Valuing Discretion over Class Attendance at University

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Abstract

We use exogenous variation in the *threat* of forced attendance to measure the value of discretion over class attendance at university. Our variation comes from a large university where students were forced to attend tutorials for all their second-year courses if their first-year GPA was less than 7 (out of 10), where all students are allowed to retake their final exams in the summer, and where at the time of the retake below-7 students were provisionally and exogenously assigned to forced attendance. This provisional assignment - the threat of forced attendance - increases the retake propensity by 7 percentage points, a more than 100 percent increase over the baseline rate of 6.3 percent. We show that the marginal value of discretion over class attendance is roughly equivalent to the marginal value of a one standard deviation increase in the grade for a course. Females value discretion substantially more than men. We explore whether discretion is valued inherently or because of the instrumental benefits it delivers.

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Decision rights over everyday decisions is a defining characteristic of higher education. Students are allowed to come and go as they please. Whether, when, where, and how they learn is up to them. Practitioners and researchers have called this centuries-old practice into question, arguing for more structured or mandated learning, particularly if students have behavioral predispositions for decisions that are costly from the perspective of lifetime utility (Lavecchia, Liu, and Oreopoulos, 2014). Further to this point, if students have these behavioral predispositions and they are aware of this, then these predispositions can generate demand for structured learning. Conversely, structured or mandated learning constrains the choices of students and, while it may be good for grades, it will reduce utility, especially over the short term. In the case of students being forced to attend class, it constrains their time for other academic activities such as self study, in addition to activities such as leisure. These "constraint effects" can decrease demand for structured learning, in addition to undermining the "spirit" of university. Whether there is demand for structured learning, and whether and to what it extent it generates value for students more generally, is ultimately an empirical question.

This paper uses exogenous variation in the *threat* of forced attendance to measure the value students attach to discretion over class attendance. Our variation comes from a natural experiment at a large university where, from 2009 to 2013, the university instituted a policy that forced students to attend 70 percent (up to 250 additional hours of travel and class time) of all second-year tutorials if their first-year GPA was below 7 on a 10-point scale. Students are forced in the sense that they are constrained in a choice, their attendance. Their attendance choice is constraint as the penalty for noncompliance was severe. Violations of the attendance requirement meant that students had to wait a full year before they could take the (required) course again.[1]

This variation becomes useful when coupled with the university policy on final exams. All students are allowed to retake their finals in the summer before second year. During this period, below-7 students are provisionally assigned to forced attendance. They, in other words, face the threat or prospect of forced attendance during the retake period. Accordingly, they can try to exploit the retakes to avoid forced attendance. We show that their differential propensity to retake (relative to above-7 students) under the provisional assignment to forced attendance - the threat of forced attendance - facilitates measurement of the value students attach to decision rights over class attendance.

[1] Consistent with students being forced, our data reveals that the policy leaves no room for noncompliance. In less than 0.50 percent of the students below 7 we observe an attendance rate below 70 percent.

Empirically we start out by estimating a reduced-form regression-discontinuity model and show that the threat of forced attendance increases the retake propensity by 7 percentage points, a more than 100 percent increase over the baseline mean of 6.5 percent. We put our estimate through a battery of tests. We evaluate the impact at GPA cutoffs of 6 and 8 and show that there is no differential retake propensity of students above or below these integers. We evaluate the impact in the year before the introduction of forced attendance and in the year after the policy was abolished and show that there is no differential retake propensity for below-7 students. Moreover, we show our estimate of 7 percentage points is effectively unchanged if (i) we include a host of control variables, (ii) we estimate our model using different bandwidths, and (iii) we use different order of polynomials for first-year GPA as running variable.

Comparing all students left and right of 7 on pre-retake GPA reveals that below-7 students marginally improve their post-retake GPA by 0.04 points (from 6.98 to 7.02). We show that, however, there is no post-retake GPA improvement of below-7 students if we restrict the sample to students who actually took a resit. That is, below-7 students do not improve their GPA because they do better on the resits, but because they take more resits. Although this differential impact might be partially driven by sample selection, it strongly suggests that the margin that is being affected by the threat of forced attendance is the decision to take a resit - the extensive margin - rather than the decision how much to study conditional on writing the resit - the intensive margin.

Having established the marginal effect on the retake probability, we turn to estimating the impact of the threat of forced attendance on the relative latent utility from retaking finals - the extensive margin. In addition to measuring the impact on latent utility, this exercise lets us value decision rights in terms of more-easily quantifiable measures of value. In particular we show that the effect of the threat on the relative value from retaking is equal to a one standard deviation increase in the grade on the initial final exam. We show further that the effect of threat is three to four times the effect of measures of the psychic cost of the retake and almost double the effect of the recall cost of the retake exam. We also use the cutoff for the *cum laude* designation, a grade of 8.25, to show that the effect of threat is many times more valuable than finishing the first year with the *cum laude* designation.[2] Taken together, these results imply that decision rights are of substantial value to the average student.

We draw on our companion paper (Kapoor, Oosterveen, and Webbink, 2017), which

[2]The university hosts a special ceremony, where it hands out certificates, for students who finished the first year with the *cum laude* designation.

uses an identification strategy that is inclusive of the post-retake grade, to explain that forced attendance decreases second-year grades by 0.35 standard deviations. These results imply that decision rights are valuable in part because they are an instrument for academic performance, at least for the average student.[3][4]

This article speaks to a large education literature that focuses on the impact of class attendance on academic performance at university (Romer, 1993).[5] To our knowledge, this is the first paper to estimate the value of discretion over class attendance and, consequently, the first to estimate the value via exogenous variation. More generally, it is the first to use exogenous variation in the *threat* of losing a thing to value that thing.[6]

The article speaks to a small education literature that examines the causes and consequences of retake decisions. (Krishna, Lychagin, and Frisancho, 2017) use a dynamic structural model to investigate the consequences of limiting or eliminating retakes for nationwide university entrance exams, while accounting for the equilibrium implications of retakes on the acceptance cutoffs of universities. (Vigdor and Clotfetter, 2003) focus on the partial equilibrium consequences of college admissions policies on decisions to retake the SAT in the United States. (Törnkvist and Henriksson, 2004) study demographic differences in the propensity to retake the Swedish SAT. Our interest is not in the retakes in and of themselves, but instead in using the retakes as tool for valuing discretion over attendance.

The article speaks to an emerging labor literature that attempts to value flexible work arrangements (Chen, Chevalier, Rossi, and Oehlsen, 2017; Mas and Pallais, 2017).[7] From

[3]It is doubtful that first-year students knew precisely what impact forced attendance would have on their second-year grades. However, because attendance in the first year is also forced, they should have a good understanding of what the impact might be.

[4]The results here lend further support to the identification argument in that paper. That paper relies on "local randomization" of GPA inclusive of retake grades. This paper shows that while the threat of forced attendance increases the propensity to retake finals, the retake ultimately has no to a negligible impact on the first-year GPAs. This result, together with the stable estimates for the donut-hole RD estimates in our companion paper, reinforce the assumption that drives identification, namely that students have imprecisely control over their final first-year GPA. Further to this point, in the first of the policy (2009-2010), students were not well aware of the policy. Consistent with this, we do not find differential retake propensity near 7 for this cohort during their first year. We do, however, find that forced attendance decreases academic performance for this cohort as well, further suggesting the validity of the regression discontinuity design in our companion paper.

[5]Much of the work in this area shows either negligible or positive correlations between attendance and performance, see (Romer, 1993), (Durden and Ellis, 1995), (Kirby and McElroy, 2003), (Stanca, 2006), (Rodgers, 2002), (Lin and Chen, 2006), (Marburger, 2006), (Martins and Walker, 2006), (Chen and Lin, 2008), (Arulampalam, Naylor, and Smith, 2012), (Latif and Miles, 2013), and (Snyder et al., 2014).

[6]Our use of the threat of forced attendance was inspired by Goolsbee and Syverson (2008), who study the responses of incumbent firms to the threat of entry.

[7](Chen et al., 2017) use a structural model and wage data from Uber to assess the value to flexible work arrangements.

this perspective, our article is most closely related to (Mas and Pallais, 2017), who, like us, rely on random variation to measure the value of flexible work arrangements. They measure the value that stems from the prospect of having the right to decide when and where to work. Our article measures the value that stems from the prospect of having these rights taken away. This is noteworthy in part because studying the natural counterpoint to flexible work arrangements, namely forced work arrangements, via randomization would be difficult, if not impossible, because of ethical considerations. That is, our article offers a rare glimpse into the value people attach to *not* having their decision rights taken away.

Mas and Pallais (2017) and Chen et al. (2017) focus on workers, whereas this article focuses on students. One of the key differences pertains to the nature of the potential constraint (or lack thereof) on choices. Forced attendance is a constraint on how, as well as where and when students study. Flexible work arrangements offer workers flexibility in where and when they work, and for the jobs that have been studied so far, to a lesser extent on how they work. Put another way, students have greater autonomy or discretion over the production function they use. From this perspective, and given that the context is a prominent Dutch University, one could argue that our findings have greater pertinence for high-end jobs.[8]

Our focus on the prospect of losing decision rights, as well as on the impact on how one works, might explain the sizeable difference between our average-value estimates. (Mas and Pallais, 2017) find that the average worker does not value flexible work arrangements, at least in terms of prospective wages. We find that the average student attaches substantial value to decision rights over attendance, it is equivalent to the marginal value of a one standard deviation increase in the grade for a course. Where our conclusions align is with estimates of gender differences in the value of the decision rights. (Mas and Pallais, 2017) find that women value flexible work arrangements more than men. We find that women value decision rights over class attendance substantially more than men.[9]

The article speaks to a developing and thought-provoking organizational economics literature on the value of decision rights (Bartling, Fehr, and Herz, 2014). Bartling, Fehr, and Herz (2014) develop a laboratory method that disentangles the intrinsic from the instrumen-

[8]With this caveat in mind, it is worth noting that it is common in the Netherlands to treat school, and in particular university, like work in the sense that students block a fixed number of hours for studying.

[9]In this regard, our article tangentially speaks to a literature that examines the causes and consequences of decision rights for women, often focusing on decisions that take place within the household and that can substantially affect the economic well-being of the family (Anderson and Eswaran, 2009; Duflo, 2003; Thomas, 1990, 1993). Our focus is not on the causes and consequences of decision rights for women, but instead on whether and to what extent they value such decision rights.

tal value of decision rights. Their method measures the compensation a principal requires in order to forgo their decision rights. Our study exploits the threat of losing decision rights, *i.e.* the threat of interference by a higher authority (the university) in payoff-relevant decisions, combined with the existence of an outlet for avoiding interference, to measure jointly the instrumental and intrinsic value of decision rights. To this end, the negative impact of forced attendance on second-year grades documented in our companion paper (Kapoor, Oosterveen, and Webbink, 2017) implies decision rights over class attendance are valuable, at least in part, because of the instrumental benefits from higher grades.[10]

1 Model

We specify an estimable model of the decision to retake a final exam.[11] The utility from retaking is generally

$$\alpha Eg - uc + A,$$

where Eg is the expected GPA following the retake, α converts expected GPA into utility, uc is the utility cost preparing for the retake, and A is the perceived ability in the retake exam. A student will generally retake a final if

$$\alpha Eg - uc + A \geq \alpha g,$$

where g is their original pre-retake GPA.

