# Does Increasing Risk Widen Gender Gaps?

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**Abstract**

We examine the response to an exogenous change in the risk profile of an important educational choice – requesting a regrade. We demonstrate how ostensibly gender-neutral policies can generate gaps across men and women because they differ in their perceptions of risk. Specifically, we show that an exogenous shift in the risk of requesting a regrade augmented the regrade request gap by nearly 100 percent. We show that this has consequential implications for students through its impact on their grade points. These findings reveal how gender differences identified in the lab manifest when men and women make real world decisions.

**JEL Codes: J16, I24, I26, D81, J7**
**Keywords: risk, gender gaps, educational choices, inequality**

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# 1 Introduction

Most policy reforms impact the risk profile of choices. This is true across a broad range of policy spheres: education finance reforms change the risk of taking loans and pursuing higher education, crime policies alter the risk of breaking the law, and changes to the welfare state alter the risk of starting new businesses and pursuing entrepreneurship. However, different groups may have systematically different risk perceptions and tolerances. As a consequence, ostensibly neutral policies can generate unintended gaps across groups that differ in their risk preferences.

A substantial body of research in the social sciences has documented gender differences in a variety of behaviors and preferences, including risk. Specifically, women tend to be considerably less confident (Barber and Odean, 2001), more risk averse (Charness and Gneezy, 2012), and opt out of competition more than men (Gneezy et al., 2003; Niederle and Vesterlund, 2007). Prior work also suggests that gender differences in primitives like preferences and confidence impact gender gaps in labor market outcomes. However, field demonstrations have focused on correlations between choices and lab-elicited preferences (e.g. Buser et al., 2014; Li and Zafar, 2022; Cortés et al., 2022). Outside the lab, there is very little causal work on the consequences of gender differences in beliefs and preferences (Flory et al., 2015; Samek, 2019).

In this paper, we study a national policy that changed the risk profile of a key educational choice in college – the decision to request a regrade. This provides us with a natural experiment to assess the effect of policies that shift the risk profile of choices on gender gaps in outcomes. The educational choice we examine has consequential downstream implications for students' grades. We provide novel evidence of the consequences of gender differences in the response to risk using real world decision environments.

The policy we examine was implemented in Norway in 2014 and altered the procedure for students to request regrades on assignments at higher education institutions. In Norway, all students have the legal right to request a regrade of any assignment that impacts their final grade. The regrade is performed by a new examiner who was not involved in the original grade decision, and the decision of that examiner is final. Prior to the reform, the new examiner would receive the assignment or exam and also would be informed about the student's initial grade, the original examiner's explanation of the initial grade, and the student's motivation for requesting a regrade. After the reform, the new examiner received only the student's assignment or exam. The goal of this reform was to provide students with a fair chance at reassessment not anchored to prior results. The reform led to a change both in the expected value as well as the variance of the regrade requests. Because the reform narrowly impacts the risk of asking for a regrade, while leaving the other elements of the

regrade process unchanged, it provides an ideal setting for examining how policies that shift the risk profile of choices impact gender gaps in education outcomes.

Using unique individual-level administrative data from Norway's leading business school on all student exams, grades, and regrades (requests and results), we document a substantial increase in the likelihood that a grade gets changed as a result of the policy. Before the policy was implemented, 84.4 percent of all regrade requests resulted in an unchanged grade, 14.7 percent resulted in a grade improvement, and 0.9 percent led to a grade reduction. Following the reform, the expected value of a regrade request increased, but so did the variance in outcomes. Specifically, after the reform, the share of regrades that receive a negative reevaluation increases to 11.7 percent of all requests (up 1300 percent) and the share of regrades that receive a favorable reevaluation increases to 32.2 percent (up 219 percent).[1]

Using a difference-in-differences framework, we exploit our unique administrative data to compare the behavior of men and women pre and post the policy change and provide three sets of results. First, we demonstrate that the policy induced a substantial increase in the gender gap in regrade requests. While both men and women became more likely to request a regrade after the policy change, men responded more to the reform. The gender gap in regrade requests increased by around 1.3 percentage points. This is off a baseline gap of 1.5 percentage points, raising the gender gap in regrade requests by nearly 100 percent.

Second, we show that the gender gap in regrade requests translates into a gender gap on consequential downstream outcomes: grade points earned via regrade. This implies that the ostensibly gender-neutral regrade policy may push qualifying females below entry thresholds imposed by employers. For example, prior studies have found that a 1 point increase in college GPA raises the employer's hiring interest by 0.8 SD (Kessler et al., 2019) and that callback rates increase by 7 percentage points when college GPA increases from 2.5 to 3.6 (Quadlin, 2018). On a general level, this highlights the role that social institutions may play in shaping the dynamics of gender gaps in society.

Finally, we show that men and women adjust their regrade request behavior based on prior regrade successes and failures. However, there are no gender differences in students' responses to prior outcomes. Experience with regrade outcomes is unlikely to close the gender gap on its own.

The key contribution of this paper is to demonstrate how ostensibly gender-neutral policies can generate gaps across men and women who differ in their risk profiles. We use unique

[1]While the exact shift in regrade outcomes was unknown at implementation, the largest student organizations in Norway believed the blinding would generate a favorable outcome for their members (see https://www.idunn.no/doi/10.18261/issn.1893-8981-2019-04-02).

individual-level student data and exploit a policy that changed the risk profile of educational choices holding all other elements constant. This allows us to provide some of the first evidence of the consequences of gender differences in the response to risk in a consequential real world decision environment.We contribute to the existing literature in several ways.

First, we build on the well-established experimental literature that examines gender differences. A key focus of this literature has been to examine how much lab measures of preferences and expectations correlate with real world behaviors and judgments (e.g., Falk et al., 2018; Sutter et al., 2013). Examples include risk preferences (e.g., Dohmen et al., 2011), time preferences (e.g., Meier and Sprenger, 2010), competitiveness preferences (e.g., Buser et al., 2014), and expectations (e.g., D'Acunto et al., 2021). Beyond the overall correlations, there has also been a growing interest in understanding the way that demographic differences in preferences lead to differences in outcomes, particularly focused on the role of gender (e.g., Buser et al., 2014; Li and Zafar, 2022; Cortés et al., 2022). Substantially less work has been able to cleanly examine the causal role that specific differences in preferences and beliefs play in driving behavior outside the lab (Flory et al., 2015; Samek, 2019). We advance this literature by isolating the role of risk in a naturally occurring decision, thus advancing our understanding of how differences in preferences and beliefs contribute to the demographic gaps that we observe in the world. We document how a change in the riskiness of a real world decision interacts with well-studied gender differences in preferences to produce gender gaps in outcomes. The precise impact of the policy on risk allows us to make stronger causal claims using data from naturally occurring environments.

Second, there is a rich empirical literature documenting gaps in key labor market outcomes across groups (gender, ethnicity, age) and trying to identify channels through which these gaps occur (Altonji and Blank, 1999; Goldin and Rouse, 2000; Bertrand, 2011; Olivetti and Petrongolo, 2016; Blau and Kahn, 2017). These studies tend to have well-identified reduced-form effects, but are often unable to relate these gaps to differences in specific traits or behaviors across groups. This is understandable, as such exercises would require two distinct sources of variation (one that allows identification of the reduced-form effects and one that allows the disentanglement of how a particular character trait interacts with that policy). We build on this literature by evaluating a policy that exclusively shifts one specific behavioral trait – the risk profile. This allows us to push the empirical literature forward, shedding light on whether specific cross-group behavioral traits contribute to the education and labor market gaps we observe in the real world.

More generally, this paper expands upon an impressive literature on gender differences in labor market outcomes to identify how group-differences in specific characteristics may interact with social institutions to augment or eliminate pre-existing gaps. The classical

studies on this topic have focused on the role of differences in occupational choice, discrimination, experience, and human capital (e.g., Hyde, 2005; Bohren et al., 2019; Card and Payne, 2021). More recent work has focused on differences in competitiveness (Niederle and Vesterlund, 2007; Tungodden and Willén, 2022), self-promotion (Exley and Kessler, 2022), bargaining ability (Card et al., 2013), asking (Small et al., 2007; Babcock and Laschevar, 2003; Leibbrandt and List, 2015; Li and Zafar, 2022; Roussille, 2022), and role models (Riise et al., 2020). Our paper should be seen as an effort to bridge the well-established lab experiments on gender differences in behaviors with the rich empirical literature on gender gaps in labor market and educational outcomes. We provide some of the first causal evidence of the consequences of gender differences in specific character traits using real world decision environments.

