Who Benefits from a Smaller Honors Track?[*]

Zachary Szlendak
University of Colorado-Boulder

Richard Mansfield
University of Colorado-Boulder

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Abstract

The vast majority of high school courses in the U.S. separate classrooms into standard and honors tracks. This paper characterizes the efficiency and distributional impact of changing the share of students enrolling in the honors track. We first introduce a model of tracking in which students choose their track for each course, but schools can adjust an array of incentives that implicitly govern the enrollment share of the honors track. We show that determining the administrator's optimal choice of honors track size requires knowledge of a set of treatment effect functions capturing the impact of alternative honors enrollment shares on different parts of the distribution of student predicted performance. We then use rich administrative data from North Carolina public high schools to estimate these treatment effect functions by quintile of predicted performance. Across a wide variety of model specifications and alternative pareto weights over achievement gains for different quintiles, we find that offering a small honors track (20% to 30% of a course's students) yields moderate performance gains for the top quintile ($\sim$ .05-.07 test score SDs relative to the absence of tracking) that decline monotonically across quintiles to become essentially zero for the bottom quintile. However, expanding the honors track beyond 30-35% of students generates further (small) achievement increases only for the middle quintile, and causes gains for top quintile students to dwindle and losses for bottom quintile students to become substantial. Since many courses either do not track or feature honors shares above 35%, we predict that creating honors tracks in each course containing around 30% of students in all North Carolina high schools would generate average gains in statewide performance for all quintiles of the predicted performance distribution.

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1 Introduction

Tracking is the process of separating students by ability in order to customize the level of content that they experience. Archbald and Keleher (2008) estimate that over 80% of high schools in the US offer courses that feature multiple tracks representing different paces and rigor. Several papers examine the achievement effect of the track choices of marginal students (e.g. Smith and Todd, 2001; Card and Giuliano, 2016). A number of others consider the impact of introducing tracking or removing it entirely (e.g. Figlio and Page, 2002; Duflo et al., 2011). Yet among schools that offer both honors and regular versions of courses, there is wide variation both across schools and within schools across courses in the share of students that enroll in the honors track. Motivated by the lack of consensus about the optimal honors track size, this paper considers the school's choice of how selective to make its honors track.

The effects of increasing the size of the honors track are ex-ante ambiguous, depend on the initial size of the honors track, and are likely to vary by the type of student. Expanding access to honors versions of courses allows the marginal students to experience the greater rigor and peer quality of the honors track. However, as more students move into honors, the honors track becomes diluted and the regular track experiences a brain drain, decreasing the average student quality in both tracks. Furthermore, after students self-sort, teachers may then alter the level of instruction to align with the new student composition of each track. Other classroom characteristics, such as teacher assignment and class size, may also be affected as schools reallocate resources between the tracks, further obfuscating the effects of the expansion on different types of students.

We investigate the distributional impact of alternative choices of honors track size by estimating separate flexible functions by category of student preparedness that map a course's fraction in honors into expected standardized test score performance. To justify and motivate our empirical approach, we also introduce a simple theoretical sorting model of a typical high school environment in which students can self-sort into their chosen track for each course, but an administrator can adjust the costs of doing so to implicitly select a preferred honors track size. Importantly, we allow students' track choices to be based not only on preferences and administrator-determined costs, but also on observed and unobserved abilities that affect how much they benefit from the honors track, thus accommodating "selection on gains". The model yields conditions under which the functions we estimate are sufficient to determine the administrator's optimal choice of course-wide enrollment shares in each track.

There are three essential challenges to estimating the impact of changing the intensive margin of honors selectivity. First, like other school policy interventions, the expected per-student achievement impact of changing the size of the honors track is likely to be small given the wide variety of other student, teacher, and school inputs that also affect test score performance. Thus, the amount of variation necessary to obtain sufficient power to detect treatment effects from alternative interior honors track shares is daunting, particularly when there are strong theoretical reasons to expect heterogeneous and non-monotonic effects from increased selectivity. In particular, the onerous sample and specification requirements generally preclude the use of small scale experiments and narrowly defined instrumental variables that would otherwise provide credible identification.

Second, because introducing an honors track or changing its size may involve altering not just the depth with which content is covered but also the scope of the curriculum itself, standardized tests may become misaligned with what students are taught, creating measurement error that is correlated with the change in the honors enrollment share. Third, valid identification of the effect of changing the size of honors is empirically difficult because the honors enrollment share is partially endogenous to school, teacher and student characteristics that affect student achievement. For example, an unobservably better-prepared student population might drive both the share of students in honors and average test score performance.

The North Carolina administrative records we use are particularly well-suited to address all three challenges. The data contain histories of elementary and middle school test scores for the near universe of public high school students from 1995 to 2011. In addition, the data feature statewide course-specific tests in eleven high school courses, of which we focus on six that were consistently offered and for which tracks are easily inferred.[1] By facilitating comparisons across schools, across school cohorts, and across courses within a cohort, these two features ensure that an enormous amount of variation in honors track sizes and contemporaneous achievement can be harnessed to identify heterogeneity in impacts at different margins of selectivity for different student subpopulations.

Furthermore, North Carolina's accountability system provides strong incentives to principals and teachers to adhere to the curriculum tested by the statewide exams regardless of track, including test score-based teacher bonuses and public ratings of schools. Such incentives mitigate concerns about misalignment between the content taught versus tested in each track.

Finally, the data provide rich controls at the school, teacher, family, classroom, and student levels, including parental educational attainment, school size, class size, student demographics, and teacher experience, education, and licensing test performance. These

[1] The courses excluded either have multiple advanced tracks such as honors and Advanced Placement, are generally taken in middle school, or are infrequently tested.

controls collectively capture many of the inputs that jointly drive test score performance and the size of the honors track, thus dramatically reducing the scope for simultaneity and omitted variable biases.

In our baseline specification, we pool the cross-sectional, time series, and cross-course variation in the share of a course's students that choose the honors track, since there are plausible sources of potentially exogenous variation at each level. In particular, phone conversations with staff at several North Carolina schools indicated that different principals and department heads exhibit idiosyncratic beliefs on the optimal size of an honors track or preference weights for relative performance of different student subpopulations. Also, relatively modest changes in cohort size may affect the number of classrooms that must be offered in a course to meet class size objectives. This could change the natural set of honors shares depending on the track of the classroom added or removed from offerings. However, given that the gravest endogeneity concerns relate to student sorting at the school level, our preferred specification features a set of school fixed effects.

We aggregate to the school-course-year-preparedness quintile level in each specification, which sidesteps the selection problems associated with individual students' choices of track that have been the focus of much of the tracking literature. We also restrict the sample to observations from schools with typical distributions of student past performance, so that the regular and honors peer environments associated with a given honors fraction are likely to be similar across schools. We then regress test scores on a cubic function of the fraction of students in honors in the associated school-course-year combination, along with a large set of controls. To account for heterogeneity in impact, separate cubic coefficients are estimated for each quintile of a regression index of student preparedness based on past test scores. Validity of our baseline estimates requires that, conditional on our full set of controls, the variation in the share of a course's students that chooses the honors track is unrelated to other unobserved school, teacher, and student inputs that may affect test score performance.

To address remaining endogeneity concerns, we employ several alternative specifications that introduce either fixed effects at various levels or instrumental variables in order to isolate different and in some cases mutually exclusive sources of variation in honors track size. We concede that no single specification represents an airtight identification strategy; instead our confidence in the results stems from their consistency across these specifications. In order for spurious correlations to drive our results, separate sources of endogeneity from different levels of variation would have to generate bias functions with the same pattern and similar magnitudes across the interval of honors enrollment shares for the first quintile of our preparedness index, and would then need to agree again on other bias functions with distinct patterns and magnitude for each of the other four quintiles we consider.

Across a range of specifications, we consistently find that students in the first (highest) predicted achievement quintile benefit the most from honors programs that comprise 25-35% of the student body; they enjoy an expected increase of 0.05-0.07 test score standard deviations relative to a version of the course without tracking. The second quintile exhibits similar but smaller effects as the first, with an average test score gain of about 0.025-0.04 standard deviations (SDs) that peaks at slightly higher honors enrollment shares around 35-40% and declines at a slower rate as the share of the student body increases past 40%. The third quintile experiences its largest gains from still larger honors programs, gaining an average of 0.02-0.03 SD when 40-50% of the student body is enrolled in honors. The fourth quintile is relatively unaffected by either the existence or the size of the honors program, exhibiting gains or losses generally within 0.01 SDs for any honors share between 0 and 60%. The fifth quintile begins to exhibits losses at around a 25-30% honors share that grow to 0.03-0.04 SDs at a 60% honors share.

When administrators value the gains of all quintiles equally, honors tracks with 20-30% of student body enrollment maximize the school's average score, with average gains of 0.02-0.03 SDs compared to the absence of an honors track. An honors enrollment share in the 20-30% range still maximizes the weighted average performance and delivers small gains relative to no tracking even with a compensatory weighting system that weighs the achievement gains of quintiles 1, 2, 3, and 4 at 20%, 40%, 60%, and 80% of those of quintile 5, respectively. Both equity and efficiency considerations argue against honors shares greater than 30%; the small benefit of having more quintile 2 and 3 students placed into the honors program seems to be more than offset by the cost of having both the regular and honors track experience decreases in their average student quality and level of instruction.

Furthermore, enough schools and courses feature either no tracking or suboptimal honors enrollment shares such that if all schools switched from their current honors program size to the optimal size, we predict that North Carolina high school students would gain an average of .006 test score SDs per course. Encouragingly, this switch would increase average test scores for all preparedness quintiles, with the top and bottom quintiles benefiting the most.

Since these relatively small gains per student-course would be enjoyed across several courses by millions of students and thousands of high schools, changing honors track enrollment shares potentially represents a low cost avenue for generating a substantial aggregate gain in student achievement. Using a back-of-the-envelope calculation that assumes that tracking-induced test score gains generate the same impact on earnings potential as Chetty et al. (2014a,b) found for teacher quality-induced test score gains, we estimate that transitioning all North Carolina high schools' current honors enrollment shares to the estimated optimal of 29.2% for six core courses would yield an aggregate increase in age 28 earnings of

$12.7 million for each cohort.

Our contribution to the tracking literature is to quantify the impacts of changing the intensive margin of honors track selectivity in a context where students self-select into tracks conditional on capacity constraints implicitly set by school administrators. Other papers have evaluated the extensive margin choice of whether to have any tracking, in several cases by exploiting experimental or quasi-experimental variation. These papers generally do not analyze the size of the honors track when it exists. Some of these papers have found that tracking helps the top students and hurts the bottom students (Betts and Shkolnik, 2000; Hoffer, 1992; Argys et al., 1996; Epple et al., 2002; Fu and Mehta, 2018). Others have found that tracking does not hurt any students (Zimmer, 2003; Figlio and Page, 2002; Duflo et al., 2011; Card and Giuliano, 2016) or has small or insignificant effects (Pischke and Manning, 2006; Lefgren, 2004). Our results suggest that these seemingly contradictory results might potentially be reconcilable if the different papers feature samples of schools with different mixes of honors enrollment shares.

Fu and Mehta (2018) represents the rare paper in this literature that incorporates an explicit role for honors track selectivity. The authors build a structural model that includes an administrator choosing how to assign elementary school students to different tracks. The model permits heterogeneous effects for tracking schemes that vary with the size of each track. However, while their approach permits a broader welfare analysis, it also requires strong assumptions to simultaneously identify the parameters that govern tracking in combination with other preference and technological parameters related to other choices in the model. Furthermore, they focus on elementary schools, and their tracking data are not as rich or reliable as the North Carolina administrative data.[2]

A second strand of the literature considers the effect on an individual student of moving into an honors or gifted track, either using regression discontinuities (Card and Giuliano, 2016) or propensity score matching (Hoffer, 1992; Long et al., 2012; Smith and Todd, 2001). These papers generally find that enrolling in advanced tracks improves test scores for the marginal students they consider. Our estimates combine the effects on the marginal students with the accompanying effects of diluting the honors track and reducing the peer quality in the regular track. Our results suggest that the impact of honors is not limited to just the marginal students, since we find that students whose past test scores strongly suggest they will be inframarginal are still affected by changes in honors track size.

Finally, this paper also contributes to the much larger literature considering peer effects on academic achievement. While our approach does not isolate the contribution of peer

[2] The authors are forced to infer the track based on variation in teachers' self reports of the quality of their students, which may in some cases reflect sampling variation rather than tracking per se.

effects, such effects are likely to be one of the driving forces for our results. Hanushek et al. (2006) and Lefgren (2004) find that having better peers improves outcomes for students across the ability distribution. Mehta et al. (2019) find that improved peer quality increases academic performance through both cognitive and non-cognitive mechanisms, such as study time. Imberman et al. (2012) also find that all students benefit from higher achieving peers, but their estimates suggest in addition that the highest ability students are the most sensitive to the quality of their peers. Our results are consistent with theirs, since we find that top students gain most from small honors programs, where the peer quality is presumably high, and bottom students are relatively unaffected by small honors programs, suggesting that they are fairly insensitive to peer effects from top students. By adding additional assumptions about student assignment, Fu and Mehta (2018) are able to separately identify peer effects, and similarly find that changing the fraction of students in honors induces changes in peer effects which differ by the type of student affected.

The remainder of the paper will be structured as follows. Section 2 presents a theoretical model that governs the administrator's implicit choice of the size and/or selectivity of the honors track. Section 3 describes the data, Section 4 lays out the empirical approach, and Section 5 presents the results. Section 6 provides several robustness checks, and Section 7 interprets the findings and concludes.

2 Model

In this section we first describe the planner's tracking problem that the school administrator must solve, which clarifies the required decision inputs that this paper seeks to provide. We then introduce a simple education production function and classroom sorting equilibrium in order to derive a methodology for estimating these decision inputs and to elucidate the assumptions this approach requires.

2.1 The Administrator's Problem

Most high schools allow students to choose their tracks for each course they take. Nonetheless, school administrators have a variety of levers within their control that can alter student incentives to enroll in honors. For example, administrators can preallocate a particular share of classrooms and associated time slots to honors that can affect the scheduling convenience of choosing the honors track. They can also adjust the homework loads in each track, set automatic GPA boosts from taking the honors version of a course, and require mandatory meetings with counselors who can encourage students to enroll in the honors track or discourage them from doing so. Given this reality, rather than assume that administrators can determine the complete allocation of students to tracks for each course, we instead assume that they select the cost of enrolling in honors as a means of implicitly choosing the fraction of students in each track.[3] Given this cost, students' and parents' choices determine the particular composition of each course's tracks.

While the administrator can adjust these incentives separately for each course and cohort, we first consider the administrator's problem for an unspecified course and year and temporarily suppress any dependence of the inputs on course and year. Let f denote the chosen fraction of students in honors. Let θ_q denote the preference weight that the administrator gives to the performance of subgroup q, and let W_q denote the share of students in subgroup q among the chosen course and cohort. While these subgroups could be arbitrary combinations of predetermined observable student characteristics, in our empirical work we will use statewide quintiles of predicted student performance based on their test score histories. The weights may reflect adminstrator preferences for gains by different types of students, the relative amount of pressure they face from different groups of parents or higher-level administrators, or the priorities for academic growth for different observed types generated by local, state, and federal educational objectives (such as those incentivized by No Child Left Behind). Finally, let $E[Y_i(f)]$ and $E[\overline{Y}_q(f)]$ capture the expected test score of student i and the expected mean test score of students in subgroup q, respectively, as a function of the chosen honors fraction f. We assume that the bulk of the information used by the administrator to predict test scores is contained in the subgroup assignment, so that $E[Y_i(f)] \approx E[\overline{Y}_{q(i)}(f)]$. Then, assuming further that administrators seek to maximize some weighted average of student performance, we can write an administrator's problem as:

$$\max_f \frac{1}{N} \sum_{i=1}^{N} \theta_{q(i)} E[Y_i(f)] \approx \max_f \sum_{q=1}^{Q} W_q \theta_q E[\overline{Y}_q(f)] \tag{1}$$

This formulation suggests that the principal does not need to predict exactly which students will switch track when the chosen honors fraction changes nor the impact on any given individual from switching track or experiencing a more selective track. Rather, the principal only needs to understand how shifting f changes the mean performance of each subgroup via the new classroom sorting equilibrium. This insight motivates our approach of aggregating over individual track choice and comparing mean outcomes of different subgroups under

[3] While most of these levers are not observable in the North Carolina administrative data, GPA boosts are an exception. A simple bivariate regression with course, year, and school fixed effects provides suggestive evidence for our assumption: a one point GPA boost that makes a "B" grade in an honors class equivalent to an "A" in a regular class is associated with a highly significant 12 percentage point increase in the share choosing the honors track. Unfortunately, GPA boosts are not reported by a sufficient number of districts to be used to form instruments.

different tracking regimes. In the next subsection we provide assumptions on the sorting equilibrium that justify this simplified approach.

Given the objective function (1) for the administrator, the optimal honors fraction only depends on the degree to which alternative fractions shift test scores of various subgroups, rather than the components of subgroups' mean test scores that are invariant to the honors fraction. Thus, it suffices to focus on the "treatment effect functions" $E[\Delta\overline{Y}_q(f)]$ associated with alternative choices of f:

$$\underset{f}{\text{argmax}}\sum_{q=1}^{Q} W_q\theta_{q(i)}E[\overline{Y}_q(f)] = \underset{f}{\text{argmax}}\sum_{q=1}^{Q} W_q\theta_q E[\Delta\overline{Y}_q(f)] \qquad (2)$$

2.2 Test Score Production

Let Y_{istj} capture the standardized test score of student i in course j taken at school s during year t. We model the educational production function as follows:

$$Y_{istj} = d^h_{istj}\tilde{h}(q_{istj},\epsilon_{istj}|\vec{q}_h,\vec{\epsilon}_h) + d^r_{istj}\tilde{r}(q_{istj},\epsilon_{istj}|\vec{q}_r,\vec{\epsilon}_r) + X^O_{istj}\beta^O + X^U_{istj}\beta^U + \mu_{istj}. \qquad (3)$$

The student's choice of track is represented by the indicator variables d^h_{istj} and d^r_{istj}, with values of 1 signifying enrollment in honors and regular tracks, respectively. Schools that do not offer separate tracks in a given course feature both $d^h_{istj} = 0$ and $d^r_{istj} = 0$. The functions $\tilde{h}(q_{istj},\epsilon_{istj}|\vec{q}_r,\vec{\epsilon}_r)$ and $\tilde{r}(q_{istj},\epsilon_{istj}|\vec{q}_r,\vec{\epsilon}_r)$ capture shifts in achievement from taking the honors and regular tracks, respectively. These shifts are functions of the student's own inputs, which are partly predictable based on the student's observable subgroup q_{istj} but also depend on an unobservable idiosyncratic component ϵ_{istj}. ϵ_{istj} captures deviations in expected performance due to, for example, accumulated skills or effort unaccounted for by subgroup. Such deviations vary not just across students but within students across school-course-year combinations. Importantly, the impact of the track choice on achievement also depends on the peer environment within the chosen track, which is reflected in the dependence of the functions $\tilde{h}(*)$ and $\tilde{r}(*)$ on the vectors $(\vec{q}_h,\vec{\epsilon}_h)$ and $(\vec{q}_r,\vec{\epsilon}_r)$ capturing the subgroups and idiosyncratic contributions of other members of the honors and regular tracks. This flexible formulation of track effects acknowledges that students' production in the classroom will be affected by how the material matches with their ability and how the peer environment interacts with their own ability and effort. Track-specific teacher inputs and course rigor are assumed to be functions of the kinds of students selecting into the track in a given school-course-year, and thus are implicitly captured by the functions $\tilde{h}(*)$ and $\tilde{r}(*)$.

X^O_{istj} and X^U_{istj} capture other observed and unobserved student, school, and course inputs, respectively, that affect i's learning, while μ_{istj} captures measurement error that causes the test score to fail to perfectly reflect the student's learning in the chosen course. Importantly, by imposing that these inputs are additively separable from the inputs that enter the track-specific functions $h(*)$ and $r(*)$, we have assumed they have the same impact on test scores regardless of track. This implicitly requires that the standardized tests used to assess knowledge in each course do not depend on the track chosen, which is true in the North Carolina context we consider.[4] While somewhat restrictive, the additive separability assumption implies that these inputs are irrelevant to the administrator's tracking problem. Thus, we can rewrite achievement in terms of the difference between performance in the chosen track and performance in a pooled version of the course with no tracks:

$$\Delta Y_{istj} = d^h_{istj} h(q_{istj}, \epsilon_{istj} | \vec{q}_h, \vec{\epsilon}_h) + d^r_{istj} r(q_{istj}, \epsilon_{istj} | \vec{q}_r, \vec{\epsilon}_r). \tag{4}$$

where $h(q_{istj}, \epsilon_{istj} | \vec{q}_h, \vec{\epsilon}_h)$ and $r(q_{istj}, \epsilon_{istj} | \vec{q}_r, \vec{\epsilon}_r)$ now capture the contribution of honors and regular tracks, respectively, compared to a trackless environment. Recasting achievement production this way facilitates a focus on the interactions between the student and peer characteristics that are likely to be of primary importance. Note that this formulation is nonetheless less restrictive than many linear specifications in the literature, since it allows the impact of observed and unobserved student ability components q and ϵ to depend on each other and on the choice of track.

