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ABSTRACT

Mean Convergence, Combinatorics, and Grade-Point Averages*

While comparing students across large differences in GPA follows one's intuition that higher GPAs correlate positively with higher-performing students, this need not be the case locally. Grade-point averaging is fundamentally a combinatorics problem, and thereby challenges inference based on local comparisons—this is especially true when students have experienced only small numbers of classes. While the effect of combinatorics diminishes in larger numbers of classes, mean convergence then has us jeopardize local comparability as GPA better delineates students of different ability. Given these two characteristics in decoding GPA, we discuss the advantages of machine-learning approaches to identifying treatment in educational settings.

JEL Classification: I21, I26, C21

Keywords: GPA, grades, program evaluation, random forest, regression discontinuity

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1 Introduction

We deconstruct grade-point averages (GPAs) to highlight the complex ways in which variation in it can challenge one's intuition about local comparability and, ultimately, interfere with causal identification. In particular, we consider how mean convergence and combinatorics influence the evolution of GPA as students engage in more classes, and demonstrate how these processes tradeoff in such a way that one's interpretation of GPA should also change with the number of contributing classes. This will be especially true of one's interpretation of local variation in GPA, which can often be misleading.

It is easy to imagine that one would find different types of student across large differences in GPA. For example, we think it's reasonable to expect that students with GPAs of 3.81 or 3.84 have relatively desirable productive characteristics on average when compared to those with GPAs of 2.29 or 2.30. That said, many important decisions are made around *small* differences in GPA. Admission decisions, the determination of probation or its end, scholarship qualification, and what majors are available to students—each of these are can be determined by relatively local considerations of GPA, and often quite early in one's college career. Similarly, we imagine academic or human resources departments wanting to compare GPAs with simple rationing rules like "applicants must have a minimum 3.0 GPA." Such rules have interesting implications if the average ability of applicants with 2.99 GPAs is higher than that at 3.00. Unfortunately, however, there is little guidance in the literature regarding the validity of making local comparisons in this way.

Given these practices, it is likewise common for researchers to use local variation in GPA to evaluate policy or the efficacy of treatment. We are made more confident when research designs identify treatment off of differences in outcomes for students with more-similar GPAs, especially when these comparisons also leverage resource allocations following a GPA-based rule for treatment assignment. For example, regression-discontinuity designs can identify the efficacy of treatment when students are treated differently on either side of a given GPA. When we employ such strategies, though, identification is supported by an appeal to smoothness—to *local* comparability. With that in mind, we will demonstrate that in the very construction of GPA one should *expect* parameter instability around

local GPA treatment cutoffs, especially when the number of classes taken by students is small.[1]

While inference across large differences in GPA will still have the ability to signal differences in the types of student generating those GPAs, we will demonstrate that local comparisons of GPA do not reliably do the same. Doing so, we draw on two characteristics that are fundamentally important to decoding GPA, and useful for practitioners and applied researchers to have in mind. First, we consider *convergence in mean*, whereby GPA better represents the student's ability as they engage in additional classes. In essence, when conditioning on students having "similar" GPAs, the remaining (and unobservable) heterogeneity across students is endogenous to the number of classes they have taken. Second, we consider the implications of the *combinatorics* of GPA, which governs the sets of GPAs that are feasible given a finite set of contributing grades.[2] Some GPAs are not available at all, for example, or are only available after having taken a specific number of classes, or are exceedingly rare and only arrived at in very particular combinations of grades. We should be mindful of this when interpreting variation in GPA, both cross-sectionally and over time.

While each of these characteristics can challenge how one interprets variation in GPA, their co-existence can be particularly challenging. Consider, for example, the evolution of GPAs as students engage in more classes—to do so, we'll think of additional classes as draws from a distribution of grades centered around the student's modal grade. As such a process evolves, different types of student will separate into occupying different parts of the domain space. In this way, GPA becomes a better reflection of a student's true type as additional draws are made (i.e., as classes are completed). However, just as that convergence makes GPA a more-informative signal of student type generally, it necessarily makes local comparisons of students across GPA quite challenging. Moreover, while "comparability" sounds like it is enhanced by considering students with "more-similar" GPAs—and it can be, in some ways—it is the comparison of more-similar GPAs that will expose us to the pitfalls of combinatorics. In fact, as we've suggested, small differences in GPA will often not signal the anticipated change

[1] This is different than considering the sensitivity of estimates to observations close to the discontinuity in treatment (Butcher et al., 2008) where the first-order concern is that treated units may have endogenously selected into a preferred side of the threshold. It's more similar to (Barreca et al., 2011) and (Barreca et al., 2016), where there is some mechanistic sorting that may or may not end up collecting masses of certain types of individuals around a threshold. However, in (Barreca et al., 2011) and (Barreca et al., 2016) it is into 100g weights (e.g., through rounding). In the case of GPA, sorting is due to a more-complex process of combinatorics, which does not leave behind the easily decipherable patterns that are evident in simple rounding processes.

[2] For example, three courses with grades of 2.3, 3.3, and 3.3 yield a GPA of 2.96, but so do three courses with grades of 2.3, 2.3, and 4.3. These sorts of distinctions are systematic, and can lead to non-smoothness, for example. With the right tools, they are also informative, however. We will demonstrate that machine learning exploits exactly this informativeness in categorizing students.

in student ability at all, as combinatorics produces *non-monotonicities* in the relationship between underlying student ability and GPA.

As part of this exercise, we will consider the informativeness of GPA using traditional methods as well as with modern machine-learning approaches—machine methods will prove to be particularly well suited to unlocking the complexity of GPA. So, while we conclude generally by arguing in favor of an extra measure of care in how one interprets variation in GPA, we also highlight a productive best response to this complexity. In particular, while combinatorics challenges linear estimators, it will directly advantage machine classifiers—combinatorics are systematic, and learnable with enough data. Thus, while we tell a cautionary tale, the particulars of that tale will also exemplify recent developments in machine learning and how those tools perform despite the complexity of GPA data. Random forests (RF), for example, can flexibly incorporate a student's entire transcript of grades in a prediction exercise. Rather than making comparisons between students at adjacent GPAs, which are sensitive to the combinatorics of grade accumulation, an RF learner can distinguish between students even when they have the same GPA—the paths to a given GPA are learnable, and to the extent that outcomes are associated with *how* students arrive at their GPAs we understand student heterogeneity better. The intuition is not always straightforward, either, and is therefore refined by this consideration. For example, consider the problem of classifying two students who have the same GPA despite only one of them having an F on his transcript. All else equal, this F would typically be associated with lower ability. Yet, *conditional on having the same GPA*, having an F on one's transcript *increases* the likelihood that one is higher ability—with a failing grade included, in order to have achieved the same GPA as others one must have received relatively better grades in other classes. Ultimately, having the same GPA is not sufficient to satisfy the "all else equal" comparison we desire, as there are different paths by which students arrive at GPAs.

We organize the remainder of the paper as follows. In Section 2 we discuss mean convergence and combinatorics in the context of grade-point averaging. In so doing, we illustrate these sources of concern for estimators that rely on localness. In Section 3 we then simulate course-taking behavior. This allows us to see the important properties within the data-generating process known (i.e., the distribution of a student's *potential* grades, in particular) and demonstrate several implications for the use and interpretation of GPA. In Section 4 we then consider how machine learning can be useful when there is incomplete information about student ability and one relies on GPA to distinguish students.

We first demonstrate the advantage of machine-learned approaches with a random forest predicting outcomes as a function of student performance—random forests are known to be good predictors, so here we find that it does quite well, even "learning" that there can be students of different ability at the same GPA. We then extend this framework to consider a the casual forest of Wager and Athey (2018), which further demonstrates the advantage of machine-learned methods when GPA is central to a research design. We draw concluding remarks in Section 5.

2 Two characteristic components of GPA

2.1 Mean convergence

In a world where high-ability students draw grades from distributions that dominate those of lower-performing students (in a first-order stochastic sense),there are still positive probabilities on relatively low grades being received in a given class. Likewise, low-ability students can still receive relatively high grades on individual classes. It's with multiple classes that the higher-ability students separate, as their GPAs converge to their "true" central tendencies. This mean convergence implies that as students take more classes, there is less overlap in the distributions of GPAs among students of different types—students of different abilities pool less and separate more over time.

For example, in a population of "C" and "B" students, suppose that "C" students receive course grades stochastically from the set {D+, C-, C, C+, B-}, corresponding to the grade points {1.3, 1.7, 2.0, 2.3, 2.7}, and that the weight on each leaves the "C" student's expected grade point equal to 2.0. A uniform probability distribution would produce this expectation, for instance. Suppose "B" students are defined similarly, but receive grades from the set {C+, B-, B, B+, A-} and an expected grade point of 3.0. Given the overlap of potential grades, "C" students can clearly outperform "B" students in a given class. However, as the number of classes taken by this population increases, the probability of observing "C" students with GPAs of 2.0 approaches 1, and likewise for "B" students and GPAs of 3.0. Thus, even with overlap in the grades two types of student can earn, the overlap in grade-point *averages* is decreasing with the number of classes taken.

This can directly challenge research designs that rely on smoothness around local changes in GPA. For example, consider the intuition that motivates regression discontinuities—as differences in GPA become vanishingly small, we are more confident in the comparability of students on either side of a

discontinuity. In the language of "types" that we are developing here, comparability on both sides of a threshold is synonymous with an overlap assumption—that the types who are present on one side of a treatment threshold are also present on the other side, and in comparable proportion within some bandwidth. Mean convergence implies that this overlap is not guaranteed, however, but rather is highly dependent on the number of classes students have taken. In the worst case, one can imagine that at some standard number of classes when a treatment is commonly applied (e.g., major entry after one year of coursework), the threshold assigning students into treated and untreated groups is also perfectly dividing students by type.

To demonstrate this intuition more concretely, we build on the simple example above. In particular, we assume that there are four types of student, with each type drawing grades from distributions that center on grade-points of 1, 2, 3, or 4, and that students draw grades with equal odds from the grade points that are within ± 1 of their central tendency. Doing so, for example, has "D" types draw grades from {0.0, 0.3, 0.7, 1.0, 1.3, 1.7, 2.0}, and "C" types drawing grades from {1.0, 1.3, 1.7, 2.0, 2.3, 2.7, 3.0}, and so on—these should be recognizable as the common way in which letter grades are numerically recorded.[3] As part of the data-generating process we will assume that students of different ability levels are also level-different in expected outcomes. Specifically, higher-ability students experience better outcomes on average—the average A type is level higher than the average B type, and the average B type is level higher than the average C type, and so on.[4] However, important to note, there is no treatment occurring anywhere in the data-generating process.

We depict this data-generating process in Figure 1, where we simulate 125 students of each type each drawing grades from classes according to this process. (While we do distinguish students' types with color, note that student heterogeneity is unobservable *ex ante*.) With no treatment occurring anywhere in the data-generating process, we then proceed to estimate discontinuities in each panel, as though one was inquiring into evidence of treatment at some GPA with a discontinuity estimator.

[3] Note that in this setup, a student whose grade distribution centers on 4 (i.e., an "A" student) can receive grade points of up to 5.0. Such a grade is outside the standard GPA range, but this difference will not materially alter the process of mean convergence—the point is that "A" students converge to higher expected GPA. We discuss the implications of this top-coding of grade point contributions in Appendix B. In reality, such "A" students would have their positive grade shocks top-coded as "A+." The consequence of this is that "A" students as we define them would have a limited ability to benefit from positive grade shocks relative to their peers. The impact of this top-coding can be significant—notice, for instance, that top-coding leads the expected GPA for each student type to be unequal to their central grade point, altering the mean GPAs to which these student types converge. However, top-coding is mechanically distinct from the issues of mean convergence and combinatorics, and also separable in terms of its impact on inference.

[4] Specifically, we will simulate that "D" students have outcomes described by $\sim N(10, 5)$, "C" students have outcomes described by $\sim N(20, 5)$, "B" students have outcomes described by $\sim N(30, 5)$, and "A" students have outcomes described by $\sim N(40, 5)$.

In Panel A, all students have drawn four classes and we consider whether one could establish evidence of "treatment" having fallen on students with GPAs at or above 2.50. Even on visual inspection, we see that there are "true 2s" on the right of 2.50—this is beneficial to the estimator, as an abrupt change in the makeup of students at 2.50 would be troubling. Despite level differences in outcomes across student type, fitting $y_i = f(\text{GPA}_i)$ on either side of a 2.50 GPA threshold in Panel A yields a confidence interval within which the true $\beta = 0$ is contained. However, in Panel B we consider these same students after they have taken four additional classes and students have further separated. We should, generally, anticipate that students will begin to separate in GPA as they engage with these classes, which we see evidence of in Panel B. (There are fewer "true 2s" above 2.50 in Panel B.) More to the point, we can see that fitting $y_i = f(\text{GPA}_i)$ to the same students just one-semester later has us moving toward mistaking unobserved heterogeneity in type (i.e., what we know are just level difference in outcomes in this example) for something that looks like a discontinuity in y_i at GPAs of 2.50. If we tightened up around the "threshold," as we do in Panel C, we would clearly identify a significant discontinuity in outcomes at 2.50 despite there being no treatment at all.[5]

Thus far, this example makes clear that observing GPA is no guarantee that we can control for unobserved heterogeneity—or, at least that linear estimators may not be sufficient. However, behind this example we also evidence that smoothness in the density of observations at the threshold does not assure that there is likewise smoothness in student *type* at the threshold.[6] What drives the estimated discontinuities in panels A through C are the 2.50 (placebo) thresholds relative to the central tendencies of students who are in the vicinity of 2.50. In Panel D we estimate a discontinuity at a different threshold, safely in the middle of a type of student. Moreover, we choose a small-enough bandwidth that there is only rare weight on other types of student when fitting $y_i = f(\text{GPA}_i)$. The implications of mean convergence should be less evident here—as one might expect, the confidence intervals overlap and the traditional RD analysis cannot reject that $\hat{\beta} = 0$. In particular, in Panel

[5] These are exemplary of the systematic variation in GPA and not meant to be prescriptive of how one models regression discontinuity estimators. For a more flexible environment in which to explore the variation in RD estimates in simulated GPA data, see `https://glenwaddell.shinyapps.io/RD-in-GPA-data/`. (Note that this shiny app includes the top coding issues we discuss in Appendix B.)

[6] In all panels of Figure 1 we fail to reject that the density is continuous (using the test provided in McCrary (2008)). As noted in Frandsen (2017), the McCrary test can over- or under-reject the assumption of smoothness when the running variable is discrete. However, the test proposed for use with a discrete running variable is also inappropriate in GPA data, as it relies on the assumption that the support of the running variable has equally spaced intervals. Grade points themselves are unequally spaced, and combinatorics yields unequal spacing. See Lee and Card (2008) and Kolesár and Rothe (2018) for discussions of standard-error estimation and the inference problems associated with research designs in which treatment is determined by a discrete covariate. This relates to our discussion as GPA should arguably be considered discrete data—especially in small numbers of classes.

D we simulate students between their first and second years of classes, around a GPA of 3.00, where "B" types are equally likely to be on the left and right sides of the discontinuity we estimate (and smoothness *in type* is reconstituted). Clearly, there is a sensitivity in $\hat{\beta}$ that is disconcerting.

We demonstrate the variability in treatment estimates more generally in Figure 2, where we consider placebo tests at every GPA in increments of 0.01. With no treatment anywhere within the data-generating process, confidence intervals should generally include zero. Yet, as students draw additional classes and their GPAs converge to their central tendencies, the risks associated with mean convergence become apparent—point estimates are deviating from true β, and with increasing frequency at larger numbers of classes.[7] In this simulated environment (where we actually know the central tendencies of student types) we see that the biases are largest at the placebo thresholds that tend to separate students, leaving different types on the left and right sides of the cutoff.

2.2 Combinatorics

As GPAs are contributed to by combinations of course-level grades—grade outcomes that we will again have arising from a distribution of potential grades—we begin by deriving PDFs for any number of classes and for varieties of grading curves. We then demonstrate two fundamental mechanisms by which combinatorics sorts students systematically into GPA—differently across grading rules students can experience, and differently across the number of classes over which the GPA is being calculated. In all cases, we will restrict our attention to GPAs measured at a 0.01 level of precision.[8]

The setup

Consider the discretized "grade points" that exist as contributions to the aggregate GPA. For example, the traditional letter grades of $\{\mathrm{F}, \mathrm{D}, \mathrm{C}, \mathrm{B}, \mathrm{A}\}$ can be notated in grade points as

$$\Gamma = \{0, 1.0, 2.0, 3.0, 4.0\}, \tag{1}$$

with student performance presumably mapping into this scale according to some rule or "curve." A curve here, amounts to a rubric by which the distribution of letters is determined, given performance.