In addition, there is an emerging literature on systemic discrimination on prima facie gender/race neutral policies generating disparate outcomes (Bohren et al., 2022). Our findings can be viewed as an example of systemic discrimination in this framework. We observe total discrimination in the form of disparate grade points earned from regrades for men and women. However, there is little scope for direct discrimination in our setting and we observe no gender differences in returns conditional on requests. As a result, nearly all of the discrimination we observe is systemic driven by gender differences in the perceptions of risk and risk tolerances that drive the decision to ask for a regrade.

The closest paper to ours is Li and Zafar (2022). Using administrative data from a US university, the authors establish that men are more likely to have their grades changed at the end of the semester than women. To explain this result, they collect survey data on students' regrade behavior and teachers' perceptions of that behavior. They then conduct an experiment that shows men are less sensitive to the costs of asking than women are. Our setting provides three core advantages relative to this paper.

First, we leverage exogenous variation in the risk profile of regrade decisions for identification. This allows us to tease out causal effects of altering the risk profile of consequential educational choices, something which was not possible in Li and Zafar (2022). Second, our data includes all formal requests for a regrade, both those which result in changes and those which do not. As a result, our setting reduces concerns about selective reporting that might arise from only observing when grades are changed. As noted in Li and Zafar (2022), this represents the ideal data set for examining gender gaps in regrade requests. Such data is not available in the US context. Third, the Norwegian regrade request system is formal and anonymous at key junctures, substantially reducing the scope for both discrimination and potential concerns about retaliation. The natural experiment in our setting restricts the scope for factors unrelated to the risks associated with submitting a regrade request.

Our results have important policy implications. Specifically, many public policies generate shifts in the risk associated with individual choices. While most of these shifts in the riskiness of choices are known to policymakers, the policies themselves are oftentimes designed to be ostensibly neutral across groups. However, if the risk perceptions and tolerances of individuals systematically varies across group, then this represents an overlooked obstacle to cross-group equality. While this paper has focused on differences between men and women, the framework can easily be expanded to encompass any groups of people that display systematic differences in response to risk. On a general level, this suggests that the extent to which social institutions are designed to balance the risk and reward of individual choices, may have substantial effects on how gender gaps are shaped in society.

# 2 Study design

## 2.1 Institutional background

Our study is conducted using data from the Norwegian School of Economics (NHH). Norway is a country known for its gender balanced labor market policies and active equity policies. Aggregate statistics suggest that the active gender equalization approach has been successful, with Norway consistently being placed at the top of the global gender equality rankings year after year (e.g., Baumann, 2021; Sharma et al., 2021). However, large differences remain. Men and women still earn different wages, sort into different occupations, pursue different educational paths, and respond to labor market shocks in different ways (e.g., Nilsen, 2020; Benavot et al., 2016; Salvanes et al., 2022). In addition, gender differences in preferences remain large, and there are important differences in how men and women make choices (e.g., Almås et al., 2016; Tungodden and Willén, 2022).

NHH is a public business school with free undergraduate and masters programs in business. Its undergraduate program is consistently ranked among the most popular programs in the country, with an acceptance rate of around 9 percent. The school has approximately 3,500 students and attracts students from all parts of Norway.

## 2.2 Grading and the Regrade Process

Grading at NHH is done on a 6 point scale that ranges from A through F, with F representing a fail and A representing the top grade. There is no cap on the percent of students that can receive a specific grade. Certain courses use a binary "pass-fail" scale instead, but this is rare.

All students have the right to request a regrade of an exam or assignment, though oral exams cannot be reassessed due to the nature of the exam. The appeal process begins with the student requesting an explanation for the grade he/she received. This appeal must take place within seven days of having received the original grade, and is done either through

the online student learning interface or via email to the exam office. The examiner then has two weeks to provide an explanation for why a certain grade was given. The examiner may choose whether to give the student a written or an oral explanation.[2] They may also convey an explanation by a lecture or in some other forum. The explanation should provide the general principles underlying the assessment of the candidate's performance.

After having received the examiner's explanation, the student can request a complete regrade of the assignment or exam. The deadline for submitting an appeal is three weeks after having received the explanation. The exam is sent to new examiners, who have not been involved in issuing the original grade assigned to the student. The result of the appeal is final and cannot be appealed.

Prior to the 2014 reform, the new examiners would receive the assignment/exam to grade and, in addition, the student's initial grade, the original examiner's justification for awarding the student that grade, and the student's motivation for requesting a justification. Following the legislative change in 2014, the new examiners would only receive the assignment/exam to grade. They would no longer receive the student's original grade, the original examiner's justification for setting that grade, or the student's justification for requesting a regrade. In other words, the new examiners would be completely blinded from the prior assessment decision. The goal of this reform was to provide students with a fair chance at reassessment not anchored to prior results. While there was a considerable public debate on the question of whether to blind the regrade process or not, the National Student Organization of Norway was a strong proponent of the policy.[3]

# 3 Conceptual Framework

Why might men and women differ in their response to the change in regrade policy? Prior research suggests three main avenues: gender differences in risk preferences and the evaluation of grades, gender differences in confidence and belief updating, and gender differences in costs of asking.

First, there is a large literature suggesting that men and women have meaningfully different tolerances for risks (e.g., Charness and Gneezy, 2012; Croson and Gneezy, 2009). As shown in figure 1, the objective likelihoods of improving or worsening a grade both increase substantially after the 2014 policy change. The expected value increases from .14 grade points in the period to .22 grade points in the post period, but the variance of outcomes increases as well. As a result, if men are more tolerant of risk than women, we would expect

[2]While both the original examiner and regrader only receive a student id number while grading, this step in the process is identified to the original grader. See appendix figure A4 for an example.

[3]See https://studenttorget.no/index.php?artikkelid=13762expand=37 or https://www.jus.uio.no/studier/aktuelle-saker/2016/bilder/kd-notat-blind-klagesensur.pdf for some discussions in Norwegian

to see a widening of the gender gap after the policy is implemented. Additionally, women may value grades more than men, which would accentuate gender differences in risk aversion in our setting (Ugalde, 2022).

Second, men tend to be more confident in their own performances than women (e.g., Niederle and Vesterlund, 2007; Baldiga, 2014; Exley and Kessler, 2022; Barber and Odean, 2001). Men, as a result, may have inflated beliefs relative to women about the likelihood that a regrade will be successful. Additionally, men may believe that the baseline quality of their work product is higher, affecting what they view as a fair grade relative to women and thus increasing their likelihood of asking for a regrade. Finally, men's increased confidence may also manifest as reduced sensitivity to feedback. Research on belief updating shows that men and women tend to respond differently to self-relevant signals (e.g., Coffman et al., 2021; Ertac, 2011). As a result, men and women may interpret the feedback they receive on their request for a justification differently. If women update more from the responses of the professors defending their own grading, then the regrade request differential could arise from differences in response to feedback.

Finally, women may be more sensitive to social costs of asking and claiming than men. Women are less likely to negotiate(e.g., Babcock and Laschevar, 2003; Exley et al., 2020; Roussille, 2022), self-promote (Exley and Kessler, 2022), ask for advice (Heikensten and Isaksson, 2019), and claim credit (Isaksson, 2018) each of which entails a potential social cost due to possible backlash. Additionally, Li and Zafar (2022) provide direct evidence in the lab that men are less sensitive to the costs of asking. While gender differences in sensitivity to social costs may impact the initial gender gap, they are unlikely to interact with the policy change we examine because the procedure for requesting a regrade is held constant.

In our analysis, we examine the overall impact of the change in risk induced by the policy and provide suggestive evidence on which of these channels is driving the results we find. The social cost component is minimized by our setting. In addition, by examining how responsive individuals are to first regrade outcomes, we are able to explore the extent to which risk aversion in the utility sense vs. beliefs can drive our overall results. We discuss these results in detail below.

# 4 Data and Method

## 4.1 Administrative Data

We analyze a unique administrative data set from NHH. This data extends from 2009 through 2019 and consists of information collected and combined from 5 separate student registers: the student population register, the student major register, the student course

register, the student grade register, and the student regrade register.

These data provide us with individual-level student information on a set of demographic characteristics (gender, year of birth, and which high school they attended). It also provides us with detailed information on the students' choice of major, which student cohort they belong to, and which courses they have taken at the school.