The retake utility for students who are provisionally assigned to forced attendance ($g < 7$ in our case) is, by contrast,

$$\pi_f EV(f) + \pi_d EV(d) + \alpha Eg - uc + A,$$

where $EV(f)$ and $EV(d)$ are the present expected values from the realized assignments to forced f and discretionary d attendance. The difference between these two outcomes reflects

[10]See (Neri and Rommeswinkel, 2017) for a lab experiment that identifies the sources of intrinsic or procedural value from decision rights. They find, interestingly, that people value decision rights because they dislike interference by others in their payoff-relevant decisions, rather than because they like having the freedom to choose for themselves.

[11]The model focuses on the decision to write a retake, and not on how much effort to put into the resit, as post-retake GPA is not affected differently for below- and above-7 students among the the students who actually took a resit. We will come back to this argument in Section **??**.

students' latent utility from having discretion over class attendance. $\pi_d = P(Eg \geq 7)$ is the probability that the student's post-retake GPA is sufficient for them to avoid forced attendance, hence we have that $\pi_f = 1 - P(Eg \geq 7)$. Provisionally forced students retake if

$$\pi_f EV(f) + \pi_d EV(d) + \alpha Eg - uc + A \geq EV(f) + \alpha g.$$

Thus, for threatened students, the retake decision generates a lottery over the expected discounted values from forced and discretionary attendance. The lottery (and post-retake GPA) is resolved a couple of weeks after the retake, when grades are distributed to students.

Let $\mathcal{D}$ denote an indicator function for the event $\{g : g < 7\}$. The net value $\mathcal{V}$ from the retake for a given student is

$$\mathcal{V} = \Big(\pi_d(EV(d) - EV(f)) + \alpha(Eg - g) - uc + A\Big)\mathcal{D} + \Big(\alpha(Eg - g) + A\Big)(1 - \mathcal{D})$$

or, equivalently,

$$\mathcal{V} = \alpha(Eg - g) - uc + \pi_d(EV(d) - EV(f))\mathcal{D} + A.$$

Our primary goal is to use information on retake decisions and grades to recover $EV(d) - EV(f)$, which measures the net expected value students derive from having discretion over their attendance in terms of latent utility. If students are optimizing their grades and utility, and choices are constrained by forced attendance, then $EV(d) - EV(f) > 0$. In what follows, we will discuss our identification in order to trace out this term.

1.1. Identification We take the specification for the latent net retake value to the data, where we assume $\mathcal{V}$ for student i of cohort c in course j is generated in accordance with

$$\mathcal{V}_{ijc} = \alpha Gain_{ic} - \mathbf{Z}_{ijc}\boldsymbol{uc} + \pi_{dic}\beta\mathcal{D}_{ic} + A_{ic} + u_{ijc}, \tag{1}$$

where $Gain_{ic} = Eg_{ic} - g_{ic}$, $\mathbf{Z}_{ijc}$ is a row vector of cost shifters, and $\boldsymbol{uc}$ is a column vector of parameters. We assume that A_{ic} is drawn from a normal distribution with a mean of 0 and variance of σ_A^2. u_{ijc} is also assumed to be normal with mean of 0, but with a variance of σ^2. Our main estimate of interest is $\beta = EV(d) - EV(f)$.

Let $R_{ijc} = \mathbb{1}\{\mathcal{V}_{ijc} > 0\}$ indicate the decision to retake, $\mathbf{R}_{ic}$ denote the vector of retake decisions for student i, $\Theta = (\alpha, \boldsymbol{uc})$, $\mathbf{X}_{ijc} = (Gain_{ic}, \mathbf{Z}_{ijc})$, and $f_{\mathbf{R}}$, f_A, and f_R denote the logged probability density functions for $\mathbf{R}_{ic}$, A_{ic}, and R_{ijc}. We will assume further that:

1. The retake decisions of one student $\mathbf{R}_{ic}$ are statistically independent of the retake decisions of any other student $\mathbf{R}_{-ic}$, conditional on $\pi_{0ic}\mathcal{D}_{ic}$, $\mathbf{X}_{ijc}$.

2. The student's retake decision for one course R_{ijc} is statistically independent of their retake decisions for any other course R_{i-jc}, conditional on $\pi_{0ic}\mathcal{D}_{ic}$, $\mathbf{X}_{ijc}$, and A_{ic}.

3. The unobserved attributes of the student A_{ic} are statistically independent of $\pi_{0ic}\mathcal{D}_{ic}$ and $\mathbf{X}_{ijc}$.

The first assumption implies a log-likelihood of

$$\ell(\beta, \Theta) = \sum_{i=1}^{N} f_{\mathbf{R}}(\mathbf{R}_{ic}|\pi_{0ic}\mathcal{D}_{ic}, \mathbf{X}_{ijc}; \beta, \Theta).$$

The second assumption implies ℓ can be rewritten into

$$\ell(\beta, \Theta) = \sum_{i=1}^{N} \int_{-\infty}^{\infty} \left[\prod_{j=1}^{10} f_{R_{ijc}}(R_{ijc}|\pi_{0ic}\mathcal{D}_{ic}, \mathbf{X}_{ijc}, A_{ic}; \beta, \Theta)\right] f_A(A_{ic}|\pi_{0ic}\mathcal{D}_{ic}, \mathbf{X}_{ijc}) dA_{ic},$$

The third assumption implies

$$\ell(\beta, \Theta) = \sum_{i=1}^{N} \int_{-\infty}^{\infty} \left[\prod_{j=1}^{10} f_{R_{ijc}}(R_{ijc}|\pi_{0ic}\mathcal{D}_{ic}, \mathbf{X}_{ijc}, A_{ic}; \beta, \Theta)\right] f_A(A_{ic}) dA_{ic},$$

The utility cost from the retake is given by

$$\begin{aligned} uc_{ijc} = uc_g g_{ijc} + \sum_{t(j)=1}^{5} uc_{t(j)} Block_{t(j)} + uc_d(Distance\ from\ a\ Major\ City)_{ic} \\ + uc_s(1st\ Year\ Tutorial\ Slack)_{ic} + uc_a(1st\ Year\ Attendance)_{ic} \\ + \sum_{c(i)=2009}^{2013} uc_{c(i)}(Start\ Year)_{c(i)}, \end{aligned}$$

where

- g_{ijc} is the initial grade in course j. All else equal, we expect students with higher grades to retake less often, suggesting that they have a higher cost or lower benefit from the retake.

- $Block_{t(j)}$ indicates whether course j took place in block $t(j)$. It reflects differences in the cost of recall, which presumably is larger for earlier courses, and which should affect the choice of which exam to retake.
- $(Distance\ from\ a\ Major\ City)_{ic}$ is the minimum distance (km) from the student's home to one of the four big cities in the Netherlands (Amsterdam, the Hague, Rotterdam, Utrecht). It is a correlate of the outside option for the student.
- $(1st\ Year\ Tutorial\ Slack)_{ic}$ is the number of tutorials that were remaining when the 70 percent attendance requirement was met. Students with more slack would still have been able to meet the attendance requirement had something unexpected happened. In this way, it is a correlate of the preference for option values.
- $(1st\ Year\ Attendance)_{ic}$ is the percentage of tutorials attended in first year. The variation over and above 70 percent is a correlate of how much the student likes tutorials.
- $(Start\ Year)_{c(i)}$ indicates the start year or cohort of the student. It correlates with a cohort-specific preference for retaking finals or with other cohort-specific factors, such as the weather in the period between the initial and retake final exams.

$\mathbf{x}_{ic}$ is a row vector of personal characteristics. It accounts for part of the unobservable variation in utility costs and students' prior beliefs about their post-retake grades. $h(g_{ic}-7;\theta)$ is a polynomial in $g_{ic}-7$ with coefficients θ. It accounts for potential nonlinearities in a neighbourhood of 7. It also encapsulates spillover effects of grades in courses other than j.

None of these assumptions are trivial. A violation of the first assumption can arise if students have private information concerning the retakes and share this information with their colleagues. The dummy variables for the block and start year account for at least some of this interdependence. The dummy variable for each block encapsulates variation from two courses, effectively soaking up much of the course-specific variation that pertains to the retake decision. The dummy variable for the start year covers the possibility that students in the same cohort have a greater propensity to communicate information concerning retake exams. In these regards our specification stops a bit short of including course-cohort dummies, which are a better option for dealing with interdependencies of this sort, but which add a lot to the computational burden of our estimates.

A violation of the second assumption can arise if the decision problem of the student is a simultaneous one that takes into account their performance in all their courses. Our specification implicitly accounts for some of this sort of interdependence. The polynomial from our RD specifications, $f(g_{ic}^{(5)}-7)$, depends on a weighted average of the students original grades in all 10 courses. This accounts for spillovers from the grades in course on the retake decision on another course, but restricts these other courses to have the same effect (after the grade is weighted by the number of course credits). We can account for other types of spillovers such as your propensity to attend the tutorials in other classes. We might do this later on as a robustness check.

The third assumption is violated if unobserved ability, for example, correlates with $\mathbf{X}_{ijc}$ or the assignment into forced attendance. The RDD nature of our design helps with this assumption. By restricting the bandwidth we are increasing the comparability of students. We are making it so that $f_A(A_{ic}|\pi_{0ic}\mathcal{D}_{ic}) = f_A(A_{ic})$. This is the real beauty of mixing RDD with a structural model of retake choice.

Note that we did not mention $\mathbf{X}_{ijc}$ in our last discussion. We have good reason for this. We don't actually need to identify the value to forced attendance. It is there for assessing the relative value of forced attendance.

Note that $f_{R_{ijc}}(R_{ijc}|\pi_{0ic}\mathcal{D}_{ic}, \mathbf{X}_{ijc}, A_{ic}; \beta, \Theta)$ has a conditional mean of $\alpha Gain_{ic} - uc_{ijc} + \beta\pi_{0ic}\mathcal{D}_{ic} + f(g_{ic}^{(5)} - 7; \theta) + A_{ic}$, a variance of σ^2, and a shape that is determined by our assumed distribution for u_{ijc}. We assume that $(\varepsilon_{ijcR}, \varepsilon_{ijc})$ follow a bivariate normal distribution, such that $u_{ijc} = \varepsilon_{ijcR} - \varepsilon_{ijc}$ is normal. We assume further that A_{ic} is also normally distributed, where $A_{ic} = \sigma_A a_{ic}$, and a_{ic} is drawn from a standard normal distribution. These distributional assumptions let us rewrite the log-likelihood

$$\ell(\beta, \Theta, \sigma_A) = \sum_{i=1}^{N} \int_{-\infty}^{\infty} \Big[\prod_{j=1}^{10} \Phi\big((2R_{ijc} - 1)(\beta\pi_{0ic}\mathcal{D}_{ic} + \mathbf{X}_{ijc}\Theta + \sigma_A a_{ic})\big) \Big] \phi(a_{ic}) da_{ic}, \quad (2)$$

where Φ and ϕ denote the cumulative distribution and probability density for a standard normal distribution. The parameters can thus be obtained up to a rescaling (by σ) via a standard random effects probit estimator.