[7] With each model in Figure 2 we adopt the optimally chosen bandwidth (Imbens and Kalyanaraman, 2012).

[8] While reporting GPAs at 0.01 precision is common, some institutions report GPA to a precision of 0.001. Adding precision in this way can exaggerate the combinatoric complexity, but does not change the concerning implications of combinatorics as we describe here.

The realization of a letter grade by a student via some curve can be regarded as a stochastic process—probabilities can be assigned to the likelihood that a student will draw each grade-point in Γ.[9] In Figure 3 we produce several probability density functions that define class-level curves—we will use and refer back to these below. In Panel A, for example, we plot the probability densities of potential grade-points for an individual student who faces the letter grades in Γ with probability weights

$$\gamma = \{.05, .10, .30, .30, .25\}, \tag{2}$$

indicating that in a specific course this student has a 25 percent change to earn an A, a 30 percent chance to earn a B, and so on. In the remaining panels of Figure 3 we offer a menu of other potential PDFs over grade outcomes.

In Figure 4 we next consider how combinatorics influences the distribution of potential grade-point averages if a student draws *multiple* grades from Γ according to the probability weights γ. Across panels in Figure 4 we plot the PDFs over GPAs at the end of two classes, at the end of the first and second semesters (4 and 8 classes), and at the end of year two (16 classes) and year four (32 classes). What is made clear in Panel B, for example, is that there is no possible combination of two draws (with replacement) from Γ in Equation (1) that yields anything other than a GPA from the set

$$\Gamma_2 = \{0, 0.5, 1.0, 1.5, 2.0, 2.5, 3.0, 3.5, 4.0\}, \tag{3}$$

where we notate the number of draws (i.e., the number of classes taken) in the subscript. Given probability weights γ (from Equation 2), we can attach weights to each of these GPAs in Γ_2 quite easily—Panel B represents *two* draws with replacement from Γ with probabilities γ. As such, we account for all paths by which one can arrive at the same grade-point average. For example, receiving an "A" in the first class and a "C" in the second combine for a grade-point average of 3.0, as would receiving a "B" in both classes, or a "C" in the first class and an "A" in the second, etc.[10] In the remaining panels of Figure 4 we fill in the potential grade-point averages that can occur with repeated

[9] We remain agnostic about the exact factors leading to these probabilities in practice. Among many factors, the probabilities attached to each letter grade could reflect a random component in the expression of a student's performance (e.g., variability in performance on exams), or could reflect an individual student's performance relative to the set of classmates drawn in a given course.

[10] Below, we will consider variation in the underlying PDFs. Note, though, that throughout our analysis we will be abstracting away from the potential that courses have unequal credit-hour weights, though that would introduce its own source of potential heterogeneity.

draws—the PDFs after one semester (of four classes), one year (eight classes), and so on. By the end of four years of classes (Panel F), we have captured the full probability density function implied by the 376,992 potential grade combinations from 32 draws.[11]

As with many combinatorics problems, seeing through the combinations quickly becomes intractable—this is true even in this simple example, where we do not allow for any variation in Γ or in γ, and do not consider unequal increments in grade point as will occur when we introduce plus/minus modifiers.[12] Though seeing through the problem is challenging, what is important is to recognize that in the complexity of it all there is something *systematic* about the evolution of GPA for individual students over time, and in the resulting variation it produces in GPAs across students. Given this mechanistic component to how students arrive at given GPAs, we should be mindful in comparing even proximate GPAs without regard for the potential heterogeneity within this common measure of student performance.

Non-random sorting of students into GPA

Given the impact of combinatorics on the evolution of a given student's potential GPAs, we now explore how heterogeneous groups of students will sort into GPAs as they accumulate classes. We will consider two natural sources of variation in the accumulation of grades: (i) variation in the grading curves students are exposed to, and (ii) variation in the number of classes students have taken. One can then attach to either source of variation *any* non-random selection of students and arrive at particular examples that give empirical context to the sorting problem. (For example, if students of varying ability levels sort into classes with different curves, or if students of varying ability levels sort into taking additional classes, comparisons across even similar GPA are confounded.)

Variation in grading curves

[11] Specifically, if there are k potential grade points in Γ, there are k^n grade permutations that a student can receive over a sequence of n classes. As an unordered sample with replacement, the set of possible grade-point combinations is then

$$\binom{n+k-1}{k} = \frac{(n+k-1)!}{k!\,(n-1)!}\ .$$

The set resulting from this operation is referred to in combinatorics as a multiset. See Brualdi (2009) for more on multisets and their properties.

[12] Were we to add the traditional plus/minus modifiers to Γ over 32 classes, the resulting combinatorics problem yields 51,915,526,432 unique combinations of grade—the distribution of potential GPAs also fills in faster than in Figure 4, with positive weight on 430 of the 431 GPAs from 0.00 to 4.30 by the end of 32 classes. In the simpler example, with only five distinct letter grades, at two-decimal places of precision there is positive weight on 145 of the 431 two-digit GPAs between 0.00 to 4.30.

In Figure 3 we produced the probability density functions that define several class-level curves. In Figure 5 we reconsider these in various pairwise comparisons, as though there are two types of student in the data-generating process and one is interested in how the probability of one or the other type being at a given GPA changes across GPA. In all cases, we compare densities at the end of one year of (eight) classes for two types of student, across GPAs of 0.00 to 4.30 (in 0.01 increments). For example, in Panel A of Figure 5, we consider the density of student types when one type experiences plus/minus grading and the other faces a similar distribution but without plus/minus grading (i.e., comparing panels A and B of Figure 3). In Panel B we consider student type across GPA when both types experience "triangle" distributions but with different modal grades (i.e., comparing those in panels C and D of Figure 3). That the probability that a student is one type or the other changes across GPA, generally, is to be expected. However, as each underlying PDF dictates its own combinatorics, what is noteworthy across panels in Figure 5 is that this probability does not change smoothly across GPA. As *smooth* changes in the composition of students are more easily accommodated, this non-monotonicity will be troubling. Through the combinatorics of GPA, variation in the curves experienced can trigger complex and irregular variation in the types of student occupying neighboring GPA—to assume that two students with "similar" GPAs are actually similar in type is at odds with combinatorics.

Variation in the number of classes

In Figure 6 we consider one of the comparisons of Figure 5 (i.e., Panel C) while varying the number of classes explicitly. As the number of classes increases the pattern of non-monotonicity clearly changes—while there are still non-monotonicities evident after two years of classes (i.e., 16 classes), by the end of three years of classes (24), student type is changing monotonically across almost all GPAs.[13] What becomes apparent, generally, is that the non-monotonicities induced by combinatorics are a "small n" problem, of sorts, but where n is the number of classes. In education environments, however, where important decisions are often made well before 24 classes, this is not easily ignored. Major choice, for example, will typically occur well before students have taken enough classes to not worry about this "small n" problem. Moreover, schools and departments might admit students who earn a GPA of 2.8 or higher across very few introductory courses. For example, Bleemer and Mehta (2020) exploits UC Santa Cruz's 2.80 GPA threshold over three introductory economics classes to evaluate the return

[13] Small non-monotonicities actually occur 7 times in Panel D (out of a possible 430 GPAs), but only at extremely high (e.g., 4.24) or low (e.g., 0.14) GPAs. Further, at these non-monotonicities the fractional change in type is vanishingly small (i.e., on the order of 10^{-22}).

to an economics major. The number of classes required to smooth out the distributions of student type will depend on the underlying grading distributions, of course. However, as a general rule, the less overlapping are the grade distributions of types of student (i.e., the less overlap there is in the potential grades that an H or L type draw from) the sooner we see the relationship between GPA and average ability become smooth.

In Figure 7 we consider variation in the number of classes from a different perspective—comparing students who have accumulated a slightly different number of classes at several benchmarks in their academic careers. In so doing, we assume that both students face the same PDFs governing class-level grades and differ only in the number of classes they have taken.[14] Across panels, we mimic what we anticipate researchers or practitioners doing—assuming that two students are comparable at various points in time during their tenures, without regard for the number of classes they may have taken. For example, in Panel B we consider two students who are both at the end of one semester of coursework with one of them having taken four classes while the other having taken five. Likewise, in Panel C we consider two students who are both at the end of one year of coursework, but with eight or nine classes contributing to their GPAs. At each GPA between 0.00 and 4.30 (in 0.01 increments) we plot the probability that a student with that GPA has taken nine classes. If this probability was smooth through the range of GPA, or even locally smooth in places, we would be less concerned—again, we are used to accommodating smooth changes in the fraction of students who are of a particular type by simply controlling for GPA (e.g., as the running variable in an RD). However, we again see that the combinatorics of GPA yields a significant amount of troubling variation in student type across the domain space of GPA given this DGP. Across two otherwise-identical students, even taking one additional class can fundamentally change which GPAs are even possible.[15] Given the resulting non-monotonicity, if there is any degree of non-random selection into taking an extra class here or there in one's tenure, then we should anticipate that these students are selecting into distinct sets of potential GPAs—this requires attention beyond our typical approach to policy evaluation.

And, if different types of student select differentially on these margins?

In figures 3 through 7 we offer examples of a more-general problem associated with interpreting GPAs.

[14] In Figure 7 we use the probability density functions from the 13-point grade distribution in Panel D of Figure 3. However, the results are representative of the comparison one could make for any class-level PDF.

[15] While we do not show this, one can produce evidence of a similar problem arising from students taking different numbers of credit hours.

In the end, as the sorting of students into GPA is governed by combinatorics, if there is any meaningful heterogeneity across students that correlates with either the grading regimes they experience or the number of classes they've taken by a point in time, then we should expect that inference that relies on comparisons (or similarity) of GPA will be challengeable by combinatorics-induced sorting.

While the above figures make pairwise comparisons, and in that way capture the roles of various contributors as comparative-static exercises, it's more likely that students select into a variety of classes, with different curves (i.e., different PDFs). In Figure 8 we plot the proportional breakdown of many students and types. In Panel A there are six types of student, for example, with each having experienced classes like those we imagined earlier Figure 3. Again, non-monotonicity is evident. In Panel B we go further, imagining that there are 13 student types, each having a different modal grade but drawing from a similarly shaped PDF (i.e., triangle distributions akin to those in Panels C and D of Figure 3). Even as we increase the number of student types populating the GPA domain, discrete changes in student composition across local changes in GPA persist.

3 Evaluating GPA-determined treatment

3.1 A setup

Given the construction of GPA, and the non-random sorting into local GPAs in particular, identifying unbiased estimates of treatment in GPA data is non-trivial—this is especially true the more local is the identifying variation. As demonstrated in Section 2, a clear violation exists in designs where one relies on smoothness around a treatment threshold, for example, and the non-monotonicity in student type across GPAs should give pause as we consider experiments with GPA as a running variable. (Moreover, with the typical interest being to collapse on *smaller* bandwidths where power allows, one might be particularly concerned that the implications of combinatorics around treatment thresholds could lead to questionable inference from well-powered RD designs.)

In the sections below we consider two thought experiments. First, we consider the scenario we feel is more relevant in practice—unobservable student heterogeneity, where their type is only learned over time as they draw from better and worse grade distributions. Second, we consider the scenario where type is observable—here we will assume that students draw from *the same* grade distribution, but will have some students take more classes. This will both demonstrate the importance of considering the

component parts of GPA and solidify some intuition around the ways in which identifying treatment in such an environment might be salvageable.

In both scenarios we envision a data-generating process in which there are two types of students who differ in their average outcomes—we keep with the idea that in the population of students there are L types and H types. In particular, suppose that each student realizes some outcome—here, we will simulate weekly wages, w_i—according to the simple process,

$$w_i = \alpha + \delta \mathbb{1}(\mathrm{H}_i = 1) + e_i \ . \tag{4}$$

The parameterization of (4) will be immaterial, so we roughly mimic the 25^{th}, 50^{th}, and 75^{th} percentiles of weekly incomes among college graduates in the United States in 2020, assuming that H types experience δ-higher average wages.[16] Other than from the level differences in outcomes associated with being an H or L type, then, wages vary randomly. To be clear, there is no treatment-induced variation in outcomes, so we are in an environment in which well-identified models should fail to reject the null hypothesis that there is no systematic discontinuity in outcomes. Nonetheless, adopting traditional methods in such an environment exposes one to the risk of identifying a discontinuity in w_i, as combinatorics facilitates a source of non-random selection into GPA by type of student.

3.2 When student heterogeneity is only partially observable

Here, we assume that H types draw grades from a distribution that first-order stochastically dominates that of L types, with modal (mean) grades at the individual class level of 2.3 and 3.7 (2.23 and 2.68), respectively.[17] In what follows, we produce regression discontinuity estimates from 200 simulations of 5,000-student panels, inquiring into whether there is evidence of a systematic discontinuity in outcomes for those with GPAs of 2.50 or above. While we consider a GPA cutoff of 2.50, the issues we illustrate generalize to any GPA where students of different type are present. We choose 2.50 as it falls between

[16] Assuming $\alpha = \$1,133$, $\delta = \$566$, and $e_i \sim N(0, 300)$ centers our DGP on the weekly incomes of college graduates, approximating the first quartile ($977), median ($1,416), and third quartile ($2,110) of weekly income of college graduates, according to the Usual Weekly Earnings of Wage and Salary Workers section of the Current Population Survey. (See Bureau of Labor Statistics (2020) for details.)

[17] Specifically, L types draw grades from the 13-point plus/minus letters as in Panel C of Figure 3, and H types draw "better" grades, on average, as in Panel D of Figure 3. In other words, L and H types draw from $\Gamma = \{0, 0.7, 1.0, 1.3, 1.7, 2.0, 2.3, 2.7, 3.0, 3.3, 3.7, 4.0, 4.3\}$ with probabilities $\gamma_L = \{0.024, 0.058, 0.073, 0.088, 0.107, 0.122, 0.136, 0.113, 0.095, 0.077, 0.053, 0.036, 0.018\}$ and $\gamma_H = \{0.016, 0.038, 0.048, 0.057, 0.07, 0.08, 0.089, 0.102, 0.111, 0.121, 0.134, 0.089, 0.045\}$, respectively. That the mean GPA from a grade distribution with mode of 3.7 is 2.68 reflects top-coding, as discussed in Appendix B.

the mean grades of L and H types, where we can anticipate that one would need a rule to separate students. As we initially allow students to have taken four classes, in that sense it's fitting to have in mind the sort of decisions that are made around the middle of the first year of college (e.g., major choice) or policies that tend to be most binding in the first year of college (e.g., the initiation of probationary status, admittance into greek affiliations, enrollment into professional schools).

In Panel A of Figure 9 we plot the mean point estimate (across simulations) associated with each bandwidth between 0.01 and 0.50 in increments of 0.01. The absence of any treatment in the DGP should have us anticipate estimated discontinuities of zero. However, consistent with combinatoric sorting, there is significant sensitivity evident in the estimated treatment effects at smaller bandwidths. In fact, the bias tends to be both large and sensitive to changes in bandwidth, and is directly resulting from combinatorics-induced violations of the required smoothness assumption. In an environment where average weekly wages are \$1,416, RD estimates across bandwidths range from \$-105 (-0.25σ) to \$47 (0.11σ).

In Panel B of Figure 9 we see how this sensitivity arises—the combinatoric sorting of different types of student around the treatment threshold. In particular, in Panel B we plot the fraction of students who are H types across the same range of bandwidths (0.01 through 0.50) both to the left- and right-hand sides of the 2.50 cutoff. The lumpy introduction of H and L types included in the sample as the bandwidth changes is clearly evident, and around small bandwidths in particular—other than noise, this imbalance is the only factor that drives estimates away from zero in Panel A. We see this from a different perspective in Panel C, where we plot counts of student type across bandwidths. This makes the source of the lumpiness is particularly evident. While there are bandwidth adjustments that do not trigger changes in the number of observations at all—by its nature, combinatoric sorting will leave behind GPAs that are not occupied—when adjusting the bandwidth allows "new" GPAs into the estimator there are discrete changes in the number of H and L types. It is at these same GPAs that mass shifts discretely to one type of student or the other—this abrupt tipping of the balance one way or the other explains fully the change in point estimates across bandwidth.

Note, importantly, that even with combinatorics playing an active role in facilitating the non-random sorting of student types into GPA, the overall density of students can still itself to the appearance of smoothness in the aggregate. In fact, our DGPs routinely pass standard tests for changes in the density of students around the threshold (i.e., McCrary 2008; Frandsen 2017).

