Individual Teacher Incentives, Student Achievement and Grade Inflation

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Executive Summary

There is great interest in understanding the potential of teacher incentives to improve student achievement. In fact, teacher incentives, either individual or collective, may improve student achievement if they succeed in aligning the public or social goals with the goals of the teacher. However, an approach in which reward is based on outputs can also be fraught with difficulties: For instance, setting specific measurable outputs may lead to potentially dysfunctional behaviour such as teaching to test. Moreover, while individual incentives may disrupt collaborative work, collective incentives may also generate free riding and, in the end, little effect on performance.

This paper sheds light into this question by examining the recent introduction of performance-related pay in all public schools in Portugal. Our approach is based on a difference-in-differences analysis drawing on two complementary control groups. These control groups either were exposed to a lighter version of the intervention (the case of public schools in the Azores and Madeira) or were not exposed at all (the case of private schools). All students in all schools were administered the same national exams.

Our results indicate that the increased focus on individual teacher performance caused a significant and sizable relative decline in student achievement, as measured by national exams. However, the decline in achievement is smaller or virtually zero when considering those marks set by teachers, suggesting an increasing importance of grade inflation. This view is consistent with the emphasis placed by the new promotion criteria on student results. Finally, the analysis of teacher early retirement across public schools supports the theoretical mechanisms (and much anecdotal evidence) that predict the empirical results, namely disruption of teacher cooperation created by tournaments for promotions and increased administrative workloads, both resulting in job dissatisfaction.

While our results are not optimistic in terms of the value of the specific reform examined here, the findings are reassuring in that they indicate that teachers respond to incentives in a predictable way. In this context, we believe that future research should move from the broad issue of whether performance-related pay has positive or negative effects to the narrower but in our view more illuminating question of which specific performance-related pay setups generate the best results for students.

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1 Introduction

Recent research has emphasized the importance of teachers in terms of students' achievement (Rivkin et al. 2005). This line of inquiry has also shown that it is particularly difficult to explain differences in teacher quality, at least when using teacher characteristics available for research analysis (Aaronson et al. 2007). These results have generated increased interest in the study of mechanisms that may raise achievement through teachers, including training, improving pay or stregthening incentives (Lazear 2003). This paper addresses the latter policy option.

In fact, teacher incentives, either individual or collective, may improve student achievement if they succeed in aligning the public or social goals with the goals of the teacher. In this case, a combination of incentive and composition effects will increase student performance (Lazear 2000, 2003). However, an approach in which reward is based on outputs can also be fraught with difficulties, which may explain the popularity of simpler input-based rewards (Kane & Staiger 2002). For instance, setting specific measurable outputs may lead to potentially dysfunctional behaviour such as teaching to test. Moreover, while individual incentives may disrupt collaborative work (Fehr & Schmidt 1999), collective incentives may also generate free riding and, in the end, little effect on performance.

Although the theoretical analysis on the effects of teacher incentives is ambiguous, the empirical literature in this area is particularly thin. We know of only four studies that explore randomized or quasi-natural experiments to address the causal relationship between teacher incentives and student achievement - Lavy (2002), Glewwe et al. (2003), Lavy (forthcoming) and Muralidharan & Sundararaman (2009). Furthermore, the results of these three papers are not entirely consistent: the two studies based on Israel (Lavy 2002, forthcoming) and the one based on India (Muralidharan & Sundararaman 2009) are supportive of the potential of collective and individual teacher incentives, while the randomized experiment based on Kenya (Glewwe et al. 2003) is not.[1]

While the literature above focuses on case studies, in our contribution we examine the effects of the recent introduction of individual teacher incentives in all public-sector schools in a given country, Portugal. The main aspects of this reform, described in more detail

[1]See also Figlio & Kenny (2007) for a study of the US based on cross-sectional data and Atkinson et al. (2009) for a recent analysis of the introduction of performance-related pay for teachers in England.

in Section 2, are the breaking up of the until then single pay scale for teachers into two and the tournament-like structure for progression between the pay scales. While before the reform progression (and wage growth) depended almost only on tenure, the new incentives placed considerable emphasis on the school-level and national-exam results of the students taught by each teacher. However, as any progressions between the two pay scales could not exceed a given number of upper-scale teacher vacancies per school determined centrally by the Ministry of Education, the incentive structure amounted to a form of tournament (Lazear & Rosen 1981). Overall, these changes established a clear contrast in the incentives faced by public-school teachers, from input- to output-based.

We study the effects of these reforms in terms of students' school-level and national-exam results. Specifically, we draw on freely-available matched student-school data covering the population of secondary school students that sat national exams from 2002 to 2009 in Portugal. We then conduct a difference-in-differences (DID) analysis based on two complementary control groups. In the first control group, we consider public schools in the Azores and Madeira. These are two autonomous regions of Portugal that were exposed to lighter versions of the reform than the rest of the country, as their pay scale remained unchanged and progression was less restricted. In the second control group, we consider private schools. These schools are again subject to the same national exams as the treatment group but their teachers were not affected by the reform, as pay and any incentives remained freely set by each private school, subject only to wage floors determined by collective bargaining.

Our research contributes to the literature on the effects of teacher incentives in different ways. First, this is the first paper that examines a reform that was applied across a country (rather than a pilot study, for instance) and that conducts the analysis drawing on population data. We are therefore able to address issues of external validity that arise in experimental settings and that typically receive little attention due to data constraints, particularly in the emerging empirical literature on incentives (Lazear 2000, Bandiera et al. 2005). Indeed, our population data also contrast with the case of the papers mentioned above that draw on randomized or quasi-experimental studies (Lavy 2002, Glewwe et al. 2003, Lavy forthcoming, Muralidharan & Sundararaman 2009), since "[a] weakness of natural experiments is that their results may not be generalizable beyond the group of individuals or firms or the setting used in the study" Meyer (1995). In fact, these papers do not claim to present representative evidence

of the countries examined as their research design generates average-treatment-on-the-treated effects instead. Moreover, our analysis of systematically-collected official data throughtout the period will alleviate measurement error bias and Hawthorne effects compared to the case of a typical experiment.

A second important aspect of our study is that, as the assignment of schools to treatment is not random, we conduct several robustness tests, on top of our consideration of two complementary control groups. For instance, our analysis of a long period of up to five years before the reform allows us to test the common trends hypothesis in some detail. Furthermore, we consider a number of different specifications that control for different sets of variables, including school and school-exam fixed effects, and different data subsets or aggregation levels. We also exploit the introduction of voluntary but costly early retirement in the public sector in the end of the period to study any possible correlation between reform-related job disatisfaction (proxied by early-retirement take-up) and student achievement.

Finally, we pay particular attention to the potential for grade inflation (Jacob & Levitt 2003). This may result from the fact that the progression criteria are affected by student results that are determined at least in part by the teachers themselves. As we have individual student data on both school- and national-level results, we can examine grade inflation from a triple-differences perspective by considering, for each student-module pair, both the internal and external marks. These results complement our evidence on the effects of the reform in terms of internal and external grades.

Overall, we find that the increased focus on individual teacher performance caused a sizable and statistically significant decline in student achievement. This decline in achievement is also much more pronounced in the case of national exams, with an effect of about half a standard deviation. Consistently with the different effects in terms of internal and external results, our triple-difference evidence also documents a significant increase of grade inflation. In addition, in support of a causal interpretation of our results, we find that in most cases there are no significant differences between the treatment and control groups before the introduction of merit pay and that the negative effects upon national exams are cumulative in the period after the reform is introduced. Moreover, we present evidence that teachers in those public schools that exhibit bigger falls in performance after the reform are more likely to take costly early retirement when it becomes available at the end of the period. This is consistent with

the potential negative effects of the incentives in terms of cooperation amongst teachers, administrative workload and, in the end, overall job satisfaction, as suggested by theory. Finally, the inclusion of different control variables or the consideration of different subsets of the data makes only very minor differences to the size of our estimates, as would be the case if assignment to treatment were random.

The structure of the paper is as follows: Section 2 describes the main characteristics of the education reform studied in the paper and discusses some of its theoretical implications. Section 3 presents the data used in the paper, a matched school-student panel data set; Section 4 describes the main results, while Section 5 presents the robustness analysis. Finally, Section 6 concludes.

2 The teacher incentives reform

A new government that came into office in 2005 decided to respond to the evidence of relatively poor performance levels in the Portuguese education system (OECD 2001). Indeed, when measured in terms of international comparisons such as the OECD's PISA tests, students in Portugal do not fare well. This is particularly true when taking into account the relatively high public expenditure levels in education in the country, of which relatively high average teacher salaries are an important component.

A key aspect of the education reform was the breaking up of the until then single pay scale for teachers into two separate scales. This and other aspects of the reform became law in January 2007, after having been subject to public discussion for several months and approved by the government in November 2006. The breakup of the pay scale marked an important contrast with the period before the 2006/07 school year as teachers were no longer ensured of virtually automatic, tenure-related progression from the bottom to the top of the pay scale over their careers. In particular, the gap between the last point in the lower scale and the first point of the higher scale was particularly large, at around 25%, from about €2,000 to about €2,500 per month (gross). On the other hand, those teachers in the higher pay scale were supposed to play a special role in management and pedagogical tasks in their schools.