2.3 A Simple Model of Student Track Choice

Now consider the student's choice of honors vs. regular track in a course that features only these two tracks. Suppose that each student chooses the track that maximizes his or her test score net of track-specific effort costs, scheduling opportunity costs, and GPA boosts. Let c_{istj} capture student i's idiosyncratic composite cost (measured in test-score utility equivalents) of joining the honors track h relative to the regular track r at school s at time t in course j. Next, let α_{stj} capture a component of the composite cost of the honors track that is common to all students in (s, t, j). Importantly, assume that the administrator has the ability to shift α_{stj} by any arbitrary amount by adjusting the relative GPA boost or homework load in the honors track.

The student's track choice can thus be written as:

$$d^h_{istj} = \begin{cases} 1, & \text{if } \underbrace{h(q_{istj}, \epsilon_{istj} | \vec{q}_h, \vec{\epsilon}_h) - r(q_{istj}, \epsilon_{istj} | \vec{q}_r, \vec{\epsilon}_r)}_{\text{Difference in academic gains}} \underbrace{- c_{istj} - \alpha_{stj}}_{\substack{\text{Effort, convenience,} \\ \text{and grade cost}}} > 0 \\ 0, & \text{otherwise} \end{cases}$$

[4] Furthermore, administrator, parent, and student preferences for high scores help ensure that the curricula for the two tracks do not diverge too far from one another.

Note that because we allow a student's unobserved ability to differentially affect their academic performance in the honors vs. the relative track, we are accommodating the possibility that students may select into courses based on unobserved ability to benefit. Along with peer effects, such "selection on gains" generally complicate efforts to extrapolate from track impacts on marginal students to broader average treatment effects of interest. We show here that peer effects and selection on unobserved gains need not undermine the ability to estimate the key inputs to the administrator's tracking problem.

Next, let $g_{stj}(\epsilon, c|q)$ denote the cohort's joint conditional distribution of students' unobserved ability components and idiosyncratic effort/scheduling costs for any given subgroup q. To simplify notation, we assume that the school cohorts in consideration are large enough and the ability groups are few enough to approximate $g_{stj}(\epsilon, c|q)$ for each q with a continuous joint density. Then we can define $\alpha^*_{stj}(f)$ as the threshold common cost component α^*_{stj} that causes a fraction f of students in the chosen school-year-course to choose the honors track. Specifically, $\alpha^*_{stj}(f)$ is implicitly defined as the solution to the following equation:[5]

$$\sum_q W_{stq} \iint d^h_{istj}(\alpha_{stj}, \vec{q}_h(\alpha_{stj}), q, \vec{q}_r(\alpha_{stj}), \epsilon, c) g_{stj}(\epsilon, c|q) d\epsilon dc = f. \tag{5}$$

Next, we assume that the composition of students across schools, years, and courses is very similar among a large subset $\mathcal{S}$ of school-year-course combinations:

Assumption 1. $g_{stj}(\epsilon, c|q) \approx g(\epsilon, c|q)\ \forall q\ \forall (s,t,j) \in \mathcal{S}$ *and* $W_{stjq} \approx W_q\ \forall\ (s,t,j) \in \mathcal{S}$

Under Assumption 1, as courses become large the threshold cost function $\alpha^*_{stj}(f)$ becomes common among sufficiently similar schools and course-year combinations within schools: $\alpha^*_{stj}(f) \approx \alpha^*(f)$ for all $(s,t,j) \in \mathcal{S}$. Furthermore, because the conditional distribution $g(\epsilon, c|d^h, q)$ also becomes common, the vectors of track-specific peers $(\vec{q_r}, \vec{\epsilon_r})$ and $(\vec{q_h}, \vec{\epsilon_h})$ also depend only on f (through $\alpha^*(f)$) rather than separately on s, t, or j. This in turn implies that $h(q_{istj}, \epsilon_{istj}|\vec{q}_h, \vec{\epsilon}_h) \approx h(q_{istj}, \epsilon_{istj}|f_{stj})$ and $r(q_{istj}, \epsilon_{istj}|\vec{q}_r, \vec{\epsilon}_r) \approx r(q_{istj}, \epsilon_{istj}|f_{stj})$. It also implies that the subgroup-specific probability of choosing honors depends only on f:

$$P(d^h = 1|q_{istj} = q, f) = \iint d^h(\alpha^*(f), \epsilon, c, q) g(\epsilon, c|q_{istj} = q) d\epsilon dc \tag{6}$$

Thus, the implicit choice of f by the administrator (through $\alpha^*(f)$) can serve as a sufficient statistic for the peer composition of both the honors and regular tracks in all

[5] Note that since d^h_{istj} depends on α_{stj} both directly and indirectly through the peer vectors $\vec{q}_h(\alpha_{stj}), \vec{\epsilon}_h(\alpha_{stj})$, we must assume that the track-specific achievement functions $h(*)$ and $r(*)$ are sufficiently insensitive to small changes in peer composition that the fraction choosing honors is monotonically and smoothly decreasing in α_{stj} for each q and spans a large range of fractions for feasible administrator choices of α_{stj}. This ensures that there exist unique solutions to equation 5 for a wide range of f values.

school-year-course combinations where this common joint distribution of ability and costs represents a sufficiently close approximation. Essentially, this assumption rules out heterogeneous treatments across schools or courses for the same honors fraction, so that differences in achievement distributions across schools or courses featuring different honors fractions can be interpreted as (possibly heterogeneous) treatment effects.

In our empirical work, we attempt to make this approximation plausible by 1) removing schools from our sample whose students exhibit a distribution of past performance on state exams that is too far from the state norm, and 2) controlling for the shares of students in the chosen school-course year with predicted test scores in each decile of the statewide predicted distribution, interacted with the student's own quintile of predicted performance.

Note that Assumption 1 is sufficient but not necessary for the fraction in honors to serve as a sufficient condition for peer environment. If, for example, each student's relative performance across tracks depends only on the tracks' relative peer quality rather than separately on the absolute peer quality in each track, then estimates of tracking effects may be unbiased even if comparisons are made between schools or school-course-years with different baseline student quality distributions, as long as these units would feature the same peer quality differences across tracks if they chose the same honors fraction f and controls for the direct achievement effects of cohortwide quality distributions are included (as they are in our empirical work).

Even if the distributions $g(\epsilon, c|q)$ are roughly common among schools, however, they may not be known by any school administrator, since both ϵ and c are unobserved for each student. Thus, any given principal will have a difficult time inferring both $g(\epsilon, c|q)$ and the track-specific achievement functions $h(q, \epsilon|f)$ and $f(q, \epsilon|f)$ from data on student performance.

Note, though, that the administrator's problem (1) only requires as inputs $E[\Delta\overline{Y}_q(f)]$, the subgroup-specific mean test score performance gains as functions of the honors fraction f. Thus, we can exploit the fact that $E[\Delta\overline{Y}_q(f)]$ can be written as a simple weighted average of the expected track-specific performance of the subsets of group q that sort into the honors and regular tracks, respectively:

$$E[\Delta\overline{Y}_q(f)] = P(d^h = 1|q, f)E[h(q, \epsilon|f)|d^h = 1] + P(d^r = 1|q, f)E[r(q, \epsilon|f)|d^r = 1] \qquad (7)$$

Since the conditional expectation functions $E[h(q, \epsilon|f)|d^h = 1]$ and $E[r(q, \epsilon|f)|d^r = 1]$ in (7) depend only on $g(\epsilon, c|q)$ and $d^h(\alpha^*(f), \epsilon, c, q)$, $h(q, \epsilon|f)$, and $r(q, \epsilon|f)$, which are themselves determined by f through $\alpha^*(f)$, $E[\Delta\overline{Y}_q(f)]$ only depends on the school, course, and year through the administrator's choice of f.[6] Since the objects $E[h(q,\epsilon|f)|d^h = 1]$ and $E[r(q,\epsilon|f)|d^r = 1]$ are means of performance among selected samples of students sorting into each track (partly on the basis of unobserved ability ϵ), they are not objects of interest in their own right, and they do not allow the recovery of the full structural functions $h(q,\epsilon|f)$ $r(q,\epsilon|f)$ without much stronger assumptions on either $h(*)$ and $r(*)$ or $g(\epsilon, c|q)$. However, the above progression makes clear that as long as $g(\epsilon, c|q)$ and W_q are roughly stable for each q across courses and time, identification of the structural functions is unnecessary to solve the administrator's problem.

Essentially, one can simply aggregate over the student-level choice of track, utilizing the fact that every student must choose some track, and compare mean outcomes of students in the same subgroup across schools, cohorts, or courses featuring different administrator choices of f to identify the conditional expectation functions $E[\overline{Y}_q(f)]$ for each subgroup q. Importantly, these functions capture not only the achievement gains or losses from students who have their track choice changed through changes to α^*_f but also how changing f alters the peer effects and level of instruction experienced by other members of the subpopulation.

3 Data & Background

We use administrative data provided by the North Carolina Department of Public Instruction for all public schools between 1995 and 2011. These data and their surrounding institutional context have several important features that make it suitable for our analysis.

First, the track associated with each high school classroom is reported for each course, both by school administrators at the beginning of the year and directly by students during assessments at the end of the year. Such dual reporting provides confidence that track is being measured correctly.[7]

Second, the large number of schools, cohorts, and students contained in the North Carolina data ensures that sufficient identifying variation exists to provide properly powered tests of the impact of alternative levels of honors enrollment shares on student performance across the ability distribution. While tracking policy is important because it affects the

[6] $E[h(q,\epsilon|f)|d^h = 1]$ and $E[r(q,\epsilon|f)|d^r = 1]$ are defined by:

$$E[h(q,\epsilon|f)|d^h = 1] = \frac{\iint d^h(\alpha^*(f),\epsilon,c,q)h(q,\epsilon|f)g(\epsilon,c|q)d\epsilon dc}{\iint d^h(\alpha^*(f),\epsilon,c,q)g(\epsilon,c|q)d\epsilon dc} \text{ and} \tag{8}$$

$$E[r(q,\epsilon|f)|d^r = 1] = \frac{\iint d^r(\alpha^*(f),\epsilon,c,q)r(q,\epsilon|f)g(\epsilon,c|q)d\epsilon dc}{\iint d^r(\alpha^*(f),\epsilon,c,q)g(\epsilon,c|q)d\epsilon dc} \tag{9}$$

[7] Naturally there are occasional discrepancies due to students changing track during the academic year or students misreporting the track of their classroom. In such cases we use the school-reported track of their classroom in our analysis, but our results are robust to dropping observations featuring discrepancies.

entire student population in every course, its test score impact per student-course is likely to be relatively small, since much of the variation in student performance is driven by student- and parent-specific factors beyond the school's control. A lack of power has heretofore forced researchers to either focus on the extensive margin of whether to offer any tracking or to pool intensive and extensive margin variation by imposing that track effects are linear in the honors share.

Third, the North Carolina administrative data offers a wide array of observed control variables at the school, teacher, classroom, and student levels. As emphasized in the following section, such rich controls are critical for addressing omitted variable bias stemming from correlations between the honors share and other school, teacher, and student inputs that contribute to test scores. Of particular note are histories of students' standardized test scores during grades 7 and 8 in math, English and (for some cohorts) science. These histories capture differential student preparedness across schools and cohorts that might both influence principal's decisions about honors track size and predict future student performance.

Finally, North Carolina required statewide standardized end-of course exams as part of 11 distinct high school courses during our sample period. Importantly, because the same exams were administered to all schools and all tracks within a school, these test scores represent a common metric by which to compare schools that choose different shares of honors enrollment.

Of course, drawing valid inferences about relative student learning using test scores from different tracking regimes requires that the alignment between the curriculum and the test content does not systematically vary by track. For example, one might be concerned that much of what is taught in the honors version of the course is not tested on the state exam. However, the North Carolina ABC accountability system that was in place throughout the sample period provides strong incentives for teachers teaching tested courses to adhere to the state curriculum. First, schools are rated publicly based on student test score growth on these exams, and underperforming schools are at risk of sanctions and even closure, so that principals risk their reputations and even their jobs when their teachers do not teach what is tested (Ahn and Vigdor, 2014). Second, teachers at underperforming schools are also at risk of losing their jobs, while teachers at high performing schools are eligible for annual salary bonuses of $1,000 to $1,500 (Vigdor et al., 2008). Finally, student performance on these exams contributes a state-mandated minimum of 25% of the student's course grade, so students have an incentive to study the tested material and parents have an incentive to ensure that teachers adhere to the curriculum regardless of track (Zinth, 2012). Hence, in this North Carolina context, the honors track is likely to primarily represent greater depth and difficulty of covered material rather than greater breadth.

We exclude five of the eleven tested courses from our sample due to either a small set of test years (Civics and Economics, Law & Politics), inconsistency in grade level (Algebra 1 is often offered in middle school rather than high school), or the existence of Advancement Placement classrooms, discussed further below (US History and Physics). Thus, our sample consists of standardized scores from the following six courses: Algebra 2, Biology, Chemistry, English 1, Geometry, and Physical Science. Appendix Figure A1, which displays the 2006 statewide distributions of student scores for our final sample from each of the six remaining courses, reveals no evidence of any floor or ceiling effects.[8]

Table 2 examines the tracking options available in each course for all school-year-courses with at least 30 student observations. There exists an honors program in most school-year-course combinations, but remedial programs are rare. Furthermore, the remedial track generally accounts for a very small portion of the student body when it exists (see Figure A2). Given insufficient power to detect the impact of alternative remedial track sizes, we control for the share of students in remedial classrooms (interacted with quintile of predicted performance), but do not estimate a separate treatment effect function for the remedial track. Because most of the courses tested by state standardized exams tend to be offered in 9th and 10th grade, they do not feature an advanced placement (AP) version of the curriculum. The two exceptions are Physics and U.S. History. We drop these courses from our sample, since we fear that teachers in AP classrooms in these courses may adapt their curricula to align more with the AP exam than the North Carolina end-of-course exam, making the latter exam a less accurate measure of learning.[9] Thus, we focus attention on regular and honors tracks, and use the share of students enrolled in the honors track in a given school-course-year as our main independent variable of interest, in alignment with the honors fraction f in Section 2 above. 20% percent of the sample's school-course-year combinations do not feature any tracking, which we code as an honors share of either zero or one, depending on whether the course code indicates honors-level rigor.[10]

3.1 Assignment to Preparedness Quintiles and Restricting the Sample of Schools

The test score histories also provide a basis for assigning students to the observed preparedness types that are necessary for providing a holistic assessment of the impact of alternative

[8] More years are available by request from the authors. No course-year in our sample exhibits bunching around the upper or lower limit of the score range.

[9] We also drop school-year-course combinations featuring classrooms adhering to international baccalaureate standards for the same reason. This proves to be inconsequential, since most schools that offer the IB curriculum are too high-achieving to satisfy our other sample restriction described below that their distributions of student predicted performance plausibly satisfy Assumption 1.

[10] We investigate sensitivity to the treatment of all-honors courses in Section 6.

choices of honors track size. Specifically, we assign each student to a predicted quintile in the statewide performance distribution (with quintile 1 denoting the highest predicted performance) based on the distribution of students' regression indices from a regression of test scores in the sampled high school subjects on grade 7 and 8 English and math scores. We allow the coefficients on these past scores to be specific to the high school course, so that the same student may be assigned to different quintiles for different courses if their past performance indicates different relative strengths in the skills required by these courses.[11] For the sake of brevity, henceforth we refer to these statewide predicted quintiles merely as quintiles, and will be explicit on the occasion in which within-school student rankings are instead used as the basis for assignment to a quintile.

Recall that formal justification for using the fraction in honors as a sufficient statistic for peer environment in each track invoked Assumption 1, which required each school-year-course combination to feature the same joint distribution of abilities (observed and unobserved) and effort costs among students. Clearly this condition will not be satisfied exactly; however, our method only requires that these joint distributions are sufficiently similar across schools and particularly across cohorts and courses within schools so that peer environments would be comparable if honors fractions were equalized. More specifically, we require that comparisons between such units featuring exogenously different honors fractions are informative about how each unit's achievement distribution would change if it were to adjust its own honors fraction.

However, a less selective honors track may result in a considerably different ability distribution among students choosing the track at an extremely privileged school relative to a school with few resources and struggling families. To gauge the scale of the problem, Appendix Figure A4 looks at how many quintiles students would need to shift, on average, in order for each schools' distribution of student preparedness quintiles to match the statewide (uniform) distribution of predicted quintiles. Panel A shows that the majority of schools appear to have nearly uniform distributions, but there is a substantial right tail of schools with quite skewed distributions. However, such schools tend to be quite small, so that the student-weighted distribution (Panel B) displays a much smaller tail. We restrict the sample to schools which require fewer than 0.5 quintile changes per student to match the uniform distribution, which removes about 30% of the observations from the original sample. Panel A of Appendix Figure A4 shows the histograms of the six schools with required per-student quintile changes closest to and less than one half. While this sample restriction ensures

[11] Specifically, we estimate $Y_{istj} = English7_{istj}\beta_{1j} + math7_{istj}\beta_{2j} + English8_{istj}\beta_{3j} + math8_{istj}\beta_{4j} + \epsilon_{istj}$. We then assign course-specific quintiles based on the distribution of $PredictedScore_{istj} = English7_{istj}\hat{\beta}_{1j} + math7_{istj}\hat{\beta}_{2j} + English8_{istj}\hat{\beta}_{3j} + math8_{istj}\hat{\beta}_{4j}$. Results are robust to the inclusion of science test scores; however, science scores are only available for a small number of years.

plausible comparability among schools, it may limit the external validity of our estimates for schools with very low or very high student past performance. As a robustness check, we also consider a specification where the above metric for the spread of student quality is less than one third. Panel B of Appendix Figure A4 displays the quintile histograms for the marginal schools at this higher standard.[12]

In addition to restrictions placed on the sets of courses and schools, we also drop all high school test scores from students with missing 8th grade math or English test scores (since they cannot reliably be assigned to a predicted performance quintile), and we drop all courses offered prior to 2000 to ensure that 7th and 8th middle school test score histories exist for nearly all students in the remaining courses in the sample. Finally, we require each school-course-year in the sample to feature at least 30 tested students so that average characteristics and average performance by quintile are subject to minimal measurement error. After all of these sample restrictions, our baseline sample contains 2,735,153 test scores from 355 high schools and 17,971 school-year-course combinations.

3.2 Summary Statistics

If Assumption 1 holds, each principal is perfectly informed about $E[\overline{Y}_q(f)]$, and each has the same preference weights θ_q, then each principal's optimal choice of f to solve (2) would be the same, and there would be no identifying variation in f. Appendix Figure A3 allays this fear by displaying the distribution of honors shares for the six courses in our final sample among those school-year-courses with honors tracks (80% of our sample). Every subinterval between 0.1 and 0.6 shows frequent use in all six courses, and Chemistry features a nontrivial share of school-year combinations with more than 60% of students in honors. Furthermore, 91.2% of schools in the sample feature both course-years with multiple tracks and course-years with only a single track, while only 4.5% never feature multiple tracks and 4.2% always feature multiple tracks. Among school-years, 49.6% feature only multi-track courses, 4.8% feature only single track courses, 55.4% have courses with multiple tracks and courses without multiple tracks. This suggests that administrators and department heads may vary in their preference weights θ_q, hold differential beliefs about $E[\overline{Y}_q(f)]$, or imperfectly set the cost of taking honors when targeting their preferred honors share. Given the dearth of convincing evidence from the literature on the particular tradeoffs associated with different honors track sizes, varied beliefs would not be surprising.

Table 1 decomposes the variance in the honors fraction f among school-year-course combinations in our estimation sample. Unconditionally, differences in school means of honors

[12] North Carolina ranks toward the middle of U.S. states for educational performance, suggesting that our results should be externally valid for most schools throughout the U.S. (U.S. News (2019)).

fractions (pooling across courses and years) account for 40.3% of the total variance, while year-specific deviations from the multi-year mean account for another 11.6%, and course-specific deviations from the school-year mean account for the remaining 48.0%. Adding our baseline control variables (described in the next section) removes about 55% of the total variance, and slightly alters the contributions of the three decomposition components to the residual variance (to 35.2%, 17.5%, and 47.3%, respectively). When evaluating robustness to our baseline specification later in the paper, we consider several specifications that systematically omit subsets of these components.

Table 3 provides means and standard deviations of a subset of the controls used in our baseline specification by category of honors enrollment share based on our estimation sample of school-year-course combinations. Relative to school-year-courses without tracking, those with smaller honors tracks ($<$ 35% of students) have very similar distributions of student demographics, teacher experience, and parents' education and only slightly inferior past achievement. The one major difference is that school-year-course combinations from small schools (low average cohort size) are more likely not to offer an honors track. Relative to both school-course-years without tracking and with smaller honors tracks, those with larger honors tracks ($>$ 35% of students) tend to have students with somewhat higher past test scores and slightly more educated parents. Overall, though, the distributions of characteristics across these three honors fraction categories overlap considerably, so at first blush it seems that much of the variation in honors fractions is not directly tied to the composition of students or teachers at the school-year-course.