In Figure 10 we produce similar plots of bandwidth sensitivity tests as students take additional classes, mimicking their progress through the institution and the potential for similar evaluations being performed at the end of two classes, one semester, one year, and two years. This highlights that the non-random sorting is particularly egregious when students have taken few classes. That said, estimates are sensitive to combinatorics at smaller bandwidths through the end of one year of classes (in Panel C). Notably, it's here where the tell-tale signs of mean convergence appear, albeit to a lesser degree than in Panel D where students have taken even more classes. This reveals a general pattern in the evolution of GPAs—while the combinatorics problem diminishes with additional classes, student populations also converge to their mean performance. Thus, the domain of GPAs over which there is overlap (i.e., in student type) is getting increasingly narrow. While the smallest bandwidths in Panel D evidence "zero" treatment effect, by the time students have taken sixteen classes the degree of mean convergence experienced is such that larger bandwidths now use increasingly different types of student to identify the discontinuity.[18] As one should anticipate, this results in biased point estimates.[19]

In Figure 11 we demonstrate how the uneven distribution of student types changes as they take additional classes. Similar to the lumpiness we saw in Figure 9, the problematic influence of combinatorics on treatment evaluation shows up where we anticipate it (i.e., panels A and B, and to some extent C) and disappears as combinatorics present less of a first-order concern (Panel D). At sufficiently large numbers of classes—16 in our data-generating process—it matters less that GPA is the product of a combinatorics problem.

In the end, the roles of combinatorics and mean convergence are clearly in tension as students progress through college. The degree to which this tension is felt should inform bandwidth choice. At smaller numbers of classes, estimates are more sensitive to combinatorics—here, larger bandwidths can act as a mitigating device, because they leave parameter estimates less sensitive to the *types* of student at particular GPAs. However, at larger numbers of classes combinatorics induces less local variation in student type and smaller bandwidths become appropriate—here, larger bandwidths expose researchers to lost comparability. Overall, considering the evaluation of treatment that falls on students around some number of classes, the researcher's choice of bandwidth is implying something of a tolerance for combinatorics-related bias over the bias induced by mean convergence. We summarize this tension as follows, highlight the particularly concerning source of bias.

[18] Recall Figure 6, where by the time students had drawn 16 classes there was no smoothness violation.
[19] We saw similar evidence in Figure 1, for example.

The first-order source of bias in RD designs that use GPA data

	With fewer classes?	With more classes?
With smaller bandwidths?	Combinatorics	
With larger bandwidths?	Combinatorics and mean convergence	Mean convergence

By choosing a smaller bandwidth we are down-weighting the potential that mean convergence will bias $\hat{\beta}$ while at the same time up-weighting the bias from combinatorics. In choosing larger bandwidths we are down-weighting the potential bias associated with combinatorics while at the same time up-weighting the potential bias from mean convergence. When researchers weigh these tradeoffs, the variation in the number of classes taken by students in the population under study cannot be ignored—employing a small-bandwidth design when students have taken many classes implies a lower overall potential for bias, but the equivalent small-bandwidth design will be much more vulnerable to bias when students have taken few classes.[20]

3.3 Does the problem go away if student heterogeneity is observable?

Before moving on to consider potential solutions to the above problem, there is good intuition in considering how the classification of students into type can move us toward better identification. In this section, we recast the problem as one in which both types draw repeatedly from the same grade distribution, but H types simply draw one extra grade (i.e., they take classes at a faster rate) than L types. In this experiment we shut down entirely on any heterogeneity coming from grades themselves.[21] This highlights the problem that can arise simply due to the mechanistic sorting of combinatorics, which allows some students to populate GPAs that other students simply cannot. We will then consider the implications of controlling for the "number of classes" in regression analyses.

[20] Another important consideration for researchers navigating this tradeoff is that the bias stemming from mean convergence is likely signable. For example, to the extent we've populated the right-hand side of a discontinuity estimator with H types who attain better average outcomes for reasons not associated directly with treatment, we expect $\hat{\beta}$ to be biased up. However, the non-monotonicity in student type introduced through combinatorics implies that there can be discretely more or less of one type on the left or right of *any* threshold. This leaves the resulting bias unsignable, and any associated inference more uncertain.

[21] All students draw all grades from a triangle centered at 2.7.

Given that we rarely control for the number of classes contributing to a student's GPA, the results of this exercise are quite striking. In Figure 12 we estimate "discontinuities" in outcomes at a variety of GPAs (i.e., 2.30 though 3.10 in increments of 0.10). There should be no discontinuities in outcomes in this environment—here there will be, as they are left behind by the combinatoric sorting of students into GPA simply through the inclinations of H types to take one more class.[22]

Consistent with combinatoric sorting, the bias is unsignable in general and particularly problematic at smaller bandwidths where the density of student "types" can be different either side of a given GPA and can change abruptly as different bandwidths allow different GPAs to populate the estimator. Across the nine thresholds we illustrate (between 2.30 and 3.10), the mean bias in point estimates is positive in four of them and negative in five of them. However, in all but two of the nine placebo thresholds the point estimate itself changes sign across bandwidth.[23] Given the combinatorics of grade-point averaging, not controlling for the number of classes that contribute to GPA is clearly problematic. As the number of classes is observable—while not often used, it often is observable—in Figure 13 we plot the bandwidth sensitivity around one of these placebo treatments (a GPA of 2.50, in this case) with and without controlling for the number of contributing classes. Even though students are drawing from the same distribution of grades, combinatorics allows the number of classes taken to transmit through to the set of GPAs H types are able to occupy. Consistent with the omitted variables bias, controlling for the number of courses also resolves the bias in estimated treatment.

While we've designed a simple experiment here, where student type is perfectly inferable through one observable attribute, the reality is that field data rarely presents such a clean opportunity to merely control for differences. Fortunately, however, while combinatoric sorting will defeat local estimators, it is also ripe for the more-sophisticated remedies made available through machine learned approaches to prediction, even when student heterogeneity is not easily inferable. Thus, having documented the potential pitfalls associated with the underlying combinatoric sorting of students into GPAs, below we turn to considering the ways in which the variation in GPA is actually learnable. In short, while

[22] Moreover, our simulated environment suggests that the bias can be large in magnitude. Granted, we've constructed this data environment. However, in so doing we've matched mean weekly wages (in 2020 for US college graduates) and 25th and 75th percentiles while assuming a level difference in wages of $566 for H types. In this environment, mean wages are $1,416, and RD estimates across the cutoffs and bandwidths range from $-1,365 to $483—the associate impacts range from -96.3 percent to 34.1 percent at the mean. That is, the bias associated with not accounting for the number of classes is roughly five times as large as the largest bias we found from not controlling for the difference between a C- student (drawing grades around 2.3) and an A- student (drawing grades around 3.7) when the number of draws was common.

[23] Only in the estimation of a discontinuity at 2.80 and 2.90 do we find the point estimates never changing—not as a rule, of course, but in this DGP.

the nature of GPA (and the combinatoric problem, in particular) has been challenging thus far, it also leaves behind evidence that can be learned—in effect, evidence that can be used to salvage better inference.

4 Machine-learned approaches to classifying students

The above analysis suggests that we should clearly not anticipate that student ability is smooth across *local* changes in GPA—the non-monotonicity evident above makes it easy to demonstrate that average student ability can even *decrease* in response to small increases in GPA. Moreover, where estimators rely on local smoothness at some threshold, the combinatorics of GPA can be of first-order importance, especially when we fail to recognize that *local* variation in GPA does not control well for student heterogeneity. However, decisions made at marginal considerations of GPA are fundamentally relying on a classification, of sorts. As problematic as GPA can be as a measure of performance—having more-complex variation than we had recognized—it is still *systematic* in its construction. As such, sophisticated methods, instead of succumbing to the complexity, find it something to be learned.

4.1 Predicting outcomes using GPA and course-level grades

In Figure 14 we compare different approaches to modeling the data-generating process of Section 3—a distribution of wages within which there are two level-different types. In all panels we plot the true average wage and the *predicted* wage for each GPA (again, at 0.01 precision). In panels A through C we consider linear approaches to this exercise, projecting w_i onto GPA_i across a variety of polynomials. With enough flexibility linear models can eventually track the *global* non-linearity in outcomes across GPA fairly well. However, even a ninth-order polynomial does a fairly poor job of capturing *local* changes in GPA.

As an alternative to these methods, in Panel D we plot the predictions from random forest regressions of w_i on GPA_i.[24] To be clear, in Panel D we restrict the model to only the single covariate (i.e., GPA) and, even then, the flexibility offered by the random forest is evident—the estimated relationship tracks the true pattern of average wages so well that it is difficult to distinguish the two lines by visual inspection. (We use a dashed line to represent the prediction to help somewhat with this inspection.) Being fully nonparametric, at every GPA the random forest yields a predicted $\hat{w}_i$ that

[24] See Breiman (2001) for details of the random forest algorithm as it applies to regression problems.

is independent of the predictions at surrounding GPAs. In this setting, then, this flexibility improves performance markedly and the non-monotonicity in the data is as evident in the predictions of the model as is it in the underlying DGP. (This bodes well for our return to considering causal estimation in this environment.) Here, we know that this non-monotonicity is driven by varying types of student who are sorted into GPAs through combinatorics. While field data would not afford the same ability to see the source of heterogeneity, a similar exercise with field data would nonetheless capture variation in outcomes that are predictable through the learned combinatorics of GPA—and to the extent learnable, they would mitigate the problems we identify.[25]

However, there is little reason to limit the RF environment to learning simply through GPA when the individual contributors to GPA were available. The obvious return to the addition of course-level information will be through the RF learning to distinguish systematic heterogeneity in outcomes even from students who have *the same* GPA. Without transcript-level information, RF learning is restricted to making one prediction for each GPA, while variation in course-level grades within given GPA is able to predict *different* outcomes for those who have the same GPA but arrived at it differently. In Figure 15, then, we consider the addition of course-level information and the RF's ability to predict outcomes of H and L types. As we know each student's type—to be clear, the RF does not—we report predicted outcomes separately for H and L types.[26] The RF learner can distinguish heterogeneous outcomes among students who share the same GPA—predicted outcomes are higher for H types than for L types more than 90 percent of the time.[27]

4.2 Can we exploit this learning in a causal environment?

Above, we have demonstrated the suitability of machine-learned approaches to disentangling the underlying combinatorics within GPA, inclusive of identifying student heterogeneity even among those who share *the same* GPA—we identify heterogeneity through variation in the combination of grades that got them to that GPA, essentially. Thus, it is natural to consider the performance of companion

[25] In Appendix A we report quantitative measures of model performance across 1,000 simulations of the above process.

[26] We only include GPAs for which there are students of both H and L type to compare.

[27] In simulating this process 1,000 time at the end of one semester of classes, the RF with GPA and course grades predicts higher average wages for H types at 78.1 percent of GPAs. After one year of classes, the RF predicts higher outcomes for H types at 90.8 percent of GPAs, and after two years of classes predicts higher outcomes for H types at 92.9 percent of GPAs. (Again, these values are based on only the inner-95 percent of the data according to GPA. When we use all of the data, there is a marginal decline in performance, to 70.9, 85.7, and 91.0 percent, respectively at one semester, one year, and two years of classes. This decline is a consequence of the sparsity of students at extremely high or low GPAs, which limits the RF models' ability to distinguish students.)

methods in a causal framework.

Here, we simulate post-graduation wages and consider the performance of causal forests with respect to their ability to retrieve the causal parameter of interest. To do so, we make one addition to the environment we've considered above—a treatment that is experienced in some discontinuous way at a GPA threshold, *but available to all students with some positive probability.* As this "overlap" is required in order to satisfy the identifying properties of the causal forest, we will benchmark the casual forest estimators against fuzzy regression discontinuities.[28]

4.2.1 Causal forests

Introduced in Wager and Athey (2018), the causal forest (CF) is an application of machine learning to causal inference in the presence of randomized treatment. CF procedures build on the strengths of random forest learning, which is known to work well as a classifier (Hastie et al., 2017). In particular, these strengths include flexibility in modeling nonlinear processes, and an ability to handle large numbers of covariates. By leveraging these strengths, it is also notable that the causal forest estimates *individual* treatment effects—heterogeneity in the effect of treatment across individuals is then likewise identifiable. A causal forest, then, is simply an algorithm that leverages a random forest's classification strength to group together those observations that are good counterfactuals for each other—having grouped them, one can then estimate the effect of treatment within those groups.[29] In our working example above, given course-level information the RF procedure produced different predicted outcomes, on average, for the two types of student in the DGP—this was true *even when they had the same GPA*. Likewise, then, the RF component within the CF estimator will exploit that there are multiple (and learnable) paths by which students arrive at given GPAs and thereby better estimate their counterfactual outcomes would have been—their outcomes in the absence of treatment.

With identifying assumptions met, a causal forest estimates a treatment effect at every set of covariates x by estimating the mean difference in the outcomes of treated and control units who have

[28] This is similar in spirit to asking if there are discontinuities in outcomes at particular GPAs, but in an environment where we likewise simulate placebo treatments with discontinuities in the odds of experiencing treatment. (As the discontinuity in the assignment rule is assumed to be independent of student heterogeneity, the consequences of mean convergence and combinatorics demonstrated in Section 3 replicate in this "fuzzy" setting.)

[29] In so doing, a CF finds similar groups within which treatment-effect variation is reduced. Of course, since potential outcomes are not observable, neither are treatment effects. However, Athey and Imbens (2016) demonstrates that maximizing the heterogeneity of estimated treatment effects between groups, subject to a penalty for within-group variance, is equivalent to minimizing the expected MSE of treatment effects within groups.

those covariates in common. Given this condition, the difference,

$$\tau(x) = E[Y_i^1 - Y_i^0 \mid X_i = x] \,, \tag{5}$$

is commonly be referred to as the *conditional average treatment effect* (CATE), as it is conditional *on a set of covariates*. As we allude to above, we adjust our DGP somewhat to meet the identifying assumptions—"unconfoundedness" and "overlap." Wager and Athey (2018) defines unconfoundedness as the independence of treatment assignment from outcomes, conditional on covariates. This is similar to the conditional independence assumption discussed at length in Angrist and Pischke (2009). Likewise, the overlap assumption should be familiar from matching-type estimators more generally, as the overlap assumption for the CF requires that $0 < Pr(W_i = 1|X_i) < 1 \; \forall \; X_i \in \mathbf{X}_i$. That is, in order to estimate the effect of treatment in our context there must be treated and control observations *within* each GPA. Of note, then, is that the overlap assumption implies that a causal forest cannot estimate treatment in a classic "sharp-RD" design, where the probability of assignment into treatment is zero on one side of a threshold and one on the other.[30]

Borrowing from the weekly wages we simulated in Section 4.1, here we augment our data-generating process to satisfy unconfoundedness and overlap by introducing a degree of noise in treatment assignment. Specifically, those at GPAs below the threshold now randomly experience treatment with 15-percent probability, while those above the threshold randomly experience treatment with 75-percent probability. In Panel A of Table 1 we evaluate the performance of a regression-discontinuity estimator employing either an optimal bandwidth (Imbens and Kalyanaraman, 2012) or a smaller bandwidth (defined as 10 percent of the optimal bandwidth)—both largely fail to identify that the average treatment effect is zero.[31] This is the benchmark against which it is informative to compare the performance of a causal forest.

[30] Other common situations can also violate the overlap assumption. For example, if students with an F on their transcript are ineligible for treatment then the overlap assumption would be violated in a causal forest that ran on course-level grades. In such cases one can, of course, resurrect the internal validity of the estimator by limiting the sample to that for which there is overlap.

[31] At the end of one semester (i.e., in Column 1), the RD estimator with an optimal bandwidth leads to a rejection of the null hypothesis in 31 percent of iterations, with an average increase in weekly wages of \$21.91 in weekly wages ($.05\sigma$) relative to the mean wage of \$1416.At the end of one and two years, this rejection rate increases to 56 percent of iterations (0.09σ) and 61 percent of iterations (0.11σ). That the bias is positive on average is merely an artifact of the data-generating process and chosen treatment threshold—the student types at the threshold we consider just happen to split with more H types on the right-hand side of 2.50. At smaller bandwidths, we again see over-rejection of the null hypothesis and biased point estimates, but the bias interacts with the number of classes differently. Applying smaller-bandwidth designs to the same data we reject the null in 13 percent of iterations (0.13σ) at the of one semester, in 8 percent (0.11σ) at the end of one year, and in 6 percent (-0.003σ) at the end of two years.

4.2.2 The CF model appropriately rejects the $\beta = 0$ null where the RD over-rejected

In Panel B of Table 1 we reconsider the same data as was drawn for the exercise in Panel A but with a CF procedure performing the same task.[32] We do this across two models, including only GPA in the first model and then adding individual course grades in the second.[33] Notably, where there is nothing introduced into the data-generating process other than the naturally occurring combinatorics-induced imbalance of type, RD-type methods falsely identify "treatment." (RD methods reject the null 31 percent of the time after one semester, 56 percent of the time after one year, and 61 percent of the time after two years.) CF procedures, however, retrieve treatment estimates that appropriately reject the null (i.e., reject that $\beta = 0$ only five percent of the time). Whether at the end of one term, one year, or two years, the estimated effect sizes identified in the RD estimates are also orders of magnitude larger than those identified in the CF procedure (which are hovering around zero, and less than $.0005\sigma$, on average).[34]

For a more-direct comparison to the regression discontinuity benchmarks, in Panel C Table 1 we consider CF performance when the samples are restricted to observations within the bandwidths we imposed on the RD estimators (in Panel A). Again, the CF estimators do not over reject the null.