Another key aspect of the reform is that the new system conditioned progression from the lower to the upper pay scale on a number of individual teacher performance variables. These

broadly measurable criteria were virtually inexistent until them. One such criterion for teacher progression, which received by far most media attention, was the academic performance of the students taught by each teacher. Another criterion that also received considerable attention was the feedback from the students' parents about the teacher. The remaining criteria included the teacher's attendance record, attendance at training sessions, management and pedagogical duties, and involvement in research projects.

According to the new law, these criteria for progression were to be assessed at each school, by those teachers in the higher pay scale. Moreover, detailed assessment sheets were made available by the Ministry of Education to be used when gathering information on the above-mentioned criteria. However, even if the teacher did well along these criteria, progression between the two pay scales was still conditional on a given number of (upper-scale) teacher vacancies per school, determined centrally every two years by the Ministry of Education as a function of the number of students in the school.[2]

On the political economy side, the reform generated heated debate and opposition from teacher unions and many teachers, including two national strikes. The extensive bargaining between government and unions that ensued created some confusion about the ultimate format of the incentive system, which is not yet absolutely clear at the time of writing. Moreover, there are reports of different levels of compliance both across and within schools, over time. However, different sources indicate that teachers acknowledged the change in incentives and adapted their behaviour to the new system. Additionally, a large number of teachers without tenure or with intermediate levels of tenure have already been assessed for the purpose of progression in the terms of new law and their assessment includes the period covered by our data (see Section 3). New assessment rounds (which are to take place every two years) are envisioned and the period of time that will be taken into account for the purpose of that assessment again includes our data.

From the above, we conclude that the reform under study involves a stark contrast in terms of teacher assessment and incentives. In particular, the new framework introduces several aspects which can be characterised not only as performance-related pay but also more specifically as tournaments (Lazear & Rosen 1981). Doing extremely well may not be enough

[2]According to the law, those teachers that did particularly well along the progression criteria and met centrally-determined quotas would also receive a one-off financial award. However, this aspect of the new system was not yet put into practice.

for a promotion if one's colleagues do even better and take all promotion vacancies available. In this context, and turning to a theoretical discussion of the predicted effects of this reform, there are different arguments to take into account. On the one hand, the greater weight placed on performance indicators will presumably induce teachers to focus their effort on those criteria highlighted in the law. This is expected to increase student achievement, which is measured by national exams and, to a less extent, school-level results.

On the other hand, tournaments are known to be potentially disruptive in terms of the collaborative work amongst agents involved in a competition (see Martins (2008) and the references therein). Moreover, collaborative work may be particularly important in the public sector and, in particular, in the education context. Fairness concerns may come to the fore and undermine teacher morale (Fehr & Schmidt 1999, 2004), given the difficulties in assessing teacher contribution (Jacob & Lefgren 2008). Moreover, setting broadly measurable outputs may lead to dysfunctional behaviour such as grade inflation, particularly in terms of internal (school-level) marks, which are direclty determined by teachers.[3] Finally, the administrative burden involved in this or any other teacher assessment process may also be considerable. For instance, the time spent handling the formal aspects of applying for progression and gathering the required supporting documents may reduce the effort that teachers put into teaching activities. Indeed, teachers and other stakeholders complained frequently about this aspect of the reform.[4]

One important final aspect in terms of the education reform studied here is that it applied only to a smaller extent in the cases of the two autonomous regions in Portugal, the Azores and Madeira islands. Indeed, these two regions have special legislative powers in some domains, including education, and they have decided not to follow fully the education reform in the mainland.[5] Specifically, the two regions also introduced greater emphasis on teacher assessment, under broadly the same criteria as in the mainland, although less so in the case of Madeira. However, one important difference that applied to both Azores and Madeira is that

[3]Tournaments will also generate extra risk in pay which would need to be compensated by higher wages in competitive markets but not necessarily in the regulated, public-sector labour market we study here.

[4]For instance, a national parents' association expressed publicly its concern about the negative effects of the reform in terms of student learning, as observed by their members. See *'Teachers' evaluation compromises students learning, say parents'* (our translation), in newspaper *Público*, 7 Nov 2008. There are also hundreds of internet blog entries written by aggrieved teachers complaining about the increased administrative workload and diminished collegiality in their schools.

[5]The relevant legal documents are Law (*Decreto-Leis*) 17/2007, of January 19th and 200/2007, of May 22nd, and Regional Laws (*Decretos Legislativos Regionais*) 28/2006/A, of August 8th; 21/2007/A, of August 30th; and 6/2008/M, of February 25th.

they did not break up their pay scale. These differences in the intensity of the treatment are exploited in our empirical analysis. The relatively long geographical distance between these two regions and the mainland (about 1600km and 950km, respectively) also minimises any possible spillover effects from the treated to the control groups (e.g. teacher mobility).

Furthermore, the reform did not apply at all to private schools, which account for almost one fifth of all secondary schools in the country. More importantly, teachers in private schools are rewarded independently according to the practices adopted by each school, following wage floors set by collective bargaining between private-sector school employer associations and national teacher unions.[6] In particular, we could not find any evidence of systematic changes to the personnel policies of private schools over the period or of any effect from the new teacher incentives in public schools upon the functioning of private schools.

3 Data

Our data cover the population of high-school national exams in Portugal over eight school years, from 2001/02 to 2008/09. The data are made available by the National Exams Committee (JNE, *Jurí Nacional de Exames*), an agency of the Ministry of Education which is responsible for all matters regarding the national exams carried out in the country. The high-school national exams, studied in this paper, are required for the award of the high-school diploma and also for university entry (European Commission 2007).

The data include information about the internal grades obtained by students in each module (a specific discipline of study, such as Portuguese or Maths) from their schools, which are based on test results and other criteria adopted by each teacher. There is also information about the students' final result in each module, after taking into account each student's internal and national-exam grades (with weights of 70% and 30%, respectively). Internal grades are truncated below 10, the pass level, in a scale of 0 to 20, in which case the student cannot sit the national exam, except in special circumstances. All data used in the paper are freely available from the JNE website.[7]

Each observation concerns a unique student-module-school-year combination. Typically,

[6] Only about one fourth of these private schools are religious, virtually all of them catholic. See Neal (1997) for an analysis of these schools in the US context.

[7] Link: `http://sitio.dgidc.min-edu.pt/JNE/Paginas/estatistica.aspx` (in Portuguese). The data were originally released openly so that the media could compile school rankings. As far as we know, the data have never been used for other purposes until now.

there will be several observations for each student but it will not be possible to match them as the data do not include any individual student identifier. However, all schools and all modules are identified by name and unique time-invariant codes. Importantly, there is also information on the school's location at the *concelho* level (up to 300 different geographical areas) and the school's public or private status. There are several variables for each student-module-school-year combination: if the exam is a resit (either because the student failed before or because the student wants to improve their grade), if the student is applying for admission to university, and if the student is sitting the exam but is not enrolled in the school. The data also include the student's gender and age, but only for the last four years (2005/06-2008/09); and the student's school year when taking the module (typically 12th, which is the last in secondary education in Portugal, but also the 11th, as some modules are subject to national exams at that stage).

We create our main sample of analysis by drawing on all student-exam pairs that meet the following five conditions: a first sit in the first call of a student that is applying to university and is also enrolled in the module of the exam in the school where they are sitting the exam; and schools that are present in the data in all eight years. These criteria are similar to those adopted by most media when compiling school rankings. Our criteria are also imposed in order to ensure that the effect in terms of internal and external grades are based on the same sample and thus are strictly comparable. The resulting 1,638,895 observations are distributed across 557 schools, of which 466 are public schools and subject to the reforms described in the previous section.[8]

Table 1 presents descriptive statistics based on school (top panel) or student-exam (bottom panel) data. Amongst other results, we find that the mean internal exam is larger than 13 while the mean external result is less than 11, both at the school- and student-module-level, in a scale of 0 to 20. This leads to an average gap between the two marks of more than 2, which is suggestive of considerable grade inflation or simply of different standards between school and national assessment. We also find that, on average, there are 417 exams per year

[8]The original size of the data is 3,487,026. 29.05% of these observations refer to second calls; 31.53% are not enrolled in the school; 7.10% are not applying for university admission; and 27.39% are resitting the exam. Of course, these exclusion categories overlap for many observations. (Extensive robustness analysis was conducted and the results presented below in Section 4 are not sensitive to different sample definitions as discussed in Section 5.) We also find that only one school is not present in the data in all years, a result that highlights the stability of the education sector in Portugal and rules out composition effects. No school switches between public and private status. There are 91 private schools and 30 public schools in Azores and Madeira.

per school. Of interest for the interpretation of the relative magnitude of our main results, the standard deviations of the logarithms of the internal and external exams are 0.05 and 0.14, respectively.

About 11% of the exams pertain to private schools and about 5% are from schools in the Azores and Madeira regions. Moreover, there is a downward trend in the number of exams in the period covered, which is consistent with the declining number of students enrolled in secondary school as indicated by national statistics. The exception to the trend is 2006, when new exams were introduced while some of the older exams were still sat by students.

