graders at the university level than at lower levels. Supportively, when we divided the sample into departments with a majority of female teachers and departments with a majority of male teachers, we find that the effect is driven purely by the departments with a majority of male teachers. However, this finding does not prove that the effect is due to in-group bias as male-dominated departments could well exhibit cultural gender bias that is also shared by female teachers. To separate the mechanisms, we conduct a second experiment. By using a particular exam, namely, the *introductory exam in macroeconomics*, we can collect more detailed information on grader gender, and more importantly, we can utilize a nonintentional *randomized* experiment setting for this exam in which graders of different genders were randomly assigned to correct different questions. First, we confirm the substantial positive effect of the anonymous grading reform on the test results of female students in the subsample using the same type of DID specification as discussed above for the full sample. Then, through the random assignment of grader gender, we can estimate the causal effect of same-sex bias among correctors. We find strong evidence of same-sex bias in the TAs' exam corrections. It is also comforting, from an internal validity point of view, that we find that in-group bias disappears once anonymous exams are introduced at the university. This finding should be a consequence if the reform is effective. Finally, when quantifying the relation between the total effect of being graded anonymously and the in-group bias effect, we find that the in-group bias effect is statistically unrelated to the main effect. Considering the point estimate change at face value, in-group bias accounts at most for 15

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are unaffected by the reform. We credibly find no change close to the reform years, which suggests that females did not change their effort level.

%, indicating that the grading reform effect is mainly determined by factors other than the graders favoring their own gender. Thus, the lion's share of the effect is explained by cultural or institutional factors, independent of the gender/grader match. Consistent with these findings are theories of how gender stereotypes (a genius is male) affect judgement.[7] However, we are not able to distinguish these mechanisms from standard theories of statistical discrimination (Arrow, 1973; Phelps, 1972).

Furthermore, we argue that the economic significance of our findings can hardly be underestimated. There is a growing literature showing that discrimination in grading can have long term consequences on further education and earnings.[8] As more than 45 % of persons aged between 25-34 attend university (slightly below the US rate) and a majority of students are female, the economic consequences could therefore be substantial. Lastly, the effects are also found in the Swedish context, one of the most gender-equal countries in the world, according to, for example, the United Nations Human Development Reports.

An increasing number of studies have investigated the different dimensions of grading bias at the pre-tertiary levels. As a whole, there are two strands in this literature. First and foremost, there are studies investigating the general gender grading bias of teachers, comparing test scores across anonymous and non-anonymous exams. Lavy (2008) looks at the gender bias

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[7] On the issue of a genius being a male, see Elmore and Luna-Lucero (2017). On how stereotypes may affect grading, see the discussion and references in Lavy (2008) as well as the idea of stereotypes and implicit discrimination developed and discussed in Bertrand et al. (2005).

[8] See, for example, Ebenstein, et al. (2016), Lavy et al. (2018), Lavy and Sand (2018), and Lavy and Megalokonomou (2019).

in Israeli matriculation exams in nine subjects among high school students. Using a DID approach, he finds evidence of bias against male students. A similar approach is adopted by Hinnerich et al. (2011).[9] Biased grading has also been studied for other groups. In a randomized field experiment, Hanna and Linden (2012) find that teachers are biased towards lower castes in India, and Burgess and Greaves (2013) find evidence that some ethnic groups are systematically underrated relative to their white peers. Relatedly, Kiss (2013) studies the grading of immigrants and girls once test scores have been taken into account and finds a negative impact on immigrants' grades in primary education.[10] Second, there are studies looking at more reduced-form effects of having a male or female teacher depending on a student's gender. The most notable study in this strand is probably (Dee, 2005), who looks at the effect of students sharing their gender or ethnicity with their teacher in eighth grade and finds a positive effect. A similar approach is taken in (Dee, 2007); however, this study considers more long-run and behavioral responses.[11] It is worth noting that none of these studies are performed at the university level.

There are, however, a few papers using the university as a testing context. Closely related to our work is Breda and Ly (2015). They use oral (non-blind) and written (blind) entry-level exams at elite universities in France and find that females' oral performance is graded better than males' in more male-dominated subjects. Our setting differs from theirs in many ways other than

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[9] See also Cornwell et al. (2013). Moreover, Lindahl (2007) finds that male test scores increase with the share of male teachers, whereas grades decrease at the same rate.

[10] See also Sprietsma (2013) and Hinnerich et al. (2015).

[11] Using Swedish data Holmlund and Sund (2008) find no strong support that a same-sex teacher improves student outcomes

the larger scale of our study. We use a change in policy over time, and the examiners in our study are typically the teachers of the students and not external examiners, as they are in Breda and Ly (2015). We study a standard examination at a large (approximately 30 000 students per year) state-financed non-selective university and we document the overall gender effect of anonymous grading at the university level, and that this effect differs from lower levels of education as female grades are improved compared to male grades. Breda and Ly (2015) estimate a non-linear model (an interaction model), and the overall average effect may still be consistent with our finding.[12]

Another closely related study, particularly in relation to our second design, is Feld et al. (2016), who investigate whether biased grading is driven by teachers favoring their own type (endophilia) or discriminating against other types (exophobia). In their field experiment, they cover approximately 1,500 examinations in 2012 at the School of Business and Economics (SBE) of Maastricht University, where graders were randomly allocated. On average, gender matching seems to be of little importance for grading even though there is some evidence of male graders favoring male students. In our second design where graders are randomly assigned we first document in-group bias using a methodology similar to Feld et al. (2016) with the sample consisting of the exams from the introductory macroeconomics course. Moreover, where Feld et al. (2016) fail to detect either same-sex bias, we document significant effects. Furthermore, we can also relate the in-group bias mechanism to the overall effect of being graded anonymously or not and our findings do suggest that although in-group bias exists, is not

[12] Breda and Ly (2012) show an overall grading bias effect of the same sign and size as ours. See also Breda and Hillion (2016).

key in determining the overall bias in grading. Thus, we are able to separate in-group bias from bias stemming from other match independent factors such as culture or institutions.

Additionally, the effect of teachers as role models at the university level is investigated in Hoffmann and Oreopoulos (2009). Other papers look at how the classroom gender composition affects student performance (Lee et al., 2014) and how the matching of TA/teacher and student ethnicity/gender affects student performance (Coenen and Van Klaveren, 2016; Lim and Meer, 2017a, 2017b; Lusher et al., 2015)

The rest of the paper is organized as follows: section 2 describes the two empirical strategies and data sets that we use, section 3 presents the results, and section 4 concludes the paper.

2 Material and methods

2.1 The grading reform of 2009

The head of school decided on a grading reform on March 5, 2009, and the reform began as a trial for a year starting in the fall term of 2009. The reform included the removal of test-takers' identity on standard exams from the start of the fall term of 2009. In May 2010, the reform was evaluated, and it was decided that the university should continue with anonymous grading on exams. Some implementation problems with the IT system were noted during the trial period in the first year. For example, some students' identities were revealed due to poor IT systems (Stockholm University, 2010). Unfortunately, we did not observe which students might have had their identities revealed during the first year, so we cannot classify and control for this. Thus, we acknowledge that the effect might be dampened during the first year of the reform.

2.2 Data from all graded activities at Stockholm University

We have data on the universe of graded activities from the fall term of 2005 to the fall term of 2013. We consider the fact that unlike standard exams, other graded activities such as theses and oral and home assignments were not anonymized for practical reasons. All departments except the law department were affected by this reform; the law department already had a long-standing practice of anonymous grading.[13] Thus, all examinations at the law department and activities such as thesis work and oral and home assignments at other departments serve as a control group in the DID design.

In the relevant time period, there were three main grading systems in place: the original, consisting of G (pass), VG (pass with distinction) and U (fail); a special grading scheme implemented for most courses at the department of law, consisting of AB (highest), BA (middle), B (lowest) and U (fail); and the system imposed by the Bologna process in the European Union. The Bologna scheme had to be implemented from the fall of 2008 at the latest, although it was used at certain departments and courses before that deadline.[14] The numeration of these different systems is given in Table A1, while Figs. A1-A3 provide histograms for each of them. The histogram plots all appear approximately normally distributed, except for the grades in the

[13] Table A3 shows that our results are robust when we exclude the law department.