Figure 1 plots in blue the average honors enrollment rate for bins of the coursewide honors fraction separately by within-school (rather than statewide) quintile of predicted performance. We also plot in red the enrollment rate one would expect if students were perfectly sorted to tracks based on their relative predicted performance. Perfect sorting on predicted performance would result in a line with a slope of 5 within the interval of honors fraction corresponding to the chosen quintile ([0,.2] for quintile 1, [.2,.4] for quintile 2, and so on) and a flat line with zero slope elsewhere. The final cell in Figure 1 shows the pooled distribution of honors fractions among all school-course-years in the sample.

Students in top quintiles unsurprisingly enroll in honors at much higher rates than students in other quintiles. Nonetheless, the plots of observed honors enrollment patterns reveal quite imperfect sorting, suggesting that unobserved ability and heterogeneous effort costs do play an important role in track choice.[13] For example, a course with 20% of students in honors tends to be chosen by only 56% of top quintile students (rather than the predicted

[13] Various measures of ranking on observed ability, including shorter or longer performance history on alternative sets of tests, all show high levels of sorting on unobservables.

100%) and by 30%, 13%, 5%, and 1% of students in quintiles, 2-5, respectively. Similarly, a course with 60% of students in honors still has 25% of quintile 2 students enrolling in the regular track while 15% of students in quintile 5 enroll in honors. For quintiles 2 and 3 in particular, these unobserved sorting factors play a large role in track choices, as both quintiles have significant honors and regular track enrollment rates for all coursewide shares of students in honors.

Figure 2 plots the average contemporaneous test score performance in test score standard deviations from the statewide mean for bins of the share of students in honors, separately by preparedness quintile. If we disregard the very noisy values for shares of honors above 65% that are observed extremely infrequently in the data, we see that regardless of quintile, the average performance is at or near its peak when the share of students in honors is around 30-40%. However, in order to verify that this finding reflects a true uniformly optimal honors share rather than a spurious correlation between honors fraction and other school and student inputs, we now describe our more rigorous estimation procedure.

4 Empirical Approach

4.1 Baseline Specification

Our primary specification is an aggregated version of the education production function (3) from Section 2.2. Recall from Section 2.3 that the objects of interest, quintile-specific treatment effect functions of the honors fraction ($E[\Delta\overline{Y}_q(f_{stj})]$), are aggregate objects that only vary at the school-year-course-quintile level. Thus, because the control variables X^O_{istj} enter linearly and are assumed to be additively separable from $E[\Delta\overline{Y}_q(f_{stj})]$, we can estimate the parameters of interest in (3) at the school-year-course-quintile level without introducing any bias and with minimal lost efficiency. Furthermore, such aggregation allows us to avoid selection problems from individual track choice. Thus, our primary specifications all take the following form:

$$\overline{Y}_{stjq} = E[\Delta\overline{Y}_q(f_{stj})] + X_{stjq}\beta^X + \Gamma_{stjq}\beta^\Gamma + \omega_{stjq}. \tag{10}$$

Each specification implements $E[\Delta\overline{Y}_q(f_{stj})]$ as a set of quintile-specific, flexibly parameterized functions of f_{stj}, the fraction taking the honors version among all students taking course j in school s in year t, with the chosen functional forms varying across specifications. Our baseline specification imposes that $E[\Delta\overline{Y}_q(f_{stj})]$ for each quintile takes the form of cubic function:

$$E[\Delta\overline{Y}_q(f_{stj})] = \gamma^{lin}_q f_{stj} + \gamma^{sq}_q f^2_{stj} + \gamma^{cb}_q f^3_{stj} \tag{11}$$

Equation (11) does not restrict the treatment effect to be the same when placing zero students in honors classes and when placing all students in honors classes, even though both scenarios arguably represent an absence of tracking, since a designation of "honors" may connote higher standards and a slightly more rigorous curriculum even when the student population is the same.[14] This functional form allows the location and level of the achievement maximum (and/or minimum) to be determined by the data while still exploiting the efficiency gains from summarizing a function with three parameters. We also present results from other functional forms, including restricted cubic and quartic specifications, as robustness checks in Section 6. Importantly, the coefficients $\vec{\gamma}^{lin} = \{\gamma_1^{lin}, ..., \gamma_5^{lin}\}$, $\vec{\gamma}^{sq} = \{\gamma_1^{sq}, ..., \gamma_5^{sq}\}$, and $\vec{\gamma}^{cb} = \{\gamma_1^{cb}, ..., \gamma_5^{cb}\}$ are quintile-specific in order to capture heterogeneous effects among different levels of student preparedness.

X_{stjq} contains a vector of observed school, teacher, and quintile-mean student control variables that in some cases are specific to the course j and/or year t. Γ_{stjq} represents a design matrix or matrices capturing fixed effects for various one- and two-dimensional combinations of (s, t, j, q). Thus, the theoretical object X_{istj}^O from equation (3) is operationalized as $[X_{stjq}, \Gamma_{stjq}] \equiv \overline{X}_{stjq}^O$ in equation (10). $\omega_{stjq} \equiv \overline{X}_{stjq}^U \beta^U + \overline{\mu}_{stjq}$ captures the combined impact of mean unobserved student, teacher, and school inputs and mean test score measurement error at the (s, t, j, q) level.

Our baseline specification pools all the variation in the honors fraction f_{stj} that occurs between schools, between years within schools, and between courses within school-year combinations. We pool partly to generate maximally precise estimates of the parameters $\vec{\gamma}^{lin}$ and $\vec{\gamma}^{sq}$, but also because there are plausible sources of exogenous variation at each level.

For example, exogenous variation at the school level might occur because smaller schools cannot support the multiple number of classrooms per course that tracking requires, and surpassing the cohort size thresholds beyond which additional classrooms can be supported may not otherwise affect student outcomes (beyond simple class size effects for which we include separate controls). Similarly, due to differential parental pressure, personal pedagogical beliefs, or accountability pressure, principals may differentially weigh performance by different quintiles or have incorrect beliefs about the impact of tracking for reasons unrelated to any of the other unobserved inputs affecting their students' performance. Switching costs from new course preparation for certain teachers may cause schools not to track even when other similar schools do so, perhaps because of differences in the past course histories of their teachers.

[14] Three of the largest sources of achievement changes from alternative honors enrollment shares are the same when the fraction of students in honors is equal to zero or one: peer effects, allocation of teachers among tracks, and specialized instruction.

Exogenous time series variation in the honors fraction occurring within schools include natural idiosyncratic changes in cohort size that require adding or removing classrooms or deterring or encouraging students to take honors to avoid exceeding classroom capacities. It might also include idiosyncratic variation in the past course preps of newly hired teachers. Exogenous between-course variation stems from idiosyncratic pedagogical preferences by department heads or slightly different student demand for different courses due to scheduling conflicts (which can also vary across cohorts).

On the other hand, the variation in honors fractions at each level is likely to contain an endogenous component as well. Schools may be more likely to dedicate a larger share of course capacity to the honors track when they serve well-prepared students, as suggested by the summary statistics above. And student demand for honors in a particular year may exceed administrator expectations when a cohort is particularly able or motivated. Furthermore, in addition to correlations with unobserved components of student composition, unobserved teacher and school inputs can also be correlated with or actively cause changes in the honors share. For example, perhaps the principals most willing to raise standards for students by encouraging the honors track also invest more time and resources in other achievement-raising policies. Or a school that has particularly effective teachers in a given course may wish to reward them by allowing them to teach honors versions more frequently, and thus increases the share of that course's classrooms that offer the honors version.

Unfortunately, instruments that isolate only the exogenous sources of variation are either not available or are too weak to detect the hypothesized heterogeneity in achievement impacts across the student ability distribution. Since we estimate the model via ordinary least squares, in order for our baseline estimates to be unbiased, unobserved inputs contained in the error term must be uncorrelated with the honors share f_{stj} as well as its square and cube, conditional on the controls X_{stjq} and Γ_{stjq}.[15] Thus, our baseline specification relies heavily on the richness of the North Carolina administrative data to provide a set of powerful controls that absorbs the most plausible sources of endogeneity.

To address bias from correlation with student composition, in our baseline specification the vector X_{stjq} contains student ability and preparedness measures: both the share and mean predicted score of each decile of the preparedness distribution among students in the school-course-year based on grade 7-8 math and English test scores. These controls are interacted with the full set of course indicator variables to allow differential predictive power in different courses. We further control separately for school-course-year, school-year, and samplewide school averages for grade 7 math and reading and grade 8 math and

[15] Specifically, we assume $E[\omega_{stjq} f_{stj} | X_{stjq}, \Gamma_{stjq}] = 0$, $E[\omega_{stjq} f^2_{stj} | X_{stjq}, \Gamma_{stjq}] = 0$, and $E[\omega_{stjq} f^3_{stj} | X_{stjq}, \Gamma_{stjq}] = 0$.

reading, each interacted with course dummies, samplewide school averages of grade 8 science scores, share with gifted status, and mean grade level. In addition, we control for student demographics (shares ever reporting limited English and race shares and long-run school averages of these shares), and family socioeconomic indicators (parental education category shares and their school-year averages). Note that introducing a variety of such measures as aggregate shares at school-course-year, school-year, or school levels rather than at the individual-level makes them stronger controls; at such higher levels of aggregation, they implicitly control for unobserved school and cohort characteristics by potentially spanning the common amenity space that lures certain kinds of observably and unobservably superior or inferior students to the school or course within the school (Altonji and Mansfield, 2018).

To address endogeneity from teacher inputs and remaining school inputs beyond those that affect student composition, X_{stjq} also includes proxies for teacher quality (experience category shares and mean certification scores) and a control for the school's total enrollment for the chosen cohort. We also control for both the number of classes offered, the mean class size, and the share in the remedial track at the (s, t, j) level, important inputs that may sometimes move in tandem with otherwise idiosyncratic changes in honors shares. Thus, we intend for our treatment effect functions to isolate the impact of changing the honors share conditional on class size (i.e. from converting a class from regular to honors track) rather than combining the impact of simultaneous changes in both class size and honors share (i.e. from adding an extra regular track class to the existing roster of classes).

Finally, in our baseline specification Γ_{stjq} includes a full set of course-quintile-year effects, which removes potential bias from secular changes in statewide course curricula or the relative difficulty of standardized test questions that target different parts of the ability distribution that may be correlated with statewide trends in honors fractions. The sample means and standard deviations of many of these controls by category of honors share are provided in Table 3.[16]

While we believe that these controls adequately address a multitude of potential endogeneity problems, we nonetheless consider three additional specifications that exchange precision for arguably superior isolation of exogenous variation to partially address remaining concerns about simultaneity bias or omitted variable bias.

Our preferred specification adds a set of school fixed effects to Γ_{stjq}, so that the parameters of interest are only identified by differential changes in honors fractions and achievement across cohorts and courses within schools. While school fixed effects address the particularly pressing concerns stemming from greater honors enrollment shares causing or responding to

[16] For the small subset of controls featuring a nontrivial share of missing values, we replace these values with zeros and introduce corresponding missing value indicator variables.

student sorting among schools, adding these fixed effects also generates noisier estimates, since between school variation accounts for 35.2% of residual identifying variation net of controls in our baseline specification.

The second alternative specification we consider uses the honors share of the previous cohort in the same school-course combination, along with its square and cube, as instruments for the corresponding contemporaneous share and its square and cube. The exclusion restriction for this IV specification requires that the past share of students in honors affects current test scores only through inertia in the share of honors over time conditional on controls. This IV approach purges estimates of any endogenous honors share response to unobservable changes in cohort quality or teacher staffing within a school. Implicitly, this specification puts greater emphasis on between-school and within-school/across-course variation at the expense of time-series variation.

Inspired by the "Maimonides rule" identification strategy of Angrist and Lavy (1999) and others, the third alternative specification uses the share of *classrooms* that are assigned to the honors track as an instrument for the share of *students* who take the honors track (with corresponding instruments for the square and cube of the share). Essentially, the high per-pupil staffing cost of offering class times with very few students may limit the set of viable honors fractions a school can choose. For example, a school with around 75 students in a cohort may have too many students for two classes and too few for four classes, so that the only feasible shares of honors classes are 0, .33, and .66. A larger cohort of 90 students might force the school to allocate four classes, leading to honors class shares of 0, .25, .5, or .75. Thus, the discreteness inherent in forming classes may cause relatively small differences in cohort sizes to cause substantial arguably exogenous differences in honors shares (conditional on controls for class size). This specification removes variation in relative sizes among honors vs. regular track classrooms that might be endogenously responding to the distribution of unobserved student quality.

While no single one of these alternative specifications is intended to allay all fears about bias in isolation, collectively they can potentially provide considerable reassurance if results are consistent across all of these specifications, since they rely on very different mixes of the three levels of variation. After all, if substantial endogeneity biases exist, they would need to operate with the same force (relative to the exogenous variation) at each level of variation *and for each quintile of student preparedness* in order to generate such consistency. Put another way, our flexibility in allowing separate cubic functions of the honors fraction for each quintile also provides more opportunities for sizeable endogeneity biases to reveal themselves through distinct results patterns across specifications that magnify or reduce the role these sources of endogeneity play in driving results.

We cluster standard errors at the school level in each specification, both to be conservative and because we expect considerable autocorrelation in errors across course-years from the same school. In addition, each specification weighs observations by the share of the students at the school-year-course that are in each quintile, so that all school-year-courses are weighted equally.[17]

5 Results

5.1 Quintile Treatment Effects

The red curves of Figure 3a display predicted values of treatment effects on achievement scaled in standard deviations of standardized test scores for a dense grid of potential honors fractions from our baseline cubic specification (Panel A), which pools all sources of residual variation in the honors fraction among school-year-course combinations, while Figure 3b plots curves for the school fixed effects specification that isolates variation in honors policies that either change over time and/or vary by course within a school. Note that all predicted values capture treatment effects for alternative honors fractions relative to an absence of tracking, which has been normalized to zero. Dashed blue lines indicate the upper and lower bounds on 95% pointwise confidence intervals that were created by using the delta method to convert the variance-covariance matrix associated with point estimates for the cubic parameters $\vec{\gamma}$ into confidence intervals for each predicted value along the grid.[18] The bottom right cell in the figure displays the support of the honors share distribution for school-year-courses that feature an honors track.[19] Note that there is limited support among honors programs with shares greater than 65% or between 0 and 15%, so our predicted values in these ranges are primarily driven by our functional form assumptions. Consequently, we focus on interpreting the level and shapes of the treatment effect functions between 15% and 65%. Table 4 provides the predicted values and the associated 95% confidence intervals separately by quintile for several candidate honors fractions that underlie Figure 3 and Figure 4. Appendix Table A1

[17] A weighting scheme based on the number of students rather than within-school-year shares would prioritize the efficacy of administrators' actions at large schools over smaller schools. Given that we are interested in providing inputs to principals of all school types, we prefer weighting schools rather than students equally. As per the recommendations of (Solon et al., 2015), specifications are available upon request in which weights proportional to the number of students in the school-year-course quintile combination. Point estimates and standard errors are similar for the different weighting schemes.

[18] We chose pointwise confidence intervals rather than confidence bands because we are generally comparing predicted values at particular honors shares against the absence of tracking, rather than evaluating joint hypotheses involving predicted values over a continuous range of honors fractions, such as whether there exists any nonzero honors fraction that makes quintile 2 worse off than the absence of tracking.

[19] Note that the regressions underlying the plotted treatment effect functions also include the 20% of school-year-courses that do not feature any tracking.

provides the underlying parameter estimates $\vec{\gamma_q}$ for each quintile q for these specifications.

Starting with quintile 1, we observe that top students benefit significantly from selective honors programs: for our baseline specification, when the treatment effect function peaks at an estimated honors share of .29, quintile 1 students gain an estimated .068 standard deviations in state test score performance relative to the absence of tracking. Adding school-fixed effects (our preferred specification) does not change the peak's location and decreases its height only slightly to .058. This gain is equivalent to the predicted increase in student achievement associated with switching from a high school teacher of median effectiveness to a 63rd percentile teacher (Mansfield, 2015). However, in both the baseline and school FE specifications, these gains quickly dissipate as the honors fraction increases beyond 35%. Since around 78% of quintile 1 students will enroll in honors if it contains at least 35% of their cohort, the sharp decrease in gains as honors becomes less selective is likely due to the dilution in peer quality within the honors track, perhaps combined with smaller and smaller gains from switching track for the remaining marginal students. Imberman et al. (2012) found that high achieving students are especially sensitive to peer effects, potentially explaining why quintile 1 experiences such a pronounced decrease as the share of students in honors is increased.

Students in quintiles 2 and 3 also seem to benefit from tracking, but their gains tend to be smaller than and to peak at higher honors enrollment shares than quintile 1. Relative to the absence of tracks, the gains for quintiles 2 and 3 from the existence of an honors track in the baseline specification rise until peak gains of about 0.045 SDs and 0.041 SDs, respectively, when 37% and 46% of all students are choosing honors. Adding school fixed effects shifts the peak location and value for quintile 2 to 43% and .034 SDs, while quintile 3's treatment effect function rises until reaching a value of .035 SDs at around 55% of students, and plateaus thereafter. Interestingly, these peaks occur at fractions where large shares of students in these quintiles are near the margin of choosing honors: around 55% of quintile 2 students and 29% of quintile 3 students generally enroll in an honors track that serves 40% of the cohort, with these shares continuing to rise significantly as the coursewide share of students in honors increases beyond 40%.

Several competing mechanisms are potentially at play for these quintiles. As the honors track increases from a very small size to a moderate size, students from these quintiles are likely to be the marginal students, and the pedagogy in the honors track is likely becoming better and better aligned with their desired pace. The regular track is beginning to lose high quality peers, but is still likely to be fairly well aligned with the desired pace for quintile 3 students. As honors selectivity continues to fall, however, there are more inframarginal quintile 2 and 3 students already in the honors track who are experiencing dilution, and the median student in the regular track may increasingly require a slower pace than is optimal for the remaining quintile 2 students and to a lesser extent quintile 3 students.

Decomposing these competing mechanisms to isolate how each incremental expansion of the honors track affects marginal students, inframarginal honors track students, and inframarginal regular track students within each quintile would require strong assumptions on the degree to which unobservable ability vs. scheduling costs is driving students' selection of track. Indeed, the appeal of our approach is that it can provide the policy-relevant inputs for administrators without requiring questionable assumptions about student sorting to tracks. Thus, we do not attempt such a decomposition here.

Quintile 4 students seem to be fairly insensitive to the size of the honors track, particularly when school fixed effects are included: the estimated function never rises above .008 nor falls below .002 for any honors share between 0 and 70%. A test of equivalence of a two track menu with a trackless course never approaches rejection with 95% confidence for any honors share between 0 and 70%.

In both specifications quintile 5 exhibits losses relative to a no tracking regime that begin around a 30% course honors share and increase in magnitude to around .03-.04 SDs as the honors program grows to 60%, beyond which the support grows thin. The losses achieve statistical significance at the 90% level around an honors share of 50% in each case, but fall just short of significance at the 95% level. Note, though, that part of the statistical imprecision captured by such tests and by the somewhat wide error bounds plotted in the figure stems from choosing trackless courses, which are observed somewhat infrequently, as the normalized group. This was done to ease comparisons between tracked and trackless courses. However, if we focus exclusively on the intensive margin, in both the baseline and school FE specifications we can reject at the 95% level the hypothesis that the treatment effects of 30% and 60% honors enrollment shares are equal for quintile 5 (the same is true for quintile 1).

The deterioration in quintile 5's predicted performance as the honors track expands is consistent with the peer effect literature that has found that lower achieving students are the least sensitive to the positive peer effects from the highest ability students (Imberman et al., 2012; Mehta et al., 2019; Fruehwirth, 2013; Fu and Mehta, 2018). Although having a small honors program decreases the average peer quality for the overwhelming majority of bottom quintile students who do not enroll in honors (over 95% remain in the regular track with a 40% cohort-wide honors share), the compositional changes may be offset by a better-paced class. However, perhaps when the honors program grows beyond 40%, the bottom quintile students who do not enroll in honors (still around 85% when the cohort-wide percent in honors is 60%) share the classroom with fewer middle tier students with whom they might profitably interact.

We next consider the two additional specifications introduced in Section 4. Figure 4a presents results from the IV specification that uses lagged course-specific honors shares as instruments for current honors shares. It seeks to remove cohort-specific variation at each school while leaving both stable between-school and stable between-course within-school differences in honors enrollment shares. It is motivated by the idea that school and department administrators may have idiosyncratic preferences or beliefs about honors efficacy that systematically shape their default choices of honors shares across years. Since there is a large persistent component of honors shares across years, the first stage is quite strong.[20] This specification yields point estimates that feature slightly higher peaks than the baseline specification. The higher peaks could simply reflect either sampling error or a slight upward bias in the between-school variation that accounts for a larger share of identifying variation.