4.2.3 The CF model also informs us of underlying heterogeneity in treatment

A unique strength of using a causal forest is that its estimation of conditional average treatment effects can retrieve underlying heterogeneous in treatment effects. Since treatment effects are identified up to every unique x in the data, a causal forest will explore whether treatment effects vary systematically across any combination of covariates. (Indeed, Athey and Imbens (2016) is motivated by a desire to identify such heterogeneity.) This is particularly useful in the setup we've developed, where the "types" of student might benefit differentially from treatment.[35] Here, we allow H types to respond

[32] As in Panel A of Table 1, then, each simulation includes 30,000 students, with grades drawn from the same distributions as in Section 4.1, with H types are again a level-difference higher than L types.

[33] In the random forests of the previous section, we saw evidence that a machine-learned estimator could find heterogeneity between students using only course grades. However, as GPA determines treatment in our DGP and H types earn higher GPAs, on average, unconfoundedness requires that we condition on GPA. (Since type is correlated with GPA, and therefore with treatment assignment, the unconfoundedness assumption is only satisfied if GPA is included in the explanatory variables provided to the CF. This is similar in spirit to including the running variable in RD estimation—capturing the relationship between the running variable and the outcome of interest that exists independent of treatment.)

[34] These CF estimates translate to an estimates treatment effect of roughly \$0.20 over a standard deviation in observed wages of roughly \$412. The most-conservative RD estimates imply a \$20.00 "increase" in wages.

[35] With a CATE for every set of x in the data, one can also then test for heterogeneity in treatment effects. The formal test for heterogeneity in estimated treatment effects is an implementation of the best linear projection method for detecting treatment heterogeneity in machine learning estimates, proposed by Chernozhukov et al. (2020).

to treatment—H types receive a $300 increase in weekly wages coincident with treatment. L types, though exposed randomly to treatment as in our baseline DGP, experience no benefit to treatment. In this way, the true parameters are $\beta_H = 300$ and $\beta_L = 0$.

In Panel A of Figure 16 we plot the distributions of CATE estimates for our two causal forests in the presence of this treatment-effect heterogeneity. Assignment into treatment is unconfounded so long as we condition on GPA, meaning that the CF algorithm can identify collections of individuals with the same underlying propensity for receiving treatment (i.e., good counterfactual groups). Thus, the first CF learner—the one provided with only GPA—still produces unbiased CATEs, even in the presence of treatment effect heterogeneity. But with only GPA as a covariate, the CATEs from this first CF can only recover the average across H and L types at a given GPA.[36] However, given that expected student type changes across GPA, even this CF demonstrates some ability to detect the heterogeneity in β_H and β_L. In the left of Panel A, many students assigned to low treatment effects are in fact low types, reflecting the fact that most "low" GPAs are associated with L types. Likewise, "high" GPAs are associated with H types, on average.

In contrast, the right side of Panel A shows that providing the CF with individual course grades allows it to identify heterogeneity much more cleanly. When provided with course grades, the students with CATEs near zero are overwhelmingly L types and the students with CATEs near 300 are overwhelmingly H types. As with RF classifiers, the CF has minimized the variance of within-group treatment effects when splitting students into groups—given that there are two types of student in our DGP drawing from different distributions of grades, transcript-level grades make the two types distinguishable.[37] That is, the addition of course grades allows the CF to make distinctions between students with the same propensity for receiving treatment (i.e., students at a certain GPA). When the causal forest ultimately groups these students together to estimate treatment effects, it is effectively

[36] Wager and Athey (2018) demonstrates that when the estimator's identifying assumptions are met (i.e., overlap and conditional unconfoundedness) each $\hat{\tau}(x)$ is point-wise consistent, asymptotically Gaussian, and centered in the sampling distribution. As such, the CATEs can be aggregated to estimate the average treatment effect with consistency and conduct valid hypothesis testing. (In an implementation of the causal forest estimator, Athey et al. (2019) constructs average treatment effects using augmented inverse probability weighting (AIPW). As noted therein, this method generally leads to estimates of average treatment effects that are more accurate than naive averages, based on results from Chernozhukov et al. (2018). We use the implementation of AIPW in Athey et al. (2019) to produce average treatment effects.

[37] We find that, in general, random covariate sampling of roughly 75 percent of available grades allows the CF algorithm to identify heterogeneity in the student population.

grouping students by type, and thereby identifying the underlying treatment-effect heterogeneity.[38]

In Panel B of Figure 16 we plot the distributions of CATE estimates associates with two causal forests with placebo treatment—again, we allow only for GPA, and then for both GPA and individual course grades. To demonstrate the CFs handling of the heterogeneity in treatment effect by type, we identify type of each student assigned to each treatment effect. In Panel B, in particular, we again see the value added in using individual course grades as the estimated parameter collapses on the true $\beta = 0$ with the additional ability to learn through course-level variation in grades.

In scaled simulations of the above experiment, we also formally test for heterogeneity in estimated treatment effects. The test is an implementation of the best linear projection method for detecting treatment heterogeneity in machine learning estimates, proposed by Chernozhukov et al. (2020). In 100 percent of simulations across both causal forest models, the heterogeneity identified is statistically significant.

5 Conclusion

We demonstrate the complex ways in which grade point averages can challenge causal identification. We model the process through which students arrive at GPAs, and demonstrate the roles of mean convergence and combinatorics. In the evaluation of treatment, in particular, we show how these two mechanisms tradeoff as students engage in additional classes, and thereby interfere with the interpretation of GPA variation in rather complex ways.

Combinatorics governs the set of feasible GPAs and determines the paths by which a student can arrive at a given GPA. Thus, while collapsing on students with more-similar GPA sounds very much in the spirit of constructing "all-else-equal" conditions, it also exposes estimators to non-monotonicities in student type across GPA. This is especially true where the number of classes is small. In the context of treatment evaluation, we demonstrate that this induces a form of selection through which combinatorics can lead to the students on either side of a given GPA not being comparable. This calls into question the validity of identification strategies that rely on local comparisons. We argue further, that the failure of linear GPA-based estimators is fundamentally due to this type of classification

[38] The fact that both L and H type students have a chance of receiving each letter grade explains why we see treatment effects massed near, but not on, the true β_L and β_H. Since each letter grade has positive probability for both types, so too does each transcript of grades. For instance, an H type student can occasionally draw an unlucky set of grades that would be more common for an L type. This means that every CATE is still an average over both L types and H types.

problem—the combinatorics of GPA require a flexibility in modeling that linear methods are not well equipped for. Machine-learned methods, however, often excel at modeling such spaces.

We've demonstrated that this sort of local non-comparability is ameliorated with additional classes, as the artifacts associated with combinatorics subside with repeated draws from a distribution. However, with additional classes, GPA can also be expected to converge to a student's "true" average grade (i.e., their type, in a way). While this increases the informativeness of GPA (i.e., as a signal-to-noise ratio, the informativeness of GPA is increasing with additional classes), this can also expose estimators to having very different types of student on either side of a given GPA. In the limit, even though the density of students across GPA might appear continuous, there is the possibility of losing overlap entirely. This highlights a trading off in the informativeness of comparing GPAs across the number of contributing classes, with implications for how local comparisons should be interpreted at various stages of students' academic progress.

In the end, we find benefits associated with supplementing traditional methods with machine-learned methods that are capable of exploiting what is learnable through *how* students arrive at given GPAs. For example, the inclusion of transcript-level data (which is easily incorporated into these methods) allows one to distinguish heterogeneity in outcomes even among students at the very same GPA. Similarly, in terms of treatment evaluation, causal forests (Athey et al., 2019) can likewise learn through the combinatorics of GPA to identify not only average treatment effects, but also the heterogeneity in individualized treatment effects.

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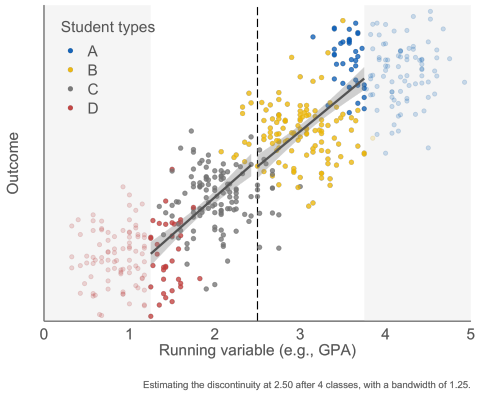
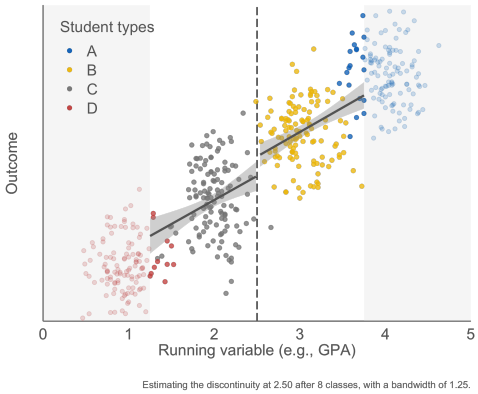
Figure 1: The effect of mean convergence on RD estimates in the absence of treatment

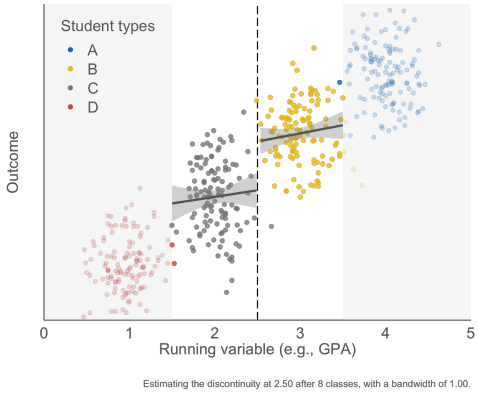
A: After one semester



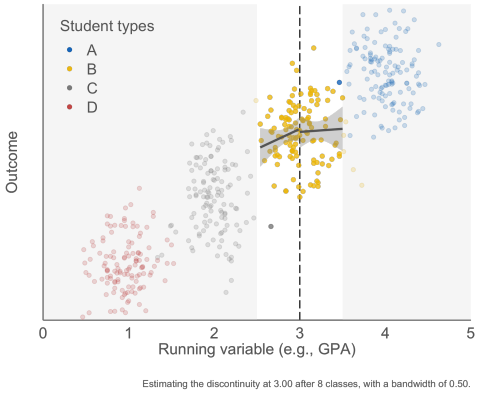
B: After one year



C: After one year with a smaller bandwidth

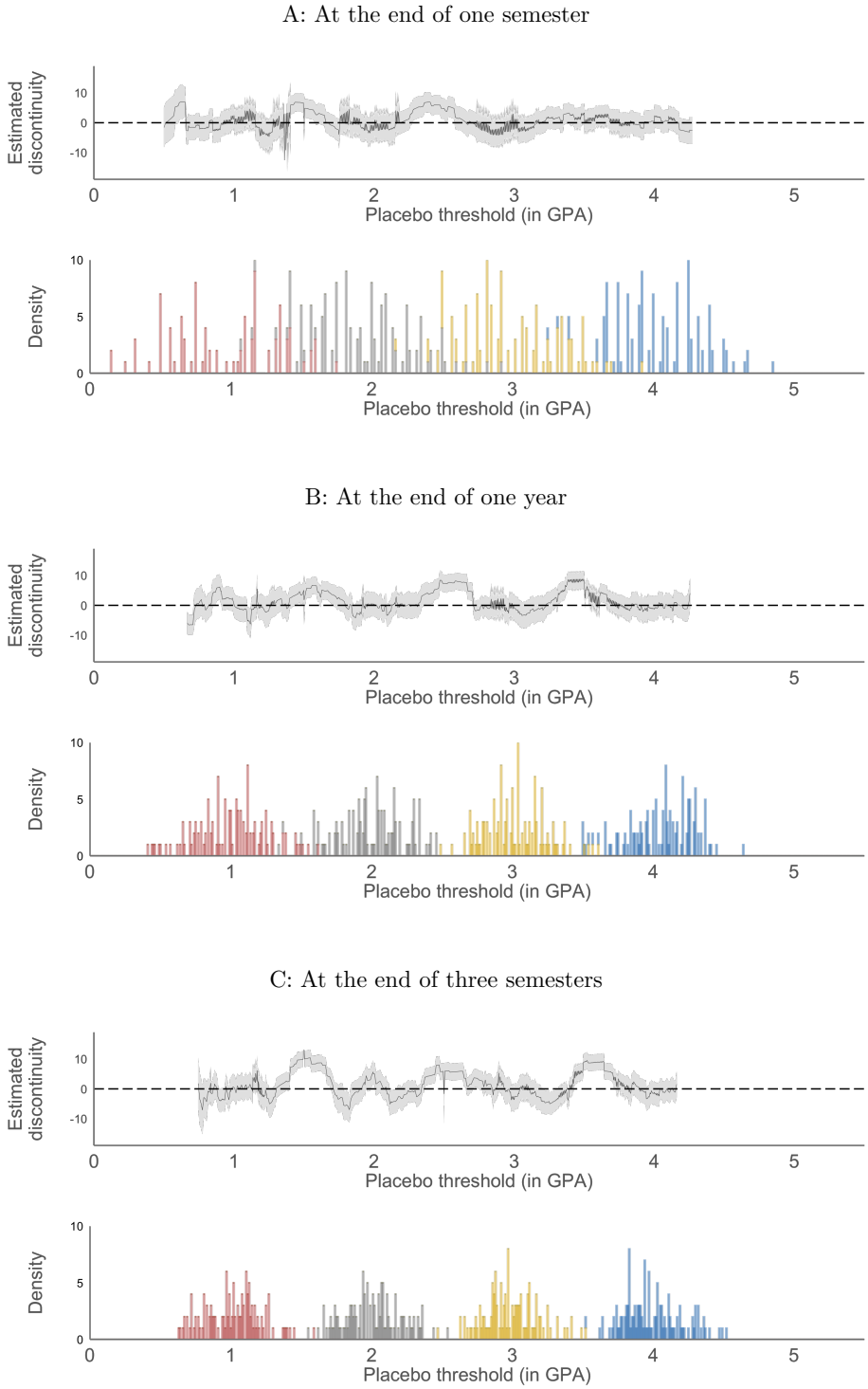


D: After one year with a smaller bandwidth where the threshold "splits" a type of student



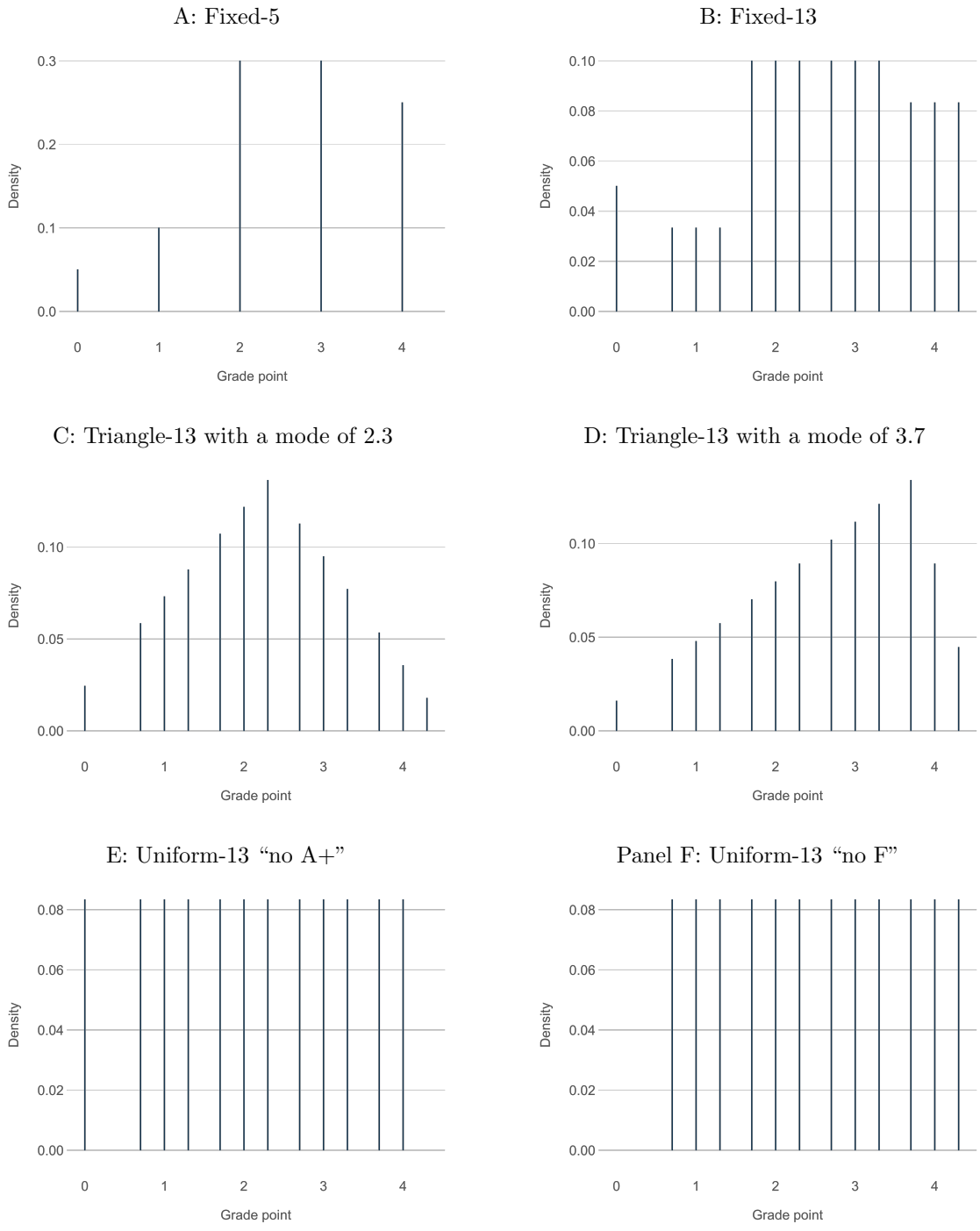
Notes: In all panels, students draw uniformly from the grades that are within ±1 of their median grade, which is anchored by their type. In all panels there are 125 students of each type (i.e., $n = 125 \times 4 = 500$ students). Students take four classes per semester and two semesters per academic year. See Section 2.1 for related discussion. (For a more flexible environment in which to explore the variation in RD estimates in simulated GPA data, see `https://glenwaddell.shinyapps.io/RD-in-GPA-data/`.)