[14] However, the department of law still has an exception to this rule. It uses the letters A through F, where A is the highest grade and F (along with Fx) indicates failure. Table A3 shows that our results are robust when we exclude different grading schemes.

department of law.[15] To make the different grading systems comparable, we standardized each of them separately by subtracting the mean and dividing it by the standard deviation.[16]

We collected data from the administrative system Ladok.[17] Our data contain information on the date of the exam, the course, the course credits, and the responsible department as well as basic information on the individual taking the exam. Summary statistics at the student-activity level are provided in Table 1. Table 1, Panel A, shows the data for all graded student activities.

[15] Anecdotal evidence suggests that the department of law strives for normally distributed grading on both main exams and retakes, which could explain why the distribution does not look normal. However, it could also be because being accepted as a law student requires high grades starting in high school. Other anecdotal evidence suggests that students always received the highest grade on their final thesis up until recently. (Dropping all observations classified as theses at the department of law does not change our results.)

[16] It is important to note that although all departments had to adopt the new grading scheme by the start of the fall 2008, some students still received grades from the old system (i.e., VG-U) after that point. This occurred for two reasons. First, certain courses are still either graded as pass (G) or fail (U); these courses are typically seminars requiring attendance or hand-in assignments. However, if a student first registered into a course when the old grades were still in use in that department, failed first and then passed it later on when the new A-F grades had been introduced, that student would still be awarded a grade on the VG-U scale.

[17] We should note that we drop the department "Lärarutbildningskansliet" since it was not a formal department over the full period and was affected by massive reforms.

We find that there is a majority of female students for these activities (63 percent) and that students are, on average, 28 years old.

Table 1. Summary statistics

	(1) Mean	(2) S.D.	(3) Min.	(4) Max.
Panel A: Full sample				
female student	0.628	0.483	0	1
age	28.196	8.964	16	88
papers and hand-ins	0.169	0.375	0	1
department of law	0.065	0.247	0	1
treated	0.768	0.422	0	1
after fall 2009	0.565	0.496	0	1
Observations	1830461			
Panel B. Introductory macroeconomics sample				
female student	0.488	0.410	0	1
female grader	0.325	0.468	0	1
same-sex grader	0.499	0.500	0	1
after fall 2009	0.799	0.401	0	1
retake	0.209	0.407	0	1
age of student	23.231	4.156	18	71
essay	0.934	0.248	0	1
multiple choice	0.066	0.248	0	1
Observations	51177			

The data do not explicitly document whether the grades were for a written exam (graded anonymously after the fall of 2009). To identify examination forms that were still not anonymous after the introduction of the reform, we made use of the fact that graded activities come with a text-based name indicating the type of examination. For example, a bachelor's thesis grade comes with text stating "thesis." Since thesis and term papers are never anonymously graded, as the name is written on the front page, we coded them as being non-anonymous. Other examination forms that can never truly be anonymously graded are lab assignments and different types of presentations requiring physical attendance. We define non-anonymously graded activities by searching through the column of text indicating examinations of these types. For example, if the word “thesis” or “home assignment” is found, that activity is coded as non-anonymous. We thus obtain a dummy indicating whether we know that a particular test is always non-anonymous even from the fall of 2009 and onwards. We then combine this with all examinations from the department of law that were anonymous throughout the entire period.[18] Our treatment group is then residually determined. We admit that we face a potential measurement error by misclassification.[19] However, this error would imply, if anything, that we were underestimating the true effect.[20] Table 1, Panel A, shows that 77 percent of the activities are classified as affected (treated) by the reform and it also shows that approximately half of the data is generated *after* the reform in the fall of 2009.

[18] For the entire coding, contact us for the code file (Stata).

[19] The misclassification problem when using the full population is also one motivation for why we subsequently focus on the data set from the department of economics, since treated and non-treated activities are clearly categorized in that setting. Furthermore, we can use a more precise outcome since we observe the students’ score on each question, which varies between 0 and 10.

[20] The logic behind this is simple: since we determine treatment status residually, we will likely classify some activities as treated when they are in fact not treated. Hence, our treatment indicator will capture some of the effect of the non-treated activities, thus biasing our estimates towards zero. This is usually referred to as classical measurement error and attenuation bias.

2.3 The introductory macroeconomics sample

For our second design, we hand-collected more detailed information from one particular exam. We employ data from the macroeconomics exam of the introductory course at Stockholm University from the spring of 2008 to the fall of 2014. This approach allows us to estimate possible in-group bias and to evaluate the importance of this bias, as the graders were randomly allocated to questions by ballot. In addition to the random assignment of teachers, which is enough to consistently estimate the in-group bias effect, our design is supported by the fact that this course was affected by the grading reform of 2009. This gives us the opportunity to replicate the main effect and to perform sanity checks, as the in-group bias effect should vanish when anonymous grading is implemented. Lastly, we can determine how much of the total effect that can be accounted for by in-group bias.

The data on student performance were collected from the course administrator and the course coordinator. The main benefit of the introductory exam is that it consists of two multiple-choice questions as well as seven essay questions. Before the fall term of 2013, each question was worth ten points, while starting in that term, the essay questions were worth 12 points and the multiple-choice questions were a prerequisite for eligibility to take the exam.[21]

[21] Details regarding the exam and the correction process are described in the appendix. To make the 12- and 10-point questions comparable, we standardize the points for each set of questions. Dropping the exams with 12 points for each question does not alter our results in any major way.

The 7 essay questions were corrected by a separate TAs who were assigned to the specific questions by ballot, thus creating a nonintentional experiment.[22] The first names of the TAs were collected from the course coordinator's correction templates and then typed into a spreadsheet by hand. These two sets of information were then merged together. Table 1, Panel B, provides some key characteristics of the collected data. In total, 49 % of the students are female, while most exams are from the anonymous period, and most TAs are male. Hence, if the male students performed better than the female students on average, we would overestimate a positive in-group bias effect simply because the most TAs are male. Thus, we must condition on the female students' average score in both the pre- and post-anonymization periods when we estimate the in-group bias effect. Since we collected both the gender of the students and the name (and thus the gender) of the TAs assigned to each question, these exams provide an optimal context for studying possible same-sex bias effects. More specifically, the randomization of TAs to questions ensured that there was no selection by gender or ability into questions of different difficulty levels. It is thus possible to compare one student's score on each question depending on whether the corrector shared their gender, as long as we condition on the average performance of each gender in order to avoid including general gender discrimination into our estimates.[23]

[22] The exam also contains an 8^{th} essay-like question that a typical student does not have to answer due to a credit system. Hence, these questions are excluded from the analysis.

[23] It is important to note that the gender of the corrector is unknown to the student at the time the exam is taken.

When testing for in-group bias as such, there is no need to have a control group unaffected by the reform, as we can rely on the randomization of graders. However, for the replication of the main bias effect and the subsequent quantitative analysis, we need the combination of randomization and the grading reform as exogenous variation. When using the introductory macroeconomics sample in combination with the grading reform, we use a different control group, namely, the multiple-choice questions. It is impossible, or at least very costly, to correct multiple-choice questions with a bias because only one correct answer exists and is publicly known and postulated. The treatment group is the essay questions. There is no measurement error when classifying the graded activities in this sample.

For brevity, in the empirical specifications, we define the exam and essay questions from the introductory macroeconomics exam as the *treated* group and theses, oral assignments, home assignments, exams at the department of law and multiple-choice tests as the *control* group. We are fully aware that our different assessment types may measure different skills and that the difference between the treated and control groups in a given cross section is not informative with respect to grading bias, as we would be comparing apples and oranges. Fortunately, we can make use of the time dimension and the policy intervention in a DID setting. Consequently, the control and treatment test types may well be measuring different skills without posing a threat to internal validity. The important assumption is that for each assessment type, treatment and control, the difference in the test scores between the sexes should move in parallel over time in the absence of anonymization, representing a regularity that can be partially evaluated by estimating pre-trends across series.