Why do we need controls? Note that our empirical design implies[12]

$$\mathbb{E}[\mathcal{V}_{ijc}(\mathcal{D}_{ic})|g_{ic} = g, \mathbf{X}_{ijc}] = \mathbb{E}[\mathcal{V}_{ijc}(\mathcal{D}_{ic})|g_{ic} = g].$$

for realizations g near 7. As such,

$$\lim_{\Delta\to 0} \mathbb{E}[\mathcal{V}_{ijc}(1)|7 < g_{ijc} < 7 + \Delta, \mathbf{X}_{ijc}] - \lim_{\Delta\to 0} \mathbb{E}[\mathcal{V}_{ijc}(0)|7 - \Delta < g_{ijc} < 7, \mathbf{X}_{ijc}]$$

[12] We use $\mathbb{E}$ to denote the statistical expectation operator and to distinguish it from E, the subjective expectation operator of the student.

is the same as

$$\lim_{\Delta \to 0} \mathbb{E}[\mathcal{V}_{ijc}(1) | 7 < g_{ijc} < 7 + \Delta] - \lim_{\Delta \to 0} \mathbb{E}[\mathcal{V}_{ijc}(0) | 7 - \Delta < g_{ijc} < 7]$$

which equals the estimand of interest

$$\mathbb{E}[\mathcal{V}_{ijc}(1) - \mathcal{V}_{ijc}(0) | g_{ijc} = 7] \quad (ATE \text{ at } 7)$$

We will see shortly that our evidence indeed suggests that $\mathbf{X}_{ijc}$ is rather uninformative for the potential net valuations. This, of course, raises questions as to why $\mathbf{X}_{ijc}$ should be included in the estimation at all.

We include $\mathbf{X}_{ijc}$ because it facilitates assessments of the relative importance of decision rights. Since σ gets cancelled out of the ratio of any pair of coefficients, we have for any explanatory variable x_{ijc}

$$\frac{\Delta \mathcal{V}_{ijc} / \Delta \mathcal{D}_{ic}}{\Delta \mathcal{V}_{ijc} / \Delta x_{ijc}}.$$

The ratio thus couches decision-right valuations in terms of valuations for other explanatory variables, such as the marginal valuation of high grades after the initial final exam.

Students take less than 1 retake on average. We need to say something about the discrete choice problem.

2 Context

2.1. Institutional Details. Our setting is the undergraduate program of the school of economics at a large public university in the Netherlands. The school of economics is large, housing more than 100 economists, and 4 economics departments, each with a different focus: economics, econometrics, business economics, and applied economics. Each department can be viewed as an economics department in their own right.

Enrollment into the undergraduate program is about 800 students per year. Students declare their major before entering the university. Students take 10 courses in each of the first two years. The courses in the first two years are standard, covering microeconomics, macroeconomics, econometrics, as well as other less standard courses such as business economics.[13] Students have no discretion in course selection until the third and last year of the

[13]Students have the option to take the program in Dutch or English. The programs are identical apart

undergraduate degree. At this point they declare a major and minor specialization, which they can continue with through to a Masters program.

The academic year is divided into five blocks of eight weeks each (including one exam week). Students take two courses per block, one that counts for 8 credits, and another that counts for 4.[14] 8-credit courses have three large-scale lectures and two small-scale tutorials per week. 4-credit Courses have two large-scale lectures and one small-scale tutorials per week. Lectures and tutorials last for 1 hour and 45 minutes. Tutorials are more personalized as they normally require active participation on the part of the student, *e.g.* via discussions of assignments.

Grading is done on a scale that ranges from 1 to 10. Students fail a course if their grade is below 5.5. The average grade in the first year is weighted by the amount of credits the student gets for completing the course. In our full sample, the pre-resit first year grade has a mean and standard deviation of 7.23 and 0.70.

2.2. Forced Attendance Policy. We outline the essence of the forced attendance policy as it pertains to the value of decision rights. For a more detailed exposition of the policy, its motivation, and implementation, see our companion paper (Kapoor, Oosterveen, and Webbink, 2017).

Students are required to attend 70 percent of the tutorials for *all* 10 of their second-year if their first-year GPA was below 7, or if they failed at least one of the 10 first-year courses.[15] The students who had to comply with the policy are summarized in the table below.

Completed first year	GPA<7	GPA ≥ 7
Yes	Forced	Free
No	Forced	Forced

Students who failed to meet the attendance requirement were forbidden from writing the final exam and had to wait a full before being able to retake the course and rewrite the final.[16] A

from their size, as there are approximately 2.5 times more students in the Dutch program.

[14]Credits are defined in accordance with the European Credit Transfer System (ECTS). One ECTS is meant to be the equivalent of 28 hours of study. 60 ECTS designates a full year of study.

[15]Courses are put into three groups based on content. Within each of the three groups, the student can compensate for *one* failing grade between 4.5 and 5.4 with a passing grade from another course. As such, a student can pass all 10 courses and complete the first year, even though he got *e.g.* one insufficient grade of a 4.6.

[16]All courses in first and second year are required to complete the degree. A student who failed to meet

severe penalty for noncompliance together with the fact that attendance is normally a choice defines the way in which attendance is forced.[17]

Forced students spend 26 hours per block in tutorials. The average student spends approximately 45 minutes travelling to campus. Adjusting for travel time, they can expect to spend 50 additional hours per block traveling to and attending tutorials.[18] The heavy time cost constrains the time use of students, it unambiguously impacts when, where, and how (much) students study. Together with the severe penalty on noncompliance, the heavy time cost implies that there is value in avoiding forced attendance.

Students have extensive experience with forced attendance, as students are collectively assigned to forced attendance in first year.[19] They were also made well aware that there was a chance they would be assigned to forced attendance in second year as well. Each incoming student is assigned to a student adviser. The adviser explains the policy to students at the start of the year and reminds them of it in meetings that are mandatory and held throughout the first year.

2.3. Retakes. Students have several means for avoiding forced attendance in second year. They can work hard and perform well, beg professors and TAs for higher grades, or take a new version of the final exam in the summer between first and second year.[20] The exam grade of the student is the maximum of their grades on the original exam and the retake. The retakes are open to all students. Students who fail a course can use the retake to pass the course. Students with good grades can use the retake to improve on their original grade.

Students must register for retake exams. They can register for as many as they want. They can do no more than 3 retakes.[21] Registering for but not attending the retake exam has no consequences for the student. The retake exam can only replace the grade obtained on the final exam.[22]

the attendance requirement, and who wished to obtain the degree, would have no choice but to wait until the next academic year.

[17] We have other reasons for referring to the policy as "forced" attendance. First, students were, in effect, told that they had no choice but to meet the 70 percent attendance requirement. Second, there was barely any room to increase attendance further. Forced students collectively violated the policy in one half of one percent of all their courses.

[18] The money cost of tutorial attendance is small because student travel is fully subsidized in the Netherlands.

[19] All students have to attend 70 percent of the tutorials for all courses throughout the first year.

[20] Final exams are prepared by the instructors of the course. The original exam and the retake are both based on the same course material.

[21] If a student takes more than 3, only the first 3 will be counted.

[22] Grades from tutorials and assignments, that might count for a minor percentage into the final grade, are unchanged.

The retakes run from early to late July, 3 to 6 weeks after Block 5 ends. During this time, students with GPA below 7 after Block 5 are provisionally assigned to forced attendance. The provisional assignment to forced attendance - the threat of forced attendance - and the retake decision facilitate an assessment of the overall value (including costs) from holding decision rights over attendance. Intuitively, and as becomes clear from the model, students to the right of 7 balance the additional value of improvements in GPA against a sizeable cost in terms of lost vacation time. Students to left of 7 balance GPA improvements and the desire to avoid forced attendance against the same cost. As the first-year GPA approaches 7 from either directions, students become similar apart from the threat of forced attendance. It is in this way that the difference picks up the value from avoiding forced attendance.

2.4. The Treatment and Placebo Years The university introduced forced attendance for all first and second year students in 2007-08. It was introduced as part of an university-wide initiative that aimed to personalize the education of students. The key part of the initiative was the introduction of small-scale tutorials. The university introduced forced attendance as a part of the initiative because it wanted to ensure a sizeable return on the additional expenditure that went into the introduction and running of these tutorials. In addition, it was thought that, by bringing students to campus, it would foster community at the university.

The policy for the second year went through some minor changes throughout the years (see the table below). All first and second years were forced in the initial years of the policy. The university relaxed the policy in 2009-10, requiring second year students to attend 70 percent of all their tutorials only if their first-year GPA was below 7. In 2014-15 the university abolished forced attendance for second year students, at the request of students and faculty who lobbied against it. Second-year students were only made aware of the abolition at the beginning of the year, after the first-year resits. That is, at the time of the resit below-7 students were still provisionally assigned to forced attendance.

Our data runs from academic year 2007-08 until 2014-15. Note that second-year students in the academic year t were first-year students in year $t-1$, and we will analyze the resit period in year $t-1$ of provisionally forced students for year t. For example, we analyze the resit period for first-year students in 2014-15 when discussing the forced attendance policy in the academic year 2015-16.

The academic years 2010-11 until 2014-15 are indicated as treatment years. Although in 2014-15 the second year had no forced tutorial policy, the above makes clear the incentives

	Attendance Requirements	
Year	1^{st} Year	2^{nd} Year
2007-2008	70% of Tutorials	70% of Tutorials
2008-2009	70% of Tutorials	70% of Tutorials
2009-2010	70% of Tutorials	70%, if 1^{st}-Year GPA<7
2010-2011	70% of Tutorials	70%, if 1^{st}-Year GPA<7
2011-2012	70% of Tutorials	70%, if 1^{st}-Year GPA<7
2012-2013	70% of Tutorials	70%, if 1^{st}-Year GPA<7
2013-2014	70% of Tutorials	70%, if 1^{st}-Year GPA<7
2014-2015	70% of Tutorials	0% of Tutorials, Announced after Resit Period
2015-2016	70% of Tutorials	0% of Tutorials

for this second-year cohort during the resit period of the first year (2013-14) was identical to the years before. The years 2008-09, 2009-10, and 2015-16 will serve as placebo years, where 2008-09 and 2015-16 are clear years before and after the forced attendance policy. In the academic year 2009-10 the forced attendance policy was in place for the first time. However, educational policy makers at the university informed us this was decided upon somewhere halfway during the previous academic year. As such, second-year students in 2009-10 were not informed by their student adviser during their first year in 2008-09. This is backed-up by the data, where below- and above-7 students have similar retake propensities in 2008-09. We treat 2009-10 as an additional placebo year.

3 Data

3.1. Data Description. Our primary analysis rests on administrative data from the university. The data includes information on retake decisions for all first- and second-year courses, exam content, grades for all three undergraduate years, including post-retake grades, attendance in all first- and second-year courses, and the demographics of each student. The full data runs from 2007-08 until 2014-15.

Our demographic data includes information on gender, age, the distance between the home of the student and campus, as well as whether the student is from inside the European Economic Area (EEA). Students from inside the EEA pay between 20-25 percent of the tuition for students from outside the EEA. For students from the Netherlands we possess information on their high school grade. The high school grade provides us with a reasonable proxy for student ability because it is a 50-50 weighted average of the actual high school grade and the grade the student obtained on nationwide exams for each of their high school courses.

We restrict our primary analysis to the sample of students who completed the first year before the resit period.[23] We do this because the students in our estimation sample will only have two reasons for writing a resit, either increasing the original or avoiding forced attendance (for below-7 students). Moreover, first year completion rates for students around GPA of 7 is 92 percent, and this sample lends itself to a balanced panel of students and retake decisions. Our primary estimation sample runs from 2009 to 2013 inclusive, has 995 students, and more than 10,000 retake decisions (from all 10 first-year courses).