Figure 4b displays the results of altering the school FE specification by using the share of classrooms allocated to honors (and its square and cube) as instruments for the share of students allocated to honors (and its square and cube). The first stage is extremely strong,[21] and all of the treatment effect functions from this IV specification have peaks/nadirs that are within .01 of their counterparts from the preferred school FE specification, as well as nearly indistinguishable shapes. This suggests that possibly endogenous deviations in class sizes among honors classes relative to regular classes are not driving our results.

Perhaps most importantly, though, all four specifications share four qualitative features: 1) students in the top quintiles benefit significantly from honors programs containing less than 30% of the student body; 2) students in the 2nd and 3rd quintiles still benefit from the existence of tracking, but less than quintile 1, and they also benefit slightly from expansions of the honors track beyond 30% of the student body; 3) students in the 4th quintile are relatively unaffected by tracking programs, regardless of the fraction of students in honors; and 4) students in quintile 5 are on average unaffected by honors programs with less than 40% of the student body in them and hurt by honors programs with more than 40% of the student body in them. As emphasized above, such consistency is unlikely to occur if endogeneity were driving the results, since different sources of endogeneity would need to cause the same pattern of bias across the interval of honors shares for all five quintiles of the preparedness distribution.

On one hand, it appears that tracking may feature an equity/efficiency tradeoff on the extensive margin, particularly when considering small honors programs serving 20% to 40%

[20] The Kleibergen-Paap Wald F statistic associated with the first stage is 109.2, far above standard critical values from Stock and Yogo (2002) for rejecting a null hypothesis of weak instruments.

[21] The Kleibergen-Paap Wald F statistic associated with the first stage for the class share IV approach is 401.0.

of students. The bulk of better-prepared students clearly benefit relative to the absence of tracking, while a smaller population of the least prepared students either do not benefit or suffer slightly. On the other hand, our results suggest a more complicated tradeoff structure on the intensive margin: small honors programs (between 20% and 40%) benefit both the top and bottom quintiles relative to large honors programs ($> 40\%$), at the expense of slightly smaller (and statistically insignificant) gains for the third quintile, who are most likely to be on the honors margin in larger programs. Interestingly, one can potentially reconcile our results with papers finding that introducing tracking does not harm any students if the samples in those papers primarily contain schools that have small honors programs (Zimmer, 2003; Figlio and Page, 2002; Pischke and Manning, 2006). Similarly, one can also potentially reconcile our results with papers finding that honors programs help top students and hurt bottom students if those papers sampled a greater share of schools with larger honors programs (Betts and Shkolnik, 2000; Hoffer, 1992; Argys et al., 1996; Epple et al., 2002).

Note that the general decline in efficiency as honors tracks expand beyond 40% seems unlikely to be attributable to a consistent negative correlation with unobserved student quality that occurs at all three levels of variation. After all, recall from Table 3 that, if anything, the observed student characteristics (in particular the distribution of past test scores and parents' education) seem more favorable for school-year-course combinations featuring honors shares above .35. Thus, to generate the estimated decline spuriously, one would need mean values of observed and unobserved favorable student characteristics to be negatively correlated across school-year-courses featuring different honors shares, which would conflict with the predictions of standard models of student sorting.

5.2 Administrator's Problem

Armed with the estimates just presented of the quintile-specific treatment effect functions $\{\hat{E}[\Delta\overline{Y}_q(f)]\}$, we can now reconsider the administrator's problem (2) from Section 2.1. Recall that solving for the optimal choice of honors selectivity also requires supplying weights $\{\theta_q\}$ capturing the relative importance the administrator places on achievement gains from each quintile of the student preparedness distribution. We consider two sets of weights. The first set weighs all quintiles equally ($\theta_q = \frac{1}{5}\ \forall q$), while the second set strongly prioritizes bottom quintiles, so that test score gains for quintiles 1, 2, 3, and 4 are weighted at 20%, 40%, 60%, and 80% of gains for quintile 5 respectively ($\theta_q = \frac{q}{15}\ \forall q$).[22]

Figures 5a and 5b show the average net student gains as a function of the honors fraction under equal weighting of quintiles, based on the estimates from the baseline and school FE

[22] Additional weighting schemes are available upon request from the authors.

specifications, respectively.[23] Assuming a uniform distribution of quintiles, the maximized gains of 0.035 SDs and 0.024 SDs relative to the absence of tracking occur when honors tracks contain 27.4% and 29.3% of students, respectively. The graphs in figures 5c and 5d display weighted average gains with the second set of weights that prioritize students in bottom quintiles. Notably, the maximum weighted average gain occurs at even more selective honors programs (23.8% and 24.4% honors enrollment shares), though with slightly lower peak weighted average impacts (0.026 and 0.014 SDs). More generally, tracking schemes in which honors accounts for 20% to 35% of enrollment dominate those with larger honors tracks for any weighting scheme that places at least 10% weight on each of the five quintiles and at least 20% on quintile 5. In other words, further increases in the share of students in honors beyond 35% generate consistent decreases in aggregate achievement gains for every reasonable weighting scheme over the remaining support of the data. The robustness of the optimal honors program size across weighting schemes is driven by gains for the top 60% of students from small honors programs relative to the absence of tracking and the lack of negative effects from small honors programs on students in the bottom 40% of the preparedness distribution.

Figure 6 displays the average effect for the two IV specifications under both weighting schemes. The lagged course-specific IV specification in Figures 6a and 6c features slightly lower optimal honors shares of around 20% (22.3% for equal weighting and 20.2% for compensatory weighting) along with somewhat larger point estimates for the weighted average gain (.054 and .045). The IV specification based on shares of honors classrooms displayed in Figures 6b and 6d features honors shares of 28.1% and 20.9% under equal and compensatory weighting, respectively, with somewhat smaller corresponding weighted average gains of .018 SDs and .009 SDs.

An aggregate gain of around .025 SD from introducing an honors track serving around 25% of students may seem relatively small; holding the baseline test score distribution fixed as a reference point, it would move a student at the statewide median to the 51st percentile. However, this mean gain includes all students in the cohort for every course in which tracking is introduced. Also, the small value may be misleading given that the lion's share of achievement variance is determined by parents, innate student ability, and previous schools and teachers. Thus, it represents a considerable change in the value added of the high school. For example, using the estimates of Branch et al. (2012), it is equivalent to replacing a principal of median quality with one at the 60th percentile of the principal quality distribution.

[23] Confidence intervals for the values of the administrator's objective function are also generated using the delta method.

Furthermore, recent papers by Chetty et al. (2014a,b) and Carrell et al. (2018) analyzing changes in teacher quality and peer quality, respectively, have shown that policies generating modest short-run academic gains can produce substantial impacts on later life outcomes. Since teacher reallocation and specialization and changes in peer composition are two of the mechanisms through which honors track size is hypothesized to affect test scores, it seems plausible that tracking-induced achievement gains might similarly translate to later outcomes. While our data do not contain long-run outcomes of interest, we can perform a rough projection of the effect of our estimated test score gains on future earnings by assuming that test score gains from varying the size of honors programs have the same effect on age 28 earnings as the test score gains from improvements in teacher quality found in Chetty et al. (2014b,a).

Under this assumption, an initially trackless school that introduces optimally sized honors tracks (29.2%, from the equally-weighted school FE specification) for each core course in the sample could expect their students' earnings at age 28 to increase by an average of 1.4%.[24] For a high school class of 100 students near the age 28 median income, this implies an increase in aggregate age 28 earnings of over $50,000. This estimate would grow further if other courses not tested, such as English classes beyond English 1, enjoyed similar gains from tracking.

However, these average forecasted gains ignore important heterogeneity among the different quintiles of preparedness: quintile 1 is predicted to gain 3.3%, while quintiles 2-4 only gain 1.8%, 1.5%, and 0.5%, respectively, and quintile 5 would lose less than 0.1%.

Of course, many schools already feature tracks near the optimal size for most of their courses. However, there remain a substantial share of school-year-courses in our sample that either do not use tracking or feature honors track sizes well outside the optimal range. If all schools in our sample switched from their current honors program size to an honors program with 29.2% of the student body in it, our estimates suggest that the average North Carolina student would experience a test score gain of 0.006 SDs (about the same amount as switching from the median teacher to a 51.4th percentile teacher (Mansfield, 2015)). Since North Carolina averages about 100,000 students per cohort, this corresponds to an aggregate statewide increase in age 28 earnings of $12.7 million. Again, though, the gains vary substantially across quintiles, with quintile 1 experiencing expected gains of .016 SDs ($6.75M), and quintiles 2-5 experiencing gains of .005 SDs ($1.93M), .002 SDs ($0.85M), .002 SDs ($0.88M), and .006 SDs ($2.35M). Using the 24.4% optimal honors share associated with

[24] This calculation assumes for simplicity that all students would have the 2018 median income at age 28 of $36,910 in the absence of tracking, and that test score gains from each subject can be translated to earnings gains and then aggregated across subjects.

the compensatory weighting scheme would produce gains of .01 SDs ($3.99M) for quintile 5 while still yielding gains relative to the status quo for all other quintiles except the 3rd, which roughly breaks even.

Two interesting patterns are worth noting. First, every quintile of the preparedness distribution would benefit on average from a statewide shift to the optimal honors share, though individual students may be worse off. Second, despite generally performing worse in tracked environments, the least prepared quintile enjoys quite substantial gains from adopting the optimal honors share, since many such students are currently at schools featuring even higher honors shares.

Clearly, such back-of-the-envelope calculations are quite speculative; for example, they ignore heterogeneity in the returns to academic skills across quintiles, general equilibrium effects from aggregate shifts in quality-adjusted labor supply, as well as the substantial costs (and possible class size benefits) associated with staffing multiple tracks at small schools.[25] Nonetheless, they serve to highlight the possibility that small gains per student-course from a superior tracking system can aggregate to large statewide productivity gains when combining effects across many courses, schools, states, and years.

6 Robustness Checks

In order to maximize power, all of the results presented to this point have imposed that each quintile's expected achievement follows a cubic function of the fraction of students in the honors track. However, to demonstrate that our main findings are not driven primarily by assumptions about functional form, here we present results from several alternative specifications for the shape of $E[\Delta\overline{Y}_q(f_{stj})]$. Predicted treatment effects at candidate honors shares for each specification are displayed in Tables 5 and 6, while coefficient estimates for most specifications are displayed in Appendix Table A2.

Figure A6 plots a flexible semi-parametric version of our school FE specification that replaces the cubic specification with a set of interactions between student preparedness quintiles and quintiles of the fraction of students in honors:

$$E[\Delta\overline{Y}_q(f_{stj})] = \sum_{q'}\sum_{f'} 1(q = q')1(f_{stj} = f')\lambda_{q'f'} \tag{12}$$

Due to considerably greater imprecision, Figure A6 plots estimated treatment effects along with 90% rather than 95% pointwise confidence intervals. Nonetheless, one can clearly see the same qualitative patterns for each quintile as Figure 3a. Specifically, quintiles 1-3 exhibit

[25] Note that a full welfare analysis also requires incorporating the effort costs paid by students. See Fu and Mehta (2018) for an example of a complete welfare assessment.

expected gains compared to no tracking, but the gains decline quickly for quintile 1 while they continue to grow until somewhere between 40% and 60% for quintiles 2 and 3. Quintile 4 shows negligible gains regardless of honors share, and quintile 5 shows losses relative to a trackless course for shares above 40%.

Next, we consider imposing the restriction that 100% of students in honors is equivalent to 0%. This addresses the possibility that both zero and unit honors shares both represent the absence of two separate tracks. The shapes of the conditional expectation functions (Appendix Figure A7) are nearly identical over the range between 20% and 70% honors that spans almost the entire support of the data, so that the specifications only meaningfully differ in their extrapolations to rarely-observed honors shares above 70%.[26]

We also consider a specification that introduces a discontinuity at 0 to distinguish the absence of tracking from a very small tracking program:

$$E[\Delta\overline{Y}_q(f_{stj})] = \gamma_q^{lin} f_{stj} + \gamma_q^{sq} f_{stj}^2 + \gamma_q^{cb} f_{stj}^3 + \gamma_q^{indicator} \mathbb{1}_{(f_{stj} \in (0,1))} \tag{13}$$

Theoretically, this captures the possibility that teacher allocation and curriculum preparation may change discretely when even a tiny honors track exists. More practically, it ensures that the fitted values for smaller honors track sizes are not primarily being driven by the performance of students in untracked courses combined with a functional form that requires smoothness at 0. Due to the lack of support for honors shares between 0 and around .15 in the data, the plotted estimates for honors shares between 0 and .1 in Appendix Figure A8 are quite sensitive to the introduction of the discontinuity. However, both the estimated levels and shapes for shares above .1 are virtually unaffected relative to the original cubic school FE specification.

Appendix Figure A9 considers a quartic specification:

$$E[\Delta\overline{Y}_q(f_{stj})] = \gamma_q^{lin} f_{stj} + \gamma_q^{sq} f_{stj}^2 + \gamma_q^{cb} f_{stj}^3 + \gamma_q^{qt} f_{stj}^4 \tag{14}$$

Again, the estimated predicted values are quite similar to their cubic counterparts except where the support of the data is quite thin.

Next, we consider an alternative IV approach that uses the mean honors share among all other contemporaneous courses (and its corresponding square and cube) as instruments for the honors share in the chosen course, its square, and its cube. By generating predicted honors fractions that are pooled across other courses, this leave-one-out IV approach removes any endogeneity stemming from higher teacher or student quality in particular courses driving higher honors fractions. While all our other specifications have relied to differing degrees

[26] Recoding $f = 1$ as $f = 0$ and leaving the cubic unrestricted produces nearly identical results for honors shares below 70%, but extremely large standard errors at very high honors shares.

on within-school-year/across-course variation, this estimator relies exclusively on between-school and within-school/across-cohort variation. Because the support of other-course means of honors shares is naturally even more concentrated among shares between .2 and .6, we also constrain this specification to equal 0 at $f = 1$ in addition to $f = 0$ to prevent outliers from causing unrealistic estimates at high honors shares. Figure A10 shows that this IV approach generally yields similar shapes and peak/nadir locations as the baseline and school FE specifications over the primary support of the data, but with modestly larger magnitudes for the treatment effect functions at these peaks/nadirs. This specification features considerably weaker instruments than our other IV approaches[27], and its resulting estimates are quite noisy, but it nonetheless serves to demonstrate that the general pattern of results does not hinge entirely on the exogeneity of the residual between-course variation (about 47.3% of total residual baseline variation).

Next, we do the opposite and augment our baseline specification with a full set of school-year combination fixed effects, so that estimates are identified exclusively by comparisons in relative performance across courses featuring different honors share within school cohorts. These fixed effects are likely to remove almost all bias caused by student sorting, since these courses are being populated by nearly the same sets of students.[28] Thus, any remaining bias would require either that the relative honors share responds to particular cohorts' unobserved mean comparative advantage in some subject (likely to be negligible) or that it responds to differential unobserved mean teacher quality or track-specific experience across courses (Cook and Mansfield, 2016). Appendix Figure A11 displays results from this specification. The patterns and peak/nadir locations are the same as the preferred school FE specification, and the peak/nadir values are within .01 SDs but generally very slightly smaller. One possible explanation for slightly smaller impacts are that across-course differences in honors shares are likely to be quite small and transitory, and may not engender some of the teacher re-optimization of pace and pedagogical approach that may drive gains from tracking.

Regardless of the functional form and source of variation, all these specifications rely heavily on the rich set of controls to remove correlations that would otherwise exist between honors shares and unobserved student, teacher, and school inputs. As a partial test of whether important sources of endogeneity remain, we examine the sensitivity of our estimated treatment effect functions to the inclusion of a small set of controls that were heretofore omitted due to a higher rate of missing data. These consist of the share of school-year-course-quintile observations in which student was ever eligible for free/reduced price lunch

[27] The Kleibergen-Paap Wald F statistic associated with the first stage is 3.95, so that we cannot reject the null hypothesis of instrument weakness at standard critical values.

[28] Some core courses, such as English 1 and Biology, are taken nearly universally, while others, such as Chemistry, are not taken by a substantial share of students.

during the sample period, as well as its samplewide school average, (s,y,j,q)-level shares of students in various learning disability categories, a set of dummies for teacher educational attainment categories, and the school average of 8th grade science scores (based on the short period of years the exam was offered), along with missing value indicators for each variable to prevent additional observations from being dropped. A comparison of the estimates from this augmented specification in Appendix Figure A12 to those from the school FE specification in Figure 3a reveals that the two are practically identical.[29] A generalized Hausman test of coefficient stability suggested by Pei et al. (2019) fails to reject the hypothesis that the predicted values from the two specifications are equal for any quintile at any honors share in $\{.1,.2,.3,.4,.5\}$. This occurs despite the fact that several of these additional controls feature coefficients of economically meaningful magnitudes that are statistically different from 0 at the 95% level. Moveover, the coefficients on the extra controls are often several times larger with considerably smaller standard errors when the original controls are excluded. Taken together, these patterns suggest that the original controls are sufficiently strong that they leave very little conditional correlation between these previously excluded inputs and either the honors enrollment share or test scores.

Finally, it is possible that the school-year-courses chosen in section 3.1 are not sufficiently similar in their joint distributions of student abilities and costs to satisfy Assumption 1 and thus make the peer environment comparable in different schools featuring the same honors fraction. Thus, we re-estimate our baseline specification on a smaller subset of schools in which students' prior achievement would need to change by less than a third of a quintile on average to match the statewide uniform distribution of quintiles. Figure A13 shows that the point estimates are nearly the same with the restricted sample, but with slightly larger confidence intervals.

7 Conclusion

In this paper we use rich administrative data to identify the treatment effects of changing the size of the honors track, operationalized via functions of the share of students who enroll in honors, with separate functions estimated for each quintile of an index of student predicted performance. Importantly, our approach explicitly accommodates endogenous self-sorting of students into the honors and regular tracks conditional on the administrator-determined capacity of the honors track. We show that our set of estimated treatment effect functions suffice to determine the optimal share of students in each track in an administrator's planning

[29] Adding these same controls to the baseline specification, where the extra school-level controls are not made irrelevant by school fixed effects, does not meaningfully change the baseline treatment effect functions either.

problem.

We find that on the extensive margin, tracking presents an equity-efficiency tradeoff. The best prepared quintile of students substantially benefit from small honors tracks compared to the absence of tracking, with gains between .05 and .07 test score SDs, but these gains decrease monotonically with lower student past performance and become losses for the least prepared quintile of students. However, the nature and severity of the tradeoff changes on the intensive margin: relative to higher honors enrollment shares, reducing the share of students in the honors track to between 20% and 30% substantially improves test scores for both the top and bottom quintiles, negligibly affects the second and fourth quintiles, and causes only very slightly lower scores for the middle quintile. Altering the size of the honors track thus represents a low cost method to improve test score performance, particularly for larger schools that are already offering the relevant courses in several class periods. Furthermore, because small achievement gains per student per course apply to such a wide population of courses, students and high schools, our back-of-the-envelope calculations suggest that they could translate to aggregate skill development worth tens of millions of dollars in future earnings potential. To provide reassurance about the validity of these findings, we show that they are extremely robust across several alternative specifications featuring different samples, controls, functional form assumptions, or segments of variation that remove different sources of endogeneity.

A few caveats about external validity are necessary. First, our approach assumes that students and their parents ultimately make track choices for each class, but that school administrators can alter incentives as necessary to induce their desired aggregate shares of students in each track. Thus, our results may not be externally valid for high schools where principals relinquish any role in shaping the honors track or for high schools where students can be assigned to tracks without their permission.

Similarly, our approach also requires drawing comparisons among the considerable majority of schools whose student populations feature distributions of past performance that minimally deviate from the statewide distribution. Thus, our results may not be externally valid to high schools with particularly large shares of very advanced or struggling students, since the peer composition in their honors or regular tracks may not be well-approximated by those at other schools, even conditional on the same student share in the honors track.

In addition, the North Carolina context we consider provides strong incentives to keep the breadth of material covered by the course similar among both tracks in order to prepare all students for a common statewide standardized exam. This feature is essential for generating internally valid estimates by facilitating comparisons on a single achievement metric. However, we cannot verify external validity for contexts in which different tracks have substantially different curricula (e.g. Advanced Placement or International Baccalaureate), though we have no *a priori* reason to believe that our results would not generalize.

Finally, while our results may provide parents with a basis for comparing the tracking policies of schools they are considering, they are not intended to provide parents with information on whether or not their child should enroll in honors in a given course. This would require estimates of a different set of parameters that capture student-level treatment effects from switching tracks. A full decomposition of the effect from expanding the honors track into effects on the marginal students and peer effects in both the expanding and contracting tracks necessitates combining our estimates with exogenous variation in student-level track choices.