2.4 Empirical designs

2.4.1 The effect of anonymization on females' grades

Our main approach uses a reform that required the removal of test-takers' identity on standard exams starting in the fall of 2009. This reform is used to study the differential effect with respect to gender. This implies that we use a fully interacted difference-in-difference-in-difference (DDD) model (Katz (1996), Yelowitz (1995)), although for simplicity, we continue to refer to it as a DID model as discussed below. Our estimating equation is:

$$(1)\ testscore_{ijt} = \delta_0 + \delta_1 female_i * after_t * exam_j + \delta_2 after_t * female_i + \delta_3 female_i * exam_j + \delta_4 after_t * exam_j + \delta_5 after_t + \delta_6 female_i + \delta_7 exam_j + \varepsilon_{ijt}.$$

Thus, we study test/question-type *j* for individual *i* during time period *t; exam* is an indicator that assumes the value of one if it is an exam question when using the full sample or essay question when using the introductory macroeconomics exam. Thus, this dummy indicates which test type is being treated by anonymous grading. This variable assumes the value of zero if it is a thesis when using the full sample or multiple-choice question when using the introductory macroeconomics exam. Thus, equation (1) can be used to estimate the total effect of anonymization in *both* samples. The dummy *after* assumes a value of one for the time period after anonymization was implemented in the fall of 2009;24 and female is a gender dummy. The variable of interest is the triple interaction $\text{female}_i * \text{after}_t * \text{exam}_j$. The coefficient of interest is δ_1, and it measures the effect of anonymization on female grades compared to male grades. Although the estimating equation seems complicated, the identifying assumption is similar to a standard DID design but is applied to the *difference* in test scores between the sexes. Hence, for internal validity, we need the difference in test scores between sexes to move in parallel in the absence of anonymization across the two test types. Under that identifying assumption, we estimate δ_1 with no bias, and it represents the causal effect of anonymization on female grades compared to male grades. To test this identifying assumption, we estimate time-separate treatment effects over time in accordance with Angrist and Pischke (2008). We estimate δ_1 first using the full sample of graded activities and then using the introductory macroeconomics sample.

Moreover, we acknowledge that the estimations of the standard errors are problematic in this type of DID setting since the treatment changes only once for one group (standard written exams), as discussed by Bertrand et al. (2004), Donald and Lang (2007) and Conley and Taber (2011). We begin by clustering the standard errors at the student level. However, in appendix Table A2, column 2, we follow Pettersson-Lidbom and Thoursie (2013) application of the results in Donald and Lang (2007). We aggregate the data to the group level twice, estimate a time series model with a structural break, and use standard errors that are robust to heteroscedasticity

[24] In Table A2 in the appendix, we allow for a flexible modeling of time, such as by expanding after to be month fixed effects. The results are not sensitive to a more flexible modeling of the time fixed effects.

and serial correlation by applying the Newey-West estimator with one lag. Our results are robust to this treatment.

2.4.2 In-group bias

A part of the treatment effect in equation (1) could be determined regardless of the graders' gender, but another part could be attributed to in-group bias. In-group bias in our context is the inclination of teachers to give superior grades to students who belong to the group with which they identify. In equation (1), δ_1 could capture both, particularly as most teachers at Stockholm University are men. The main benefit of the data from the introductory macroeconomics course is that the graders are randomized. This is enough to consistently estimate a pure in-group bias effect based on the period before anonymous grading was implemented as:

(2) $testscore_{it} = \vartheta + \lambda_4 same\_sex\_grader + \epsilon_{it},$

where $same\_sex\_grader$ is a dummy variable for cases in which the student answers the question and the grader correcting it has shares the student's gender and λ_4 measures in-group bias. We could also verify that in-group bias disappears when the anonymous grading reform is introduced by including and interaction with *after* as:

(3) $testscore_{it} = \alpha + \lambda_4 same\_sex\_grader + \lambda_5 same\_sex\_grader * after_t + \pi_{it}.$

2.4.3 Relating in-group bias to the overall effect of anonymous grading

To further understand the degree to which the treatment effect is culturally determined and how much is explained by in-group bias we can unfortunately not simply add interacted indicators of same-sex graders to equation (1). The macroeconomics exam consists of two types of questions: essay questions and multiple-choice questions. Although we perfectly observe the grader's identity for the essay questions, we do not observe the grader's identity for the multiple-choice questions. Thus, we have no control contrast in the multiple-choice questions dimension, ruling out the simple expansion of equation (1). To separate the in-group bias effect, we first need to be able to estimate the treatment effect by relying on a before-and-after design and studying only the essay questions of the introductory macroeconomics exam. In fact, the two models (DID and before-and-after) are equivalent under one assumption. Specifically, we need the difference in gender ability in exam performance to be constant from the control period to the treatment period. This corresponds to $\delta_2 = 0$ in equation (1), a condition that we will test for. Under this condition, the treatment effect can be retrieved by estimating a simple before-and-after model as follows: [25]

(4) $testscore_{it} = \delta_0 + \delta_1 female_i * after_t + \theta_2 female_i + \theta_3 after_t + e_{it}$,

where $\mathrm{testscore_{it}}$ measures the essay questions of the introductory macroeconomics test score. The variable of interest collapses to $\mathrm{female_i} * \mathrm{after}_{\mathrm{t}}$ and again the coefficient of interest is δ_1, and it measures the effect of anonymization on female grades compared to male grades. Next, we

[25] This is verified in a simple simulation exercise in Stata in the file generating the main results and can be shown analytically as in appendix 6.2. See also Cameron et al. (2005, pp. 55).

can separate the in-group bias effect from the total effect by using the following regression equation:

$$(5)\ testscore_{it} = \lambda_0 + \lambda_1 female_i * after_t + \lambda_2 after_t + \lambda_3 female_i + \lambda_4 same\_sex\_grader + \lambda_5 same\_sex\_grader * after_t + u_{it},$$

First, if $\hat{\lambda}_1$ differs from $\hat{\delta}_1$ estimated in equation (2), a part of the overall bias effect is due to in-group bias. Moreover, λ_4 measures in-group bias, and λ_5 measures whether the in-group bias changes after the introduction of anonymous grading. Again, λ_4 and λ_5 are estimated consistently due to randomization.[26] No other assumption is needed for these parameters.[27] With regard to the standard errors in this specification, we use a two-way cluster at the individual and TA levels.

3 Results

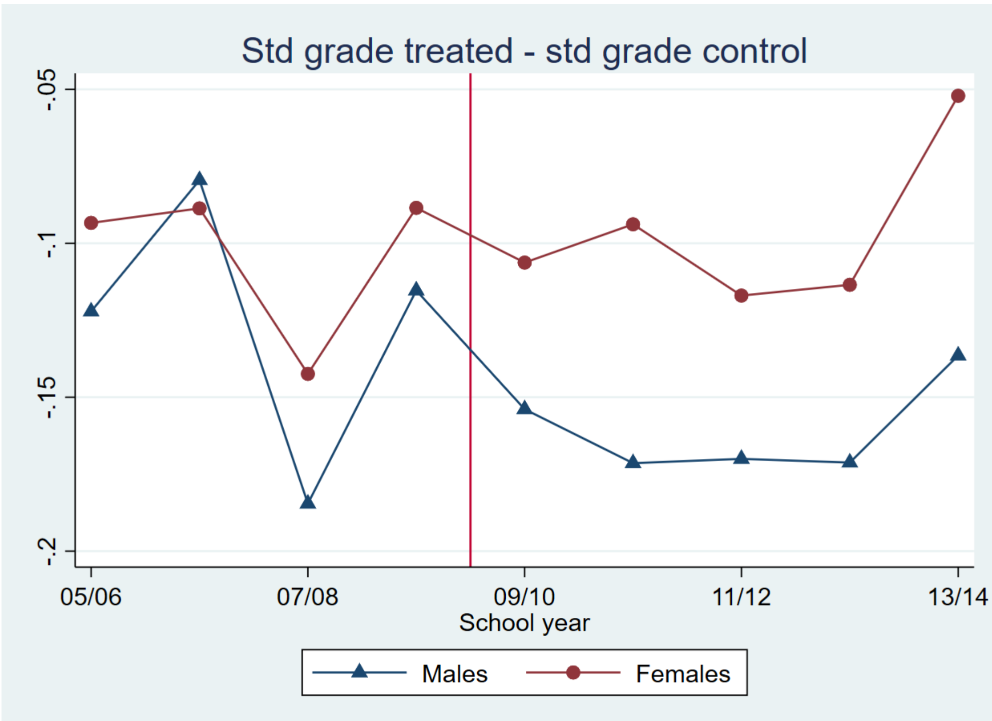
3.1 The treatment effect of female students being graded anonymously

In this section, we present our results of the treatment effect of females being graded anonymously. Since the underlying assumption is the parallel trends assumption, we begin by plotting the difference in standardized grades across gender as a two-time series (Fig. 1, Panel A). Panel A displays the difference between the control and treatment test types over time for

[26] As a validity check, we present different combinations of estimating the in-group bias