3.2. Basic Descriptives. Table 1 summarizes the variables which are key to our analysis. The first row summarizes the retake decision and shows that the baseline retake probability is 6.5 percent. The second row summarizes the number of retakes per student and shows students take 0.63 retakes on average.

Rows 3 and 4 summarize the attendance decisions of students in first year. Students attend 92 percent of the tutorials in each of their courses. The attendance rate is higher than 70 percent because there are discrete changes in the rate for each additional tutorial the student attends. For example, if there are 6 tutorials in total, the 70 percent criteria requires that the student 5/6 (83.3 percent). This is discrete jump over the fourth tutorial (4/6 = 66.6). High attendance rates are unsurprising because there is a attendance requirement for all first-year tutorials. The variable in row 4 counts the tutorials that remain at the time when the 70 percent criteria is met. Consistent with the 92 percent attendance rate, the average student has 1.54 tutorials to spare when the criteria is met. We will use this last variable to measure the option value for students.

Row 5 summarizes pre-retake GPAs, the running variable behind our identification strategy. Students enter the retake period with a GPA of 7.23. Row 6 shows that they exit the retake period with a GPA of 7.28, which is slightly higher. Row 7 summarizes the success rate of below-7 students, showing 5.2 percent of students enter the retake period with a GPA less than 7 and exit with a GPA which is more than 7. This 5 percent successfully avoids forced attendance.

Rows 8-12 summarize student demographics. 27.3 percent is comprised of females. Students are 19.25 years of age on average, live 26 kilometers from the university campus, are mostly from inside the European Economic Area, and enter with an average high school

[23] That is, they already "passed" all 10 courses before the resit period.

GPA of 7.17.

3.3. Are Decision Rights Valuable? Figure 1 plots the retake probability (per student) against pre-retake GPA. The figure uses a wider bandwidth than the one in our primary estimation sample in order to illustrate the natural variability in the data. To compensate for the wider bandwidth, we use cubic polynomials in the pre-retake GPA.

The figure shows that retake probabilities are increasing as the pre-retake GPA approaches 7 from the left. It increases to around 10 percent. Once the pre-retake GPA crosses 7, the retake probability drops to around 5 percent. The sharp drop suggests that the threat of forced attendance increases retake propensities, which itself suggests that there is a value in discretion over attendance, at least for the average student.

The figure displays a clear absence of other jumps over the whole range of the pre-resit GPA, also around other round numbers such as an 8 and near other interesting cutoffs such as 8.25, which is the area of the *cum laude* certification - student earn the *cum laude* in first year if their GPA is above 8.25.

4 Baseline Results

4.1. Empirical Specification. The reduced-form regression discontinuity model for retake decisions is as follows:

$$Retake_{ijc} = \beta_0 + \beta_1 D_{ic} + f(\bar{g}_{ic}^{(5)} - 7) + f(\bar{g}_{ic}^{(5)} - 7)D_{ic} + C_{jc} + \mathbf{X}_{ic}\mathbf{\Gamma} + \varepsilon_{ijc} \tag{3}$$

where i, j, and c denote the student, course, and cohort. D_{ic} indicates whether the student has a cumulative GPA $\bar{g}_{ic}^{(5)}$ below 7 after the fifth (last) block of the academic year. $f(\cdot)$ is a polynomial in $\bar{g}_{ic}^{(5)} - 7$, which differs to the left and right of 7. $\mathbf{X}_{ic}$ includes demographics such as gender. We use the notation $\mathbf{X}_{ic}$ instead $\mathbf{X}_i$ to stress the non separability of students from cohorts. $C_{jc}^{(2)}$ are fixed effects which allow for variability in a number of factors, including the local "randomization" that takes place from cohort to cohort, the entry and exit of professors and TAs, as well as changes in course content or materials.

Our baseline analysis focuses on β_1. It measures the marginal effect of the threat of forced attendance on the probability of retake of students near 7. Our expectation is that the threat will increase the probability of retake, *i.e.* $\beta_1 > 0$.[24]

[24] We could have $\beta_1 < 0$, however, if students are aware of their predispositions for non-academic activities

Our primary estimations will use the data-driven bandwidths of (Calonico, Cattaneo, and Titiunik, 2014), henceforth denoted CCT (2014), and a first-order (locally linear) polynomial. We will explore the robustness of our estimates to various alternative bandwidths and to a second-order polynomial.[25] Standard errors will all be clustered on the student.

4.2. Identifying Assumption. The estimates can be interpreted as causal if students have imprecise control over their cumulative GPA at the end of Block 5 (Lee, 2008). Because students knew of the policy before and throughout the first year, they could, in principle, take actions to avoid the prospect of forced attendance at the time of the retake. Our identifying assumption allows the student to have some, but not full, control over whether they face the prospect of forced attendance.

The assumption is reasonable in our setting because this prospect depends on what the student obtains on *average* across their 10 first-year courses. As the year proceeds they lose the capacity to control the average. In a single course, for example, they could ask the professor for and receive a slight bump in their grade. Across 10 courses this becomes more difficult. If a student wants to increase their Block-5 GPA, they would either have to ask one professor for a very large increase, or ask several professors for a small increase. Neither increase is a likely event. [26] Further to this point, Table A1.1 and A1.2 in the Appendix shows students do not try avoid forced attendance throughout the first year (before the retake). They show the results of RD models, where grades and tutorial attendance in block t have no discontinuity near 7 in first-year GPA up until block $t-1$. Given their limited control over their Block-5 GPA, aggregate shocks that affect all course work should be enough to generate random assignment around a GPA of 7.

4.3. Continuity of Personal Characteristics and GPA Probability Density. Table 2 reports estimates of our main specification where instead of the retake decision the dependent variables are personal characteristics. The estimates show that women, distant students, and students from inside the EEA are under-represented to the left of 7. Older students and students with higher high school grades are under-represented to the right of 7. The p-

and, consequently, there is a demand for forced attendance.

[25]Using the data-driven bandwidths of (Calonico, Cattaneo, and Titiunik, 2014), the local linear specifications have an optimal bandwidth of 0.4 [6.6-7.4], while the second-order polynomial has an optimal bandwidth of 0.6 [6.4-7.6]. We do not opt for more polynomials than two, see (Gelman and Imbens, 2017) for a discussion of "why high-order polynomials should not be used in regression discontinuity designs".

[26]Students who are infinitesimally close to 7 can, in principle, manipulate their grade via grade grubbing. Because of this, we estimate donut-hole RD models as robustness.

values suggest that the estimates are statistical zeroes, however. We conclude that personal characteristics vary smoothly around the cutoff.[27]

Figure 2 draws on McCrary (2008) to evaluate the continuity of the density for cumulative (Block 5) GPA around 7. If students can manipulate their GPA, then we should observe an excess mass of students just above 7.[28] Table **??** reports the accompanying estimates of the blue and black line, where the normalized counts of students is explained by the GPA before the resits to test whether there is a discontinuity in the number of students near 7. Both the figure and table show no evidence of bunching and suggest that the probability density for cumulative GPA varies smoothly around 7. The local continuity of the probability density and of personal characteristics support the interpretation that students are locally randomized near 7.

4.4. Baseline Estimates. Table 4 reports estimates of the differential retake propensity of left-of-7 students. Columns (1) to (3) use the CCT bandwidth of 0.4 [6.6,7.4] together with a first-order polynomial. Columns (4) to (6) a second-order polynomial with the CCT bandwidth of 0.6 [6.4,7.6]. Within each polynomial-bandwidth configuration we show how the estimates change as we add controls and course-cohort fixed effects.

The retake probability of left-of-7 students is 6.7 to 7.3 percentage points higher than the retake probability of right-of-7 students. The range for our preferred local linear specifications (Columns 1 to 3) is 6.7 to 7.2 percentage points. Note that these estimates are all significant at the 1 percent level. Our preferred estimates represent a more than 100 percent increase over the mean retake rate of 6.5 percent.

Figure 3 explores the robustness of the estimates to the bandwidth, where the polynomial is held fixed at 1. The point estimates are positive for all bandwidths. They are statistically different from 0 at the 5 percent level at bandwidths of 0.2 or higher. The largest point estimate is at bandwidth of 0.3, which is close to the CCT (2014) bandwidth.

One explanation for the differential retake propensity left of 7 is that students simply have a higher retake propensity whenever their GPA falls short of salient benchmarks, like increasing your GPA to the nearest integer. To investigate this possibility, we re-estimate our baseline specification at pre-retake GPAs of 6 and 8.[29] The estimates are found in Table

[27] Table A1.3 in the Appendix shows very similar results when analyzing the continuity of personal characteristics after each block.

[28] The dots in the figure are counts based on the bin sizes McCrary (2008) proposes. Our conclusions are robust to varying the bin size.

[29] We do not use 9 because there are very little students near this grade and they almost never retake their finals.

5, where Columns (1) and (3) report estimates at 6 and 8, and Column (2) reports our preferred estimate at 7. The estimates show, if anything, that left-of-6 and left-of-8 students are less likely to retake their finals. Our main estimates reflect something different than the response to salient benchmarks.

Table 6 investigates the grade improvement students can expect from retaking their finals. It reports estimates of the effect of provisional assignment to forced attendance on the difference between post- and pre-retake GPA. If we restrict the sample to students who took a resit, the provisional assignment does not improve post-retake GPA, as the point estimates hover around zero with p-values no smaller than 0.6. However, for the whole sample, the GPA gains from the retake range from 0.040 to 0.045 (from 6.98 to 7.02). The estimates are statistically significant at the 5%-level. Below-7 students do not improve their GPA because they do better on the resits, but because they take more resits. Our interpretation is that the decision to do a retake, and not those how much to study on a resit, is the one that distinguishes provisionally forced students from free students.

5 Structural Estimates

5.1. Estimation and Inference. We do not have direct information on $E[g_{icR}|\mathcal{I}_{ic}]$ and π_{ic0}. We construct nonparametric estimates: $E[\widehat{g_{icR}|\mathcal{I}_{ic}}]$ is estimated via the average grade gain in the resits compared to the regular exam separately for every course-cohort combination, and $\hat{\pi}_{ic0}$ is constructed out of predictions, where a second order polynomial in GPA after block 5, for below-7 students, is used to predict the probability of them ending up above 7 after resits.[30] We replace the true values for their estimates, which allows us to back out directly the estimate for the value of discretion over class attendance (β)

$$\ell(\beta, \Theta, \sigma_A) = \sum_{i=1}^{N} \int_{-\infty}^{\infty} \Big[\prod_{j=1}^{10} \Phi\big((2R_{ijc} - 1)(\beta \hat{\pi}_{ic0} \mathcal{D}_{ic} + \widehat{\mathbf{X}}_{ijc}\Theta + \sigma_A a_{ic})\big) \Big] \phi(a_{ic}) da_{ic}, \quad (4)$$

We evaluate the log likelihood via Gaussian Quadrature and simulated maximum likelihood. We will bootstrap the standard errors to account for the additional variation generated by our plugged-in estimates $\widehat{Gain}_{ic}$ and $\hat{\pi}_{ic0}$. Note that our primary sample for estimation is

[30]Note, our previous estimates in Table 6 imply the expected GPA gains from resits are small but not zero. Although this means the probability of ending up above the 7 for provisionally forced students is low, but it is not equal to zero. Moreover, this probability increases as a students' pre-retake GPA approaches 7. Therefore, we predict this probability as a flexible polynomial in pre-retake GPA.

the same as with your baseline results, including students whose cumulative GPA at the end Block 5 is within 0.4 GPA points of 7. We adjust standard errors for within-student correlation across the different course exams students can retake (*i.e.* cluster standard errors on the student).