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Tables

Table 1: Decomposing the Total and Residual Variance in Honors Enrollment Share

Variance Component	% of Total Variance $Var(f_{stj})$	% of Residual Variance $Var(f_{stj} - \overline{X}_{stj}\hat{\beta})$
Between School	40.3%	35.2%
Within School/Across Year	11.7%	17.5%
Within School-Year/Across Course	48.0%	47.3%

Notes: The subscripts s, t, and j denote school, year, and course, respectively. "Between School" captures $Var(\overline{f}_s)$ in Column 1 and $Var(\overline{f}_s - \overline{X}_s\hat{\beta})$ in Column 2. "Within School/Across Year" captures $Var(\overline{f}_{st}) - Var(\overline{f}_s)$ and $Var(\overline{f}_{st} - \overline{X}_{st}\hat{\beta}) - Var(\overline{f}_s - \overline{X}_s\hat{\beta})$, respectively. "Within School-Year/Across Course" captures $Var(f_{stj}) - Var(\overline{f}_{st})$ and $Var(f_{stj} - \overline{X}_{stj}\hat{\beta}) - Var(\overline{f}_{st} - \overline{X}_{st}\hat{\beta})$, respectively.

Table 2: Frequency of Tracking Offerings by Course

Course Name	No tracking	Only honors	Only remedial	Honors & remedial	Honors & AP	Only AP	Honors, AP, & remedial
Algebra 1	8,357	657	126	15	0	0	0
Algebra 2	418	3,645	3	17	0	0	0
Biology	934	4,065	15	138	1	0	0
Chemistry	437	2,201	0	1	0	0	0
English 1	251	4,300	12	332	0	0	0
Geometry	791	3,440	2	30	0	0	0
PSCI	2,104	1,047	83	82	0	0	0
Physics	83	537	0	0	177	32	0
US History	142	691	8	18	2,169	246	58

Notes: Each cell provides the total number of school-year combinations in which the course indicated by the row title is offered under the tracking regime featured in the column titles. "PSCI" denotes physical science. The sample of school-years is limited to those with at least 30 test scores.

Table 3: Summary Statistics for Control Variables in X_{sjtq} by Honors Enrollment Share

	No honors tracking	Share $\in (0, 0.35)$	Share $\in [0.35, 1)$
	mean	mean	mean
VARIABLES	(sd)	(sd)	(sd)
Cohort size	145.4	272.4	302.1
	(109.1)	(165.2)	(269.9)
Pupil to teacher ratio	12.63	14.05	13.61
	(6.046)	(4.679)	(6.056)
Average class size	16.88	18.24	17.93
	(4.362)	(3.877)	(9.210)
Share of seats in remedial classes	0.00309	0.00226	0.000957
	(0.0361)	(0.0154)	(0.00906)
Number of students at school	856.7	1,079	1,141
	(459.2)	(433.2)	(568.1)
Share of students that are Hispanic	0.0496	0.0519	0.0568
	(0.0516)	(0.0478)	(0.0522)
Share of students that are Black	0.207	0.225	0.254
	(0.177)	(0.170)	(0.174)
Share of students that are white	0.709	0.692	0.652
	(0.197)	(0.189)	(0.192)
Share of students that are Asian	0.0160	0.0145	0.0208
	(0.0244)	(0.0206)	(0.0223)
7th grade math scores	0.304	0.276	0.497
	(0.541)	(0.449)	(0.450)
8th grade math scores	0.623	0.592	0.787
	(0.574)	(0.442)	(0.444)
7th grade read scores	0.265	0.237	0.433
	(0.450)	(0.371)	(0.415)
8th grade read scores	0.566	0.537	0.725
	(0.440)	(0.369)	(0.403)
Average Praxis score	417.7	434.9	386.0
	(266.5)	(247.8)	(271.8)
Teacher share with			
0 years exp	0.218	0.222	0.298
	(0.388)	(0.390)	(0.435)
1 year exp	0.0282	0.0271	0.0286
	(0.136)	(0.114)	(0.127)
2 years exp	0.0372	0.0332	0.0309
	(0.163)	(0.129)	(0.128)
3-5 years exp	0.101	0.0975	0.0770
	(0.258)	(0.217)	(0.204)
6-11 years exp	0.162	0.168	0.160
	(0.318)	(0.277)	(0.292)
12+ years exp	0.454	0.453	0.406
	(0.440)	(0.396)	(0.410)
Fraction of students			
Whose parents lack a HS diploma/GED	0.0651	0.0603	0.0430
	(0.0671)	(0.0631)	(0.0594)
Whose parents have a HS diploma	0.213	0.205	0.156
	(0.113)	(0.104)	(0.114)
Whose parents have some college	0.119	0.114	0.106
	(0.0933)	(0.0680)	(0.0806)
Whose parents attended trade or business school	0.0299	0.0286	0.0261
	(0.0403)	(0.0243)	(0.0232)
Whose parents attended community college	0.196	0.175	0.154
	(0.131)	(0.0916)	(0.107)
Whose parents have a 4-year degree	0.201	0.212	0.245
	(0.121)	(0.115)	(0.139)
Whose parents have graduate degrees	0.0835	0.0840	0.128
	(0.0877)	(0.0734)	(0.120)
With gifted status	0.104	0.108	0.134
	(0.187)	(0.122)	(0.178)
With learning disabilities	0.0309	0.0364	0.0277
	(0.0382)	(0.0564)	(0.0612)
Limited English Proficiency	0.0277	0.0282	0.0337
	(0.0488)	(0.0530)	(0.0774)
School-course-years	2,767	7,536	6,333

Notes: Each entry provides mean values (and standard deviations in parentheses) for the control variable listed in the row label among all school-year-course observations. The sample here matches the one used for our baseline specification, which is limited to school-years with at least 30 test score observations and which feature typical distributions of student quality (See Section 3.1).

Table 4: Estimates of the Values of the Quintile-Specific Treatment Effect Functions $E[\Delta\overline{Y}_q(f)]$ at Several Candidate Honors Enrollment Fractions for the Baseline and Alternative Specifications

Specification	Share in Honors	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Baseline (OLS)	.15	.0543	.0315	.0266	.0175	.0159
		(.0294, .0792)	(.0103, .0526)	(.0066, .0467)	(- .0024, .0373)	(-.004, .0359)
	.3	.0685	.044	.0386	.0189	.0051
		(.0363, .101)	(.0164, .0717)	(.0117, .0656)	(- .0072, .045)	(-.0211, .0314)
	.45	.0561	.0438	.0415	.0122	-.0161
		(.0224, .0897)	(.0136, .0741)	(.0112, .0718)	(- .0164, .0408)	(-.0459, .0137)
	.6	.0302	.037	.0407	.0053	-.0317
		(-.0069, .0672)	(.002, .0721)	(.0056, .0759)	(- .0277, .0383)	(-.0671, .0037)
School FEs (OLS)	.15	.0458	.0207	.0157	.0065	.007
		(.0231, .0685)	(.0015, .0399)	(-.0032, .0345)	(-.012, .0251)	(-.0134, .0273)
	.3	.058	.0311	.0259	.0072	-.0024
		(.0287, .0873)	(.0058, .0565)	(.001, .0509)	(- .0172, .0316)	(-.0292, .0244)
	.45	.0475	.0337	.0319	.005	-.0177
		(.0168, .0782)	(.0063, .061)	(.0053, .0585)	(- .0209, .0309)	(-.0469, .0115)
	.6	.0253	.0306	.0345	.0029	-.0286
		(-.0084, .059)	(0, .0612)	(.005, .064)	(- .0256, .0314)	(-.0619, .0047)
Lagged IV	.15	.0735	.0562	.0458	.0314	.0382
		(.0407, .106)	(.0282, .0841)	(.0186, .0729)	(.0049, .0579)	(.01, .0665)
	.3	.0843	.0676	.051	.0254	.0194
		(.0433, .125)	(.032, .103)	(.0162, .0858)	(- .0084, .0591)	(-.015, .0538)
	.45	.0581	.0533	.0347	.0011	-.024
		(.0148, .101)	(.0139, .0928)	(-.0044, .0739)	(- .0369, .0391)	(-.062, .014)
	.6	.0206	.0326	.0161	-.0223	-.0596
		(-.0295, .0708)	(-.0148, .08)	(-.0311, .0633)	(- .0685, .0239)	(-.107, -.0119)
Class share IV	.15	.0401	.0144	.0132	.0047	-.0003
		(.0163, .0638)	(-.0064, .0352)	(-.0075, .0339)	(-.0156, .0251)	(-.0221, .0215)
	.3	.0508	.024	.023	.0033	-.0127
		(.0205, .0811)	(-.0029, .0509)	(-.0037, .0497)	(-.0229, .0295)	(-.0414, .0159)
	.45	.0411	.0287	.0295	-.0009	-.0285
		(.0094, .0729)	(-.0003, .0577)	(.001, .058)	(-.0286, .0268)	(-.0598, .0028)
	.6	.0201	.0282	.0329	-.0044	-.039
		(-.0155, .0556)	(-.0047, .0611)	(.0006, .0652)	(-.0354, .0266)	(-.0742, -.0038)

Notes: Predicted values are generated from the estimates $\hat{\vec{\gamma}}$ for the specifications named in the row panel for the values of the honors enrollment share f listed in the row labels. 95% confidence intervals computed using the delta method are displayed in parentheses. Each column presents estimates for a different quintile of the statewide predicted performance distribution among students. "Baseline (OLS)" refers to the baseline specification that pools all sources of variation in the honors enrollment share. "School FE (OLS)" uses a full set of school fixed effects to isolate within-school variation. "Lagged IV" uses the previous year's honors enrollment share (and its square and cube) as instruments for its contemporary counterparts in the chosen school-year-course. "Class Share IV" uses the honors classroom share (and its square and cube) as instruments for the student-weighted honors enrollment share (and its square and cube).

Table 5: Estimates of the Values of the Quintile-Specific Treatment Effect Functions $E[\Delta\overline{Y}_q(f)]$ at Several Candidate Honors Enrollment Fractions for Various Specifications Examining the Robustness of Results (Part 1)

Specification	Share in honors	Quintile 1	Quintile 2	Quintile3	Quintile 4	Quintile 5
Bin Specification	.15	.0684	.0289	.0195	.0176	.0051
		(.0368, .1)	(.0022, .0555)	(-.0049, .0438)	(-.0089, .0442)	(-.0222, .0325)
	.3	.0622	.0348	.0279	.0105	.0005
		(.033, .0915)	(.0099, .0597)	(.0035, .0522)	(-.0133, .0343)	(-.0256, .0267)
	.45	.0516	.0359	.0386	.0121	-.0097
		(.0188, .0843)	(.0067, .065)	(.0109, .0663)	(-.0149, .0392)	(-.0396, .0201)
	.6	.011	.0218	.0344	.0141	-.0077
		(-.0294, .0514)	(-.0177, .0612)	(-.0028, .0717)	(-.0222, .0504)	(-.0508, .0354)
Constrained Cubic	.15	.0495	.0189	.0099	.0037	.0002
		(.0293, .0697)	(.0013, .0364)	(-.0075, .0273)	(-.0134, .0208)	(-.0186, .0191)
	.3	.0632	.0298	.0194	.0039	-.0101
		(.0367, .0897)	(.0064, .0531)	(-.0037, .0425)	(-.0187, .0266)	(-.0353, .015)
	.45	.0528	.0335	.0265	.0021	-.0244
		(.0247, .081)	(.0083, .0588)	(.002, .0511)	(-.0217, .0259)	(-.0518, .0031)
	.6	.0303	.031	.0293	-.0005	-.0357
		(-.0001, .0607)	(.0036, .0584)	(.0032, .0553)	(-.0255, .0245)	(-.0656, -.0058)
Honors Indicator	.15	.0697	.0333	.0246	.0202	.0132
		(.0389, .1)	(.0078, .0587)	(.0004, .0488)	(-.0052, .0457)	(-.0124, .0389)
	.3	.0621	.0334	.0278	.0098	-.0009
		(.0321, .092)	(.0076, .0592)	(.0026, .053)	(-.0148, .0345)	(-.0277, .0259)
	.45	.0512	.036	.0337	.008	-.0162
		(.0197, .0826)	(.008, .0639)	(.0067, .0608)	(-.0183, .0343)	(-.0455, .0131)
	.6	.0374	.0374	.0394	.0112	-.0251
		(.0002, .0745)	(.0038, .071)	(.0077, .071)	(-.02, .0425)	(-.0599, .0096)
Quartic	.15	.0631	.0321	.0262	.0202	.0104
		(.0357, .0904)	(.0084, .0559)	(.0037, .0486)	(-.0023, .0427)	(-.0139, .0347)
	.3	.0636	.0347	.0292	.0112	-.001
		(.0335, .0936)	(.0088, .0607)	(.0039, .0545)	(-.0134, .0359)	(-.0278, .0259)
	.45	.0464	.0331	.0314	.0044	-.0175
		(.0158, .077)	(.0059, .0602)	(.0048, .058)	(-.0214, .0302)	(-.0467, .0118)
	.6	.035	.0377	.0414	.0126	-.0267
		(-.0013, .0713)	(.0046, .0708)	(.0098, .0729)	(-.018, .0432)	(-.0624, .009)

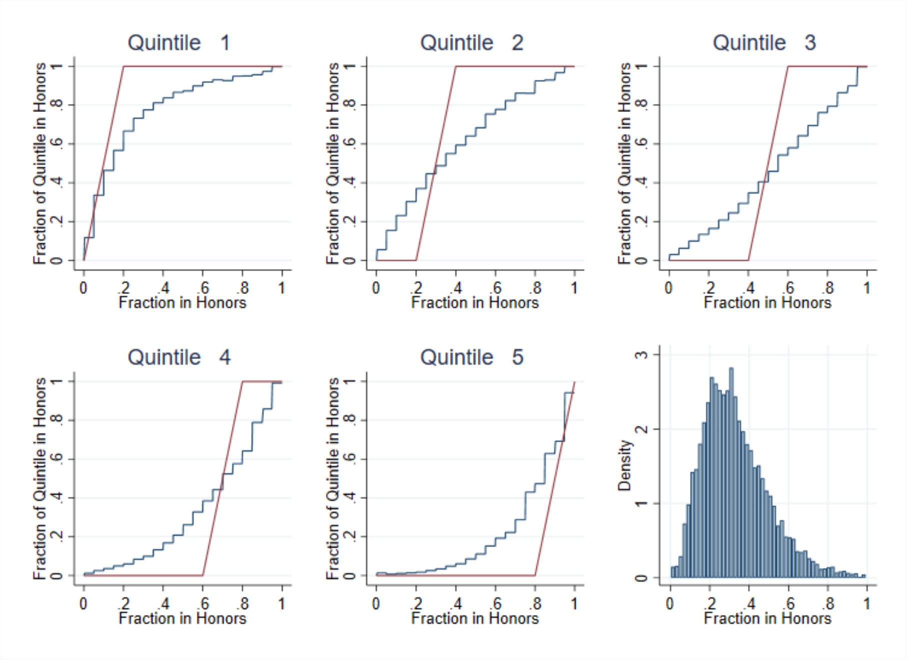
Notes: Predicted values are generated from the estimates $\hat{\vec{\gamma}}$ for the specifications named in the row category for the values of the honors enrollment share f listed in the row labels. 95% confidence intervals computed using the delta method are displayed in parentheses. Each column presents estimates for a different quintile of the statewide predicted performance distribution among students. "Bin Specification" alters the baseline specification by replacing the cubic function with separate indicators for whether the share of students in honors falls within mutually exclusive intervals of length 0.2. "Constrained Cubic" imposes the restriction that the treatment effects for 0% and 100% honors enrollment are equal. "Honors Indicator" alters the baseline specification by including a separate indicator for an honors enrollment share of 0. "Quartic" fits a quartic rather than a cubic polynomial.

Table 6: Estimates of the Values of the Quintile-Specific Treatment Effect Functions $E[\Delta\overline{Y}_q(f)]$ at Several Candidate Honors Enrollment Fractions for Various Specifications Examining the Robustness of Results (Part 2)

Specification	Share in honors	Quintile 1	Quintile 2	Quintile3	Quintile 4	Quintile 5
Other Course IV	.15	.0667	.0464	.0289	.0217	.0177
		(.0362, .0972)	(.0159, .0769)	(-.0016, .0595)	(-.0088, .0522)	(-.0129, .0482)
	.3	.0747	.0575	.0439	.0255	.0103
		(.0378, .1120)	(.0207, .0944)	(.0070, .0808)	(-.0114, .0623)	(-.0266, .0471)
	.45	.0465	.0455	.0472	.0179	-.0101
		(.0084, .0847)	(.0074, .0836)	(.0091, .0853)	(-.0202, .0561)	(-.0482, .0280)
	.6	.0049	.0225	.0414	.0057	-.0313
		(-.0406, .0504)	(-.0230, .0680)	(-.0040, .0869)	(-.0398, .0511)	(-.0768, .0142)
School-Year FEs	.15	.0414	.0177	.0096	-.0002	-.0017
		(.0183, .0646)	(-.0027, .0381)	(-.0107, .0298)	(-.0196, .0192)	(-.0229, .0194)
	.3	.0523	.0275	.0176	-.0025	-.0137
		(.0228, .0819)	(.0006, .0544)	(-.009, .0442)	(-.0279, .0229)	(-.0414, .0139)
	.45	.0426	.0305	.0236	-.005	-.0279
		(.0117, .0735)	(.0013, .0598)	(-.0046, .0519)	(-.0318, .0218)	(-.0571, .0013)
	.6	.0219	.0281	.0273	-.0059	-.0361
		(-.0129, .0567)	(-.0053, .0615)	(-.0041, .0588)	(-.0356, .0239)	(-.0683, -.0039)
Augmented Controls	.15	.0577	.0223	.0135	.015	.0103
		(.0302, .0852)	(-.0017, .0463)	(-.0086, .0357)	(-.0067, .0367)	(-.0131, .0337)
	.3	.0727	.0312	.0203	.0162	-.0015
		(.0376, .108)	(0, .0625)	(-.0088, .0495)	(-.0124, .0448)	(-.0323, .0294)
	.45	.0599	.031	.0229	.0103	-.0208
		(.0246, .0952)	(-.0013, .0634)	(-.0074, .0533)	(-.0197, .0403)	(-.0538, .0122)
	.6	.0339	.0259	.0237	.0037	-.0331
		(-.0032, .071)	(-.0091, .0608)	(-.0089, .0564)	(-.0285, .036)	(-.0694, .0033)
Restricted School Set	.15	.0447	.0188	.0141	.0052	.0052
		(.0223, .0671)	(-.0001, .0377)	(-.0044, .0326)	(-.013, .0235)	(-.015, .0253)
	.3	.0571	.0292	.0243	.0061	-.0042
		(.0281, .0861)	(.0043, .0541)	(-.0002, .0487)	(-.0178, .03)	(-.0306, .0222)
	.45	.0477	.0327	.031	.0047	-.0186
		(.0173, .0781)	(.0059, .0596)	(.0049, .0571)	(-.0204, .0299)	(-.0472, .01)
	.6	.0267	.0309	.0346	.0035	-.0288
		(-.0067, .06)	(.0007, .0611)	(.0055, .0636)	(-.0242, .0312)	(-.0613, .0036)

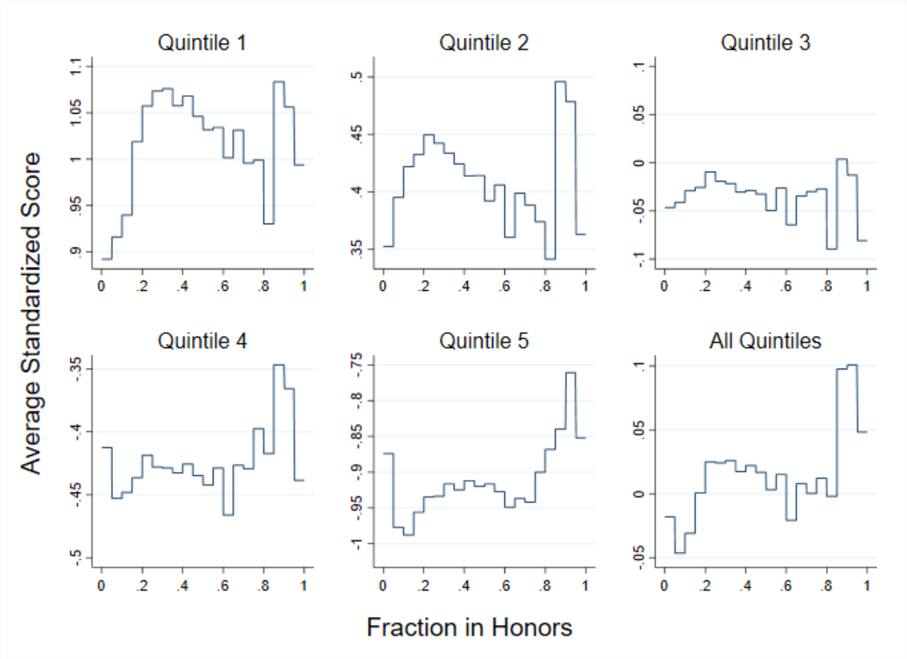
Notes: Predicted values are generated from the estimates $\hat{\vec{\gamma}}$ for the specifications named in the row category for the values of the honors enrollment share f listed in the row labels. 95% confidence intervals computed using the delta method are displayed in parentheses. Each column presents estimates for a different quintile of the statewide predicted performance distribution among students. "Other Course IV" uses the contemporaneous honors enrollment share (and its square) in the other tested courses as instruments for the share and its square in the chosen course. "School-Year FEs" introduces a full set of fixed effects for school-year combinations. "Augmented Controls" adds a set of additional controls capturing learning disability status, free/reduced price lunch eligibility, and teacher education category shares. "Restricted School Set" restricts the sample to schools whose distributions of preparedness stray from the uniform distribution by less than 1/3 of a quintile per student on average, rather than the 1/2 threshold used in the baseline sample.