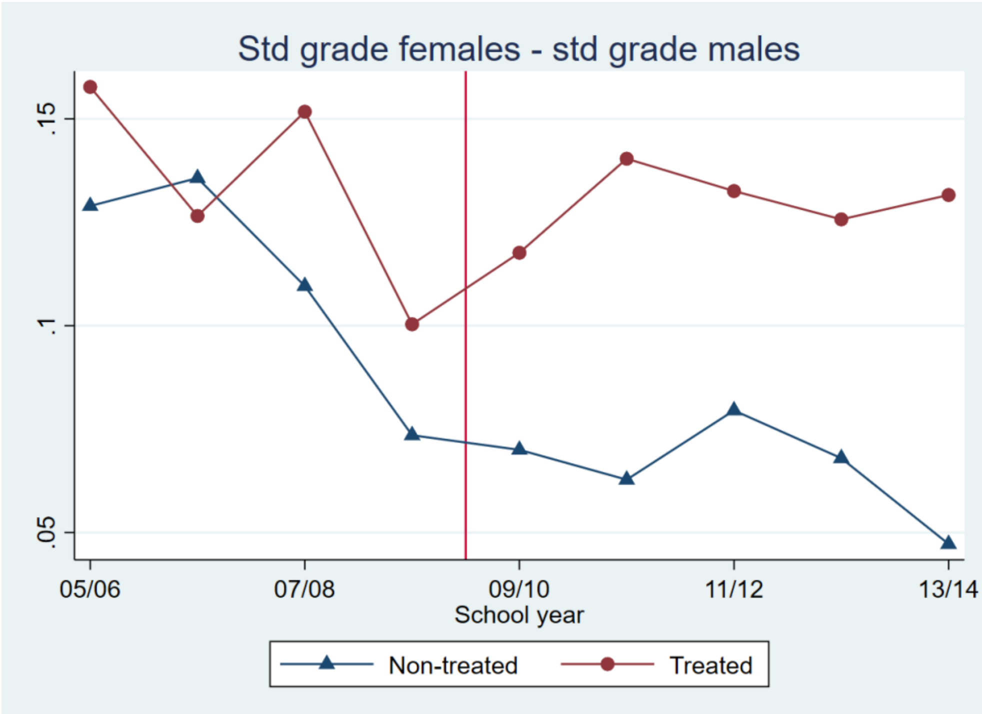
[27] The balance tests of a few available variables are presented in Table 3.

male and female students. We can see that prior to the reform, the series exhibit similar trends and levels. For both genders, we see an effect of lower grades due to anonymization, in line with previous evidence. [28] However, male students lose more by being graded anonymously, and the time series separates.

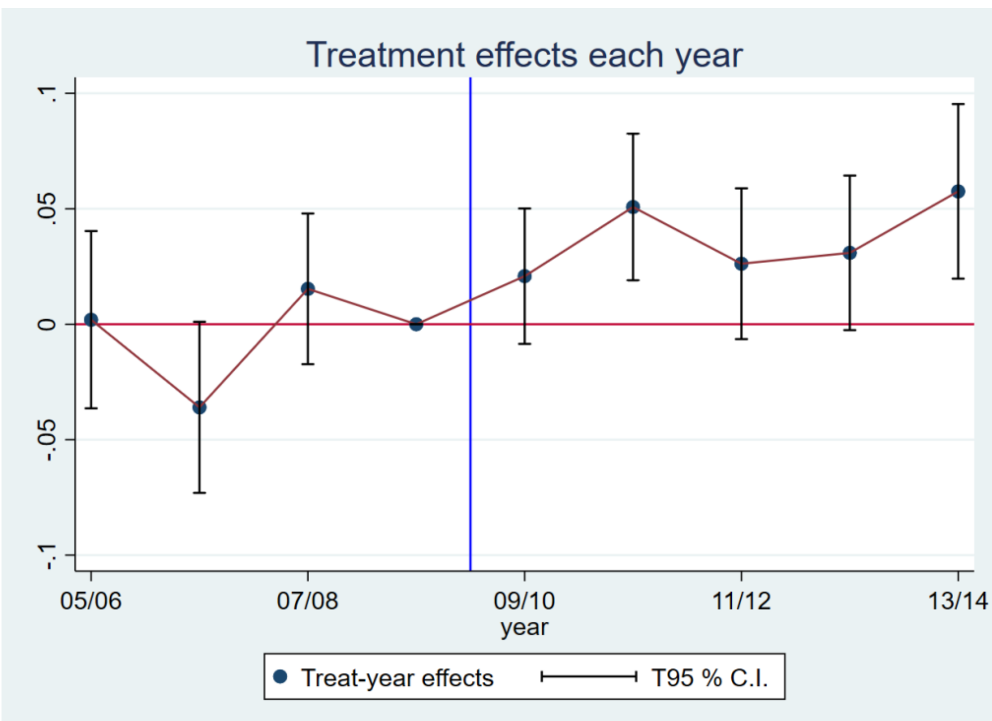


Panel A: Difference in levels between treated- and control activity scores of males and females.

[28] See, for example, Lavy (2008) or Hinnerich and Vlachos (2017). One could also infer "annual" treatment effects of the anonymity grading reform as shown in both genders separately in Panel A. If we normalized the year before treatment to a baseline year as in Panel B, we would detect noise before the reform and lower grades after the reform for 4 periods.



Panel B: Difference in levels between males' and females' scores in the treated and control activities



Panel C: Annual effects across the treated and non-treated groups.

Note: Standard errors clustered at the individual level.

Fig. 1 Impact of anonymous grading

In Panel B, we plot the difference between the male and female test scores in the control and treatment test types over time. As a validity check, we are interested in the degree to which the treatment effect is driven by changes in the control dimension. As shown in Panel B, both the treated and non-treated activities exhibited a similar downward trend before the reform. This finding indicates that the positive female grade gap is closing over time. However, after the reform, the downward trends in the grade gap are abruptly halted in the treated activities and the gender gaps remain rather constant for treated activities. Reassuringly, the downward trend in the control activity gender gap continues also after the reform. Thus, we conclude that Panel A in Fig. 1 supports the parallel trend assumption and that Panel B demonstrates that the change in the trend of the gender gap is caused by the treated activities.

Moreover, as proposed by Angrist and Pischke (2008), we plot annual treatment effect ($\hat{\delta}_{1t}$) estimates from a regression analysis of both before and after the implementation of the reform, showing "placebo" effects before the reform and dynamic causal effects after the reform (Fig. 1, Panel C). Panel C quantifies this difference during the post-treatment period and plots the coefficient of the treatment effect over time. Not surprisingly, the estimates are fairly stable around zero in the pre-treatment period and then increase in the post-treatment periods, with the estimates being consistently positive, in contrast to the pre-treatment period. We conclude that even if the test types may measure different skills, there is strong evidence that this does not pose a threat to internal validity, as the parallel trends assumption seems likely to hold. There is some evidence of a dynamically growing treatment effect, as the first year post-treatment effect is smaller than that in the second year. The somewhat delayed treatment effect could be in line with the evidence of the discussed reform implementation problems, but given the rather sizable standard errors, we cannot rule out a stable treatment effect.

We continue with our regression results, which are presented in Table 2. Column 1 reports the results from a regression corresponding to equation (1). As observed, the anonymous examination raises female grades relative to male grades by approximately 0.04 of a standard deviation. For males, there is a 0.042 decrease in grades on exams (column 1, row 2), while for females, there is an *additional* effect the 0.04 increase discussed above. Thus, female students' exam scores are only slightly lowered by anonymous grading, while male students' exam scores are lowered to a larger extent (relative to the development of the control test type). Overall, this finding suggests an average decrease of approximately 0.02 of a standard deviation in exam scores due to anonymous grading, in line with previous research as discussed before.[29] In column 2, we present the results from a regression corresponding to equation (1), but this time, we use the number of course credits as weights, giving more weight to more important examinations. As the introductory macroeconomics course is as extensive course accounting for 15 ECTS-points and since many minor courses in the full sample are only pass or fail and thus allow limited room for biased grading, we argue that this is an important specification for subsequent comparability with introductory macroeconomics designs. We expect our estimates to increase when we assign more weight to more important examinations. Indeed, the coefficient increases slightly, indicating that the effect is larger for more important examinations. Another way to increase

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[29] The calculation of 0.02 is based on the assumption of 50 % female students. In fact, a DID estimate of the effect of anonymization yields 0.018.

comparability with the macro exam is to use only activities with a similar ECTS weight. In column 3, we estimate the model in equation (1) using only graded activities accounting for 15 or more ECTS points. We conclude that the estimate of 0.066 is of the same magnitude as the weighted estimate and is highly significant. Moreover, we conclude that approximately 75 % of the sample is still included after excluding activities with less than 15 ECTC credits.