Crucial to our analysis is the ability to observe the students' performance on every exam and assignment in every course during their time at the school, whether the student requested a regrade, and what the final outcome of the regrade was. We collect this information from the student grade and regrade registers.[4] We restrict the data to all grades in the range B to E since only these grades are affected by the change in risk profile.[5] One benefit of the Norwegian setting is the formalization of the grade and regrade process, ensuring that we observe all regrade requests ever made during a semester, thus removing any concerns of selective reporting.

We combine these five data sets to construct our analysis data. Our sample includes data on 11792 unique students. 40% of the students are women. The average year of birth is 1990. 5894 students are observed only in one program (i.e., bachelors, masters), while 5898 are observed in two. As a result, our data include 8882 bachelors level students and 11774 masters level students.

## 4.2 Estimation Strategy

To examine if the policy induced a change in the gender gap in regrades, we rely on the following difference-in-differences specification:

$$Y_{ict} = \alpha_0 + \beta_1 Female_i + \beta_2 Post_t + \beta_3 (Female_i * Post_t) + \rho_t + \gamma_c + \tau_g + \epsilon_{ict} \qquad (1)$$

where $Y_{ict}$ is an outcome for student $i$ in class $c$ and year $t$. $Female$ is a binary variable taking the value of one if the student's sex is female. $Post$ is an indicator variable that is switched on if the observation is from the post-reform period ($\geq$ 2014). The coefficient $\beta_1$ provides information on the difference in outcomes between men and women prior to the reform, $\beta_2$ provides information on the effect of the policy on male regrade behavior, and $\beta_3$ identifies whether the policy had a differential effect on female students. The baseline specification includes no fixed effects. Additional specifications include a full set of year ($\rho_t$),

[4]For some regrades, no information on the initial grade the student received was recorded. In these cases we impute the original grade using information on the post-regrade grade and information on the direction of any grade change from the original grade, adjusting grades by one level in the appropriate direction.

[5]Students receiving A's have no reason to request a regrade; students receiving F's have no downside risk associated with requesting a regrade. Appendix Figure A1 shows that including F's does not alter our results.

class ($\gamma_c$), and original course grade ($\tau_g$) fixed effects.

The identifying assumption underlying our estimation strategy is that males and females were not trending differently in their outcomes prior to the policy, such that the pre-reform gender gap can be used as a plausible counterfactual of what the gap would have looked like in the post-reform period absent the policy. In addition, we must assume that there are no other shocks contemporaneous with the regrade reform that is correlated with the outcomes and that differentially impact males and females.

To examine the plausibility of the parallel trend assumption, we estimate a set of event studies of the following form:

$$Y_{it} = \alpha + \sum_{t=2010}^{t=2019} [\delta_t(Female_{it})] + \epsilon_{ict}, \tag{2}$$

where $\delta_t$ non-parametrically traces out any pre-treatment relative trends across gender ($t =$ 2010 through $t = 2013$) as well as any time-varying treatment effects ($t = 2014$ through $t = 2019$). Examining potential time-varying treatment effects are particularly interesting as students may adjust their expectations over time following the implementation of the new policy. This is something we examine directly in the next section.

The second assumption, that there are no other shocks contemporaneous with the regrade reform that is correlated with the outcomes and that differentially impact males and females, is more difficult to test. However, we note that the school did not implement any other large-scale policies at the time of the regrade reform. In addition, we present results from a series of robustness and sensitivity analyses in which we alter the sample, the model specification, and the fixed effects. The robustness of our results across these specifications provide additional credibility for a causal interpretation of our findings.

# 5 Results

In this section, we present our key findings on the effect of changing the risk profile of consequential education choices on the gender gap in outcomes. We establish that the expected value and the risk of asking for a regrade changed following the new regrade policy. We then examine how this policy affected the probability of asking for a regrade for men and women. We show that these differences translate into gender differences in the students' average grade points earned per year via regrades. Finally, we turn to belief updating, and examine if prior regrade outcomes affect students likelihood to request regrades in the future.

Figure 1 shows the final outcome of all regrade requests over time, from 2009 through 2019. Panel A shows the share of regrade requests that results in a worse grade than the initial grade, Panel B shows the share of regrade requests that results in a better grade than the initial grade, and Panel C shows the share of regrade requests that result in no grade

change.

The share of regrade requests that were marked up, marked down, or not changed, was very stable in the pre-policy years, and there are no noticeable time trends in the final outcome of regrade requests before 2014. This suggest that there were no contemporaneous policies in the grade and regrade process in the years leading up to the reform that altered the incentives to request regrades, which could risk contaminating our findings.

Prior to the policy change, 84.4 percent of all regrade requests resulted in an unchanged grade, 14.7 percent resulted in a grade improvement, and only 0.9 percent led to a grade reduction. The expected return on asking for a regrade was .14 grade points in the pre-reform period. In other words, the majority of regrade requests prior to the reform resulted in no change of the initial grade.

Following the policy change, there is a dramatic rise in the share of regrades that result in a better grade as well as a worse grade. After the reform, the share of regrades that receive a negative reevaluation increases to 11.7 percent of all requests (up 1300 percent) and the share of regrades that receive a favorable reevaluation increases to 32.2 percent (up 219 percent). The expected return on asking for a regrade in the post-reform period was .22 grade points; a 50 percent increase from the pre-reform value.

Taken together, Figure 1 demonstrates that the regrade policy generated a shift in the expected value as well as the risk associated with asking for a regrade. We use the change in the objective risk of requesting a regrade to examine whether altering the risk profile of choices has an impact on the choices made by students, and if that impact differ across the genders.

In Figure 2, we present evidence on the extent to which the change in risk profile altered the regrade request probability, both overall (Panel A) and by gender (Panel B). Panel A of Figure 2 demonstrates that the probability of requesting a regrade for an assignment was relatively stable at around 5 percent of all assignments prior to the policy change. After the policy reform, this probability increased to around 8 percent – a 60 percent increase relative to baseline. Students responded strongly to the reform, suggesting that their beliefs about the expected value of a regrade request went up in response to the policy change.

Panel B of Figure 2 provides three important results. First, prior to the reform there was a substantial gender gap in the propensity to request a regrade. Second, both men and women responded to the reform by being more likely to request a regrade of an assignment. Third, the male response to the policy was much greater than the female response to the policy, such that the policy amplified the pre-existing gender gap in regrade requests.

To assess the gender gap in regrade requests, Figure 3 plots point estimates of the interaction between female and time obtained from estimating Equation (3). Panel A provides

results for all assignments, while Panel B provides results focusing exclusively on written exams. Both panels illustrate that the introduction of the regrade policy generated a statistically significant and economically meaningful increase in the gender gap in regrade requests, with males becoming much more likely to request a regrade relative to females after the policy.

To summarize the results from the event studies and examine robustness across different specifications, Table 1 plots the results from estimating the more conventional difference-in-differences model, Equation (2), on the probability of asking for a regrade. In Panel A, we show results for all assignments. In Panel B, we provide results obtained from restricting the analysis to only written exams. In each of the panels, we show how the results change as we sequentially add additional layers of fixed effects, from none (column 1) to a full set of year, class, original grade, and whether the course was retaken fixed effects (column 4).

The results demonstrate that there was a significant gender difference in regrade requests even before the policy was implemented of approximately 1.7 percent (coefficient on *Female*). The results further show that the reform induced a significant increase in the probability of requesting a regrade, up 3.4 percent in the baseline specification (coefficient on *Post Blinding*). Finally, the results demonstrate that this increase was significantly lower for females (coefficient on *Female x Post Blinding*). Taken together, the results demonstrate that the policy change of blinding the regrade process generated a close to 100 percent increase in the gender difference in regrade requests.

To better understand which students are impacted by the policy, we consider how the 2014 policy change affects gender gaps both on the extensive and intensive margins.[6] Women are 7 percentage points less likely than men to ask for a regrade in the pre-reform period. The gender gap on the extensive margin widens in the post-reform period by 3.5 percentage points, or about a 50 percent increase in the gender gap in ever asking for a regrade. This suggests that marginal men perceive the returns to asking for a regrade as larger than marginal women do in the post-period. Turning to the question of the intensive margin, we condition on students who ever ask for a regrade. There is an initial gender gap of 0.16 requests which increases by 0.18 requests (110 percent) in the post reform period. Even among women who are willing to ask at all, they are less willing to ask than men are. Our results differ somewhat from Li and Zafar (2022) who find a gender gap in self-reported intentions to request a regrade on the intensive but not extensive margin.

Next, we ask whether the augmented gender gap in regrade requests has consequential implications for the students, both in terms of average yearly grade changes from regrades as well as the average grade point change per regrade request. We present results on these

[6]For full regression output see Table A1.

questions in Figure 4.