Estimates are found in Table 7. The point estimates in the top row measure the marginal effect of provisional assignment to forced attendance on the latent variable, *i.e.* the net value of the retake relative to the no-retake option. As shown earlier, this effect equals $EV(d) - EV(f)$. The estimates in the other rows measure the marginal effects of our presumed measures of the costs and benefits of the retake. These other estimates provide the basis for assessing the relative importance of decision rights.

The effect of provisional assignment to forced attendance is more than double the effect of a standard deviation increase in the grade for the course; 3-4 times the effect of our measure of the psychic cost of the retake, *i.e.* the slack the student had when they met the attendance requirement for their first-year tutorials; almost double the effect of our measure of the recall cost of the retake exam.

The effect of tutorials to spare aligns with the idea that it measures measures the (inverse of the) psychic cost of preparing for the retake exam. Students who meet the first-year attendance requirement earlier presumably incur a lower cost to time management and exam preparation. In accordance with this logic, tutorials to spare increases the net value of the retake. The effects of the block dummies aligns with the idea that they measure the recall costs of preparing for retake exams. The effects of the later block dummies are larger than the effects of the early block dummies. This is what we would expect if it is less costly to retake recent final exams. The effect of distance to the big-four cities is statistically negligible. This suggests it is an inadequate measure of the outside option of students.

5.2. Discussion. Our framework for valuing decision rights comes with several assumptions. First, it assumes students choose one course for which they retake the exam. In reality, students have a menu of courses to choose from, being able to retake any 1, 2, or 3 of their 10 exams. We did not model the data-generating process this way because it entails $\binom{10}{3} + \binom{10}{2} + \binom{10}{1} + \binom{10}{0} = 176$ possible alternatives and because techniques that allow for identification of interrelationships among choices (such as complementarities) are still developing. Second, it assumes away study effort and learning between initial and retake exams. The threat might induce below-7 students to prepare more for the retake exams. While this does not pose problems from the perspective of identification, it would be ideal from the perspective of statistical efficiency to have information on the study effort and learning of students. Third, it assumes that students are naive in the sense that they do not account for their actions in second year at the time of retake. Relaxing this assumption requires an equilibrium (multiple selves) model of first-year retake choices and second-year time use. While this might be interesting exercise, it would add substantial complexity to the problem without yielding clear and large benefits. Finally, it assumes no discounting of the value to decision rights in second year. We view this as a secondary problem for our purposes. If students were allowed to the value from their second-year time use, then we would be underestimating the relative value of decision rights.

5.3. Are Students Right to Value Decision Rights? Our companion article (Kapoor, Oosterveen, and Webbink, 2017) uses a similar identification strategy to examine the causal impact of forced attendance on academic performance. It shows the impact of the university-wide policy on second-year grades depends on how courses dealt with above-7 students. In courses where the counterfactual was full voluntary attendance for above-7 students, the policy decreased their grades by 0.35 standard deviations (see Table 8, Column 1, of this article). This suggests that students are right, at least from the perspective of grades, to value decision rights.

6 Conclusion

This article uses exogenous variation in the *threat* of forced attendance to measure the value of decision rights over class attendance at university. The threat of forced attendance increases the propensity of students to retake their finals by 7 percentage points, a more than 100 percent increase over the baseline mean of 6.5 percent. The effect of the threat on the relative value from retaking is equal to one standard deviation increase in the initial grade, three to four times the effect of the psychic cost of the retake, and almost double the effect of the recall cost of the retake exam. These results imply that decision rights are of substantial value to the average student. The evidence suggests, moreover, that students are right to value their decision rights, as forced attendance in second year ultimately decreases grades for the average student.

The article raises several questions. Our estimates also show that forced attendance decreases grades by about 0.35 standard deviations. Does the decrease in grades line up with the value students attach to decision rights over attendance? Are students even aware of the impact of forced attendance on grades? We have spoken with many students, instructors, and academics about our findings. Many expressed surprised at the negative impact of forced attendance. Several students expressed their discontent with the policy, not because they thought that it necessarily decreased their grades, but instead because they felt they had the capacity to make good decisions on their own. If the grade decrease is not commensurate with the value they attach to decision rights, then what are the origins of the residual value? Further to this point, it would be of interest to know whether decision rights over attendance are valuable because students were deprived of these rights in the first year. That is, do people value decision rights only when they know what life is like without them?

The results and conclusions of this and our companion article (Kapoor, Oosterveen, and Webbink, 2017) beg questions as to the merit of forced attendance policies. Knowing this, it is important to recognize that our results are local in the sense that they based on the average student from a prominent university in the Netherlands. The applicability of the conclusions to students at the lower end of the performance distribution remains an open question. If these students have predispositions towards grade-decreasing activities, then they may benefit from forced attendance or structured learning more generally. If they are aware of their predispositions, then they may demand forced attendance or structured learning, or even sort into universities that follow such policies.

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Table 1: **Descriptive Statistics.**

	Definition	Mean	S.D.	Min	Max	Count
Retake	Writes a New Version of Final Exam in the Summer	0.063	0.243	0.000	1.000	9949
# Retakes	Number of Retakes per Student	0.630	0.965	0.000	4.000	995
Tutorial Attendance	Tutorial Attendance in Percentage Terms	0.919	0.093	0.667	1.000	9655
Tutorials to Spare	Tutorials Left when 70% Attendance Requirement Met	1.955	0.752	0.000	4.000	9655
Pre-Retake GPA	GPA After Block 5 (/10)	7.230	0.696	5.773	9.200	995
Post-Retake GPA	GPA After Retake (/10)	7.283	0.690	5.887	9.200	995
Successful Retake	Enters Retake with $GPA < 7$, Exits Retake with $GPA >= 7$	0.052	0.223	0.000	1.000	995
Successful Retake	Sample Restricted to $6.9 <= GPA <= 7.1$	0.197	0.399	0.000	1.000	117
Female		0.273	0.446	0.000	1.000	995
Age (years)		19.246	1.340	16.00	38.00	995
Distance (km)	From Home to University	26.153	33.740	0.237	195.783	995
From Inside EEA		0.945	0.229	0.000	1.000	995
High School Grade		7.165	0.651	4.929	9.091	830

Notes:
1. Statistics are based on the primary estimation sample, which uses 1^{st}-year data for 2009 to 2013, inclusive.
2. EEA denotes European Economic Area.
3. High School Grade is an average over all high school courses. Each course grade is a 50-50 weighted average of the school grade and the grade on a nationwide exam for that course. High School Grade is only observed for students who completed secondary school in the Netherlands.

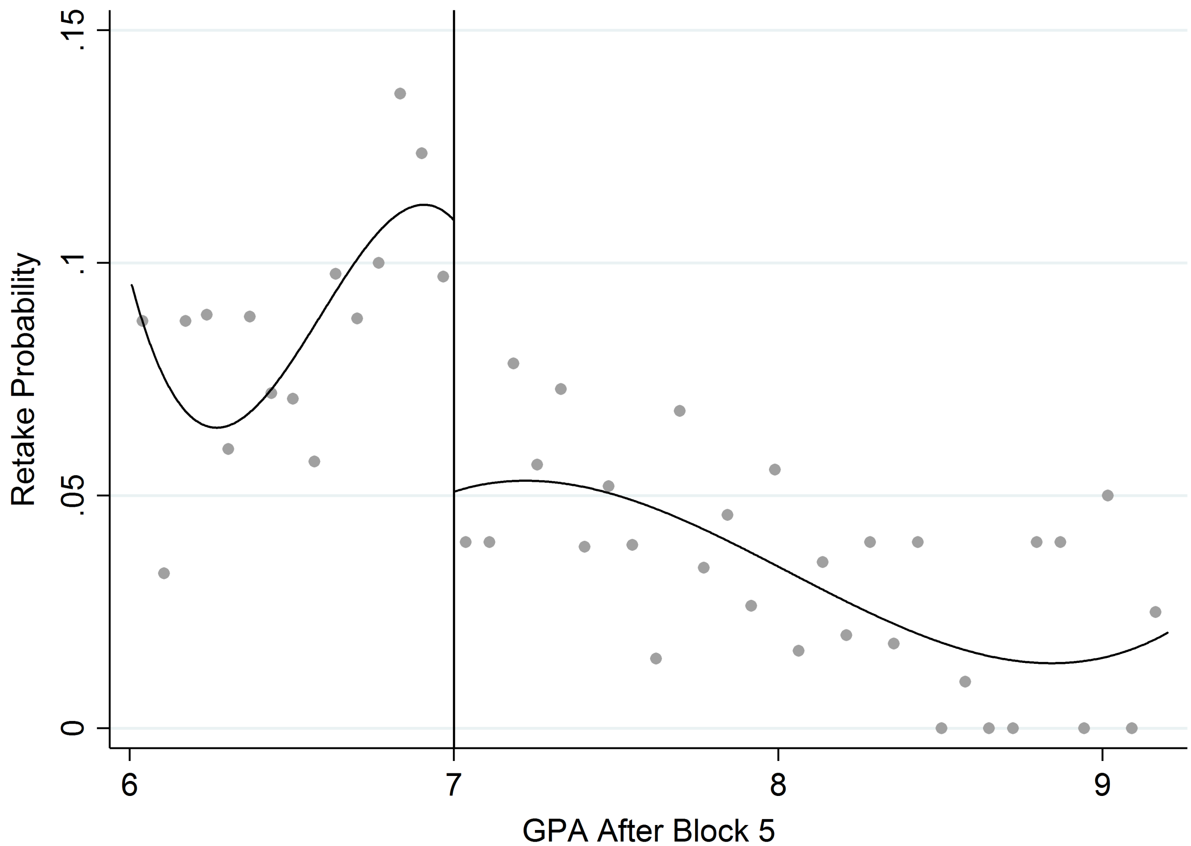


Figure 1: **Retakes and the Threat of Forced Attendance (2009-2013).** The graph shows the retake probability conditional on the GPA students have accumulated over Blocks 1 through 5 of first year. Students with a GPA below 7 are threatened with the loss of decision rights over their attendance in second year. The scatterplots are averages within bins of size 0.0667. The solid line is the outcome as predicted by a cubic polynomial.

Table 2: **Continuity of Personal Characteristics.**

	Female	Age	Distance	EEA	HS Grade
	(1)	(2)	(3)	(4)	(5)
	First-order Polynomial				
1^{st}-Year GPA Below 7	0.021	0.270	-5.254	-0.070*	0.208
(Before Retakes)	(0.802)	(0.167)	(0.364)	(0.085)	(0.303)
Observations	447	447	447	447	388
Adjusted R^2	-0.010	-0.006	0.001	-0.001	0.015
	Second-order Polynomial				
1^{st}-Year GPA Below 7	0.005	0.351	-5.187	-0.053	0.155
(Before Retakes)	(0.959)	(0.168)	(0.478)	(0.251)	(0.536)
Observations	618	618	618	618	533
Adjusted R^2	-0.004	-0.005	-0.009	0.002	0.011

Notes:
1. Regressions include a first-order or second-order polynomial in pre-retake GPA, its interactions with the treatment indicator, and cohort fixed effects.
2. Regressions use the CCT (2014) bandwidth of 0.4 for the first-order polynomial and 0.6 for the second-order polynomial.
3. p-values in parentheses, standard errors are robust.