Figure 2: Placebo tests across GPA in the absence of any treatment (i.e., true $\beta = 0$)



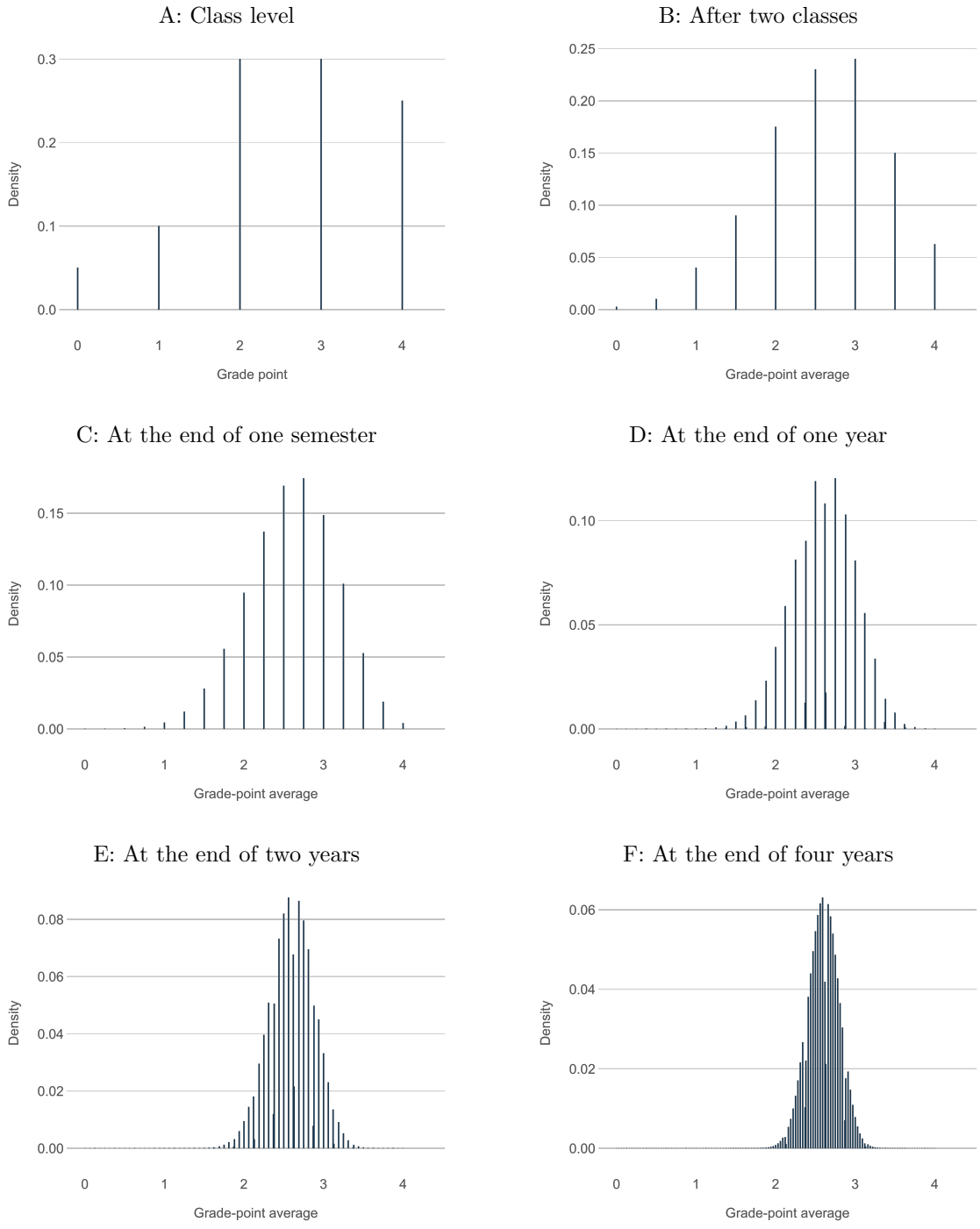
Notes: In all panels, students draw uniformly from the grades that are within ± 1 of their median grade, which is anchored by their type. In all panels there are 125 students of each type (i.e., $n = 125 \times 4 = 500$ students). Students take four classes per semester and two semesters per academic year. See Section 2.1 for related discussion.

Figure 3: The combinatorics of GPA: PDFs of different grading curves



Notes: In all panels, the probability densities (at the class level) are identified in the panel titles. See Section 2.2 for related discussion.

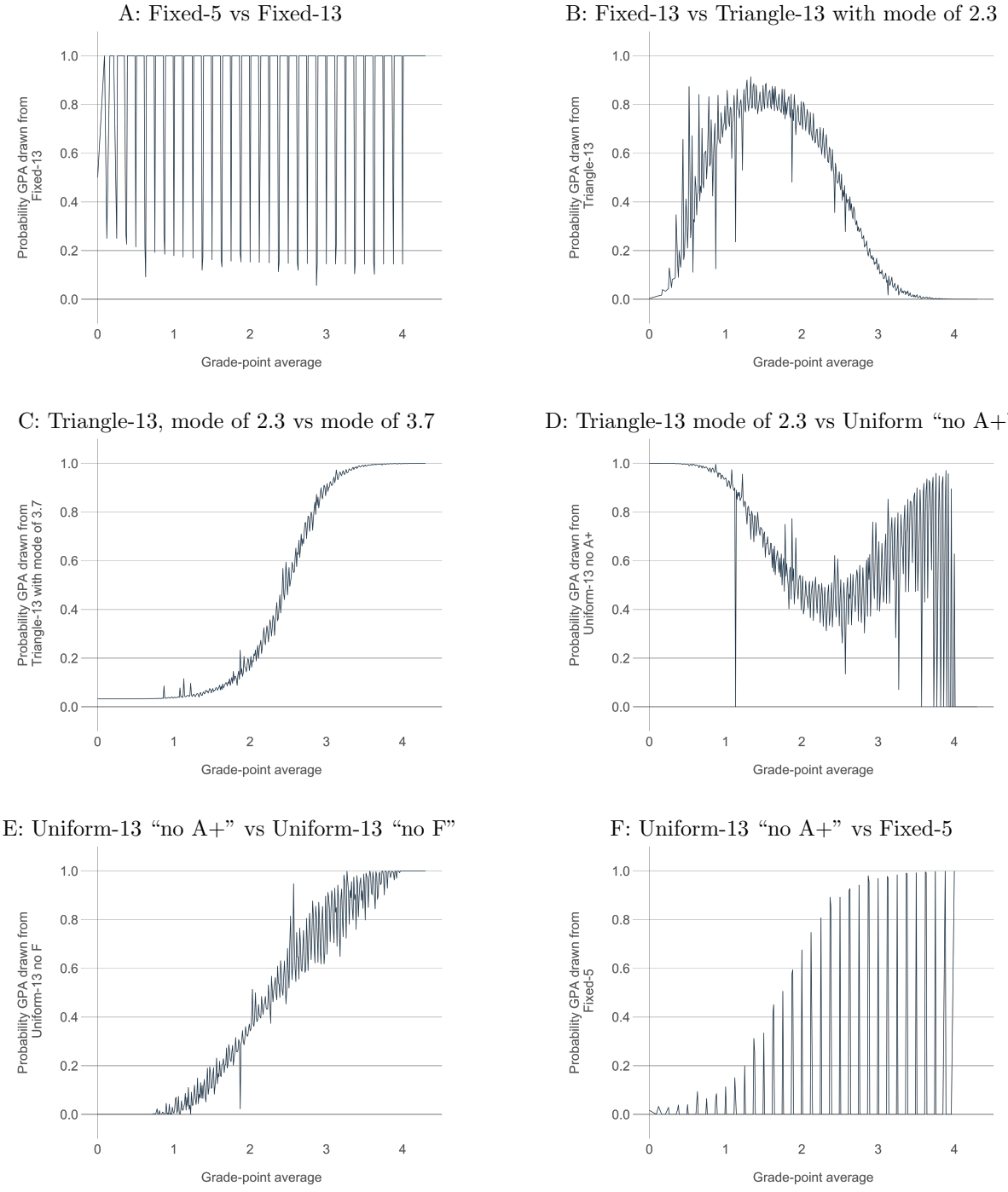
Figure 4: The combinatorics of GPA: PDFs across the number of classes (for a "Fixed-5" curve)



Notes: In Panel A we plot the underlying probability density function at the class level—the student's expectation of grade-point contributions at the class level. In panels B through F we plot the probability densities of having repeatedly drawn from that class-level PDF a number of times, for each GPA between 0.00 and 4.30 in increments of 0.01. In their production, we assume four classes per semester and two semesters per academic year. Thus, across all six panels we span the equivalent of having completed 1 through 32 classes. See Section 2.2 for related discussion.

Figure 5: Non-random sorting into GPA: Pairwise comparisons across students who have experienced two different grading curves for one year of classes

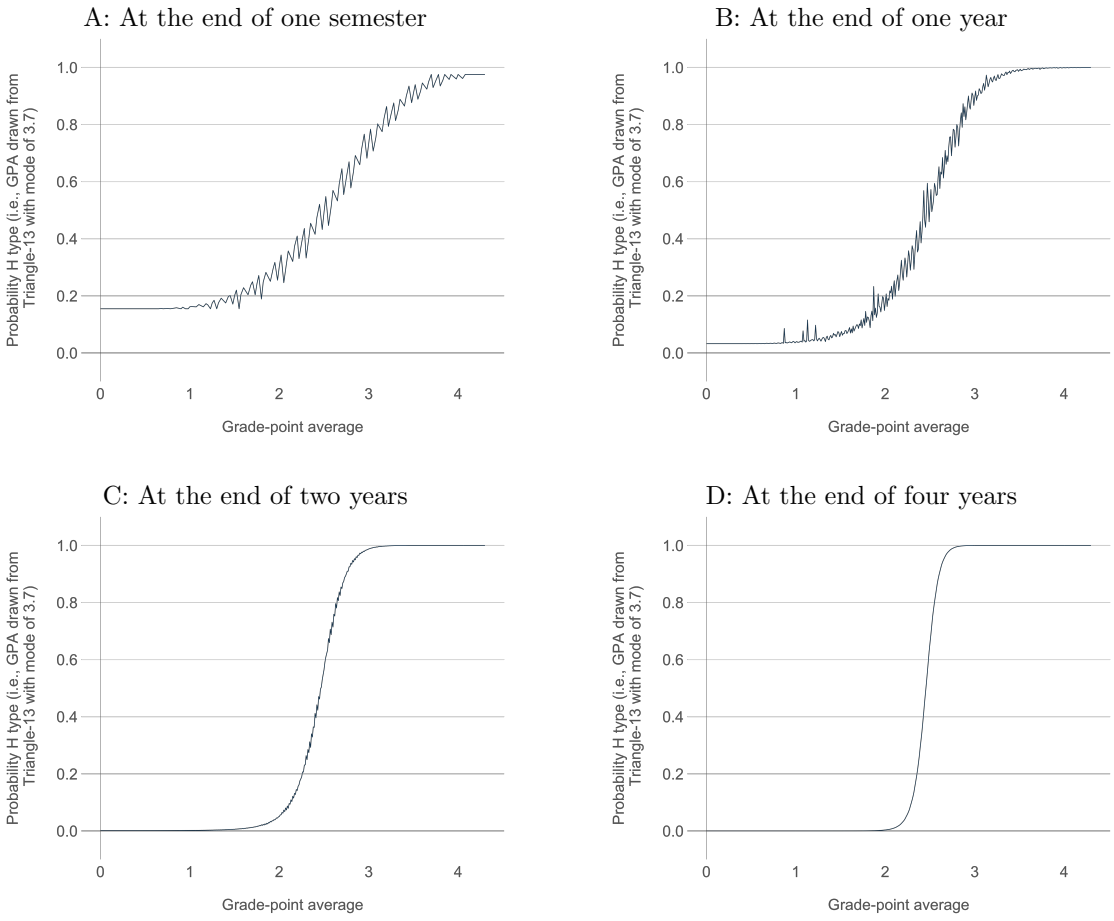
Here we evaluate how students will be distributed across GPAs if there is heterogeneity in grade accumulation across students. In each panel, we evaluate a DGP with two types of student—each draws grades from one of the grade distributions in Figure 3. Across GPAs, we ask how likely it is that a student observed at a GPA has drawn their grades from each of those distributions.



Notes: For each GPA between 0.00 and 4.30 (in increments of 0.01) we plot the probability that a student with that observed GPA was drawing grades from one of the PDFs (identified on the y axis). In each panel, the population of students is split equally between the two types. See Section 2.2 for related discussion.