Panel A illustrates that the policy change is associated with a significant increase in the gender gap in average yearly grade change, going from no gender gap in 2013 to a gender gap of approximately 25 percent in 2016. Panel B shows the average grade point change earned by men and women. If anything, this panel shows women earn slightly more grade points per request, but as shown in Panel A, this effect is far outweighed by men asking more frequently.[7]

We consider the impact of a student's first regrade outcome on their subsequent propensity to request regrades as suggestive for the role of beliefs in regrade request decisions.[8] The first regrade outcome is unlikely to shift risk preferences, but may lead students to update their beliefs about the likelihood of grade increases/decreases from regrade requests. We find that men are 2 percentage points more likely to submit for a regrade after a positive regrade outcome and 2.7 percentage points less likely to submit after a negative regrade outcome. While we find a 2.2 percentage point negative main effect for women, we find no significant interactions between prior outcomes. Our core results are therefore unlikely to be driven by standard risk aversion and implicate beliefs and belief updating in the decision to request a regrade.

# 6 Discussion

We examine how changing the risk profile of a consequential education choice impacts the gender gap in willingness to ask. We find that a Norwegian policy that blinded examiners to the original grade increased the expected value of requesting a regrade, but also increased the risk of a grade reduction. Changing the risk of requesting a regrade led to an increase in overall regrade requests and a 100 percent increase in the gender gap. The gender gap widens on both the intensive and extensive margins after the implementation of the blinding policy. While our results are likely determined by multiple mechanisms, we provide suggestive evidence that our results are not exclusively driven by gender differences in risk preferences but are likely also driven by differences in beliefs.

This paper bridges the gap between the literature on gender differences in preferences conducted using lab experiments (e.g., Niederle and Vesterlund, 2007; Buser et al., 2014; Li and Zafar, 2022), and the literature on gender gaps using observational data (e.g., Altonji and Blank, 1999; Goldin and Rouse, 2000; Bertrand, 2011; Olivetti and Petrongolo, 2016; Blau and Kahn, 2017). Each of these approaches comes with a downside. It is difficult to make strong causal claims about particular gender gaps in the world based on results from the lab. In the observational studies, it is difficult to pin down the exact behavioral

[7]For simple difference-in-differences estimates, see table A2.

[8]For a more detailed description of the analysis and full regression results, please see the appendix.

mechanism underlying the reduced-form effects.

We bridge these literatures by using real world decisions with a narrowly defined policy treatment. A major advantage of our setting is the limited scope for mechanisms other than risk differences to drive our results. For example, discrimination (Altonji and Blank, 1999; Goldin and Rouse, 2000; Bohren et al., 2019) and biased expectations of evaluators (Babcock et al., 2017) are unlikely to drive the results since students know the actual grading is anonymous. As a result, our setting provides an ideal test in real-world behavior of the role of risk perception and tolerance in generating gender gaps.

We also contribute to a literature on who takes up government benefits and why (Currie, 2004; Heckman and Landersø, 2022). Most policy reforms impact the risk profile of choices. The changes to the risk of choices caused by policy alterations are sometimes intentional and sometimes unintentional. However, it is very rare for such policy-induced changes in the riskiness of choices to be motivated by a desire to differentially affect groups that may have systematically different risk perceptions and tolerances.

In this paper, we demonstrate that failing to account for systematic differences in behaviors and perceptions across groups when making policies can have consequential and detrimental effects on certain groups. If policy changes alter the risk profile of choices, the impact will likely vary across people due to the type of selection we document. While men might disproportionately benefit from some polices, as we document here, women may disproportionately benefit from other policies.

More broadly, our paper raises the question of how differences across groups in fundamental economic preferences interacts with policy structures to influence the take-up of opportunities. We show that even in the absence of meaningful discrimination, the structure of the decision environment can widen gaps in who takes advantage of services and opportunities.

Figure 1: The Proportion of Regrade Outcomes By Year for All Requests

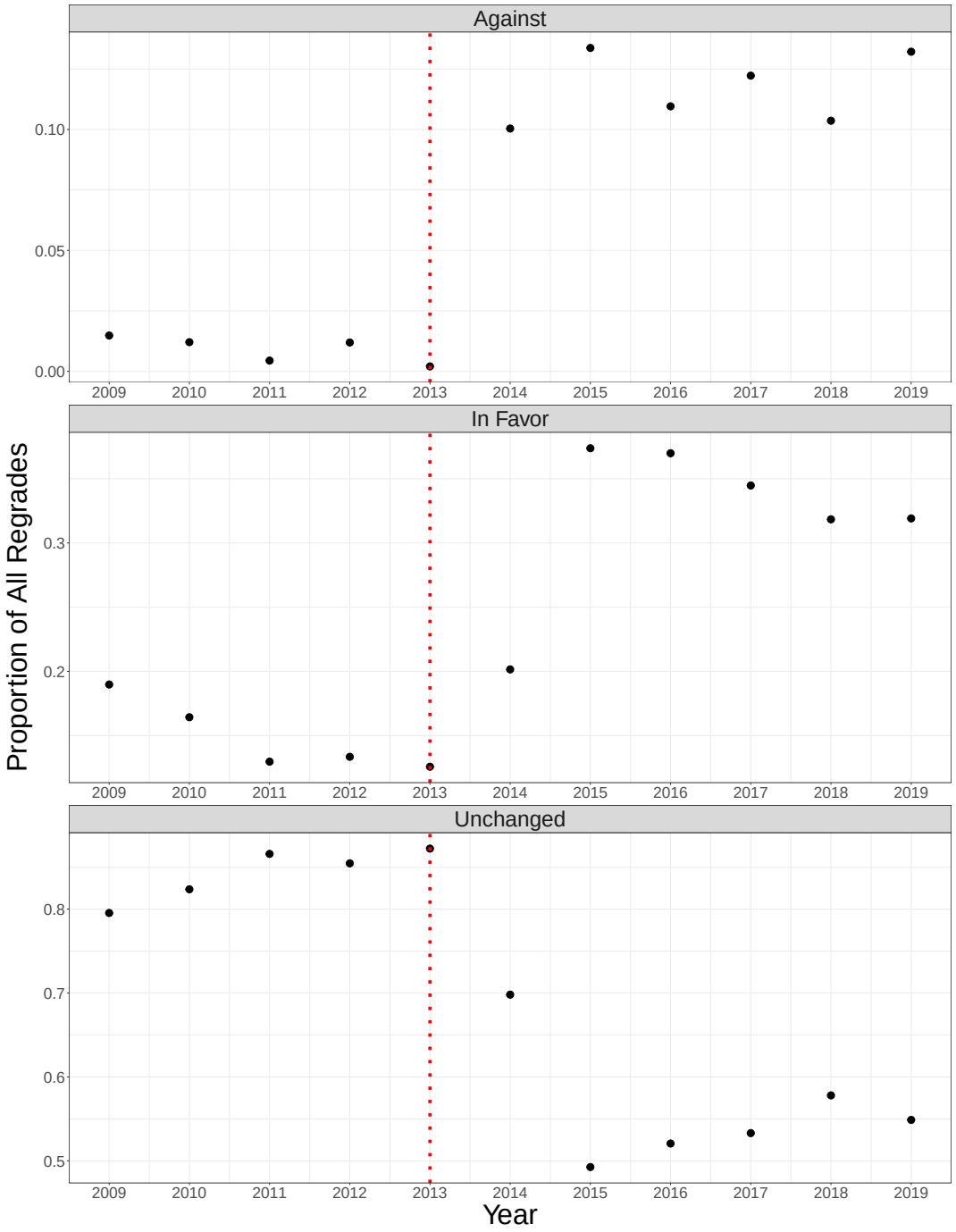
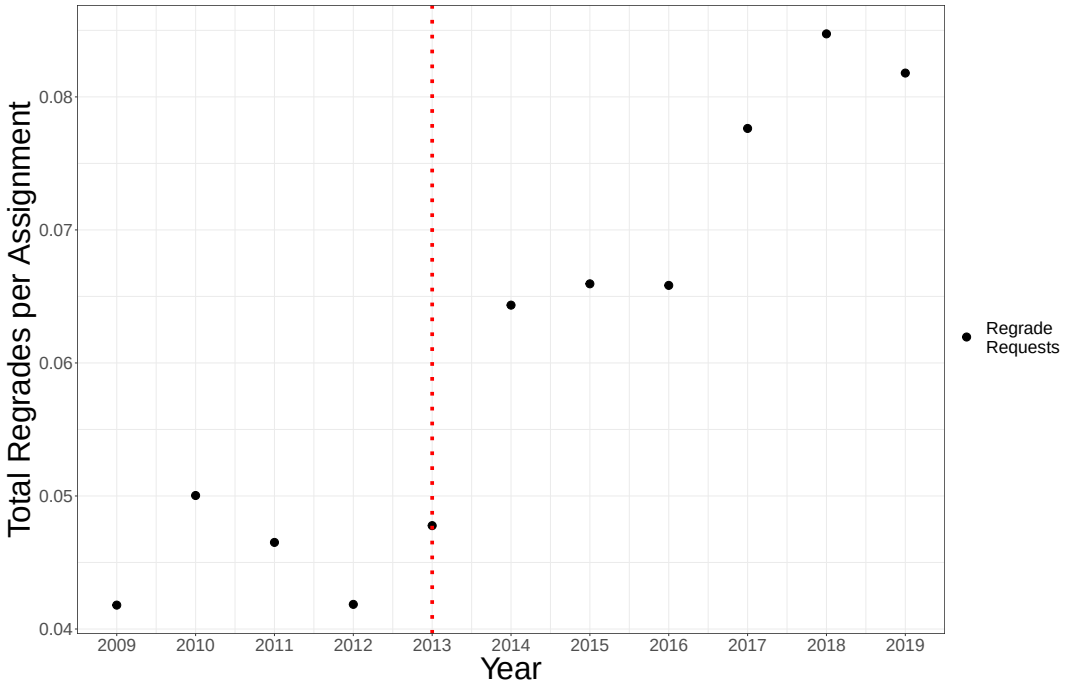
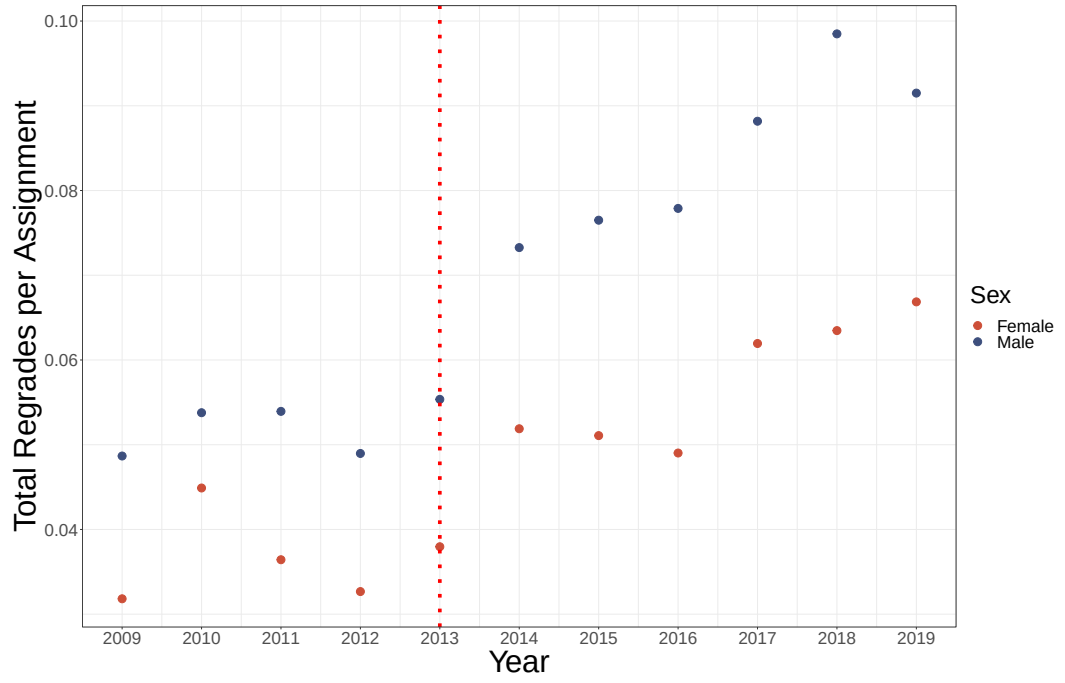