Figure 2: **Continuity of Density for Pre-Retake GPA.** RD plot of the density for the number of students.

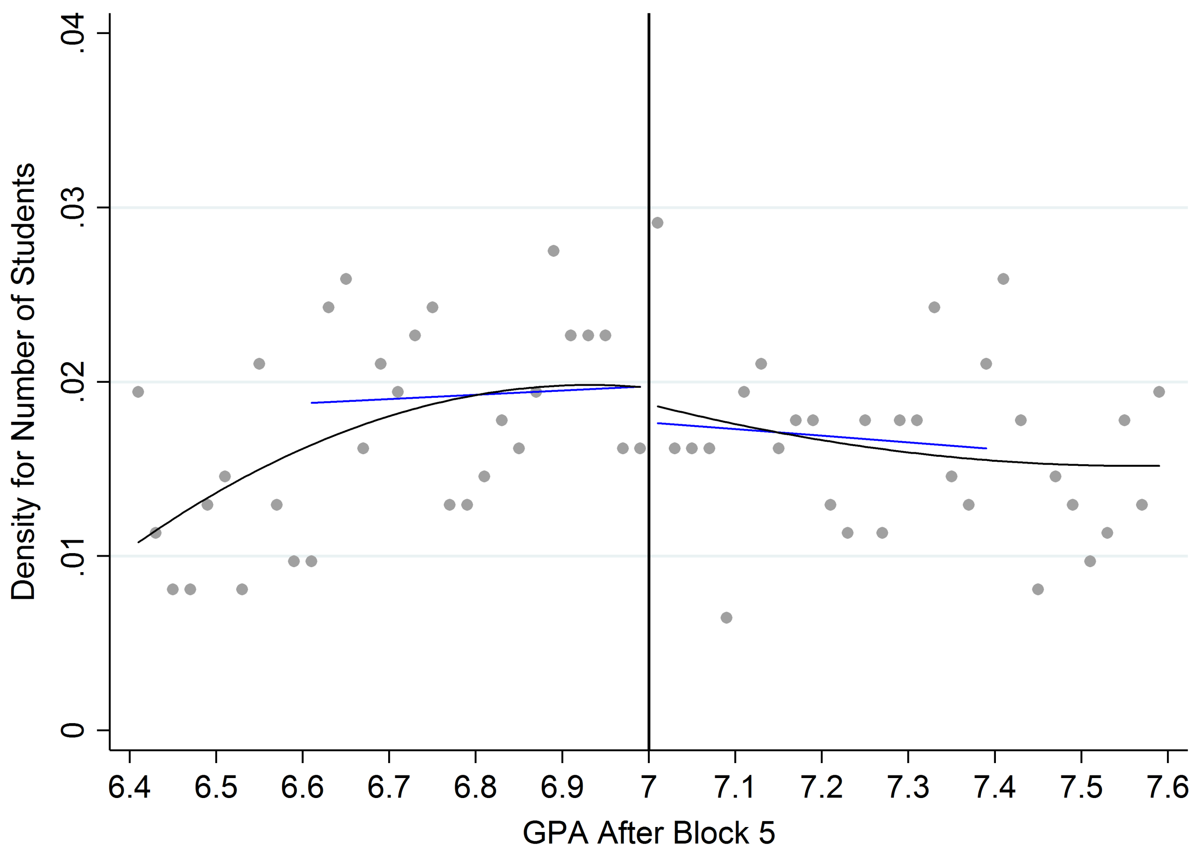


Table 3: **No Bunching Just Above 7.** Tested through the method proposed by McCrary (2008).

	Counts of Number of Students		
	Local linear regression	Second-order polynomial	Third-order polynomial
	(1)	(2)	(3)
Binsize is somewhat smaller than suggested by McCrary (2008)			
1^{st}-year GPA Below 7	0.002	0.001	-0.003
(Before Retakes)	(0.568)	(0.834)	(0.549)
Observations	40	60	60
Adjusted R^2	-0.015	0.089	0.081
	Bins two times as small as in upper panel		
1^{st}-year GPA Below 7	0.001	0.000	-0.003
(Before Retakes)	(0.784)	(0.950)	(0.437)
Observations	80	120	120
Adjusted R^2	-0.019	0.015	0.013

Notes:
1. The local linear regression is estimated on the bandwidth of 0.4, whereas the second- and third order polynomial is estimated on the bandwidth of 0.6. Polynomial is interacted with the treatment.
2. The panels refer to the different binsize as to compute the histogram for the number of students. Results are robust to the binsize.
3. p-values in parentheses, standard errors are robust.

Table 4: **Do Students Value Decision Rights Over Attendance?**

	Retake Final Exam					
	(1)	(2)	(3)	(4)	(5)	(6)
1^{st}-Year GPA Below	0.072***	0.067***	0.070***	0.073***	0.070***	0.072***
(Before Retakes)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Polynomial Order	$1st$	$1st$	$1st$	$2nd$	$2nd$	$2nd$
Controls	No	Yes	Yes	No	Yes	Yes
Course-Cohort FE	No	No	Yes	No	No	Yes
Observations	4470	4470	4470	6179	6179	6179
Adjusted R^2	0.008	0.015	0.062	0.010	0.016	0.057

Notes:

1. Regressions include polynomials in pre-retake GPA as well as their interactions with the treatment indicator.
2. Controls include gender, age, distance to the university, and an indicator for whether the student is from inside the European Economic Area (EEA).
3. Regressions based on the CCT (2014) bandwidth. The bandwidth for Columns 1 through 3 is 0.4. For Columns 4 through 6 it is 0.6.
4. p-values in parentheses, standard errors are clustered on the student level.

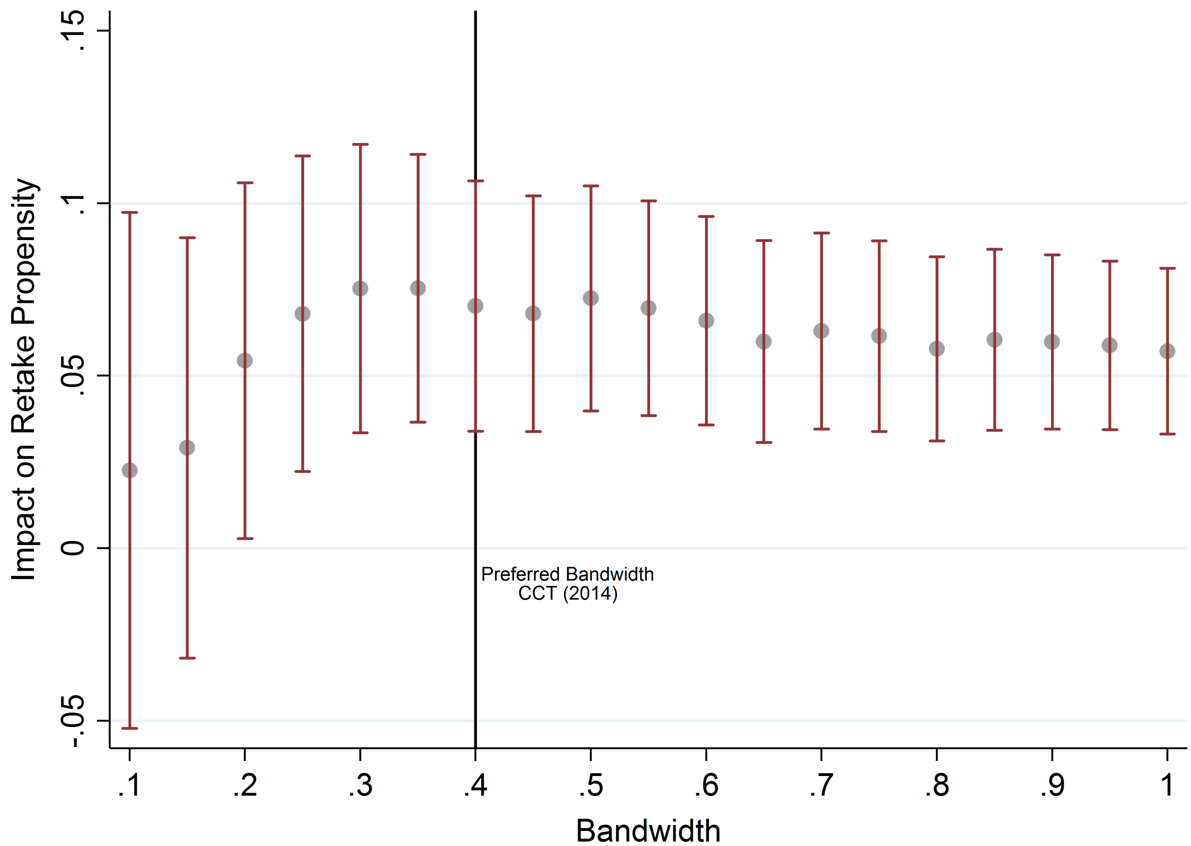
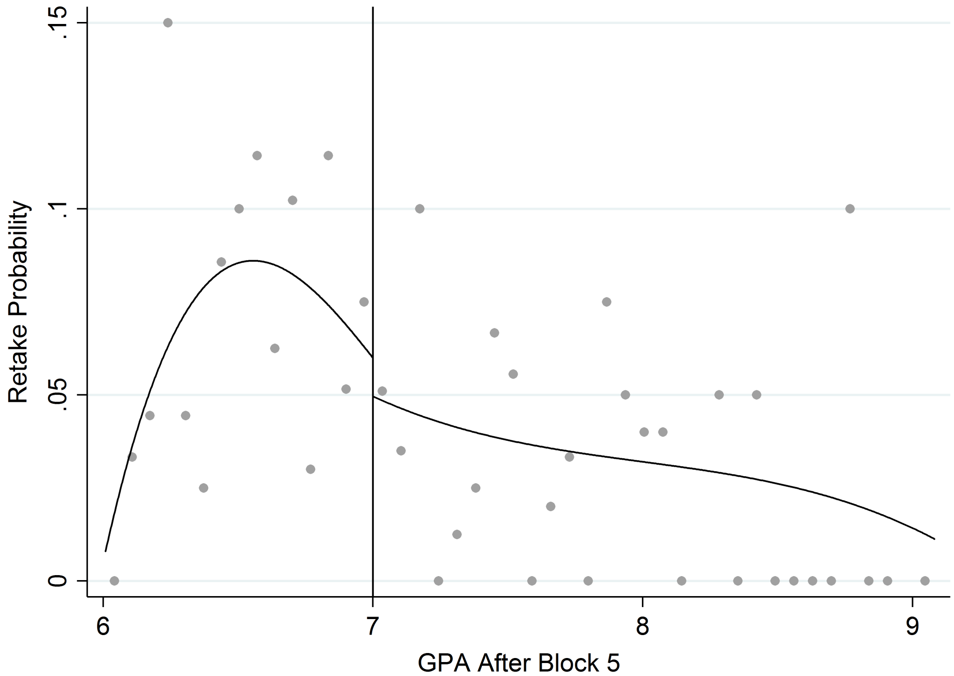


Figure 3: **Robustness to Bandwidth.** The figure plots RD estimates for bandwidths of 0.1, 0.15, etc. The grey dots are the point estimates at the respective bandwidth. The red lines around the dot represent 95 percent confidence intervals. The polynomial order is 1. The regressions include all controls and course-cohort fixed effects.