Given that our DID estimates rely on variation over time across different groups of students, we present in Figure 1 the mean internal and external grades in each year from 2002 to 2009 at the three groups of schools we consider in our analysis: public schools in continental Portugal, public schools in the Azores and Madeira, and private schools (in the continent). We find that internal grades are very stable over the period in public schools (either in the continent or in the islands), while private schools exhibit an upward trend at the second part of the period. On the other hand, external marks are not only considerably lower - as documented before - but also exhibit greater fluctuation over time, including a pronounced increase across the three groups of schools from 2007 to 2008. However, the increase in external marks is more pronounced in the cases of private and public/islands schools. In particular, it can be seen in Figure 1 that while the gap between internal and external marks was higher for private schools than for continent public schools in 2002-2006, this is reversed by 2007.

For additional information, Figure 2 presents the distributions of internal and external grades, focusing on the cases of the Azores and Madeira and the continent subsets, in 2005 (before the reform) and 2009 (after the reform) - results for other years are similar. We find that these distributions do not change in a pronounced way over the period, except perhaps for some evidence of relatively fewer very low pass internal marks. Moreover the distributions for private schools (not reported but available upon request) are again very similar except that internal grades tend to follow a more uniform distribution in those schools.

4 Results

We estimate the effects of the introduction of performance-related pay from DID models of student grade equations. Our identification assumption is that there is no effect upon

achievement specific to public schools (in the continent) with respect to the control group from the 2006/07 school year onwards other than anything that results from the education reform. Specifically, in the case of our first control group, schools located in the Azores and Madeira, we estimate equations as follows:

$$y_{ijt} = \beta_0 + \beta_1 Continent_j + \beta_2 After_t + \beta_3 Continent_j \times After_t + u_{ijt}. \quad (1)$$

Depending on the specification, y_{ijt} denotes the logarithm of the grade (internal or external) of the student-exam pair i in school j in year t. Alternatively, the dependent variable is a measure of grade inflation, namely the difference between the internal and the external grade of the same student-exam pair (a triple differences specification) or a dummy variable taking value one if the internal grade exceeds the external grade.

Our analysis of different nonlinear functions of grade inflation also serves a useful robustness purpose. Indeed, if there are other relevant interaction effects that break down the identification assumption, our results are less likely to hold across different dependent variables. More important, the triple-difference specification is based on a weaker identifying assumption: it simply requires that there are no shocks that affect the relative outcomes of the treatment group in the same years as the education reform.

In all cases, $Continent_j$ is a dummy variable with value one if school j is located in mainland, continental Portugal (henceforth the *continent*): this variable will pick up permanent differences in terms of the dependent variable between those schools located in the continent and those located in the Azores or Madeira. $After_t$ is another dummy variable, with value one if year t is 2007 (i.e. school year 2006-2007) or later, the period when the incentives reforms was in force, as discussed in Section 2: this variable will pick up across-the-board differences between the period before the intervention and the period after the intervention. This is important particularly in the case of national grades, as their standards may have varied over time.

Finally, $Continent_j \times After_t$ is the product of the two previous dummy variables and its parameter, β_3, is the object of interest in this paper. Its estimate will pick up the effect of the education reforms upon student achievement or grade inflation, i.e. any additional difference between the two types of schools that emerges after the intervention.

From the benchmark specification in equation 1, we consider three extended versions with

different additional controls. The first version includes controls for school size (the total number of exams sat in each year) and regions (*distritos*, or up to 20 fixed effects). The second specification includes school size and school fixed effects. Finally, the third additional specification includes school size and school-exam fixed effects. Because the structure of exams changes over the period, we focus on a subset of four of the most important topics covered in these tests: Portuguese, Maths, Biology, and Physics/Chemistry. Importantly, all models are estimated with robust standard errors, allowing for clustering at the school level.

The first set of results, based on internal grades, are presented in Table 2 (top panel). These results draw on the student-level data described in Table 1, except that private schools are dropped. Across all four specifications, we find negative point estimates, indicating that the levels of achievement of public schools in the continent fell with respect to public schools in the Azores and Madeira after the introduction of the incentives reform. The magnitude of the estimates is very similar across the specifications, but also very small, ranging from -.006 to -.007, or about one seventh of the standard deviation of the dependent variable. On the other hand, the statistical significance of the coefficients is poor, as it does not meet the 5% threshold.

However, when we turn to Table 3 (top panel), which presents similar specifications but considering external grades instead, we find much higher effects, ranging from -.044 to -.064. These correspond to almost half of a standard deviation of the log average grade across schools. In addition, all estimates here are also significant, even at the .1% level. The comparison of the two sets of results (internal and external grades) therefore indicates that while national-exam results of public schools in the continent fall significantly and by a meaningful size with respect to the same change for public schools in the Azores and Madeira, the equivalent effect for internal marks is insignificant.[9]

The contrast between the internal and external results suggests that grade inflation in the public schools in the continent is a presumably unintended consequence of the reform. Indeed, our triple-difference estimates - see Table 4 (top panel) - indicate precisely that. We find that the average gap between internal and external marks increases by .271 to .352 (or about one third of a standard deviation of the average gap) in public schools in the continent with respect to their counterparts in the Azores and Madeira. In all cases the coefficients are

[9]We also find, both in Table 2 and in Table 3, that the isolated $After$ coefficients are always significantly positive, suggesting a trend towards higher marks, particularly in national exams.

significant at the 5% level.

Similarly, we also find evidence that the probability of grade inflation increases in the continent with respect to the Azores and Madeira - see Table 5 (top panel). The fact that these estimates are relatively small (at .02, although almost all significant at the 5% level and some at the 1% level) indicates that the grade inflation effects were felt mostly along the intensive margin, i.e. increased inflation in schools that already tended to exhibit inflation. This is related to the limited room for expansion along the extensive margin, as only 14% of the marks exhibit 'deflation' and only 23% of the marks are the same at the internal and external levels (when defining an equality as an absolute difference of less than .5).

4.1 Private schools

We now turn to our second and complementary control group. Although private schools on average tend to exhibit better results in terms of the academic achievement of their students when compared to public schools (because of a selection process or better academic practices or some combination of the two), our DID approach will control for permanent differences in achievement between the two types of schools.

Similarly to the case of the first control group (equation 1), here we estimate the following DID specification:

$$y_{ijt} = \beta_0^{'} + \beta_1^{'} Public_j + \beta_2^{'} After_t + \beta_3^{'} Public_j \times After_t + u_{ijt}, \tag{2}$$

All variables take the same interpretation as before; $Public_j$ is a dummy variable with value one if school j is a State school; and $\beta_3^{'}$ is now the parameter of interest. All models in this section are estimated with the full set of student-level data described in Table 1, except that schools located in the Azores and Madeira islands (public or private) are dropped.

Table 2 (bottom panel) presents the results for internal grades. In contrast to the case when the public schools in Azores and Madeira served as the control group, we find here evidence that the introduction of the individual teacher incentives had a significant detrimental effect on student achievement. In the present case, the coefficients range from -.020 to -.029, or about three times as much as in the equivalent specification under the first control group, and are always statistically significant, even at the .1% level.

In terms of the external grade results, we find that achievement in public schools, when

compared to private schools, falls by between -.052 to -.070, and is again always significant at the .1% level - see Table 3 (bottom panel). The magnitude of these effects corresponds to about one third to one half of a standard deviation of external results. The increase in the magnitude of these estimates when compared to the results based on public schools in the islands is consistent with the intermediate intensity level of the treatment there, as discussed in Section 2.

Finally, the importance of grade inflation suggested from the stronger effects on external grades when compared to internal grades, is once again corroborated from the triple-difference results. In Table 4 (bottom panel), we find that grade inflation increases by between .206 to .291 across the four specifications considered there, all of which are significant at least at the 5% level. Moreover, similar results to those of the first control group are found for the grade inflation probability variable too (Table 5, bottom panel). This is reassuring as it is evidence against interaction effects between the possibly evolving difficulty level of the national exams and any ability differences between students in treatment and control groups. If, for instance, national exams get easier when the reform is introduced (as suggested from the analysis of the raw data) and if high-ability private-school students also respond better to such possibly easier exams, then this could generate misleading evidence of relatively lower achievement in public schools. The results would also suggest higher grade inflation in public schools to the extent that the internal results do not change at the same time (as, again, may be the case from the analysis of the raw data). However, the extensive margin results presented above are evidence of increasing grade inflation across the board, not only for high-ability students. Moreover, our findings of lower achievement and higher inflation also arise when focusing on public schools in the islands, where the ability interaction argument presumably does not apply.

Overall, the results suggest strongly that the onset of individual teacher incentives led to a decrease in student achievement (when measured by national exams) and an increase of grade inflation. According to our theoretical discussion, these empirical results are consistent with incentives-related disruption in colaborative work in schools, once teachers are facing tournaments for promotions, and as internal (teacher-determined) results carry a considerable weight in final marks, thus enhancing a teacher's chances of promotion.