Figure one shows the proportion of grades submitted for regrading that are decreased, increased, and left unchanged over the period 2009-2019. The red dotted line denotes 2013, the last full year before the new regrade policy went into effect.

Figure 2: The Proportion of All Grades Submitted for a Regrade



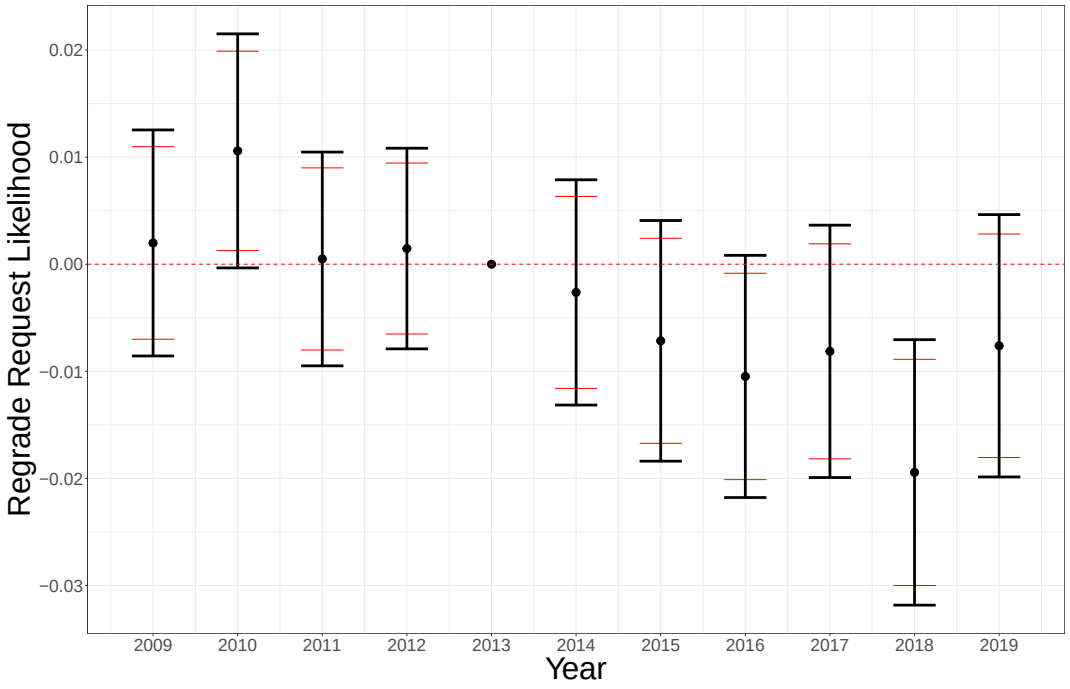

(a) Year



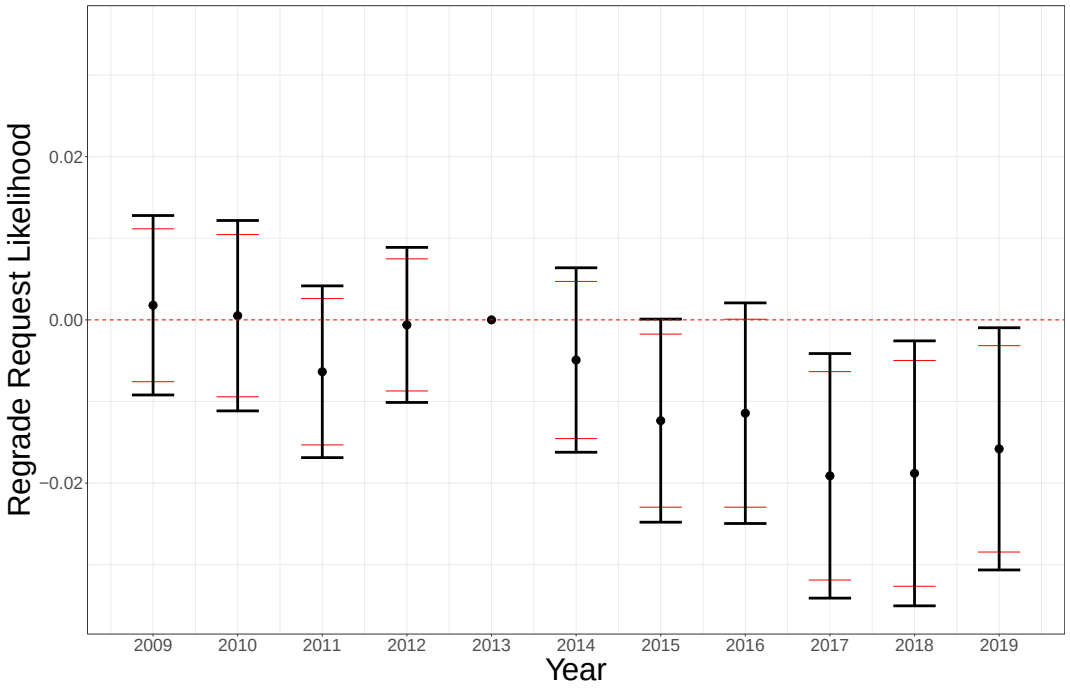

(b) By Year and Gender

Figure two shows the proportion of all grades submitted for regrade requests plotted by year in panel a and by year and sex in panel b.The red dotted line denotes 2013, the last full year before the new regrade policy went into effect. In panel b, the blue dots show the proportion of assignements by men submitted for regrades and the orange dots show the proportion of assignments by women submitted for regrades.