Figure 6: Non-random sorting into GPA: How a pairwise comparison of H- and L-type students changes over time

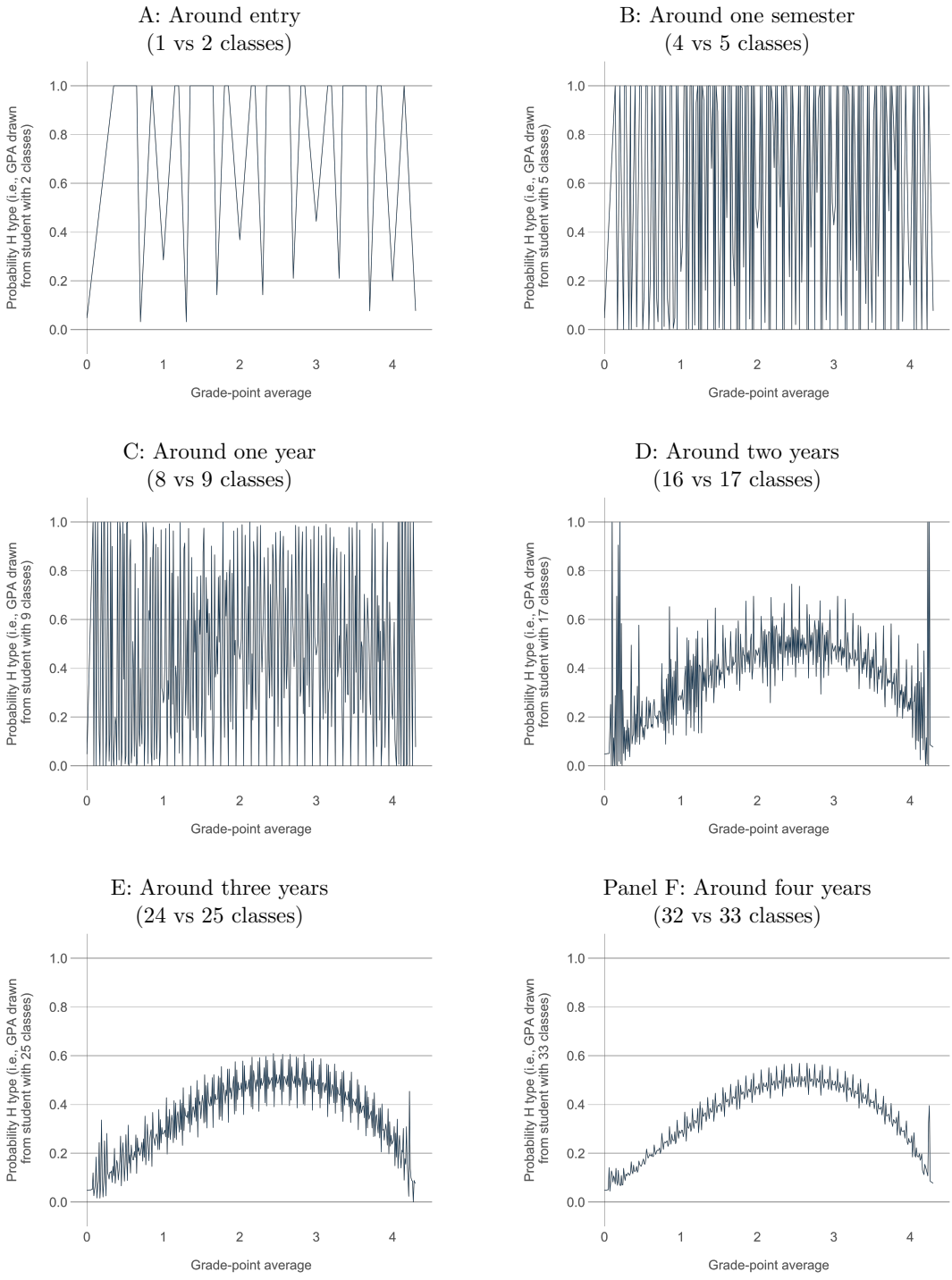
In each panel we evaluate a DGP with two types of student—they draw grades from a "triangle-13" PDF with either a mode of 2.3 (L types) or a mode of 3.7 (H types), as in Panel C of Figure 5. Across panels, we ask how the sorting of students into GPAs systematically changes as students take more classes.



Notes: For each GPA between 0.00 and 4.30 (in increments of 0.01) we plot the probability that a student with that observed GPA had taken classes with the mode of 3.7. Across panels, we reconsider this relationship as the number of classes increases. In each panel, the population of students is split equally between the two types. See Section 2.2 for related discussion.

Figure 7: Non-random sorting into GPA: What if H types just take an extra class?

In each panel we evaluate a DGP with two types of student—they both draw grades from a "triangle-13" PDF with a mode of 3.7 (as in Panel D of Figure 3) but L types draw c grades from that PDF and an equal number of H types draw $c+1$ grades from that PDF. Across panels, we ask how the sorting of students into GPAs systematically changes as students take more classes.

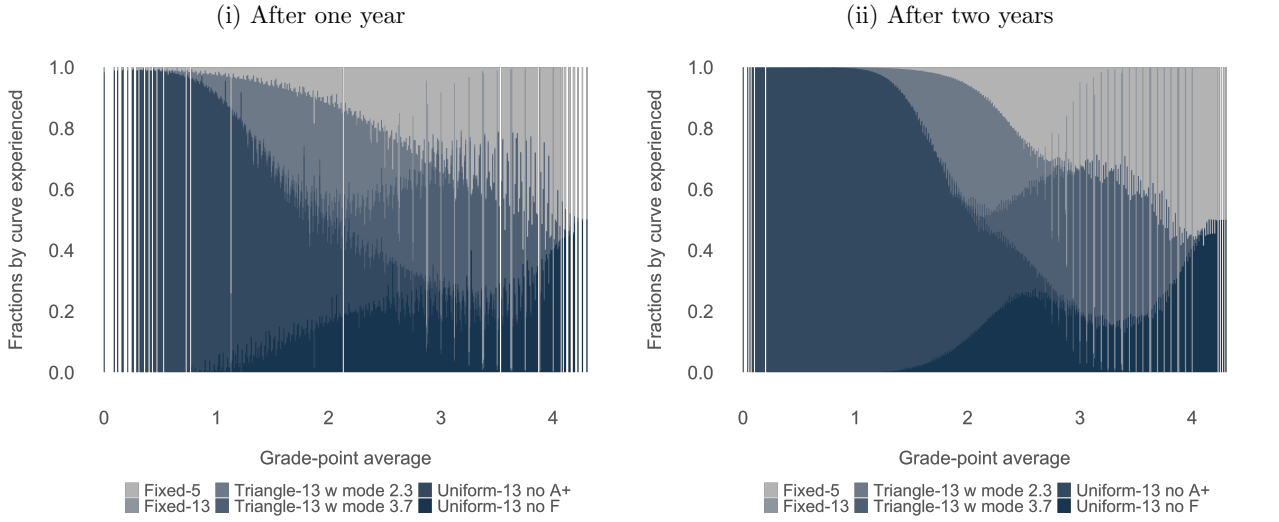


Notes: For each GPA between 0.00 and 4.30 (in increments of 0.01) we plot the probability that a student with that observed GPA had taken $c+1$ classes. See Section 2.2 for related discussion.

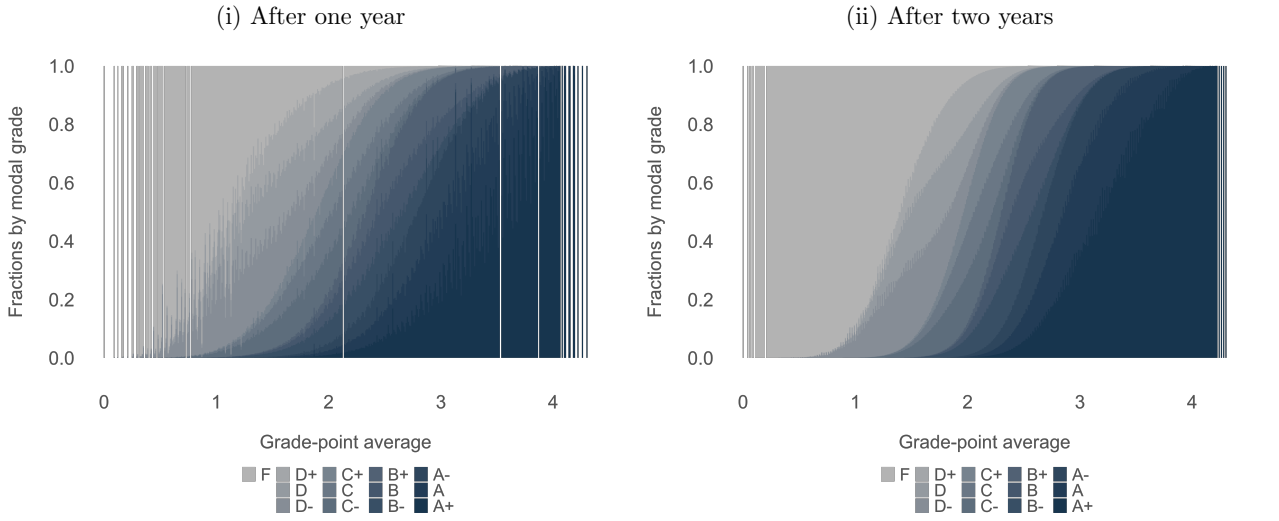
Figure 8: The proportional breakdown of students sorted into GPA by curves experienced

Here, we assume that there are many types of students are present in a population (instead of only two), and ask how those students will be distributed across GPAs. In Panel A we visualize the distribution of six types of students, each reflecting one of the grade PDFs shown in Figure 4. In Panel B we assume 13 types of students, each drawing grades from a triangle distribution with a modal grade that corresponds to the 13 traditional letter grades (i.e., F to A+).

Panel A: Six types of grading types (i.e., those from Figure 4)



Panel B: Thirteen triangle distributions, centered on each grade point

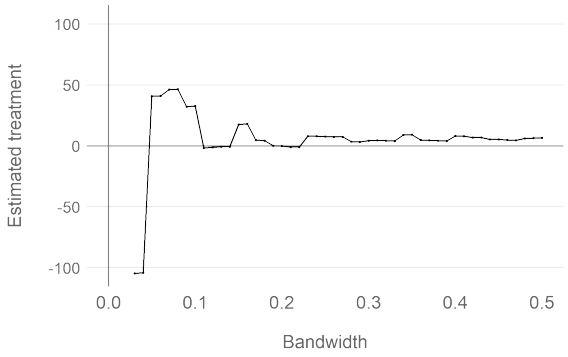


Notes: For each GPA between 0.00 and 4.30 (in increments of 0.01) we plot the stacked probabilities that a student with that observed GPA had experienced the associated grading curves. See Section 2.2 for related discussion.

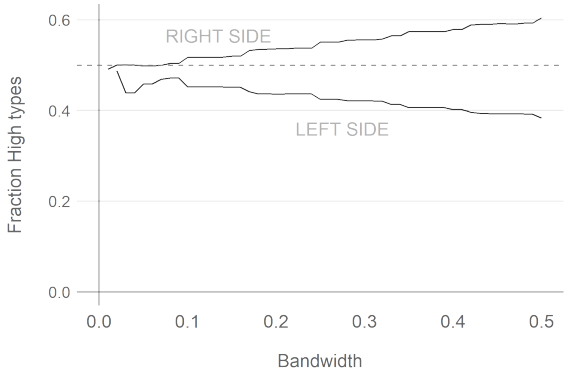
Figure 9: Bandwidth sensitivity in RD estimates evidences combinatorics

In the absence of any treatment, we retrieve estimates of the discontinuity in outcomes at GPAs at or above 2.50. Due to combinatorics, H and L types populate the domain of GPAs differently—this amounts to a violation of the smoothness assumption if this sorting is around the RD threshold. Here, as H types are level different in outcomes, this violation is transmitted through to estimated treatment effects. Estimates at smaller bandwidths are more likely to reflect the non-monotonicity in student-type across GPA. See Section 3 for related discussion.

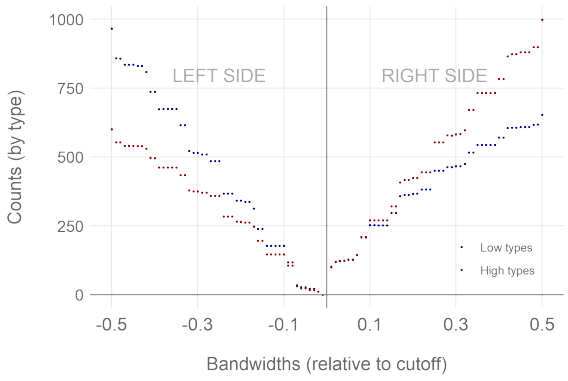
A: Bandwidth sensitivity at the end of one semester (4 classes)



B: On either side of the cutoff, the fraction of "High" types



C: The combinatorics-induced sorting of types across bandwidths

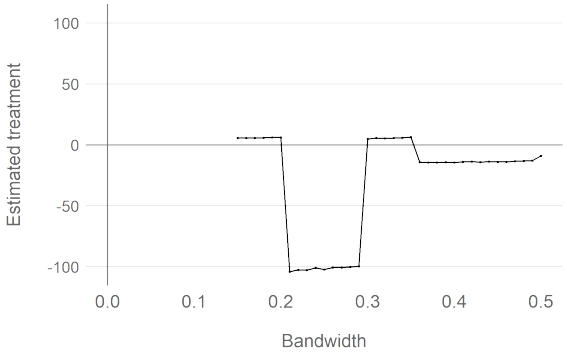


Notes: In each panel we consider bandwidths in increments of increments of 0.01 and report means across 200 simulations of 5,000 students.

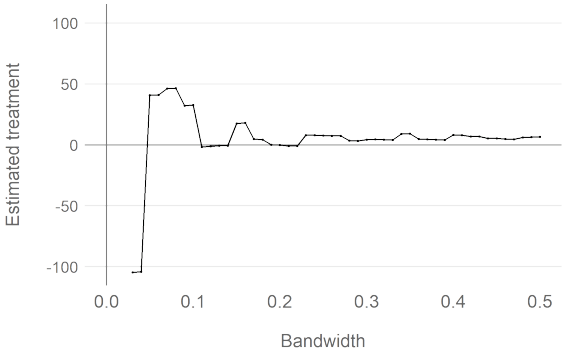
Figure 10: Bandwidth sensitivity in RD estimates at different times

As in Panel A of Figure 9, we retrieve estimates of the discontinuity at GPAs at or above 2.50. (As there is no treatment at 2.50, these should be zero.) This demonstrates that combinatorics is more of a concern at smaller numbers of classes and at smaller bandwidths, with mean convergence becoming more of a concern at larger numbers of classes and at larger bandwidths. See Section 3.2 for related discussion.

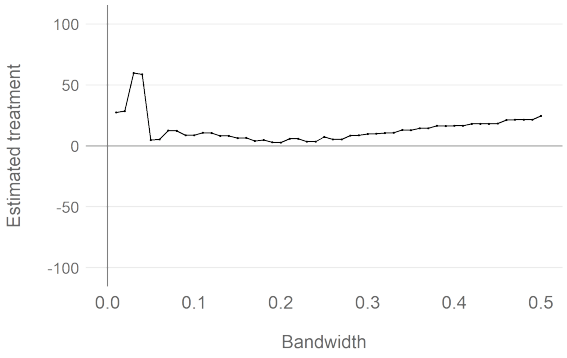
A: At the end of two classes
(*highly sensitive to combinatorics*)



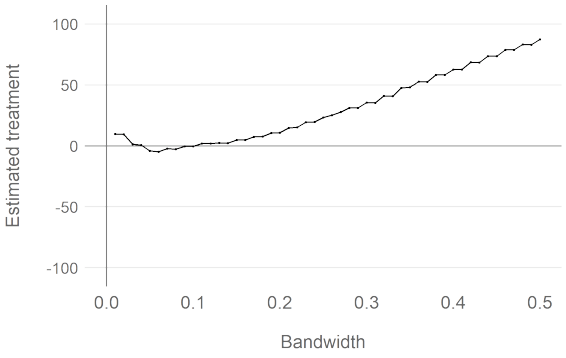
B: At the end of one semester



C: At the end of one year



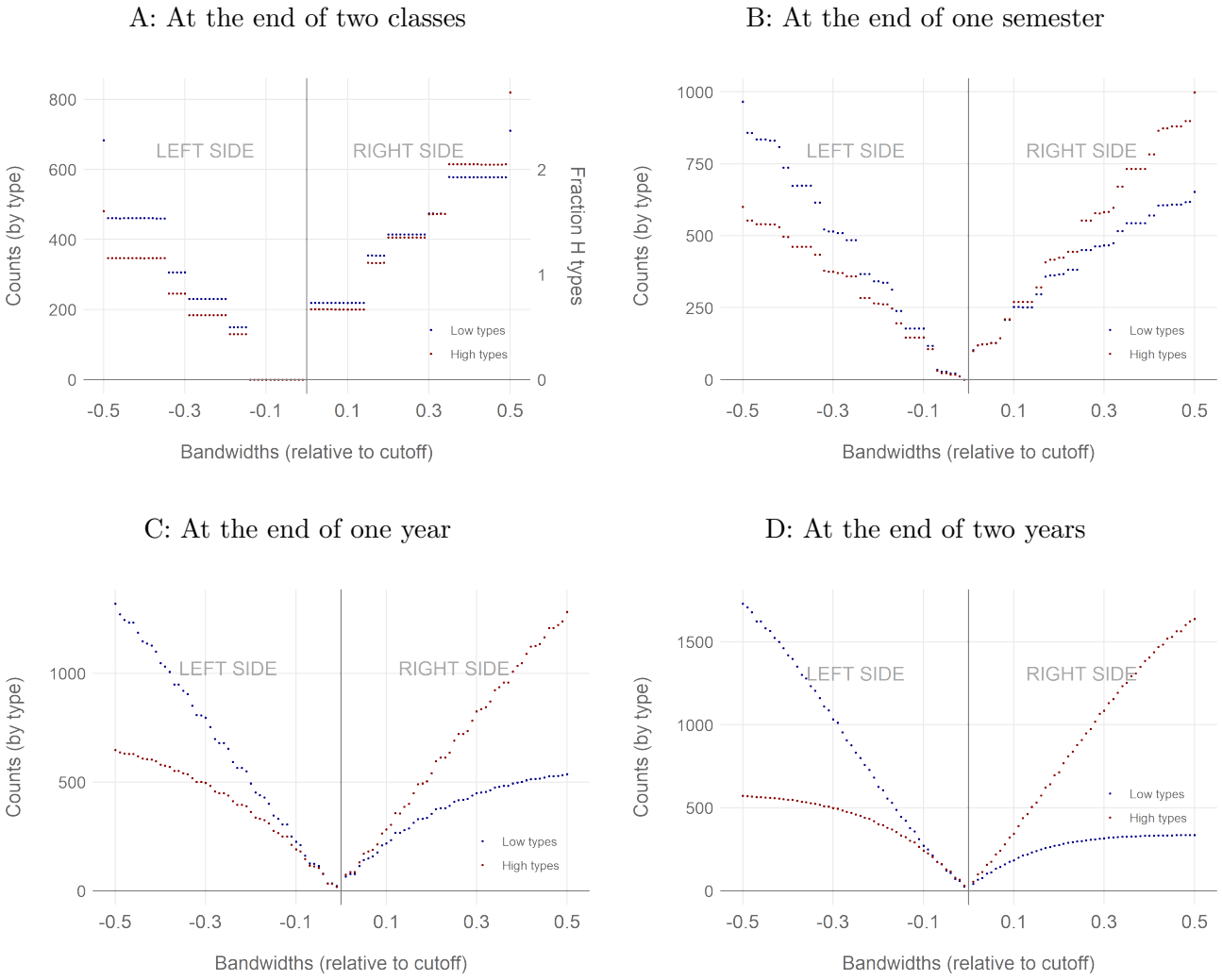
D: At the end of two years
(*highly sensitive to mean convergence*)



Notes: In each we panel consider bandwidths in increments of increments of 0.01 and report means across 200 simulations of 5,000 students. In the data-generating process, students take four classes per semester (i.e., eight classes per year).