Figure 1: Student Probability of Choosing the Honors Track as a Function of the Coursewide Honors Enrollment Share by Quintile of the School-Specific Predicted Performance Distribution



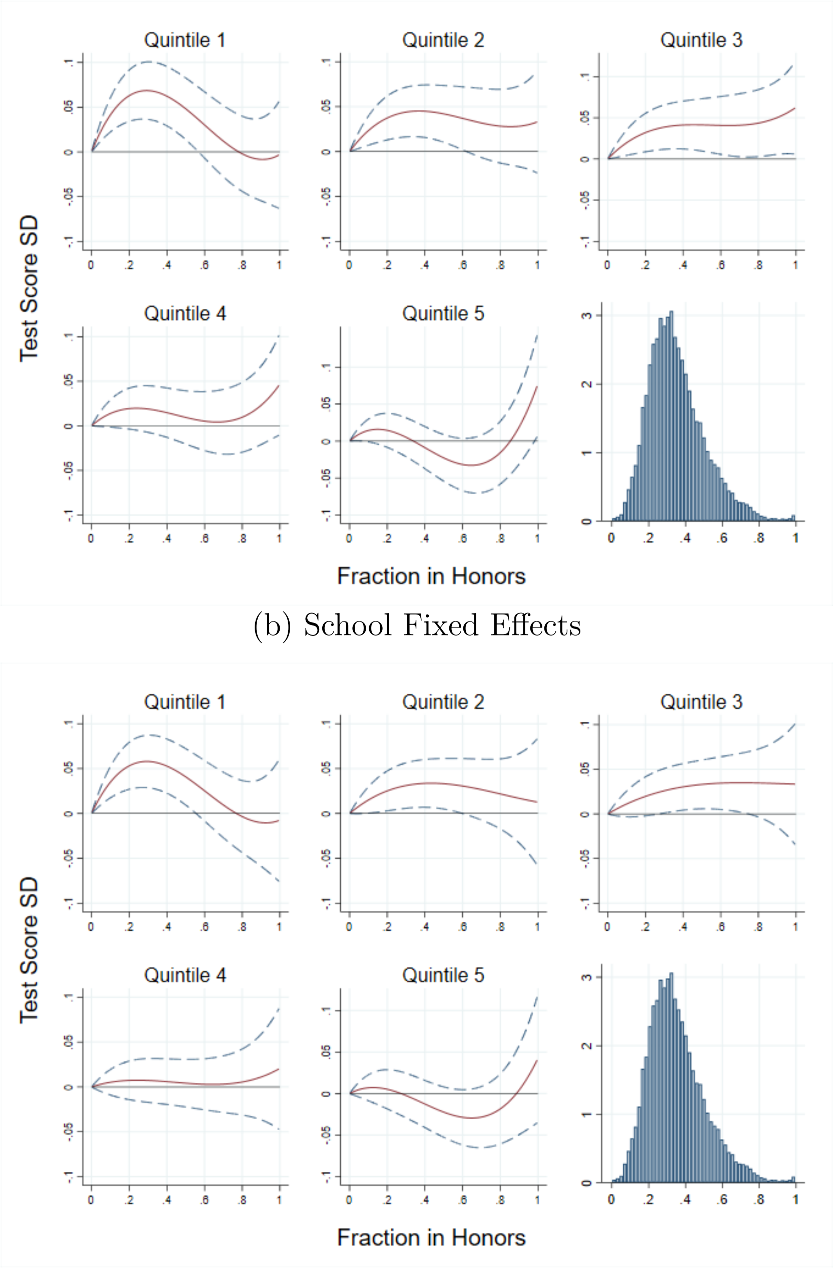
Notes: The first five graphs plot the share of students in the chosen quintile of predicted performance that selects the honors track among narrow bins of the coursewide honors enrollment share. Quintiles for this figure are based on school-specific rather than statewide predicted performance rankings. Each bin includes shares in (bin minimum, bin maximum]. The bottom right cell plots the support of the data used for the other five cells, excluding school-year-courses where either none of the students or all of the students are enrolled in honors. The figures are based on the final sample of school-course-year-quintile observations used to estimate the baseline specification.

Figure 2: Average Standardized Score as a Function of the Coursewide Honors Enrollment Share by Quintile of Student Predicted Performance



Notes: Each graph plots the mean standardized test score by narrow bins of the share of the course's students enrolled in honors (pooled across six subjects) for a different quintile of a regression index of predicted student performance based on grade 7 and 8 test scores. The bin for the lowest share of students in honors includes school-year-courses where no tracking occurs. The remaining bins consider honors enrollment shares in the interval (bin minimum, bin maximum].

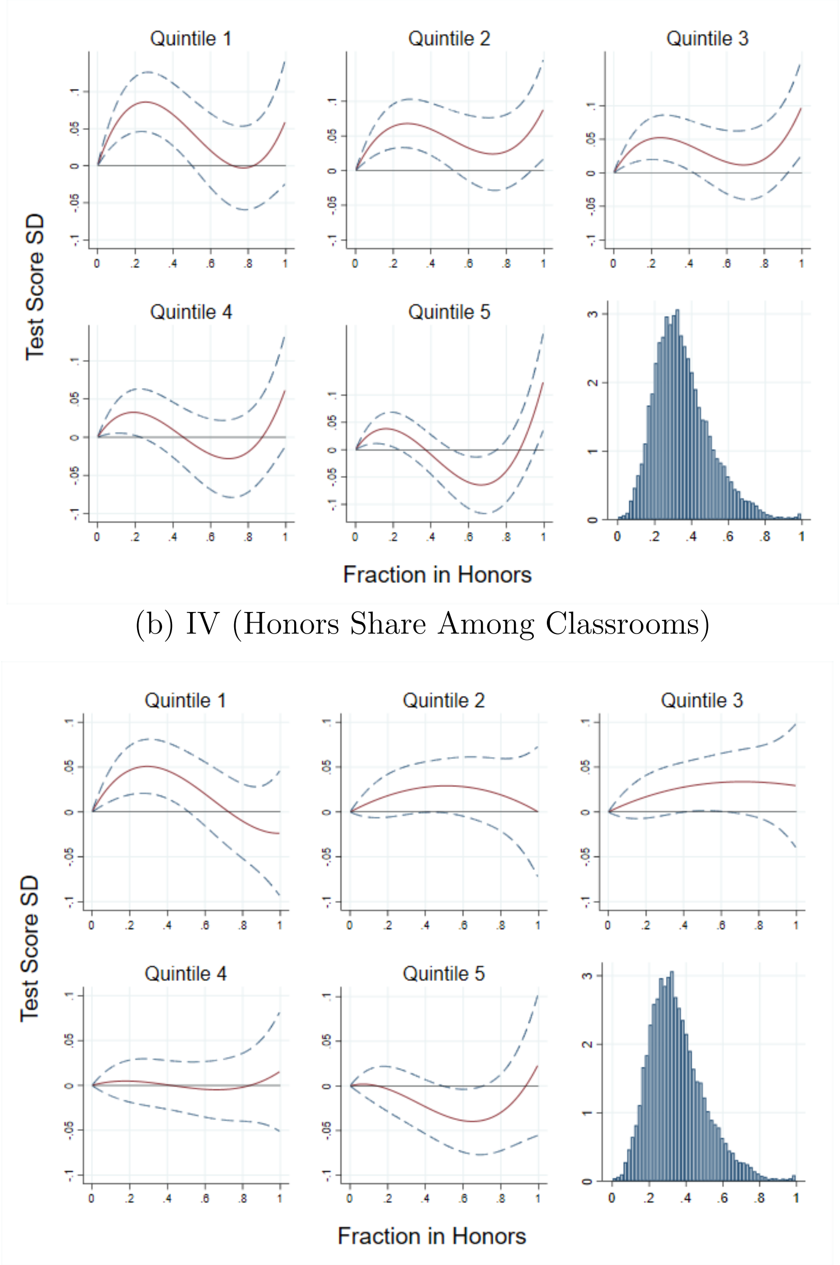
Figure 3: Treatment Effect Functions for the Honors Enrollment Fraction by Quintile of Predicted Student Achievement ($E[\Delta\overline{Y}_q(f)]$): Baseline and School Fixed Effects Specifications


(a) Baseline

(b) School Fixed Effects

Notes: The first five graphs in each panel plot estimates of the function $E[\Delta\overline{Y}_q(f)]$ that maps coursewide honors enrollment fraction into expected standardized test performance by quintile of predicted performance for the baseline (Panel (a)) or school fixed effects (Panel (b)) specifications. The bottom right graph in each panel displays the density of honors enrollment shares for the baseline sample that is used in both specifications. The sample is restricted to school-year-course combinations that serve at least 30 students, do not offer IB nor AP tracks, and whose schools' distributions of student preparedness closely resemble the statewide distribution (See Section 3.1). 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes.

Figure 4: Treatment Effect Functions for the Honors Enrollment Fraction by Quintile of Predicted Student Achievement ($E[\Delta\overline{Y}_q(f)]$): Alternative Specifications

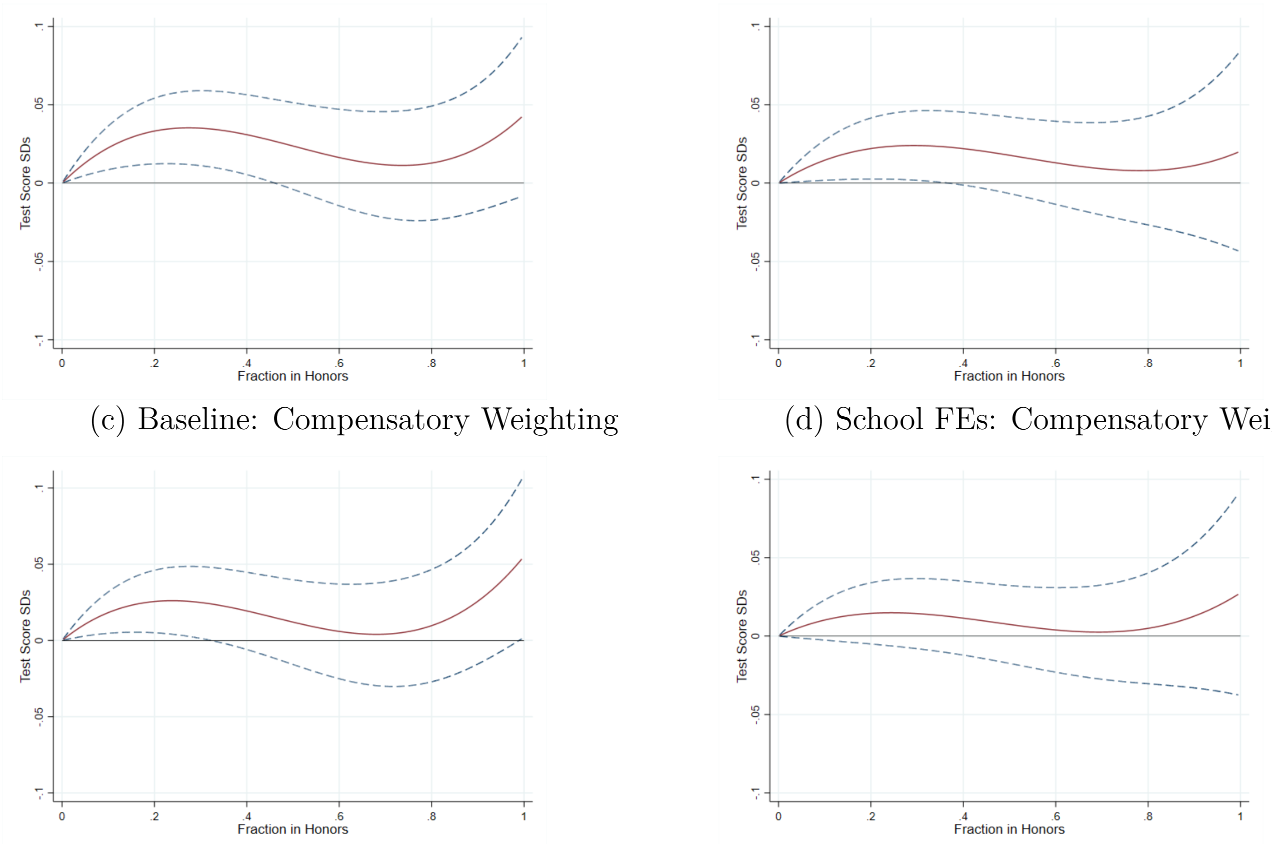


(a) IV (Previous Year's Honors Fraction)

(b) IV (Honors Share Among Classrooms)

Notes: Panel (a) displays estimates of the function $E[\Delta\overline{Y}_q(f)]$ that maps coursewide honors enrollment fraction into expected standardized test performance by quintile of student predicted performance for a specification in which the current course's honors enrollment share at the chosen school-year (and its square and cube) are instrumented with the previous year's share (and its square and cube). Panel (b) plots analogous estimates for a specification in which the current course's share of enrollment in the honors track (and its square and cube) are instrumented with the course's share of honors classrooms (and its square and cube). Both figures use the baseline sample of school-course-year-quintile observations (See Section 3.1 for details). The bottom right graph in each panel displays the sample's density of honors enrollment shares. 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes.

Figure 5: School Average Test Score Gains as a Function of the Honors Enrollment Fraction Using Equal vs. Compensatory Weights: Baseline and School FE Specifications


(a) Baseline: Equal Weighting

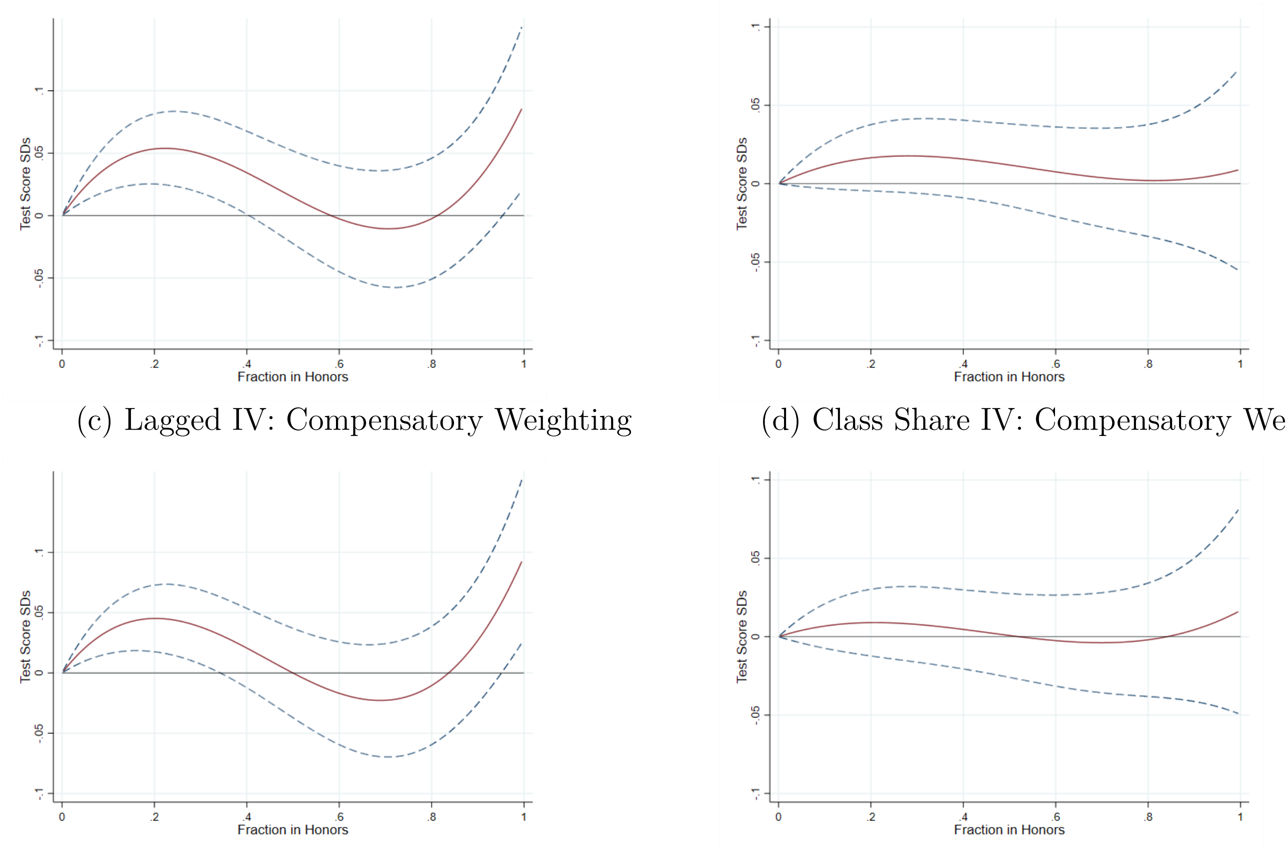
(b) School FEs: Equal Weighting

(c) Baseline: Compensatory Weighting

(d) School FEs: Compensatory Weighting

Notes: Each figure displays estimates of the value of the administrator's objective $\max_f \sum_{q=1}^{Q} W_q \theta_q E[\Delta \overline{Y}_q(f)]$ as a function of the coursewide honors enrollment fraction, where W_q is the share of the course's students who belong to the q-th predicted performance quintile and θ_q is the preference weight given to the achievement of quintile q. The left two graphs use estimates of $E[\Delta \overline{Y}_q(f)]$ from the baseline specification, while the right two graphs use estimates from the school fixed-effects specification. "Equal Weighting": test scores gains by all quintiles are weighted equally. "Compensatory Weighting": quintiles 1, 2, 3, 4, and 5 are assigned weight $\frac{1}{15}$, $\frac{2}{15}$, $\frac{3}{15}$, $\frac{4}{15}$, and $\frac{5}{15}$. Each figure relies on the baseline sample of school-course-year-quintile observations (See Section 3.1 for details). 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes.

Figure 6: School Average Test Score Gains as a Function of the Honors Enrollment Fraction Using Equal vs. Compensatory Weights: Alternative Specifications



(a) Lagged IV: Equal Weighting

(b) Class Share IV: Equal Weighting

(c) Lagged IV: Compensatory Weighting

(d) Class Share IV: Compensatory Weighting

Notes: Each figure displays estimates of the value of the administrator's objective $\sum_{q=1}^{Q} W_q \theta_q E[\Delta \overline{Y}_q(f)]$ as a function of the coursewide honors enrollment fraction, using estimates of treatment effects $E[\Delta \overline{Y}_q(f)]$ from either the "Lagged IV" or "Class Share IV" specification listed in the subtitle. "Lagged IV" uses the previous year's honors enrollment share (and its square and cube) as instruments for its contemporary counterparts in the chosen school-year-course. "Class Share IV": instruments for the current course's honors enrollment share (and its square and cube) using its honors classroom share (and its square and cube). "Equal Weighting": test score gains by all quintiles are weighted equally. "Compensatory Weighting": quintiles 1, 2, 3, 4, and 5 are assigned weight $\frac{1}{15}$, $\frac{2}{15}$, $\frac{3}{15}$, $\frac{4}{15}$, and $\frac{5}{15}$, respectively. Both figures use the baseline sample of school-course-year-quintile observations (See Section 3.1 for details). 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes.