5 Robustness

5.1 Common trends

One important test of the strength of a causal interpretation of DID estimates concerns common trends. Indeed, if there are no interactions between treatment and other variables, as assumed for identification purposes, one would expect parallel movement between the treatment and control groups before the treatment began. We conduct this test here by considering more flexible versions of equations 1 and 2. Specifically, we allow the difference in the outcomes between treatment and control groups to vary during the period prior to the intervention. If our earlier estimates are indeed capturing a causal effect, then we expect that there will be no statistically significant differences between the two groups until the occurrence of the treatment. Moreover, we also allow the effect of the education reform to vary over the $After$ period. This serves as another robustness test, as it allows us to investigate any cumulative effects of the reform.

In this context, the first equation we estimate is as follows:

$$y_{ijt} = \alpha_0 + \alpha_1 Continent_j + \sum_{k=2003}^{2009} \delta_k I(year_t = k) + \sum_{k=2003}^{2009} \gamma_k Continent_j \times I(year_t = k) + u_{ijt}. \tag{3}$$

All variables have the same meaning as before; and $I()$ is the indicator function. The parameters of interest are now the γ_k (k=2003, ..., 2009), which will indicate any differences in the yearly effects of the treatment group with respect to the benchmark year (2002). As before, we consider specifications without any controls (column 1) or with school or school-exam fixed effects (columns 2 and 3, respectively).

Table 6 (left panel) presents the results based on the internal grades as the dependent variable. We find that, across all specifications, there are no differences in trends between public schools in the Azores and Madeira and those in the continent. We also find that there are significant treatment effects in 2007, but not in 2008 or 2009. In terms of external results - Table 7 (left panel) -, we find again no evidence of different trends between the two types of schools. On the other hand, the significant negative results of public schools in the continent are concentrated in 2008 and 2009 (although the point estimates also increase considerably from 2006 to 2007).[10] These results are also displayed in Figure 3 (left panel).

[10]Here we also find examples of pre-reform years in which national exams exhibit significantly different means

The analysis of grade inflation is again consistent with the earlier findings. Table 8 (left panel) indicates no systematic differences between the two types of schools until 2008, when grade inflation effects jump in magnitude and become statistically significant in all cases except one. Before that, in 2007, point estimates are already typically higher than before. In any case, grade inflation jumps even further in 2009, when all effects are significant at least at the 5% level. (The results on the grade inflation probability are again similar and are available upon request.)

Overall, we regard these results are supportive of a causal interpretation of our main findings. Furthermore, the cumulative nature of the effects is also consistent with the cumulative nature of the reform, in the sense that the cohorts that take their exams later (in 2008 rather than in 2007, for instance) are also typically cohorts that have been exposed to the treatment for a longer period. The relative consistency of the effects on the external marks over the 'after' period in both control groups is also evidence against any possible one-off disruption across public schools or amongst their teachers that coincided with exam time, even if we are unaware of any example of such an event.[11]

We also test the common trends assumption (and the cumulativeness of the effects) in terms of the public vs private schools comparison:

$$y_{ijt} = \alpha_0^{'} + \alpha_1^{'} Public_j + \sum_{k=2003}^{2009} \delta_k I(year_t = k) + \sum_{k=2003}^{2009} \gamma_k^{'} Public_j \times I(year_t = k) + u_{ijt}. \quad (4)$$

Here we generally find evidence of no statistically significant differences during the *before* period for all three variables considered and across the three specifications estimated for each variable - see the right panels of Tables 6 and 7 (and of Figure 3) for the results on internal and external marks and of Table 8 for the results on grade inflation. The only exception to this pattern is some evidence of lower external results and higher inflation in 2004 and 2005. However, those point estimates are generally quite smaller than their 2007, 2008 and 2009 counterparts. Moreover, without exception, all point estimates in 2008 or 2009 are bigger (in absolute terms) than in 2007 (even if their differences are typically not statistically significant),

but without correspondence in terms of a differential effect between continent and islands school. This is further evidence against a spurious relationship driven by interactions between student ability and exam difficulty.

[11] The results indicate a time gap between the effects on internal marks (which are circumscribed to 2007) and to the effects on external marks (2008 and 2009). This time gap could result from teachers learning how to play the tournament by inflating internal marks, of which there is only evidence in 2008 and 2009. Alternatively or complementarily, the time gap in these effects could be a consequence of the fact that external marks reflect the learning of students over a three-year period while internal marks can be adjusted quickly by teachers.

which we take as further evidence of cumulative effects of the reform.

5.2 Early retirement

According to our theoretical discussion, the negative effects upon student achievement documented above would be driven by a combination of decreased cooperation amongst teachers and increased administrative workloads, both of which would shift resources away from teaching with a potentially detrimental effect upon student learning. Another likely consequence of less cooperation and more administration would be increased job dissatisfaction.

This subsection offers some indirect evidence about the importance of these mechanisms, by studying the effects of a recent early retirement law that applied to all public servants in Portugal. The retirement law we exploit here (Law 11/2008) was introduced in February 2008 and allowed workers in all branches of the public sector to retire before the statutory retirement age of 61.5 (and at least 33 years of service). (See Martins et al. (2009) and the references there for more on the Portuguese retirement law.) However, public servants that took early retirement faced a hefty penalty of 4.5% of their final (defined-benefit) pension per each year of early retirement.

We exploit this additional reform to argue that if the new teacher incentives decreased both job satisfaction (because of the above-mentioned cooperation and administrative effects) and student achievement, then, *ceteris paribus*, the take-up of early retirement made possible by the 2008 law will be higher in schools where student achievement fell the most.[12] We then test this hypothesis by merging the JNE data with information on the number of retirements by school that ocurred after the early retirement law was introduced and assessing the extent to which schools with a greater proportion of retirements are also schools where student achievement falls the most.

Our data comes from the monthly lists of public-sector retirements which are published by the agency responsible for handling the retirements of public servants, CGA (*Caixa Geral de Pensões*). The data include information about the ministry and establishment where the retiree last worked and the retiree's pension.[13] We then computed the ratio between the total number of teachers and the total number of non-teachers that retired over the March 2008-August 2009 period, in each public school. The information about non-teachers (mostly

[12] See Green & Heywood (2008) for a study of the correlation between performance pay and job satisfaction.
[13] These data are again freely available, at `http://www.cga.pt/listamensalDR.asp` (in Portuguese).

administrative staff but also guards, cooks, janitors, etc) is useful as it serves as a control for other variables that may affect the number of teacher retirements per school, including school size and the school staff age distribution, and it should not be subject to the job dissatisfaction effects that may have applied to teachers (the structure of incentives did not change for other workers than teachers).

According to our discussion above, we take this ratio between teacher retirements and non-teacher retirements in each school as a proxy of the likely disruption effect from the introduction of teacher incentives. However, given that the data does not discriminate between early and normal retirements, the retirement ratio we use will be subject to measurement error. On the other hand, one should be able to rule out other problems including some forms of reverse causality. For instance, results up to 2008 should not be affected by any impacts on student achievement driven by the teachers' early retirement as we are studying achievement effects that typically occur before the early retirements: not only we examine achievement data that in part covers a period before the retirement law was is force as there are also lags between the publication of the retirement lists and the actual retirement of the teacher.[14]

After we merge the retirement data with our main JNE data on student achievement by school, we find a total of 2,831 retirees (teachers and non-teachers) across the 279 public schools for which we have at least one retirement (several schools could not be matched because their names were not the same on the JNE and CGA data sets). We obtain a mean of 7.57 (2.58) teacher (non-teacher) retirements per school and a mean .699 teacher/all retirees ratio. Moreover, the mean pension of teachers is €2,147 (standard deviation €232) while the mean pension of non-teachers is €634 (standard deviation €183). Both values are already net of any early-retirement cuts that may have applied.

Finally, we split public schools into 'high' and 'low' teacher retirement ratios groups. According to our discussion, the former schools are the ones that are likely to have suffered most from increased cooperation breakdown and administrative workload effects according to our hypothesis. As mentioned in Section 2, compliance with the incentives reform was not uniform across public schools. Such heterogeneity in compliance may explain variability in retirement ratios as it will have generated different drops in teacher cooperation and different

[14]However, results from 2009 will reflect in part the teacher composition effects generated by both the incentives and early retirement reforms. Given the evidence presented above, such composition effects will not necessarily be positive, in contrast to earlier research (Lazear 2000, 2003).

increases in administrative workloads.

Specifically, we create a dummy variable equal to one if the teacher retirement ratio in each school is greater than the 66th percentile of the distribution of the teacher retirement ratio (.82) and equal to zero if the same ratio is less than the 33rd percentile of the same distribution (.68). Students in schools with retirement ratios between the 33th and 66th percentiles are excluded from the analysis to ensure a greater contrast between the two types of schools given the noise in our measurement of early retirement. We then contrast the levels of student achievement and grade inflation before and after the introduction of the reform and across the two types of public schools, following again the structure of equation 1 and considering, alternatively, the same three main dependent variables.

We find from these specifications results that are reasonably similar to our benchmark results - see Table 9 (top panel). In particular, public schools that exhibit higher teacher retirement ratios in 2008/09 (after the early retirement law was in force) also display declines in student achievement (columns 1 to 3) and increases in grade inflation (columns 4 to 6) but no effects upon internal marks (not reported). This is particularly the case when controlling for school-subject fixed effects (columns 3 and 6), where the estimates are significant at least at the 1% level. We find similar qualitative results (available upon request) for different cut-offs between 'high'- and 'low'-retirement ratios. The same applies when also taking into account the retirement pension by adding an extra interaction effect. In this specification, we find that the lower the teachers' pension - which at least in part will be reflect early retirement cuts -, the more likely student achievement to fall and grade inflation to increase.