Figure 11: The combinatorics-induced sorting into bandwidths at different times

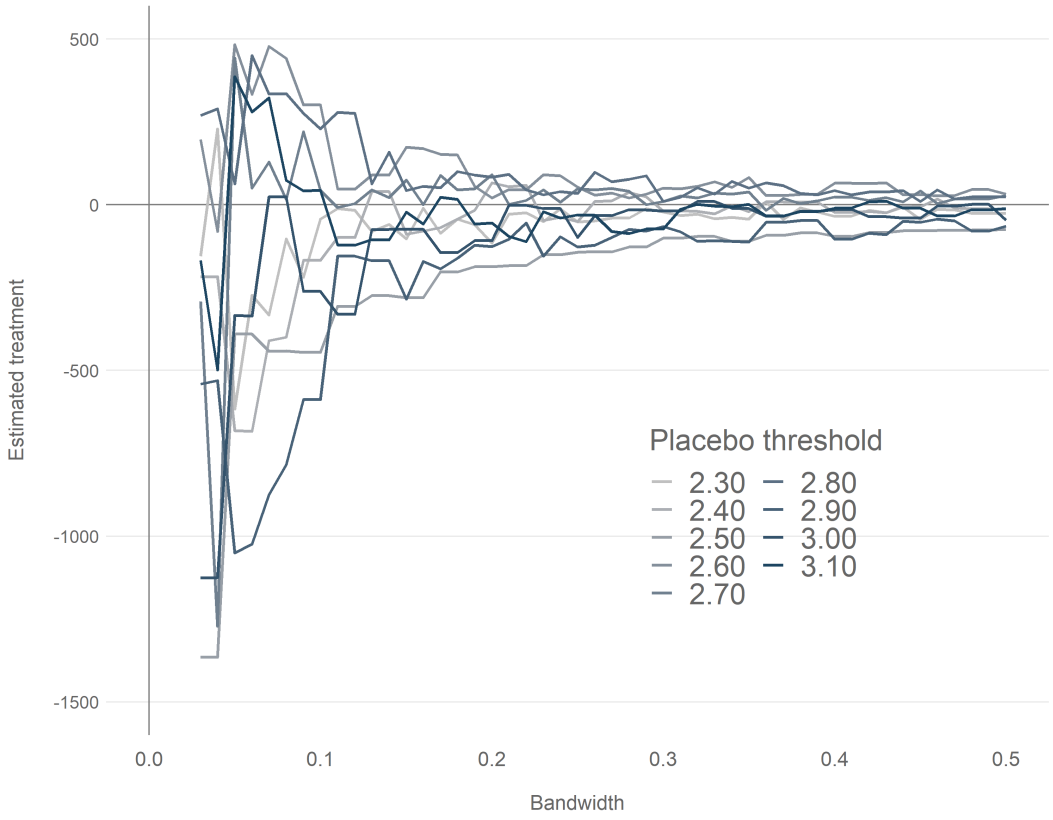
As in Panel C of Figure 9, we visualize how the population of High and Low type students are distributed around a GPA threshold. When students have taken few courses, only some GPAs can be reached, leading the sample included on either side of an RD threshold to change discretely as bandwidths grow. Smoothness in the distribution of High and Low type students only begins to appear after one year of courses have been taken (Panel C), though mean convergence is also begins to appear. See Section 3.2 for related discussion.



Notes: In all cases, the cutoff is a GPA of 2.50 and above. In each panel we consider bandwidths in increments of increments of 0.01 and report means across 200 simulations of 5,000 students. In the data-generating process, students take four classes per semester (i.e., eight classes per year).

Figure 12: Bandwidth sensitivities when there is student-level variation in the number of classes

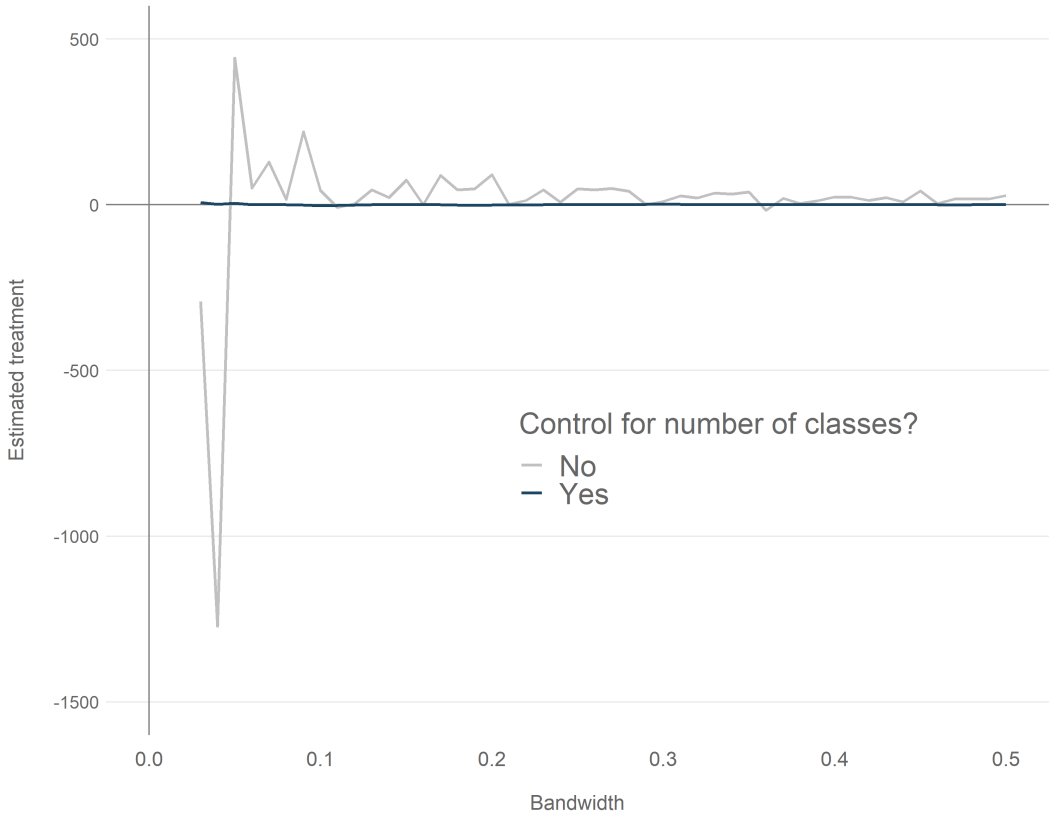
In the absence of any treatment, we retrieve estimates of the discontinuity in outcomes at various GPA thresholds. All students have drawn course grades from the same probability distribution (i.e. a triangle distribution with a modal grade of 2.7), but half of the sample has taken four classes while the other half has taken five classes. In this way we mimic the set of decisions that commonly occur for students after one semester of coursework, such as entry into a specific degree program. We demonstrate that variation across students in the number of classes taken can induce bias through combinatorics. We also evidence here that combinatorics bias is generally unsignable—the sign of point estimates here varies with bandwidth and treatment threshold. See Section 3.3 for related discussion.



Notes: In each panel we consider bandwidths in increments of increments of 0.01 and report means across 200 simulations of 5,000 students.

Figure 13: Bandwidth sensitivity controlling for student-level variation in the number of classes

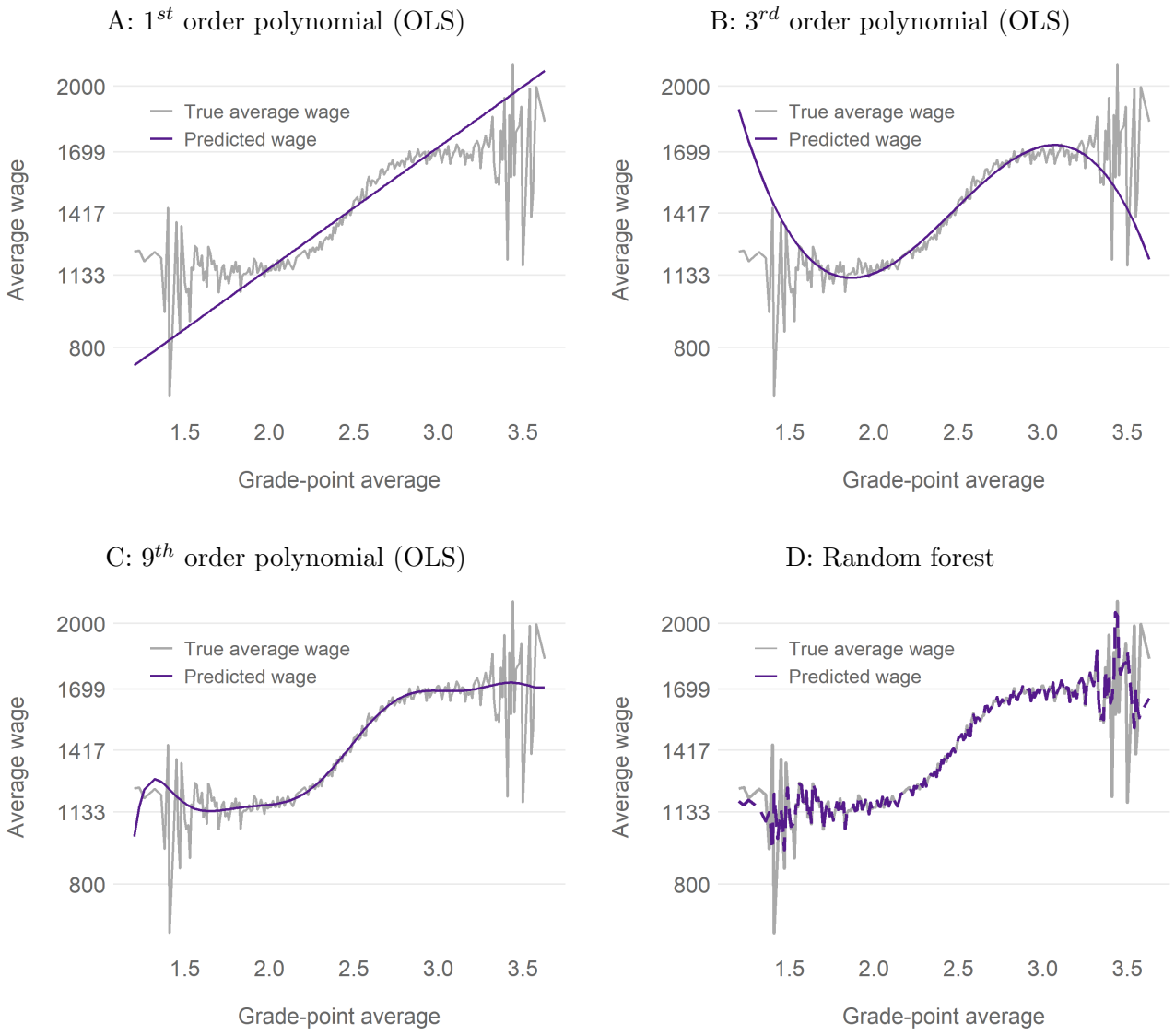
In the absence of any treatment, we evaluate the bandwidth sensitivity of an RD estimator at a 2.70 GPA threshold (using the same data as in Figure 12), with and without controlling for the number of courses taken. All students have drawn course grades from the same probability distribution (i.e. a triangle distribution with a modal grade of 2.7), but half of the sample has taken four classes while the other half has taken five classes. We demonstrate that the bias induced by combinatorics is eliminated when a visible signal of the heterogeneity between students in grade accumulation can be used as a control. See Section 3.3 for related discussion.



Notes: In each panel we consider bandwidths in increments of increments of 0.01 and report means across 200 simulations of 5,000 students.

Figure 14: How well does GPA predict outcomes when there is unobserved student heterogeneity?

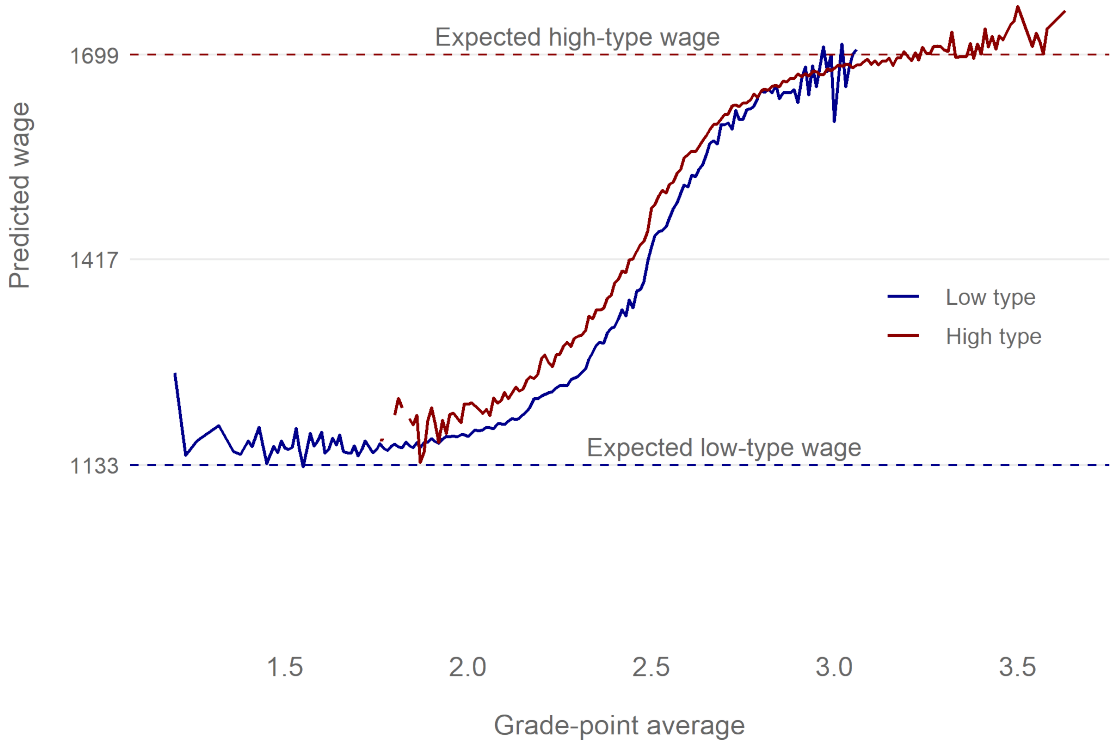
We evaluate the ability of various estimators to predict simulated wages when provided only with GPA. With enough flexibility, OLS estimators can predict the global non-linearity in outcomes, but fail to capture the local nonlinearities. In contrast, the random forest captures local nonlinearities in wages. All panels use the same sample of 30,000 students observed at the end of two years of classes. See Section 4.1 for related discussion.



Notes: L-type students draw from the PDF in Panel C of Figure 3, and H-type students draw from the PDF in Panel D of Figure 3—these PDFs are both triangles with modes of 2.3 and 3.7 respectively. We plot the predicted wage for each GPA in the domain over which we observe at least one student—GPAs between 1.20 and 3.63. Outcomes are level different between the two types and include a randomly drawn error that is normally distributed with a standard deviation of 300. Out-of-bag predictions are used for the random forest results presented in Panel D.

Figure 15: Can machine-learned methods capture student heterogeneity using transcript-level information?