Table 2. Overall gender grading bias

	(1) Full Sample, Using DID Design	(2) Full Sample, Using DID Design	(3) 15 ECTC Sample, Using DID Design	(4) Macro Sample, Using DID Design	(5) Macro Sample, Using Before-and-After Design
female*after*exam	0.040***	0.063***	0.066***	0.085**	
	(0.011)	(0.011)	(0.011)	(0.038)	
exam*after	-0.042***	-0.030***	-0.035***	-0.15***	
	(0.0085)	(0.0084)	(0.0085)	(0.027)	
female*exam	0.023**	-0.018**	-0.010	-0.071**	
	(0.0094)	(0.0087)	(0.0086)	(0.032)	
female*after	-0.046***	-0.064***	-0.061***	0.009	0.10**
	(0.0091)	(0.0085)	(0.0090)	(0.043)	(0.040)
exam	-0.12***	-0.15***	-0.14***	-0.42***	
	(0.0073)	(0.0069)	(0.0067)	(0.023)	
female	0.11***	0.15***	0.14***	-0.041	-0.11***
	(0.0080)	(0.0070)	(0.0073)	(0.038)	(0.037)
after	-0.062***	-0.062***	-0.080***	0.088***	-0.065**
	(0.0066)	(0.0060)	(0.0065)	(0.031)	(0.027)
Constant	0.070***	0.094***	0.083***	0.38***	0.065***
	(0.0057)	(0.0050)	(0.0053)	(0.028)	(0.025)
Course credits weights	No	Yes	No	No	No
N	1830461	1830461	1349181	49700	51177

Note: Standard errors clustered at the student level. The dependent variable is the standardized score. The period in the first three columns is from autumn 2005 to autumn 2013; in the fourth column, the period is from spring 2008 to spring 2013; and in the fifth column, the period is from spring 2008 to autumn 2014. In the fifth column the treatment effect is estimated by equation (4) and thus female*after is the variable of interest.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In column 4, we present the results from estimating equation 1 but where we use the subsample from the introductory macroeconomics course.[30] We observe a slightly higher coefficient of approximately 0.085 of the standard deviation compared to the full sample, which is more similar to the weighted estimate. However, this finding is consistent with the fact that we see no measurement error in relation to the dependent variable and hence have no attenuation bias, in contrast to the previous design.

Notably, the coefficient of *female*$*after$, δ_2 in equation (1) is very close to zero (0.009) and is far from significant at any level. This enables us to use a before-and-after design and still obtain an unbiased estimate of δ_1 in this setting, as discussed in section 2.4.3. Furthermore, it suggests that the estimated bias is not due to increased effort by female students, since *female** $after$ measures the change in females' performance on multiple-choice questions in relation to males' performance. Arguably, any increased effort by females to answer to the essay questions better should spill over to some degree into the multiple-choice questions. Thus, this is evidence that female students are not exerting a different level of effort when grading is anonymous.

The results from a before-and-after regression (estimating equation 4) are presented in column 5. Recall that if δ_2 is zero in equation (1) then *female*after* is the variable interests and

[30] Figure A4 in the appendix shows the equivalent of the event study graph shown in Panel C of Figure 1 but for introductory macroeconomics sample. Due to the short period of observation before the reform, we show semester effects instead of yearly effects. Aggregating to a lower frequency could provide very minimal guidance regarding the parallel trends before the reform. Overall, A4 is well aligned with the results shown in Panel C of Figure 1.

the relevant regressor in order to estimate δ_1, the effect of anonymization on female grades compared to male grades. We note that the coefficient is essentially unchanged at approximately 0.1 of the standard deviation and is still highly significant. The before-and-after design uses only essay questions, as discussed previously, and for this test type, we are able to retrieve another year of data that explain the increase in the number of observations between columns 4 and 5.

Table A2 in the appendix provides additional control specifications to the main estimates. Column 1 replicates the first column in Table 2 above; however, column 2 presents the results from a regression on the collapsed data as a time series. We note that the standard error is essentially unchanged compared to the first column. Moreover, the fact that aggregation leaves the estimate unchanged makes it likely that any potential compositional bias is of little importance. In column 3, we restrict our analysis to a narrow window surrounding the reform date (2007-2011). The effect is smaller, indicating that there is some evidence of a dynamically growing treatment effect consistent with the evidence suggesting reform implementation problems, but given the sizable standard errors, the effect is not statistically distinguishable when comparing columns 1 to 3.

In column 4, we include nonparametric gender- and exam-specific trends.[31] This is possible thanks to the DDD-like identification design and adds more flexibility in how time is allowed to affect the model. The estimate decreases slightly, though it is still close to the coefficient in column one. Hence, the results in this column further support the credibly of our design, as the estimated effect does not seem to be driven by unobserved trends. In column 5, we

[31] In other words, we include gender*month fixed effects and treatment group (exams or papers)*month fixed effects.

add department fixed effects. Thus, all time-invariant department factors are controlled for such that some departments have a larger share of home assignments or other specific types of tests. The estimate decreases to 0.022, which, taken at face value, may be interpreted as a large decrease in the effects. However, as discussed by Wooldridge (2002), fixed effects transformations “exacerbate” classical measurement errors. Thus, we argue that even though we add department fixed effects in a setting of attenuation bias, we still retrieve a positive and statistically significant effect, although it is somewhat attenuated. Column 5 includes individual fixed effects, and column 6 uses only a sample of students who appear in all test-type activities both before and after the reform, resulting in a loss of precision as we use only students who appear in all test-type activities. Table A3 reports the same information for activities of 15 or more ECTS credits. The pattern is rather similar, although the point estimates are larger.

Additional robustness tests are performed in Table A4 in the appendix. The first column replicates column 1 in Table 2, column 2 excludes the department of law from the analysis entirely, and columns 3 and 4 restrict the analysis to A-F grades and A-F grades during the mandatory period, respectively. All of these restrictions slightly increase the coefficient. Finally, column 5 alternates the numbers from Table A1 such that B for law students is 1, BA is 2 and AB 3, while G is 1 and VG is 2. Reassuringly, this does not change any estimate at all likely because we standardize each grading scheme. Thus, our results are not driven by changes in the enumeration of grades.

A very interesting result reported by Lavy (2008) is that external graders with no personal ties to the students grade with no bias even though they know the gender of the student. Thus, biased grading is driven by a combination of some repeated personal interaction. Table 3 presents results based on the approximation of the number of participants in a course. Here, we approximate the size by exam type, course and term. Interestingly, we observe that the effect is visible in smaller classes but not in large classes, implying that large classes act as a de-biasing device.

Table 3. Effect depending on the course size

	(1) stand. score	(2) stand. score	(3) stand. score
female*after*exam	-0.0057	0.052^{***}	0.038^{***}
	(0.022)	(0.010)	(0.012)
Course participants	More than 99	Less than 100	Less than median (48)
N	548326	1282135	910550

Note: Standard errors are clustered at the student level. The dependent variable is the standardized score.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As discussed in the introduction, literature documents gender differences that depend on subjects, departments and majors.[32] For example, the gender composition of teachers is likely to vary depending on the department and major. If the main effect is driven by the in-group bias mechanism and the fact that there are more male teachers at the university level, then it is reasonable to expect the grading bias to vary across departments with different gender compositions. Table 4 below divides the sample into departments with a 2/3 majority of female teachers (Column 1), a rather gender balanced group below 2/3 to above 1/3 (Column 2) and

[32] See, for example, Breda and Ly (2015) for a discussion on the literature.

departments with a clear minority of female teachers below 1/3 in Column 3.[33] The picture is clear. The effect is monotonically decreased in the share of female teachers. These results are consistent with an in-group bias mechanism. However, the results are also consistent with the finding that some departments are culturally more masculine, i.e., it may well be that the effect is explained by some departments/fields being more masculine as they attract more male teachers, contain a masculine culture and discriminate against women regardless of the teacher/student gender match. [34] Consistent with these findings are theories of how gender stereotypes (a genius is male) affect judgement, as discussed in the introduction. To further dissect the total effect, we proceed by explicitly estimating the in-group bias effect and attempt to judge the degree to which the total effect can be explained by this channel in the next section.