5.3 School-by-school analysis

In Section 1 we highlighted some concerns about external validity that have been directed towards case-study experimental settings. Here we revisit this issue by seeking to understand the dispersion of our estimates across different treatment schools. In particular, we reestimate equations 1 and 2, including school fixed effects and a control for the number of exams in each school-year (as in column 3 of Tables 3 and 4). We then compare each continental public school separately against either all public island schools or all private schools. We also consider both the external exam result or the grade inflation measure.

This approach generates as many DID estimates as the number of treatment-group schools

(436) from which we can compute measures of their dispersion. Specifically, we find standard deviations of these coefficients of .078 in both comparisons. As these correspond to twice the absolute value of the mean coefficients, at -.045 (-.043) in the case of the regression of external marks on the islands-after (public-after) interaction, we interpret these results as suggesting a considerable scope for variation of the effects across different schools. Similarly, we find considerable dispersion in the effects in terms of grade inflation, as the standard deviations of those coefficients (.581 and .583, in the comparison against islands and private schools, respectively) are two to four times the mean coefficients (.237 and .139, respectively).

The dispersion of our school-by-school coefficients can also be seen from Figure 4, where we present kernel density estimates for the two dependent variables and the two control groups. Although as indicated above the dispersion in the effects is much more pronounced in the case of grade inflation, such dispersion is still considerable in the case of national exams. More importantly, the results presented in this extension highlight the possible issues of external validity in case-study approaches that draw on a limited number of companies or individuals. For instance, we found that 106 (98) out of 436 estimates of the effects on external results based on the islands (private) schools control group are positive. Similarly, 141 (169) estimates of the effects on grade inflation are negative.

5.4 Other control variables and tests

Student achievement is affected by many other variables than those related to teacher incentives. In particular, socio-economic variables may matter greatly. Given the non-experimental setting of our analysis, it is not impossible (even if unlikely, given the evidence produced so far) that the different types of schools that we contrast experience different trends in such socio-economic variables which just happen to coincide with the introduction of the new teacher incentives.

In order to assess the empirical content of this alternative view, we add to our specifications (equations 1 to 4) different characteristics of the local labour market of each school that will proxy for the socio-economic environment of these students. Specifically, we draw on the *Quadros de Pessoal* (QP) matched employer-employee data set, which reports detailed firm-, establishment- and worker-level information of all firms in Portugal that employ at least one worker (see Martins et al. (2009) for more detail about these data). We focus

on the establishment- and worker-level dimensions, as this allows us to compute region-year characteristics at the most detailed level of aggregation available on the JNE data (the *concelho* level). The QP variables we add to our student-level equations are the mean real hourly wage, the female ratio, the average schooling attainment and the total number of workers. These variables are computed from all workers employed in the same *concelho* where the student's school is located and in the same year to which the student's results refer.[15]

The results - Table 9 (bottom panel), covering the comparison between continent and islands public schools - again present strong evidence of lower achievement in terms of national exams and increasing grade inflation. The results (not reported but available upon request) are also similar when considering the contrast between public and private schools.

We have also conducted a number of additional robustness tests. First, we considered different subsets of our original data. Specifically, we study possible differences from the benchmark results alternatively in urban areas, in large schools or in core subjects only. Second, we extended the range of data examined from first-sit results (which account for about 85% of the total number of exams) to first- and second-sits. Third, we extended the range of data considered even further, considering also resit students and those not applying for university entry. Fourth, we replicated our results using data collapsed into before and after periods, as suggested in Bertrand et al. (2004). Fifth, we run our analyses separately by discipline, considering the four most popular subjects (Maths, Portuguese, Biology and Physics/Chemistry). Sixth, we control for some student characteristics, namely age, gender and year grade (11th or 12th), available in the JNE data, although only from 2006. In all cases, the qualitative results across the different specifications were unchanged and only relatively minor differences were found in terms of the quantitative findings. Finally, our preliminary evidence regarding the effects of the reform on primary schools, based on 9th-grade results (as opposed to the case of secondary schools studied here) indicates similar qualitative results (Martins 2009).

[15] We match QP data of year t to JNE data of year $t+1$, given that the JNE data concern academic years that begin in September of year t to June of year $t+1$ and the QP data refers to October of each year. Since that worker-level QP data is not available for 2001 and 2008, our merged JNE-QP data covers the period 2003-2008 (in terms of JNE years) or 2002-2007 (in QP years).

6 Conclusions

There is great interest in understanding the potential of teacher incentives to improve student achievement. This paper sheds light into this question by examining the recent introduction of performance-related pay in all public schools in Portugal. Our approach is based on a difference-in-differences analysis drawing on two complementary control groups. These control groups either were exposed to a lighter version of the intervention (the case of public schools in the Azores and Madeira) or were not exposed at all (the case of private schools). All students in all schools were administered the same national exams.

Our results consistently indicate that the increased focus on individual teacher performance caused a significant and sizable relative decline in student achievement, as measured by national exams. However, the decline in achievement is smaller or virtually zero when considering those marks set by teachers, suggesting an increasing importance of grade inflation. This view is supported by our triple-difference evidence and is consistent with the emphasis placed by the new promotion criteria on student results. Furthermore, we find additional support for a causal interpretation of our results from our analysis of common trends, robustness to different control variables, different data subsets and different aggregation levels. Finally, the analysis of teacher early retirement across public schools supports the theoretical mechanisms (and much anecdotal evidence) that predict the empirical results, namely disruption of teacher cooperation created by tournaments for promotions and increased administrative workloads, both resulting in job dissatisfaction.

On a methodological note, our use of official population data should ensure greater reliability in terms of the external validity of the findings (as suggested from our school-by-school analysis) and other potential problems such as measurement bias and Hawthorne effects. On the other hand, it is possible that our analysis is picking up implementation problems that may generally tend to erode over time. However, here we examine a period of up to three years after the reform was introduced, which is longer than most other related studies. Moreover, we find negative effects upon achievement that get bigger over time.

While our results are not optimistic in terms of the value of the specific reform examined here, the findings are reassuring in that they indicate that teachers respond to incentives in a predictable way. In this context, we believe that future research should move from the broad issue of whether performance-related pay has positive or negative effects to the narrower

but in our view more illuminating question of which specific performance-related pay setups generate the best results for students. Barlevy & Neal (2009) is a recent theoretical example of this approach, focusing on collective teacher incentives.