A Appendix

Table A1: Estimates of the Parameters $\{\gamma\}$ Governing the Quintile-Specific Treatment Effect Functions of the Honors Enrollment Fraction $E[\Delta\overline{Y}_q(f)]$ for the Baseline and Alternative Specifications

	(1)	(2)	(3)	(4)
Quintile 1-Linear Coefficient	0.525***	0.442***	0.757***	0.385***
	(0.127)	(0.116)	(0.173)	(0.124)
Quintile 2-Linear Coefficient	0.287***	0.177*	0.566***	0.111
	(0.109)	(0.0978)	(0.148)	(0.108)
Quintile 3-Linear Coefficient	0.239**	0.125	0.483***	0.100
	(0.102)	(0.0952)	(0.143)	(0.107)
Quintile 4-Linear Coefficient	0.188*	0.0699	0.376***	0.0599
	(0.101)	(0.0940)	(0.141)	(0.105)
Quintile 5-Linear Coefficient	0.231**	0.124	0.517***	0.0573
	(0.104)	(0.104)	(0.154)	(0.112)
Quintile 1-Squared Coefficient	-1.187***	-0.991***	-1.968***	-0.851**
	(0.346)	(0.319)	(0.503)	(0.350)
Quintile 2-Squared Coefficient	-0.557*	-0.279	-1.419***	-0.100
	(0.303)	(0.268)	(0.438)	(0.307)
Quintile 3-Squared Coefficient	-0.447	-0.143	-1.325***	-0.0828
	(0.280)	(0.255)	(0.424)	(0.301)
Quintile 4-Squared Coefficient	-0.534*	-0.197	-1.256***	-0.214
	(0.280)	(0.253)	(0.423)	(0.294)
Quintile 5-Squared Coefficient	-0.953***	-0.593**	-1.988***	-0.461
	(0.302)	(0.291)	(0.477)	(0.317)
Quintile 1-Cubic Coefficient	0.659***	0.542**	1.273***	0.443*
	(0.242)	(0.231)	(0.363)	(0.255)
Quintile 2-Cubic Coefficient	0.304	0.114	0.943***	-0.0111
	(0.215)	(0.199)	(0.318)	(0.229)
Quintile 3-Cubic Coefficient	0.271	0.0519	0.943***	0.0116
	(0.200)	(0.186)	(0.309)	(0.220)
Quintile 4-Cubic Coefficient	0.393**	0.148	0.945***	0.170
	(0.199)	(0.186)	(0.310)	(0.216)
Quintile 5-Cubic Coefficient	0.800***	0.512**	1.600***	0.428*
	(0.226)	(0.218)	(0.356)	(0.239)
Observations	171,012	171,012	157,266	169,660
School FEs	NO	YES	NO	NO
Constrained Coefficients	NO	NO	NO	YES
Lagged IV	NO	NO	YES	NO
Class Share IV	NO	NO	NO	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the school level are in parentheses. "School FEs": A full set of school fixed effects is included. "Constrained Coefficients": Cubic coefficients are constrained to ensure a zero treatment effect at an honors share of 1 as well as zero. "Lagged IV" uses the previous year's honors enrollment share (and its square and cube) as instruments for its contemporary counterparts in the chosen school-year-course. "Class Share IV": instruments for the current course's honors enrollment share (and its square and cube) using its honors classroom share (and its square and cube).

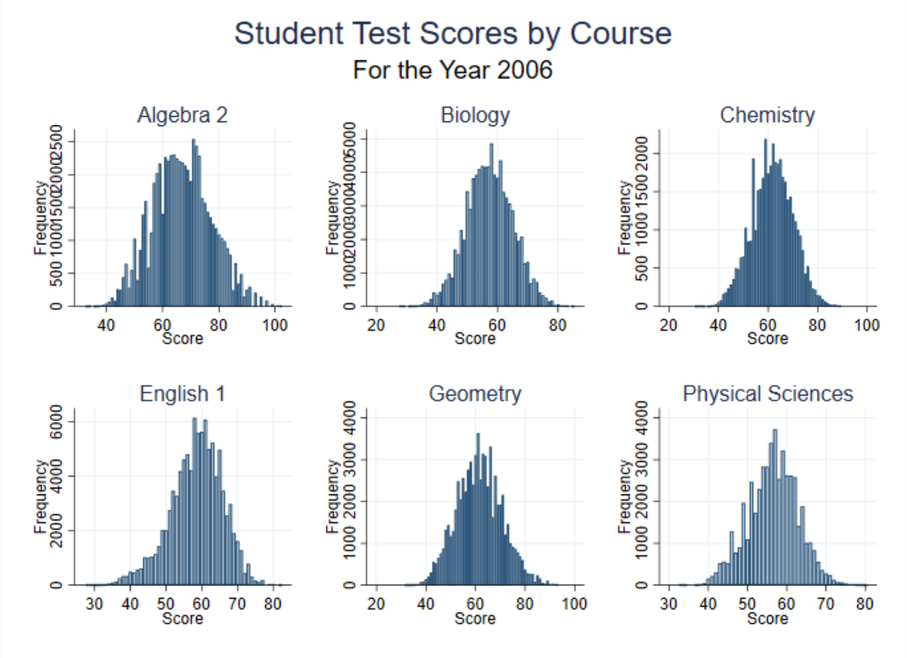
Table A2: Estimates of the Parameters $\{\gamma\}$ Governing the Quintile-Specific Treatment Effect Functions of the Honors Enrollment Fraction $E[\Delta\overline{Y}_q(f)]$ for Several Specifications Testing Robustness to Functional Form and Endogeneity Assumptions

	(1)	(2)	(3)	(4)	(5)
Quintile 1-Linear Coefficient	0.476***	0.709***	0.400***	0.559***	0.428***
	(0.103)	(0.247)	(0.120)	(0.141)	(0.115)
Quintile 2-Linear Coefficient	0.154*	0.173	0.147	0.203*	0.157
	(0.0884)	(0.233)	(0.104)	(0.122)	(0.0966)
Quintile 3-Linear Coefficient	0.0630	0.108	0.0683	0.118	0.108
	(0.0876)	(0.217)	(0.102)	(0.112)	(0.0936)
Quintile 4-Linear Coefficient	0.0394	0.119	0.00936	0.160	0.0547
	(0.0857)	(0.213)	(0.0990)	(0.110)	(0.0931)
Quintile 5-Linear Coefficient	0.0522	0.190	0.0404	0.175	0.103
	(0.0948)	(0.235)	(0.108)	(0.120)	(0.103)
Quintile 1-Squared Coefficient	-1.059***	-1.383**	-0.896***	-1.273***	-0.945***
	(0.279)	(0.661)	(0.335)	(0.378)	(0.315)
Quintile 2-Squared Coefficient	-0.196	0.122	-0.204	-0.392	-0.221
	(0.240)	(0.638)	(0.289)	(0.327)	(0.266)
Quintile 3-Squared Coefficient	0.0352	0.116	-0.0263	-0.203	-0.0963
	(0.234)	(0.573)	(0.274)	(0.297)	(0.252)
Quintile 4-Squared Coefficient	-0.108	-0.122	-0.0862	-0.452	-0.148
	(0.227)	(0.574)	(0.269)	(0.295)	(0.252)
Quintile 5-Squared Coefficient	-0.387	-0.760	-0.407	-0.817**	-0.530*
	(0.258)	(0.645)	(0.292)	(0.331)	(0.288)
Quintile 1-Cubic Coefficient	0.583***	0.674	0.484**	0.726***	0.509**
	(0.188)	(0.434)	(0.243)	(0.270)	(0.228)
Quintile 2-Cubic Coefficient	0.0420	-0.295	0.0610	0.210	0.0752
	(0.163)	(0.423)	(0.216)	(0.239)	(0.198)
Quintile 3-Cubic Coefficient	-0.0982	-0.224	-0.0193	0.121	0.0207
	(0.157)	(0.378)	(0.199)	(0.217)	(0.184)
Quintile 4-Cubic Coefficient	0.0688	0.00331	0.0905	0.325	0.112
	(0.151)	(0.380)	(0.197)	(0.219)	(0.185)
Quintile 5-Cubic Coefficient	0.335*	0.570	0.398*	0.721***	0.462**
	(0.176)	(0.434)	(0.212)	(0.254)	(0.215)
Observations	171,012	171,034	171,118	127,314	171,012
School FEs	YES	YES	NO	YES	NO
Constrained Coefficients	YES	NO	NO	NO	NO
School-Year FEs	NO	NO	YES	NO	NO
Other Course IV	NO	YES	NO	NO	NO
Restricted School Set	NO	NO	NO	YES	NO
Augmented Controls	NO	NO	NO	NO	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the school level are in parentheses. "Constrained Coefficients": Cubic coefficients are restricted so that the treatment effect function equals zero at an honors enrollment share of 1 as well as 0. "Other Course IV" uses the contemporaneous honors enrollment share (and its square and cube) in the other tested courses in the same school-year as instruments for the share and its square in the chosen course. "Restricted School Set": the sample is restricted to schools featuring a distribution of preparedness quintiles such that at most .33 quintile shifts per student are required on average to match the statewide uniform distribution. "Augmented Controls": includes additional controls for free/reduced price lunch eligibility, sets of indicators for various learning disabilities and teacher education categories, and school-average math scores.

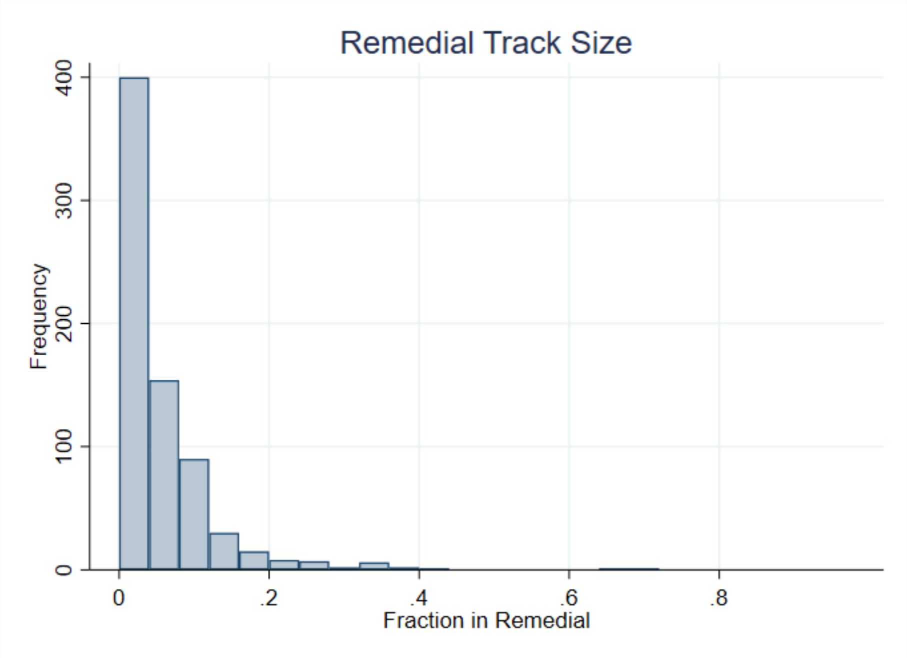
Figures

Figure A1: Confirming the Absence of Floor and Ceiling Effects - The 2006 Empirical Distribution of Pre-Standardized Scale Scores for the Sample Courses



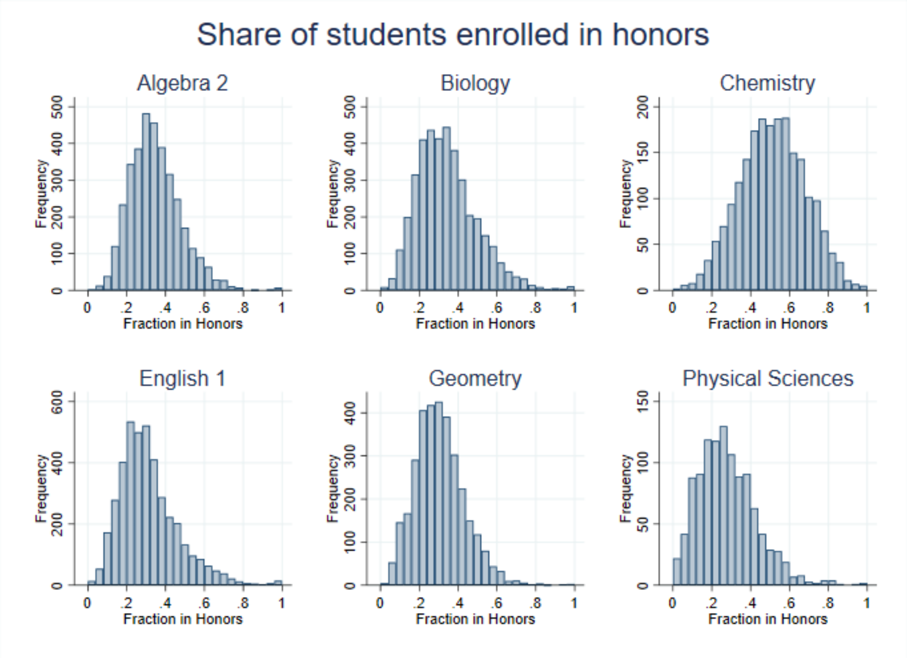
Notes: Each histogram depicts the distribution of pre-standardized student scale scores for the courses included in the final sample for the year 2006. The histograms confirm the absence of bunching near the ceiling or floor of the test score range. More years are available upon request.

Figure A2: The Distribution of Remedial Enrollment Shares



Notes: This figure depicts the fraction of students in the remedial track for school-year-courses from the baseline sample in which a remedial track exists. Fewer than 4% of school-year-courses in the sample contain a remedial track.

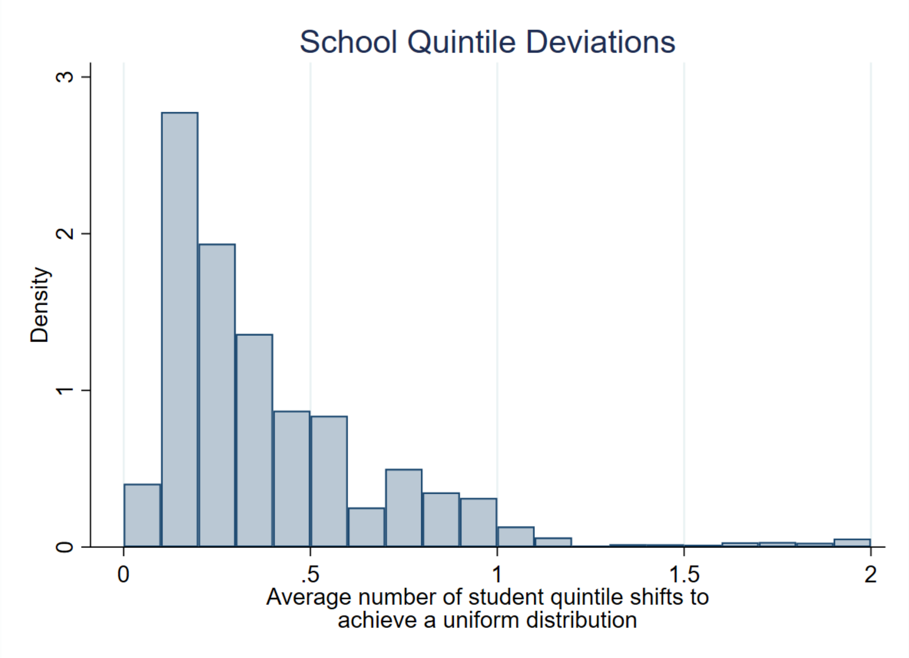
Figure A3: The Distribution of Honors Track Enrollment Shares by Course



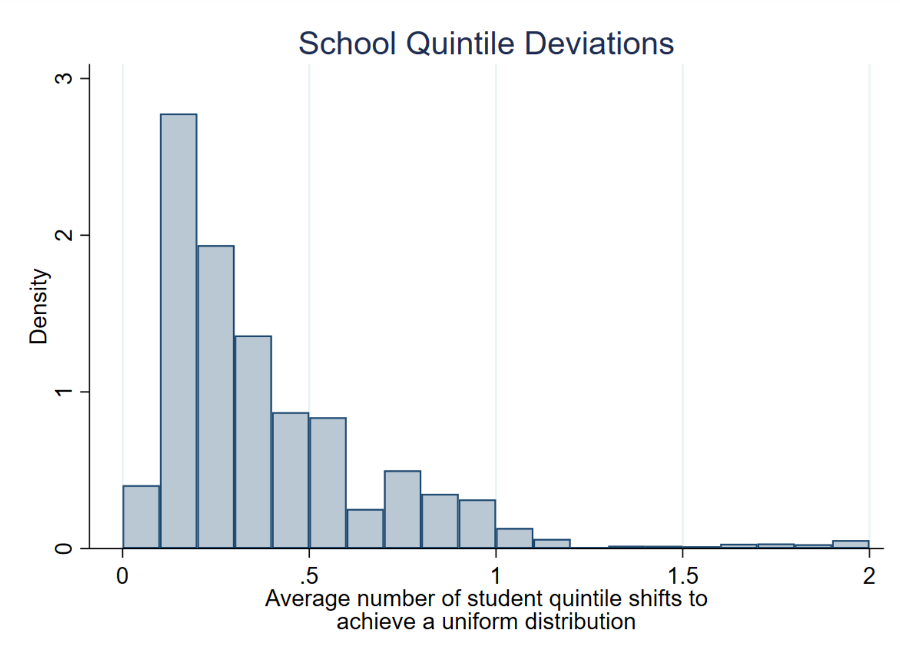
Notes: Each figure depicts the distribution of the fraction of students who enroll in the honors track for school-year-courses in which an honors track exists for the labeled course. The figures rely on the baseline sample of school-course-year observations (See Section 3.1 for details).

Figure A4: Assessing the Validity of Assumption 1 - The Distribution of School-Specific Departures from the Statewide Composition of Student Predicted Performance

(a) School-Weighted Distribution

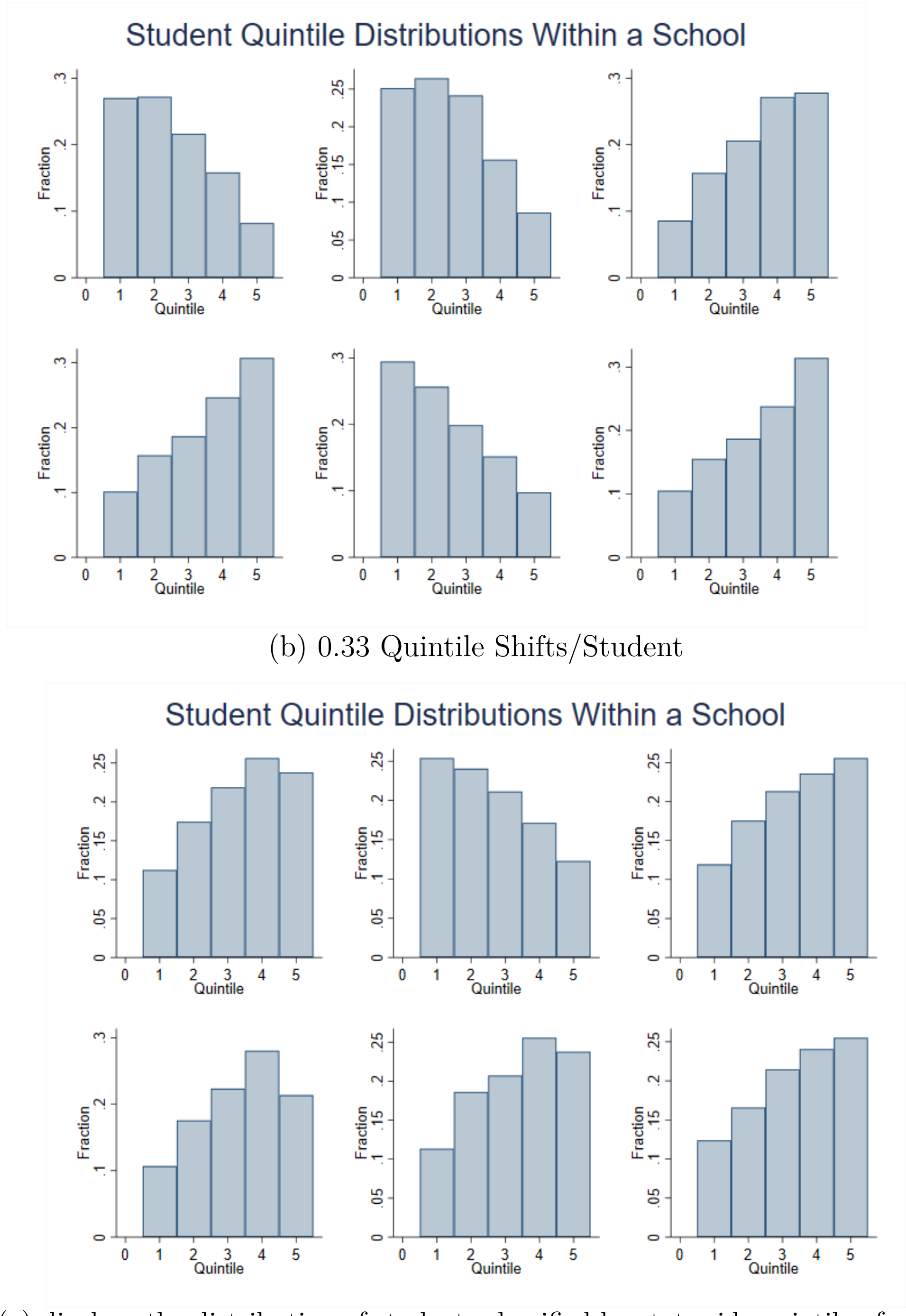


(b) Student-Weighted Distribution



Notes: This figure displays the school-weighted (Panel A) and student-weighted (Panel B) distributions among high schools of the average number of quintiles of an index of predicted test score performance by which the school's students would need to be shifted to match the statewide (uniform) distribution of student predicted performance quintiles. Larger values indicate that the school's student population is more atypical.