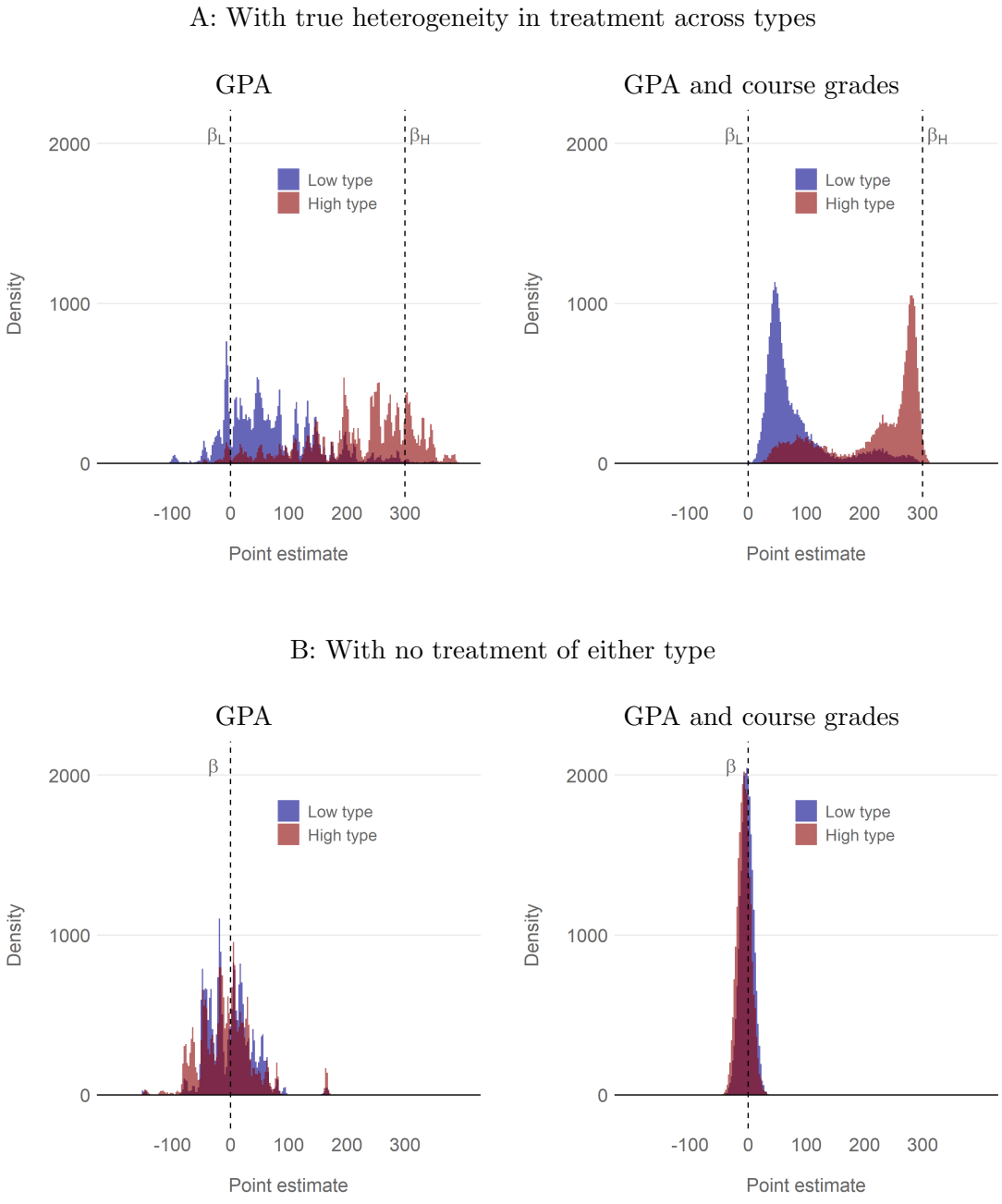
While the random forest on GPA in Figure 14 tracks average outcomes well, the addition of transcript-level data allows a random forest to identify heterogeneity at individual GPAs. This panel reflects a sample of 30,000 students observed at the end of two years of classes.



Notes: We plot the predicted wage for each GPA in the domain over which we observe at least one student—GPAs between 1.20 and 3.63.

Figure 16: How well does a causal forest distinguish heterogeneous treatment across student type?

We estimate conditional average treatment effects (CATE) with and without heterogeneity in treatment effects. The causal forest estimator identifies the presence of treatment effect heterogeneity when present, even when only provided with GPA. Adding individual course grades enhances the causal forest's ability to capture the treatment differences between types (or lack of difference in the absence of treatment). We test for heterogeneous effects in the CF estimators following Chernozhukov et al. (2020) and Athey et al. (2019). More generally, causal forests reject the null hypothesis of no heterogeneity in 100% of simulations.



Notes: In all panels we simulate 30,000 students observed at the end of two years of classes, with a GPA threshold for treatment of 2.50 and above. We plot the density of estimated treatment (in wage). See the GRF manual (link) for more details on causal forest estimation and formal tests for treatment effect heterogeneity. See Section 4.2 for related discussion.

Table 1: The natural variation in GPA exposes RD estimators to over rejecting the $\beta = 0$ null

Treatment estimates are expressed as point estimates relative to standard deviations of the dependent variable. Values in parentheses show the share of simulations in which the estimator (incorrectly) rejects the null hypothesis that $\beta = 0$. The impact of combinatorics on the same RD estimator using a smaller bandwidth is more apparent when students have taken fewer classes. However, as the number of classes taken increases the RD estimator is increasingly vulnerable to the bias induced by mean convergence. The DGP underlying these simulations is shown in Figure 5.

	Estimated effect sizes (fraction of times $p < 0.05$)		
	At the end of one semester (1)	At the end of one year (2)	At the end of two years (3)
Panel A: Regression discontinuities over-reject the $\beta = 0$ null			
Optimal bandwidth	0.0531	0.0888	0.1146
	(0.31)	(0.56)	(0.61)
Optimal bandwidth × .1	0.1340	0.1069	-0.0039
	(0.13)	(0.08)	(0.06)
Panel B: Causal forests reject the $\beta = 0$ null appropriately			
GPA	-0.0004	-0.0001	0.0005
	(0.04)	(0.05)	(0.05)
GPA and individual course grades	-0.0003	0.0000	0.0005
	(0.04)	(0.05)	(0.05)
Panel C: Causal forests, with bandwidth-restricted samples			
Sample restricted to the RD's optimal bandwidth			
GPA	-0.0001	0.0001	0.0009
	(0.04)	(0.05)	(0.04)
GPA and individual course grades	-0.0001	0.0002	0.0009
	(0.04)	(0.05)	(0.04)
Sample restricted to the RD's optimal bandwidth × .1			
GPA	-0.0015	0.0004	0.0011
	(0.05)	(0.04)	(0.06)
GPA and individual course grades	-0.0010	0.0005	0.0015
	(0.05)	(0.04)	(0.06)

Notes: Estimates are means across 1,000 samples of 30,000 students using the same DGP as in Panel A of Figure 16. Optimal bandwidth selection for the fuzzy RD estimator follows that of Imbens and Kalyanaraman (2012). If the smaller bandwidth RD estimator would lead to empty bandwidth on either side of the treatment threshold, we default to the smallest populated bandwidth. We test for heterogeneous effects in the CF estimators following Athey et al. (2019), Chernozhukov et al. (2020).

Appendices

A Performance of OLS and ML wage prediction models

In Table A1 we report quantitative measures of the performance of the wage prediction models shown in Figure 14. The data generating process through which students accumulate grades and realize post-graduation wages is unchanged from that described in Section 4.1.[39] We test the performance of these methods across 1,000 simulations when evaluating a population of 30,000 students observed at the end of 1 semester (Panel A), one year (Panel B), and 2 years (Panel C). Here we highlight the results from Panel A, where the population of students in question has taken four classes, but we note that the results and relative performance of our prediction models are similar when students have taken more classes. In Column (1) we report the frequency at which a 0.01 increase GPA is associated with a decrease in the weekly wage of the average student. At the end of one semester, a 0.01 increase in GPA predicts a *decrease* in weekly wages 43.7 percent of the time, across simulations.[40] While the fitted polynomials rarely predict such a decrease, the random forest predicts that wages rise as GPA rises 43.5 percent of the time, close to the true share in the data. Moreover, as we report in Column (2), roughly 98 percent of these predictions are accurate. As a complement to Column (2), in Column (3) we report the fraction of GPAs at which average wages decrease that are missed by each of our models. Again, the random forest performs especially well, and fails to "catch" a GPA at which average wages fall only 2 percent of the time.

In columns (1) through (3) we demonstrate that the random forest predictor is effective at capturing local non-monotonicities in average wages. But the question of how closely each estimator tracks average wages is also important. To address this question, in Column (4) we report the mean distance between the predicted and true average outcome at each GPA.[41] We express the distances in Column (4) relative to that of the linear model. For example, when evaluating students at the end of one year of classes, the random forest predicts wages that are over ten-times closer to the true average wage than the predictions of the linear model.

[39] Due to the sparsity of students in the tails of GPA, we report these measures of model performance for the inner-95 percent of observations. In the tails, this sparsity leads to erratic changes in the average wage across GPAs, which all of our models have difficulty in tracking.

[40] Absent any statistical noise, combinatorics alone would lead a 0.01 increase in GPA to predict a decrease in weekly wages 41.8 percent of the time when this population of students was observed after one semester of classes.

[41] We compute distance as the absolute difference between the predicted wage and the average actual wage at a GPA. This statistic captures how well the prediction lines in Figure 14 track the true average wage line, but in a way that can be easily averaged across multiple simulations. In plain language, Column (4) captures how far apart the purple and grey lines are in Figure 14.

Table A1: Across methods, how well does GPA predict (simulated) weekly income variation?

	How often does wage decrease? [a]	How often is the predicted decrease correct?	How often does it not catch the decrease?	Distance between the predicted and actual wage at a GPA, relative to 1^{st} order polynomial [b]
	(1)	(2)	(3)	(4)
Panel A: At the end of one semester (4 classes)				
DGP	43.5%	–	–	–
OLS				
1^{st} order polynomial	0.0%	-	100%	1.000
3^{rd} order polynomial	0.1	53.1	99.9	0.911
9^{th} order polynomial	0.3	57.5	99.7	0.898
Random forest	43.5	98.1%	2.0	0.082
Panel B: At the end of one year (8 classes)				
DGP	44.7%	–	–	–
OLS				
1^{st} order polynomial	0.0%	-	100%	1.000
3^{rd} order polynomial	0.1	46.4	99.9	0.864
9^{th} order polynomial	0.3	46.8	99.7	0.836
Random forest	44.6	97.6%	2.6	0.119
Panel C: At the end of two years (16 classes)				
DGP	44.8%	–	–	–
OLS				
1^{st} order polynomial	0.0%	-	100%	1.000
3^{rd} order polynomial	5.5	50.3%	93.9	0.629
9^{th} order polynomial	3.8	50.2	95.8	0.469
Random forest	44.8	98.5	1.5	0.021

Notes: Based on 1,000 simulations of 30,000 students. We calculate all values using only the students with GPAs in the central 95% of the data. At 8 classes, the average minimum GPA in the central 95% of the data is 1.64and the maximum is 3.29, while at 16 classes the minimum GPA is 1.82 and the maximum is 3.11. Actual wage decreases are defined as instances in the simulated data wherein the average wage at a GPA falls compared to the average wage at the next lower and at which we observe students.

[a] In the actual wage data generated by the DGP, and then in the wages predicted at each GPA by each of the methods.

[b] In plain language, Column (4) captures the average distance (across 1,000 simulations) between the purple and grey lines shown in Figure 14, normalized by the value of the first-order polynomial.

B Top-coding in GPA

In the above analysis, one will have noted that we allowed for "true 5s" in the underlying data generating process—given the abstraction exercise this worked fine. However, while we anticipate that very strong students might well be expected to achieve at that level (in outcomes y_i, that is), their *measured* performance (in GPA_i) might not fully reflect their relative aptitude within the traditional domain of GPAs.

In Figure B1 we have enforced the top-coding of grades at 4.30, which binds on these "true 5s" in particular. Of consequence, then, is this: if the relationship between outcomes and underlying ability continues while the *measure* of ability (i.e., GPA) is top-coded, then the econometrician is unable to model $y_i = f(\text{GPA}_i)$ well among the highest-ability students.[42] Here, this biases estimates of treatment down, which is demonstrated in the estimated discontinuity in Panel A. Had GPA not been top-coded at 4.30, the econometrician would have been able to fit this relationship to outcomes and thereby identify the true null effect within the data. However, where bandwidths are such that top-coded students receive weight in the estimator, the resulting bias is best thought of as unsignable—our recommendation is that regression-discontinuity exercises limit bandwidths to GPAs *strictly* less than the limit of the domain of all GPA (e.g., 4.30). In Panel B of Figure B1 we confirm that we do not reject the $\hat{\beta} = 0$ null when limiting the bandwidth to exclude the top-coded students at 4.30. Alternatively, one could consider formally modeling whether there are heaps at the limits—unexpected mass in the distribution of students at 4.30 would be informative, for example, and the potential sensitivity of $\hat{\beta}$ to the inclusion of (potentially) top-coded students may be a valuable experiment.[43]

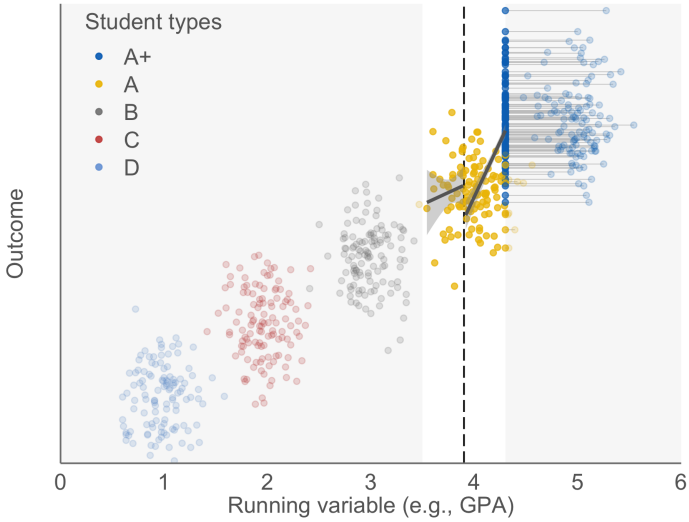
For a more flexible environment in which to explore the variation in RD estimates in simulated GPA data, see `https://glenwaddell.shinyapps.io/RD-in-GPA-data/`.

[42] In the data-generating process we've again allowed for an equal level increase in y_i for the "5" types.

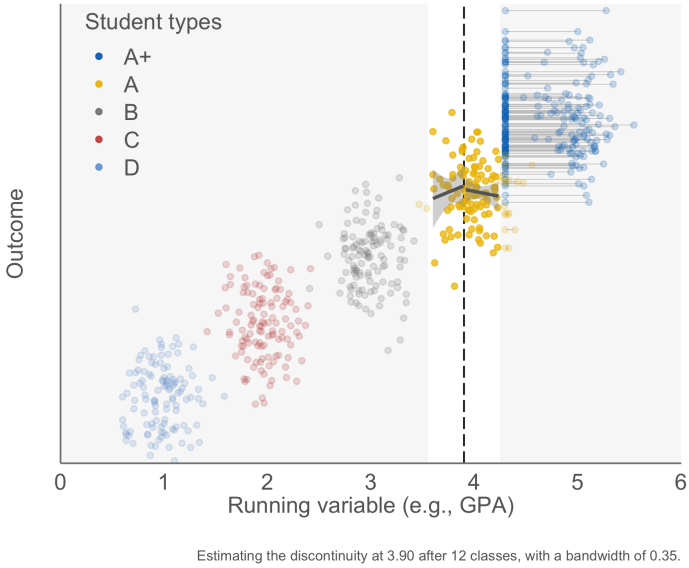
[43] Note that it is only in the absence of top-coding that positive and negative grade shocks contribute symmetrically to GPA-measured performance. For example, the *measurable* effect of positive shocks is attenuated for better-performing students (e.g., a "true 5" is only recorded as a 4.3) while negative shocks transmit fully to GPA. (Likewise, low-performing students will receive the full weight of positive shocks, while the measured effect of negative shocks will be be tempered. This seems less concerning, in practice.)

Figure B1: The effect of top-coding GPA on RD estimates in the absence of treatment

A: The effect of top-coded students on $\hat{\beta}$



B: Bandwidth selection to remove potential top-coding



Notes: In both panels, students draw uniformly from the grades that are within ± 1 of their median grade, which is anchored by their type. In both panels there are 125 students of each type (i.e., $n = 125 \times 5 = 600$ students). Students take four classes per semester and two semesters per academic year. See Section 2.1 for related discussion.