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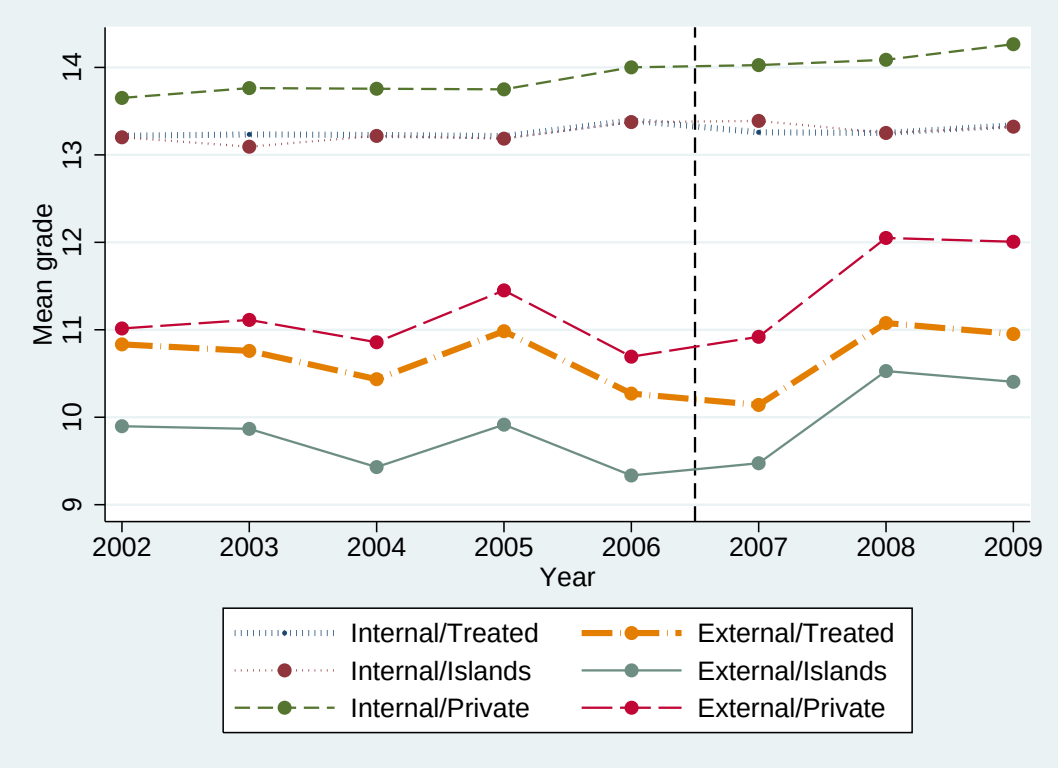
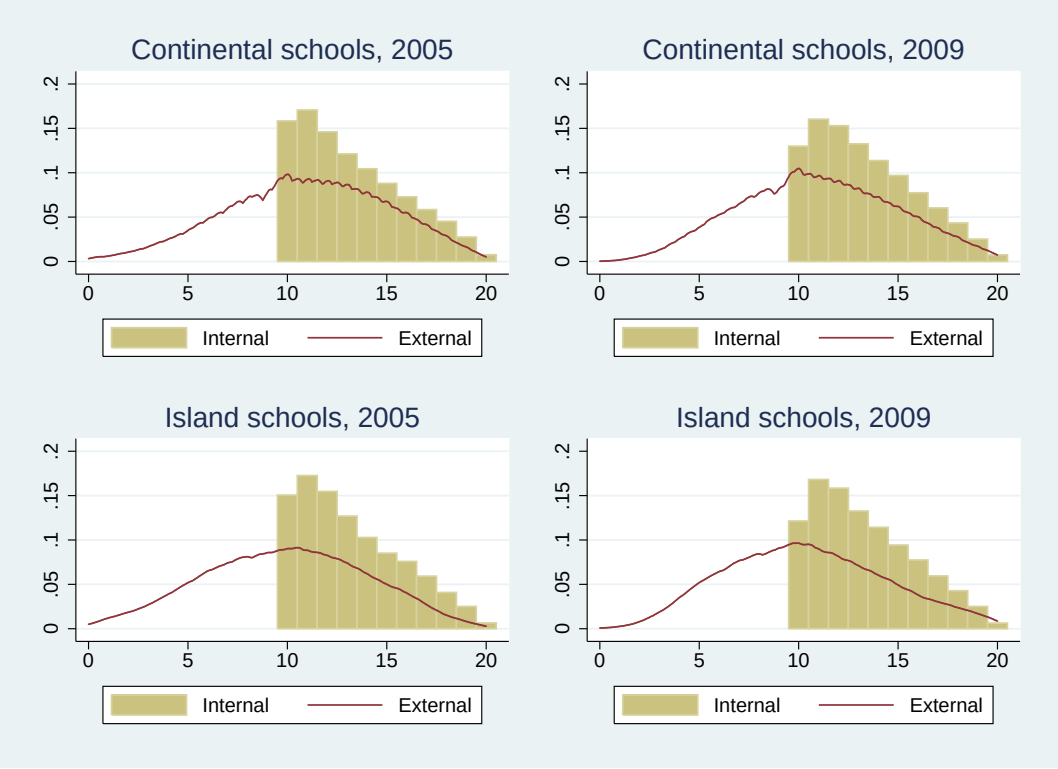
Figures

Figure 1: **Internal and external grades across groups and time**



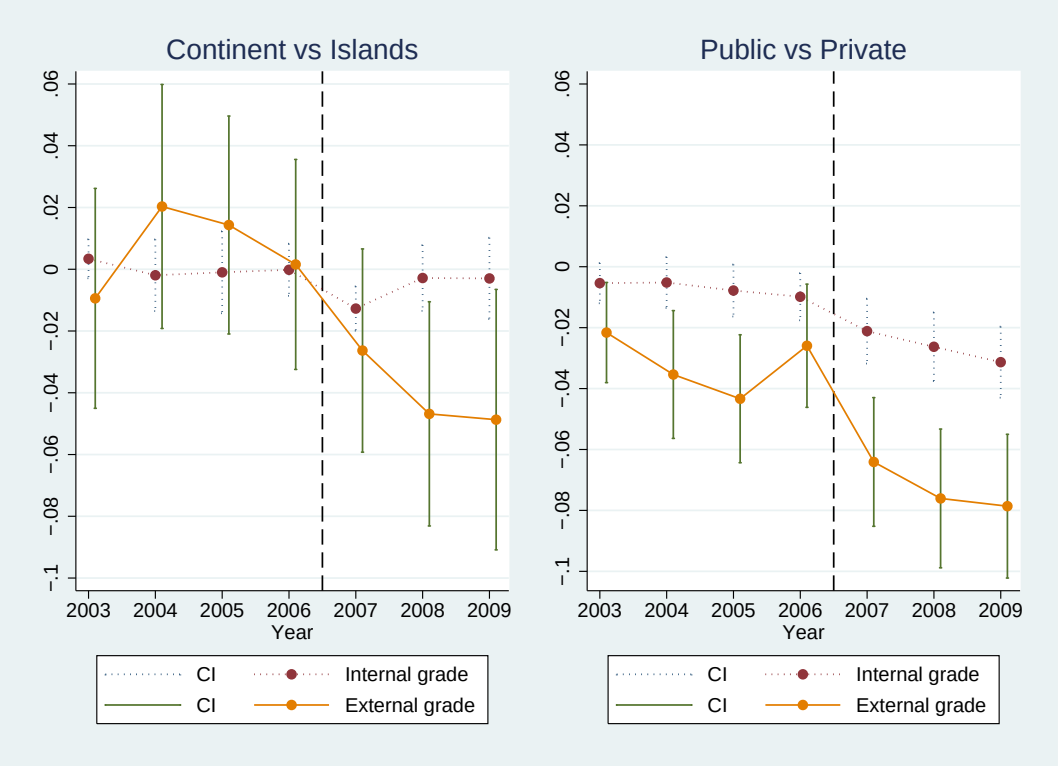
Source: Author's calculations based on JNE data. Mean internal and external marks of students by year and type of school (public schools in continental Portugal - 'treated'; public schools in the Azores and Madeira - 'islands'; and private schools in continental Portugal - 'private'). The vertical dashed line indicates the introduction of the teacher incentives reform.

Figure 2: **Distribution of grades: Islands and Continent, 2005 and 2009**



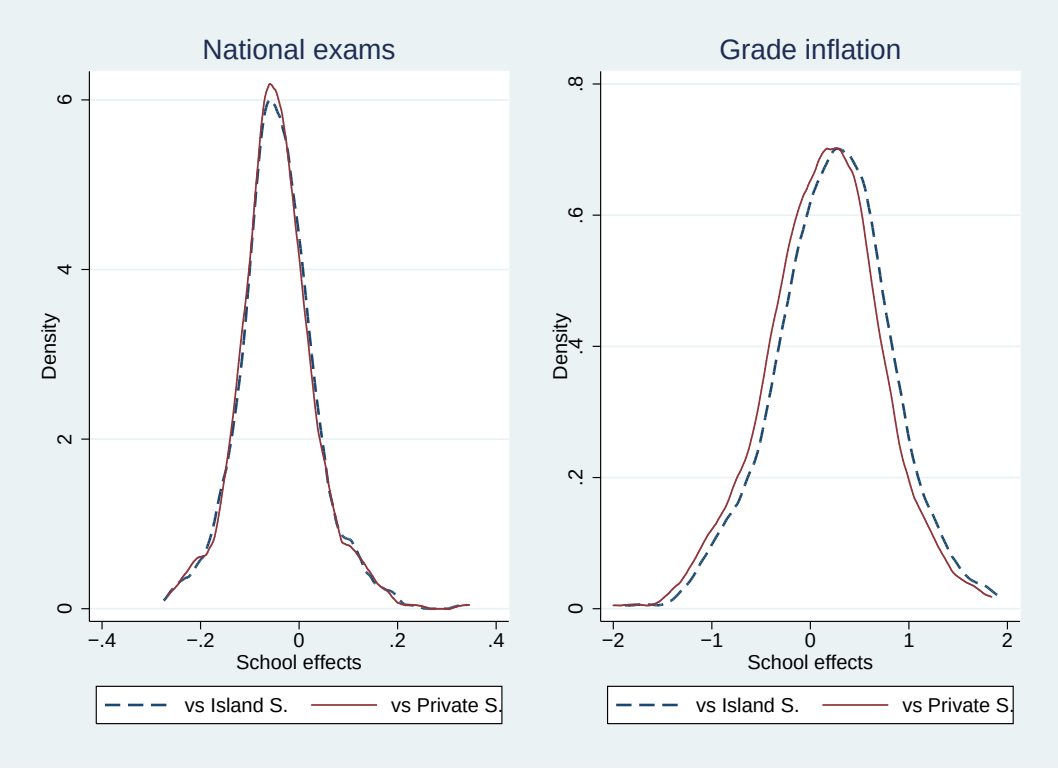
Source: Author's calculations based on JNE data. The external marks result from kernel estimation.

Figure 3: **Difference-in-difference estimates**



Notes: Author's estimates based on JNE data and reported in more detail in Columns 2 and 5 of Tables 6 and 7. The confidence intervals (CI) correspond to a range of +/- 1.6 standard errors.

Figure 4: **School-by-school estimates**



Notes: Author's estimates based on JNE data.