Figure 3: Event Study Plot for the Interaction between Year and Female



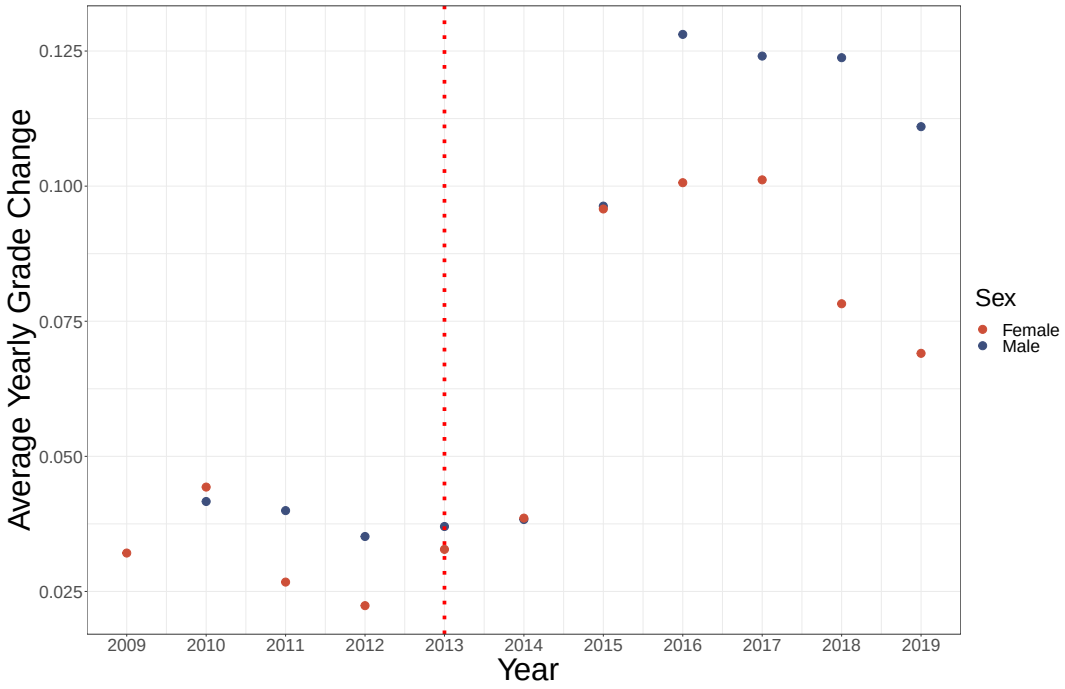

(a) All Assignments



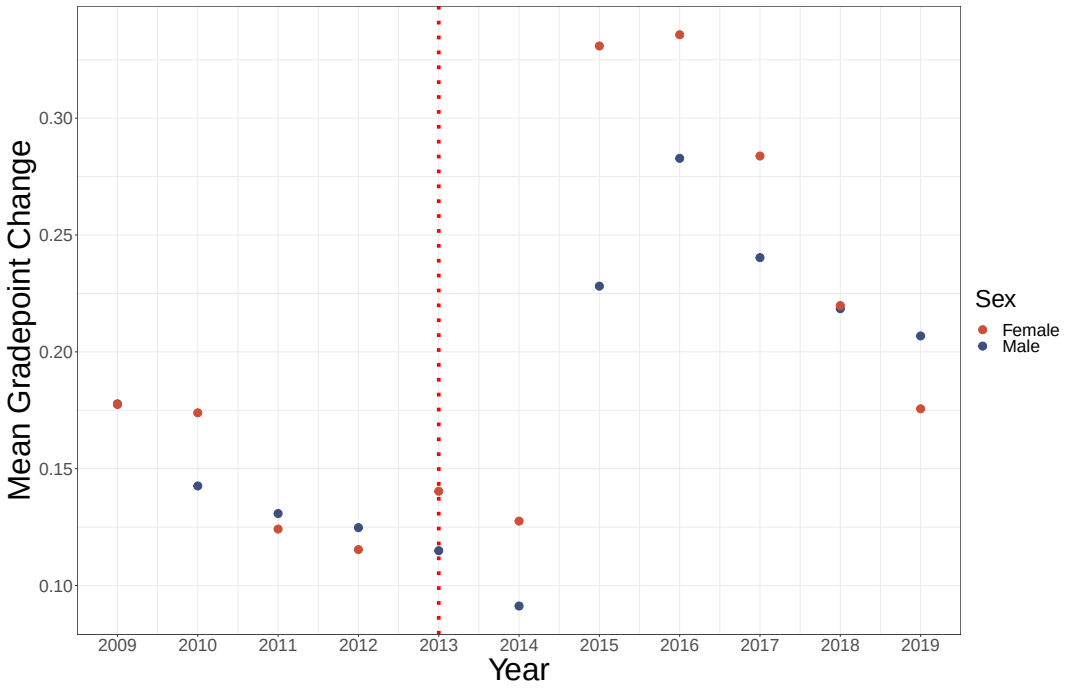

(b) Written Exams Only

Figure three shows the interaction coefficients $\delta_t$ for all exams in panel a and written exams in panel b. The red dotted line denotes the 0 point, which can be interpreted as the gender gap in 2013, the year before the policy was implemented. The black error bars denote 95% confidence intervals while the red error bars denote 90% confidence intervals. Standard errors are clustered at the student level.

Figure 4: Average grade points per year and average grade points per assignment awarded to men and women




(a) Total Gradepoints Per Year Over Time




(b) Average Gradepoints per Submission Over Time

Figure four shows the returns to asking for a regrade split by sex. Panel a shows at the average gradepoints changed in each year by men and women over time. Panel b shows the average return to a regrade submission in each year split by men and women.

Table 1: Effect of policy on regrade request probability using simple difference in differences estimates.

(a) DiD estimates for All Assignment Types

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| Intercept | 0.058*** | | | |
| | 0.001 | | | |
| Female | −0.017*** | −0.017*** | −0.016*** | −0.016*** |
| | 0.002 | 0.002 | 0.002 | 0.002 |
| Post Blinding | 0.034*** | | | |
| | 0.002 | | | |
| Female x Post | −0.012*** | −0.012*** | −0.011*** | −0.011*** |
| | 0.003 | 0.003 | 0.003 | 0.003 |
| Observations | 199 253 | 199 253 | 199 253 | 196 946 |
| FE: Year | | X | X | X |
| FE: Class | | | X | X |
| FE: Original Grade | | | X | X |
| FE: Retake | | | | X |

Note: + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

(b) DiD estimates for Written Exams

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| Intercept | 0.047*** | | | |
| | 0.002 | | | |
| Female | −0.015*** | −0.015*** | −0.014*** | −0.014*** |
| | 0.002 | 0.002 | 0.002 | 0.002 |
| Post Blinding | 0.042*** | | | |
| | 0.003 | | | |
| Female x Post | −0.013*** | −0.012*** | −0.012** | −0.011** |
| | 0.003 | 0.003 | 0.004 | 0.004 |
| Observations | 132 733 | 132 733 | 132 733 | 131 027 |
| FE: Year | | X | X | X |
| FE: Class | | | X | X |
| FE: Original Grade | | | X | X |
| FE: Retake | | | | X |

Note: + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table one shows the simple difference in difference estimates of the likelihood of submitting a regrade request. Panel A presents the estimates for all assignment types. Panel B presents the estimates for just written exams. Column one shows the model without fixed effects, column two includes year fixed effects, column three includes year, class identifier, and original grade fixed effects, and column four includes year, class identifier, original grade, and a fixed effect for whether the exam was the student's second time taking an exam in that class code. Standard errors are presented below the estimates and are clustered at the student level.