Figure A5: The Distribution of Student Predicted Performance Quintiles for the Schools on the Margin of Sample Inclusion

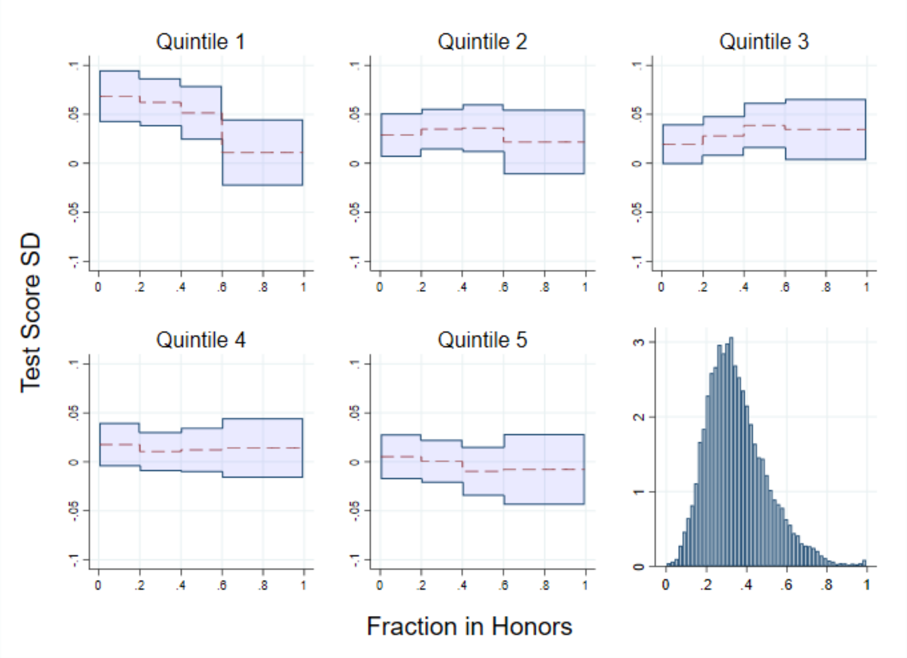


(a) 0.5 Quintile Shifts/Student

(b) 0.33 Quintile Shifts/Student

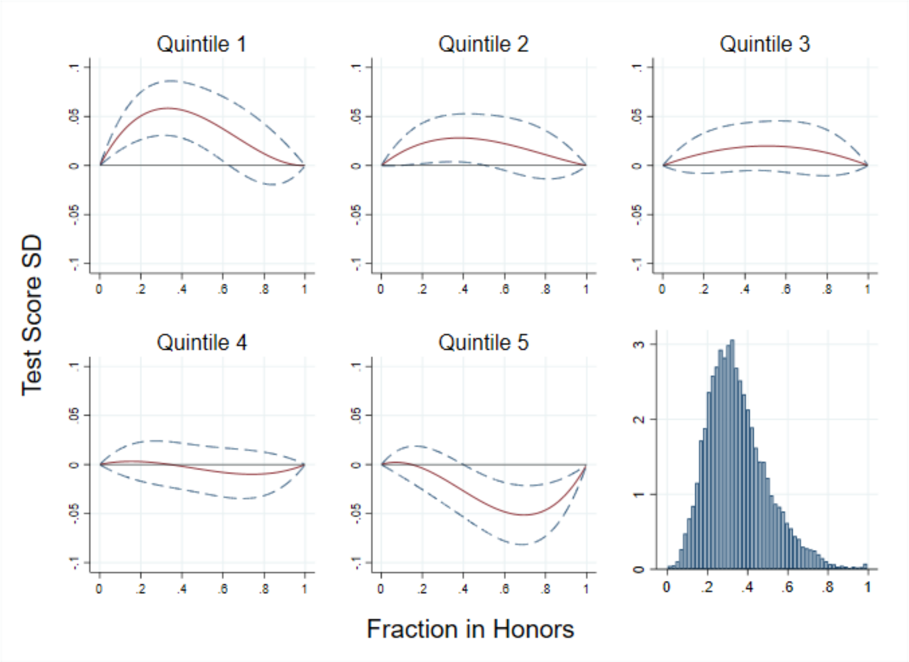
Notes: Figure (a) displays the distribution of students classified by statewide quintile of a regression index of predicted test scores for the six schools with the highest deviations from the statewide (uniform) distribution of quintiles that still qualified for the baseline sample (0.5 required quintile shifts per student on average to reach the uniform distribution). Figure (b) plots the distributions for the six marginal schools when the standard is lowered to one-third quintile shifts per student.

Figure A6: Testing Robustness to Alternative Functional Forms for the Treatment Effect Functions $E[\Delta\overline{Y}_q(f)]$ - Estimating Quintile-Specific Treatment Effects Separately by 20% Interval of Honors Enrollment Share



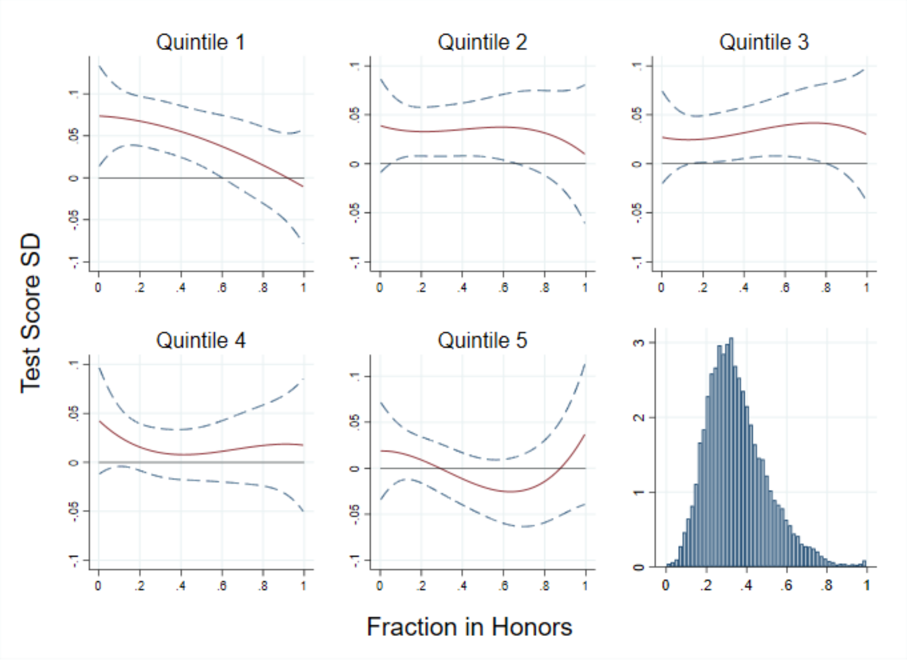
Notes: The first five graphs plot estimates of the function $E[\Delta\overline{Y}_q(f)]$ that maps coursewide honors enrollment fraction into expected standardized test performance by quintile of predicted performance for a school FE specification that replaces the baseline cubic polynomial with interactions between indicators for student preparedness quintile and indicators for whether the current course' honors share falls in a particular interval of width 0.2 (with the last two intervals combined due to minimal support). The bottom right graph in each panel displays the density of honors enrollment shares for the chosen sample. 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes. The figures rely on the baseline sample of school-course-year-quintile observations (See Section 3.1 for details).

Figure A7: Testing Robustness to Alternative Functional Forms for the Treatment Effect Functions $E[\Delta\overline{Y}_q(f)]$ - Restricted Cubic Specification



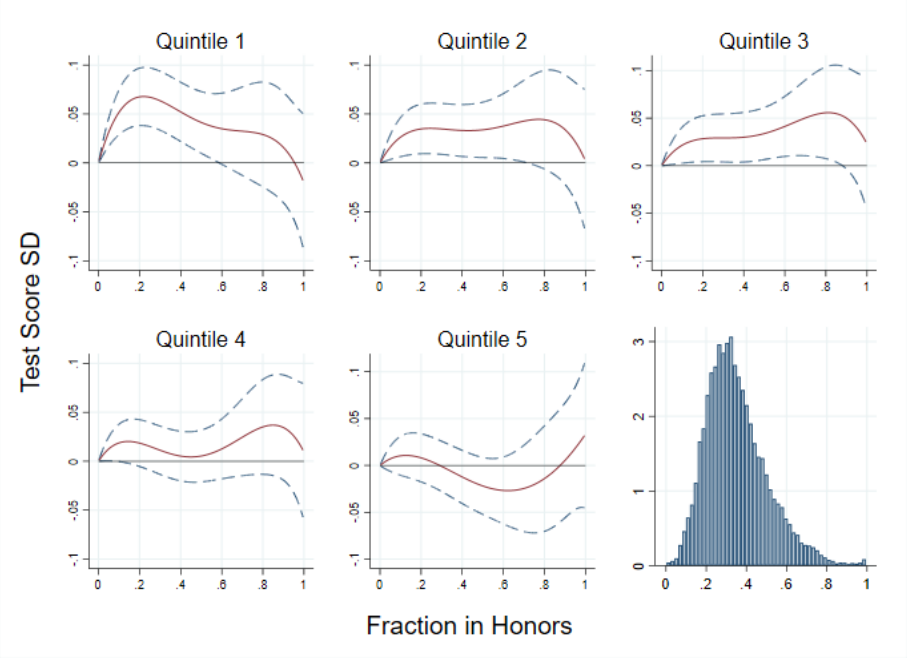
Notes: This figure plots estimates of the function $E[\Delta\overline{Y}_q(f)]$ that maps coursewide honors enrollment fraction into expected standardized test performance by quintile of predicted performance for a school FE specification that restricts the value of the treatment effect to be zero at the right end of the unit interval in addition to the left end in order to capture the idea that 100% of students in the honors track also represents an absence of meaningful tracking. 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes. The bottom right graph in each panel displays the density of honors enrollment shares for the chosen sample. The figures rely on the baseline sample of school-course-year-quintile observations (See Section 3.1 for details).

Figure A8: Testing Robustness to Alternative Functional Forms for the Treatment Effect Functions $E[\Delta\overline{Y}_q(f)]$ - Discontinuity Permitted at a Zero Honors Enrollment Share



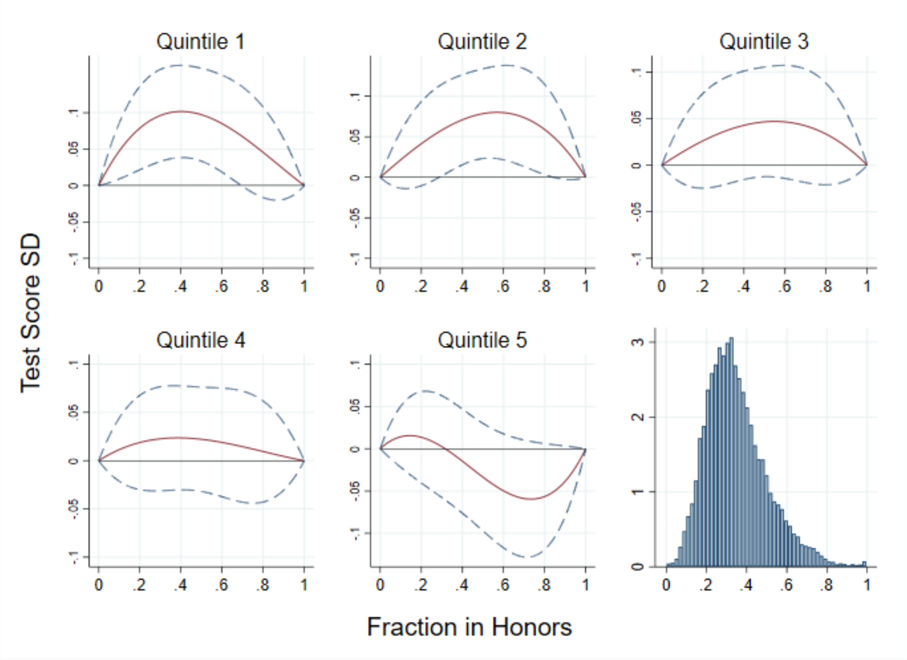
Notes: This figure plots estimates of the function $E[\Delta\overline{Y}_q(f)]$ that maps coursewide honors enrollment fraction into expected standardized test performance by quintile of predicted performance for a school FE specification that also includes a separate indicator for whether the course features any tracking. This ensures that predicted values at low enrollment shares are not affected by performance in untracked schools or courses. 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes. The bottom right graph in each panel displays the density of honors enrollment shares for the chosen sample. The figures rely on the baseline sample of school-course-year-quintile observations (See Section 3.1 for details).

Figure A9: Testing Robustness to Alternative Functional Forms for the Treatment Effect Functions $E[\Delta\overline{Y}_q(f)]$ - Quartic Specification



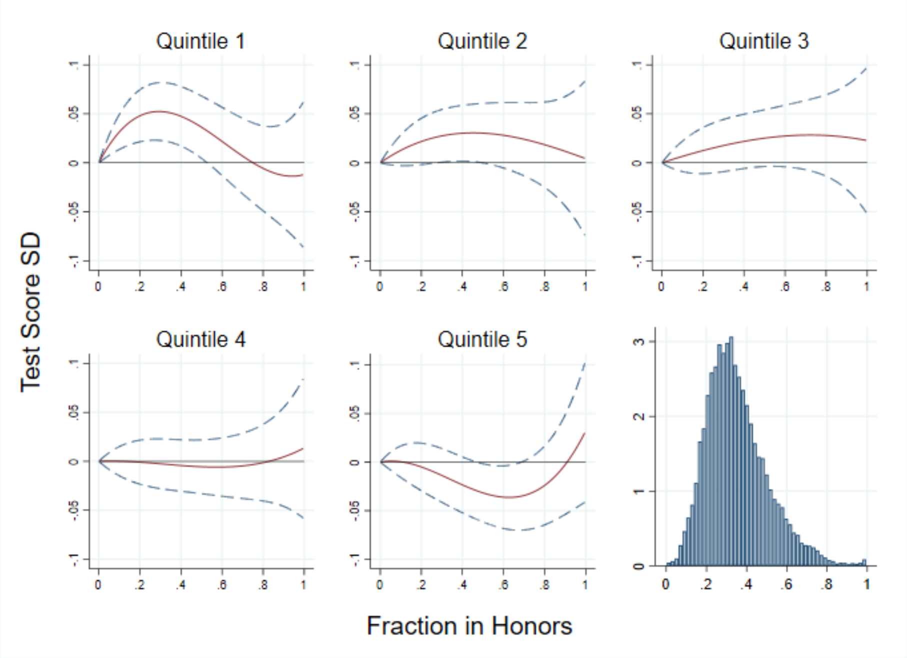
Notes: This figure plots estimates of the function $E[\Delta\overline{Y}_q(f)]$ that maps coursewide honors enrollment fraction into expected standardized test performance by quintile of predicted performance for a school FE specification that parameterizes the treatment effect function as a quartic rather than cubic polynomial. 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes. The bottom right graph in each panel displays the density of honors enrollment shares for the chosen sample. The figures rely on the baseline sample of school-course-year-quintile observations (See Section 3.1 for details).

Figure A10: Testing Robustness to Alternative Functional Forms for the Treatment Effect Functions $E[\Delta\overline{Y}_q(f)]$ - Using the Share of Honors Classrooms as an Instrument for the Honors Enrollment Share



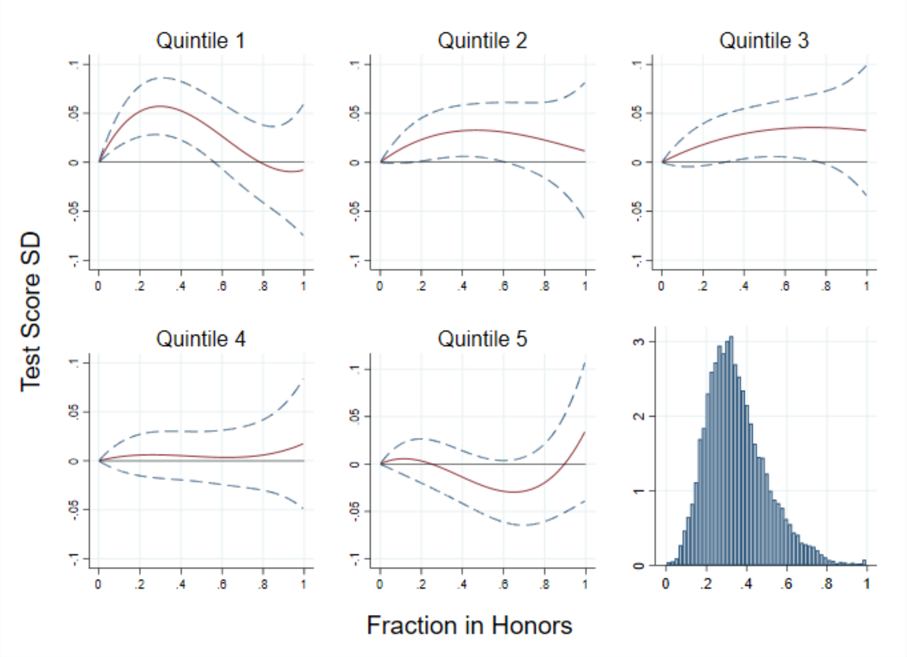
Notes: This figure plots estimates of the function $E[\Delta\overline{Y}_q(f)]$ that maps coursewide honors enrollment fraction into expected standardized test performance by quintile of predicted performance for a specification in which the current course's honors enrollment share is instrumented with the mean share among other courses in the same school-year combination. 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes. The bottom right graph in each panel displays the density of honors enrollment shares for the chosen sample. The figures rely on the baseline sample of school-course-year-quintile observations (See Section 3.1 for details).

Figure A11: Testing Robustness to Alternative Functional Forms for the Treatment Effect Functions $E[\Delta\overline{Y}_q(f)]$ - Specification Featuring School-Year Fixed Effects



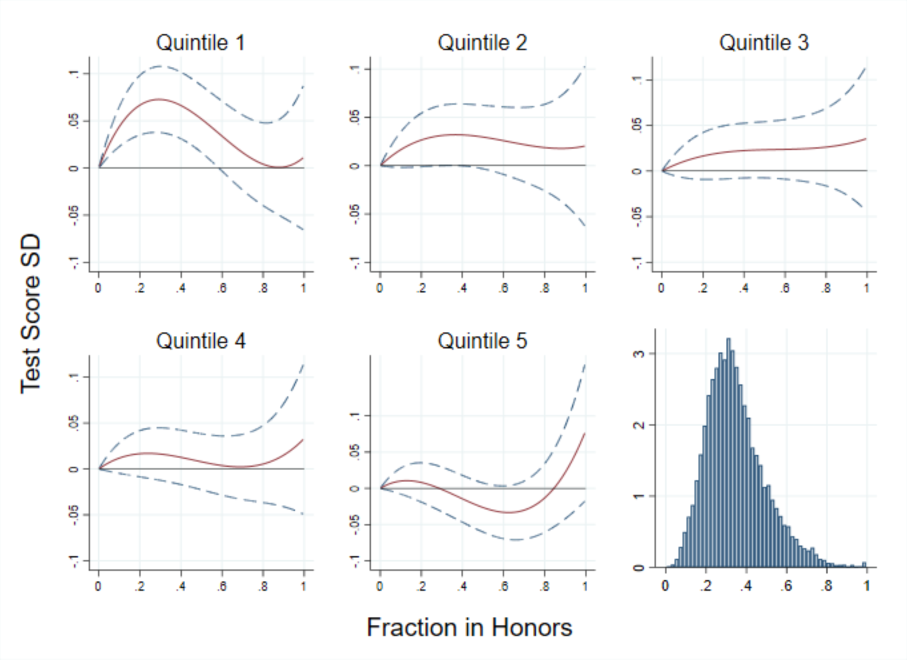
Notes: This figure plots estimates of the function $E[\Delta\overline{Y}_q(f)]$ that maps coursewide honors enrollment fraction into expected standardized test performance by quintile of predicted performance for a specification that augments the baseline specification by including a set of school-year fixed effects. 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes. The bottom right graph in each panel displays the density of honors enrollment shares for the chosen sample. The figures rely on the baseline sample of school-course-year-quintile observations (See Section 3.1 for details).

Figure A12: Testing Robustness of the Treatment Effect Functions $E[\Delta \overline{Y}_q(f)]$ to Additional Controls - Low Income and Learning Disability Indicators, and Teacher Education Category Shares



Notes: This figure plots estimates of the function $E[\Delta \overline{Y}_q(f)]$ that maps coursewide honors enrollment fraction into expected standardized test performance by quintile of predicted performance for the school fixed-effects specification but using an augmented set of controls that includes a common but coarse administrative indicator for low parental income, indicators for various forms of learning disabilities, shares of the teachers in the chosen school-course year who received bachelor's, master's, professional, and PhD degrees. 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes. The bottom right graph in each panel displays the density of honors enrollment shares for the chosen sample. The figures rely on the baseline sample of school-course-year-quintile observations (See Section 3.1 for details).

Figure A13: Testing Robustness to Violations of Assumption 1 - Specification Featuring a Restricted Sample of Schools Featuring More Typical Distributions of Predicted Student Performance Based on Middle School Performance



Notes: This figure plots estimates of the function $E[\Delta\overline{Y}_q(f)]$ that maps coursewide honors enrollment fraction into expected standardized test performance by quintile of predicted performance for the school fixed-effects specification but using an alternative sample that restricts the set of schools to those where the average student would need to shift their quintile of the preparedness index by less than 1/3 in order for the school to match the statewide (uniform) distribution of quintiles. 95% pointwise confidence intervals computed using the delta method are displayed with blue dashes. The bottom right graph in each panel displays the density of honors enrollment shares for the chosen sample.