Tables

Table 1: **Descriptive Statistics**

Variable	Mean	Std. Dev.	Min.	Max.
School-level data				
Internal Exam	13.265	0.655	10.826	16.639
External Exam	10.479	1.255	6.195	16.049
Internal - External Exam	2.786	1.023	-3.021	6.323
Log Internal Exam	2.567	0.047	2.376	2.802
Log External Exam	2.258	0.142	1.593	2.758
Public	0.837	0.37	0	1
Continent	0.944	0.229	0	1
No. Exams	416.942	311.62	4	3152
Exam-level data				
Internal Exam	13.337	2.596	10	20
External Exam	10.707	3.985	0.1	20
Internal - External Exam	2.63	3.123	-10	18.9
Log Internal Exam	2.572	0.19	2.303	2.996
Log External Exam	2.282	0.465	-2.303	2.996
Public	0.889	0.315	0	1
Continent	0.949	0.22	0	1
No. Exams	648.582	389.117	4	3152
2002	0.164	0.37	0	1
2003	0.147	0.354	0	1
2004	0.107	0.31	0	1
2005	0.12	0.325	0	1
2006	0.145	0.352	0	1
2007	0.099	0.299	0	1
2008	0.102	0.303	0	1
2009	0.115	0.32	0	1

Notes: Author's calculations based on *Jurí Nacional de Exames* data. The internal (external) exam refers to the mark obtained by each student in each module by the school (national exam). 'Public' and 'Continent' are dummy variables which are equal to one for students in public schools or schools located in mainland Portugal, respectively. There are 557 schools, all of them observed in all eight years, resulting in 4,456 school-year observations and 1,592,399 exam-level observations.

Table 2: **Effects on internal grades**

A. Islands control group	(1)	(2)	(3)	(4)
After	.012	.015	.011	.021
	(.007)*	(.007)**	(.007)	(.006)***
Continent	.003			
	(.006)			
Continent-After	-.006	-.006	-.007	-.007
	(.007)	(.007)	(.007)	(.006)
Obs.	1209636	1209636	1209636	782697
R^2	.0002	.004	.022	.048

B. Private control group	(1)	(2)	(3)	(4)
After	.030	.037	.025	.033
	(.006)***	(.007)***	(.005)***	(.005)***
Public	-.035	-.035		
	(.010)***	(.010)***		
Public-After	-.025	-.029	-.022	-.020
	(.006)***	(.007)***	(.006)***	(.006)***
Obs.	1292781	1292781	1292781	840510
R^2	.006	.011	.039	.069

Notes: Dependent variable is the log of the school-level grade of each student in each exam in each year. Dummy $After$ is one for 2007, 2008 and 2009 only. Data used: 2002 to 2009, except for 2006. Columns 2-3 includes a control for the number of exams taken in each school in each year. Column 3 controls for school fixed effects (up to 527 dummy variables); and column 4 controls for school-subject fixed effects (only the four main subjects, Portuguese, Math, Biology and Physics/Chemistry, are considered in this column). Robust standard errors, allowing for clustering at the school level. Significance levels: *: 0.05; **: 0.01; ***: 0.001.

Table 3: **Effects on external grades**

A. Islands control group	(1)	(2)	(3)	(4)
After	.050	.063	.047	.103
	(.012)***	(.014)***	(.014)***	(.017)***
Continent	.115			
	(.019)***			
Continent-After	-.044	-.048	-.048	-.064
	(.013)***	(.015)***	(.014)***	(.017)***
Obs.	1209055	1209055	1209055	782261
R^2	.002	.015	.044	.117

B. Private control group	(1)	(2)	(3)	(4)
After	.068	.082	.049	.088
	(.012)***	(.014)***	(.011)***	(.011)***
Public	-.030	-.016		
	(.017)*	(.019)		
Public-After	-.061	-.070	-.052	-.052
	(.012)***	(.015)***	(.012)***	(.013)***
Obs.	1292162	1292162	1292162	840043
R^2	.002	.014	.05	.12

Notes: Dependent variable is the log of the national exam grade of each student in each exam in each year. Dummy $After$ is one for 2007, 2008 and 2009 only. Data used: 2002 to 2009, except for 2006. Columns 2-3 includes a control for the number of exams taken in each school in each year. Column 3 controls for school fixed effects (up to 527 dummy variables); and column 4 controls for school-subject fixed effects (only the four main subjects, Portuguese, Math, Biology and Physics/Chemistry, are considered in this column). Robust standard errors, allowing for clustering at the school level. Significance levels: *: 0.05; **: 0.01; ***: 0.001.

Table 4: **Effects on grade inflation**

A. Islands control group	(1)	(2)	(3)	(4)
After	-.181	-.255	-.176	-.374
	(.107)*	(.117)**	(.118)	(.139)***
Continent	-.916			
	(.129)***			
Continent-After	.271	.301	.305	.352
	(.111)**	(.120)**	(.119)**	(.141)**
Obs.	1209636	1209636	1209636	782697
R^2	.004	.018	.049	.16

B. Private control group	(1)	(2)	(3)	(4)
After	-.169	-.223	-.069	-.269
	(.096)*	(.096)**	(.083)	(.084)***
Public	-.159	-.267		
	(.132)	(.147)*		
Public-After	.259	.291	.206	.260
	(.100)***	(.104)***	(.089)**	(.090)***
Obs.	1292781	1292781	1292781	840510
R^2	.0003	.013	.051	.158

Notes: Dependent variable is the difference between the internal (school) grade and the external (national exam) grade of each student in each exam in each year. Dummy $After$ is one for 2007, 2008 and 2009 only. Data used: 2002 to 2009, except for 2006. Columns 2-3 includes a control for the number of exams taken in each school in each year. Column 3 controls for school fixed effects (up to 527 dummy variables); and column 4 controls for school-subject fixed effects (only the four main subjects, Portuguese, Math, Biology and Physics/Chemistry, are considered in this column). Robust standard errors, allowing for clustering at the school level. Significance levels: *: 0.05; **: 0.01; ***: 0.001.

Table 5: **Effects on grade inflation probability**

A. Islands control group	(1)	(2)	(3)	(4)
After	-.003	-.008	-.005	-.006
	(.008)	(.009)	(.009)	(.011)
Continent	-.068			
	(.009)***			
Continent-After	.021	.023	.026	.021
	(.009)**	(.010)**	(.010)***	(.011)*
Obs.	1209636	1209636	1209636	782697
R^2	.002	.008	.023	.08

B. Private control group	(1)	(2)	(3)	(4)
After	-.006	-.011	0.000	-.016
	(.008)	(.008)	(.007)	(.007)**
Public	-.020	-.028		
	(.012)*	(.013)**		
Public-After	.025	.028	.021	.031
	(.009)***	(.009)***	(.008)***	(.007)***
Obs.	1292781	1292781	1292781	840510
R^2	.0005	.006	.024	.082

Notes: Dependent variable is one if the internal (school) grade is greater than the external (national exam) grade, for each student in each exam in each year. Dummy $After$ is one for 2007, 2008 and 2009 only. Data used: 2002 to 2009, except for 2006. Columns 2-3 includes a control for the number of exams taken in each school in each year. Column 3 controls for school fixed effects (up to 527 dummy variables); and column 4 controls for school-subject fixed effects (only the four main subjects, Portuguese, Math, Biology and Physics/Chemistry, are considered in this column). Robust standard errors, allowing for clustering at the school level. Significance levels: *: 0.05; **: 0.01; ***: 0.001.

Table 6: **Effects on internal grades, year by year**

Treated group:	Continent public schools			Continent public schools		
Control group:	Azores and Madeira schools			Private schools		
	(1)	(2)	(3)	(4)	(5)	(6)
2003	-.008	-.003	.0002	.006	.005	.006
	(.006)	(.004)	(.004)	(.004)	(.004)	(.004)
2004	.0009	.001	.013	.011	.002	.004
	(.008)	(.007)	(.007)**	(.006)*	(.005)	(.006)
2005	-.002	-.001	.005	.009	.004	.006
	(.008)	(.008)	(.007)	(.006)*	(.005)	(.006)
2006	.013	.011	.019	.027	.020	.025
	(.005)**	(.005)**	(.006)***	(.006)***	(.004)***	(.005)***
2007	.015	.014	.031	.036	.020	.031
	(.005)***	(.004)***	(.006)***	(.007)***	(.005)***	(.006)***
2008	.004	.003	.023	.040	.024	.033
	(.007)	(.007)	(.009)***	(.008)***	(.006)***	(.007)***
2009	.009	.010	.024	.051	.036	.046
	(.007)	(.008)	(.009)***	(.009)***	(.007)***	(.007)***
Treated	.0005			-.035		
	(.006)			(.010)***		
Treated-2003	.009	.003	.002	-.003	-.005	-.005
	(.006)	(.004)	(.005)	(.004)	(.004)	(.005)
Treated-2004	-.0005	-.002	-.006	-.004	-.005	-.0003
	(.008)	(.007)	(.007)	(.006)	(.005)	(.006)
Treated-2005	.001	-.001	-.002	-.005	-.008	-.005
	(.009)	(.008)	(.007)	(.005)	(.005)	(.006)
Treated-2006	.0004	-.0002	-.001	-.013	-.010	-.008
	(.005)	(.005)	(.006)	(.006)**	(.005)**	(.006)
Treated-2007	-.011	-.013	-.015	-.025	-.021	-.019
	(.005)**	(.004)***	(.006)**	(.007)***	(.007)***	(.007)**
Treated-2008	-.001	-.003	-.008	-.030	-.026	-.021
	(.007)	(.007)	(.009)	(.008)***	(.007)***	(.008)**
Treated-2009	.0002	-.003	-.003	-.037	-.031	-.028
	(.008)	(.008)	(.009)	(.008)***	(.007)***	(.008)***
Obs.	1415829	1415829	918573	1512173	1512173	985754
R^2	.0007	.022	.05	.009	.039	.07

Notes: Dependent variable is the log of the school-level grade of each student in each exam in each year. Columns 2-3 and 5-6 include a control for the number of exams taken in each school in each year. Columns 2 and 5 control for school fixed effects (up to 527 dummy variables); and columns 3 and 6 control for school-subject fixed effects (only the four main subjects, Portuguese, Math, Biology and Physics/Chemistry, are considered in this column). Robust standard errors, allowing for clustering at the school level. Significance levels: *: 0.05; **: 0.01; ***: 0.001.