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# 7 Appendix

Figure A1: Simple Difference in Differences Interaction Coefficient for Different Sample Restrictions

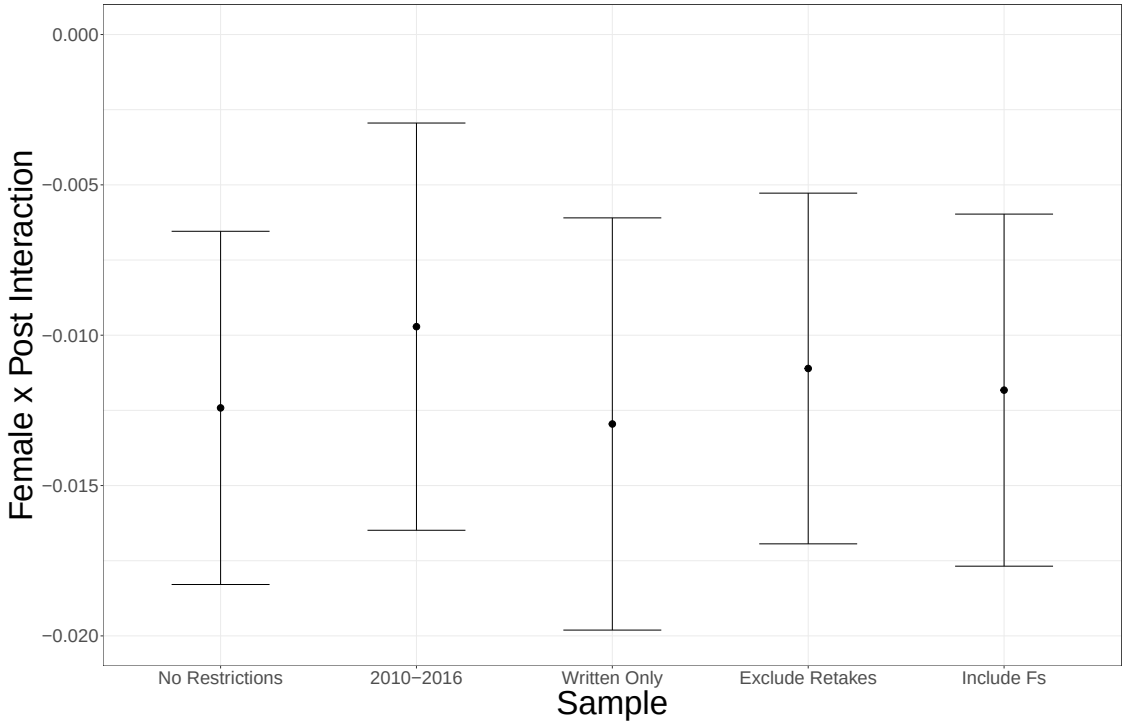


Figure A1 plots the simple difference in differences interaction coefficient for a variety of sample restrictions. The first point is the no restriciton sample, the second point restricts to years closer to the policy implementation, the third includes only written exams, the fourth excludes any retakes, and the fifth includes original grades of F. The size of the interaction coefficient is similar across samples. Error bars represent 95% confidence intervals with standard errors clustered at the student level.

Figure A2: Average Grade Submitted for a Regrade by Men and Women over Time

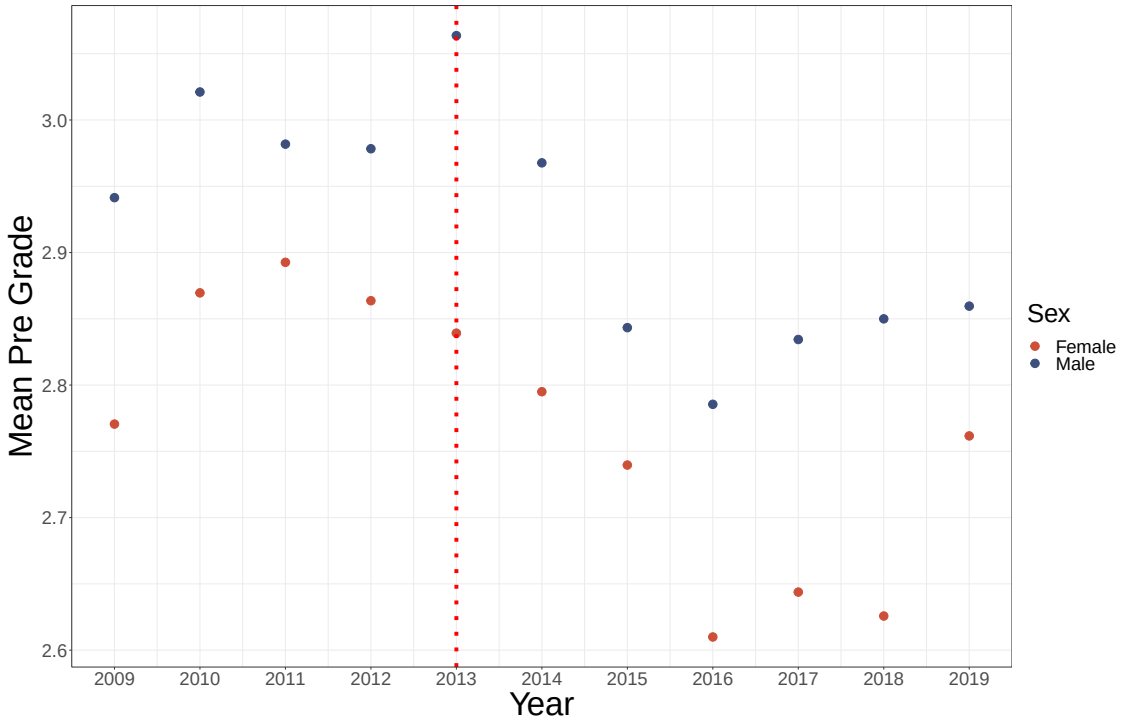


Figure A2 shows the average grade submitted for a regrade by men and women. The blue dots represent the average for men and the orange dots represent the average for women. The dotted red line shows the last year before the policy was implemented. The average grade submitted for a regrade goes down after the policy goes into effect ($\beta_{post} = -.15$, $p < .001$) but the gender gap in grades submitted does not substantially expand in the post period ($\beta_{post*female} = -.01$, $p = .77$).

Figure A3: Number of Requests For Men and Women Split by Original Assigned Grade

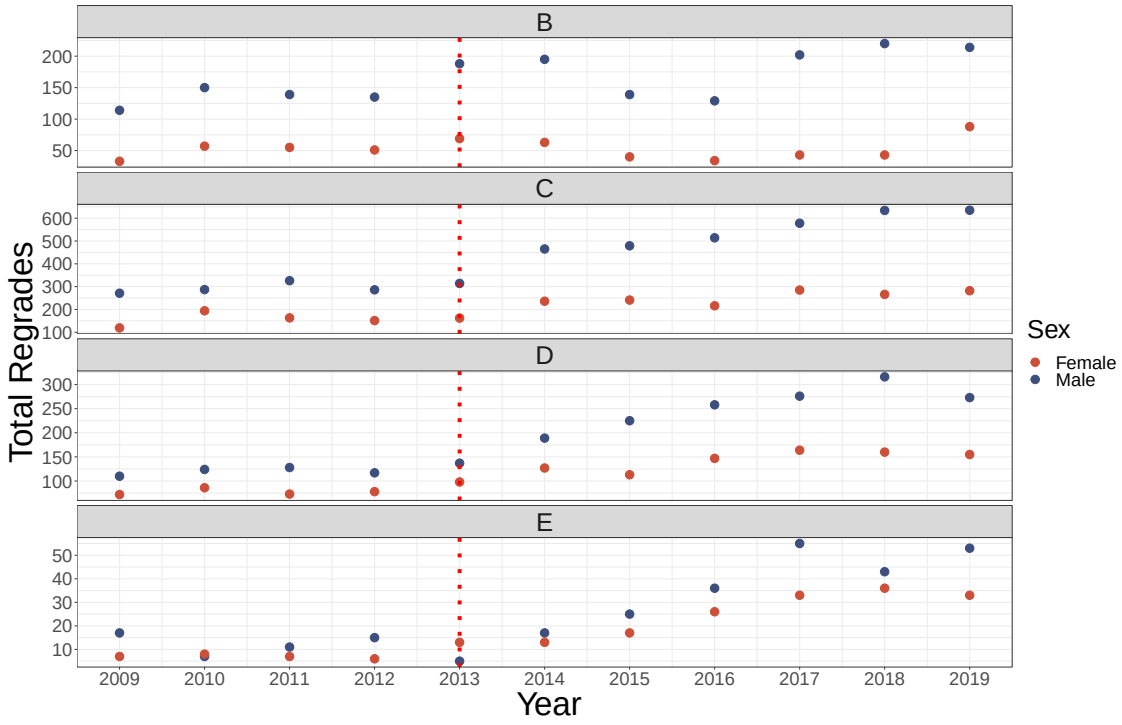


Figure A3 shows the overall number of grades submitted for regrade split out by original grade from B to E. The blue dots represent men and the red dots represent women. Across the original grade distribution we observe widening gender gaps, though the pattern is somewhat less clear for Bs and there are relatively few Es overall.