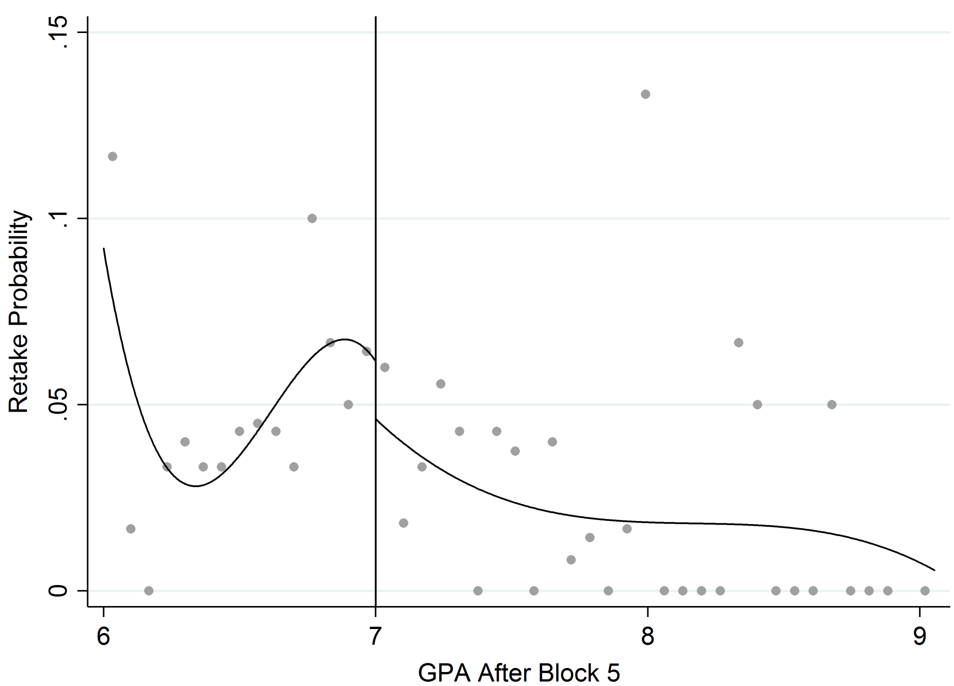
Table 5: **Retake Propensity at other Cutoffs.**

	Retake Final Exam			
	(1)	(2)	(3)	(4)
1^{st}-Year				
GPA Below 6.0	-0.036			
	(0.303)			
GPA Below 7.0		0.070***		
		(0.000)		
GPA Below 8.0			-0.005	
			(0.774)	
GPA Below 8.25				-0.010
				(0.665)
Observations	1040	4470	2080	1620
Adjusted R^2	0.087	0.062	0.053	0.049

Notes:
1. Regressions include a first-order polynomial in the pre-retake GPA, its interactions with the treatment indicator, gender, age, distance to the university, an indicator for whether the student is from inside the European Economic Area (EEA), and fixed effects for the course-cohort combination.
2. All regressions use the (optimal) bandwidth of 0.4.
3. p-values in parentheses, standard errors are clustered on the student level.



(a) Before (2007-2008)



(b) After (2014)

Figure 4: **Retakes without the Threat of Forced Attendance Policy.** The graphs show the retake probability conditional on the pre-retake GPA. The scatterplots are averages within bins of size 0.0667. The solid line is the outcome as predicted by a cubic polynomial.

Table 6: **First-Year GPA Gain from Retake.**

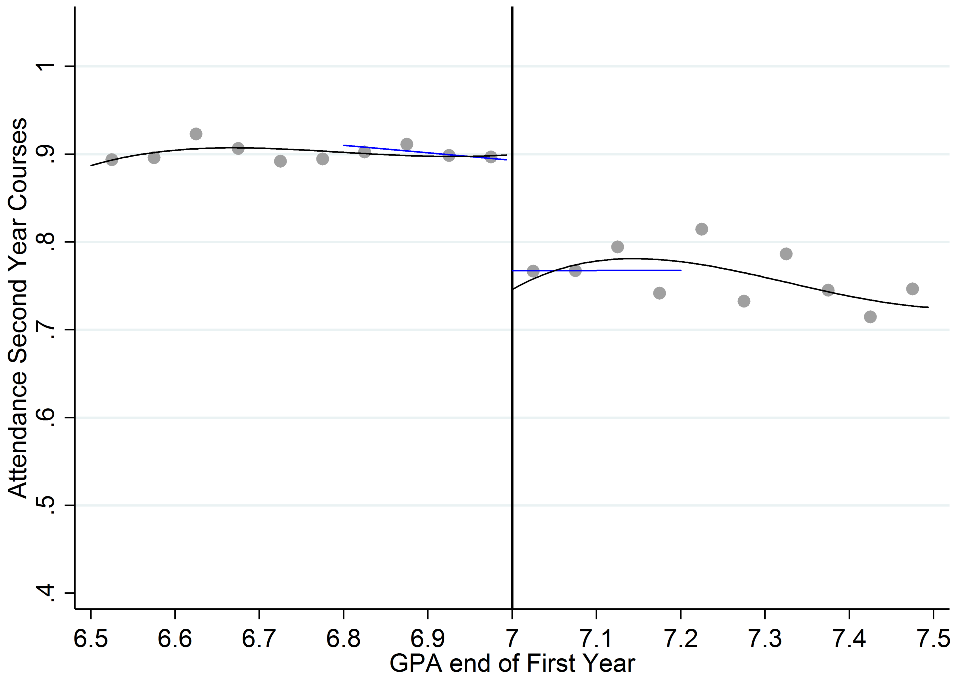
	Difference Between Post- and Pre-Retake GPA					
	(1)	(2)	(3)	(4)	(5)	(6)
	Sample=All Students					
1^{st}-Year GPA Below 7	0.045**	0.040**	0.041**	0.047**	0.043*	0.043*
(Before Retakes)	(0.021)	(0.039)	(0.033)	(0.047)	(0.062)	(0.064)
Controls	No	Yes	Yes	No	Yes	Yes
Cohort FE	No	No	Yes	No	No	Yes
Observations	447	447	447	618	618	618
Adjusted R^2	0.024	0.056	0.056	0.024	0.042	0.046
	Sample=Students who Took a Resit					
1^{st}-Year GPA Below 7	0.025	0.020	0.025	0.011	0.005	0.005
(Before Retakes)	(0.513)	(0.600)	(0.511)	(0.820)	(0.916)	(0.917)
Polynomial Order	$1st$	$1st$	$1st$	$2nd$	$2nd$	$2nd$
Controls	No	Yes	Yes	No	Yes	Yes
Cohort FE	No	No	Yes	No	No	Yes
Observations	205	205	205	261	261	261
Adjusted R^2	0.007	0.029	0.019	-0.000	0.017	0.012

Notes:
1. Regressions include polynomials in pre-retake GPA as well as their interactions with the treatment.
2. Controls include gender, age, distance to the university, and an indicator for whether the student is from inside the European Economic Area (EEA).
3. Regressions based on a bandwidth of 0.4 for the first-order polynomial and 0.6 for the second-order polynomial.
4. p-values in parentheses, standard errors are robust.

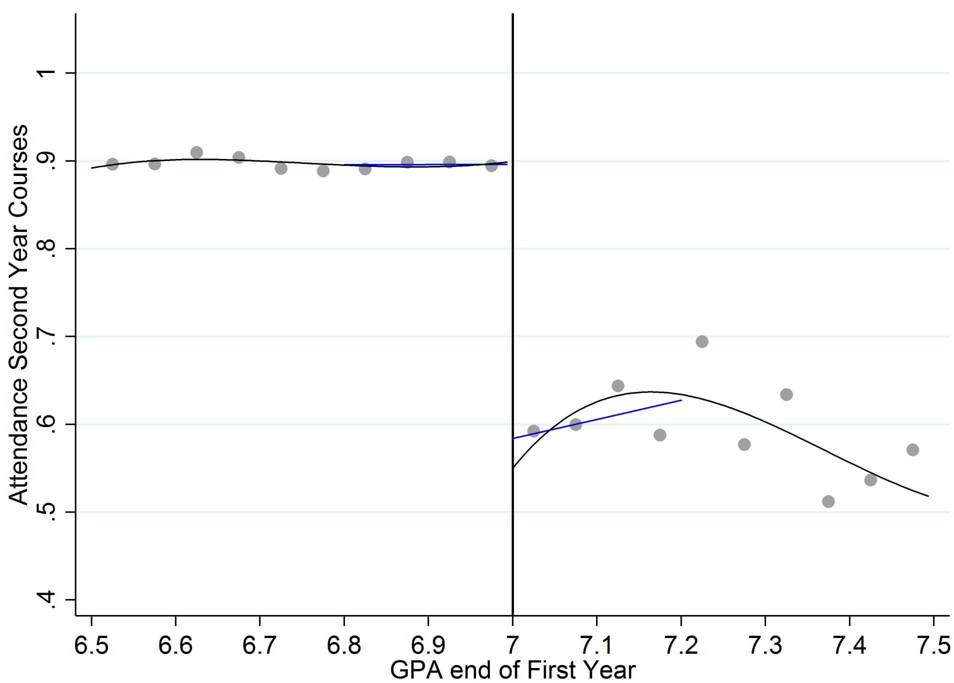
Table 7: **Value of Decision Rights (2009-2014).**

	Latent Variable = Net Utility ($\mathcal{V}$)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$V(0) - V(\kappa)$	1.111***	1.104***	1.415***	1.420***	1.375***	1.389***	1.015**	1.084**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.015)	(0.025)
Expected Grade Increase (Standardized)			0.080**	0.083**	0.083**	0.082**	0.079**	0.030
			(0.022)	(0.018)	(0.018)	(0.019)	(0.025)	(0.372)
Original Grade in Course			-1.115***	-1.127***	-1.140***	-1.142***	-1.147***	-1.093***
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Block 2 (0/1)			0.016	0.032	0.029	0.031	0.026	-0.027
			(0.875)	(0.754)	(0.777)	(0.763)	(0.799)	(0.764)
Block 3 (0/1)			0.044	0.110	0.122	0.123	0.110	-0.019
			(0.722)	(0.375)	(0.325)	(0.323)	(0.377)	(0.863)
Block 4 (0/1)			0.082	0.125	0.138	0.138	0.131	0.091
			(0.389)	(0.208)	(0.175)	(0.176)	(0.200)	(0.321)
Block 5 (0/1)			0.286***	0.339***	0.365***	0.367***	0.360***	0.351***
			(0.002)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Minimum Distance to Big-4 (Standardized)			0.028	0.036	0.050	0.051	0.052	-0.028
			(0.782)	(0.728)	(0.637)	(0.629)	(0.618)	(0.757)
Attendance in First Year (Standardized)				0.122**	0.196**	0.253*	0.255*	0.221*
				(0.038)	(0.010)	(0.074)	(0.064)	(0.064)
Tutorials to Spare Standardized					-0.009	-0.051	-0.041	-0.026
					(0.820)	(0.432)	(0.525)	(0.669)
Attendance × Treatment						-0.084	-0.087	-0.131
						(0.617)	(0.600)	(0.372)
Tutorials to Spare × Treatment						0.062	0.051	0.044
						(0.445)	(0.529)	(0.554)
Female		0.231***	0.284***	0.283***	0.292***	0.293***	-0.040	0.002
		(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.810)	(0.986)
Female × Treatment							0.518***	0.463***
							(0.009)	(0.005)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LogLikelihood	-1254.990	-1234.699	-873.540	-867.551	-856.039	-855.802	-850.077	-1108.626
Observations	4470	4470	4469	4448	4344	4344	4344	5994

Notes:
1. Index functions include an intercept, first-order polynomials in pre-retake GPA, as well as their interactions with the treatment indicator. The exception is Column 8, which includes a second-order polynomial.
2. Controls include gender, age, distance to university, and an indicator for whether the student is from inside the European Economic Area (EEA).
2. Estimations use the CCT (2014) bandwidth of 0.4 (Columns 1-7) and 0.6 (Column 8).
3. p-values in parentheses, standard errors are clustered on the student level.