Table 7: **Effects on external grades, year by year**

Treated group:	Continent public schools			Continent public schools		
Control group:	Azores and Madeira schools			Private schools		
	(1)	(2)	(3)	(4)	(5)	(6)
2003	.005	.003	-.040	.009	.014	-.007
	(.021)	(.022)	(.023)*	(.013)	(.010)	(.012)
2004	-.058	-.076	-.073	.0004	-.024	-.010
	(.020)***	(.025)***	(.030)**	(.016)	(.012)*	(.018)
2005	-.007	-.017	-.034	.054	.037	.043
	(.018)	(.022)	(.028)	(.014)***	(.012)***	(.015)***
2006	-.059	-.065	-.065	-.018	-.038	-.036
	(.022)***	(.021)***	(.028)**	(.016)	(.012)***	(.014)**
2007	-.040	-.066	-.025	.025	-.032	.006
	(.018)**	(.020)***	(.024)	(.019)	(.012)***	(.014)
2008	.088	.060	.115	.140	.085	.136
	(.020)***	(.023)***	(.029)***	(.020)***	(.013)***	(.014)***
2009	.071	.051	.102	.129	.078	.112
	(.020)***	(.026)*	(.032)***	(.020)***	(.014)***	(.016)***
Treated	.108			-.025		
	(.027)***			(.021)		
Treated-2003	-.007	-.009	.010	-.005	-.022	-.024
	(.021)	(.022)	(.023)	(.013)	(.010)**	(.013)*
Treated-2004	.019	.020	.027	-.019	-.035	-.040
	(.020)	(.025)	(.030)	(.016)	(.013)***	(.018)**
Treated-2005	.019	.014	.026	-.027	-.043	-.053
	(.018)	(.022)	(.028)	(.015)*	(.013)***	(.016)***
Treated-2006	.007	.002	.003	-.030	-.026	-.028
	(.023)	(.021)	(.028)	(.017)*	(.013)**	(.015)*
Treated-2007	-.025	-.026	-.028	-.070	-.064	-.064
	(.019)	(.021)	(.024)	(.020)***	(.013)***	(.015)***
Treated-2008	-.048	-.047	-.052	-.080	-.076	-.077
	(.021)**	(.023)**	(.029)*	(.021)***	(.014)***	(.015)***
Treated-2009	-.046	-.049	-.074	-.091	-.079	-.087
	(.021)**	(.026)*	(.033)**	(.021)***	(.015)***	(.017)***
Obs.	1415164	1415164	918075	1511472	1511472	985227
R^2	.008	.048	.135	.011	.054	.137

Notes: Dependent variable is the log of the national exam grade of each student in each exam in each year. Columns 2-3 and 5-6 include a control for the number of exams taken in each school in each year. Columns 2 and 5 control for school fixed effects (up to 527 dummy variables); and columns 3 and 6 control for school-subject fixed effects (only the four main subjects, Portuguese, Math, Biology and Physics/Chemistry, are considered in this column). Robust standard errors, allowing for clustering at the school level. Significance levels: *: 0.05; **: 0.01; ***: 0.001.

Table 8: **Effects on grade inflation, year by year**

Treated group:	Continent public schools			Continent public schools		
Control group:	Azores and Madeira schools			Private schools		
	(1)	(2)	(3)	(4)	(5)	(6)
2003	-.078	-.006	.416	.028	-.029	.241
	(.182)	(.206)	(.187)**	(.105)	(.084)	(.100)**
2004	.483	.652	.768	.209	.313	.239
	(.160)***	(.203)***	(.197)***	(.102)**	(.084)***	(.131)*
2005	-.033	.071	.246	-.377	-.306	-.352
	(.158)	(.185)	(.172)	(.111)***	(.101)***	(.125)***
2006	.737	.773	.935	.644	.737	.797
	(.185)***	(.163)***	(.229)***	(.116)***	(.111)***	(.138)***
2007	.610	.809	.789	.360	.669	.530
	(.159)***	(.173)***	(.220)***	(.132)***	(.110)***	(.129)***
2008	-.581	-.364	-.488	-.700	-.421	-.718
	(.170)***	(.191)*	(.213)**	(.135)***	(.111)***	(.121)***
2009	-.387	-.208	-.280	-.463	-.196	-.296
	(.156)**	(.194)	(.227)	(.119)***	(.097)**	(.107)***
Continent	-.918			-.192		
	(.182)***			(.152)		
Treated-2003	.167	.127	-.029	.037	.151	.145
	(.184)	(.206)	(.189)	(.106)	(.088)*	(.105)
Treated-2004	-.077	-.119	-.222	.103	.225	.302
	(.163)	(.199)	(.195)	(.102)	(.088)**	(.135)**
Treated-2005	-.122	-.115	-.216	.151	.266	.378
	(.162)	(.183)	(.172)	(.115)	(.106)**	(.129)***
Treated-2006	-.0006	.033	-.023	.074	.070	.114
	(.188)	(.166)	(.232)	(.121)	(.116)	(.144)
Treated-2007	.121	.112	.035	.270	.257	.288
	(.164)	(.171)	(.218)	(.138)**	(.112)**	(.132)**
Treated-2008	.370	.338	.267	.394	.399	.492
	(.174)**	(.188)*	(.209)	(.144)***	(.115)***	(.125)***
Treated-2009	.387	.361	.473	.403	.352	.486
	(.161)**	(.193)*	(.226)**	(.128)***	(.102)***	(.114)***
Obs.	1415829	1415829	918573	1512173	1512173	985754
R^2	.016	.059	.186	.015	.061	.184

Notes: Dependent variable is the difference between the internal (school) grade and the external (national exam) grade of each student in each exam in each year. Columns 2-3 and 5-6 include a control for the number of exams taken in each school in each year. Columns 2 and 5 control for school fixed effects (up to 527 dummy variables); and columns 3 and 6 control for school-subject fixed effects (only the four main subjects, Portuguese, Math, Biology and Physics/Chemistry, are considered in this column). Robust standard errors, allowing for clustering at the school level. Significance levels: *: 0.05; **: 0.01; ***: 0.001.

Table 9: **Robustness: early retirement and economic controls**

Dependent variable:	Log external grade			Difference between external and internal grade		
	(1)	(2)	(3)	(4)	(5)	(6)
Early retirement						
After	.008	.010	.053	.047	.026	-.132
	(.007)	(.007)	(.008)***	(.054)	(.050)	(.057)**
High Retirement	.085			-.567		
	(.014)***			(.101)***		
(High Retirement)-After	-.002	-.013	-.030	.077	.128	.218
	(.010)	(.009)	(.011)***	(.080)	(.076)*	(.087)**
Obs.	632632	632632	408861	632936	632936	409085
R^2	.009	.041	.107	.008	.046	.149
Extra control variables						
After	.007	-.014	.028	.178	.461	.392
	(.013)	(.014)	(.015)*	(.126)	(.129)***	(.137)***
Continent	.096			-.745		
	(.017)***			(.113)***		
Continent-After	-.047	-.054	-.072	.246	.377	.440
	(.013)***	(.013)***	(.015)***	(.126)*	(.126)***	(.130)***
Obs.	811995	811995	536667	812419	812419	536992
R^2	.012	.045	.115	.017	.054	.159

Notes: Dependent variable is the log of the national exam grade of each student in each exam in each year (left panel) or the difference between the internal and external grades (right panel). Top panel: '(High Retirement)' is a dummy variable equal to one (zero) if the ratio of retirements of teachers in the school from April 2008 to August 2009 to retirements of non-teachers in the same school is above the 66th (below the 33rd) percentile of the distribution of that ratio across all schools. Data only includes students in public schools. Bottom panel: benchmark data and specifications but including extra control variables (wages, female ratio, schooling and workforce size per *concelho*). Columns 2-3 and 5-6 include a control for the number of exams taken in each school in each year. Columns 2 and 4 control for school fixed effects (up to 527 dummy variables); and columns 3 and 6 control for school-subject fixed effects (only the four main subjects, Portuguese, Math, Biology and Physics/Chemistry, are considered in this column). Robust standard errors, allowing for clustering at the school level. Significance levels: +: 0.10; *: 0.05; **: 0.01; ***: 0.001.