Figure A4: Representative information received by the original grader when a justification was requested by a student

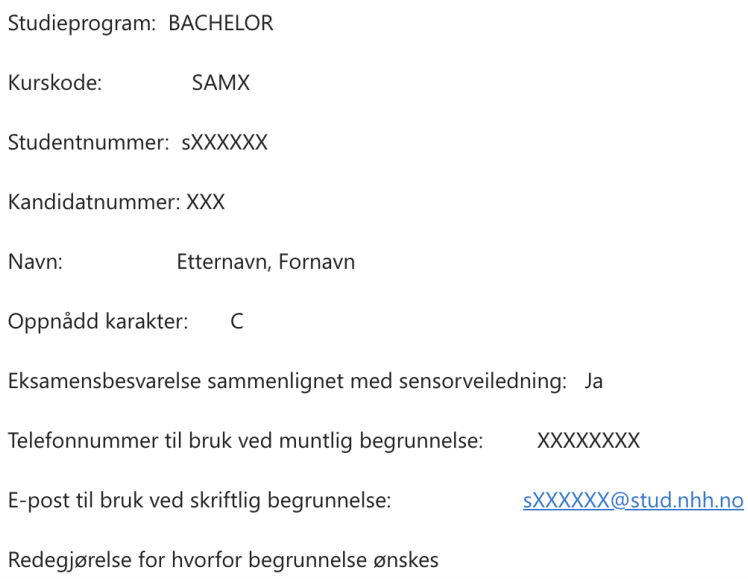


Figure A4 shows a representative email received by the original grader when a student requests a regrade. The information included is the program of study, bachelors or masters, the course code, two student identity numbers (one general use and one specific to the exam), the student's name, the original grade, and information to contact the student by phone or email to transmit the grade justificaiton.

Table A1: Extensive and Intensive Margin Responses to the Policy Change by Gender

| | Ever Requested | Total Requests Conditional on Ever Requesting |
|---|---|---|
| Intercept | 0.497*** | 1.475*** |
| | 0.008 | 0.033 |
| Female | −0.075*** | −0.161** |
| | 0.012 | 0.053 |
| Post | 0.097*** | 0.710*** |
| | 0.009 | 0.054 |
| Female x Post | −0.035** | −0.185* |
| | 0.013 | 0.082 |
| Observations | 14 601 | 7473 |

Note: + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A1 shows the intensive and extensive margin responses difference in difference estimates. Column one shows the estimates for the likelihood a student ever requests a regrade, the extensive margin. Column two shows the estimates for the number of requests conditional on ever requesting. Standard errors are presented below the estimates and are clustered at the student level.

Table A2: Change in Grade Points And Returns to Regrades

| | Grade Point Change Per Year | Count of Up Changes | Count of Down Changes | Returns Per Request |
|---|---|---|---|---|
| Intercept | 0.040*** | 0.042*** | 0.002*** | 0.137*** |
| | 0.002 | 0.002 | 0.000 | 0.007 |
| Female | −0.009** | −0.010** | −0.001 | 0.009 |
| | 0.003 | 0.003 | 0.001 | 0.012 |
| Post Blinding | 0.064*** | 0.113*** | 0.057*** | 0.077*** |
| | 0.005 | 0.005 | 0.002 | 0.011 |
| Female x Post | −0.015* | −0.036*** | −0.022*** | 0.019 |
| | 0.008 | 0.007 | 0.003 | 0.020 |
| Observations | 37 894 | 37 894 | 37 894 | 13 414 |

Note: + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A2 shows the difference in differences estimates on the returns to regrading. Column one presents the average change in grade points per year. Column two presents the count of changes that go in favor of the student per year. Column three presents the count of changes that go against the student per year. Column four shows the average return per regrade request. Standard errors are presented below the estimates and are clustered at the student level.

Table A3: Likelihood of Submitting a Subsequent Regrade Conditional on First Regrade Result by Gender

| | All | Post | Written | Written Post |
|---|---|---|---|---|
| Intercept | 0.093*** | 0.101*** | 0.090*** | 0.105*** |
| | 0.003 | 0.003 | 0.003 | 0.005 |
| Female | −0.022*** | −0.026*** | −0.021*** | −0.024*** |
| | 0.004 | 0.005 | 0.005 | 0.007 |
| In Favor | 0.020*** | 0.016** | 0.032*** | 0.030*** |
| | 0.005 | 0.006 | 0.007 | 0.009 |
| Against | −0.027** | −0.033** | −0.005 | −0.017 |
| | 0.010 | 0.011 | 0.013 | 0.014 |
| Female x In Favor | 0.000 | 0.001 | 0.000 | −0.003 |
| | 0.007 | 0.009 | 0.010 | 0.012 |
| Female x Against | 0.016 | 0.019 | 0.001 | 0.005 |
| | 0.014 | 0.015 | 0.020 | 0.022 |
| Observations | 60 170 | 42 741 | 36 673 | 23 843 |

Note: + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A3 shows the impact of a student's first regrade outcome on their likelihood of submitting for regrades in the future. We estimate

$$Y_i = \alpha_0 + \beta_1 Female + \beta_2 InFavor + \beta_3 Against + \beta_4 (Female * InFavor) + \beta_5 (Female * Against) + \epsilon_i$$

We consider this outcome a proxy for belief updating about the likelihood of future success. Column one presents the estimates for all regrades submitted, column two presents the estimates for just post period regrades, column three presents the estimates for all written exams, and column for presents the estimates for written exams in the post period. Because of the low number of grades that are changed downward in the period before the policy change, we do not use the difference in differences specification here. Standard errors are presented below the estimates.

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**10/22** July, **Alexander W. Cappelen**, **Ranveig Falch**, **Zhongjing Huang** and **Bertil Tungodden**. "Acceptance of inequality between children: Large-Scale Experimental Evidence from China and Norway"

**11/22** August, **Øivind A. Nilsen** and Arvid Raknerud. "Dynamics of First-Time Patenting Firms"

**12/22** September, Pedro Carneiro, **Kjell G. Salvanes**, Barton Willage, and **Alexander Willén**. "The Timing of Parental Job Displacement, Child Development and Family Adjustment"

**13/22** October, Gozde Corekcioglu, Marco Francesconi and **Astrid Kunze**. "Expansions in Paid Parental Leave and Mothers' Economic Progress"

**14/22** October, Ryan McWay, **Pallavi Prabhakar** and Ayo Ellis. "The Impact of Early Childhood Development Interventions on Children's Health in Developing Countries: A Systematic Review and Meta-analysis"

**15/22** October, Lars Ivar Oppedal Berge, **Kjetil Bjorvatn**, Fortunata Makene, Linda Helgesson Sekei, **Vincent Somville** and **Bertil Tungodden**. "On the Doorstep of Adulthood: Empowering Economic and Fertility Choices of Young Women"

**16/22** October, **Astrid Kunze** and Katrin Scharfenkamp. "Gender diversity, labour in the boardroom and gender quotas"

**17/22** November, **Samuel Dodini**, **Katrine Løken**, and **Alexander Willén**. "The Effect of Labor Market Competition on Firms, Workers, and Communities"

**18/22** November, **Yuzuru Kumon** and Kazuho Sakai. "Women's Wages and Empowerment: Pre-industrial Japan, 1600-1890"

**19/22** December, **Alexander Willén**, Barton Willage, and Julie Riise. "Employment Protection and Child Development"

**20/22** December, **Samuel D. Hirshman** and **Alexander Willén**. "Does Increasing Risk Widen Gender Gaps?"

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