Table 4. Effect depending on the share of female teachers in the department

	(1) stand. score	(2) stand. score	(3) stand. score
female*after*exam	-0.049 (0.036)	-0.031* (0.017)	0.081*** (0.018)
Share of female teachers	Above a 2/3 share of female teachers	Between 1/3 to 2/3 share of female teachers	Below 1/3 share of female teachers
N	267913	666716	564467

[33] In appendix A5, we also show the results obtained after dividing the department into simple majority of females and males; moreover, we show that the results are robust when defining the majority based on yearly updates of the share of female teachers.

[34] The opposite of the female dominated departments may also hold true, even though the estimates are not statistically significant in columns 1 and 2 in Table 3

3.2 In-group-bias? Results of the introductory macroeconomics sample

As discussed in section 2.4.2, to estimate the in-group bias consistently, we need the graders to be randomized. As a consequence, the background characteristics should be balanced across the grader types. Table 5 shows the background characteristics we include as outcome variables in the regressions using a dummy for the TA gender. If the TAs are successfully randomly assigned to the questions, then the question characteristics and student characteristics should be the same for male and female TAs.[35] Column 1 starts by comparing the question number between male and female correctors. Indeed, there is no significant difference between the genders. The second column then examines the probability that a female TA corrects a female student's exam. If female TAs corrected questions that female students found easier to answer, we might see that female TAs were more likely to correct answers by female students because fewer females answered the questions corrected by male TAs. However, if anything, the reverse seems to be true, as we find a small negative coefficient that is significant at the 10 percent level. As the coefficient is so small, approximately 1 percent with a baseline of 49 percent, we argue that it is to be interpreted as a rather precisely estimated zero that should not cause any concern. Next, the third column looks at the age of the answering student, following a similar reasoning as in column 2. Again, the coefficient is very small and indicates that female TAs correct questions answered by students who are 0.08 years younger, though the estimate is insignificant. Finally, column 4 shows the probability that females are more likely to correct questions on retake exams. Since randomization takes place within exams, it could be the case

[35] The latter is an indication that certain students do not avoid answering questions corrected by, for instance, females.

that there is still sorting in gender across exams, though the fixed effects of questions, added as controls in the appendix, take care of any such bias. Nonetheless, it is reassuring to see an insignificant coefficient. In total, this finding shows that our in-group bias effects are likely to be consistently estimated.

Table 5. Randomization of TAs to questions

	(1) question number	(2) female student	(3) age of student	(4) retake
female teacher	0.083	-0.011*	-0.080	-0.016
	(0.44)	(0.0059)	(0.051)	(0.068)
Constant	6.22***	0.49***	23.3***	0.21***
	(0.30)	(0.0072)	(0.064)	(0.028)
N	51177	51177	51177	51177

Note: Standard errors clustered at the TA (49 clusters) and student (6 521 clusters) levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As the second design also passes the internal validity check, we proceed with the estimation. We start by estimating in-group bias in a separate model of the pre-reform period. Due to randomization, we should estimate the in-group bias effect consistently. In column 1 in Table 6 we estimate equation (2) and we show that being graded by one's own sex increases grades by 4 % of a standard deviation. In column 2-5 we include an interaction with *after* and estimate equation (3). Starting with column 2, the in-group bias disappears during the period in which the graders do not know the sex of the students due to anonymization as the interaction is approximately the same size as the pre-reform effect. At the bottom of the table, the row "Sum treatments" gives the sum of coefficients λ_4 and λ_5 in equation (3), i.e., the sum of the same-sex coefficients before and after anonymization, respectively. As observed, this estimate is close to zero for all columns 2-5. The row below provides the p-value of a Wald test of the hypothesis that $\lambda_4 + \lambda_5 = 0$, which cannot be rejected. Thus, removing the student's name from the exam seems to be sufficient to prevent same-sex bias in correctional behavior. Since many suspect that content and handwriting style may also signal gender after the anonymization reform, this is indeed an interesting finding.[36] In column 3, we add TA fixed effects, while in column 4, we add question-specific fixed effects, and the coefficients in essence are unchanged. This finding is reassuring as the randomization of TAs to questions seems to have worked.[37] Column 5 adds gender-specific nonparametric trends, i.e., the female student dummy multiplied by the date of the exam fixed effects. This step was performed to ensure that the estimated same-sex effects are not driven by any underlying trends in gender performance at the cost of not being able to estimate the female student coefficients in Table 6. Since the in-group bias coefficients are essentially unchanged, we conclude that this does not seem to be a concern.

[36] However, Breda and Ly (2015) demonstrate that female handwriting is not easily distinguishable from male handwriting.

[37] Notably, the question-specific fixed effects are even more flexible and reliable than the TA fixed effects.

Table 6. Results of in-group bias with additional controls

	(1) stand. score	(2) stand. score	(3) stand. score	(4) stand. score	(5) stand. score
same_sex_grader	0.044***	0.041***	0.050***	0.041***	0.037***
	(0.010)	(0.011)	(0.010)	(0.011)	(0.013)
after* same_sex_grader		-0.033**	-0.044***	-0.033**	-0.029**
		(0.015)	(0.015)	(0.015)	(0.014)
Sum treatments		0.0085	0.0061	0.0085	0.0073
P-value		0.41	0.60	0.41	0.084
Question FEs	No	No	No	Yes	Yes
Gender-specific trends	No	No	No	No	Yes
TA Fes	No	No	Yes	No	No
N	10323	51177	51177	51177	51177

Note: Standard errors are clustered at the TA (49 clusters) and student (6 521 clusters) levels.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.3 Is the anonymization effect due to in-group-bias?

In this subsection, we combine the two designs into one model. In the first column in Table 7, we expand on the model estimated in Table 2 in column 4 and investigate the importance of same-sex bias in relation to the main effect (equation 5). The first column in Table 7 shows the estimation results when we include the in-group bias variables. We conclude that having a teacher-student gender match raises the points on the question by 0.044 of a standard deviation. Again and importantly, from an internal validity perspective, this effect also returns to zero as soon as anonymous exams are introduced. At the bottom of the table, the row "Sum treatments" gives the sum of coefficients λ_4 and λ_5 in equation (5), i.e., the sum of the same-sex coefficients before and after anonymization, respectively. As observed, this estimate is close to zero. The row below this one then provides the p-value from a Wald test of hypothesis that $\lambda_4 + \lambda_5 = 0$, which cannot be rejected. These results reassuringly mimic the results shown in Table 6.

In columns 2-4, we control for a dummy indicating whether the exam is a retake exam and control for the age of the student. Overall, the in-group bias estimates remain stable.

Table 7. The effect of anonymization and in-group bias

	(1) stand. score	(2) stand. score	(3) stand. score	(4) stand. score
female*after	0.086^{**}	0.082^{**}	0.085^{**}	0.081^{**}
	(0.039)	(0.037)	(0.039)	(0.037)
same_sex_grader	0.044^{***}	0.044^{***}	0.044^{***}	0.044^{***}
	(0.010)	(0.0097)	(0.010)	(0.0097)
after * same_sex_grader	-0.030^{**}	-0.034^{**}	-0.030^{**}	-0.034^{**}
	(0.014)	(0.014)	(0.014)	(0.014)
after	-0.041	-0.028	-0.041	-0.027
	(0.098)	(0.094)	(0.098)	(0.094)
female	-0.087^{**}	-0.088^{***}	-0.087^{**}	-0.087^{***}
	(0.034)	(0.032)	(0.034)	(0.032)
Constant	0.032	0.089	0.15	0.17^{*}
	(0.075)	(0.077)	(0.096)	(0.094)
Sum treatments	0.014	0.0097	0.014	0.0096
P-value	0.16	0.38	0.17	0.38
Retake dummy	No	Yes	No	Yes
Age of student	No	No	Yes	Yes
N	51177	51177	51177	51177

Note: Standard errors clustered at the TA (49 clusters) and student (6 521 clusters) levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Finally, we aim to discuss the effect of anonymization after we include the "same sex grade" indicator. The bias coefficient decreases from approximately 0.010 to 0.086, altered by approximately 0.014 (comparing Table 2, column 4, with Table 7, column 1). Considering the point estimates at face value, it seems that only a small part (approximately 14-16 % depending on the base) of the gender difference is due to in-group bias. However, we must acknowledge that the estimate shown in Table 7 in column one is not statistically distinguishable from that shown in Table 2 in column 4. Therefore, we conclude that the overall effect is independent of the in-group bias mechanism and that the evidence is consistent with grading bias stemming from other match independent factors, apart from the in-group bias effect.