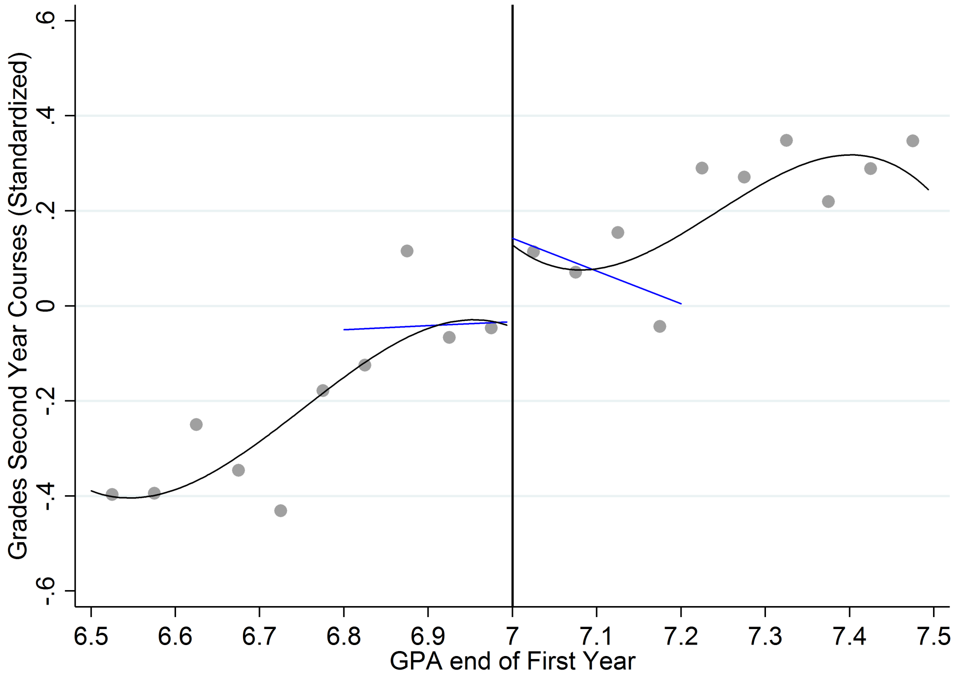


(a) All courses

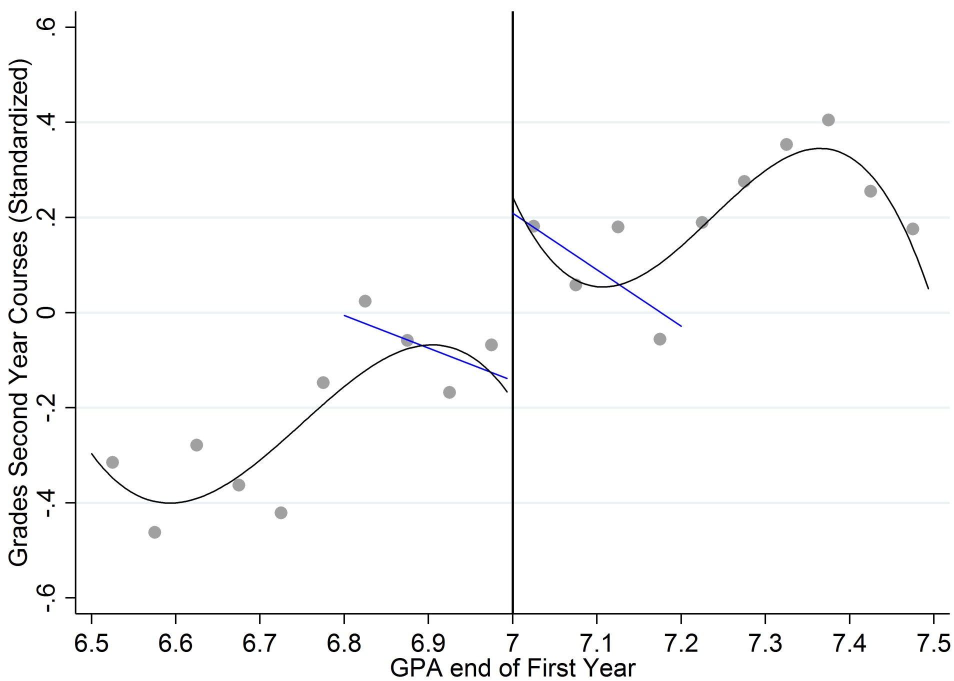


(b) Courses where Attendance was Voluntary

Figure 5: **Attendance in Second Year (after Realization of Forced Attendance).** The graphs show the attendance in second year conditional on the post-retake GPA. The scatterplots are averages within bins of size 0.05. The blue line is a local linear regression and the black line a cubic regression. See Kapoor, Oosterveen, and Webbink (2017) for details on the bandwidth- and polynomial choice.



(a) All courses



(b) Courses where Attendance was Voluntary

Figure 6: **Standardized Grades in Second Year (after Realization of Forced Attendance).** The graphs show the standardized grades in second year conditional on the post-retake GPA. The scatterplots are averages within bins of size 0.05. The blue line is a local linear regression and the black line a cubic regression. See Kapoor, Oosterveen, and Webbink (2017) for details on the bandwidth- and polynomial choice.

Table 8: **Impact of Forced Attendance on Second-Year Attendance and Grades.**

	Average Effect				Marginal Effects by Course Type	
	First-order Polynomial		Third-order Polynomial		First-order	Third-order
	(1)	(2)	(3)	(4)	(5)	(6)
	Attendance (% Tutorials Attended)					
1^{st}-Year GPA Below 7	0.127***	0.118***	0.151***	0.147***	0.113***	0.146***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)	(0.003)
Attendance is Voluntary × Treatment					0.174***	0.195***
					(0.000)	(0.000)
Absence is Penalized × Treatment					-0.121***	-0.149***
					(0.003)	(0.003)
Observations	2136	2136	4901	4901	2136	4901
Adjusted R^2	0.305	0.316	0.306	0.311	0.369	0.370
	Standardized Grades					
1^{st}-Year GPA Below 7	-0.144	-0.139	-0.153	-0.154	-0.007	0.026
	(0.127)	(0.139)	(0.207)	(0.199)	(0.954)	(0.869)
Attendance is Voluntary × Treatment					-0.335**	-0.447**
					(0.024)	(0.019)
Absence is Penalized × Treatment					-0.131	-0.185
					(0.333)	(0.289)
Observations	2136	2136	4901	4901	2136	4901
Adjusted R^2	0.166	0.167	0.210	0.210	0.166	0.210

Notes:
1. Attendance and Grades are for second-year students. Sample is from all second-year courses.
2. All regressions include a first- or third-order polynomial in the first-year final GPA, its interactions with the treatment, fixed effects for the course-cohort combination, gender, age, distance to the university, and an indicator for whether the student is from inside the European Economic Area (EEA).
4. The bandwidth is 0.2 for the first-order polynomial and 0.5 for the second-order polynomial. See Kapoor, Oosterveen, and Webbink (2017) for a discussion on the bandwidth- and polynomial choice.
5. p-values in parentheses, standard errors are clustered on the student level.

Valuing Decision Rights over Class Attendance

Appendix

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April 4, 2018

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Table A1.1: **Can Students Improve their Grade Before the Retake?**

	Grade in Block 3	Grade in Block 4	Grade in Block 5
	(1)	(2)	(3)
	First-order Polynomial		
1^{st}-Year GPA Below 7	0.310^{*}	-0.308^{**}	-0.076
(After Previous Block)	(0.075)	(0.044)	(0.565)
Observations	346	381	422
Adjusted R^2	0.103	0.121	0.171
	Second-order Polynomial		
1^{st}-Year GPA Below 7	0.328	-0.415^{**}	-0.104
(After Previous Block)	(0.117)	(0.032)	(0.517)
Observations	510	539	591
Adjusted R^2	0.139	0.085	0.187

Notes:
1. All regressions include a first-order or second-order polynomial in the cumulative GPA after each block, their interactions with the treatment, cohort fixed effects, gender, age, distance to the university, and an indicator for whether the student is from inside the European Economic Area (EEA).
2. The bandwidth is 0.4 for the first-order polynomial and 0.6 for the second-order polynomial.
3. p-values in parentheses, standard errors are robust.

Table A1.2: **Do Students Try to Avoid Forced Attendance Before the Retake?**

	Attendance in			Tutorials to Spare in		
	Block 3	Block 4	Block 5	Block 3	Block 4	Block 5
	(1)	(2)	(3)	(4)	(5)	(6)
1^{st}-Year GPA Below 7	0.007	-0.015	-0.002	0.200	-0.153	-0.003
(After Previous Block)	(0.867)	(0.455)	(0.850)	(0.219)	(0.379)	(0.967)
Observations	346	381	422	341	381	422
Adjusted R^2	0.211	0.002	0.029	0.004	0.013	0.015

Notes:
1. All regressions include a first-order polynomial in the cumulative GPA after each block, its interaction with the treatment, cohort fixed effects, gender, age, distance to the university, and an indicator for whether the student is from inside the European Economic Area (EEA).
2. The bandwidth used is 0.4.
3. p-values in parentheses, standard errors are robust.

Table A1.3: **Pre-Retake Continuity of Personal Characteristics.**

	Female	Age	Distance	EEA	HS Grade
	(1)	(2)	(3)	(4)	(5)
			Block 3		
1^{st}-Year GPA Below 7	0.224**	0.093	2.498	0.046	-0.509*
(After Block 2)	(0.024)	(0.721)	(0.752)	(0.323)	(0.068)
Observations	346	346	346	346	299
Adjusted R^2	0.008	-0.003	0.001	0.014	0.033
			Block 4		
1^{st}-Year GPA Below 7	0.021	0.316	-6.764	0.005	0.134
(After Block 3)	(0.829)	(0.117)	(0.335)	(0.917)	(0.551)
Observations	381	381	381	381	333
Adjusted R^2	-0.008	0.003	-0.012	0.010	0.006
			Block 5		
1^{st}-Year GPA Below 7	-0.040	-0.055	-4.248	0.058	0.315
(After Block 4)	(0.629)	(0.796)	(0.481)	(0.188)	(0.284)
Observations	422	422	422	422	365
Adjusted R^2	-0.014	0.006	0.004	0.011	0.008

Notes:
1. All regressions include a first-order polynomial in GPA after the respective block, its interaction with the treatment, and cohort fixed effects.
2. The bandwidth used is 0.4.
3. p-values in parentheses, standard errors are robust.