4 Conclusions

There are few studies investigating biased grading at the university level. Bias at the university level is important since it typically is not enough for students to be accepted at a university to get a job in their field. For many jobs a master's degree is necessary and admission is highly competitive and based on grades from the bachelor level. Furthermore, students' choice of courses, and ultimately the degree they end up with, might depend on the signals they receive from their grades in that area, as suggested by the model presented in Mechtenberg (2009). We find evidence of overall bias against female students.[38] This finding is in sharp contrast to most of the literature studying bias before the university level, which has typically found bias against boys or no effects.

Biased grading could in theory be explained by in-group bias. At first glance, the reversed sign compared to studies at lower levels seems to verify this explanation, since a major difference between the university level and lower levels of education is that a majority of the teachers are male. We also document that the effect is driven by departments with a majority of

[38] Our results suggest that the estimated effect is consistent with biased grading and is not due to changed behavior (effort) among female students as discussed in section 3.1.

male teachers. However, this finding does not prove that the effect is due to in-group bias as male-dominated departments could well exhibit cultural gender bias, which is also shared by female teachers. To test for this explanation, we performed a second experiment in which we use an unintended randomized experiment to provide evidence that TAs correcting exams at the university level favor students of their own gender. As expected, we find a sizable and significant in-group bias effect. Moreover, the in-group bias disappears when exams are graded anonymously, indicating the effectiveness of removing identity from exams, even though handwriting and content are otherwise left unchanged.

In addition, our unique design allows us to relate the total effect to in-group bias. When estimating both the effect of anonymous grading on female student grades and in-group bias mechanisms in the same model, we find that the effects are approximately independent. Thus, in addition to graders favoring their own sex, our findings note cultural or institutional factors independent of the student-grader gender match, such as gender stereotypes and their consequences on grading, as discussed by previous authors, such as Bertrand et al. (2005) and Lavy (2008). As the bias effect stemming from other factors could be as large as 10 % of a standard deviation in our analysis and the in-group bias effect adds an additional factor, the total gain for female students from anonymous grading at the university level is not trivial. As acceptance into master´s programs is selective and determined by outcomes at the bachelor level, a non-anonymous grading system could directly affect the probability of continuation into higher studies for females. Moreover, our finding implies that an equal gender representation of university teachers would not provide unbiased grading, at least not in the short run, as stereotyped students will continue to be unfairly rewarded. Further, our results directly prove the effectiveness of anonymous evaluation and could also potentially provide guidance, for example, for public sector recruitment.

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Declarations of interest

None

Data availability

Data used in this study is student level data in from LADOK. They can be asked for at Stockholm University as they are public information. However, we cannot legally post individual data publicly. But, we can provide original do-files that can be used, after asking for data at Stockholm University, to replicate our results. Moreover, as treatment is on a higher level than individual data, we can post aggregated data that clearly can be used for replication.

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Appendix

The procedure underlying the correction of exams in the introductory macroeconomics course

Each of the 7 questions is corrected by a TA, usually a separate TA for each question, although there are some exceptions, particularly for retakes. Before the correcting process starts, all TAs, the lecturer and the course coordinator assemble and discuss in broad terms how many points should be given for different answers. At the end of this meeting, the allocation of TAs to questions 4-10 is determined by lottery.

Once this process is completed, each TA receives approximately 500 answers to the questions assigned to him or her (approximately 100 if it is a retake) and is then left with the daunting task of correcting each answer as fairly as possible. Swedish law requires that students know the results within 3 weeks the latest, and thus, graders have less time than this to actually complete the correction. Hence, after approximately 2-2.5 weeks, the TAs and the course coordinator gather once more to look at students 1-2 points below a higher grade and then try to move them above the threshold. Students are still anonymous in this stage as of the fall of 2009. After this, the results are posted, and a session is announced, during which the template that everyone agreed upon during the first meeting is presented to the students. At the end of this session, students are allowed to make complaints directly in person to the TAs, which usually leads to a 1-2 point increase to 1-2 students at most. It is important to note that we generally have data on the students' points immediately after they have been determined by the TAs only; thus, they are not subject to bias from anyone other than the TA. The exceptions are one exam from the fall of 2009 and one question on another exam.

Reduction of DD to before and after

It is stated in section 2.4.2 that if $\delta_2 = 0$, we can consistently estimate the DD effect using a simple before-and-after framework. Equations (4)-(8) illustrate how this works in our simple regression framework, where $Y_{\mathrm{j=1,t=1,f=1}}$ is the standardized test score for females in the treated group in the post-treatment period, Y_{101} is the standardized test score for females in the treated group in the pre-treatment period, Y_{110} is the standardized test score for males in the treated group in the post-treatment period, and Y_{100} is the standardized test score for males in the treated group in the pre-treatment period. Formally, we can write this as follows:

(6) $Y_{j=1,t=1,f=1} = \delta_0 + \delta_1 treament + \delta_2 after_{t=1} * female_{f=1} + \delta_3 female_{f=1} * exam_{j=1} + \delta_4 after_{t=1} * exam + \delta_5 after_{t=1} + \delta_6 female_{f=1} + \delta_7 exam_{j=1} + \varepsilon_{111}$

$= \delta_0 + \delta_1 + \delta_2 + \delta_3 + \delta_4 + \delta_5 + \delta_6 + \delta_7 + \varepsilon_{111}$

(7) $Y_{101} = \delta_0 + \delta_3 + \delta_6 + \delta_7 + \varepsilon_{101}$

(8) $Y_{110} = \delta_0 + \delta_4 + \delta_5 + \delta_7 + \varepsilon_{110}$

(9) $Y_{100} = \delta_0 + \delta_7 + \varepsilon_{100}$

Thus, we obtain the difference:

(10) $(Y_{111} - Y_{101}) - (Y_{110} - Y_{100}) = (\ \delta_0 + \delta_1 + \delta_2 + \delta_3 + \delta_4 + \delta_5 + \delta_6 + \delta_7 + \varepsilon_{111} - \delta_0 - \delta_3 - \delta_6 - \delta_7 - \varepsilon_{101}) - (\delta_0 + \delta_4 + \delta_5 + \delta_7 + \varepsilon_{110} - \delta_0 - \delta_7 - \varepsilon_{100})$

$= \delta_1 + \delta_2 + \varepsilon_{111} - \varepsilon_{101} + \varepsilon_{110} - \varepsilon_{100}$

Thus, if $\delta_2 = 0$, we can estimate the true treatment effect δ_1 using equation (8), which is the simple before and after in gender difference in the treatment group, by a regression corresponding to equation (2).

Figures and tables

Fig. A1. Histogram for the Bologna grading system

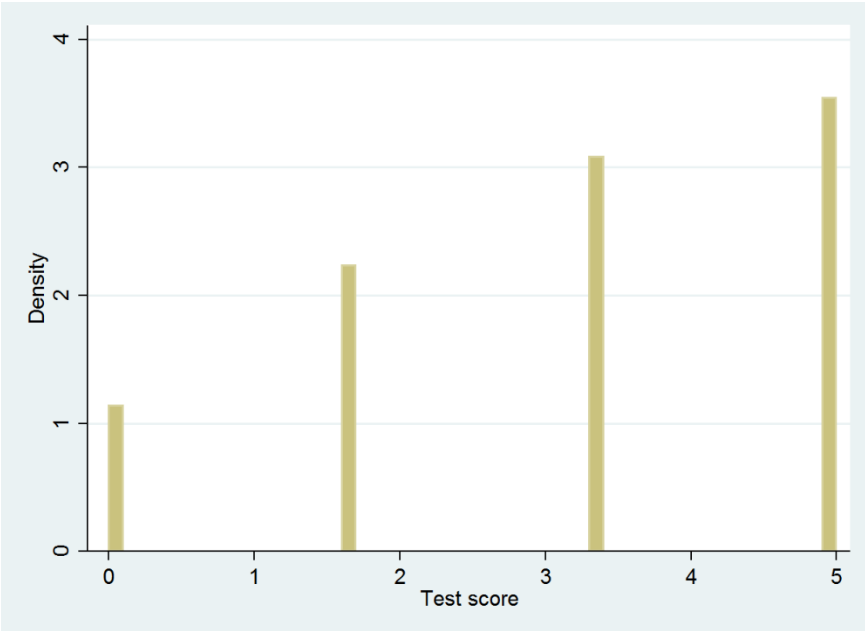

Fig. A2. Histogram for the department of law grading system

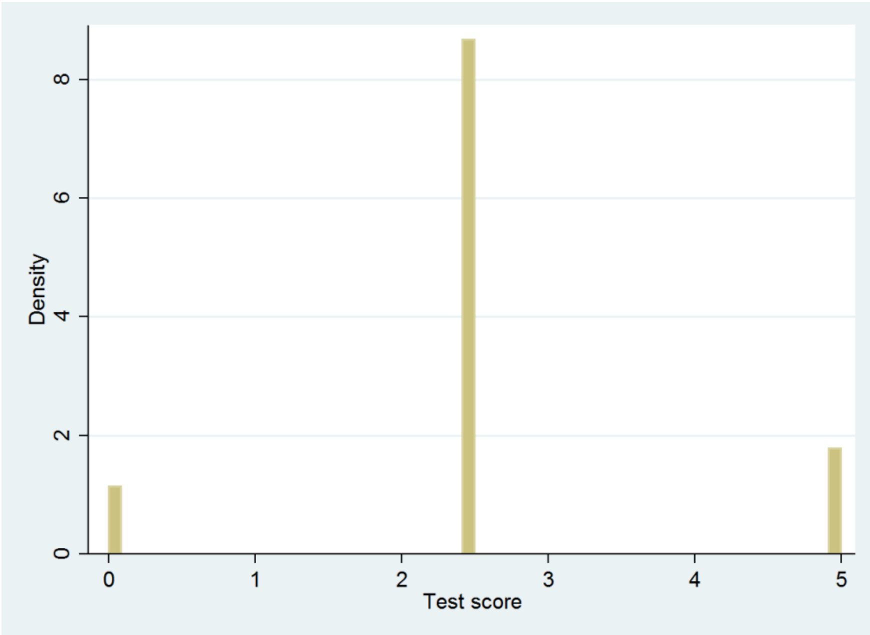

Fig. A3. Histogram for the original grading system

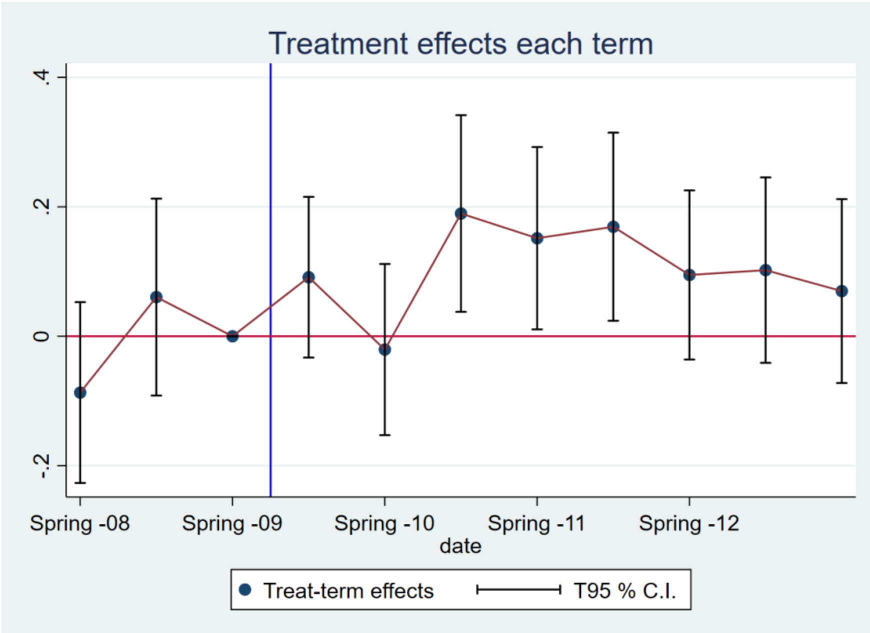

Fig. A4. Annual effects across the treated and non-treated groups in the sub-sample introductory macroeconomics

Tables

Table A1. Grades and their values

Grades A-F	Values	Grades AB-U	Values	Grades VG-U	Values
A	5	**AB**	5	**VG**	5
B	4	**BA**	3.33	**G**	2.5
C	3	**B**	1.67	**U**	0
D	2	**U**	0	**-**	-
E	1	**-**	-	**-**	-
F/Fx	0	**-**	-	**-**	-

Table A2. Gender grading bias, additional controls. Full sample gender grading bias and additional controls

	(1) stand. score	(2) stand. score	(3) stand. score	(4) stand. score	(5) stand. score	(6) stand. score	(7) stand. score
female*after*exam	0.040***	0.042***	0.025**	0.030***	0.022**	0.022**	0.024
	(0.011)	(0.0096)	(0.013)	(0.011)	(0.011)	(0.010)	(0.017)
Month*gender FEs	No	No	No	Yes	No	No	No
Month*treated FEs	No	No	No	Yes	No	No	No
Collapsed	No	Yes	No	No	No	No	No
Narrow timespan	No	No		Yes	No	No	No
Department FEs	No	No	No	No	Yes	No	No
Individual FEs	No	No	No	No	No	Yes	Yes
In 4 cells	No	No	No	No	No	No	Yes
N	1830461	9	839311	1830461	1830461	1830461	372027

Standard errors are shown in parentheses

Note: Standard errors clustered at the student level except in column 2. In column 2, Newey-West standard errors are used with one lag. The dependent variable is the standardized score. Column 3 includes both month*gender and month*treated FE's and the interactions between gender*fall 2009 and treated*fall 2009 to saturate the model. Since we have data regarding the exact date of the exams and we define treatment as starting on the official date of the fall term in 2009, which is the 22nd of August, we need these additional interactions to saturate the model. Excluding them increases the coefficient of interest.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3. Gender grading bias and additional controls. Sample of activities of 15 or more ECTS points.

	(1) stand. score	(2) stand. score	(3) stand. score	(4) stand. score	(5) stand. score	(6) stand. score	(7) stand. score
female*after*exam	0.066***	0.069***	0.040***	0.053***	0.051***	0.036***	0.035*
	(0.011)	(0.015)	(0.014)	(0.011)	(0.011)	(0.011)	(0.020)
Month*gender FEs	No	No	No	Yes	No	No	No
Month*treated FEs	No	No	No	Yes	No	No	No
Collapsed	No	Yes	No	No	No	No	No
Narrow timespan	No	No	Yes	No	No	No	No
Department FEs	No	No	No	No	Yes	No	No
Individual FEs	No	No	No	No	No	Yes	Yes
In 4 cells	No	No	No	No	No	No	Yes
N	1349181	9	620968	1349181	1349181	1349181	232038

Standard errors are shown in parentheses

Note: Standard errors are clustered at the student level, except for in column 2. In column 2, Newey-West standard errors are used with one lag. The dependent variable is the standardized score. Column 3 includes both month*gender and month*treated FE's and the interactions between gender*fall 2009 and treated*fall 2009 to saturate the model. Since we have data regarding the exact date of the exams and we define treatment as starting on the official date of the fall term in 2009, which is the 22nd of August, we need these additional interactions to saturate the model. Excluding these interactions increases the coefficient of interest.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4. Additional robustness excluding sub-groups

	(1) stand. score	(2) stand. score	(3) stand. score	(4) stand. score	(5) stand. score
female*after*exam	0.040^{***}	0.052^{***}	0.051^{***}	0.050^{**}	0.040^{***}
	(0.011)	(0.0092)	(0.018)	(0.020)	(0.011)
Exclude dep. of law	No	Yes	No	No	No
Only A-F grades	No	No	Yes	Yes	No
A-F grades are mandatory	No	No	No	Yes	No
Alternative numbers	No	No	No	No	Yes
N	1830461	1711444	954715	883165	1830461

Note: Standard errors clustered at the student level. The dependent variable is the standardized score.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5. Effect depending on the share of female teachers in the department

	(1) stand. score	(2) stand. score	(3) stand. score	(4) stand. score
female*after*exam	-0.012	0.037^{***}	-0.028	0.055^{***}
	(0.021)	(0.014)	(0.023)	(0.015)
Share female teachers	Majority female teachers 2008	Majority male teachers 2008	Majority female teachers	Majority male teachers
N	673765	1100479	582285	916811

Standard errors are shown in parentheses. Note: Standard errors are clustered at the student level.

The dependent variable is the standardized score. * p < 0.10, ** p < 0.05, *** p < 0.01